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Essays on Empirical Labor Economics

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Summary

This dissertation empirically examines labor markets and how shocks to these markets impact different levels of local labor markets, firms, and workers.

The **first chapter** analyzes the labor market from a historical perspective. Specifically, it examines how the Second World War impacted the local industrial structure in post-war Japan. The war resulted in a large gender imbalance due to the death of 2 million male soldiers. Moreover, this loss was concentrated among certain cohorts and prefectures due to a combination of historical and institutional reasons, namely, the conscription system, the hometown regiment system, and the US military strategy. By exploiting the variation in changes in gender balance cohort-by-prefecture, the chapter finds that the imbalanced gender ratio led to a decrease in manufacturing sector employment after the war and an increase in agricultural employment shares. However, this effect lasted only until the 1960s, indicating that the long-run industrial structure is determined by location fundamentals. Additionally, it finds that female shares in each industry increased, coinciding with increased female labor force participation.

The **second chapter** examines how legislative reductions in working hours impact firms' employment, output, and productivity. It exploits a Portuguese reform that reduced standard hours from 44 to 40 hours in 1996. The empirical analysis relies on a difference-in-differences approach, building on the fact that some firms were not affected because they already had shorter working hours due to collective agreements and occupation-specific exceptions. The findings indicate that the reform had adverse effects on the employment and output of affected firms. These effects can be attributed to an increase in hourly labor costs induced by the legal obligation not to reduce monthly salaries. Treated firms adjusted their employment by reducing hiring and significantly improved hourly labor productivity, in part through an intensified use of capital. In contrast, firms that reduced working hours through collective agreements prior to the reform were able to increase productivity without adverse effects on employment and output. A key policy takeaway from these combined findings is that estimating effects on early adopters is likely to provide a biased estimate of the overall cost of the switch to lower hours.

The **third chapter** investigates the worker-level effects of the working hour reductions, exploiting the 1996 reform in Portugal that established the 40-hour workweek. Specifically, this chapter analyzes how it affects men and women differently. It finds that the reform reduced the share of women preferring shorter hours and increased the incentive to work full-time, but not for men. This suggests that women likely gain more welfare from the reduction in working hours than men do. However, contrary to this finding, the analysis of job transitions using exhaustive employer-employee data indicates that women's job separation did not decrease more than men's; if anything, it might have increased. The gender wage gaps within establishments did not close either. This suggests that while shorter standard hours may improve welfare more significantly for women, they may not directly impact gender wage gaps.

Keywords: Labor demand, Labor supply, War, Gender ratio, Industrial structure, Working hours, Productivity, Gender gap

Résumé

Cette thèse examine empiriquement les marchés du travail et comment les chocs sur ces marchés impactent différents niveaux des marchés du travail locaux, des entreprises et des travailleurs.

Le **premier chapitre** analyse le marché du travail d'un point de vue historique. Plus précisément, il examine comment la Seconde Guerre mondiale a impacté la structure industrielle locale dans le Japon d'après-guerre. La guerre a entraîné un important déséquilibre entre les sexes en raison de la mort de 2 millions de soldats masculins. De plus, cette perte était concentrée parmi certaines cohortes et préfectures en raison d'une combinaison de raisons historiques et institutionnelles, à savoir le système de conscription, le système des régiments locaux et la stratégie militaire américaine. En analysant la variation des changements dans l'équilibre des sexes par cohorte et par préfecture, le chapitre constate que le ratio déséquilibré des sexes a conduit à une diminution de l'emploi dans le secteur manufacturier après la guerre et à une augmentation des parts d'emploi dans l'agriculture. Cependant, cet effet n'a duré que jusqu'aux années 1960, indiquant que la structure industrielle à long terme est déterminée par les fondamentaux locaux. De plus, il constate que la part des femmes dans chaque industrie a augmenté, coïncidant avec une augmentation de la participation des femmes à la population active.

Le **deuxième chapitre** examine comment les réductions législatives des heures de travail impactent l'emploi, la production et la productivité des entreprises. Il analyse une réforme portugaise qui a réduit les heures standard de 44 à 40 heures en 1996. L'analyse empirique repose sur une approche de la méthode des doubles différences, en s'appuyant sur le fait que certaines entreprises n'ont pas été touchées car elles avaient déjà des heures de travail plus courtes en raison d'accords collectifs et d'exceptions spécifiques à certaines professions. Les résultats indiquent que la réforme a eu des effets négatifs sur l'emploi et la production des entreprises concernées. Ces effets peuvent être attribués à une augmentation du coût horaire du travail, induite par l'obligation légale de ne pas réduire les salaires mensuels. Les entreprises concernées ont ajusté leur emploi en réduisant les embauches et ont considérablement amélioré la productivité horaire du travail, en partie grâce à une utilisation intensifiée du capital. En revanche, les entreprises qui ont réduit les heures de travail grâce à des accords collectifs avant la réforme ont pu augmenter leur productivité sans effets négatifs sur l'emploi et la production. Un enseignement politique clé de ces résultats combinés est que l'estimation des effets sur les premiers adoptants est susceptible de fournir une estimation biaisée du coût global du passage à des heures réduites.

Le **dernier chapitre** examine les effets de la réduction des heures de travail sur les travailleurs, en analysant la réforme de 1996 au Portugal qui a établi la semaine de travail de 40 heures. Plus précisément, ce chapitre analyse comment cela affecte différemment les hommes et les femmes. Il constate que la réforme a réduit la part des femmes préférant des heures plus courtes et augmenté l'incitation à travailler à temps plein, mais pas pour les hommes. Cela suggère que les femmes bénéficient probablement davantage du bien-être résultant de la réduction des heures de travail que les hommes. Cependant, contrairement à cette constatation, l'analyse des transitions professionnelles utilisant des données exhaustives employeur-employé indique que la séparation des femmes de leur emploi n'a pas diminué plus que celle des hommes ; si ce n'est qu'elle a peut-être augmenté. Les écarts de salaire entre les sexes au sein des établissements ne se sont pas non plus réduits. Cela suggère que, bien que des heures standard plus courtes puissent améliorer le bien-être de manière plus significative pour les femmes, elles peuvent ne pas avoir d'impact direct sur les écarts de salaire entre les sexes.

Mots-clés : Demande de travail, Offre de travail, Guerre, Ratio de genre, Structure industrielle, Horaires de travail, Productivité, Écart entre les sexes

Contents

General Introduction

Most people work at some stage in their life, and their work life significantly impacts their financial well-being and overall welfare. Consequently, labor is one of the central topics in public and political debate, encompassing issues such as unemployment, minimum wages, working hours, gender wage gaps, income inequality, taxation, and retirement.

In a classical labor market model with perfect competition, market equilibrium is achieved at the point where labor supply and labor demand intersect, determining the equilibrium wage. Workers supply their labor based on wage levels, while employers decide on the size of their workforce based on these wages, which they cannot influence. While labor market dynamics are driven by these market mechanisms of supply and demand, they are also shaped by institutional factors. For example, minimum wage laws set a lower limit on the wages that firms can offer, and working hours are often regulated by legislation. Consequently, adjustments of labor demand and supply are often not as flexible as the classical model assumes. Economic agents extend beyond workers and firms to include the government, unions, and associations, all of which have a significant impact on how the labor market operates. These influences often vary across countries and time, and the evolution of different labor markets is rooted in historical and social contexts. Analyzing different labor markets from various periods and regions deepens our theoretical understanding of how labor market forces interact and the extent to which they are mediated and structured by institutions and sociohistorical factors. In turn, the economic knowledge accumulated can enlighten public debates and policy discussions today, contributing to the improvement of the overall welfare of economic agents.

Empirically studying labor markets often poses a common challenge: we only observe the equilibrium state at any given location and time. Multiple hypotheses can match the descriptive observations, making it difficult to disentangle different mechanisms. In this context, one useful approach is to exploit "shocks" or "changes" brought to the labor market and analyze how different economic agents respond to them. These shocks can vary widely, such as quantity shocks versus price shocks, changes due to economic conditions versus institutional changes, and impacts on the supply side versus the demand side. Contrasting the empirical results regarding these shocks with theoretical models helps us test the plausibility of the assumptions in the models, disentangle different mechanisms, and estimate crucial parameters such as elasticities. Recently, the empirical study of labor markets has been fueled by an increased availability of high-quality, large datasets, such as administrative data, leading to a substantial body of fine empirical evidence. Newly established causal estimation methods (e.g., differences-in-differences, regression discontinuity designs) have enhanced the ability to more precisely identify the effects of these shocks and changes, often referred to as the credibility revolution (Angrist and

Pischke, 2010). While debates about their effectiveness and limitations continue, these methods have crucial advantages in answering some of the fundamental questions in labor economics. Key to these estimation methodologies is the presence of a quasi-experimental setting in which the treatment can be regarded as exogenous to the outcomes studied. However, the changes brought to the labor market are often products of specific historical, political, and institutional contexts. Therefore, it is crucial to describe the setting in which the treatment takes place and to carefully incorporate these contexts into the empirical analysis to convincingly identify the effects.

The study of labor markets can be done at different levels. A worker-level approach, for example, can examine how workers make decisions regarding their labor supply and how these decisions vary across their characteristics. At the firm level, we gain a better understanding of how firms make decisions about their production size and inputs (e.g., capital and labor) and how they react to changes in labor costs. It is clear that workers and firms interact: workers' reactions are often influenced by firms' decisions. For example, a worker leaving a firm can result from either the worker's supply decision or the firm's demand decision. Aggregate-level analysis, such as at the level of sectors, regions, or countries, is also insightful, especially in relation to general equilibrium. It encompasses extensive and intensive labor supply, firm exits and entries, and changes in the composition of workers and firms.

This thesis positions itself within the strand of empirical work that exploits significant events affecting the labor market to understand its functioning. In the first chapter, I explore the labor market from a historical perspective, specifically examining the effect of World War II on industry structure in postwar Japan. The war caused a significant and permanent shock to the availability of local human capital due to soldiers' deaths, presenting a unique opportunity to investigate how the labor market reacts to changes in local labor endowment and its composition. This chapter studies aggregate-level or market-level employment and industry composition. The focus is primarily on labor demand responses (i.e., how industry's employment demand is shaped by local labor endowment), but it also incorporates labor supply factors, such as changes in female labor supply in response to the death of young male soldiers. The analysis uses newly digitized historical data from the population census reports for 1920-1980.

In the second and third chapters, I examine an institutional change: the reduction of standard working hours from 44 to 40 in Portugal during the 1990s. This legislation-induced shock primarily impacted the labor market by imposing a maximum number of hours and altering wage rates. Chapter 2 focuses on firms, studying how labor demand was affected by the stringent working hour legislation and its implications for firms' economic performance, such as their sales and productivity. This analysis uses a partial equilibrium approach, comparing firms operating in similar labor markets to accurately examine the reactions of firms less influenced by unrelated factors such as economic cycles. Chapter

3, on the other hand, focuses on workers. Specifically, it examines the extent of gender differences in preferences for different working hours and their economic implications, using the reduction in hours as a natural experiment. This chapter abstracts from the effect of the hour reductions on labor demand, aiming to distill gender differences in the consequences of such reductions driven more by the supply side. Both chapters use high-quality employer-employee linked data from Portugal, which is well-suited to accurately examine the effects on workers and firms.

War and the labor market

War often symbolizes a situation of the humanitarian crisis and injustice: it can result in a large number of death and injuries, brings enormous trauma and sorrow to individuals even after the survival. Unfortunately, the world today still sees wars and conflicts happening, even with the global efforts to prevent them since the two catastrophic world wars in the first half of the 20th century.

War can have significant consequences for the economy and the labor market. Often, wars are accompanied by economic recessions, and the destruction of capital during the war can profoundly affect postwar development and industries. Wars can also significantly alter income distribution among individuals. Institutional changes are often brought about by the war winners, and they can largely impact the economic structure, welfare systems, labor relations, and so on. War represents a rare instance where substantial changes occur in a short period, providing a unique case study to observe how economies "absorb" these changes and to examine the stability of economic systems.

As such, economists have studied war with scientific interest. Some researchers focus on how war impacts economic development and growth (e.g., Craft, 1995; Milionis and Vonyo, 2015). These studies often reveal the market economy's strong restorative capacity: despite the disruptions caused by war, economies tend to return to their original growth paths during the postwar recovery period. This supports the notion of market resilience, suggesting that even significant shocks like wars have only short-lived effects on economic growth. However, recovery in growth does not necessarily mean that income distributions revert to prewar conditions; in fact, wars can sometimes lead to greater societal equality as a byproduct (e.g., Moriguchi and Saez, 2008). Factors contributing to these outcomes include capital destruction, wartime taxation, postwar inflation, and institutional changes.

The effect of the war can go beyond the macroeconomic indicators. Indeed, it can also be used to as a natural experiment to study the labor market, in particular, on the change in the composition of the workforce. War often draws many male workers out of the labor force, whether temporarily or permanently. Its consequence on female labor supply and their integration into the labor market is an important question that scholars have examined (e.g. Goldin, 1991; Acemoglu et al., 2004; Goldin and Olivetti, 2013; Cardoso and Morin, 2018; Boehnke and Gay, 2022). While the magnitude of this effect and its historical role in advancing women's social and economic status remains complex, the absence of male labor generally stimulates an increase in female labor supply, observed in various contexts (e.g., France, Portugal, USA). In fact, the effect of the disappearance of men can go beyond the labor market, leading some to study the consequence on marriages and fertility (e.g. Brainerd, 2017; Abramitzky et al., 2011; Ogasawara and Komura, 2022).

The extensive literature on the impact of male soldiers' absence or death often focuses on labor supply, particularly women's labor supply. However, the gender imbalance in the workforce can also influence labor demand. Certain sectors, such as manufacturing, traditionally depend more on young male labor. A shortage of male workers in the local labor market can alter the industrial composition of labor demand. This shift is also important for interpreting the increase in female labor supply: if the industry composition moves toward sectors that are more female-intensive, it can drive up female labor force participation. Nonetheless, there is limited evidence in the literature regarding how the loss of young male labor affects the industry composition of employment.

Chapter 1 explores the impact of changes in the gender composition of workers, driven by soldiers' deaths, on the industrial structure. It examines Japan's experience during World War II, where the loss of 2 million male soldiers, representing 5-6% of the working-age population, provides a unique case study. Estimating the effects of WWII in Japan presents challenges due to simultaneous economic changes during and after the war, including recessions, institutional changes by the occupying US forces, air raids and capital destruction, and postwar inflation. To isolate the effect of male soldiers' deaths, the chapter leverages within-country variations in casualty rates. Importantly, the chapter exploits crucial historical and institutional contexts to plausibly estimate the causal effects of changes in the gender ratio. First, the conscription system before the war examined all males at age 20, with recruitment focusing on recently conscripted active-duty soldiers. Second, the Imperial Japanese Army had a hometown regiment system, where regiments were organized at the prefecture level, meaning soldiers in the same regiment came from the same prefecture and were sent to the same battlefield. Third, the USA's military strategy in the Pacific Ocean, where most Japanese soldiers' deaths occurred, resulted in binary outcomes (near total destruction or full survival) for each regiment based on different small islands.

These factors create two sources of variation in casualty rates: cross-cohort and cross-prefecture. Consequently, cohort-prefecture units experienced significant differences in gender ratio changes between 1935 and 1947. Newly digitized population census reports from 1920 to 1980 provide data on population and employment by cohort-prefecture-gender. By comparing cohort-prefectures during the 1955-1980 period, the study finds that the deaths of young soldiers, as proxied by the reduction in the gender ratio from 1935 to 1947, led to a decrease in manufacturing employment shares and an increase in agriculture and service shares. However, this effect is temporary and dissipates in the 1960s, indicating that industrial structure is strongly influenced by permanent factors such as location fundamentals (Davis and Weinstein, 2002). Moreover, industries also responded to the shock to the gender ratio by increasing the share of women in each sector, and this effect persisted through 1980. Consistent with previous literature, there was also an increase in female labor force participation in cohort-prefectures with higher male losses during the war. The combined results show that the increase in labor supply was matched by two changes in labor demand structure: a shift towards sectors (agriculture and service) that employ more female labor, and an increase in the female share within each sector. The main contribution of the chapter is highlighting the role of labor demand responses to changes in workforce composition induced by the war.

Working Hours and Labor Demand

An economic shock can be triggered through legislative measures. Chapters 2 and 3 examine the impact of the 1996 national reform in Portugal on working hours. Working hours are a crucial aspect of working life, deeply connected to workers' health and well-being. In 1930, John Maynard Keynes famously predicted that by 2030, people would work only 15 hours per week. However, with only six years remaining until 2030, working hours globally are still far from his prediction. Nonetheless, workers and unions have historically fought to improve working conditions, and, aided by technological progress, they have successfully achieved shorter working hours. In many advanced countries, working hours per worker have steadily decreased from over 60 hours at the beginning of the 20th century to around 40 hours by the end of the century. Yet, working hours still vary significantly across countries: even within the OECD, average annual hours per worker exceed 2000 hours in some countries, while in others they are below 1400 hours. The large variations in working hours not only reflect economic factors, such as technological level and industrial structures, but also the differences in national legislation and social norms. Recent public attention toward the four-day workweek in many countries highlight that there is a continued desire to reduce working hours.

Employers might view this debate differently. Proponents of shorter work hours often argue that such changes benefit employers by boosting productivity, enhancing the firm's attractiveness to retain workers, and improving worker welfare. If these benefits are substantial, one might wonder why employers have not historically been more willing to reduce working hours. A key factor to understand the barriers for employers is labor cost. Working hours and employment are not necessarily perfect substitutes. For instance, three people working 40 hours each and two people working 60 hours each are not equivalent. One reason for this is the presence of fixed costs: employers often incur fixed costs for each worker, regardless of the hours worked, such as expenses related to office space, uniforms, training, and commuting subsidies. Consequently, the hourly wage for workers with shorter hours may be higher compared to those with longer hours, because the proportion of fixed costs relative to variable hours is greater. Another factor is institutional constraints. There are often limits to the number of hours a person can effectively work, which caps the potential for labor use. Additionally, hours beyond the standard are frequently compensated with higher hourly wages. In the previous example, if the standard hours are below 60, the amount of hours that require premium pay will be higher for those working longer hours.

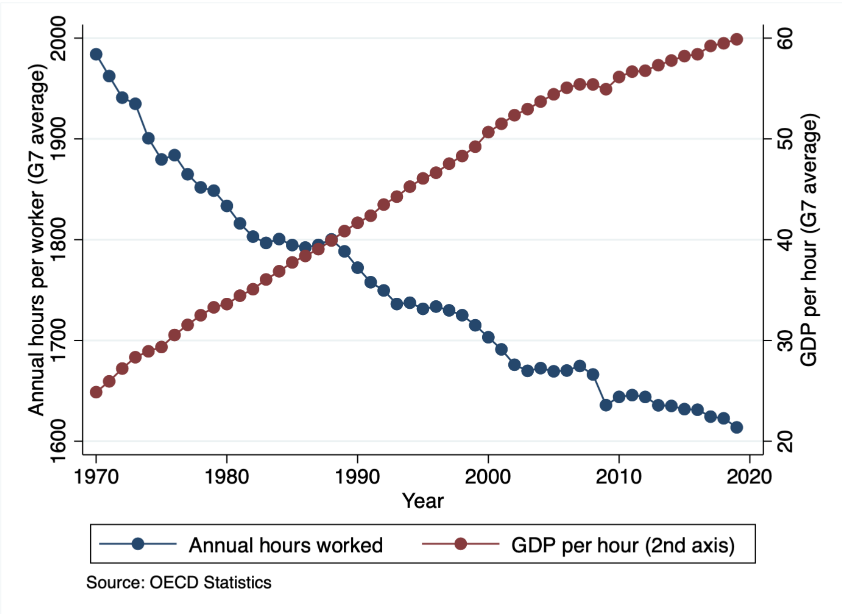
However, it remains true that working hours and the number of workers can have some degree of substitutability. This implies that if working hours per worker are reduced, employers might have an incentive to hire more workers. This is why reducing working hours can be an appealing political tool: policymakers may believe that limiting the maximum standard hours will induce more hiring, thereby reducing overall unemployment. This rationale has led to various reforms aimed at reducing working hours, with France's introduction of the 35-hour workweek around 2000 being one of the most notable examples. Economists have sought to understand the effects of such reductions on employment (see Batut et al. (2023) for a review). However, the debate about whether firms are willing to increase the number of workers remains inconclusive due to a lack of clean case studies estimating the effects of working hour reductions. Additionally, discussions on employment have often focused on aggregate-level measures (such as sector or regional employment), which are influenced by business cycles and shocks related to hour reductions, providing a limited view of how individual firms respond to changes in working hours.

Another intriguing aspect of working hours is how they relate to productivity. Figure 1 illustrates the evolution of annual working hours per worker and GDP per hour averaged across the G7 countries, clearly showing that reductions in working hours have occurred alongside steady increases in productivity. This relationship is also evident in cross-sectional data: Figure 2 demonstrates a clear negative correlation between the length of working hours and GDP per hour, with countries that have longer working hours generally exhibiting lower GDP per hour. This correlation can be interpreted in various ways. For example, technological progress might reduce the demand for labor and facilitate shorter working hours. Alternatively, countries with strong institutions that protect workers' welfare might also experience robust economic performance. Another possibility is that reducing working hours could boost hourly productivity by incentivizing firms to enhance productivity through technological and organizational changes. However, empirical evidence on whether reductions in working hours can increase productivity remains limited in the literature.

Chapter 2 aims to fill these gaps by studying Portugal's 1996 reform that reduced standard hours from 44 to 40 hours. This reform did not come with a simultaneous policy to stimulate job creation, providing a clean case study to examine how firms solve the tradeoff between increasing employment and incurring higher labor costs. The study focuses on labor demand at the firm level, specifically to determine if firms that had to reduce standard hours created more jobs. Moreover, the employer-employee linked data in Portugal also allows us to study how firms' sales and productivity changed.

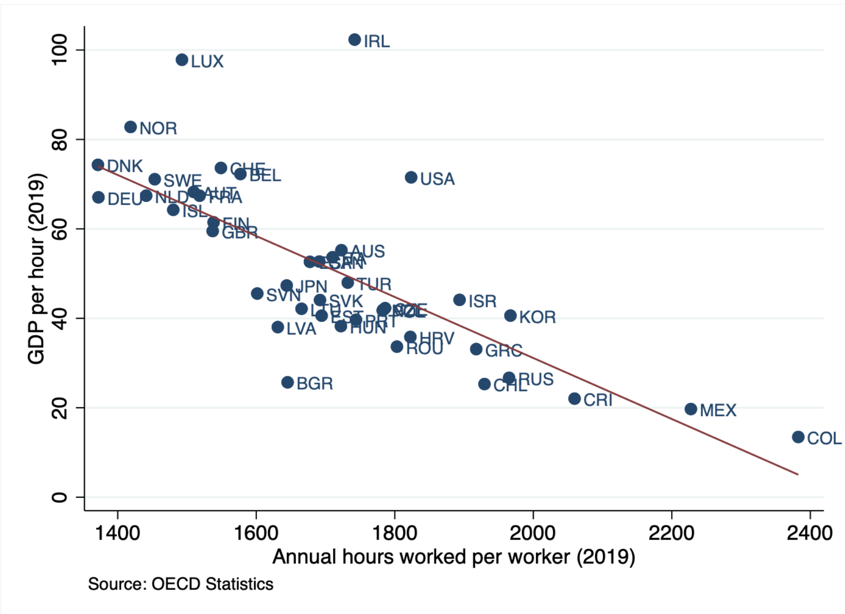
Formulating empirical strategies to exploit the legislative changes requires a careful under-

Figure 1: Evolution of Working Hours in G7 Countries



Note: The figure shows the average annual hours worked per worker, averaged across G7 countries. The data comes from the OECD Statistics.

Figure 2: Relationship Between Working Hours and Productivity



Note: The figure shows the relationship between the average annual hours worked per worker and the GDP per hour across OECD countries in 2019. The data comes from the OECD Statistics.

standing of the institutional setting in which they take place. In the case of the 1996 reform in Portugal, it was part of a long process that began in 1990, reducing the standard hours from 45 to 44 hours, with an agreement between the government, unions, and business associations to reduce them further to 40 hours through collective bargaining. Moreover, some collective agreements had already set shorter hours in the 1970s and 1980s, and some workers, including office workers, enjoyed shorter hours (e.g., 42 hours before 1996) by national legislation. Another important aspect is that the law in Portugal prohibited employers from reducing the monthly salaries of workers even when the standard hours were reduced—this mechanically increased the hourly wages of workers whose hours had been reduced.

The chapter begins by estimating the effects of the 1996 reform, which targeted workers who were still working over 40 hours per week at the beginning of the 1990s. We leverage the fact that some workers were already working shorter hours due to pre-existing collective agreements or their occupational type (e.g., office workers). The empirical strategy involves comparing firms affected by the 1996 reform—where some workers had to reduce their hours—with firms not impacted by the reform, because their employees had shorter hours for the aforementioned reasons. This comparison is made using a difference-in-differences approach with employer-employee linked data covering the entire private sector. Our findings reveal that labor costs play a crucial role: in firms where working hours were reduced, employment growth slowed in the post-reform period. This slowdown is attributed to firms' decisions to reduce hiring new workers. The increase in labor costs, stemming from higher wages due to fixed monthly income and increased fixed costs, explains this trend. Additionally, consistent with the reduction in total labor input (both the number of workers and hours per worker), nominal sales also decreased in the treated firms relative to the control firms. This evidence highlights why employers may be reluctant to reduce working hours. However, hourly productivity, measured as sales per hour, increased following the reform. This improvement could be due to factors such as the concavity of the production function and work intensification. We also find indirect evidence suggesting that capital use might have increased, potentially explaining the rise in hourly labor productivity.

Furthermore, the chapter examines firms that had already reduced working hours through collective agreements prior to the 1996 reform. For these firms, we did not observe negative impacts on employment or sales; instead, there was a gain in hourly productivity. This indicates heterogeneity in how firms and sectors adapt to shorter working hours and suggests that voluntary collective bargaining might lead firms with lower adaptation costs to reduce hours. This has important implications for public and policy discussions on reducing working hours: trials involving voluntarily participating firms may underestimate the overall cost of adapting to shorter working hours.

Working Hours and Gender

The implications of changes in working hours extend to individuals as well. According to classical labor supply models, reductions in working hours, ceteris paribus, should increase workers' utility. In many instances of working hour reductions, such as the Portuguese reform in 1996, the decrease in hours does not lead to a loss in total income. Consistent with these models, the reform was found to enhance the subjective well-being of workers whose hours were reduced (Lepinteur, 2019). However, the benefits of reducing working hours may not be uniformly experienced across all individuals, with gender potentially being an important factor. Recent literature highlights substantial gender differences in preferences for non-wage job characteristics (Wiswall and Zafar, 2017; Mas and Pallais, 2017; Maestas et al., 2023), and the usual hours of work can significantly impact the balance between work and leisure across genders. Additionally, recent studies indicate that specific aspects of working hours, such as lengthy overtime and flexibility, can contribute to gender wage gaps (Gicheva, 2013; Goldin, 2014; Cortés and Pan, 2019; Erosa et al., 2022; Wasserman, 2022; Le Barbanchon et al., 2021). These differences are in part driven by varying constraints in labor supply between men and women.

Chapter 3 explores the hypothesis that the 1996 reductions in working hours had different impacts on men and women. The chapter begins by analyzing the Labor Force Survey to compare actual working hours with desired hours for both genders. Before 1996, a larger proportion of female workers expressed a preference for shorter working hours compared to their male counterparts, though this gap had been narrowing during the early 1990s. To formally assess the impact of the reform, I employ a regression model to link an individual's likelihood of working more than 40 hours before 1996 with various demographic and job characteristics. The resulting coefficients are used to categorize individuals into those likely treated or unlikely treated by the reform, based on their probability of working more than 40 hours prior to the reform. Using a difference-in-differences approach to compare these groups, I find that after the reform, the proportion of female workers preferring fewer hours decreased, while the preference of male workers remained unchanged. This suggests that women benefited more from the reform by aligning their working hours more closely with their ideal preferences. Additionally, the reform appears to have influenced labor supply: both men and women expressed a preference for working longer hours after the reform. This shift is likely due to the increase in hourly wages for those who worked additional hours, leading to a substitution effect.

On the other hand, the analysis of the employer-employee linked dataset reveals no significant gender differences in the impact of the reform on economic outcomes. Although workers in establishments where working hours were reduced were less likely to leave their jobs post-reform, likely due to improved working conditions with no income loss, the effect was similar for both men and women. Additionally, the evolution of wages within the same establishment were comparable across genders, with no noticeable impact on gender wage gaps, at least in the short term.

Overall, while female workers generally prefer shorter working hours, this preference does not appear to significantly influence their decision to remain with their current employer compared to male workers. Although the reform's reduction in usual working hours may be more beneficial to female workers, the impact on their job retention does not show a marked difference from that of their male counterparts. Policies aimed at narrowing the gender wage gap by enhancing working conditions for women might be more effective if they address issues such as lengthy overtime hours, hour precarity, or flexibility. Nonetheless, the benefits of reduced working hours should not be overlooked, as they can be notably advantageous for female workers.

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Chapter 1

The Consequences of Hometown Regiment: What Happened in Hometown When the Soldiers Never Returned?[1]

-with Ryo Kambayashi

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Abstract

Sometimes, war results in a large gender imbalance in certain cohorts and areas that changes the path of economic development. However, there is ambiguity around this notion because the market economy has a strong restoring force. This study contributes to the existing literature by presenting the Japanese experience during the Second World War. Japan lost approximately 2 million soldiers during 1938-1945. Furthermore, the loss of young males concentrated in certain cohorts of certain geographical areas owing to hometown regiment system. By exploiting the variation of changes in gender balance cohort-by-prefecture, we examined the effect of the loss of young males on the post-war industrial structure. We observed that the reduction in the gender ratio may have led to slower industrialization, although to a limited extent quantitatively.

Keywords: war, gender ratio, industrial structure

1.1 War Damage and Economic Recovery

War affects the industrial structure not only through damaging the physical capital stocks but also through the tremendous loss of human capital. In Japan's case, besides the fact that it lost 2 million soldiers in combat, most casualties were males who were concentrated in certain areas and cohorts in the country due to the institution of *hometown regiment*. Applying the difference in gender imbalances between geographical areas and cohorts because of war, this study empirically examines how permanent loss of human capital relates to economic recovery. The regression results show that the permanent loss of males may have led to slower industrialization and a tentative increase in agriculture. However, quantitatively, such slow-down effects were limited and gradually disappeared about 15 years after the war. These findings imply that gender may have augmented average technology during high-speed economic growth after the war. In the medium term, technological change and internal migration may have neutralized the permanent loss of human capital in certain geographical areas.

1.2 Brief Literature Review

To examine the stability of the market economy, the literature has considered the loss of resources during the war as an exogenous negative shock to the economy. Many researchers have examined questions such as whether the economy returns to the expected trajectory from the pre-war era or to what extent the speed of recovery depends on the magnitude of damage. For example, Craft (1995) interpreted that the "Golden Age" of European economic growth between 1950 and 1973 was a catch-up stage from the aftermath of war. More recently, Milionis and Vonyo (2015) also found the resilience of economic growth, using data from 57 countries between 1950 and 1980, developed by the Madison project.

However, the research is still inconclusive since the OLS regression from post-war variables on wartime loss easily suffered from bias due to unobservable country effects. For example, considering military or political strategy, wartime loss can be determined by factors that also characterized the post-war economy, such as the development of industry. Milionis and Vonyo (2015) proposed the instrumental variable approach to exploit exogenous variation in wartime loss using the differential proximity of individual countries in the Second World War.[2]

Some literature extended the study using cross-sectional (regional) variation within a country to control for the long-term effect of unobservable regional factors. These studies focused on the efficiency of the reallocation process after the war rather than aggregated economic growth. Davis and Weinstein (2002) studied the resilience of the distribution of

[2] Specifically, the distance between each country's capital and the closest location of a major battle during the Second World War is used as an instrument for the country's post-war output gap (see the geographical IVs in economic history, section 3.2).

the city size using the destruction of physical capital due to air attacks in Japan as an instrument for the changes in the population density of a region during the war. They noted that the city size recovered 15 years after the war, indicating the strong resilience of the market economy. Brakman et al. (2004) corroborated this, using the German bombing experience to compare the differences between West and East Germany. Their finding that the reversion in the distribution of city sizes existed only in West Germany provided additional evidence about the resilience of the market economy. Conversely, Giorcelli and Bianchi (2021) argued that not only the market mechanism but also the policy intervention may have contributed toward recovery. They studied bombing destruction in Italy and found the direct effect of the Marshall Plan on the reconstruction of the Italian economy.

Although physical capital can be restored through markets or governments, damage to human capital permanently affects the economy. In the United States, as most mobilized soldiers returned home, human capital in society was temporarily affected during the war. Historians such as Chafe (1972) argued that the work experience of females, although temporal during the war, permanently changed their skills and attitudes toward the labor market, resulting in a change in the labor supply after the war. Goldin (1991) statistically examined this view based on the retrospective work histories of about 4,000 women collected by Gladys Palmer. She reported that the transition matrix on employment status between pre- and post-war suggests that wartime experiences did *not* increase women's employment. However, Acemoglu et al. (2004) found that wartime mobilization shifted female labor supply after the war. They defined the mobilization rate as the fraction of registered males between 18 and 44 years who were recruited for the entire war period, in other words, a single value for each state. They used it as an explanatory variable with the interaction of the postwar dummy.

As mobilization rates vary cross-sectionally, the OLS estimator is easily impacted by state-specific unobservable factors, which require remedying the issue of endogeneity.[3] Further, Goldin and Olivetti (2013) showed other evidence, using population censuses in the United States, and found that the shift in labor supply occurred only in the educated group.[4] Studies in the United States indicate that heterogeneous responses played an important role in the female labor supply after the Second World War owing to a sufficiently industrialized economy. Boehnke and Gay (2022) extended the literature by introducing the French experience during the First World War, where the industries had not matured enough to differentiate workers. Furthermore, the loss of more than a million men during the war led to persistent wartime damage. Using the difference in casualties among regions, they showed that the gender imbalance, created by the permanent loss of soldiers, increased

[3] They used two strategies to address the endogeneity. First, they used age and ethnic structure at the state level in 1940 as instruments for mobilization rates (Table 7). The other strategy is to decompose the mobilization rates into the economy-related component and the economy-independent component. The control related to the economy is imputed by (i) the fractions of men who were farmers and were non-white in 1940 and the average schooling of men in 1940 (Table 5) and (ii) the share of the industry in 1940 (Table 6).

[4] Goldin (1991) has already found heterogeneity among occupations.

the female labor participation. [5]

Research conducted in the United States and France indicated the importance of identifying the causes and phases of social development after the war, suggesting the importance of the demand side economic mechanism. Acemoglu et al. (2004) interpreted the mobilization as a shifter in labor supply and estimated the demand elasticity for males with respect to the female supply. They found that females are imperfect substitutes, except for high-school graduate males. This is consistent with the heterogeneous responses of female employment. Rose (2018) modified the mobilization data to capture missing men more precisely and found that the demand shift was the primary driver of the increase in female employment. Interestingly, he did not find strong effects on manufacturing, but a weak overall effect masks slightly faster growth in durable manufacturing employment and declines in jobs in non-durable goods industries like apparel and textiles, particularly for white women. Shatnawi and Fishback (2018) also estimated the Goldin-Katz type demand curve shift, for females in the United States, by using business data at the establishment level during the war. Cardoso and Morin (2018) is particular for highlighting the role of the employer. They used cohort-region-level data from Portugal after the 1980s and examined the effects of changes in gender ratio on labor market outcomes, such as employment. As an instrument for the gender ratio, they used the cohort-region variation about war casualties between 1961 and 1974 in colonies of Portugal. They found that the increase in female employment accompanied with narrowing pay gap. They interpret it as the change in employers' social norm on female employment.[6]

This study contributes to the literature by providing additional evidence on the economic effects of wartime damage in a broader perspective. We examine the impact of the male soldier loss on the industrial structure, which has not been extensively investigated in the literature. We provide comprehensive analyses on how the labor market absorbs the negative shock to the gender composition of the local labor market, by examining how this change in the demand side is related to the labor force participation of women. Moreover, as wartime damage indicated the decline in the proportion of males relative to females (gender imbalance), it was a permanent and large loss of gender-specific human capital for the economy, which is different from the cases of the United States. Lastly, the gender imbalances varied not only in regions but also in cohorts due the historical context.

[5] Their findings are also consistent with other literature that argues demographical gender imbalance increased female participation in labor markets in the long run. For example, Teso (2019) examined the change in gender balance in sub-Saharan Africa due to slavery trade. Grosjean and Khattar (2019) discussed the case of Australia, where most male convicts were sent. However, as pointed by Qian (2008) and Carranza (2014), it is difficult to identify shocks on gender balance from the change in the structure of the labor market because such shocks are gradual and continue in the long run. Specifically, gender imbalance can affect long-term supply conditions through marriage and other demographic changes (Abramitzky et al., 2011; Grossbard, 2015; Brainerd, 2017; Ogasawara and Komura, 2022).

[6] The consequences of wartime experience are also examined at the individual level, mainly in the interest of health and welfare economics. For example, Cesur et al. (2013) discussed that military combat on the ground during the war on terrorism leads to a higher probability of suicidal ideation and PTSD. In historical context, Costa and Kahn (2003) and Costa and Kahn (2007) argued the effect of the Civil War in the United States.

Therefore, we effectively control for the potential regional confounders, such as pre-war economic structure, by taking the difference between cohorts.

1.3 Hometown Regiment and the War in Pacific Ocean

During the Second World War, the Imperial Japanese Army (IJA) implemented the *hometown regimental system* that organized each infantry regiment by recruiting males from certain cohorts, e.g. aged 20-21, who lived in certain geographical areas. IJA dispatched regiments to defend the islands scattered in the Pacific Ocean, and the defensive force of the islands sometimes lost all its war potential, facing the Allies' fierce counterattack. However, IJA did not expect that the Allies would pass through some islands without any combat. This fact indicated that, in Japan, certain geographical areas lost men massively in certain cohorts, but other areas and cohorts did not.

1.3.1 Construction of Imperial Japanese Army

1.3.1.1 *First Difference*: conscription system

The Japanese government had the mandatory draft system before 1945. Each male (20 years) was examined for conscription, usually between April and August.[7] In 1944/45, the threshold was lowered to 19 years. Therefore, in 1944, the examination covered two cohorts at the same time. For entering the military service, the examination classified males into five categories: "Kou," "Otsu," "Hei," "Tei," and "Bo." Only the first two categories (Kou or Otsu) were appropriate for the active service. The last two categories indicated unsuitability for the military service, namely disabled (Tei) or sick (Bo). The evaluation was solely based on physical and health conditions and not on the cognitive skills and economic conditions of the participants. It was argued that members of wealthy families or society's elite could evade conscription; however, after the major revision of the conscription institution in 1927, there remains no evidence of bias. Thus, the yearly shares of the first category "Kou" were stable between 30% and 40%, toward the latter half of the 1920s. The actual number of mobilizations was calculated from the military operation plan determined by the General Staff Office in Tokyo.[8] Each regiment chose males among those classified as "Kou" at first and then from "Otsu."

The above conscription rule implies that a man's probability of having military service largely depends on the number of mobilizations at the age of 20 (or 19). And the number of mobilizations increased rapidly, especially after 1943. A rough estimate shows that the number of drafted males directly after the examination was 170K in 1937, 320K-360K during 1938-1943, 1,000K in 1944, and 500K in 1945. Therefore, the chance for the active

[7] Oyama (1943), p.4. Current age on the last day of November in the previous year. The exact time of examination varied among prefectures. The prefectures in Japan correspond to states in the United States, provinces in Canada, and regions in France.

[8] The procedure of mobilization was determined by The Ordinance for Mobilization Plan for the Army.

military service concentrated in certain cohorts: age 20 in 1943, age 19 and 20 in 1944, and age 19 in 1945. Based on the above estimate, given that the male population was around 700K for one cohort at age 20, almost half of the cohort was drafted at age 20 during 1938-1943, and around 70% of the cohort was drafted in 1944/45, which was approximately 25% in 1937.

1.3.1.2 *Second Difference*: two principles of the hometown regimental system

In IJA, an infantry regiment was constructed at the prefecture level, choosing 3K-4K males aged 20-21.[9] When IJA exhausted the generation, they organized a backup regiment recalling men over 22 years of age.[10] On December 1941, IJA mobilized 51 divisions that comprised 160 infantry regiments.

Generally, the regimental system has two principles. First, it functions as an independent frontline military unit responsible for recruiting new soldiers, equipping weapons, and training the troops. Second, it works as an administrative unit in peacetime. Once drafted, most Japanese males belonged to a certain regiment and their military service record was registered in the regiment (*heiseki*).[11] Even after retiring from active service in IJA, they maintained their relationship with the registered regiment and were invited for ceremony sometimes. Furthermore, as the regiment organization was based on geographical territory, the hometown community closely accompanied it. Therefore, the regiment was called *hometown regiment (kyodo rentai)* in Japan.

The regimental system was the major principle of organizing a modern army all over the world, until the beginning of the First World War. The famous example is *Kantonsystem* in Prussia. After the elaboration by Gerhard von Scharnhorst, then by Hans von Seeckt, it was inherited to Wehrmacht.[12] The British Army also had a long tradition of the regimental system for more than 100 years. Edward Cardwell and Hugh Childer reorganized it by 1881, clarifying its territorial principle.[13] The Third Republic of France reorganized its regimental system by the law of 24 July 1873. They divided the continental territory into 18 self-contained Army Corps Regions. Boehnke and Gay (2022) discussed that French Army was organized by the territorial regimental system at the beginning of the First World War. Until the First World War, a standing strength of the United States Army was organized only as territorial regiments. Specifically, divisions and brigades were the temporal organizations during the wartime and were dissolved when the war ended.[14]

The territorial regimental system has several advantages. First, it develops *l'esprit de*

[9] Usually 1-99 numbered regiment.
[10] Usually 100 to 299 numbered regiment.
[11] However, the Imperial Japanese Navy had a centralized registration system.
[12] Bartov (1991) pp.30-35. In this book, he used the word “primary group" rather than *Kantonsystem* and discussed how the primary group was destroyed after 1942, especially in Chapter 3.
[13] French (2005) Ch.1.
[14] McGrath (1947) pp.45-47.

corps by recruiting members of the same community.[15] Especially, when the main infantry tactics in the battlefield was the bayonet charge (for example, the Napoleonic War, the American Civil War), the most essential element of war fighting was to keep the group morale/discipline of infantry.[16] Second, it may prevent the Army from carrying out *coup d'Etat*. For example, the fragmentation of the regimental system prevented the British Army from being the politically destabilized factor.[17]

However, the territorial regimental system started disintegrating since the middle of the First World War. First, without conscription, the regiment could not afford the necessary wartime capacity only from its territory, as was the case for Britain and the United States.[18] Second, even with conscription, non-negligible regiments were unable to renew their strength by support only from its territory because of the tremendous loss of soldiers, horses, and supplies like France.[19] Third, the infantry tactics in the battlefield changed from mass charge to small-group fire and movement. Small-group tactics utilized several divisions (artillery, engineers, medic, telegraph, machine guns, and so on) even at the level of regiment that required the soldiers with specific skills and experiences. The territory of the regiment was too small to retain enough soldiers for each division. Therefore, the United States, Britain, France, and the Soviets abolished or substantially changed the territorial regiment system as their primary organizational scheme of the army. They introduced the centralized system at the national level during the First World War.[20]

In contrast to the global trend, IJA maintained the hometown regimental system even around 1940. Additionally, the infantry regiment remained the main military power in IJA through the Second World War. Poorly mechanized, IJA increased infantry branch to approximately 50% of total strength; whereas the United States Army decreased the share to 20%. In particular, in the last year of war, about 70% to 80% of newly drafted soldiers were assigned to infantry under the hometown regimental system.

1.3.2 Heterogeneous causalities among theatres

With the conscription system, the hometown regimental system of IJA may have lost a certain number of males of certain prefectures if the regiment is completely destroyed. Unfortunately, the particular situation around IJA during the Second World War actually concentrated the losses to certain regiments; due to the heterogeneous opponents due to long front line and the leapfrogging strategy taken by the United States Forces.

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[15] Costa and Kahn (2003)
[16] French (2005) Ch.1.
[17] Strachan (1997) pp.195-214.
[18] French (2005).
[19] Bourlet (2010).
[20] King (2013) Ch.6.

1.3.2.1 Long front line

At the end of the Second World War, IJA scattered about 3 million soldiers from the North-East China, Continental China, Indo-China, Thailand, Burma, Malaysia, Java, Borneo, Philippine to New Guinea. As the intensity of fighting depended on the confronting opponents, the casualties of troops varied by their garrison. A report by the Ministry of Welfare in 1963 estimated the ratio of the number of killed in action (KIA)/missing in action (MIA) to the number of soldiers at the end of the Second World War by the theatre.[21] They varied from 0.07 in the North-East China to 3.92 in Philippines, whereas the overall average accounted 0.27.[22] The probability of being killed or missing was on average 21% in the Japanese military force, assuming that troops did not move from their garrison until the end of war. The most severe theatre was Philippines accounting for 80%, followed by 70% in the central Pacific Islands, 67% in Burma, 61% in the south Pacific Islands, and 7% in the North-East of China. These ratios indicate that the high-intensity fighting concentrated in the islands in the Pacific Ocean since the counterattack by the Allies was mainly by the United States Force.

Some argue that IJA allocated the regiment with a particular economic attribute (for example, agricultural area) to face the United States Force. However, reportedly, there is no evidence regarding IJA intentionally allocating the regiments based on the hometown attribute; even IJA could not anticipate the severity of a particular battlefield because of the strategy adopted by the United States Force.

1.3.2.2 The Pacific islands front and leapfrogging

During the Second World War, the Japanese forces occupied the south, southwest, and central Pacific Islands to impede the communication and transportation between the United States and Australia. The first objective of the United States Force was to secure the approaches to Japan by expelling the Japanese forces from these islands for which they adopted a strategy called “Leapfrogging”. Samuel Morison explained it as “instead of invading every island which held a Japanese garrison, we bypassed the strongest concentrations such as Rabaul, Truk, and Wewak; landed amphibious forces on beaches comparatively free of the enemy; built an airfield; and, using our sea supremacy to seal off the bypassed enemy garrisons, left them to ‘wither on the vine’.”[23]

Thus, among the Pacific Islands, some islands were directly hit by the United States Force, while some islands were bypassed. IJA did not anticipate the targets of attack, thereby isolating substantial troops. Thus, without naval and air supremacy, isolated troops were neutralized until the end of the war, keeping their soldiers away from severe warfighting.

[21] Total of the Army and the Navy. Estimated KIA/MIA is the accumulated number since 1937 for Continental China, while the number for other theatre is estimated as is since 1941.
[22] 212.2M/788.9M.
[23] Morison (1968) p.7.

1.3.2.3 Example of Palau

Japan began colonizing the Palau Islands after the Treaty of Versailles in 1919. The Japanese government established one of their local branches in Koror and provided public services to nearby islands.[24] At the beginning of Japanese domination, the total population of islands was approximately 54,358 in 1923, which doubled to 113,562 by June 1939.[25]

On April 1944, anticipating counter attack by the United States Force, IJA dispatched the 14th division (14D) to defend the Palau Islands. The 14D constituted three infantry regiments, and each infantry regiment had three battalions; the second infantry regiment (2iR; Ibaraki, capacity 3,166), the 15th infantry regiment (15iR; Gunma, capacity 3,964), and the 59th infantry regiment (59iR; Tochigi, capacity 3,166). The location of 2iR and III/15iR and I/59iR was Peleliu Island and Angaur Island, respectively. The remaining four battalions of 15iR and 59iR defended the Koror Island (main island of Palau). For Yap Island, 14D located the 49th independent mixed brigade (49MBs; 8 independent infantry battalions, capacity 5,591). Lastly, the 53rd independent mixed brigade (53MBs; 6 independent infantry battalions, capacity 4,263) was assigned to the mainland of Palau. After occupying the Mariana Islands on July 1944, the next target of the United States Force was the Philippines. As the Palau Islands were located between the Mariana Islands and Philippines, the United States Force had to occupy it to serve as the staging base to recapture the Philippines.[26]

On 15 September 1944, the US 1st Marine Division began landing on Peleliu Island. After 74 days, the United States Force successfully expelled the Japanese forces from the island destroying 2iR, III/15iR and II/15iR (which counter landed from Palau as reinforcement). Of 6,822 soldiers, only 190 survived. The US 81st Infantry Division invaded Angaur Island 2 days after the landing of 1st Marine Division. After 31 days, approximately 1,194 soldiers of I/59iR (and 6 soldiers of Navy) lost their lives and 59 were captured as prisoners of war (POWs). However, the United States Force left the main island of Palau and Yap. After the war, about 25,000 soldiers from the mainland of Palau and 5,500 soldiers from Yap returned to Japan (both the Army and Navy). KIA in these areas was estimated at 770.

1.3.2.4 The Breaking Jewel

The particularity of the Pacific theater was the small number of POWs relative to KIA/MIA. Generally, if defeated in a war, a substantial number of soldiers were captured as POWs by the opponents; some surrender and the wounded are left behind. For example, the German 6th Army during the Second World War, after losing 147,200 soldiers, left 90,000 as POWs when General Paulus surrendered at the end of January 1943 at Stalinglad; the ratio of

[24] South Sea Agency

[25] The average annual growth rate was approximately 4.7% for 16 years, while it was 2.6% in Taiwan from 1925 to 1940 and 1.3% in the mainland Japan from 1925 to 1940.

[26] The distance between Saipan Islands in Mariana and Leyte Gulf in Philippine is more than 2,000 km. The air-cover by P-51 from Saipan cannot reach the Philippines directly.

KIA to POWs was 1.6 at one of the most dreadful battlefields in Europe. Contrastingly, this ratio was 42.5 and 19.2 in the battle of Peleliu and Angaur Islands, respectively.

The small number of Japanese POWs may have been due to the mental training by their military code. On January 1941, the Japanese force revised their military code and stressed the culture of shame. Since they defined surrender as shame, even after exhausting their arms, and punishment to their families, Japanese soldiers avoided surrender under the hometown regimental system. While Ruth Benedict, an anthropologist who worked for United States Office of War Information, contrasts it as "shame culture" to "guilt culture" in the United States, this is a general mechanism reinforced by the hometown regimental system in the battle field.[27]

Therefore, the hometown regiment that faced the counterattack by the United States Force in the Pacific Islands was likely to lose its soldiers, which means that specific prefecture lost a specific cohort of males massively, relative to other cohorts and prefectures. Ibaraki prefecture lost the entire infantry regiment in the battle of Palau, while neighboring Gunma prefecture and Tochigi prefecture lost two-thirds and one-third, respectively. Approximately, the number of missing men was 3K, 2K, and 1K in Ibaraki, Gunma, and Tochigi, respectively. They lost 21.2%, 17.4%, and 10.1% of one cohort of male in a single battle in Palau islands, and this variation indicated the decline in the gender ratio of each prefecture.[28]

1.3.3 Its consequences in Japan: decline in gender ratio

During the Second World War, approximately 2 million Japanese soldiers died. This loss was massive not only from the humanitarian perspective but also from the economic viewpoint, as the economy lost nearly 5% of the working population at that time, representing a large decline in the male-female ratio. Figure 1.1 shows that the gender ratio (number of men divided by number of women) of the population aged 15-64 years dropped from 1.014 to 0.939 between 1940 and 1947.[29] Notably, the gender ratio was permanently imbalanced, even though the population continued to grow, led by the post-war baby boom.

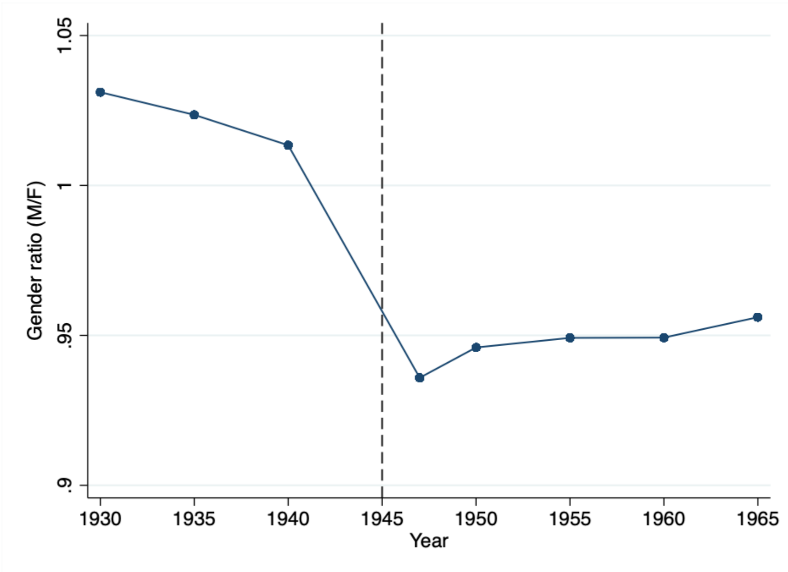
We interpret this decrease in the gender ratio as a gender-specific loss of human capital. Since a comprehensive list of war casualties is not available in Japan, we cannot directly use death counts by cohort-prefecture and instead approximate them by the gender ratio changes. Nevertheless, it is natural to use the gender ratio change in our reduced-form regressions, as we are interested in how the death of soldiers affected the economic outcomes through the change in the composition of local human capital, that is gender ratio. One

[27] Benedict (1946). Costa and Kahn (2003) discussed it during the Civil War in the United States.

[28] According to the number of examinations for conscription, which is the number of males aged 20, average cohort sizes of males are 14155.5, 11500.8, and 9858.5 in Ibaraki, Gunma, and Tochigi, respectively, between 1940 and 1943.

[29] A small recovery in the gender ratio between 1947 and 1950 is due to the return of Japanese migrants overseas such as from the Korean Peninsula and mainland China, commonly known as Hikiage-sha. Ministry of Health, Labour and Welfare estimates the number of these returnees to be more than 3 million, in addition to the return of approximately 3 million soldiers.

Figure 1.1: Evolution of gender ratio at national level



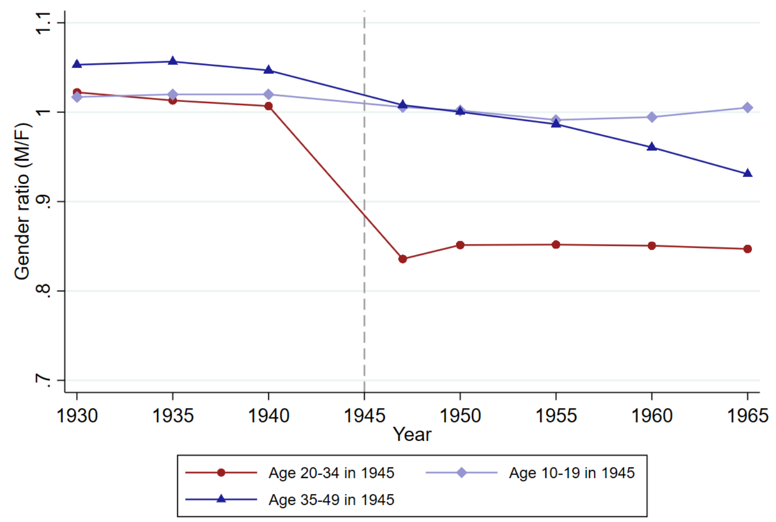
Note: The figure shows the gender ratio (defined as the number of men divided by number of women) of population aged 15-64 in Japan, calculated from the published Census reports. The gender ratio declined from 1.014 to 0.939 between 1940 and 1947. A small recovery in the gender ratio between 1947 and 1950 is due to the return of Japanese migrants overseas.

caveat of using gender ratio is that when it is defined area by area, it is also affected by social migration between areas. However, the change in gender ratio reasonably approximates the death of male soldiers. First, the decrease in the total young male population between 1935 and 1947 imputed from the cross-prefecture variation of the gender ratio is 1.75 million. This is close to the estimated 2 million soldier deaths from the official statistics.[30] Second, in a case study in Iwate prefecture, where the local authority created the list of war casualties, we find that roughly 70%-80% of the change in the gender ratio is consistent with the death of male soldiers. Third, while casualties from air raids could also lead to a human capital loss, they were fewer in number (approximately 0.3 million) and gender neutral. Fourth, we will check the robustness of our approximation later, by using a new prefecture-level approximation constructed from *Image Information Retrieval System for the Code-Number List of Adjudication Notice for Condolence Payments*.

The erosion of the gender ratio was particularly strong for the younger cohorts heavily recruited for the war. Figure 1.2 shows the evolution of the gender ratio across three different birth cohorts (aged 10-19, 20-34, and 35-49) in 1945. The gender ratio sharply drops for the cohorts aged 20-34 in 1945 between 1940 and 1947: the decline as large as

[30] We calculated the estimate decline in young male population in the following way: First, for each prefecture, we estimated the decline in male population of the treated cohorts (aged 20-34 in 1945) by multiplying population in 1935 of these cohorts by their change in the gender ratio and divided by 2. Then, we aggregated these estimates for all prefectures. This aggregate estimate of male disappearance can be significantly different from the estimated number of soldier deaths (from the official source) if there is systematic migration between large and small prefectures. Our estimate is 1.75 million, close to official statistics, which confirms that the change in gender ratio approximates the death rate relatively well.

Figure 1.2: Evolution of gender ratio, by cohort



Note: The figure displays the evolution of the gender ratio of three cohorts groups, those aged 20-34 in 1945 (treated cohorts), and aged 10-19 and 35-34 in 1945 (control cohorts), between 1930 and 1965. The figure indicates that the decline in the gender ratio concentrated among the young cohorts heavily recruited for the war.

-0.17, going from 1.01 to 0.84. In contrast, the gender ratio of the two other cohorts was hardly affected, as they were either young or old enough to avoid military recruitment.

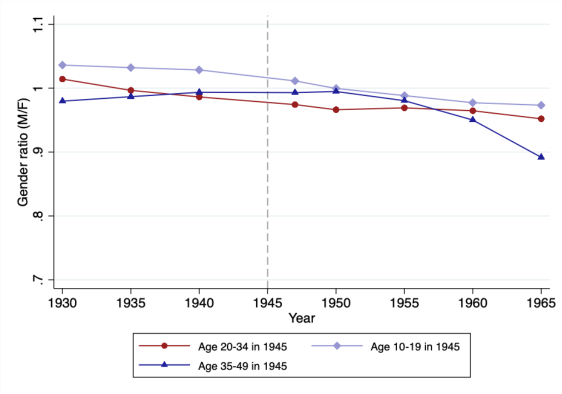
To compare the experience of Japan with the victorious countries of the Second World War, Figure 1.3 shows the evolution of the gender ratio by birth cohort for the United States and France. Note that the figure for the United States includes military overseas. The first panel indicates there is no discernible decrease in the gender ratio for any of the cohorts in the United States. In France, the gender ratio drops temporarily for the cohorts aged 20-34 in 1945, but it recovers almost to the pre-war level by 1950, as detained soldiers and evacuees return. The figure demonstrates that a large and permanent decline in the gender ratio is a particular experience of some countries that participated in the war, including Japan.[31]

We exploit a substantial geographical variation in the magnitude of gender ratio changes. This is a combined result of the hometown regiment system of the IJA and US military strategy. Figure 1.4 provides the changes in the gender ratio between 1935 and 1947 by prefecture for the three cohort groups. Among the treated cohorts who were aged 20-34 in 1945, on average across 46 prefectures, the gender ratio declined by 0.17, with a standard deviation of 0.05. The largest decline is 0.31 in Nagasaki prefecture, while the smallest

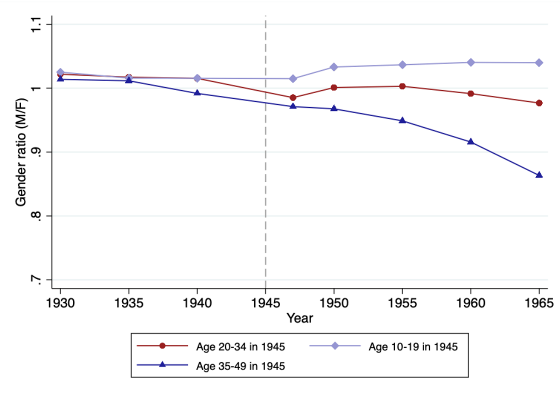
[31] Soviet Union also experienced a significant change in sex ratio, as documented by Brainerd (2017). Germany also have experienced a large number of soldeirs death, but the return migration of German expatriates was a larger demographic shock for Germany, and Germany's post-war population census does not cover the East Germany until the unification, which makes it harder to follow the precise gender ratio changes caused during the war.

