Firm-Level Effects of Reductions in Working Hours*

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Abstract

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

JEL codes: J22, J23, J31

Keywords: working hours, wages, labor demand, productivity

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1 Introduction

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

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

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

1996, following national elections, the new government unilaterally implemented the change to a 40-hour workweek through national legislation.

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

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

Our analysis shows that firms affected by the reform experienced a moderate *decrease* in employment and sales, compared to the control group. This is primarily the result of a substantial increase in the hourly wage rate, driven by the legal restraint to reduce monthly salaries for workers whose standard hours were decreased. This increase in labor costs resulted in negative scale effects on firm output and substitution with capital, leading to reduced labor demand. In terms of magnitude, a 6% increase in the hourly wage rate led to roughly a 2% decrease in employment, 9% decline in total hours, and a 4% reduction in sales. These estimates imply a firm-level labor demand elasticity of -1.48 in terms of hours and -0.33 in terms of employment, which is slightly larger, but within the range of estimates in the literature (Hamermesh, 1996; Lichter, Peichl, & Siegloch, 2015). Firms adjusted their employment size through a reduction in new hires, rather than increased separations of incumbent workers, consistent with the strict regulations on firing in Portugal. Treated firms were able to partially offset the decrease in labor input by significantly increasing hourly labor productivity, measured by sales per hour, partially mitigating the negative effect on sales. This increase in hourly labor productivity amounts to 4.4%, more than two thirds of the increase in hourly wages. By exploiting 2-digit sector-level capital and price information from

national accounts, we observe that there is an insignificant but positive relationship at the sector level between the reduction in hours and the growth of capital services, and little relationship with increase in prices. Moreover, there is no evidence of an increase in average worker quality in treated firms, as measured by workers' educational qualifications.

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

Our paper relates to several strands of literature.[1] First, we contribute to the empirical literature on the impact of working hour reductions on employment. Previous research mainly focused on the effects of working hour reforms on worker-level employment dynamics, finding mixed effects on separations (Crépon & Kramarz, 2002; Estevão & Sá, 2008; Gonzaga, Menezes Filho, & Camargo, 2003; Raposo & van Ours, 2010; Sánchez, 2013). 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 (Batut et al., 2023; Chemin & Wasmer, 2009; Hunt, 1999;

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

Skuterud, 2007). The only study indicating a positive association between employment and a reduction in standard hours is Raposo and Van Ours (2010), who suggest that local labor markets, defined by region-sector combinations, which were more impacted by the 1996 reform in Portugal subsequently experienced higher employment growth.

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

Although some articles have already attempted to explore firm-level effects, empirical evidence in this regard is scant and far from conclusive. Kawaguchi, Naito, and Yokoyama (2017) study the reduction in standard hours in Japan during the 1990s, but find only small first-stage effects on hours.[2] Another study by Crépon, Leclair, and Roux (2004) on France's reduction in standard hours to 35 hours showed that firms affected earlier by the change in hours experienced relative increases in employment. However, as acknowledged by the authors, their evaluation is complicated by simultaneous cuts in social security contributions aimed at easing the transition of firms to the lower hours standard.[3] In contrast, we argue that a key advantage of the Portuguese reform is the absence of such compensating measures for affected firm, making it a cleaner case study.

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

[2]For a subset of firms with a significant drop in hours, they find a negative but insignificant employment effect.
[3]Similar compensating measures were put in place for the Belgian reform in 2002 (Batut et al., 2023).

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

Our contribution also extends to the literature on firm performance and productivity, where evidence on the impact of working hours reductions is extremely scarce. To the best of our knowledge, only Crépon, Leclair, and Roux (2004) study how the mandatory reduction in standard hours affects productivity, within the limits of the setting of the French reform.[4] Some studies have examined the correlation between working hours and productivity in specific occupations (Brachet, David, & Drechsler, 2012; Collewet & Sauermann, 2017; Pencavel, 2014), typically finding a linear relationship between hours and output up to a certain point, followed by diminishing marginal returns.[5] Our paper provides evidence that reductions in working hours significantly increase hourly labor productivity at the firm level, indicating that the productivity gains found in specific occupations might be be applicable in a more general setting.

Lastly, this article contributes to the literature examining the impact of increased labor costs on labor demand and employment. Whether a surge in labor costs has a negative effect on employment is deeply linked to the debate surrounding the "elusive" employment effects of minimum wages (Cengiz, Dube, Lindner, & Zipperer, 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

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

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

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 briefly the varying theoretical predictions regarding a reduction in working hours. Section 4 introduces the data that are used in the analysis and defines the sample. In Section 5, we study the effects on the 1996 national reform through a difference-in-differences approach. Section 6 provides the effects of working hours reductions for early-adopters through collective agreements. Section 7 summarizes and 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 most relevant determinants of the actual length of the working week are standard hours, overtime and the overtime rate. Standard hours refer to the length of the *usual* working week, that is, how many hours a worker usually works or the hours specified in her contract in the absence of overtime, and are usually averaged over a certain reference period. In other words, standard hours set the daily and/or weekly limits at which overtime hours begin. Overtime hours refer to the additional hours worked beyond the standard hours, with limitations usually imposed at the daily, weekly, or yearly levels, or a combination of

these.[6] Overtime hours are paid at an overtime rate, which sets the wage increase a worker should earn on each extra hour. The interplay of the policies directly impacts the number of hours workers actually work. For example, in the United States, where standard hours are set at 40, similar to most European countries, the less strict limits and premiums on overtime result in significantly higher average working hours. In contrast, Portugal follows a different approach – overtime is rigorously capped and comes at a higher cost, making it a rarely used option for firms, as we will elaborate on in the subsequent sections.

2.2 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 rate for the first hour of overtime, and the second number refers to the rate for subsequent hours. All reforms are highlighted in bold; additional aspects of working time legislation not covered in the table can be found in the text. The law specified different working hours for office workers prior to 1996 (7-hour day, and 42-hour week).

In Portugal, working time are regulated by both national legislation and collective agreements, which typically vary by sector and location. The majority of workers are covered by collective agreements.[7] Generally, national legislation sets the upper bound, while collective agreements

[6] For example, maximum weekly hours are capped at 48 hours in the European Union by the 1993 EU WT Directive.
[7] According to the administrative data used in this paper, approximately 80% of workers are directly covered by a collective agreement.

specify lower levels. The national legislation concerning working time in Portugal dates back to 1971, when the *Decreto-lei 409/71* set standard working time at 8 hours daily, and 48 hours weekly, and at 7 hours per day and 42 hours per week for office workers, with one day of mandatory rest. The law also allowed employers to extend the daily limit by one hour if an additional half-day or day of rest was provided. In practice, this meant a daily limit of 8 hours for office workers and 9 hours for other workers, with a corresponding working week of 40 and 44 hours (45 hours prior to 1990), respectively. According to the Labor Force Survey, as shown in Figure A12, 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.[8] However, around 80% of full-time workers were not working weekends in 1992, suggesting that they were on a 9-hour day (8-hour day if office workers) and 44-hour working week (40-hour if office workers). We summarize the major changes in the national legislation in Table 1.

The law also allowed for 2 daily hours of overtime, with a maximum of 240 hours per year. The first hour of overtime was paid a 25% premium and the second a 50% premium. Yearly overtime was reduced from 240 to 160 hours in 1983, and the premium increased to 50% for the first hour and 75% from the second. Notably, the law explicitly mentioned "work-sharing" as the rationale for this reform.[9] The limit was then pushed back up to 200 hours in 1991.

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

[8] 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 A6), it is evident that prior to 1991, most workers were indeed on a 45-hour week, with only a few working 48 hours.

[9] "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).

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

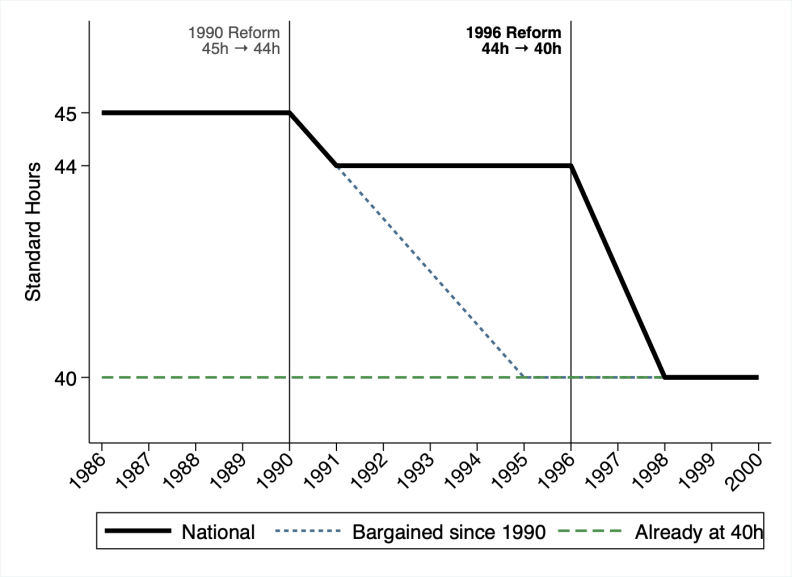
The reform did not increase the maximum overtime hours, but only allowed for a longer reference period over which to average standard hours. It also did not include specific provisions for the adjustment of salaries and wages as hours decreased. However, Portuguese law specifies that the reduction in standard hours must not result in the damage to the economic situation of workers.[10] Lastly, it is crucial to emphasize that, contrary to the other important European reforms in working hours, such as the French and Belgian reforms around 2000, there were no compensating cuts in social security contributions put in place for firms affected by the reform. This allows for a much cleaner interpretation of the effects of reductions in standard hours and is the main comparative advantage of the Portuguese setting.[11]

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. Standard hours can be set at levels lower than the national standard. The green dashed line represents scenarios where standard hours have consistently remained lower, typically due to collective agreements or occupation-specific reasons. Lastly, the blue dashed line represents the

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

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

Figure 1: Stylized Evolution of Standard Hours



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

stylized reduction in standard hours resulting from collective agreements that were initiated due to the bargaining process encouraged by the 1990 memorandum. This should have led to a gradual reduction of standard hours from 44 to 40 hours between 1991 and 1995. In practice, as we will show later, the reduction in standard hours began at different times, mostly starting in 1991, 1992, or 1993. Furthermore, in certain cases, the reduction through bargaining did not implement the full cut to 40 hours.

