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Firm-Level Effects of Reductions in Working Hours

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Réduction du temps de travail : effets à l'échelle de l'entreprise [1]

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Résumé : Cet article examine comment les réductions législatives des heures de travail impactent l'emploi, la production et la productivité des entreprises. Nous exploitons une réforme portugaise qui a réduit les heures standard de 44 à 40 heures en 1996. Nos 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 mécanique du coût horaire du travail induite par la restriction imposée aux entreprises de réduire les salaires mensuels en même temps que les heures. Les entreprises traitées ont ajusté leur emploi en réduisant les embauches. De plus, les entreprises traitées ont considérablement amélioré la productivité horaire du travail, et il existe des preuves suggérant une utilisation intensifiée du capital. Les entreprises qui avaient réduit les heures de travail par des accords collectifs avant la réforme ont pu augmenter leur productivité sans effets négatifs sur l'emploi et la production. Ensemble, ces résultats montrent que les réductions des heures de travail peuvent diminuer l'emploi pour les entreprises concernées tout en induisant une utilisation plus efficace du travail.

Mots-clés : Temps de travail, Réduction du temps de travail, productivité, Demande de travail, Salaires

Firm-Level Effects of Reductions in Working Hours

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 a mechanical increase in hourly labor cost induced by the restriction imposed on firms to reduce monthly salaries along with hours. Treated

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firms adjusted their employment by reducing hiring. Furthermore, treated firms significantly improved hourly labor productivity, and there is some evidence suggesting an intensified use of capital. Firms that reduced working hours through collective agreements prior to the reform were able to increase productivity without adverse effects on employment and output. Together, these results show that working hour reductions can decrease employment for affected firms while simultaneously inducing a more efficient use of labor.

Keywords : Working hours, Wages, Labor demand, Productivity

JEL Codes: 22, J23, J31.

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. This type of 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 frequently debated in the public arena. There has been a long-standing debate on the effectiveness of reducing working hours as a policy to stimulate job creation, aiming to lower unemployment, commonly referred to as "work-sharing". More recently, there is a growing global interest in initiatives with the four-day workweek and its impact on productivity.

Yet, there is still limited empirical 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. 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 new, 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, many 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 by means of national legislation.

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. Some firms were not affected because their workers already had a 40-hour workweek, due to collective agreements prior to the 1990s, differences in occupational composition, or other firm-specific factors related to the production process.

Using a comprehensive, matched employer-employee dataset, we first document that firms impacted by the 1996 national reform significantly reduced hours worked. The reduction in average working hours is around 2.5 hours, 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, set at 50% for the first hour and 75% for subsequent hours. 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 and mechanical increase in the hourly wage rate, driven by the legal constraint forbidding to reduce monthly salaries for workers whose standard hours were decreased. This increase in labor costs resulted in negative scale effects on firm output, 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, slightly larger, but within the range of estimates in the literature (Hamermesh, 1996; Lichter et al., 2015).[1] 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%, rationalizing a large portion of the increase in hourly wages.

To understand the mechanisms behind the hourly productivity gains, we exploit 2-digit sector-level capital information from the EU KLEMS database and investigate the role of capital. Our analysis reveals a larger negative employment effect and higher productivity gains for firms operating in sectors with high capital intensity. This is consistent with the idea that firms in sectors where it was possible substituted labor with capital in response to the increase in labor cost induced by the reform. At the sector level, we also observe an insignificant but positive relationship between the reduction in hours and the growth of capital services. Yet, we also observe an increase in hourly labor productivity in labor-intensive sectors. This suggests the existence of alternative labor-specific mechanisms, such as work intensification or concavity in the production function with respect to hours. On the other hand, we did not find supporting evidence for price adjustments (i.e., the increase

[1] Note that one reason for the estimated larger elasticity is the implicit increase in the labor cost per hour due to the fixed cost, which is not reflected in the effects on the observed wages.

in labor cost was not passed on to final prices) or an increase in average worker quality, as measured by workers' educational qualifications.

In the final section of the paper, we evaluate the impact of reducing working hours for firms that reduced standard hours through collective agreements prior to the 1996 reform. Similarly to the consequences of 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 appear less 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. This is likely related to the heterogeneous effects of working hour reductions induced by the endogenous bargaining process of collective agreements. Firms 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.

Our article 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 in firms affected by the reforms (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.

While these studies provide valuable insights on the consequences of working hour reduction on employment, our analysis at the firm level offers new insights by directly studying how firms adjusted their overall labor demand. Firm-level employment encompasses both separation and hiring, while previous worker-level studies primarily focused on the former, without addressing the total employment size of the firm. Indeed, we find that an important margin of employment adjustment for firms was less hiring of new workers, rather than firing existing workers. In comparison to sectoral or regional studies, firm-level analysis can better control for potential confounding factors by comparing affected and unaffected firms within

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

the same sector and region. Additionally, it provides a direct examination of the theoretical predictions regarding the firm's adjustment mechanisms in response to the reform.

Although some research has explored 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, their evaluation is complicated by simultaneous cuts in social security contributions aimed at easing the transition of firms to the lower hours standard. In contrast, one of the main advantages 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 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. In addition, we provide separate estimates of the impacts of reducing working hours through collective agreements. Secondly, we extend the analysis to other key outcomes, 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. To the best of our knowledge, this is the first study to demonstrate how the forced reduction in standard hours affects output at the firm level and the productivity of workers and hours. Theories have suggested potential consequences on output and productivity resulting from a reduction in working hours (Crépon and Kramarz, 2002), but these predictions have not been empirically tested. 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.[4] Our paper provides evidence that

[3]For a subset of firms with a significant drop in hours, they find a negative but insignificant employment effect. Nonetheless, the result is not easy to generalize due to the regression discontinuity design that can estimate only local effects around the given firm size threshold.

[4]Related to this literature, some studies examine the impact of part-time employment on firms' productivity,

reductions in working hours significantly increase hourly labor productivity at the firm level, indicating that the productivity gains from reducing working hours can 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.[5] In the context of working hour reductions, labor costs increase because overtime pay with a wage premium starts at earlier hours and, as observed in the Portuguese case, monthly salaries often do not adjust along with hours. Whether this 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 provides an overview of the working-time legislation in Portugal and describes its chronological evolution. Section 3 discusses the varying theoretical predictions regarding a reduction in working hours. Section 4 introduces the data that are used in the analysis and defines our sample. In Section 5, we describe our empirical strategy based on a difference-in-differences approach. Section 6 presents the results. Section 7 provides additional analyses to validate and enhance our main empirical findings. Section 8 concludes.

2 Working Time Legislation in Portugal

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

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).

[5]Lichter et al. (2015) provides a meta-analysis on the own-wage elasticities of labor demand.

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 refers 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. For example, maximum weekly hours are capped at 48 hours in the European Union. Overtime hours are paid at a higher rate, referred to as the *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 like in many 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 seldom-used option for firms, as we will elaborate on in the subsequent sections.

2.2 Working Hours Legislation in Portugal over Time

Table 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 first hour of overtime, and the second to the second hour. All reforms are in bold, see text for some remaining aspects of working time legislation not covered in the Table. The law specifies different working hours for office workers (7 hour day, and 42 hour week).

Working time in Portugal is regulated by both national legislation and collective agreements. Collective agreements vary by sector and location and cover approximately 80% of workers. Generally, national legislation sets the upper bound, while collective agreements typically specify either lower levels and/or exceptions. 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 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, respectively.[6] As shown in Figure A9 in the Appendix, according to the Labor Force Survey, 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.[7] However, around 80% of full-time workers were not working weekends as of 1992, suggesting that they were either 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 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 first 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.[8]

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, There was a memorandum of understanding that the working week would be further lowered to 40 hours by 1995 through collective agreements. While formally the national legislation prior to 1991 specified a usual working week of 48 hours, the *de facto* usual working week was 45 hours because employers could extend daily hours to 9 (instead of 8), by providing an additional rest day. The typical arrangement was

[6] As employers had to provide an additional day of rest, the standard weekly hours decreased to 45 for most workers (from 48) and to 40 for office workers (from 42).

[7] 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 A5), it is evident that prior to 1991, most workers were indeed on a 45-hour week, with only a few working 48 hours.

[8] "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).

a 5-day week with a 9-hour day, totaling 45 weekly hours. Starting from 1991, this was reduced to 44 hours. 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, most workers were still working more than 40 hours per 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. The reform did not include specific provisions for the adjustment of salaries and wages as hours decreased. However, the Portuguese law specifies that the reduction in the maximum regular hours must not result in the damage to the economic situation of workers.[9] Consistent with this, we show that monthly salaries did not adjust downward, resulting in significant wage increases. Lastly, it is crucial to emphasize that, contrary to the famous French reform around 2000 that reduced standard hours to 35 hours, no compensating measures for firms were put in place (such as cuts in social security contributions). This allows for, we argue, a much cleaner interpretation of the effects of reductions in standard hours at the firm level.[10]

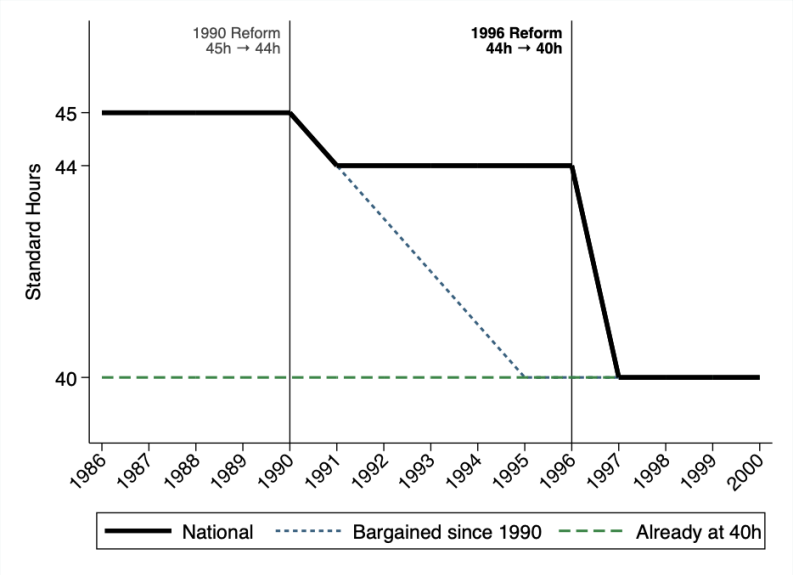
Figure 1 presents three stylized evolutions of standard hours since 1986, reflecting the legislative context described earlier. The solid black line displays the national standard hours, which were initially reduced from 45 to 44 hours in 1991 and subsequently to 40 hours between 1996 and 1998.[11] Standard hours can be set at levels lower than the national standard. The

[9]DL. 409/71 of 27.9, Article 8.1 "A redução dos limites máximos dos períodos normais de trabalho pode ser estabelecida por decreto regulamentar ou instrumento de regulamentação colectiva de trabalho."; 8.2 "Da redução dos limites máximos dos períodos normais de trabalho não pode resultar prejuízo para a situação económica dos trabalhadores, nem qualquer alteração das condições de trabalho que lhes seja desfavorável.". These state that: "The reduction of the maximum limits of normal working periods may be established by regulatory decree or collective labor regulation instrument." and "The reduction of the maximum limits of normal working periods must not result in prejudice to the economic situation of workers or any unfavorable alteration of their working conditions.".

[10]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.

[11]Prior to the 1991 reform, national standard hours were 48, but the *de facto* standard hours were 45, with 9 hours per day and 5 working days per week. Hence, we consider 45 hours as the standard hours before the 1990 reform.

Figure 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 first line represents 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 second line marks the nationwide reform in 1996, which decreased the national standard hours from 44 to 40 hours. 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.

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 reform. This led to a gradual reduction of standard hours from 44 to 40 hours between 1991 and 1995. The reduction in standard hours began at different times, mostly starting in 1991, 1992, or 1993. Furthermore, in certain cases, the reduction was not fully implemented (e.g., only reducing to 42 hours).

2.3 Hours Around the 1996 Reform

In examining the impact of working hour reductions, we primarily focus on the 1996 reform, which reduced standard hours from 44 to 40 hours.[12] This is because the majority of workers

[12]Details on difficulties associated with estimating the treatment through collective agreements are discussed in Section 5.

and firms with standard hours exceeding 40 hours in the 1980s reduced standard hours through the national reform – in our estimation sample, they constitute roughly 70% of the treated firms. Furthermore, the treatment and its timing are exogenous, in contrast to the reduction in hours through the bargaining process.

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 2 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), signifying 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. In reality, overtime is rarely used in Portugal due to the high overtime premium. In our sample, only 1% of firms had a history of using overtime before 1996. Consequently, standard hours essentially act as a ceiling on working hours.

To visualize the impact of the 1996 reform on working hours, Figure 3 presents the distribution of employer-declared standard hours and actual hours per week from the administrative data used in this article.[13] In Panel (A), there is a noticeable shift in standard hours towards 40 hours following the implementation of the reform. This shift is closely mirrored in the distribution of actual hours per week, as depicted in Panel (B). Post-1998, actual hours worked also peak at 40 hours.[14] Employers may sometimes underdeclare the hours worked by their employees. Therefore, Figure 4 examines the distribution of usual hours worked as reported by workers in the Labor Force Survey. By comparing Panel (A) and (B), it is evident that self-reported usual hours worked also shifted towards 40 hours after the implementation of the reform. This demonstrates that the reduction in working hours in Portugal had tangible effects on actual working hours.

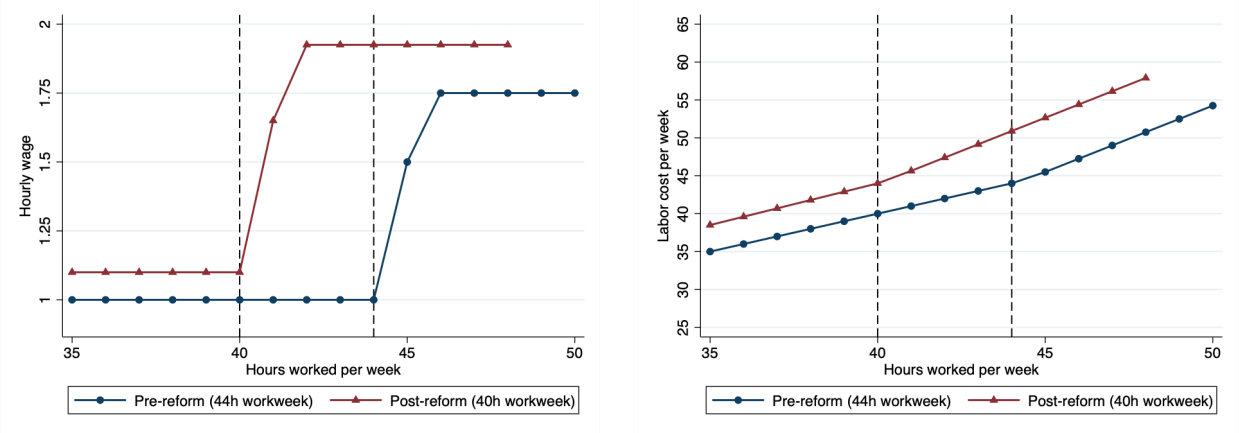
[13] Additional figures on the evolution and distribution of hours since the 1980s can be found in Appendix Section A.4.

[14] Weekly actual hours are calculated 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. Due to variations in the number of working days in the reference month across years, this introduces some measurement error. Consequently, and due to business-related volatility, actual hours are more scattered than standard hours.

Figure 2: Expected Change in Wage and Monthly Salary Schedule

(A) Hourly Wage

(B) Monthly Salary

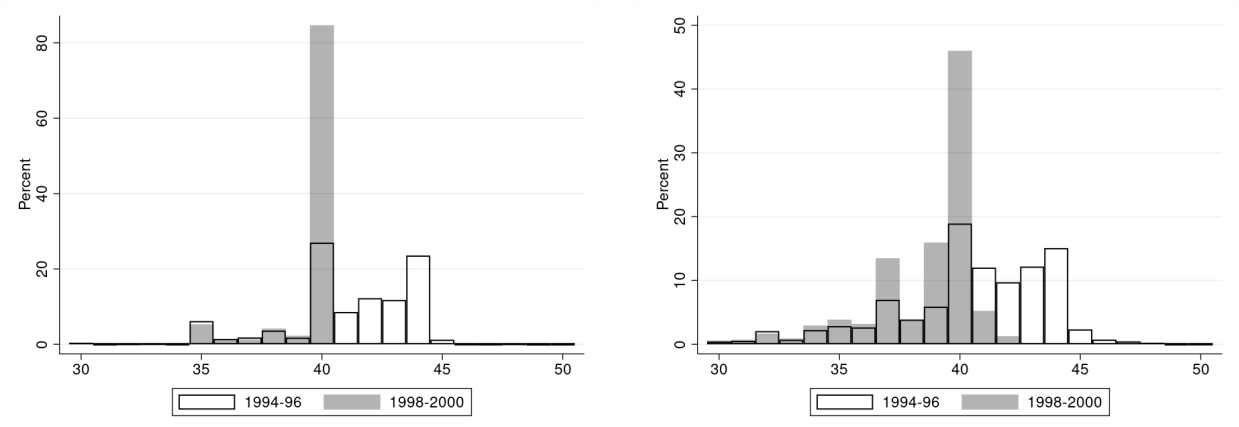


Note: This figure illustrates the expected changes in both the hourly wage schedule and monthly salary schedule for workers whose standard hours were reduced from 44 to 40 hours. In Panel (A), the wage schedule is presented, with the regular hourly wage set at 1, as a function of hours worked. The red 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.

Figure 3: Distribution of Weekly Hours, QP

(A) Standard Hours

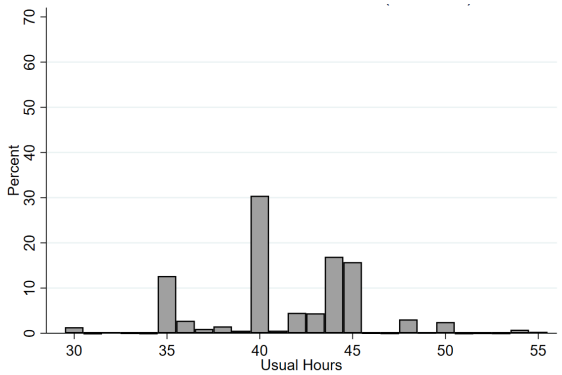
(B) Actual Hours



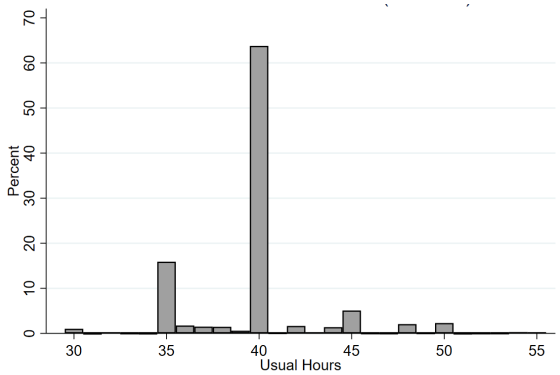
Note: These figures plot the distribution of standard hours for full-time employees, as recorded in the administrative data, QP. Standard hours is available only from 1994 in the data. Actual hours worked is provided as a total of the reference month. To derive the actual hours worked per week, we divide this monthly hours by 21.625 (i.e., average number of working days per month). In the years spanning from 1986 to 1989, standard hours were set at 48h, or 45h over 5 days. In the period 1991-96, the standard hours were set at 44 hours. Starting from 1998, a 40-hour workweek was introduced.
Source: Quadros de Pessoal

Figure 4: Distribution of Self-reported Usual Working Hours, LFS

(A) Prefer less hours)



(B) Post-reform (1998-2004)



Note: These graphs plot the distribution of usual working hours for full-time employees over the two different periods in which the legislation was different. 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 Labour Force Survey

3 Conceptual Framework

The theoretical underpinnings of the effects of reductions in standard hours are documented in the literature. In this section, we provide an overview of the theoretical foundations. Appendix A.2 presents a simple one-input model of profit-maximizing firms aligned with the Portuguese context, highlighting the core mechanisms associated with the impact of working hour reductions.