Figure 1.3: The evolution of the gender ratio in victorious countries

(a) United States



(b) France



Note: This figure repeats Figure 1.2 for the United States and France, displaying the evolution of the gender ratio of three cohort groups, those aged 10-19, 20-34, and 35-34 years in 1945, calculated based on the population census of each country. The figure for the United States includes military overseas. Thus, there is no temporary decline in the gender ratio during the war. Compared to Japan, there is virtually no permanent and large decline in the gender ratio.

is 0.08 in Hyogo prefecture.[32] Note that the variation in the gender ratio change is not systematically related to factors such as urbanity. For example, Tokyo's gender ratio decline was 0.16 (27th of 46 prefectures), while it was 0.23 (7th) for Osaka. Furthermore, neighboring prefectures also had different experience; within Shikoku islands, the change in gender ratio varied from Kagawa prefecture's 0.24 (5th) to Ehime prefecture's 0.17 (21st). Thus, Figure 1.4 confirms that the gender ratio change is a unique experience of the treated cohorts. If the change in the gender ratio is entirely due to prefecture-specific factors such as migration, we can expect a similar trend for the other cohorts. The two figures (on either side) on the changes in the gender ratio of the two control groups confirm that this is not the case.

1.4 Data and Sample

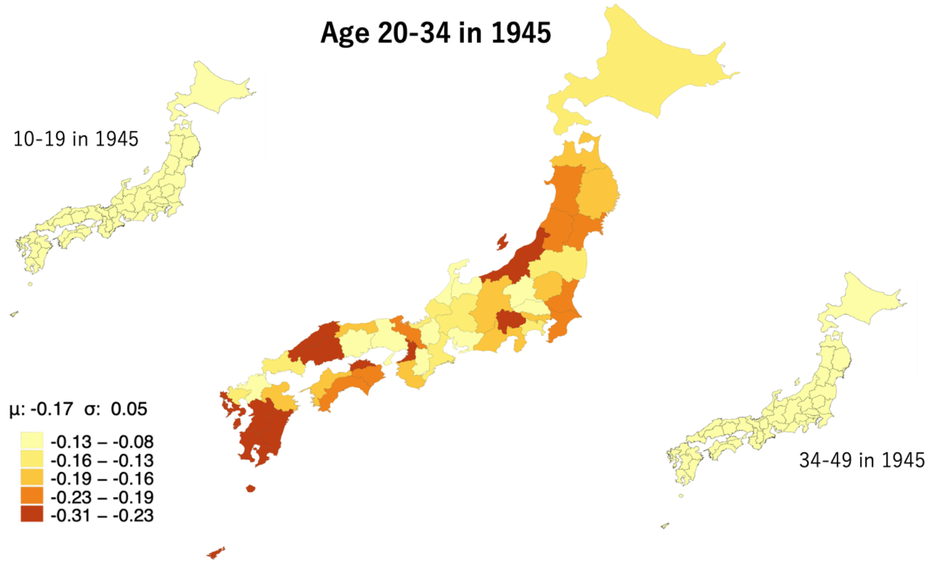
Data Our main objective is to examine the impact of human capital loss on industrial share of employment, which should be computed for each prefecture-age-industry-gender-year combination. To obtain these variables, we use published tables of population census from 1920 to 1980, available for every 5 years except for 1945, which was replaced by the concise version in 1947.[33] While population census report headcounts by prefecture-age-gender combination, employment status, and worker counts are consistently reported only from 1955, by 1-digit sector.[34] Although, prior to 1955, it is available decennially

[32] The variation does not change much when measured by percentage changes that take into account the initial differences in the gender ratio in 1935.

[33] The microdata of population census in Japan is available only for those after 1990.

[34] For the other years, they are reported by prefecture-gender or by age-gender.

Figure 1.4: Geographical variation in gender ratio changes



Note: The figure shows the variation in the gender ratio changes across 46 prefectures. The map in the center classifies prefectures by the size of 1935-1947 change in gender ratio of the treated cohorts aged 20-34 in 1945, where the threshold values are based on quantiles. The mean change in the gender ratio of the treated cohorts is 0.17, with the standard deviation of 0.05. The side figures supplement the gender ratio changes of control cohorts aged 10-19 and 34-49 in 1945, respectively, where the same threshold values are applied.

for 1920, 1930, and 1940, the definitions of employment status used in the pre-war census were different from the post-war census, making it difficult to make meaningful pre- and post-war comparisons. For this reason, we restricted the years 1955-1980 for our main regression estimation, and pre-war employment information is only used for supplementary analyses and robustness checks. We digitized this information for 46 prefectures (excluding Okinawa) and 11 age groups (10-14, 15-19,..., 60-64). We use the terms "age" and "age group" interchangeably.

We aggregate 1-digit industries into three large industry categories for simplicity: agriculture, manufacturing, and service. Agriculture combines 1-digit sectors of agriculture, forestry, and fishery. Manufacturing consists of 1-digit sectors of mining, construction, and manufacturing. Lastly, service includes all other sectors (in other words, retail and wholesale, finance, real estate and insurance, transport, communication and utility, service, government service, and others). In Appendix, we also provide the results based on 1-digit industry classifications. Employment rate and labor force participation rate by gender are calculated for each prefecture-age combination similarly.

Sample and descriptive statistics Our estimation sample is an unbalanced panel of prefecture-cohort pairs from 1955 to 1980. We follow the eight birth cohort groups that were aged 10-49 in 1945 (i.e., cohorts aged 10-14, 15-19, ..., 45-49 in 1945). We refer to each group of five cohorts simply as one "cohort". Among these birth cohorts, the three cohorts

Table 1.1: Descriptive Statistics

	Prefecture-cohort	Prefecture	
	1955-1980	1955	1980
Population (in thousand)	123 (112)	1,002 (756)	1,521 (1,428)
Gender ratio	0.90 (0.09)	0.92 (0.04)	0.94 (0.04)
Industry Share of Employment			
Agriculture	0.38 (0.16)	0.46 (0.14)	0.14 (0.07)
Manufacturing	0.24 (0.09)	0.20 (0.07)	0.33 (0.06)
Service	0.38 (0.09)	0.33 (0.07)	0.53 (0.05)
Employment and Participation			
Employment rate	0.75 (0.09)	0.76 (0.05)	0.76 (0.04)
Labor force participation rate (female)	0.58 (0.13)	0.58 (0.10)	0.58 (0.07)
Labor force participation rate (male)	0.95 (0.06)	0.93 (0.02)	0.92 (0.02)
Observations	1748	46	46

Note: The table provides summary statistics of our sample. It first summarizes sample means and standard deviations of population, gender ratio, and outcome variables with prefecture-cohort as a unit, pooled over the period of 1955-1980 (second column). The last two columns provide prefecture-level information to characterize the developmental transitions of our studied period. No population weight is applied in computing means and standard deviations.

whose age was between 20 and 34 in 1945 correspond to our "treated groups," whereas the other five cohorts aged between 10-19 and 35-49 in 1945 are termed "control groups." The cohorts leave the sample after the age 60-64.

Table 1.1 provides summary statistics of our sample. It first shows the mean and standard deviation of our estimation sample in which prefecture-cohort is the unit of observation. On average, prefecture-cohort population size is slightly above 100K, with the gender ratio of 0.90 in 1955-1980 without any population weighting. Mean employment share (without population weighting) is 0.38 for agriculture, 0.24 for manufacturing, and remaining 0.38 for service. Employment rate is 0.75 on average, while mean labor force participation rate is 0.58 and 0.95 for women and men, respectively.

To give a sense of the industrial characteristics of our studied period, Table 1.1 also provides the prefecture-level information. Average population size across prefectures grew by approximately 50% between 1955 and 1980. The period, well known as "high speed growth era," is characterized by rapid industrialization. On average, the agricultural share of employment dropped from 0.46 in 1955 to 0.14 in 1980, while that of the manufacturing share and service sector increased from 0.20 to 0.33 and 0.33 to 0.53, respectively. Therefore, the impact of the loss of young males is examined in the specific context of whether the industrialization was partially impeded by the loss of young male labor.

1.5 Empirical Strategy

1.5.1 Conceptual framework

1.5.1.1 Variable definition and baseline econometric model

We use the change in the gender ratio as a measure of a gender-specific human capital loss. For each prefecture-cohort combination, we define the gender ratio as:

$$Ratio_{cjt} = \frac{M_{cjt}}{F_{cjt}}$$

where, M_{cjt} and F_{cjt} are, respectively, the number of men and women for a cohort c, in a prefecture j, in year t. The value is less than 1 when there are fewer men than women. For each prefecture-cohort pair, we calculate the change in the gender ratio before and after the war:

$$\Delta Ratio_{cj} = Ratio_{cj,1947} - Ratio_{cj,1935}$$

where we take 1935 and 1947 as reference pre-war and post-war years.[35] When a cohort in a prefecture loses more men from the war, $\Delta Ratio_{cj}$ becomes more negative. We are interested in the effect of the change in the gender ratio on own cohort outcomes:

$$Y_{cjt} = \alpha + \beta \cdot \Delta Ratio_{cj} + \epsilon_{cjt} \tag{1.1}$$

where Y_{cjt} is a post-war outcome ($t \geq 1955$) and ϵ_{cjt} is the error term. Given the exogeneity of $\Delta Ratio_{cj}$ (between c and between j), the estimate β provides the average impact of the change in the gender ratio during the war on the economic outcome of the own cohort after the war. This is a difference-in-differences estimator that compares the outcomes of treated cohorts and control cohorts, between prefectures that lost more vs. less men in the treated cohorts. Note that Equation 1.1 is essentially a reduced form of the structural relationship where the outcomes in 1955-1980 are determined by the contemporaneous gender ratio, which is influenced by the past change in the gender ratio during the war.

1.5.1.2 Impact on industrial structure

Focusing on the employment shares of industry as outcomes, we empirically discuss the economic mechanism behind Equation 1.1. Specifically, the permanent loss of male

[35] We used 1935 as the pre-war reference year, instead of 1940, because Japan was already in the Second Sino-Japanese War from 1937 and military deployment had already commenced.

soldiers would reduce the employment share of manufacturing, which uses male labor in production relatively more intensively, hindering post-war industrialization. In theory, however, it is ambiguous whether the loss of young men would alter the industrial structure. The Rybczynski theorem from the international trade literature predicts that, based on the classical Heckscher-Ohlin model, the economy responds to an exogenous decline in endowment of one factor by decreasing the output of the sector that uses that factor relatively more intensively (Rybczynski, 1955). Nevertheless, labor market may also absorb the labor supply shock through the change in production technology or input mix *within* sector especially in the long run, theorized as directed technological change (Acemoglu, 1998, 2002) or optimized production factor choices (Beaudry and Green, 2005).

These theoretical predictions have been empirically tested. For example, Dustmann and Glitz (2015) confirmed that both mechanisms of between- and within-sector adjustments were effective in the context of immigrant influx, but found a larger role of within-sector adjustments. The decomposition method in the international trade literature led some to a similar conclusion (Blum, 2010).[36] Overall, the extent of the change in factor supply absorbed through between- and within-sector adjustments depends on parameters such as substitutability between production factors and considered economic contexts.

Applying the tentative conclusion of literature, this study hypothesizes that the gender comparative advantage plays a key role in our context, encompassing both endowment changes (total female and male population) in the trade literature, and labor supply changes (total female and male workforce) in the labor literature. Our study departs from the previous literature in some respects. First, the large and permanent supply shock was realized in a very short period, rather than gradually.[37] Second, although the role of the demand side on female labor supply was acknowledged in the related literature, the analysis in a unified empirical framework and context has not been implemented. Finally, our context offers an interesting case study in which the change in factor endowment arises from an absolute decrease in one labor input, in contrast to the large body of work that studies the increases of the educated workforce or the influx of immigrants. The economy's response might be asymmetric between gaining and losing of production input, if there is strong inertia or recovering force of the market (for example, invested fixed cost).

1.5.2 Exogeneity of the change in the gender ratio

The crucial assumption to identify the causal effect of gender-biased loss of human capital on industrial composition is that the observed changes in the gender ratio were exogenous. Further, we already discussed why the death rates of soldiers were likely to be random across prefectures from the historical and institutional perspectives. However, there are a few concerns relative to the plausibility of the exogeneity assumption. First, the variation in the gender ratio might be correlated with the pre-war industrial structure. For example,

[36] We thank Kensuke Teshima for informing us of the literature on decomposition methods.
[37] Remember that most soldiers died in the last few years of the war.

conscription used physical examination to assess health conditions and physical strength (height-weight ratio) in ranking one's ability as a soldier. If male workers in the agricultural sector were more likely to pass the examination, then prefectures that lost more men could have been relatively more agricultural-intensive even before the war. Second, since the gender ratio change is affected by age-gender-specific migration, a part of the variation may come from the pattern of cross-prefecture mobility in certain ages related to work or education, which could also be correlated with industrial and economic characteristics of the prefecture.

To assess randomness of the changes in the gender ratio, we regress the changes in the gender ratio of the treated cohorts (aged 20-24, 25-29, and 30-34 in 1945) on observed pre-war prefecture-level characteristics. We study how they are correlated with the agricultural share of employment.[38] We do not aggregate the changes in gender ratio at the prefecture level to have more statistical power. Moreover, we do not include other outcomes such as female labor force participation since they are highly correlated with agricultural employment share.

Table 1.2 shows the results. Column (1) includes only the share of agricultural employment at the prefecture level in 1930 as a regressor. There is a modest negative relationship, indicating that prefectures with an initially higher agricultural share experienced a greater decrease in the gender ratio, although the coefficient is not significant. Since our hypothesis is that male loss during the war led to a higher share in agriculture in the post-war periods, this result raises the concern that any detected negative effect of the gender ratio change on post-war industrial development merely captures the permanent differences in industrial compositions across prefectures.

Column (2) shows that adding urban prefecture dummies and population size makes the coefficient on agricultural share smaller. Similarly, column (3) further adds war-related variables related to recruitment (average height-weight ratio of examined individuals in 1939 conscription) and war damage (log of number of air raid casualties). The coefficient on the agriculture share is even more reduced. In all cases, the estimated coefficients are far from statistically significant. Importantly, the obtained R^2 across the models are small, indicating that a large share of changes in the gender ratio are not correlated with pre-war observable characteristics of prefectures.

The simple regression results above support the exogeneity of the gender ratio changes relative to industrial compositions. Of course, this is only partial evidence, and we are not able to perfectly control for prefecture-level unobservables. This motivates our estimation strategy to take a second difference among cohorts, on top of the cross-prefecture differences.

[38] Our digitized data have narrowly defined (1-digit) agriculture share and manufacturing share in employment, but no other sectors. We use agriculture in the explanatory variable because 1-digit agriculture represents more employment share in the broad agriculture sector (defined in Section 1.4) than 1-digit manufacturing does for the broad manufacturing sector. Note that the employment concept of the pre-war census is not the same as that of the post-war census, requiring caution in interpretation.

Table 1.2: $\Delta Ratio_{cj}$ and pre-war characteristics

	Change in gender ratio 1935-47 (treated cohorts)		
	(1)	(2)	(3)
Agriculture employment share in 1930	-0.059	-0.040	-0.031
	(0.050)	(0.088)	(0.109)
Urban prefecture dummy		0.039	0.035
		(0.038)	(0.042)
Log of prime age population in 1935		-0.024	-0.012
		(0.020)	(0.024)
Height-weight ratio (1939 examination)			-0.958
			(1.767)
Log of air raid casualties			-0.005
			(0.005)
Observations	138	138	138
R-squared	0.010	0.022	0.032

Note: The table provides the regression result where the gender ratio changes of the treated cohorts (aged 20-24, 25-29, and 30-34 in 1945) are regressed on the observed pre-war prefecture-level characteristics. Outcome variables have cohort-prefecture variations, but explanatory variables vary only across prefectures. Note that employment concept in the pre-war census is not directly comparable to the post-war census and interpretation requires caution. Urban prefecture dummy takes 1 for Tokyo, Osaka, Hokkaido, Hyogo, Aichi, Fukuoka, Kanagawa, and Kyoto and 0 otherwise. Standard errors are in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Our regression specification includes prefecture fixed effects, which controls for differences in pre-war industry characteristics and prefecture-specific unobservables, such as gender norms.

1.5.3 Estimation Strategy

We identify the effect of the gender ratio change on industrial composition and labor market outcomes using the variations among prefectures and cohorts. Our baseline specification takes the following form:

$$\begin{aligned} Y_{cjt} = \alpha + \beta \cdot \Delta Ratio_{cj} + \gamma_1 X_{cjt} + \gamma_2 X_{cj} \\ + (Cohort_c \times Time_t) + (Pref_j \times Time_t) + \epsilon_{cjt} \end{aligned} \tag{1.2}$$

where Y_{cjt} is the outcome of interest of cohort c, in a prefecture j, in year t; $\Delta Ratio_{cj}$ is the gender ratio change from 1935 to 1947; X_{cjt} represents contemporaneous cohort-prefecture controls; X_{cj} is time-invariant cohort-prefecture controls; $Cohort_c \times Time_t$ and $Pref_j \times Time_t$ represent, respectively, the cohort-year fixed effects and prefecture-year fixed effects; ϵ_{cjt} is the error term. The coefficient of interest is β. As explained earlier, it is a difference-in-differences estimator that captures the difference in average outcomes over 1955-1980 between the treated cohorts and control cohorts and among prefectures that differ in the size of the gender ratio change of the treated cohorts.

With respect to X_{cjt}, we include only log of population (or log of number of workers where appropriate). We do not include contemporaneous gender ratio or migration because we consider these factors as mechanisms that magnify or mitigate the effect of human capital loss in the long run. For time-constant prefecture-cohort controls X_{cj}, we add the gender ratio and the log of population in 1935. Moreover, to distinguish the effect of male soldier loss (gender-specific) from general population loss (gender-neutral), such as from the air raids, we also include the change in log of population between 1935 and 1947.

Prefecture-time fixed effects deal with differential trends in outcomes across prefectures or time-specific shocks that are specific to a prefecture. They also control for prefecture-specific time-constant factors, such as location specificity, pre-war industrial compositions, and other war damages (for example, capital destruction), as well as time-specific factors such as economic conditions in a specific year. Lastly, with cohort-time fixed effects, the specification controls for factors such as the life cycle. It also controls for other cohort-common factors that are fixed over time.

Under the specification 1.2, the only source of potential endogeneity is the correlation of $\Delta Ratio_{cj}$ with *prefecture-cohort* unobservables. Among others, most concerning is cohort-specific (or age-specific) cross-prefecture migration between 1935 and 1947. This can bias our results if it is correlated with pre-war local industrial composition of employment, when it is different across cohorts (or across ages). For example, young people may have worked temporarily in factories located in a neighboring prefecture or went to a school in larger prefectures just before the war. If such migration is gender-biased, $\Delta Ratio_{cj}$ could correlate with the error term.[39] We deal with this issue in two ways. First, we control for the initial gender ratio in 1935 in the regression to capture cohort-prefecture specific factors influencing the cross-prefecture migration. Second, we perform two robustness checks: one test uses 1930 outcomes as a placebo, and the other uses age-fixed gender ratio change as an alternative measure of $\Delta Ratio_{cj}$ that are robust to prefecture-age specific migration. These robustness checks confirm that our main results are unlikely to be driven by the endogeneity of the gender ratio changes with respect to outcomes.

[39] The direction of the bias is not clear a priori. Suppose that the gender ratio change of the treated cohorts is more negative for agricultural intensive sector because of temporary migration of young men. However, since we consider differences among cohorts, the direction of the bias depends on how the difference between treated and control cohorts in agricultural employment share (or industrial employment share in general) in the pre-war period in agricultural intensive prefecture compared to the corresponding difference in the manufacturing intensive sector – it is difficult to say whether this difference in the differences is positive or negative.

Table 1.3: Effect of the gender ratio change on industry composition and employment

	Industry Employment Share			Employment and Participation		
	Manufacturing	Agriculture	Service	Employment rate	Female LFPR	Male LFPR
	(1)	(2)	(3)	(4)	(5)	(6)
Gender ratio change 1935-47	0.052*	-0.019	-0.032	-0.057	-0.159***	-0.013
	(0.027)	(0.033)	(0.021)	(0.037)	(0.061)	(0.053)
ln(pop) change 1935-47	-0.029**	-0.001	0.030**	0.083***	0.146***	0.071**
	(0.015)	(0.019)	(0.013)	(0.024)	(0.042)	(0.028)
Gender ratio in 1935	0.091***	-0.085**	-0.007	-0.042	-0.142**	-0.043
	(0.030)	(0.034)	(0.023)	(0.040)	(0.065)	(0.052)
ln(pop) in 1935	-0.032**	-0.028	0.060***	0.101***	0.184***	0.077***
	(0.015)	(0.020)	(0.012)	(0.022)	(0.043)	(0.023)
ln(worker)	-0.037***	0.133***	-0.096***			
	(0.011)	(0.015)	(0.008)			
ln(pop)				0.037**	-0.025	0.018
				(0.016)	(0.032)	(0.019)
Mean outcome 1955-1980	0.241	0.382	0.377	0.747	0.583	0.948
Prefecture-year FE	Yes	Yes	Yes	Yes	Yes	Yes
Cohort-year FE	Yes	Yes	Yes	Yes	Yes	Yes
R-squared	0.924	0.948	0.922	0.610	0.671	0.320
Observations	1748	1748	1748	1748	1748	1748

Note: The table provides our main results on the effect of the gender-specific human capital loss on post-war economic outcomes, corresponding to the estimated coefficients on the gender ratio change 1935-1947 (first row). Columns (1)-(3) suggests that the loss of men led to lower employment share of manufacturing, and higher share of agriculture and service, although imprecisely estimated. Columns (4)-(6) explore the effect on labor force participation and employment rate. The results show that the female labor force participation significantly increased. Standard errors are clustered at prefecture-year level. Standard errors are in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

1.6 Results

1.6.1 Industrial composition

Table 1.3 shows the estimated impact of the gender-specific human capital loss on industrial composition and employment outcomes identified in the specification of Equation 1.2. Since the gender ratio change is negative for a larger loss of men, the coefficient signs must be transposed to interpret the effect of the gender specific human capital loss.

The first three columns study industrial employment shares. Column (1) shows that the decrease in the gender ratio is associated with a lower share of employment in the manufacturing sector between 1955 and 1980 and is statistically significant at 10% level. On the contrary, columns (2) and (3) indicate that the employment share of agriculture and service sectors increased, although the estimates are imprecise individually.[40] The signs of the coefficients are consistent with the hypothesis that the permanent loss of young men led to a lower employment share of manufacturing sector, that is, lower level of industrial development in the post-war period.

However, the magnitude of the effects is modest: the decline in the gender ratio by 0.1,

[40] Notice that the three coefficients add up to 0 as they represent shares.

corresponding to approximately 10% of men disappearing from the local labor market for a fixed number of women, is associated with a 0.52-point decrease in manufacturing employment share, and a corresponding increase in employment share in agriculture and service by 0.19 and 0.32 points, respectively. During 1955-1980, the mean share of employment of the prefecture cohort is 0.38 in manufacturing, 0.24 in agriculture, and 0.38 in service (see Table 1.1). The largest decline in the gender ratio at the cohort-prefecture level was around 0.37, indicating that the decline in the gender ratio does not account for much share of the cross-cohort-prefecture variation in the sector composition of employment.[41]

1.6.2 Labor force participation and employment

Column (5) in Table 1.3 indicates that the skewed gender ratio has led to a higher female labor force participation rate, consistent with the previous literature. Labor force participation rate is 1.6-point higher for women from a cohort in a prefecture that experienced 0.1 decline in the gender ratio, relative to those whose gender ratio was unaltered. This effect is much smaller than estimates from the previous literature that also exploited a permanent loss of men, finding an approximately 3-4 percentage points increase in female labor force participation rate in response to a gender ratio difference of 0.1 (Boehnke and Gay, 2022; Cardoso and Morin, 2018).

There are several potential explanations for a smaller effect on women's labor force participation. First, limited labor supply was possibly due to child care duties in a high-fertility, post-war period — total fertility rate exceeded 4 before 1949, which gradually decreased until it stabilized at slightly above 2 during late 1950s. Since women in the treated cohorts were aged 20-34 in 1945, our sample period overlaps the stage of the traditional life cycle of women when child care duties could prevent them from supplying labor. This is consistent with Ogasawara and Komura (2022) that showed a significant association between imbalanced sex ratio and higher fertility in the post-war Japan. Second reason may relate to the conservative gender norm. Although the new constitution enacted in 1947 guaranteed the political, economic, or social equality between men and women, and the society was moving toward a progressive gender norm, it is unclear if the institutional barriers and normative biases were dissolved in practice, especially relating to the economic participation.[42] The smaller effects on the female labor force participation might be due to the econometric specification that takes cross-cohort differences. Income shock is one of the supply-side mechanisms of increased female labor force supply in response to the permanent loss of men (Boehnke and Gay, 2022). Possibly, the labor supply decision

[41] For example, a simple computation indicates that the most treated prefecture-cohort had 1.9-point lower manufacturing employment share compared to non-treatment. This is only one-fifth of the standard deviation in manufacturing employment share.

[42] Interestingly, Okuyama (2021) finds that the GHQ's radio program promoting Japanese women's social, political, and economic rights increased women's political participation in the post-war period, but not labor market participation in 1950. This is consistent with the existence of various barriers for women's economic participation.

was undertaken within household, whereby other members of the household supplied labor instead of the women in the treated cohorts. In this case, our diff-in-diff estimate attenuated because of spillover effect across cohorts.[43] This interpretation is consistent with the supplementary results in the following section, where we find a larger effect on female labor force participation in the specification that controls for the change in the gender ratio of the other cohorts, and the change in the gender ratio of the other cohorts in the same prefecture increase labor force participation of the own cohort.[44]

In contrast, there is no significant association with the male labor force participation rate as shown in column (6), and the estimated coefficient is very small. This is consistent with the fact that the male labor force participation rate has always been high. Column (4) indicates that the employment rate increased when the gender ratio reduced, possibly reflecting an improvement in labor market tightness due to the scarcity of male labor, although the coefficient is not statistically significant.

1.6.3 Change in gender mix within sector

The change in the composition of human capital can be absorbed by the change in gender mix within sector. Faced with the shortage of male labor in the local labor market, employers may adjust their production technology or input mix to accommodate female labor. Even without the change in technology, hiring female labor may intensify if there is sufficiently high substitutability between two genders in each sector. To test these hypotheses, we study the impact of the gender ratio change on female share in each sector, using the same specification in Equation 1.2.

Table 1.4 summarizes the results. In each sector, the female share is higher when there is a larger loss of males due to the war. For example, female share in manufacturing is 0.95 points higher in cohort-prefecture where the gender ratio declined by 0.1 during the war. Unfortunately, it is difficult to compare the magnitude of the effect across sectors because we do not have a benchmark female shares in each sector in the pre-war period based on the same employment definition. Nevertheless, the coefficients suggest that the gender ratio change explains large share of female share variation within-sector than the variation in sector share of employment studied in Table 1.3.[45]

[43] The post-war period had a relatively high share of multi-generation household (over one-third of all households in 1955, although it decreased to one-fifth by 1980).

[44] Labor demand side mechanisms would rather facilitate female labor force participation since the industry moved toward more female intensive sectors and, as discussed later, all sectors increased female share in their labor input. Hence, the change in the demand side led to an increased estimated effect on the female labor force participation. In addition, military pension for bereaved family does not explain the lower effect because it did not exist until 1952 and was not generous enough to negatively affect labor supply decisions on the extensive margin.

[45] For example, for manufacturing sector, a reduction in the gender ratio by 0.37 leads to a higher female share by 3.5 points, two fifths of the standard deviation of the female share in manufacturing in our sample. This is almost twice what the change in gender ratios explains in terms of variation in the manufacturing share of employment (one-fifth of the standard deviation) in Table 1.3.

Table 1.4: Effect of the gender ratio change on within-sector gender composition

	Female Share in Employment		
	Manufacturing (1)	Agriculture (2)	Service (3)
Gender ratio change 1935-47	-0.095*	-0.130***	-0.050**
	(0.050)	(0.025)	(0.022)
ln(pop) change 1935-47	0.014	0.098***	0.065***
	(0.030)	(0.019)	(0.015)
Gender ratio in 1935	-0.077	-0.129***	-0.079***
	(0.052)	(0.028)	(0.025)
ln(pop) in 1935	0.062**	0.099***	0.073***
	(0.030)	(0.018)	(0.015)
ln(worker)	0.024*	0.019*	0.006
	(0.014)	(0.011)	(0.009)
Mean outcome 1955-1980	0.261	0.524	0.369
Prefecture-year FE	Yes	Yes	Yes
Cohort-year FE	Yes	Yes	Yes
R-squared	0.909	0.946	0.949
Observations	1748	1748	1748

Note: The table provides the effect of the gender-specific human capital loss on female share within each sector. Negative and significant coefficients on the first row indicates that all sectors used female worker more intensively when they lost young male working force. Standard errors are clustered at prefecture-year level. Standard errors are in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Overall, the results provide new empirical evidence on the demand-side factors that facilitate the integration of women into the labor market when there is a negative shock to the local availability of male labor. In the case of Japan, due to the loss of men during the war, the economy shifted toward sectors that used relatively more female labor (agriculture and service) than the male-intensive sector (manufacturing). The size of the effects are not large, however, and statistically insignificant at 5%.[46] Meanwhile, within all sectors, the presence of women increased. These changes in the demand structure should have increased the economic return of entering the labor market for women. However, the estimated increase in female labor force participation rate was rather small, indicating the existence of obstacles and counteracting factors that prevented women from supplying labor.

1.6.4 Long-run effects

It is unclear whether the impact of gender-specific human capital loss is permanent or temporary. On the one hand, the initial "shock" can determine the path of the local industrial development and, therefore, the effect of the male human capital loss persists,

[46] Although the average effects are statistically insignificant, as shown in the next section, they were significant in earlier years for the agriculture and manufacturing sector, which eventually disappears in the long run.

or even propagates, over time. However, the shock can also be transitory and neutralized in the long run through, for example, cross-prefecture migrations and entry of new birth cohorts into the local labor market.

We study these alternative hypotheses by looking at the time-varying effects of the change in gender ratio in the following specification:

$$\begin{aligned} Y_{cjt} = \alpha + \beta \cdot (\Delta Ratio_{cj} \times I_t) + \gamma_1 X_{cjt} + \gamma_2 X_{cj} \\ + (Cohort_c \times Time_t) + (Pref_j \times Time_t) + \epsilon_{cjt} \end{aligned} \tag{1.3}$$

where I_t is an indicator for time periods.

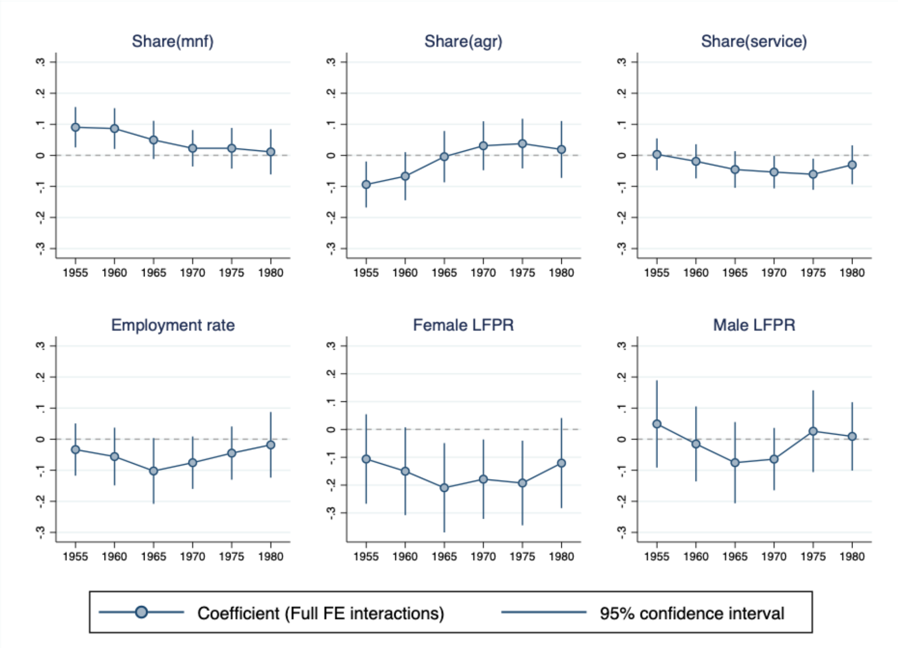
Figure 1.5 shows the estimated time-varying coefficients and their confidence interval for different outcomes. The top panels indicate that it was only until 1960 that the decline in the gender ratio had a negative impact on the employment share of the manufacturing sector, resulting in a higher share of the agriculture sector. However, the effects gradually disappear afterward. These results suggest that the loss of male workers hampered industrialization initially, but this negative impact attenuated subsequently in the catching up process.

The bottom middle panel shows the time-varying effects on female labor force participation. The impact of permanent male loss had always been an increased female labor force participation that lasted after 30 years since the end of the Second World War. The coefficient becomes large enough to be significant only after 1965. This pattern is consistent with the life cycle: the treated cohorts were aged 20-34 in 1945, so women from these cohorts were constrained by housework and child care early in the post-war period.

Figure 1.6 shows the time-varying effects of the gender ratio on within-sector female shares. In all three sectors, the increase in female share has been a persistent phenomenon, although it is not always significant. Focusing on manufacturing and agriculture sectors, this is in stark contrast to the results found in Figure 1.5, that is, even though the impact of the gender ratio on the industrial structure has disappeared in the long run, more intensive use of female labor within sectors has remained in the long run.

The controversy remains in the United States on how the Second World War transformed women's economic life: specifically, if the temporary absence of men during the war have had a positive and persistent impact on women's labor force even after the war, and what are the economic areas (in terms of occupations and industries) in which female representation changed. We find that, in the case of Japan where many soldiers did not return, the loss of men increased female labor force participation permanently in the post-war period. Moreover, in the short-run, the economy shifted toward the sectors that used female labor more intensively, as seen in the tentative increase in agriculture sector and decline in manufacturing sector. However, in all sectors, female representation

Figure 1.5: Long-run effect on industrial share and employment outcomes



Note: The figure provides the long-term effects of the gender ratio changes on industry share of employment (top three panels) and employment and labor force participation (bottom three panels). These are estimated using the interaction of year dummies with the change in gender ratio in the specification of Equation 3, which include full set of fixed effects. For each outcome, we provide the coefficients and 95% confidence interval. The figure shows that the declined gender ratio led to lower (higher) employment share of manufacturing (agriculture) up to 1960, after which the effect decays to null. The effect on service sector is gradual. While somewhat imprecisely estimated, the effect on female labor force participation is persistent over time.

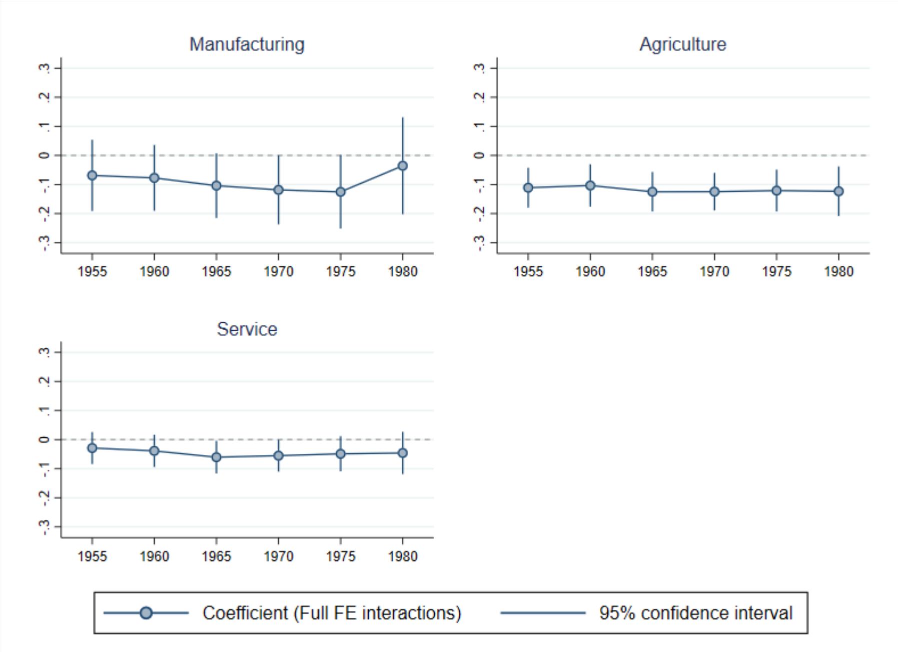
has increased and the effect persisted in the long run, even during the era of massive industrialization and high-speed economic growth.

1.7 Discussion

1.7.1 Prefecture-age-specific effect does not matter

Our identification assumption is that the change in the gender ratio is not systematically correlated with prefecture-cohort specific unobservables, once added controls and fixed effects. One major threat is the prefecture-specific age effect. People migrate across prefectures, and such migration can be prefecture-age specific and gender-biased. For example, young men in rural prefectures might temporarily relocate to urban prefectures for jobs or schools. In this case, some of the variation in the change in the gender ratio between 1935 and 1947 could be endogenous to prefecture-age specific migration.

Figure 1.6: Long-run effects on within-sector female share



Note: The figure provides the long-run effects of the gender ratio changes on female share in manufacturing, agriculture, and service sector. These are estimated using the interaction of year dummies with the change in gender ratio in the specification of Equation 3, which include full set of fixed effect. For each outcome, we provide the coefficients and 95% confidence interval. The figure shows that increased female share was relatively a persistent phenomenon.

1.7.1.1 Placebo test

To see whether such prefecture-age effects bias our estimates, we run a placebo test using the data from 1930, the closest pre-war census for which labor force participation is available at prefecture-cohort level.[47] We run the same regression specified in Equation 1.2 on the outcomes in 1930 of those whose age corresponds to the age in 1945 of our sample cohorts who were aged 20-49 in 1945.

Table 1.5 shows that there is no statistically significant association between the change in gender ratio between 1935 and 1947, and labor force participation in 1930. While the signs of the coefficient for female labor force participation is negative, the coefficient is not as large as estimated in our main regression results. Although this placebo test remains only suggestive, the results are consistent with the exogeneity assumption of the gender ratio changes.

[47] In the pre-war census, definition of employment was different and it was a closer concept to the post-war labor force participation. We use the pre-war employment rate as the labor force participation rate in these placebo tests.

Table 1.5: Results of placebo test using 1930 outcomes

	Outcome in 1930		
	Total LFPR	Female LFPR	Male LFPR
	(1)	(2)	(3)
Gender ratio change 1935-47	-0.023	-0.076	-0.014
	(0.052)	(0.108)	(0.025)
ln(pop) change 1935-47	0.083***	0.244***	-0.063
	(0.027)	(0.064)	(0.039)
Gender ratio in 1935	-0.005	0.003	-0.022
	(0.051)	(0.108)	(0.021)
ln(pop) in 1935	0.048**	0.065	-0.000
	(0.023)	(0.051)	(0.018)
ln(pop)	0.009	-0.012	0.024
	(0.014)	(0.026)	(0.016)
Prefecture FE	Yes	Yes	Yes
Cohort FE	Yes	Yes	Yes
Year fixed FE	No	No	No
R-squared	0.963	0.966	0.888
Observations	276	276	276

Note:The table shows the results of placebo test where the gender ratio changes are regressed on some of the labor force participation rate in 1930 of the same age groups. Standard errors are clustered at prefecture level. Standard errors are in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

1.7.1.2 Alternative definition of gender ratio

As discussed above, it is possible that $\Delta Ratio_{cj}$ changed due to prefecture-age specific migration, which could correlate with prefecture cohort unobservables, because we defined $\Delta Ratio_{cj}$ as the change in the gender ratio from 1935 to 1947 of the same *cohort*, and a weakness of this definition is that the age of the cohort at the reference year (1935) matters. In addition to the placebo test, we performed robustness checks by introducing an alternative measure of the gender ratio change robust to the prefecture-age specific unobservables. Instead of comparing the gender ratio in 1947 to that in 1935 of the same cohort, we compared it with the gender ratio in 1935 of the same *age group*. Formally, for each cohort-prefecture pair, we construct:

$$\Delta Ratio^{al}_{cj} = Ratio_{a(c)j,1947} - Ratio_{a(c')j,1935}$$

where $a(c)$ refers to the age group of a cohort c and $a(c')$ is the age group of another cohort c'. The first term of the RHS is the same as before, that is, $Ratio_{a(c)j,1947}$ is the gender ratio of a cohort c in a prefecture j in 1947. The second term, however, is now the gender ratio of another cohort c' whose age group in 1935 is the same as that of the cohort c in

Table 1.6: Comparison of coefficients with the alternative gender ratio change

	Industry Employment Share			Employment and Participation		
	Manufacturing (1)	Agriculture (2)	Service (3)	Employment rate (4)	Female LFPR (5)	Male LFPR (6)
Coefficients from the model with:						
Change in gender ratio (own-cohort)	0.052*	-0.019	-0.032	-0.057	-0.159***	-0.013
	(0.027)	(0.033)	(0.021)	(0.037)	(0.061)	(0.053)
Change in gender ratio (age-fixed)	0.088***	-0.088***	-0.000	-0.035	-0.079*	-0.020
	(0.016)	(0.019)	(0.010)	(0.23)	(0.042)	(0.024)

Note:The table provides the estimated effects of the gender-specific human capital loss when the alternative measure of the gender ratio change is used, which is robust to prefecture-age specific unobservable. The first row provides the coefficients from our main results using the gender ratio change fixing the cohort. In the second row, we provide the estimated coefficients on the alternative measure of the gender ratio change that fix age rather than cohort. Specification is the same for both (Equation 1.2). Standard errors are clustered at prefecture-year level. Standard errors are in parentheses. $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

1947.[48] Since this alternative measure fixes age rather than cohort, it is not contaminated by prefecture-age specific factors affecting the gender ratio, based on the assumption that these factors were constant between 1935 and 1947.

Table 1.6compares the coefficients of the two alternative gender ratios estimated from the base specification with full fixed effects. In general, the signs and qualitative implications are similar. Using the alternative gender ratio change, the effect on the manufacturing share in employment and agriculture is large and statistically significant at 1% level, while the coefficient on the service sector becomes null.[49] With respect to employment outcomes, the effect of female labor force participation becomes smaller, but the coefficient is still negative and remains significant at 10% level.

1.7.2 Control for other cohort

In our main regression framework, we assume that $\Delta Ratio_{cj}$ independently affects own cohort outcome. However, through indirect mechanisms such as substitution, the gender ratio of other cohorts may affect own cohort outcome. If there are correlations between the gender ratio change across cohorts within a prefecture, this might bias the estimate of the own cohort effect Furthermore, cross-cohort effect provides some insights in itself. We control for $\Delta Ratio_{c'j}$ (change in gender ratio of other combined cohorts) in the base specification:

[48] For example, for a cohort who was aged 30-34 in 1945, this alternative gender ratio change was calculated by the difference between the gender ratio in 1947 when they were aged 32-36, and the gender ratio in 1935 of the other cohort aged 32-36 in 1935. Previously, the comparison was made against the gender ratio of the own cohort in 1935, when this cohort was at age 20-24.

[49] Given that some young male workers (typically sons other than the eldest who did not take over the family agriculture business) moved to neighboring prefectures to work temporarily in factories located in neighboring prefectures, it seems important to control for the age-prefecture effect.

$$
\begin{aligned}
Y_{cjt} = \alpha + \beta \cdot \Delta Ratio_{cj} + \theta \cdot \Delta Ratio_{c'j} + \gamma_1 X_{cjt} + \gamma_2 X_{cj} \\
+ Cohort_c + Pref_j + Time_t \\
+ (Cohort_c \times Time_t) + (Pref_j \times Time_t) + \epsilon_{cjt}
\end{aligned}
\tag{1.4}
$$

with $\Delta Ratio_{c'j}$ representing the gender ratio change of all other cohorts c' in the same prefecture.

Table 1.7 shows the results. The coefficients of the own-cohort gender ratio change have same signs as the main results, but the magnitude is relatively large: the 0.1 decline in the gender ratio led a prefecture cohort to have a 5.2-point lower share in manufacturing employment and 3.8-point and 1.4-point higher for the agriculture and service sector, respectively. The coefficient on female labor force participation are also higher.[50]

The increase in own-cohort effect is linked to the reduced coefficient of log of population in 1935 (which declined from -0.034 to -0.009 in case of manufacturing). For its average value of 11.7, it translates to a 30 ppt difference in manufacturing share, while it is 26 ppt for the other cohorts' gender ratio, whose mean value is -0.08. Overall, controlling for the correlation across cohort does not alter the sign of the coefficient, while the magnitude implies that the own-cohort was possibly even larger than originally estimated.

1.7.3 Attention to prefecture-level variation

Our main specification uses industry employment share at cohort-prefecture level to capitalize on the cohort-prefecture variation in the change in the gender ratio. To interpret the estimated coefficient as the effect on the local industrial structure, we implicitly assumed cohort-level segmentation of the labor market. However, if the labor market is not perfectly segmented by cohort and there are between-cohort substitutions, our estimate will be attenuated owing to cross-cohort spillovers within prefecture; in this case, our main specification only provides the lower bound. Here, we estimate the effect of young male loss under the assumption that the local industrial structure is determined by the total labor stock at the prefecture level. This motivates a specification that exploits only the cross-prefecture variation in the gender ratio change.

Specifically, we use the gender ratio change of the treated cohorts (those aged 20-34 in 1945), which varies only across prefectures:

[50] The effect of other cohorts is even larger because of a much smaller variation due to the consolidation of different cohorts: while mean and standard deviation of the own-cohort change in the gender ratio is -0.083 and 0.093, they are -0.083 and 0.029, respectively, for the other-cohort's change. Moreover, part of the variation comes from the untreated cohorts (as we have three treated and five untreated cohorts), whose gender ratio change might be correlated with the unobservables.

Table 1.7: Results of the regression controlling for the rest of cohorts

	Industry Employment Share			Employment and Participation		
	Manufacturing	Agriculture	Service	Employment rate	Female LFPR	Male LFPR
	(1)	(2)	(3)	(4)	(5)	(6)
Gender ratio change 1935-47	0.521***	-0.377***	-0.144**	-0.179*	-0.296*	-0.063
	(0.056)	(0.079)	(0.056)	(0.092)	(0.161)	(0.116)
Gender ratio change 1935-47 (rest)	2.805***	-2.135***	-0.669**	-0.737	-0.819	-0.304
	(0.278)	(0.407)	(0.269)	(0.534)	(0.915)	(0.613)
ln(pop) change 1935-47	-0.004	-0.021	0.024*	0.076***	0.138***	0.068**
	(0.014)	(0.019)	(0.013)	(0.023)	(0.043)	(0.030)
Gender ratio in 1935	0.147***	-0.127***	-0.020	-0.056	-0.158**	-0.049
	(0.031)	(0.036)	(0.026)	(0.039)	(0.064)	(0.053)
ln(pop) in 1935	-0.009	-0.046**	0.054***	0.095***	0.177***	0.075***
	(0.014)	(0.019)	(0.013)	(0.022)	(0.043)	(0.025)
ln(worker)	-0.039***	0.135***	-0.096***			
	(0.010)	(0.014)	(0.008)			
ln(pop)				0.038**	-0.023	0.018
				(0.016)	(0.032)	(0.019)
3-way fixed effect	Yes	Yes	Yes	Yes	Yes	Yes
Prefecture-year FE	Yes	Yes	Yes	Yes	Yes	Yes
Cohort-year FE	Yes	Yes	Yes	Yes	Yes	Yes
R-squared	0.971	0.977	0.965	0.913	0.900	0.632
Observations	1748	1748	1748	1748	1748	1748

Note: The table provides the results of the specification where the gender ratio change of the rest of the cohorts is included as a control. The gender ratio change of the rest of the cohort is calculated by the gender ratio change of all other cohorts combined in the same prefecture excluding own cohort. The estimated coefficient of the change in the gender ratio on own cohort (first row) is much larger than before, suggesting that our previous estimates attenuated due to cross-cohort spillovers. The estimated effect of the gender ratio change of the other cohorts is large because the size of the gender ratio change is much smaller compared to own-cohort gender ratio changes. All standard errors are clustered at prefecture-year level. Standard errors are in parentheses. $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

$$\begin{aligned} Y_{cjt} = \alpha + \beta \cdot \Delta Ratio_j + \gamma_1 X_{cjt} + \gamma_2 X_{cj} + \delta Z_j \\ + Cohort_c + Time_t \\ + (Cohort_c \times Time_t) + \epsilon_{jct} \end{aligned} \tag{1.5}$$

where $\Delta Ratio_j$ is the change in the gender ratio of those aged 20-34 years in each prefecture (and therefore has only a subscript j). In this specification, we cannot include prefecture fixed effects. Our approach to minimize the endogeneity of $\Delta Ratio_j$ is to add appropriate prefecture-level controls, represented by Z_j. Since our treatment variable is now at the prefecture level, we also include cohorts who were less than 9 years old or not born in 1945 in our estimation sample.

Regarding prefecture controls Z_j, we include variables related to military recruitment (average height-weight ratio of the male examined for conscription in 1939), other mechanisms of the war effect (number of bomb casualties per capita, number of destroyed buildings per capita, and prefecture-level log of population change between 1935 and 1947), and urbanity (large prefectures—Tokyo, Kanagawa, Osaka, Aichi, Kyoto, and Hyogo).

Moreover, we add the prefecture-level outcome in 1930 (of working population) to control for the permanent prefecture characteristics. We include time-varying prefecture controls, such as age structure (share of those aged below or 19, 20s, 30s, 40s, 50s, and 60 or above, respectively, and contemporaneous log of population or workers). We maintain cohort-prefecture controls such as pre-war controls population and gender ratio in 1935.

Table 1.8 shows the results. The coefficients on the sector employment share have expected signs and are statistically significant. They are larger than the previous estimates from the regressions that exploit both cross-prefecture and cross-cohort variations (in Table 1.3). For example, the manufacturing share in employment is 2.6-point higher when the gender ratio declines by 0.1, larger than the effect of 0.5 point from the previous model. This is consistent with the idea that our main specification underestimated the effect of the gender ratio changes due to cross-cohort substitution within the prefecture.

However, the gender ratio changes (of the treated cohorts) at the prefecture level may still correlate with prefecture-specific unobservables because we cannot include prefecture fixed effects in this specification. This concern is reflected in the (wrong) signs of the coefficients on the war-related variables — while we expect negative signs because air strikes and building destruction hinder the development of manufacturing, they enter with positive signs. These signs may rather reflect the endogeneity that bombing was strategic, targeting prefectures with high manufacturing intensities.

If such endogeneity is present, in particular if the gender ratio declined more (less) in prefectures that were already intensive in agriculture and service (manufacturing), the estimated effects of the gender ratio on the employment share in Table 1.8 provides only an upper bound (in absolute terms). Since both the lower-bound estimates (from Table 1.3) and the upper-bound estimates (from Table 1.8) have the same signs, our overall conclusion remains unchanged that the loss of male soldiers negatively affected the industrial development of the prefecture in the post-war period.

On the other hand, the effect on employment and labor force participation is inconclusive. The coefficients in Table 1.8 are drastically different (both in terms of signs and magnitudes) from the regression results using cohort-prefecture variations. Notably, the gender ratio decline is associated with *lower* female labor force participation rate.

The direction of the endogeneity bias deserves some discussion. it is opposite: the bias is downward (more negative) for agricultural employment share, whereas it is upward (positive) for female labor force participation. One interpretation is that the direction of bias is different before and after the war. For example, there might be an unobservable related to potential frontier or productivity of the areas, which also correlates with the gender ratio changes. This unobservable is positively correlated with the agriculture sector share (as the largest source of economic income) and predicts higher female labor force participation (due to higher gains from labor market participation) before the war. However,

Table 1.8: Prefecture-level treatment, with pre-war outcome control

	Industry Employment Share			Employment and Participation		
	Manufacturing	Agriculture	Service	Employment rate	Female LFPR	Male LFPR
	(1)	(2)	(3)	(4)	(5)	(6)
Gender ratio change (treated cohorts)	0.258***	-0.179***	-0.085**	0.134***	0.192***	0.033**
	(0.055)	(0.050)	(0.036)	(0.042)	(0.068)	(0.016)
Bomb casualty per capita	0.524*	-0.485*	-0.082	0.630***	1.080***	0.095
	(0.297)	(0.276)	(0.163)	(0.188)	(0.283)	(0.113)
Destroyed buildings per capita	0.190*	-0.217	-0.071	-0.049	-0.119	0.013
	(0.110)	(0.152)	(0.104)	(0.098)	(0.160)	(0.043)
Height weight ratio	-3.999***	4.200***	0.048	0.657*	1.308**	0.227
	(0.747)	(0.709)	(0.374)	(0.367)	(0.599)	(0.197)
ln(pop) change 1935-47	0.042	-0.221***	0.193***	-0.122***	-0.263***	-0.011
	(0.054)	(0.053)	(0.034)	(0.043)	(0.071)	(0.017)
Gender ratio in 1935	0.090	-0.270***	0.072	-0.182***	-0.235***	-0.017
	(0.063)	(0.068)	(0.049)	(0.050)	(0.082)	(0.017)
ln(pop) in 1935	0.011	-0.163***	0.100***	-0.019	-0.078	0.013
	(0.028)	(0.041)	(0.028)	(0.040)	(0.065)	(0.013)
ln(worker)	-0.016	0.159***	-0.088***			
	(0.028)	(0.041)	(0.028)			
ln(pop)				0.005	0.052	-0.014
				(0.040)	(0.065)	(0.013)
Cohort FE and time FE	Yes	Yes	Yes	Yes	Yes	Yes
Prefecture FE	No	No	No	No	No	No
Cohorot-time FE interaction	Yes	Yes	Yes	Yes	Yes	Yes
Prefecture-level controls	Yes	Yes	Yes	Yes	Yes	Yes
Pre-war prefecture-level controls	Yes	Yes	Yes	Yes	Yes	Yes
Pre-war outcome (1930 value)	Yes	Yes	Yes	Yes	Yes	Yes
R-squared	0.834	0.933	0.856	0.787	0.771	0.624
Observations	2484	2484	2484	2484	2484	2484

Note: The table provides the regression results where we only exploit cross-prefecture differences in the gender ratio change (of treated cohorts who were aged 20-34 in 1945). While maintaining the same specification as 1.2, prefecture FE and prefecture-year FE cannot be included. To minimize the endogeneity of the gender ratio change, we include various prefecture-level controls. In columns (1)-(3), the estimated effects on the industrial compositions have expected signs, while the magnitude is larger than our main results, because this specification does not take within-prefecture cross-cohort differences. Column (4) shows a surprising negative effect of male worker loss on female labor force participation when using cross-prefecture variations. Standard errors are clustered at prefecture-year level. Standard errors are in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

after the war, the correlation reverses for the agricultural sector share as manufacturing becomes the most productive sector. In contrast, the relationship remains unchanged for the participation as the economic motive still give positive incentives for labor market participation. Testing this interpretation requires the measure of potential frontier or productivity of each prefecture, but we do not have such data. Another and more indirect way is to use the prefecture-cohort level outcomes to compare the role of fixed effects before and after the way. We have not digitized the prefecture-cohort outcomes that are available for the 1930 census.

1.7.4 Robustness check using prefecture-level death counts

New Data and Artificial Gender Ratio As pointed out in 1.3.3, the Japanese government has been reluctant to count and publicize the number of casualties during the Second World War. Recently, the Ministry provided a new prefecture-level table that was constructed from *Image Information Retrieval System for the Code-Number List of Adjudication Notice for Condolence Payments*. This data is a mechanical extraction, by prefecture, of the number of condolence payment documents listed in the Image Information Retrieval System, which is used by the Ministry of Health, Labor and Welfare as an index to search for original condolence payment documents in its archives. Therefore, the reported numbers are not the numbers of condolence payments by prefecture itself, as some may be missing or duplicated. In addition, the recipients of condolence payments do not necessarily coincide with the war dead according to the eligibility, defined by the Law for the Relief of Bereaved Families of War Victims. Regardless of these limitations, the table can be a direct approximation of the number of deaths by prefecture [51].

Using the newly counted number of death, it is possible to construct a measure of gender ratio changes that is more robust to the cross-prefecture migration. We newly define artificial gender ratio, that is:

$$\Delta Ratio_j^{Art} = \frac{M_{j,1935} - Death_j}{F_{j,1947}} - \frac{M_{j,1935}}{F_{j,1935}}$$

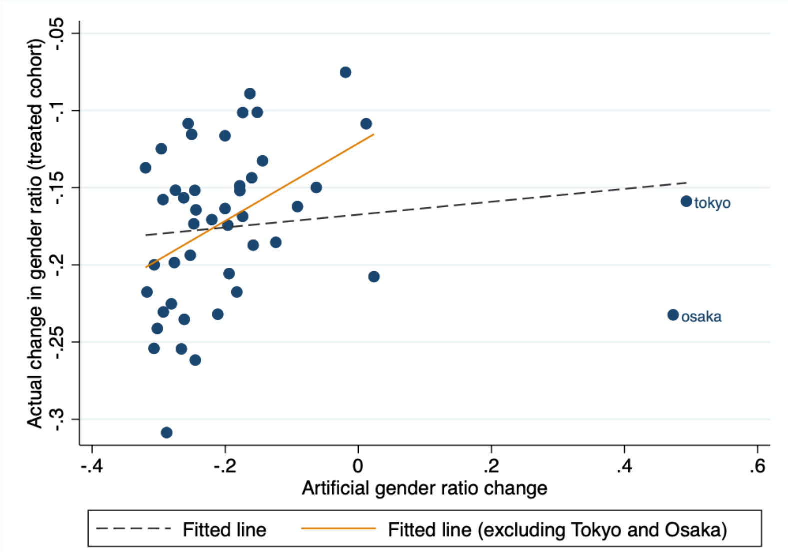
where male population size changes only through soldier deaths (ignoring the cross-prefecture migration) between 1935 and 1947. We continue to use the treated cohorts to construct this measure for each prefecture.

Figure 1.7 shows the correlation between this artificial gender ratio change and the original gender ratio change. Tokyo and Osaka have exceptionally high (and positive) artificial gender ratio changes because the population of the treated cohorts was already male-skewed in 1935, and there was a large outmigration in particular for men between 1935 and 1947. As a result, $\Delta Ratio_j^{Art}$ is inflated for these two prefectures, because it ignores this outmigration in the numerator. Apart from these two outliers, Figure 1.7 show a positive correlation between the two measures, confirming that the gender ratio change in population census does largely capture the male soldier deaths.

Table 1.9 replicates Table 1.8 using the artificial gender ratio, but we add a dummy variable that takes 1 for Tokyo and Osaka. The signs of the coefficients are consistent with the results in Table 1.8. However, the coefficients are smaller for the industry employment shares. This confirms that the previous estimates using prefecture-level gender ratio change may indeed suffer an omitted variable bias, that is related to the movement of male population between 1935 and 1947. The size of the coefficients here is closer to one from the coefficients in Table 1.3, that are robust to unobservable prefecture characteristics. In the appendix, we provide the result using the artificial gender ratio change that also ignores female migration (Table A.7) and controlling for the population changes due to migration for men and women separately (Table A.8).

[51] Due to the contract with the Ministry, we cannot publish the table by ourselves.

Figure 1.7: Correlation between actual and artificial gender ratio change



Note: The figure shows the correlation between actual gender ratio change of the treated cohort and artificial gender ratio change.

Death rate It is also possible to use death rate by prefecture directly in the estimation. This resembles the approach taken by Boehnke and Gay (2022) and Cardoso and Morin (2018).

We impute death rate for each prefecture by dividing death count by number of men in the treated cohorts when they were aged 10-14. We do not use number of men in the treated cohorts in 1935 in the denominator. This is because our death count is based on the prefecture of registration and population size at the age of 10-14 is closer to the registered population of the treated cohorts, rather than the current population size in 1935 when they were aged 20-34. In fact, in our data, we observe that population size (and gender ratio) starts to diverge across prefectures from the age group of 15-19. Death rate is therefore imprecise and endogenous to migration if we simply use population size in 1935 in the denominator.[52] For example, death rate of large prefectures becomes lower (as receiving prefectures of young men) and that of small prefectures becomes higher (as sending prefectures). Our imputed death rate is on average 0.2, with the highest of 0.26 for Saga and the lowest of 0.12 for Hokkaido.