2.3 Evolution of Working Hours

Figure 2 illustrates the evolution of working hours between 1986 and 2000, according to the employer-employee linked data used in this paper. These hours are reported by employers, but self-reported hours from workers in the Labor Force Survey show a similar pattern, as detailed in Appendix

Section A4.

Panel (A) presents the average standard hours and average actual hours per week.[12][13] Panel (B) shows the proportion of workers with standard hours and actual hours exceeding 40 and 44 hours, respectively. The average standard hours remained stable around 42 hours until 1990, when a decline began due the tripartite agreement that reduced the national standard hours from 45 to 44 hours, resulting in an average of 41.5 hours in 1991. This decline continued gradually until 1996, reaching around 41 hours, as some collective agreements further reduced standard hours. Following the national reform in 1996, the average standard hours dropped sharply to approximately 39.5 hours by 1998 and then stabilized. The trend in average actual hours worked per week also mirrors the evolution of standard hours. Panel (B) confirms a similar pattern: the share of workers exceeding 44 hours sharply declined after 1990, following the reduction to 44 hours per week. The share of those working over 40 hours decreased more gradually between 1991 and 1996 due to gradual reductions by some collective agreements, then dropped sharply after the national reform. Appendix Section A.3.3 provides further evidence showing the distribution of hours across different periods, consistent with the trends in average hours.

3 Conceptual Framework

In general, the theoretical underpinnings of the effects of reductions in standard hours are well-documented in the literature. In its simplest form with exogenous wages and no fixed costs, a standard labor demand model predicts that a reduction in hours would unambiguously increase employment.[14] This is because firms substitute hours for workers, resulting in a higher number of

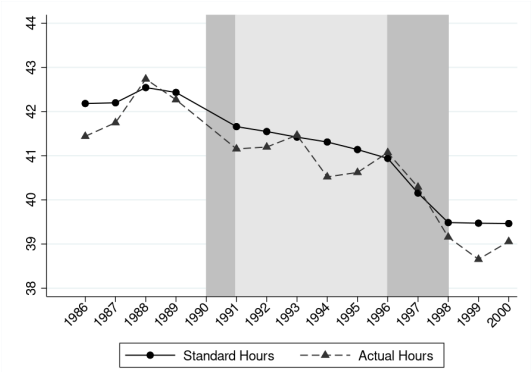
[12] Weekly actual hours are calculated by dividing the total hours worked in the reference month by 21.625 (the average number of working days per month) and then by 5 (the length of the working week).

[13] Standard hours before 1994 are imputed using the method described in Appendix Section A.2.3. Actual hours per week fluctuate due to variations in the number of working days in the reference month across years, idiosyncratic shocks, and business cycles.

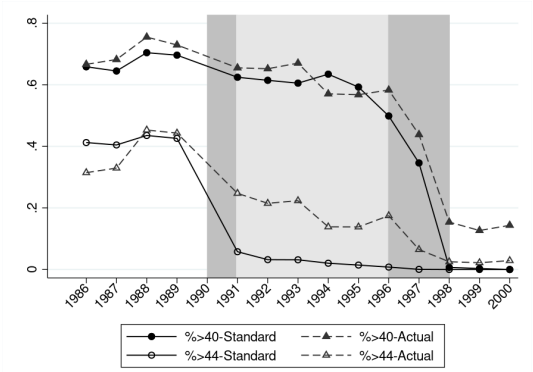
[14] As Estevão and Sá (2008) state: "In a partial equilibrium model of labor demand where average hours of work and employment are perfect substitutes and the only relevant labor cost is the hourly wage, a reduction in the standard workweek reduces average hours and raises employment". Such a model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

Figure 2: Average Standard and Actual Hours of Full-Time Workers and Share Above 40 and 44 hours, 1986 to 2000, QP

(A) Average Actual and Standard Hours



(B) Share Above 40 and 44 hours



Note: The figures show the evolution of standard hours and actual hours worked per week in the employer-employee data used in the paper. Standard hours before 1994 are imputed from actual hours using the method described in Appendix Section A.2.3. Panel (A) shows the average standard and actual hours for workers working between 30h and 55h. Panel (B) shows the share of workers with standard hours and actual hours above 40h and 44h respectively. The darker shaded area indicates the period of the national reforms: the 1990 reform that reduced national standard hours from 45 to 44 hours, and the 1996 reform that decreased the standard hours from 44 to 40 hours over two years. The lighter shaded area represents the period when some collective agreements, encouraged by the 1990 tripartite agreement, reduced standard hours.

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

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

Others consider models with endogenous wages, where wages can adjust in response to the re-

duction 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.[15] Crépon and Kramarz (2002) formally addresses the scenario in which nominal monthly salaries remain unchanged. When nominal monthly salaries are fixed and hourly wages increase, it intensifies the negative scale effect on employment, as highlighted by Calmfors and Hoel (1988). Crépon and Kramarz (2002) also underline its impact on worker flows, as firms have an incentive to replace workers hired under the old standard with new workers at lower wages under the new standard. This underscores the importance of examining employment at the firm level: an increase in the separation rate for existing workers, as some studies have shown, does not necessarily imply an overall negative impact on labor demand if the hiring rate also increases.

As highlighted by Boeri and Van Ours (2013) and Raposo and Van Ours (2010), the presence of monopsonistic power in the labor market can offer a rationale for legislative reductions in standard hours. This is because in such scenarios, wages are lower than the marginal product of labor, and working hours are longer than what would be optimal for the workers. This concept relates to the ongoing discussion surrounding the "elusive employment effect of the minimum wage" as discussed by (Manning, 2021).[16] When we deviate from the notion of perfectly competitive labor markets and take into account the potential positive effects on labor supply, the consequences of an hourly wage increase resulting from reduced working hours become less clear. Moreover, the models mentioned earlier revolved around the assumption of perfectly competitive markets for goods and services, where firms act as price-takers. In this context, the negative scale effect on output

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

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

was unambiguously negative. Nevertheless, if firms possess some degree of market power and can adjust their prices to offset increased labor costs, the projected effects become again uncertain.

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

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.2: it includes information on hours, remunerations, and demographic characteristics at the worker level, as well as sector, location, and sales at the firm level.[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 1994.

[17] On the other hand, there is no information on the cost and profit of the firm.
[18] Specifically, there are no worker files for these years.

As discussed before, standard hours are defined as hours normally worked by contract in a week, which can be averaged over a certain reference period. The QP also measures actual hours, i.e., the hours actually worked during the reference month (March until 1993, October since 1994), available in all years, as well as overtime hours in that month. We compute weekly actual hours by dividing the total hours worked in the reference month recorded in the data by 21.625, the average number of working days per month. Although there is a strong correlation between the standard and actual hours, they are not identical. Moreover, actual hours fluctuate much more than standard hours due to business cycles and idiosyncratic factors.[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 A9, this can vary between 20 and 23 days per month, depending on the number of weekends and national holidays that fall within a given month.

For some descriptive statistics, we also use the *Labor Force Survey* (LFS). This data provides information on usual hours of work per week and actual hours worked in a reference week. The advantage of the LFS is that workers directly report their hours, including any informal or uncompensated overtime hours that may not be recorded in the administrative hours. This allows us to show that the reform affects "real" hours worked, not only those reported by employers. The Appendix Section A.2 specifies all relevant variables.

4.2 General Sample Selection

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

More importantly, we limit our analysis to the manufacturing, retail, wholesale, and hotel and

[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.

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

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

5 The Effects of the 1996 National Reform

The objective of this paper is to estimate the impact of working hour reductions on firm-level outcomes. As noted earlier, the reduction in working hours in Portugal from 44 to 40 hours was first attempted through collective bargaining. Since this transition did not succeed to a significant extent, it was subsequently enacted through the 1996 reform. In our first estimation, we focus on this national reform for several reasons: first, the majority of firms reduced working hours through the national reform; second, the reform led to a rapid and sudden decrease in working hours. Importantly, the timing of the reform was unanticipated until the election that brought the change in government in 1996, which is key for obtaining a causal estimate.[20] On the contrary, studying the effects of the collective agreement is more challenging due to the lack of precise

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

information on the content and timing of collective agreement changes. Finally, information on contract hours is available in our data only since 1994. For these reasons, we focus on the national reform in our main estimation. Nevertheless, despite the limitations, we also estimate the effects of working hour reductions through collective agreements in the next section.