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, resulting in a higher number of workers working shorter hours. This concept, known as "work-sharing", is straightforward and often favored in public and political debates.

However, the introduction of fixed cost per worker and firms' endogenous overtime use complicates the predictions significantly, as demonstrated by a seminal paper by Calmfors and Hoel (1988). The effect on employment will depend on hours before the reform, and whether firms are cost-minimizers facing a fixed demand, or profit-maximizers who can adjust output. In cases of cost minimization, work-sharing is more likely to occur, but the

[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).

impact on employment becomes ambiguous when overtime is considered. This is because 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 results in workers actually working *more* hours. However, 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 makes work-sharing more probable as firms are more inclined to hiring new workers. In the case of profit-maximizing firms with variable output, reducing working hours results in a negative scale effect on output, making the employment impact even more uncertain and likely to be negative. This happens because the cost per worker, including fixed costs, rises even when hourly wages remain constant.

Others have considered 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. This is the case for the 1996 Portuguese reform we study and in most other standard hour reforms, especially in Europe. An exception is the Canadian case (Skuterud, 2007), where monthly salaries could and did adjust to match the fewer hours. 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 elucidated by Calmfors and Hoel (1988). Importantly, Crépon and Kramarz (2002) also formalizes 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 rises.

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 employment effect of the minimum wage" as discussed by (Manning, 2021).[16] When we deviate from the

[16]"The strong a priori belief held by many that a rise in the minimum wage must cost jobs ultimately derives

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 were built upon 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 uncertain.

In summary, theoretical predictions regarding the effects of reducing standard hours are not straightforward and depend on various assumptions. The 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 requires empirical examination, which we aim to tackle in our 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 equilibrium.

4 Data and Sample

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 A.3: it includes hours, wages, firm and establishment code, sales at the firm level, and several characteristics of the worker and the firm.[17] Importantly, it also collects information on which collective agreement covers a given worker. The data have two gaps, in 1990 and 2001.[18] 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

from the assumption that the low-wage labor market is close to perfectly competitive." (Manning, 2021)

[17] 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 based on sales are based on this variable.

[18] 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 period, called the reference period. The QP measures also actual hours, i.e., the hours actually worked during the entire reference month (i.e., March until 1993, October since 1994), available in all years. Although there is certainly a strong correlation between the two, they are not identical. Moreover, actual hours can (and do) fluctuate much more than standard hours due to business cycles and idiosyncratic factors.[19] 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 A10, 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.

This distinction between actual hours and standard hours will be important in our definition of firms' treatment status later in the paper. In particular, defining a treatment status based on actual hours can be problematic because they may correlate with the economic cycle, which can be a potential source of endogeneity bias in the econometric estimations. For this reason, for years prior to 1994, we imputed standard hours from the actual hours worked in the month, to obtain a more stable measure. Appendix Section A.3.3 outlines the method to impute the standard hours. The data also contains information about overtime hours, meaning that we know the number of hours paid at an overtime premium rate, and whether firms increase overtime after the reduction in standard hours.

For descriptive purposes, we also use the *Labor Force Survey* (LFS) in the harmonized version available through Eurostat. Contrary to the QP, this data provide information on the usual hours worked in a reference week and a measure of *self-reported* actual hours worked. The advantage of the LFS is that workers directly report how many hours they work, thereby incorporating any informal or uncompensated overtime hours that may not be recorded in the administrative hours. This allows us to show that the reform is reflected in "real" hours, not only in those reported by employers in administrative data. All relevant variables across our data sources are outlined in the Appendix A.3.

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. These results in only a drop of 0.15% of the QP total sample of firms. We also exclude firms that reside in the islands, rather than on the

[19] 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.

continent, due to extremely different labor market conditions (3.5% of firms).

More importantly, we limit our analysis to the manufacturing sector and low-skill service sector (i.e., retail, wholesale, and hotel and restaurant). These sectors represent 64% of the QP sample in terms of workers and 63% in terms of firms. The 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 & 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 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 extreme outliers is necessary to account for the strong dynamics in some firms that would otherwise significantly increase the noise in the estimations.

5 Empirical Strategy

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, and, when that transition failed, through a nationwide reform in 1996. In our main estimation, we focus on the 1996 national reform for multiple reasons: first, the majority of firms reduced working hours through the national reform; second, the reform led to a sudden shock in working hours that applied to all firms nationwide. Importantly, the reform was unanticipated until the election that brought the change in government in 1996. Therefore, the implementation and the timing of this treatment provide a suitable case study in terms of causal identification. Studying the effects of the collective agreement is more challenging due to the lack of precise information on the timing and content of the collective agreement.[20] Finally, since 1994, the data provides standard hours (i.e., the usual hours worked in a given week as per the employment contract) of each worker. This crucially allows us to exactly know which firms were and were not

[20] We tried to identify which collective agreement agreed on reducing the hours and its timing from the published labor and employment bulletins, available at: https://bte.gep.msess.gov.pt/. However, we did not succeed in extracting exhaustive (and reliable) information on the agreements related to working hours and in linking each signed agreement to the collective agreement code in the QP. This is further complicated by the lack of coherent collective agreement codes across years and unavailability of contractual hours before 1993 in our data, which make it more difficult to "observe" a collective agreement reform.

affected by the reform. Nonetheless, we provide later in Section 7 that estimate the effects of the working hour reductions through collective agreement.

5.1 Sample

The main analysis focuses on the years between 1994 and 2000. We start from 1994 because standard hours is available in our data only since 1994 and the reference month of the QP has changed to October since this year.[21] Our sample consists of firms that existed between at least once between 1994 and 1996 and in our focused sectors (i.e., manufacturing, wholesale, retail, and hotel and restaurant). We stop in 2000 because of the lack of the worker files in 2001 as noted earlier. 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 the drop of 30% of the firms. However, estimating the effects on the totality of the firms without any exclusion leads virtually to the same results, as shown in the robustness checks.[22] Our final sample consists of 73,612 distinct firms.

5.2 Treatment Definition

Our estimation strategy exploits the *firm-level* variations in the exposure to the treatment of 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 the lower standard hours 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.[23]

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:

$$HourShareTreat_j = \frac{\sum_j \max(Hour_{ij} - 40, 0)}{\sum_j Hour_{ij}} \tag{1}$$

[21] We also provide the results of the long-run approach starting from 1986 in Section 7. The results are largely consistent, with minor differences due to the estimation based on firms that survived from 1986 until 1996.

[22] This is consistent with the fact that the firms treated through collective agreements are a minority in our sample and these firms did not experience a significant changes in most of the outcomes, as estimated later in Section 7. We also provide alternative robustness checks regarding the sample, such as the use of firms that were born before 1994.

[23] Note that the firms treated through the collective agreement *after* 1990 are largely excluded from our analysis because we drop firms with changing mode of the standard hours after 1994. Again, however, the inclusion of them in our sample does not significantly alter our estimation results.

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 0.909 (= 4/44), that is 9.09% of the hours were expected to be reduced in this firm due to the reform. We take average between pre-treatment years between 1994 and 1996 to construct a single measure for each firm. It is worth stressing that, unlike actual hours worked, standard hours are a stable measure and do not fluctuate much on a year-to-year basis, so that it is unlikely correlated with the economic situations of a given point in time. Moreover, taking average over 1994-1996 further diminishes the potential influence of mean reversions.

Figure 5 shows the distribution of this treatment measure across firms in our sample. 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.[24] We define treated firms as those with any exposed worker (i.e., strictly positive value in share of treated hours), that constitute roughly 74% of the firms in our sample. Our control group consists of firms with none of their workers affected by the reform (i.e, those with a 0% share of treated hours), corresponding to the remaining 26% of the firms. The average hour share of treatment among the treated group is 0.074. In robustness checks, we also use alternative ways of constructing control firms, such using bottom third or below-median in the hour share of treatment, and the results are largely consistent.

5.3 Empirical specification

We estimate the effect of the working hour reductions in a difference-in-differences specification. After defining the treatment and control groups as described above, we run the following regression:

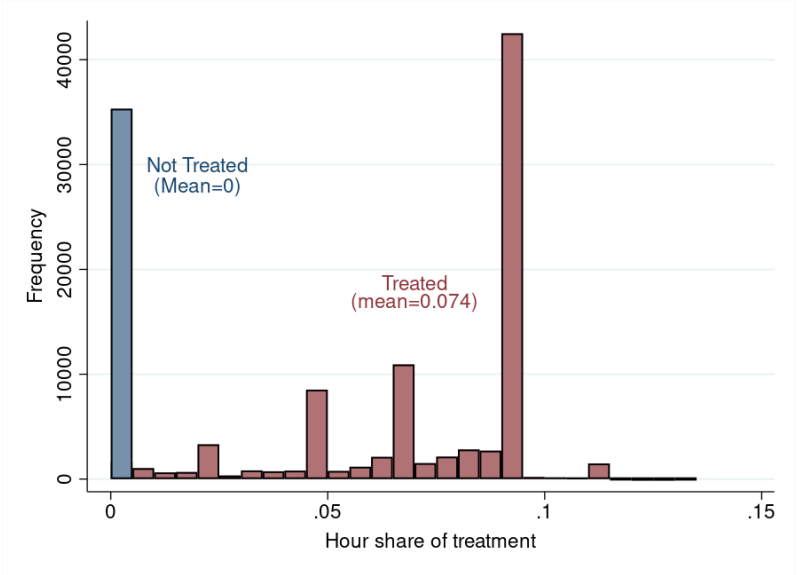
$$Y_{jt} = \gamma_j + \delta_{s(j)t} + T_j \sum_{t=1994}^{2000} \beta_t \mathbb{1}\{year = t\} + \varepsilon_{jt} \tag{2}$$

where Y_{jt} are the outcomes of interest at the firm-level (expressed in log, except for the hour measures); γ_j and $\delta_{s(j)t}$ are firm and year fixed effects respectively. $\zeta_{s(j)t}$ are sector-year fixed effects.[25] T_j is our treatment variable which takes 1 for the treated firms and 0 for the control firms. β_t identifies the dynamic effects of the treatment, with the year 1996 as the reference. β_t provides the average treatment effects on the treated (ATT), given the unbalanced nature of our control and treated firms, as further discussed below. Standard errors are clustered

[24] Note that for some firms, the treated share of hours exceed the theoretical maximum of 0.909 – this is likely due to a handful of firms misreporting some of their workers' standard hours as above 44 hours.

[25] We group sectors into 4 large categories: Manufacturing, Wholesale, Retail, and Hotel & Restaurants.

Figure 5: Firm-level share of treated hours



Note: This figure plots the share of treated hours for each firm as calculated in Equation 1, calculated 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 average over 1994-1996 so that each firm has a single value of this treatment measure that is robust to potential mean reversions.

at the firm level.

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 a deeper 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, which are likely to be impacted directly by collective agreements and to be correlated with the same firm and worker-specific characteristics that determine hours. For example, firms with different hours cannot have, by definition, the same output and, at the same time, the same workers' and hours' 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.[26] In the case of employment, for example, it is important to determine whether we expect the same employment change in percentage or in number of workers. In addition to the case of 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.[27] Importantly, we show that the parallel trend assumption holds for most outcomes we study. Moreover, we provide the estimation results on the average treatment effects in a specification where we control for the firm-specific linear trend to validate our results.

5.4 Descriptive Statistics

Table 2 provides an overview of the characteristics of the firms in two different groups: the control group and the treated group. The control group consists of 15,225 firms, while the treated group comprises 58,387 firms. The table reveals that the firms in the control group are smaller in terms of size, but they are more productive. 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.[28] 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 always specified shorter standard hours. Regarding the sector composition, control group firms have a higher proportion in retail sectors but a smaller share in manufacturing and the hotel and restaurant sectors. It is important to note that our difference-in-differences estimation focuses on the effects of the reform on the treated firms, which, in comparison, are slightly larger in terms of firm size but less productive.

[26] The only case in which this is not true is if there is no trend, i.e. the evolution is flat.

[27] Taking the log also has the advantage of helping us to deal with outliers due to large firms, which are particularly troublesome for wages and sales, without having to significantly trim the sample.

[28] Some of the differences may be attributed to the fact that treated firms are situated in the Lisbon metropolitan area, which generally has higher price levels.

Table 2: Characteristics of Control and Treated Firms (1994-96)

	Control firms	Treated firms
(A) Firm characteristics		
Mean firm size	9.3	14.1
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 presents a comparison of the characteristics of firms in the control group and the treated group. Panel (A) displays average firm size, the proportion of firms located in the Lisbon metropolitan area, mean wages, and the median sales. Panel (B) provides information on the sector composition of firms in both groups. The table highlights that, on average, companies in the control group are smaller in size but exhibit higher productivity.

6 Results

6.1 The Effects of the 1996 Reform

Figure 6 presents the results on the dynamic effects of the reform on firm-level outcomes estimated in the difference-in-differences outlined above. It shows the estimated coefficients and their 95% confidence intervals. We also provide average treatment effects in Table 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 A.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. The coefficients are estimated with such precision that there are no visible confidence intervals. In alignment with the reform's two-year phased implementation, standard hours were initially reduced 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. This demonstrates that actual hours worked closely matched

the standard hours on average. Although firms theoretically had the option to increase overtime hours (capped at 8 hours per week) in response to the reform, Panel (C) reveals that there was no increase in mean overtime hours, indicating that firms did not replace regular hours with overtime hours. This is likely attributed to the high overtime premium in Portugal at that time, set at 50% for the first hour and 75% for subsequent hours. Even before the reform, only 1% of the firms in our sample used overtime hours.

As mentioned earlier 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.[29] 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.[30] The reduction in hours and the increase in wages stand in contrast to the contractual theory outlined in Trejo (1991), which formalizes the possibility that employers and employees simply move to a new contract with lower hourly wages, ensuring that neither working hours nor monthly salaries change in response to the reduction in standard hours.

Table 3 summarizes the average treatment effects of the reform, obtained from a regression similar to equation 2, replacing year dummies with a dummy variable that takes the value 1 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. Column (3) confirms that the effect on overtime hours was close to zero and statistically insignificant. Consistent with the fact that the employers were not allowed to adjust monthly salaries downward, column (4) shows that the effect on the monthly salary was null. The average effects on the hourly wage rate is 0.061 as shown in column (5).

Employment and total hour input Panel (F) of Figure 6 demonstrates the effects of the reduction in working hours on employment. There is a small pre-existing increasing trend

[29] A small, temporary increase immediately after the reform likely reflects the negative employment effects as shown later.

[30] 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. After this period, 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.

for treated firms relative to the control group before 1996. However, this trend reversed in 1997, coinciding with the first year of the national reform. Post-reform coefficients suggest a negative impact on employment size, aligning with theoretical predictions that emphasize the impact of increased labor costs. Note that this is not driven by firm exits, as shown Appendix Table A5. 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 3, and total labor input decreased by 9% as shown in column (7).

The effect of working hour reduction is most apparent on per-worker hours. Simultaneously, the reduction in standard hours can be seen as a shift in the wage schedule, as depicted in Figure 2. Given the new wage schedule, in particular the higher wage rate in our context, firms would determine the new equilibrium total number of hours and workers used for production. Therefore, the estimated coefficients can provide a rough 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 and argues that these elasticities might still be underestimated. The labor demand elasticity in terms of employment is -0.33 (=-0.02/0.061), which is also within a reasonable range relative to previous literature. The meta-analysis indicates an average of -0.551 with a standard deviation of 0.747 for the own-wage elasticities in the literature (Lichter et al., 2015). Our results indicate that labor demand elasticities could be significantly different depending on whether hours or workers are used. It is important to emphasize that the wage increase in our study impacted a large number of workers located at varying 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. Additionally, as shown later, our elasticity estimate encompasses negative effects stemming from the scale effect.

Sales and hourly labor productivity Our dataset includes sales information, enabling us to examine the impact of reduced working hours on firm-level output and productivity. This offers fresh 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%

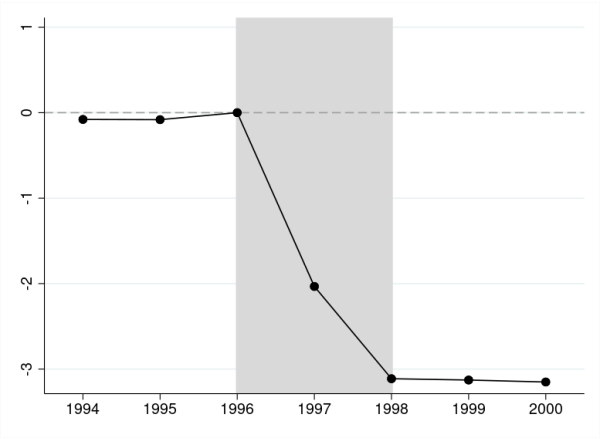
by 2000.[31] Given the larger negative impact on sales compared to employment, we observe a modest but significant decline in per-worker sales in Panel (I). However, the reduction in sales is not as substantial as the decrease in total labor hours input illustrated in Panel (G). Consequently, the reduction 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 3 in column (8) reveals an average reduction of 4% in nominal sales for firms. 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%, evidenced by the positive effect on sales per hour.

In summary, our results indicate that the significant cut in standard hours, coupled with a substantial rise in hourly wages, caused treated firms to operate at a new equilibrium with reduced input and output levels compared to the pre-reform period and control firms. 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 will explore this finding in greater detail below.

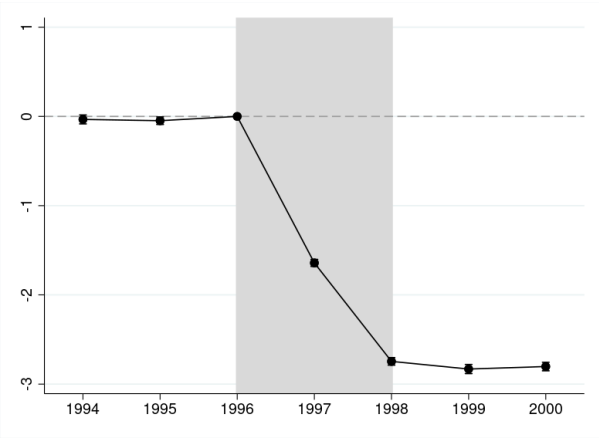
[31] It is worth noting that we find considerably smaller effects on sales in the subsequent section when we focus on a sample of long-standing firms existing since 1986. While some of the variation in magnitude can be attributed to differences in the first-stage effects on hours, it also suggests that the negative impacts were more pronounced for younger firms.