Table 1.10 shows the results. Note that the sign of the coefficient must be reversed from the previous tables as male loss is larger for higher death rate. The estimated coefficient for the industry employment shares have expected signs, that is, higher death rate of young soldiers is associated with a lower (higher) share of manufacturing (agricultural and service)

[52] The reference age for the male population in the denominator can be younger than 10-14. The result is generally unchanged when we use age 5-9. However, we do not use age 0-4 as a reference because it will be contaminated by infant mortality rate, that is likely to be correlated with prefecture-level economic indicators.

Table 1.9: Regression with artificial gender ratio change

	Industry Employment Share			Employment and Participation		
	Manufacturing	Agriculture	Service	Employment rate	Female LFPR	Male LFPR
	(1)	(2)	(3)	(4)	(5)	(6)
Artificial gender ratio change	0.121*	-0.016	-0.080*	0.201***	0.302***	0.053***
	(0.061)	(0.059)	(0.041)	(0.046)	(0.075)	(0.016)
Bomb casualty per capita	-0.129	0.075	-0.067	0.555***	1.051***	0.064
	(0.318)	(0.300)	(0.182)	(0.193)	(0.283)	(0.118)
Destroyed building per capita	0.339***	-0.298*	-0.121	0.034	-0.002	0.036
	(0.117)	(0.155)	(0.102)	(0.089)	(0.145)	(0.040)
Height weight ratio	-4.657***	4.208***	0.258	-0.158	0.181	-0.033
	(0.805)	(0.728)	(0.419)	(0.389)	(0.642)	(0.219)
ln(pop) change 1935-47	0.019	-0.150**	0.106*	-0.033	-0.103	0.009
	(0.060)	(0.074)	(0.061)	(0.060)	(0.099)	(0.023)
Gender ratio in 1935	0.023	-0.188***	0.090*	-0.187***	-0.244***	-0.017
	(0.058)	(0.066)	(0.048)	(0.046)	(0.078)	(0.016)
ln(pop) in 1935	0.028	-0.167***	0.100***	-0.050	-0.125*	0.005
	(0.030)	(0.045)	(0.030)	(0.041)	(0.066)	(0.014)
ln(worker)	-0.036	0.163***	-0.086***			
	(0.030)	(0.045)	(0.030)			
ln(pop)				0.030	0.089	-0.007
				(0.040)	(0.065)	(0.013)
Cohort FE and time FE	Yes	Yes	Yes	Yes	Yes	Yes
Prefecture FE	No	No	No	No	No	No
Cohorot-time FE interaction	Yes	Yes	Yes	Yes	Yes	Yes
Prefecture-level controls	Yes	Yes	Yes	Yes	Yes	Yes
Pre-war prefecture-level controls	Yes	Yes	Yes	Yes	Yes	Yes
Pre-war outcome (1930 value)	Yes	Yes	Yes	Yes	Yes	Yes
R-squared	0.854	0.940	0.919	0.794	0.776	0.625
Observations	2484	2484	2484	2484	2484	2484

Note:The table provides the regression results for artificial gender ratio change, controlling for 1930 outcomes. In addition to the variables listed in the table, the prefecture-level controls include age share of population (10s, 20s, 30s, 40s, 50s and 60s), dummy for large prefecture, dummy for Tokyo and Osaka. Standard errors are clustered at prefecture-year level. Standard errors are in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

sector share in employment. However, again, higher death rate is negatively related to female labor force participation and other employment outcomes, similarly to the previous results that exploit cross-prefecture variations in the gender ratio.[53]

1.7.5 Cross-cohort relation

We extend the analysis based on cross-prefecture variation in the gender ratio change to study the heterogeneous effect on different cohorts. The estimation sample is restricted between 1955 and 1960 to examine the immediate effect of the gender imbalance and highlights the mechanism of between-cohort substitution or complementarity.

Figure 1.8 provides the cohort-specific treatment effect. For the manufacturing sector employment share, the negative effects are detected for the treated cohort, and even more strongly for the younger cohort, while the coefficient is close to zero for older cohorts. This

[53] We provide the regression results using the version of death rate based on number of men in the treated cohorts in 1935 in the appendix (Table A.9).

Table 1.10: Regression using death rate

	Industry Employment Share			Employment and Participation		
	Manufacturing (1)	Agriculture (2)	Service (3)	Employment rate (4)	Female LFPR (5)	Male LFPR (6)
Death rate	-0.297***	0.233**	0.028	-0.356***	-0.391***	-0.111***
	(0.109)	(0.110)	(0.064)	(0.073)	(0.116)	(0.035)
Bomb casualty per capita	0.240	-0.186	-0.048	0.727***	1.089***	0.133
	(0.410)	(0.389)	(0.193)	(0.215)	(0.322)	(0.126)
Destroyed building per capita	0.323**	-0.302*	-0.121	-0.032	-0.066	0.016
	(0.128)	(0.165)	(0.103)	(0.085)	(0.145)	(0.042)
Height weight ratio	-4.142***	4.502***	-0.193	-0.474	-0.073	-0.215
	(0.682)	(0.681)	(0.359)	(0.422)	(0.649)	(0.243)
ln(pop) change 1935-47	-0.021	-0.165***	0.134***	-0.180***	-0.284***	-0.040
	(0.054)	(0.063)	(0.050)	(0.051)	(0.079)	(0.025)
Gender ratio in 1935	0.055	-0.201***	0.134***	-0.208***	-0.290***	-0.011
	(0.070)	(0.068)	(0.039)	(0.046)	(0.074)	(0.023)
ln(pop) in 1935	-0.057***	-0.019	0.069***	-0.011	0.017	-0.011*
	(0.020)	(0.031)	(0.020)	(0.016)	(0.027)	(0.006)
ln(worker)	0.048***	0.017	-0.058***			
	(0.018)	(0.028)	(0.019)			
ln(pop)				-0.014	-0.059**	0.007
				(0.015)	(0.025)	(0.005)
Cohort FE and time FE	Yes	Yes	Yes	Yes	Yes	Yes
Prefecture FE	No	No	No	No	No	No
Cohorot-time FE interaction	Yes	Yes	Yes	Yes	Yes	Yes
Prefecture-level controls	Yes	Yes	Yes	Yes	Yes	Yes
Pre-war prefecture-level controls	Yes	Yes	Yes	Yes	Yes	Yes
Pre-war outcome (1930 value)	Yes	Yes	Yes	Yes	Yes	Yes
R-squared	0.854	0.940	0.919	0.794	0.776	0.625
Observations	2484	2484	2484	2484	2484	2484

Note: The table provides the regression results for death rate where death rate is calculated based on number of death counts in each prefecture, divided by number of male population of treated cohorts when they were aged 10-14. The regression controls for 1930 outcomes. In addition to the variables listed in the table, the prefecture-level controls include share of young population (aged 20-30) in 1935, dummy for large prefecture, dummy for Tokyo and Osaka. Standard errors are clustered at prefecture-year level. Standard errors are in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

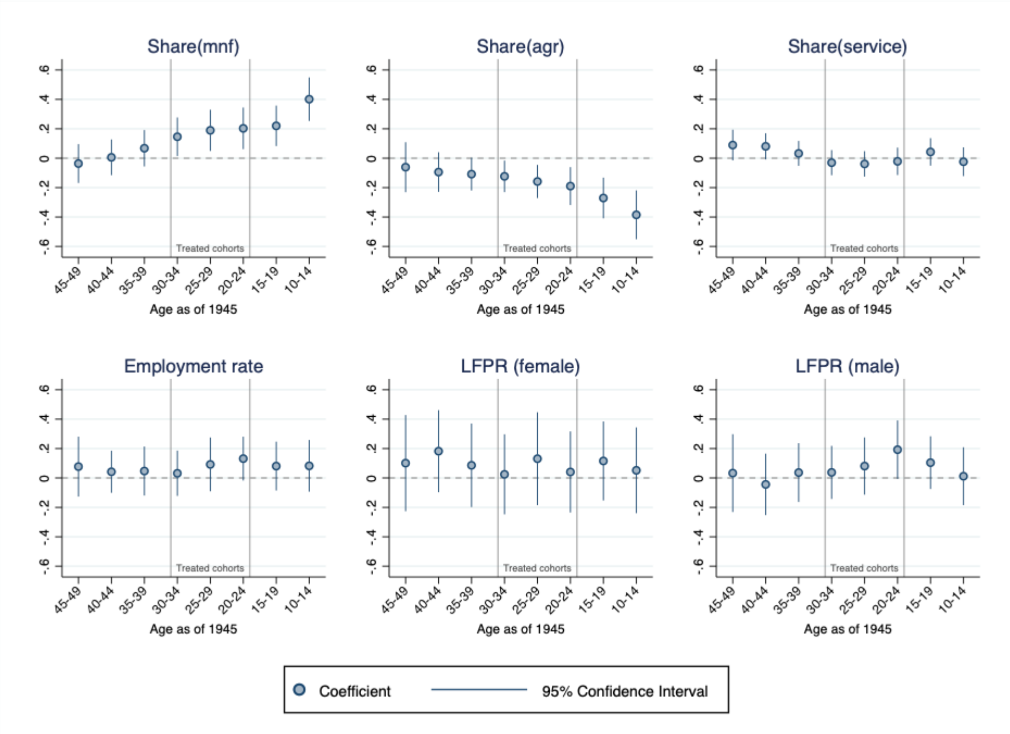
pattern is similar in the agricultural sector. We do not see a clear pattern of heterogeneity with respect to the employment and participation outcomes.

An interesting pattern emerges when we analyze the younger cohort and study the entire 1955-1980 period, shown in Figure 1.9. It demonstrates that the effect diminishes for younger cohorts, indicating catch-up process as the young and large cohorts enter the labor market. In the case of agriculture, the sign even reverses (although not precisely estimated).

1.8 Conclusion

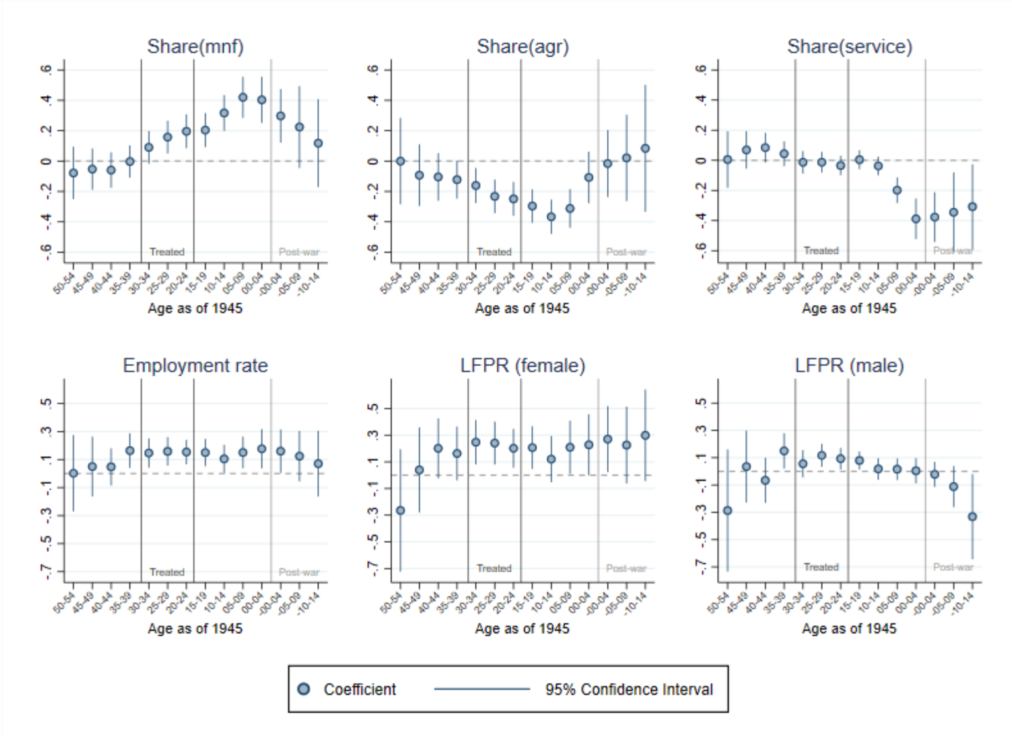
In this study, we examined how the permanent loss of human capital is related to economic recovery that led to the integration of women into the labor market owing to the loss of men during the war. War affects the industrial structure not only through damage to physical capital stocks but also through the tremendous loss of human capital. In the

Figure 1.8: Coefficient by cohorts, until 1960



Note: The figure provides the heterogeneous effect of the gender ratio change on the outcomes across cohorts. The regression is based on Equation 1.5, but the heterogeneous coefficients are obtained by interacting the cohort dummies with the change in the gender ratio of treated cohorts that varies only across prefectures.

Figure 1.9: Coefficient by cohorts, all years 1955-1980



Note: The figure extends Figure 1.8 by adding cohorts that were not born in 1945 in the sample.

Second World War, Japan lost 2 million soldiers, and most casualties were males from certain areas and cohorts in the country owing the institution of hometown regiment.

Using the difference in gender imbalances between geographical areas and cohorts because of the war, we examined the effects of permanent loss of males on the consecutive economic development of areas. The regression results show that the permanent loss of males may have led to slower industrialization and a tentative increase in agriculture. However, such slow-down effects were limited quantitatively and had gradually disappeared after approximately 15 years from the end of the war.

These findings imply that average technology is augmented by gender during high-speed economic growth after the war. In the long term, technological change and internal migration may have neutralized the permanent loss of human capital in certain geographical areas. Further, we find that all sectors increased female share in employment in response to the imbalanced gender ratio, indicating that the within-sector production technology adjustment was one mechanism for the local economy to absorb the negative shock in human capital composition. This effect was persistent even after the impact on the industrial structure has disappeared. Consistently, the permanent loss of men moderately increased female labor force participation rate, that persisted over 30 years since the end of the war, highlighting the demand-side pulling factors that led to the integration of women into the labor market.

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Chapter 2

Firm-Level Effects of Reductions in Working Hours[1]

-with Marta C. Lopes and Alessandro Tondini

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Abstract

This paper examines how legislative reductions in working hours impact firms' employment, output, and productivity. We exploit a Portuguese reform that reduced standard hours from 44 to 40 hours in 1996. Our findings indicate that the reform had adverse effects on the employment and output of affected firms. These effects can be attributed to an increase in hourly labor costs induced by the legal obligation not to reduce monthly salaries. Treated firms adjusted their employment by reducing hiring and significantly improved hourly labor productivity. In contrast, firms that reduced working hours through collective agreements prior to the reform were able to increase productivity without adverse effects on employment and output. A key policy take-away of these combined findings is that estimating effects on early-adopters is likely to give a biased estimate of the overall cost of the switch to lower hours.

Keywords: working hours, wages, labor demand, productivity

2.1 Introduction

Working hours are a key economic variable; if workers and hours are not perfect substitutes, working hours (should) directly enter into the production function (Feldstein, 1967). This implies a direct link between working hours and key economic outcomes such as output, employment and productivity. Consequently, working hours *legislation* – regulatory limits to the number of hours one can work – has significant economic implications. Such legislation is common across countries, especially in Europe, where an EU Directive (1993) regulates maximum working hours, on top of more stringent national legislation. The issue of working hours is also key in labor relations and frequently debated in the public arena, where there has been a long-standing debate on the effectiveness of reducing working hours as a policy to lower unemployment, commonly referred to as "work-sharing". More recently, there has been growing global interest in initiatives and trials for the four-day workweek and its potential benefits on productivity, but fueled only by limited descriptive evidence often only on "volunteer" firms who willingly opted for this change. This highlights the need for more rigorous studies on the effects of shorter working time.

Indeed, amid such interest, there is still limited causal evidence on the consequences of a shorter workweek, in particular regarding how firms adapt to a stricter limit on working hours. The existing gap in the literature can be attributed to the absence of a clear empirical context, either due to the gradual or mild implementation of hour reductions or the simultaneous provision of subsidies to firms (Batut et al., 2023). Moreover, the existing literature has primarily concentrated on aggregate-level employment or worker-level separations, leaving the consequences for firms inadequately explored.

In this article, we present novel, clearly identified evidence on the impact of reduced working hours on firms, focusing on the transition from a 44-hour workweek to a 40-hour workweek in Portugal between 1991 and 1998. Portugal's labor relations system combines national regulations and collective agreements. In 1990, a tripartite agreement was signed among unions, employers' associations, and the government to gradually reduce standard hours to 40 by 1995 through collective agreements. However, most collective agreements did not comply with this reduction. In 1996, following national elections, the new government unilaterally implemented the change to a 40-hour workweek through national legislation.

First, we analyze the effects of the 1996 reform, which affected the majority of firms and workers through an exogenous reduction in standard hours. Our empirical approach employs a difference-in-differences estimation, where we compare firms with at least one worker affected by the reform to those with no affected workers.

Using a comprehensive, matched employer-employee dataset, we first document that firms impacted by the 1996 national reform significantly reduced hours worked by around 2.5 hours on average, corresponding to a 6% decrease compared to the pre-reform period. These firms did not compensate for the loss of regular hours by increasing overtime hours, likely due to the high overtime wage premium. Self-reported working hours in the Labor Force Survey corroborate the decrease in employer-declared hours in the administrative data.

Our analysis shows that firms affected by the reform experienced a moderate *decrease* in employment and sales, compared to the control group. This is primarily the result of a substantial increase in the hourly wage rate, driven by the legal restraint to reduce monthly salaries for workers whose standard hours were decreased. This increase in labor costs resulted in negative scale effects on firm output and substitution with capital, leading to reduced labor demand. In terms of magnitude, a 6% increase in the hourly wage rate led to roughly a 2% decrease in employment, 9% decline in total hours, and a 4% reduction in sales. These estimates imply a firm-level labor demand elasticity of -1.48 in terms of hours and -0.33 in terms of employment, which is slightly larger, but within the range of estimates in the literature (Hamermesh, 1996; Lichter et al., 2015). Firms adjusted their employment size through a reduction in new hires, rather than increased separations of incumbent workers, consistent with the strict regulations on firing in Portugal. Treated firms were able to partially offset the decrease in labor input by significantly increasing hourly labor productivity, measured by sales per hour, partially mitigating the negative effect on sales. This increase in hourly labor productivity amounts to 4.4%, more than two thirds of the increase in hourly wages. By exploiting 2-digit sector-level capital and price information from national accounts, we observe that there is an insignificant but positive relationship at the sector level between the reduction in hours and the growth of capital services, and little relationship with increase in prices. Moreover, there is no evidence of an increase in average worker quality in treated firms, as measured by workers' educational qualifications.

In the second part of the paper, we evaluate the impact for firms that reduced standard hours through collective agreements prior to the 1996 reform (hereinafter, "early-adopters"). Similar to the national reform, working hours decreased and the hourly wage rate increased. However, in comparison to firms treated by the national reform later, these firms are not adversely affected: we do not observe a decline in employment or sales, while we still observe a positive effect on hourly labor productivity, perfectly proportionate to the increased hourly wage rate. In our interpretation, this is likely due to the heterogeneous effects of working hour reductions induced by the endogenous bargaining process of collective agreements. Sectors that would incur fewer costs in reducing working hours were more likely to implement the reduction earlier through collective bargaining. This aligns with the fact that firms treated through collective agreements outperformed reform-treated firms in terms of observed productive characteristics, such as sales per hour. We argue that these combined finding are an important take-away for debates around future reductions in the length of the working week (whether hours or days): estimating effects on voluntary early-adopters is likely to give a biased estimate of the overall cost of the switch to lower hours.

Our paper relates to several strands of literature.[2] First, we contribute to the empirical literature on the impact of working hour reductions on employment. Previous research mainly focused on the effects of working hour reforms on worker-level employment dynamics, finding mixed effects on separations (Crépon and Kramarz, 2002; Gonzaga et al., 2003; Raposo and van Ours, 2010; Sánchez, 2013; Estevão and Sá, 2008). Some studies also examined employment effects at the sectoral or regional level to consider the aggregate equilibrium effect, with most finding no positive employment effect (Hunt, 1999; Skuterud, 2007; Chemin and Wasmer, 2009; Batut et al., 2023). The only study indicating a positive association between employment and a reduction in standard hours is Raposo and Van Ours (2010), who suggest that local labor markets, defined by region-sector combinations, which were more impacted by the 1996 reform in Portugal subsequently experienced higher employment growth.

With respect to these studies, our firm-level analysis offers new insights by directly studying how firms adjusted their overall labor demand. Firm-level employment encompasses both separation and hiring, while worker-level studies are unable to address both channels and so the total employment size of the firm. Indeed, we find that most important margin of employment adjustment for firms was less hiring of new workers, rather than firing incumbent workers. At the same time, in comparison to sectoral or regional studies, firm-level analysis can better control for potential confounding factors, by comparing affected and unaffected firms within the same sector and region, and provide a direct examination of the theoretical predictions regarding the firm's adjustment mechanisms.

Although some articles have already attempted to explore firm-level effects, empirical evidence in this regard is scant and far from conclusive. Kawaguchi et al. (2017) study the reduction in standard hours in Japan during the 1990s, but find only small first-stage effects on hours.[3] Another study by Crépon et al. (2004) on France's reduction in standard hours to 35 hours showed that firms affected earlier by the change in hours experienced relative increases in employment. However, as acknowledged by the authors, their evaluation is complicated by simultaneous cuts in social security contributions aimed at easing the transition of firms to the lower hours standard.[4] In contrast, we argue that a key advantage of the Portuguese reform is the absence of such compensating measures for affected firm, making it a cleaner case study.

Preliminary to our work is Varejao (2005), who study the same Portuguese reform on establishment-level employment, suggesting a potentially zero to negative effect depending on the specification. Our paper improves on this work along two important dimensions. Firstly, our approach takes into account the full institutional context of the reform, notably

[2] We provide a more detailed review of the literature on the effects of working hour reductions in the Appendix Section B.1.

[3] For a subset of firms with a significant drop in hours, they find a negative but insignificant employment effect.

[4] Similar compensating measures were put in place for the Belgian reform in 2002 (Batut et al., 2023).

the preceding collective bargaining process since 1990 – an aspect overlooked in the previous literature on this reform (Varejao, 2005; Raposo and Van Ours, 2010; Raposo and van Ours, 2010; Lepinteur, 2019). In analyzing the 1996 reform, we carefully construct the sample by excluding firms likely to be influenced by the prior collective bargaining process, and provide separate estimates of the impacts of reducing working hours through collective agreements. Secondly, we extend the analysis to other key outcomes beyond employment, such as output and productivity. This is crucial for understanding firms' overall responses to the reduction in hours, as firms' labor demand is jointly determined with output and shaped by the extent to which labor productivity could increase.

Our contribution also extends to the literature on firm performance and productivity, where evidence on the impact of working hours reductions is extremely scarce. To the best of our knowledge, only Crépon et al. (2004) study how the mandatory reduction in standard hours affects productivity, within the limits of the setting of the French reform.[5] Some studies have examined the correlation between working hours and productivity in specific occupations (Brachet et al., 2012; Pencavel, 2014; Collewet and Sauermann, 2017), typically finding a linear relationship between hours and output up to a certain point, followed by diminishing marginal returns.[6] Our paper provides evidence that reductions in working hours significantly increase hourly labor productivity at the firm level, indicating that the productivity gains found in specific occupations might be be applicable in a more general setting.

Lastly, this article contributes to the literature examining the impact of increased labor costs on labor demand and employment. Whether a surge in labor costs has a negative effect on employment is deeply linked to the debate surrounding the "elusive" employment effects of minimum wages (Cengiz et al., 2019; Manning, 2021). While conventional economic models posit a negative employment effect resulting from higher labor costs, this prediction is contingent on the assumption of perfectly competitive labor markets, which may not always hold. An important advantage of our paper in exploring the link between labor cost and employment is that we exploit a sizable increase in the wage rate, roughly 6%, simultaneously applied to a large number of workers in the labor market. Consequently, we can estimate the effects on labor cost in a more general setting, compared to studies that focus on wage changes in specific groups of workers or only at the lowest end of the wage distributions (e.g., minimum wage).

The paper proceeds as follows. Section 2.2 provides an overview of the working-time

[5] They found negative associations between the reduction of hours and total factor productivity in the French case. Again, however, this analysis is complicated by the simultaneous cuts in social security contributions targeted to affected firms.

[6] Related to this literature, some studies examine the impact of part-time employment on firms' productivity, finding that firms with a higher share of part-time workers are more productive in specific sectors, such as pharmacies (Künn-Nelen et al., 2013), or when part-time employees work more than a certain number of hours (Garnero et al., 2014).

legislation in Portugal and describes its chronological evolution. Section 2.3 discusses briefly the varying theoretical predictions regarding a reduction in working hours. Section 2.4 introduces the data that are used in the analysis and defines the sample. In Section 2.5, we study the effects on the 1996 national reform through a difference-in-differences approach. Section 2.6 provides the effects of working hours reductions for early-adopters through collective agreements. Section 2.7 summarizes and concludes.

2.2 Working Time Legislation in Portugal

2.2.1 General Aspects of Working Time Legislation

Working time regulations include all legislation that limits the number of working hours a worker can work and regulate the organization of the working week (as well as the day, year, etc.). There are various aspects to working time legislation, including, among others, regulations on night-shifts, weekend work, paid leave, and national holidays. Arguably, the most relevant determinants of the actual length of the working week are standard hours, overtime and the overtime rate. Standard hours refer to the length of the *usual* working week, that is, how many hours a worker usually works or the hours specified in her contract in the absence of overtime, and are usually averaged over a certain reference period. In other words, standard hours set the daily and/or weekly limits at which overtime hours begin. Overtime hours refer to the additional hours worked beyond the standard hours, with limitations usually imposed at the daily, weekly, or yearly levels, or a combination of these.[7] Overtime hours are paid at an overtime rate, which sets the wage increase a worker should earn on each extra hour. The interplay of the policies directly impacts the number of hours workers actually work. For example, in the United States, where standard hours are set at 40, similar to most European countries, the less strict limits and premiums on overtime result in significantly higher average working hours. In contrast, Portugal follows a different approach – overtime is rigorously capped and comes at a higher cost, making it a rarely used option for firms, as we will elaborate on in the subsequent sections.

2.2.2 Working Hours Legislation in Portugal over Time

In Portugal, working time are regulated by both national legislation and collective agreements, which typically vary by sector and location. The majority of workers are covered by collective agreements.[8] Generally, national legislation sets the upper bound, while collective agreements specify lower levels. The national legislation concerning working time in Portugal dates back to 1971, when the *Decreto-lei 409/71* set standard working time at 8 hours daily, and 48 hours weekly, and at 7 hours per day and 42 hours per week for office

[7]For example, maximum weekly hours are capped at 48 hours in the European Union by the 1993 EU WT Directive.

[8]According to the administrative data used in this paper, approximately 80% of workers are directly covered by a collective agreement.

Table 2.1: Working Time Legislation in Portugal, 1971–2003

Year	Standard Hours		Overtime		Overtime Rate
	Daily	Weekly	Daily	Yearly	
1971	7-8	42 (40) or 48(45)	2	240	25%, 50%
1983	7-8	42 (40) or 48(45)	2	**160**	**50%, 75%**
1991	7-8	42 (40) or **44**	2	**200**	50%, 75%
1996	7-8	**40**	2	200	50%, 75%
2003	7-8	40	2	**150-200**	50%, 75%

Note: This information was collected by the authors based on the national legislation. *Year* refers to when the law was published in the official gazette, not the date of effective implementation. *Standard Hours* indicates the maximum usual hours specified in the national legislation, both at the daily and weekly levels. *Overtime* refers to the maximum number of hours that can be worked on top of standard hours, by paying the *overtime premium*: the first number refers to the rate for the first hour of overtime, and the second number refers to the rate for subsequent hours. All reforms are highlighted in bold; additional aspects of working time legislation not covered in the table can be found in the text. The law specified different working hours for office workers prior to 1996 (7-hour day, and 42-hour week).

workers, with one day of mandatory rest. The law also allowed employers to extend the daily limit by one hour if an additional half-day or day of rest was provided. In practice, this meant a daily limit of 8 hours for office workers and 9 hours for other workers, with a corresponding working week of 40 and 44 hours (45 hours prior to 1990), respectively. According to the Labor Force Survey, as shown in Figure B.12, the majority of workers were already following a Monday-to-Friday working week in the early 1990s, despite an 8-hour maximum and a 44-hour week would have required them to work at least half a day on the weekend.[9] However, around 80% of full-time workers were not working weekends in 1992, suggesting that they were on a 9-hour day (8-hour day if office workers) and 44-hour working week (40-hour if office workers). We summarize the major changes in the national legislation in Table 2.1.

The law also allowed for 2 daily hours of overtime, with a maximum of 240 hours per year. The first hour of overtime was paid a 25% premium and the second a 50% premium. Yearly overtime was reduced from 240 to 160 hours in 1983, and the premium increased to 50% for the first hour and 75% from the second. Notably, the law explicitly mentioned "work-sharing" as the rationale for this reform.[10] The limit was then pushed back up to 200 hours in 1991.

[9] Unfortunately, the data does not provide information on weekend work in the late 1980s, it is likely to be very similar: based on the distribution of hours in the labor force surveys (Figure B.6), it is evident that prior to 1991, most workers were indeed on a 45-hour week, with only a few working 48 hours.

[10] "A necessidade de distribuir o trabalho existente pelo maior número possível de trabalhadores impõe que a prestação de trabalho fora do horário normal só seja permitida nos casos em que se mostre necessário" "The need to distribute existing work among as many employees as possible means that work outside normal working hours is only permitted where it is necessary." (Decreto-lei 421/83, Portugal, 1983).

Reforms in the length of the working week in Portugal began in 1990 when the government, unions, and employers' associations signed a tripartite agreement to immediately reduce the working week to 44 hours (Conselho Permanente De Concertação Social, 1990). In the memorandum of understanding, it was stated that the working week would be further lowered to 40 hours by 1995 through collective agreements in order to avoid a more stringent legislative adjustment.

The non-binding memorandum of understanding to further decrease hours to 40 by 1995 had only limited effectiveness. Some collective agreements reduced working hours but the majority did not. Therefore, when elections took place in Portugal in 1996 and the Socialist Party came into power, a majority of workers were still working more than 40 hours a week. The government then decided to unilaterally reduce hours to 40 in 1996, through national legislation. The reform allowed for a one-year adjustment period; the limit was initially lowered to 42 hours in 1997 and was further reduced to 40 in 1998.

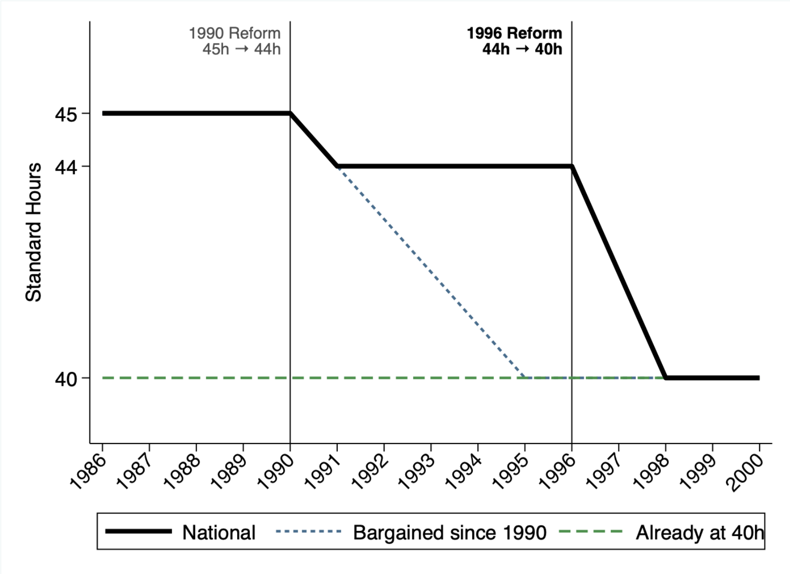
The reform did not increase the maximum overtime hours, but only allowed for a longer reference period over which to average standard hours. It also did not include specific provisions for the adjustment of salaries and wages as hours decreased. However, Portuguese law specifies that the reduction in standard hours must not result in the damage to the economic situation of workers.[11] Lastly, it is crucial to emphasize that, contrary to the other important European reforms in working hours, such as the French and Belgian reforms around 2000, there were no compensating cuts in social security contributions put in place for firms affected by the reform. This allows for a much cleaner interpretation of the effects of reductions in standard hours and is the main comparative advantage of the Portuguese setting.[12]

Figure 2.1 presents three stylized evolutions of standard hours since 1986, reflecting the legislative context described earlier. The solid black line displays the national standard hours, which were initially reduced from 45 to 44 hours in 1991 and subsequently to 40 hours between 1996 and 1998. Standard hours can be set at levels lower than the national standard. The green dashed line represents scenarios where standard hours have consistently remained lower, typically due to collective agreements or occupation-specific reasons. Lastly, the blue dashed line represents the stylized reduction in standard hours resulting from collective agreements that were initiated due to the bargaining process encouraged by the 1990 memorandum. This should have led to a gradual reduction of standard hours from 44 to 40 hours between 1991 and 1995. In practice, as we will show

[11] DL. 409/71 of 27.9, 8.2.

[12] Other relatively less important changes in working time legislation took place in 1998, when the European Working Time Directive of 1993 was ratified, thereby setting a limit to maximum weekly working hours (i.e., standard time plus overtime) at 48 hours. *De facto*, this did not introduce a binding threshold. Lastly, the 2003 Labor Code set yearly overtime at 150 hours maximum for firms above 50 employees, 175 for those below. However, this limit could still be raised to 200 hours by collective agreements. In any case, Castro and Varejão (2007) documented that overtime is very rarely employed by Portuguese firms, probably because of the high overtime premium. Our data shows that, in the period of interest, less than 3% of the firms used paid overtime, and this share did not change even as standard hours were reduced.

Figure 2.1: Stylized Evolution of Standard Hours



Note: This figure provides an overview of changes in standard hours since the mid-1980s. Two vertical lines denote the following key events: (i) the 1991 reform, which reduced the maximum standard hours from 45 to 44 hours and set the stage for subsequent reductions through collective agreements; and (ii) the nationwide reform in 1996, which decreased the national standard hours from 44 to 40 hours over two years (42 hours in 1997 and 40 hours in 1998). The solid black line represents the national standard hours. The green dashed line depicts standard hours that have been consistently lower than the national standard for an extended period, typically due to collective agreements or occupation-specific reasons. Lastly, the blue dashed line illustrates the stylized decline in standard hours resulting from collective agreements encouraged by the 1990 reform.

later, the reduction in standard hours began at different times, mostly starting in 1991, 1992, or 1993. Furthermore, in certain cases, the reduction through bargaining did not implement the full cut to 40 hours.

2.2.3 Evolution of Working Hours

Figure 2.2 illustrates the evolution of working hours between 1986 and 2000, according to the employer-employee linked data used in this paper. These hours are reported by employers, but self-reported hours from workers in the Labor Force Survey show a similar pattern, as detailed in Appendix Section B.4.

Panel (A) presents the average standard hours and average actual hours per week.[13][14] Panel (B) shows the proportion of workers with standard hours and actual hours exceeding 40 and 44 hours, respectively. The average standard hours remained stable around 42 hours until 1990, when a decline began due the tripartite agreement that reduced the

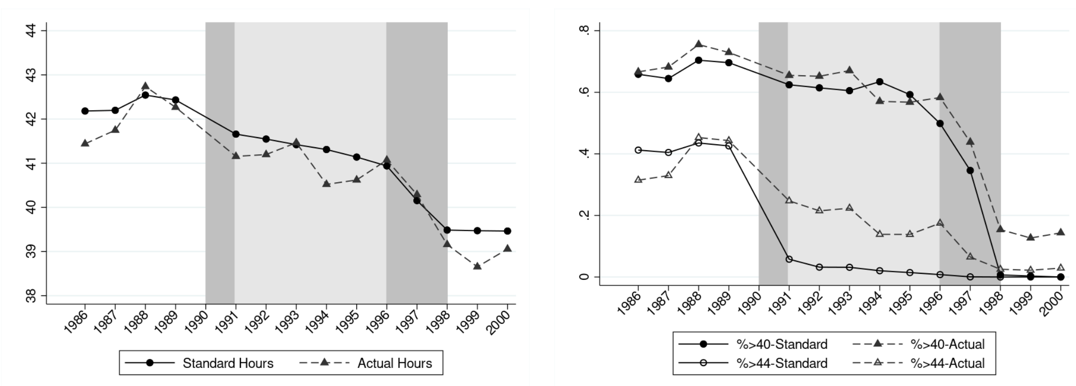
[13]Weekly actual hours are calculated by dividing the total hours worked in the reference month by 21.625 (the average number of working days per month) and then by 5 (the length of the working week).

[14]Standard hours before 1994 are imputed using the method described in Appendix Section B.3.3. Actual hours per week fluctuate due to variations in the number of working days in the reference month across years, idiosyncratic shocks, and business cycles.

national standard hours from 45 to 44 hours, resulting in an average of 41.5 hours in 1991. This decline continued gradually until 1996, reaching around 41 hours, as some collective agreements further reduced standard hours. Following the national reform in 1996, the average standard hours dropped sharply to approximately 39.5 hours by 1998 and then stabilized. The trend in average actual hours worked per week also mirrors the evolution of standard hours. Panel (B) confirms a similar pattern: the share of workers exceeding 44 hours sharply declined after 1990, following the reduction to 44 hours per week. The share of those working over 40 hours decreased more gradually between 1991 and 1996 due to gradual reductions by some collective agreements, then dropped sharply after the national reform. Appendix Section B.4.3 provides further evidence showing the distribution of hours across different periods, consistent with the trends in average hours.

Figure 2.2: Average Standard and Actual Hours of Full-Time Workers and Share Above 40 and 44 hours, 1986 to 2000, QP

(A) Average Actual and Standard Hours (B) Share Above 40 and 44 hours



Note: The figures show the evolution of standard hours and actual hours worked per week in the employer-employee data used in the paper. Standard hours before 1994 are imputed from actual hours using the method described in Appendix Section B.3.3. Panel (A) shows the average standard and actual hours for workers working between 30h and 55h. Panel (B) shows the share of workers with standard hours and actual hours above 40h and 44h respectively. The darker shaded area indicates the period of the national reforms: the 1990 reform that reduced national standard hours from 45 to 44 hours, and the 1996 reform that decreased the standard hours from 44 to 40 hours over two years. The lighter shaded area represents the period when some collective agreements, encouraged by the 1990 tripartite agreement, reduced standard hours.

2.3 Conceptual Framework

In general, the theoretical underpinnings of the effects of reductions in standard hours are well-documented in the literature. In its simplest form with exogenous wages and no fixed costs, a standard labor demand model predicts that a reduction in hours would unambiguously increase employment.[15] This is because firms substitute hours for workers,

[15] As Estevão and Sá (2008) state: "In a partial equilibrium model of labor demand where average hours of work and employment are perfect substitutes and the only relevant labor cost is the hourly wage, a reduction in the standard workweek reduces average hours and raises employment". Such a model can be found in textbooks such as Hart and Sharot (1978) or Hamermesh (1996).

resulting in a higher number of workers working shorter hours. This concept, known as "work-sharing", is intuitive and for this reason has often been favored in public and political debates.

However, the introduction of fixed cost per worker and overtime complicates the predictions significantly, as demonstrated by a seminal paper by Calmfors and Hoel (1988). When overtime is considered, lowering standard hours reduces the cost of overtime hours relative to the cost per worker, which incentivizes firms to replace workers with overtime hours. Paradoxically, this might result in workers actually working *more* hours. Due to the high overtime premium and rare use of overtime in Portugal, this counter-intuitive mechanism is unlikely to be important in our context; instead, the high overtime premium might potentially make work-sharing more probable as firms are more inclined to hiring new workers. Nonetheless, considering the negative scale effect on output, the employment impact remains uncertain and likely to be negative. This happens because the cost per worker, including fixed costs, rises even when hourly wages remain constant.

Others consider models with endogenous wages, where wages can adjust in response to the reduction in working hours. Trejo (1991) proposes a "fixed-job" model in contrast to a "fixed-wage" model. In the "fixed-job" model, workers and firms agree on a package of weekly remuneration and working hours. In this scenario, a legislative reform on standard hours would have no real effects because firms fully adjust hourly wages and overtime in a way that keeps both hours and monthly salary unchanged. However, in practice, working hours are often reduced while maintaining constant earnings, resulting in higher hourly wages.[16] Crépon and Kramarz (2002) formally addresses the scenario in which nominal monthly salaries remain unchanged. When nominal monthly salaries are fixed and hourly wages increase, it intensifies the negative scale effect on employment, as highlighted by Calmfors and Hoel (1988). Crépon and Kramarz (2002) also underline its impact on worker flows, as firms have an incentive to replace workers hired under the old standard with new workers at lower wages under the new standard. This underscores the importance of examining employment at the firm level: an increase in the separation rate for existing workers, as some studies have shown, does not necessarily imply an overall negative impact on labor demand if the hiring rate also increases.

As highlighted by Boeri and Van Ours (2013) and Raposo and Van Ours (2010), the presence of monopsonistic power in the labor market can offer a rationale for legislative reductions in standard hours. This is because in such scenarios, wages are lower than the marginal product of labor, and working hours are longer than what would be optimal for the workers. This concept relates to the ongoing discussion surrounding the "elusive

[16] This is the case for the 1996 Portuguese reform we study and in most other standard hour reforms, especially in Europe (Batut et al., 2023). An exception is the Canadian case (Skuterud, 2007), where monthly salaries could and did adjust to match the fewer hours.

employment effect of the minimum wage" as discussed by (Manning, 2021).[17] When we deviate from the notion of perfectly competitive labor markets and take into account the potential positive effects on labor supply, the consequences of an hourly wage increase resulting from reduced working hours become less clear. Moreover, the models mentioned earlier revolved around the assumption of perfectly competitive markets for goods and services, where firms act as price-takers. In this context, the negative scale effect on output was unambiguously negative. Nevertheless, if firms possess some degree of market power and can adjust their prices to offset increased labor costs, the projected effects become again uncertain.

In summary, theoretical predictions regarding the effects of reducing standard hours are ambiguous and depend on various assumptions. Work-sharing, although theoretically plausible, holds true only under restrictive conditions. Conversely, a negative impact on employment appears probable in competitive markets but less certain in other scenarios. In brief, this question is empirical, as we aim to tackle in this paper. This is also the case for the expected effects on productivity. The impact on workers' productivity depends on assumptions about the shape of the production function, and whether marginal returns to hours are increasing or diminishing at around the equilibrium.

2.4 Data and Sample

2.4.1 Data

The bulk of the analysis is carried out on *Quadros de Pessoal* (QP - "Lists of Personnel"), administrative, matched employer-employee data collected every year by the Ministry of Employment. This data covers the universe of workers and firms with at least one worker. The data at our disposal cover the period from 1986 to 2016. The QP is collected in a specific month every year (referred to as "reference month" hereafter); until 1994, this snapshot took place in the month of March, since then, it has changed to October. The full information available in the QP is specified in the Online Appendix B.3: it includes information on hours, remunerations, and demographic characteristics at the worker level, as well as sector, location, and sales at the firm level.[18] Importantly, it also collects information on which collective agreement covers a given worker. The data have two gaps, in 1990 and 2001.[19] For the purpose of our analysis, we focus on the period 1986 to 2000 in our working sample.

In terms of working time, the QP records *standard* or *contractual* hours, but only from

[17] "The strong a priori belief held by many that a rise in the minimum wage must cost jobs ultimately derives from the assumption that the low-wage labor market is close to perfectly competitive." (Manning, 2021)

[18] Sales in the QP refers to annual sales in the previous year. Therefore, we created an alternative sales variable containing the sales of the corresponding year. In what follows, analyses on sales are based on this variable.

[19] Specifically, there are no worker files for these years.

1994. As discussed before, standard hours are defined as hours normally worked by contract in a week, which can be averaged over a certain reference period. The QP also measures actual hours, i.e., the hours actually worked during the reference month (March until 1993, October since 1994), available in all years, as well as overtime hours in that month. We compute weekly actual hours by dividing the total hours worked in the reference month recorded in the data by 21.625, the average number of working days per month. Although there is a strong correlation between the standard and actual hours, they are not identical. Moreover, actual hours fluctuate much more than standard hours due to business cycles and idiosyncratic factors.[20] The fluctuation in actual hours in the QP also comes from the difference in the number of working days in the reference month of each year. As shown in the Appendix Figure B.9, this can vary between 20 and 23 days per month, depending on the number of weekends and national holidays that fall within a given month.

For some descriptive statistics, we also use the *Labor Force Survey* (LFS). This data provides information on usual hours of work per week and actual hours worked in a reference week. The advantage of the LFS is that workers directly report their hours, including any informal or uncompensated overtime hours that may not be recorded in the administrative hours. This allows us to show that the reform affects "real" hours worked, not only those reported by employers. The Appendix Section B.3 specifies all relevant variables.

2.4.2 General Sample Selection

We make some standard drops in terms of sample selection that are common to all estimations in this paper. First, we exclude publicly participated firms, which we identify as those firms with a non-zero share of public capital. This results in only a drop of 0.15% of the QP total sample of firms. We also exclude firms that reside on the islands, rather than on the continent, due to extremely different labor market conditions (3.5% of firms).

More importantly, we limit our analysis to the manufacturing, retail, wholesale, and hotel and restaurant sectors. These sectors represent 64% of the QP sample in terms of workers and 63% in terms of firms. One reason is that the QP is not representative of firms outside of these sectors, such as public administration, education and health, electricity, and gas, which have a strong public component. We also exclude the finance and real estate sectors, which tend to have lower hours since the beginning of the data period, have little variation in terms of hours within the sector, and within which the concept of working hours and value added is fuzzier. For similar reason, we also remove agriculture, fishing and mining from our sample.

For each different estimation, outliers in the sample are identified in terms of employment

[20]For example, in the year 2000, when both measure are available in the data, we observe that - among full-time workers, in the month of October - 48% of workers have actual hours equal to standard hours, 37% have higher standard hours than actual hours, 14% have higher actual than standard hours.

and sales growth. We drop the observation if the firm's growth from the previous year in employment or sales is at the top 1% or the bottom 1%. Excluding these outliers is necessary to account for the strong dynamics in some firms that would otherwise significantly increase the noise in the estimations.

2.5 The Effects of the 1996 National Reform

The objective of this paper is to estimate the impact of working hour reductions on firm-level outcomes. As noted earlier, the reduction in working hours in Portugal from 44 to 40 hours was first attempted through collective bargaining. Since this transition did not succeed to a significant extent, it was subsequently enacted through the 1996 reform. In our first estimation, we focus on this national reform for several reasons: first, the majority of firms reduced working hours through the national reform; second, the reform led to a rapid and sudden decrease in working hours. Importantly, the timing of the reform was unanticipated until the election that brought the change in government in 1996, which is key for obtaining a causal estimate.[21] On the contrary, studying the effects of the collective agreement is more challenging due to the lack of precise information on the content and timing of collective agreement changes. Finally, information on contract hours is available in our data only since 1994. For these reasons, we focus on the national reform in our main estimation. Nevertheless, despite the limitations, we also estimate the effects of working hour reductions through collective agreements in the next section.

2.5.1 Sample

We focus this analysis on the years between 1994 and 2000. We start from 1994 because standard hours are available in the QP only from 1994 and the reference month has changed to October since that year. Our sample is an unbalanced panel of firms that existed between at least once between 1994 and 1996 and in our sectors of interest (i.e., manufacturing, wholesale, retail, and hotel and restaurant). To minimize the potential influence of the reduction in working hours through the collective agreement process prior to the reform, our main estimation excludes firms in which the mode of standard hours changed between 1994 and 1996 by more than 1 hour, resulting in a drop in 30% of the firms. However, estimating the effects on the full sample of firms without any exclusion leads to qualitatively similar results, as shown in the Appendix Table B.9. Our final sample consists of 73,612 distinct firms.

[21] The reform could still have been foreseen as a result of the 1990 tripartite agreement, which had already envisioned the transition to the 40 hour week. However, in this institutional setting, anticipation predominantly occurred at the collective agreement level, through sector-level reductions in hours and not at the firm-level. We tackle this separately in separate section of the paper. For firms in sectors that did not have collective agreement reforms, we see little evidence of anticipation prior to the national reform.

2.5.2 Treatment Definition

Our estimation strategy exploits the *firm-level* variation in the exposure to the reform. Essentially, we use the fact that, while the legislative standard hours were set at 44 hours, some firms were not (or less) exposed to the reform. These firms had lower standard hours because their collective agreement specified so since before the 1990s, the composition of workers were different (e.g., office workers having 42 hours or 40 hours), or possibly some firm-specific production characteristics having required relatively shorter hours per worker.[22]

To define treated and control firms, we construct the measure of the exposure to the treatment for each firm. We compute the share of the treated hours for each firm, that is:

$$HourShareTreat_j = \frac{\sum_j \max(Hour_{ij} - 40, 0)}{\sum_j Hour_{ij}} \tag{2.1}$$

where $Hour_{ij}$ are the standard hours of worker i at firm j. For example, if all workers' standard hours are 44 hours, the treatment size is 0.0909 (= 4/44), that is 9.09% of the hours were expected to be reduced in this firm due to the reform. We take the average of pre-treatment years across 1994 and 1996 to construct a single measure for each firm. Unlike actual hours worked, standard hours do not fluctuate on a year-to-year basis, making this exposure measure unlikely to be correlated with the economic situations of a given point in time. Moreover, averaging over 1994-1996 further diminishes the potential risk of mean reversion.

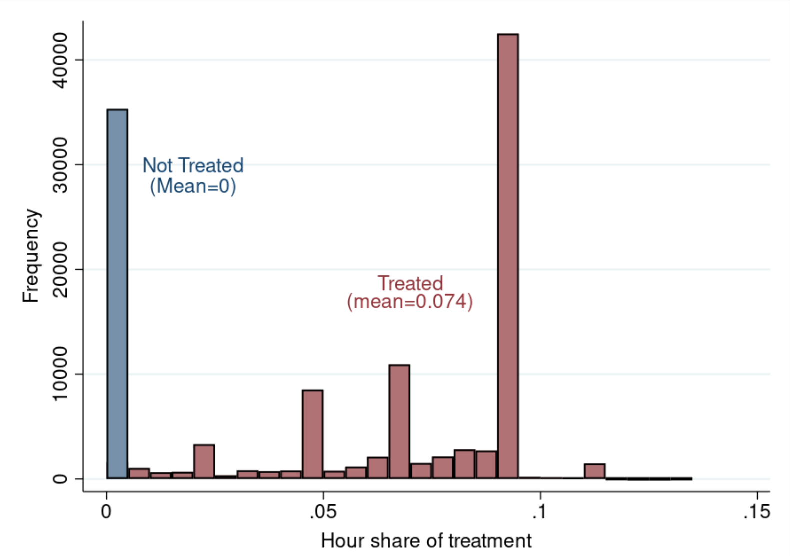
Figure 2.3 shows the distribution of this treatment measure across firms. At the two extremes of the distribution, there are firms with all workers treated at 44 hours and with no workers treated (two groups of roughly equal size); in-between, there are firms with some workers above 40 hours, but not all, or all workers having hours between 40 and 44 hours.[23]

We define treated firms as those with any exposed worker (i.e., strictly positive value in share of hours treated), that constitute roughly 74% of the firms in our sample. Our control group consists of firms with 0% share of treated hours, corresponding to the remaining 26% of the firms. The average share of hours treated among the treated group is 0.074. In the robustness checks (Table B.8), we also use alternative ways of constructing control firms, such using bottom third or below-median in the treatment measure, and the results are largely consistent.

[22] Note that the firms treated through the collective agreement *after* 1990 are excluded from our analysis because we drop firms with changing mode of the standard hours after 1994.

[23] Note that for a handful of firms, the treated share of hours exceed the theoretical maximum of 0.0909 – this is likely due to misreporting of some of their workers' standard hours as above 44 hours.

Figure 2.3: Firm-level share of treated hours



Note: This figure plots the share of hours treated for each firm as calculated in Equation 2.1. It is computed as the sum of standard hours in excess of 40, divided by the total sum of standard hours in the firm. Firms with 0 treated hours have all workers at or below 40 hours. We take the average over 1994-1996 so that each firm has a single value of this treatment measure that is robust to potential mean reversion.

2.5.3 Empirical specification

We estimate the effect of the working hour reductions in a difference-in-differences, which takes the following specification:

$$Y_{jst} = \gamma_j + \delta_{st} + T_j \sum_{t=1994}^{2000} \beta_t \mathbb{1}\{year = t\} + \varepsilon_{jst} \tag{2.2}$$

where Y_{jst} are the outcomes of interest for firm j, in sector s, in year t (expressed in log, except for the hour measures); γ_j and δ_{st} are firm and sector-year fixed effects respectively.[24] T_j is our treatment variable that takes 1 for treated firms and 0 for control firms. β_t identifies the effect of the treatment at time t, using 1996 as the reference year. Standard errors are clustered at the firm level.

To summarize the effects of the reform, we also run the alternative specification:

$$Y_{jst} = \gamma_j + \delta_{st} + \beta \cdot (T_j \times Post_t) + \varepsilon_{jst} \tag{2.3}$$

where $Post_t$ takes 1 for years after 1997, and 0 for 1996 and before. β provides the average

[24]We group sectors into 4 large categories: Manufacturing, Wholesale, Retail, and Hotel & Restaurants. With sector×year fixed effects, our estimates are indetified only by comparing firms within these sectors over time.

treatment effect in post reform years.

The identification assumption necessary to obtain unbiased coefficients β_t is that the treated firms would have followed the same trend in the absence of the reform relative to the control firms. This is the "classic" diff-in-diff assumption, but as some recent papers have highlighted (Kahn-Lang and Lang, 2019; Roth and Sant'Anna, 2023), it deserves further discussion. As the definition of the treatment status itself highlights, treated and control firms have to be initially different in at least some dimensions (here, hours) as the difference itself is what determines that some firms are more affected than others. In our setting, this is the result of different workers/firms being covered by different collective agreements, or by firms making different use of working hours in the production function, or by the presence of different professions across firms. Therefore, it is expected that treated and control firms would differ across other dimensions, too, such as employment levels and productivity. The relevant question for identification then becomes to what extent we could have expected the *evolution* of affected and non-affected firms to have been parallel in the absence of the intervention, *considering that these firms were starting from different levels*. As Kahn-Lang and Lang (2019) emphasize, this also implies, indirectly, a structural assumption: when the starting level is different, a "common trend" assumption cannot hold in both absolute or relative value at the same time, i.e., treated and control firms cannot evolve in the same way in log *and* level at the same time.

Except for the variables for hours, where the absence of a trend allows us to estimate the effect both on units or logs, we put all our outcomes in logarithmic form, since the analysis of the pretrend shows that the relative evolution is more likely to hold for firms starting from different levels.[25] Importantly, we show that the parallel trend assumption holds for most outcomes. Moreover, we also provide results from a specification that includes firm-specific linear trends to validate our findings.

2.5.4 Descriptive Statistics

Table 2.2 provides an overview of the characteristics of firms in the control and treated groups. The control group consists of 15,225 firms, while the treated group comprises 58,387 firms. On average, the size of firms in the control group is 9.3, while it is 14.1 for those in the treated group. Firms in the control group are more likely be located in the Lisbon metropolitan area, offer higher wages, exhibit higher median total sales, and have higher productivity, as measured by sales per worker or sales per hour. This reflects that more productive firms were covered by collective agreements that had shorter hours even before the 1990s, or they had a larger share of jobs and occupations, such as office workers, for which the legislation already specified shorter standard hours. Regarding the sector composition, control group firms have a higher proportion in retail sectors, and a smaller share in manufacturing or the hotel and restaurant sectors.

[25] Taking the log also has the advantage of dealing with outliers, notably very large firms, which are particularly troublesome for the outcomes such as wages and sales.

Table 2.2: Characteristics of Control and Treated Firms (1994-96)

	Control firms	Treated firms
(A) Firm characteristics		
Mean firm size	9.3	14.1
Median firm size	4	4
Lisbon metropolitan area	0.53	0.24
Mean wage (in euro)	4.1	3.0
Median sales (in euro)	253,056	209,197
Median sales per worker (in euro)	5,811	3,734
Median sales per hour (in euro)	48.9	28.4
(B) Sector composition		
Manufacturing	0.20	0.31
Retail	0.32	0.11
Wholesale	0.37	0.36
Hotel and restaurant	0.11	0.22
Number of firms	**15,225**	**58,387**

Note: The table compares the characteristics of firms in the control group and the treated group. Panel (A) shows mean and median firm size, the proportion of firms located in the Lisbon metropolitan area, mean wages, and median sales, sales per worker, and sales per hour. Panel (B) details the sector composition of firms in both groups. The table underscores that, on average, firms in the control group are smaller in size but demonstrate higher productivity.

2.5.5 Results

Figure 2.4 presents the results on the dynamic effects of the reform on firm-level outcomes estimated in the difference-in-differences. It shows the estimated coefficients and their 95% confidence intervals.[26] We also provide average treatment effects in Table 2.3 to quantify the overall effects of the reform on each outcome. Robustness checks with alternative samples, treatment definitions, and specifications are provided in the Appendix Section B.5.

Hours, monthly salaries and wages. Panel (A) shows that the firms affected by the reform experienced a substantial decrease in the average standard hours. In line with the reform's two-year phased implementation, standard hours were reduced on average by 2 hours from 1996 to 1997 and then up to 3 hours by 1998, with no further decreases afterward. This 3-hour drop corresponds to roughly a 7% decrease in mean standard hours. Panel (B) displays the corresponding decline in average actual hours worked, which also decreased by nearly 3 hours. Although firms theoretically had the option to increase overtime hours (capped at 8 hours per week and 200 hours per year) in response to the reform, Panel (C) reveals that there was no increase in overtime hours. This is likely attributed to the high overtime premium in Portugal at that time, set at 50% for the first

[26] Appendix Table B.3 provides the corresponding results in regression tables.

hour and 75% for subsequent hours. Before the reform, only 1% of the firms in our sample used overtime hours.

As mentioned in the institutional context, employers were not allowed to reduce monthly earnings when standard hours were reduced. Panel (D) confirms that there was no adjustment in the mean monthly salary following the reform.[27] Consequently, the reduction in working hours led to an increase in labor costs, measured by hourly wages. Panel (E) illustrates that the mean hourly wage in treated firms rose by slightly less than 7% from 1996 to 1998 and then stabilized.[28]

The results are presented in regression form in Table 2.3, where we present the average effect for years after 1997. The coefficients in columns (1) and (2) indicate that, relative to the pre-reform periods, mean standard hours and mean actual hours were, respectively, 2.757 and 2.425 hours lower for treated firms in comparison to the control group. There is no effect on overtime or monthly salaries. The average effects on the hourly wage rate is 0.061 as shown in column (5).

Employment and total hour input The effects of the reduction in working hours on employment are shown in Panel (F) of Figure 2.4. There is a small pre-existing increasing trend for treated firms relative to the control group before 1996, which is reversed in 1997, when the reform kicks-in. Post-reform coefficients suggest a negative impact on employment, in line with theoretical predictions of increased labor costs. Note that this effect is not driven by firm exits, as shown Appendix Table B.6. Given the reduction in per-worker hours and employment, the total labor input of the firm significantly decreased in treated firms, as shown in Panel (G). The average effect on employment is estimated as -2% in column (6) in Table 2.3, and total labor input decreased by 9% as shown in column (7).

The reduction in standard hours can be regarded as a shift in the wage schedule, as depicted in Figure B.3. The estimated coefficients can therefore provide an estimate of the labor demand elasticity in terms of hours and employment. Taking the ratio of the coefficient on wages to that on total hours, the demand elasticity for hours points to -1.48 (=-0.09/0.061). This magnitude, while relatively large, is in line with Hamermesh (1996) who finds the elasticity of overtime hours of -0.76 to -1.09. but argues that these elasticities

[27] The small, temporary increase immediately after the reform likely reflects the selection driven by the employment effects as shown later.

