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The Effects of the Legal Minimum Working Time on Workers, Firms and the Labor Market

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Abstract

This paper examines the effects of working time regulations on the allocation of workers and hours. I exploit a unique reform introducing a minimum workweek of 24 hours in France in 2014, affecting 15% of jobs. Drawing on administrative data and an event study design, I find a firm-level reduction in total hours worked, showing imperfect substitutability between workers and hours. The effects differ by gender: women working part-time were replaced by men working longer hours. Importantly, workers also reallocate between firms. To quantify the aggregate impact accounting for these effects, I build and estimate a search and matching model with firm and worker heterogeneity. Overall, the minimum workweek reduced employment by 1.4%, largely driven by women, and decreased total hours by 0.5%.

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Keywords: Working time regulations, Hours of work, Reallocation effects, Gender inequality

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1 Introduction

Hours of work are not only determined by labor demand and labor supply, but are also shaped by working time regulations. These institutions include the legal definition of the full-time workweek, the maximum number of hours and the conditions for the use of overtime. Policymakers often introduce changes in these regulations to achieve specific objectives. For instance, work-sharing policies have reduced the full-time workweek in many countries with the aim to reduce unemployment (OECD 2021). The effects of working time regulations depend on the degree of substitutability between workers and hours and on the allocation of jobs between firms. Furthermore, allocating hours differently may have distributional effects as workers supply hours heterogeneously.

This paper exploits a unique reform implementing a minimum workweek in France to provide new evidence on how working time regulations affect the allocation of workers and hours. The reform consists of a legal minimum working time of 24 hours per week, targeting new hires starting in 2014. The policy shock was sizable as 15% of jobs had a workweek below 24 hours before that date. To the best of my knowledge, this is the first paper on the effects of a minimum workweek. I provide evidence on the effects at several levels: worker, firm, market and aggregate. I first quantify these effects for all workers and then decompose them between men and women. I offer a comprehensive assessment by combining administrative data, reduced-form methods and a structural general equilibrium model. This allows me to document the effects of a minimum working time on employment (in terms of jobs and hours), welfare and gender inequality. Beyond the causal effects of the policy, my analysis provides a better understanding of (i) the labor demand determinants of hours, especially the firm-level substitutability between hours and workers, (ii) the reallocation effects of labor market reforms between firms, and (iii) gender heterogeneity in labor supply and in the effects of working time regulations. My analysis proceeds in three steps.

In a first step, I rely on French linked employer-employee data and an event study design to estimate the impact of the minimum workweek at the firm level. My empirical strategy leverages firm-level differences in the pre-reform share of jobs with a workweek below 24 hours. Identification assumes that firms with different shares of affected jobs would have had the same evolution in employment, had the reform not been implemented. I show that this assumption is credible over the pre-reform period.

I find that the minimum workweek decreased the number of workers employed in the firm. This negative extensive-margin effect is driven by a reduction in low-hour jobs. Meanwhile, there is a positive effect on average hours per job (intensive margin effect), driven by an increase in the number of full-time workers. Overall, the negative extensive margin effect dominates: total hours worked in the firm decrease. An initial share of jobs below 24 hours higher by 1 percentage point results in a 0.2% decrease in hours. This result suggests that firms cannot flexibly allocate hours between workers.

Importantly, the minimum workweek has different effects for men and women. The firm-level decrease in total hours is mostly driven by a reduction in female employment. Two channels explain this pattern. First, the negative impact on the number of jobs is stronger for women. Second, the increase in the number of jobs with longer hours (i.e. at least 35 hours) is stronger for men. Pre-reform, women are more likely to work below 24 hours (24% of female employment) than men (10%). Therefore, I decompose these gender gaps and show that 20–25% of the differential effects on total hours and employment reflect heterogeneous responses of male and female workers to the same initial exposure, and the remaining share reflects differences in composition. The changes by type of jobs are purely driven by the heterogeneous response: jobs below 24 hours are more likely to prevail for women, and men are more likely to get access to full-time employment. These results hold within industries and occupations, suggesting that men working longer hours have replaced women with low hours for the same jobs.

In a second step, I go beyond firm-level estimates and document worker-level effects, between-firm reallocation, and market-level adjustments. The goal is to identify which workers gain access to full-time jobs and whether the reform induces reallocation across firms. I first document that, in the pre-reform period, work hours are highly persistent across job spells, allowing workers to be classified by their typical attachment to low-hour jobs. Leveraging this persistence, I show that workers with a history of low-hour employment account for the negative employment effects. While the firm-level analysis shows that full-time employment rises primarily for men, the worker-level evidence reveals that these are men previously working long part-time hours (23–34h). Finally, I find evidence of reallocation: workers previously in low-hour jobs are more likely to move to firms with a lower pre-reform part-time share.

To analyze between-firm effects more systematically, I study how market-level exposure (industry $\times$ commuting zone) shapes firm-level responses. Using a triple-difference design, I compare more versus less exposed firms across labor markets with high versus low overall exposure to the reform. Average market-level exposure attenuates firm-level employment effects by 24% for men and 12% for women. Consistently, market-level event studies yield more muted employment responses than firm-level estimates. Those results suggest that the aggregate effects of the minimum workweek can be different from the firm-level estimates. A structural general equilibrium model allows me to take into account both the firm-level effects as well as indirect reallocation.

In a third step, to quantify the aggregate impact of the minimum workweek, I build and estimate a search and matching model with two-sided heterogeneity. The framework features random search, multi-job firms, and right-to-manage: wages are bargained, after which firms choose hours. Men and women differ ex-ante with respect to their labor disutility, as suggested by differences in preferred working time in the French Labor Force Survey. Firms are heterogeneous in productivity, and matches have an idiosyncratic productivity component. Under right-to-manage, firm-level effects are a priori ambiguous: pre-reform hours may be inefficiently high or low. The model also incorporates general equilibrium adjustments through labor market tightness and workers' outside options, which can operate in the opposite direction of firm-level effects. I develop an estimation strategy allowing me to separately identify labor supply and labor demand parameters. I combine information on preferred working time from the Labor Force Survey, with data from job ads, informative about firms' demand in hours, and administrative data on actual hours worked in the economy. The policy parameter corresponding to the minimum workweek is identified using the reduced-form result.

The estimated model shows that, while most low-hour jobs feature hours below the efficient level, the gap is generally between one and two hours per week, limiting the scope for welfare improvements. Accounting for both firm-level and general-equilibrium adjustments, I estimate that the minimum workweek reduced employment by 1.4% (2.6% for women and 0.15% for men). Importantly, the model uncovers a large attenuation of the negative effects through general-equilibrium reallocation: total hours fell by 0.5%, but the decline would have been 2.0% without the adjustment in labor market tightness and unemployment values. Finally, I find a 8.1% decrease in average worker welfare, driven by lower employment probabilities and higher work disutility insufficiently compensated by wages.

I use the estimated model to evaluate alternative hour floors, the role of full-time regulations, and their interaction. First, the negative employment and welfare effects, as well as the increase in gender inequality, grow with the bite of the floor. Second, the floor on hours would have had very similar effects if implemented in a context without the pre-existing 35-hour regulation. Third, full-time restrictions (such as the 35-hour rule) reduce worker welfare but raise employment: most jobs are maintained but at lower worker value, which expands the set of acceptable matches. Because these jobs arise through lower outside options rather than direct exposure, they are spread across all firms and may be missed by empirical strategies relying on cross-firm variation.

Taken together, these results indicate that the reform did not achieve its main objectives. While the policy aimed to help low-hour workers transition into longer-hour jobs, a substantial fraction of them no longer hold a job after the reform. Rather than reducing inequality driven by differences in working time, the policy has amplified it by widening the gap between workers who are now unemployed and those in longer-hour positions.

This paper contributes to three strands of the literature.

First, I add to the literature on policy evaluation and on the effects of working time regulations. Several papers have studied the effects of reductions of the full-time workweek (Hunt (1999), Marimon & Zilibotti (2000), Crepon & Kramarz (2002), Rocheteau (2002), Chemin & Wasmer (2009), Raposo & van Ours (2010), Goux et al. (2014), Lopes & Tondini (2022), Batut et al. (2023)). These papers find no employment increase in response to workweek reductions, consistent with my firm-level results indicating limited substitutability between workers and hours. My model assesses the aggregate and welfare effects of a workweek reduction (and how it interacts with a floor on hours). Focusing on overtime coverage in the U.S., Quach (2024) also finds results indicating costs to substitute hours for workers. The existing literature also provides evidence on the effects of regulating atypical types of contracts (Scarfe (2019) and Dolado et al. (2022) on zero-hour contracts in the United Kingdom, and Carrillo-Tudela et al. (2021) on mini-jobs in Germany). This paper exploits a new type of regulation, minimum working hours, to study the effects of restricting part-time jobs. I uncover two types of effects. I show that average policy effects mask substantial composition effects, especially by gender, by quantifying the respective contributions of differential exposure and heterogeneous responses. Also, I quantify large general equilibrium effects in response to working time regulations. Reallocation or indirect effects of labor reforms have been documented with other types of policies (Crépon et al. (2013), Hagedorn et al. (2013), Dustmann et al. (2021), Giupponi & Landais (2022)). This paper contributes to the literature on the aggregate effects of labor reforms (Hopenhayn & Rogerson (1993), Flinn (2006), Gautier et al. (2018), Boone et al. (2021), Engbom & Moser (2022), Cahuc et al. (2022)) by combining reduced-form evidence with a structural model.

Second, this paper adds to the literature on the determination of working hours and firms labor adjustments. A large literature focuses on part-time jobs and hours (Altonji & Paxson (1988), Aaronson & French (2004), Hirsch (2005), Blundell et al. (2008), Prescott et al. (2009), Booth & Ours (2013), Kline & Tartari (2016), Devicienti et al. (2018), Devicienti et al. (2020), Kopytov et al. (2023), Borowczyk-Martins & Lalé (2019), Bick et al. (2022), Labanca & Pozzoli (2022), Lachowska et al. (2023), Jarosch et al. (2025)). This paper considers jointly the role of labor supply and labor demand in the determination of hours as well as both intensive and extensive margin adjustments. The firm-level effects provide new evidence on how firms allocate hours between workers and the lack of substitutability between the two. The estimation of labor supply parameters by gender also contributes to the literature on gender differences in labor supply (Flabbi & Moro (2012), Goldin (2015), Kleven et al. (2019), Erosa et al. (2022)).

Third, I build a new structural model with intensive and extensive margin adjustments and develop an identification strategy for the two-sided heterogeneity. My framework contributes to the search and matching literature and especially models with hours. I add to Bloemen (2008) and Frazier (2018) by including both intensive and extensive margin adjustments in a general equilibrium framework. My model features two-sided heterogeneity and hence differs from Cooper et al. (2007), Fang & Rogerson (2009), Cooper et al. (2017), Dossche et al. (2019) and Kudoh et al. (2019). Combining data from job ads, employment records and the Labor Force Survey, I identify separately labor supply and labor demand parameters, an estimation procedure that could be used in different contexts.

The rest of the paper is structured as follows. Section 2 describes the institutional context and the minimum workweek reform. Section 3 presents the data and some aggregate descriptive evidence. In Section 4, I detail the reduced-form strategy and the firm-level effects of the reform. In Section 5, I present worker-level, between-firm and market-level evidence. I introduce the structural model, its estimation, and the results for the aggregate impact in Section 6. Section 7 concludes.

2 The reform implementing the minimum workweek

2.1 Institutional context

The legal minimum working time was introduced in France for the first time in 2014. However, there were already several regulations affecting working hours. These regulations include (1) the working time of full-time jobs, (2) rules regarding the use and compensation of overtime hours as well as (3) the maximum legal working time. First, individual labor contracts specify the regular number of hours of work per week and the compensation. Since 2002, the regular full-time workweek has been equal to 35 hours.[1] A few firms and industries have exceptions to the 35h-rule and can implement a workweek between 35 and 39 hours.

Second, hours worked on top of contractual hours are overtime hours. They are subject to specific rules. For full-time workers, overtime hours are paid at a higher rate than standard hours. This rate depends on the size of the firm. For employees working fewer than 35 hours per week, overtime hours are paid at the same rate as contractual hours but are subject to a limit of $1/10^{th}$ of contractual hours.

Third, there is a legal maximum number of hours worked, decided at the European Union level. A worker can never work more than 48 hours per week, including overtime. Furthermore, the working time should not exceed 44 hours per week on average over a period of 12 weeks.

On top of working time laws, two additional regulations shape the design of labor contracts in France. First, there is a national hourly minimum wage, supplemented by minimum wages in collective agreements that are industry and occupation-specific. Second, there are two main types of labor contracts: fixed-term and open-ended contracts. Fixed-term contracts have a specified duration while open-ended contracts can only be terminated under specific circumstances. Rules regarding hours worked are the same for both types of contracts.

2.2 The 24h minimum workweek

Reform objectives. In July 2014, the French government introduced a legal minimum workweek of 24 hours per week,[2] motivated by three objectives: to raise earnings for low-

[1] The French full-time workweek was 39 hours until 2000. From 2000 to 2002, most firms implemented the 35h full-time workweek.

[2] Or equivalently, 104 hours per month when contracted on a monthly basis.

income workers, reduce gender inequality (given women's overrepresentation at low hours), and curb involuntary part-time employment. Appendix A.2 documents these goals with excerpts from the law's presentation to the National Assembly.

Figures A.1 and A.2 show a large pre-reform earnings gap between jobs below and above 24 hours. While the policy partly targeted a presumed hourly-wage penalty for part-time work, the evidence is more nuanced. In the raw data, hourly wages rise with hours, but conditional on age, occupation, and firm type, the relationship is U-shaped: very short-hour and very long-hour jobs pay more per hour, while jobs with 20-30 hours pay less. Finally, although low-hour workers are more likely to prefer longer hours, two thirds of part-time employees report being satisfied with their hours in 2013 (Figure A.3).

Policy. The reform introduced a minimum number of working hours for jobs created after July 2014. While there is no systematic check of compliance with the policy, workers can sue their employers in labor court for a working time lower than 24h. In that case, the judge will decide on a compensation usually equal to the wages the worker would have had, had she been working 24h.[3] The minimum workweek was implemented with some exceptions. The primary exception permits workers to request jobs with fewer than 24 hours per week. In practice, the worker should explain in a letter the reasons why she prefers to work fewer than 24h (e.g. family constraints). The letter is then given to the employer as a proof that the worker is asking for an exception. However, the risk of being sued still exists in this case. There are also cases in which the judge ruled in favor of the worker, even though the employer presented the letter, as there was a risk that the worker signed the letter under pressure. This exception is not directly observed in the data. Because of the way the policy is enforced and this exception, the reform can be seen as making jobs with working time below 24h more costly and risky for employers, rather than a strict ban.

Exceptions. The minimum workweek policy allowed for other types of exceptions that are more specific and in some cases, observable in the data. An exception can apply if (1) the worker is a student younger than 27, (2) the worker is employed by a household (e.g. gardener, housekeeper), (3) the job is a fixed-term contract lasting less than a week, (4) the job is a fixed-term contract used to temporarily replace a worker on sick leave usually working fewer than 24h. Moreover, when the policy was implemented, the government allowed for the possibility to negotiate collective industry agreements with exceptions to the 24h rule. I collected these agreements and found that 40 industries have bargained exceptions since 2014 (see Online Appendix A.3 for more details). In the data, I am able to identify firms and jobs covered by such agreements and exclude them for the analysis. Industries with exceptions

[3] Labor court rulings are not systematically digitalised, making it impossible to know the number of workers going to court on that ground. However, there are many examples of compensations for working time below 24h decided in appeal courts, which are published by the Ministry of Justice.

employ 4% of the workforce over the pre-reform period and account for 6% of jobs with fewer than 24 hours in 2013. Two facts can be emphasized about these agreements. First, many agreements apply only to very specific occupations. For instance, in the sports equipment retail industry, the minimum number of hours is 24 for all workers except accountants and cleaning staff. Second, a large share of these agreements specify minimum working hours above 10 hours per week. For instance, it is 18 hours for publishing activities, 14 hours for zoological parks and 16 hours for medical biology laboratories. Figure A.4 shows stable employment and firm shares in those industries after the reform implementation. I am not using those industries as a control group for the effects of the reform, because (i) most also implement a minimum working time, (ii) the fact that unions of employers managed to negotiate such an exception is endogenous to the potential effects of the reform.

Timing. The minimum workweek was implemented in July 2014 but announced in June 2013. A few changes occurred between the announcement and the implementation. First, the minimum number of hours was supposed to be mandatory for hires starting January 1^{st}, 2014, while firms would benefit from a two-year transition period for jobs created before 2014. Hence, the law initially targeted all jobs. In January 2014, the government decided to postpone its application arguing that firms were not ready to change their organization. It was decided that the reform would finally be effective in July 2014. On July 1^{st}, 2014, the mandatory minimum number of hours was implemented for new hires. Finally, in January 2015, the government announced that workers hired before the implementation of the reform would not be subject to the minimum workweek. Hence jobs already created finally did not have to comply. As a result of this change, it is very unlikely that firms anticipated the implementation of the law by hiring more workers with fewer than 24h since the reform was supposed to be extended to all jobs at the end of the transition period.

Other policy changes. The 24h-rule was part of a package of labor market reforms (*Loi Sécurisation de l'Emploi*) aiming at improving labor market trajectories. On top of the minimum workweek, another change targeted part-time jobs. Before the reform, part-time workers were not allowed to work more overtime hours than $1/10^{th}$ of contractual hours. The law removed this cap, increased the wage rate for hours below $1/10^{th}$ of contractual hours by 10% and the wage rate for hours above this cutoff by 25%. This regulatory change is unlikely to have significant effects for two reasons. First, before 2014, firms did not rely much on overtime hours for part-time workers, even if there was no wage premium. In 2010, 34% of part time workers were working overtime hours. On average, they were doing 14 minutes per week of overtime hours (Pak 2013). Second, other papers have found no effect of policies affecting the compensation of overtime hours in the French context (Cahuc & Carcillo 2014). I describe the other policy changes, that are unrelated to the working time, as well as details about the legislative process in Online Appendix A.

3 Data and descriptive evidence

3.1 Data

To investigate the effects of the minimum workweek, I combine rich data at the firm and worker levels. First, I rely on administrative data to compute job- and firm-level outcomes. Second, I use survey data and job ads to recover information on labor supply and labor demand.

Administrative data. The main job-level and firm-level outcomes are measured from the French employment records, the *Déclarations Annuelles des Données Sociales* (DADS) over 2003-2017. They are built by the French Institute of Statistics (INSEE) and provide information at the job spell level from firms' mandatory fiscal declarations. Every year, firms have to declare, for each job $\times$ worker, the wage, the number of hours worked and the length of the employment spell in days. The data include demographic details such as age and gender. They also include the type of contract (open-ended or fixed term), industry and occupation codes. I restrict the sample to private sector firms. I exclude workers employed by households or associations because these contracts are subject to specific rules. I also exclude workers in temporary agencies because we cannot identify in which firm they actually work. The data provide the collective agreement number, hence allowing to identify the firms covered by exceptions to the reform.

For each year and each firm, I compute the average workweek of each job spell using the total number of hours worked, the starting date and the end date of the job spell. This has two implications. First, the total number of hours observed for each job spell is the number of hours paid by firms, which is the sum of contractual and overtime hours. It is not possible to distinguish between the two. Second, I cannot observe variations in working time within the year. I aggregate spell-level data to recover employment, total hours worked and the share of women at the firm level. Finally, firms can be followed over time using a unique identifier.

Online Appendix Table B.1, presents descriptive statistics on jobs with workweek below and above 24h before the implementation of the minimum workweek. Employees working fewer than 24h in 2013 are more likely to be women (58% against 39% for jobs above 24h) and in low-skilled white-collar occupations. These jobs are over-represented in the services, and especially in accommodation and food services (15% of jobs below 24h are in accommodation and food while this industry represents 9% of jobs above 24h). The large differences in the share of jobs below 24h between industries and occupations suggest structural differences in firms' demand for these jobs.

Finally, I combine the DADS with balance sheet data (Ficus-Fare) to investigate additional firm-level outcomes. These data provide information on value added, the stock of capital, total wage bill and purchased services. The French Institute of Statistics compiles these data from firms' tax declarations.

Other data. I complement the administrative information with data on firms' recruitment behavior and on workers' preferences. The former is obtained from job ads. The French Public Employment Service (*Pôle Emploi*) administers a centralized job search platform. This platform offers employers the possibility to include job ads with a standard application procedure. Pôle Emploi estimates that they deal with about 50% of the total French vacancies.[4] Since 2013, the number of hours of work required for the job has been observed for 70% of job ads. Half of the vacancies contain information on both the number of hours and the hourly wage. Applying the same industry restrictions as for the administrative data, there are 744,293 job ads in 2013 in which both the required hours and the wage are observed.

Information on the worker side is obtained from the French Labor Force Survey. Each year, the survey is administered to a representative sample of about 75,000 households. Interestingly, employed workers are asked both about their preferred working time, keeping the current hourly wage constant, and about their actual working time. The Labor Force Survey is also useful to observe additional demographic characteristics not observed in the linked employer-employee data (see Online Appendix Table B.2 for descriptive statistics by household composition and hours of work).

3.2 Aggregate descriptive evidence

In Figure 1, I document the aggregate evolution in the use of jobs below 24h. Panel (a) shows the evolution of the share of new hires for jobs with a workweek below 24h, to consider the bite and compliance with the reform. I separately consider all industries and the ones not affected by exceptions. From 2002 to 2013, the share of new jobs with fewer than 24 hours increased. In 2013, the year before the implementation of the minimum workweek, 30% of new hires were for jobs with working time below 24h (industries with exceptions excluded). From 2014 onwards, this share progressively declined, dropping to 15% by 2017. The reform was hence followed by a large decrease in hires below 24h. Because workers can ask for an exception and the absence of systematic enforcement, we don't expect the share to reach 0%. The lack of an immediate sharp decline suggests incomplete awareness of the policy among workers and employers upon its implementation.[5] Panel (b) shows that for women, jobs below 24h represented 40% of new hires in 2013, while it was 23% for men. Following the reform, the share was divided by two for both women and men, reaching 20% and 11% respectively in 2017. The evolution of the share of jobs with fewer than 24h in the stock of jobs followed a similar evolution (Panel (c)), even if the share in the stock was initially lower than for new

[4] See Cahuc et al. (2018) for details.

[5] Online Appendix Figure A.4 shows a drop in jobs below 24h as well in industries with exceptions, as those also implemented minimum working times, although lower than 24. Figure B.1 decomposes the evolution by industry and by firm size and shows that even if initial shares are heterogeneous between firms, there is a decrease for all types of firms.

hires (15% when exceptions are excluded). The last panel of Figure 1 presents the evolution of the share of job ads with working time below 24h. While 18% of job vacancies required a working time lower than 24h in 2013, this share is between 10% and 14% after the reform. These graphs show that, even with imperfect enforcement and several exceptions, the use of jobs with working time below 24h strongly decreased from 2014. This indicates that firms internalized the risk of workers going to labor courts and reduced their demand for low-hour jobs.

Changes in the aggregate share of jobs below the 24h cutoff suggest important changes in the distribution of hours worked. In Panel (a) of Figure 2, I plot the distribution of hours worked in the stock of jobs before the implementation of the minimum workweek. There are two striking patterns. First, there is a large spike at 35h, corresponding to full-time jobs, as well as a substantial share of overtime hours strictly above 35h.[6] Second, there is a sizable share of jobs below 24h, uniformly distributed between 1 and 24 and representing 15% of employment in 2011-2012. Panel (b) of Figure 2 shows the change in the distribution, following the implementation of the reform. The decrease in the share of jobs below 24h comes from all types of workweeks below that cutoff. There is a slight increase in the number of jobs with exactly 24h but this increase is small compared to the bunching that could be expected with this type of policy. In contrast, there is an important increase in the number of full-time jobs. Online Appendix Figure B.2 shows similar patterns for men and women, with greater magnitudes of variations for the latter. These figures provide suggestive evidence that some substitutions took place with long-hour jobs instead of part-time jobs at exactly 24h. These aggregate changes raise two questions. Are the substitutions between part-time and full-time jobs taking place in the same firms? Second, are they for the same types of workers or due to composition effects? The next section provides evidence on the within-firm substitutions.

4 Firm-level effects

In this section, I quantify the effects of the minimum workweek at the firm level. First, I focus on the employment effects, both in terms of total hours worked and number of jobs. I then decompose the effects between male and female workers. Finally, I investigate additional margins of adjustments and analyze labor flows.

4.1 Main reduced-form strategy

The reduced-form strategy leverages differences in the use of jobs with fewer than 24h between firms. Before implementation of the French minimum working time, the demand for those

[6] Full-time managers are not required to clock their hours, so their working time is typically recorded as 35 hours in the administrative data, which may not reflect actual hours worked. By contrast, hours for lower-skilled and part-time workers are contractually specified and recorded.

jobs was very heterogeneous between firms. To capture these differences, I define a firm-level variable measuring the intensity of exposure to the policy. This variable is a proxy for the firm's structural need for jobs below 24h. I denote this variable Share24$_i$, which is equal to the pre-reform share of workers working fewer than 24h per week in firm i. I compute this exposure variable on average over 2009-2013, to have a measure that is not volatile and sensitive to temporary shocks. The period of interest is 2009-2017. Hence, the main reduced-form strategy relies on the comparison of firms with different initial shares of affected jobs before and after the reform. I estimate the following specification

$$y_{it} = \sum_{\substack{k=-4 \\ k\neq 0}}^{k=4} \beta_k \times \text{Share24}_i \times \mathbb{1}_{t=2013+k} + \psi_t X_i + \mu_i + \eta_t + \epsilon_{it}, \tag{1}$$

where y_{it} is the outcome variable, for instance the logarithm of the number of workers employed in firm i in year t.[7] μ_i are firm fixed effects and control for firms' characteristics that are constant over time. η_t are year fixed effects and X_i are firm characteristics, hence allowing the time effects and impact of firm characteristics (ψ_t) to vary flexibly over time. I estimate three versions of Equation (1): (i) the model with only firm and year fixed effects, (ii) the one with added industry-year and area-year fixed effects, and (iii) the full model with also time-varying size and age effects. β_k corresponds to the effect of Share24 being equal to 100% instead of 0, k years after implementation of the policy or to placebo parameters in pre-reform years. As a result, estimates of this specification measure the effect of being more versus less exposed to the policy, at different dates before and after implementation of the minimum workweek. This type of identification strategy has been extensively used in the policy evaluation literature (see for instance Harasztosi & Lindner (2019) for an application to the minimum wage and Saez et al. (2019) about payroll tax cuts). The identification assumption is that, without the reform, firms with varying exposures would have followed similar employment trajectories. As a check, Figure 3 shows that the relationship between Share24 and the outcomes of interest was very stable in the years preceding the reform. Furthermore, estimates of the parameters over the pre-reform period allow me to test this parallel trend assumption before implementation of the policy.

To investigate the drivers of Share24, I first compute a variance decomposition of this variable. I find that between-industry variation explains between 30% (at 2-digits) and 42% (at 5-digits) of the total variation in exposure to the reform. This means that at least 58% of the variance is due to within-industry variation. Second, I regress the Share24 variable on firm characteristics, by industry, to understand the drivers of within-industry variations. Online Appendix Table B.3 presents the R^2 of OLS regressions from which one set of regressors is removed at a time. The distribution of occupations in the firm, within industry, is one of the

[7] For outcome variables that can be equal to 0 (e.g. low-hour jobs in the firm), I use the inverse hyperbolic sine transformation, such that $y_{it} = log(Y_{it} + \sqrt{1 + Y_{it}^2})$, where Y_{it} is the variable in levels.

main drivers of exposure to the policy.

Equation (1) is estimated over a balanced panel of firms. The sample is composed of firms with 5 workers or more before implementation of the minimum workweek that are not covered by collective agreements with a different minimum workweek. I show in Section 4.4 that the results are robust to alternative samples and that firm entry and exit are not impacted by the reform. The balanced panel is composed of 187,065 firms in retail, manufacturing, services (accommodation and food and other services) and construction. Table 1 presents summary statistics on the estimation sample. Average firm size in the sample is 46.86 workers and average exposure to the reform is 12% (respectively 8% for the median). Online Appendix Figure B.3 shows the distribution of Share24, the pre-reform exposure.

4.2 Main firm-level results

Employment effects. Figure 4 presents estimates of β_k for several versions of Equation (1). A natural first step is to consider the effect of the floor on hours worked on jobs targeted by the reform, shown by Panel (a). First, over the pre-reform period (2009-2013), estimated parameters are very close to 0, even if significant.[8] From 2014, the first post-reform year, there is a decrease in the number of jobs below 24h. This decrease in the stock of jobs below 24h becomes even larger over time, consistent with the fact that the policy applied to new hires only. In Table 2, I show that this impact on the stock of jobs is actually driven by the hiring margin. Regarding the magnitude of the effect, I find that a 1 percentage point higher share of jobs below 24h before the reform implies a decrease in the number of jobs below 24h in the firm by 1.6% in 2016. Due to this drop in low-hour jobs, the total number of jobs decreases, as depicted by Panel (b). This figure presents estimates for the log number of workers in the firm. The number of jobs declined by 0.38% more in firms where 10% of pre-reform jobs were below 24h relative to firms with no such job. Some jobs below 24h have been replaced by jobs with longer hours: Panel (c) shows that the number of full-time workers increases in the firm in response to the policy. However, I don't find any increase in jobs with workweek between 24 and 35 (see Online Appendix Figure C.1). Overall, the increase in long-hour jobs is not enough to offset the decrease in the number of jobs: the total number of hours worked in the firm every year is decreasing. An initial share of jobs below 24h higher by 1 percentage point is associated with a decrease in total hours by 0.17% in 2016 (Panel (d)). The negative effect on the number of jobs dominates the increase in hours per job. Firms more exposed to the reform are shrinking relative to firms with lower exposure. This result suggests that firms cannot freely reallocate hours across workers, implying limited within-firm substitutability between workers and hours.

[8] The standard errors are particularly small for two reasons: The sample size is very large because of comprehensive data and the outcomes are in logarithm. As a robustness check, I compute in Section 4.4 alternative confidence intervals accounting for potential differential pre-trends using the procedure by Rambachan & Roth (2023) and find consistent results.

There was limited scope for the reform to induce workers to replace multiple simultaneous jobs with a single longer-hour job: as shown in Online Appendix D, only 3.5% of part-time workers held at least two jobs at the same time pre-reform. This share falls slightly to 3.3% afterward, and a market-level analysis indicates that the decline is driven by the reform. However, the magnitudes are too small for multiple-job holding to materially influence the employment effects estimated at the firm level.

Other adjustment margins. Consistent with the negative effects on total hours, I find a decrease in total output and other inputs. Panel (a) of Figure Online Appendix Figure C.2 shows estimates for total sales in the firm. A 1 percentage point higher exposure to the policy reduces sales by 0.16% in 2016, implying a semi-elasticity similar to that for total hours. The decrease in output suggests that the workers hired on full-time jobs are not producing more to compensate for the decrease in the number of workers in the firm. Panels (b) and (c) show that firms reduce their capital stock proportionately with employment. Finally, using purchased services as a proxy for outsourcing, I study whether firms have relied more on external contractors to mitigate the impact of the policy. Panel (d) shows that firms do not increase service expenditures, indicating that the decrease in employment is not compensated by purchasing services to self-employed workers.

In terms of pay, I estimate a semi-elasticity of -0.04 in 2016 for the average hourly wage (Online Appendix Figure C.3). Although this firm-level average may partly reflect changes in worker composition, the pattern is confirmed in worker-level estimates (Online Appendix Figure F.2). Average earnings per job in the firm increase (semi-elasticity of 0.4), but this large effect is mechanically driven by the destruction of low-hour jobs; I find no corresponding increase in worker-level earnings.

4.3 Gender heterogeneity

Because women are initially much more likely to work fewer than 24 hours, the reform's firm–level effects may differ by gender.

Employment by gender. I re-estimate Equation (1) where the outcome is gender specific, for instance the number of women (or men) working in the firm. Exposure to the policy is, as before, Share24, the share of affected jobs in the firm before the reform. Figure 5 (panels (a) and (b)) shows that employment declines are systematically larger for women, in both job counts and total hours. In 2016, a 1 percentage point increase in exposure is associated with a 0.2% decrease in men's jobs and a 0.4% decrease in women's jobs (Panel (a)). For total hours (Panel (b)), the semi–elasticities are -0.1 for men and -0.4 for women, implying that most of the firm–level reduction in hours comes from the female margin. Online Appendix Figure C.4 documents two mechanisms behind this gap: (i) the percentage drop in jobs below 24h is similar across genders, but because women's baseline share is higher, the level decline is larger for women; and (ii) the increase in longer–hours jobs is stronger for men. Together, these patterns suggest that part of female part–time employment is replaced by male full–time employment.[9] To assess whether these substitutions occur within similar job types rather than through broad compositional shifts, I estimate effects separately by occupation, industry, and firm characteristics; results are plotted in Online Appendix Figures C.5 and C.6. For job counts (panel (a) of both figures), estimates for women are negative and statistically significant across all occupations and are typically larger in absolute value than for men. For men, signs and magnitudes vary by occupation and are generally smaller. For total hours (panels (b)), effects are negative for women across most occupations and industries, whereas men display substantial heterogeneity, with positive effects in several high– and low–skill categories. These findings indicate that gender differences are not solely due to between–occupation or between–industry composition; they also occur within similar jobs.

Decomposition of differences by gender. Different effects by gender can arise from two channels: (i) a *composition effect* channel, because firms with higher Share24_i tend to employ more women part–time; and (ii) *heterogeneous responses* by gender, whereby holding gender–specific exposure constant, women's and men's employment respond differently. I proceed in two steps to decompose those channels. As a preliminary step, I estimate the within-firm gender gap in the effect of the reform. This first step permits to consider directly the outcome I aim to decompose and to ensure that observed different effects by gender are happening within-firm. I estimate the following specification, using the overall exposure on the right–hand side:

$$\Delta^G y_{it} \equiv y_{it}^{Women} - y_{it}^{Men} = \sum_{\substack{k=-4 \\ k\neq 0}}^{k=4} \delta_k \times \text{Share24}_i \times \mathbb{1}_{t=2013+k} + \psi_t X_i + \mu_i + \eta_t + \epsilon_{it}, \quad (2)$$

where δ_k trace the *overall* within–firm gender gap in the exposure effect. As shown in Panels (c) and (d) of Figure 5, these align with the differences between the separate-by-gender estimates in Panels (a)–(b), confirming that the gap is not driven by between–firm composition. In a second step, I use gender–specific pre–reform exposure shares to isolate differential responses holding exposure constant by gender:

$$\Delta^G y_{it} = \sum_{\substack{k=-4 \\ k\neq 0}}^{k=4} [\pi_k^{Women} \times \text{Share24}_i^{Women} + \pi_k^{Men} \times \text{Share24}_i^{Men}] \times \mathbb{1}_{t=2013+k} + \psi_t X_i + \mu_i + \eta_t + \epsilon_{it}, \quad (3)$$

The *heterogeneous response* at event time k is given by $\pi_k^{\text{Women}} + \pi_k^{\text{Men}}$ (see Online Appendix

[9]Online Appendix Table C.1 confirms that these findings also hold when computing the corresponding changes in levels. The average decrease in the number of jobs in the firm is 1.65 for women and 0.90 for men.