Figure 6: Total Effects of the 1996 reform

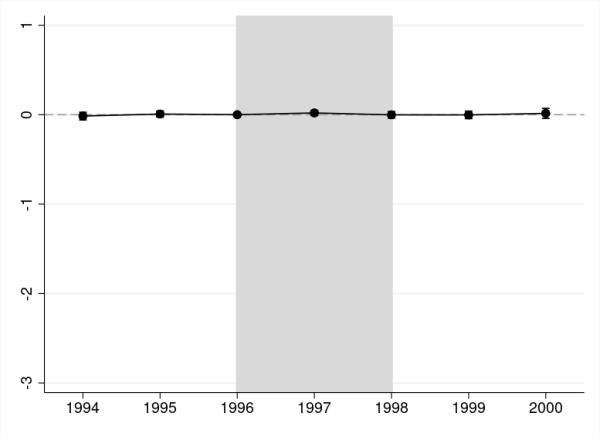
(A) Mean Standard Hours



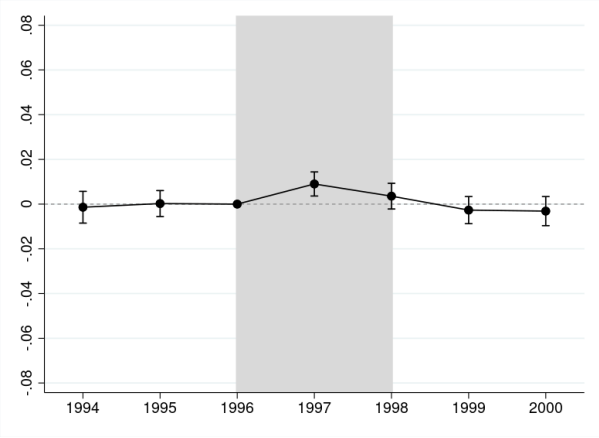
(B) Mean Actual Hours



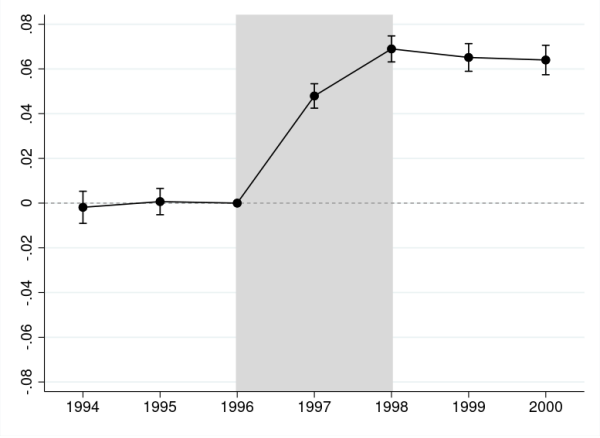
(C) Mean overtime Hours



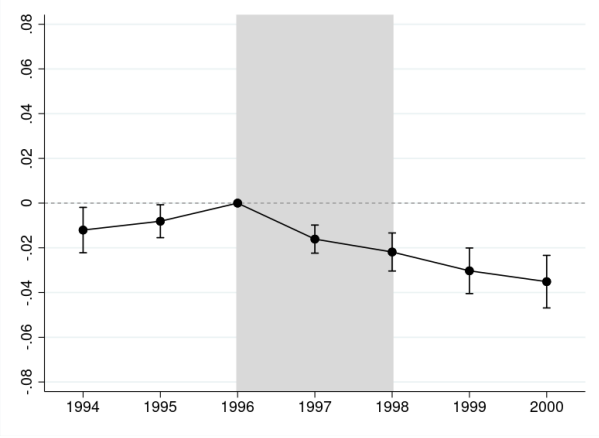
(D) Mean Monthly Salary



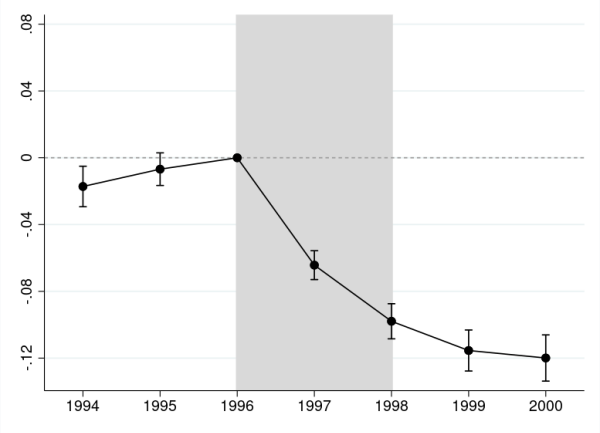
(E) Mean Wage



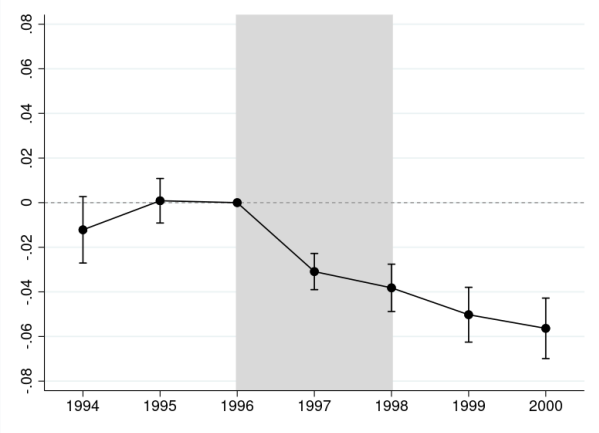
(F) Employment



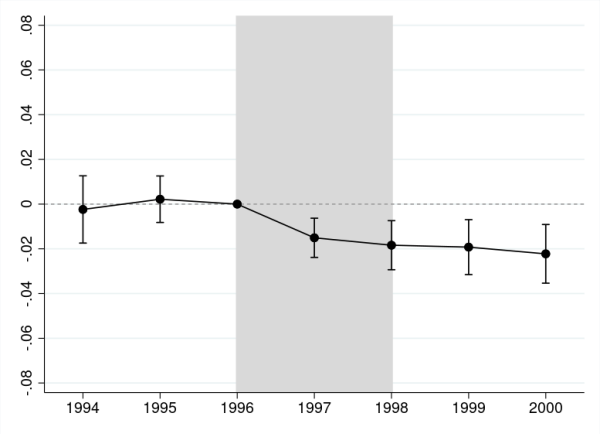
(G) Total Hours



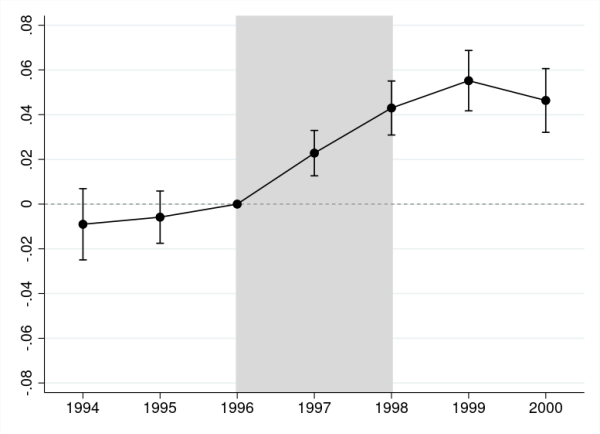
(H) Sales



(I) Sales per Worker



(J) Sales per Hour



Note: This figure displays the dynamic effects of the 1996 reform on firm outcomes, as estimated in Equation 2, providing both point estimates and confidence intervals. The coefficients are normalized to zero in 1996. Except for those involving sales, the outcomes are firm-level means computed from workers who worked at least 30 hours and are expressed in logarithmic form. The shaded area represents the reform period when standard hours were reduced to 40 hours over two years. Treated firms are defined as those with at least one worker having standard hours exceeding 40, while the control group consists of firms where all workers had 40 hours or less prior to the reform. All regressions include firm fixed effects and sector-year fixed effects. Standard errors are clustered at the firm level.

Table 3: The Effects of the 1996 Reform

(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 displays 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 outcomes in the columns (1)-(5) are of the firm-level average values of the fulltime-equivalent workers who worked at least 30h per week. 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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6.2 Employment Effects: Worker Flows and Market Concentration

One of the key findings from our analysis is that firms reduced their level of employment in response to the mandated reduction in per-worker hours and the subsequent increase in labor costs. We present additional results to offer a more nuanced perspective on this negative employment effect.

Worker flows. Firms have two ways for adjusting their workforce: letting existing 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. We estimate the average effect on these worker flows in difference-in-differences methodology, interacting the treatment group dummy with a post-1997 dummy. Due to the existence of the pre-existing trend, we also include firm-specific linear trends to ensure our estimates were not driven by differential trends.

The estimated effects on worker flows are presented in Figure 7.[32] The figure shows that *both* separation rates and hiring rates decreased. 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), indicating the overall negative impact on employment, consistent with our previous estimation. This finding indicate that firms adjusted their total employment size, on average, by refraining from new hiring, rather than by firing incumbent workers. This corresponds to the increasing trend in firm size during our study period, which suggests frequent hiring. Consequently, refraining from hiring became a viable instrument to reduce overall employment (relative to the trend). Moreover, dismissals are highly regulated in Portugal, which explains why firing was not the main mechanism for employment adjustment. In fact, we observe that the separation rate decreased. This is likely attributed to a decrease in voluntary quits because, following the reform, workers could enjoy shorter working hours without affecting their monthly salaries.

The overall reduction in worker flows is in opposition to the prediction that reduced working hours should increase these flows, as employers seek to substitute workers hired under the previous standard with new ones under the new standard (Crépon and Kramarz, 2002).[33] This result is likely a consequence of the inflexibility of the Portuguese labor market, where reducing new hiring is 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.

Role of market concentration There is a growing body of evidence highlighting that the assumption of perfect labor market competition may not stand in reality. Under imperfect competition, the impact of increased labor costs on labor demand may not necessarily

[32] For the corresponding regression table, see Appendix Table A3.

[33] However, such theory assumes that firms can offer lower wages to newly hired workers than wages paid to incumbent workers, even if they have same profiles. However, this may not hold when such differential wages are made difficult by collective agreement or because workers demand wage fairness (Dube et al., 2019).

Figure 7: 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 as defined in the previous section. We estimate the average treatment effects by interacting the post-1997 dummies with the treatment dummy variable. 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. See Table A3 for the corresponding regression table. Standard errors are reported in parentheses; * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

be negative (Manning, 2003). In fact, in the context of reducing working hours, a firm's monopsonistic power could indirectly facilitate work-sharing. To investigate this, we examine the heterogeneity in employment effects across labor markets, which we define by a combination of municipality and 2-digit sector.[34] For each labor market, we compute the Herfindahl-Hirschman Index (HHI) based on employment, utilizing each firm's employment share in the market, and categorize the markets into three groups (bottom, middle, and top third) according to the index.[35] We then estimate the heterogeneous employment effects by interacting the treatment variable with dummies representing each of the three market groups.

The Figure 8 illustrates the heterogeneous employment effects, 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 is the smallest.[36]

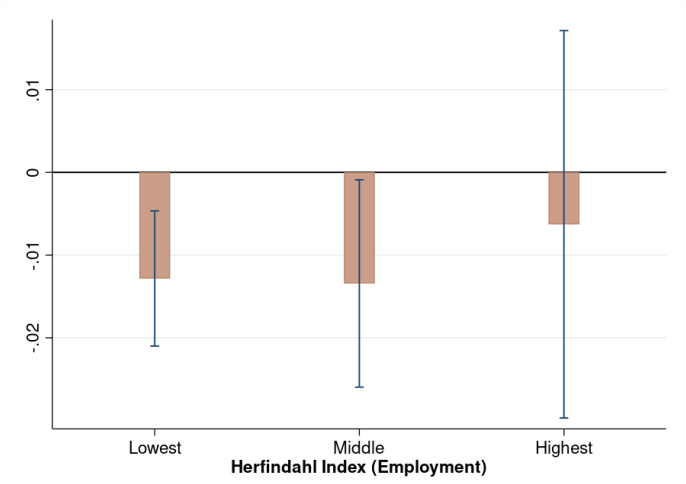
[34] We also conducted an analysis using only the municipality dimension and obtained similar results.

[35] 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.

[36] 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.

This suggests that monopsony power also plays a role in the context of working hour reforms. However, note that even in the most concentrated labor markets, the firm-level employment effects are still not positive. This again underscores the difficulty of implementing work-sharing mechanisms at the firm level.

Figure 8: 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. 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 firm's 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.

6.3 Mechanisms of Labor Productivity Gains

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

Production function concavity. Perhaps the most straightforward 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 higher average output per hour. Some research has demonstrated 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. Our results are not restricted to selected occupations or tasks, and demonstrate the relationship in terms of firm-level total output and total hours worked.[37] While on average this the production concavity align with our results, it is difficult to argue that this is the sole mechanism, as for example, there are other production such as capital may play a role.

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.[38]

Empirically assessing work intensification is challenging because worker effort is not observable. However, Lepinteur (2019) examined subjective well-being and found that workers impacted by the 1996 Portuguese reform experienced increased job satisfaction. Importantly, the study shows that satisfaction improved not only in terms of working hours but also in terms of working conditions. If work intensification is linked to a more demanding work environment and increased pressure, it would likely have a negative, or at least non-positive, effect on satisfaction with working conditions.

As highlighted by Askenazy (2004), who studied the reduction in working hours in France around 2000, work intensification could 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 A11 and A12 in the Appendix, the proportion of workers engaged in shift work, night work, or weekend work did not increase in the Labor Force Survey. This suggests that in Portugal, work intensification through

[37] For instance, we can express the simplest production function as $Y = H^{\alpha}$, where Y represents output and H signifies total hours worked, and the diminishing marginal return emerges with a value of $0 < \alpha < 1$. By taking log and using the coefficients obtained from Table 3, we obtain $\alpha = 0.44$ (=0.04/0.09).

[38] This is empirically consistent with the reduction in the separation rate of incumbent workers after the reform, in line with the efficiency wage theory suggesting that employers may offer higher wages to increase commitment of employees to their job and reduce shirking or absenteeism.

alternative work schedules appears limited. While it is possible that work intensification improved labor productivity per hour, the evidence presented above indicates that its impact was likely modest.

Price adjustment. Our measure of labor productivity is based on *nominal* sales divided by the total number of hours worked. One limitation of nominal sales is that it is influenced by changes in prices. 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, our results on improved labor productivity may also capture the increased unit price of the produced goods. Even if the production size itself decreased in response to the reduction in working hours, the *real* labor productivity may not have increased as much, but the nominal productivity may appear largely improved due to higher prices. Our unfortunately data do not provide information on prices or quantity of goods sold. However, we provide two indirect evidence to examine the role of prices.

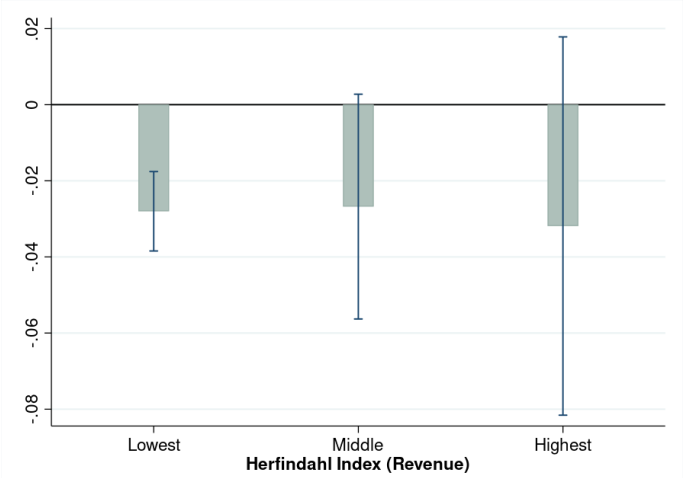
We first look at how the reduction in working hours affected sales in different types of markets. In theory, a firm's ability to raise prices to cover the increased labor costs depends on how much competition it faces in the product market. This leads to a testable prediction that firms in less competitive 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 results are shown in Figure 9. The estimated effects on sales are similar across firms operating in product markets with different levels of sales concentration.[39] This provides indirect evidence that price was not a major mechanism for firms to compensate for the loss in output.

We also test the price effects by matching the yearly output price index at the 2-digit sector level available in the EU KLEMS database.[40] The underlying idea is that if price adjustment is the primary mechanism for firms to maintain their nominal sales, it should be reflected at the sector level as an increase in the output price. In other words, sectors with a higher proportion of firms affected by the reform should experience more significant growth

[39]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.

[40]EU KLEMS March 2011 Update, available here.

Figure 9: 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 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.

in output prices following the reform. To investigate this, we calculate the average of each firm's treated hour share (as defined in equation 1) in each of the 25 2-digit sectors, weighted by each firm's sales share within the sector before 1996. We use industries with an above-median and below-median treatment major as the treated and control groups, respectively, and compute the average treatment effects using a difference-in-differences approach. The results are presented in Table 5. Column (1) shows that, on average, more treated industries reduced actual hours worked by approximately 1.3%. Although the coefficient on the price index (in logarithmic form) is positive in column (2), it is very small. This analysis suggests that price adjustment was not the primary mechanism for firms to adapt to the reduction in standard hours.[41]

Improvement in worker quality. The increase in labor productivity may be attributed to changes in worker compositions. Firms could have sought to enhance their average worker quality to maximize the output for each unit of labor input. Given our finding that firms adjusted their workforce size more through reduced hiring than separations, this adjust-

[41] As an additional test, we constructed sales per hour, which is adjusted for the sector-specific price index, and re-estimated the effects on sales per hour using this measure. We still find a positive effect of the similar magnitude, indicating that the effects on hourly labor productivity were largely real.

Table 5: Sector-level Effects on Prices

	Mean Actual Hours	Price Index
	(1)	(2)
$Treat \times Post$	-0.013***	0.006
	(0.003)	(0.014)
R-squared	0.98	0.92
Observations	175	175

Note: The table shows the effects of the reduction in working hours at the sector level price index based on 25 2-digit sectors obtained from the EU KLEMS database. We calculate the average of each firm's treated hour share (as defined in equation 1) in each sector, weighted by each firm's sales share within the sector before 1996. We use industries with an above-median and below-median treatment major as the treated and control groups, respectively, and compute the average treatment effects using a difference-in-differences approach, controlling for year fixed effects, sector fixed effects, and sector-specific linear trend. The first column shows the effects on the mean actual hours worked from the QP. The second column shows the effects on price index. The outcomes are expressed in log. Standard errors are reported in parentheses; * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

ment might have occurred by reducing the hiring of less qualified workers and reorganizing the production process with a smaller number of productive workers. We utilize worker qualifications as a proxy for worker quality and test whether the treated firms increased the proportion of more educated employees. Figure 6 displays the results using the share of workers with a high school diploma or higher, and with a college degree as outcomes. 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 average worker quality, as measured by their qualifications, did not significantly change after the reform. Therefore, 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.[42]

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

Increasing the utilization of capital is likely more feasible for firms operating with higher capital intensity in their production processes. Conversely, firms that heavily rely on labor

[42]We also do not find significant changes in terms of demographic compositions, such as age group or gender, as shown in Table A4.

Table 6: Effect of Working Hour Reductions on Worker Compositions

	Share of Workers	
	High school or above	College
	(1)	(2)
$Treat \times Post$	-0.001	0.001
	(0.002)	(0.001)
R-squared	0.89	0.90
Observations	398,788	398,788

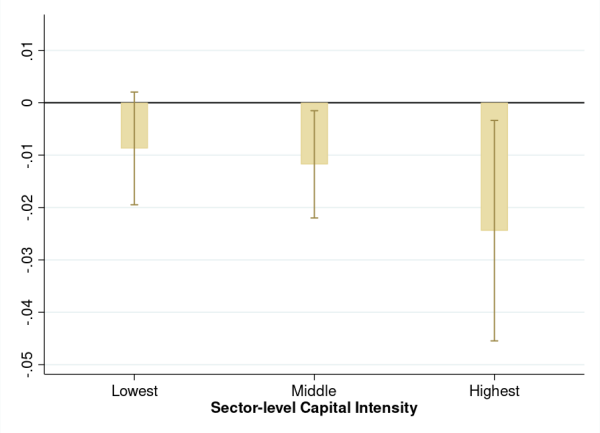
Note: The regression table shows the effect of the 1996 reform on the average quality of workers in the firm, proxied by educational qualifications. In Column (1) and Column (2), the dependent variables are the proportion of workers with at least a high school degree and the proportion of workers with a college degree, respectively. $Treat$ 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. All regressions include fixed effects in year, firm and sector-year combination and firm-specific linear trend. Standard errors are clustered at the firm level. Standard errors in parentheses; * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

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.[43]. The results are presented in Figure 10. 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 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.