[28] The legal constraint preventing the reduction of monthly salaries alongside hours technically only apply during the implementation period of the reform, namely, 1997 and 1998. Moreover, firms might have had the chance to reduce the wage growth of treated workers, for example, by exploiting the inflation rate, slightly above 2% inflation rate at that time. However, panel (E) shows that this was not the case, as the wage increase remained permanent, with only a slight decrease from 1998 to 2000. Several explanations could account for this. Firstly, collective bargaining may have deterred the practice of reducing wages using inflation. Secondly, wage levels are also influenced by collective agreements, especially at the lower end of the wage distribution (Card and Cardoso, 2022), making it difficult for firms to significantly suppress wages. Thirdly, our analysis indicates an increase in hourly labor productivity. This suggests that the new wage level might actually be close to the post-reform marginal productivity of hours.

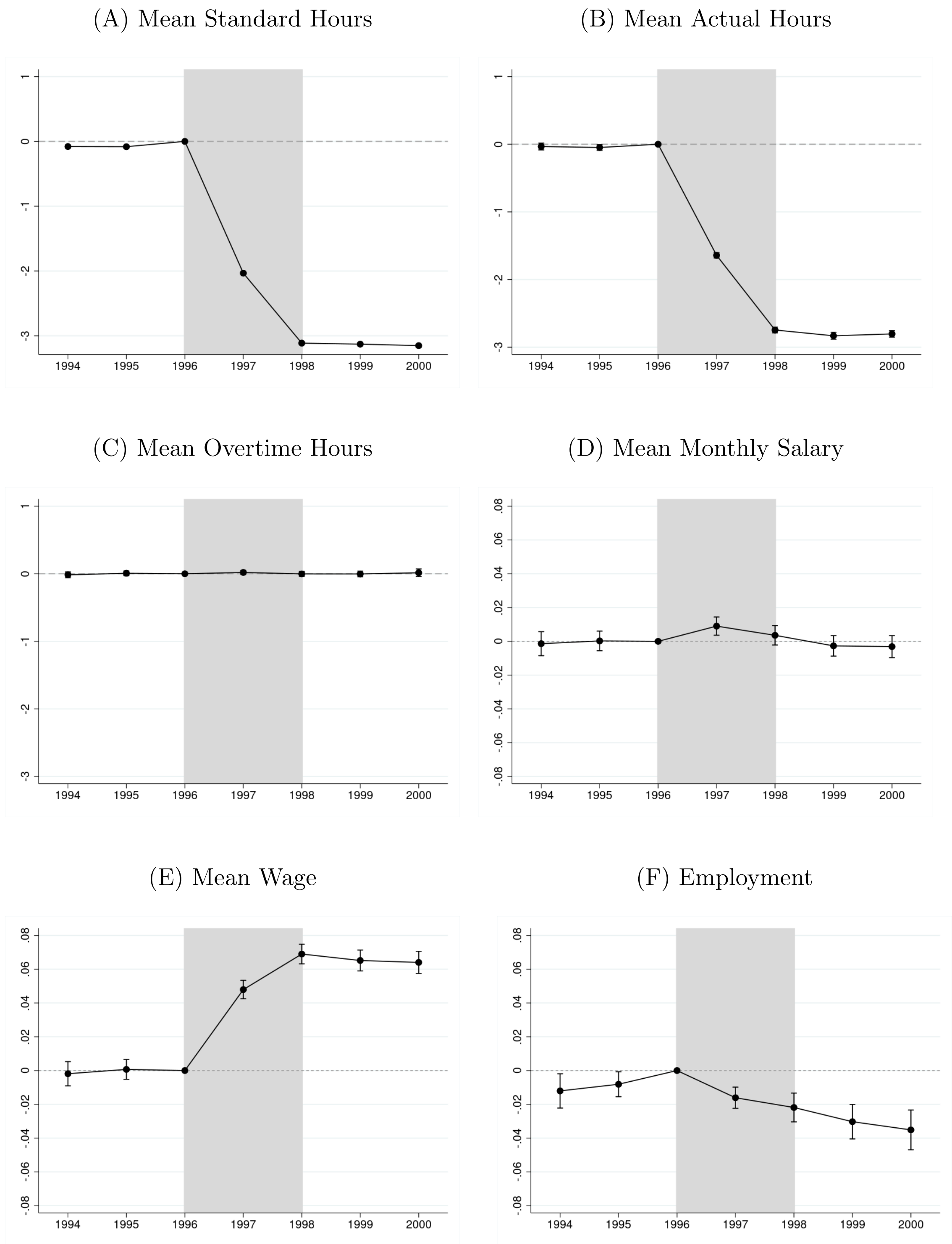
might still be underestimated. The labor demand elasticity in terms of employment is -0.33 (=-0.02/0.061), which is also within the range of the literature.[29] It is important to emphasize that the wage increase in our study impacted a large number of workers located at various points in the wage distribution and across different types of workers. This is an advantage relative the literature that focuses on specific groups of workers or those located at the lowest end of the wage distribution, as in the case of the minimum wage studies. In Appendix Section B.5, we show that the negative effects on employment are relatively smaller for firms operated in the concentrated labor market.

Sales and hourly labor productivity Our dataset includes information on nominal sales, allowing us to examine the impact of reduced working hours on firm-level output and productivity. This offers new insights into existing research, shedding light on the potential role of scale effects on employment. Panel (H) reveals a noticeable and negative effect of the reform on the nominal sales of treated firms, resulting in a cumulative decrease of approximately 6% by 2000. Given the larger negative impact on sales compared to employment, we observe a modest but significant decline in per-worker sales in Panel (I). However, the reduction in sales is not as substantial as the decrease in total labor hours illustrated in Panel (G). Consequently, the reduction in working hours significantly improved hourly labor productivity, as measured by sales per hour in Panel (J). This indicates that the increase in labor productivity partially offsets the negative effects on sales. Table 2.3 in column (8) reveals an average reduction of 4% in nominal sales for firms for all the post-reform years. Per-worker sales decreased by 1.8%, as shown in column (9). However, column (10) confirms a notable improvement in hourly labor productivity of 4.4%, as shown by the positive effect on sales per hour.

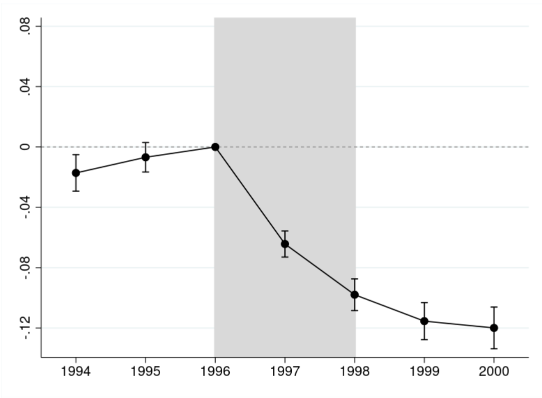
In summary, our results indicate that the significant cut in standard hours, coupled with a substantial increase in hourly wages, caused treated firms to operate at a new equilibrium with reduced input and output levels compared to controls firms over the same period. These results support the theoretical predictions of a negative scale effect, where higher labor costs lead to decreased employment and output. However, this new equilibrium appears to be more efficient in labor utilization, allowing firms to produce more with each labor hour. We explore this finding in greater detail below.

[29] Lichter et al. (2015) indicates an average of -0.551 with a standard deviation of 0.747 for own-wage elasticities.

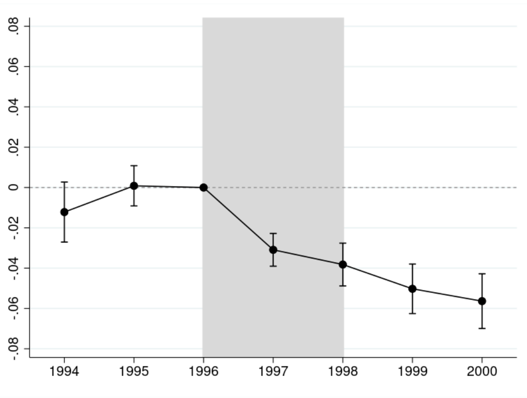
Figure 2.4: Dynamic Effects of the 1996 National Reform



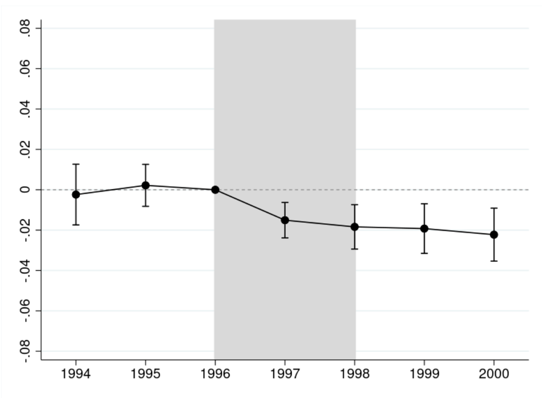
(G) Total Hours



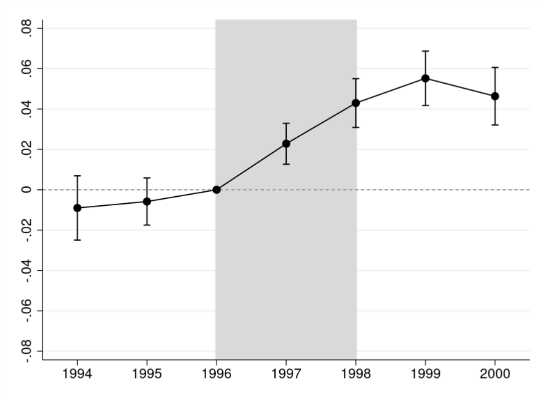
(H) Sales



(I) Sales per Worker



(J) Sales per Hour



Note: The figures display the treatment effects of the 1996 reform on firm outcomes, estimated using a difference-in-differences method as specified in Equation 2.2, providing both point estimates and confidence intervals. When confidence intervals are not visible, it indicates high precision of the estimate. The outcomes are expressed in absolute values for hours and in log for the other outcomes. The coefficients are normalized to zero in 1996. The shaded area represents the reform period during which standard hours were reduced from 44 to 40 over two years. Treatment status is based on the average share of hours treated from 1994 to 1996, as defined in Equation 2.1. Treated firms are defined as those with a positive value in the share of hours treated, while the control group consists of firms with a share of hours treated equal to zero. All regressions include firm fixed effects and sector-year fixed effects, with standard errors clustered at the firm level. Appendix Table B.3 provides the corresponding results in a regression table.

Table 2.3: The Effects of the 1996 National Reform: Average for Post-Reform Years

(a) Hours and Labor Cost

	Hours			Labor Cost	
	Standard	Actual	Overtime	Monthly salary	Wage
	(1)	(2)	(3)	(4)	(5)
$Treat \times Post$	-2.757***	-2.425***	0.010	0.003	0.061***
	(0.009)	(0.016)	(0.015)	(0.002)	(0.002)
Mean Outcome	41.3	40.7	0.2	6.3	1.1
R-squared	0.82	0.67	0.62	0.81	0.81
Observations	398,791	398,791	398,791	398,791	398,791

(b) Labor Input and Sales

	Labor Input		Sales		
	Employment	Total Hours	Total	Per Worker	Per Hour
	(6)	(7)	(8)	(9)	(10)
$Treat \times Post$	-0.020***	-0.090***	-0.040***	-0.018***	0.044***
	(0.004)	(0.005)	(0.005)	(0.005)	(0.005)
Mean Outcome	1.6	6.5	12.6	10.9	6.1
R-squared	0.95	0.93	0.96	0.90	0.89
Observations	398,791	398,791	398,791	398,791	398,791

Note: The tables display the results of the working hour reduction the 1996 reform, as estimated in Equation 2.3. All outcomes are in log, except for the hour measures in the columns (1)-(3). The variable $Treat$ takes 1 for the treated firms and 0 for the control firms. $Post$ takes 1 the years after 1997 and 0 otherwise. The outcome variables are regressed on the interaction of $Treat$ and $Post$ to provide the difference-in-differences estimate of the effects of the working hour reductions. Standard errors are clustered at the firm level. Standard errors in parentheses; * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

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2.5.6 Employment Effects: Worker Flows

One of the key findings is that firms reduced their employment in response to the mandated reduction standard hours and the subsequent increase in labor costs. Firms have two ways for adjusting their workforce: letting incumbent workers go or refraining from hiring new ones. To differentiate between these two channels, we leverage the worker-level dataset to track the number of workers who left and joined each firm each year. Then, we calculated the proportion of workers who separated from and were newly hired by the firm relative to the previous year's employment size. The estimated effects on worker flows are presented in Figure 2.5.[30] The figure shows that *both* separation rates and hiring rates decreased.

[30] For the corresponding regression table, see Appendix Table B.4. We estimate the average effect on these worker flows in difference-in-differences methodology, interacting the treatment group dummy with a post-1997 dummy, as specified in Equation 2.3. Due to the existence of the pre-existing trend in worker flows, we also include firm-specific linear trends to ensure our estimates are not driven by differential trends. Since the outcome measures the change from one year to the next, the data points where the firm does not exist in consecutive years are excluded from the estimation.

However, the reduction in the hiring of new workers is considerably larger than the decrease in separations (2.4 percentage points versus 0.9 percentage points). This finding indicates that firms adjusted their total employment size by refraining from new hiring rather than firing incumbent workers. Strict regulations on dismissals in Portugal explain why firing was not the main adjustment mechanism. In fact, the separation rate decreased, likely due to fewer voluntary quits as workers could enjoy shorter hours without a decrease in monthly income.

The overall reduction in worker flows goes in the opposite direction to the prediction that employers would seek to substitute workers hired under the previous standard with new ones under the new standard (Crépon and Kramarz, 2002).[31] This result is likely a consequence of the low flexibility of the Portuguese labor market, where reducing new hiring is a more feasible adjustment mechanism for firms. Furthermore, these findings emphasize the importance of conducting the analysis *at the firm level*. Worker-level analysis, as seen in studies such as Raposo and van Ours (2010) and Crépon and Kramarz (2002), would only capture the separation aspect, missing the impact on new hiring, which we found to be the primary driver behind the negative employment response of firms.

Figure 2.5: The Effects on Separation and New Hiring

Note: The figure shows the effects of the 1996 reform on worker flows at the firm level. The outcomes are share of workers separated and share of new workers hired, relative to the previous year's employment size. The regression employs data from the years 1994-2000 and difference-in-differences comparing treated and control firms. We estimate the average treatment effects interacting the post-1997 dummies with the treatment dummy variable (as in the equation 2.3), and add firm-specific trends to deal with the potential pre-existing trend observed for the outcomes. See Table B.4 for the corresponding regression table. Standard errors are reported in parentheses; * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

[31] However, such theory assumes that firms can offer lower wages to newly hired workers, which in Portugal would be made difficult by collective agreements.

2.5.7 Mechanisms of Labor Productivity Gains

Another important finding is the substantial increase in hourly labor productivity, measured by sales per hour, among firms that reduced working hours. We discuss potential mechanisms that could explain this result. We particularly focus on price adjustment, capital use, production function concavity, work intensification, and worker composition. We offer indirect evidence to examine the plausibility of some of these mechanisms.

Price adjustment. Our measure of output is based on *nominal* sales, which might be influenced by potential price effects. While perfect product market competition assumes firms are price takers, recent empirical evidence suggests that some firms may be able to increase prices in response to rising labor costs (Harasztosi and Lindner, 2019; Renkin et al., 2020). In this case, the observed increase in labor productivity may also capture the higher unit price of the produced goods. The data at our disposal unfortunately do not provide information on prices or quantity of goods sold. However, we provide an indirect test to examine the role of prices.

We study the price effects by matching the QP with the yearly output price index at the 2-digit sector level available in the EU KLEMS database.[32] The underlying idea is that if price adjustment is the primary mechanism for firms to maintain their nominal sales, sectors with a higher proportion of firms affected by the reform should experience more significant growth in prices following the reform. To investigate this, we calculate the average of firms' treated hour share (as defined in Equation 2.1) in each of the 25 2-digit sectors, weighted by each firm's sales share within the sector before 1996. We compare industries with an above-median and below-median exposure, respectively, and estimate the treatment effects using a difference-in-differences approach. The results are presented in Table 2.4. Column (1) shows that, on average, more treated industries reduced actual hours worked by approximately 1.3% in the QP. Although the coefficient on the price index (in log) is positive in column (2), it is small and does not, at least fully, explain the increase in labor productivity.[33] In the Appendix, we also present additional tests examining the heterogeneity in productivity effects across firms operating along varying degrees of product market concentration (Figure B.13). We do not find a larger increase in sales per hour in concentrated product markets where firms can more easily raise prices. This evidence further supports the idea that price increases were not the primary mechanism for firms to boost nominal productivity.

Capital use. Firms may have increased the use of capital in production in response to the reform, either to compensate for the reduction in total labor input, or substituted labor hours with capital given the higher cost of labor. This would result in the higher hourly

[32] EU KLEMS March 2011 Update, available here. The sector classification is NACE Rev. 1.1.

[33] When we adjust nominal sales using the sector-specific price index and re-estimate the treatment effects at the firm level as in 2.2, we still observe a significant increase in (real) sales per hour of similar magnitude.

Table 2.4: Sector-level Effects on Capital and Price

	Actual hours	Price Index	Labor Services	Capital Services
	(1)	(2)	(3)	(4)
$Treat \times Post$	-0.013***	0.006	-0.017	0.014
	(0.003)	(0.014)	(0.019)	(0.013)
R-squared	0.98	0.92	0.87	0.99
Observations	175	175	175	175

Note: The table illustrates the impact of the 1996 reform on sector-level price levels, labor services, and capital services. Each observation represents a sector, with the sample consisting of 25 sectors spanning manufacturing, wholesale, retail, and hotels and restaurants. Mean actual hours in the first column come from the QP. Data for price index, labor services, and capital services in the second to fourth columns come from the EU KLEMS database. Treated sectors are those with average treated hour shares above the median, as defined in Equation 2.1 at the sector level, while control sectors are those at or below the median. The specification is based on Equation 2.3, adding time fixed effects, sector fixed effects, and sector-specific linear trends. Standard errors are in parentheses; * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

labor productivity because more capital is used per unit of labor.

Since there is no information on capital available in our data over the period we study, we again examine sector-level capital information from the EU KLEMS. Identical to the approach for prices, we compare more treated sectors to less treated sectors as measured by the average of firm-level hours share treated. Table 2.4 provides the results. First, column (3) shows the decrease in labor services due to the reform in the EU KLEMS database. The coefficient is of similar magnitude to the decrease in hours in the QP in the first column, although it is insignificant due to imprecise estimation. In column (4), there is an insignificant but *positive* coefficient on the estimated effect on capital services, which aligns with the idea that capital could have partially substituted for labor. Moreover, the effect on capital services matches in magnitude with the decrease in labor services. Table B.5 in the appendix separately estimates the impact on capital services for non-ICT and ICT capital and shows a larger effect on ICT capital.

In line with this evidence, we posit that, in the short run, intensifying capital use is likely easier for firms that already use capital in production. Indeed, as shown in Appendix Figure B.14, we observe larger negative employment effects and higher productivity gains in firms operating in capital-intensive sectors. This supports the notion that firms substituted capital for labor, leading to increased hourly labor productivity.

Production function concavity. One simple explanation for the increased hourly labor productivity is that the production function is concave with respect to labor hour input, an assumption made in standard economic models. Under such assumption of decreasing marginal productivity, even if working hours are reduced from the equilibrium level, the decrease in output is proportionally smaller, leading to a higher average output per hour.

Some research has found evidence of such concavity, focusing on specific occupations and tasks and examining the individual-level relationship between output and hours (Brachet et al., 2012; Pencavel, 2014; Collewet and Sauermann, 2017). These studies often emphasize the role of fatigue as a source of decreasing productivity. Our findings offer support for the presence of such concavity in production, but within a more generalized context.

Work intensification. Another potential explanation is that work intensity increased, leading to higher labor productivity per hour. This could occur, for instance, if workers exert more effort (Green and McIntosh, 2001; Lazear et al., 2016). In certain contexts, with a higher level of effort per working hour, workers may complete the same tasks in a shorter amount of time. This hypothesis is also related to the concept of efficiency wages (Stiglitz, 1976), as the wage rate increased for workers affected by the reform, which could incentivize them to exert more effort. As highlighted by Askenazy (2004), work intensification can also result from changes in work schedules, such as the change in work time or the introduction of more precarious work arrangements across different days and weeks. However, as shown in Table B.11 and B.12 in the Appendix, the proportion of workers engaged in shift work, night work, or weekend work in the Labor Force Survey did not increase over the period of the reform. Moreover, Lepinteur (2019) examined subjective well-being and found that workers impacted by the 1996 Portuguese reform experienced increased job satisfaction, suggesting that they might not have been put under pressure to make up for the lost hours.[34] While it remains inconclusive, this evidence is rather inconsistent with the mechanism of work intensification.

Improvement in worker quality. The increase in labor productivity may be attributed to changes in the composition of workers. Firms could have sought to increase their average worker quality to maximize the output for each unit of labor input. Table 2.5 displays the effects of the reform on worker composition. As a proxy for worker quality, we use workers' education level recorded in the data. Column (1) shows that the share of workers with a high school diploma or higher did not increase, and similarly, column (2) provides no evidence of an increase in the share of college graduates. This suggests that the improvement in labor productivity is likely a result of efficiency gains within a given set of worker qualities rather than a change in the composition of worker qualities. Finally, in columns (3)-(5), we also test the age and gender composition of workers and find no significant changes in these demographic characteristics.

In summary, the increase in labor productivity may arise from multiple sources simultaneously. First, it is consistent with the concavity of the production function. There is a lack of strong evidence to suggest work intensification or changes in worker composition. Beyond these mechanisms related to labor input, we find indirect but consistent evidence

[34]The paper also finds that subjective well-being increased not only with respect to working hours but also with respect to working conditions.

Table 2.5: Effects of 1996 National Reform on Worker Composition

	Share of Workers				
	High school+	College+	Age>=50	Age<=25	Female
	(1)	(2)	(3)	(4)	(5)
$Treat \times Post$	-0.001	0.001	0.000	0.000	0.002
	(0.002)	(0.001)	(0.002)	(0.003)	(0.002)
R-squared	0.89	0.90	0.90	0.85	0.93
Observations	398,788	398,788	398,788	398,788	398,788

Note: The regression table illustrates the impact of the 1996 reform on the composition of workers within firms. The first two columns assess worker quality, measured by educational qualifications: high school degree or above in column (1) and college degree or above in column (2). The last three columns focus on demographic characteristics: the share of workers aged 50 or above in column (3), aged 25 or below in column (4), and the share of female workers in column (5). The estimation is based on Equation 2.3, where we further add firm-specific trends. $Treat$ indicates 1 for treated firms and 0 for control firms, and $Post$ is a dummy variable indicating years in the post-treatment period from 1997. Standard errors are clustered at the firm level. Standard errors in parentheses; * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

of capital substituting for labor hours in sector-level analysis. We do not find evidence that firms are increasing nominal sales per hour through price increases, indicating that these productivity gains are realized in real terms.

2.5.8 Heterogeneous Effects

We present additional heterogeneity in the effects on employment, sales and output in Figure B.15. We focus on dimensions related to firm quality (measured as pre-reform mean wages), firm size, and sector. In brief, the initial quality of firms does not appear to be a significant source of heterogeneity; the coefficients for all three outcomes are relatively similar. Conversely, smaller firms with fewer than 10 employees displayed the smallest negative impact on employment. This could be attributed to difficulties in adjusting the workforce size, where each worker's contribution is more significant. These smaller firms also experienced a relatively substantial decline in sales. In terms of sectors, the manufacturing sector did not reduce employment in response to the reform, while the hotel and restaurant sector was most severely impacted in terms of both employment and sales. Notably, it was also the only sector where no increase in hourly labor productivity is observed.

2.6 Collective-Agreement Reforms

The transition from a 44-hour to a 40-hour workweek was initially attempted through collective bargaining. In this section, we adopt a long-term perspective starting from the 1980s to encompass the complete institutional process that ultimately led to the 1996 reform. Crucially, this approach enables us to estimate the effect of collective agreement reforms occurring between 1991 and 1995, an aspect that has been neglected in the previous literature. From this, we obtain a different parameter: the effect of a working hours reform implemented through collective, voluntary regulations for the sample of firms in sectors that engaged in negotiations for reduced hours.

This estimation is built on the long-term institutional context described in Figure 2.1. In a difference-in-differences approach, we compare the outcomes of firms affected by the national reform or collective agreements with those firms that had lower working hours since the 1980s. This approach aligns with the previous section in spirit but with several distinctions: i) the studied period covers a longer duration, from 1986 to 2000; ii) the control firms exclusively comprise those with consistently low hours throughout the entire period; and iii) treated firms are divided between those first impacted by a collective agreement reform ("early adopters") and those only impacted later by the national reform ("late adopters").

2.6.1 Treatment Definition

Data preparation. Our data lacks information on standard hours before 1994. Therefore, we impute standard hours from actual hours worked in the month, following a methodology outlined in Appendix Section B.3.3. The objective of the procedure is to obtain hours closer to standard hours that are more robust to variations in the number of working days in the reference month across different years.

To identify firms treated through collective agreements, we exploit collective agreement codes associated with each worker in our dataset. These codes can change when a collective agreement is renewed, split, or merged with another. While many codes remain consistent throughout the period, a substantial portion (roughly 40%) undergoes a change at least once over time. To address these inconsistencies, we leverage the worker-level panel structure and track the entire flow of collective agreement codes. We assign a new consistent code for the collective agreements that underwent changes in the code. In cases of splits or merges, we use a snowballing approach: we combine these agreements and assign a new code. This allows us to reconstruct a comprehensive, balanced panel of collective agreements covering the studied period. Appendix Section B.3.4 provides a detailed explanation of this process. In our final sample, there are a total of 164 newly defined collective agreements.

Defining groups. We first restrict our sample to firms that we observe from the beginning of the sample period in 1986. As a control group, we use firms that were less affected by the process of reducing working hours due to having low working hours from the outset. We define this control group as firms in the lowest quartile of average standard hours between 1986 and 1996. We use the bottom quartile of firms as our threshold, rather than a fixed 40-hour mark, to expand the size of the control group. It allows us to include sector-year fixed effects in the estimation and improve precision in the estimates.[35] In this way, our control group includes both firms that were entirely unaffected by the reduction process and those that were *less* affected. Additionally, we base our group definitions on average standard hours from 1986 to 1996, covering a longer time span rather than just the period before 1991, in order to minimize the impact of mean reversion.[36]

We define collective agreement treated (CA-treated) as those not in the control group and covered by collective agreements that reduced the *mode* of standard hours by at least 2 hours during the period 1991-1996. The year in which the mode declined or when the proportion of workers at the mode dropped by 10 percentage points is considered the initial treatment year.[37] It is important to note that it is an upper-level bargaining process that negotiated the reduction of working hours, rather than individual firms and workers. Firm-level agreements are rather rare in Portugal. The collective agreement usually overlaps, at least, with a well-defined economic sector and only sometimes is declined differently across geographical areas (districts or municipalities). With the extension rule approved by the government, which often happens in practice, it covers all firms and workers within its scope.

The remaining firms are classified as reform-treated firms (Reform-treated). These firms are not part of the control group and belong to collective agreements where the mode of standard hours did not change by more than 2 hours during the period from 1991 to 1996.

Descriptive Statistics. Table B.11 provides the characteristics of the firms that belong to each of the three different groups. In general, the firms in the control group are relatively more productive, larger, pay higher wages and record more sales. Firms affected by collective agreement changes are more similar in terms of observable characteristics to those that are impacted by the reform. Nearly half of the firms affected by collective agreement reforms are located in the Lisbon metropolitan area, which is similar to firms in the control group. In terms of the sectoral distribution, collective agreement that autonomously change hours are concentrated in the manufacturing and wholesale sectors. Appendix Figure B.16 shows graphically the distribution of the treatment groups across sectors.

[35] Although the choice of the lowest quartile cutoff is somewhat arbitrary, our results remain robust when considering alternative thresholds like the bottom fifth or the bottom third.

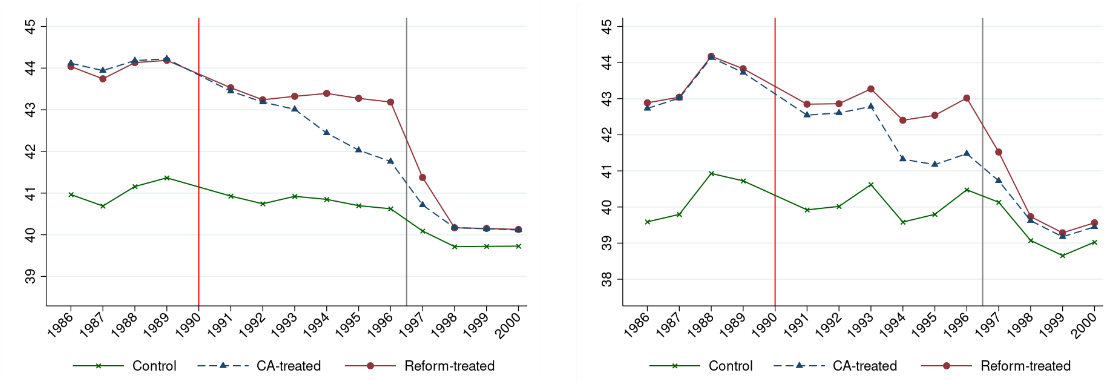
[36] Mean reversion is a concern because our standard hours prior to 1994 are imputed from actual hours worked, which can fluctuate due to economic cycles, year-to-year economic variations, and the number of working days in the reference month. If we defined the control group based solely on the average standard hours between 1986 and 1989, it would result in a sudden jump in hours (and economic outcomes) right after 1989 due to a tendency to return to the mean. This is why we choose to calculate averages over a longer time frame. We have conducted tests to confirm the consistency of our findings when considering different time frames for computing the average.

[37] In a few cases, negotiations for collective agreements may have started in 1990, and the treatment began in 1991. To identify these situations, we calculate the mode of standard hours before 1989 (rather than just for 1989 to avoid potential mean reversions). If the mode of hours before 1989 was 45 hours, we assume that the treatment via the collective agreement began in 1991 if the mode reduced by 2 hours or more by 1991. We require this 2-hour reduction to differentiate it from the first reform's effect, which lowered the maximum standard hours from 45 to 44. We also consider an agreement as treated in 1991 if the pre-1989 mode was 44 hours or less, and the mode decreased by at least 1 hour by 1991.

Figure 2.6 shows a clear distinction in the evolution of hours across the three groups of firms. The control group exhibits relatively stable hours around 41 hours, with a slight decline after the national reform in 1996 to below 40 hours. In contrast, the blue and red lines depict the separate evolution of firms affected by collective agreements and those only impacted by the national reform, respectively. Firms affected by collective agreement reforms started reducing hours earlier, having 42 hours in 1996. Firms impacted by the national reform maintained stable hours during the bargaining period, but their hours reduced significantly from around 43 to 40 hours after 1996.

Figure 2.6: Evolution of Hours Across Treatment Groups

(A) Mean Standard Hours (B) Mean Actual Hours



Note: The figure shows the evolution of the average firm-level mean hours for the three groups: Control, CA-treated, and Reform-treated. The Control group comprises firms in the lowest quartile of average standard hours between 1986 and 1996. CA-treated firms are those not in the Control group and are in collective agreements where the mode of standard hours changed by at least 2 hours between 1991 and 1996. The remaining firms are Reform-treated firms. Panel (A) shows the evolution of standard hours, while panel (B) shows that of actual hours worked. These mean hours are computed from workers who had at least 30 hours in the respective hour category. The large fluctuations in actual hours are due to the variations in the number of working days within the reference month across different years, as shown in Appendix Figure B.9.

2.6.2 Estimation method

Similar to Daruich et al. (2023), we employ a staggered difference-in-differences approach that takes into account the differential timing of hour reductions across collective agreements. Most of these agreements began to reduce standard hours by 1993 (roughly 80%). Appendix Table B.13 summarizes the number of firms and collective agreements according to the first year of the reduction in hours. To account for the different starting years of treatment, we employ the estimator suggested by Callaway and Sant'Anna (2021) and avoid comparisons with earlier-treated units that could confound the estimates. Some collective agreements did not fully reduce to 40 hours by 1996 and were subject to further reduction in hours due to the 1996 reform. For this reason, we study only up to 1996 to isolate the effects of collective agreements.

Treated firms are all those that reduced standard hours by collective agreements. Only firms in the control group, i.e., with consistently low hours throughout the period, are used as control units. However, including not-yet-treated units in the control unit does not alters our results. Firms treated only by the national reform in 1996 are not part of the control group.

We present below the results on the effects of reducing working hours through collective agreements. Table 2.6 provides the average treatment effects. In Appendix Section B.6, we also present the re-estimated results for firms treated by the 1996 national reform and not by collective agreements. Overall, the results are qualitatively similar to those presented in the previous section.[38]

2.6.3 Results of Collective Agreement Reform

Hours, wages and salaries. Figure 2.7 shows the effects of the collective agreement treatment on hours, wages, and monthly salary. Panel (A) shows that, compared to the control group, mean standard hours were gradually and stably reduced. Cumulatively, treated firms reduced hours by approximately 1.5h after 5 years, equivalent to roughly a 4% reduction. Note that the reduction in hours is relatively small for two reasons: first, not all collective agreements fully reduced hours to 40 by 1996 but only, for example, to 42. Second, in our definition, a part of the control group firms are “less treated” with on average higher hours than 40. Actual hours worked per week shown in panel (B) display a similar pattern.[39] Panel (C) shows that there was no response in terms of overtime hours. Panel (D) confirms that the monthly remuneration of workers at the collective bargaining-treated firms were not reduced.[40] Finally, Panel (E) shows that the mean wage rate of the treated firms increased by about 4% after 5 years since the treatment, relative to the control group.

Employment, sales and productivity. Panel (F) shows that firms treated by the collective agreement and control firms had similar employment trends before the treatment. Overall, we estimate no significant effects on employment during the gradual reduction in working hours. This contrasts with the finding of the negative employment effects of the

[38]This serves as a robustness check of the previous estimation, using a different sample of firms (those in the sample since 1986) and over a longer period. We find a decline in working hours, an increase in hourly wages, and moderate negative effects on employment. The only notable difference is the smaller effect on output, which is closer to zero and insignificant when controlling for differential pre-trends.

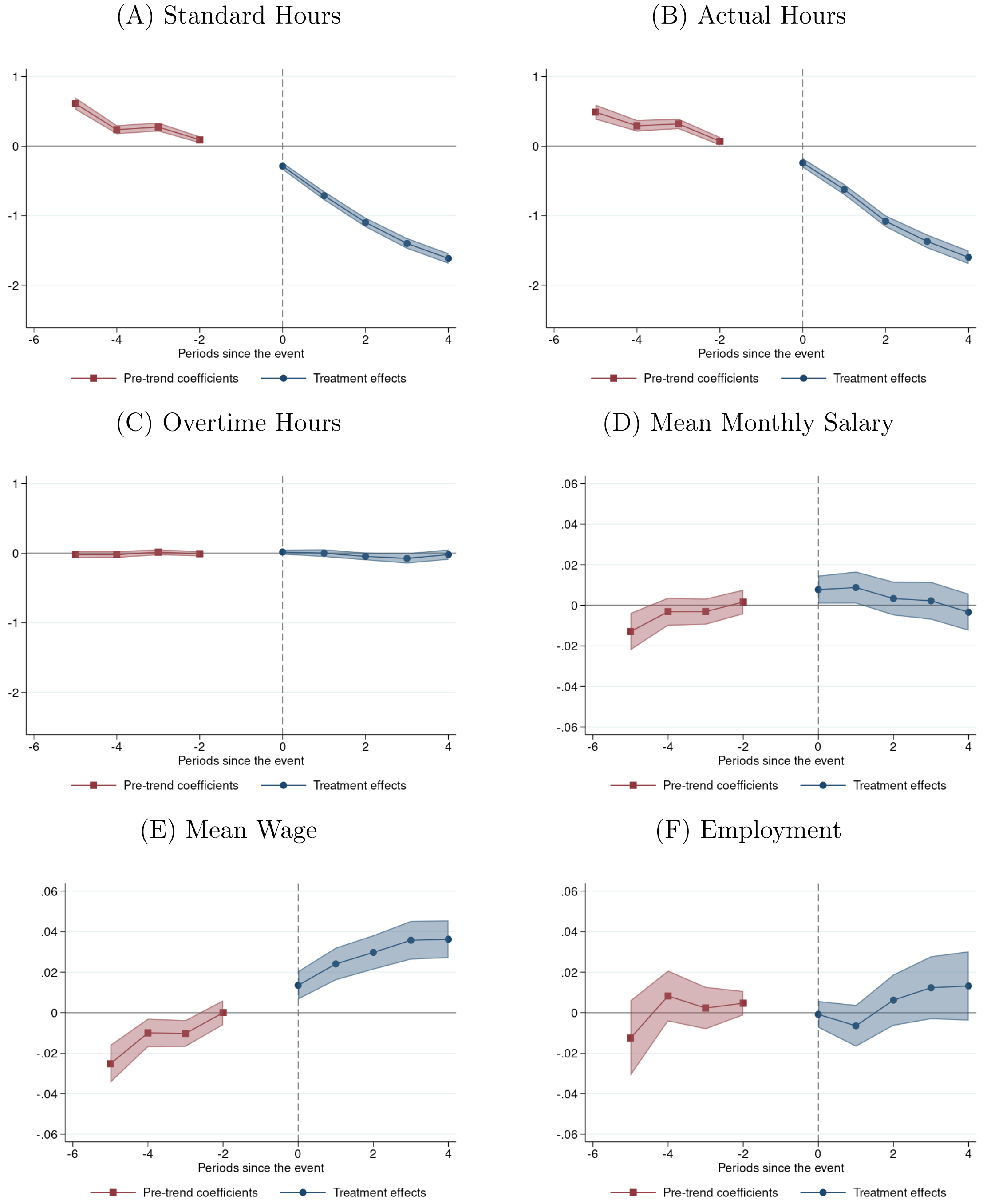
[39]There is a small differential pre-trends before the treatment for hours. This is likely attributable to factors affecting hours that are *common* between treated and control units (e.g., change in number of working days), which are bigger in the absolute value for the firms with longer hours. In particular, the number of working days in the reference month decreased from 23 to 21 days from 1989 to 1991. This created a systemic decline in hours (and increase in calculated hourly wages), but in absolute terms, this impact was greater for firms with shorter hours.

[40]The figure shows that, if anything, there was a small immediate increase in salaries at least for the first two years since the reform. This is likely due to the (periodical) negotiation on wages that might have been simultaneously bargained upon in the process.

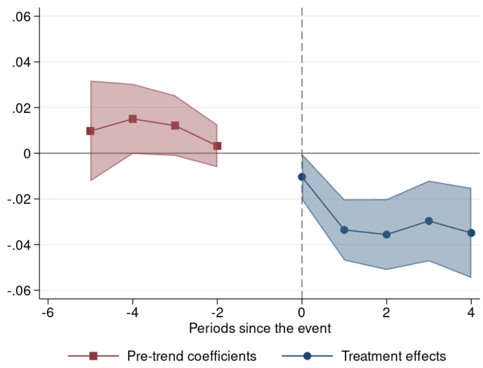
1996 reform. With stable employment levels and decreasing per-worker hours, total hours per firm decreased by close to 4%, as shown in panel (G).

The output measured by nominal sales in panel (H) is not significantly impacted by the treatment. Absent any price adjustment, this would indicate that firms could maintain the level of production with smaller number of working hours, which has significant implication for productivity. Panel (I) confirms that per-worker productivity did not drop significantly after the treatment, consistent with the results on the employment and sales. This necessarily implies per-hour sales gradually rose for treated firms, as confirmed in panel (J). After 5 years since the treatment started, there is nearly 4% increase in the hourly labor productivity, which matches virtually the full the increase in the hourly labor cost.

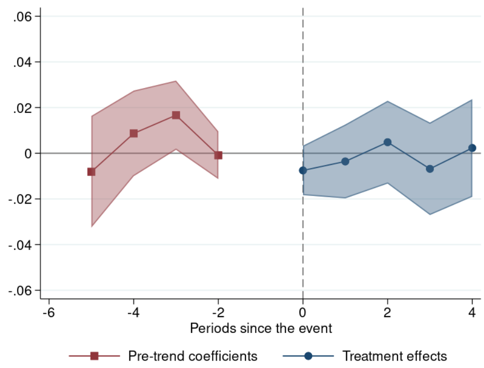
Figure 2.7: Effect on Hours, Wages and Salaries of Collective Agreement Reforms



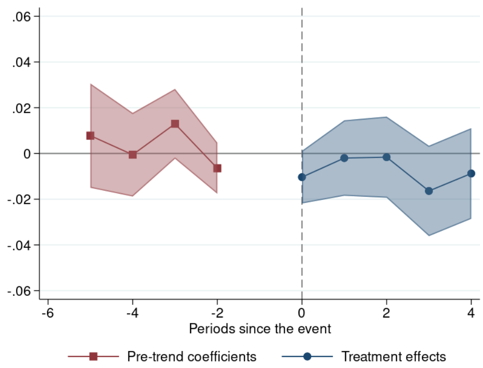
(G) Total Hours



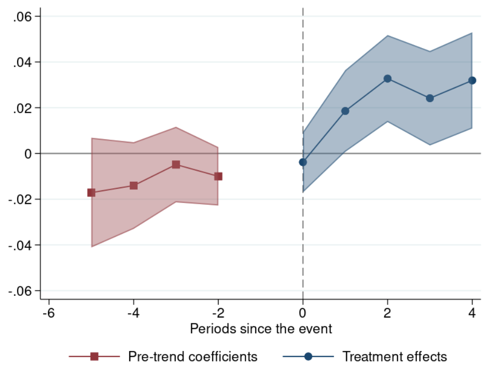
(H) Sales



(I) Sales per Worker



(J) Sales per Hour



Note: The figure shows the dynamic effects of the reduction in working hours through collective agreement, estimated using the staggered difference-in-differences by Callaway and Sant'Anna (2021), where the control group are firms at the bottom fourth of the distribution of the mean standard hours over 1986-1996. The figure shows 5 years of dynamic treatment effects as well as for 4 years of pre-treatment periods. The outcomes are in log except for hours.

Table 2.6: Average Treatment Effects of Collective Agreement Reforms

(A) Hours and Labor Cost

	Hours			Labor Cost	
	Standard	Actual	Overtime	Wage	Monthly salary
	(1)	(2)	(3)	(4)	(5)
ATT	-1.308***	-1.256***	-0.038	0.032***	0.001
	(0.028)	(0.034)	(0.025)	(0.004)	(0.003)
Observations	252,406	252,406	252,406	252,406	252,406

(A) Labor Input and Sales

	Labor Input		Sales		
	Employment	Total Hour	Total	Per Worker	Per Hour
	(6)	(7)	(8)	(9)	(10)
ATT	-0.001	-0.041***	0.001	-0.001	0.035***
	(0.006)	(0.007)	(0.008)	(0.008)	(0.008)
Observations	252,406	252,406	252,406	252,406	252,406

Note: The tables summarizes the average treatment effects (ATT) of the collective agreement estimated using the staggered difference-in-differences from Callaway and Sant'Anna (2021). All outcomes are in log, except for the hour measures in the columns (1)-(3). Standard errors in parentheses; * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

2.6.4 Summary and comparison with the results of the 1996 reform

Our findings show that treatment through collective agreements also resulted in a substantial reduction in working hours. Similarly to the national reform, firms did not use overtime hours to offset the loss of regular hours due to the costly overtime premium. With monthly salaries remaining unaltered in both cases, the reduction in hours led to a substantial increase in the hourly wage rate. In the context of Portugal, the combination of the high overtime premium and the legal constraint preventing downward adjustments to monthly salaries compelled firms to accept the reduction in per-worker hours and the increase in wage rates.

Despite a substantial reduction in working hours per employee, firms did not increase employment to keep the same total amount of hours of production. As a result of the 1996 reform, both employment and sales were negatively impacted by the reduction in hours and the increase in labor cost. These firm-level results provide empirical support for the theoretical prediction that stricter limits on working hours may lead to negative scale effects resulting in decreased labor demand. On the contrary, for firms treated earlier through collective agreements, which also experienced a decrease in hours and increase in labor cost, the effect on employment and output is not distinguishable from zero.

There are several potential explanations for why firms treated through collective agreements exhibited less negative effects on employment and sales. First, the size, timing and process of treatment were different. In our estimation, the first-stage effects on hours were slightly smaller for the collective agreement treatment, and the reduction in working hours was also gradual. Similarly, the symmetric positive effect on wages was smaller, roughly half that induced by the national reform. Second, since the collective agreement was a voluntary bargaining process, sectors that would incur fewer costs from reducing hours may have been more inclined to bargain the reduction. This hypothesis resonates with the comparison of productive characteristics presented in Table B.11: firms treated through collective agreements tend to have slightly higher wages and are more productive in terms of sales per hour and per worker compared to those treated later by the reform. More productive firms or sectors may better adapt to the reduction in hours through organizational restructuring, improved work practices, and other measures. Third, the endogenous process of collective agreements may have considered the anticipated growth in product demand. Firms facing an expected decrease in future demand might find it difficulty and costly to reduce their workforce size in a regulated labor market like Portugal. In such scenarios, reducing working hours can serve as an effective means to decrease labor input without reducing overall employment levels.

Finally, an important aspect of the different effects observed between firms treated by the national reform and those treated through collective agreements relates to the different dynamics of the impact on wages and productivity. In the case of the national reform, the effect on wages (+6%) is only partially counterbalanced by an increase in hourly productivity (+4%), resulting in a 2% decrease in employment. Conversely, in the case of collective agreement, the wage increase is entirely offset by a similar increase in productivity (both roughly +3%), with no adverse impact on employment. This suggests that when wage increases are matched by a corresponding rise in hourly labor productivity, employment levels might not need to adjust. Conversely, when wages increase more than productivity, firms are less inclined to hire new workers. The comparison of the treatment through the reform and through collective agreements demonstrates that the effect on employment, therefore, depends on the ability of firms to match higher wages by hourly productivity gains through different mechanisms.

2.7 Conclusion

This paper investigates the impact of a reduction in working hours at constant monthly salaries on firms. Specifically, we investigate the Portuguese transition to the 40-hour week, by focusing on a reform that reduced standard hours from 44 to 40 hours in 1996, one of the few instances where this occurred without any compensating measures for firms. We show that firms affected by the national reform experienced a decline in both employment and sales compared to non-affected firms, mostly as a result of the increased wage rate.

However, the total labor input (i.e., employment times hours) of treated firms decreased more than proportionally relative to sales, resulting in a significant increase in hourly labor productivity.

These findings represent the first clearly identified results on how mandated reductions in standard hour impact firms. Our *firm-level* approach directly tests and finds support for the theories highlighting the role of increasing labor costs, causing negative scale effects (and potentially substitution of labor with capital). In the context of the Portuguese reform, the adjustment in employment size primarily occurred through a reduction in new hiring, rather than increased separations. Furthermore, we show that the hourly labor productivity gain might stem from the intensified use of capital, not only through the production function concavity. We find less empirical support for price adjustment, changes in average worker quality, or the introduction of alternative working time arrangements.

Importantly, the paper also shows that firms and sectors that voluntarily opted for early adoption of the hour reductions experienced different effects compared to those affected by mandatory legislative changes. Our results show that firms in sectors that voluntarily reduced working hours through collective agreements prior to the 1996 reform did not experience negative effects on employment or sales, as they were able to increase productivity to match the increase in wages. We argue that this indicates an interplay between heterogeneity in the treatment effects and the endogenous bargaining process, and not necessarily that collective agreements are a better instrument for regulating working hours. In our view, these combined results offer a key insight for the ongoing debate on the reductions on working hours and days: early-adopters and firms that voluntarily select into these reductions are likely to be those most capable of coping with (or benefiting from) the change. Extrapolating (only) from their experience could lead to a biased understanding of the overall effects (and costs) of the transition to lower hours.

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Chapter 3

Gender Differences in the Effects of Reducing Working Hours[1]

[1]I am thankful for all comments and suggestions received from Thomas Breda, Ana Rute Cardoso, Eric Maurin, François Rycx, and Alessandro Tondini. All errors are my own.

Abstract

This paper investigates gender differences in the effects of working hour reductions, examining Portugal's 1996 reform that reduced standard hours from 44 to 40 while maintaining a constant monthly salary. I find that the reform reduced the share of women preferring shorter hours and increased the incentive for women to work full-time, but this effect was not observed for men. However, analyzing job transitions and wages using exhaustive employer-employee data, I did not find that women's job separation decreased more than it did for men. The gender wage gaps within establishments did not close either, at least in the short-run. This suggests that while shorter standard hours may improve welfare more significantly for women, its effect on gender wage gaps might be limited.

Keywords: working hours, gender, worker mobility

3.1 Introduction

The literature increasingly documents gender differences in preferences regarding non-wage job characteristics (Wiswall and Zafar, 2017; Mas and Pallais, 2017; Maestas et al., 2023). This can translate into a gender wage gap due to compensating wage differentials or by giving employers more bargaining power on wages against women. Aspects of non-wage job characteristics that could be related to the gender wage gap is working hours, including overtime hours, hour flexibility, and commuting distances as related factors (Gicheva, 2013; Goldin, 2014; Cortés and Pan, 2019; Erosa et al., 2022; Wasserman, 2022; Le Barbanchon et al., 2021).

One of the most standard yet under-investigated aspects of working hours in relation to gender gaps is standard hours, which are the hours usually worked by individuals beyond which employers must pay overtime premiums. Standard hours largely determine the usual length of working hours for individuals and are an important benchmark for workers in terms of balancing their work life and leisure. In many countries, standard hours are often set at the national level, with the possibility of adjustment based on collective bargaining or by agreement between employers and employees within the given limit.

Standard hours may also have implications for gender gaps. Some workers may prefer to have shorter standard hours, which ultimately determines the choice set of a firm they are willing to work for, or it may affect the decision to separate from or stay at the current firm to find hours that are more desirable (Altonji and Paxson, 1992). Similar to other non-wage job characteristics, if female workers tend to prefer shorter hours, this could also result in a larger gender gap, such as through shorter tenures. On the other hand, since the variation in standard hours among full-time workers is often not very large, it might not significantly affect workers' decisions in firm choices. In either case, there is little empirical evidence on how standard hours affect worker mobility differentially by gender and ultimately on gender gaps. This is in part attributable to the fact that it is difficult to isolate the effect of standard hours from other job characteristics, and instances in which hours exogenously change are rare.

This paper aims to fill this gap by examining the Portuguese reform in 1996 that reduced the national standard hours from 44 to 40 hours. This is a unique opportunity to examine if there are any differences in how women and men were affected by the usual hours of work. One crucial aspect of the reform is that, due to the law, monthly salaries did not decrease even if standard hours decreased. This means that there was no simultaneous income effect for workers; thereby, gender differences in preferences about working hours play a major role in driving the gender differences in effects.

First, I examine how the reduction in usual hours worked impacted stated preferences regarding working hours using the Labor Force Survey. I find that prior to the reform, women were more likely than men to state that they wanted to work fewer hours, even if it meant a decrease in gross income, although the gender gap steadily declined from the early to mid-1990s. To examine how the reform affected hour preferences, I compare workers who were treated by the reform — those with usual hours over 40 hours — to non-treated individuals — those with usual hours at 40 hours or below — in a difference-in-differences setting. I find that treated women were less likely to state a preference to work fewer hours after the reform, but this was not the case for men. This indicates that the reduction in usual hours worked helped women achieve hours closer to their desired amount. Conversely, I also find an increase in the probability of wanting to work more hours with no marked gender differences. I interpret this result as a substitution effect: the reduction in working hours came with constant monthly earnings, implying a higher hourly wage rate. Therefore, workers were willing to supply more labor at the intensive margin. This effect is also observed in women, but it primarily comes from those working fewer than 35 hours who are willing to work more than 35 hours, indicating that the reform incentivized women in part-time work arrangements to move to more full-time work. Overall, the reduction in usual hours seemed to benefit some female workers who initially desired fewer hours. This result is consistent with Lepinteur (2019), who found an increase in subjective well-being from this Portuguese reform, more strongly for women.

Motivated by these observations from the survey, I examine whether this reform had any observable gender-specific effects on economic outcomes, particularly job separation and wages, using a linked employer-employee dataset. Contrary to the effect on stated preferences, I find little evidence that it disproportionately benefits either gender. Working hours for both male and female workers decreased to a similar extent, averaging around 1.5 hours for treated workers. The reform reduced the likelihood of separation from their establishment, but there are no significant gender differences in these effects. The hourly wage rate for workers increased significantly following the reform due to the legal constraint on monthly salary adjustments. The post-reform wage growth was lower for female workers than for male workers. However, this difference is not distinguishable from pre-existing trends.

There are multiple explanations for why, despite the indication of higher satisfaction from shortening hours for women than men, there are no observable economic consequences. One interpretation is that there was a counter-force from the labor demand side. There was a reduction in labor demand due to wage increases, and this may have impacted women more strongly. Moreover, as female workers potentially benefited more from the reduced hours, this could have given employers greater bargaining power. In summary, while the reduction in working hours through the reform benefited female workers who used to work longer than desired, this alone did not necessarily translate into a reduced separation rate for women more so than men, or a reduction in the gender gap. This suggests that standard hours may not be as important as other documented factors in driving gender gaps in employer choices and wage differentials. Alternatively, the reduction in hours with a constant monthly income may also strongly benefit men, making it difficult to detect significant gender differences in the effects. In either scenario, while the reduction in standard hours enhances overall welfare, it may not substantially contribute to closing the gender wage gap.

This paper closely relates to the literature on gender differences in preferences for various job characteristics, including preferences (or aversions) for hour flexibility and precarity, which have been gaining increasing attention lately (Wiswall and Zafar, 2017; Mas and Pallais, 2017; Maestas et al., 2023). In particular, this paper contributes to the literature associating working hours and working conditions as drivers of the gender wage gap.(Gicheva, 2013; Goldin, 2014; Cortés and Pan, 2019; Erosa et al., 2022; Wasserman, 2022). The literature mostly focuses on aspects including long working hours, overtime hours, and hours worked outside of work, which can penalize women in making occupational choices and in competing for promotions within firms. I add to this literature by highlighting the role of standard hours. Related to this paper, some study regulations on working hours and their impact on the employment of women. For example, Goldin (1988) and Kato and Kodama (2018) examine gender-specific regulations on working hours that have historically been present and lifted. Hunt (1999) estiamtes the employment effects of the reduction in working hours, disaggregated by gender. All of these works focus on employment and rely on aggregated sector or sector-occupation level data. This paper studies gender-neutral legislation, which is much more common at present, and examines its effects on hour preferences, job separations, and gender wage gaps. Finally, I use individual-level survey microdata and administrative employer-employee datasets, which provide more accurate and causally well-identified estimates.

Lastly, this paper also contributes to the long-standing literature on working hours and employment.[2] The literature has predominantly focused on the labor demand side, examining the effects on employment. Related to this paper are some studies that examine individual-level job separations. Crépon and Kramarz (2002) studies the French introduction of a 35-hour workweek, finding that affected workers were more likely to be separated from their employer. Raposo and van Ours (2010) examines the same Portuguese reform as this paper, finding a reduction in the separation rate for workers who were mildly affected by the reform.

While related to these works, this paper provides new insights by focusing more on the supply side. This aspect has been largely neglected both theoretically and empirically, with a notable exception of Goux et al. (2014), who study the effects of working hour reductions on the intensive labor supply of spouses. Moreover, I examine the gender differences in the impact of separations as well as the gender wage gap, drawing on the fact that preferences for hours might be heterogeneous across genders. I show that the reform does close the gap between desired labor supply and the actual hours supplied. Finally, the working hour

[2]Examples include Hunt (1999); Crépon and Kramarz (2002); Gonzaga et al. (2003); **?**); Varejao (2005); Skuterud (2007); Estevão and Sá (2008); Chemin and Wasmer (2009); Raposo and van Ours (2010); Raposo and Van Ours (2010); Sánchez (2013); Kawaguchi et al. (2017); Batut et al. (2023)

reform that results in higher wages, which is the case in most reforms, may potentially induce higher labor supply in terms of the intensive margin, which should also be taken into account when studying the effect of the reform on employment. On the other hand, I do not find a significant difference in job separations between genders, and the gender wage gap seems to have not closed as a result of the reform.

The paper proceeds as follows. Section 3.2 provides the institutional context of the reform in Portugal studied in this paper. Section 3.3 presents the data used in the analyses. In Section 3.4. the results on working hour preferences from the survey data are presented. Section 3.5 uses employer-employee linked data to study the gender differences in the effects of working hour reductions on job separations and gender wage gaps. Section 3.6 concludes.

3.2 Institutional Context

Standard hours in Portugal are regulated by national legislation. Prior to 1996, the maximum standard hours were set at 44 hours as per national legislation, with the possibility to lower them through collective agreements. Overtime wage premiums were set at 50% for the first hour and 75% for subsequent hours. In 1996, the newly elected government implemented the reform to reduce standard hours from 44 to 40 under national legislation. The law was enacted in July 1996 and implemented by December of the same year. The primary goal of this reform was to harmonize working hours with EU standards (Varejao, 2005). Previous attempts to achieve this reduction through collective agreements, spurred by the 1990 tripartite agreement among the government, unions and business associations, was not largely successful. A crucial aspect of the reduction in working hours is that the Portuguese law effectively prohibited employers from decreasing workers' monthly salaries when their contractual hours were reduced. Consequently, the reduction in standard hours did not lead to a corresponding decrease in monthly salaries, resulting in higher hourly wages.

Not all workers have standard hours that are set by national legislation, as collective agreements can establish lower standard hours. There are also occupation-specific regulations set by national legislation. For instance, before the 1996 reform, standard hours for office workers were set as 42 hours per week. Collective agreements in Portugal typically operate at the sector or regional level, covering the majority of workers. These agreements are negotiated between higher-level unions and business associations. With the approval of the government, which is a common practice, these agreements apply universally within their scope. According to data from the Labor Force Survey used in this study, nearly 40% of workers were already working usual hours of 40 or fewer prior to the reform.

3.3 Data and conceptual framework

3.3.1 Labor Force Survey

The first section of the paper uses the Portuguese Labor Force Survey (*Inquérito ao Emprego*). The dataset used in the study covers the years 1991 to 2000. The Labor Force Survey gathers information on work-related activities, including job search for the unemployed, from households, focusing on individuals within the active population every quarter, yielding approximately 35,000 to 37,000 observations annually. Due to frequent revisions in sampling procedures and questionnaires, there are breaks in data continuity between 1992 and 1993, and between 1997 and 1998, within the period covered by the LFS data used in this paper. Participation in the survey is compulsory by law, ensuring population representativeness through the survey design. All analyses conducted in this paper using this dataset are weighted according to the provided survey weights.

The Labor Force Survey provides detailed information on demographic characteristics such as age, gender, and educational qualifications. It also includes data on working status, income brackets, sector and occupational categories, among other details. Moreover, the survey provides various information on working hours, including usual weekly hours worked, actual hours worked in the previous week, and any extra or overtime hours performed. Crucially, the survey asks whether individuals desire to work different hours than their usual hours, and if so, how many hours they wish to work. The subsequent section will elaborate on these question items and their interpretations.

3.3.2 QP

The second part of the paper uses *Quadros de Pessoal* (QP), also known as “Lists of Personnel”. It is a matched employer-employee dataset collected annually by the Ministry of Employment. This dataset covers all workers and firms in the private sector[3]. The data used in this paper covers years between 1991 and 2000. I concentrate on this time frame because worker files are unavailable for the years 1990 and 2001, preventing observation of job-to-job transitions between adjacent years. Therefore, this paper studies only the short-run effects of the reform up to 2000. The QP provides information on workers, establishments, and firms during a reference month each year. In 1994, the reference month changed from March to October, introducing a structural break in some variables.

Regarding the information in the QP, at the worker level, the dataset provides hours worked, both standard or contractual hours and actual hours worked during the reference month, distinguishing between regular and extra hours.[4] It also provides information on monthly remuneration, which allows me to calculate hourly wages. Unfortunately, the information on standard or contractual hours is only available from 1994 onward.

[3]Self-employees are not covered by the data

[4]I obtain actual hours worked per week, I divide actual hours worked per month by 21.625.

Additionally, individual characteristics such as age, tenure at the firm, qualifications for the position, and occupation are included. Establishment-level information includes region codes and industry classifications.