C.2 for the derivation),[10] which I report in Panels (c)–(d). The gap between the overall gender gap and the heterogeneous response corresponds to the composition effect. In 2016, a 100 ppt increase in Share24$_i$ makes women's employment decline by 0.2 log points more than men's. Roughly one–fifth of this *overall* gap is due to heterogeneous responses for a given exposure, and four–fifths to composition (women's greater initial exposure). For total hours, about 25% of the larger decline for women comes from heterogeneous responses and 75% from composition. The heterogeneous response explains all of the differential change in full-time employment between men and women (Online Appendix Figure C.4). These heterogeneous effects provide a mechanism for the observed shift from female part–time toward male full–time positions: the reform changes who is hired, hence the optimal hours per job. The structural model presented in Section 6 features ex-ante heterogeneity between workers to account for this channel.

4.4 Robustness analysis

In this Section, I discuss the validity of the parallel trend assumption and examine the robustness of the firm-level results, to mean reversion, to the definition of exposure to the policy and to changes in the estimation sample. I discuss the robustness checks for the two main outcomes, namely the number of jobs and total hours worked. Results are shown in Online Appendix E.

Parallel trend assumption. The analysis assumes that without the policy, firms with different shares of jobs below 24h would have evolved similarly. Although untestable during the treatment period, I test for this assumption in the pre-reform years. First, for all outcomes of interest, I have presented placebo estimates over years 2009 to 2012. In all cases, the estimates are very small in magnitude, relative to the post-reform estimates. In addition, Figure 3 shows that the relationship between Share24 and outcomes is extremely stable over time over the pre-reform period. This relationship is different after implementation of the minimum workweek: the higher the exposure, the larger the change in outcomes compare to the pre-reform period. Third, I apply the procedure proposed by Rambachan & Roth (2023) and find that the significant decrease in the number of jobs and total hours is robust to potential differential pre-trends before the policy (Figure E.2).

Mean reversion. The share of affected jobs is computed on average over a period of five years (2009-2013) for firms above 5 workers so that identification of the effects relies on a source of variation not too volatile and sensitive to temporary shocks. To provide additional checks, I estimate difference-in-difference regressions over two-year rolling periods by comput-

[10] Working with the delta in outcomes between women and men allows to account for the correlation between Share24$_i^{Women}$ and Share24$_i^{Men}$. Using only one at a time could result in an omitted variable bias.

ing exposure in the first year and defining the second year as the treated year (Figure E.3). For example, the estimate in 2010 comes from a difference-in difference regression estimated on 2009 and 2010, with Share24 computed in 2009. I find that, for both outcomes, the pre-reform estimates are significant but very small in magnitude compared to the estimates after implementation of the policy. For the number of jobs, estimates are in the range (-0.07, -0.02) for the pre-reform period while the point estimate is equal to -0.21 in 2014, the year the policy is implemented. For the total number of hours worked, estimates over 2009-2013 are in the range (0.01, 0.09) while the estimate in 2014 is -0.30. Even with this method, which is more likely to be affected by mean reversion as exposure is computed over one year only, I find that mean reversion cannot drive the results found.

Balanced sample. The main results are estimated over a balanced panel of firms. Hence, they do not take into account potential effects of the policy on firm entry nor exit. I estimate Equation (1) on the set of firms existing at the beginning of the period, with an indicator for firm exit as outcome (Figure E.4). Using the entire sample of firms, I find small positive estimates that seem driven by small pre-trends, but no change in the trend post-reform. Both the pre-trends and the subsequent effect disappear when restricting the sample to firms with at least 10 workers pre-reform. To assess the effect on firm entry, I compute exposure to the policy at the industry level in 2009-2013 and estimate Equation (1) where the outcome is the firm entry or exit rate of the industry for each year. I find that there is no effect. These results show that while firms are shrinking because of the minimum workweek, entry and exit do not seem affected. Second, to check that the estimated effects are not specific to older firms, I estimate Equation (1) on alternative and less restrictive samples, including younger firms (Figure E.5). Each sample is composed of firms existing during at least 5, 4, 3, 2 and 1 year before implementation of the policy. I find that estimates for β_k are very close to the ones previously estimated. If anything, including younger firms in the sample increases slightly the magnitude of estimates.

Weights. The results presented so far are unweighted. To assess whether aggregate implications could be very different from the average firm-level estimates, I re-estimate Equation (1) and weight each firm of the panel by its pre-reform size (Figure E.6). The results are very similar to the unweighted ones for all main outcomes as well as the gender differences.

Definition of exposure to the reform. I find that Share24 is highly persistent over time (Figure E.7). Hence, the former exposure to the policy should be a good proxy for the firms' current exposure. As a robustness check, I reproduce the analysis using an alternative proxy for firm exposure to the policy. I compute a GAP-exposure to the reform: the average increase in hours that would be needed in the firm to have all workers working at least 24h (Figure E.8). The GAP is computed on average over 2009-2013. I plot estimates of β_k from

Equation (1) in which Share24 is replaced by the GAP variable in the Online Appendix. The results are consistent with the main firm-level effects: both the number of jobs and total hours worked in the firm decrease.

This Section has presented relative changes in outcomes in firms with a high exposure to the minimum working time, relative to firms with a low exposure. Overall, I find a decrease in the total number of hours worked at the firm level, indicating that firms do not reallocate hours flexibly between workers. The effect is very heterogeneous by gender: while the negative effect on the extensive margin is higher for women, the increase in full-time employment is larger for men. The objective of the next section is to assess the potential importance of between-firm effects, as a preliminary step before quantifying the aggregate effects.

5 Beyond Firm-Level Estimates: Worker, Between-Firm, and Market Effects

The previous section shows that firms initially relying more on low-hour jobs reduce employment. This prompts two questions: whether affected workers become unemployed or reallocate toward less-exposed firms, and which workers move into full-time jobs. I proceed in three steps: (i) document persistent worker types and their trajectories; (ii) examine between-firm spillovers; and (iii) track outcomes at the market level.

5.1 Worker-level descriptive evidence

Persistent worker types. A challenge for worker-level analysis is that the reform targets new hires starting in 2014, while incumbent workers with workweeks below 24 hours are not directly treated. However, persistent working-time patterns—where workers employed in low-hour jobs are more likely to remain in low-hour jobs in subsequent spells—allow informative comparisons based on past hours. To measure such persistence, I compare the workweek of the same individual before and after a job change, thereby abstracting from persistence mechanically driven by the job or employer. Online Appendix Figure F.1 shows strong persistence: (i) workers employed 20 hours work on average 27 hours in their next job, while those previously employed 40 hours work around 34 hours; (ii) 48% of workers employed below 24 hours previously also work below 24 hours after a job change, compared with 24% among those previously employed above 24 hours.

Evolution of worker-level outcomes by worker type. I use this persistence to proxy worker type and document the evolution of individual outcomes. Following Dustmann et al.

(2021), who track outcomes by bins of prior wages, I consider bins of prior hours and estimate:

$$y_{it} - y_{it-2} = \sum_{d} (\mathbb{1}[b_{d-1} < h_{it-2} \leq b_d]\gamma_{d2011} + \mathbb{1}[b_{d-1} < h_{it-2} \leq b_d]\delta_{dt}) + \alpha X_{it-2} + \nu_{it}. \quad (4)$$

The coefficients δ_{dt} capture two-year growth in outcomes in 2016 versus 2014 (post-reform) and in 2013 versus 2011 (pre-reform), relative to 2011 versus 2009. The terms γ_{d2011} absorb macro trends and mean reversion under the assumption that they are constant over time. Estimates for 2013–2011 serve as a benchmark to detect potential differential time-varying shocks across worker types. Controls X_{it-2} include age, occupation and location. Figure 6 reports results separately for men and women (see Online Appendix Figure F.2 for additional outcomes—e.g., wage effects). Panel (a) displays the pre-reform female share by workweek, providing an aggregate lens for interpreting worker-level patterns. First, workers with low prior hours experience a decline in employment probabilities—around 5 percentage points for those previously working 15–19 hours per week—for both genders. Given women's over-representation in low-hour jobs, this implies a larger absolute reduction in female employment. In contrast, I find no employment change for individuals previously employed full-time, and the pre-policy placebo estimates are flat. Second, the increase in full-time employment observed at the firm level appears to originate from workers previously in long-hour part-time jobs (23–34 hours). The worker-level evidence also mirrors the gender heterogeneity found at the firm level: conditional on prior working time, men are more likely than women to experience increases in hours and transitions to full-time work. Finally, workers previously in part-time jobs also change the type of employer they work for after the reform. Using pre-reform firm-level Share24 as a measure of employer type, I find that employed workers are more likely to work in firms with lower reliance on low-hour jobs, with larger movements for men. This indicates that some workers reallocate toward firms less affected by the policy. The next subsection further investigates these spillovers.

Although workers who tend to work low hours are less likely to be employed *overall* after the reform, I find mild hoarding *within the current firm* for jobs created below 24 hours pre-reform. Online Appendix F.3 details the worker-level difference-in-discontinuity design used to track these jobs. Workers with a workweek below 24 hours before the reform are about 1 percentage point (2% of baseline) more likely to remain in the same firm in 2016 than workers previously above 24 hours. Although modest, this suggests either that some firms retained these jobs to avoid the reform's application or that workers seeking low-hour jobs had limited outside options.

5.2 Between-firm spillovers

To further assess reallocation across firms, I exploit variation in exposure to the reform across local labor markets. For each firm i in market m, I construct a leave-one-out market exposure measure, Share24$_{m(-i)}$, defined as the share of jobs below 24 hours in the market over 2009–2013 (excluding firm i). Markets are defined as commuting zone $\times$ 2-digit industry. The distribution appears in Online Appendix Figure G.1.

Firm-level effect by market exposure. I begin by re-estimating Equation (1) separately for firms in above- and below-median exposure markets. As shown in Figure 7 (panel (a)), firms in low-exposure markets face significantly larger employment losses. For men, the 2016 semi-elasticity to Share24 is 0.35 in low-exposure markets (0.60 for women), compared with 0.15 in high-exposure markets (0.35 for women). This pattern suggests between-firm spillovers: high-exposure firms experience a larger employment contraction when surrounded by less affected competitors, whereas high overall market exposure mitigates the loss. Because firm- and market-level exposure are positively correlated (Online Appendix Figure G.1), firms in high- and low-exposure markets differ in observable ways, and market exposure may interact with firm exposure. I therefore estimate a triple difference to isolate the moderating role of the market.

Triple difference. To examine whether the bite of the reform on the rest of the labor market dampens a firm's employment loss, I estimate:

$$y_{imt} = \sum_{\substack{k=-4 \\ k \neq 0}}^{k=4} \left[\lambda_k \text{Share24}_i + \gamma_k \left[\text{Share24}_i \times \text{Share24}_{m(-i)}\right]\right] \times \mathbb{1}_{t=2013+k} + \mu_i + \delta_{mt} + \eta_t + \epsilon_{it}, \quad (5)$$

where y_{imt} is log employment. Here, λ_k measures the effect of firm exposure in a market with zero exposure (Share24$_{m(-i)} = 0$), while γ_k captures how this effect changes with market exposure. The term δ_{mt} absorbs market-specific shocks and the baseline effect of market exposure for firms with zero own exposure. To assess magnitudes, I compute:

$$\underbrace{\widehat{\lambda}_k}_{\substack{\text{Firm effect if} \\ \text{Share24}_{m(-i)}=0}} + \underbrace{\widehat{\gamma}_k \times s}_{\substack{\text{Spillover} \\ \text{moderation}}} .$$

for several values of market exposure s: 0 (a firm in an otherwise unexposed market), the economy-wide mean, and the 90th percentile (a highly exposed market). Figure 7 (panel (b)) presents the corresponding estimates by gender.[11] Employment effects remain negative even in highly exposed markets, but are attenuated relative to markets with low exposure. Online Appendix Figure G.2 expresses the moderation as a share of the average effect, and Table G.1 reports robustness to alternative market definitions. Overall, the bite of the reform on other firms in the market reduces the firm-level employment impact by 16–27% for men and 9–17% for women. These between-firm spillovers are larger for men, consistent with the worker-

[11]Online Appendix Figure G.3 reports estimates imposing $\gamma_k = 0$.

level evidence showing greater changes in employer type for them. Appendix Figure G.4 documents heterogeneity in spillover moderation across firm types. Indirect between-firm effects are larger in retail than in manufacturing, consistent with the fact that retail firms compete more intensely at the local level.

5.3 Market-level evidence

Finally, I adapt the event-study specification in Equation (1) to the market level.[12] This captures employment effects net of within-market reallocation between firms. Figure 7 reports the results for employment (panel (c)) and total hours (panel (d)), alongside firm-level estimates for comparison. Firm- and market-level exposure variables are standardized to facilitate magnitude comparisons. The market-level estimates show negative employment effects that closely track the firm-level patterns in 2014 and 2015. In 2016 and 2017, the effects remain negative—especially for women—but are attenuated relative to the firm-level results. For total hours, estimates are initially negative but subsequently recover: the market-level effect is near zero for women and positive for men. Online Appendix Figure G.5 shows that the market-level results are not driven by mean reversion by computing exposure before the years displayed on the figure and using the first four years as a placebo test. Taken together, these findings suggest that between-firm reallocation mitigates the impact on highly exposed firms at a more aggregate level.

Implications. The empirical strategy identifies the effect of the minimum workweek from differential changes between more and less exposed firms. This section presents three patterns consistent with between-firm spillovers, notably reallocation from highly to less exposed firms: (i) workers previously in low-hour jobs are more likely to move to less exposed firms; (ii) firm-level employment losses are attenuated in markets where other firms are highly exposed; and (iii) market-level employment effects are smaller than firm-level effects.

These findings have two implications. First, firm-level reduced-form estimates represent relative differences between more versus less exposed firms and the latter are indirectly affected. Second, the aggregate impact of the minimum workweek can differ substantially from firm-level effects. Because unemployment and welfare depend on general equilibrium adjustments, aggregation is central for policy evaluation. The next section develops and estimates a general equilibrium model that incorporates both firm responses and reallocation to quantify aggregate effects.

[12] The estimated specification is $y_{mt} = \sum_k \beta_k \times \text{Share24}_m \times \mathbb{1}_{t=2013+k} + \mu_m + \eta_t + \epsilon_{mt}$, with Share24_m the market-level exposure. A market is a commuting zone x 2-digits industry.

6 Aggregate employment and welfare effects

6.1 Structural model pre-reform

To study the aggregate impact of the 24-hour minimum workweek, I develop a random search-and-matching model based on Pissarides (1985) to which I incorporate (i) multiple-job firms, (ii) two-sided heterogeneity and (iii) hours determination by right-to-manage and wage bargaining.[13] The model generates both within- and between-firm heterogeneity in working hours. I first describe the pre-reform environment and then introduce the policy.

6.1.1 Population and technology

The framework is a random search and matching model with multiple-job firms. Time is discrete with an infinite horizon. There is a large number of workers and an endogenous number of firms, all discounting at rate β. Firms all produce an identical homogeneous good, using labor as the only input. Firms make endogenous hiring decisions and jobs are destroyed exogenously at constant rate μ. A firm is a collection of its jobs. Firm entry is endogenous and firms are exogenously destroyed at rate δ. I denote by $\sigma = \delta + (1 - \delta)\mu$ the probability that a job is destroyed, independently from the cause. Aside from exogenous job and firm destruction, the environment is deterministic. The model is presented at the steady state.

Technology. Firms are characterized by time-invariant productivity y. Within each firm, jobs differ in match-specific productivity z, drawn from distribution $H(\cdot)$ upon matching and fixed until separation. Output from a job with hours h is

$$z \times y \times h^{\alpha},$$

where $\alpha < 1$ governs returns to hours at the job level.

Entry and vacancies opening. I assume that there is a finite mass $\mathcal{E}$ of potential entrepreneurs who may decide to create a firm. Each entrepreneur draws a firm productivity y from the cumulative distribution function $F(\cdot)$ before deciding whether to operate. Entry is costless, and entrepreneurs choose to operate if and only if doing so is profitable. The corresponding truncated distribution of profitable firms is denoted $\tilde{F}(.)$. To hire workers, an entrepreneur has to open vacancies, the number of which is denoted $v(y)$ for a type-y firm, at cost $C(v(y))$, which is increasing and convex. Vacant jobs are matched with workers at a rate $m(\theta)$, where $\theta = \frac{V}{U}$ is the labor market tightness, V the total number of vacancies posted by all firms and U is the total number of unemployed workers. The matching function

[13] In a previous version of this work, circulated as a working paper in 2024, I analyze a variant with joint bargaining over wages and hours in which hours are efficient at the match level. The simulation results for the 2014 reform are qualitatively similar for both employment and welfare.

$m(.)$. describes the matching technology. Unemployed workers meet employers at rate $\theta m(\theta)$.

Population. Workers differ ex ante in labor disutility. There are $\mathcal{N}$ types and each type is denoted by i. In the quantitative model, I use six (three types of men and three of women), reflecting gender differences in preferences for hours. Instantaneous utility is

$$c - x_i \frac{h^{1+\phi}}{1+\phi},$$

where c is equal to consumption, and $x_i \frac{h^{1+\phi}}{1+\phi}$ is the labor disutility associated with h hours worked per week. x_i is type-specific, and $\phi > 0$ is common to all workers.[14] Both x_i and ϕ are constant over time. Therefore, x_i characterizes the ex-ante heterogeneity for worker types.

Timing. The timing of events for a given period is as follows: (1) Vacancies posted in the previous period get matched to unemployed workers. (2) When a match is formed, the firm observes i, the worker type and draws z, match-specific productivity. All parameters are observed by the firm and the worker. (3) For new matches, firms and workers bargain over the hourly wage and then the firm chooses hours. The wage bargaining and hours-setting processes are described below. A job is created if participation constraints are satisfied. Otherwise, the job remains vacant and the worker unemployed. Contracts are unchanged for workers hired during previous periods.[15] (4) Workers hired in current and previous periods work the number of hours in their contract and get paid. (5) A share μ of jobs is destroyed and a share δ of firms exits the market. (6) New firms enter the market. (7) New and existing firms decide how many vacancies to open.

In what follows, I first present the value of filled jobs, for workers and firms. Then, I describe the unemployment value and the posting and creation decisions.

6.1.2 Value of filled jobs

The flow utility of a worker is denoted by b for an unemployed worker and corresponds to earnings for an employed worker. Earnings equal the product of the number of hours worked, h, and the hourly wage, w. As a result, the value of a job for a type-i worker in a type-y firm, with match-productivity z is

$$W(i, y, z) = wh - x_i \frac{h^{1+\phi}}{1+\phi} + \beta(1-\sigma)W(i, y, z) + \beta\sigma W_u(i), \tag{6}$$

where $W_u(i)$ is the value of unemployment for a worker of type i. Conditional on the job surviving, the continuation value is the same because job destruction is the only shock.

[14] Work disutility is equal to 0 for unemployed workers. Disutility increases with hours, i.e. $x_i > 0$ for all worker types.

[15] There is no shock for surviving jobs, which implies no renegotiation.

For the firm, the instantaneous profit of a job is equal to the difference between the production of the job and the wage paid. For a type-y firm, the value of a job with hours h, match productivity z, and worker of type i is denoted by $J(i, y, z)$ and is equal to

$$J(i, y, z) = zyh^{\alpha} - wh + \beta(1 - \sigma)J(i, y, z). \tag{7}$$

To account for the fact that w and h depend on the type of the worker and firm, the value function for the firm is also indexed by i and y. If the job is destroyed or if the firm exits, the value of the job for the firm is equal to 0. The firm determines the number of vacancies exhausting all profitable opportunities.

6.1.3 Determination of hours and wages

Under right-to-manage (RTM), the worker and the firm bargain on the hourly wage once a match is formed, and then the firm chooses the number of hours. It is known at the time of the negotiation that the firm will pick the number of weekly hours. I assume that the bargaining power of the worker is equal to γ while the power of the firm is $1 - \gamma$. Given the bargained wage w, the firm chooses hours to maximize profits subject to the worker's participation constraint:

$$\max_{h} \; zyh^{\alpha} - wh \quad \text{s.t.} \quad W(i, y, z) - W_u(i) \geq 0 \tag{8}$$

Online Appendix H.2 derives the firm policy function from Equation (8) and discusses non-interior cases. The interior solution gives

$$\alpha zyh^{\alpha-1} = w \tag{9}$$

The firm picks the number of hours that equalizes marginal labor productivity with the hourly wage. With decreasing returns in hours at the job level ($\alpha < 1$), the hours chosen by the firm decrease with the bargained hourly wage (in the space in which the worker's participation constraint is satisfied). The worker and the employer bargain on the hourly wage knowing the firm's best response function for hours, in Equation (9). The wage bargaining problem is

$$\max_{w} (W(i, y, z) - W_u(i))^{\gamma} (J(i, y, z))^{1-\gamma}. \tag{10}$$

Online Appendix H.2 presents the derivation. Section 6.1.6 discusses how hours under right-to-manage compare to hours under efficient bargaining.

6.1.4 Job creation and firm entry

I now present the unemployment value functions, for the $\mathcal{N}$ types of workers as well as the equation that determines vacancy posting. Details on the computation of the value functions are in Online Appendix H.2. First, I define the job creation condition for a match. Under right-to-manage, the firm's value satisfies $J(i,y,z) > 0$ for some $h > 0$ (since $wh = \alpha zyh^{\alpha}$ from Equation (9)), so the binding constraint on job creation is the worker's participation. A match converts into a job if and only if $z \geq \underline{z}(i,y)$, where

$$\underline{z}(i,y) = \{z \mid W(i,y,z) - W^u(i) = 0\}. \tag{11}$$

This cutoff depends on the worker type not only because of the value of the disutility parameter, but also because of the value of the worker's outside option, $W_u(i)$. When a worker is matched with a type-y firm, which happens at rate $\theta m(\theta)\frac{nv(y)}{V}$, she receives a value $W(i,y,z)$ if the match converts into a job. n is the total number of firms and V is the total number of vacancies in the economy. Therefore, the value of unemployment for type i satisfies

$$(1-\beta)W_u(i) = b + \beta\,\theta m(\theta)\int\left[\int_{z\geq\underline{z}(i,y)}[W(i,y,z) - W_u(i)]\,\mathrm{d}H(z)\right]\frac{nv(y)}{V}\,\mathrm{d}\tilde{F}(y). \tag{12}$$

Due to random matching, a firm cannot target a specific type of worker. Hence, the probability to be matched with a type-i worker depends on the distribution of types among unemployed workers. This distribution is endogenous, but observed by firms. I denote by $U(i)$ the number of unemployed workers of type i and $s(i) = \frac{U(i)}{\sum_{i=1,\ldots,\mathcal{N}} U(i)}$ the share of type-i workers in the unemployment pool. The expected value of a marginal vacancy equals 0 in equilibrium. With $J(i,y)$ the expected value of a job filled with a type-i worker for the firm, I obtain the vacancy posting equation

$$C'(v(y)) = \beta m(\theta)\sum_{i=1,\ldots,\mathcal{N}} s(i)\int_{z\geq\underline{z}(i,y)} J(i,y,z)\,\mathrm{d}H(z). \tag{13}$$

Equation (13) equates the marginal cost of a vacancy to its expected payoff, pinning down firm size under convex vacancy costs.[16]

An entrepreneur enters if and only if the value of operating, $\Pi(y)$, is non-negative. Hence, entry is characterized by an endogenous productivity cutoff $\underline{y}$ defined by

$$\Pi(\underline{y}) = 0. \tag{14}$$

It implies a mass of firms $n = \mathcal{E}[1 - F(\underline{y})]$.

[16] The size of a firm is dynamic and depends on its type, y, and its age. A firm posts the same number of vacancies at each period and faces a constant separation rate. The size progressively converges to its steady state level.

6.1.5 Equilibrium

This section characterizes the general equilibrium of the model in the pre-reform setting.

Definition 1 *A steady-state equilibrium is a tuple* $(\theta, \{s(i)\}_i, \{W_u(i)\}_i, \{v(y)\}_y, \underline{y}, \{\underline{z}(i,y)\}_{i,y}, \{w(i,y,z), h(i,y,z)\}_{i,y,z})$ *such that:*

1. **Values.** *The values of a job for the worker and the firm satisfy Equations (6) and (7) respectively, and the value of unemployment* $W_u(i)$ *satisfies Equation (12).*

2. **Hours and wages.** *For each match* (i, y, z)*, the hourly wage* $w(i, y, z)$ *solves the bargaining problem (10) and hours satisfy the firm's optimality condition (9).*

3. **Job creation.** *A job* (i, y, z) *is created if and only if* $z \geq \underline{z}(i, y)$*, as defined in (11).*

4. **Vacancy posting.** *For each firm with* $y \geq \underline{y}$*, the number of vacancies* $v(y)$ *satisfies Equation (13).*

5. **Firm entry.** *The entry cutoff* $\underline{y}$ *satisfies Equation (14).*

6. **Steady-state flows.** *For each worker type,* $U(i) = \frac{N(i)\sigma}{\sigma+\theta m(\theta)\int[1-H(\underline{z}(i,y))]\frac{nv(y)}{V}d\tilde{F}(y)}$*, with* $s(i) = U(i)/\sum_j U(j)$*.*

7. **Consistency.** *Labor market tightness satisfies* $\theta = V/U$*, where* $V = n\int v(y)\,d\tilde{F}(y)$ *and* $U = \sum_i U(i)$*.*

The equilibrium has a recursive structure. For a given $(\theta, \{s(i)\})$, conditions 1–5 pin down all remaining objects: values of unemployment, vacancy policies, the entry cutoff, job-creation cutoffs, and contract terms. The fixed point in $(\theta, \{s(i)\})$ is then determined by two relationships. The Beveridge curve (condition 6, derived in Online Appendix H.2) maps tightness into the composition of unemployment. The vacancy-posting condition maps the composition of unemployment back into tightness, since firms' expected match value depends on the types of workers they encounter. When job-finding rates are increasing in θ and vacancy creation is decreasing in the share of high-disutility worker types, the two relationships have a unique intersection (standard Inada conditions on the matching function guarantee that they cross). The mapping is continuous, which guarantees existence. With multi-dimensional worker heterogeneity, however, uniqueness is not guaranteed in general; regulations may also introduce kinks in policy functions. I therefore verify both existence and uniqueness numerically across all parameterizations considered, including post-reform.

6.1.6 Comparison: hours under right-to-manage versus efficient

Motivated by the prevalence of involuntary part-time employment, I assume right-to-manage: firms choose hours subject to worker acceptance. This generates potential inefficiencies that may justify working time regulations. Efficient hours would maximize joint surplus (computed in Online Appendix H.2), yielding the first-order condition

$$\alpha z y h^{\alpha-1} = x_i h^{\phi}, \tag{15}$$

which equates marginal productivity to marginal disutility. This outcome would arise if firms and workers jointly bargained over wages and hours. Comparing Equations (9) and (15), whether hours under RTM exceed or fall short of the efficient level depends on how the negotiated wage compares to marginal disutility, as illustrated in Panel (a) of Figure 8. I denote $w(i, y, z)$ and $h(i, y, z)$ the wage and hours chosen under right-to-manage to emphasize in the comparison that they are endogenous.

Three cases arise. If $w(i, y, z) = x_i h(i, y, z)^{\phi}$, hours are efficient. If $w(i, y, z) < x_i h(i, y, z)^{\phi}$, hours exceed the efficient level: additional hours are relatively cheap for the firm, which does not internalize workers' marginal disutility. If $w(i, y, z) > x_i h(i, y, z)^{\phi}$, hours fall short of the efficient level, because additional hours are costly to the firm.

Because the distortion depends on worker-level characteristics—disutility x_i and outside option $W_u(i)$—the same firm may schedule excessively long hours for some workers and inefficiently short hours for others. Whether the minimum workweek raises or lowers aggregate surplus is therefore a quantitative question, addressed in the calibration.

6.1.7 Discussion of model choices

Online Appendix H.1 reports additional empirical exercises motivating the model's key assumptions.

I introduce heterogeneity at the worker, firm, and match levels to capture the margins along which the reform operates. Workers differ ex ante in their disutility of hours, consistent with the persistence of worker types across job switches documented in Section 5.1. In the quantitative analysis, I allow for three types of women and three of men, motivated by large gender differences in working-time distributions, firm effects, and stated hour preferences (Figure H.1, panel (a)). Firms are heterogeneous in productivity, reflecting the strong empirical relationship between productivity and working time (panel (b)). Jobs also differ in match-specific productivity z, generating worker- and firm-specific job-creation cutoffs and allowing for within-firm selection; without this dimension, a given worker type would either always or never be hired by a firm.

In practice, search is likely random for some segments of the market and directed for others; a framework that nests both is beyond the scope of this paper. I therefore choose between the two polar cases and adopt random search, for the following reasons. First, I directly test for sorting between workers with heterogeneous preferences and firms with heterogeneous technologies, as a directed search model would predict. Following Lachowska et al. (2023), I estimate an AKM-type variance decomposition of log hours into worker and firm components, applying the split-sample correction to purge the correlation estimate of limited-mobility bias arising from noisy estimation of effects for infrequent movers. The corrected correlation is essentially zero (Table H.1; Figure H.2), consistent with findings in U.S. data, whereas directed search would generate positive sorting between worker preferences and firm technologies. At the same time, the worker component explains 43% of the dispersion in hours in France (versus 7% in the U.S.), supporting heterogeneous preferences in the model. Second, hours do not appear to be effectively posted ex ante: matching employment records to job ads, I find that the median absolute gap between posted and realized hours exceeds four weekly hours, and more than half of postings at 20 hours per week result in a full-time job (Figure H.3). Beyond these empirical findings, random search has two appealing modeling properties. It implies that firms employ multiple worker types rather than specializing, ruling out separating equilibria and allowing the model to capture within-firm substitution across worker types in response to policy. Moreover, it may generate misallocation if low productivity firms that create low hour jobs become too large and create congestion, thereby opening scope for policy intervention.

Finally, the model features a unique non-employment state rather than distinguishing unemployment from non-participation. While labor-force exit is potentially relevant, especially for workers with high disutility of hours, introducing it here would generate unrealistic discrete responses, as an entire worker type would enter or exit the labor force at once.

6.2 Regulations in the model

I compare the equilibrium without a minimum workweek to that with a 24-hour floor, taking as given the pre-existing 35-hour full-time regulation, which I also analyze in the quantitative exercises.

Pre-existing full-time regulation. The pronounced spike in hours at 35 in France (Panel (a) of Figure 2) reflects the full-time workweek regulation. I model this as a cost τ for jobs above 35 hours, proportional to the deviation from 35, capturing institutional constraints and red tape costs.[17] The cost paid by a firm for a job with workweek h is equal to $\tau \max(h-35, 0)$ per week. The 2014 minimum-workweek reform is therefore evaluated conditional on this regulation, and I also study counterfactuals without the 35-hour rule.

Introduction of the minimum working time in the model. I model the 24-hour

[17] Employers must consult workers' representatives and report overtime hours to the local authority. Workers must be notified a week in advance of the overtime request.

rule as a cost for jobs with weekly hours below 24, rather than as a strict ban, since many sub-24-hour jobs persist in the data. The cost captures the risk and consequences of legal challenges and makes compliance endogenous: firms may choose to operate below 24 hours and pay the cost, operate above 24 hours, or not create the job. As for hours above 35, this is a sunk cost borne by the firm. I specify the per-period cost as proportional to the gap to 24, $\rho\max(24-h,0)$, and consider alternative thresholds in the quantitative analysis.

Strategy to decompose the effects of the reform. I distinguish partial-equilibrium effects, which hold labor market tightness θ and the value of unemployment $W_u(i)$ fixed, from general-equilibrium effects that allow all endogenous variables to adjust. In partial equilibrium, only directly affected firms change behavior: firms with no pre-reform exposure to sub-24-hour jobs do not react, so that the Stable Unit Treatment Value Assumption (SUTVA) holds across firms. In general equilibrium, aggregate adjustments can generate reallocation across all firms. Because the reduced-form firm-level estimates capture relative differences between more and less exposed firms in the post-reform equilibrium, they reflect a combination of direct and indirect effects.

Partial-equilibrium effects. The reform operates through two firm-level channels. First, by raising the cost of sub-24-hour jobs, it changes expected match value for the firm. Vacancy posting responds to expected value (Equation (13)): firms that relied on low-hour jobs adjust their vacancy posting. Second, the reform shifts the job-creation cutoff. A match converts into a job if and only if the worker's value exceeds her outside option, $W(i,y,z)\geq W_u(i)$ (Equation (11)). The reform changes $W(i,y,z)$ for affected matches, and the direction depends on the pre-existing distortion. For matches where the firm previously chose inefficiently low hours $(w(i,y,z)>x_i h(i,y,z)^{\phi})$, the reform pushes hours upward and can raise worker value, lowering the cutoff $\underline{z}(i,y)$ and expanding the set of viable matches. For matches where hours were near or above the efficient level, the added cost reduces value and raises the cutoff, destroying jobs. The bite is heterogeneous across worker types: for high-disutility workers, increasing hours is more likely to reduce job value. While the partial-equilibrium effect of the reform on employment is a priori ambiguous, the comparison between the estimated right-to-manage hours distribution and the counterfactual efficient one in the estimated model suggests that room to increase job value is limited (panel (b) of Figure 8).

General-equilibrium effects. In general equilibrium, labor market tightness θ, the value of unemployment $W_u(i)$, and the composition of the unemployment pool $s(i)$ all adjust, so that even firms with no pre-reform exposure to sub-24-hour jobs respond. If the negative employment effect dominates in partial equilibrium, aggregate vacancy posting falls: θ decreases, and $W_u(i)$ declines (Equation (12)). In response, expected profit rise and firms post more vacancies, partially offsetting the initial decline.

This feedback operates through two margins. The decline in θ reduces the meeting rate $\theta m(\theta)$ for all workers, regardless of type. But the decline in $W_u(i)$ lowers the job-creation cutoff $\underline{z}(i, y)$ (Equation (11)), expanding the set of matches that convert into jobs. The magnitude of this expansion is heterogeneous: it depends on how large the decline in $W_u(i)$ is and on how far the pre-reform conversion probability $1 - H(\underline{z}(i, y))$ was from one. Since firms with no low-hour jobs pre-reform are only affected through those indirect effects, the reform may reallocate workers from high to low-exposure firms. The pre-existing 35-hour regulation also shapes this reallocation, as it affects the relative profitability of long- versus short-hour jobs; I assess its role in the quantitative analysis.