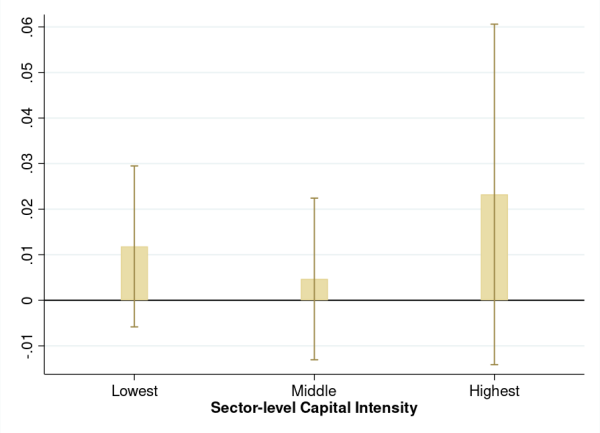
[43]Capital intensity is calculated by dividing total capital compensation by total value added, and then averaging the values over the period from 1994 to 1996.

Figure 10: 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. 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.

Sector-level capital information from EU KLEMS can also be used as a dependent variable. We take the same approach as our sector-level price study as above, and compare the more treated sectors to less treated sectors as measured by the average of firm-level hours share treated. Table 7 investigates the 2-digit sector-level effect on capital services.[44] Same as in the previous analysis on prices, the sectors exposed to more treatment reduced average actual hours by 1.3% in our data, as shown in column (1). Consistently, in the last column, total labor services in the EU KLEMS database exhibited a similar decline. In column (2), there is an insignificant but *positive* coefficient on the estimated effect on capital services, aligning with the idea that capital substituted for labor.[45] It is important to emphasize that

[44]We do not use capital intensity as an outcome variable, which was used in the previous heterogeneity analysis. This is because the EU KLEMS calculate capital compensation as a residual by subtracting labor compensation from value added. Since the reform significantly increased wages, the labor compensation in the post-reform period would understate the decrease in labor input. Consequently, this results in an underestimation of capital input when we rely on capital compensation measure. Therefore, although capital intensity is a useful measure for assessing the relative importance of capital use across sectors *prior to the reform*, it does not effectively capture how capital use changed after the reform. On the other hand, capital services capture how capital stock grew from one year to another, which is useful in examining if capital substituted labor in response to the reform.

[45]It is important to note that, given the small number of sectors, instead of comparing firms above and below the median, we also tried using a continuous measure of the sector-level treatment in our regression analysis. The results were similar.

Table 7: Sector-level Effects on Capital

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

Note: The table displays the effects of the 1996 reform on capital and labor services. The unit of observation is the 2-digit sector. Capital services and labor services are obtained 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 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; * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

if capital and labor are complementary, we would expect a negative sign on capital services. Table A6 in the appendix separately estimates the impact on capital services for non-ICT and ICT capital and shows a larger effect on ICT capital.

Summary. Overall, our analyses suggest that the increase in hourly labor productivity, as measured by nominal sales per hour, is unlikely to be driven by price increases. Instead, it appears to be a real effect. We do not find an increase the share of workers with high school or college degrees, indicating that improvements in the average worker quality do not account for this productivity gain. Conversely, we find indirect yet consistent evidence of capital substitution for labor hours. Nevertheless, the productivity increase is also found in labor-intensive sectors. Therefore, it is reasonable to assume that part of the productivity gains stemmed from alternative explanations, such as concavity in the production function with respect to working hours and increased work intensification.

6.4 Heterogeneous Effects

In our final analysis, we present summarize additional heterogeneity in the effects on employment, sales and output, as shown in Figure A13. We delve into dimensions related to firm quality (measured by pre-reform mean wages), firm size, and sector. 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 their need for a minimum number of workers to maintain a certain production level and 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. The hotel and restaurant sector was most severely impacted in terms of sales. Notably, it was only in this sector that no increase in hourly labor productivity was observed.

7 Extension: Collective-Agreement Reforms

As mentioned earlier, the reduction in working hours in Portugal, from 44 hours to 40 hours, was initially attempted through collective bargaining. When this transition proved unsuccessful, a nationwide reform was introduced in 1996. However, the process of gradually reducing workweek in Portugal began, at least formally, as early as 1991. In this section, we adopt a longer-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, as well as to estimate the 1996 reform's effects specifically for the sample of firms in operation before 1991, Consequently, we can 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. As we will elaborate later, when interpreting this unique policy parameter, it is essential to consider which firms and sectors opted for a gradual reduction in working hours through this policy instrument.

Our empirical framework is essentially built on the long-term institutional context as described in Figure 1. The underlying idea is to use firms that have consistently maintained lower working hours from the beginning of our data period, and as a result, were less or not affected by legislative changes, as the control group for our estimation. Subsequently, we use a difference-in-differences methodology to compare the outcomes of the firms affected by the national reform or collective agreements with those firms that have had lower working hours from the outset. This approach aligns with previous methods in spirit, but there are several noteworthy distinctions: i) treatment status is defined for firms alive in 1986, the first data point in our data and before any change took place in working hours legislation, ii) the control firms exclusively comprise those with consistently low hours throughout the entire period, and lastly iii) affected firms are divided between those that are first impacted by a collective agreement reform, and those that are only impacted later by the national reform.

7.1 Treatment Definition

Data preparation. Our data lacks information on standard hours before 1994. Using actual hours worked to determine treatment status is problematic because it strongly correlates with economic conditions and suffers substantial measurement errors due to variations in the number of working days in the reference month across years. To address this, we imputed standard hours based on actual hours worked in the month, following a methodology outlined in Appendix Section A.3.3. While the imputed standard hours still exhibit fluctuations and measurement errors, they provide a significantly more stable measure compared to actual hours worked.

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 with these inconsistencies, we leverage the worker-level panel structure, enabling us to track the entire flow of collective agreement codes. This allows us construct a comprehensive, balanced panel of collective agreements covering the entire period. Appendix Section A.3.4 provides a detailed explanation of this process. In our final sample, there are a total of 164 distinct collective agreements.

Defining groups. As a control group, we select firms that were unaffected or less affected by regulatory changes or collective agreements. These firms consistently had lower working hours even before the reform period began. We define this control group as firms in the lowest quartile regarding their 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.[46] Consequently, our control group comprises both firms that were entirely unaffected by the reform 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 when the tripartite agreement was first signed. This approach minimizes the impact of mean reversion.[47]

[46] 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.

[47] 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

We define collective agreement treated (CA-T) as those not in the control group and are 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.[48] It is important to note that 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, more than 95% of workers in our sectors of reference are covered by collective agreements. 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). Once the collective agreement is signed, it applied to all workers and firms covered by that agreement.

The remaining firms are classified as reform-treated firms. These firms are not part of the control group and belong to collective agreements where the mode of standard hours changed by no more than 2 hours during the period from 1991 to 1996.

There are some important differences in the treatment between the national reform and the reduction through collective agreements: i) firstly, the collective agreement was inherently voluntary, at least at the aggregate level. It resulted from negotiations between employers' associations and unions, influencing the change (and potentially, its timing). For most firms, this can be considered a shock as external as the national reform, assuming no single firm had the bargaining power to influence the decision. Nevertheless, this could be influenced by sectoral dynamics or other sector-specific factors such as union coverage; ii) the treatment might be less extensive than the national reform, as many collective agreement reforms did not fully transition to the 40-hour workweek; lastly, iii) the years when these changes were implemented differ from those of the national reform, and varying macroeconomic conditions may contribute to differences in the estimated effects between collective agreement reforms and the change in national legislation.

7.2 Descriptive Statistics

Table A13 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,

frames for computing the average.

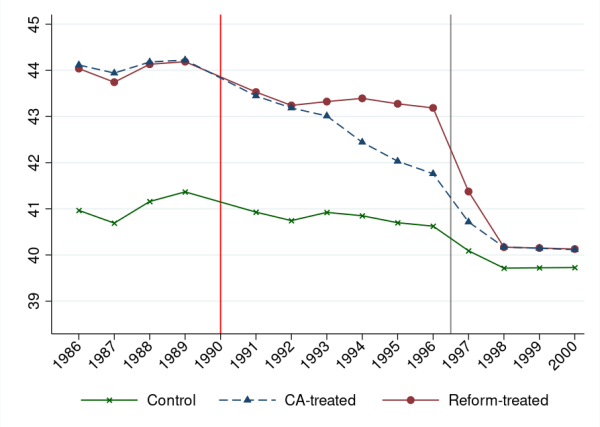
[48] In a few cases, negotiations for collective agreements may have started in 1990, and the treatment began in 1991. To identify these situations, we calculate the mode of standard hours before 1989 (rather than just for 1989 to avoid potential distortions). If the mode of hours before 1989 was 45 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.

pay higher wages and record more sales. On the other hand, firms affected by collective agreement changes are more similar in terms of observable characteristics to those that are only 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 A14 shows graphically the distribution of the treatment groups across sectors.

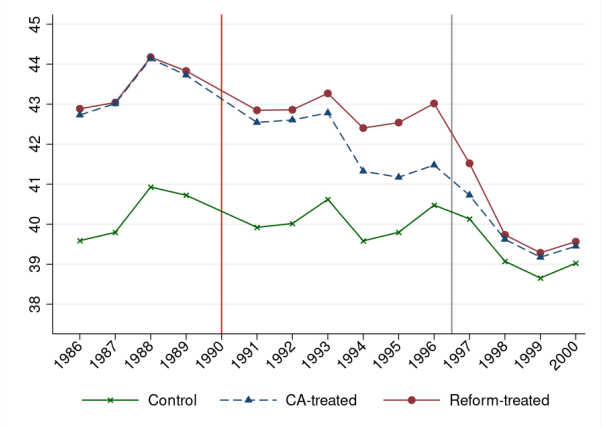
Figure 11 shows clearly distinction in the evolution of hours across the three groups of firms, i) those with low hours since the beginning of the period, ii) those affected by changes in collective agreements, and iii) those only affected by the reform. The first group experiences a relative stability in hours over the whole period, with only a small decline after the national reform in 1996. The blue and red line show instead the separate evolution of those affected by collective agreement reforms and those that are only affected by the national reform, respectively. We clearly observe that firms affected by collective agreement reforms decrease hours starting sooner, although they are still partly impacted by the national reform.

Figure 11: Evolution of Hours Across Treatment Groups

(A) Mean Standard Hours



(B) Mean Actual Hours



Note: The figure shows the evolution of the average of firm-level mean hours for the three groups: Control, CA-treated, and Reform-treated. Panel (A) shows the evolution of standard hours, while panel (B) shows the evolution 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 A10.

7.3 Specification

Given the nature of the collective agreement treatment, we take the staggered difference-in-differences approach to take into the account that CA-treated firms started treatment in different years. Most of collective agreements started to reduce standard hours by 1993 (roughly 80%), but there are some that started later. Appendix Table A16 summarizes the number of firms and collective agreements according to the first year of the reduction in hours. We implement the methodology suggested by Callaway and Sant'Anna (2021) to avoid the issue of dynamic effects confounding the estimates. In short, this deals with the issue of negative weights and wrong comparison arising from the standard two-way fixed effects specification by avoiding the comparison of late-treated units (in our case, collective agreements) with early-treated units. In the results presented, only firms in the control group, i.e., with low hours throughout the period, are used as control units, but adding not-yet-treated units in the control units hardly alter our results.[49]

In Appendix Section A.6, we also present results for firms solely affected by the reform and not by collective agreement changes. This serves as a robustness check of the previous estimation, on a different sample of firms (those in the sample since 1986) and over a longer period. Overall, the results are qualitatively similar to those presented before, hence showing a moderate decrease in employment and labor input as a result of the increase in hours and labor cost. The only notable difference is the smaller effect on output, which closer to zero and insignificant when controlling for differential pre-trends.

7.4 Results of Collective Agreement Treatment

Hours, wages and salaries. Figure 12 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 for the treated firms. Cumulatively, the treated firms reduced hours by approximately 1.5h after 5 years, equivalent to roughly a 4% reduction in per-worker hour input. Actual hours worked per week shown in panel (B) display a similar pattern.[50] Panel (C) confirms that the monthly remuneration of workers at the treated firms were not reduced as a result in the reduction in standard hours. 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

[49] The approach is similar to Daruich et al. (2023) that uses the differential timing of collective agreement reforms in a staggered estimation.

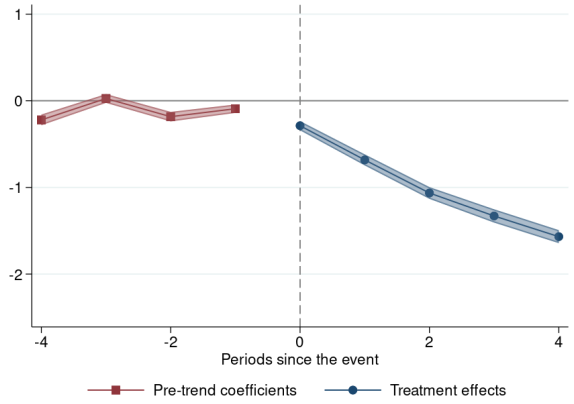
[50] Note that the estimated reduction in hours is smaller than for firms impacted by the reform for two reasons: first, not all collective agreements fully reduced hours to 40 but only, for example, to 42. Second, in our definition, a part of the control group firms are "less treated" with on average higher hours than 40.

wages that might have been simultaneously bargained upon in the process. Finally, Panel (D) shows that the mean wage rate of the treated firms increased by about 4% after the 5 years since the treatment, relative to the control group.

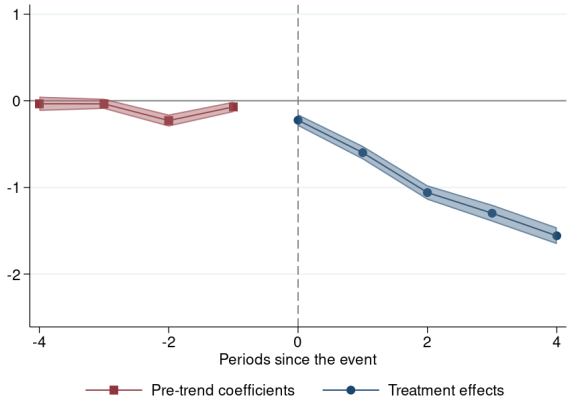
Employment, sales and productivity. Figure 13 shows the results on employment, sales and productivity. Panel (A) shows no significant difference in pre-trend between the firms treated by the collective agreement and the control firms, which hints at the validity of the parallel trend assumption necessary for the diff-in-diff estimation. Overall, we estimate no significant effects on employment levels between the two groups during the gradual reduction in working hours. This is contrasted against the finding of the negative employment effects of the 1996 reform. Similarly, the output measured by nominal sales in panel (B) 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 (C) confirms that per-worker productivity did not alter significantly after the treatment, consistent with the results on the employment and sales. This necessarily implies per-hour sales gradually rose for the treated firms, as confirmed in panel (D). After 5 years since the treatment started, there is nearly 4% increase in the hourly labor productivity, which virtually matches the full the increase in the hourly labor cost.

Figure 12: Effect on Hours, Wages and Salaries

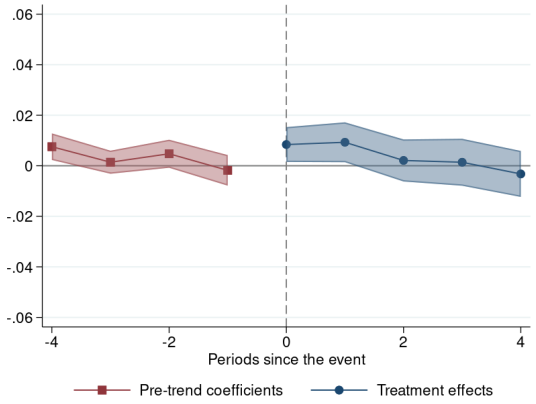
(A) Mean Standard Hours



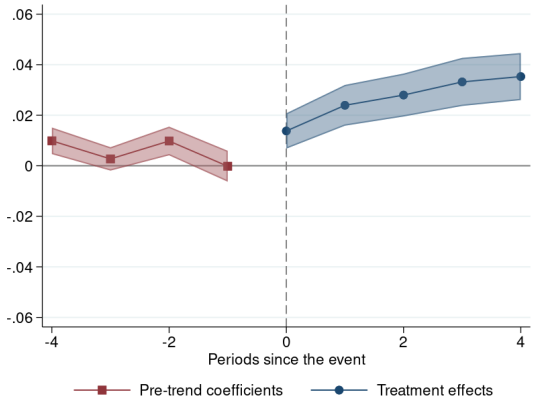
(B) Mean Actual Hours



(C) Mean Monthly Salary

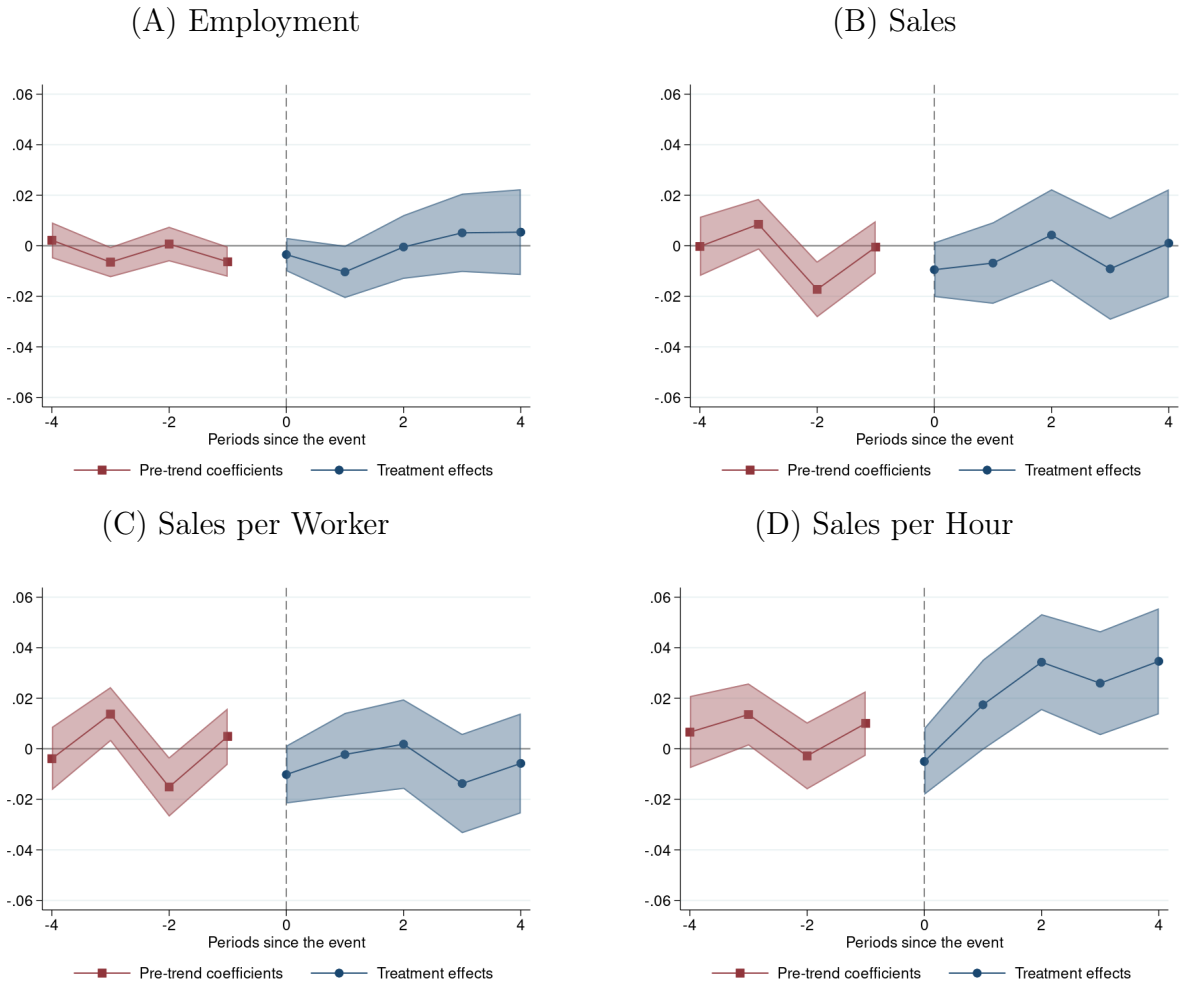


(D) Mean Wage



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 the hours.