5.1 Sample

We focus this analysis on the years between 1994 and 2000. We start from 1994 because standard hours are available in the QP only from 1994 and the reference month has changed to October since that year. Our sample is an unbalanced panel of firms that existed between at least once between 1994 and 1996 and in our sectors of interest (i.e., manufacturing, wholesale, retail, and hotel and restaurant). To minimize the potential influence of the reduction in working hours through the collective agreement process prior to the reform, our main estimation excludes firms in which the mode of standard hours changed between 1994 and 1996 by more than 1 hour, resulting in a drop in 30% of the firms. However, estimating the effects on the full sample of firms without any exclusion leads to qualitatively similar results, as shown in the Appendix Table A9. Our final sample consists of 73,612 distinct firms.

5.2 Treatment Definition

Our estimation strategy exploits the *firm-level* variation in the exposure to the reform. Essentially, we use the fact that, while the legislative standard hours were set at 44 hours, some firms were not (or less) exposed to the reform. These firms had lower standard hours because their collective agreement specified so since before the 1990s, the composition of workers were different (e.g., office workers having 42 hours or 40 hours), or possibly some firm-specific production characteristics having required relatively shorter hours per worker.[21]

To define treated and control firms, we construct the measure of the exposure to the treatment

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

for each firm. We compute the share of the treated hours for each firm, that is:

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

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

Figure 3 shows the distribution of this treatment measure across firms. At the two extremes of the distribution, there are firms with all workers treated at 44 hours and with no workers treated (two groups of roughly equal size); in-between, there are firms with some workers above 40 hours, but not all, or all workers having hours between 40 and 44 hours.[22]

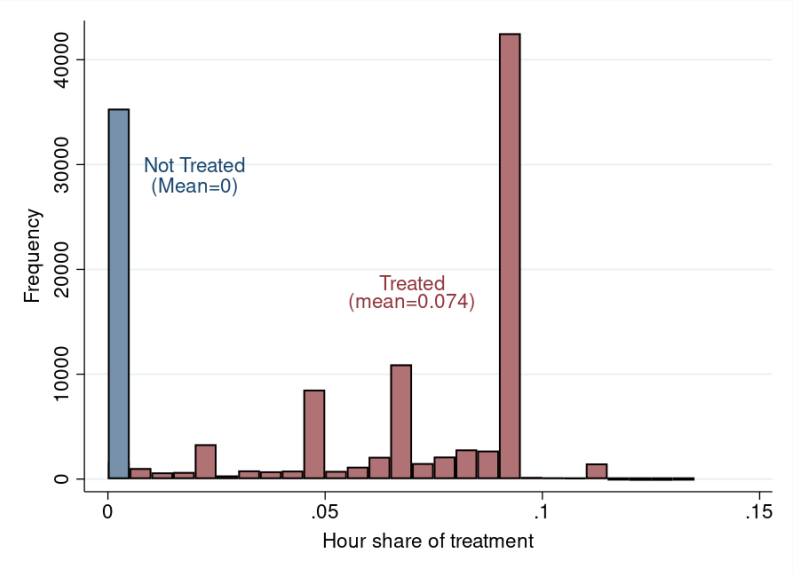
We define treated firms as those with any exposed worker (i.e., strictly positive value in share of hours treated), that constitute roughly 74% of the firms in our sample. Our control group consists of firms with 0% share of treated hours, corresponding to the remaining 26% of the firms. The average share of hours treated among the treated group is 0.074. In the robustness checks (Table A8), we also use alternative ways of constructing control firms, such using bottom third or below-median in the treatment measure, and the results are largely consistent.

5.3 Empirical specification

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

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

Figure 3: Firm-level share of treated hours



Note: This figure plots the share of hours treated for each firm as calculated in Equation 1. It is computed as the sum of standard hours in excess of 40, divided by the total sum of standard hours in the firm. Firms with 0 treated hours have all workers at or below 40 hours. We take the average over 1994-1996 so that each firm has a single value of this treatment measure that is robust to potential mean reversion.

$$Y_{jst} = \gamma_j + \delta_{st} + T_j \sum_{t=1994}^{2000} \beta_t \mathbb{1}\{year = t\} + \varepsilon_{jst} \tag{2}$$

where Y_{jst} are the outcomes of interest for firm j, in sector s, in year t (expressed in log, except for the hour measures); γ_j and δ_{st} are firm and sector-year fixed effects respectively.[23] T_j is our treatment variable that takes 1 for treated firms and 0 for control firms. β_t identifies the effect of the treatment at time t, using 1996 as the reference year. Standard errors are clustered at the firm level.

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

$$Y_{jst} = \gamma_j + \delta_{st} + \beta \cdot (T_j \times Post_t) + \varepsilon_{jst} \tag{3}$$

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

where $Post_t$ takes 1 for years after 1997, and 0 for 1996 and before. β provides the average treatment effect in post reform years.

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

Except for the variables for hours, where the absence of a trend allows us to estimate the effect both on units or logs, we put all our outcomes in logarithmic form, since the analysis of the pretrend shows that the relative evolution is more likely to hold for firms starting from different levels.[24] Importantly, we show that the parallel trend assumption holds for most outcomes. Moreover, we also provide results from a specification that includes firm-specific linear trends to validate our findings.

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

5.4 Descriptive Statistics

Table 2 provides an overview of the characteristics of firms in the control and treated groups. The control group consists of 15,225 firms, while the treated group comprises 58,387 firms. On average, the size of firms in the control group is 9.3, while it is 14.1 for those in the treated group. Firms in the control group are more likely be located in the Lisbon metropolitan area, offer higher wages, exhibit higher median total sales, and have higher productivity, as measured by sales per worker or sales per hour. This reflects that more productive firms were covered by collective agreements that had shorter hours even before the 1990s, or they had a larger share of jobs and occupations, such as office workers, for which the legislation already specified shorter standard hours. Regarding the sector composition, control group firms have a higher proportion in retail sectors, and a smaller share in manufacturing or the hotel and restaurant sectors.

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

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

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

5.5 Results

Figure 4 presents the results on the dynamic effects of the reform on firm-level outcomes estimated in the difference-in-differences. It shows the estimated coefficients and their 95% confidence intervals.[25] 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.4.

Hours, monthly salaries and wages. Panel (A) shows that the firms affected by the reform experienced a substantial decrease in the average standard hours. In line with the reform's two-year phased implementation, standard hours were reduced on average by 2 hours from 1996 to 1997 and then up to 3 hours by 1998, with no further decreases afterward. This 3-hour drop corresponds to roughly a 7% decrease in mean standard hours. Panel (B) displays the corresponding decline in average actual hours worked, which also decreased by nearly 3 hours. Although firms theoretically had the option to increase overtime hours (capped at 8 hours per week and 200 hours per year) in response to the reform, Panel (C) reveals that there was no increase in overtime hours. This is likely attributed to the high overtime premium in Portugal at that time, set at 50% for the first hour and 75% for subsequent hours. Before the reform, only 1% of the firms in our sample used overtime hours.

As mentioned in the institutional context, employers were not allowed to reduce monthly earnings when standard hours were reduced. Panel (D) confirms that there was no adjustment in the mean monthly salary following the reform.[26] 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.[27]

[25] Appendix Table A3 provides the corresponding results in regression tables.

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

[27] The legal constraint preventing the reduction of monthly salaries alongside hours technically only apply during the implementation period of the reform, namely, 1997 and 1998. Moreover, firms might have had the chance to reduce the wage growth of treated workers, for example, by exploiting the inflation rate, slightly above 2% inflation rate at that time. However, panel (E) shows that this was not the case, as the wage increase remained permanent, with only a slight decrease from 1998 to 2000. Several explanations could account for this. Firstly, collective bargaining may

The results are presented in regression form in Table 3, where we present the average effect for years after 1997. The coefficients in columns (1) and (2) indicate that, relative to the pre-reform periods, mean standard hours and mean actual hours were, respectively, 2.757 and 2.425 hours lower for treated firms in comparison to the control group. There is no effect on overtime or monthly salaries. The average effects on the hourly wage rate is 0.061 as shown in column (5).

Employment and total hour input The effects of the reduction in working hours on employment are shown in Panel (F) of Figure 4. There is a small pre-existing increasing trend for treated firms relative to the control group before 1996, which is reversed in 1997, when the reform kicks-in. Post-reform coefficients suggest a negative impact on employment, in line with theoretical predictions of increased labor costs. Note that this effect is not driven by firm exits, as shown Appendix Table A6. 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 reduction in standard hours can be regarded as a shift in the wage schedule, as depicted in Figure A3. The estimated coefficients can therefore provide an estimate of the labor demand elasticity in terms of hours and employment. Taking the ratio of the coefficient on wages to that on total hours, the demand elasticity for hours points to -1.48 (=-0.09/0.061). This magnitude, while relatively large, is in line with Hamermesh (1996) who finds the elasticity of overtime hours of -0.76 to -1.09. but argues that these elasticities might still be underestimated. The labor demand elasticity in terms of employment is -0.33 (=-0.02/0.061), which is also within the range of the literature.[28] It is important to emphasize that the wage increase in our study impacted a large number of workers located at various points in the wage distribution and across different types of workers. This is an advantage relative the literature that focuses on specific groups of workers or

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 & 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.