3.3.3 Conceptual Framework

In standard economic models, ceteris paribus, working longer hours leads to lower utility. Denoting utility as $U(h, R)$, where h represents hours worked and R denotes revenue, with $R = wh$ and w being the wage rate, the (exogeneous) change in working hours would have the following effect on utility:

$$\begin{aligned}\frac{\partial U(h.R)}{\partial h} &= \frac{\partial U}{\partial h} + \frac{\partial U}{\partial R}\frac{\partial R}{\partial h} \\ &= U_h + U_R w\end{aligned}$$

where $U_h < 0$ and $U_R > 0$ by assumption, highlighting a fundamental trade-off between leisure and consumption. However, in the case of the Portuguese reform, monthly remuneration was not allowed to decrease, implying $\frac{\partial R}{\partial h} = 0$. Therefore, the effect on utility is solely determined by U_h, which is definitely positive in the case of hour reductions.

The utility function, particularly regarding the component of working hours, may differ between women and men, i.e., $U = U^j$ where $j = F, M$. In such a scenario, the gender differences in utility resulting from the reduction of working hours, with wage adjustment, among those working the same hours, would be:

$$\begin{aligned}U_h^F - U_h^M &= U_h^F + U_R^F w^F - (U_h^M + U_R^M w^M) \\ &= (U_h^F - U_h^F) + U_R^F w^F - U_R^M w^M\end{aligned}$$

Once more, if working hours change while remuneration remains constant, the factor affecting the gender differences in utility from the change in hours would be $U^F h - U^M h$. Therefore, investigating the Portuguese reform is interesting as it can elucidate gender differences in labor supply preferences (U_h), which may subsequently result in differential effects on economic outcomes, including quit behavior (Altonji and Paxson, 1992).

3.4 Analysis using Labor Force Survey

In this section, I analyze the preferences for working hours using the LFS.

3.4.1 Hour preferences

The unique advantage of the LFS is that it not only records workers' working hours but also asks about their desired hours of work. Specifically, one question asks employed individuals if they wish to have different working hours than their current schedule, and if so, how many hours they desire to work. Importantly, this question is asked to all individuals who are employed, not just those actively seeking new jobs.[5] Therefore, this aspect of the questionnaire remains unaffected by changes in job search behavior that may be influenced by national reforms.

The exact question items related to the working hour preferences are as follows:

- *Would you like to work more or fewer hours per week than you currently do (with a pay adjustment)?* (until 1997)
- *Would you like to work more or fewer hours per week than you currently do?* (since 1998)
- *How many hours would you be willing to work per week?*

It is important to discuss how the question is framed. Until 1997, the LFS posed the question: "Would you like to work more or fewer hours per week than you currently do (with a pay adjustment)?". This question included the phrase "with pay adjustment", indicating that any change in working hours would result in a corresponding change in earnings. Therefore, if an individual indicated a desire for fewer hours, it implies a willingness to reduce working hours even if her income decreases. This question aligns with individuals' intensive-margin labor supply decisions, considering the tradeoff between leisure and income, i.e., substitution effects. However, it does not directly measure workers' preference parameters, which may potentially differ by gender.

From 1998, the question removed the phrase "with a pay adjustment", introducing ambiguity for respondents regarding how earnings will change when working hours change. This modification made the question item difficult to interpret and, as a result, made comparisons with data collected before 1998 impossible. If individuals assumed no change in earnings, then all respondents should prefer fewer hours. Indeed, there was a sharp increase in respondents indicating a preference for shorter hours after 1998, likely due to some respondents assuming they could maintain the same earnings with fewer hours.[6]

Due to the inconsistency introduced in 1998 and the ambiguity of the question item, this paper restricts its study to the period up to the final quarter of 1997. A drawback of this approach is that in 1997, the reform only partially implemented the reduction of standard hours from 44 to 42, which was further reduced to 40 by 1998.

[5] This is a major advantage over the European Household Community Panel, a smaller-scale panel survey which also asks about desired hours of work, but only to those who are looking for a (new) job.

[6] Nonetheless, the share of workers expressing this preference remained small, indicating varied interpretations among respondents.

Based on the question regarding desired hours (up to 1997), a dummy variable is constructed, taking a value of 1 if a worker desires fewer (more) hours when their usual hours of work are longer (shorter) than their desired hours. I also use the absolute gap between desired and actual hours. Additionally, several types of preferences for different hours are examined – working full-time currently but desiring part-time, working part-time currently but desiring full-time, working full-time and still desiring full-time but preferring fewer or more hours. Full-time is defined as hours longer than 35 hours, while part-time is defined as 35 hours or below.

3.4.2 Sample and descriptive stats

The sample consists of all workers aged between 18 and 60, not working in the primary sector, public sector, or construction sector, and has hours between 30 and 60 in terms of both usual hours and actual hours worked. Table 3.1 provides the summary statistics of the sample used in the analyses in this section.

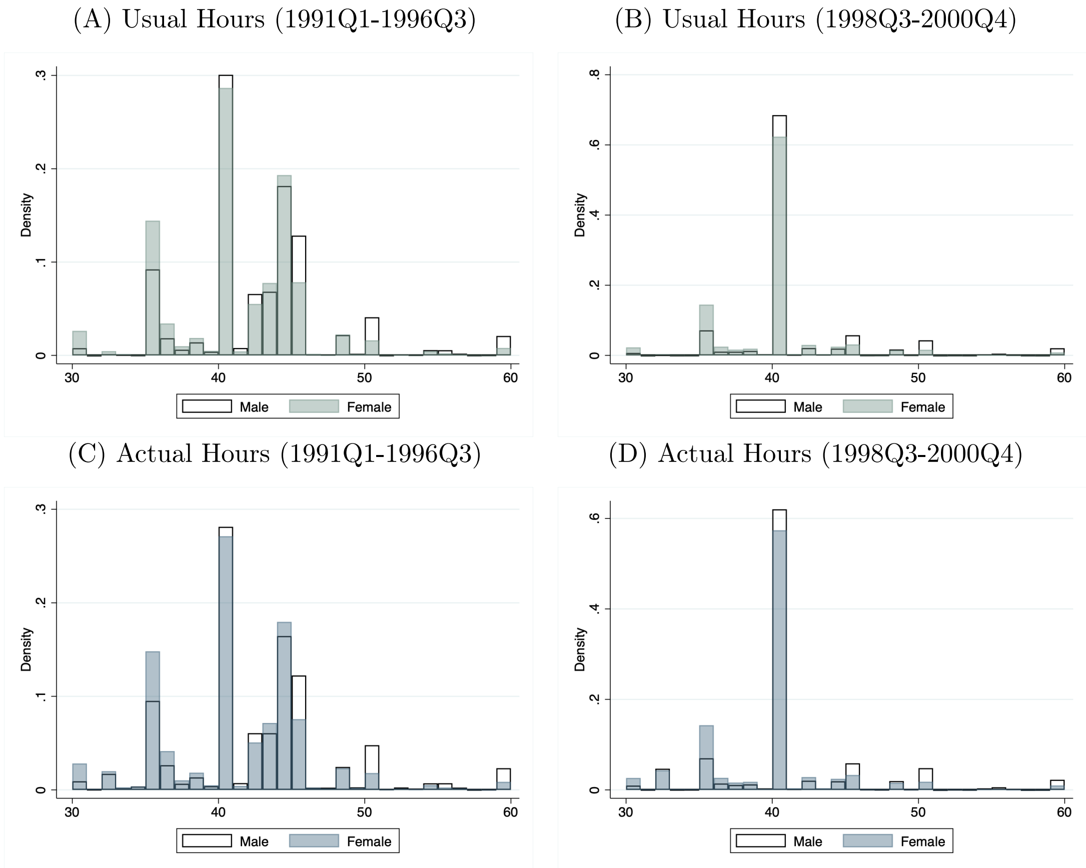
Figure 3.1 illustrates the distribution of working hours by gender, comparing the periods before and after the reform. The most frequently hours were already at 40 hours per week prior to the reform for both men and women. In both usual and actual hours worked in Panel (A) and (C), female workers were more represented at lower hours than 40 hours and less represented among those working long hours. However, generally, the distribution of working hours is relatively similar between the two genders. This is likely attributed to the institutional context in Portugal, where part-time work is relatively limited and hours are regulated by legislation and collective agreements. The reform notably resulted in a more concentrated distribution of working hours at 40 hours, by reducing the hours of those previously working more than 40 hours, as shown in Panel (B) and (D). Men continue to be slightly more represented at longer hours after the reform, exceeding the 40-hour threshold.

3.4.3 Identifying treatment groups

The objective of this section is to study the effect of the 1996 reform on the hour preferences for women and men. The empirical strategy uses those who were already working at or fewer than 40 hours as the control group, whereas workers working longer than 40 hours are considered the treated group. Because the LFS is a repeated cross-section (not a panel), it is not possible to precisely identify which workers in the data were affected by the national reform. Therefore, alternatively, I predict individual treatment status based on their personal characteristics and the characteristics of the job they have. Specifically, using periods prior to the reform (up to the third quarter of 1996), I run the following regressions:

$$Above40_{it} = \alpha + \boldsymbol{\beta X}_{it} + \epsilon_{it} \tag{3.1}$$

Figure 3.1: Distribution of working hours in LFS, by gender



Note: The figure shows the distribution of self-reported usual hours of work per week and actual hours worked in the previous week by gender, comparing the pre- and post-reform periods.
Source: Labour Force Survey (Inquérito ao Emprego)

Table 3.1: Summary Statistics

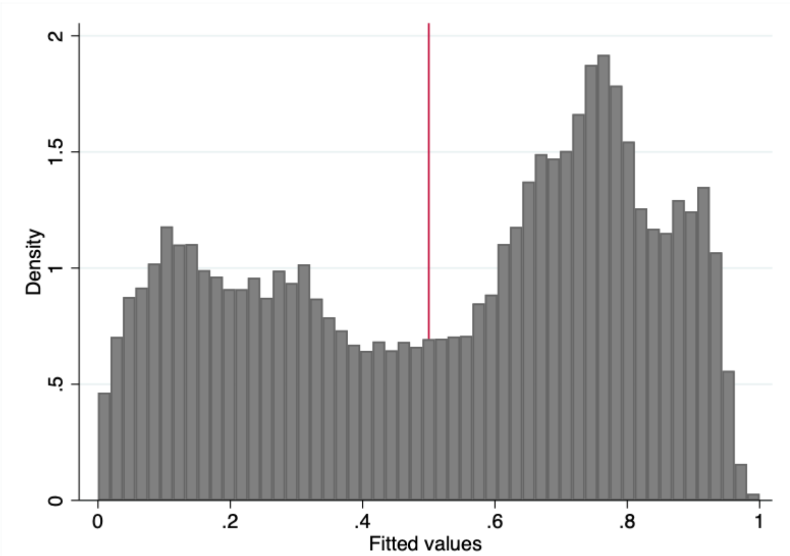
	All	Male	Female
Age	36.69 (10.73)	37.55 (10.95)	35.77 (10.41)
Female	0.48 (0.5)		
College degree	0.08	0.09	0.08
Married	0.71	0.74	0.67
Usual hours	41.55 (4.78)	42.23 (4.84)	40.82 (4.61)
Actual hours	41.4 (5.19)	42.15 (5.33)	40.59 (4.91)
Extra hours	0.392 (2.85)	0.522 (3.49)	0.252 (1.92)
Prefer different hours	0.042	0.042	0.041
Prefer less hours	0.013	0.011	0.015
Prefer more hours	0.03	0.03	0.03
Desired hours (if prefer different hours)	45.14 (13.14)	46.95 (13.47)	43.12 (12.47)
Region			
Norte	0.388	0.394	0.381
Centro	0.158	0.157	0.158
Lisboa E Vale Do Tejo	0.391	0.390	0.393
Alentejo	0.033	0.031	0.034
Algarve	0.031	0.029	0.033
Industry			
Manufacturing	0.404	0.452	0.355
Wholesale and retail trade	0.127	0.134	0.12
Hotel and restaurants	0.047	0.041	0.053
Transport, storage, and communications	0.083	0.125	0.041
Finance	0.058	0.079	0.037
Real estate	0.054	0.055	0.054
Education	0.081	0.038	0.126
Health and social work	0.082	0.04	0.125
Other	0.062	0.035	0.089
Occupation			
Members of legislative bodies, senior public service officials, directors and managers	0.018	0.029	0.006
Intellectual and scientific professions	0.063	0.064	0.062
Intermediate technical professions	0.144	0.141	0.148
Administrative employees	0.167	0.140	0.197
Personal service and security staff, personal and domestic service workers	0.171	0.126	0.220
Industrial production workers and craftsmen	0.246	0.289	0.199
Operators of industrial plants and stationary machines, drivers and assemblers	0.106	0.156	0.053
Unskilled workers in agriculture, industry, commerce, and services	0.084	0.056	0.114
Number of observations	124,182	64,435	59,747

Note: The table provides summary statistics of the final sample of workers in the Labor Force Survey. The values represent means (standard deviation) between the first quarter of 1991 to the third quarter of 1996.

where $Above40_{it}$ is a dummy variable that takes 1 if worker i has usual hours of work above 40 at time t. $\mathbf{X}_{it}$ represents a vector of individual and job characteristics. α is the intercept, β is the coefficient vector for the characteristics, and ϵ_{it} is the error term.

I use the estimated coefficients $\hat{\boldsymbol{\beta}}$ to predict the individual's probability of working above 40 hours over the entire period. A worker is defined as "treated" if this predicted probability exceeds 0.5, and "control" if the probability is 0.5 or below. The predictor variables used include female dummy, age, age squared, marital status, marital status interacted with female dummy, education levels, region, and a combination of 2-digit sector and 1-digit occupation (salary is not used due to the inconsistency before and after 1998). Our results are consistent across different sets of predictor variables. Appendix Table C.1 presents the regression results. Figure 3.2 displays the distribution of the predicted probability of working 40 hours – the distribution is not uniform, with a large volume away from the cut-off of 0.5, indicating that the dummy variable of treatment status is relatively well-predicted.

Figure 3.2: Predicted Probability for Having Usual Hours $> 40h$



Note: The figure shows the predicted probability of a worker working above 40 hours over the 1991-2000 period in the LFS, calculated using a vector of individual and job characteristics and their estimated coefficients as in Equation 3.1, which uses only data from years before the last quarter of 1996. I define a worker to be treated if the probability exceeds 0.5.

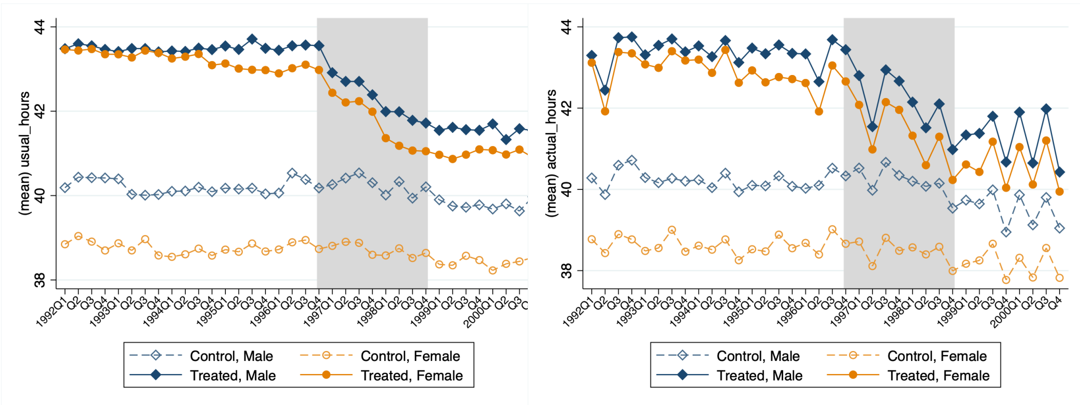
3.4.4 Aggregate series

I first graphically present the evolution of hours and hour preferences across treatment groups and genders.

Working hours. Figure 3.3 shows the evolution of the average usual hours of work per week for treated and control workers, separately for men and women. In panel (A), the average usual hours of the treated groups were close to 43.5 hours. There is a small increasing gap between men and women, with women's average usual hours being around 43 hours in the third quarter of 1996, just before the reform starts to kick in. The evolution of the usual hours is relatively stable until the reform, but after the reform, their usual hours decline by about 2 hours each. The average working hours of control groups are stable over time without being affected by the reform, but with a marked gender difference: while the average working hours for men are 40 hours, women's are slightly below 39 hours. Panel (B) shows the evolution of actual hours worked. There are more fluctuations due to the economic cycles and idiosyncratic factors, which are much larger for workers in the treated group. However, apart from these fluctuations, the levels of working hours and their evolutions are generally similar with usual hours worked, with the effects of the reform clearly visible for the treated workers. The contrast in the evolution of hours between the treated and control groups in the figure also validates the approach of predicting treatment status based on a vector of characteristics.

Figure 3.3: Evolution of Working Hours in LFS

(A) Hours usually worked (B) Actual hours worked



Note: These figures show the evolution of average hours worked, with usual hours on the left panel and actual hours worked on the right panel, across treatment status and gender.
Source: Labour Force Survey (Inquérito ao Emprego)

Preference for hours. Panel (A) in Figure 3.4 shows the evolution of the probability of preferring different hours (either less or more) than the currently working hours. At the beginning of the sample period, on average around 6-7% of men and women in the treated group stated a preference for different hours, which has gradually decreased toward the mid-1990s. There are large fluctuations across the period, indicating that the preference for working different hours is also impacted by temporary factors. Control group workers are less likely to state a preference for different hours, with a large decline observed from 1992 toward the end of 1994, which then stabilizes around 2-3%.

Panel (B) shows the evolution of preferences for shorter hours. Treated workers, who generally have longer working hours, clearly prefer shorter hours, with a gender difference:

women prefer shorter hours. However, this gap has been closing since the beginning of the sample and is not largely visible at the time of the reform.[7] Note that the reduction in the gender gap is visible in both control and treated groups, suggesting that this convergence might be due to aggregate factors such as changes in worker composition, increasing wages, normative changes, and so on. At the aggregate level, the reform does not seem to have a clear effect; in fact, the probability of preferring fewer hours has actually increased for some groups.

In Panel (C), on the other hand, the figure shows that, in general, workers with longer working hours want to work even longer. This is likely because the question asks about preferences for different hours with wage adjustments. Workers with long working hours tend to have lower wages on average, and they may want to work longer to increase their monthly income. The probability of wanting more hours is also higher for treated groups, but women in the treated group are slightly less willing to work longer hours. The probability of preferring longer hours has been slightly decreasing over time, but among the treated groups, it seems to have increased at the time of the reform.

3.4.5 Difference-in-difference results

The previous aggregate series do not provide a conclusive picture of the impact of the reform on hour preferences. While the preference for shorter hours had been more noticeable for women, it was already on a decreasing trend before the reform. On the other hand, the reform may have increased the preference for longer hours. These series capture aggregate factors and trends and are influenced by the change in worker compositions. Therefore, I formally compare the treatment and control groups using a difference-in-differences approach, accounting for these aggregate factors and controlling for potential confounders.

Econometric specification. I estimate the treatment effects using the following econometric specification:

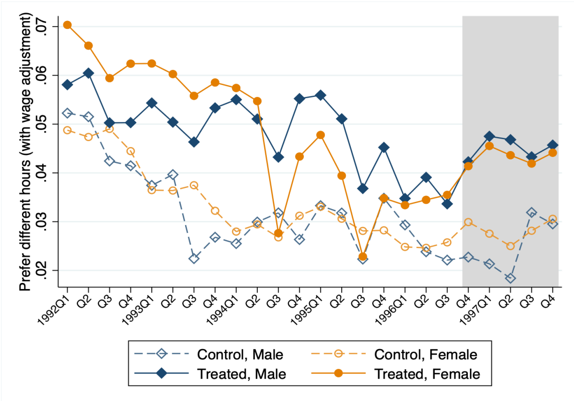
$$Y_{it} = Treat_i \cdot \sum_{t=1991Q1}^{1997Q4} \beta_t \mathbb{1}\{time = t\} + \boldsymbol{\gamma} \boldsymbol{X}_{it} + \varepsilon_{it} \tag{3.2}$$

where, Y_{it} represents the outcomes for individual i at time t (year-quarter); $Treat_i$ represents a dummy variable for workers in the treated group; $\mathbb{1}\{time = t\}$ are dummy variables for each year-quarter t (analyzed until 1997Q4 for hour preferences, while until 2000Q4 for hours worked); $\boldsymbol{X}_{it}$ represents the vector of control variables; and ε_{it} denotes the error term. The control variables $\boldsymbol{X}_{it}$ include age, age squared, educational category dummies, marital status dummies, treatment status dummy, a female dummy, sector-occupation fixed effects, and year-quarter fixed effects. Additionally, when the outcome is hour preferences, I also

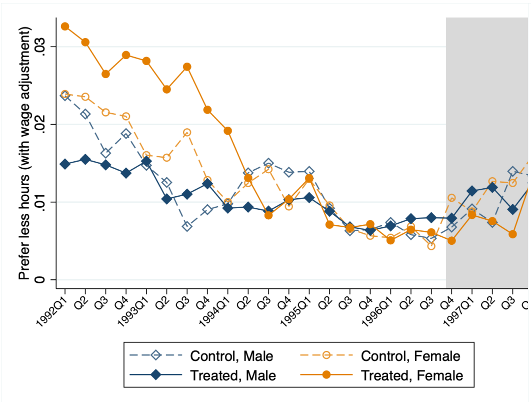
[7] However, the gender gap still exists when we condition on individuals working longer than 40 hours, indicating that among those who work longer hours, women still tended to prefer shorter hours in the mid-1990s.

Figure 3.4: Evolution of Preferences for Different Hours in LFS

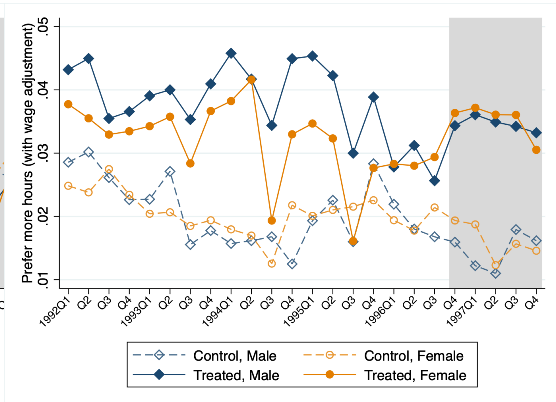
(A) Prefer to work *different* hours



(B) Prefer to work *less* hours



(C) Prefer to work *more* hours



Note: These figures show the evolution of the probability of preferring different hours than usual hours across treatment status and gender. Panel (A) displays the results for preferring either fewer or more hours, while Panels (B) and (C) respectively show the results for fewer and more hours.
Source: Labour Force Survey (Inquérito ao Emprego)

control for usual hours of work in dummy variables. The outcome of interest is β_t, which provides the difference-in-difference estimates of the effect of the working hour reform.

To provide average treatment effects, I also complement the results with the following regression:

$$Y_{it} = \beta \cdot (Treat_i \times Post_t) + \boldsymbol{\gamma X}_{it} + \varepsilon_{it} \tag{3.3}$$

where $Post_t$ is a dummy variable that takes the value 1 after the final quarter of 1996. β here provides the average treatment effects in the post-reform periods.

The effects on hours worked. Figure 3.5 presents the results of the difference-in-differences estimated from equation 3.2. Conditional on the covariates, Panel (A) shows that usual hours of work dropped slightly less than 2 hours for treated workers compared to non-treated workers. The magnitude is similar between men and women, indicating that the size of the shock was comparable between the two genders, with a small increase in usual hours for men after the full implementation of the reform since the first quarter of 1999. Similar effects for both genders provide an ideal setting to evaluate the impact of the hours reduction on hour preferences. Note that there is no major differential pre-trend for both groups before the reform. Panel (B) provides very similar results for actual hours worked, again for both genders, pointing to the substantive effect of the reform on hours worked. Panel (C) show no visible change in overtime hours worked.

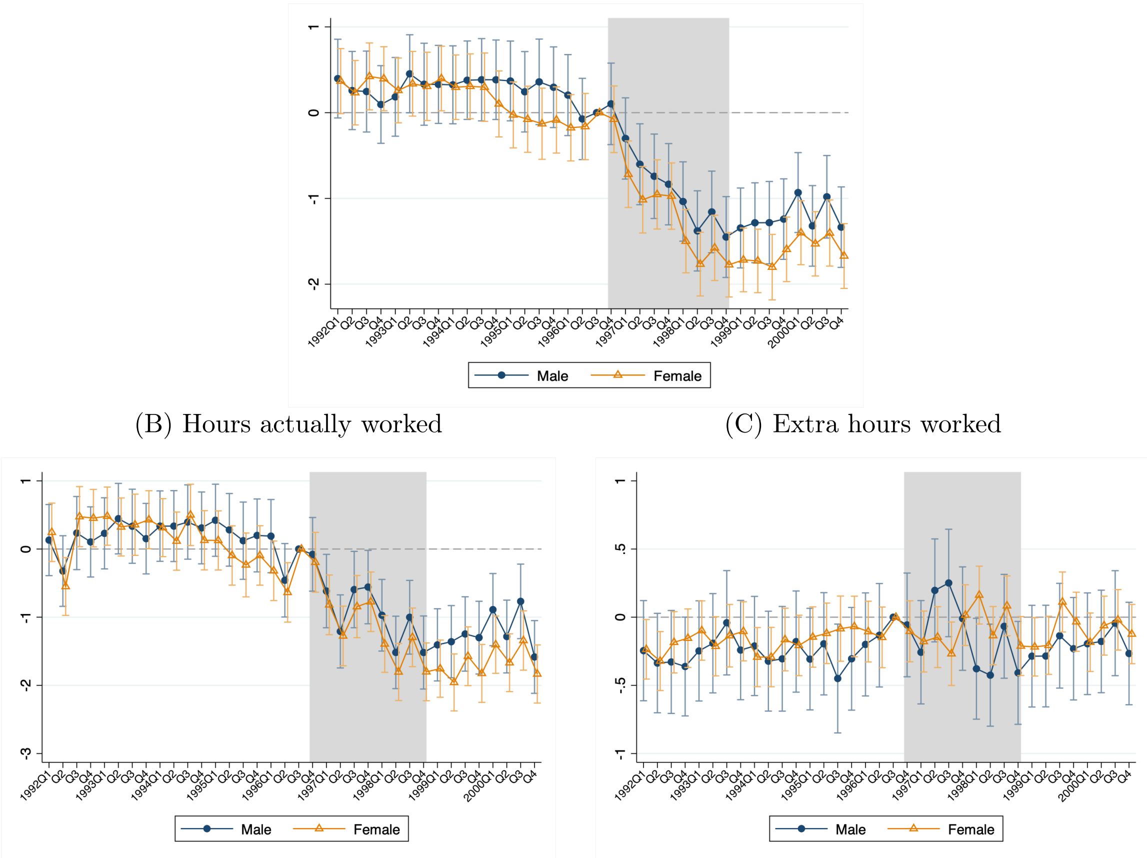
Table 3.2 displays the average effects on hours, estimated as in equation 3.3. Usual hours and actual hours worked declined on average by -1.38 hours and -1.36 hours, respectively, for the treated male workers. Overtime hours, on the other hand, increased slightly, but this increase is much smaller compared to the overall effects on actual hours. The second row shows gender differences. Women's working hours decreased more than men's, but the magnitude is relatively minor. Overtime hours increased less for women, too. Since our hour presence variables are consistently measured only up to 1997, the last three columns provide the treatment effects until 1997, showing that the total hours declined slightly less than 1 hour.

Figure 3.5: Effects of the Reform on Working Hours in LFS

(A) Hours usually worked

(B) Hours actually worked

(C) Extra hours worked



Note: These figures show the evolution of the probability of preferring different hours than usual hours across treatment status and gender. Panel (A) displays the results for preferring either fewer or more hours, while Panels (B) and (C) respectively show the results for fewer and more hours.
Source: Labour Force Survey (Inquérito ao Emprego)

Table 3.2: The average effects on working hours

	Until 2000			Until 1997		
	Usual	Actual	Overtime	Usual	Actual	Overtime
	(1)	(2)	(3)	(4)	(5)	(6)
$Treat \times Post$	-1.375***	-1.336***	0.123***	-0.807***	-0.803***	0.241***
	(0.038)	(0.044)	(0.027)	(0.061)	(0.069)	(0.046)
$Treat \times Post \times Female$	-0.060	-0.068	-0.070**	-0.018	-0.059	-0.225***
	(0.039)	(0.045)	(0.028)	(0.071)	(0.079)	(0.053)
R-squared	0.259	0.214	0.013	0.276	0.228	0.016
Observations	228,450	228,450	228,450	148,170	148,170	148,170

Note: The table displays the average treatment effects of the working hour reform, estimated using the difference-in-differences. $Treat_i$ represents a dummy variable for workers in the treated group, while $Post_t$ is a dummy variable for all periods from the final quarter of 1996 onward. All regressions include controls: age, age squared, educational category dummies, marital status dummies, treatment status dummy, female dummy, sector-occupation fixed effects, and year-quarter fixed effects. The first three columns use the entire data period available (1991-2000), and the last three columns are based on years until the last quarter of 1997, after which the hour preferences questionnaire changed. Standard errors are reported in parentheses; * $p < 0.10$. ** $p < 0.05$. *** $p < 0.01$.

The effects on hours preferences. Figure 3.6 Panel (A) shows the effects of the reform on the probability of workers stating a preference for different working hours than their current hours. There appears to be a gender difference. While there is no obvious effect detected for women, there is an increase in the chance of men stating a preference for different hours. When looked at separately for preferring shorter or longer hours, Panel (B) shows negative coefficients after the reform for preferring shorter hours, meaning that treated individuals were less likely to state a desire to work fewer hours, with a clearer break for women compared to men. While the post-reform coefficients are not significant individually, the treatment effect is jointly significant, as shown in Table 3.3 later.

On the other hand, there is an increase in the probability of preferring longer hours in Panel (C). Despite a decreasing gap between the treated and control groups around 1994 and 1995, the gap increases again following the reform. For women, the treatment effect is less obvious due to the small pre-existing trend from the last quarter of 1995.

The increases in the preference to work longer can be explained by the fact that the reform reduced hours without a decline in monthly hours. That is, the hourly wage increased for treated workers. Given that there is no income effect (since monthly hours remained unchanged), the result is consistent with substitution effects, where treated workers, especially men, wanted to increase hours to earn more. This shows a counter-intuitive consequences of the hour reduction policy on the labor *supply* side that have been neglected in the literature. While theoretical models on labor demand would point to negative employment effects from wage increases, it might also have created a mismatch in terms of working hours because of the effect on the labor supply. Panel (D), studying the effect on the gap between desired hours and usual hours, shows that the gap has actually increased for both men and women, although the effect is not statistically significant. The magnitude is not large.

Table 3.3 Panel (A) summarizes the average effects on hour preferences. Column (1) shows that the reform significantly increased male workers' probability of preferring different hours by 0.6 percentage points, but this is not the case for women (as the interaction term shows a -0.6 percentage point effect). In column (2), the reform decreased the propensity to prefer shorter hours only for women, by 0.7 percentage points. This indicates that women were more satisfied with working shorter hours. On the other hand, column (3) indicates that male workers preferred longer hours following the reform, and the response of females was similar. The gap (desired hours minus usual hours) increased for both genders, with a positive but insignificant gender-difference coefficient.

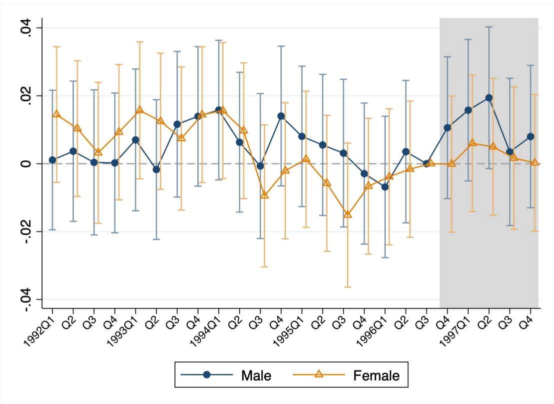
Preference for different hours encompasses various types of hour preferences. For example, preferring fewer hours might mean that a person wants to reduce their hours by several hours while staying on a full-time schedule, or it might mean that the worker prefers to move to very short, part-time hours. To understand more about these differences, Table 3.3 Panel (B) further disentangles different types of hour preferences. Column (1) shows the effect on workers preferring to move from full-time (>35 hours) to part-time (≤35 hours). It is clear that women preferred less to move to shorter hours or part-time hours because their full-time work schedule improved. On the other hand, in column (2), women preferring to work long hours reflected a change from part-time to full-time hours.[8] Given the improved conditions of previously long-hour jobs, female workers were more inclined to move to full-time jobs after the reform. Column (3) shows the case in which a person works full-time and their desired hours are still full-time, but they want to reduce hours. Here too, only women were more satisfied with shorter hours. On the other hand, column (4) shows the within-full-time increase in labor supply. The preference to work more is largely driven by this for men and less so for women. For women, the increased preference to work longer came both from part-time workers and full-time workers.[9]

[8] Note that due to the prediction approach, there are some part-time workers also in the treated group. For example, within some sectors and occupations in which the predicted probability of being treated is high, there are some workers with short-hour arrangements. This indicates that there was a new opportunity (i.e., full-time work with shorter hours) in the same occupation-sector.

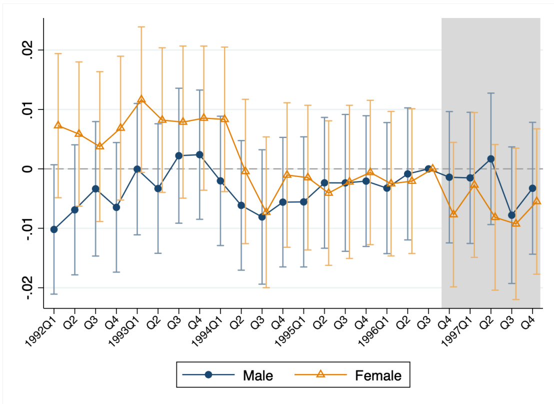
[9] All these results are robust to taking into account differential trends, including a treated group-specific linear trend, or adding the interaction of all control variables and fixed effects with a female dummy.

Figure 3.6: Effects of the Reform on Preferences for Different Hours in LFS

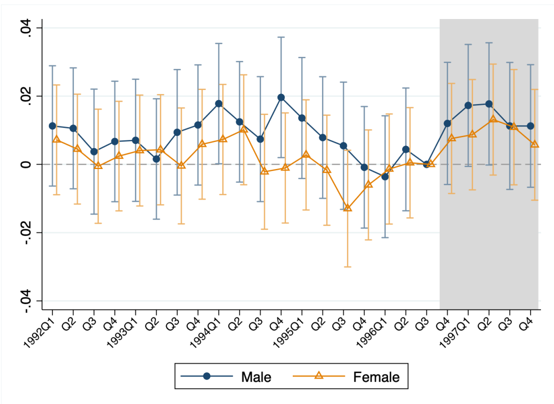
(A) Prefer *different* hours



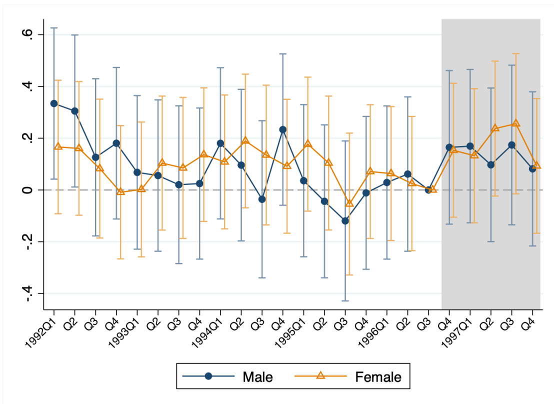
(B) Prefer *less* hours



(C) Prefer *more* hours



(D) Gap between current and desired hours



Note: These figures show the evolution of the probability of preferring different hours than usual hours across treatment status and gender. Panel (A) displays the results for preferring either fewer or more hours, while Panels (B) and (C) respectively show the results for fewer and more hours.
Source: Labour Force Survey (Inquérito ao Emprego)

Table 3.3: The Effects of the 1996 Reform

(A) Preference for different hours

	Preference for Different Hours			
	Prefer different hours (1)	Less hours (2)	More hours (3)	Hours gap (4)
Treat × *Post*	0.006**	0.001	0.005**	0.043
	(0.003)	(0.002)	(0.002)	(0.040)
Treat × *Post* × *Female*	-0.006*	-0.008***	0.002	0.024
	(0.003)	(0.002)	(0.003)	(0.046)
R-squared	0.025	0.012	0.032	0.028
Observations	148,170	148,170	148,170	148,170

(B) Different hour preferences disaggregated

	Disaggregation			
	Prefer PT (1)	Prefer FT (2)	Less within FT (3)	More within FT (4)
Treat × *Post*	-0.000	-0.001*	0.001	0.005**
	(0.001)	(0.001)	(0.001)	(0.002)
Treat × *Post* × *Female*	-0.003*	0.002***	-0.005***	-0.001
	(0.002)	(0.001)	(0.001)	(0.003)
R-squared	0.011	0.092	0.013	0.037
Observations	148,170	148,170	148,170	148,170

Note: The table displays the average treatment effects of the working hour reform, estimated using the difference-in-differences. The data spans from 1991 to the end of 1997. Panel (A) shows the effects on hour preferences and the gap between the person's desired hours and usual hours (the former minus the latter). Panel (B) disaggregates the preference for less and more hours, where PT represents part-time, defined as working 35 hours or below, and FT is full-time, defined as working longer than 35 hours. *Treat* represents a dummy variable for workers in the treated group, while *Post* is a dummy variable for all periods from the final quarter of 1996 onward. *Female* is a dummy for female workers. All regressions include controls: age, age squared, educational category dummies, marital status dummies, treatment status dummy, female dummy, usual hours dummies, sector-occupation fixed effects, and year-quarter fixed effects. Standard errors are reported in parentheses; * $p < 0.10$. ** $p < 0.05$. *** $p < 0.01$.

3.4.6 Summary and implications for labor market outcomes

These results collectively demonstrate clear impacts of the national reform on gender differences in working hour preferences. Women were generally more satisfied with the reduction in working hours, as evidenced by a significant reduction in the probability of preferring shorter hours. Simultaneously, some workers in both genders significantly preferred longer hours as a result of the reform, due in part to a sharp increases in wage rate. For men, this effect is driven by those working full-time. On the other hand, for women, it also comes from those who are currently employed part-time and are willing to transition to a full-time hour schedule, as a result of improved working conditions in terms of hours (and wages) in full-time jobs. These results are generally consistent with Lepinteur (2019), which also finds increased satisfaction with the reduction in working hours, particularly for women.

3.5 Analysis using employer-employee linked data

The findings from the previous section suggest that the reduction in working hours might have helped narrow the gap between actual and desired hours for women in particular. The question remains as to whether the reform translated into observed economic outcomes and gender gaps. Hours are oftentimes difficult to adjust within the same job, and workers would change job in order to achieve desired working hours (Altonji and Paxson, 1992). This is especially the case in the context such as Portugal in which standard working hours are largely determined by collective agreements, leaving less flexibility for individual employees and employers in determining the usual hours of work.

Building on this, I hypothesize that, as a consequence of the reduction in working hours, women were more satisfied with their jobs, leading to a larger reduction in quitting their jobs relative to men. An increase in tenure would indicate higher wage growth within the workplace and potential promotions, which could contribute to the reduction in the gender wage gap. In this section, I use Portuguese linked employer-employee data to examine these questions. Specifically, I study the job separation of workers from the establishments where they work, and the gender wage gap (conditional on covariates) within the establishments.[10]

3.5.1 Sample

The analysis uses data from the years 1991 to 2000, just after the data break in 1990 and before the break in 2001. All analyses are conducted at the worker level. I exclude primary sectors, sectors with a strong public nature (i.e., utility and transport), and the construction sector.

Our focus is on analyzing the balanced panel of establishments that existed in all years between 1991 and 2000. I use a balanced panel to distinguish the separation of workers from their employers from establishment closures. Furthermore, I restrict the analysis to establishments that had at least one male and one female worker, allowing me to compare genders within establishments. The role of exits and entries is critical in understanding the dynamics of the labor market. By focusing on a balanced panel, we control for potential biases that could arise from establishments entering or exiting the market during the study period. This ensures that the observed separations are due to worker movements rather than establishment turnover.

[10] I use establishment instead of firm for several reasons. First, different establishments within the same firm can abide by different collective agreements in Portugal, which allows me to better identify the workplace that were subject to changes in hours. Second, worker separation from the current workplace is better captured at the establishment level than at the firm level due to within-firm, cross-establishment worker flows. Finally, I can compare the wage gap between men and women who work in exactly the same workplace.

The unit of analysis is the worker, and the sample includes workers who are aged between 20 and 60 and work in the selected establishments. I exclude those aged 60 or older because their job separation might be confounded with retirement.

3.5.2 Treatment Definition

The treatment is defined at the establishment level where the worker is employed, although the analysis focuses on individual workers. This approach enables tracking the separation rate of workers and the gender wage gap within a given establishment over time. Defining treatment at the individual level would precisely identify who received the treatment, but has disadvantages, such as the inability to track workers who leave the sample and difficulties in studying the gender wage gap. Instead, I compare establishments that improved working conditions (e.g., reduced hours) to those that did not experience any changes.

I classify an establishment as treated if it has at least one worker with standard hours exceeding 40 between 1994 and 1996.[11] Given that standard hours tend to be similar within establishments, there is a high correlation between a worker exceeding 40 hours and her colleagues in the same establishment also exceeding 40 hours. This is because standard hours are usually determined according to each worker's collective agreement, which tends to be similar within the firm or establishment due to strong industry and regional dimensions.

Table 3.4 provides the descriptive statistics of wokers and establishments.

3.5.3 Empirical Strategy

The empirical strategy employed here is similar to the one used in the previous section. I use a difference-in-differences approach, comparing the outcomes of workers in the treated and control groups. Fully exploiting the panel nature of the data, which covers both workers and establishments, and leveraging the large sample size, I adopt a specification with a large set of fixed effects:

$$\begin{aligned} Y_{ijt} = {} & \delta_{st} + \phi_{gt} + \xi_{gj} + \boldsymbol{\beta} \boldsymbol{X}_{it} \\ & + Treat_j \cdot \sum_{t=1991}^{2000} \beta_t \mathbb{1}\{year = t\} \\ & + Treat_j \cdot \sum_{t=1991}^{2000} \beta_t^F \mathbb{1}\{year = t\} \cdot Female_i + \varepsilon_{ijt} \end{aligned} \tag{3.4}$$

[11] I define this over the 1994-1996 period instead of only 1996 to avoid mean reversion affecting the estimates. However, the results remain largely unchanged if I define the treatment status based solely on standard hours of workers in 1996.

Table 3.4: Summary Statistics

	All	Treated	Control
Worker-level information			
Age	35,77 (11,55)	35,08 (11,57)	38,44 (11,04)
Female	0,47	0,48	0,43
High school degree	0,11	0,08	0,24
College degree	0,04	0,02	0,1
Standard Hours	41,13 (2,9)	41,99 (2,26)	37,69 (2,6)
Actual Hours	40,31 (5,33)	41,05 (5,22)	37,42 (4,72)
Positive Overtime	0,06	0,05	0,1
Overtime hours	1,24 (6,82)	1,06 (6,36)	1,95 (8,34)
Hourly wage	5,19 (5,69)	4,31 (4,67)	8,55 (7,67)
Tenure	9,44 (9,11)	8,97 (8,9)	11,25 (9,64)
Observations	2,285,798	1,818,229	467,569
Establishment-level information			
Lisbon metropolitan area	0,36	0,26	0,66
Manufacturing	0,37	0,45	0,11
Wholesale, retail and hotel and restaurants	0,4	0,43	0,3
Banking, finance and real estate	0,11	0,02	0,4
Community, social and personal services	0,13	0,1	0,19
Median establishment size	11	10	11
Mean establishment size	28,49 (75,86)	29,73 (75,29)	24,59 (77,51)
Median sales	923,286.9	817,135.3	1,733,842
Observations	83,445	63,209	20,236

Note: The table provides summary statistics of the final sample of workers and establishments in the QP. The values represent means (standard deviations) unless otherwise stated, for the period between 1991 and 1996. The control group consists of establishments with no workers whose standard hours exceed 40 hours during the 1994-1996 period.

where, Y_{ijt} is the outcome for individual i in establishment j at time t; δ_{st} is the 1-digit-sector-year fixed effects; ϕ_{gt} is the gender-year fixed effects; ξ_{gj} is the gender-establishment fixed effects; $\boldsymbol{\beta X}_{it}$ are time-varying controls (age and tenure, and the interactions terms with female dummies); $Treat_j$ is the treatment indicator. β_t provides the effects of the reform (for men), while β_t^F identifies the gender differences in these effects. These coefficients are normalized to 0 in the year just before the reform.[12] The treatment effects are identified by comparing the evolution of outcomes between treated and control establishments, separately for men and women. The systematic differences between the two genders within an establishment are controlled for by gender-establishment fixed effects. In addition, fixed effects for the sector year ensure that comparisons between establishments are made within the same sector, accounting for any business cycles or sector-specific shocks that might affect the results.

Regarding the outcomes studied, I first analyze standard hours (starting from 1994), actual

[12]It is normalized to 0 in 1996 for hours and wage outcomes, and is normalized in 1995 for worker mobility outcomes (which measure the change from t to $t+1$). Additionally, the data points stop in 1999 for worker mobility outcomes because there is no worker information for 2001, making it impossible to know mobility from 2000 to 2001. Finally, due to the change in the reference month from March to October in 1994, there is a systemic jump in the separation rate in 1993 compared to other years (12 months vs. 19 months).

hours worked, and overtime hours. I also examine the effects on log wages to understand the evolution of the gender wage gap within the same establishment. Finally, I study worker mobility from t to $t+1$, focusing on the separation of a worker from their current employer (identified as the worker not working for the establishment at $t+1$). Furthermore, I break down this separation into two components: workers changing establishments and workers leaving the data.[13]

3.5.4 Results

Figure 3.7 shows the dynamic effects on standard hours.[14] The panels on the left display the treatment effects for men, captured by β_t in Equation 3.4. The right panels show the differences between the two genders in terms of the treatment effect (i.e., β_t^F in Equation 3.4). Panel (A) shows that male workers in the treated firms decreased their standard hours by about 1.6 hours after the full implementation of the reform in 1998.[15] There seems to be no significant difference between genders in the size of the reduction in hours, as shown in the right panel. Panel (B) confirms similar effects on actual hours worked per week, although the magnitude is slightly smaller, around -1.5 hours, and slightly bounces back after 1999.[16] Table 3.5 shows that the decrease was -1.67 hours for standard hours and -1.23 hours for actual hours. It also shows that overtime hours increased by 0.5 hours.[17]

Panel (C) provides the results on the log of hourly wages. The hourly wage rate has increased because of the law-induced rigidity of monthly income. The wage rate for men increased by about 5% following the reform, as shown in the left panel. The wage increase for women is very similar, although the negative coefficients on the right panel in 1999 and 2000 suggest that, over time, the wage growth of women might have been slightly smaller. However, the decrease in wage growth of women is in also observed in the pre-reform periods and it is difficult to distinguish the negative coefficients from the pre-existing trends.

Job separation. The analysis from the LFS suggests a potential decrease in job separation among treated workers, possibly more pronounced for women. Panel (D) illustrates the effect on separations. Since the reform's implementation, the separation rate has decreased for men, up to 4 percent in the final year of the sample. This aligns with the interpretation

[13] A worker leaving the data could mean various things: exiting the labor force, becoming unemployed, becoming self-employed, or working in a firm not covered by the QP (e.g., public sector jobs).

[14] See Appendix Table C.4 for the corresponding regression tables.

[15] A small decreasing trend indicates that some workers reduced hours due to the collective agreement.

[16] Part of the explanation for the bounce back is the measurement of actual hours worked in the QP. Since the QP only provides hours worked for the reference month, actual hours worked are influenced by the number of workdays in the reference month, which varies across years due to the presence of national holidays and weekends. Specifically, the number of workdays decreased from 1996 to 1997, and further to 1998, then increased from 1999. The fluctuations in working hours for the control group were slightly larger than those for the treatment group, resulting in a small bounce back from 1999. This does not happen for standard hours because they are not influenced by the number of working days per month.

[17] However, the parallel trends assumption does not hold for this outcome, as shown in Appendix Figure C.1.

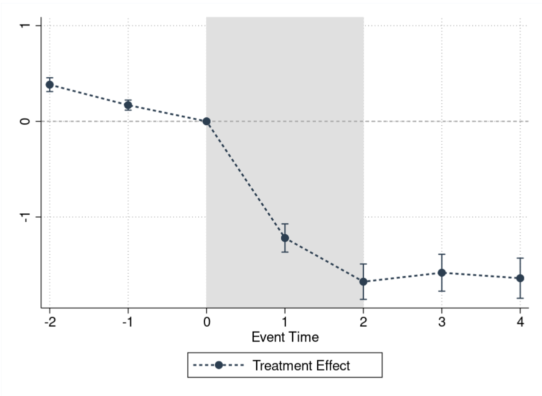
that workers were able to reduce their hours without a loss in monthly earnings, thereby increasing satisfaction with working conditions. The left panel indicates that there is no significant gender gap in the treatment effect on separation rates. If anything, the effect on separation could have been *less* for women compared to men toward the end of the sample, as shown in the right panel.[18]

Panel (E) and (F) reveal that the decrease in separation primarily stems from workers leaving the sample, rather than a reduction in changing firms. This suggests that the decrease in working hours reduced separations among workers who might have otherwise left the sample altogether, but did not significantly affect the propensity to change employers.

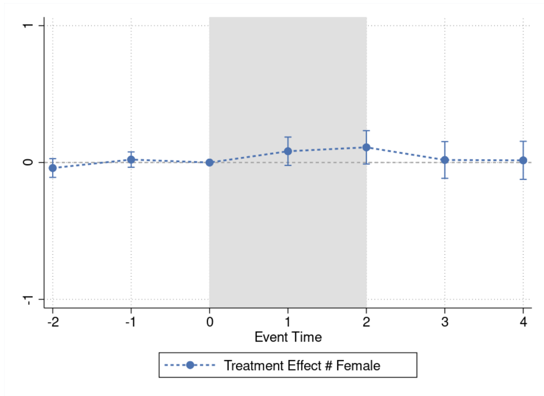
[18]Note that the discontinuity between the time periods -2 and -1 (i.e., 1993-1994 and 1994-1995) is due to the change in the reference month from March to October starting from 1994, which created a structural break in the separation rates. Aside from this systematic jump, separation rates remained stable in the pre-reform periods.

Figure 3.7: Effects of the 1996 Reform on Hours, Wages, and Hour Preferences

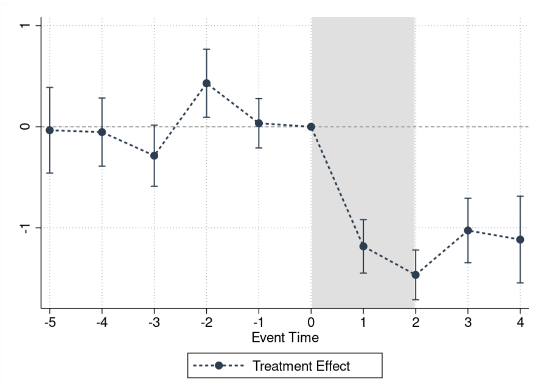
(A) Standard Hours



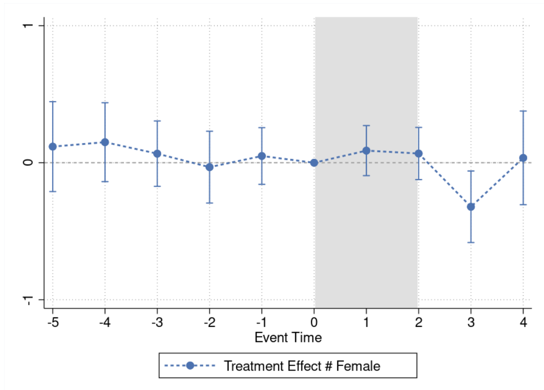
(gender differences)



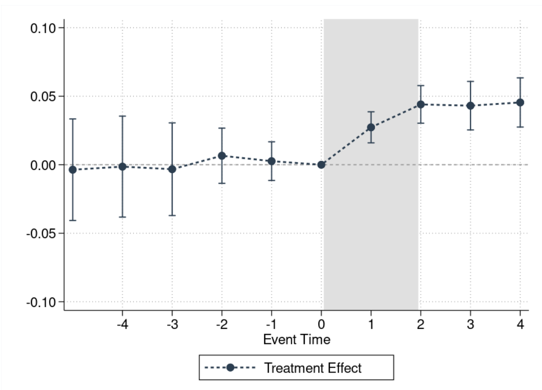
(B) Actual Hours



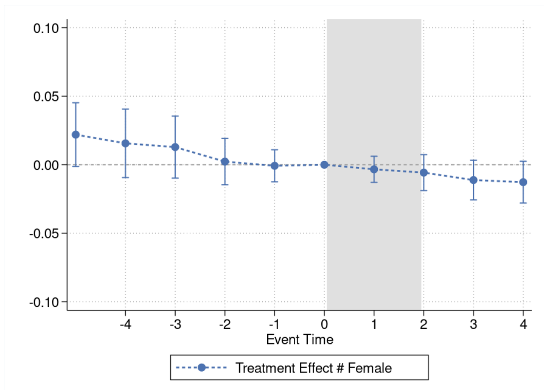
(gender differences)



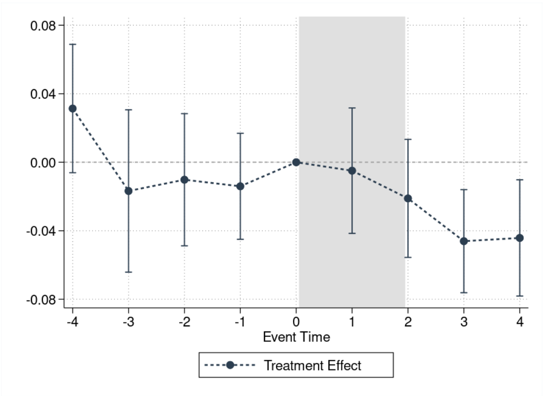
(C) Log wage



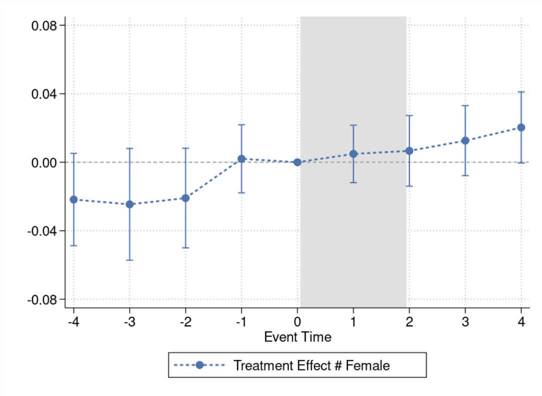
(gender differences)



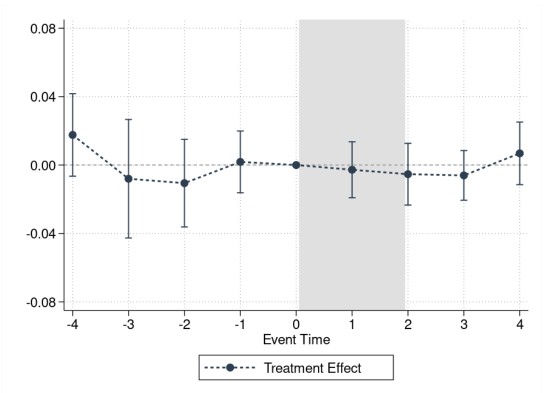
(D) Separation from t to $t+1$



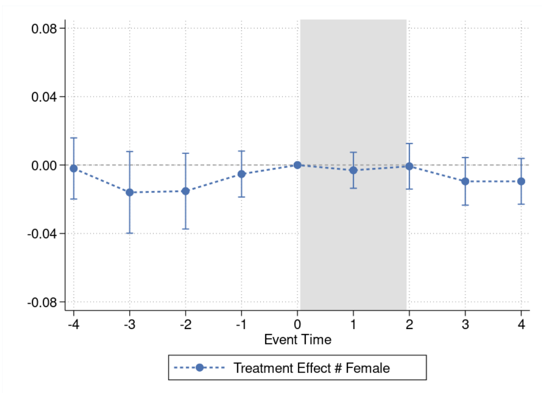
(gender differences)



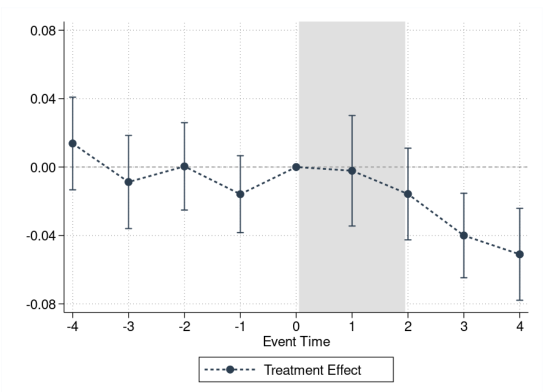
(E) Change establishment from t to $t+1$



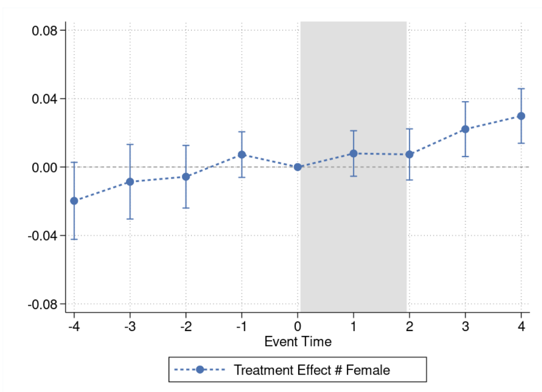
(gender differences)



(F) Leave QP from t to $t+1$



(gender differences)



Note: The figure shows the difference-in-differences effects of the reform on the economic outcomes, comparing each gender in the treated establishments to the corresponding gender in the control establishments (Equation 3.4). Panels on the left show coefficients for men and women separately, while on the right-hand side, it shows the difference between men and women in terms of the effect.

Table 3.5: The Average Effects of the 1996 Reform

(A) Hours and Wages

	Hours and Wages:			
	Standard Hours (1)	Actual Hours (2)	Overtime (3)	Log Wage (4)
Treatment × *Post*	-1.671***	-1.236***	0.502**	0.039***
	(0.089)	(0.092)	(0.233)	(0.009)
Treatment × *Post* × *Female*	0.059	-0.072	-0.133	-0.014**
	(0.055)	(0.081)	(0.173)	(0.006)
R-squared	0.87	0.42	0.54	0.88
Observations	2,456,074	3,494,825	3,494,825	3,494,825

(B) Worker Mobility

	Worker Mobility from t to $t+1$		
	Separation (1)	Change establishment (2)	Leave sample (3)
Treatment × *Post*	-0.015	-0.001	-0.014*
	(0.010)	(0.005)	(0.008)
Treatment × *Post* × *Female*	0.002	-0.001	0.003
	(0.008)	(0.004)	(0.007)
R-squared	0.32	0.37	0.30
Observations	3,118,968	3,118,968	3,118,968

Note: The tables summarize the average treatment effects (ATT) of the reform on hours, wages, and mobility. *Treatment* is a dummy variable for the treated group; *Post* is a dummy for post-treatment periods (1997 and after for panel A, and 1996 and after for panel B); *Female* is a dummy variable that takes the value 1 for females and 0 for males. Standard hours are available only from 1994. Separation is defined as taking the value 1 if a worker is separated from the establishment from time t to $t+1$, and 0 otherwise. Separations are disaggregated into a worker leaving the data or a worker moving to another establishment. Standard errors are in parentheses; * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

3.5.5 Discussion

The results obtained from the linked employer-employee data do not necessarily align with the workers' satisfaction regarding the reduction in hours. If female workers gained more from the reduction in hours, we would expect a stronger reduction in job separations for women than for men. I discuss below potential explanations that reconcile these findings.