Figure 13: Effect on Employment, Sales and Productivity



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.

7.5 Summary and discussions

Our findings show that both treatment through the reform and collective agreements resulted in a substantial reduction in working hours. Firms did not use overtime hours to offset the loss of regular hours due to the costly overtime premium. With monthly salaries remaining unaltered, the reduction in hours led to a substantial increase in the hourly wage rate. These results stand in contrast to the contractual theory outlined in Trejo (1991), which formalizes the possibility that employers and employees simply move to a new contracts with lower

Table 8: Average Treatment Effects of Collective Agreement

(A) Hours and Labor Cost

	Hours			Labor Cost	
	Standard	Actual	Overtime	Wage	Monthly salary
	(1)	(2)	(3)	(4)	(5)
ATT	-1.258***	-1.210***	-0.034	0.030***	0.000
	(0.028)	(0.034)	(0.024)	(0.003)	(0.003)
Observations	252,406	252,406	252,406	252,406	252,406

(B) Labor Input and Sales

	Labor Input		Sales		
	Employment	Total Hour	Total	Per Worker	Per Hour
	(6)	(7)	(8)	(9)	(10)
ATT	-0.007	-0.045***	0.000	0.002	0.036***
	(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). The outcomes in the columns (1)-(5) are of the firm-level average values of the fulltime-equivalent workers who worked at least 30h per week. Standard errors in parentheses; * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

hourly wages, ensuring that neither working hours nor monthly salaries change in response to the reduction in standard hours. In the context of Portugal, which we examine in our study, 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 the size of employment to keep the same total amount of hours in the production. In fact, in 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. However, for firms treated through collective agreements, which also experienced a decrease in hours and increase in labour cost, the effect on employment and output is not distinguishable from zero.

There are several potential explanations for why firms treated through collective agree-

ments exhibited less negative effects on employment and sales. First, the size 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 of the reform treatment. Second, since the collective agreement was a bargaining process and voluntary, firms that would incur fewer costs from reducing hours may have been more inclined to agree to the reduction.[51] This possibility aligns with the comparison of productive characteristics presented in Table A13. Firms treated through collective agreements tend to have slightly higher wage and are more productive in terms of sales per hour and per worker compared to those treated later by the reform. More productive firms may better adapt to the reduction in hours through organizational restructuring, improved work practices, and other measures. This heterogeneity in effects induced by the endogenous process of early reduction in standard hours through collective agreements provides a consistent explanation. 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. This aligns with the observation that the manufacturing sector, which experienced less demand growth compared to the expanding service sector, is more represented among firms subject to collective agreement treatments. To pinpoint the reason behind the less negative effects of collective agreements, it would be necessary examine the specific content discussed during the bargaining process. Unfortunately, such documentation is not available to the best of our knowledge.

Finally, an interesting aspect of the distinct 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 reforms, the wage increase is entirely offset by a similar increase in productivity (both +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 do not need to adjust. Conversely, when wages increase more than productivity, firms are less inclined to hire new workers. If we abstract away from the substitution with other pro-

[51] However, note that the process was not voluntary at the firm level, as collective agreements were negotiated at a higher level between unions and business associations, subsequently applying to all workers, and therefore ultimately firms, covered by the agreement. Instances of firm-level agreements are negligible in our data.

duction inputs, the comparison of the treatment through the reform and through collective agreements demonstrates that the effect on employment, therefore, depends on the ability of employers to increase hourly productivity gains through different mechanisms.

8 Conclusion

This paper investigates the impact of a reduction in working hours at constant monthly salaries on firms. Specifically, we investigate the Portuguese reform that reduced standard hours from 44 to 40 hours in 1996, which is an ideal policy shock to examine, as it did not offer any compensating measures for firms. We show that firms affected by the national reform in 1996 experienced declines in both employment and sales compared to non-affected firms. This is likely attributed to the increased wage rate, as employers were not allowed to reduce monthly salaries in response to the reduction in standard hours. 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 standard hour reductions impact firms. Our *firm-level* approach directly tests and finds support for the theories highlighting the role of increasing labor costs and negative scale effects, factors that pose difficulties to achieving firm-level work-sharing. In our context, 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 likely stems from the intensified use of capital, alongside work intensification and the production function's concavity. However, we did not find empirical support for price adjustment, changes in average worker quality, or the introduction of alternative working time arrangements.

This paper also addresses questions regarding the different legislative instruments that can be used to reduce working hours. In our results, firms that reduced working hours through collective agreements prior to the 1996 reform did not experience negative effects on employment or sales. This is likely related to the endogenous bargaining process, which introduces heterogeneity in the treatment effects. It is important to note that this does not imply that collective agreements are a better means of reducing working hours, as the different treatments (national vs. collective bargaining) were applied to different sectors and firms, making direct comparisons difficult. Moreover, the inability of collective bargaining institutions to autonomously decrease hours was the main reason behind the implementation of the national reform in 1996.

In summary, the primary contribution of this paper is to provide firm-level estimates of an exogenous reduction in working hours, illustrating that while such reductions can be un-

favorable for employment and sales, they can be advantageous for hourly labor productivity. Many questions remain open for further research, including more detailed analyses on prices and capital, general equilibrium effects, and considerations regarding the most appropriate legislative instrument for reducing working hours.

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Online Appendix

A.1 Literature

Table A1 summarizes the literature, separated 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.[52] 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.[53] 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. In this paper, we argue that Portugal, which also experienced a large and sudden reduction in standard time but without compensating measures, provides a much cleaner case study. 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 effects. As we underline throughout the article, the legislative process that brought Portugal to the transition to the 40-hour week was much more complex than put forward by the previous literature on this reform (Lepinteur, 2019; Varejao, 2005; Raposo

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

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

and Van Ours, 2010; Raposo and van Ours, 2010). The national reform was not an out-of-the-blue change in the length of the working week, but the end of a process of 7 years of reform in working hours, occurring through several legislative instruments. In this paper, we try to shed light on the full process that brought Portugal to the 40-hour working week, and incorporate it within the empirical strategy through which we study how reductions through different legislative instruments impacted firms.

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 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 A1 provides the summary of the literature which studies the effect of the working hour reduction.

Table A1: 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	Higher firing (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 × Region	Positive
Chemin and Wasmer (2009)	France - 1998	39h to 35	Region	Null
Batut et al. (2023)	EU - 1995-2002	Several	Country×Sector	Null

Note: *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).

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A.2 Theoretical Illustration

A simplified single-input model with labor hours suffices 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:

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

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 $F'' < 0$.[54] 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.[55] 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.[56]

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:[57]

[54] 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.

[55] 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.

[56] The examples of such cost includes goods provided by employers such as office desks, computers and work cloths, commuting subsidy, minimum social security contribution and so on.

[57] We obtain same expression by solving for N, by expressing $\pi(H) = p \cdot F(N\bar{h}) - wN\bar{h} - kN$.

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

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:

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

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.[58] 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. Additionally, note that in the absence of fixed costs, we have $H^* = G(\underset{-}{w}, \underset{+}{p})$, which means the total hours demanded are independent of $\bar{h}$.

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

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

$\bar{h}$ 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, similar to the situation in Portugal where monthly hours reduction was not allowed when standard hours were reduced, this further tends to decrease employment.

Output and productivity Regarding output and productivity, the equilibrium output, $Y^* = F(H^*)$, unambiguously falls when $\bar{h}$ decreases due to scale effects. However, this does not hold true when fixed costs are absent. 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 equilibrium hours.

This 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 increased labor costs resulting from fixed costs. This negative impact on employment may be exacerbated if the wage rate increases as a result of working hour reductions, a common occurrence in reality. 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.[59] 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

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

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.

A.3 Data Appendix

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

A.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; *municipfirm*: 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 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 (base plus extra time, plus bonus); *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)

A.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.

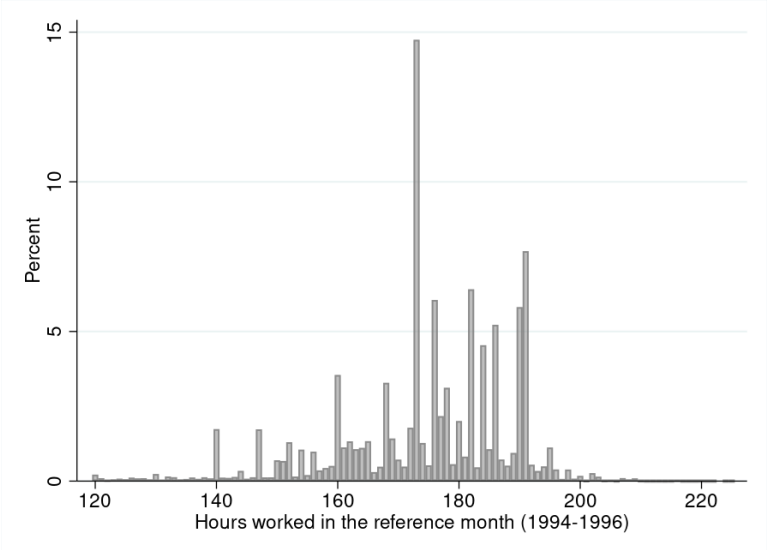
A.3.3 Recovering standard hours in the QP before 1994

As mentioned in the Section 4, unfortunately, the information on the standard hours is available in the QP only since 1994. However, it is important to have standard hours for the entire period of our study, because, unlike actual hours worked, it is not affected by economic cycles and temporary shocks, and hence is less subject to endogeneity problems when defining treatment status of firms. Moreover, it is natural to use standard hours because it was the target of the two reforms. 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. This is due to the fact that, in filling hours information, 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 simply by 20, corresponding to 4 weeks. In this example, these results in the monthly hours of 173h and 160h, respectively.

This fact is reflected as spikes in the distribution of monthly actual hours recorded in the QP, shown in Figure A1. 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. In particular, using the pooled sample of workers in the 1994-1996 period, during which the legal maximum weekly hours was still 44h and the QP records contractual hours, we identify the most frequent standard hours within each bin of monthly hours worked. Figure A2 shows some examples. In each panel, we show the distribution of standard hours for workers with particular

Figure A1: Distribution of Actual Hours Worked in the Reference Month

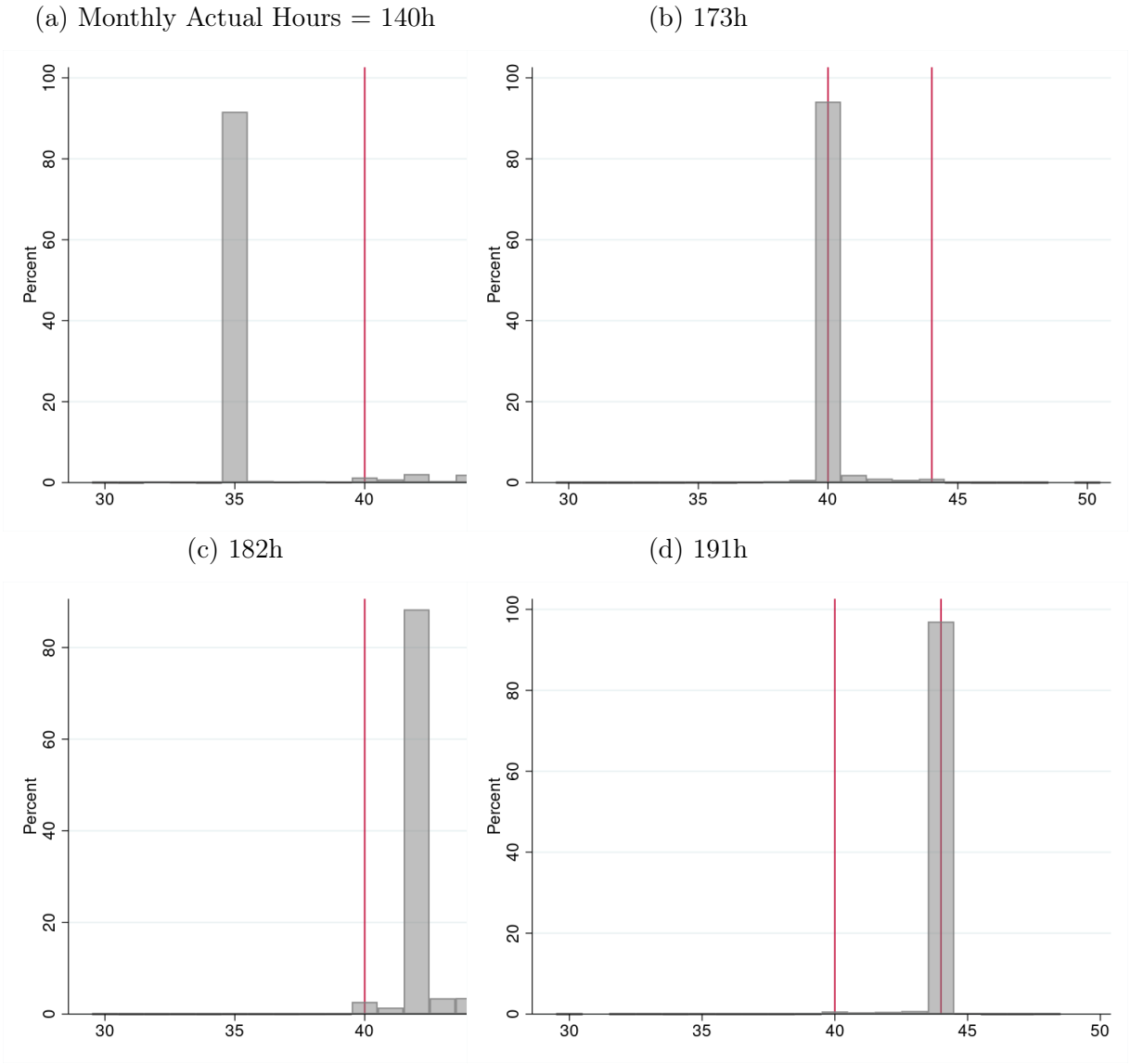


Note: The figure shows the distribution of actual hours worked per month in the QP for years 1994-1996. The distribution displays many spikes associated with particular contractual hours per week.
Source: Authors' calculations based on the QP.

monthly hours: 140h, 173h, 182h and 191h. We identify the most frequent standard hours as, respectively, 35h, 40h, 42h and 44h for these monthly hours. We do 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 these imputed standard hours according to the worker's monthly hours worked.[60] Since the legal maximum standard hours was 45h until 1989, 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) for years in the 1980s.

[60]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 A2: Examples of Distribution of Standard Hours According to Actual Hours per Month



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.
Source: Authors' calculations based on the QP.

A.3.4 Creating a Panel of Collective Agreement

The QP records the unique collective agreement (CA) codes assigned to each individual worker. Every year, there are around 500-600 distinct CA codes, varying in terms of their sizes (i.e. number of workers covered by each CA). Working with these codes over time can be intricate for various reasons. There are instances where different CAs combine into one, workers under a specific agreement shift to another CA, or conversely, one CA could split into multiple new agreements. Furthermore, even without such changes, a CA might be renewed in a way identical to before but with 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 workers. This involves recursively identifying connected groups of collective agreements, a process explained in greater detail below.

Creating Year-by-year Crosswalks First, we create crosswalks that track the dynamics of CA's between adjacent years. We use worker-level data that drops the observations with a worker-ID duplicated within the same year (i.e. the case of mis-coding and multiple job holdings).

(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.[61] 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.

(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 $CA_t = CA_{t-1}$.[62]

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

[61] 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 wre 1002 workers whose CA in t was 358 and it was also 358 one year ago.

[62] 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 $CA_t = CA_{t-1}$

(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)
   (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.[63]

(4) Merge it with the CW_{t-1} based on the CA_{t-1}.[64]

(5) Do the same step (2) and (3), using the pairs of CA_{t-1} and CA_{t-2}.[65]

(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 CA's, avoiding to have a small number of new ID's containing many different CAs, thanks to not using the weak links in the merging process. In the estimation sample, we have less number of the new codes because of the sample selection process (e.g. in terms of sectors). 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.

[63]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.

[64]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.

[65]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.

A.4 Supplementary Descriptive Statistics

A.4.1 Evolution of Hours

The effects of the period of reform are clearly visible in Figure A3, 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 A5 in the Online Appendix). 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.[66] 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).

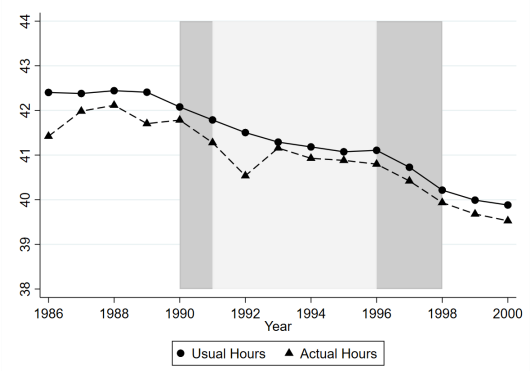
Figure A5 provides the distribution of self-reported usual hours worked in the Labor Force Survey. In panel (a), the period 1986-89 exhibits a bimodal distribution with 40h and 45h. The 40 hour contract were for workers in the collective agreements that had a 40h-regime or the office workers whose standard hours were set lower.[67] From panel (b), in the early- to mid-1990s, working hours shifted to 44h, given the 1990 reform that reduced the maximum standard hours from 45h to 44h. However, the figure indicates that many 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. It is important to emphasize that these

[66] Details on the data are provided in the following Data and Descriptive Statistics section.

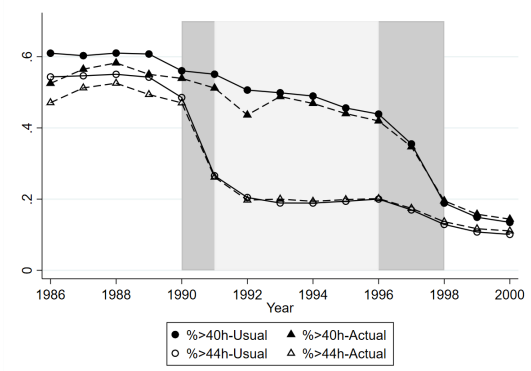
[67] Specifically, the law specified 7 hours per day and 42 hours per week for office workers. The law also specified that employers could increase the daily limit by one hour if an additional half-day or day of rest was provided. Therefore, in practice the daily limit could be increased at 8 hours per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

Figure A3: 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



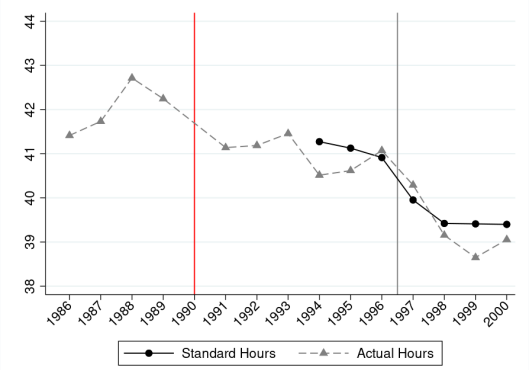
(b) Share Above 40 and 44 hours



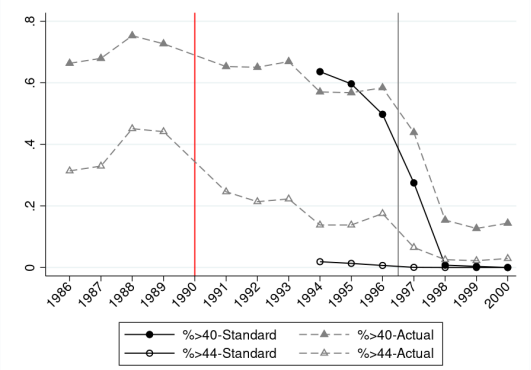
Note: The darker shaded area indicates the period of the national reform, while the lighter gray indicates the period of reform of collective agreements. The LFS measures both weekly hours usually worked throughout the year, and actual hours worked in the past week. The sampling structure of the LFS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

Figure A4: 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 for non-agricultural workers working between 30h and 55h. 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 A8 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 red vertical line indicates the reform reducing the standard hours from 45h to 44h and the commencement of the tripartite agreements encouraging the reduction of hours through collective agreement. The gray vertical line indicates the national reform that reduced the standard hours from 44h to 40h.
Source: Authors' calculations based on the QP.

hours are declared by survey respondents, not employers, indicating the reduction in hours were real and substantial.