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

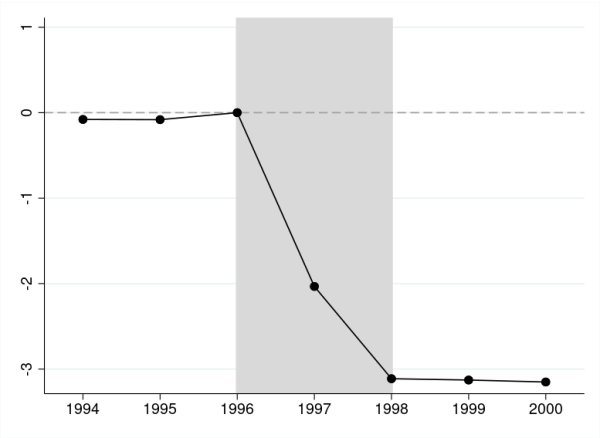
those located at the lowest end of the wage distribution, as in the case of the minimum wage studies. In Appendix Section A.4, we show that the negative effects on employment are relatively smaller for firms operated in the concentrated labor market.

Sales and hourly labor productivity Our dataset includes information on nominal sales, allowing us to examine the impact of reduced working hours on firm-level output and productivity. This offers new insights into existing research, shedding light on the potential role of scale effects on employment. Panel (H) reveals a noticeable and negative effect of the reform on the nominal sales of treated firms, resulting in a cumulative decrease of approximately 6% by 2000. Given the larger negative impact on sales compared to employment, we observe a modest but significant decline in per-worker sales in Panel (I). However, the reduction in sales is not as substantial as the decrease in total labor hours illustrated in Panel (G). Consequently, the reduction in working hours significantly improved hourly labor productivity, as measured by sales per hour in Panel (J). This indicates that the increase in labor productivity partially offsets the negative effects on sales. Table 3 in column (8) reveals an average reduction of 4% in nominal sales for firms for all the post-reform years. Per-worker sales decreased by 1.8%, as shown in column (9). However, column (10) confirms a notable improvement in hourly labor productivity of 4.4%, as shown by the positive effect on sales per hour.

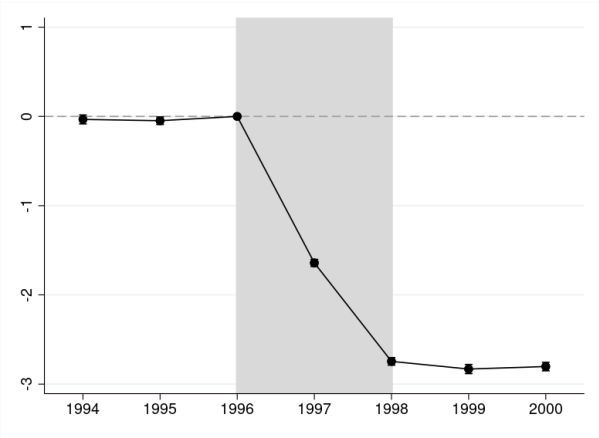
In summary, our results indicate that the significant cut in standard hours, coupled with a substantial increase in hourly wages, caused treated firms to operate at a new equilibrium with reduced input and output levels compared to controls firms over the same period. These results support the theoretical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