Labor demand – One possibility is that the result of the labor demand. Asai et al. (2024) find that the Portuguese 1996 reform has decreased labor demand for treated firms due to the sharp surge in labor costs. It is possible that this has affected men and women differently, leading to less decrease in separation rate for women. Although firing is strictly regulated in Portugal, it is possible that employers may have reduced the employment of women more than men, for example, by not extending the fixed-term contracts that female workers are more likely to have than men. Moreover, it is increasingly documented that women are in a disadvantageous position in obtaining firm-specific wage premiums (Card et al., 2016) – given the increase in labor costs, employers might have decreased (or increased less) the wages of women compared to men (for which there is fuzzy evidence from Figure 3.7). This is also consistent with the fact that women's increased satisfaction in terms of working hours gives bargaining power to employers because women would be willing to accept slower wage growth to keep their jobs (similarly, employers may have to pay more to men who want to work longer hours). This hypothesis is backed by Bloemen (2008), who show that the reservation wage rates would be lower around the desired working hours, analyzed in the job search model combined with the survey data on desired hours.

Magnitude of the effect and statistical power – in the LFS, although there were significant gender gaps in the preferences for working hours, the effects can be considered small. Only a few percentages of the workers stated they wanted to reduce working hours just before the reform[19], and the treatment on the probability to prefer fewer hours for women was -0.008, a decrease of 0.8 percentage points in the probability to want to work fewer hours. While this is large relative to the mean of the pre-reform period, the overall effects are still limited. It might be difficult to detect meaning economic effects on separations even with the rich employer-employee data. This is indicative of the fact that standard hours alone do not necessarily generate large effects on workers' choice of employer and job mobilities, given the decision would be made in conjunction with other factors, including wages and other non-wage characteristics of jobs.

Sample selection bias – since the study on LFS is based on those who continued to work after the reform, there might be a selection bias: some workers were unhappy with the shorter workweek and therefore left the sample, leading to a lower probability of finding workers who would like to work less after the reform. However, this is unlikely for two reasons. Theoretically, the utility of workers should increase when hours decreased due to longer leisure and with no income loss. There were no negative income effects. Moreover, working shorter hours with higher wage was not possible (because control firms with short hours were unaffected so no wage increases) and increasing working hours was difficult due to the lack of overtime hours in Portugal. Given this situation, it is unlikely that workers sought another opportunity that would be more welfare-improving than accepting to work at the current firm with the same salary and reduced hours.

[19]However, note that the hour preferences in the LFS asked about working hour preferences in the face of the leisure-income trade-off, and therefore not necessarily the preference parameter of hours in the utility function for each gender, for which the gap might be even larger than shown in the LFS question.

3.6 Conclusion

The paper examines the effect of the Portuguese working hour reform on the hour preferences and economic outcomes of women and men. I find that women were less likely to want to work fewer hours following the reform, but not men. At the same time, there was an increase in the share of those wanting to work more equally for both genders. This is consistent with the substitution effect induced by the increase in hourly wages, the result of the legal imposition that prevented monthly salaries to go down in response to the standard hour reductions. This effect for women is driven by part-time (≤35h) workers willing to work more than 35 hours, indicating that the reduction of usual hours for full-time workers incentivized women in part-time work arrangements to move to more full-time work. Overall, the reduction in usual hours seemed to have benefited some female workers who initially desired fewer hours, consistent with the increased subjective well-being studied by Lepinteur (2019).

However, based on the linked employer-employee dataset, I did not observe a greater decrease in women's job separation rates compared to men's. If anything, there is an indication that the decline in job separation rates might have been less pronounced for women over time. There is also no discernible narrowing of the wage gap between men and women, and the wage increases for women have been smaller over time. These results suggest that while the 1996 reform in Portugal improved well-being for workers, it did not necessarily lead to gender equality in terms of job stability and wage differentials. This implies that factors other than usual working hours (such as hours flexibility or long overtime hours) may play a more significant role in shaping gender disparities in employer choices and wages. Alternatively, the reduction in hours while maintaining monthly income could also largely benefit men, which could explain the lack of significant gender differences in the observed effects. In either scenario, reducing standard working hours, while enhancing welfare, may not contribute to economic parity between men and women.

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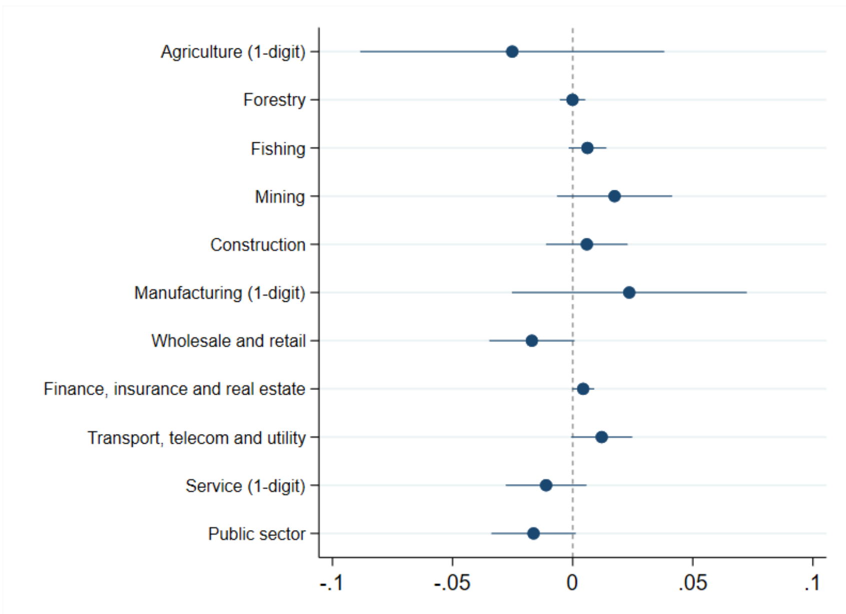
Appendix A

Appendix to Chapter 1 : The Consequences of Hometown Regiment

A.1 Data

Our main data source is census published tables. Prefecture-age-gender data for industry share in employment is only available since 1940. Moreover, a direct comparison of employment is difficult for census waves before and after the war because the pre-war census used different definitions of employment (in other words, *Hongyo* in 1920 and *Yugyo* in 1930 and 1940), and coverage also was not the same (for example, not covering self-employees). The definition of employment in 1930 and 1940 is close to labor force participation in the post-war census. For this reason, we use the employment rate in the pre-war periods as a proxy for labor force participation rate. Note that industry classifications are slightly different in the pre-war and post-war census. When we use prefecture-level outcomes, it is of those aged 20-64 when cohort-level information is available (post-1940); otherwise, the values refer to those aged 14 or older, directly taken from the census. Regarding the sector employment share at prefecture level in 1930, we have digitized only for 1-digit manufacturing and agriculture for the moment, and service sector share is extrapolated by one minus the employment shares of these two sectors combined. It will be updated after digitizing all other 1-digit sectors to make the sector grouping comparable to the one used in the main regression.

Figure A.1: Effect on industry share - 1-digit industry



Note: The figure shows the impact of the change in the gender ratio estimated from the specification of Equation 1.2, where the outcomes are industrial employment shares based on 1-digit sector dis-aggregation.

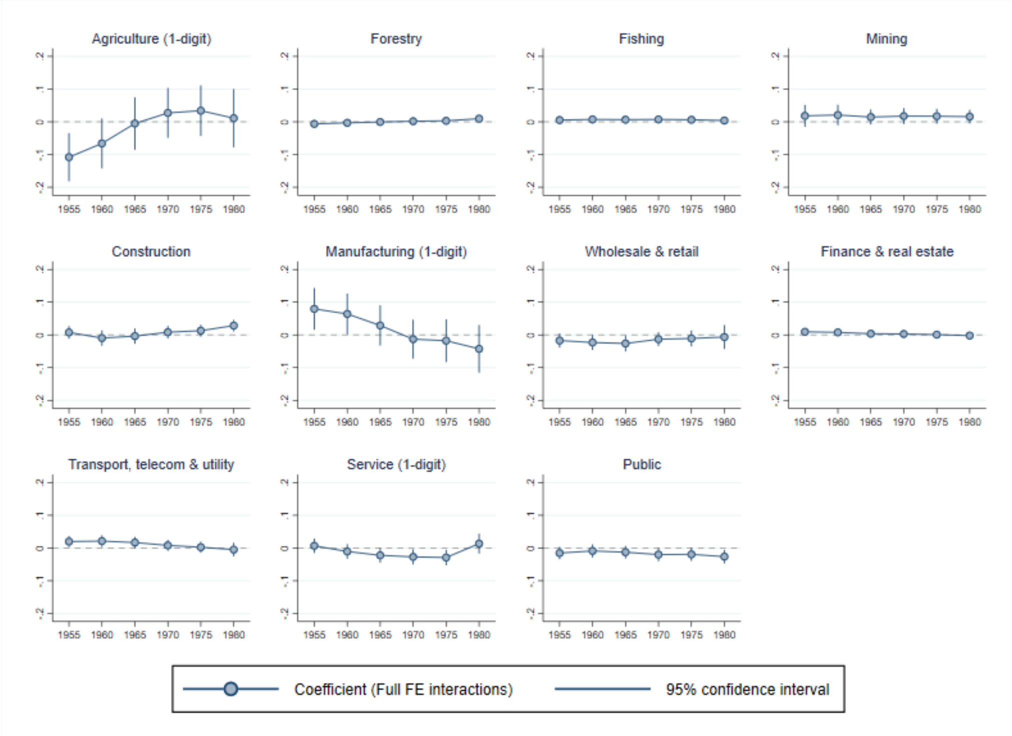
A.2 Supplementary Regression Results

A.2.1 Results based on 1-digit industry classifications

The Figures A.1-A.4 provide the main results on the industry employment share and within-sector female share at 1-digit level (without aggregating them into three large sector groups as in the main text). Figure A.1 shows that male loss has led to the higher employment share of Agriculture and certain service sectors, namely wholesale and retail, service, and Public Sector. It decreased not only the employment share of all manufacture sector but also some relatively high-paying service industries such as finance, insurance, and real estate, and transport, telecommunication, and utility. The effects are in general short-lived, as confirmed in Figure A.2.

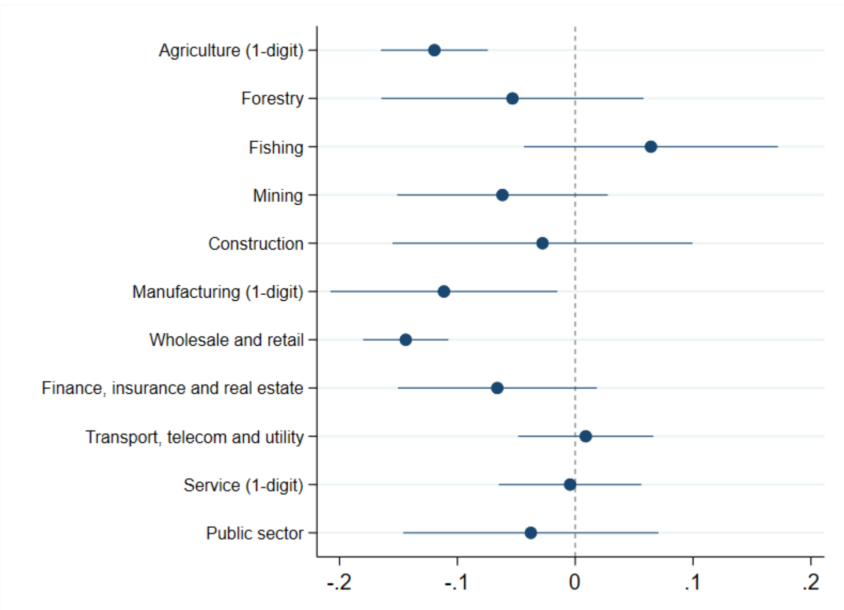
Regarding within-sector female share, Figure A.3 shows that female representation has increased in many 1-digit industries in response to the loss of men, although there are some variations. Figure A.4 shows the time-varying effects, in general confirming a persistent change in gender composition of workers in sectors where we find an effect.

Figure A.2: Effect on industry share (long run) - 1-digit industry



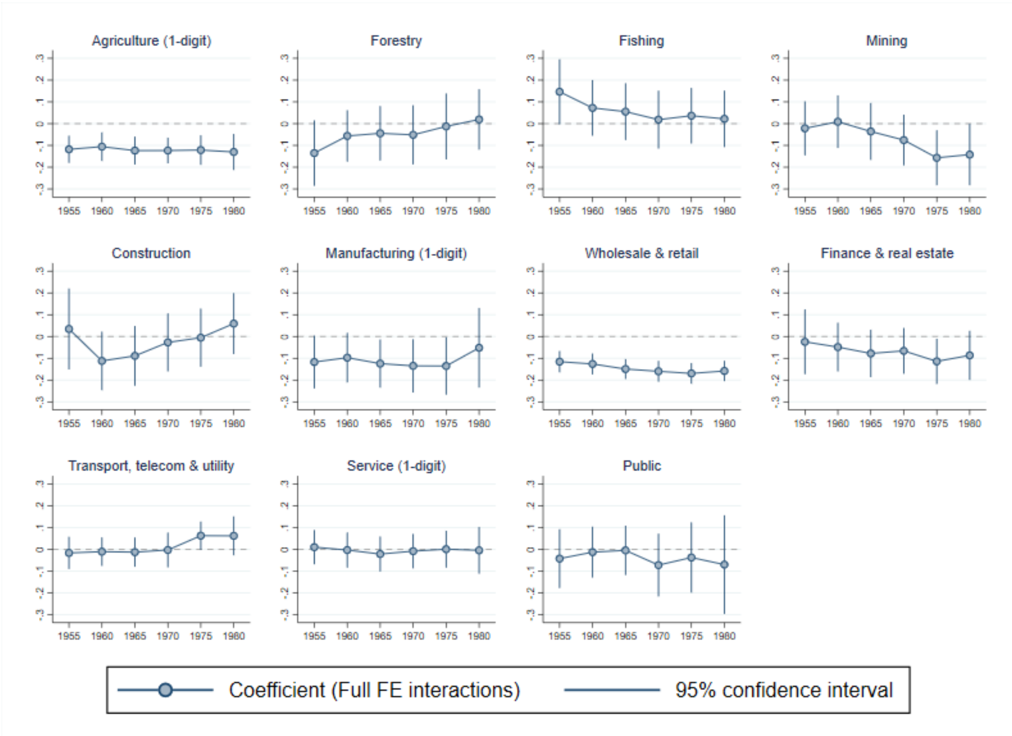
Note: The figure repeats Figure 1.5 for the industrial employment shares based on 1-digit classification.

Figure A.3: Effect on within sector female share - 1-digit industry



Note: The figure shows the impact of the change in the gender ratio estimated from the specification of Equation 1.2 where the outcomes are female share wihtin each 1-digit sector.

Figure A.4: Effect on within sector female share (long run) - 1-digit industry

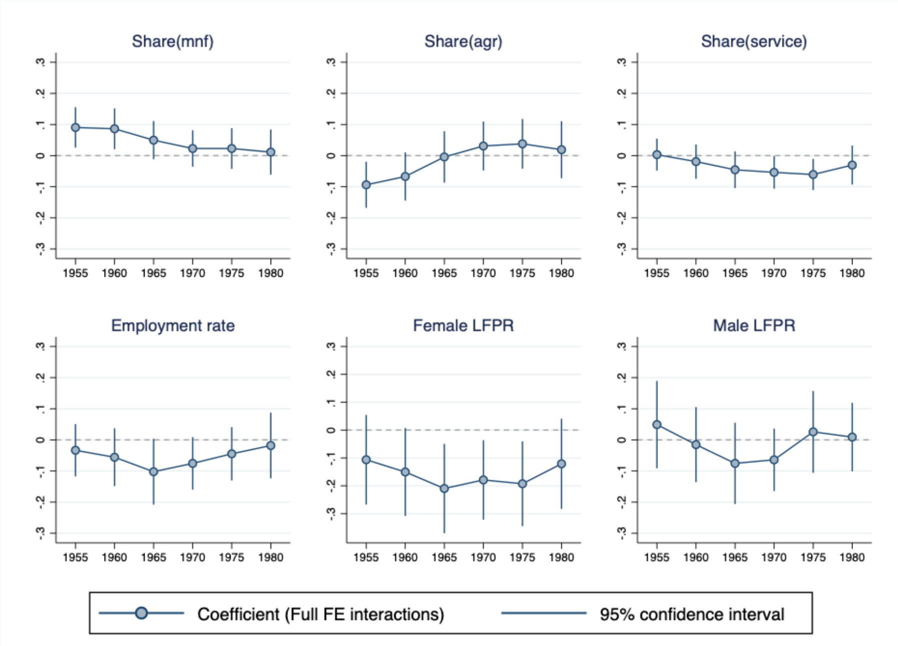


Note: The figure repeats Figure 1.6 for the industrial employment shares based on 1-digit classification.

A.2.2 Removing Hiroshima and Nagasaki

The war impacted the Japanese economy through factors other than the imbalanced gender ratio. Most notably, the atomic bombs that completely destroyed physical capital and caused a large number of casualties. In the main specification, such effects are accounted for by the prefecture fixed effects and the inclusion of the change in the log of population. However, we estimate the effects of the imbalanced gender ratio on the industrial structure (and employment measures) by excluding Hiroshima and Nagasaki, as shown in Figure A.5. The exclusion of these two prefectures hardly affects our estimates, indicating that our main specification had already sufficiently controlled for the prefecture-level war damages.

Figure A.5: The effect on industrial composition, exlucing Hiroshima and Nagasaki



Note:The figure replicates 1.5 by excluding Hiroshima and Nagasaki prefectures. It provides the long-term effects of the gender ratio changes on industry share of employment (top three panels) and employment and labor force participation (bottom three panels).

Table A.1: Comparison of coefficients across models

	Industry Employment Share			Employment and Participation		
	Manufacturing (1)	Agriculture (2)	Service (3)	Employment rate (4)	Female LFPR (5)	Male LFPR (6)
Coefficient from the model with:						
No FE interactions (Baseline)	0.171***	-0.249***	0.078***	0.135*	0.060	0.147**
	(0.032)	(0.044)	(0.026)	(0.075)	(0.103)	(0.067)
Prefecture-year FE interaction	0.177***	-0.267***	0.091***	0.236***	0.179*	0.186***
	(0.031)	(0.043)	(0.026)	(0.077)	(0.105)	(0.071)
Cohort-year FE interaction	0.075**	-0.051	-0.024	-0.056	-0.161***	0.019
	(0.030)	(0.035)	(0.022)	(0.037)	(0.061)	(0.052)
Both FE interactions	0.052*	-0.019	-0.032	-0.057	-0.159***	-0.013
	(0.027)	(0.033)	(0.021)	(0.037)	(0.061)	(0.053)

Note: The table summarizes the estimated coefficient of the change in the gender ratio with alternative specifications. All regressions are based on Equation 1.2, but the first row removes the interaction fixed effects. The second and the third rows include interaction fixed effects only along prefecture-year and cohort-year, respectively. The last row is our main results from Table 1.3. Standard errors are clustered at prefecture-year level. Standard errors are in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

A.2.3 Role of FE interactions

Our main results are based on the specification (in Equation 1.2) which includes interaction fixed effects. Table A.1 shows the comparison of the estimated coefficients with the alternative specifications. The first row shows that, without adding any interaction fixed effects (in other words, specification with only one-dimensional fixed effects in year, cohort and prefecture), the estimated effects on industrial share are overestimated, as indicated in columns (1)-(3). This remains similar when the prefecture-year FE is added in the second row. The third row shows that the effect attenuates significantly when we add cohort-year fixed effects, indicating the importance of controlling for the overall differences in employment share across cohorts (or ages). The estimated coefficients are much closer to the main specification that fully interacts fixed effects as shown in the last row. Column (5) shows that controlling for cohort-year specific effect is important in identifying the effect on female labor force participation because the female labor supply has a strong life cycle component. Column (5) shows that controlling for cohort-year specific effect is important in identifying the effect on female labor force participation. This is due to the fact that the female labor supply has a strong life cycle component.

Table A.2: Time-varying effects with two FE interactions (3)

	Industry Employment Share			Employment and Participation		
	Manufacturing	Agriculture	Service	Employment rate	Female LFPR	Male LFPR
	(1)	(2)	(3)	(4)	(5)	(6)
Gender ratio change 1935-47	0.091***	-0.094**	0.003	-0.033	-0.106	0.049
	(0.033)	(0.038)	(0.026)	(0.043)	(0.082)	(0.071)
× 1960	-0.004	0.027	-0.022	-0.022	-0.044	-0.064
	(0.028)	(0.030)	(0.027)	(0.047)	(0.088)	(0.061)
× 1965	-0.041	0.090***	-0.049*	-0.069	-0.103	-0.125*
	(0.027)	(0.033)	(0.028)	(0.048)	(0.082)	(0.064)
× 1970	-0.068***	0.125***	-0.057**	-0.042	-0.073	-0.113**
	(0.026)	(0.032)	(0.025)	(0.035)	(0.069)	(0.051)
× 1975	-0.068**	0.132***	-0.064**	-0.011	-0.086	-0.023
	(0.029)	(0.036)	(0.026)	(0.036)	(0.073)	(0.061)
× 1980	-0.079**	0.113**	-0.034	0.015	-0.015	-0.040
	(0.034)	(0.045)	(0.033)	(0.049)	(0.082)	(0.061)
ln(pop) change 1935-47	-0.028*	-0.003	0.031**	0.082***	0.147***	0.071**
	(0.015)	(0.019)	(0.013)	(0.024)	(0.043)	(0.028)
Gender ratio in 1935	0.093***	-0.086**	-0.007	-0.043	-0.144**	-0.044
	(0.030)	(0.034)	(0.023)	(0.040)	(0.065)	(0.052)
ln(pop) in 1935	-0.030**	-0.030	0.060***	0.100***	0.183***	0.076***
	(0.015)	(0.020)	(0.012)	(0.022)	(0.043)	(0.024)
ln(worker)	-0.038***	0.136***	-0.097***			
	(0.011)	(0.015)	(0.008)			
ln(pop)				0.037**	-0.026	0.017
				(0.016)	(0.032)	(0.019)
3-way fixed effect	Yes	Yes	Yes	Yes	Yes	Yes
Prefecture-year FE	Yes	Yes	Yes	Yes	Yes	Yes
Cohort-year FE	Yes	Yes	Yes	Yes	Yes	Yes
R-squared	0.968	0.977	0.965	0.913	0.900	0.634
Observations	1748	1748	1748	1748	1748	1748

Note: The table shows the results of the time-varying effects in the specification that includes both prefecture-year and cohort-year interacted fixed effects. The first row corresponds to the effect of the gender ratio in 1955, and the next five rows are the estimated interaction coefficient for subsequent years (representing the difference from 1955 effect). All standard errors are clustered at prefecture-year level. Standard errors are in parentheses. $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

A.2.4 Time-varying effect regression results

This section presents the complete regression results for time-varying effect. Table A.2 summarizes the results corresponding to the main text that includes a full set of fixed effect interactions; we also supplement Table A.3 and Table A.4 that include interaction fixed effects only along cohort-year and prefecture-year, respectively. We also visually compare the time-varying effects across the regression specifications: Figure A.6 compares the results between specifications without interacted fixed effects and with both fully interacted effects; Figure A.7 compares the result of the specification with full fixed effect interactions to the results that add only one of the interaction fixed effects. The figure confirms the important role of cohort-year fixed effects in controlling the life cycle effects.

Table A.3: *Time-varying* effects with *cohort-year* FE interaction

	Industry Employment Share			Employment and Participation		
	Manufacturing	Agriculture	Service	Employment rate	Female LFPR	Male LFPR
	(1)	(2)	(3)	(4)	(5)	(6)
Gender ratio change 1935-47	0.109**	-0.103**	-0.006	-0.043	-0.150*	0.078
	(0.044)	(0.052)	(0.030)	(0.045)	(0.086)	(0.061)
× 1960	-0.000	0.021	-0.020	-0.056	-0.068	-0.084*
	(0.046)	(0.056)	(0.035)	(0.049)	(0.092)	(0.046)
× 1965	-0.019	0.050	-0.031	-0.081*	-0.113	-0.124**
	(0.038)	(0.048)	(0.032)	(0.043)	(0.076)	(0.048)
× 1970	-0.010	0.063	-0.053*	-0.029	-0.041	-0.095**
	(0.038)	(0.048)	(0.030)	(0.038)	(0.069)	(0.040)
× 1975	-0.045	0.082	-0.036	0.033	0.029	-0.004
	(0.047)	(0.059)	(0.035)	(0.041)	(0.081)	(0.051)
× 1980	-0.161***	0.120	0.041	0.101**	0.199*	-0.023
	(0.059)	(0.074)	(0.043)	(0.051)	(0.101)	(0.046)
ln(pop) change 1935-47	-0.057***	0.025	0.032**	0.070***	0.131***	0.053*
	(0.019)	(0.024)	(0.014)	(0.024)	(0.042)	(0.027)
Gender ratio in 1935	0.133***	-0.135***	0.002	-0.062	-0.185***	-0.009
	(0.034)	(0.036)	(0.025)	(0.040)	(0.064)	(0.051)
ln(pop) in 1935	-0.033*	-0.028	0.060***	0.074***	0.139***	0.058**
	(0.019)	(0.025)	(0.013)	(0.021)	(0.041)	(0.023)
ln(worker)	-0.007	0.087***	-0.080***			
	(0.013)	(0.020)	(0.011)			
ln(pop)				0.037**	-0.017	0.039***
				(0.015)	(0.028)	(0.013)
3-way fixed effect	Yes	Yes	Yes	Yes	Yes	Yes
Prefecture-year FE	No	No	No	No	No	No
Cohort-year FE	Yes	Yes	Yes	Yes	Yes	Yes
R-squared	0.936	0.960	0.940	0.883	0.859	0.608
Observations	1748	1748	1748	1748	1748	1748

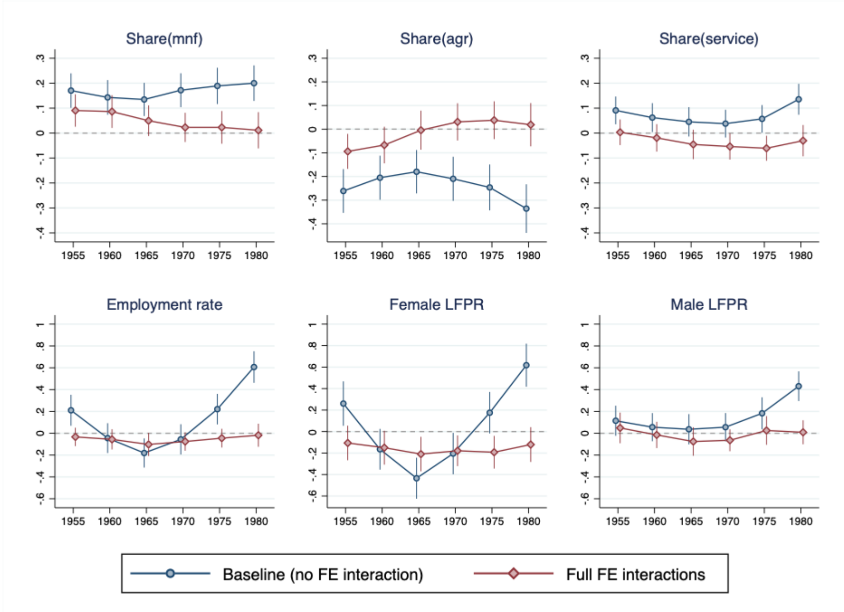
Note: The table shows the results of the time-varying effects in the specification that includes only cohort-year interacted fixed effects. The first row corresponds to the effect of the gender ratio in 1955, and the next five rows are the estimated interaction coefficient for subsequent years (representing the difference from 1955 effect). All standard errors are clustered at prefecture-year level. Standard errors are in parentheses. $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A.4: *Time-varying* effects with *pref-year* FE interaction

	Industry Employment Share			Employment and Participation		
	Manufacturing (1)	Agriculture (2)	Service (3)	Employment rate (4)	Female LFPR (5)	Male LFPR (6)
Gender ratio change 1935-47	0.162***	-0.262***	0.100***	0.278***	0.359***	0.124
	(0.031)	(0.043)	(0.027)	(0.072)	(0.103)	(0.076)
×1960	-0.029	0.052***	-0.023	-0.244***	-0.431***	-0.039
	(0.018)	(0.018)	(0.015)	(0.033)	(0.051)	(0.044)
×1965	-0.036**	0.072***	-0.036**	-0.385***	-0.715***	-0.054
	(0.017)	(0.019)	(0.016)	(0.030)	(0.052)	(0.048)
×1970	-0.004	0.039*	-0.035**	-0.261***	-0.483***	-0.039
	(0.018)	(0.023)	(0.016)	(0.028)	(0.044)	(0.040)
×1975	0.032	-0.003	-0.029*	0.018	-0.106**	0.086*
	(0.020)	(0.028)	(0.016)	(0.037)	(0.052)	(0.049)
×1980	0.080***	-0.113***	0.033	0.426***	0.344***	0.362***
	(0.022)	(0.037)	(0.023)	(0.040)	(0.057)	(0.048)
ln(pop) change 1935-47	-0.086***	0.112***	-0.027	-0.019	0.035	-0.002
	(0.020)	(0.032)	(0.018)	(0.041)	(0.063)	(0.043)
Gender ratio in 1935	0.218***	-0.343***	0.125***	0.210***	0.134	0.145**
	(0.034)	(0.043)	(0.026)	(0.074)	(0.099)	(0.070)
ln(pop) in 1935	-0.073***	0.055*	0.018	0.034	0.108**	0.030
	(0.020)	(0.032)	(0.017)	(0.037)	(0.051)	(0.042)
ln(worker)	0.067***	-0.076***	0.010			
	(0.007)	(0.012)	(0.007)			
ln(pop)				0.288***	0.249***	0.203***
				(0.024)	(0.034)	(0.026)
3-way fixed effect	Yes	Yes	Yes	Yes	Yes	Yes
Prefecture-year FE	Yes	Yes	Yes	Yes	Yes	Yes
Cohort-year FE	No	No	No	No	No	No
R-squared	0.957	0.967	0.950	0.725	0.779	0.391
Observations	1748	1748	1748	1748	1748	1748

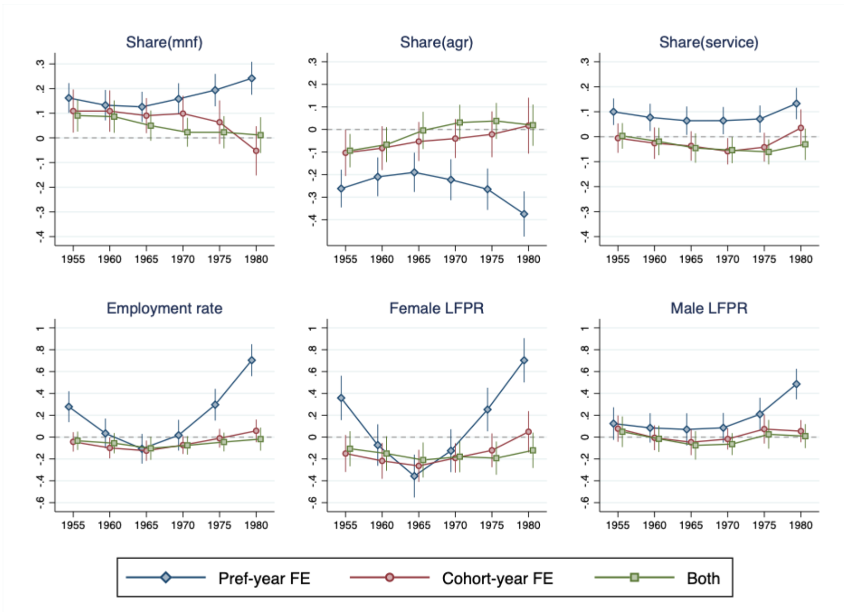
Note: The table shows the results of the time-varying effects in the specification that includes only prefecture-year interacted fixed effects. The first row corresponds to the effect of the gender ratio in 1955, and the next five rows are the estimated interaction coefficient for subsequent years (representing the difference from 1955 effect). All standard errors are clustered at prefecture-year level. Standard errors are in parentheses. $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Figure A.6: *No* interaction vs *full* interactions



Note: The figure repeats Figure 1.5. It adds the results from the specification that includes no interaction fixed effects.

Figure A.7: *No* interaction vs *full* interactions



Note: The figure repeats Figure 1.5, comparing the main results with two less strict models that add only one of the interacted fixed effects.

Table A.5: Controlling for contemporaneous gender ratio

	Industry Employment Share			Employment and Participation		
	Manufacturing	Agriculture	Service	Employment rate	Female LFPR	Male LFPR
	(1)	(2)	(3)	(4)	(5)	(6)
Gender ratio change 1935-47	0.004	0.044	-0.047**	-0.091**	-0.200***	0.050
	(0.030)	(0.037)	(0.022)	(0.040)	(0.072)	(0.073)
ln(pop) change 1935-47	-0.003	-0.035*	0.039***	0.100***	0.166***	0.040
	(0.016)	(0.021)	(0.013)	(0.023)	(0.040)	(0.032)
Contemporaneous gender ratio	0.102***	-0.134***	0.032*	0.066**	0.079	-0.121
	(0.025)	(0.031)	(0.017)	(0.032)	(0.071)	(0.082)
Gender ratio in 1935	0.028	-0.002	-0.026	-0.086*	-0.195**	0.038
	(0.035)	(0.041)	(0.025)	(0.046)	(0.082)	(0.081)
ln(pop) in 1935	-0.003	-0.065***	0.069***	0.119***	0.205***	0.045
	(0.016)	(0.022)	(0.013)	(0.021)	(0.040)	(0.029)
ln(worker)	-0.055***	0.157***	-0.102***			
	(0.011)	(0.015)	(0.008)			
ln(pop)				0.021	-0.044	0.048*
				(0.017)	(0.030)	(0.027)
3-way fixed effect	Yes	Yes	Yes	Yes	Yes	Yes
Prefecture-year FE	Yes	Yes	Yes	Yes	Yes	Yes
Cohort-year FE	Yes	Yes	Yes	Yes	Yes	Yes
R-squared	0.968	0.977	0.964	0.913	0.900	0.634
Observations	1748	1748	1748	1748	1748	1748

Note: The table shows the results of the regression that adds contemporaneous gender ratio in the main specification. All standard errors are clustered at prefecture-year level. Standard errors are in parentheses. $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

A.2.5 Contemporaneous gender ratio

The skewed gender due to the war can be alleviated by the migration in the post-war period. In theory, such effect can be captured by including the contemporaneous gender ratio partially for the sake of comparison with the previous literature The result is presented in Table A.5. Note that the regression suffers a high correlation between the change in the gender ratio during 1935-1947 (our treatment variable) and the contemporaneous gender ratio. This makes the separate identification difficult and partially explains why the estimated effect of the change in the gender ratio is close to zero for manufacturing sector, and the sign of the effect has changed for agriculture, although the result is consistent for the other outcomes.

Table A.6: Regression results using alternative definition of the gender ratio change

	Industry Employment Share			Employment and Participation		
	Manufacturing	Agriculture	Service	Employment rate	Female LFPR	Male LFPR
	(1)	(2)	(3)	(4)	(5)	(6)
Gender ratio change 1935-47 (age-controlled)	0.091***	-0.065***	-0.016***	-0.033	-0.079*	-0.020
	(0.015)	(0.020)	(0.005)	(0.023)	(0.042)	(0.024)
ln(pop) change 1935-47 (age-controlled)	-0.044***	-0.061***	0.031***	0.016	0.007	-0.008
	(0.010)	(0.021)	(0.005)	(0.013)	(0.020)	(0.021)
Gender ratio in 1935	-0.005	-0.040**	0.003	0.019	0.035	-0.024
	(0.014)	(0.016)	(0.004)	(0.021)	(0.037)	(0.022)
ln(pop) in 1935	0.019**	-0.021	0.021***	0.036***	0.072***	0.031**
	(0.009)	(0.016)	(0.006)	(0.013)	(0.023)	(0.016)
ln(worker)	-0.046***	0.076***	-0.060***			
	(0.011)	(0.018)	(0.004)			
ln(pop)				0.052***	0.010	0.025
				(0.015)	(0.030)	(0.018)
3-way fixed effect	Yes	Yes	Yes	Yes	Yes	Yes
Age-year FE	Yes	Yes	Yes	Yes	Yes	Yes
Prefecture-year FE	Yes	Yes	Yes	Yes	Yes	Yes
R-squared	0.970	0.978	0.964	0.912	0.899	0.631
Observations	1748	1748	1748	1748	1748	1748

Note: The table shows the full results of the specification where we use the age-fixed version of the gender ratio change. All standard errors are clustered at prefecture-year level. Standard errors are in parentheses. $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

A.2.6 Alternative definition of gender ratio

In Section 1.7.1.2, we implemented the robustness check using an alternative definition of the gender ratio change that fixed age rather than cohort. Table A.6 shows the full results for this age-fixed alternative gender ratio change under the main specification of Equation 1.2. Figure A.8 shows the time-varying when the alternative measure of the gender ratio is used, compared with the main result using the cohort-fixed measure.

A.2.7 Correlation across cohorts

Figure A.9 provides the correlation matrix across cohorts in terms of the change the gender ratio.

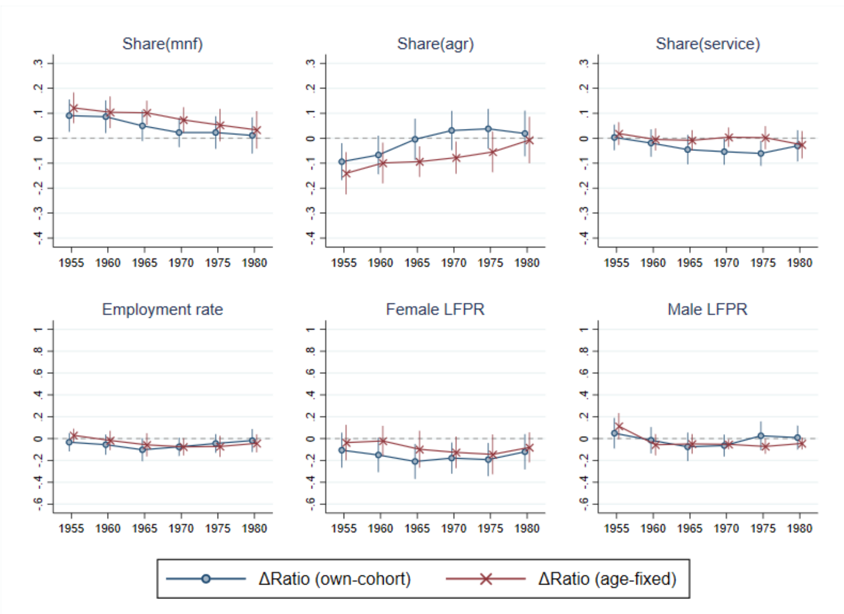
A.2.8 Regressions related to the artificial gender ratio and death rate

The artificial gender ratio change defined in Discussion 1.7.4 is subject to the endogenous change in female social movement across prefectures. We can also create another version of the artificial gender ratio change that fixes female population as 1935. By doing so, this gender ratio change in this measure reflects solely the change in male population by death.

$$Ratio_j^{Art2} = \frac{M_{j,1935} - Death_j}{F_{j,1935}} - \frac{M_{j,1935}}{F_{j,1935}}$$

Table A.7 shows the results and find relatively similar results. The coefficients for sector employment shares are larger in magnitude compared to Table 1.9.

Figure A.8: Time varying effect: own-cohort vs age-fixed $\Delta Ratio$



Note: The figure compares the estimated time-varying effects of the gender ratio change when the gender ratio change calculated is cohort-fixed or age-fixed.

Figure A.9: Correlation of $\Delta Ratio$ across cohorts

Matrix of correlations

Variables	189600	190105	190610	191115	191620	192125	192630	193135
189600	1.000							
190105	0.876	1.000						
190610	0.734	0.796	1.000					
191115	0.414	0.385	0.627	1.000				
191620	-0.018	-0.087	0.211	0.521	1.000			
192125	-0.415	-0.279	-0.130	-0.131	0.341	1.000		
192630	-0.176	0.040	0.068	-0.228	-0.254	0.646	1.000	
193135	-0.065	0.093	-0.071	-0.223	-0.556	-0.039	0.425	1.000

Note: Correlation of change in gender ratio 1935-47 across cohorts

Since the artificial gender ratio ignores the (cross-prefecture) mobility, Table A.8 controls for the percentage change in male and female population from 1935 to 1947 in the treated cohorts that are due to migration. For men, the change in population due to migration is imputed from the change in population less death count of soldiers. For women, it is simply the population change, since there are no military death for female.

Lastly, Table A.9 supplements Table 1.10 by using the death rate calculated using the treated cohorts' male population size in 1935, instead of fixing ages.

Table A.7: Regression with alternative artificial gender ratio change

	Industry Employment Share			Employment and Participation		
	Manufacturing	Agriculture	Service	Employment rate	Female LFPR	Male LFPR
	(1)	(2)	(3)	(4)	(5)	(6)
Artificial gender ratio (female pop fixed)	0.217**	-0.090	-0.107*	0.366***	0.507***	0.093***
	(0.086)	(0.094)	(0.060)	(0.061)	(0.103)	(0.028)
Bomb casualty per capita	0.051	-0.038	-0.127	0.833***	1.407***	0.129
	(0.339)	(0.322)	(0.201)	(0.211)	(0.318)	(0.126)
Destroyed building per capita	0.333***	-0.301**	-0.116	0.019	-0.025	0.033
	(0.112)	(0.152)	(0.101)	(0.082)	(0.137)	(0.041)
Height weight ratio	-4.487***	4.324***	0.137	-0.029	0.456	-0.012
	(0.743)	(0.711)	(0.405)	(0.410)	(0.674)	(0.216)
ln(pop) change 1935-47	-0.062	-0.137**	0.160***	-0.172***	-0.307***	-0.028
	(0.045)	(0.057)	(0.051)	(0.053)	(0.087)	(0.021)
Gender ratio in 1935	-0.010	-0.185***	0.108**	-0.237***	-0.320***	-0.028*
	(0.056)	(0.065)	(0.045)	(0.042)	(0.071)	(0.017)
ln(pop) in 1935	0.034	-0.168***	0.092***	-0.039	-0.104*	0.008
	(0.030)	(0.045)	(0.029)	(0.037)	(0.061)	(0.013)
ln(worker)	-0.045	0.166***	-0.077**			
	(0.030)	(0.045)	(0.030)			
ln(pop)				0.015	0.062	-0.012
				(0.036)	(0.061)	(0.012)
Cohort FE and time FE	Yes	Yes	Yes	Yes	Yes	Yes
Prefecture FE	No	No	No	No	No	No
Cohorot-time FE interaction	Yes	Yes	Yes	Yes	Yes	Yes
Prefecture-level controls	Yes	Yes	Yes	Yes	Yes	Yes
Pre-war prefecture-level controls	Yes	Yes	Yes	Yes	Yes	Yes
Pre-war outcome (1930 value)	Yes	Yes	Yes	Yes	Yes	Yes
R-squared	0.854	0.940	0.919	0.794	0.776	0.625
Observations	2484	2484	2484	2484	2484	2484

Note: The table provides the regression results for alternative artificial gender ratio change that fixes female population. The regression controls for 1930 outcomes. Standard errors are clustered at prefecture-year level. Standard errors are in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A.8: Regression with alternative artificial gender ratio change, controlling for movement for men and women

	Industry Employment Share			Employment and Participation		
	Manufacturing (1)	Agriculture (2)	Service (3)	Employment rate (4)	Female LFPR (5)	Male LFPR (6)
Artificial gender ratio (female pop fixed)	0.340***	-0.236***	-0.094*	0.332***	0.441***	0.080**
	(0.080)	(0.090)	(0.055)	(0.062)	(0.105)	(0.032)
Percentage change by migration (male)	0.274***	-0.239***	-0.064*	-0.007	-0.026	-0.005
	(0.050)	(0.058)	(0.038)	(0.043)	(0.071)	(0.021)
Percentage change by migration (female)	-0.179**	0.006	0.124**	-0.087	-0.155	-0.030
	(0.070)	(0.065)	(0.048)	(0.060)	(0.095)	(0.022)
Bomb casualty per capita	0.218	-0.307	-0.144	0.797***	1.328***	0.114
	(0.307)	(0.305)	(0.190)	(0.210)	(0.325)	(0.130)
Destroyed building per capita	0.212**	-0.234	-0.081	0.008	-0.040	0.030
	(0.106)	(0.148)	(0.103)	(0.082)	(0.138)	(0.041)
Height weight ratio	-4.812***	3.998***	0.176	-0.181	0.142	-0.071
	(0.806)	(0.719)	(0.396)	(0.419)	(0.690)	(0.224)
ln(pop) change 1935-47	-0.176**	0.068	0.107	-0.096	-0.162	0.001
	(0.080)	(0.095)	(0.074)	(0.074)	(0.118)	(0.035)
Gender ratio in 1935	0.102*	-0.247***	0.057	-0.213***	-0.279***	-0.021
	(0.061)	(0.069)	(0.053)	(0.051)	(0.085)	(0.018)
ln(pop) in 1935	0.031	-0.173***	0.105***	-0.050	-0.123*	0.004
	(0.028)	(0.044)	(0.030)	(0.039)	(0.064)	(0.013)
ln(worker)	-0.035	0.167***	-0.092***			
	(0.029)	(0.044)	(0.030)			
ln(pop)				0.026	0.083	-0.008
				(0.038)	(0.063)	(0.013)
Cohort FE and time FE	Yes	Yes	Yes	Yes	Yes	Yes
Prefecture FE	No	No	No	No	No	No
Cohorot-time FE interaction	Yes	Yes	Yes	Yes	Yes	Yes
Prefecture-level controls	Yes	Yes	Yes	Yes	Yes	Yes
Pre-war prefecture-level controls	Yes	Yes	Yes	Yes	Yes	Yes
Pre-war outcome (1930 value)	Yes	Yes	Yes	Yes	Yes	Yes
R-squared	0.854	0.940	0.919	0.794	0.776	0.625
Observations	2484	2484	2484	2484	2484	2484

Note:The table provides the regression results for alternative artificial gender ratio change that fixes female population. The regression controls for 1930 outcomes. Standard errors are clustered at prefecture-year level. Standard errors are in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A.9: Regression using alternative death rate

	Industry Employment Share			Employment and Participation		
	Manufacturing	Agriculture	Service	Employment rate	Female LFPR	Male LFPR
	(1)	(2)	(3)	(4)	(5)	(6)
Death rate	-0.071	0.060	-0.039	-0.258***	-0.316***	-0.065**
	(0.080)	(0.086)	(0.050)	(0.058)	(0.097)	(0.028)
Bomb casualty per capita	-0.105	0.088	0.023	0.470**	0.839***	0.046
	(0.398)	(0.374)	(0.172)	(0.210)	(0.302)	(0.120)
Destroyed building per capita	0.365***	-0.337**	-0.130	0.029	-0.000	0.035
	(0.135)	(0.169)	(0.102)	(0.086)	(0.147)	(0.040)
Height weight ratio	-3.595***	4.129***	-0.393	-0.424	-0.134	-0.098
	(0.666)	(0.670)	(0.366)	(0.419)	(0.664)	(0.235)
ln(pop) change 1935-47	-0.006	-0.177***	0.125**	-0.174***	-0.281***	-0.035
	(0.057)	(0.065)	(0.050)	(0.050)	(0.079)	(0.024)
Gender ratio in 1935	0.026	-0.177**	0.133***	-0.254***	-0.341***	-0.028
	(0.074)	(0.073)	(0.039)	(0.046)	(0.071)	(0.023)
ln(pop) in 1935	-0.062***	-0.017	0.070***	-0.012	0.016	-0.011*
	(0.021)	(0.031)	(0.020)	(0.016)	(0.028)	(0.006)
ln(worker)	0.058***	0.009	-0.061***			
	(0.019)	(0.028)	(0.019)			
ln(pop)				-0.013	-0.059**	0.008
				(0.015)	(0.026)	(0.005)
Cohort FE and time FE	Yes	Yes	Yes	Yes	Yes	Yes
Prefecture FE	No	No	No	No	No	No
Cohorot-time FE interaction	Yes	Yes	Yes	Yes	Yes	Yes
Prefecture-level controls	Yes	Yes	Yes	Yes	Yes	Yes
Pre-war prefecture-level controls	Yes	Yes	Yes	Yes	Yes	Yes
Pre-war outcome (1930 value)	Yes	Yes	Yes	Yes	Yes	Yes
R-squared	0.854	0.940	0.919	0.794	0.776	0.625
Observations	2484	2484	2484	2484	2484	2484

Note:The table provides the regression results for death rate where death rate is calculated based on number of death counts in each prefecture, divided by number of male population in treated cohorts in 1935 in each prefecture. The regression controls for 1930 outcomes. In addition to the variables listed in the table, the prefecture-level controls include share of young population (aged 20-30) in 1935, dummy for large prefecture, dummy for Tokyo and Osaka. Standard errors are clustered at prefecture-year level. Standard errors are in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A.10: With pre-war outcome control and other cohorts' ΔRatio

	Industry Employment Share			Employment and Participation		
	Manufacturing (1)	Agriculture (2)	Service (3)	Employment rate (4)	Female LFPR (5)	Male LFPR (6)
Delta Ratio (age 20-34 in 1945)	0.176***	-0.203***	-0.040	0.109**	0.173**	0.014
	(0.051)	(0.056)	(0.035)	(0.045)	(0.072)	(0.018)
Delta Ratio (age 10-19 in 1945)	-0.278***	-0.071	0.259***	-0.208**	-0.194	-0.149***
	(0.105)	(0.123)	(0.087)	(0.094)	(0.147)	(0.043)
Delta Ratio (age 35-49 in 1945)	-0.208**	0.157	0.113	0.153*	0.218*	0.082**
	(0.087)	(0.119)	(0.078)	(0.089)	(0.128)	(0.040)
Bomb casualty per capita	0.123	-0.590**	0.501***	0.618***	1.114***	0.058
	(0.244)	(0.296)	(0.157)	(0.196)	(0.302)	(0.114)
Destroyed building per capita	0.386***	-0.162	-0.278***	-0.083	-0.186	0.006
	(0.104)	(0.140)	(0.087)	(0.100)	(0.161)	(0.043)
Height weight ratio	-2.958***	4.884***	-1.329***	0.479	1.054*	0.062
	(0.674)	(0.659)	(0.333)	(0.383)	(0.622)	(0.205)
ln(pop) change 1935-47	0.127**	-0.208***	0.109***	-0.132***	-0.276***	-0.014
	(0.051)	(0.058)	(0.029)	(0.043)	(0.072)	(0.016)
Gender ratio in 1935	0.047	-0.096	-0.017	-0.068	-0.083	0.049
	(0.087)	(0.099)	(0.064)	(0.077)	(0.119)	(0.031)
ln(pop) in 1935	0.020	-0.156***	0.092***	-0.013	-0.065	0.013
	(0.026)	(0.041)	(0.029)	(0.041)	(0.066)	(0.014)
ln(worker)	-0.011	0.153***	-0.098***			
	(0.026)	(0.040)	(0.028)			
ln(pop)				0.001	0.038	-0.013
				(0.040)	(0.066)	(0.013)
Cohort FE and time FE	Yes	Yes	Yes	Yes	Yes	Yes
Prefecture FE	No	No	No	No	No	No
Cohorot-time FE interaction	Yes	Yes	Yes	Yes	Yes	Yes
Prefecture-level controls	Yes	Yes	Yes	Yes	Yes	Yes
Pre-war prefecture-level controls	Yes	Yes	Yes	Yes	Yes	Yes
R-squared	0.850	0.933	0.869	0.789	0.772	0.625
Observations	2484	2484	2484	2484	2484	2484

Note: The table extends the results of Table 1.8 by adding the change in the gender ratio of the control cohorts. Standard errors are clustered at prefecture-year level. Standard errors are in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

A.2.9 Prefecture level variation controlling for other cohorts

We provide supplementary results to the regression that exploits prefecure-level variation (Table 1.8). Table A.10 controls for the change in the gender ratio of the control cohorts — added separately for those aged 10-19 and 35-49 in 1945.

A.2.10 Cohort substitutions

In the main text, we provided the results on the cohort-substitution effect in Figure 1.8 which used balance panel of the cohorts until 1960. We provide the results using a balance panel of the cohorts for 1955-1980 (with the attrition of the older cohorts later in the sample) in Figure A.10.

Figure A.10: Coefficient for selected cohorts (unbalanced panel), all years

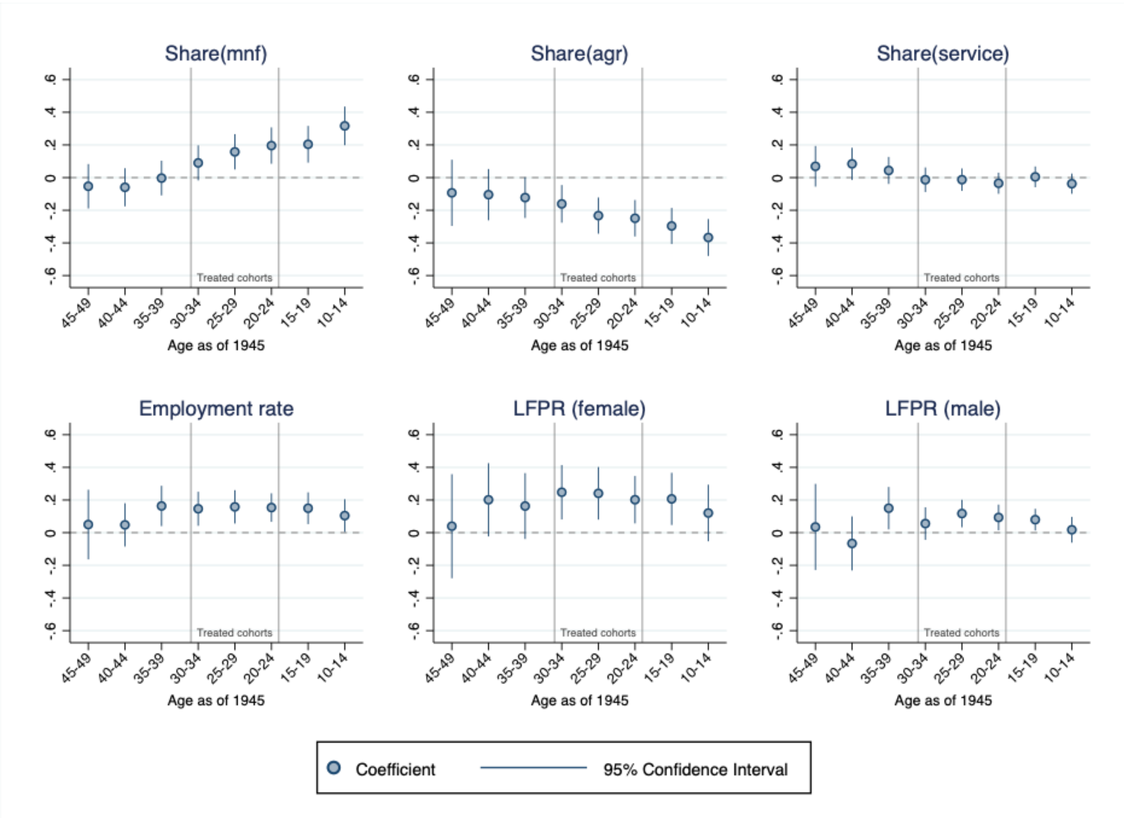


Table A.11: Results excluding outlier prefectures

	Outcome					
	Share(mnf)	Share(agr)	Share(service)	Employment rate	Female LFPR	Male LFPR
	(1)	(2)	(3)	(4)	(5)	(6)
Change in gender ratio 1935-47	0.113***	-0.085**	-0.028	-0.032	-0.111*	0.020
	(0.035)	(0.040)	(0.025)	(0.038)	(0.064)	(0.064)
change_lnpop_1935_47	-0.081***	0.070**	0.011	0.107***	0.190***	0.047
	(0.025)	(0.033)	(0.018)	(0.031)	(0.053)	(0.035)
1935 gender ratio	0.171***	-0.156***	-0.014	-0.020	-0.099	-0.009
	(0.039)	(0.040)	(0.027)	(0.038)	(0.064)	(0.060)
lnpop_1935	-0.062***	0.047	0.015	0.128***	0.247***	0.040
	(0.023)	(0.031)	(0.016)	(0.027)	(0.049)	(0.027)
lnworker	0.019	0.034	-0.054***			
	(0.014)	(0.022)	(0.012)			
lnpop				0.012	-0.068**	0.039**
				(0.019)	(0.034)	(0.015)
3-way fixed effect	Yes	Yes	Yes	Yes	Yes	Yes
Prefecture-year FE	No	No	No	No	No	No
Cohort-year FE	Yes	Yes	Yes	Yes	Yes	Yes
R-squared	0.919	0.969	0.961	0.885	0.857	0.591
Observations	1520	1520	1520	1520	1520	1520

Note: The table provides robustness checks of our main results in Table 1.3 that excludes top three and bottom three prefectures in terms of manufacturing employment share in 1955 to check that our results are not driven by specific prefectures. Excluded prefectures are Osaka, Tokyo, Aichi, Kagoshima, Aomori, and Ibaraki. Standard errors are clustered at prefecture-year level. Standard errors are in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

A.2.11 Robustness to outliers

In order to make sure that our main results on the impact on the industrial share is not driven by some prefecture, we exclude prefectures with top and bottom 3 prefectures in terms of the 1955 manufacturing share. Table A.11 show the results. The estimates coefficients have consistent signs, and the impact on agricultural share becomes actually significant.

Appendix B

Appendix to Chapter 2 : Firm-Level Effects of Reductions in Working Hours

B.1 Literature

Table B.1 summarizes the literature on the employment effects of working hours reductions, sorted by reform and level of analysis (workers, firms, sectors, regions). Overall, while there are well-identified but conflicting estimates of the impact of a reduction in working hours on *incumbent* workers, evidence of the effect on labor demand is scarce. Crépon and Kramarz (2002) find that, in France, a reduction in standard time from 40 to 39 hours in 1982 increased the probability of *incumbent* workers being fired.[1] Using the same approach, Gonzaga et al. (2003) look at the effect of a reduction from 48 to 44 hours in Brazil in 1988, and estimate no effect on job losses, while Raposo and van Ours (2010) find ambiguous effects on separation rates when Portugal reduced standard hours from 44 to 40.[2] Between 2001 and 2005 in Chile, Sánchez (2013) finds no impact on employment transitions for a reform that allowed for a 4-year adjustment period. Estevão and Sá (2008) look at aggregate employment in large versus small firms in France, which, while impacted by the reduction to 35 hours at different times, have no visible difference in their employment dynamics.

Empirical evidence at the firm level, which should allow us to capture the overall effect on labor demand short of general equilibrium effects, is scarce and inconclusive. Kawaguchi et al. (2017) look at the reduction in standard hours in Japan in the 1990s, but find no significant first stage overall (i.e., average hours were not significantly impacted). For a subset of firms with a significant drop in hours, they estimate a negative but insignificant employment effect. Crépon et al. (2004) analyze the employment and productivity effects of the French reduction in standard time to 35 hours. They find that firms affected earlier by the change in hours had a relative increase in employment. However, evaluation of the French reform is made difficult by the simultaneous implementation of important cuts in social security contributions (SSC) meant to ease the transition to the shorter working week. The authors argue that the relative increase in employment can be explained by the lower labor cost. The only paper to look at standard hours reduction in Portugal impacted on firms' employment dynamics was the preliminary work by Varejao (2005), finding zero or negative employment effect depending on the specification.

Lastly, some studies have focused on the sector and/or regional level, in an attempt to capture aggregate equilibrium effects not limited to labor demand (firms) or incumbent workers. Hunt (1999) shows that in Germany, in the late 1980s and early 1990s, sectors that adopted agreements regulating working time experienced a *relative* decrease in employment. Both Skuterud (2007) and Chemin and Wasmer (2009) use regional legislative specificities to capture the effect of a reduction in working hours. Skuterud (2007) shows that, when Quebec (Canada) reduced standard hours from 44 to 40 hours, there has been no positive

[1] This does not imply that the total employment effect at the firm level is negative, as it does not take hirings into account.

[2] Separation rate decreases for workers directly impacted by the reform, but increases for workers indirectly impacted.

effect on employment, despite an adjustment in monthly wages (as opposed to most European reforms). Chemin and Wasmer (2009) show that Alsace-Moselle (France), which for historical reasons experienced a relatively smaller reduction in working hours than the rest of France, had similar employment dynamics to other regions after the reform. The only study indicating a positive correlation between employment and a reduction in standard time is Raposo and Van Ours (2010), who show that local labor markets (region $\times$ sector) that were more impacted by the 1996 reform in Portugal subsequently experienced higher employment growth. Batut et al. (2023) exploits the country$\times$sector exposure to national reforms in Europe over the period 1995 to 2007, finding that national reforms led to decreases in hours and increases in wages, without measuring any negative effect on employment and output at the sector level.