Overall, the aggregate effects of the minimum working time depend on how the policy reshapes vacancy creation, the job-creation cutoff across worker types, and the resulting reallocation of employment across firms — all of which are quantitative questions, addressed next.

6.3 Empirical strategy

I calibrate and estimate the model using pre-reform data (2011–2013), and then identify the policy parameter of the 24-hour rule using reduced-form evidence. All parameter values, targets, and data sources are reported in Table 3.

6.3.1 Assumptions

Functional forms and distributions. The quantitative model is evaluated annually.[18] Vacancy costs are $C(v) = c_0 v^{c_1}$ with $c_1 > 1$ and $c_0 > 0$, ensuring finite firm size. The matching function is Cobb–Douglas, $m(\theta) = m_0 \theta^{-\eta}$. Workers' disutility is $x_i h^{1+\phi}/(1+\phi)$ with $\phi > 0$ and $x_i > 0$. The quantitative model features six worker types (three women, three men), corresponding to six values of x_i.

Firm productivity y follows a Gamma distribution, match productivity z is uniform on $[1 - \bar{z}, 1 + \bar{z}]$, and hours are bounded between 0 and $h_{\max}$.

Parameter values fixed externally. I set β using the average interest rate, calibrate μ to the job separation rate (8%) and δ to the firm exit rate (3.6%). I set $\eta = 0.5$, normalize $c_0 = 1$ (since it is not separately identified from m_0), and set $\phi = 2$ (Chetty 2012, Jarosch et al. 2025). I estimate the distribution of the disutility scale x_i, which is not separately identified from curvature. Sensitivity to η and ϕ is assessed below.

[18] Production of a job and work disutility are equal to $52 \times zyh^{\alpha}$ and $52 \times x_i \frac{h^{1+\phi}}{1+\phi}$, respectively.

6.3.2 Calibration and estimation of the structural parameters

The sequential estimation of the model proceeds in four steps and makes use of job ads and administrative and survey data. In what follows, I describe the main procedure for each step. Additional details are provided in Online Appendix H.3.

Step 1: Work disutility and the distribution of worker types. The model features three types of men and three types of women. Preferences over hours are given by the disutility function $x_i\, h^{1+\phi}/(1+\phi)$, so that heterogeneity across workers operates through the scale parameter x_i. The objective of this step is to recover the empirical distribution of x_i and approximate it by a discrete distribution.

I use the French Labor Force Survey (2011–2012), restricting the sample to employed workers with observed hourly wages, stated preferred hours, and gender. Preferred hours at the current wage are constructed from the questions: *"Would you have preferred to work fewer (more) hours than in the reference week, with proportional variation in earnings?"* and, when applicable, *"How many hours would you have preferred to work?"*. For workers satisfied with their current hours, preferred hours are set equal to actual hours, yielding desired hours holding the hourly wage fixed. To isolate variation not explicitly modeled, I residualize both hourly wages and preferred hours with respect to age, occupation, education, and industry, yielding for each worker a pair (w, h^{pref}).

In the model, preferred hours at wage w satisfy the first-order condition $x_i h^{\phi} = w$. Given ϕ, which is fixed and common across workers, I invert this condition at the individual level to recover an implied value of x_i for each observation. I then approximate the resulting empirical distribution by a discrete distribution with three support points for each gender, which defines the six worker types used in the quantitative model. On average, the estimated disutility shifter is 38% higher for women than for men, a difference largely driven by the 8% of women with the highest disutility.

Step 2: Technology parameters. I combine within- and between-firm variation in job advertisements to identify the parameters governing the production technology. I use the relationship between posted wages and hours and interpret the latter as what would be the firm's optimal choice before the match characteristics are observed (consistent with posted hours departing from realized ones in Figure H.3). As a preliminary step, I set the upper bound on hours $h_{\max}$ equal to the 99th percentile of posted hours, and calibrate the cost of hours above 35, τ, to 25% of the average posted wage, consistent with overtime regulation. Next, I estimate the elasticity of output with respect to hours, α, using within-firm variation from firms posting multiple vacancies. Model Equation (9) implies a log-linear relationship between the posted hourly wage and the number of hours chosen by the firm. I therefore regress log wages on log hours with firm fixed effects. By exploiting variation across vacancies within the same firm, this specification abstracts from firm-level heterogeneity and identifies $\alpha - 1$. Finally, given α, I use between-firm variation in postings to estimate the distribution of firm productivity, parameterized as a Gamma distribution with shape and scale parameters $(y_{\text{shape}}, y_{\text{scale}})$. For each firm, I randomly select one vacancy as representative of the ex-ante wage–hours relationship in that firm, before matching takes place and thus abstracting from match-specific characteristics. I estimate $(y_{\text{shape}}, y_{\text{scale}})$ by maximum likelihood: conditional on a posted wage w, I compute the likelihood of observing posted hours h using the firm's policy function (Equation (9)). While I remain agnostic about the process generating posted wages; conditional on the posted wage, I assume the firm sets expected hours optimally.

Step 3: Equilibrium hours. Remaining parameters are estimated conditional on Steps 1 and 2. I use the cross sectional distribution of hours worked to identify the vacancy-cost curvature parameter c_1, the worker bargaining parameter γ, the dispersion of match specific productivity $\bar{z}$, and unemployment values W_u, which are treated as free parameters at this stage (normalizing W_u for one worker type and estimating the others relative to it). In Step 4, I calibrate unemployment flow values b to ensure consistency.

These parameters affect the hours distribution through distinct channels. First, given the productivity distribution, the curvature of vacancy costs c_1 governs firms' vacancy posting decisions and therefore the relative weight of different firm types in the economy. Equation (22) in Appendix shows that the number of hours of a given job depends on γ (through its effect on the wage bargain). Third, $\bar{z}$ affects the within-firm distribution of hours and therefore the equilibrium distribution. Finally, the vector of unemployment values determine the bite of workers' participation constraint in the hours setting. I estimate those parameters by generalized method of moments, and I minimize the distance between the distribution of hours generated by the model and its empirical counterpart. Figure 9 compares the two distributions. The model matches well the share of jobs below 24 hours and the fact that women are more likely to hold low-hour jobs, although it does not fully account for the presence of very short-hour contracts (below 8 hours).

Step 4: Equilibrium unemployment. In the final step, I discipline the remaining parameters governing job finding and workers' outside options. The scale of the matching function, m_0, is calibrated so that the model reproduces the aggregate unemployment rate observed in the data. In addition, I set the flow utility of unemployment, b, to ensure internal consistency of the unemployment value equations across worker types. I allow b to differ by type. Online Appendix Table H.2 reports the model fit for a set of non-targeted moments.

6.3.3 Estimation of the policy parameter

The 24-hour reform is represented as a cost on jobs with weekly hours below 24: $\rho \max(24 - h, 0)$, consistent with how compensation is computed in labor courts. I estimate ρ by matching the reduced-form effect of the reform on the number of jobs in 2017. Specifically, I simulate within the model the same firm-level difference-in-differences specification as in the data. In both the data and the model, this specification compares the relative changes of more versus less exposed firms, based on their pre-reform Share24 as in Equation (1), and allows for general-equilibrium adjustments and indirect effects. Having multiple-job firms is therefore key to the mapping between the model and the empirics. Online Appendix Figure H.4 shows that two solutions match the reduced form; however, the first is discarded because it implies a low value of ρ and virtually no aggregate change in low-hour jobs. Online Appendix Table H.4 compares the difference-in-difference estimates in the data and in the model for other outcomes. I obtain $\hat{\rho} = 2.13$, implying that a job with a 14-hour workweek (i.e., 10 hours below the floor) entails an additional cost of 21.3 euros per week for the firm.

In the model, I compare two steady states. Two caveats apply. First, I match the event study coefficient in the data three years after the reform's introduction, which may miss longer-run adjustments.[19] Second, the model abstracts from technological responses to the reform, which appear limited in the short run based on evidence on the capital to labor ratio in Online Appendix Figure C.2.

6.4 Simulation results: 24-hour reform

Employment. Table 4 reports the effects of the reform on employment, hours, and unemployment, in both partial and general equilibrium (as defined in Section 6.2). The reform reduces the number of jobs below 24 hours by 53% in partial equilibrium and 22% in general equilibrium. These figures compare to two benchmarks: the aggregate decline observed in Figure 1 (40% in 2016 and 50% in 2017) and the back-of-the-envelope prediction implied by the firm-level estimates in Table C.1 (67%). The general-equilibrium figure is smaller because the decline in outside options restores some sub-24-hour matches, suggesting that some low-hour jobs may reappear as labor market adjustments fully materialize.

In partial equilibrium, total employment falls by 5.2% and total hours by 2.0%. The effects are driven by women, for whom the reform raises the job-creation cutoff $\underline{z}(i, y)$ for many matches: high-disutility workers lose payoff when hours are pushed above 24, making previously viable matches unprofitable. For men, the cutoff moves in the opposite direction: with low disutility, some matches that would not have been created—because optimal hours under right-to-manage would have been too low to generate positive worker value—become viable when the reform pushes hours upward. As a result, male employment increases by 1.5% and male total hours by 1.0%.

[19] Models of this kind are known to converge quickly to the new steady state (Jolivet et al. 2006).

The picture changes substantially in general equilibrium. The aggregate employment decline is attenuated to 1.4% and total hours fall by only 0.5%. For men, the effect on employment and total hours flips sign (-0.15% and -1.5%, respectively). As the reform reallocates jobs toward less exposed firms offering longer hours, the decline in $W_u(i)$ has a lower bite for low-disutility workers: at these firms, the pre-reform conversion probability $1 - H(\underline{z}(i, y))$ was already close to one, leaving little room for the acceptance margin to expand. For women, the reallocation is powerful: their pre-reform cutoff was high, so the decline in $W_u(i)$ brings a large mass of matches into the acceptance region at low-exposure firms. Female employment losses are attenuated from 11.4% to 2.6%, and female total hours flip sign to $+0.4\%$. The unemployment rate increases by 0.13 percentage points for men and 2.4 percentage points for women. The number of active firms is essentially unchanged, consistent with the reduced-form evidence.

Wages and welfare. Table 5 reports the effects on wages and welfare. Average hourly wages and annual earnings increase for women and decline for men, suggesting a substantial narrowing of the gender gap. However, these changes largely reflect the mechanical destruction of low-hour, low-wage jobs and the resulting shift in the composition of employed workers within each gender, rather than genuine wage gains.

Welfare provides a more comprehensive measure, as it accounts for both employed and unemployed workers. Average welfare declines by 8.1%, with nearly identical losses for men and women—so the reform barely affects the gender gap. This similar aggregate decline masks different channels, however. For women, the rise in the job-creation cutoff for high-disutility types sharply reduces the option value of search, thereby substantially reducing the value of unemployment. For men, the loss is larger among the employed: the decline in outside options compresses wages, there is little positive selection among surviving matches (since male employment barely changes), and reallocation toward jobs at the 35-hour kink raises the weight of the full-time distortion.

Panel (b) of Figure 8 compares the estimated right-to-manage equilibrium with the counterfactual efficient-hours allocation. The two are close: the share of jobs with workweeks below 24h is only one percentage point higher under right-to-manage. Appendix Figure H.5 confirms that, while hours are systematically too low for low-hour jobs, the gap to efficient hours remains modest — between one and two hours. This suggests that the reform had limited room to correct inefficiencies, as confirmed by the additional counterfactuals in Section 6.5.

Total output. Table 6 reports the changes in production. Gross market production declines by 1.02%. Although the reform reallocates workers to more productive firms, this is not sufficient to compensate for two forces: fewer total hours worked and decreasing returns to hours at the job level ($\hat{\alpha} = 0.80$), which implies that concentrating more hours in fewer jobs is less efficient than spreading them across workers. Output net of hiring costs falls by only 0.74%, as lower vacancy posting reduces hiring costs by 5.40% and partially offsets the gross decline. Costs associated with the 35-hour regulation decline by 1.85% as there are fewer jobs with hours strictly above 35h after the reform. Output net of the costs associated with the 24-hour reform falls by 1.4%. Finally, removing all costs yields a net output decline of 1.15%. Net output nonetheless declines less than welfare, because it does not account for the increase in work disutility borne by employed workers.

Alternative parameter values. I assess sensitivity by varying key parameters one at a time around the baseline values. Results are reported in Online Appendix H.4 (Figures H.7, H.8; H.9). Changes in employment, total hours, and output in response to the reform are broadly similar across alternative values of η, ϕ, and γ; the main differences concern the magnitude of the welfare effects. A higher matching elasticity η accentuates the welfare reduction for women, because it makes the unemployment value more sensitive to the job-finding rate—and the reform disproportionately affects women's conversion probability, amplifying this channel. A higher curvature of work disutility ϕ also amplifies the welfare decline for women, as a larger mass of low-hour jobs exists pre-reform and the floor binds more strongly.

6.5 Working time regulations: counterfactuals

I now use the model to explore three sets of exercises. First, I vary the floor on hours between zero and 30h. Second, I depart from the 35-hour full-time regulation and consider full-time workweeks from 33 to 39 hours. Third, I consider interactions between those two sets of policies. The 24-hour restriction was introduced in a context where France already featured a 35-hour full-time regulation—a relatively low threshold compared to most countries. While the reduced-form evidence can only speak to this specific context, the model allows me to assess whether the effects of a floor on hours would differ under a different full-time regulation. Figure 10 reports the results for employment and welfare (and Appendix Figure H.10 for total hours and output).

Varying the floor on hours. I consider minimum workweeks ranging from 0 to 30 hours, comparing steady states (panels (a) and (b)). Floors at 10 hours and below have almost no effect, as the model generates very few jobs at those workweeks. Above that threshold, employment and welfare losses increase with the floor. The employment decline is increasingly concentrated among women: high-disutility workers are disproportionately affected as the floor eliminates the low-hour matches on which they rely. For floors above 25 hours, the welfare decline becomes substantially larger for women, widening the gender gap. Total hours always increase for women as they reallocate toward longer-hour jobs, but this is more than offset by a decline in male total hours, so the net effect on total hours is always negative. Strikingly, these results are nearly identical whether or not the 35-hour regulation is in place—the effects of the floor on hours do not depend on the pre-existing full-time constraint.

Varying the full-time workweek. I consider full-time workweeks between 33 and 39 hours (panels (c) and (d)). A lower mandated workweek reduces welfare but increases the number of jobs, with both effects monotonically increasing in the stringency of the regulation. The mechanism is as follows: the full-time cost reduces the value workers derive from affected jobs, as hours and earnings fall, but those jobs remain profitable. The reduction in the expected job value deteriorates workers' outside option, which lowers the job creation cutoff, making marginal matches viable. The employment gain is larger for men, who are disproportionately in long-hour jobs where the regulation binds. Firms set hours based on the wage and regulatory costs, making them overly responsive to the full-time regulation, which generates jobs with inefficiently low hours at the cutoff. Total hours and output decline despite the increase in employment, reflecting lower hours at the job level. As with the floor on hours, the results are similar whether or not a 24-hour minimum workweek is in place.

The two regulations operate through distinct margins. The floor on hours targets marginally profitable matches and acts primarily through between-firm reallocation, as jobs shift from high- to low-exposure firms. The full-time regulation changes the within-job value without destroying matches: it compresses hours and wages, deteriorating outside options and creating new but lower-surplus jobs. Despite these different channels, the two regulations do not meaningfully interact: the effects of each are largely invariant to whether the other is already in place.

7 Conclusion

Working time regulations are widely used as policy tools. This paper provides a comprehensive assessment of the margins along which workers, firms and the labor market adjust to the introduction of a minimum working time. I exploit the implementation of a floor of 24 hours per week in France in 2014 to document the firm-level and macroeconomic effects of a restriction on low-hour jobs.

In response to the minimum workweek, I find a decrease in the number of workers employed and in total hours in firms ex-ante more exposed to the reform, relative to firms less exposed. This result suggests that workers and hours are not perfect substitutes within firms. I find that workers hired with more hours, because of the minimum workweek, are not the same as the workers who would have been hired with low hours. In particular, men working long hours are replacing women working fewer than 24 hours per week.

While within-firm reallocation of hours is limited in response to the policy, I find evidence of reallocation of workers between firms. The reform generates reallocation of workers from firms relying on low hours to firms relying on longer hours. At the aggregate level, however, total hours worked decreased by 0.5%. The aggregate impact is also heterogeneous by gender: the number of jobs decreased by 2.6% for women and by 0.15% for men. The model shows that the 24-hour floor would have had similar effects in the absence of the pre-existing 35-hour regulation.

Finally, this paper sheds new light on the reallocation effects and gender-heterogeneous impact of working time regulations. These effects are likely relevant with other types of regulations such as zero-hours contracts or minijobs. The developed structural framework and estimation strategy offer tools to analyze other regulations that may differentially impact workers based on their labor supply.

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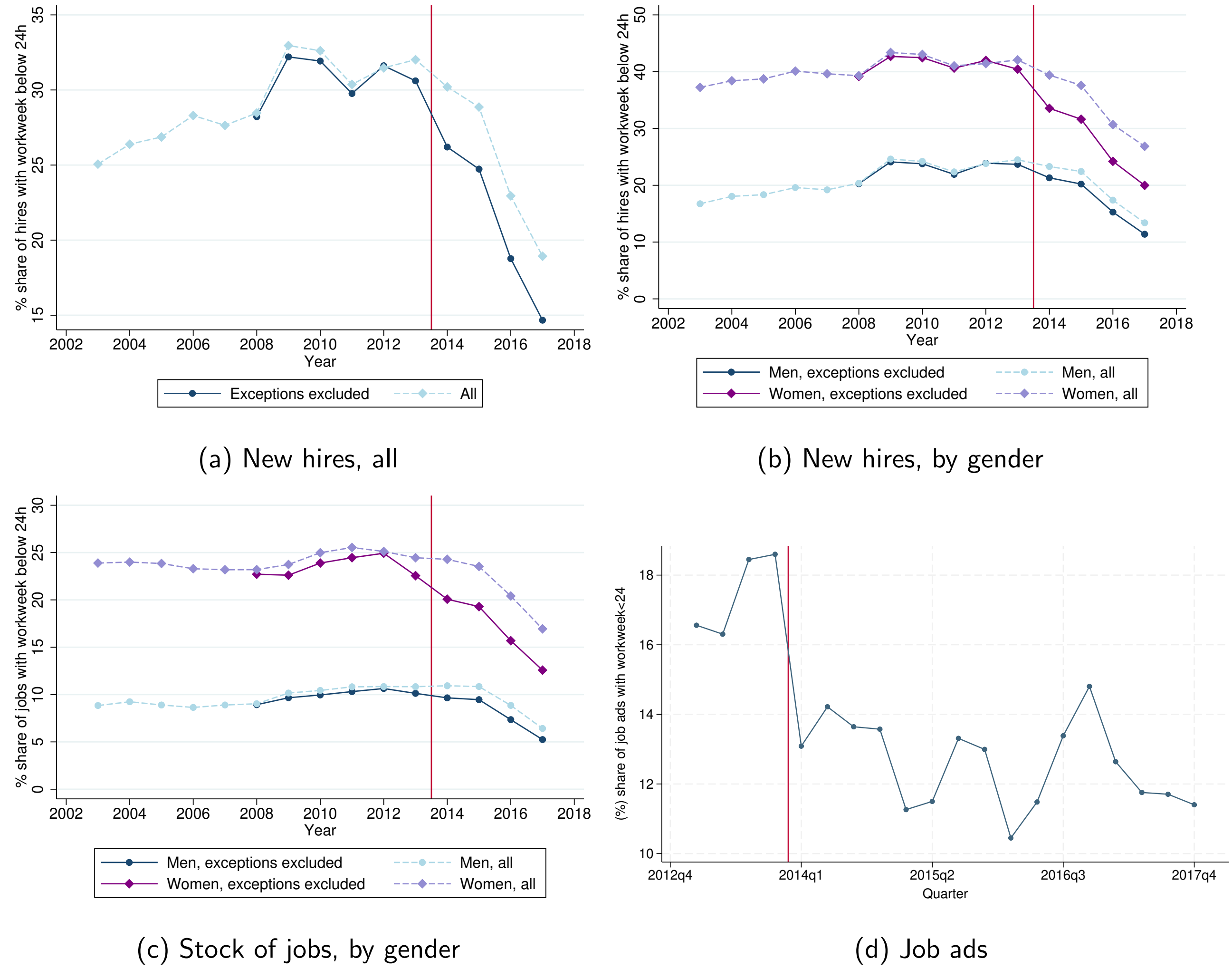
FIGURES



(a) New hires, all

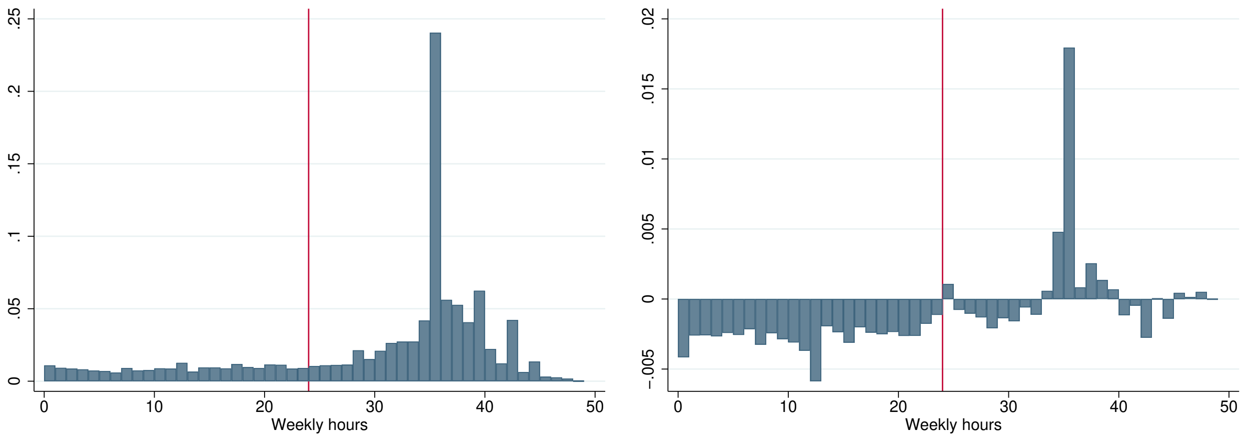
(b) New hires, by gender

(c) Stock of jobs, by gender

(d) Job ads

Figure 1: Aggregate use of jobs with working time below 24h over time

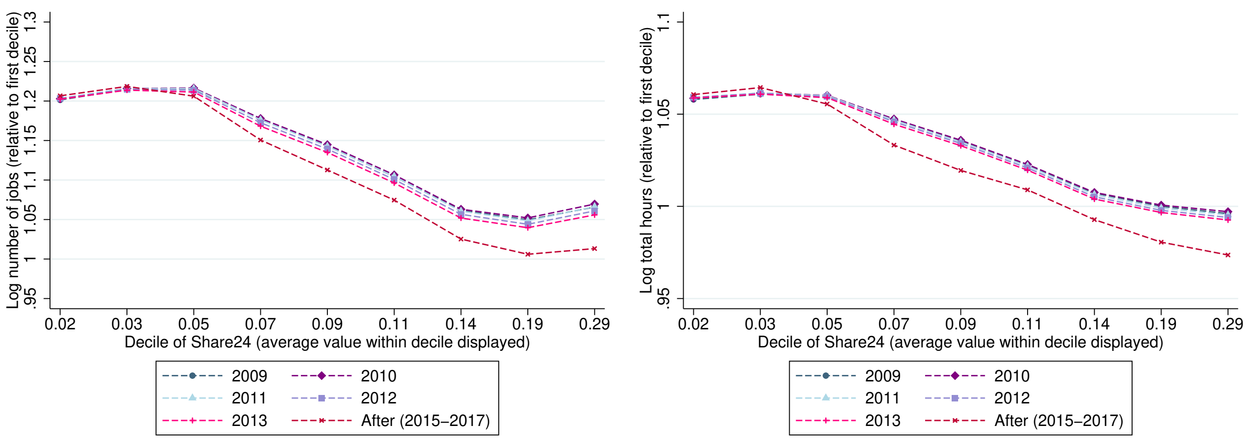
Notes: Panels (a), (b) and (c) show the share of jobs below 24h among new hires and in the employment stock, per year. 'Exceptions excluded' indicates that firms covered by industry agreements with different minimum working times are removed. They are obtained from the DADS. Panel (d) plots the quarterly share of job ads posted with required working time below 24h. It is computed using the Pôle Emploi data. **Go back to main text.**



(a) Pre-reform (b) Change after reform

Figure 2: Distribution of working time

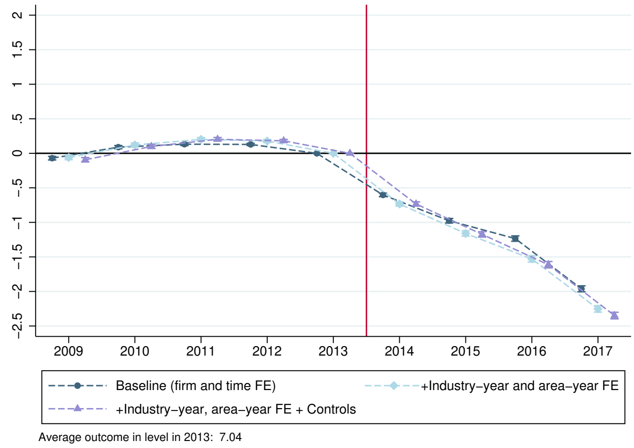
Notes: This figure plots the distribution of weekly hours worked in the stock of jobs on average over 2011-2012 (Panel (a)) and the change between 2015-2016 and 2011-2012 (Panel (b)). The average workweek includes both contractual and overtime hours. Computed from the DADS. Private sector only. Workers younger than 24 years old excluded. Industries covered by exceptions to the 24h-rule are excluded. In Panel (b), each bar shows the difference between the number of jobs in the bin after the policy and the number before, normalized by the total number of jobs before: $\frac{NbJobs(h)_{after} - NbJobs(h)_{before}}{NbJobs_{before}}$. **Go back to main text.**



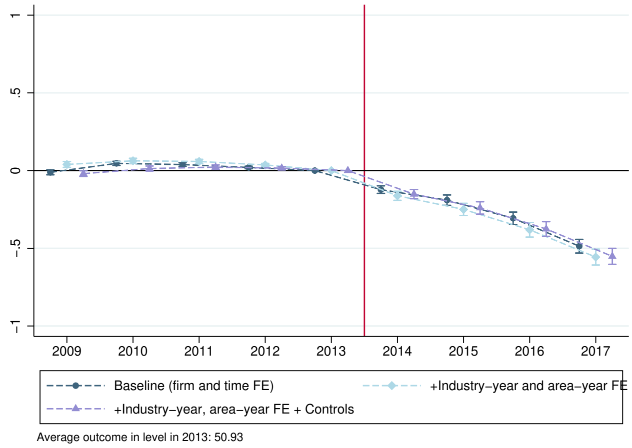
(a) Number of jobs (b) Total hours

Figure 3: Relationship between Share24 and outcomes over time

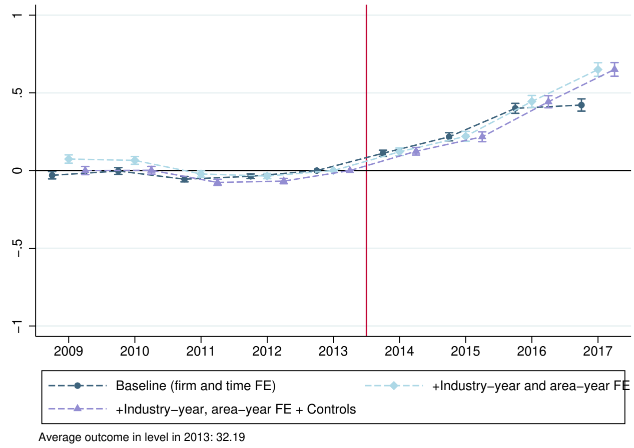
Notes: This Figure plots, for each decile of exposure to the reform (Share24), the average log outcome, for every year. The average value of Share24 in each decile is reported on the x-axis. For each year, log outcomes are normalized by the value of the variable in the first decile (and the value of the first decile, equal to 1, is omitted). All post-reform years (2015 to 2017) are pulled together and pre-reform years are computed separately. 2014 is excluded because it is only partially treated. Online Appendix Figure E.1 shows the year-by-year evolution after the reform. Outcomes are computed in the baseline estimation panel of firms, from which firms smaller than size 5 before the policy and firms covered by industry agreements with exceptions to the policy are excluded. **Go back to main text.**



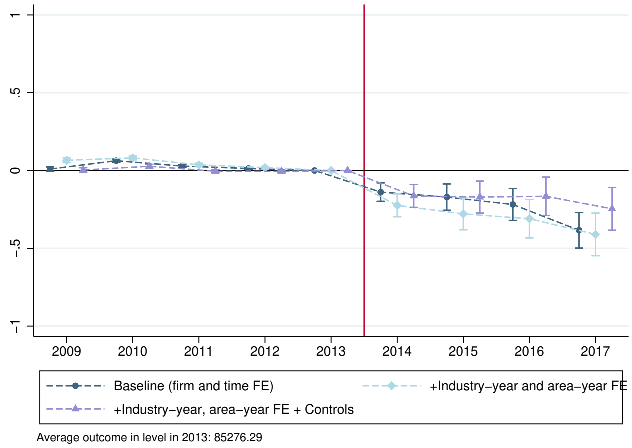
(a) Jobs<24h



(b) Number of jobs



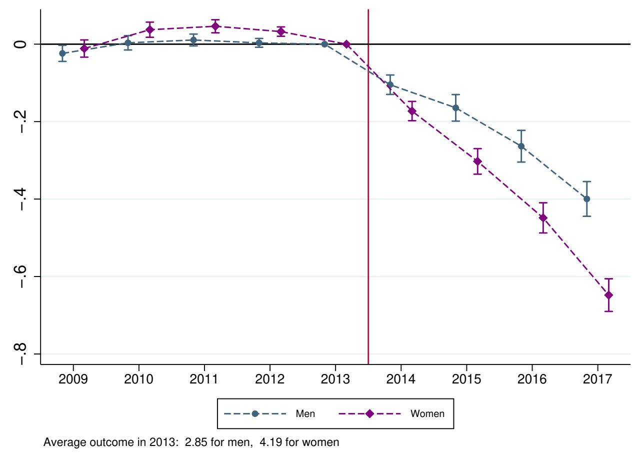
(c) Full-time jobs



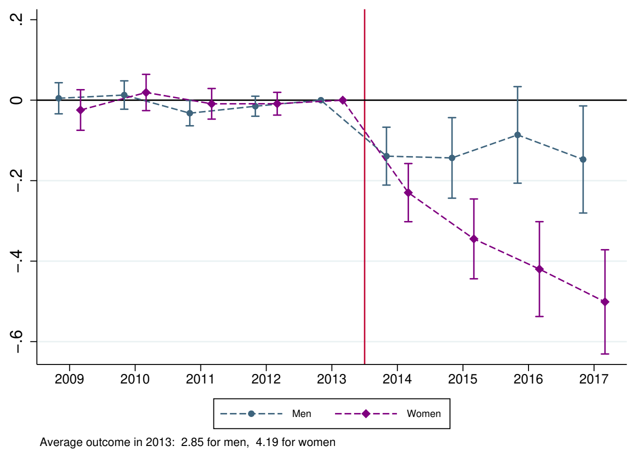
(d) Total hours

Figure 4: Firm-level effects

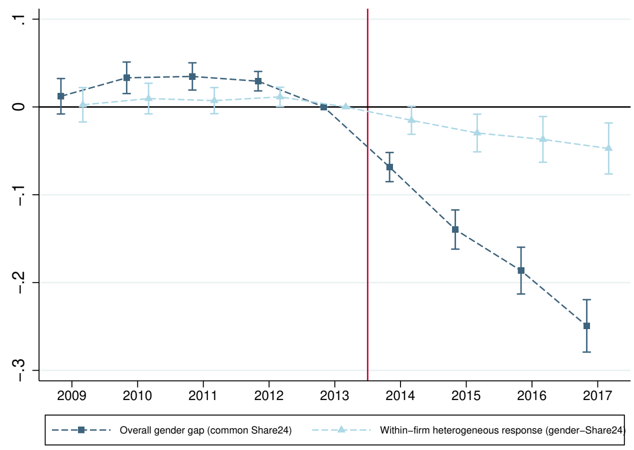
Notes: This figure plots the estimates of the β_k parameters in Equation (1), for each year, as well as the 95% confidence intervals (standard errors are clustered at the firm level). For each outcome, I report estimates of a regression with firm and year fixed effects only (Baseline), with added industry-time and area-time fixed effects and the full model with also time-varying age and size effects. Estimation on the balanced panel of firms from which firms smaller than size 5 before the policy and firms covered by industry agreements with exceptions to the policy are excluded. Interpretation for 2016 for the full model: a 1 percentage point higher share of jobs below 24h before the policy is associate with a decrease of 1.6% in the number of jobs below 24h, of 0.4% in the total number of jobs, of 0.2% in total hours worked in the firm and an increase by 0.4% in the number of full-time jobs. **Go back to main text.**



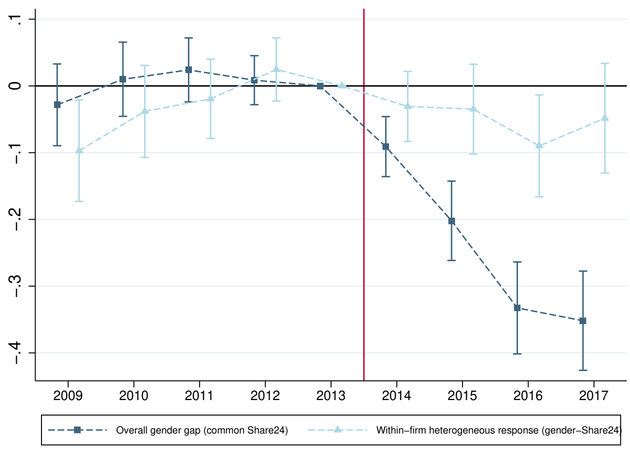
(a) Number of jobs



(b) Total hours



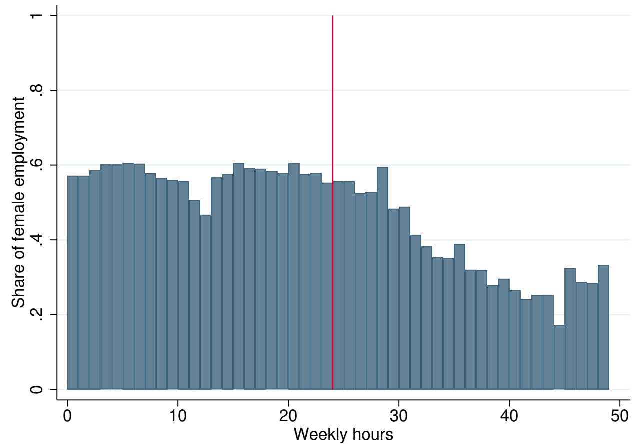
(c) Gender gap decomposition: Number of jobs