Figure 4: Dynamic Effects of the 1996 National Reform

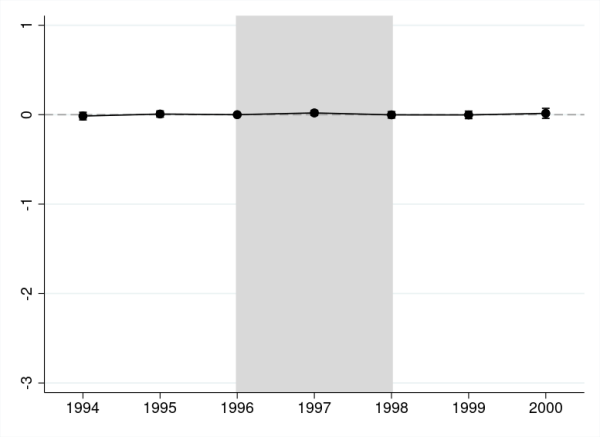
(A) Mean Standard Hours



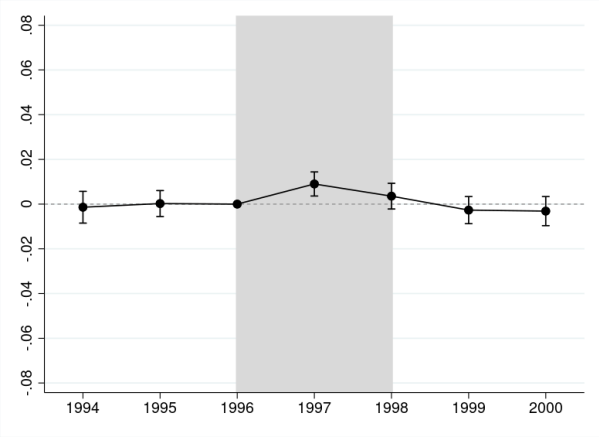
(B) Mean Actual Hours



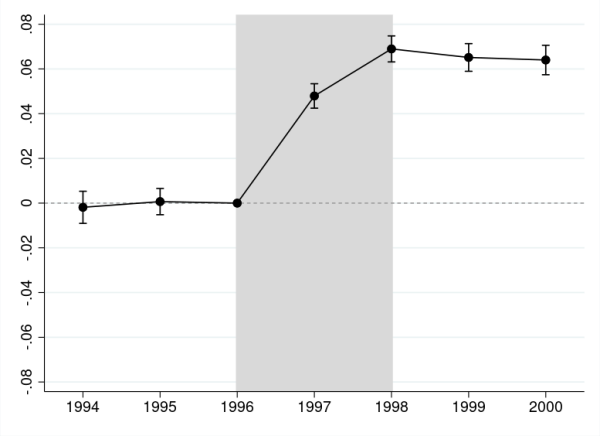
(C) Mean Overtime Hours



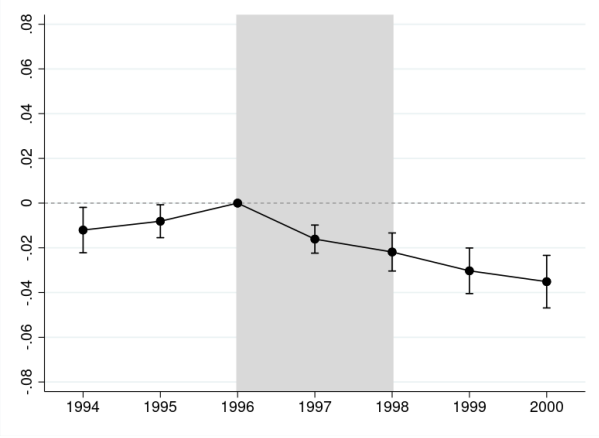
(D) Mean Monthly Salary



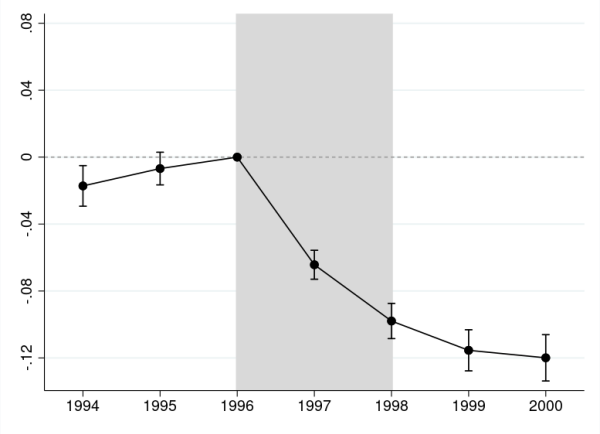
(E) Mean Wage



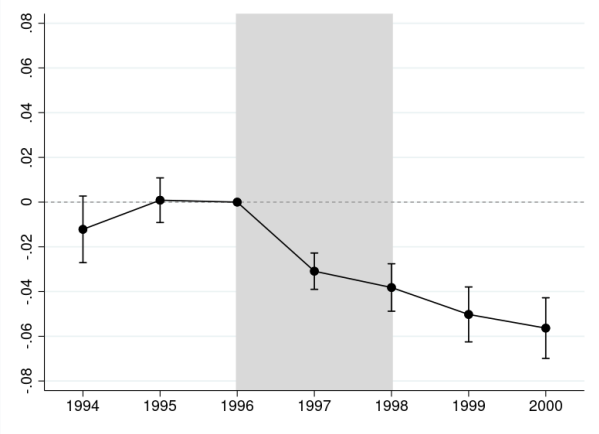
(F) Employment



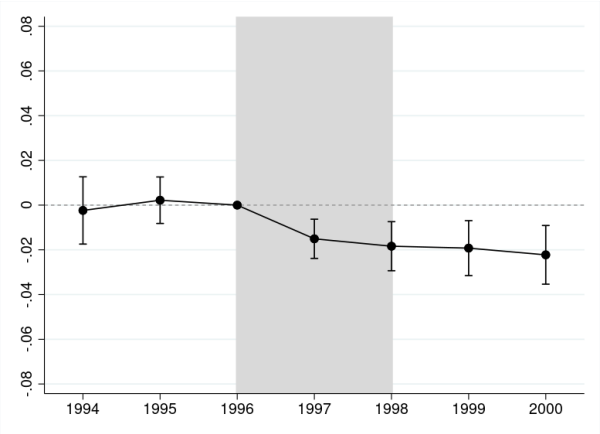
(G) Total Hours



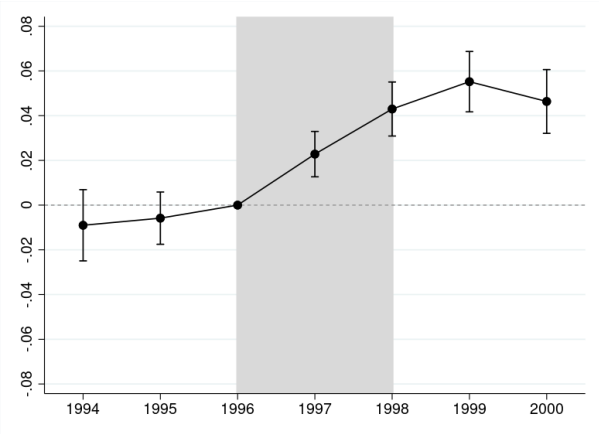
(H) Sales



(I) Sales per Worker



(J) Sales per Hour



Note: The figures display the treatment effects of the 1996 reform on firm outcomes, estimated using a difference-in-differences method as specified in Equation 2, providing both point estimates and confidence intervals. When confidence intervals are not visible, it indicates high precision of the estimate. The outcomes are expressed in absolute values for hours and in log for the other outcomes. The coefficients are normalized to zero in 1996. The shaded area represents the reform period during which standard hours were reduced from 44 to 40 over two years. Treatment status is based on the average share of hours treated from 1994 to 1996, as defined in Equation 1. Treated firms are defined as those with a positive value in the share of hours treated, while the control group consists of firms with a share of hours treated equal to zero. All regressions include firm fixed effects and sector-year fixed effects, with standard errors clustered at the firm level. Appendix Table A3 provides the corresponding results in a regression table.

Table 3: The Effects of the 1996 National Reform: Average for Post-Reform Years

(a) Hours and Labor Cost

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

(b) Labor Input and Sales

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

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

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5.6 Employment Effects: Worker Flows

One of the key findings is that firms reduced their employment in response to the mandated reduction standard hours and the subsequent increase in labor costs. Firms have two ways for adjusting their workforce: letting incumbent workers go or refraining from hiring new ones. To differentiate between these two channels, we leverage the worker-level dataset to track the number of workers who left and joined each firm each year. Then, we calculated the proportion of workers who sepa-

rated from and were newly hired by the firm relative to the previous year's employment size. The estimated effects on worker flows are presented in Figure 5.[29] 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). This finding indicates that firms adjusted their total employment size by refraining from new hiring rather than firing incumbent workers. Strict regulations on dismissals in Portugal explain why firing was not the main adjustment mechanism. In fact, the separation rate decreased, likely due to fewer voluntary quits as workers could enjoy shorter hours without a decrease in monthly income.

The overall reduction in worker flows goes in the opposite direction to the prediction that employers would seek to substitute workers hired under the previous standard with new ones under the new standard (Crépon & Kramarz, 2002).[30] This result is likely a consequence of the low flexibility of the Portuguese labor market, where reducing new hiring is a more feasible adjustment mechanism for firms. Furthermore, these findings emphasize the importance of conducting the analysis *at the firm level*. Worker-level analysis, as seen in studies such as Raposo and van Ours (2010) and Crépon and Kramarz (2002), would only capture the separation aspect, missing the impact on new hiring, which we found to be the primary driver behind the negative employment response of firms.

5.7 Mechanisms of Labor Productivity Gains

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

[29] For the corresponding regression table, see Appendix Table A4. We estimate the average effect on these worker flows in difference-in-differences methodology, interacting the treatment group dummy with a post-1997 dummy, as specified in Equation 3. Due to the existence of the pre-existing trend in worker flows, we also include firm-specific linear trends to ensure our estimates are not driven by differential trends. Since the outcome measures the change from one year to the next, the data points where the firm does not exist in consecutive years are excluded from the estimation.

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

Figure 5: The Effects on Separation and New Hiring



Note: The figure shows the effects of the 1996 reform on worker flows at the firm level. The outcomes are share of workers separated and share of new workers hired, relative to the previous year's employment size. The regression employs data from the years 1994-2000 and difference-in-differences comparing treated and control firms. We estimate the average treatment effects interacting the post-1997 dummies with the treatment dummy variable (as in the equation 3), and add firm-specific trends to deal with the potential pre-existing trend observed for the outcomes. See Table A4 for the corresponding regression table. Standard errors are reported in parentheses; * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

the plausibility of some of these mechanisms.

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

We study the price effects by matching the QP with the yearly output price index at the 2-digit sector level available in the EU KLEMS database.[31] The underlying idea is that if price adjustment is the primary mechanism for firms to maintain their nominal sales, sectors with a higher

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

Table 4: Sector-level Effects on Capital and Price

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

Note: The table illustrates the impact of the 1996 reform on sector-level price levels, labor services, and capital services. Each observation represents a sector, with the sample consisting of 25 sectors spanning manufacturing, wholesale, retail, and hotels and restaurants. Mean actual hours in the first column come from the QP. Data for price index, labor services, and capital services in the second to fourth columns come from the EU KLEMS database. Treated sectors are those with average treated hour shares above the median, as defined in Equation 1 at the sector level, while control sectors are those at or below the median. The specification is based on Equation 3, adding time fixed effects, sector fixed effects, and sector-specific linear trends. Standard errors are in parentheses; * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

proportion of firms affected by the reform should experience more significant growth in prices following the reform. To investigate this, we calculate the average of firms' treated hour share (as defined in Equation 1) in each of the 25 2-digit sectors, weighted by each firm's sales share within the sector before 1996. We compare industries with an above-median and below-median exposure, respectively, and estimate the treatment effects using a difference-in-differences approach. The results are presented in Table 4. Column (1) shows that, on average, more treated industries reduced actual hours worked by approximately 1.3% in the QP. Although the coefficient on the price index (in log) is positive in column (2), it is small and does not, at least fully, explain the increase in labor productivity.[32] In the Appendix, we also present additional tests examining the heterogeneity in productivity effects across firms operating along varying degrees of product market concentration (Figure A13). We do not find a larger increase in sales per hour in concentrated product markets where firms can more easily raise prices. This evidence further supports the idea that price increases were not the primary mechanism for firms to boost nominal productivity.

[32] When we adjust nominal sales using the sector-specific price index and re-estimate the treatment effects at the firm level as in 2, we still observe a significant increase in (real) sales per hour of similar magnitude.

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.

Since there is no information on capital available in our data over the period we study, we again examine sector-level capital information from the EU KLEMS. Identical to the approach for prices, we compare more treated sectors to less treated sectors as measured by the average of firm-level hours share treated. Table 4 provides the results. First, column (3) shows the decrease in labor services due to the reform in the EU KLEMS database. The coefficient is of similar magnitude to the decrease in hours in the QP in the first column, although it is insignificant due to imprecise estimation. In column (4), there is an insignificant but *positive* coefficient on the estimated effect on capital services, which aligns with the idea that capital could have partially substituted for labor. Moreover, the effect on capital services matches in magnitude with the decrease in labor services. Table A5 in the appendix separately estimates the impact on capital services for non-ICT and ICT capital and shows a larger effect on ICT capital.

In line with this evidence, we posit that, in the short run, intensifying capital use is likely easier for firms that already use capital in production. Indeed, as shown in Appendix Figure A14, we observe larger negative employment effects and higher productivity gains in firms operating in capital-intensive sectors. This supports the notion that firms substituted capital for labor, leading to increased hourly labor productivity.

Production function concavity. One simple explanation for the increased hourly labor productivity is that the production function is concave with respect to labor hour input, an assumption made in standard economic models. Under such assumption of decreasing marginal productivity, even if working hours are reduced from the equilibrium level, the decrease in output is proportionally smaller, leading to a higher average output per hour. Some research has found evidence of such concavity, focusing on specific occupations and tasks and examining the individual-level re-

lationship between output and hours (Brachet et al., 2012; Collewet & Sauermann, 2017; Pencavel, 2014). These studies often emphasize the role of fatigue as a source of decreasing productivity. Our findings offer support for the presence of such concavity in production, but within a more generalized context.