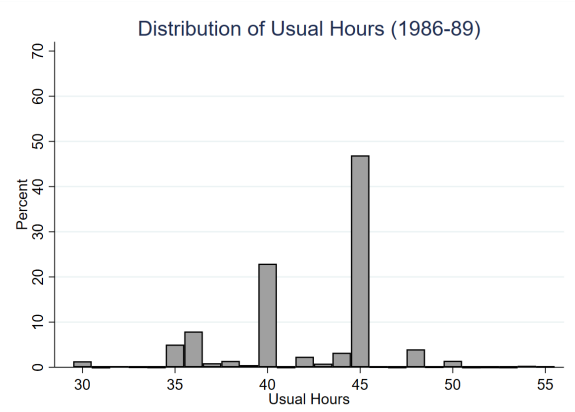
Figure A6 shows the distribution of standard hours recorded in the QP, the dataset used in our estimations. The standard hours before 1994 are imputed. The figure also exhibit similar shift in the hours distribution as in the Labor Force Survey. The distribution of actual hours worked per week in the QP provide similar shift as is clear from the Figure A7. Since the actual hours fluctuate more and is 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 A8 is supplementary to Figure A4, where the evolution of standard hours are extend to pre-1993 years with the imputed standard hours by the methodology outlined in Section A.3.3. Figure A9 shows the evolution of holiday work

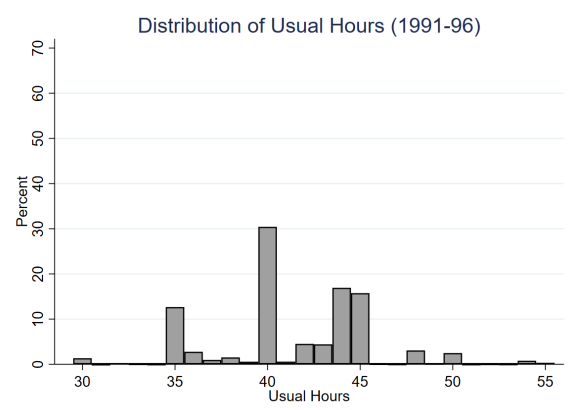
Figure A10 shows the differences in number of working days in the reference month of the QP across difference years. The maximum is 23 days and the minimum is 20 days, being the source of fluctuation in monthly hours worked in the data.

Figure A5: Distribution of Usual Working Hours for Full-Time Employees, Labor Force Survey, 1986-89, 1991-96, & 1998-04

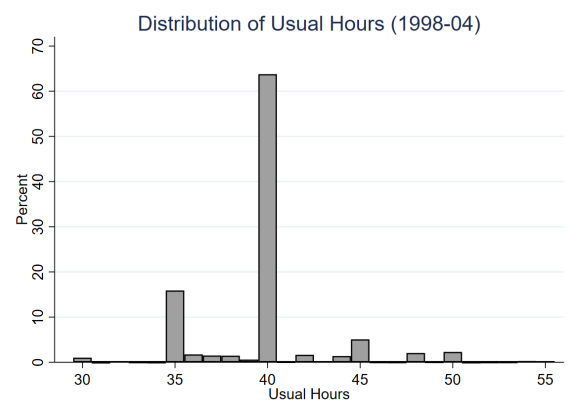
(a) 1986-89



(b) 1991-96



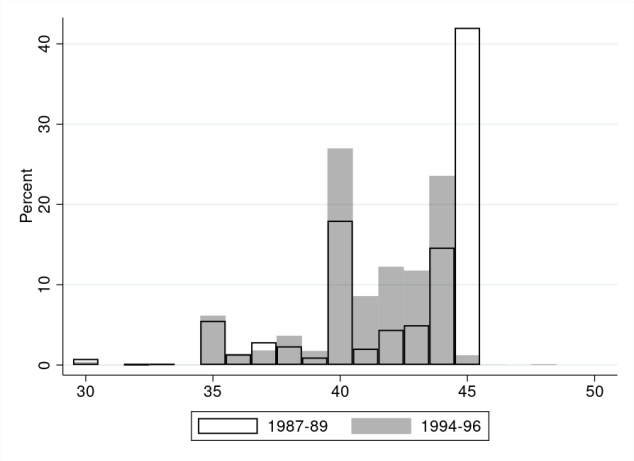
(c) 1998-04



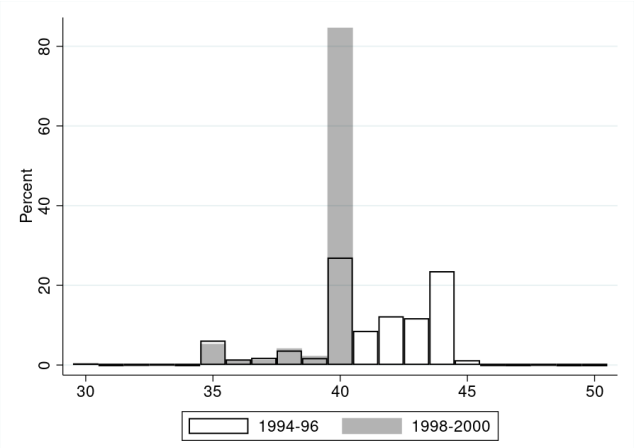
Note: These graphs plot the distribution of usual working hours for full-time employees over the three different periods in which the legislation was different. In the period 1986 to 1989, standard hours were set at 48, or 45 over 5 days. In the period 1991-96, they were set at 44 hours per week. As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

Figure A6: Distribution of Standard Hours for Full-Time Employees, QP

(a) 1987-89 and 1994-96



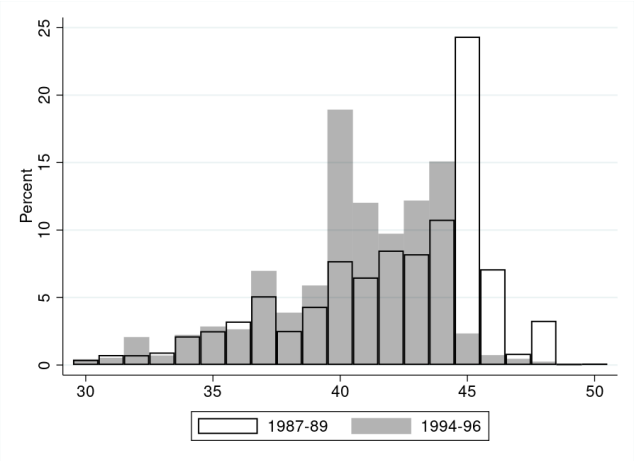
(b) 1994-96 and 1998-2000



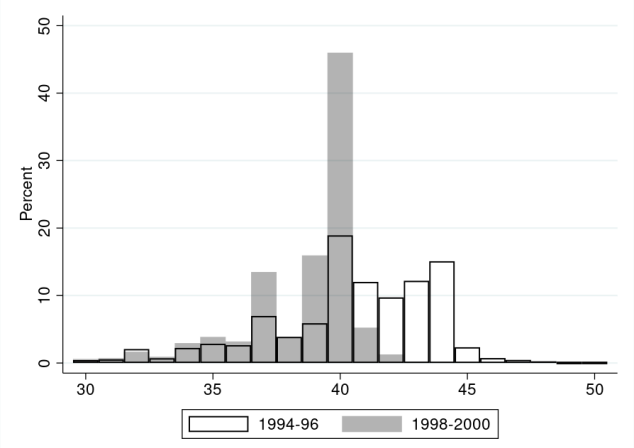
Note: These graphs plot the distribution of weekly standard hours for full-time employees, as recorded in the QP. In the period 1986 to 1989, standard hours were set at 48, or 45 over 5 days. In the period 1991-96, they were set at 44 hours per week. As from 1998, a 40 hour working week was introduced.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

Figure A7: Distribution of Actual Hours Worked for Full-Time Employees, QP

(a) 1987-89 and 1994-96



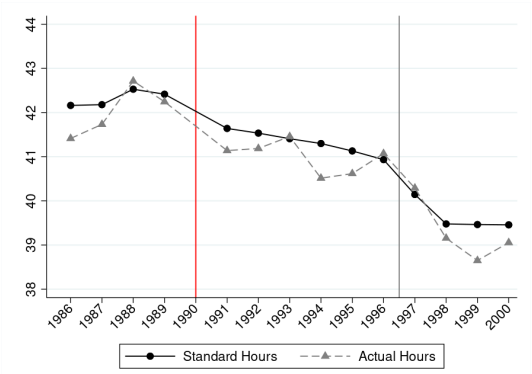
(b) 1994-96 and 1998-2000



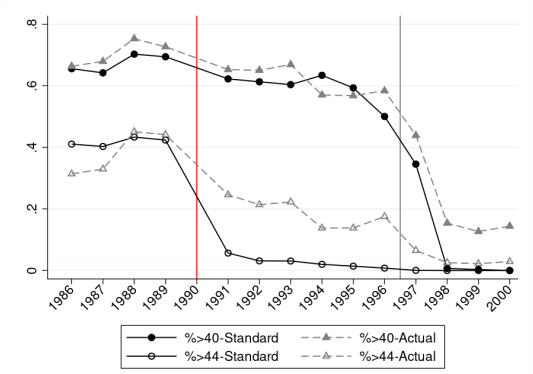
Note: These graphs plot the distribution of weekly standard hours for full-time employees, as recorded in the QP. In the period 1986 to 1989, standard hours were set at 48, or 45 over 5 days. In the period 1991-96, they were set at 44 hours per week. As from 1998, a 40 hour working week was introduced.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

Figure A8: Average Standard and Actual Hours of Full-Time Workers and Share Above 40 and 44 hours, 1986 to 2000, QP

(a) Average Actual and Usual Hours

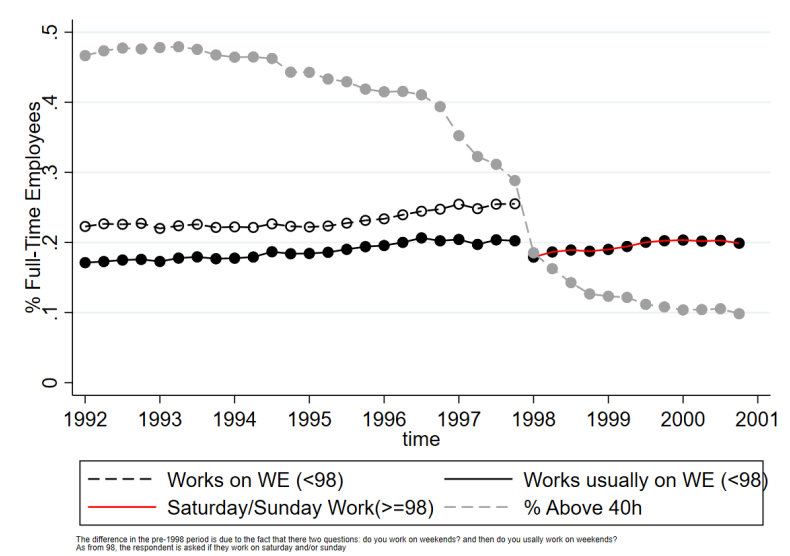


(b) Share Above 40 and 44 hours



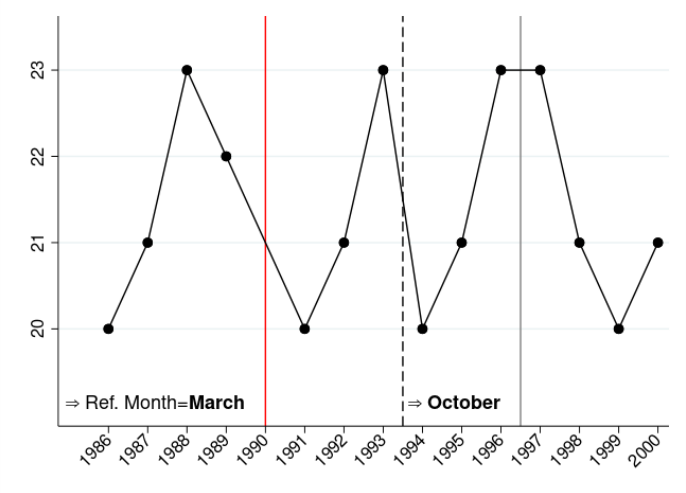
Note: The figure replicates Figure A4 after imputing standard hours for missing standard hours. The panel (a) shows the average standard and actual hours for non-agricultural workers working between 30h and 55h. The panel (b) shows the share of workers with standard hours and actual hours above 40h and 44h respectively. The red vertical line indicates the reform reducing the standard hours from 45h to 44h and the tripartite agreement encourage the reduction of hours by collective agreements started. The gray vertical line indicates the national reform that reduced the standard hours from 44h to 40h.
Source: Authors' calculations based on the QP.

Figure A9: Share of Full-Time Workers Working on Weekends, 1992 to 2000, National Labor Force Survey



Note: this graph plots the share of full-time workers who work on weekends (Saturday and/or Sunday over the period 1992 to 2000). There is a break in the questionnaire as from 1998. Prior to 1998, two different questions were asked: do you work on weekends? and then, if yes, do you usually work on weekends? As from 1998, workers are asked whether they are in any of the following situations: night work, shift work, Saturday work, Sunday work.
Source: Authors' calculations on national LFS data.

Figure A10: 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.

A.4.2 Variation of Standard Hours

In our empirical approach, we identify treated units using the standard hours recorded in QP. To understand the sources of variation in standard hours, Table A2 provides a summary of the R^2 obtained from regressions where worker-level standard hours are regressed with fixed effects in various dimensions. We restrict the analysis to the 1996 sample, which includes workers with standard hours ranging from 30 to 50 hours.

In Table 1, our analysis reveals factors influencing the variation in standard hours. In Column (1), we find that collective agreements account for 56% of this variation. Moving to 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 a slight improvement, with 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 largely reflect a combination of sector and geographical dimensions. Further, in Column (6), we include occupation fixed effects, which increase the R^2 by 0.1. However, it is worth mentioning that occupation does not account for a significant portion of the variation, even when we regress standard hours exclusively on occupation fixed effects.

Finally, in Column (7), we further add firm-fixed effects, and at this stage, 87% of the variation in standard hours is explained. Cross-firm variations would have a substantial explanatory power surprisingly, as firm fixed effects encompass sector, geographical location, and collective agreement dimensions.

Table A2: 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.

A.5 Supplementary Figures and Tables for Main Analysis

A.5.1 Additional Results

Table A3 provides the regression table corresponding to the figure 7, studying the effects of the reduction in working hours on the share of worekrs separated and newly hired.

Figure A11 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 A12 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.

Table A4 examines the effect of the 1996 reform on the demographic composition of workers. Column (1) and (2) show the effects on the share of workers aged 50 or above and aged below 25, respective. Column (3) displays the effect on female share in the firm. The results show that the worker composition did not significantly change in the treated firms.[68]

Table A5 investigate 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 rectangularize the data so that all firms are recorded in the data after their first appearance in the data. The dependent variable takes 1 if the firm operates and 0 for all years after the exit. The results show that there is no significant effects of the reform on the probability to be alive. The fact that the signs of the coefficient are not consistent between columns indicate no systematic difference in treated and control firms in terms of exit probability.

Table A6 supplements the sector-level analysis of capital in Table 7 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.

[68] Note that this lack of significant change in worker composition within treated firms does not necessarily imply that there were no effects, particularly when across sectors. For example, Labor Force Survey indicates a higher proportion of female (currently-working) job seekers who expressed a preference for working 40 hours compared to their male counterparts prior to the reform.