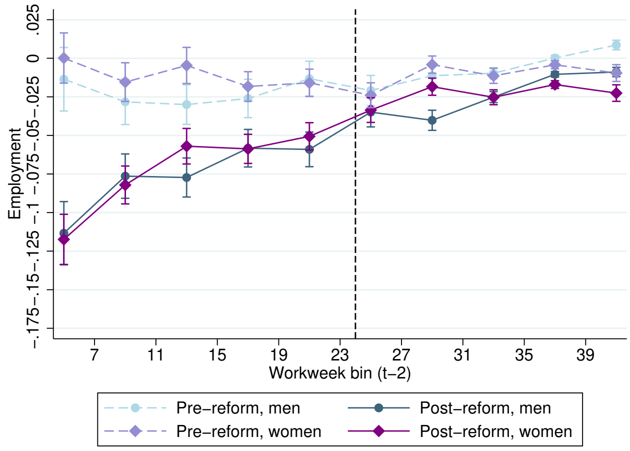
(d) Gender gap decomposition: Total hours

Figure 5: Firm-level effects by gender

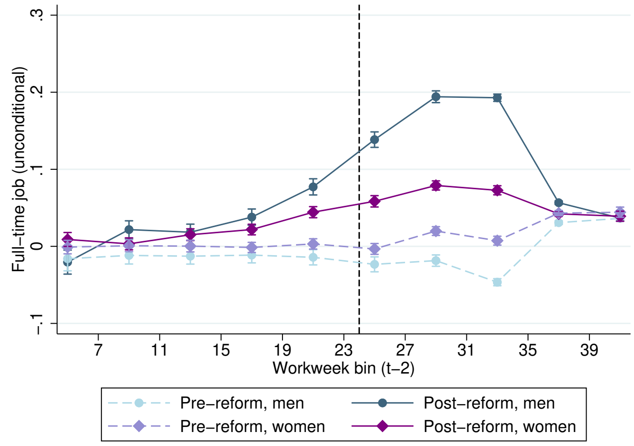
Notes: This figure plots the effects by gender (panels (a) and (b)) and the gender gap and its decomposition (panels (c) and (d)). Panels (a) and (b) report the estimates of the β_k parameters in Equation (1), for each year, as well as the 95% confidence intervals (standard errors are clustered at the firm level). The regression in which the outcome is for men has been estimated separately from the one in which the outcome is for women. In panels (c) and (d), the "Overall gender gap" corresponds to the estimates of δ_k in Equation (2). The "Within-firm heterogeneous response" reports estimates of $\pi_k^{Women} + \pi_k^{Men}$ in Equation (3). Estimation on the balanced panel of firms from which firms smaller than size 5 before the policy and firms covered by industry agreements with exceptions to the policy are excluded. Reported estimates are for the full model with firm and year FE, time-varying industry, area, age and size effects. Estimated coefficients (y-axis) can be interpreted as the % change in outcome associated with a 100 percentage points increase in exposure to the reform. **Go back to main text.**



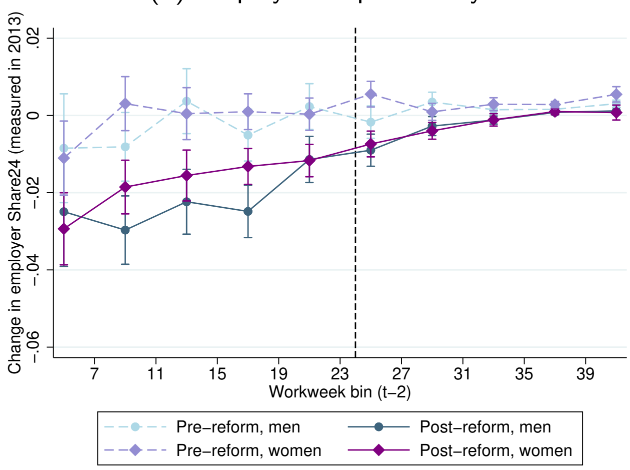
(a) Pre-reform female share by workweek



(b) Employment probability



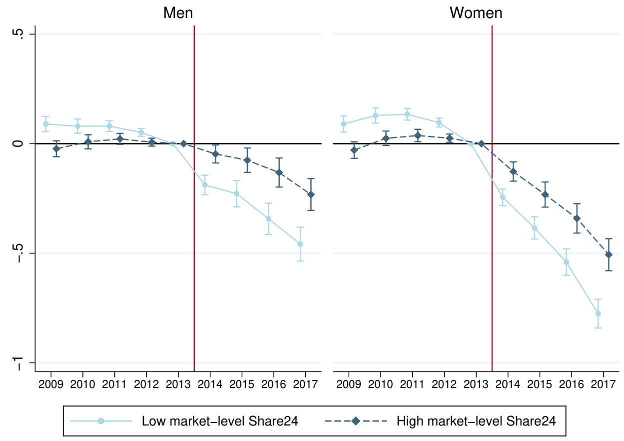
(c) Full-time job probability



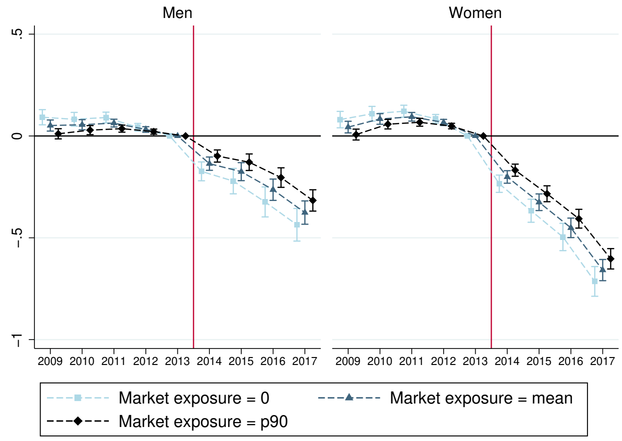
(d) Change in employer type

Figure 6: Worker-level changes by previous workweek

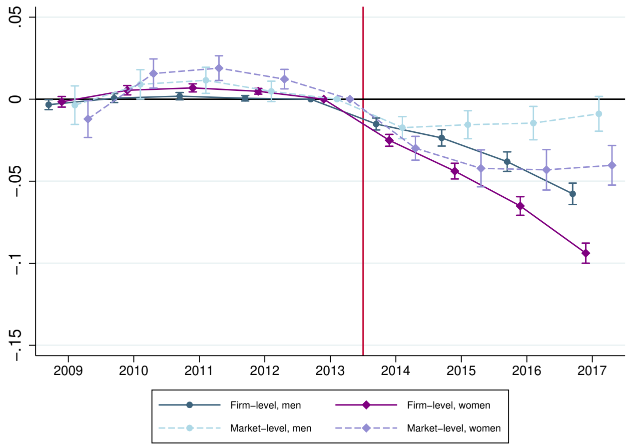
Notes: Panel (a) reports the share of female employed by working time bin before the reform (in 2012). Panel (b)-(d) plot estimates of δ_{d2016} and δ_{d2014} in Equation (4), separately for men and women. The two solid lines connect estimates for 2016 versus 2014 relative to 2011 versus 2009. The two dashed lines present a pre-reform benchmark and connect estimates for 2013 versus 2011 relative to 2011 versus 2009. Outcomes are the employment probability (panel (b)), the two-year change in the probability to work full-time (panel (c)) and the two-year change in the type of employer (panel (d)). The type of employer is measured by the pre-reform share of jobs below 24h. Estimates are per hour bin two years before. Online Appendix Figure F.2 shows additional outcomes. **Go back to main text.**



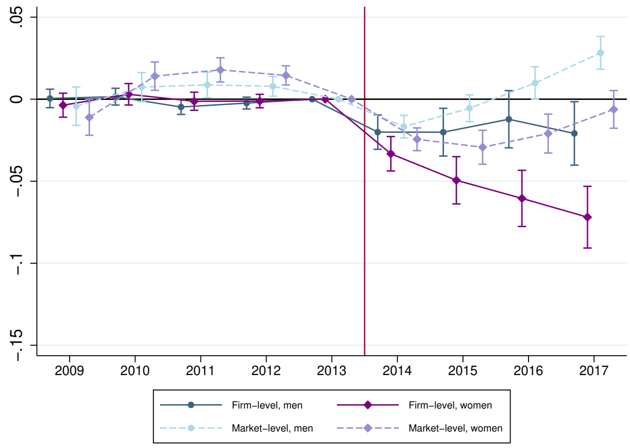
(a) Firm-level employment effects by market-exposure



(b) Employment effects from the triple-differences



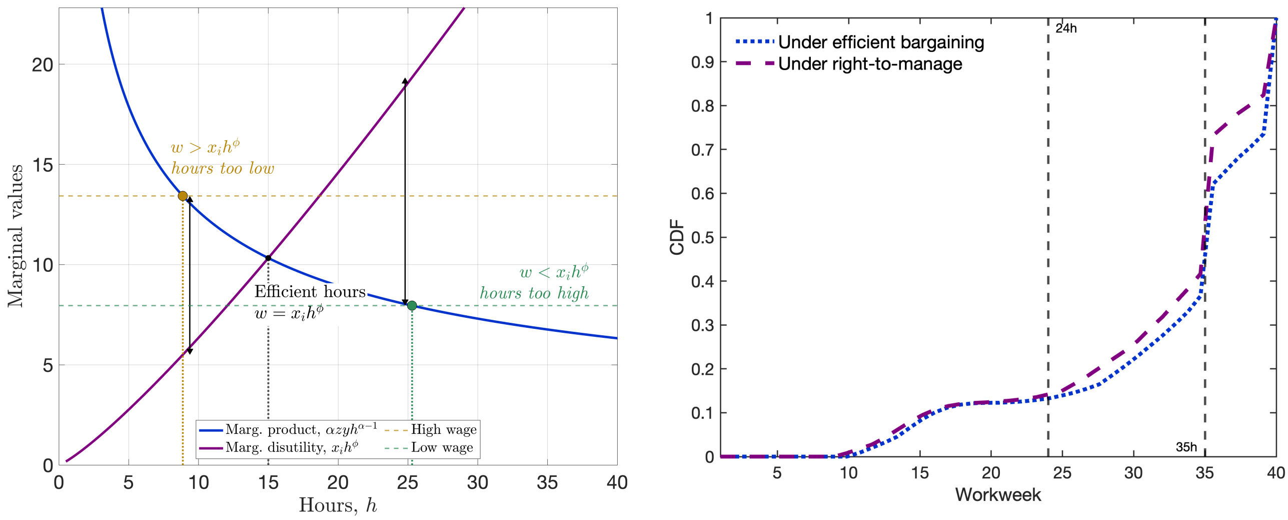
(c) Market versus firm level: employment



(d) Market versus firm level: total hours

Figure 7: Evidence of between-firm reallocation

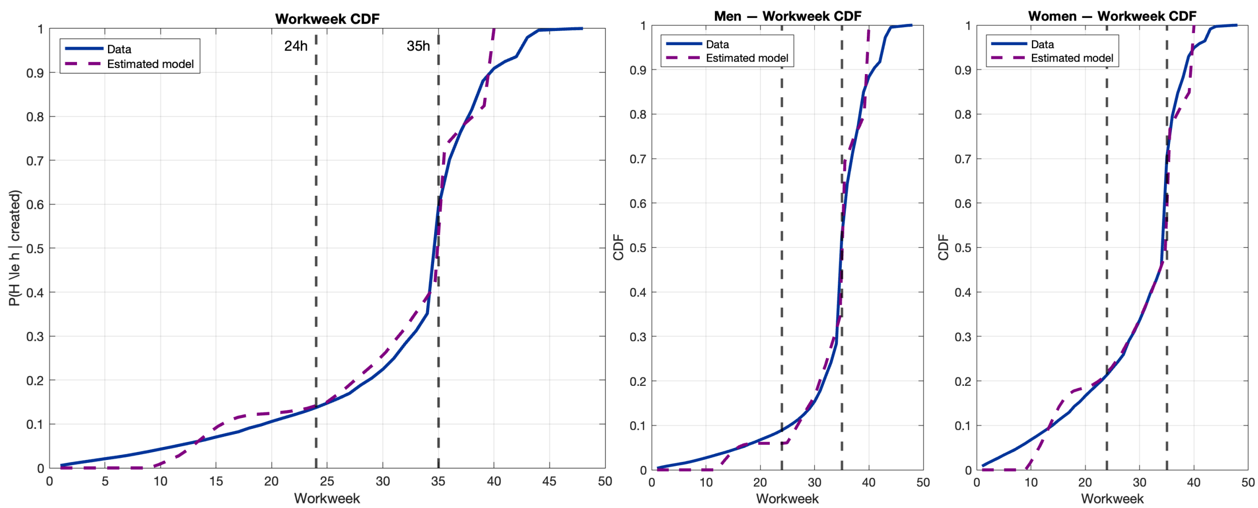
Notes: Panel (a) reports the results of four separate estimations of Equation (1): separately for firms in markets with Share24$_{m(-i)}$ above versus below median, and by gender. Panel (b) plots the results from the triple-differences (Equation (5)), $\widehat{\lambda_k} + \widehat{\gamma_k} \times s$, for various values of s, the market-exposure: 0, the mean, and the 90th percentile. Panels (c) and (d) report estimates of event studies at the level of the market, along with the firm-level event studies. For those two panels, the firm-level and market-level exposure measures have been standardized for comparability. In all four panels, a market is defined by a commuting zone and industry. **Go back to main text.**



(a) Theory: illustration of RTM (b) Comparison for estimated parameters

Figure 8: Hours worked under right-to-manage versus efficient ones

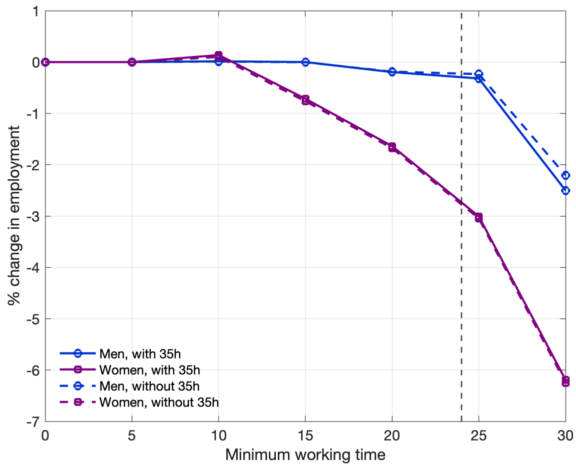
Notes: Panel (a) illustrates the choice of hours of a given firm for a given worker, for two types of negotiated hourly wages, under right-to-manage. It compares it with the efficient hours. This is an illustration for a theoretical job. In panel (b), I plot the distribution of hours in my right-to-manage model for the estimated values of the parameters, along with the hours distribution I would obtain under efficient bargaining. **Go back to main text.**



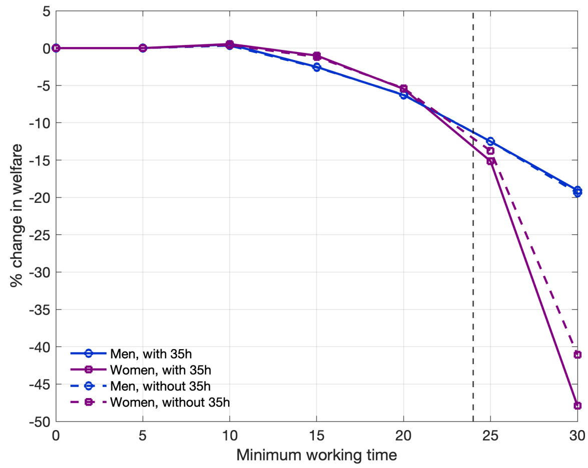
(a) All workers (b) By gender

Figure 9: Pre-reform distribution of hours in model and data

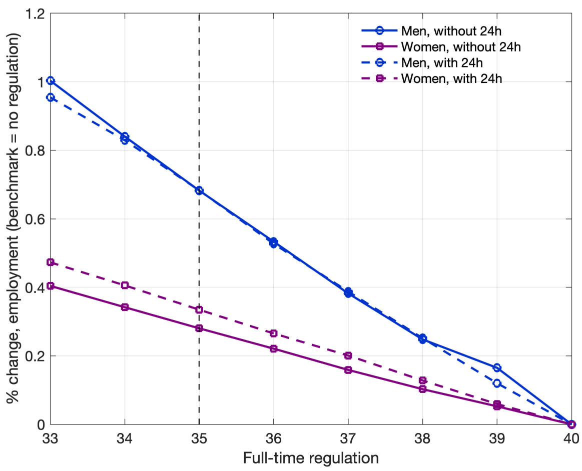
Notes: This figure plots the distribution of hours worked in the economy, computed with the DADS, and in the model for estimated values of the structural parameters. Those distributions are targeted in step 3 of the estimation procedure. **Go back to main text.**



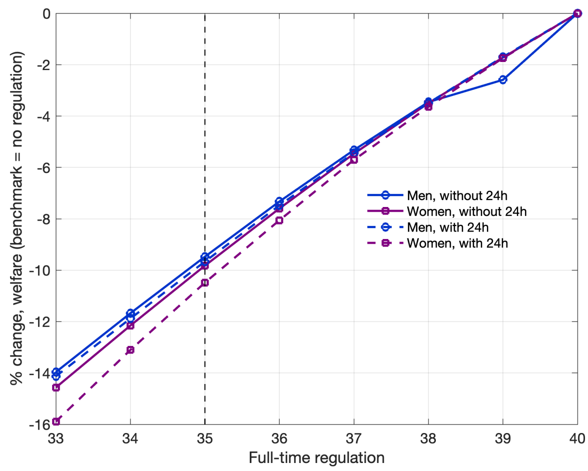
(a) Varying floor on hours: employment



(b) Varying floor on hours: welfare



(c) Varying full-time workweek: employment



(d) Varying full-time workweek: welfare

Figure 10: Policy counterfactuals: changes in minimum and full-time workweek

Panels (a) and (b) display the employment and welfare effects, by gender, of introducing a legal minimum working time, as indicated on the x-axis. The benchmark is the equilibrium without a minimum working time. For each counterfactual, I consider two scenarios: (i) the introduction of a minimum working time in the presence of a pre-existing 35-hour regulation (solid lines), and (ii) the introduction of a minimum working time in the absence of any full-time regulation (dashed lines). The vertical dashed line indicates the 24-hour floor implemented in 2014 (the estimates connected by solid lines at this value correspond to those reported in Table 4). Panels (c) and (d) show the employment and welfare effects of introducing a legal full-time working time, as indicated on the x-axis. It is modeled as a cost for jobs exceeding the legal full-time workweek. The vertical dashed line indicates the 35-hour regulation as implemented in 2000 in France. The benchmark is an equilibrium without a full-time regulation, but with an upper bound on hours at $\bar{h}$ =40, as in the main analysis. I consider two scenarios for the implementation of the full-time regulation: (i) implementation in the absence of a minimum workweek (solid lines), and (ii) implementation when a legal minimum working time of 24 hours is already in place (dashed lines). In all panels, circles denote estimates for men and squares denote estimates for women. Each marker reports a separate policy change and depicts the % change in outcome between the pre-reform and post-reform equilibrium. All counterfactuals are simulated for the estimated parameter values reported in Table 3. Online Appendix Figure H.10 shows the changes in total hours and output. **Go back to main text.**

TABLES

		All	Acc & food	Construction	Manuf.	Services	Retail
Number of workers		46.86	25.43	26.49	70.69	53.18	43.27
Part-time share		0.33	0.41	0.49	0.24	0.31	0.27
Average workweek		33.71	31.78	33.43	34.94	33.43	34.12
Permanent jobs		0.85	0.68	0.89	0.89	0.85	0.84
GAP		2.01	4.25	1.03	0.98	2.78	1.59
Share24	Mean	0.12	0.22	0.09	0.07	0.15	0.11
	SD	0.16	0.19	0.09	0.09	0.20	0.12
	p5	0.00	0.00	0.00	0.00	0.00	0.00
	p25	0.01	0.08	0.00	0.00	0.01	0.01
	p50	0.08	0.17	0.07	0.05	0.08	0.08
	p75	0.16	0.32	0.13	0.11	0.18	0.16
	p95	0.44	0.63	0.25	0.24	0.64	0.33
Number of firms		187,065	16,879	31,399	32,677	60,831	45,279

Table 1: Firm-level summary statistics of characteristics in 2013

This table shows summary statistics of the firms in the main sample. All characteristics are evaluated in 2013 (one year before the implementation of the reform). Firms smaller than 5 workers are excluded, as well as firms subsequently covered by agreements with exception to the 24h-rule. Share24 corresponds to the average share of jobs below 24h. The GAP measures the average increase in hours per week needed to have all jobs above 24h. **Go back to main text.**

	Jobs $<$ 24h	Full-time	Part time $\geq$ 24h	All jobs	Total hours
A. All workers					
Share24 x After	-0.880***	0.158***	-0.006	-0.587***	-0.862***
	(0.016)	(0.013)	(0.012)	(0.018)	(0.062)
Mean in 2013	1.572	2.598	1.217	5.387	7529.094
B. Women					
Share24 x After	-0.594***	0.085***	0.029**	-0.443***	-0.918***
	(0.014)	(0.009)	(0.009)	(0.015)	(0.062)
Mean in 2013	0.929	0.917	0.565	2.411	2943.650
C. Men					
Share24 x After	-0.627***	0.123***	0.006	-0.415***	-0.791***
	(0.014)	(0.011)	(0.010)	(0.015)	(0.064)
Mean in 2013	0.643	1.681	0.651	2.976	4585.444
N	748,264	748,264	748,264	748,264	748,264

Table 2: Difference-in-difference estimates for new hires

Notes: This table shows estimates of a difference-in-difference equation estimated over 2013-2017 when the outcome corresponds to the type of new hires described in the first row. 2014 is excluded so that estimates present the average effect over 2015-2017. Estimation on the balanced panel of firms from which firms smaller than size 5 and firms covered by industry agreements with exceptions are excluded. Reported estimates are for the full model with firm and year FE, time-varying industry, area, age and size effects. Rows 'Share24 x After' show the % change in hires of the type of job indicated in the first row, associated with a 1 percentage point increase in Share24. For instance, a 1 percentage point increase in Share24 is associated with a decrease in hours worked by new hires by 0.9% on average over 2015-2017. Standard errors clustered at the firm level and shown in parentheses. Significance levels: * 0.10, ** 0.05, *** 0.01. **Go back to main text.**

Parameter	Definition	Target / Source	Value
0. Fixed externally			
β	Discount rate	3% (Annual interest rate)	0.9709
μ	Job destruction rate	8.10% (DADS)	0.0810
δ	Firm exit rate	3.64% (DADS)	0.0364
η	Matching elasticity	Fixed	0.5
c_0	Scale, vacancy cost	Normalized	1
ϕ	Elasticity of work disutility	Fixed	2
1. Workers' preferences			
$[x_{M1}, x_{M2}, x_{M3}]$	Disutility parameters, men types	LFS	$[0.005, 0.009, 0.081]$
$[p_{M1}, p_{M2}, p_{M3}]$	Probability distri., men types	LFS	$[0.23, 0.22, 0.03]$
$[x_{W1}, x_{W2}, x_{W3}]$	Disutility parameters, women types	LFS	$[0.005, 0.009, 0.077]$
$[p_{W1}, p_{W2}, p_{W3}]$	Probability distri., women types	LFS	$[0.20, 0.21, 0.09]$
2. Firms' technology			
α	Elasticity of production	Posted (w, h) (Job ads)	0.8017
τ	Cost of hours above 35	Posted (w, h) (Job ads)	3.3245
h_{max}	maximum task duration	Posted (w, h) (Job ads)	40
$[y_{shape}, y_{scale}]$	y distribution parameters	Posted (w, h) (Job ads)	$[17.04, 1.63]$
3. Equilibrium hours			
γ	Workers' bargaining power	Hours distri. (DADS)	0.7257
c_1	Elasticity of vacancy cost	Hours distri. (DADS)	3.5032
$\bar{z}$	z distribution parameter	Hours distri. (DADS)	0.4861
4. Equilibrium unemployment			
m_0	Scale matching function	10% (U. rate)	0.5720
$[b_{M1}, b_{M2}, b_{M3}]$	Utility, unemployed men	Labor supply eq.	$[-5.9, -5.6, -5.4] .10^4$
$[b_{W1}, b_{W2}, b_{W3}]$	Utility, unemployed women	Labor supply eq.	$[-9.1, -8.7, -3.1] .10^4$
5. 24-hour reform			
ρ	Cost, jobs<24h	-0.55 (DiD result)	2.1346

Table 3: Parameters of the structural model

Notes: This Table shows all the parameters of the structural model. It indicates if the parameter was fixed, calibrated or estimated and the source. Details on the procedure are presented in Section 6.3. Parameters from Panel 0. to Panel 4. are set using data on the pre-reform period. The policy parameter ρ is calibrated using the reduced-form estimate for the post-reform period. x_{M1} is the value of the disutility parameter for the first type of men and p_{M1} is the corresponding share of these workers in the economy. There are three types of men and three types of women. b_{M1} is the instantaneous utility of an unemployed man of type 1. **Go back to main text.**

	Partial equilibrium			General equilibrium		
	Men	Women	All	Men	Women	All
Number of jobs	1.502	-11.394	-5.165	-0.150	-2.635	-1.435
Number of jobs $< 24h$	-15.250	-62.533	-52.870	-4.465	-26.383	-21.904
Number of jobs $\geq 24h$	2.565	2.781	2.667	0.124	3.948	1.926
Total hours worked	0.970	-5.019	-1.962	-1.471	0.422	-0.544
Unemployment rate (ppt)	-1.342	10.364	4.658	0.134	2.397	1.294

Table 4: Employment effects in partial and general equilibrium (% changes)

Notes: This table presents the % variations in aggregate employment and unemployment in the model after implementation of the policy. The first three columns present the results in partial equilibrium, when the market tightness and the expected value of unemployment do not adjust. The last three columns present the changes in the new general equilibrium, after adjustments of all endogenous variables. For example, after implementation of the policy, 1.43% of all jobs are destroyed compared to the pre-reform steady state. The last row reports percentage point changes in the unemployment rate. **Go back to main text.**

	All	Men	Women	Gender gap (ppt)
A. Welfare				
Unemployed	-18.529	-5.782	-32.794	11.672
Employed	-6.727	-8.566	-5.009	-1.941
Weighted average	-8.143	-8.393	-7.906	-0.263
B. Wages				
Annual earnings	1.492	-3.673	7.110	-9.479
Hourly wage	3.700	-1.064	8.363	-9.195

Table 5: Effects of the minimum workweek on welfare and wages (% changes)

Notes: This table presents the % variations in average welfare and wages. Panel A. focuses on the welfare effects, for employed and unemployed workers. The last raw of Panel A. computes the change in average welfare, defined as the weighted average of the welfare of employed and unemployed workers. Panel B. is for employed workers only. Annual earnings corresponds to average employment income per worker. The last column reports percentage point changes in the gender gap between women and men. For example, the gender gap in average welfare decreased by 0.26 percentage points. **Go back to main text.**

	% Δ	% Δ(Production - c_j)	% Δ (Production - $\sum_{i=1}^{j} c_i$)
Production	-1.016	-	-
Aggregate costs c_j:			
Hiring costs	-5.405	-0.739	-0.739
Cost due to full-time regulation	-1.854	-1.010	-0.730
Cost due to the 24h-rule	-	-1.405	-1.148
Total net output	-1.148	-	-

Table 6: Effects of the reform on aggregate output (% changes)

Notes: This table presents the % variations in aggregate market production, aggregate red tape costs and production net of costs, after the introduction of the policy in the model. 'Cost due to full-time regulation' are expenses associated with jobs with workweeks above 35h, induced by parameter τ. The first column present the % change in aggregate production, aggregate costs and production net of all costs following the reform. The second column shows the change in the production net of the cost indicated on the same row in first column. The last column shows variations in total production net of all costs indicated in all rows up to current row in the first column. As an example, the total red tape costs associated with jobs with workweeks above 35h have decreased by 1.854%, the market production net of these costs has decreased by 1.010% and the market production net of these costs and hiring costs has decreased by 0.730%. **Go back to main text.**

ONLINE APPENDIX

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A Details on the 24h-reform and context

A.1 Loi Sécurisation de l'Emploi: details

The law 2013-504 (*Loi de Sécurisation de l'Emploi*) was announced by the government on June 14 2013 while François Hollande was the French president. First, on January 11 2013, three unions of workers (CFDT, CFTC and CGC) and three unions of employers (Medef, CGPME and UPA) signed an agreement to create this new law. This reform is the result of a bargain between unions. Most of the elements of this agreement were kept in the final law decided in June 2013. This law was a package of several labor market reforms with two objectives. The first objective was to create new individual rights for workers. Four reforms were related to this objective: (i) generalization of supplemental health insurance with minimum insurance requirements for dental and optical care, (ii) creation of a new system of on-the-job training that follows the worker even if she changes labor market situation, (iii) possibility to try working for a new firm without leaving the current firm to foster job-to-job mobility and (iv) workers on boards who can vote and who are trained for this. As a result, reforms targeting the first objective are unrelated to the number of hours of work and are unrelated to the 24h floor. Workers in part time jobs are also entitled to these new rights. The second objective of the law was to reduce precarious employment. For this second objective, a first reform is a change in the unemployment insurance system for workers who alternate between employment spells and spells of unemployment. After the reform, if a worker finds a job before exhaustion of unemployment benefits, these benefits will be postponed to the next unemployment spell. A second reform for the second objective is to tax fixed-term contracts and to implement hiring credits for the first months of employment of young workers under open-ended contracts. This policy has been documented as ineffective because many industries, occupations and contracts were exempted.[20] The last reform of the second objective is the minimum workweek and changes for the wage rate of overtime hours for part-time jobs, described in section 2.2. More details about those policies can be found at https://www.gouvernement.fr/action/la-securisation-de-l-emploi.

A.2 Objectives of the reform and empirical assessment

On March 5$^{\text{th}}$ 2013, the reform project was presented at the French National Assembly. The accompanying presentation document, titled *Projet de Loi Relatif à la Sécurisation de l'Emploi*, described the motivations for the policy. Three objectives were particularly emphasized: (i) the overrepresentation of part-time jobs among workers with low monthly earnings; (ii) the contribution of part-time employment to gender inequality; and (iii) the prevalence of involuntary part-time work. Below, I report several translated excerpts from that document.

[20]Details in Cahuc, P., Charlot, O., Malherbet, F, Benghalem, H. & Limon, E. (2019), 'Taxation of Temporary Jobs: Good Intentions with Bad Outcomes?', The Economic Journal.

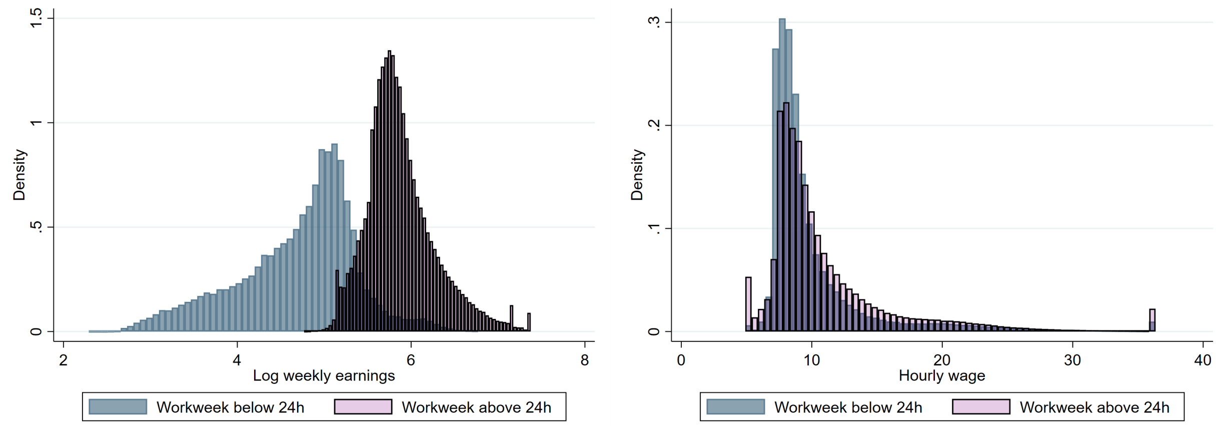
- "The major social conference held on 9–10 July 2012 highlighted that the growing prevalence of involuntary part-time work was a factor contributing to precarious employment and a major source of constraints for the workers concerned—particularly women, who represent 80% of part-time employees. Part-time work alone accounts for nearly half of the differences in monthly wages between men and women: the average gross monthly pay gap in the private sector is 24%, whereas the hourly wage gap is 14%."

- "It is therefore with the aim of improving the situation of part-time employees and moving toward greater workplace equality that the social partners took up the issue of part-time work."

- "Involuntary part-time work contributes to this all the more insofar as it corresponds to less-skilled—and therefore lower-paid—jobs than those associated with "voluntary" part-time work or full-time employment."

Figure A.1 shows substantial differences in the earnings distribution of workers above versus below 24 hours pre-reform, with limited overlap between the two groups (panel a). Most of this gap reflects differences in working time, although hourly wage distributions also differ: hourly wages are higher, on average, in jobs above 24 hours (panel (b)). Figure A.2 compares the relationship between hourly wages and workweek length in the raw data and conditional on controls. In the raw data, longer-hour jobs are associated with higher hourly wages (panel (a)). After controlling for firm fixed effects, worker age, and occupation, the pattern becomes less monotonic (panel (b)): the relationship is U-shaped, with the highest hourly wages observed in either very short-hour or very long-hour jobs, and lower wages in long part-time jobs. These results indicate that while most of the gap in total earnings between part-time and full-time jobs is driven by hours worked, evidence for a penalty on the hourly wage at low hours is weaker. Much of the hourly-wage gap appears to reflect other job attributes correlated with hours, such as worker skill or firm type.

In Figure A.3, I report pre-reform preferred working time (at the current hourly wage) from the Labor Force Survey. Part-time workers are more likely than full-time workers to prefer an increase in hours, yet most are satisfied with their current schedules. About one third of part-time workers report wanting to work more hours at the same wage.