Work intensification. Another potential explanation is that work intensity increased, leading to higher labor productivity per hour. This could occur, for instance, if workers exert more effort (Green & McIntosh, 2001; Lazear, Shaw, & Stanton, 2016). In certain contexts, with a higher level of effort per working hour, workers may complete the same tasks in a shorter amount of time. This hypothesis is also related to the concept of efficiency wages (Stiglitz, 1976), as the wage rate increased for workers affected by the reform, which could incentivize them to exert more effort. As highlighted by Askenazy (2004), work intensification can also result from changes in work schedules, such as the change in work time or the introduction of more precarious work arrangements across different days and weeks. However, as shown in Table A11 and A12 in the Appendix, the proportion of workers engaged in shift work, night work, or weekend work in the Labor Force Survey did not increase over the period of the reform. Moreover, Lepinteur (2019) examined subjective well-being and found that workers impacted by the 1996 Portuguese reform experienced increased job satisfaction, suggesting that they might not have been put under pressure to make up for the lost hours.[33] While it remains inconclusive, this evidence is rather inconsistent with the mechanism of work intensification.

Improvement in worker quality. The increase in labor productivity may be attributed to changes in the composition of workers. Firms could have sought to increase their average worker quality to maximize the output for each unit of labor input. Table 5 displays the effects of the reform on worker composition. As a proxy for worker quality, we use workers' education level recorded in the data. Column (1) shows that the share of workers with a high school diploma or higher did

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

Table 5: Effects of 1996 National Reform on Worker Composition

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

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

not increase, and similarly, column (2) provides no evidence of an increase in the share of college graduates. This suggests that the improvement in labor productivity is likely a result of efficiency gains within a given set of worker qualities rather than a change in the composition of worker qualities. Finally, in columns (3)-(5), we also test the age and gender composition of workers and find no significant changes in these demographic characteristics.

In summary, the increase in labor productivity may arise from multiple sources simultaneously. First, it is consistent with the concavity of the production function. There is a lack of strong evidence to suggest work intensification or changes in worker composition. Beyond these mechanisms related to labor input, we find indirect but consistent evidence of capital substituting for labor hours in sector-level analysis. We do not find evidence that firms are increasing nominal sales per hour through price increases, indicating that these productivity gains are realized in real terms.

5.8 Heterogeneous Effects

We present additional heterogeneity in the effects on employment, sales and output in Figure A15. We focus on dimensions related to firm quality (measured as pre-reform mean wages), firm size, and sector. In brief, the initial quality of firms does not appear to be a significant source of heterogeneity; the coefficients for all three outcomes are relatively similar. Conversely, smaller firms with fewer than 10 employees displayed the smallest negative impact on employment. This could be attributed to difficulties in adjusting the workforce size, where each worker's contribution is more significant. These smaller firms also experienced a relatively substantial decline in sales. In terms of sectors, the manufacturing sector did not reduce employment in response to the reform, while the hotel and restaurant sector was most severely impacted in terms of both employment and sales. Notably, it was also the only sector where no increase in hourly labor productivity is observed.

6 Collective-Agreement Reforms

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

This estimation is built on the long-term institutional context described in Figure 1. In a difference-in-differences approach, we compare the outcomes of firms affected by the national reform or collective agreements with those firms that had lower working hours since the 1980s. This approach aligns with the previous section in spirit but with several distinctions: i) the studied period covers a longer duration, from 1986 to 2000; ii) the control firms exclusively comprise those with consistently low hours throughout the entire period; and iii) treated firms are divided between

those first impacted by a collective agreement reform ("early adopters") and those only impacted later by the national reform ("late adopters").

6.1 Treatment Definition

Data preparation. Our data lacks information on standard hours before 1994. Therefore, we impute standard hours from actual hours worked in the month, following a methodology outlined in Appendix Section A.2.3. The objective of the procedure is to obtain hours closer to standard hours that are more robust to variations in the number of working days in the reference month across different years.

To identify firms treated through collective agreements, we exploit collective agreement codes associated with each worker in our dataset. These codes can change when a collective agreement is renewed, split, or merged with another. While many codes remain consistent throughout the period, a substantial portion (roughly 40%) undergoes a change at least once over time. To address these inconsistencies, we leverage the worker-level panel structure and track the entire flow of collective agreement codes. We assign a new consistent code for the collective agreements that underwent changes in the code. In cases of splits or merges, we use a snowballing approach: we combine these agreements and assign a new code. This allows us to reconstruct a comprehensive, balanced panel of collective agreements covering the studied period. Appendix Section A.2.4 provides a detailed explanation of this process. In our final sample, there are a total of 164 newly defined collective agreements.

Defining groups. We first restrict our sample to firms that we observe from the beginning of the sample period in 1986. As a control group, we use firms that were less affected by the process of reducing working hours due to having low working hours from the outset. We define this control group as firms in the lowest quartile of average standard hours between 1986 and 1996. We use the bottom quartile of firms as our threshold, rather than a fixed 40-hour mark, to expand the size of the control group. It allows us to include sector-year fixed effects in the estimation and improve

precision in the estimates.[34] In this way, our control group includes both firms that were entirely unaffected by the reduction process and those that were *less* affected. Additionally, we base our group definitions on average standard hours from 1986 to 1996, covering a longer time span rather than just the period before 1991, in order to minimize the impact of mean reversion.[35]

We define collective agreement treated (CA-treated) as those not in the control group and covered by collective agreements that reduced the *mode* of standard hours by at least 2 hours during the period 1991-1996. The year in which the mode declined or when the proportion of workers at the mode dropped by 10 percentage points is considered the initial treatment year.[36] It is important to note that it is an upper-level bargaining process that negotiated the reduction of working hours, rather than individual firms and workers. Firm-level agreements are rather rare in Portugal. The collective agreement usually overlaps, at least, with a well-defined economic sector and only sometimes is declined differently across geographical areas (districts or municipalities). With the extension rule approved by the government, which often happens in practice, it covers all firms and workers within its scope.

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

Descriptive Statistics. Table A11 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,

[34] 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.

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

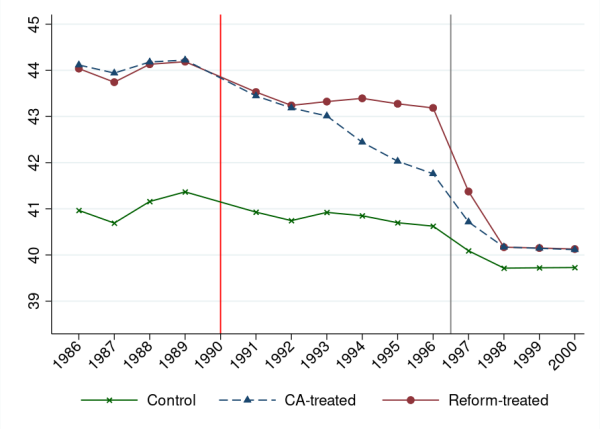
[36] In a few cases, negotiations for collective agreements may have started in 1990, and the treatment began in 1991. To identify these situations, we calculate the mode of standard hours before 1989 (rather than just for 1989 to avoid potential mean reversions). If the mode of hours before 1989 was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

larger, pay higher wages and record more sales. Firms affected by collective agreement changes are more similar in terms of observable characteristics to those that are impacted by the reform. Nearly half of the firms affected by collective agreement reforms are located in the Lisbon metropolitan area, which is similar to firms in the control group. In terms of the sectoral distribution, collective agreement that autonomously change hours are concentrated in the manufacturing and wholesale sectors. Appendix Figure A16 shows graphically the distribution of the treatment groups across sectors.

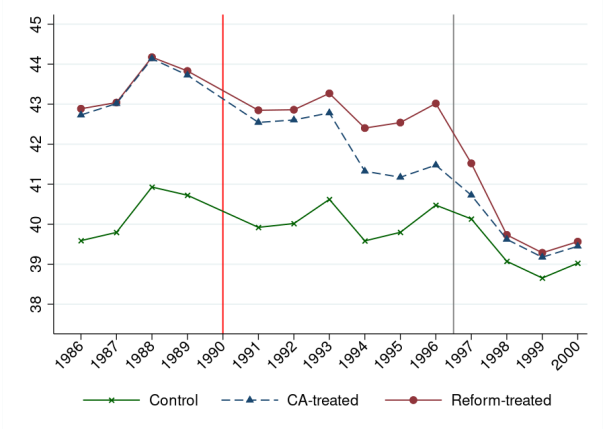
Figure 6 shows a clear distinction in the evolution of hours across the three groups of firms. The control group exhibits relatively stable hours around 41 hours, with a slight decline after the national reform in 1996 to below 40 hours. In contrast, the blue and red lines depict the separate evolution of firms affected by collective agreements and those only impacted by the national reform, respectively. Firms affected by collective agreement reforms started reducing hours earlier, having 42 hours in 1996. Firms impacted by the national reform maintained stable hours during the bargaining period, but their hours reduced significantly from around 43 to 40 hours after 1996.

Figure 6: Evolution of Hours Across Treatment Groups

(A) Mean Standard Hours



(B) Mean Actual Hours



Note: The figure shows the evolution of the average firm-level mean hours for the three groups: Control, CA-treated, and Reform-treated. The Control group comprises firms in the lowest quartile of average standard hours between 1986 and 1996. CA-treated firms are those not in the Control group and are in collective agreements where the mode of standard hours changed by at least 2 hours between 1991 and 1996. The remaining firms are Reform-treated firms. Panel (A) shows the evolution of standard hours, while panel (B) shows that of actual hours worked. These mean hours are computed from workers who had at least 30 hours in the respective hour category. The large fluctuations in actual hours are due to the variations in the number of working days within the reference month across different years, as shown in Appendix Figure A9.