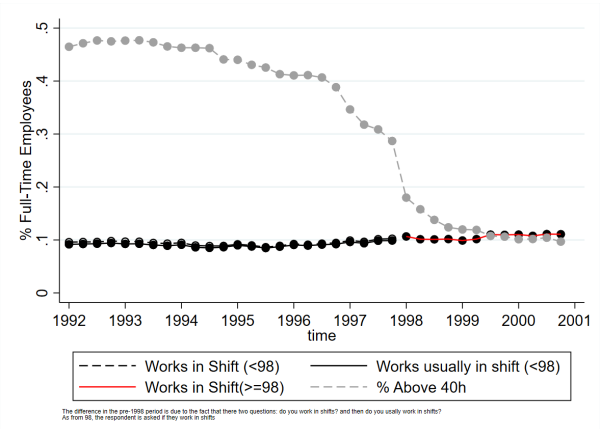
Table A3: Effect of Working Hour Reductions on Worker Flows

	Share of Workers:	
	Separated	Newly Hired
	(1)	(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

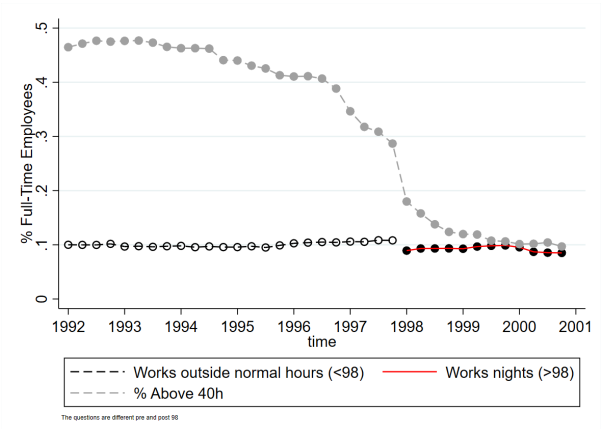
Note: The regression table displays the effects of the 1996 reform on worker flows at the firm level. The outcomes are share of workers separated and share of new workers hired, relative to the previous year's employment size. The regressions employ data from the years 1994-2000 and difference-in-differences 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; * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Figure A11: Evolution of Shift and Night Work Over Time

(A) % of Workers Working in Shifts

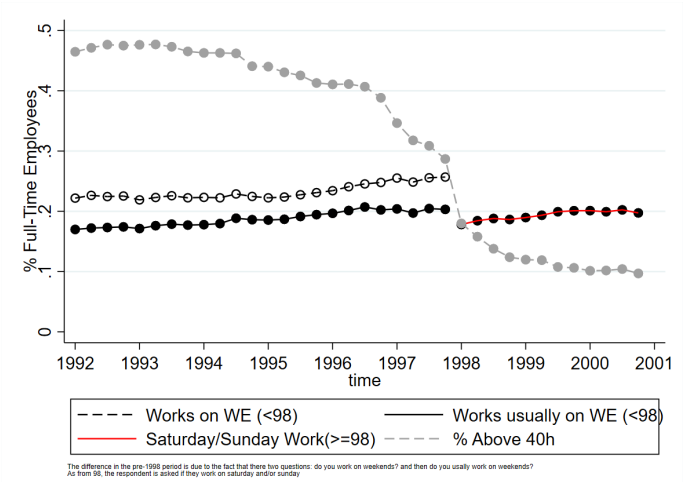


(B) % of Workers Working Nights



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 A12: % Workers Working of Week-Ends



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

Table A4: Effect of Working Hour Reductions on Worker Compositions

	Share of Workers:		
	Age>=50	Age<=25	Female
	(1)	(2)	(3)
$Treat \times Post$	0.000	0.000	0.002
	(0.002)	(0.003)	(0.002)
Mean Outcome	0.2	0.2	0.5
R-squared	0.90	0.85	0.93
Observations	390,392	390,392	397,195

Note: The regression tables show the effects of the 1996 reform on the share of workers aged 50 or above, 25 or below and who are female. *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. All regressions include fixed effects in year, firm and sector-year combination and firm-specific linear trend. Standard errors are clustered at the firm level. Standard errors in parentheses; * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A5: The Effect of the 1996 Reform on the Firm Exit Probability

	Exit of Firms Observed Since:		
	1994	1995	1996
	(1)	(2)	(3)
treatment	0.006	-0.004	0.010
	(0.006)	(0.014)	(0.012)
R-squared	0.08	0.13	0.11
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; * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

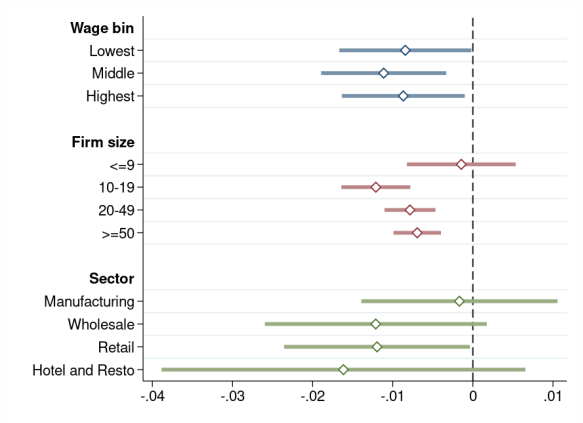
Table A6: Disaggregation of Capital Effects

	Capital Services			Capital Services Per Hour	
	Overall	Non-ICT	ICT	Non-ICT	ICT
	(1)	(2)	(3)	(4)	(5)
$Treat \times Post$	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

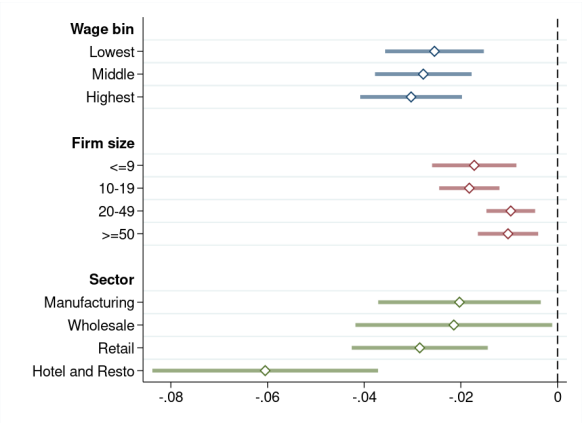
Note:This table complements Table 7 by examining the impact on capital, with a breakdown into non-ICT capital and ICT capital, as well as their values per labor hour. The unit of analysis is the 2-digit sector, and all information regarding capital is sourced from the EU KLEMS database. Treated sectors are those above the median in the average of firm's treated hour shares, as defined in Equation 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; * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Figure A13: Heterogeneous Effects of Working Hour Reductions

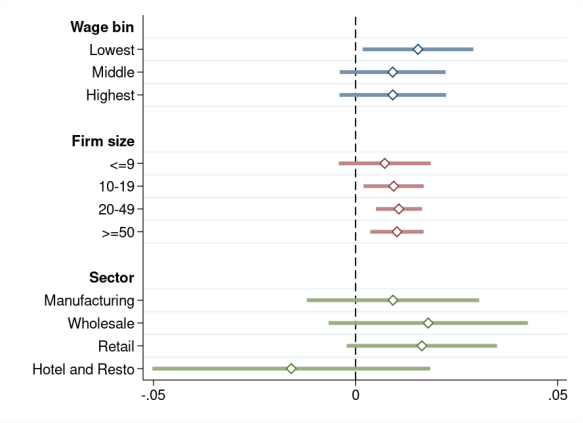
(a) Employment



(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.

A.5.2 Robustness checks

Here, we provide the robustness checks for our main results in Table 3.

To test if any of our findings are driven by the differential pre-trends between firms in the control and the treated groups, Table A7 adds the firm-specific linear trend in the estimation. Doing so attenuates the coefficients because a part of the variation in outcomes are reduced by the firm-specific trend, the results are largely consistent with the main results.

Our main results use firms with no treated workers as our control group. To examine the robustness of our findings under different treatment definitions, Table A9 compares the effect on the key outcomes using different treatment definitions. We maintain the inclusion of firm-specific linear trends to demonstrate the robustness of our results. The first row repeats the main results (with firm-specific trends) using firms with no treated workers as a control In the second and third rows, we adopt different cutoffs by using the bottom third and firms below the median as criteria to define control and treated firms. The coefficients' magnitudes decrease as the firms categorized as control and treated become more similar. Nevertheless, our findings remain consistent across the alternative treatment definitions. Note that 26% of firms in our sample has no workers with standard hours above 40h. Thus, defining control firms as the bottom fifth or quarter provide renders the same results as the first row.

Our main 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 A10 presents the results without excluding any firms. In other words, it is based on all firms that existed between 1994 and 1996 (but after the common sample selection such as on sectors). Once again, we provide the most conservative estimates with firm-specific linear trends: the results remain largely consistent. This aligns with the fact that the majority of firms were impacted by the national reform, and those treated through collective agreements did not exhibit significant changes in the main outcomes. Nonetheless, notably, per-hour sales saw a significant increase for firms treated by collective agreement, which might explain why the estimated effects on sales per hour in Table A10 appear less pronounced compared to our previous findings.

Finally, we show that our results are robust to the exclusion of firms born just before 1996. Table A12 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-create firms, indicating a sudden reduction in horus and increas in labor cost was particularly large for them.

Table A7: 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)
$Treat \times Post$	-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)
$Treat \times Post$	-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 displays the results of the working hour reduction the 1996-reform analogous to Table A7, but control for the firm-specific linear trends. All outcomes are in log, except for the hour measures in the columns (1)-(3). The outcomes in the columns (1)-(5) are of the firm-level average values of the fulltime-equivalent workers who worked at least 30h per week. 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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Table A9: Robustness: Using Alternative Definition for Control Group

	Mean Actual Hours (1)	Employment (2)	Sales (3)	Sales per Hour (4)
Control group defined as:				
$ShareHourTreat = 0$	-1.585***	-0.014***	-0.032***	0.019***
(Baseline)	(0.025)	(0.004)	(0.005)	(0.007)
Bottom Third	-1.615***	-0.013***	-0.031***	0.023***
	(0.024)	(0.004)	(0.005)	(0.006)
Below Median	-1.276***	-0.012***	-0.027***	0.014***
	(0.023)	(0.003)	(0.004)	(0.005)

Note: The table tests the stability of our main results by using alternative definition of treatment groups. The first row repeats the results with firm-specific trends when the control firms are those with no workers treated by the reform. The second and the third rows use instead the bottom third and below median in terms of share of hours treated. The results are largely consistent across alternative definitions of treatments. Standard errors in parentheses; * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

.

Table A10: Robustness: Using All Firms

(a) Hours and Labor Cost

	Hours			Labor Cost	
	Standard	Actual	Overtime	Monthly salary	Wage
	(1)	(2)	(3)	(4)	(5)
$Treat \times Post$	-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)
$Treat \times Post$	-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 displays the results of the working hour reduction the 1996-reform analogous to Table A7, 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 A12: Robustness: Exclusion of Firms Created Just Before the Reform

	Mean Actual Hours (1)	Employment (2)	Sales (3)	Sales per Hour (4)
Sample:				
All firms (Baseline)	-1.585*** (0.025)	-0.014*** (0.004)	-0.032*** (0.005)	0.019*** (0.007)
Firms created by 1995	-1.572*** (0.026)	-0.012*** (0.004)	-0.018*** (0.005)	0.031*** (0.007)
Firms created by 1994	-1.547*** (0.027)	-0.009** (0.004)	-0.015*** (0.005)	0.031*** (0.007)

Note: The table provides the results on the main outcomes by excluding firms that were created just before the reform. The first row shows the original results (with firm-specific trends controlled). The second row excludes firms created in 1996. The third row uses firms which were created by 1994. Standard errors in parentheses; * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

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A.6 Supplementary Figures and Tables for Long Term Estimation: 1996 Reform

A.6.1 Descriptive Statistics of the Three Treatment Groups

Table A13: 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

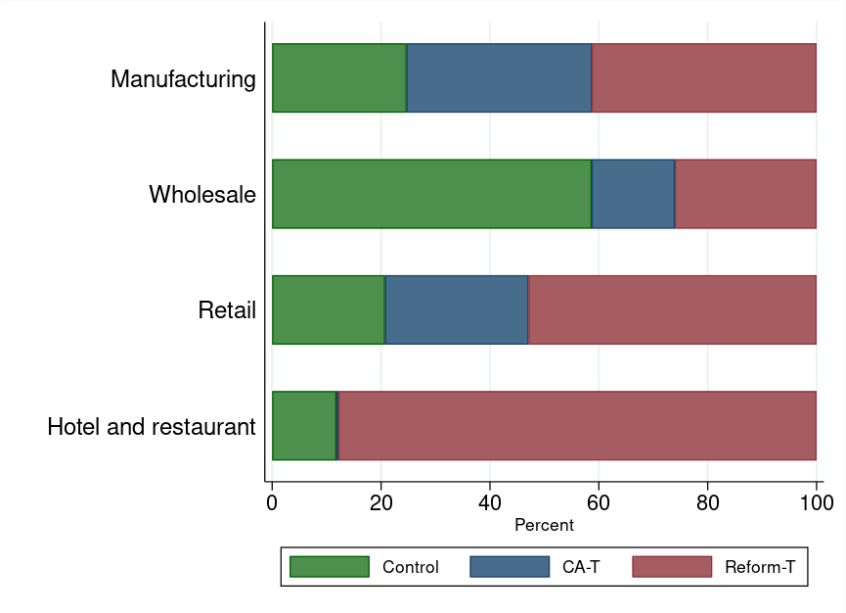
Note: The tables compares the characteristics of firms belonging to each of the three groups defined in our analyses, for firms existing in 1986: Reform-treated, CA-treated, and Control. All values are calculated based on the years between 1986-1989. The table shows that firms in the control group are relatively larger, more productive firms and more likely to be located in the Lisbon metropolitan region.

A.6.2 Sector Distribution of the Three Treatment Groups

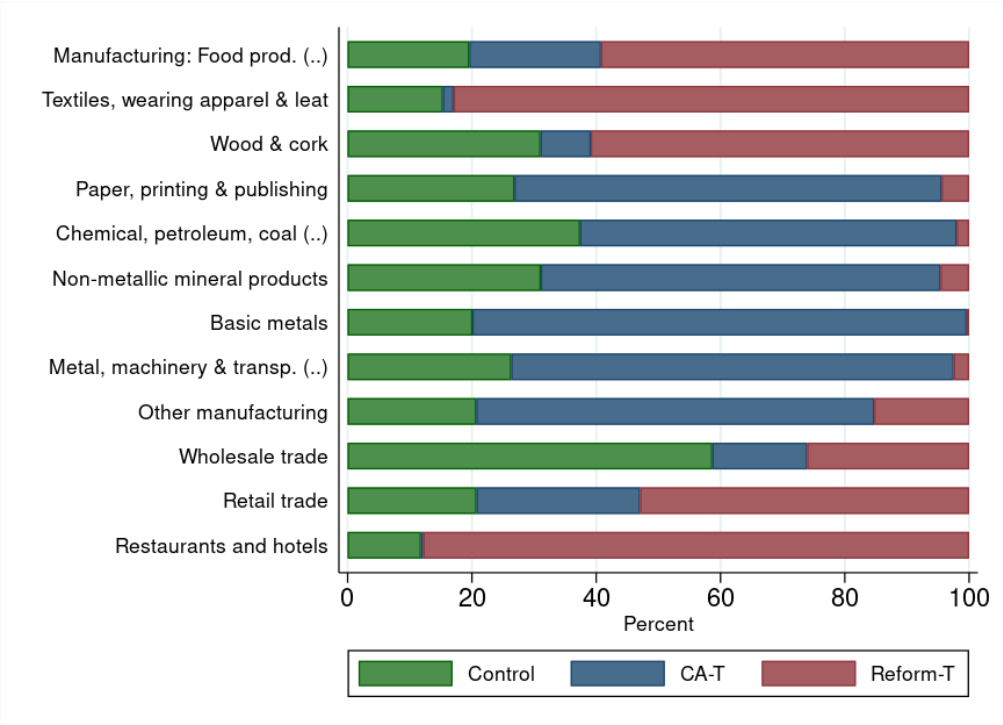
Figure A14 provides the distribution of the treated groups (as defined in Section 5) across sectors. Panel (A) shows the distribution across large sector, while Panel (B) shows the sectors at the two digit level that disaggregates manufacturing sector further. Figure A15 similarly provides the distribution across districts.

Figure A14: Sector Distribution of Treatment Groups

(A) Aggregated Sector

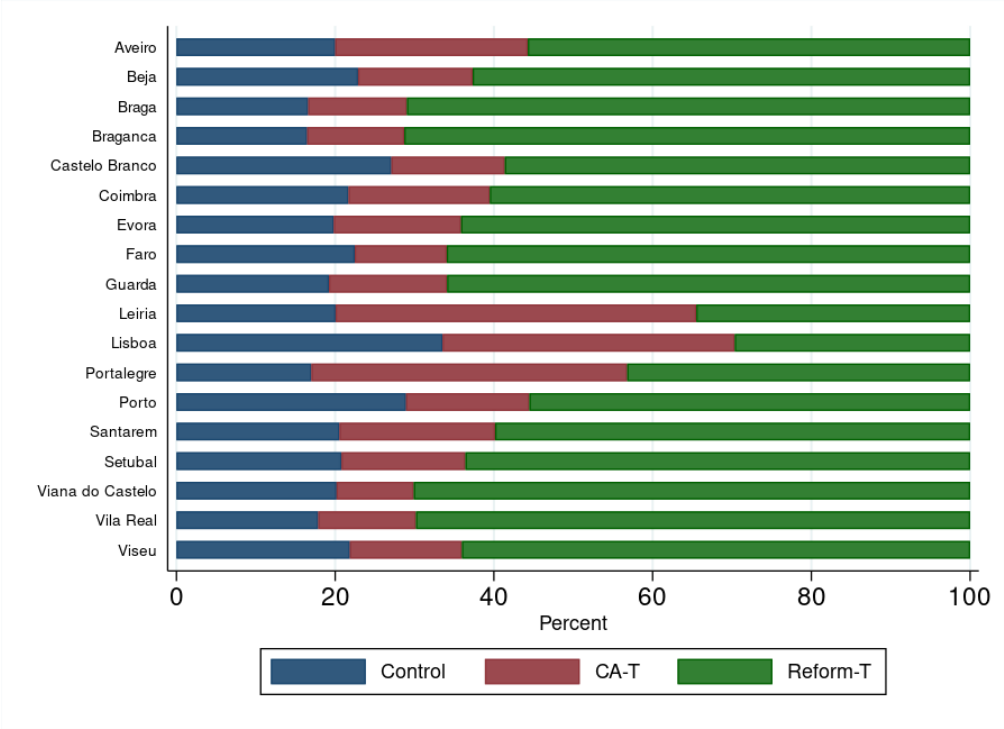


(B) Two-digit Sector



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 A15: Geographical Distribution of Treatment Groups



Note: The figures show the distribution of the treatment groups across districts.

A.6.3 Specification

Our estimation strategy to estimate the effect of reform-treatment relies on a difference-in-differences specification. After defining treatment status as described above, we run the following regression:

$$Y_{jt} = \gamma_j + \delta_{s(j)t} + T_j \sum_{t=1986}^{2000} \beta_t \mathbb{1}\{year = t\} + \varepsilon_{jt} \tag{3}$$

where Y_{jt} are the outcomes of interest for firm j at year t; γ_j and $\delta_{s(j)t}$ are firm fixed effects and sector-year fixed effects, respectively. T_j is our treatment variable that takes 1 for reform-treated (Ref-T), and 0 for the control firms. β_t are the coefficients of interest, which identify the dynamic effects of the treatment. The reference year is 1996. Standard errors are clustered at the firm level.

A.6.4 Effects of 1996 national reform

We provide the results of the dynamic treatment effects of the 1996 reform estimated with the difference-in-differences specification outlined in equation 3. The dependent variables are expressed in log, with the exception of hour outcomes, which are expressed in the absolute terms. The firm-level outcomes that are computed from the worker-level information (i.e., hours, wages and salaries) are based only on workers who performed at least 30 hours of work. We dealt with the missing values by assigning the median value of each firm. Note again that 1990 is omitted from the analyses due to the lack of a worker file for this year.

Figure A16 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 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 A14.

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 the control firms prior the reform. The effect of the 1996 reform is evident: between 1996 and 1998, the 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, demonstrating a sharp decrease of 2 hours following the reform.

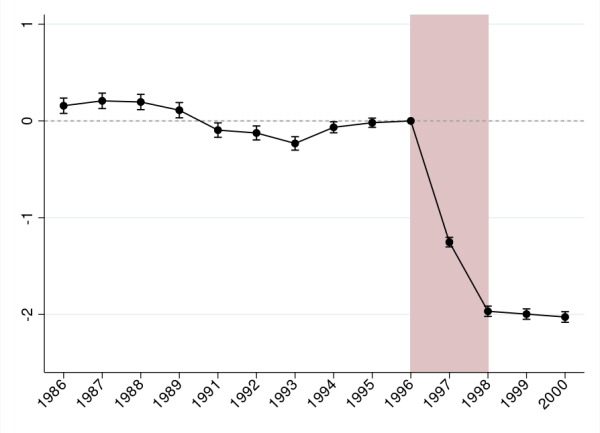
In Panel (C), we observe that there is no adjustment in the mean monthly salary after 1996.[69] Consequently, the reduction in working hours resulted in an increase in the cost of labor, a trend corroborated by Panel (D1) which outlines the effects on mean hourly

[69] If anything, there is a positive impact on monthly salary relative to a long-term pre-trend. This small positive effect (roughly 1%) on mean monthly salary might be linked to the change in the composition of workers after the reform, caused by a negative impact on employment, as shown later.

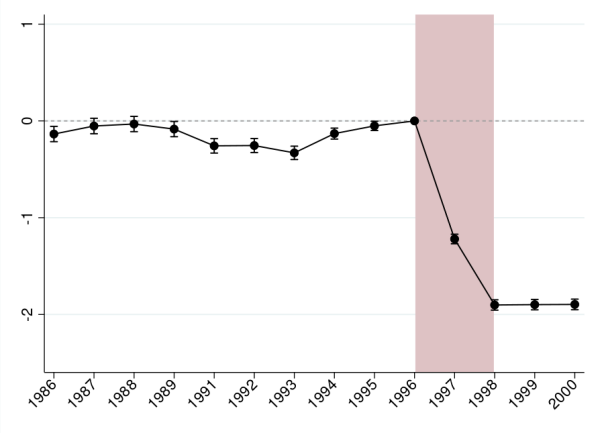
wage. After a gradual decline in the mean hourly wage rate for treated firms relative to the Control until 1996, the wage rate experiences a sharp increase following the reform's implementation. In comparison to the pre-trend, there is nearly a 6% rise in the mean wage rate after the completion of the reform. Columns (1) to (5) of Table A14 show the same results in regression form, with the difference that a firm-specific trend is fitted for each firm.