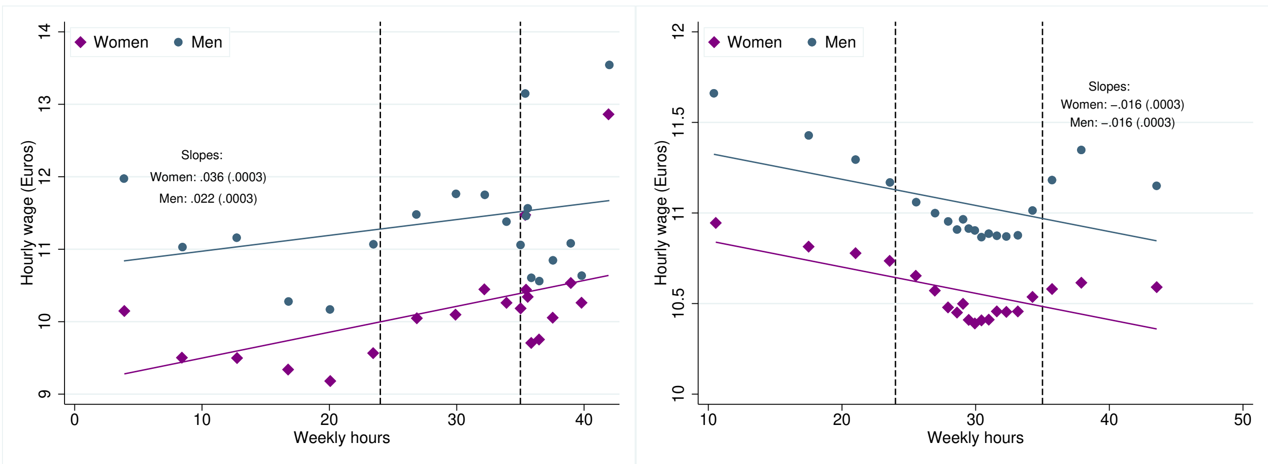


(a) Earnings

(b) Hourly wage

Figure A.1: Distribution of pay for workers above versus below 24h per week pre-reform

Notes: This Figure plots the distribution of weekly earnings (panel (a)) and hourly wage (panel (b)) by working time. Each panel overlaps the distribution for individuals with a working time below 24h per week, versus above 24h, in 2012-2013 (pre-reform). **Go back to main text.**



(a) Raw relationship

(b) Controls: occupation, age, firm FE

Figure A.2: Relationship between workweek and hourly wage pre-reform

Notes: This Figure plots the relationship between the number of hours worked per week and the hourly wage in 2012-2013 (pre-reform), by gender. Slopes of a regression of the wage on hours are printed, with robust standard errors in parentheses. Panel (a) reports the raw relationship in the data. In panel (b), I control for worker age, occupation and firm fixed effects. **Go back to main text.**

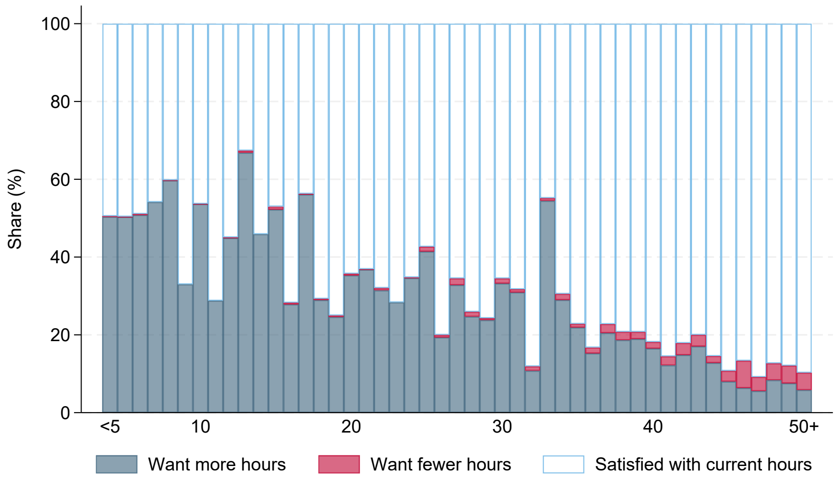


Figure A.3: Preferred working time along the workweek distribution (2013 LFS)

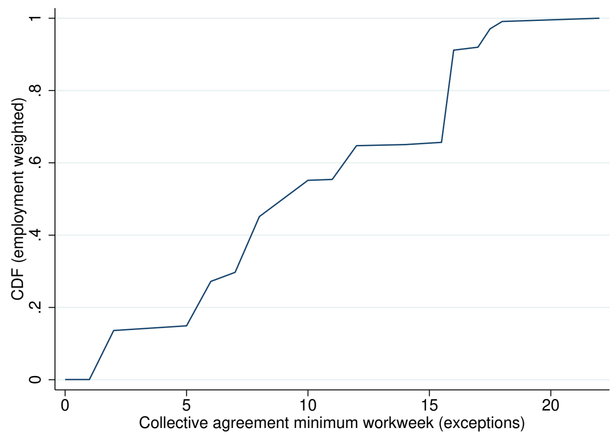
Notes: This Figure plots the distribution of work hour preferences by actual hours worked per week. The graph shows three mutually exclusive categories as stacked bars: workers who want to work more hours than they currently, workers who want fewer hours than they currently work, and workers satisfied with current hours. Hours worked are grouped into 1-hour bins, with the first bin representing workers with fewer than 5 hours per week and the last bin representing those working 50 or more hours. This graph is computed from the French Labor Force Survey in 2013. The question is about whether workers would prefer to work a different number of hours, with proportional variation in earnings. Private sector workers only. Workers younger than 18 and older than 65 are excluded. **Go back to main text.**

A.3 Collective industry agreements with exceptions

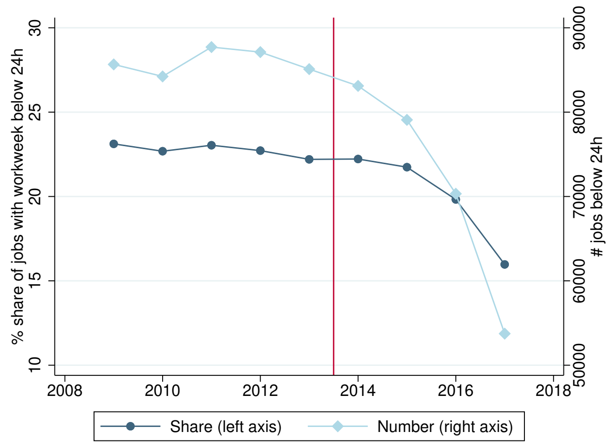
Between 2014 and 2017, 40 industries signed collective agreements with different minimum hours than 24. Below is a list of these industries. This list is characterized by heterogeneity: in a few cases, the exception is for all workers of the industry while in most cases, the exception only applies to specific occupations. For instance, in publishing and zoological gardens, the exception covers all workers while in tourism agencies and retail of sport equipment, they are only for a set of occupations. In a few cases, the application of the exception also depends on the size of the firm. This is for instance the case for social centers and bakeries.

List of industries with exceptions: private education, training providers, journalism, funeral services, entertainment, veterinary clinics, sport, deli meats retail, law firms, private sector live entertainment, dental offices, outdoor accommodation, tourism agencies, social centers, recreational boating, zoological gardens, recycling manufacturing and retail, retail pharmacy, retail of sport equipment, shoe-making, bakeries, private online learning, furniture trading, shipping companies, building caretaker, medical biology laboratories, agricultural cooperatives, milk inspection agencies, cooperative wineries, pharmaceutical distribution, equipment maintenance companies for agriculture or public works, medical offices, wellness and spa services, technical services for artistic activities, real estate, workers in social housing, cleaning services, employed veterinarian, publishing, employees of equestrian centers.

Descriptive evidence for those industries. Figure A.4 reports the evolution of the balanced panel of industries that negotiated an exception to the reform at any point during the period. Panel (a) shows that most of these industries still implemented a minimum working time, though below 24 hours. More than half, in terms of initial employment shares, adopted a minimum workweek above 10 hours. Panel (b) shows a reduction in employment below 24 hours—both in levels and as a share of total employment—also driven by the alternative minimum workweeks implemented in these industries. Finally, panel (c) documents that the shares of employment, total hours, and firms operating in industries with exceptions remain remarkably stable over time. This rules out large-scale reallocation of employment from industries without exceptions to those with one.



(a) Mininum workweek in industries with exceptions



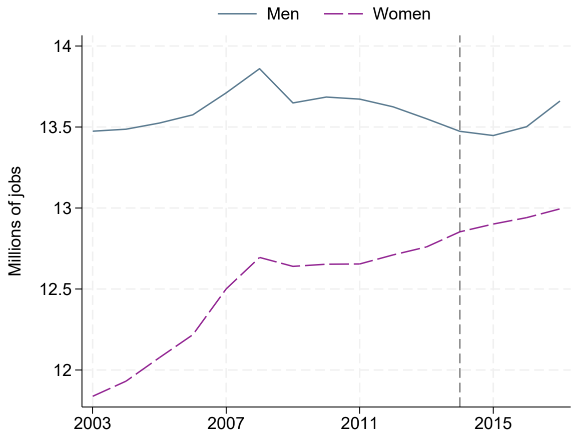
(b) Evolution of jobs below 24h

(c) Employment in industries with exceptions

Figure A.4: Descriptive evidence: industries with exceptions to the minimum workweek

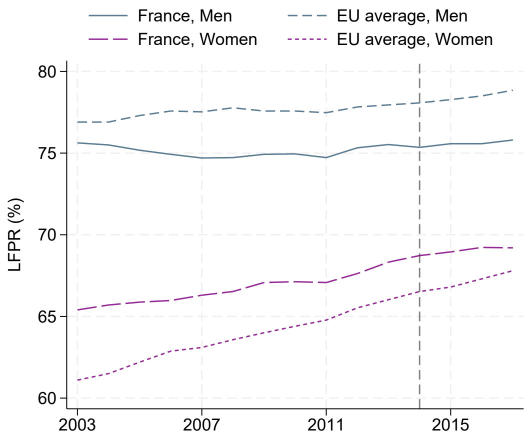
Notes: This Figure plots descriptive evidence for the industries that have signed an exception to the 24h-minimum workweek (see list of such industries above). Panel (a) reports the distribution of the alternative minimum working times decided in those industries (the employment share covered). Panel (b) presents the evolution of the number and share of jobs with workweek below 24h in those industries. Panel (c) reports the employment share of those industries over time. **Go back to main text.**

A.4 Macroeconomic trends

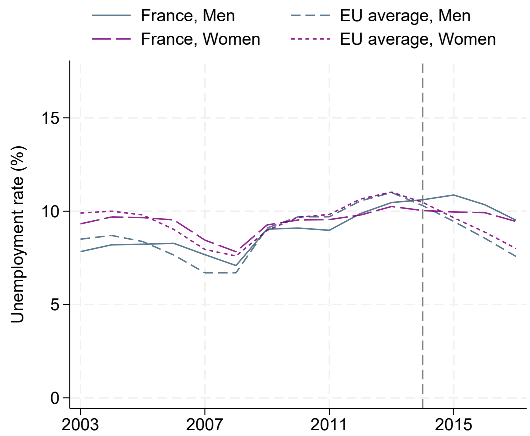


(a) Total Employment

(b) Share Public Employment



(c) Labor Force Participation Rate



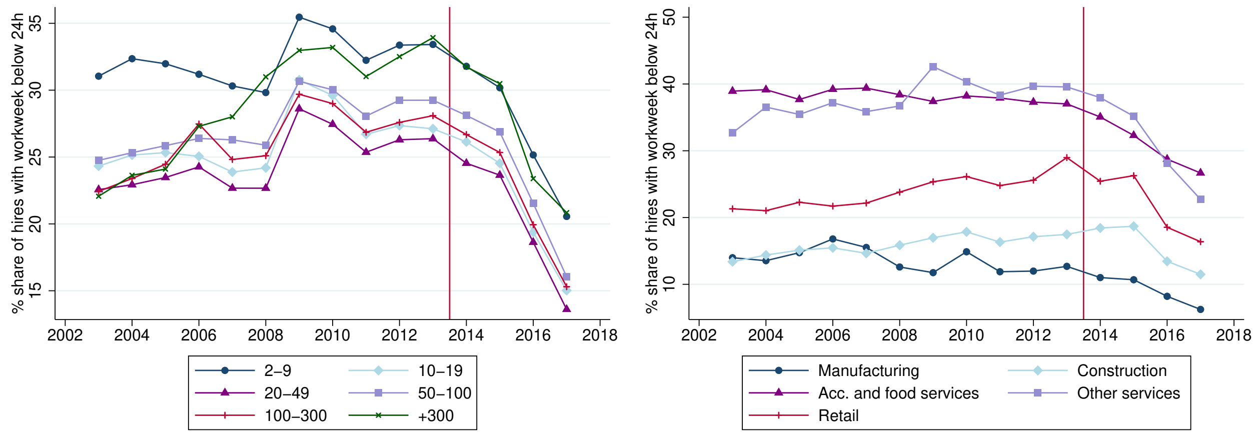
(d) Unemployment Rate

Figure A.5: Timeseries in Employment

Notes: This Figure reports labor-market indicators for France over 2003–2017. Employment levels, labor force participation rates, and unemployment rates are constructed from harmonized labor force statistics provided by Eurostat and disseminated via the Federal Reserve Economic Data (FRED) database. Labor force participation and unemployment rates refer to individuals aged 15–64 and are reported separately by gender. EU comparison series correspond to EU averages, constructed by Eurostat using population-weighted aggregates across member states. All quarterly series are collapsed to annual means.

Public sector. Between 27–30% of employed women and 16–19% of employed men work in the public sector, but the 2014 reform is unlikely to affect it directly because it applies only to private-sector contracts. Most public-sector workers are civil servants or employed under public-law contracts and are therefore exempt from the 24-hour minimum. Indirect effects through worker reallocation are also likely limited. Low-hour jobs are relatively rare in the public sector (only 8.5% of jobs were at or below 60% of full time in 2013, about half the private-sector share), and labor demand is largely insulated from workers' outside options because employment is constrained by fixed staffing levels, competitive examinations, and budgetary rules, with largely fixed wages. The main potential indirect channel is thus faster filling of existing vacancies rather than job creation.

B Descriptive evidence on jobs<24h



(a) By firm size

(b) By industry

Figure B.1: New hires of jobs with workweek below 24h over time

Notes: This Figure plots the share of jobs with working time below 24h among new hires. Panel (a) decomposes by firm size and Panel (b) by industry. Computed from the DADS. **Go back to main text.**

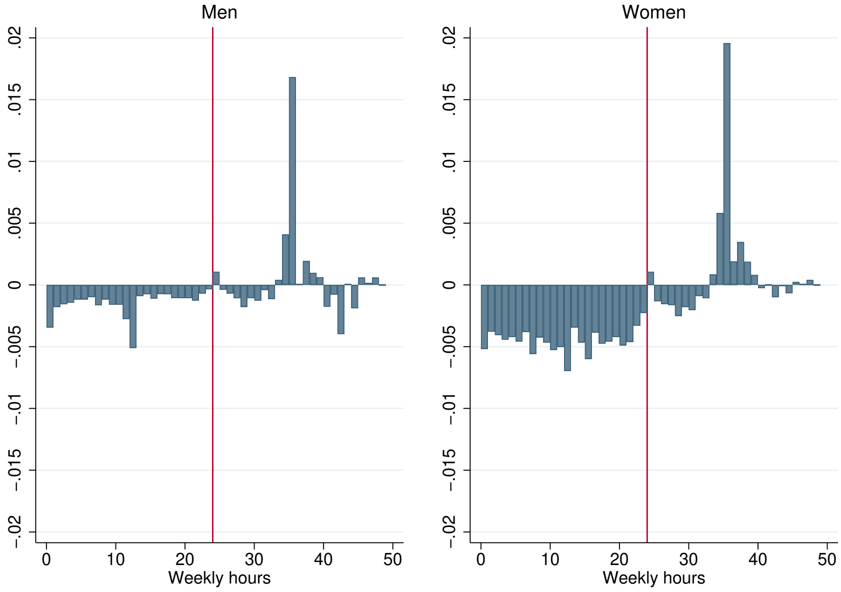


Figure B.2: Change in the distribution of hours by gender

Notes: This figure plots the change in weekly hours worked between 2015-2016 and 2011-2012, separately for men and women. The average workweek includes both contractual and overtime hours. Computed from the DADS. Private sector only. Workers younger than 24 years old are excluded. Industries covered by exceptions to the 24h-rule are excluded. Each bar shows the difference between the number of jobs in the bin after the policy and the number before, normalized by the total number of jobs before: $\frac{NbJobs(h)_{after} - NbJobs(h)_{before}}{NbJobs_{before}}$.
Go back to main text.

	Before (2013)		After (2016)	
	$h < 24$	$h \geq 24$	$h < 24$	$h \geq 24$
1. Demographics				
Age less than 27	0.30	0.24	0.28	0.24
Age 27-49	0.46	0.56	0.45	0.54
Age more than 50	0.24	0.20	0.26	0.22
Women	0.58	0.39	0.59	0.39
2. Industry composition				
Manufacturing	0.06	0.19	0.05	0.18
Construction	0.05	0.10	0.04	0.09
Retail	0.18	0.23	0.17	0.23
Accommodation and food	0.15	0.09	0.16	0.09
Other services	0.57	0.39	0.58	0.41
3. Labor contract				
Hourly wage <1.2 x Min wage	0.39	0.24	0.46	0.26
Fixed-term contracts	0.25	0.16	0.39	0.21
4. Occupations				
Managers	0.13	0.17	0.10	0.19
Technicians and professionals	0.14	0.17	0.15	0.18
White collars (low-skilled)	0.42	0.34	0.43	0.33
Blue collars	0.31	0.32	0.31	0.29
5. Most frequent occupations with $h < 24$				
Janitors	0.16	0.02	0.17	0.02
Kitchen help	0.05	0.02	0.05	0.02
Waiters	0.03	0.01	0.03	0.01
Secretaries	0.02	0.02	0.02	0.02
Waiters	0.04	0.02	0.04	0.02
Retail technicians	0.02	0.00	0.02	0.01

Table B.1: Summary statistics at the job level in 2013 and in 2016

Notes: This table shows how jobs below 24h and jobs with at least 24h are distributed along a set of characteristics. The first two columns are for 2013, the last year before implementation of the policy. The two subsequent columns are for 2016, a year and a half after the reform. The table shows statistics for jobs in the private sector. **Go back to main text.**

	h <24	h ≥24
A. All		
Married	0.49	0.48
Have kids	0.48	0.54
Average number of kids (if have some)	1.87	1.79
Have kids younger than 6	0.17	0.21
B. Women		
Married	0.52	0.47
Have kids	0.54	0.56
Average number of kids (if have some)	1.87	1.74
Have kids younger than 6	0.18	0.20
C. Men		
Married	0.38	0.49
Have kids	0.31	0.52
Average number of kids (if have some)	1.82	1.84
Have kids younger than 6	0.14	0.22

Table B.2: Family situation of workers by working time

Notes: This table presents average household composition characteristics in 2013, separately for workers with a workweek above and below 24h. The first panel corresponds to all employed workers with age between 18 and 64. Panels B and C decompose between men and women. Variables 'Married', 'Have kids' and 'Have kids younger than 6' are average shares. These statistics are computed from the Labor Force Survey. Observations are weighted thanks to the weights provided by INSEE. **Go back to main text.**

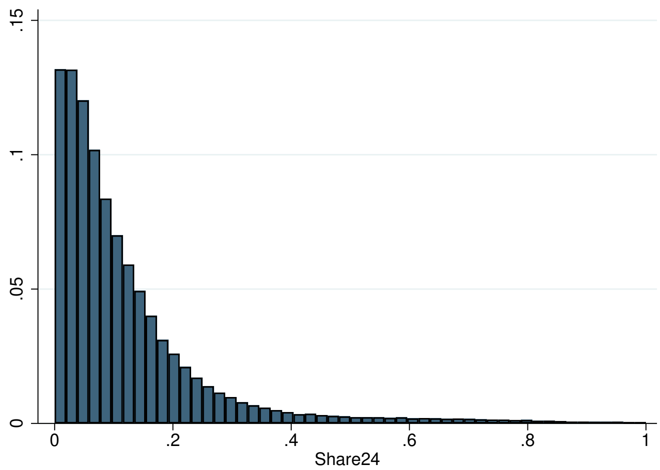


Figure B.3: Distribution of Share24 in estimation sample in 2013

Notes: This figure plots the distribution of the share of jobs below 24h at the firm level over 2009-2013 in the estimation sample. Firms with size smaller than 5 or covered with industry agreements with exceptions to the 24h-rule are excluded. **Go back to main text.**

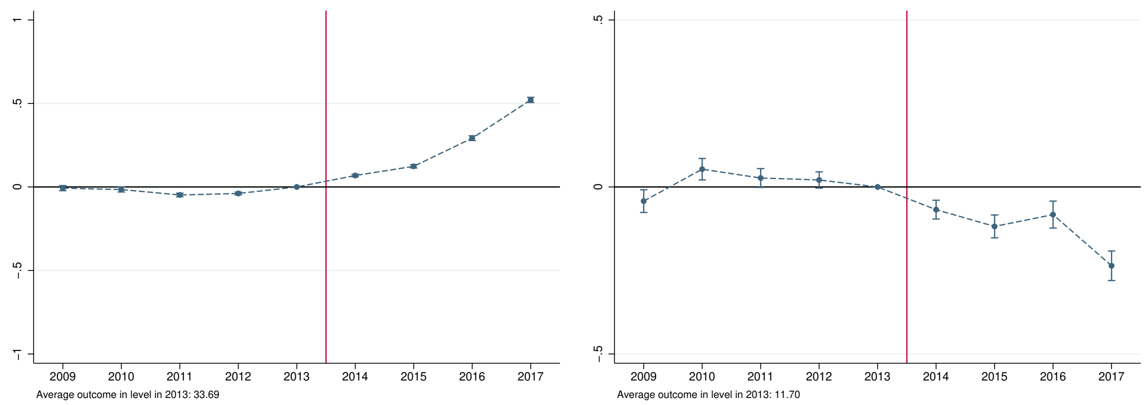
	All	Acc. & food	Construction	Manuf.	Services	Retail
All regressors	0.332	0.251	0.085	0.209	0.438	0.202
Firm size	0.332	0.251	0.083	0.208	0.437	0.202
Firm location	0.328	0.221	0.075	0.200	0.434	0.191
Share of women	0.318	0.247	0.085	0.194	0.433	0.183
Share of OEC	0.331	0.244	0.081	0.206	0.426	0.200
Distri. of occupations	0.112	0.166	0.042	0.109	0.107	0.141
Distri. of workers' age	0.322	0.194	0.077	0.200	0.424	0.187

Table B.3: Explanatory power of determinants of exposure to the reform at the firm level

Notes: The first row reports the R^2 of an OLS regression with Share24 (exposure to the policy) as dependent variable and including all regressors stated in rows 2-7. Rows 2-7 reports R^2 of the regressions in which the set of regressors reported in first column is dropped. "Share of OEC" corresponds to the share of workers employed under open-ended contracts in the firm. **Go back to main text.**

C Additional firm-level results

C.1 Additional outcomes

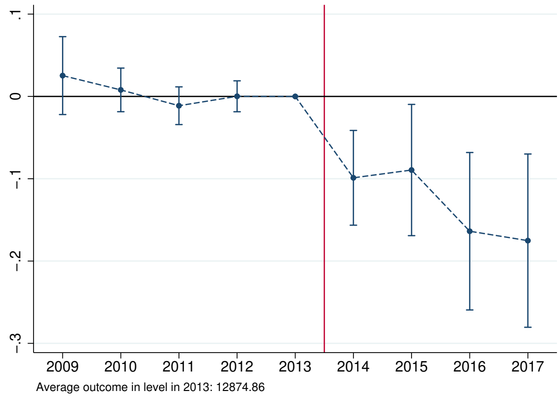


(a) Average weekly hours per job

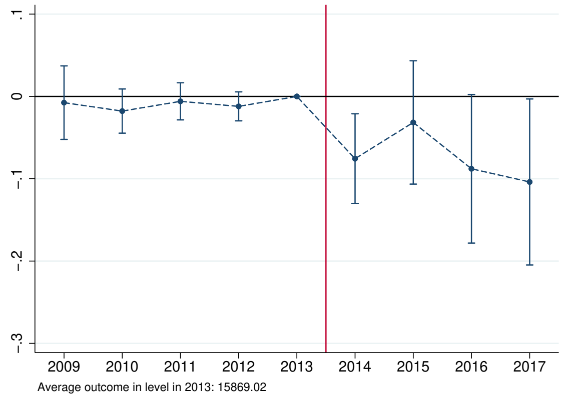
(b) Jobs with hours between 24 and 35

Figure C.1: Firm level effects: additional outcomes

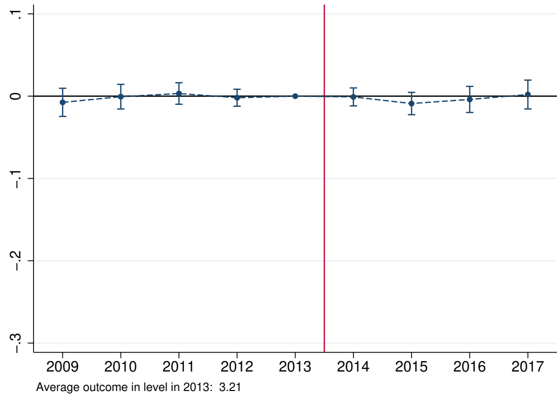
Notes: Notes: This figure plots the estimates of the β_k parameters in Equation (1), for each year, as well as the 95% confidence intervals (standard errors are clustered at the firm level). Estimation on the balanced panel of firms from which firms smaller than size 5 before the policy and firms covered by industry agreements with exceptions to the policy are excluded. Reported estimates are for the full model with firm and year FE, time-varying industry, area, age and size effects. Estimated coefficients (y-axis) can be interpreted as the % change in outcome associated with a 1 percentage point increase in exposure to the reform. **Go back to main text.**



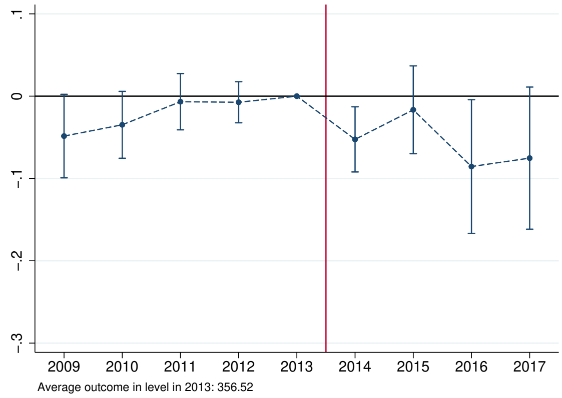
(a) Total sales



(b) Stock of capital



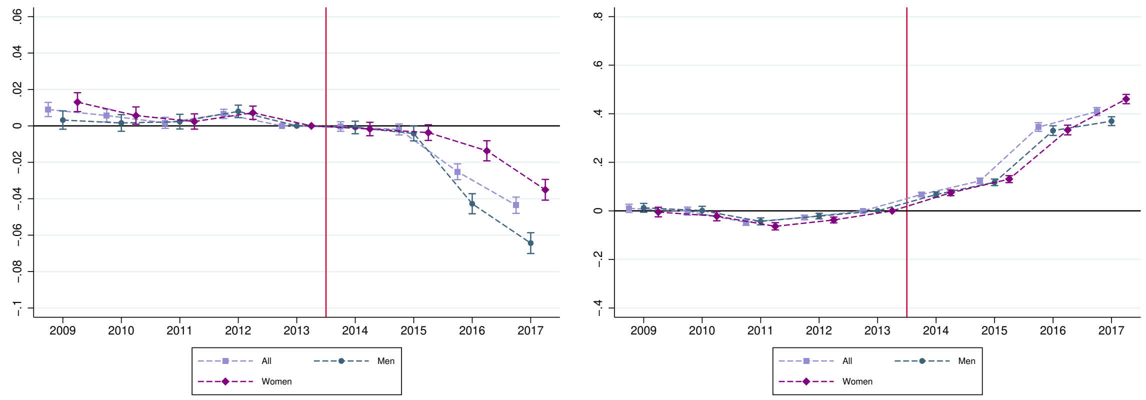
(c) Capital-to-labor ratio



(d) Purchased services

Figure C.2: Firm-level effects: other inputs and output

Notes: This figure plots the estimates of the β_k parameters in Equation (1), for each year, as well as the 95% confidence intervals (standard errors are clustered at the firm level). Outcome variables are computed from the Ficus-Fare. Estimation on the balanced panel of firms from which firms smaller than size 5 before the policy and firms covered by industry agreements with exceptions to the policy are excluded. Reported estimates are for the full model with firm and year FE, time-varying industry, area, age and size effects. Estimated coefficients (y-axis) can be interpreted as the % change in outcome associated with a 1 percentage point increase in exposure to the reform. **Go back to main text.**



(a) Average hourly wage (b) Average earnings

Figure C.3: Firm-level effects on average pay per job

Notes: Notes: This figure plots the estimates of the β_k parameters in Equation (1), for each year, as well as the 95% confidence intervals (standard errors are clustered at the firm level). Hourly wage and earnings by job spell by year are averaged at the level of the firm. Estimation on the balanced panel of firms from which firms smaller than size 5 before the policy and firms covered by industry agreements with exceptions to the policy are excluded. Reported estimates are for the full model with firm and year FE, time-varying industry, area, age and size effects. Estimated coefficients (y-axis) can be interpreted as the % change in outcome associated with a 1 percentage point increase in exposure to the reform. **Go back to main text.**

C.2 Decomposition of different effects by gender

Decomposition method: details for Equation (3). For each gender $g \in \{\text{Women}, \text{Men}\}$, consider the reduced-form event study

$$\log y_{it}^g = \sum_{\substack{k=-4 \\ k\neq 0}}^{4} \theta_k^g (\text{Share24}_i^g \times \mathbb{1}\{t = 2013 + k\}) \; + \; \psi_t^g X_i \; + \; \mu_i^g \; + \; \eta_t^g \; + \; \varepsilon_{it}^g, \tag{16}$$

where Share24_i^g is firm i's pre-reform exposure for gender g. Taking the within-firm gender difference,

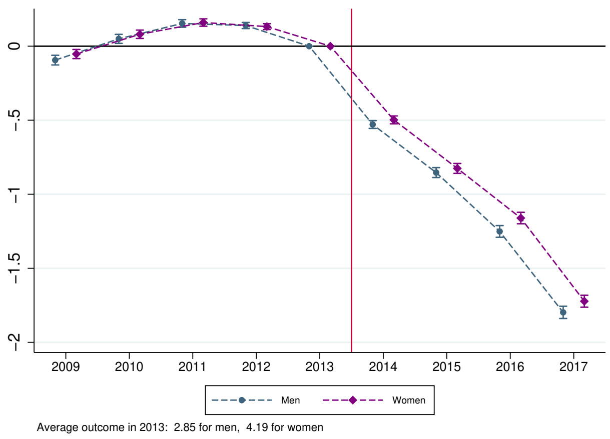
$$\begin{aligned}
\Delta^G y_{it} &\equiv \log y_{it}^{\text{Women}} - \log y_{it}^{\text{Men}} \\
&= \sum_{\substack{k=-4 \\ k\neq 0}}^{4} \Big[\theta_k^{\text{Women}} (\text{Share24}_i^{\text{Women}} \times \mathbb{1}\{t = 2013 + k\}) \\
&\qquad\qquad - \theta_k^{\text{Men}} (\text{Share24}_i^{\text{Men}} \times \mathbb{1}\{t = 2013 + k\})\Big] \\
&\quad + (\psi_t^{\text{Women}} - \psi_t^{\text{Men}}) X_i + (\mu_i^{\text{Women}} - \mu_i^{\text{Men}}) + (\eta_t^{\text{Women}} - \eta_t^{\text{Men}}) \\
&\quad + (\varepsilon_{it}^{\text{Women}} - \varepsilon_{it}^{\text{Men}}).
\end{aligned} \tag{17}$$

This identity motivates estimating the gender–difference outcome $\Delta^G y_{it}$ using *both* gender–specific exposures on the right–hand side. In that Δ–specification, the term built from

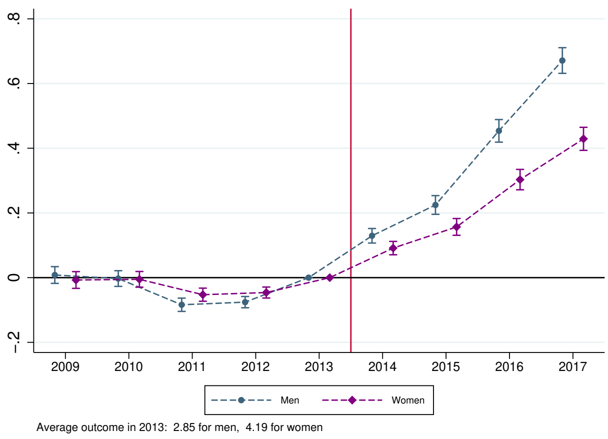
$\text{Share24}_i^{\text{Women}}$ loads on women's own response at event time k, while the term built from $\text{Share24}_i^{\text{Men}}$ loads with the opposite sign of men's own response. Consequently, the heterogeneous response (women minus men) at k is

$$\pi_k^{\text{Women}} + \pi_k^{\text{Men}} \;=\; \theta_k^{\text{Women}} - \theta_k^{\text{Men}}.$$

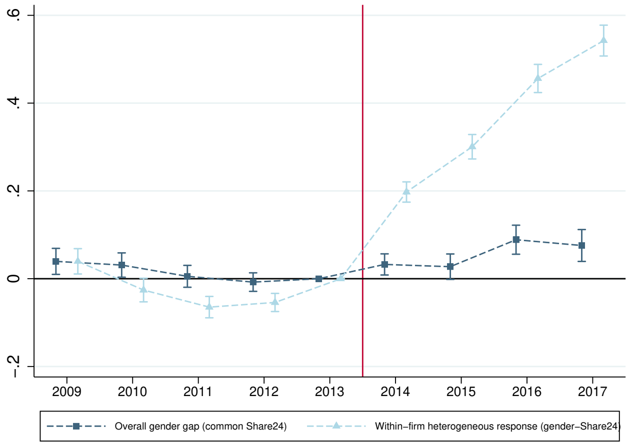
Working with $\Delta^G y_{it}$ also lets me include $\text{Share24}_i^{\text{Women}}$ and $\text{Share24}_i^{\text{Men}}$ *simultaneously*; because these pre–reform exposures are typically correlated within firms, conditioning on both in the same regression accounts for that correlation and isolates heterogeneous responses net of composition.