6.2 Estimation method

Similar to Daruich, Di Addario, and Saggio (2023), we employ a staggered difference-in-differences approach that takes into account the differential timing of hour reductions across collective agreements. Most of these agreements began to reduce standard hours by 1993 (roughly 80%). Appendix Table A13 summarizes the number of firms and collective agreements according to the first year of the reduction in hours. To account for the different starting years of treatment, we employ the estimator suggested by Callaway and Sant'Anna (2021) and avoid comparisons with earlier-treated units that could confound the estimates. Some collective agreements did not fully reduce to 40 hours by 1996 and were subject to further reduction in hours due to the 1996 reform. For this reason, we study only up to 1996 to isolate the effects of collective agreements.

Treated firms are all those that reduced standard hours by collective agreements. Only firms in the control group, i.e., with consistently low hours throughout the period, are used as control

units. However, including not-yet-treated units in the control unit does not alters our results. Firms treated only by the national reform in 1996 are not part of the control group.

We present below the results on the effects of reducing working hours through collective agreements. Table 6 provides the average treatment effects. In Appendix Section A.5, we also present the re-estimated results for firms treated by the 1996 national reform and not by collective agreements. Overall, the results are qualitatively similar to those presented in the previous section.[37]

6.3 Results of Collective Agreement Reform

Hours, wages and salaries. Figure 7 shows the effects of the collective agreement treatment on hours, wages, and monthly salary. Panel (A) shows that, compared to the control group, mean standard hours were gradually and stably reduced. Cumulatively, treated firms reduced hours by approximately 1.5h after 5 years, equivalent to roughly a 4% reduction. Note that the reduction in hours is relatively small for two reasons: first, not all collective agreements fully reduced hours to 40 by 1996 but only, for example, to 42. Second, in our definition, a part of the control group firms are "less treated" with on average higher hours than 40. Actual hours worked per week shown in panel (B) display a similar pattern.[38] Panel (C) shows that there was no response in terms of overtime hours. Panel (D) confirms that the monthly remuneration of workers at the collective bargaining-treated firms were not reduced.[39] Finally, Panel (E) shows that the mean wage rate of the treated firms increased by about 4% after 5 years since the treatment, relative to the control group.

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

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

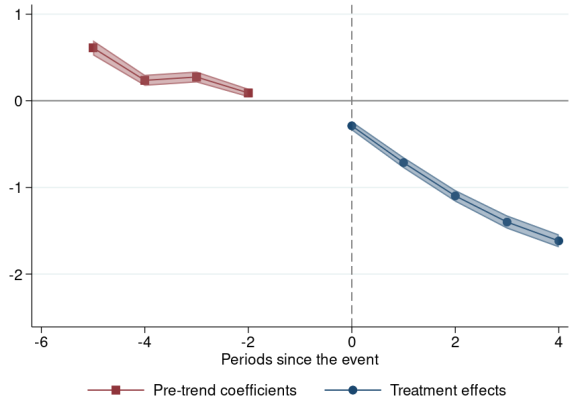
[39] The figure shows that, if anything, there was a small immediate increase in salaries at least for the first two years since the reform. This is likely due to the (periodical) negotiation on wages that might have been simultaneously bargained upon in the process.

Employment, sales and productivity. Panel (F) shows that firms treated by the collective agreement and control firms had similar employment trends before the treatment. Overall, we estimate no significant effects on employment during the gradual reduction in working hours. This contrasts with the finding of the negative employment effects of the 1996 reform. With stable employment levels and decreasing per-worker hours, total hours per firm decreased by close to 4%, as shown in panel (G).

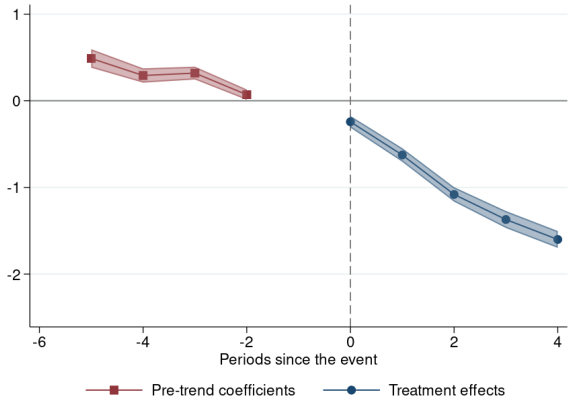
The output measured by nominal sales in panel (H) is not significantly impacted by the treatment. Absent any price adjustment, this would indicate that firms could maintain the level of production with smaller number of working hours, which has significant implication for productivity. Panel (I) confirms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