Figure A16: Effects of 1996 Reform on Hours, Wages and Salaries

(A1) Mean Standard Hours

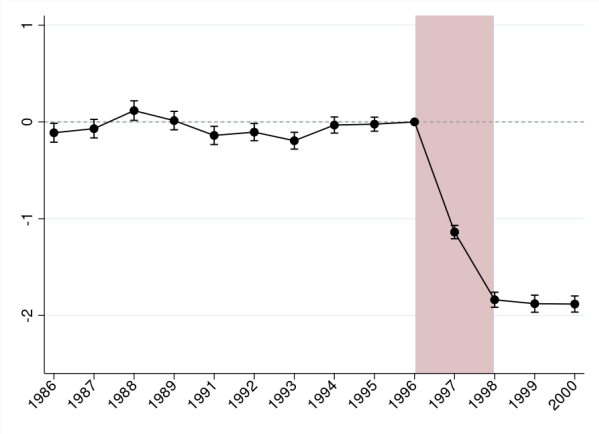


(A2) Detrended

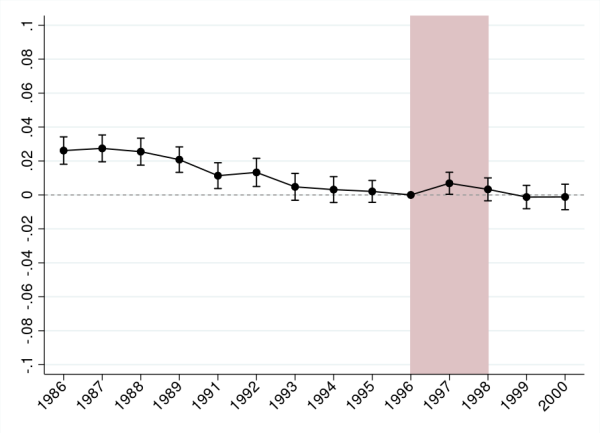


(B1) Mean Actual Hours

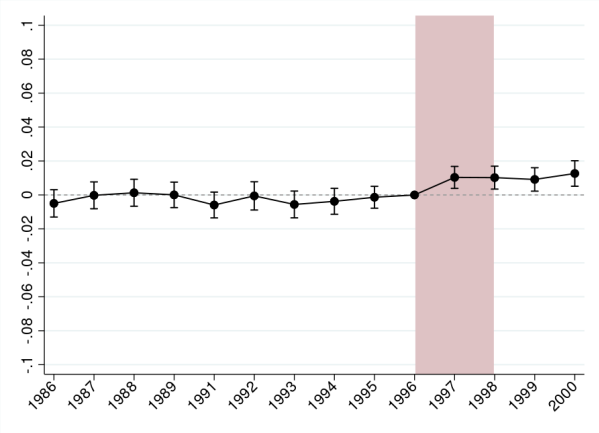
(B2) Detrended



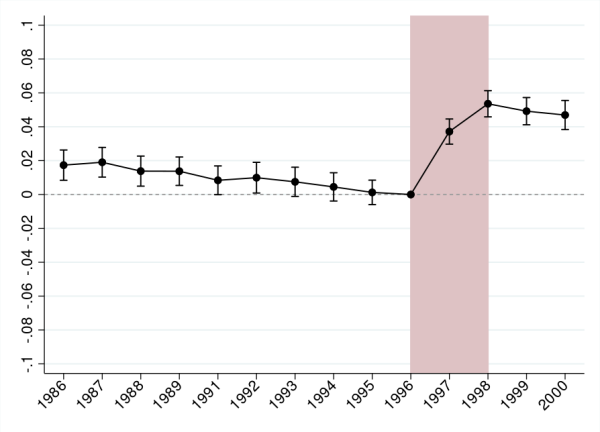
(C1) Mean Monthly Salary



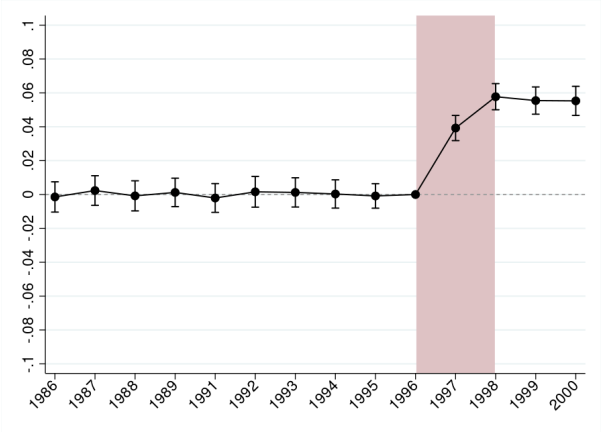
(C2) Detrended



(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 specified in the equation 3. 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.

Employment, sales and productivity. We investigate responses of firms faced with the reduction in working hours and the symmetric increase in labor cost. Figure A17 shows the effects of the reform on first order economic outcomes: 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 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 A14. After the reform, firm's total labor input decreased by 5% for the Ref-T group compared to the control group.

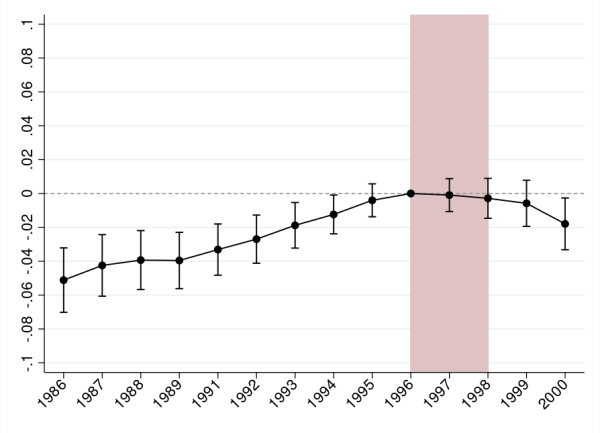
Panel (B1) provides the effects of the reform on sales. The effect is ambiguous: the coef-

ficients 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 A14, 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).

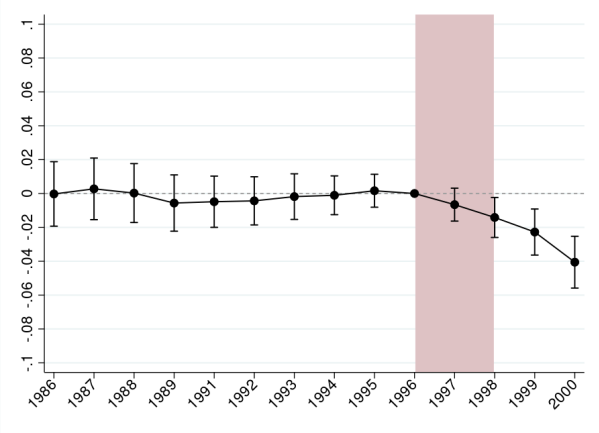
In terms of productivity, as seen in Panel (C1) and Table A14, per-worker sales were not significantly altered by the reform. The coefficients are positive in the graphical analysis presented in panel (C2). When we analyze the results with firm-specific trends, the effect on per-worker productivity is 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). This suggests that, in terms of nominal sales, firms were able to produce significantly more for each unit of labor hour, partially offsetting the decline in both average hours and employment. The rise in sales per hour is also indicative of improved labor efficiency.

Figure A17: Effects of 1996 Reform on Employment, Sales, and Productivity

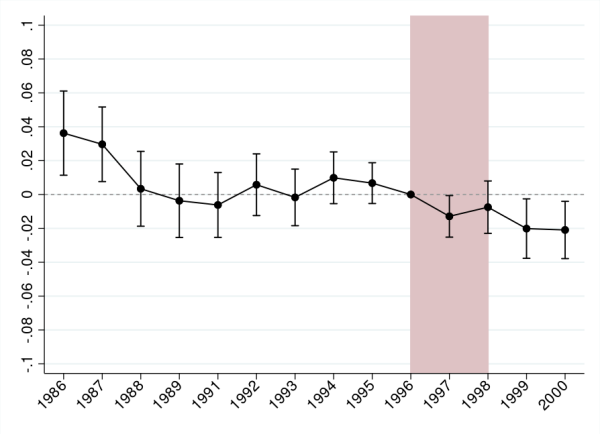
(A1) Employment



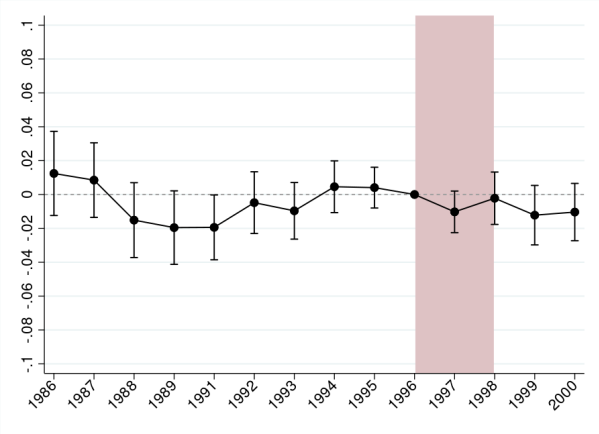
(A2) Detrended



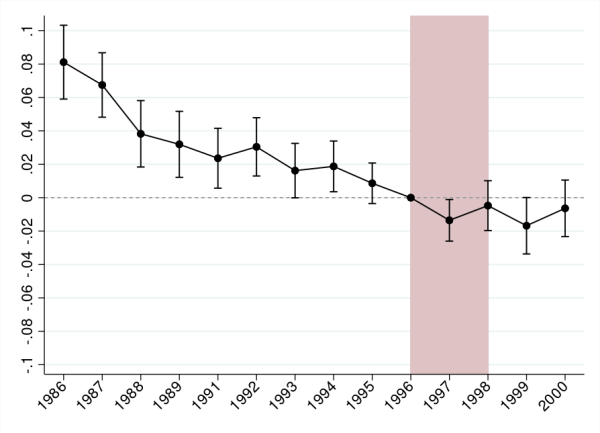
(B1) Sales



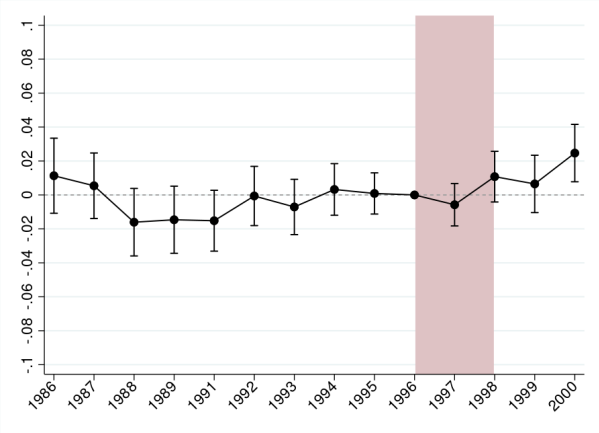
(B2) Detrended



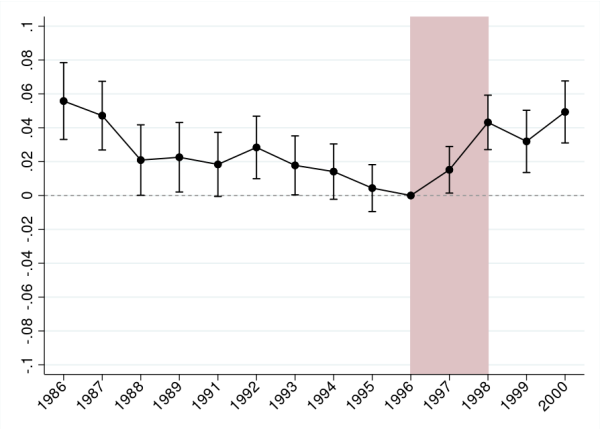
(C1) Sales per Worker



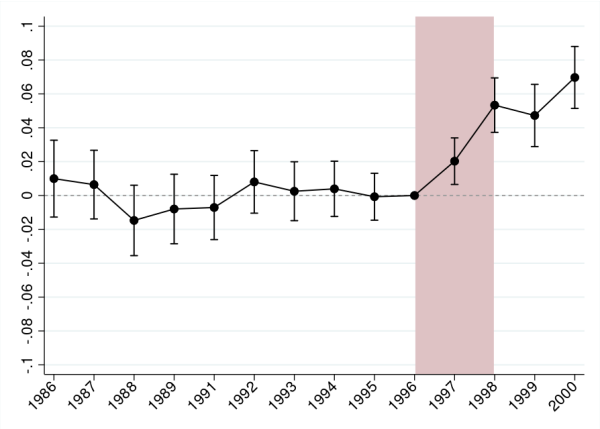
(C2) Detrended



(D1) Sales per Hour



(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 specified in the equation 3. On the right, we show the detrended version of the same dynamic effects. Detrending was obtaine 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 A14: The Effects of the 1996 Reform, Controlling for Firm-specific Trend

(A) Hours and Labor Cost

	Hours			Labor Cost	
	Standard	Actual	Overtime	Wage	Monthly salary
	(1)	(2)	(3)	(4)	(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	Total Hour	Total	Per Worker	Per Hour
	(6)	(7)	(8)	(9)	(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 displays 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 outcomes in the columns (1)-(5) are of the firm-level average values of the fulltime-equivalent workers who worked at least 30h per week. 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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Table A16: 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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A.7 Supplementary Figures and Tables for Long-term Estimation: Collective Agreement Treatment

A.7.1 Supplementary Figures

Table A16 displays the number of collective agreements that reduced standard hours following the 1990 reform, categorized 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 0.1 for the first time. The majority of collective agreements and firms began reducing hours in either 1991, 1992, or 1993.

A.7.2 Alternative Specification

Alternative to the main results presented above, we reinforce the findings on collective agreement treatment by focusing exclusively on those collective agreements that reform over the period 1991-1996, and making use of the variation in the staggered implementation of these changes. 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, and exploiting exclusively variation in treatment timing. This estimation takes advantage of the fact that collective agreements could reform at different times, and have done so over the period 1991 to 1996, so that we can compare firms in treated collective agreements to those that have not yet changed their hours.

Treatment Definition Whether a collective agreement reforms working hours is identified in the same way as in the previous estimation, namely that the mode of hours within that collective agreement code decreases by at least 2 hours over the period. In terms of timing, the first year of treatment is identified as the first one in which the mode decreases or the share of workers at the mode of the hours decreases by 10 percentage point.

Sample Definition The sample is limited to firms within collective agreements that are identified as decreasing their hours over the period 1991 to 1996. Contrary to group (2) of the main estimation, this also includes firms that are born within a given treated collective agreements after 1991, as well as some firms that are part of group (1) in the previous estimation: those that are within treated collective agreements but that have low hours since the beginning of the period. As a robustness check, the estimation can also be run on the subsample of firms born before 1991 to increase the similarity with the sample in the main estimation. These results are presented in the main robustness check section.

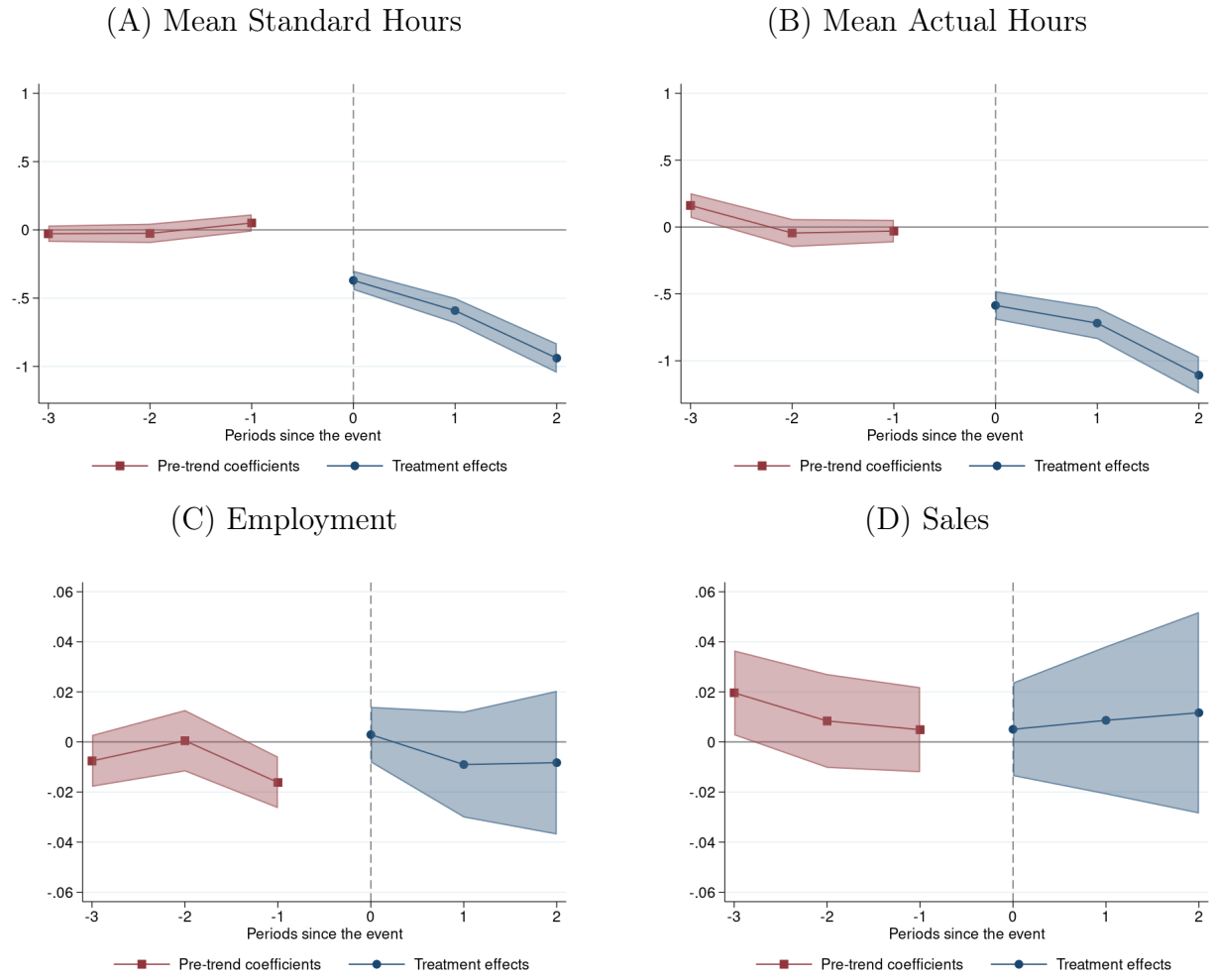
Specification Given the staggered nature of the changes in the collective agreement hours, we implement the methodology suggested by Callaway and Sant'Anna (2021) to avoid the issue of dynanmic effects confounding the estimates. In short, this deals with the issue of negative weights and wrong comparison arising from the standard two-way fixed effects specification by avoiding the comparison of late-treated units (in our case, collective agreements) with early-treated units.

This estimation can be run in two different ways: i) setting all firms into a collective agreement that changes the mode of hours as treated, and only using the staggered implementation of the collective agreements to identify the effects on firms; ii) using firms that, within collective agreements, already have low hours since the beginning of the period (comparable to group (1) in the main estimation), and running the estimation with collective agreements fixed effects. In this second version of the specification the coefficient is identified by comparing affected and unafffected firms within treated collective agreements.

Results Figure A18 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 the 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 A18: 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.