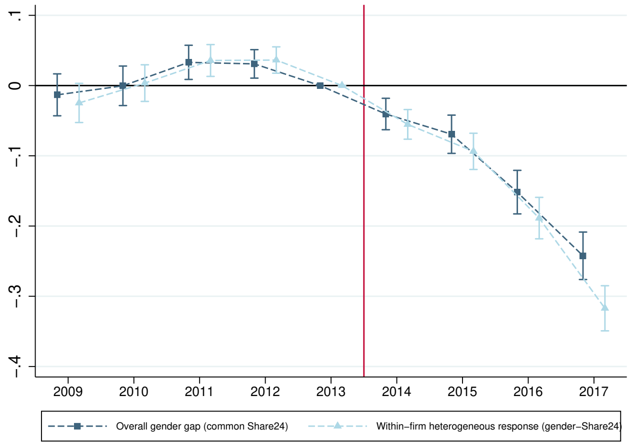
(a) Jobs < 24h



(b) Full-time jobs



(c) Gender gap decomposition: Jobs < 24h



(d) Gender gap decomposition: Full-time jobs

Figure C.4: Firm-level effects by gender by type of jobs

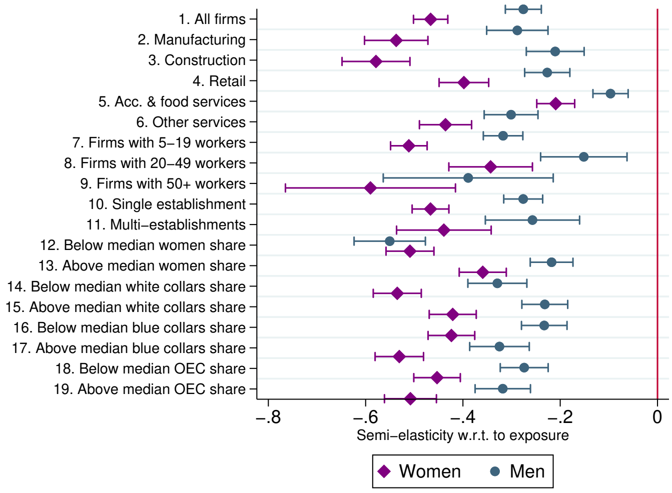
Notes: This figure plots the effects by gender (panels (a) and (b)) and the gender gap and its decomposition (panels (c) and (d)). Panels (a) and (b) report the estimates of the β_k parameters in Equation (1), for each year, as well as the 95% confidence intervals (standard errors are clustered at the firm level). The regression in which the outcome is for men has been estimated separately from the one in which the outcome is for women. In panels (c) and (d), the "Overall gender gap" corresponds to the estimates of δ_k in Equation (2). The "Within-firm heterogeneous response" reports estimates of $\pi_k^{Women} + \pi_k^{Men}$ in Equation (3). Estimation on the balanced panel of firms from which firms smaller than size 5 before the policy and firms covered by industry agreements with exceptions to the policy are excluded. Reported estimates are for the full model with firm and year FE, time-varying industry, area, age and size effects. Estimated coefficients (y-axis) can be interpreted as the % change in outcome associated with a 100 percentage points increase in exposure to the reform. **Go back to main text.**

	Jobs <24h	Full-time jobs	All jobs	Total hours
A. All				
Estimate (2016)	-1.613***	0.443***	-0.376***	-0.166**
	(0.023)	(0.020)	(0.024)	(0.063)
Average in level	-4.716	1.202	-2.674	-1471.719
Total in %	-66.990	3.734	-5.250	-1.726
B. Women				
Estimate (2016)	-1.161***	0.303***	-0.449***	-0.420***
	(0.020)	(0.016)	(0.020)	(0.060)
Average in level	-2.118	0.323	-1.650	-1710.757
Total in %	-50.552	3.092	-8.343	-5.637
C. Men				
Estimate (2016)	-1.251***	0.454***	-0.264***	-0.086
	(0.020)	(0.018)	(0.021)	(0.061)
Average in level	-1.376	0.748	-0.905	-414.863
Total in %	-48.253	3.442	-2.904	-0.755

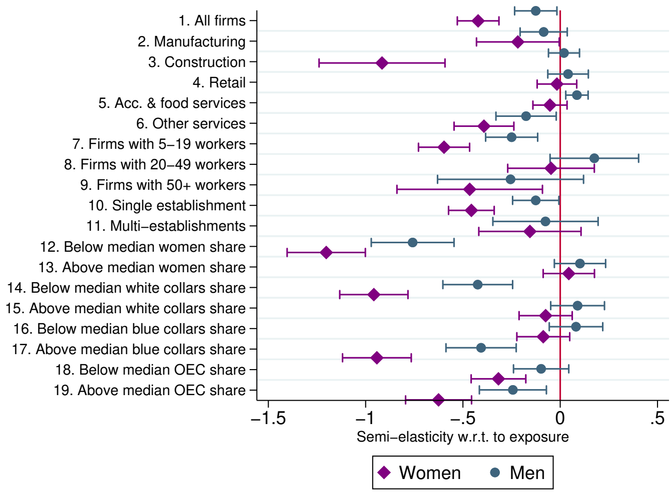
Table C.1: Corresponding employment changes in level

Notes: This table presents the back-of-the-envelope results for the effects of the policy in level on average and in % in the economy. 'Estimate (2016)' corresponds to β_{2016} in Equation (1). Standard errors clustered at the firm level are in parentheses. The average effect in level for outcome Y is computed from $\hat{\beta}_{2016} \frac{1}{N} \sum_{i=1}^{N} Share24_i \times Y_{i,2013}$, where i is a firm. "Average change in level" gives the change in the number of jobs on average for a firm. "Total in %" shows the corresponding variation as percentage of the outcome in the economy. Significance levels: * 0.10, ** 0.05, *** 0.01. **Go back to main text.**

C.3 Heterogeneity analysis



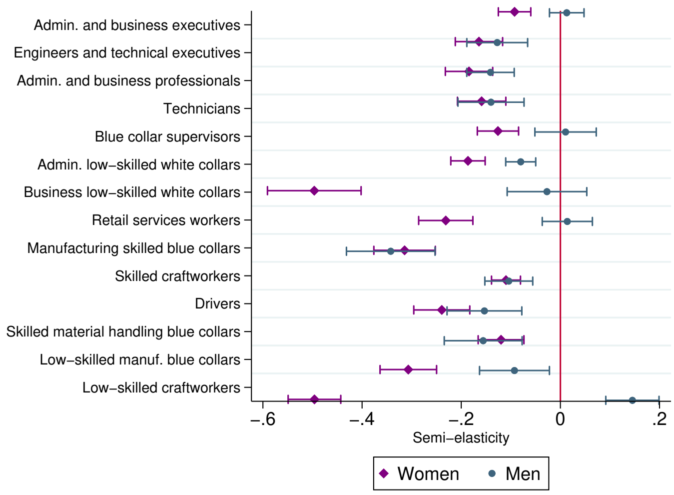
(a) Number of jobs



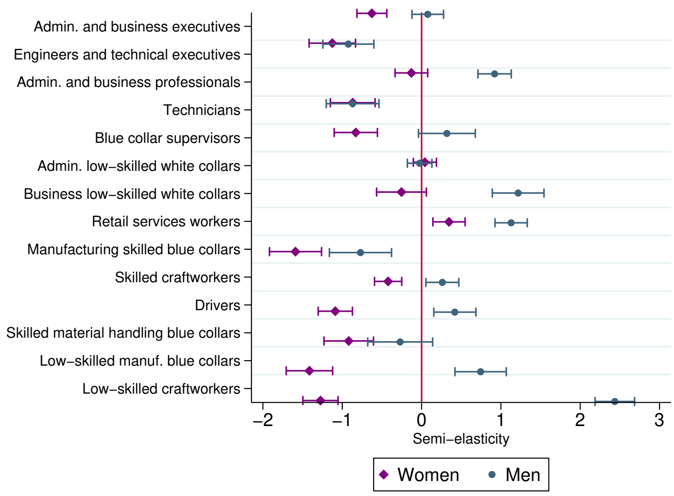
(b) Total hours

Figure C.5: Heterogeneity of the effects by firm type

Notes: This Figure plots estimates of a difference-in-differences specification for the period 2013-2017 for different subgroups of firms, and 95% confidence intervals. 2014 is excluded as it is partially treated, so that estimates show the average effects for 2015-2017. Each regression has been estimated separately in the corresponding subsample of firms. Subsamples are extracted from the baseline balanced panel, which means that each subsample is also a balanced panel with firms larger than 5 workers before the policy. Firms covered by industry agreements with exceptions to the 24h-rule are excluded. Reported estimates are for the full model with firm and year FE, time-varying industry, area, age and size effects. The estimates plot the semi-elasticity with respect to exposure: the % change in outcome associated with a 1 percentage point increase in Share24.



(a) Number of jobs



(b) Total hours

Figure C.6: Effects by occupation

Notes: This Figure plots estimates of a difference-in-differences specification for the period 2013-2017 as well as 95% confidence intervals. The regression has been estimated at the firm level, when the outcome variable is the number of workers of a given gender in a given occupation. Each estimate comes from a different estimation. Estimated on the balanced panel with firms larger than 5 workers before the policy. Firms covered by industry agreements with exceptions to the 24h-rule are excluded. Reported estimates are for the full model with firm and year FE, time-varying industry, area, age and size effects. The estimates plot the semi-elasticity with respect to exposure: the % change in outcome associated with a 1 percentage point increase in Share24.

D Multiple job holding

Does the policy allow workers previously having several part-time jobs to get access to a unique job with more hours? To provide evidence on this, I investigate the impact of the reform on the multiple job holding rate.

I rely on a panel version of the linked employer-employee data, the *Panel DADS*. Contrary to the main data sources used in this paper, the panel version provides an individual identifier allowing to link all jobs of a given worker. Combining this information with the starting and ending dates of each job spell, I compute, for each individual, the amount of time spent with at least two jobs at the same time over the year. This information can then be aggregated.
First, before implementation of the 24h-reform, the share of part-time workers with at least two jobs is equal to 3.5% in 2012-2013. This share is lower after the policy, equal to 3.3% in 2015-2016. I assess here whether this decline is due to the impact of the reform. To do so, I compare the evolution multiple job holding rates between markets with different exposure to the policy. The specification is

$$MJH_{mt} = \alpha_0 + \sum_{\substack{k=-4 \\ k\neq 0}}^{k=4} \beta_k \times \text{Share24}_m \times \mathbb{1}_{t=2013+k} + \mu_m + \eta_t + \epsilon_{mt} \tag{18}$$

where MJH_{mt} is the multiple job holding rate in market m in year t. A market m is a commuting zone-industry cell, where industry is at 2-digits. Share24$_m$ is the average share of jobs with working time below 24h in market m over 2009-2013. μ_m and η_t are market and year fixed effects, respectively.
Figure D.1 presents the estimated parameters. First, the parallel trends assumption seems to hold on the pre-treatment period. Second, we observe a significant drop in the multiple job holding rate after the policy for markets more exposed, relative to markets with a lower exposure. However, the magnitude of the effect is small. An increase in 1 percentage point in exposure to the policy is associated with a decrease in the share of workers with multiple jobs in the market by 0.0006 percentage points in 2016.
This result suggests that the aggregate decrease in multiple-job holding is likely due to the reform. However, I only consider multiple jobs held in the same market, potentially understating the aggregate effect.

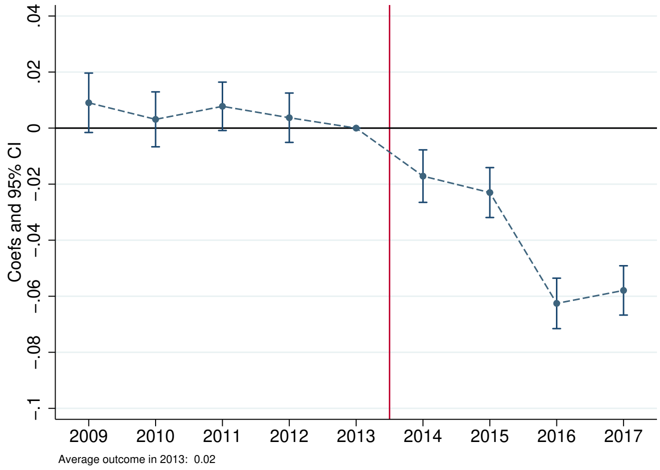
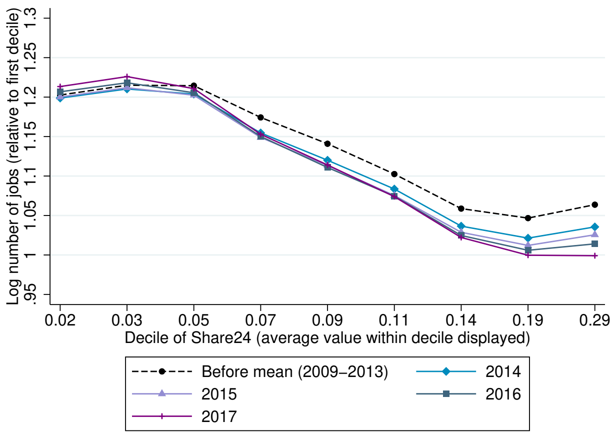


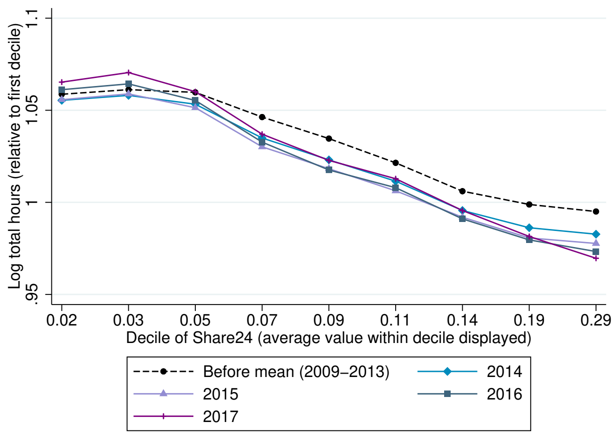
Figure D.1: Effect of the reform on the market-level multiple job holding rate

Notes: This figure plots the estimates of the event study specification estimated at the market level, where the outcome variable is the multiple job holding rate in the market, i.e. the share of workers with more than one job at a time. A market is a commuting zone and industry (at 2-digits). Exposure to the policy (Share24) is computed at the market level as well. 95% confidence intervals shown and standard errors are clustered at the market level.

E Robustness for the firm-level effects



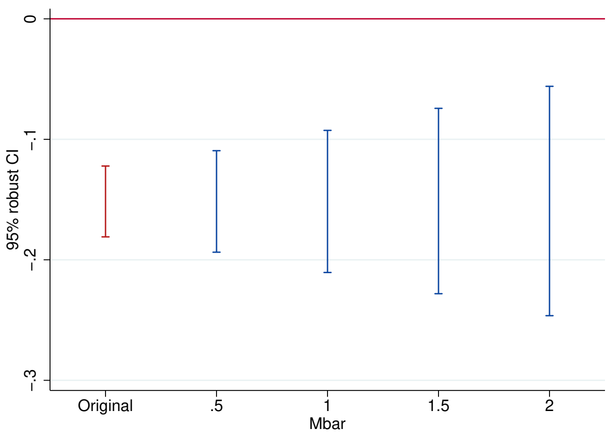
(a) Number of jobs



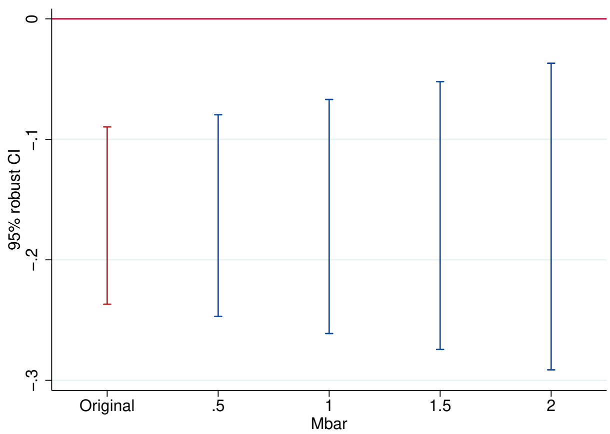
(b) Total hours

Figure E.1: Relationship between Share24 and outcomes in the post-reform period

Notes: This Figure plots, for each decile of exposure to the reform (Share24), the average log outcome, for every year. The average value of Share24 in each decile is reported on the x-axis. For each year, log outcomes are normalized by the value of the variable in the first decile (and the value of the first decile, equal to 1, is omitted). All pre-reform years (2009 to 2013) are pulled together and post-reform years are computed separately. Outcomes are computed in the baseline estimation panel of firms, from which firms smaller than size 5 before the policy and firms covered by industry agreements with exceptions to the policy are excluded. **Go back to main text.**



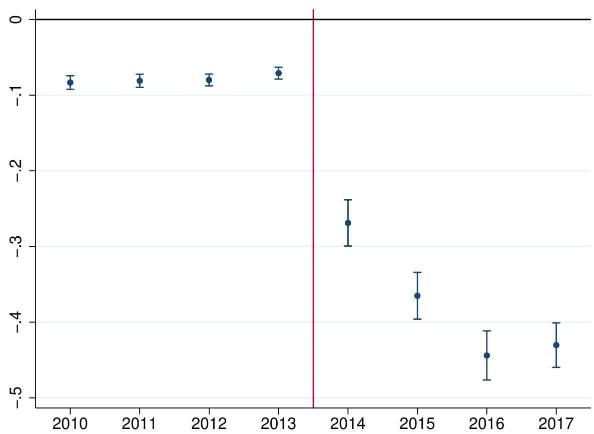
(a) Number of jobs



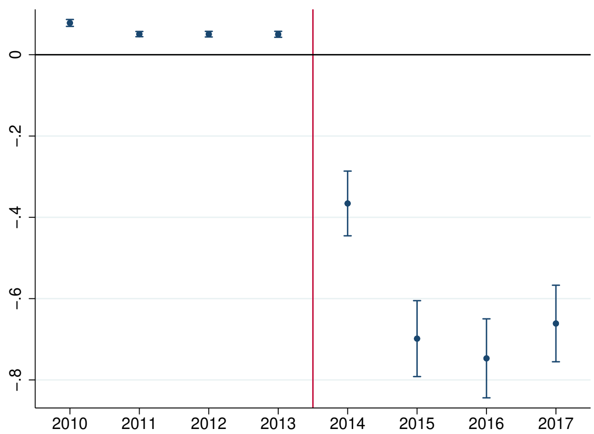
(b) Total hours

Figure E.2: Honest pre-trends following Rambachan & Roth (2023)

Notes: This Figure plots alternative estimated confidence intervals for β_{2014} (the first post-reform year) in Equation (1). These confidence intervals allow for deviations from parallel trend in the pre-reform period, following the procedure in Rambachan & Roth (2023). Each confidence interval is computed assuming that the post-treatment violation of parallel trends is at most Mbar larger than the maximum violation of parallel trends in the pre-treatment period. For instance, Mbar equals to 2 means that the post-treatment violation of parallel trends is no more than twice that in the pre-treatment period. 95% confidence intervals are shown. **Go back to main text.**



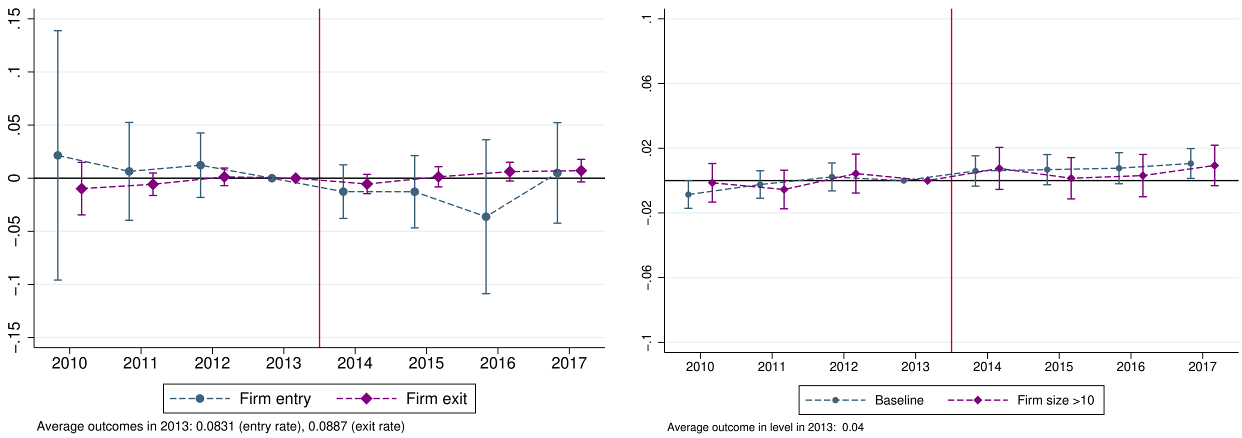
(a) Number of jobs



(b) Total hours

Figure E.3: Robustness check for mean reversion

Notes: These two figures plot estimates of separate difference-in-difference regressions estimated on two consecutive periods, where the second period is considered as after the policy. Each estimated parameter is from an estimation over two periods, in which exposure is computed during the first period. For example, the estimate in 2012 for the number of jobs is obtained by estimating a regression on 2011 and 2012 where 2012 is considered as the "after" period and 2011 as the "before" period. In this case, exposure to the policy is computed in 2011. Estimated on the baseline balanced panel of firms. Specification includes firm and year fixed effects, time-varying industry, area, age and size effects. 95% confidence intervals shown. **Go back to main text.**

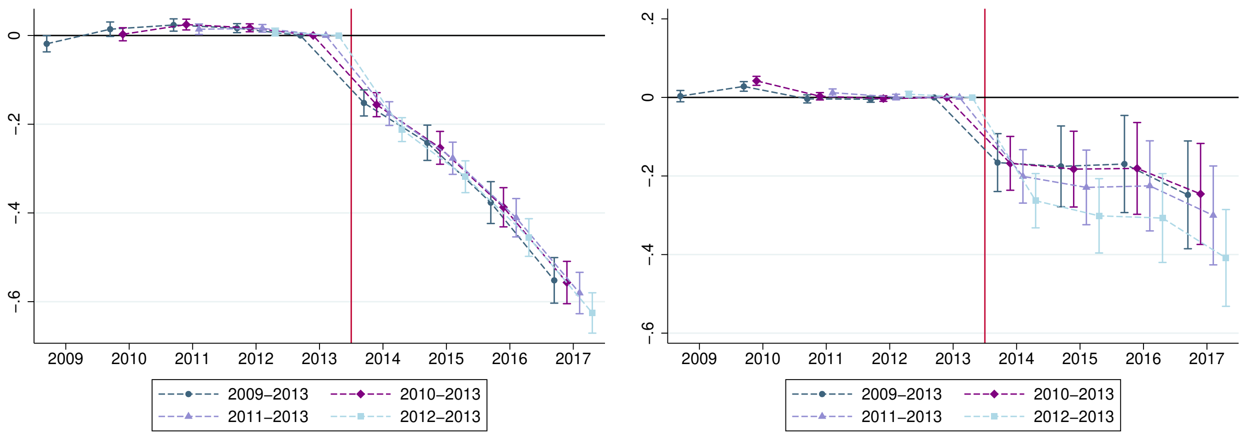


(a) Industry-level: entry and exit

(b) Firm-level: exit

Figure E.4: Effects on firm entry and exit

Notes: Panel (a) plots estimates of β_k in Equation (1), where this equation is estimated at industry level instead of firm level. Share24 is the average share of jobs below 24h in the industry over 2009-2013. Outcomes are the firm entry and exit rates in the corresponding industry. Industries are defined at the 3-digit level. Panel (b) reports firm-level results, with an indicator for firm exit as an outcome. The blue circle reports the results for the baseline restriction (i.e. firms with at least five workers pre-reform), and the purple diamond show the effect on the subset of firms with at least ten. **Go back to main text.**

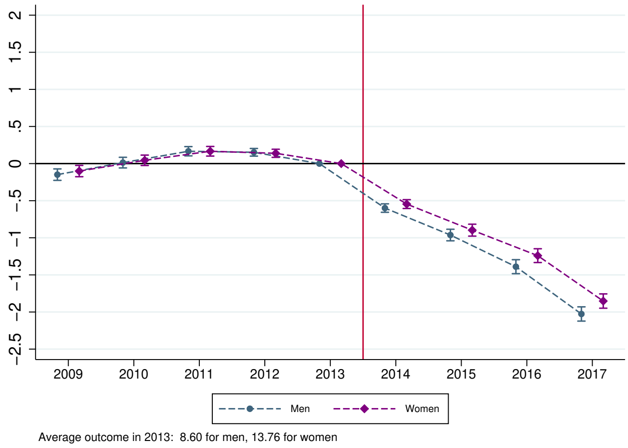


(a) Number of jobs

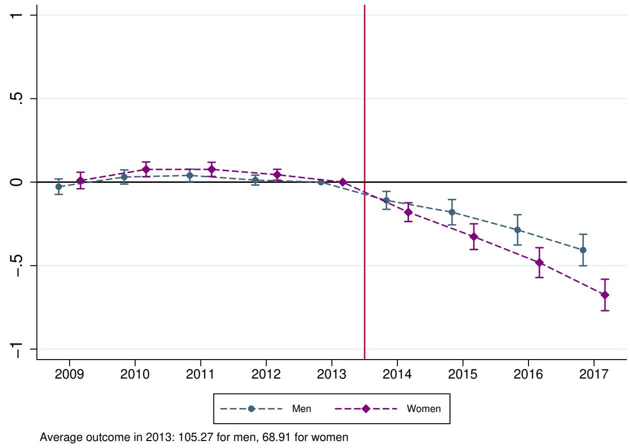
(b) Total hours

Figure E.5: Estimates on alternative samples including younger firms

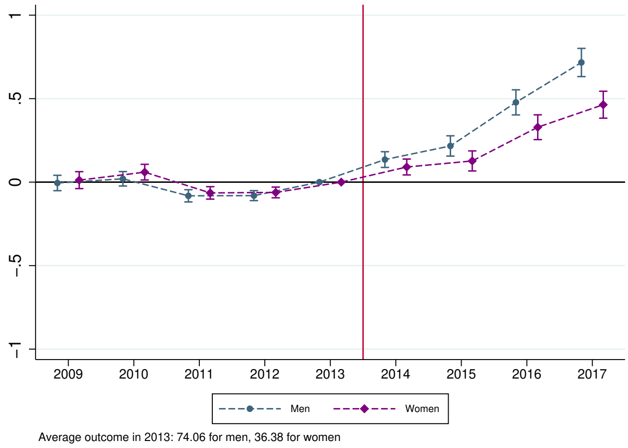
Notes: This Figure plots estimates of β_k in Equation (1) and 95% confidence intervals. Each line in the figure connects estimates obtained on a different sample. Each sample is a balanced panel of firms. The line connecting 2009 to 2017 corresponds to my baseline results for the baseline sample of firms (with firms that were created in 2009 or before). The other lines consider larger samples in which I include younger firms. For instance, the line connecting estimates from 2010 to 2017 also include firms that were created in 2010. Reported estimates are for the full model with firm and year FE, time-varying industry, area, age and size effects. **Go back to main text.**



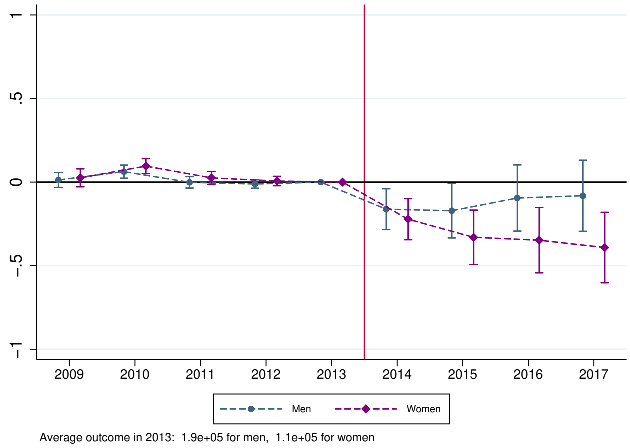
(a) Jobs<24h



(b) Number of jobs



(c) Full-time jobs



(d) Total hours

Figure E.6: Results weighted by pre-reform firm size

Notes: This figure plots the estimates of the β_k parameters in Equation (1), by gender and for each year, as well as the 95% confidence intervals (standard errors are clustered at the firm level). Estimation on the balanced panel of firms from which firms smaller than size 5 before the policy and firms covered by industry agreements with exceptions to the policy are excluded. Results are weighted by pre-reform firm size. **Go back to main text.**

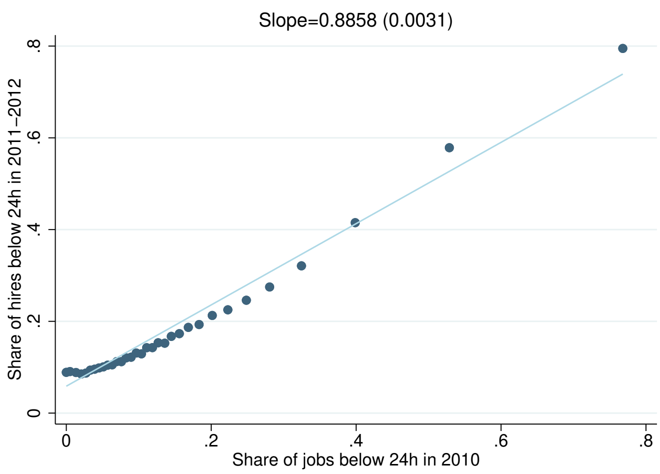
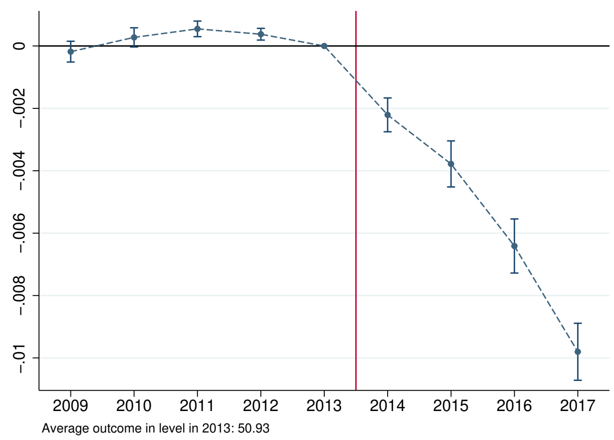
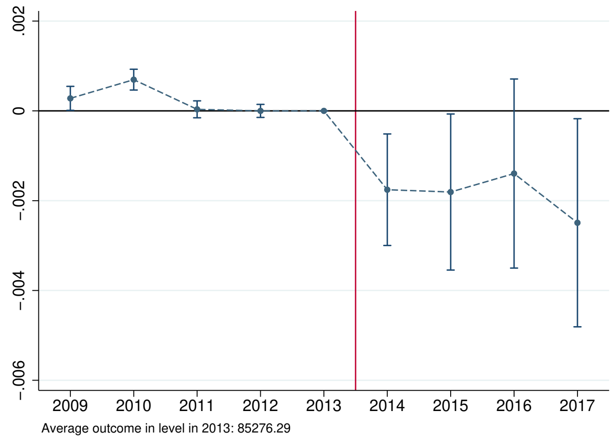


Figure E.7: Persistence of Share24 over time

Notes: This Figure is a binscatter of the relationship between the share of jobs below 24h in the firm in 2010 and hires of these jobs in 2011-2012. **Go back to main text.**



(a) Number of jobs



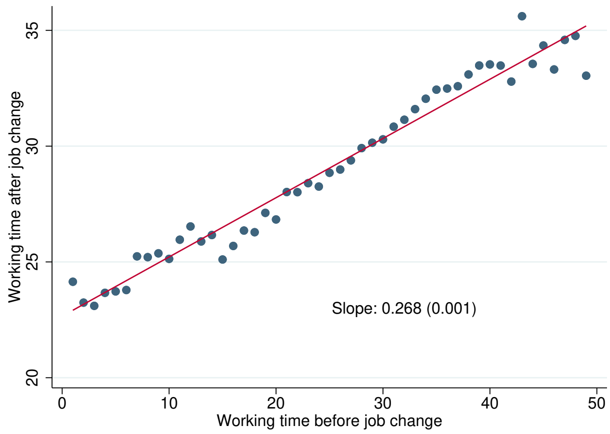
(b) Total hours

Figure E.8: Estimates with GAP-design

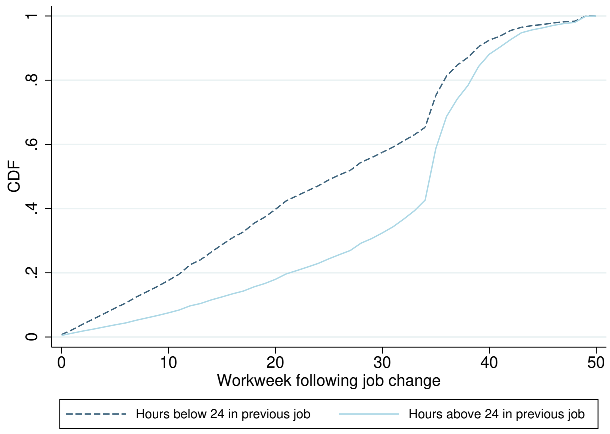
Notes: This Figures plots the estimates of β_k in Equation (1), where instead of Share24, the exposure of the firm to the policy is the GAP-exposure. The GAP-exposure corresponds to the average increase in hours that would be needed in the firm to have all jobs with at least 24h per week, over 2009-2013. Estimation on the balanced panel of firms from which firms smaller than size 5 before the policy and firms covered by industry agreements with exceptions to the policy are excluded. Reported estimates are for the full model with firm and year FE, time-varying industry, area, age and size effects. 95% confidence intervals are shown. **Go back to main text.**

F Worker-level evidence: additional results

F.1 Persistent worker types



(a) Workweek before and after a job change

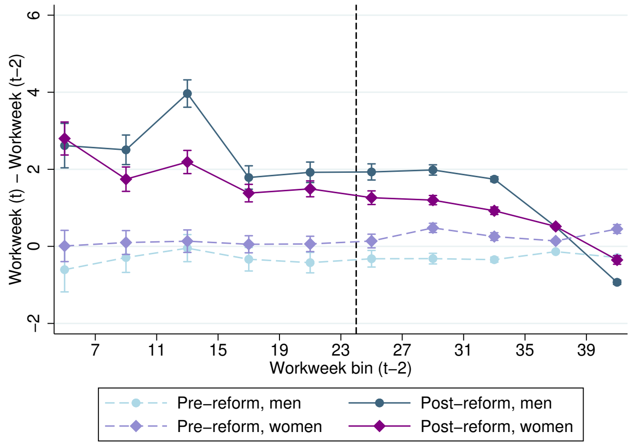


(b) Distribution of hours after a job change

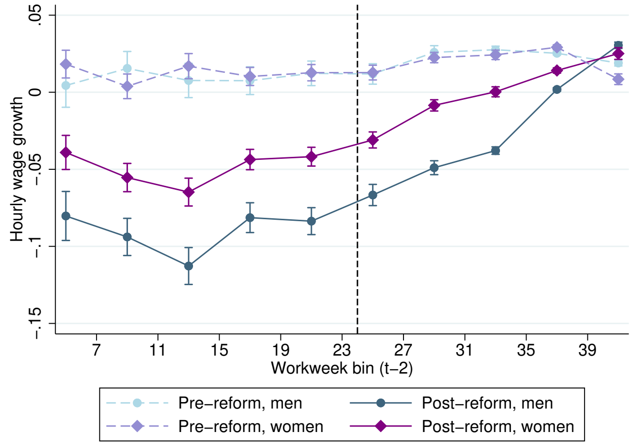
Figure F.1: Correlation in worker's workweek before and after a job change

Notes: This Figure depicts the relationship between a worker's working time before and after a change in employer, over 2011-2013. Panel (a) shows the correlation between working time in two consecutive employers. Panel (b) reports the distribution of working hours, separately for workers working more than 24 hours in the previous job and those working fewer than 24 hours previously. **Go back to main text.**

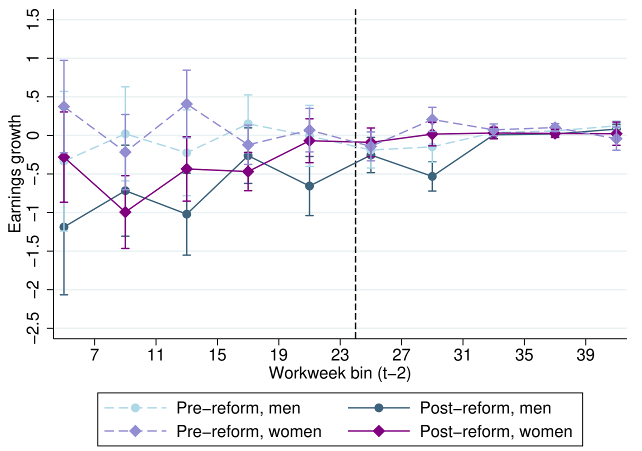
F.2 Additional worker-level outcomes



(a) Hours per week



(b) Hourly wage



(c) Earnings

Figure F.2: Worker-level changes by previous workweek

Notes: Notes: This Figure plots estimates of δ_{d2016} and δ_{d2014} in Equation (4), separately for men and women. The two solid lines connect estimates for 2016 versus 2014 relative to 2011 versus 2009. The two dashed lines present a pre-reform benchmark and connect estimates for 2013 versus 2011 relative to 2011 versus 2009. Outcomes are average hours per week (panel a)), hourly wages (panel b)) and annual earnings (panel c)). **Go back to main text.**

F.3 Change in probability to retain low-hour jobs in the same firm

The French minimum working time only targeted new jobs created from July 2014. Since jobs created before that date do not have to comply with the reform, one may wonder if firms tried to keep these workers longer to avoid the policy. In this Section, I investigate the effect of the minimum working time on existing jobs. I combine the event-study design with a regression discontinuity approach. I now rely on individual-level data. It allows me to compare workers working fewer than 24h before the policy with the ones working more. I exploit the panel version of the DADS (Panel DADS), which provides a worker identifier allowing me to follow workers over time. This panel is a random sample of 1/12th of the standard DADS, composed of workers born in October of each year. I estimate the following specification

$$\text{JobExists}_{jt} = \sum_{\substack{k=-2 \\ k\neq 0}}^{k=4} \alpha_k \times \mathbb{1}_{h<24,j} \times \mathbb{1}_{t=2013+k} + \mu_j + \eta_t + \epsilon_{jt}, \tag{19}$$

where JobExists_{jt} is a variable equal to 1 if the job j exists in year t, meaning that a given worker works in a given firm. On the right hand side is an interaction between a year dummy and a variable equal to 1 if the worker is working fewer than 24h in the firm in 2010. I estimate the regression on the panel of jobs that exist in 2010. μ_j and η_t are job and year fixed effects, respectively. I estimate this equation over the balanced panel of jobs with workweeks between 19 and 29 hours in 2010. These jobs are in firms from the main balanced panel of firms. Figure F.3 presents estimates of the α_k parameters for each year between 2011 and 2017. I find that a worker with a workweek below 24h before the reform is 1 percentage point more likely to continue working in the same firm in 2016 relative to a job with a workweek above 24h in 2010. The baseline outcome in 2013 is 47%. Even if the magnitude of the effect is small, this is significant evidence of a small hoarding effect of jobs with less than 24h created before the policy. Two different reasons may explain why the magnitude of the effects is small. First, jobs with low hours are more likely to be fixed-term contracts, implying that these jobs are going to end anyway. Second, the reform changes outside options on the labor market and some workers may be more likely to quit their jobs.