Table B.1 provides the summary of the literature which studies the effect of the working hour reduction.

Table B.1: Standard Time Reduction and Employment: Overview of the Literature

Paper	Country/Year	Reform	Level of Analysis	Sign on Emp.
Crépon and Kramarz (2002)	France - 1982	40h to 39	Worker	Negative*
Gonzaga et al. (2003)	Brazil - 1988	48h to 44h	Worker	Null
Raposo and van Ours (2010)	Portugal - 1996	44h to 40h	Worker	Ambiguous
Sánchez (2013)	Chile - '01-'05	48h to 45h	Worker	Null
Estevão and Sá (2008)	France - 1998	40h to 35h	Worker	Null
Varejao (2005)	Portugal - 1996	44h to 40h	Firm	Null**
Kawaguchi et al. (2017)	Japan - 1997	44h to 40h	Firm	Negative***
Crépon et al. (2004)	France - 1998	39h to 35h	Firm	Ambiguous
Hunt (1999)	Germany - '84–'95	Various	Sector	Negative
Skuterud (2007)	Canada - '97–'00	44h to 40h	Sector/Region	Null
Raposo and van Ours (2010)	Portugal - 1996	44h to 40h	Sector $\times$ Region	Positive
Chemin and Wasmer (2009)	France - 1998	39h to 35	Region	Null
Batut et al. (2023)	EU - 1995-2002	Several	Country$\times$Sector	Null

Note: *Specifically, they find increased firing. This does not, by definition, imply that the total employment effect is negative, as it does not account for potential changes in hiring.** Varejao (2005) finds a null effect on employment when defining treatment and control firm in a binary way for the period '96-'99, he estimates a negative coefficient when including treatment as a continuous variable. *** Kawaguchi et al. (2017) do not find a significant first stage on hours overall: for a subsample of firms with a significant first stage, they find a negative but insignificant effect on new hires. This is an updated version of the table that was published in Batut et al. (2023).

B.2 Theoretical Illustration

We employ a simplified single-input model with labor hours to illustrate the core mechanisms underlying the impact of reduced working hours on employment, output, and productivity.

Setup. Consider a profit maximizing firm that chooses the level of total hour input used in the firm:

$$\max_{H} \quad \pi(H) = p \cdot Y(H) - C(Y)$$
$$= p \cdot F(H) - wH - kN$$

Here, p represents the price of the produced goods (exogenously given), $Y(H)$ denotes the total output produced by the production function $F(H)$, and $C(Y)$ signifies the costs associated with production. The production function $F(H)$ is assumed to be concave, indicating that $F^{'} > 0$ and $F^{''} < 0$.[3] The total labor hours H for the firm can be expressed as $H = N\bar{h}$, where N stands for the number of hired workers, and $\bar{h}$ represents the hours worked per worker. $\bar{h}$ is assumed to be exogenously determined and same for all workers. Thus, $\bar{h}$ can be considered as a value set by national legislation or collective agreements. The model assumes away overtime hours.[4] Because per-worker working hours is pre-determined, firm's decision on how many hours they use in production automatically decides how many workers they hire. The cost structure C is given by $C = wH + kN$, where w denotes the exogenous hourly wage and k denotes the fixed cost associated with each worker, regardless of hours worked by them.

Demand for total hours. The firm determines the optimal level of total hours H^*, and that determines the optimal level of employment N^*. Taking the derivative of the objective function after expressing kN as $k\frac{H}{\bar{h}}$ and setting it equal to zero, the optimality condition becomes:

$$p \cdot F'(H^*) = w + \frac{k}{\bar{h}}$$

This condition essentially states that the marginal revenue from an additional hour input must equal the marginal cost associated it, which is comprised of wages plus the fixed cost per hour. It is crucial to note that in the absence of fixed costs, the optimality condition simplifies to $p \cdot F'(H^*) = w$. By expressing the inverse function of $F^{'}$ as G, we can establish the relationships between total hours and various parameters as follows:

$$H^* = G(\underset{-}{w}, \underset{+}{p}, \underset{+}{\bar{h}}, \underset{-}{k})$$

[3]Note that it is not necessary to assume concavity at all levels of hours. It is possible that marginal productivity increases with hours initially, then decreases after reaching a certain threshold, which determines the equilibrium hours.

[4]These simplifications closely align with the Portuguese context. Standard hours are set by national legislation or industry-level collective agreements. Working hours tend to be similar across workers within the same firm because collective agreement agreement is usually at the industry- or industry-geography level. Furthermore, on average, standard hours and actual hours are similar in our data. Finally, overtime is also rarely used in Portugal.

The demand for total hours decreases as wage w increases and increases with p. Since there's only one unit of input, these relationships involve scale effects. An increase in k reduces the demand for hours.[5] Importantly, the demand for total hours increases with $\bar{h}$. This is because longer hours per worker lead to a smaller share of fixed costs per worker, reducing the marginal cost of hours. This already indicates that a reduction in working hours, $\bar{h}$, through legislation or collective agreements, tends to decrease the firm's demand for hours.

Demand for employment This optimality will also determine the equilibrium level of employment:

$$N^* = \frac{H^*(w, p, \bar{h}, k)}{\bar{h}}$$

Several important observations can be made regarding the demand for workers:

- In the absence of fixed costs, we have $H^* = G(\underset{-}{w}, \underset{+}{p})$, indicating that the total number of demanded hours remains unchanged. In this scenario, shorter working hours per worker, denoted as $\bar{h}$, unambiguously increases employment in firms, which corresponds to the concept of "work sharing".
- However, this prediction becomes ambiguous when fixed costs are introduced, leading to $H^*(w, p, \bar{h}, k)$. On one hand, a reduction in $\bar{h}$ tends to increase employment (as $\bar{h}$ in the denominator decreases). On the other hand, H decreases in the numerator, making the overall prediction regarding N^* ambiguous.
- Importantly, if $\bar{h}$ also causes the hourly wage w to increase, as in Portugal, this tends further to decrease employment.

Output and productivity Regarding output and productivity, in the prensence of fixed costs the equilibrium output, $Y^* = F(H^*)$, unambiguously falls due to scale effects when $\bar{h}$ decreases. Productivity, measured by the average output per hour, is expressed as:

$$\frac{F(H^*)}{H^*}$$

With the concavity of the production function, the decrease in $F(H^*)$ will be less than proportional to the decrease in $\bar{h}$. The extent to which average hourly labor productivity increases depends on the concavity of the production function around the level of equilibrium hours.

This simple illustration demonstrates that while a reduction in per-worker hours may incentivize firms to employ more workers, it can also lead to negative scale effects due to

[5]When the hours per worker are also determined by firms, higher fixed costs tend to increase working hours per worker.

increased labor costs resulting from fixed costs. This negative impact on employment may be further exacerbated if the wage rate increases as a result of working hour reductions, a common occurrence in most reforms of this type. The increase in labor costs will inevitably lead to a reduction in production scale, but hourly labor productivity will increase due to the concavity of the production function.

In scenarios where both labor and capital are involved ($F(H, K)$), these predictions may change. When labor and capital act as substitutes, the increased labor costs also introduce substitution effects that tend to reduce employment. Negative scale effects could be mitigated if capital complements the production process, which would also result in a further increase in hourly labor productivity with more capital employed per unit of labor hour.[6] These predictions are highly dependent on the elasticity of substitution between labor and capital, as well as the share of capital used in production. Firms may respond to these changes in various ways. If we assume a lack of market competition and the presence of labor market power, the traditional predictions regarding labor costs and labor demand may not hold. Firms with product market power may increase prices, thereby reducing the negative impact on revenue even if output decreases. Another possible response is that firms may reorganize their production processes with a new production function. Lastly, workers may intensify their work, increasing the value of F', leading to smaller reduction in sales.

B.3 Data Appendix

This appendix lists how the variables are measured in the administrative data (QP), and also in the Labor Force Survey.

B.3.1 Quadros de Pessoal (QP - "Lists of Personnel")

Years available: 1985 to 2016

Firm-Level variables

firmbirth: year of firm creation; *legal*: firm legal status; *capital*: firm social capital, in euros; *capitalpriv*: firm share of private domestic capital; *capitalpub*: firm share of public capital; *capitalfor*: firm share of foreign capital; *nut1firm to nut3firm*: firm region at the NUT 1, 2, 3 level; *distfirm*: district location of the firm's headquarter; *municip-firm*: municipality location of firm's headquarter; *caef1 to 6*: economic activity of firm from 1 to 6 digits of disaggregation; *sales*: sales value from October t-1 to October t in euros; *nest*: number of establishments; *workersfirm*: number of workers employed by firm

Establishment-Level

headquarter: dummy equal 1 if establishment is headquarter; *nut1estab to nut3estab*: firm

[6] Less negative scales effects could also partly mitigate the negative effect on employment.

establishment at the NUT 1, 2, 3 level; *distestab*: district location of the establishment; *municipfirm*: municipality location of the establishment; *caest1 to 5*: economic activity of establishment; *workersest*: number of workers of establishment

Worker-Level

nationality: nationality of the worker; *gender*: gender of the work; *workerbirth*: year and month of birth; *age*: age in years of the worker; *hiring date*: year and month of hiring; *tenure*: tenure in years of the worker; *promdate*: year and month of last promotion; *colective*: collective agreement covering the worker; *employment*: worker employment type (employee, employer, self-employed, family-worker); *contract*: worker's contract type (fixed-term contract, permanent contract); *schedule*: worker's schedule (part-time, full-time); *educ1 to 3*: worker's education from 1 to 3 digits; *prof 1 to 6*: worker's profession 1 to 6 digits of disaggregation; *wage*: worker's monthly wage (separately for base remuneration, regular additional remuneration, and irregular remuneration); *hours month*: worker's hours in March (before 1993), or October (after 1993); *hours extra*: worker's overtime hours in March (before 1993), or October (after 1993); *hours_ week*: usual working hours (after 1993)

B.3.2 Labor Force Survey

Years available: 1985 to 2016

Worker-Level

ILOSTAT: ILO employment status; *stapro*: worker's professional status (employee, self-employed, employer); *ftpt*: worker's schedule (part-time, full-time); *hwusual*: working hours usually worked in a given week.

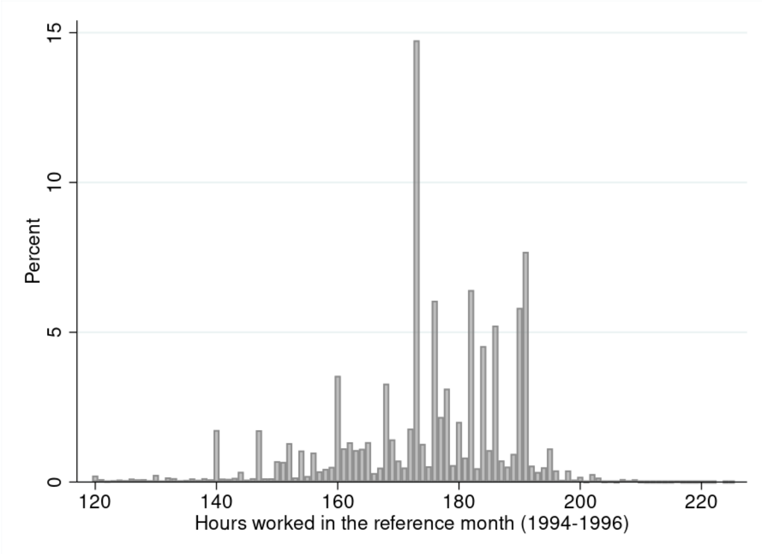
N.B.: Additional information is available in the national LFS (not the harmonized version obtained through Eurostat), which is not listed here.

B.3.3 Recovering standard hours in the QP before 1994

As mentioned in the Section 2.4, the information on standard hours is available in the QP only since 1994 (while actual hours worked in the reference month are always available). Therefore, for workers with missing information on standard hours, we impute them using the method outlined below.

To recover the standard hours of the workers, we use the fact that actual hours worked in the reference month recorded in the QP is strongly related to specific weekly standard hours. When filling hours information in the reference month, firms often calculate hours worked in the month based on the standard hours. For example, for a worker with standard hours of 40 per week and if the month has 22 work days, firms often report monthly actual hours as 176h (i.e., 8h per day multiplied by 22 days). In other cases, firms may multiply per-day standard hours by 21.625, the average number of working days per month, or

Figure B.1: Distribution of Actual Hours Worked in the Reference Month



Note: The figure shows the distribution of actual hours worked per month in the QP for years 1994-1996. The distribution displays many spikes associated with particular contractual hours per week.

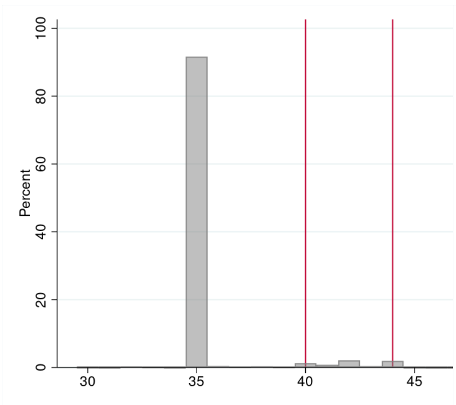
simply by 20, corresponding to 4 weeks. In this example, these results in the monthly hours of 173h and 160h, respectively.

This pattern is reflected as spikes in the distribution of monthly actual hours recorded in the QP, shown in Figure B.1. The largest spike is at 173h, amounting to approximately 15% of workers in the data between 1994 and 1996. The figure also indicates the presence of many other spikes in the distribution. We take advantage of these spikes in order to inversely extrapolate the standard hours from monthly actual hours worked. To do so, we exploit the 1994-1996 period, during which the legal maximum weekly hours were still 44h and the QP records both actual and contractual hours, to identify the most frequent standard hours within each bin of monthly hours worked. Figure B.2 shows some examples. In each panel, we plot the distribution of standard hours for workers with particular monthly hours: 140h, 173h, 182h, and 191h. We identify the most frequent standard hours as, respectively, 35h, 40h, 42h, and 44h for the corresponding hours in the reference month. We run the procedure for each bin of monthly actual hours to create a full correspondence between monthly hours and weekly standard hours. Then, for years before 1994, we assign the corresponding imputed standard hours according to the worker's monthly hours worked.[7] Since the legal maximum standard hours was 45h until 1989, for years up to then we assign 45h (i) in the case of the monthly actual hours that are likely be associated with 45h per week (i.e. 180, 189, 192, 194, 195, 198, 207) or (ii) if the worker's monthly actual hours exceed 202.4 (i.e. maximum number of hours that can be achieved under 44h regime with maximum number of working days per month in the data, 23 days).

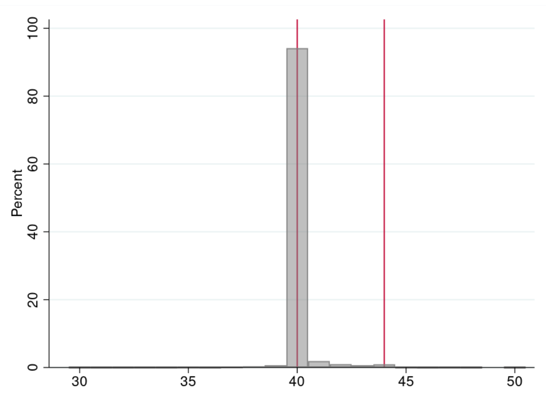
[7] For some workers, standard hours information is also missing or recorded as zero for years after 1994. We also use the imputed standard hours in these cases.

Figure B.2: Examples of Distribution of Standard Hours According to Actual Hours per Month

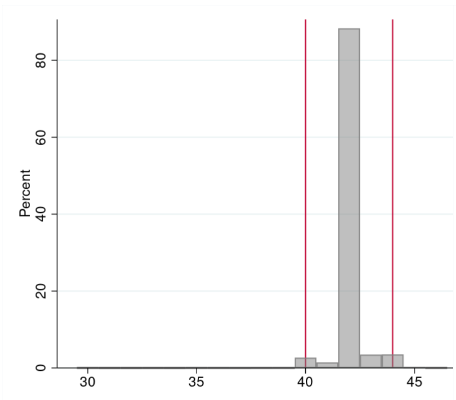
(a) Monthly Actual Hours = 140h



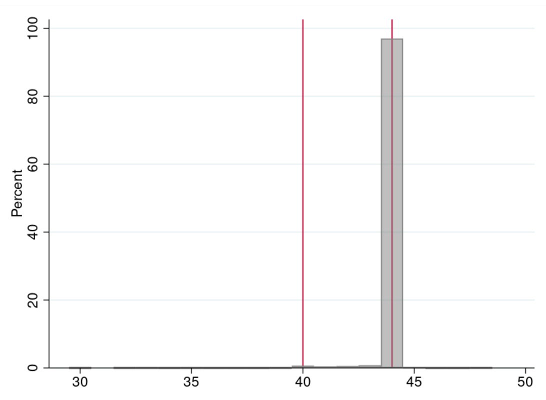
(b) 173h



(c) 182h



(d) 191h



Note: The figure presents the distribution of recorded weekly contractual hours in relation to the recorded actual hours worked per month for the period 1994–1996, taking specific examples of 140 hours, 173 hours, 182 hours, and 191 hours. The figure indicates the presence of specific contractual hours associated with particular monthly hours worked.

B.3.4 Creating a Panel of Collective Agreement

The QP records a unique collective agreement (CA) codes assigned to each individual worker. Every year, there are around 500-600 distinct CA codes, varying significantly in terms of their sizes (i.e., number of workers covered). Using these codes in a longitudinal dimension can be complicated for various reasons: there are instances where different CAs merge into one, workers under a specific agreement shift to another CA, or one CA splits into multiple new agreements. Furthermore, even without such changes, a CA might be renewed and assigned a new code. These dynamics create challenges in accurately identifying firms and workers that experienced treatment through collective agreements between 1991 and 1995/6.

To address this, we create a consistent panel of CA codes by leveraging the panel dimension of our data at the worker-level. The core idea of the procedure is simple: track the year-to-year transition of collective agreements and create a transition matrix for them. By restricting the sample to workers who remained in the same firm, we can identify the same collective agreement in the following year for each agreement. We repeat this for each pair of years to create a collective agreement panel and assign a new identifier to each agreement. However, as mentioned earlier, the year-to-year transition of collective agreements is not unique, particularly due to splits and merges. Therefore, we apply a slightly more complex but systematic approach to handle these instances. The idea is similar to snowballing: when a collective agreement splits or several collective agreements merge, we aggregate them and treat them as one collective agreement.

Below, we explain the exact procedure. This involves recursively identifying connected groups of collective agreements.

Creating Year-by-year Crosswalks First, we create crosswalks that track the dynamics of CAs between adjacent years.

(1) Keep years t and $t-1$ and workers who did not change firm between these two years.

(2) Collapse the data such that each row contains a pair of CA in t and CA in $t-1$ observed in the data (called CA_t and CA_{t-1} respectively hereafter), as well as the corresponding number of workers in each pair.[8] If there is a row with $CA_t = CA_{t-1}$, this CA exists in both periods. If a new agreement was signed and the new CA code had been assigned, then $CA_t \neq CA_{t-1}$ without either of the CA's having any duplicates. Multiple CA_t would appear if several CA_{t-1} were merged or one CA_{t-1} joined another. Similarly, if a CA_{t-1} has multiple entries, it means that this CA_{t-1} were split into different (or new) CA or a part of workers were separated into different (or new) CA in the subsequent year.

[8] For example, the row with $CA_t = 358$ and $CA_{t-1} = 358$ and the worker size in this row is 1002, then it indicates that there were 1002 workers whose CA in t was 358 and it was also 358 one year ago.

(3) For each CA_t, identify: (i) most frequent CA_{t-1} (i.e. the row with the largest number of workers recorded); (ii) if it has at least one CA such that $CA_t = CA_{t-1}$.[9]

(4) Repeat the step (3) for CA_{t-1} against CA_t ,

(5) Keep the rows that meet at least one of the following conditions:

   (i) $CA_t = CA_{t-1}$

   (ii) For CA_t that have no rows that satisfy the condition (i), we only keep the row with the most frequent CA_{t-1} (i.e. largest number of workers)

   (iii) Same condition as (ii) for CA_{t-1} against CA_t

   Note that, with this conditioning, we are not (always) keeping the other rows of CA_t than the row with $CA_t = CA_{t-1}$, if CA_t has at least one row that satisfies the condition (i). This is because, when one CA continues from $t-1$ until t, workers who held the same CA are the vast majority among all the workers (including those coming from other CA) at time t. However, theoretically, these rows will not necessarily be deleted because it can still meet the condition (iii), that is, if all workers covered previously by CA_{t-1} joins CA_t.

(6) Repeat the steps (1)-(5) for each year of t between 2000 and 1987. Note that we treat 1989 and 1991 as adjacent years as no worker file exists for 1990.

Creating a Collective Agreement Panel We combine the crosswalks to construct a panel of collective agreements. The basic idea is that we recursively merge the crosswalks from 2000 to 1986, and in the process, we assign a new CA ID to the group of connected collective agreements. Let us call the crosswalk containing the correspondence between CA's in t and $t-1$ (and the number of workers in each pair) as CW_t. Below details the process:

(1) Start with the CW_t. In effect, we use CW_{2000} as a starting point.

(2) We would like to avoid small linkages (i.e. CA pairs with small number of workers) connecting many collective agreements, leading to a small number of extremely large new CA groups. Therefore, we identify the rows (i.e. CA-pairs) with at least 100 workers (called "strong link") and drop the rows that fall in one of the following conditions:

   (i) If CA_t has multiple corresponding CA_{t-1} and at least one strong link, drop the rows of weak link (with below-100 workers)

[9] In the vast majority of cases, the same CA does appear in the adjacent year. Among CA's in 2000, nearly 95% of them has at least one row satisfying $CA_t = CA_{t-1}$

(ii) If CA_t has multiple corresponding CA_{t-1} but none of the rows have a strong link, keep the row with the CA_{t-1} that has the largest number of workers

(iii) Same as (i) and (ii), but the other way around looking from CA_{t-1}

(3) For any CA_t and CA_{t-1} that have multiple rows, we identify all the CA_t and CA_{t-1} that are linked with that CA. We then assign a new CA ID to all of them. For simplicity, we simply assign the highest value of the original CA code as a new ID. The dataset is reshaped such that it contains two columns, one for the list of the new ID (possibly duplicated) and the other the (original) CA codes corresponding to each new ID. The second column (with the original code) are then named simply as CA_{t-1} for the merging process in the next step.[10]

(4) Merge it with the CW_{t-1} based on the CA_{t-1}.[11]

(5) Do the same step (2) and (3), using the pairs of CA_{t-1} and CA_{t-2}.[12]

(6) Repeat the steps until reaching to the first year of the data (i.e. 1986).

At the end of this process, there are 341 distinctive new CA ID's containing 637 distinct original codes. Among the 341 new ID's, 55% have a unique corresponding CA (i.e. CA code that existed in all years, the code did not change, and not mixed with other CA's). 92% (97%) of the new ID's have maximum 3 (5) distinct CA's. The largest number of distinctive CA per new ID is 13. These numbers indicates that we successfully created a panel of CAs, avoiding to have a small number of new IDs containing many different CAs, by not using the weak links in the merging process. We merge this collective agreement correspondence with each worker based on their original CA code. In this way, our data contains a collective agreement identifier that is consistent over years, enabling us to identify firms treated by collective agreements.

[10]For example, if $CA_t = 57$ has two rows with each having $CA_{t-1} = 57$ and $CA_t = 102$, we treat the CA code 57 and 102 as in the same group and simply assign the highest number in the group as the as the new ID, i.e. 102. At the end of this step, the data has a column two new code with 102 and the other column with old code of 52 and 102.

[11]We use the STATA command *joinby* so that all the possible combinations are created in case of multiple CA_{t-1} existing in both crosswalks.

[12]Sometimes, after the step (3), there are still duplicates of the original CAs entering into multiple new codes. In this case, we repeat the grouping process so that the CA's connected to this CA will form again one new group with a new code.

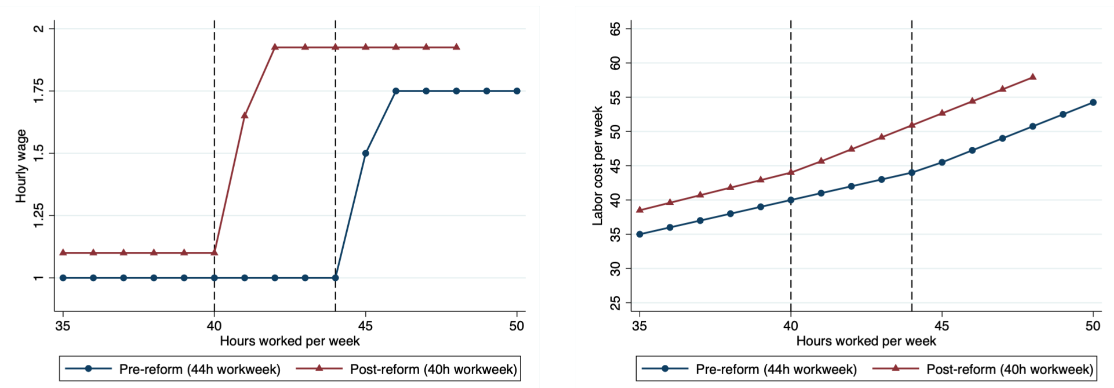
B.4 Supplementary Descriptive Statistics

B.4.1 Impact of the Hours Reduction on Salaries and Wages

In theory, the reduction in standard hours by the reform essentially changes the threshold hour from which the overtime premium applies. Panel (A) in Figure B.3 illustrates the expected change in the hourly wage schedule for workers who transitioned from a 44-hour workweek to a 40-hour workweek, assuming an initial base wage rate of 1. Prior to the reform, the wage rate remains at 1 until the 44th hour, but with the introduction of overtime premiums, it rises to 1.5 for the 45th hour and 1.75 for subsequent hours. Following the reform, the threshold shifts to 41 hours. A crucial aspect of the reform is that employers were not allowed to reduce the monthly salary for workers whose standard hours were reduced. This is depicted by the post-reform base wage rate of 1.1 (=44/40), indicating a 10% increase in hourly labor costs. Panel (B) portrays the corresponding monthly salary. Note that the monthly salary at 44 hours in the pre-reform schedule matches that at 40 hours in the post-reform schedule. To maintain a 44-hour workweek, the employer must pay a 16% higher monthly salary.

Figure B.3: Expected Change in Wage and Monthly Salary Schedule

(A) Hourly Wage (B) Monthly Salary



Note: This figure illustrates the expected changes in both the hourly wage schedule and monthly salary schedule for workers whose standard hours were reduced from 44 to 40 hours. In Panel (A), the wage schedule is presented, with the regular hourly wage set at 1, as a function of hours worked. The red line indicates the wage schedule post-transition to the 40-hour workweek, where the baseline wage rate is higher because employers are not permitted to reduce monthly remunerations in response to the reduction in standard hours. Panel (B) depicts the corresponding monthly salary schedule. The post-reform schedule ends at 48 hours because overtime is restricted to a maximum of 8 hours per week.

B.4.2 Evolution of Hours

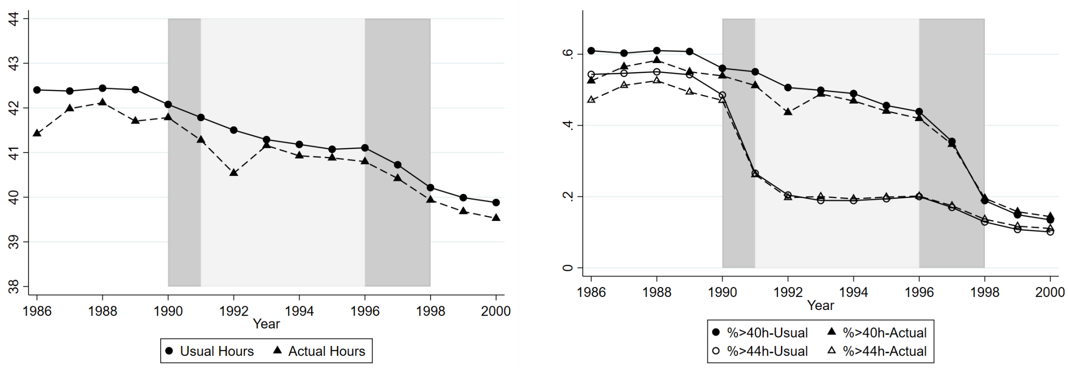
The effects of the reforms are clearly visible in LFS survey data, as shown in Figure B.4, where average hours and the share of full-time workers above 40 and 44 hours a week are plotted over the period 1986 to 2004. Hours appear relatively stable before 1990, when, as expected, a decline begins. This decline continues linearly until 1996, when the national reform occurs, and then hours drop sharply to then stabilize. A very similar picture emerges from panel (b): the share of workers above 44 hours drops sharply after 1990, when hours were decreased to 44 per week. The share above 40 decrease more gradually between 1991 and 1996, as some collective agreements gradually decrease hours, and then drops sharply after the national reform. The effects of the different legislative changes are also clearly visible in the distribution of hours (Figure B.6). The distribution of hours that peaked at 45 hours before 1990 shifts to 44 and lower hours. After 1998, when the second national reform is fully implemented, the peak is clearly at 40 hours.

We observe a similar trend in working hours recorded in the administrative dataset used throughout this paper in Figure B.5 (see Figure 2.2 that use imputed standard hours for years before 1994). In panel (a), there is a progressive decline in the average actual weekly working hours observed throughout the early- and mid-1990s, subsequently leading to a significant reduction following the reform implemented in 1996. Similarly, the proportion of workers performing 44 hours or more demonstrates a corresponding trend, as depicted in panel (b).

Figure B.4: Average Actual and Usual Hours of Full-Time Workers and Share Above 40 and 44 hours, 1986 to 2004, EU Labor Force Survey

(a) Average Actual and Usual Hours

(b) Share Above 40 and 44 hours



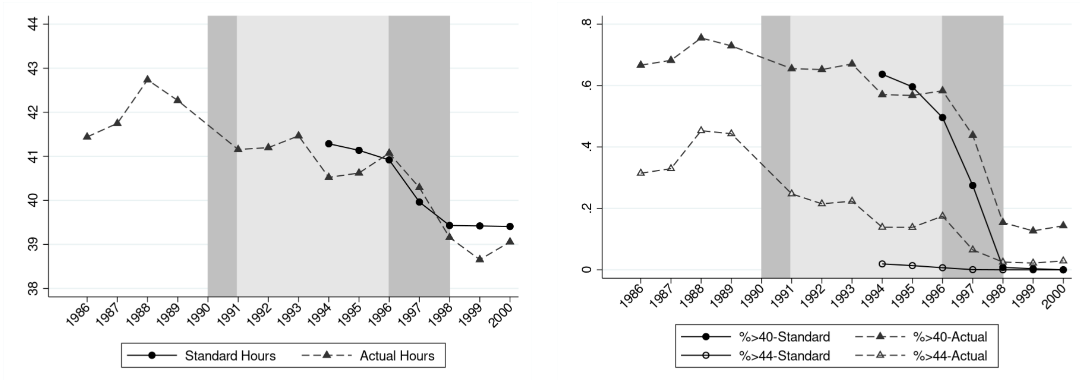
Note: The darker shaded area indicates the period of the national reform, while the lighter gray indicates the period of reform of collective agreements. The LFS measures both weekly hours usually worked throughout the year, and actual hours worked in the past week. The sampling structure of the LFS was changed to quarterly in 1993, which explains the lower variation in actual hours.
Source: Authors' calculations on EU-LFS data.

B.4.3 Distribution of Hours

Figure B.6 provides the distribution of self-reported usual hours worked in the Labor Force Survey. In panel (a), the period 1986-89 exhibits a bimodal distribution with 40h and 45h. The 40 hour contract were for workers in the collective agreements that had a 40h-regime or

Figure B.5: Average Standard and Actual Hours of Full-Time Workers and Share Above 40 and 44 hours, 1986 to 2000, QP

(a) Average Actual and Standard Hours (b) Share Above 40 and 44 hours



Note: The panel (a) shows the average standard and actual hours. The panel (b) shows the share of workers with standard hours and actual hours above 40h and 44h, respectively. Standard hours are available in the QP only from 1994. See Figure 2.2 which replicates the figures with imputed standard hours. Actual working hours were derived by dividing the actual hours worked in the reference month by 21.625, which is the average number of working days per month. The darker shaded area indicates the period of the national reform, while the lighter gray indicates the period of reform of collective agreements.
Source: Authors' calculations based on the QP.

the office workers whose standard hours were set lower.[13] From panel (b), in the early- to mid-1990s, working hours shifted to 44h, given the 1990 reduction of standard hours from 45h to 44h. However, the figure indicates that many workers responded to work 45h. It is perhaps due to misreporting or overtime hours (which were calculated over the average of 3 months – thus some workers could have worked more than 44h in some weeks). Note also that there is more mass at 40h because some collective agreements reduced standard hours during this period. Lastly, panel (c) indicates that most workers moved to 40h following the reform in 1996.

Figure B.7 shows the distribution of standard hours recorded in the QP, the dataset used in our estimations. The standard hours before 1994 are imputed. The figure also exhibit similar shift in the hours distribution as in the Labor Force Survey. The distribution of actual hours worked per week in the QP provide similar shift as is clear from the Figure B.8. Since actual hours fluctuate more and are influenced by the number of working days in the reference month in each year's QP, the distribution is fuzzier than the standard hours.

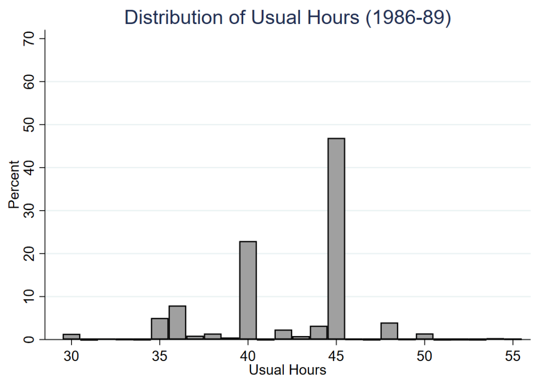
Figure 2.2 is supplementary to Figure B.5, where the evolution of standard hours are extend to pre-1993 years with the imputed standard hours by the methodology outlined

[13]Specifically, the law specified 7 hours per day and 42 hours per week for office workers. The law also specified that employers could increase the daily limit by one hour if an additional half-day or day of rest was provided. Therefore, in practice the daily limit could be increased at 8 hours per day for office workers and most office workers worked 40h per week with two rest days.

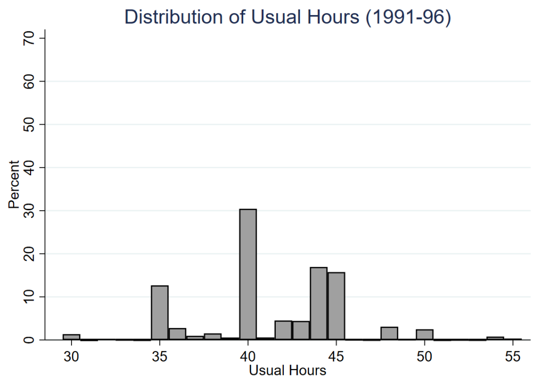
in Section B.3.3. Figure B.12 shows the evolution of the share of workers working on weekends between 1992 and 2001.Figure B.9 shows the differences in number of working days in the reference month of the QP across difference years. The maximum is 23 days and the minimum is 20 days.

Figure B.6: Distribution of Usual Working Hours for Full-Time Employees, Labor Force Survey, 1986-89, 1991-96, & 1998-04

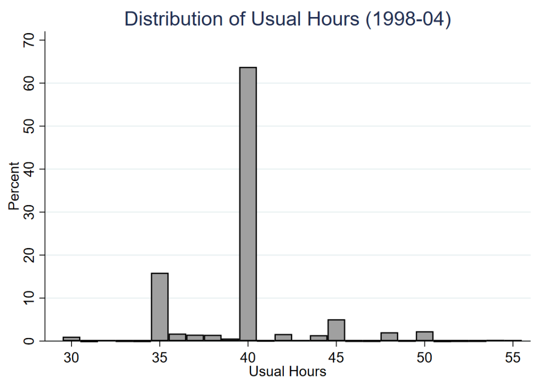
(a) 1986-89



(b) 1991-96



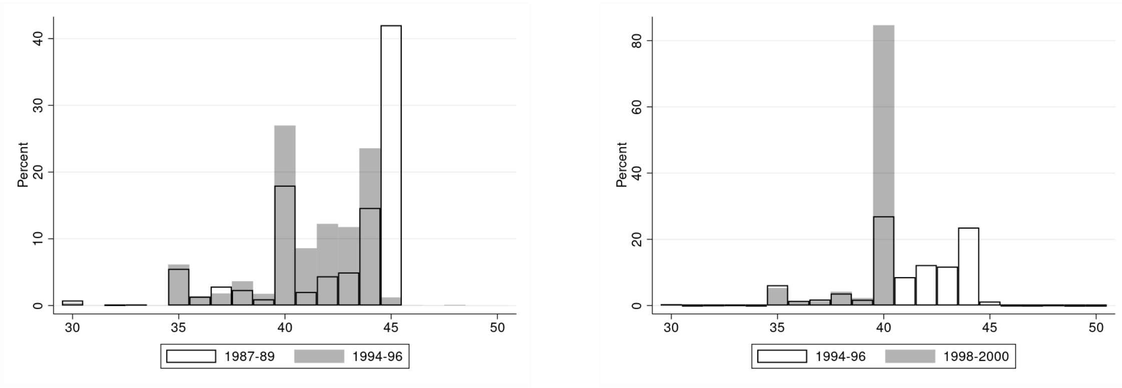
(c) 1998-04



Note: These graphs plot the distribution of usual working hours for full-time employees over the three different periods in which the legislation was different. In the period 1986 to 1989, standard hours were set at 48, or 45 over 5 days. In the period 1991-96, they were set at 44 hours per week. As from 1998, a 40 hour working week was introduced.
Source: EU- LFS

Figure B.7: Distribution of Standard Hours for Full-Time Employees, QP

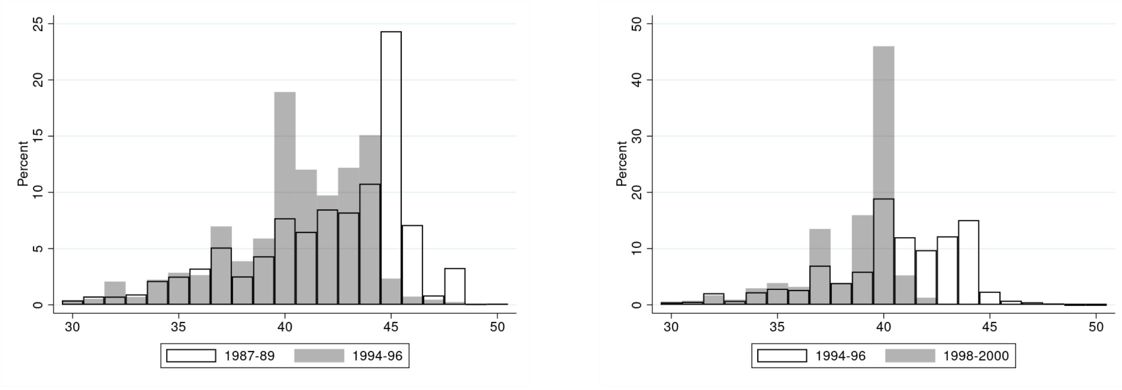
(a) 1987-89 and 1994-96 (b) 1994-96 and 1998-2000



Note: These graphs plot the distribution of weekly standard hours for full-time employees, as recorded in the QP. In the period 1986 to 1989, standard hours were set at 48, or 45 over 5 days. In the period 1991-96, they were set at 44 hours per week. As from 1998, a 40 hour working week was introduced.
Source: QP

Figure B.8: Distribution of Actual Hours Worked for Full-Time Employees, QP

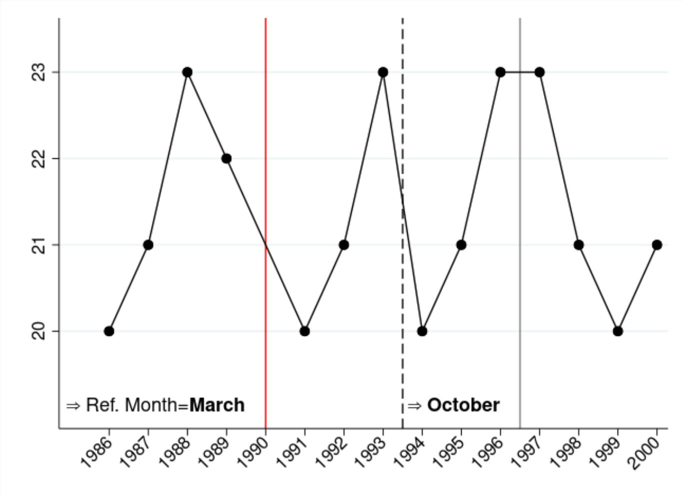
(a) 1987-89 and 1994-96 (b) 1994-96 and 1998-2000



Note: These graphs plot the distribution of weekly standard hours for full-time employees, as recorded in the QP. In the period 1986 to 1989, standard hours were set at 48, or 45 over 5 days. In the period 1991-96, they were set at 44 hours per week. As from 1998, a 40 hour working week was introduced.
Source: QP

B.4.4 Number of Working Days in the Reference Month

Figure B.9: Number of Working Days in the Reference Month in the QP



Note: This graph plots the count of working days within the reference month of the QP from 1986 to 2000. The reference month changed from March to October since 1994. The fluctuations in the count of working days over the years stem from the differences in the number of Saturdays, Sundays, and national holidays within the reference month for each respective year.

B.4.5 Variation of Standard Hours

To understand the sources of variation in standard hours, Table B.2 provides a summary of the R^2 obtained from regressions where worker-level standard hours are regressed with fixed effects of various dimensions. We restrict the analysis to the 1996 sample for full-time workers with standard hours ranging from 30 to 50 hours.

In Table B.2 Column (1), we find that collective agreements account for 56% of variation in standard hours. In Column (2), we observe that 3-digit industry fixed effects alone explain nearly half of the variation, while in Column (3), municipality fixed effects capture only 20% of the variation. When we consider the fixed effects of the combination of industry and municipality in Column (4), we see these two factors jointly explaining slightly over 60% of the variation.

In Column (5), when we introduce collective agreement fixed effects alongside industry-municipality fixed effects, R^2 only marginally improves, suggesting that collective agreements already largely reflect a combination of sector and geographical dimensions. Further, in Column (6), adding occupation fixed effects increases the R^2 by 0.1. Occupation alone does not account for a significant portion of the variation.

Finally, in Column (7), we further add firm-fixed effects, which explain 87% of the variation in standard hours.

Table B.2: Source of Variation in Standard Hours

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Adjusted R^2	0.562	0.497	0.197	0.612	0.645	0.740	0.871
CA-FE	✓	No	No	No	✓	✓	✓
Other FE	No	Sector	Location	Sec-Loc	Sec-Loc	Sec-Loc-Occ	+Firm
Observations	1,879,545	1,879,550	1,815,970	1,814,434	1,814,428	1,765,740	1,718,625

Note: The table compares the R^2 from the regressions explaining the standard hours by different combinations of fixed effects. The sample is of the year 1996 and consists of workers that are employees with standard hours between 30 and 50. Column (1) only uses 492 collective agreements fixed effects; Column (2) uses 215 3-digit industry classifications; Column (3) uses 275 municipalities; Column (4) uses the interaction of industry and municipalities; Column (5) adds industry-municipality fixed effects on top of the collective agreement fixed effects; Column (6) further interact industry-municipality pairs with 118 3-digit occupation categories; lastly, column (7) further adds firm-level fixed effects.

B.5 Supplementary Results for the Empirical Analysis

B.5.1 Supplementary tables

Table B.3 provides the regression table for Figure 2.4. Similarly, Table B.4 provides the regression table corresponding to the Figure 2.5, showing the effects of the reduction in working hours on the share of workers separated and newly hired.

Table B.4: Effect of Working Hour Reductions on Worker Flows

	Share of Workers:	
	Separated (1)	Newly Hired (2)
$Treat \times Post$	-0.009***	-0.024***
	(0.003)	(0.005)
Mean Outcome	0.1	0.2
R-squared	0.56	0.49
Observations	398,171	398,171

Note: The regression table displays the effects of the 1996 reform on worker flows at the firm level. The outcomes are share of workers separated and share of new workers hired, relative to the previous year's employment size. The regressions employ data from the years 1994-2000 and difference-in-differences comparing treated and control firms as defined in the previous section. On top of the firm fixed effects and sector-year fixed effects, the regression includes firm-specific trends to deal with the pre-existing trend observed for these outcomes. There are slightly smaller observations because for a few firms, no workers' hiring date was recorded in the data. Standard errors are reported in parentheses; * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

B.5.2 Additional Results

Role of market concentration Under imperfect competition, the impact of increased labor costs on labor demand may not necessarily be negative (Manning, 2003). To investigate this, we examine the heterogeneity in employment effects across labor markets, which we define by a combination of municipality and 2-digit economic sector.[14] For each labor market, we compute the Herfindahl-Hirschman Index (HHI) based on employment, utilizing each firms' employment share in the market, and categorized the markets into three groups (bottom, middle, and top third) according to the index.[15] We then estimate the heterogeneous employment effects by interacting the treatment variable with dummies representing each of the three market groups.

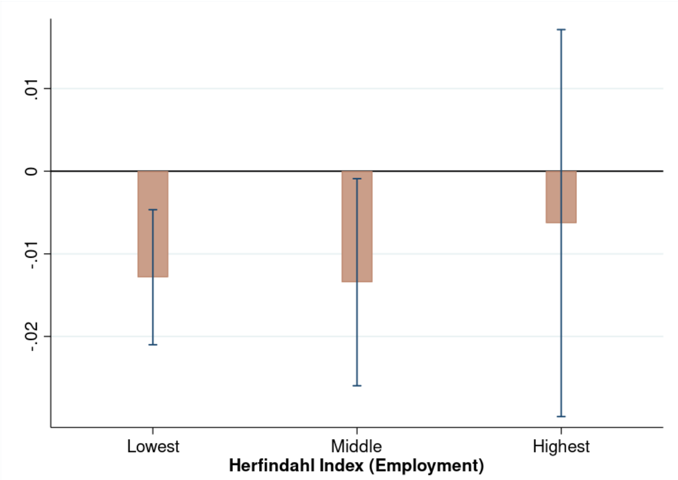
Figure B.10 illustrates the heterogeneous employment effects across HHI, showing that the negative employment effects were least pronounced among firms operating in more concentrated labor markets. While there are substantial standard errors, the point estimate

[14] We also conducted an analysis using only the municipality dimension and obtained similar results.

[15] The Herfindahl-Hirschman Index (HHI) is computed as follows: $HHI_m = \sum s_{jm}^2$, where sjm represents the share of firm j's employment in market m.

is the smallest in markets with the highest concentration.[16] This is suggestive evidence that monopsony power might also play a role in the context of working hour reforms. However, even in the most concentrated labor markets, we can rule out a positive work-sharing effect.

Figure B.10: Heterogeneity in the Employment Effects, by Market Concentration



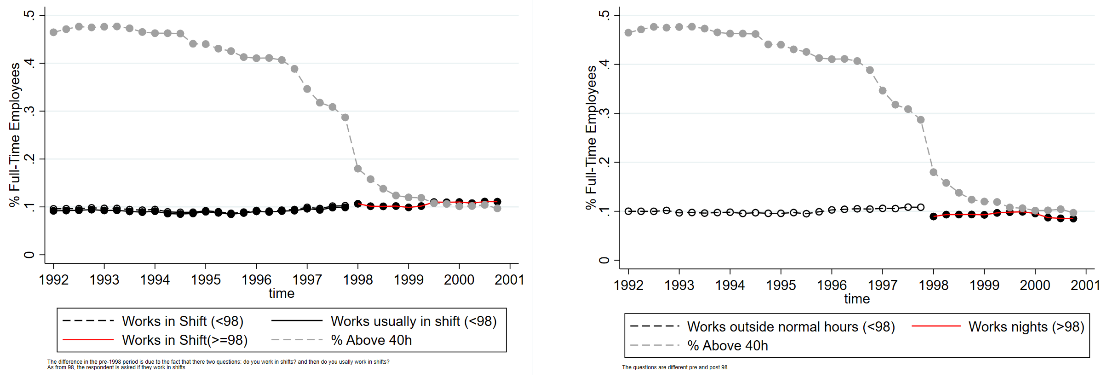
Note:The figure shows the heterogeneity in employment effects of the 1996 reform across labor markets with differing levels of market concentration. The estimation is based on Equation 2.3, where we further interact the treatment variable with dummy variables indicating each group of labor market concentration, and add firm-specific trends. Labor markets are defined by a combination of municipality and 2-digit sector, and their concentration is measured using the Herfindahl-Hirschman Index based on each firms' employment share within each labor market (averaged over the 1994-1996 period). We categorize the markets into three groups (bottom, middle, and top third) according to the index. The heterogeneous coefficients are obtained by interacting the treatment variable with dummies indicating three groups of markets.

Weekend, night and shift work. Figure B.11 displays the changes in the percentage of workers engaged in shift work or night shifts, as calculated from the Portuguese national Labor Force Survey. Note that there was a survey questionnaire change in 1998, which creates a discontinuity in the data. Likewise, Figure B.12 illustrates the trends the percentage of workers working on weekends. None of these outcomes exhibits a significant shift around the time of the reform between 1996 and 1997.

[16]A higher HHI indicates a smaller number of firms per market by design. Consequently, the number of firms in the most concentrated market is mechanically smaller, which can lead to larger standard errors.

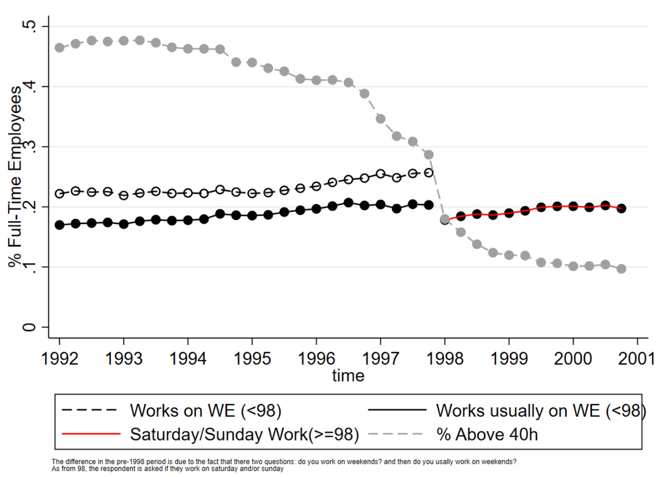
Figure B.11: Evolution of Shift and Night Work Over Time

(A) % of Workers Working in Shifts (B) % of Workers Working Nights



Note: The figure shows the share of workers working in shifts (panel A) and performing night work (panel B) calculated from the Portuguese national Labor Force Survey. Note that the survey questionnaire changed since 1998, creating a break in a series.

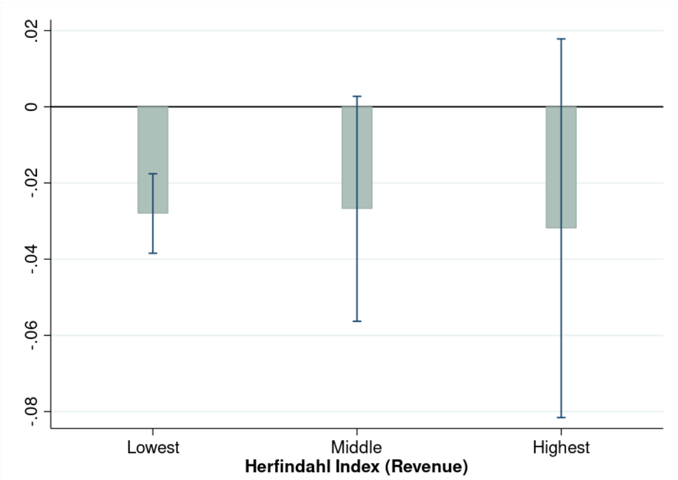
Figure B.12: % Workers Working of Week-Ends



Note: The figure shows the share of workers working who work during the weekend, calculated from the Portuguese national Labor Force Survey. Note that the survey questionnaire changed since 1998, creating a break in a series

Role of prices. The positive impact of the 1996 reform on sales per hour may potentially reflect that firms increased the prices of their goods to compensate for the loss of sales. To test this hypothesis, we examine how the reduction in working hours affected sales in different types of markets. In theory, a firm's ability to raise prices to cover the increased labor costs depends on how much competition it faces in the product market. This leads to a testable prediction that firms in less competitive market should experience less negative effects on nominal sales. To test this hypothesis, we quantify the level of product market concentration by using firms' sales data. Following Autor et al. (2020), we calculate the Herfindahl index of sales in each 4-digit sector, split these sectors into three groups (bottom, middle, and top third), and estimate the heterogeneity in the effects on firms' sales across these three groups. The result is shown in Figure B.13. The estimated effects on sales are similar across firms operating in product markets with different levels of sales concentration.[17] This provides indirect evidence that price was not a major mechanism for firms to compensate for the loss in output.

Figure B.13: Heterogeneity in Sales Effects, by Product Market Concentration



Note: The figure shows the heterogeneity in sales effects of the 1996 reform across 4-digit industries with differing levels of sales concentration. The estimation is based on Equation 2.3, where we further interact the treatment variable with dummy variables indicating each group of product market concentration, and add firm-specific trends. The concentration is measured using the Herfindahl-Hirschman Index based on each firm's sales share within each industry (averaged over the 1994-1996 period). We categorize the industries into three groups (bottom, middle, and top third) according to the index. The heterogeneous coefficients are obtained by interacting the treatment variable with dummies indicating three groups.

Capital intensity. Increasing the capital use in the short run is likely more feasible for firms operating with higher capital intensity in their production processes. Conversely, firms that heavily rely on labor may face difficulties in reducing their labor input. To explore the heterogeneity effects on employment and productivity, we consider the capital intensity of the sector. Using data from the EU KLEMS database that provide capital compensation measures at the 2-digit sector level, we compute the sector-level capital intensity.[18] The results are presented in Figure B.14. In Panel (A), we observe that the negative employment effects are more pronounced for firms in sectors with high capital intensity. This is consistent with the notion that firms increased their use of capital in

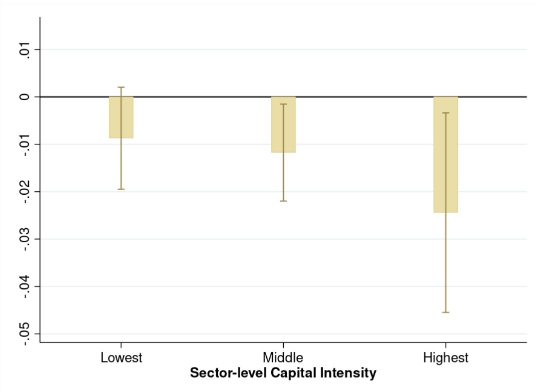
[17] Standard errors become larger for more concentrated markets because there are fewer firms in those markets by construction. We also tried alternative definitions of concentration, such as the sales share of treated firms in each 4-digit sector, using 6-digit sectors, or using municipality-level concentration for the service sector (assuming that products are locally consumed). We still did not detect differential effects on sales across market concentration in these cases.

[18] Capital intensity is calculated by dividing total capital compensation by total value added, and then averaging the values over the period from 1994 to 1996.

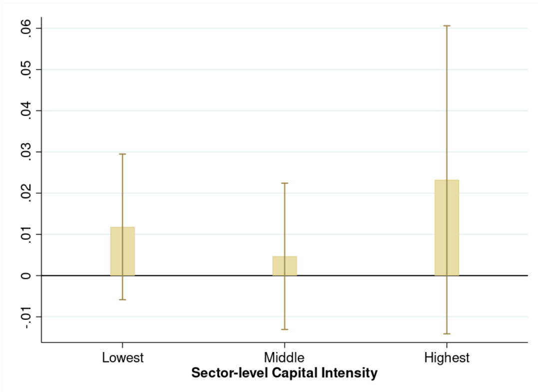
response to reduced work hours and higher labor costs. Furthermore, in line with this, Panel (B) demonstrates that in these sectors, the increase in hourly labor productivity exhibits the most significant in terms of point estimates. This suggests that intensified capital in production leads to higher productivity per labor hour input. It is important to note, however, that even firms in sectors with the lowest capital intensity experienced an increase in hourly labor productivity. This provides indirect evidence of the presence of alternative mechanisms, such as diminishing marginal returns or work intensification, as discussed earlier.

Figure B.14: Treatment Effects by Capital Intensity

(A) Employment Effects



(B) Productivity Effects



Note: The figure shows the heterogeneity in employment and productivity effects of the 1996 reform across 2-digit sectors differing in the pre-reform capital intensity. The estimation is based on Equation 2.3, where we further interact the treatment variable with dummy variables indicating each group of capital intensity, and add firm-specific trends. Capital intensity is defined as a fraction of capital compensation and total value added in each sector and averaged over the 1994-1996 period. Both information is obtained from EU KLEMS database, Sectors are subsequently categorized into three groups (bottom, middle, and top third) based on their capital intensity scores. To estimate the coefficients for each group, we interact the group dummy variables with the treatment variable. Note that the standard errors are larger the high capital intensity group because there are smaller number of firms in this group.

ICT and non-ICT capital. Table B.5 supplements the sector-level analysis of capital in Table 2.4 by breaking down capital into ICT and non-ICT categories and examining the capital service per labor hour. The results indicate that 2-digit sectors with greater reductions in working hours also experienced increased growth in capital services and capital service use per hour. These effects are more pronounced for ICT capital.

Firm survival. Table B.6 investigates if the probability of firm survival was affected by the 1996 reform. We analyze firms separately depending on their first appearance in the pre-treatment years (1994–1996). We fill the data so that each firm is tracked until the end of the sample period (i.e., 2000) following its initial appearance. We then create a variable indicating firm survival, assigned a value of 1 if the firm continues to operate and 0 for all years after its exit. Our findings indicate that the reform has no significant effect on the probability of firms remaining operational.

Table B.6: The Effect of the 1996 Reform on the Firm Exit Probability

	Exit of Firms Observed Since:		
	(1)	(2)	(3)
	1994	1995	1996
Treatment	0.006	-0.004	0.010
	(0.006)	(0.014)	(0.012)
Observations	355,810	62,316	61,980

Note: The regression tables present the impact of the 1996 reform on firms' likelihood of exiting the market. The dependent variable is an indicator that takes the value 1 if the firm is still active and 0 if it has ceased to exist in the data. We conduct separate analyses based on the firm's appearance in the data between 1994 and 1996. The regression includes year fixed effects, sector-year fixed effects, and treatment group-specific linear trends. Standard errors in parentheses; * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

B.5.3 Robustness checks

In this subsection, we provide the results of several robustness checks for the results presented in Table 2.3.

Firm-specific trend To test if the results are driven by the differential pre-trends between firms in the control and the treated groups, Table B.7 adds the firm-specific linear trend in the estimation. Doing so attenuates the coefficients because a part of the variation in outcomes is absorbed by the firm-specific trend, but the results are largely consistent with the main results.

Robustness to cut-offs In the main specification we use firms with no treated workers as our control group. To examine the robustness of our findings under different treatment definitions, Table B.8 compares the effect on the key outcomes using different treatment definitions, with the inclusion of firm-specific linear trends. The first row replicates the main results (with firm-specific trends) using only firms with no treated workers as a control group. In the second and third rows, we use firms in the bottom third and firms below the median in terms of share of treated hours as criteria to define control and treated firms. The magnitudes of the coefficients decrease as firms categorized as control and treated become more similar. Nevertheless, our findings remain consistent across the alternative treatment definitions.

All firms The results are based on a sample of firms where the mode of standard hours did not change by more than one hour between 1994 and 1996. This is to prevent contamination from firms that might have been undergoing a reduction in working hours through collective agreements before the 1996 reform. Therefore, our findings rely on a sample of firms operating at "equilibrium hours". To assess whether our results are influenced by this specific sample selection, Table B.9 presents the results without excluding any firms. In other words, all firms that existed between 1994 and 1996 (but after the common sample selection such as on sectors) are included in the sample. In the most conservative specification with firm-specific linear trends, the results remain largely consistent.