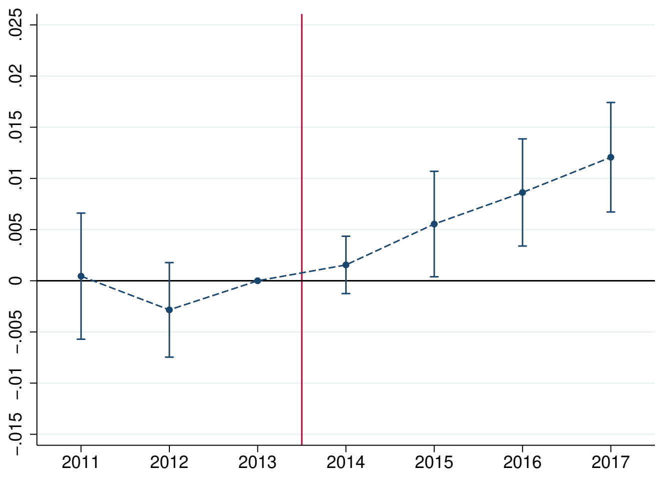
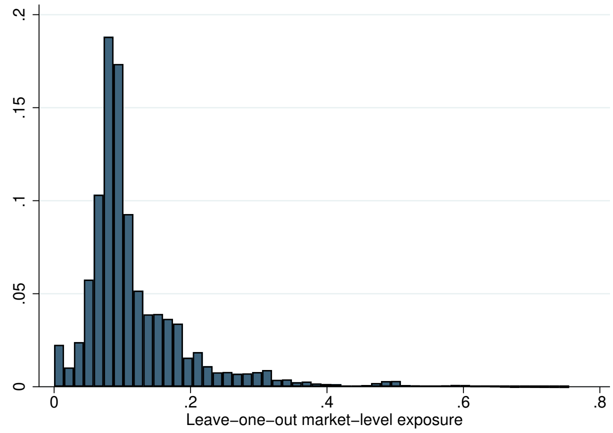


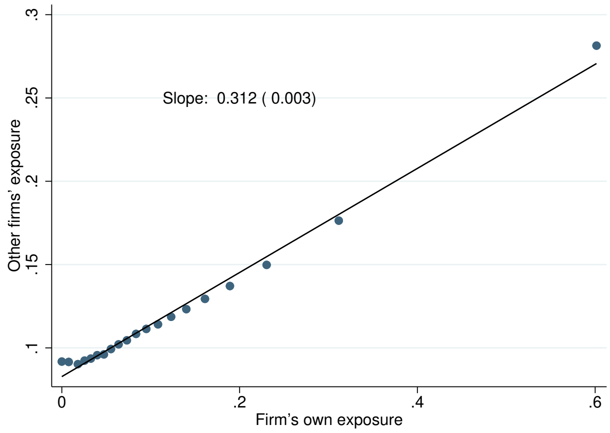
Figure F.3: Job-level analysis: survival probability for jobs created before the reform

Notes: This figure plots the results of the difference-in-discontinuity design estimating the hoarding effect for jobs below 24h created before implementation of the policy from Equation (19). The coefficients are estimated on the sample of jobs having between 19 and 29 hours of work and existing in 2010. Jobs considered are in firms from the baseline balanced panel. The outcome is a dummy variable equal to 1 if the jobs still exists in the given year. Average outcome is equal to 0.47 in 2013.

G Reallocation between firms: additional evidence



(a) Distribution of Share24$_{m(-i)}$



(b) Correlation between firm-level and market-level exposure

Figure G.1: Leave-one-out market-level exposure to the reform, Share24$_{m(-i)}$

Notes: Panel (a) displays the distribution of the market-level leave-one-out exposure to the reform, Share24$_{m(-i)}$. Panel (b) is a binscatter graph depicting the correlation between the firm-level exposure (Share24) and the market-level leave-one-out one (Share24$_{m(i)}$). The slope of a regression of the market-level exposure on the firm-level exposure is printed, with the robust standard error in parentheses. **Go back to main text.**

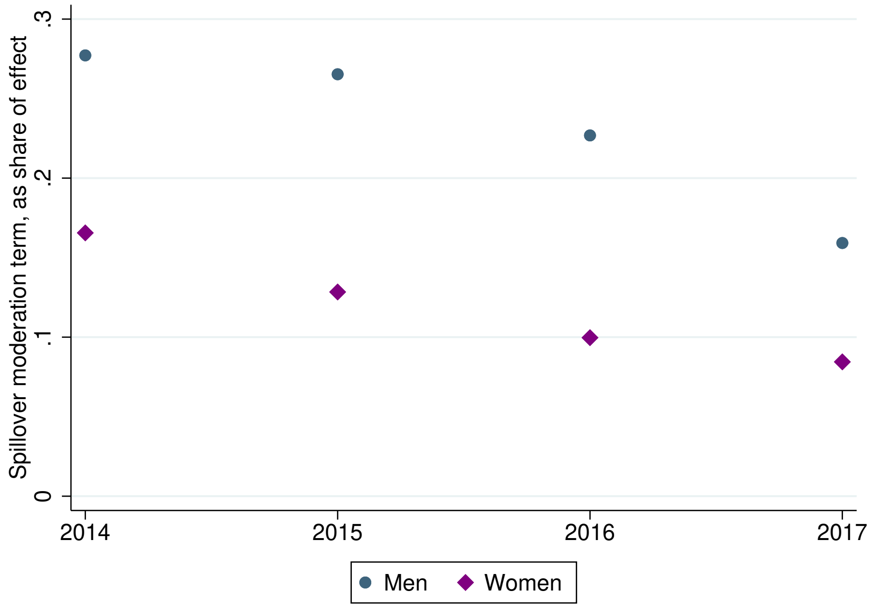


Figure G.2: Spillover moderation estimate as a share of total effect $\left(\frac{\widehat{\gamma_k}\times\bar{s}}{\widehat{\lambda_k}+\widehat{\gamma_k}\times\bar{s}}\right)$

Notes: This Figure reports the year-by-year ratio between the spillover moderation term, $\widehat{\gamma_k} \times \bar{s}$ and the semi-elasticity to Share24 for firms in a market with average exposure $\bar{s}$, $\widehat{\lambda_k} + \widehat{\gamma_k} \times \bar{s}$. Estimates are obtained from the estimation of Equation (5). For example, in 2016, the market-level spillovers attenuate the firm-level effect by 10% for female employment. **Go back to main text.**

	All	Men	Women
A: CZ $\times$ industry (2 digits)			
Spillover moderation term ($\bar{s} \times \widehat{\gamma}$)	0.065	0.067	0.060
	(0.017)	(0.015)	(0.014)
Effect ($\widehat{\lambda} + \bar{s}\widehat{\gamma}$)	-0.411	-0.283	-0.493
	(0.026)	(0.023)	(0.021)
Spillover moderation as % of effect	15.80	23.60	12.14
B: CZ $\times$ industry (1 digit)			
Spillover moderation term ($\bar{s} \times \widehat{\gamma}$)	0.119	0.064	0.144
	(0.028)	(0.024)	(0.023)
Effect ($\widehat{\lambda} + \bar{s}\widehat{\gamma}$)	-0.360	-0.194	-0.473
	(0.022)	(0.019)	(0.019)
Spillover moderation as % of effect	32.94	33.11	30.34
C: Province $\times$ industry (1 digit)			
Spillover moderation term ($\bar{s} \times \widehat{\gamma}$)	0.127	0.082	0.162
	(0.031)	(0.027)	(0.025)
Effect ($\widehat{\lambda} + \bar{s}\widehat{\gamma}$)	-0.367	-0.202	-0.484
	(0.023)	(0.02)	(0.019)
Spillover moderation as % of effect	34.56	40.48	33.53

Table G.1: Spillover-moderation estimates from the triple-differences

Notes: This Table reports the results from estimations of Equation (5) for three different definitions of a market. Each panel, A, B and C considers a different market definition. Within each panel, the table reports the estimate of γ_{after} multiplied by the average market-level exposure, $\bar{s}$. It corresponds to the average moderation term, i.e. how much the effect of the firm-level Share24 changes with the effect of the market-spillovers. Second, I report the overall firm-level semi-elasticity to Share24, $\widehat{\lambda} + \bar{s}\widehat{\gamma}$. Finally, I present the ratio of the spillover moderation and the overall effect, in percentage. For instance, in panel A, a 1ppt increase in firm-level exposure decreases firm-level employment by 0.41% on average. This effect would be a reduction by 0.476 in a market with a zero exposure to the policy. Therefore, the spillovers moderate the effects by $0.065/0.411 = 15.8\%$. **Go back to main text.**

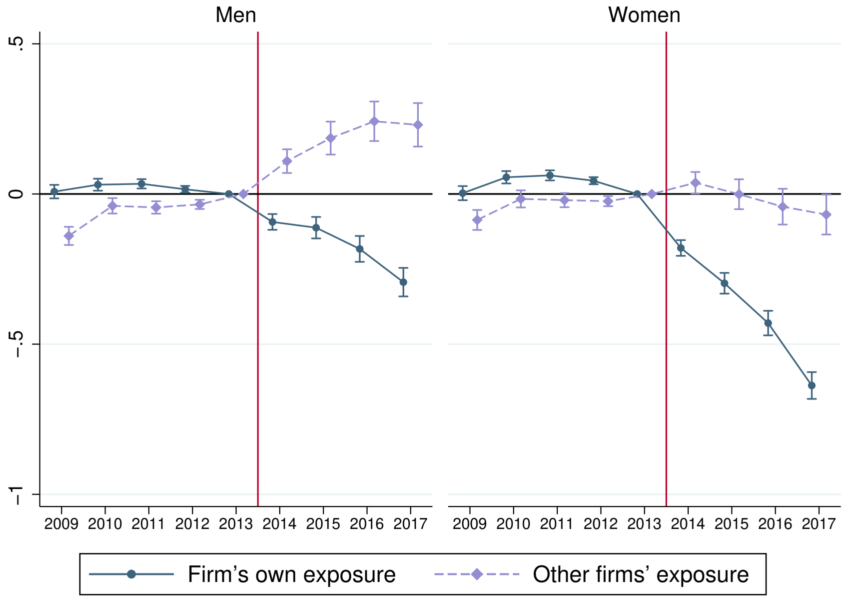


Figure G.3: Effect of firm exposure and market exposure, imposing no interaction between the two

Notes: This figure plots estimates of λ_k and δ_{mt} in Equation (5) and 95% confidence intervals, imposing that $\gamma = 0$. The solid blue line connects estimates for the λ_k and the dashed purple line connects estimates for the δ_{mt}. Both are estimated in a unique regression (with separate regressions for men and women). Estimation using the balanced panel of firms with at least 5 workers before implementation of the policy. Firms covered by industry agreements with exceptions to the 24h-rule are excluded. Estimated coefficients (y-axis) can be interpreted as the % change in outcome associated with a 1 percentage point increase in firm-level or market-level exposure to the reform. **Go back to main text.**

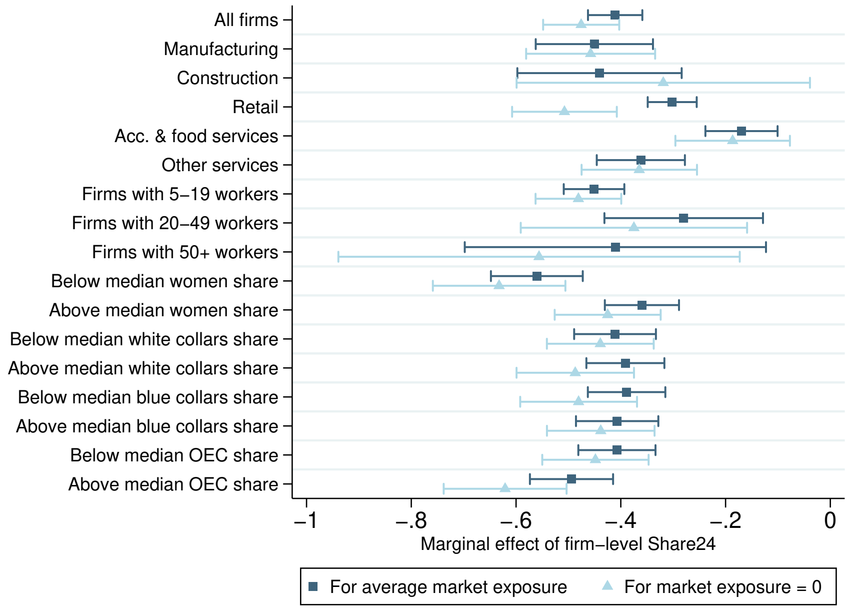
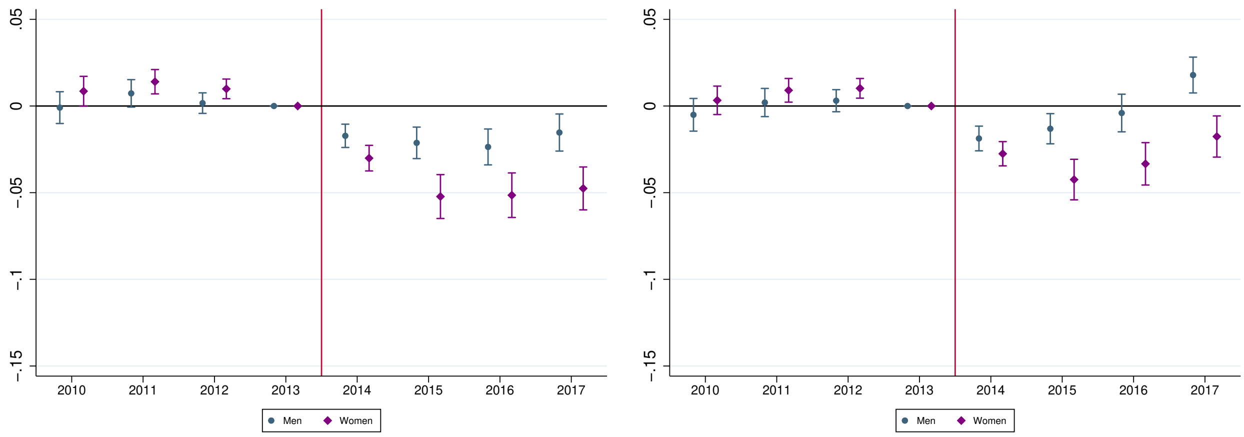


Figure G.4: Heterogeneity analysis of the triple difference: firm-level employment effects of the reform in market more versus less exposed

Notes: This figure plots estimates of the triple-differences (Equation (5)), $\widehat{\lambda_k} + \widehat{\gamma_k} \times s$, for two values of s: 0, and the average. I estimate several version for different groups of firms. Estimation using the balanced panel of firms with at least 5 workers before implementation of the policy. Firms covered by industry agreements with exceptions to the 24h-rule are excluded. Estimated coefficients (y-axis) can be interpreted as the % change in outcome associated with a 1 percentage point increase in firm-level or market-level exposure to the reform. **Go back to main text.**



(a) Number of jobs in market

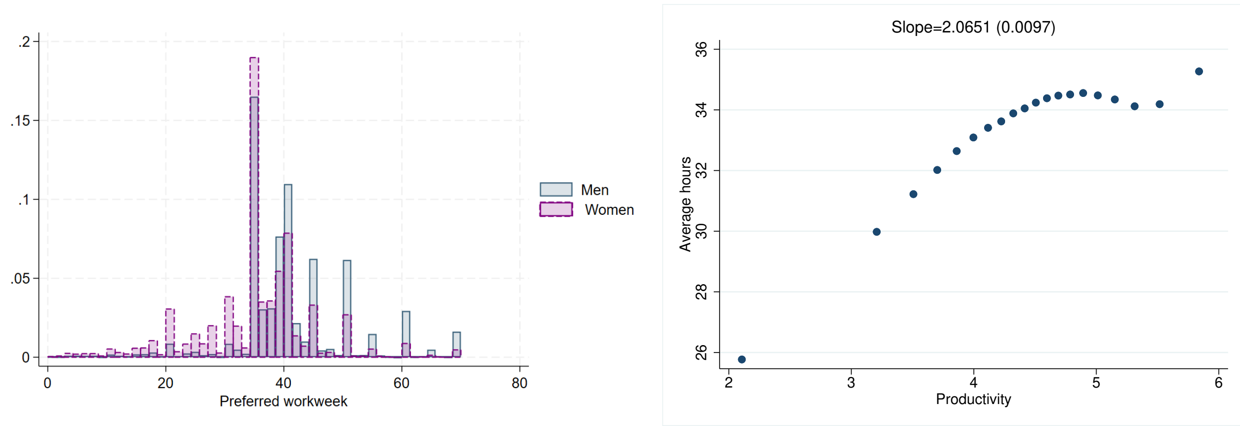
(b) Total hours in market

Figure G.5: Market-level results with built-in placebos testing for mean reversion

Notes: This Figure plots estimates of β_k in Equation (1) estimated at the market level. A market is a commuting zone x 2-digit industry. While Share24 is computed over 2009-2013 for the baseline analysis, I implement a different strategy here to obtain built-in placebos as well as the reform effect. I now compute the market-level exposure Share24$_m$ in 2009 only, and exclude the year 2009 from the regression. This way, estimates 2010-2013 now test for mechanical effects such as mean reversion. **Go back to main text.**

H Structural model: technical appendix

H.1 Model choices



(a) Workers' preferred working time by gender

(b) Firm productivity and hours

Figure H.1: Motivation for ex-ante worker and firm heterogeneity in the model

Notes: Panel (a) plots the distribution of preferred working time, separately for men and women, in 2013. It is computed from the French Labor Force Survey, in which people are asked how many hours they want to work per week. Workers younger than 18 and older than 65 are excluded. Panel (b) plots a firm-level binscatter graph. The x-axis corresponds to the logarithm of the total value added divided by the total number of hours in 2012. The y-axis plots the average weekly hours worked in the firm. Firms smaller than 5 workers are excluded. The slope corresponds to the OLS estimate when the average hours is regressed on productivity. The standard error is in parentheses. **Go back to main text.**

Estimation of worker-firm sorting in hours.

To assess whether workers sort into jobs based on working hours (e.g., by screening employers on hours prior to applying) I estimate an AKM-style decomposition of hours following Lachowska et al. (2023):

$$\log h_{ijt} = \alpha_i + \phi_{j(i,t)} + \eta_t + r_{it}. \tag{20}$$

Here, α_i denotes worker fixed effects, $\phi_{j(i,t)}$ firm fixed effects, and η_t year effects. The model is estimated on pre-reform data (2009–2013), with identification coming from workers who move across employers. To address concerns about limited mobility and the resulting attenuation bias, I implement a split-sample procedure that ensures the estimation noise in worker effects is orthogonal to the noise in firm effects. Specifically, I randomly divide workers into two groups and estimate the model separately in each subsample.

Table H.1 reports the resulting variance decomposition, alongside the estimates of Lachowska et al. (2023) for the U.S. While the overall dispersion of log hours is similar in the two countries (standard deviation of 0.35 in the U.S. and 0.32 in France), the sources of that dispersion differ markedly. Employer effects account for a comparable share of the variance (27% in the U.S. and 20% in France). In contrast, the contribution of worker heterogeneity is substantially larger in France: worker fixed effects explain 44% of the variance in hours, compared with only 7% in the U.S. This indicates that persistent worker characteristics play a central role in shaping working-time outcomes in France, supporting the introduction of ex-ante heterogeneity in hours preferences in the structural model.

Finally, in both countries there is little evidence of worker–firm sorting on hours. The covariance between estimated worker and firm effects explains only 1.3% of the variance in the U.S. and 0.7% in France. Figure H.2 illustrates this result, showing a near-flat relationship between average worker and firm effects.

	(1) Lachowska et al. (2023) U.S.A.		(2) This paper France	
Standard deviation of log hours	0.35		0.32	
Variance components				
Std. of employer effects	0.18	26.81%	0.15	20.5%
Std. of worker effects	0.09	7.19%	0.22	43.9%
Covariance of worker, employer effects	0.00	1.27%	0.00	0.73%
Share of variance explained		35.26%		65.13%

Table H.1: Variance decomposition of log hours per week

Notes: The table reports AKM variance decompositions of log hours into worker and employer components in two settings. Column (1) reproduces the decomposition for the universe of employers and employees in Washington State, as estimated by Lachowska et al. (2023), who implement the KSS leave-one-out variance correction. Column (2) presents my corresponding decomposition for France over 2009–2013, using a split-sample variance correction. For each column, the left panel reports the standard deviation (or covariance) of each component, while the right panel reports the share of the total variance in log hours accounted for by that component (the covariance between worker and employer effects is multiplied by two). All statistics are weighted by the number of worker-year observations associated with each employer. Year effects are omitted.
Go back to main text.

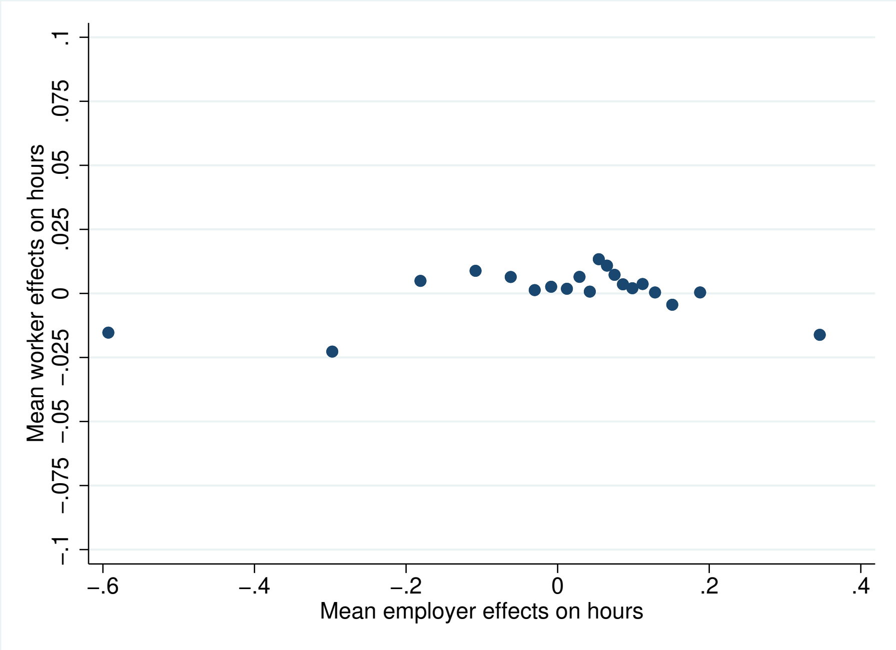


Figure H.2: Lack of worker-employer sorting on hours

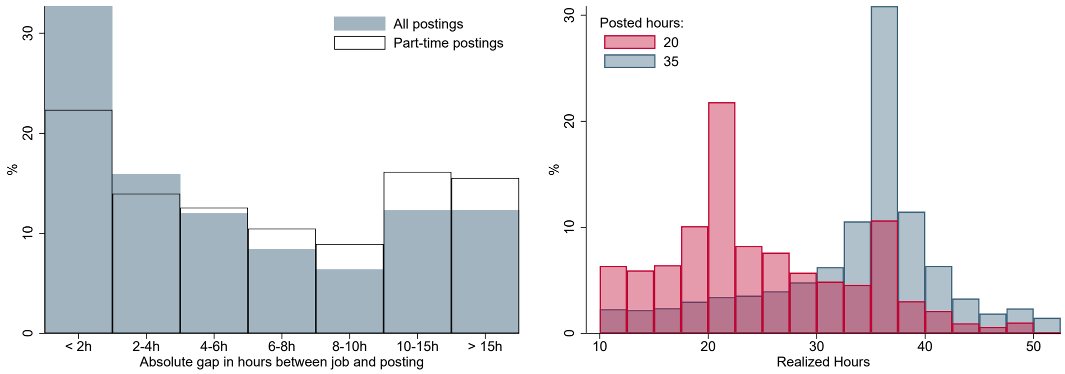
The figure shows the relationship between mean worker and mean employer effects on hours, using a split-sample approach to address measurement error. I randomly divide workers into two subsamples and estimate the AKM model separately in each. Worker effects are obtained from the first subsample and employer effects from the second, ensuring that estimation noise in the two components is orthogonal. **Go back to main text.**

Matching between job ads and employment records.

To assess whether hours worked stem from posting or if there is a role for the match to affect the workweek, I link administrative employment records of job spells from the DADS to a near universe of job postings. Postings are aggregated by Pôle Emploi, France's public employment service, covering 2013-2017. Because firms may hire many employees with similar postings or workers may find jobs without postings, the matching process is fuzzy. I apply the deterministic procedure outlined by Lochner (2019) to match records.[21] I begin by restricting matches to posting-spell pairs at the same firm with matching coarse occupation codes. I then iterate through matching variables (establishment, contract type, and full occupation code), eliminating strictly dominated postings. I resolve remaining ties by minimizing time between job posting and job. After restricting to the sample of workers with complete hours and wages data, this process generates a working sample of approximately 5.5 million matches that resembles the universe of job spells on observables not used in matching, including wages, hours, and demographic characteristics. The results are consistent across matching specifications including initial date spread window, variable ordering, and tie-breaking rules. I do not use wage or hours information for the matching, as I aim at analyzing the gap in those

[21] Lochner, B. (2019), A simple algorithm to link "last hires" from the Job Vacancy Survey to administrative records, FDZ MethodenReport.

variables between the posting and the job created.



(a) Gap between posted and realized hours (b) Realized hours distribution for postings at 20 and 35 hours

Figure H.3: Deviations in realized hours from posted hours

Notes: Panel (a) plots the distribution of absolute differences between posted hours and realized hours for matched posting-job spell pairs. Panel (b) plots the distribution of realized hours for postings with around 20 hours per week (19-21 inclusive, $\approx$ 300 thousand observations) and 35 hours per week (34-36 inclusive, $\approx$ 3 million observations). **Go back to main text.**

H.2 Derivations

Value functions.

The value of unemployment for a type-i worker is

$$W_u(i) = b+\beta\theta m(\theta)\int\left[\int[\max(W(i,y,z),W_u(i))\mathrm{d}H(z)]\right]\frac{v(y)}{V}\mathrm{d}\tilde{F}(y)+\beta(1-\theta m(\theta))W_u(i).$$

b is the instantaneous utility of an unemployed worker. The second term is the expected value associated with meeting a firm. The last term is the value next period if no firm has been met.

Let us denote by s^i the share of type-i workers among unemployment. This is an endogenous variable that will be determined in equilibrium. The value of opening a marginal vacant job in a firm with productivity y is denoted $\mathcal{V}(y)$ and is

$$\mathcal{V}(y) = -C'(v(y))+\beta m(\theta)\sum_{i=1,\ldots,\mathcal{N}} s(i)\left[\int\max(J(i,y,z),\mathcal{V}(y))\mathrm{d}H(z)\right]+\beta(1-m(\theta))\mathcal{V}(y).$$

The first term is the cost associated with the marginal vacancy. The second term is the expected value associated with meeting a worker. The last term is the value if no worker has been met.

Surplus of a job.

The surplus of a job with a worker of type i, in a firm of type y and match productivity z is

$$S(i,y,z) = W(i,y,z) + J(i,y,z) - W_u(i).$$

Using

$$W(i,y,z) = wh - x_i \frac{h^{1+\phi}}{1+\phi} + \beta(1-\sigma)W(i,y,z) + \beta\sigma W_u(i),$$

and

$$J(i,y,z) = zyh^{\alpha} - wh + \beta(1-\sigma)J(i,y,z).$$

I find that the surplus of a job with h hours of work is

$$S(i,y,z) = \frac{1}{1-\beta(1-\sigma)}[zyh^{\alpha} - x_i \frac{h^{1+\phi}}{1+\phi} - (1-\beta)W_u(i)].$$

Hours and wage under right-to-manage.

The bargaining problem for the wage is

$$\max_w (W(i,y,z) - W_u(i))^{\gamma}(J(i,y,z))^{1-\gamma}$$

It is equivalent to

$$\max_w \gamma \log(W(i,y,z) - W_u(i)) + (1-\gamma)\log(J(i,y,z))$$

The first order condition is

$$\gamma \frac{\frac{d(W(i,y,z)-W_u(i))}{dw}}{W(i,y,z) - W_u(i)} + (1-\gamma)\frac{\frac{dJ(i,y,z)}{dw}}{J(i,y,z)} = 0. \quad (21)$$

Under right-to-manage, the firm chooses hours after the wage is set, so h responds to w through the firm's best-response $\alpha zyh^{\alpha-1} = w$.

Because the firm chose h to maximize J, the envelope theorem applies:

$$\frac{dJ(i,y,z)}{dw} = \frac{-h}{1-\beta(1-\sigma)}.$$

The worker's surplus is

$$W(i,y,z) - W_u(i) = \frac{wh - x_i \frac{h^{1+\phi}}{1+\phi} - (1-\beta)W_u(i)}{1-\beta(1-\sigma)}.$$

The total derivative is

$$\frac{d(W(i,y,z)-W_u(i))}{dw} = \frac{h+(w-x_ih^{\phi})\frac{dh}{dw}}{1-\beta(1-\sigma)}.$$

Differentiating the firm's best response $\alpha zyh^{\alpha-1} = w$ gives $\frac{dh}{dw} = \frac{h}{(\alpha-1)w}$. Substituting,

$$\frac{d(W(i,y,z)-W_u(i))}{dw} = \frac{h}{1-\beta(1-\sigma)} \cdot \frac{\alpha w - x_ih^{\phi}}{(\alpha-1)w}.$$

Substituting both derivatives into (21) and simplifying (using $wh = \alpha zyh^{\alpha}$), we obtain

$$wh = \frac{\gamma}{\alpha}x_ih^{1+\phi} + (1-\gamma)\left[\frac{x_ih^{1+\phi}}{1+\phi} + (1-\beta)W_u(i)\right].$$

Replacing $wh = \alpha zyh^{\alpha}$ on the left-hand side yields the equilibrium hours equation:

$$\alpha zyh^{\alpha} = \left[\frac{\gamma}{\alpha} + \frac{1-\gamma}{1+\phi}\right]x_ih^{1+\phi} + (1-\gamma)(1-\beta)W_u(i). \tag{22}$$

The interior Nash solution applies only when it satisfies the worker's participation constraint. When the unconstrained wage implies $W(i,y,z) - W_u(i) < 0$, the participation constraint binds and the equilibrium allocation is instead determined by the firm's hours best response, $\alpha zyh^{\alpha-1} = w$, together with the participation condition $wh - x_ih^{1+\phi}/(1+\phi) = (1-\beta)W_u(i)$, which pins down the participation wage and the associated hours. If this system admits no solution with positive surplus, the match is not created. In the quantitative implementation, I solve numerically for the relevant case—interior Nash, participation-binding, or no match.

Firm value.

The expected value of a firm with productivity y is denoted $\Pi(y)$ and is equal to

$$\Pi(y) = -C(v(y)) + \beta((1-\delta)\Pi(y) + m(\theta)v(y)J(y)),$$

where $J(y)$ denotes the expected value of a filled job in a type-y firm, averaging over worker types and match-specific productivity draws:

$$J(y) \equiv \sum_{i=1}^{N} s(i)\int_{z\geq \underline{z}(i,y)} J(i,y,z)\,dH(z).$$

δ is the probability that the firm is destroyed at each period. Rearranging, we get

$$\Pi(y) = \frac{\beta m(\theta)v(y)J(y) - C(v(y))}{1-\beta(1-\delta)}.$$

The vacancy cost function, $C(.)$ is homogeneous of degree c_1. Combining that with the labor demand Equation (13), we have

$$\Pi(y) = \frac{\beta m(\theta)v(y)(1-\frac{1}{c_1})J(y)}{1-\beta(1-\delta)},$$

which shows that the firm value is proportional to the expected value of a job in that firm.

Unemployment rate.

I denote by $N(i)$ the size of the workforce of type i, which is exogenous. Hence, $N(i) = U(i)+L(i)$, where $U(i)$ is the number of unemployed workers, and $L(i)$ is the number of employed workers of type i. Each period, the number of entries into unemployment for the type i is equal to $(N(i)-U(i))\sigma$. The number of exits out of unemployment is equal to $U(i)\theta m(\theta)\int[1-H(\underline{z}(i,y))]\frac{nv(y)}{V}\mathrm{d}\tilde{F}(y)$. In equilibrium, the number of entries into unemployment is equal to the number of exits. We can deduce the number of workers unemployed, for each type:

$$U(i) = \frac{N(i)\sigma}{\sigma+\theta m(\theta)\int\left[1-H(\underline{z}(i,y))\right]\frac{nv(y)}{V}\mathrm{d}\tilde{F}(y)}, \tag{23}$$

where $1-H(\underline{z}(i,y))$ is the probability that a match in a type-y firm is profitable (and converted into a job) for a type-i worker. Differences in unemployment rates between different types of workers are due to differential probabilities that matches are converted into jobs. As a result, the unemployment rate is equal to the ratio of the job destruction rate and the sum of the destruction and creation rates.

Employment.

The number of jobs in a given firm depends on the age of the firm. At the firm level, employment is not constant over time. The firm size distribution depends on the age distribution. The number of workers of type i working in a firm of type y and of age τ is equal to

$$\ell^\tau(i,y) = v(y)m(\theta)s(i)\left[1-H(\underline{z}(i,y))\right]\frac{1-(1-\mu)^\tau}{\mu}.$$

Since firm exit rate, δ, is constant, the age distribution does not depend on y. Using the age distribution, we can deduce the average number of workers of type i working in a type y firm:

$$\ell(i,y) = v(y)m(\theta)s(i)\left[1-H(\underline{z}(i,y))\right]\frac{(1-\delta)}{[1-(1-\mu)(1-\delta)]}.$$

Aggregating over all types of firms, we can then compute total employment for each type of worker,

$$L(i) = n\int \ell(i,y)\mathrm{d}\tilde{F}(y),$$

where n is the number of firms operating in the economy.

Therefore, total employment is

$$L = \sum_{i=1}^{\mathcal{N}} L(i).$$

In equilibrium, when $\theta = \frac{V}{U}$, this coincides with $\sum_{i=1}^{\mathcal{N}} N(i) - \sum_{i=1}^{\mathcal{N}} U(i)$, where $U(i)$ is computed in Equation (23).

Total hours.

In a type-y firm, the total number of hours worked by workers of type i is

$$v(y)m(\theta)s(i) \int h(i,y,z) \ \mathrm{d}H(z) \frac{(1-\delta)}{[1-(1-\mu)(1-\delta)]},$$

where $h(i,y,z)$ is the number of hours of work in the contract (which depends on the firm type, the worker type and the match-specific productivity). Therefore, the total number of hours worked in the economy is

$$n \times m(\theta) \frac{(1-\delta)}{[1-(1-\mu)(1-\delta)]} \sum_{i=1}^{\mathcal{N}} s(i) \left[\int v(y) \int h(i,y,z) \ \mathrm{d}H(z) \mathrm{d}\tilde{F}(y) \right].$$

H.3 Model estimation: details

Step 1: construction of worker disutility types

This appendix describes how the work-disutility parameter x_i is constructed from survey data and discretized into worker types.

I use individual-level data from the French Labor Force Survey (2011–2012), restricting the sample to employed wage and salary workers aged 18–65 for whom hourly wages, preferred hours, and gender are observed.

To remove systematic variation not explicitly modeled, both hourly wages and preferred hours are residualized with respect to age, occupation, education, and industry. For each variable, I estimate linear regressions on these controls, take the residuals, and add back the sample mean to preserve levels. The resulting residualized wage and preferred hours are then combined at the individual level.

In the model, preferred hours at wage w satisfy the static first-order condition $x_i h^{\phi} = w$, where ϕ is fixed and common across workers. I invert this condition for each individual to recover an implied value of the disutility parameter,

$$x_i = \frac{w}{(h^{\text{pref}})^{\phi}}.$$

This yields an empirical distribution of x_i for men and women separately.

To implement the model, I approximate this distribution by a discrete distribution with three support points for each gender. For men and women separately, I partition the support of x_i into three intervals and compute, within each interval, the mean value of x_i and the corresponding population weight using survey sampling weights. These means and weights define the six worker types used in the quantitative analysis.

Step 2: estimation of technology and firm distribution parameters

This appendix provides additional details on the identification of the technology parameters and the assumptions underlying their estimation.
Equation (9) gives the firm's chosen hours for a given hourly wage. For vacancies below 35 hours, the optimality condition is

$$w = \alpha\, y\, h^{\alpha-1},$$

while above 35 hours it becomes

$$w + \tau = \alpha\, y\, h^{\alpha-1}.$$

For $h \neq 35$, it implies a log-linear relationship between posted hourly wages and hours at the job level:

$$\log w_{vf} = a_f + (\alpha - 1)\log h_{vf} \text{ if } h_{vf} < 35,$$

$$\log(w_{vf} + \tau) = a_f + (\alpha - 1)\log h_{vf} \text{ if } h_{vf} > 35,$$

where v indexes vacancies and f firms, and a_f collects firm-specific components, including productivity. I estimate α using firms that post multiple vacancies by regressing $\log w_{vf}$ (or $\log(w_{vf} + \tau)$) on $\log h_{vf}$. I include firm fixed effects to difference out a_f and to identify $\alpha - 1$ from within-firm variation in posted hours and wages. This strategy relies on the assumption that the technological relationship between output and hours is common across firms and does not depend on whether a firm posts one or multiple vacancies.

Given α and τ, I then use the optimality conditions above to estimate y_{shape} and y_{scale}. For each observed vacancy j with hours h_j and wage w_j, these conditions imply the level of productivity consistent with the posted contract. I construct the probability of observing h_j at wage w_j under the model and define the log-likelihood as

$$\mathcal{L}(y_{\mathsf{shape}}, y_{\mathsf{scale}}) = \sum_{j=1}^{N} \log \Pr(h_j \mid w_j;\, y_{\mathsf{shape}}, y_{\mathsf{scale}}, \alpha, \tau)\,.$$

I estimate $(y_{\mathsf{shape}}, y_{\mathsf{scale}})$ by maximizing this likelihood over the sample of vacancies. To avoid overweighting firms that post many vacancies, I randomly select one vacancy per firm so that each firm contributes equally to the likelihood. I assume that the randomly selected vacancy represents the firm-specific relationship between the wage and hours, characterized by y. The estimation assumes that the posted contract reflects the ex-ante relationship between wages and hours implied by firm technology, prior to worker matching, so that match-specific heterogeneity does not affect the identification of $(y_{\text{shape}}, y_{\text{scale}})$.

Step 3: parameters informing equilibrium hours

This appendix provides additional details on the estimation of the parameters $(c_1, \gamma, \bar{z})$.

Given worker types and technology parameters from Steps 1 and 2, the model implies an equilibrium distribution of hours as a function of the parameter vector $\theta = (c_1, \gamma, \bar{z}, \{W_u(i)\}_i)$. I normalize $W_u(1)$ and estimate $\{W_u(i)\}_{i=2,\dots,6}$. Let H^{data} denote a vector of moments of the empirical hours distribution, and let $H^{\text{model}}(\theta)$ denote the corresponding moments simulated from the model for a given θ.

I estimate θ by generalized method of moments:

$$\hat{\theta} = \arg\min_{\theta} \left[H^{\text{data}} - H^{\text{model}}(\theta)\right]' \Omega \left[H^{\text{data}} - H^{\text{model}}(\theta)\right].$$

I target the following empirical moments: the share of jobs with workweek below 13, 22, 34 and 37 for men and for women. Ω is the identity matrix.

Step 4: unemployment

In the model, the aggregate unemployment rate depends on the overall job-finding rate, which is proportional to the product of the matching efficiency parameter m_0 and vacancy creation. Because vacancy posting is determined by the cost function $C(v) = c_0 v^{c_1}$, the unemployment rate identifies the ratio m_0/c_0 rather than each parameter separately. I therefore normalize $c_0 = 1$ and calibrate m_0 to match the empirical unemployment rate.

The flow utility of unemployment b enters workers' labor supply conditions. For each worker type i, the value of unemployment satisfies

$$(1-\beta)W_u(i) = b_i + \beta\,\theta m(\theta) \int \left[\int_{z \geq \underline{z}(i,y)} [W(i,y,z) - W_u(i)]\,\mathrm{d}H(z)\right] \frac{nv(y)}{V}\,\mathrm{d}\tilde{F}(y), \quad (24)$$

where θ denotes labor market tightness, $m(\theta)$ the matching function, $v(y)$ the vacancy policy, and $\tilde{F}(y)$ the equilibrium distribution of firm productivity. I allow b_i to differ across worker types and set these values so that the labor supply equations are jointly satisfied for all six types in equilibrium. This ensures internal consistency of workers' outside options with the equilibrium job-finding rates implied by the calibrated matching technology.

	Data	Model
Average firm size	50.98	50.99
Average hours per man	34.72	33.73
Average hours per woman	31.31	30.23
Average hours per worker	33.72	31.92
Number of men below 24h	2.87	1.47
Number of women below 24h	4.29	5.72
Total hours worked in the year	85,064	84,631

Table H.2: Model fit with respect to non-targeted moments, firm-level averages

Notes: This table shows firm-level average moments in the data, computed from the DADS, and in the model. The model moments are computed in the pre-reform framework for estimated values of the structural parameters. **Go back to main text.**

Policy parameter ρ

The event study estimated in reduced form is an extended version of a difference-in-difference. Taking the corresponding difference-in-difference equation in first difference, I obtain

$$\frac{L_{i,after} - L_{i,before}}{L_{i,before}} = \lambda_0 + \lambda_1 Share24_i + u_i, \tag{25}$$

where the left-hand side is the change in the number of jobs in the firm after implementation of the minimum workweek. As before, Share24$_i$ is the share of jobs with workweek below 24h in the firm before implementation of the policy. In reduced form, I have estimated the empirical counterpart $\hat{\lambda_1}^{DiD}$. In the model, I simulate a large number of firms, conditional on estimated values of the structural parameters. For each firm, I compute the number of jobs and exposure, before any policy change. Then, for any value of the policy parameter ρ, I can compute the new general equilibrium and the new number of jobs in each firm. It is then possible to deduce $\hat{\lambda_1}^{Model}$, the regression estimate from the model. Finally, I pick the value of $\hat{\rho}$ such that

$$\hat{\lambda_1}^{Model} = \hat{\lambda_1}^{DiD}. \tag{26}$$

	DiD estimate (data)	Model-based DiD
A. Jobs below 24h		
All	-2.35	-10.75
Women	-1.72	-10.70
Men	-1.80	-6.55
B. Number of jobs		
All	-0.55	-0.49
Women	-0.65	-1.15
Men	-0.40	1.01
C. Total hours		
All	-0.24	0.09
Women	-0.50	-0.38
Men	-0.14	0.97

Table H.3: Comparison of difference-in-difference estimates in model and data

Notes: This Table reports estimates of Equation (1) in 2017 for several outcomes of interest (first column) and the corresponding difference-in-difference estimates obtained from the structural model (second column). The estimate for the number of jobs for all workers is used to calibrate the policy parameter. Other estimates shown in this table are not targeted in the estimation procedure.

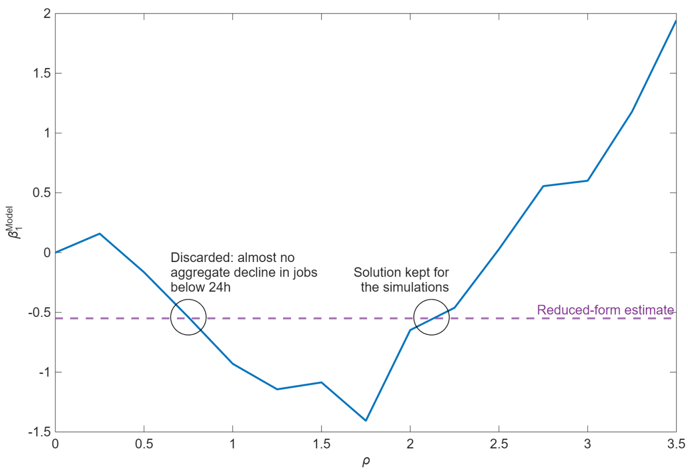


Figure H.4: Model simulations for different values of ρ

Notes: This Figure shows the difference-in-difference estimate simulated in the structural model, for several values of the policy parameter, ρ. The outcome is the number of jobs in the firm. Each estimate is obtained after computing the new general equilibrium of the model for the corresponding policy shock. The reduced-form estimate corresponds to β_{2017} in Equation (1). The two circles report the two solutions found. The lowest value for ρ is discarded as it implies almost no aggregate reduction in low-hour jobs, which is rejected by the data. **Go back to main text.**

H.4 Additional quantitative exercises

Fraction of jobs with inefficiently low hours

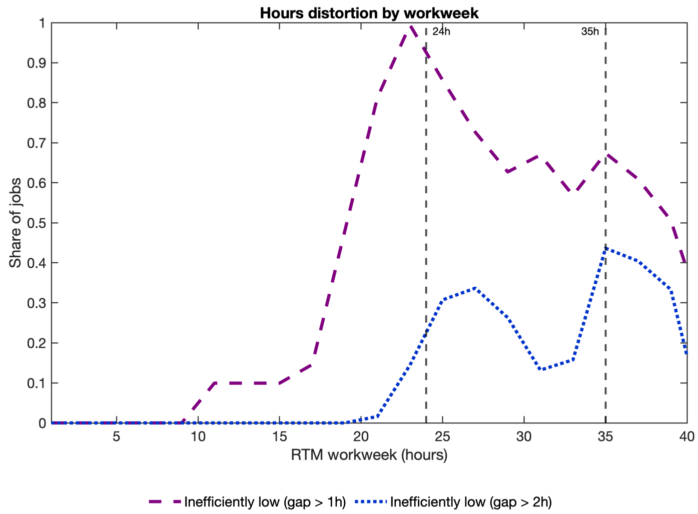


Figure H.5: Share of jobs with inefficiently low hours

Notes: This figure plots the fraction of jobs for which hours under RTM are lower than the efficient level, by workweek. The dashed purple line reports the share of jobs for which the gap with efficient hours above one hour. The dotted blue line reports the share with a gap above two hours. **Go back to main text.**

Synthetic DiD

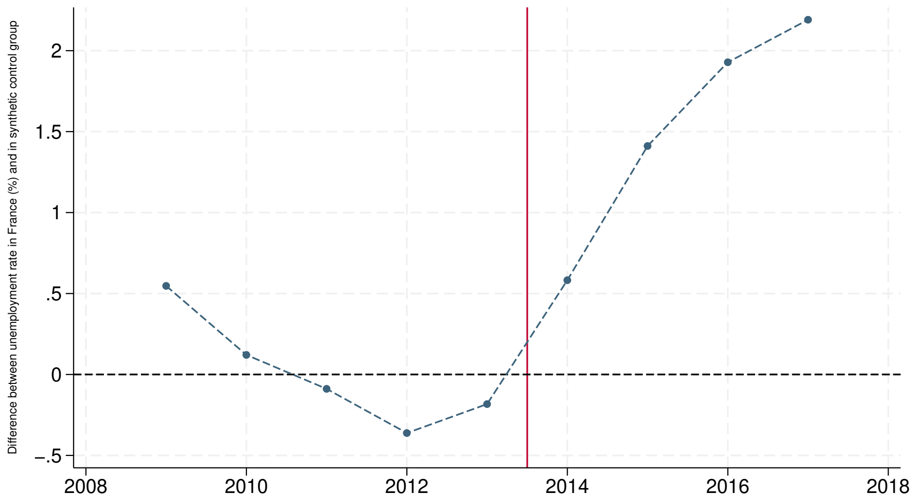
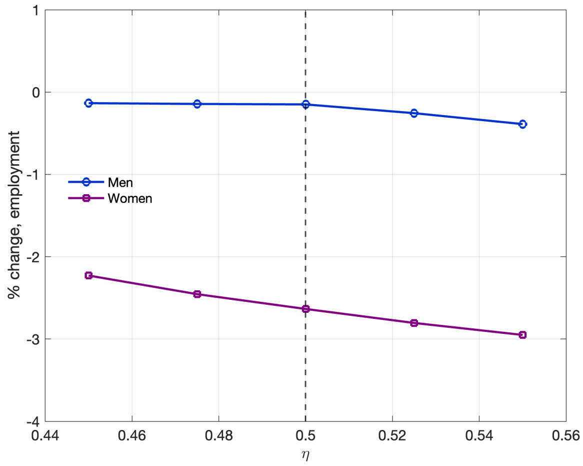


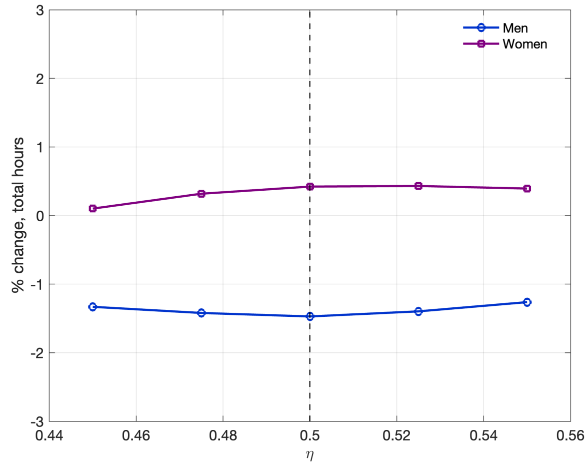
Figure H.6: Difference between French unemployment rate and synthetic unemployment rate

Notes: This figure plots the gap between the log unemployment rate in France each year and the log unemployment rate computed in the synthetic control group. The synthetic control group is determined based on Abadie et al. (2010). Countries in the synthetic control group are Austria, Belgium, Denmark, Germany, Hungary, Italy, Luxembourg, Poland, Portugal, Spain, Sweden, United Kingdom. **Go back to main text.**

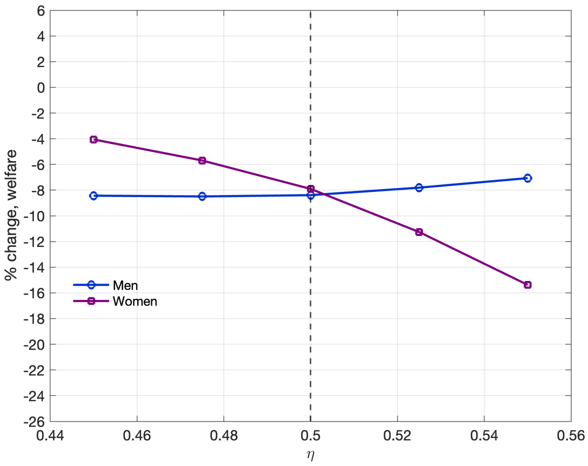
Effects of the reform for alternative parameter values



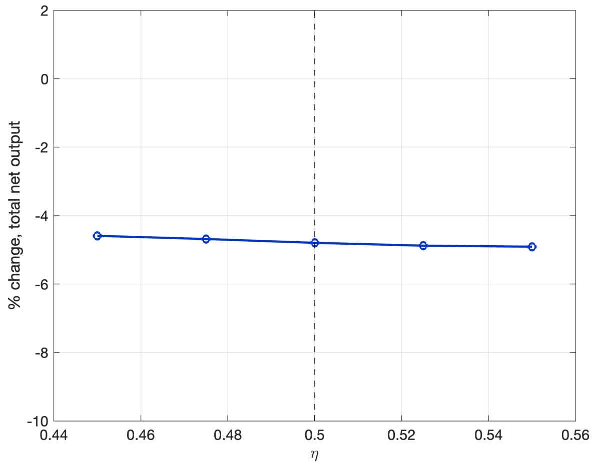
(a) Number of jobs



(b) Total hours



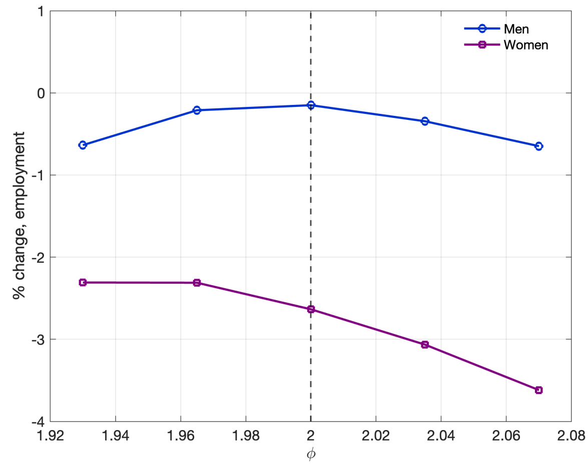
(c) Welfare



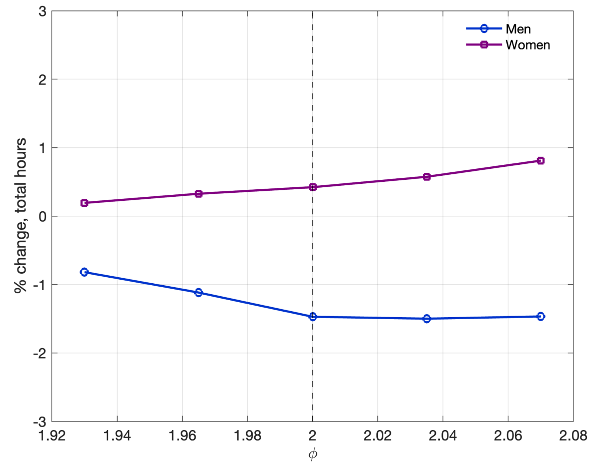
(d) Output

Figure H.7: Effects of the 24-hour reform under alternative values for η

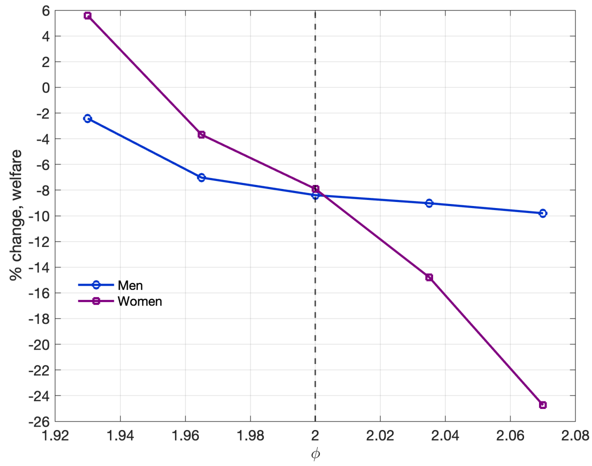
Notes: This Figure shows the effects of implementing the 24-hour reform in the estimated model, for alternative values of the elasticity of the matching function η. For each value of η, I compute the pre-reform and the post reform equilibria and the % change in outcomes. All other parameter values are kept as estimated and described by Table 3. The vertical dashed line shows the baseline value used for the main simulations. In the baseline, the value of η is fixed at 0.5. **Go back to main text.**



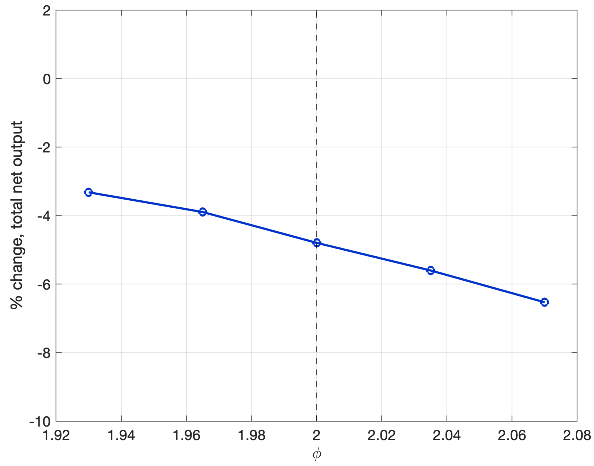
(a) Number of jobs



(b) Total hours



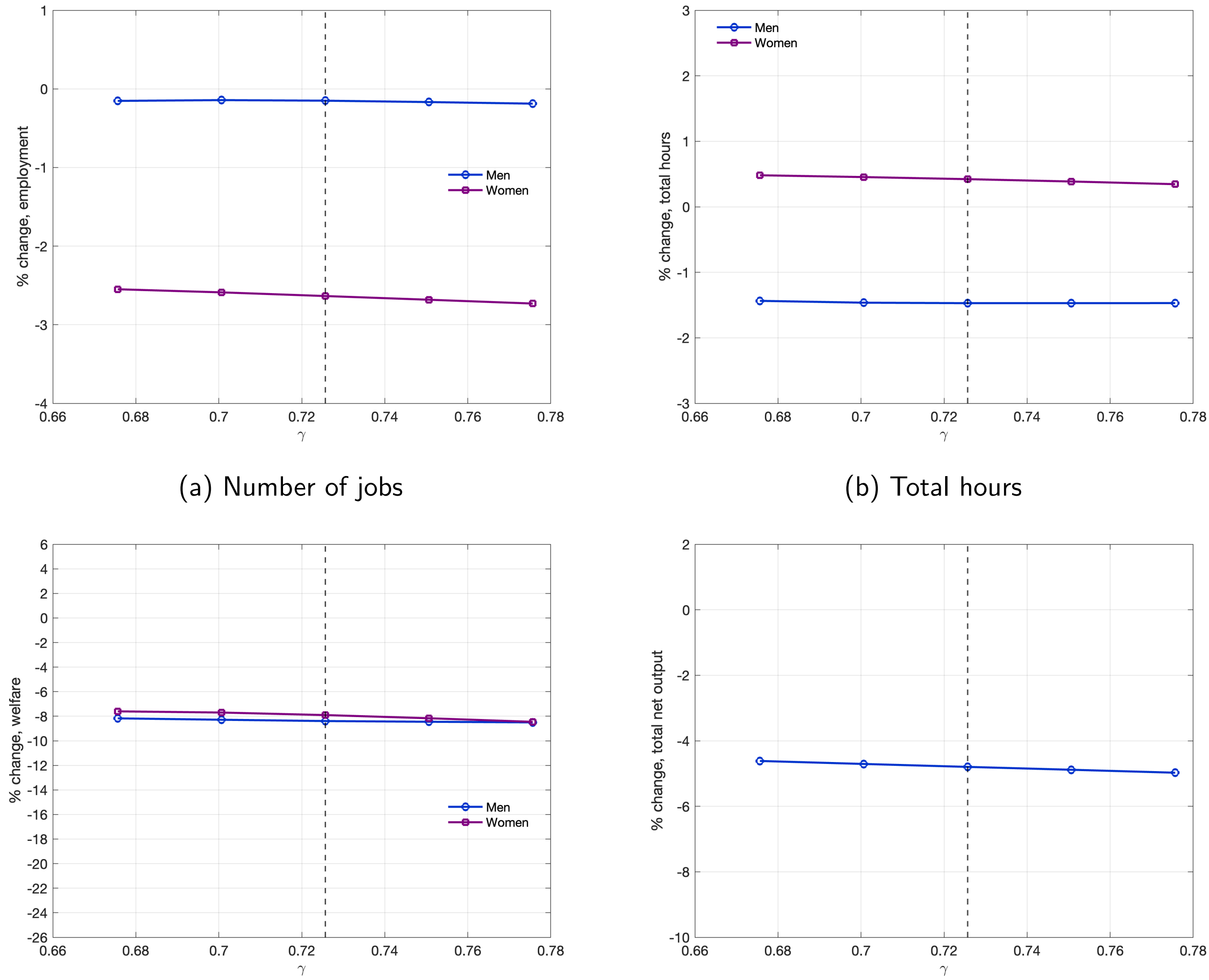
(c) Welfare



(d) Output

Figure H.8: Effects of the 24-hour reform under alternative values for ϕ

Notes: This Figure shows the effects of implementing the 24-hour reform in the estimated model, for alternative values of the curvature of the disutility function ϕ. For each value of ϕ, I compute the pre-reform and the post reform equilibria and the % change in outcomes. All other parameter values are kept as estimated and described by Table 3. The vertical dashed line shows the baseline value used for the main simulations. In the baseline, the value of ϕ is fixed at 2. **Go back to main text.**



(a) Number of jobs

(b) Total hours

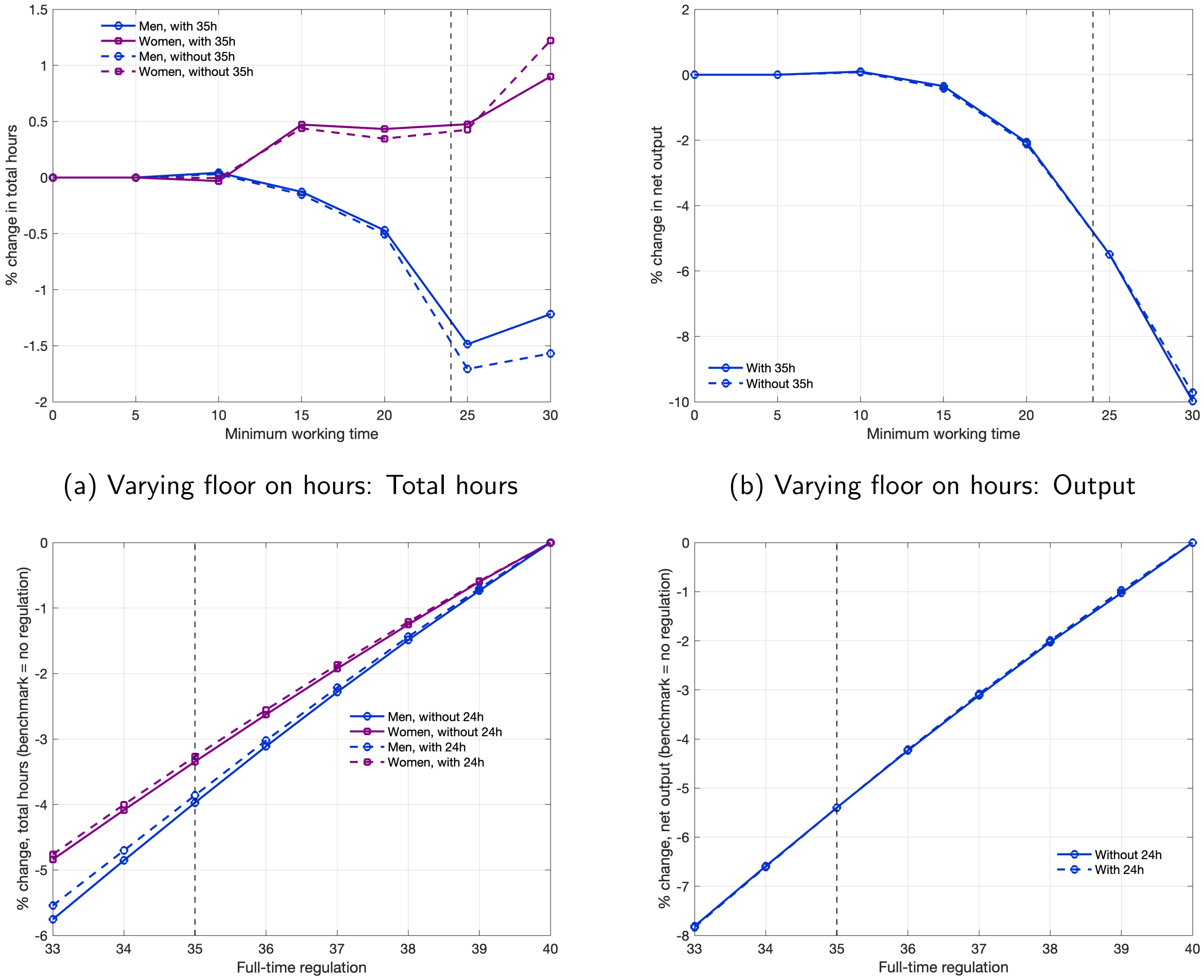
(c) Welfare

(d) Output

Figure H.9: Effects of the 24-hour reform under alternative values for γ

Notes: This Figure shows the effects of implementing the 24-hour reform in the estimated model, for alternative values of the bargaining parameter γ. For each value of γ, I compute the pre-reform and the post reform equilibria and the % change in outcomes. All other parameter values are kept as estimated and described by Table 3. The vertical dashed line shows the baseline value used for the main simulations. In the baseline, the value of γ is estimated in Step 3 of the estimation procedure. **Go back to main text.**

Policy counterfactuals: additional outcomes



(a) Varying floor on hours: Total hours

(b) Varying floor on hours: Output

(c) Varying full-time workweek: Total hours

(d) Varying full-time workweek: Output

Figure H.10: Policy counterfactuals: effects on total hours and output

Notes: Panels (a) and (b) display the change in total hours and in output, of introducing a legal minimum working time, as indicated on the x-axis. The benchmark is the equilibrium without a minimum working time. For each counterfactual, I consider two scenarios: (i) the introduction of a minimum working time in the presence of a pre-existing 35-hour regulation (solid lines), and (ii) the introduction of a minimum working time in the absence of any full-time regulation (dashed lines). The vertical dashed line indicates the 24-hour floor implemented in 2014 (the estimates connected by solid lines at this value correspond to those reported in Table 4). Panels (c) and (d) show the total hours and output effects of introducing a legal full-time working time, as indicated on the x-axis. It is modeled as a cost for jobs exceeding the legal full-time workweek. The vertical dashed line indicates the 35-hour regulation as implemented in 2000 in France. The benchmark is an equilibrium without a full-time regulation, but with an upper bound on hours at $\bar{h} = 40$, as in the main analysis. I consider two scenarios for the implementation of the full-time regulation: (i) implementation in the absence of a minimum workweek (solid lines), and (ii) implementation when a legal minimum working time of 24 hours is already in place (dashed lines). In all panels, circles denote estimates for men and squares denote estimates for women. Each marker reports a separate policy change and depicts the % change in outcome between the pre-reform and post-reform equilibrium. All counterfactuals are simulated for the estimated parameter values reported in Table 3. **Go back to main text.**