Sample selection Finally, we show that our results are robust to the exclusion of firms born just before 1996. Table B.10 shows the estimated effects on key outcomes by excluding firms by restricting firms born before 1995 (second row) and by 1994 (third row). The coefficients point to the similar conclusion, that is, hours are reduced, employment and sales were decreased, and hourly labor productivity increased. The estimated effects on sales are substantially smaller when we exclude recently-created firms, indicating a sudden reduction in hours and increase in labor cost was particularly large for newly-born firms.

Table B.3: The Effects of the 1996 National Reform

(a) Hours and Labor Cost

	Hours			Labor Cost	
	Standard	Actual	Overtime	Monthly salary	Wage
	(1)	(2)	(3)	(4)	(5)
Treated × 1994	-0.079***	-0.033	-0.015	-0.001	-0.002
	(0.010)	(0.026)	(0.022)	(0.004)	(0.004)
×1995	-0.082***	-0.048**	0.007	0.000	0.001
	(0.008)	(0.021)	(0.018)	(0.003)	(0.003)
×1997	-2.033***	-1.642***	0.020	0.009***	0.048***
	(0.012)	(0.020)	(0.015)	(0.003)	(0.003)
×1998	-3.113***	-2.746***	-0.002	0.004	0.069***
	(0.011)	(0.022)	(0.019)	(0.003)	(0.003)
×1999	-3.128***	-2.831***	-0.001	-0.003	0.065***
	(0.012)	(0.026)	(0.021)	(0.003)	(0.003)
×2000	-3.151***	-2.803***	0.015	-0.003	0.064***
	(0.012)	(0.024)	(0.029)	(0.003)	(0.003)
Mean Outcome	41.3	40.7	0.2	6.3	1.1
R-squared	0.83	0.67	0.62	0.81	0.81
Observations	398791	398791	398791	398791	398791

(b) Labor Input and Sales

	Labor Input		Sales		
	Employment	Total Hours	Total	Per Worker	Per Hour
	(6)	(7)	(8)	(9)	(10)
Treated × 1994	-0.012**	-0.017***	-0.012	-0.003	-0.009
	(0.005)	(0.006)	(0.008)	(0.008)	(0.008)
×1995	-0.008**	-0.007	0.001	0.002	-0.006
	(0.004)	(0.005)	(0.005)	(0.005)	(0.006)
×1997	-0.016***	-0.064***	-0.030***	-0.015***	0.023***
	(0.003)	(0.004)	(0.004)	(0.004)	(0.005)
×1998	-0.022***	-0.098***	-0.039***	-0.019***	0.043***
	(0.004)	(0.005)	(0.005)	(0.006)	(0.006)
×1999	-0.030***	-0.115***	-0.050***	-0.020***	0.055***
	(0.005)	(0.006)	(0.006)	(0.006)	(0.007)
×2000	-0.035***	-0.120***	-0.056***	-0.023***	0.046***
	(0.006)	(0.007)	(0.007)	(0.007)	(0.007)
Mean Outcome	1.6	6.5	12.6	10.9	6.1
R-squared	0.95	0.93	0.96	0.90	0.89
Observations	398,791	398,791	398,791	398,791	398,791

Note: The tables display the regression results corresponding to Figure 2.4, estimating the effects of the 1996 reform in Equation 2.3. All outcomes are in log, except for the hour in columns (1)-(3). The outcomes in columns (1)-(5) represent firm-level average values for full-time equivalent workers who worked at least 30 hours per week. The variable *Treat* is 1 for treated firms and 0 for control firms, and it is interacted with year dummies. There is no coefficient for the reference year of 1996. Standard errors are clustered at the firm level. Standard errors in parentheses; * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table B.5: Disaggregation of Capital Effects

	Capital Services			Capital Services Per Hour	
	Overall	Non-ICT	ICT	Non-ICT	ICT
	(1)	(2)	(3)	(4)	(5)
Treat × *Post*	0.014	0.004	0.065	0.024	0.085*
	(0.013)	(0.013)	(0.041)	(0.023)	(0.043)
R-squared	0.99	0.99	0.99	1.00	1.00
Observations	175	175	175	175	175

Note: This table complements Table 2.4 by examining the impact on capital, with a breakdown into non-ICT capital and ICT capital, as well as their values per labor hour. The unit of analysis is the 2-digit sector, and all information regarding capital is sourced from the EU KLEMS database. Treated sectors are those above the median in the average of firm's treated hour shares, as defined in Equation 2.1, while the control sectors are those at or below the median. The regression includes year fixed effects, sector fixed effects and sector-specific linear trend. Standard errors in parentheses; * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table B.7: Robustness: Controlling for Firm-specific Trends

(a) Hours and Labor Cost

	Hours			Labor Cost	
	Standard	Actual	Overtime	Monthly salary	Wage
	(1)	(2)	(3)	(4)	(5)
Treat × *Post*	-1.986***	-1.585***	0.011	0.011***	0.049***
	(0.014)	(0.025)	(0.022)	(0.004)	(0.004)
Mean Outcome	41.3	40.7	0.2	6.3	1.1
R-squared	0.90	0.79	0.77	0.88	0.88
Observations	398,791	398,791	398,791	398,791	398,791

(b) Labor Input and Sales

	Labor Input		Sales		
	Employment	Total Hour	Total	Per Worker	Per Hour
	(6)	(7)	(8)	(9)	(10)
Treat × *Post*	-0.013***	-0.056***	-0.032***	-0.021***	0.019***
	(0.004)	(0.006)	(0.005)	(0.006)	(0.007)
Mean Outcome	1.6	6.5	12.6	10.9	6.1
R-squared	0.98	0.97	0.98	0.95	0.94
Observations	398,791	398,791	398,791	398,791	398,791

Note: The tables display the results of the working hour reduction the 1996 reform analogous to Table B.7, but control for the firm-specific linear trends. All outcomes are in log, except for the hour measures in the columns (1)-(3). The variable *Treat* takes 1 for the treated firms and 0 for the control firms. *Post* takes 1 the years after 1997 and 0 otherwise. The outcome variables are regressed on the interaction of *Treat* and *Post* to provide the difference-in-differences estimate of the effects of the working hour reductions. Standard errors are clustered at the firm level. Standard errors in parentheses; * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

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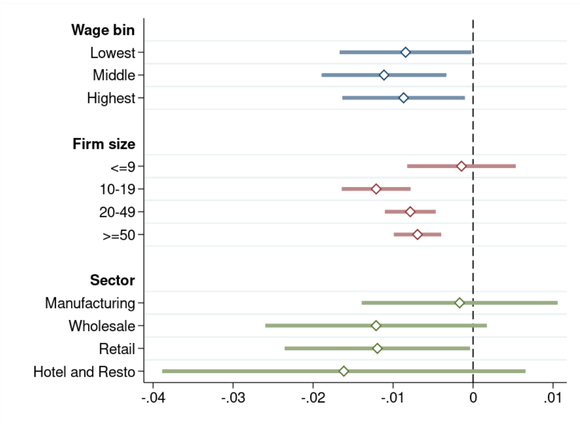
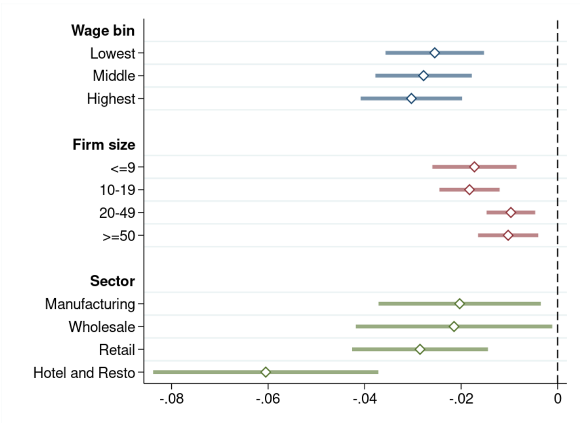
Figure B.15: Heterogeneous Effects of Working Hour Reductions by National Reform

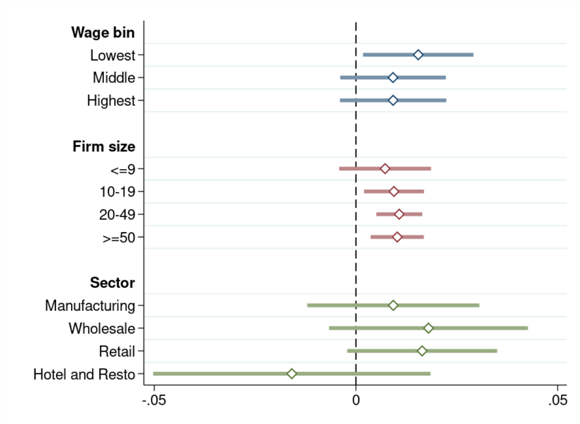
(a) Employment



(b) Sales



(c) Sales per hour



Note: The figures illustrate the heterogeneity in the effects on employment, sales, and sales per hour. The coefficients are obtained by interacting the treatment variables with group indicators. Wage bins and firm sizes are based on the 1994-1996 averages. Grouping for wage bin is in the bottom, middle, and top third in each variable.

Table B.8: Robustness: Using Alternative Definition for Control Group

(a) Hours and Labor Cost

	Hours			Labor Cost	
	Standard (1)	Actual (2)	Overtime (3)	Monthly salary (4)	Wage (5)
Control group defined as:					
$ShareHourTreat = 0$	-1.986***	-1.585***	0.011	0.011***	0.049***
(Baseline)	(0.014)	(0.025)	(0.022)	(0.004)	(0.004)
Bottom Third	-2.026***	-1.615***	0.025	0.013***	0.051***
	(0.013)	(0.024)	(0.026)	(0.003)	(0.003)
Below Median	-1.613***	-1.276***	0.004	0.010***	0.040***
	(0.014)	(0.023)	(0.021)	(0.003)	(0.003)

(b) Labor Input and Sales

	Labor Input		Sales		
	Employment (6)	Total Hour (7)	Total (8)	Per Worker (9)	Per Hour (10)
Control group defined as:					
$ShareHourTreat = 0$	-0.013***	-0.056***	-0.032***	-0.021***	0.019***
	(0.004)	(0.006)	(0.005)	(0.006)	(0.007)
Bottom Third	-0.013***	-0.057***	-0.031***	-0.021***	0.022***
	(0.004)	(0.005)	(0.005)	(0.006)	(0.006)
Below Median	-0.012***	-0.041***	-0.027***	-0.016***	0.014**
	(0.003)	(0.004)	(0.004)	(0.005)	(0.005)

Note: The tables display the results of the working hour reduction the 1996 reform analogous to Table B.7, using alternative definitions of treatment groups. Instead of using the bottom quarter in terms of the share of hours treated (where the share of hours treated is effectively zero), we use the bottom third and below median. Standard errors in parentheses; * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

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Table B.9: Robustness: Using All Firms

(a) Hours and Labor Cost

	Hours			Labor Cost	
	Standard	Actual	Overtime	Monthly salary	Wage
	(1)	(2)	(3)	(4)	(5)
Treat × *Post*	-1.675***	-1.327***	0.052**	0.006**	0.038***
	(0.013)	(0.022)	(0.021)	(0.003)	(0.003)
Mean Outcome	41.2	40.6	0.2	6.3	1.1
R-squared	0.84	0.76	0.76	0.87	0.88
Observations	588,820	588,820	588,820	588,820	588,820

(b) Labor Input and Sales

	Labor Input		Sales		
	Employment	Total Hour	Total	Per Worker	Per Hour
	(6)	(7)	(8)	(9)	(10)
Treat × *Post*	-0.009***	-0.048***	-0.028***	-0.022***	0.011*
	(0.004)	(0.005)	(0.005)	(0.005)	(0.006)
Mean Outcome	1.6	6.5	12.5	10.9	6.1
R-squared	0.98	0.97	0.98	0.95	0.93
Observations	588,820	588,820	588,820	588,820	588,820

Note: The tables display the results of the working hour reduction the 1996 reform analogous to Table B.7, but we use all sample of firms (without excluding those where the mode of standard hours decreased by more than 1 hour) and control for the firm-specific linear trends. Standard errors in parentheses; * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

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Table B.10: Robustness: Exclusion of Firms Created Just Before the Reform

(a) Hours and Labor Cost

	Hours			Labor Cost	
	Standard (1)	Actual (2)	Overtime (3)	Monthly salary (4)	Wage (5)
Sample:					
All firms	-1.986***	-1.585***	0.011	0.011***	0.049***
(Baseline)	(0.014)	(0.025)	(0.022)	(0.004)	(0.004)
Firms created by 1995	-1.980***	-1.572***	0.005	-0.012***	0.049***
	(0.015)	(0.026)	(0.022)	(0.004)	(0.004)
Firms created by 1994	-1.970***	-1.547***	0.013	0.012***	0.048***
	(0.015)	(0.027)	(0.022)	(0.004)	(0.004)

(b) Labor Input and Sales

	Labor Input		Sales		
	Employment (6)	Total Hour (7)	Total (8)	Per Worker (9)	Per Hour (10)
Sample:					
All firms	-0.013***	-0.056***	-0.032***	-0.021***	0.019***
(Baseline)	(0.004)	(0.006)	(0.005)	(0.006)	(0.007)
Firms created by 1995	-0.012***	-0.052***	-0.018***	-0.008	0.031***
	(0.004)	(0.006)	(0.005)	(0.006)	(0.007)
Firms created by 1994	0.009**	-0.049***	-0.015***	-0.007	0.031***
	(0.004)	(0.006)	(0.005)	(0.006)	(0.007)

Note: The tables test the robustness of the results of the working hour reduction from the 1996 reform, analogous to Table B.7, by excluding firms created just before the reform. The first column presents the baseline results, the second column restricts the sample to firms created by 1995, and the third column also restricts the sample to firms created by 1995. Standard errors in parentheses; * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

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B.6 Supplementary Results for Long Term Estimation: 1996 Reform

In this subsection of the Appendix, we provide alternative estimates of the results of the national reform by running an estimation over the full period 1986 to 2010, and defining treated and control groups on firms existing before the reform process began in 1990.

B.6.1 Descriptive Statistics of the Three Treatment Groups

Table B.11: Comparison of Treatment Groups (1986-1989)

	Control	CA-treated	Reform-treated
Firm characteristics			
Average firm size	25.3	15.8	17.9
Mean wage (in euro)	3.1	2.5	2.3
Lisbon metropolitan area	0.43	0.48	0.24
Median sales (in euro)	392,258	255,958	238,615
Median sales per worker (in euro)	4,742	3,460	3,043
Median sales per hour (in euro)	37.8	25.3	23.1
Growth			
Average sales growth	0.185	0.168	0.157
Average firm size growth	0.059	0.053	0.058
Sector composition			
Manufacturing	0.35	0.54	0.31
Retail	0.31	0.09	0.07
Wholesale	0.27	0.37	0.37
Hotel & Restaurant	0.06	0.00	0.25
Number of firms	14,176	12,940	27,142

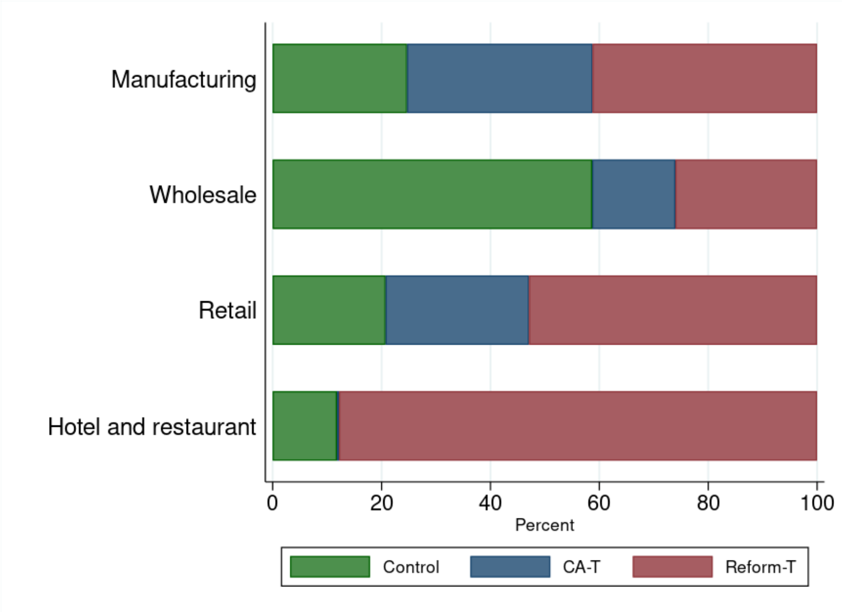
Note: The tables compares the characteristics of firms belonging to each of the three groups defined in our analyses, for firms existing in 1986: Reform-treated, CA-treated, and Control. All values are calculated based on the years between 1986-1989. The table shows that firms in the control group are relatively larger, more productive firms and more likely to be located in the Lisbon metropolitan region.

B.6.2 Sectoral Distribution of the Three Groups

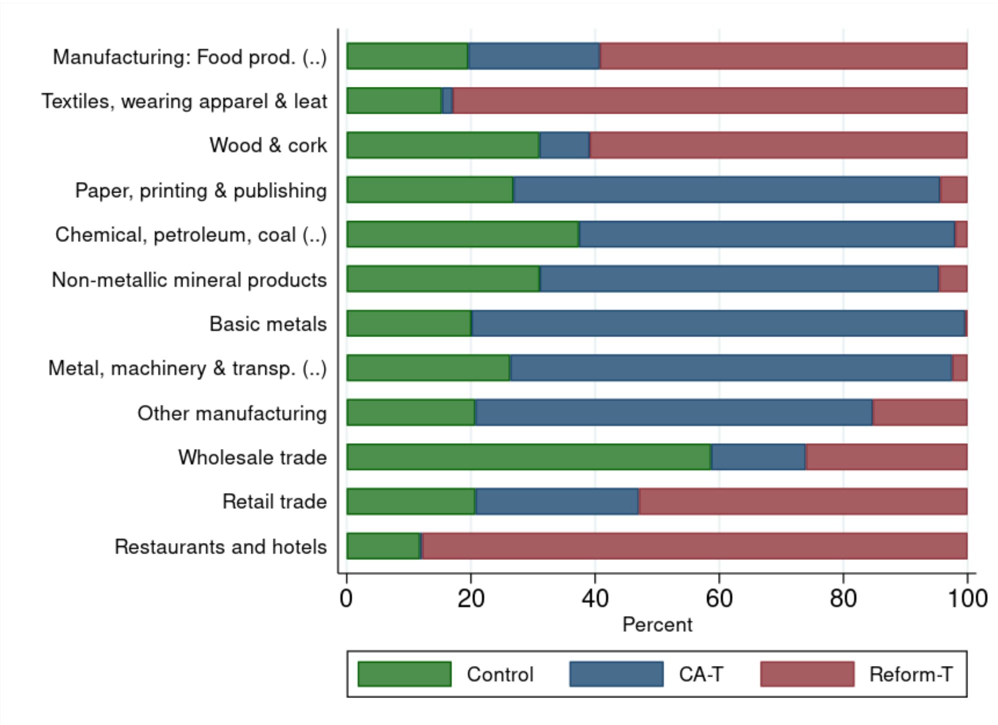
Figure B.16 provides the distribution of the treated groups (as defined in Section 2.6) across sectors. Panel (A) shows the distribution across large sectors, while Panel (B) shows the sectors at the two digit level that disaggregates manufacturing sector further. Figure B.17 similarly provides the distribution across districts.

Figure B.16: Sector Distribution of Treatment Groups

(A) Aggregated Sector

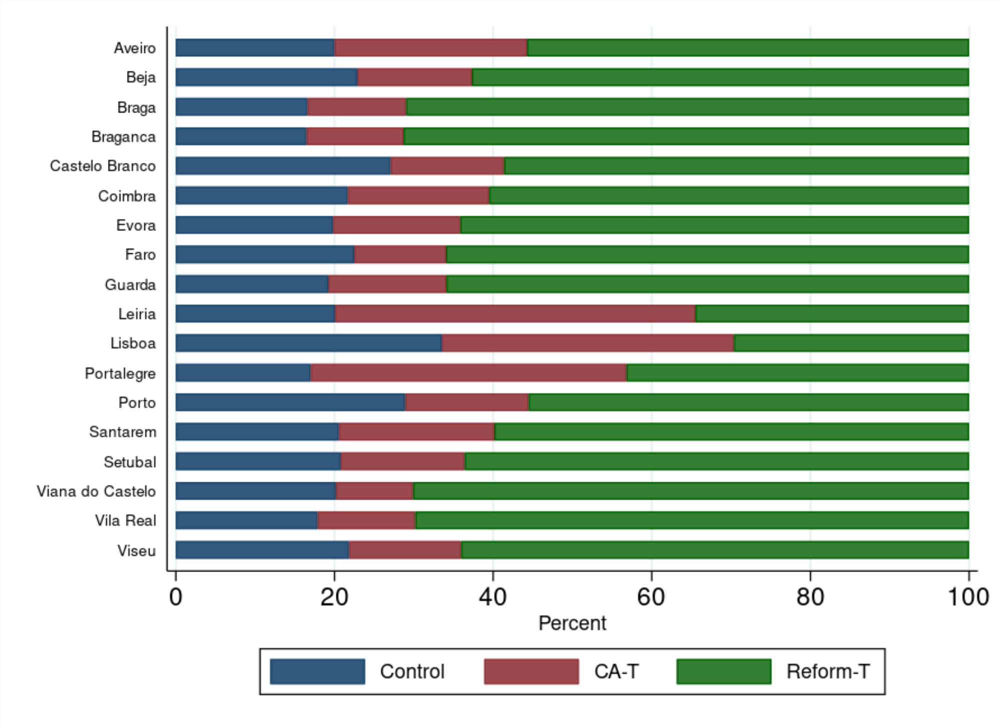


(B) Two-digit Sector



Note: The figures show the distribution of the treatment groups across sectors. Panel (A) shows the aggregate sectors and panel (B) shows the 2-digit sectors.

Figure B.17: Geographical Distribution of Treatment Groups



Note: The figures show the distribution of the treatment groups across districts.

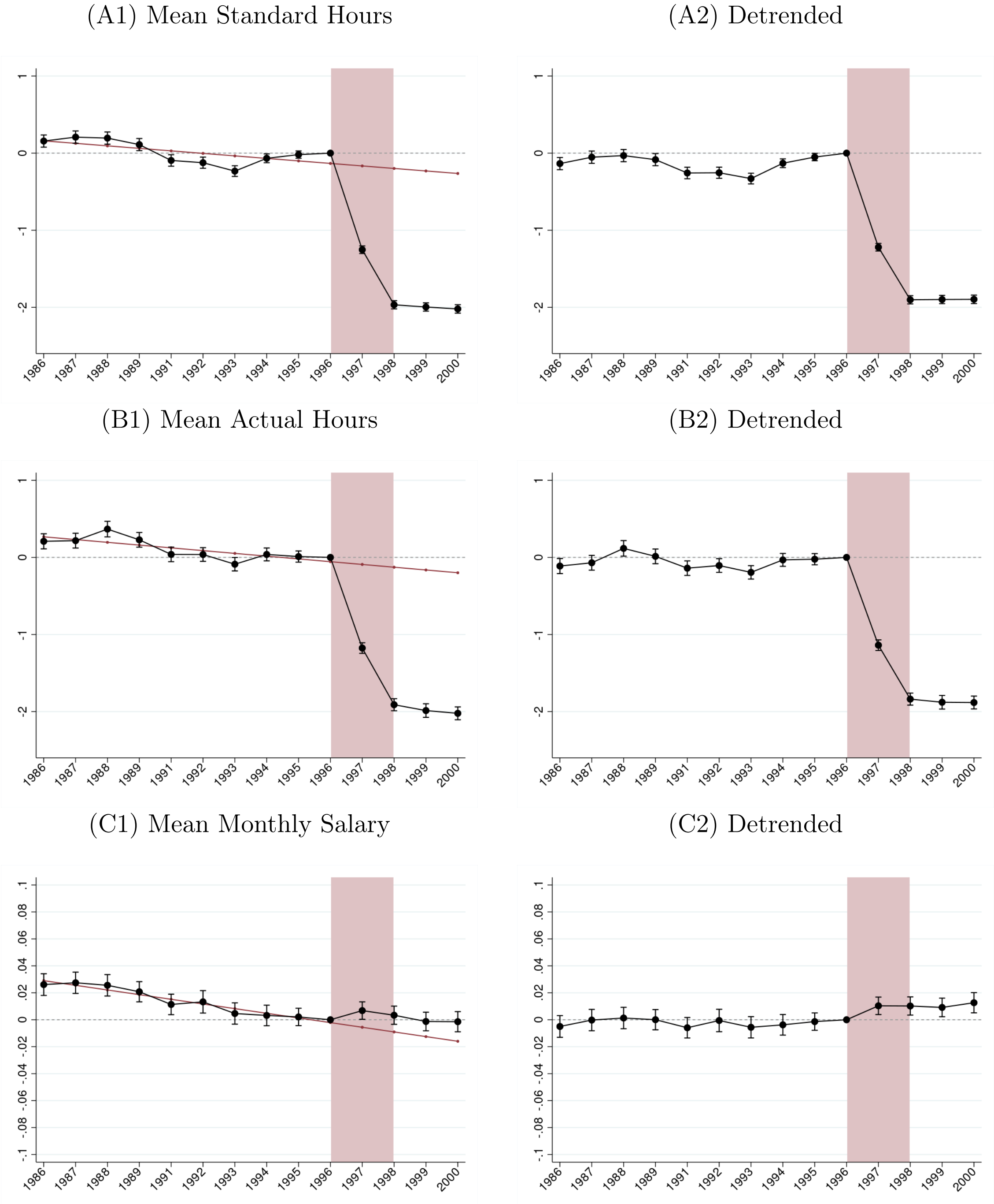
B.6.3 Effects of 1996 national reform (long-term estimation)

Figure B.18 illustrates the effects of the 1996 national reform on hours, wages, and monthly salaries. Given the long pre-treatment period in this analysis, differential pre-trends emerge between the Ref-T group and the control group for certain outcomes. In such cases, the reform's effect becomes apparent as a deviation from the pre-existing trend. To provide a clearer visual representation of the effects, we also present dynamic effect results adjusted for the pre-existing trend, in an identical fashion to Figure VII of Dustmann et al. (2022): We fit a linear trend based on the estimated dynamic coefficients from 1986 to 1996, and use the estimated trend coefficient to predict outcomes values across the entire time period. These predicted values are then subtracted from the initial dynamic coefficients, with the re-centering at the year 1996. We retain the standard errors from the original regression, since the purpose here is solely to visualize the deviations from the trend in the post-intervention period. To provide a formal estimate, we estimate the treatment effects based on the regression that controls for the firm-specific trend, which are presented in Table B.12.

Hours, wages and salaries. Panel (A1) in the figure shows the effect on average weekly standard hours. The coefficients leading up to 1996 are close to zero, indicating a similar evolution in standard hours between the reform-treated firms and control firms prior the reform. The effect of the 1996 reform is evident: between 1996 and 1998, Ref-T firms experienced an average reduction of 2 hours in mean standard hours compared to the control group. In relative terms, this amounts to roughly a 5% decline in mean standard hours. Similarly, Panel (B1) exhibits a comparable result for mean weekly actual hours worked.

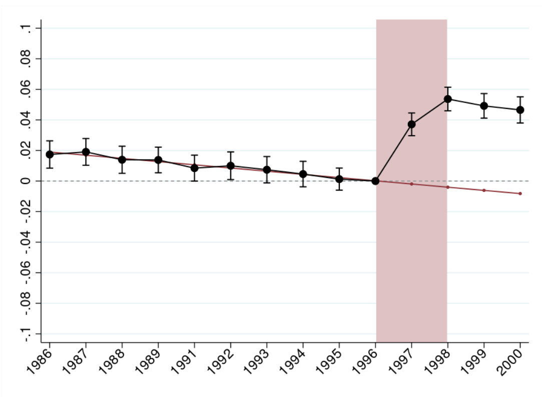
In Panel (C), we observe that there is no adjustment in the mean monthly salary after 1996. Consequently, the reduction in working hours resulted in an increase in the cost of labor, a trend corroborated by Panel (D1) which outlines the effects on mean hourly wage. In comparison to the pre-trend, there is nearly a 6% rise in the mean wage rate after the completion of the reform. Columns (1) to (5) of Table B.12 show the same results in regression form, with the difference that a firm-specific trend is fitted for each firm.

Figure B.18: Effects of 1996 Reform on Hours, Wages and Salaries

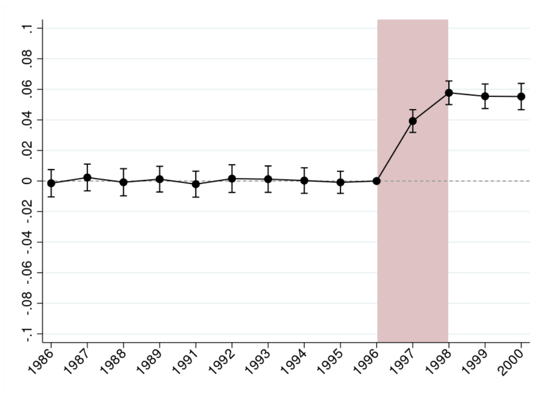


Employment, sales and productivity. Figure B.19 shows the effects of the reform on employment, sales, and productivity (sales per worker and per hour). In panel (A1) of the figure, the evolution of employment in pre-treatment periods was significantly different between the reform-treated firms and control firms. Specifically, Ref-T firms displayed consistently higher growth in employment. This could help explain why these firms were not treated through collective agreements: in the face of growing labor (and potentially

(D1) Mean Wage



(D2) Detrended



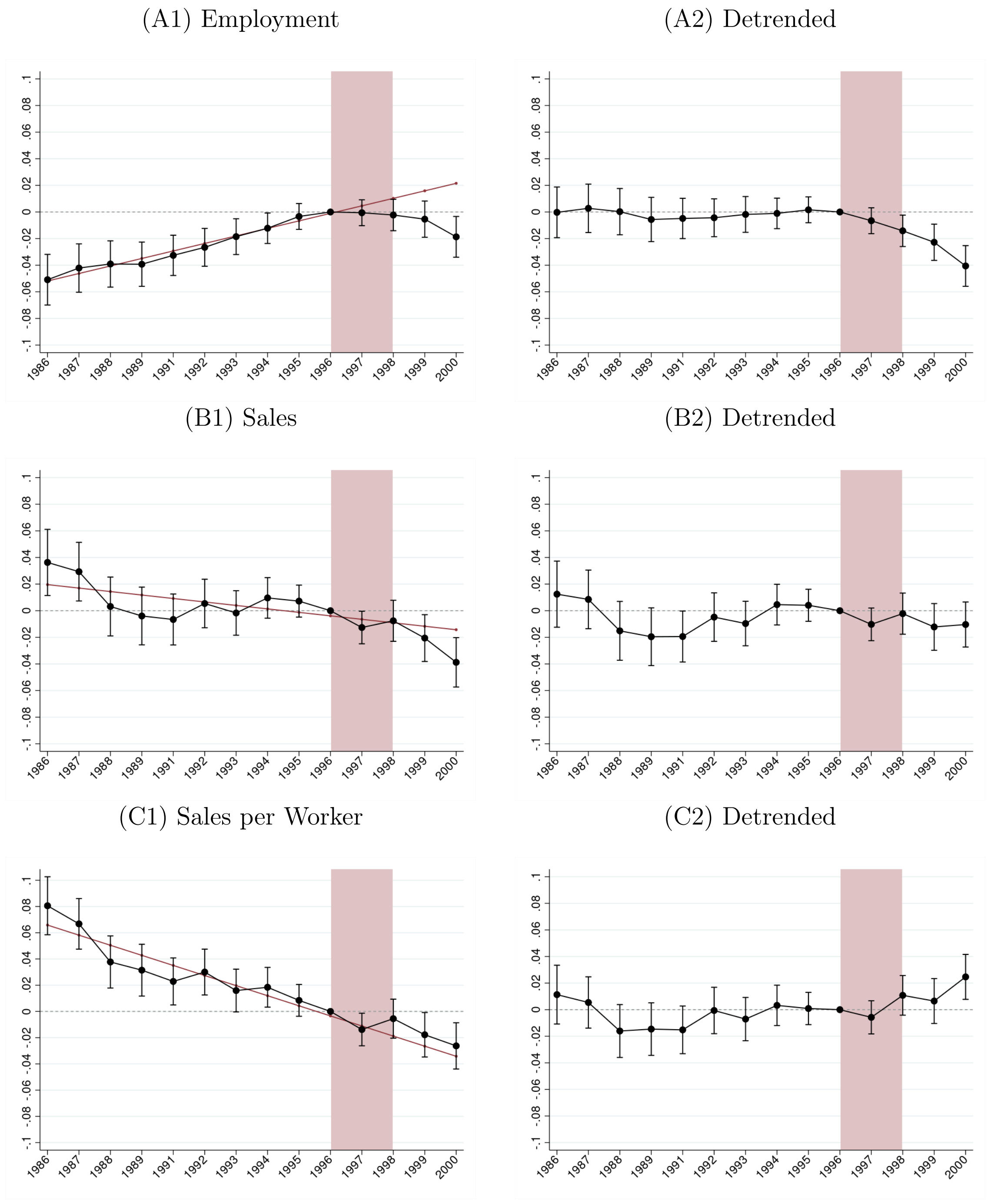
Note: The left-panel figures show the dynamic effects of the reduction in working hours through the national reform in 1996, using the difference-in-differences estimation. On the right, we show the detrended version of the dynamic effects. detrending was using the coefficients estimated, we fit the linear trend between 1986-1996 and take the difference of each coefficient from the predicted values, and re-centered at 1996 again while keeping the stand errors unchanged. Our control group consists of firms situated in the lowest fourth of the distribution of mean standard hours spanning from 1986 to 1996. The red-shaded areas corresponds to the treatment period of the 1996 reform. The outcomes are in absolute terms for hours and in log otherwise. Standard errors are clustered at the firm level.

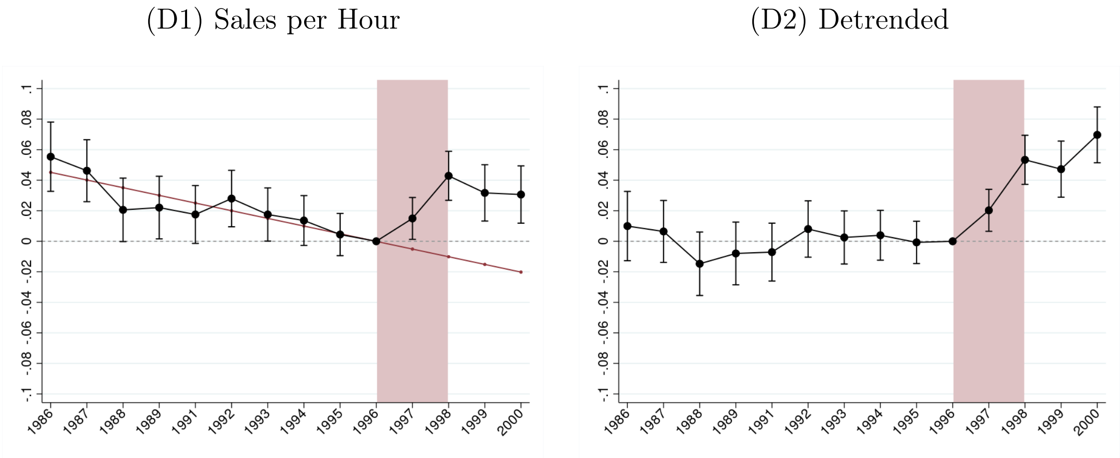
product) demand, reducing labor input through working hour reductions might have been more difficult to be agreed upon by collective bargaining institutions. Importantly, the figure also displays a marked break in the positive trend starting from the year 1997, after the implementation of the reform. The previously stable trend of positive employment growth relative to the control group vanished from this point onward and even exhibited a slight reversal. This impact becomes evident in Panel (A2), where the negative post-reform coefficients signify that the trajectory of employment evolution deviated significantly from what it would have been had the trend prior to the 1996 reform continued. This graphical result is confirmed in regression form, where we account for firm-specific trends, column (6) of Table B.12. After the reform, firm's total labor input decreased by 5% for the Ref-T group compared to the control group.

Panel (B1) provides the effects of the reform on sales. The effect is ambiguous: the coefficients in the post-reform years (panel B1) are constantly negative and significant, hinting to the output down-scaling for the firms treated by the reform. However, controlling for the pre-reform trend (B2), shows a much more ambiguous picture: the post-reform coefficients are small and close to zero. As shown in Table B.12, in the regression with linear firm-specific trends, the joint coefficient in the post-treatment period on sales is indeed estimated as roughly zero (-0.003). However, the null effect under the specification with firm-specific linear trend is strongly influenced by the two large coefficients in 1986 and 1987, that fit a strong negative trend in the estimation. If we start the analysis from 1988, the effect on sale is actually negative and statistically significant. The ambiguous effect emerging from this specification might be due to an imperfect detrending (e.g., the trend is non-linear), or the fact that our sample in this estimation comprises firms alive before 1991 (and hence, possibly, more resilient).

In terms of productivity, as seen in Panel (C1) and Table B.12, per-worker sales were not significantly altered by the reform. The coefficients are positive in the graphical analysis presented in panel (C2). When we analyze the results with firm-specific trends, the effect on per-worker productivity is around 1% after the reform, but not statistically significant. This indicates a relatively similar magnitude of the reduction in sales and employment, leaving per-worker productivity unaffected. On the contrary, Panels (D1) and (D2) clearly illustrate a noticeable increase in sales per hour since the reform. Towards the end of the data period, this increase amounts to approximately 6%, relative to the pre-existing trend (or about +4% when estimated with firm-specific trends).

Figure B.19: Effects of 1996 Reform on Employment, Sales, and Productivity





Note: The left-panel figures show the dynamic effects of the reduction in working hours through the national reform in 1996 using the difference-in-differences estimation. On the right, we show the detrended version of the same dynamic effects. Detrending was obtained by fitting a linear trend on the coefficients estimated between 1986-1996 and taking the difference of each coefficient from the predicted values, which are then re-centered at 1996 again while keeping the stand errors unchanged, as in Dustmann et al. (2022). The control group consists of firms situated in the lowest fourth of the distribution of mean standard hours spanning from 1986 to 1996. The red-shaded areas corresponds to the treatment period of the 1996 reform. The outcomes are in absolute terms for hours and in log otherwise. Standard errors are clustered at the firm level.

Table B.12: The Effects of the 1996 Reform, Long-run Approach, with Firm-specific Trend

(a) Hours and Labor Cost

	Hours			Labor Cost	
	Standard (1)	Actual (2)	Overtime (3)	Wage (4)	Monthly salary (5)
$Treat \times Post$	-1.530***	-1.559***	-0.006	0.050***	0.013***
	(0.031)	(0.039)	(0.031)	(0.003)	(0.003)
Mean Outcome	42.2	41.6	0.3	1.0	6.2
R-squared	0.73	0.70	0.69	0.86	0.86
Observations	396,136	396,136	396,136	396,136	396,136

(b) Labor Input and Sales

	Labor Input		Sales		
	Employment (6)	Total Hour (7)	Total (8)	Per Worker (9)	Per Hour (10)
$Treat \times Post$	-0.014**	-0.053***	-0.003	0.011	0.042***
	(0.006)	(0.007)	(0.008)	(0.008)	(0.009)
Mean Outcome	2.0	6.9	12.8	10.8	5.9
R-squared	0.97	0.96	0.96	0.90	0.89
Observations	396,136	396,136	396,136	396,136	396,136

Note: The tables display the results of the working hour reduction the 1996 reform. All outcomes are in log, except for the hour measures in the columns (1)-(3). The variable $Treat$ takes 1 for firms in the Reform-treatment group and 0 for the firms in the Control group. $Post$ takes 1 the years after 1997 and 0 otherwise. The outcome variables are regressed on the interaction of $Treat$ and $Post$ to provide the difference-in-differences estimate of the effects of the working hour reductions. Standard errors are clustered at the firm level. Standard errors in parentheses; * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

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B.7 Supplementary Figures and Tables for Collective Agreement Treatment

B.7.1 Supplementary Figures

Table B.13 displays the number of collective agreements that reduced standard hours following the 1990 reform by the initial year of treatment. The first year of treatment is determined as the year in which the mode of standard hours in the collective agreement first decreased or as the first year in which the share of workers at the mode in the collective agreement declined by 10% for the first time. The majority of collective agreements and firms began reducing hours in either 1991, 1992, or 1993.

Table B.13: First Year of Collective Agreement Treatment

First Year of Treatment	Number of:	
	Collective Agreements	Firms
1991	6	4,270
1992	16	1,272
1993	16	6,201
1994	4	671
1995	3	156
1996	3	1,079

Note: The table provides the number of different collective agreements and firms in the CA-treated group according to the first year of the treatment. The first year of treatment is identified as the year in which the mode in the collective agreement declined for the first time, or as the first year in which the share of workers at the mode in the collective agreement declined by 0.1 for the first time.

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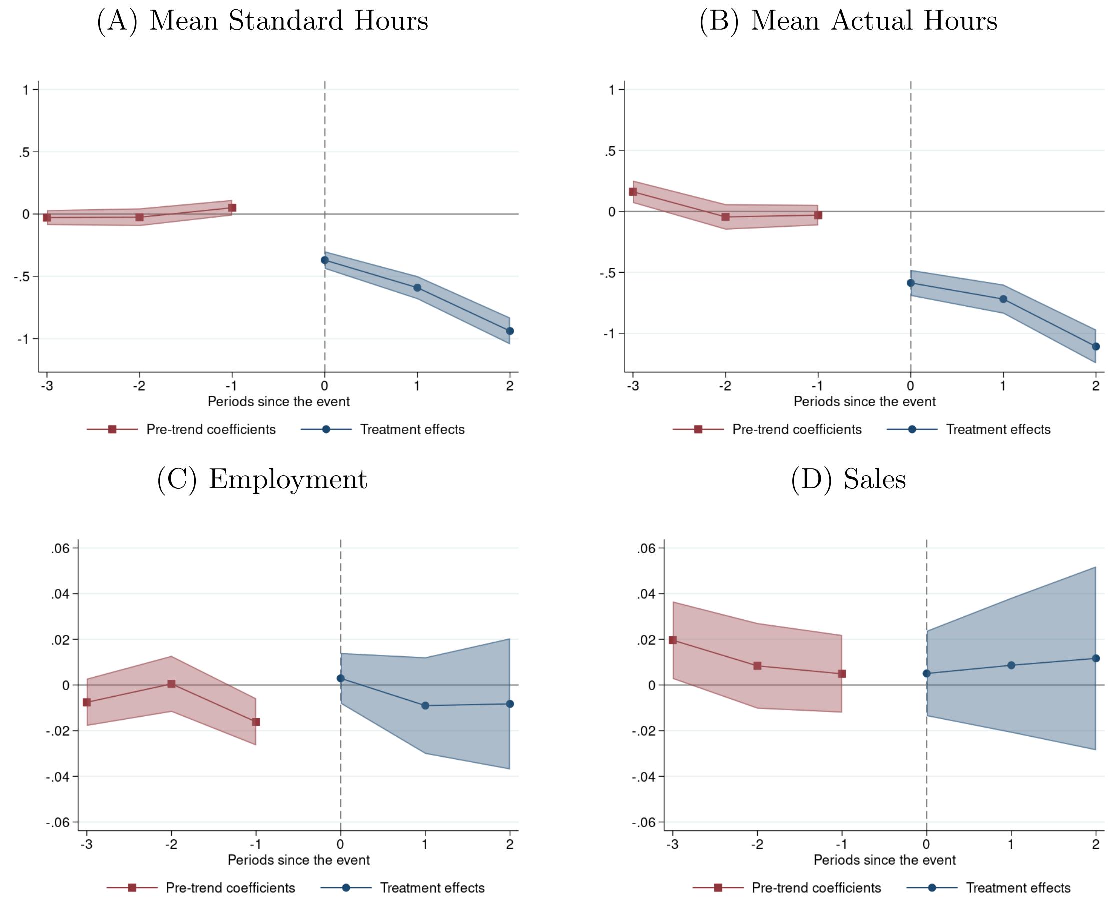
B.7.2 Within-collective agreement comparisons.

Alternative to the results presented in Section 2.6, we run the estimation by focusing exclusively on firms treated by collective agreements that reduced hours over the period 1991-1996. We exploit only the variation in the *timing* of treatment. Relative to the previous approach, the advantage here is to compare only firms within the sample of collective agreements that autonomously decide to lower their hours. This specification exclusively compares firms treated by the collective agreements earlier to those treated later.

Figure B.20 shows the results. Panel (A) and (B) show that both mean of standard hours and actual hours were reduced by 1 hour for the firms treated by collective agreement, relative to the firms treated by collective agreement later. Although the gradual reduction is consistent with the prediction, the total reduction is rather small. This is in part due to the firms that had already lower hours than the collective agreement mode, but also because of the difficulty to identify the precise timing of the first treatment year. Panel (C) shows that there is no significant change in the level of coefficients before and after the reduction in working has started, consistent with the finding from the main difference-in-differences results in our main results. Similarly, we detect no discernible effects on sales, as shown in panel (D). These results show that our main results are not driven by unobserved characteristics that endogenously divide CA-treated firms and the control firms.

Figure B.20: Effects of Collective Agreement Changes on Employment, Sales, and Productivity



Note: The figure shows the effects of the reduction in working hours adapted by collective agreements. The estimation is based on the staggered difference-in-differences Callaway and Sant'Anna (2021), where the control group constitutes the firms that were treated through collective agreement later. The figure shows the treatment effects over 3 years, as well as 3 years prior to the treatment.

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Appendix C

Appendix to Chapter 3 : Gender Differences in the Effects of Reducing Working Hours

C.1 Appendix for LFS results

Table C.1 shows the regression used to predict the treatment status (working longer than 40 hours) using the data period before the reform. i.e.. 1991Q1-1997Q3. The first column shows that personal characteristics do not predict the treatment status much. with the R^2 around 0.034. Adding region fixed effects increases R^2 nearly by 5 to 6 times. and the R^2 further increases to 0.336 once the sector-occupation fixed effects are included. This is consistent with the fact that the standard hours are decided according to the collective agreements that have strong industry and geography dimensions. often specifying differential rules also for occupations.

Table C.2 and C.3 provide the heterogeneity in the effects of the 1996 reform on hour preferences along education levels, marital status, and age groups.

Table C.1: Regression predicting to work longer than >40h

	Prediction for working >40h		
	(1)	(2)	(3)
Female	-0.030***	-0.038***	-0.007
	(0.005)	(0.005)	(0.005)
Age	-0.022***	-0.022***	-0.009***
	(0.001)	(0.001)	(0.001)
Age squared	0.000***	0.000***	0.000***
	(0.000)	(0.000)	(0.000)
Married	0.099***	0.084***	0.036***
	(0.005)	(0.005)	(0.005)
Female × Married	-0.087***	-0.068***	-0.025***
	(0.006)	(0.006)	(0.006)
Household size	0.009***	0.001	-0.003**
	(0.002)	(0.002)	(0.002)
Education (Base=Lower secondary)			
Upper secondary	-0.361***	-0.339***	-0.099***
	(0.004)	(0.004)	(0.004)
Tertiary	-0.287***	-0.264***	-0.044***
	(0.005)	(0.005)	(0.008)
Region fixed effects		✓	✓
Sector-occupation fixed			✓
R-squared	0.124	0.143	0.333
Observations	124,182	124,182	119,309

Note: The table shows the regression results predicting workers' usual hours longer than 40 hours between 1991Q1 and 1996Q3 in the Labor Force Survey. The first column includes characteristics of individuals, the second column includes region fixed effects, and the last column includes occupation-sector fixed effects. * $p < 0.10$. ** $p < 0.05$. *** $p < 0.01$.

Table C.2: Heterogeneity in the Effects: Prefer Less Hours

	Education		Married		Age group			
	Below HS	HS+	Not married	Married	18-29	30-39	40-49	50+
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Treat × *Post*	0.000	0.000	0.004	-0.001	-0.000	0.007**	-0.001	-0.002
	(0.002)	(0.005)	(0.003)	(0.002)	(0.003)	(0.003)	(0.003)	(0.004)
Treat × *Post* × *Female*	-0.009***	0.001	-0.007**	-0.008***	-0.001	-0.020***	-0.008*	0.003
	(0.002)	(0.007)	(0.003)	(0.002)	(0.003)	(0.004)	(0.004)	(0.005)
R-squared	0.016	0.020	0.033	0.014	0.018	0.023	0.039	0.043
Observations	112,156	36,003	44,317	103,842	44,758	41,902	40,330	21,145

Note: The table displays the heterogeneous treatment effects of the working hour reform on the preference for working fewer hours. The estimation method, specification, and data period are the same as the results in Table 3.3, except that the sample is split by worker characteristics to study the group-specific treatment effects. Columns (1) and (2) explore the heterogeneity along education, columns (3) and (4) estimate the heterogeneity by marital status, and the last four columns study the heterogeneity along age groups. Standard errors are reported in parentheses; * $p < 0.10$. ** $p < 0.05$. *** $p < 0.01$.

Table C.3: Heterogeneity in the Effects: Prefer More Hours

	Education		Married		Age group			
	Below HS	HS+	Not married	Married	18-29	30-39	40-49	50+
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Treatment	0.006**	-0.002	-0.006	0.010***	-0.007	0.021***	0.005	-0.003
	(0.003)	(0.005)	(0.005)	(0.003)	(0.005)	(0.005)	(0.004)	(0.005)
Treatment times Female	0.004	-0.011	-0.001	0.003	0.012**	-0.018***	0.004	-0.002
	(0.003)	(0.008)	(0.005)	(0.003)	(0.005)	(0.006)	(0.005)	(0.007)
R-squared	0.036	0.036	0.040	0.035	0.054	0.047	0.032	0.065
Observations	112156	36003	44317	103842	44758	41902	40330	21145

Note: The table displays the heterogeneous treatment effects of the working hour reform on the preference for working more hours. The estimation method, specification, and data period are the same as the results in Table 3.3, except that the sample is split by worker characteristics to study the group-specific treatment effects. Columns (1) and (2) explore the heterogeneity along education, columns (3) and (4) estimate the heterogeneity by marital status, and the last four columns study the heterogeneity along age groups. Standard errors are reported in parentheses; * $p < 0.10$. ** $p < 0.05$. *** $p < 0.01$.

C.2 Appendix for QP results

C.2.1 Gender-specific coefficients

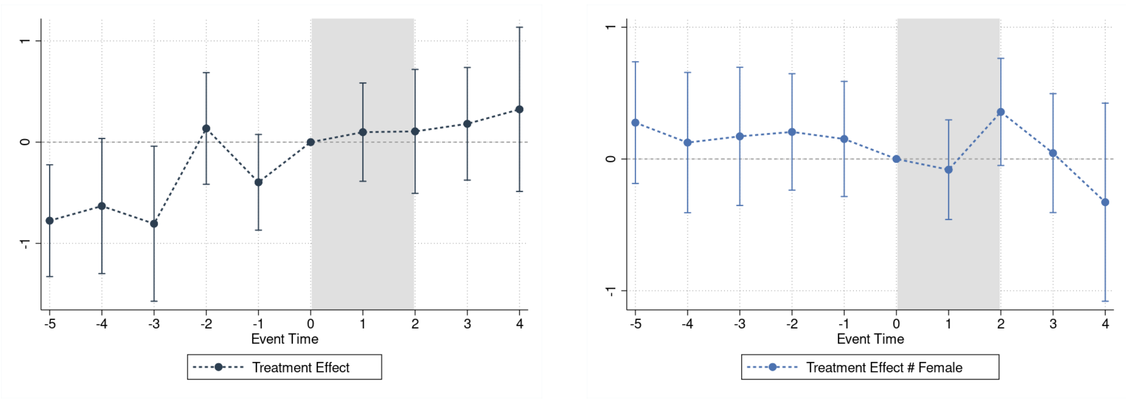
Figure C.1 complements Table 3.7 by providing the gender-specific treatment effects, estimated as below:

$$\begin{aligned} Y_{ijt} = \delta_{st} + \phi_{gt} + \xi_{gj} + \boldsymbol{\beta X}_{it} \\ + Treat_j \cdot \sum_{t=1}^{T} \beta_t^M \mathbb{1}\{year = t\} \cdot Male_i \\ + Treat_j \cdot \sum_{t=1}^{T} \beta_t^F \mathbb{1}\{year = t\} \cdot Female_i + \varepsilon_{jt} \end{aligned} \tag{C.1}$$

where, β_t^M and β_t^F are obtained from the difference-in-differences term interacted with the gender dummies $Female_i$ and $Male_i$, respectively.

Figure C.1: Effects of the 1996 Reform on Overtime Hours

(A) Standard Hours (gender differences)



Note: The figure shows the difference-in-differences effects of the reform on overtime hours, comparing each gender in the treated establishments to the corresponding gender in the control establishments (Equation 3.4). Panels on the left show coefficients for men and women separately, while on the right-hand side, it shows the difference between men and women in terms of the effect.

Table C.4: Dynamic Effects of the 1996 Reform

(A) Hours and Wages

	Hours and Wages:			
	Standard Hours (1)	Actual Hours (2)	Overtime (3)	Log Wage (4)
$Treatment \times 1991$		-0.035	-0.776***	-0.004
		(0.216)	(0.282)	(0.019)
$Treatment \times 1992$		-0.053	-0.631*	-0.001
		(0.172)	(0.340)	(0.019)
$Treatment \times 1993$		-0.286*	-0.806**	-0.003
		(0.154)	(0.391)	(0.017)
$Treatment \times 1994$	0.383***	0.431**	0.135	0.007
	(0.037)	(0.172)	(0.281)	(0.010)
$Treatment \times 1995$	0.170***	0.035	-0.397*	0.003
	(0.027)	(0.125)	(0.241)	(0.007)
$Treatment \times 1996$	0.000	0.000	0.000	0.000
	(.)	(.)	(.)	(.)
$Treatment \times 1997$	-1.220***	-1.184***	0.099	0.027***
	(0.075)	(0.135)	(0.248)	(0.006)
$Treatment \times 1998$	-1.676***	-1.466***	0.106	0.044***
	(0.094)	(0.126)	(0.312)	(0.007)
$Treatment \times 1999$	-1.582***	-1.027***	0.181	0.043***
	(0.098)	(0.163)	(0.284)	(0.009)
$Treatment \times 2000$	-1.640***	-1.117***	0.324	0.045***
	(0.107)	(0.219)	(0.414)	(0.009)
$Treatment \times Female \times 1991$		0.118	0.276	0.022*
		(0.167)	(0.236)	(0.012)
$Treatment \times Female \times 1992$		0.150	0.124	0.016
		(0.147)	(0.272)	(0.013)
$Treatment \times Female \times 1993$		0.066	0.172	0.013
		(0.122)	(0.268)	(0.012)
$Treatment \times Female \times 1994$	-0.040	-0.032	0.205	0.002
	(0.035)	(0.134)	(0.225)	(0.009)
$Treatment \times Female \times 1995$	0.022	0.049	0.152	-0.001
	(0.029)	(0.105)	(0.223)	(0.006)
$Treatment \times Female \times 1996$	0.000	0.000	0.000	0.000
	(.)	(.)	(.)	(.)
$Treatment \times Female \times 1997$	0.083	0.089	-0.081	-0.003
	(0.053)	(0.093)	(0.193)	(0.005)
$Treatment \times Female \times 1998$	0.111*	0.067	0.357*	-0.006
	(0.062)	(0.097)	(0.207)	(0.007)
$Treatment \times Female \times 1999$	0.019	-0.321**	0.044	-0.011
	(0.068)	(0.133)	(0.230)	(0.007)
$Treatment \times Female \times 2000$	0.016	0.035	-0.328	-0.013
	(0.071)	(0.174)	(0.384)	(0.008)
R-squared	0.87	0.42	0.54	0.88
Observations	2,456,074	3,494,825	3,494,825	3,494,825

(B) Worker Mobility

	Worker Mobility from t to $t+1$:		
	Separation	Change establishment	Leave sample
	(1)	(2)	(3)
$Treatment \times 1991$	0.031	0.018	0.014
	(0.019)	(0.012)	(0.014)
$Treatment \times 1992$	-0.017	-0.008	-0.009
	(0.024)	(0.018)	(0.014)
$Treatment \times 1993$	-0.010	-0.011	0.000
	(0.020)	(0.013)	(0.013)
$Treatment \times 1994$	-0.014	0.002	-0.016
	(0.016)	(0.009)	(0.011)
$Treatment \times 1995$	0.000	0.000	0.000
	(.)	(.)	(.)
$Treatment \times 1996$	-0.002	-0.005	-0.003
	(0.016)	(0.019)	(0.008)
$Treatment \times 1997$	-0.021	-0.005	-0.016
	(0.018)	(0.009)	(0.014)
$Treatment \times 1998$	-0.046***	-0.006	-0.040***
	(0.015)	(0.007)	(0.013)
$Treatment \times 1999$	-0.044**	0.007	-0.051***
	(0.017)	(0.009)	(0.014)
$Treatment \times Female \times 1991$	-0.022	-0.002	-0.020*
	(0.014)	(0.009)	(0.011)
$Treatment \times Female \times 1992$	-0.025	-0.016	-0.009
	(0.017)	(0.012)	(0.011)
$Treatment \times Female \times 1993$	-0.021	-0.015	-0.006
	(0.015)	(0.011)	(0.009)
$Treatment \times Female \times 1994$	0.002	-0.005	0.007
	(0.010)	(0.007)	(0.007)
$Treatment \times Female \times 1995$	0.000	0.000	0.000
	(.)	(.)	(.)
$Treatment \times Female \times 1996$	0.005	-0.003	0.008
	(0.009)	(0.005)	(0.007)
$Treatment \times Female \times 1997$	0.007	-0.001	0.007
	(0.011)	(0.007)	(0.008)
$Treatment \times Female \times 1998$	0.013	-0.010	0.022***
	(0.010)	(0.007)	(0.008)
$Treatment \times Female \times 1999$	0.020*	-0.010	0.030***
	(0.011)	(0.007)	(0.008)
R-squared	0.54	0.88	0.42
Observations	3,494,825	3,494,825	3,494,825

Note: The table shows the corresponding regression results for Figure 3.7, which estimates the effects of the reform on the economic outcomes as specified in Equation 3.4, comparing each gender in the treated establishments to the corresponding gender in the control establishments. The coefficients are normalzied in 1996 for panel (A) and in 1995 for panel (B), corresponding to $t = 0$ in Figure 3.7. Standard errors are in parentheses; * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table C.5: The Average Effects of the 1996 Reform, with Occupation-year FE

(A) Hours and Wages

	Hours and Wages:			
	Standard Hours (1)	Actual Hours (2)	Overtime (3)	Log Wage (4)
$Treatment \times Post$	-1.520***	-1.075***	0.372	0.043***
	(0.088)	(0.087)	(0.237)	(0.008)
$Treatment \times Post \times Female$	0.105*	-0.009	-0.051	-0.017***
	(0.056)	(0.073)	(0.161)	(0.006)
R-squared	0.87	0.42	0.54	0.88
Observations	2,456,074	3,494,825	3,494,825	3,494,825

(B) Worker Mobility

	Worker Mobility from t to $t+1$		
	Separation (1)	Change establishment (2)	Leave sample (3)
$Treatment \times Post$	-0.011	0.000	-0.011
	(0.010)	(0.005)	(0.008)
$Treatment \times Post \times Female$	0.001	-0.001	0.001
	(0.008)	(0.004)	(0.007)
R-squared	0.32	0.37	0.30
Observations	3,118,907	3,118,907	3,118,907

Note: The tables summarize the average treatment effects (ATT) of the reform on hours, wages, and mobility. *Treatment* is a dummy variable for the treated group; *Post* is a dummy for post-treatment periods (1997 and after for panel A, and 1996 and after for panel B); *Female* is a dummy variable that takes the value 1 for females and 0 for males. Standard hours are available only from 1994. Separation is defined as taking the value 1 if a worker is separated from the establishment from time t to $t+1$, and 0 otherwise. Separations are disaggregated into a worker leaving the data or a worker moving to another establishment. The regression add 3-digit-occupation-year fixed effects. Standard errors are in parentheses; * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

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