Figure 7: Effect on Hours, Wages and Salaries of Collective Agreement Reforms

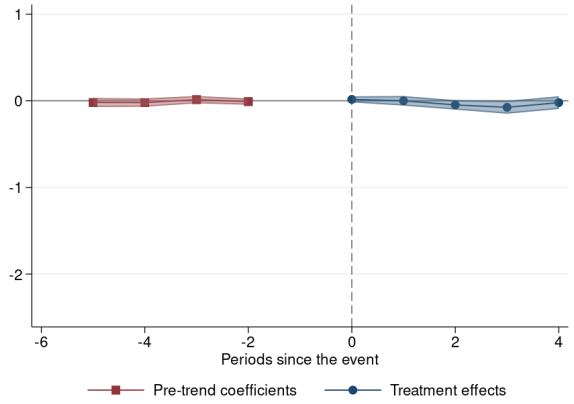
(A) Standard Hours



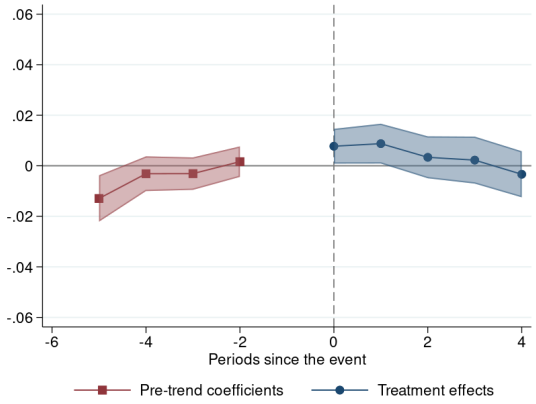
(B) Actual Hours



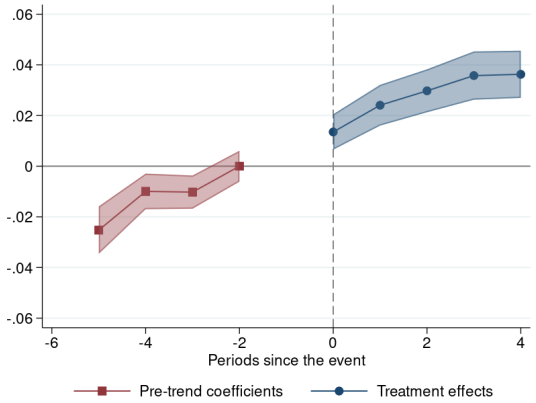
(C) Overtime Hours



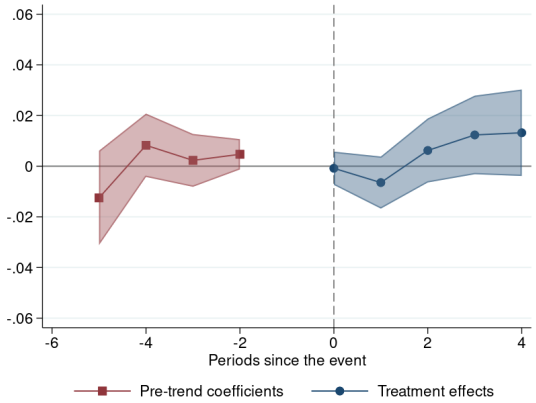
(D) Mean Monthly Salary



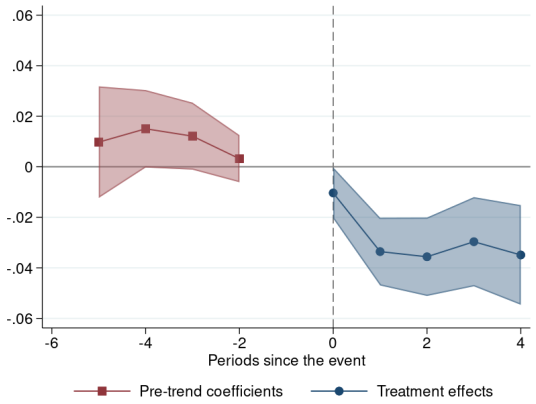
(E) Mean Wage



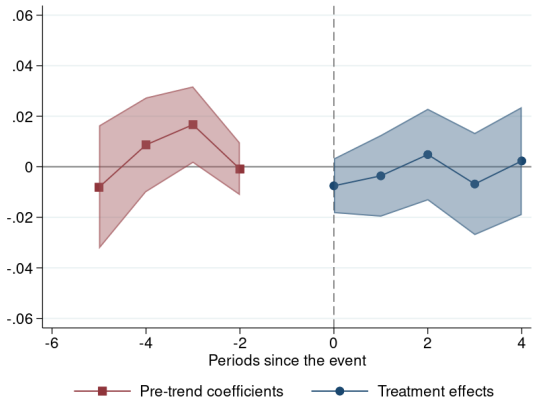
(F) Employment



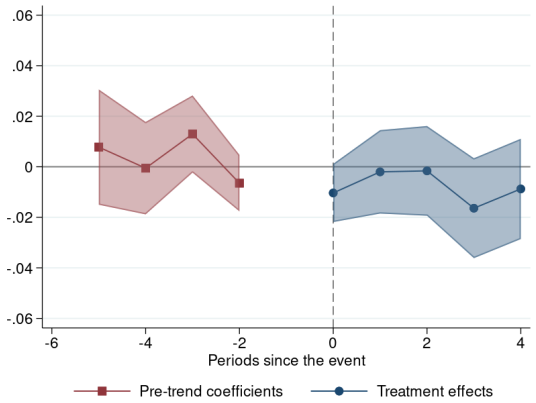
(G) Total Hours



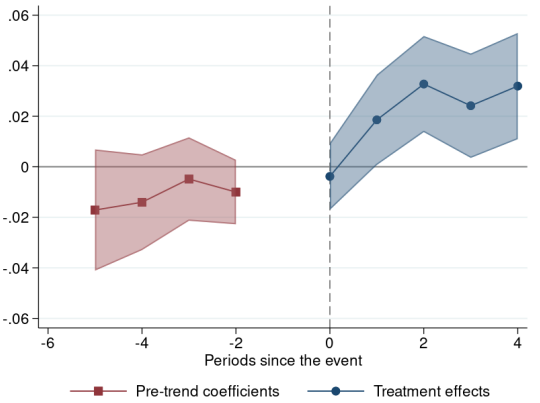
(H) Sales



(I) Sales per Worker



(J) Sales per Hour



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

Table 6: Average Treatment Effects of Collective Agreement Reforms

(A) Hours and Labor Cost

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

(A) Labor Input and Sales

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

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

6.4 Summary and comparison with the results of the 1996 reform

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

Despite a substantial reduction in working hours per employee, firms did not increase employment to keep the same total amount of hours of production. As a result of the 1996 reform, both

employment and sales were negatively impacted by the reduction in hours and the increase in labor cost. These firm-level results provide empirical support for the theoretical prediction that stricter limits on working hours may lead to negative scale effects resulting in decreased labor demand. On the contrary, for firms treated earlier through collective agreements, which also experienced a decrease in hours and increase in labor cost, the effect on employment and output is not distinguishable from zero.

There are several potential explanations for why firms treated through collective agreements exhibited less negative effects on employment and sales. First, the size, timing and process of treatment were different. In our estimation, the first-stage effects on hours were slightly smaller for the collective agreement treatment, and the reduction in working hours was also gradual. Similarly, the symmetric positive effect on wages was smaller, roughly half that induced by the national reform. Second, since the collective agreement was a voluntary bargaining process, sectors that would incur fewer costs from reducing hours may have been more inclined to bargain the reduction. This hypothesis resonates with the comparison of productive characteristics presented in Table A11: firms treated through collective agreements tend to have slightly higher wages and are more productive in terms of sales per hour and per worker compared to those treated later by the reform. More productive firms or sectors may better adapt to the reduction in hours through organizational restructuring, improved work practices, and other measures. Third, the endogenous process of collective agreements may have considered the anticipated growth in product demand. Firms facing an expected decrease in future demand might find it difficulty and costly to reduce their workforce size in a regulated labor market like Portugal. In such scenarios, reducing working hours can serve as an effective means to decrease labor input without reducing overall employment levels.

Finally, an important aspect of the different effects observed between firms treated by the national reform and those treated through collective agreements relates to the different dynamics of the impact on wages and productivity. In the case of the national reform, the effect on wages (+6%) is only partially counterbalanced by an increase in hourly productivity (+4%), resulting in a 2% decrease in employment. Conversely, in the case of collective agreement, the wage increase

is entirely offset by a similar increase in productivity (both roughly +3%), with no adverse impact on employment. This suggests that when wage increases are matched by a corresponding rise in hourly labor productivity, employment levels might not need to adjust. Conversely, when wages increase more than productivity, firms are less inclined to hire new workers. The comparison of the treatment through the reform and through collective agreements demonstrates that the effect on employment, therefore, depends on the ability of firms to match higher wages by hourly productivity gains through different mechanisms.

7 Conclusion

This paper investigates the impact of a reduction in working hours at constant monthly salaries on firms. Specifically, we investigate the Portuguese transition to the 40-hour week, by focusing on a reform that reduced standard hours from 44 to 40 hours in 1996, one of the few instances where this occurred without any compensating measures for firms. We show that firms affected by the national reform experienced a decline in both employment and sales compared to non-affected firms, mostly as a result of the increased wage rate. However, the total labor input (i.e., employment times hours) of treated firms decreased more than proportionally relative to sales, resulting in a significant increase in hourly labor productivity.

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

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

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Online Appendix

A.1 Literature

Table A1 summarizes the literature on the employment effects of working hours reductions, sorted by reform and level of analysis (workers, firms, sectors, regions). Overall, while there are well-identified but conflicting estimates of the impact of a reduction in working hours on *incumbent* workers, evidence of the effect on labor demand is scarce. Crépon and Kramarz (2002) find that, in France, a reduction in standard time from 40 to 39 hours in 1982 increased the probability of *incumbent* workers being fired.[40] 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.[41] Between 2001 and 2005 in Chile, Sánchez (2013) finds no impact on employment transitions for a reform that allowed for a 4-year adjustment period. Estevão and Sá (2008) look at aggregate employment in large versus small firms in France, which, while impacted by the reduction to 35 hours at different times, have no visible difference in their employment dynamics.

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

[40] This does not imply that the total employment effect at the firm level is negative, as it does not take hirings into account.
[41] Separation rate decreases for workers directly impacted by the reform, but increases for workers indirectly impacted.

impacted on firms' employment dynamics was the preliminary work by Varejao (2005), finding zero or negative employment effect depending on the specification.

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

A.2 Data Appendix

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

A.2.1 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 (separately for base remuneration, regular additional remuneration, and irregular remuneration); *hours month*: worker's hours in March (before 1993), or October (after 1993); *hours extra*: worker's overtime hours in March (before 1993), or October (after 1993); *hours_week*: usual working hours (after 1993)

A.2.2 Labor Force Survey

Years available: 1985 to 2016

Worker-Level

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

in a given week.

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

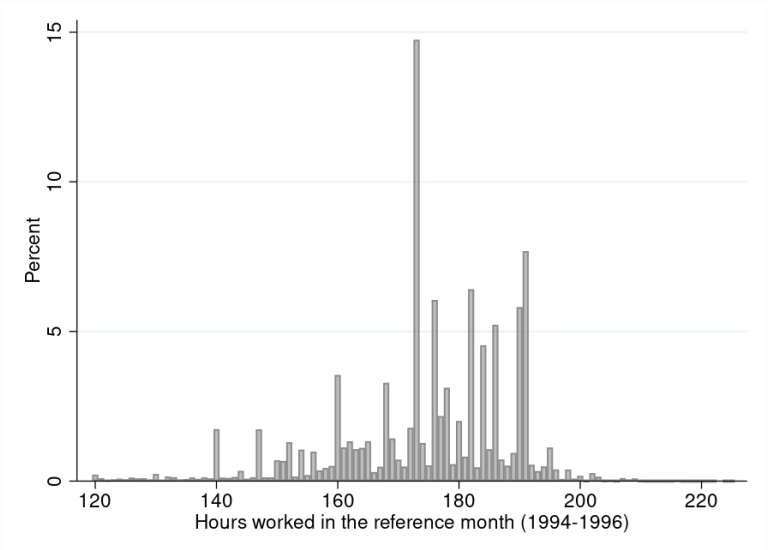
A.2.3 Recovering standard hours in the QP before 1994

As mentioned in the Section 4, the information on standard hours is available in the QP only since 1994 (while actual hours worked in the reference month are always available). Therefore, for workers with missing information on standard hours, we impute them using the method outlined below.

To recover the standard hours of the workers, we use the fact that actual hours worked in the reference month recorded in the QP is strongly related to specific weekly standard hours. When filling hours information in the reference month, firms often calculate hours worked in the month based on the standard hours. For example, for a worker with standard hours of 40 per week and if the month has 22 work days, firms often report monthly actual hours as 176h (i.e., 8h per day multiplied by 22 days). In other cases, firms may multiply per-day standard hours by 21.625, the average number of working days per month, or simply by 20, corresponding to 4 weeks. In this example, these results in the monthly hours of 173h and 160h, respectively.

This pattern is reflected as spikes in the distribution of monthly actual hours recorded in the QP, shown in Figure 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. To do so, we exploit the 1994-1996 period, during which the legal maximum weekly hours were still 44h and the QP records both actual and contractual hours, to identify the most frequent standard hours within each bin of monthly hours worked. Figure A2 shows some examples. In each panel, we plot the distribution of standard hours for workers with particular monthly hours: 140h, 173h, 182h, and 191h. We identify the most frequent standard hours as, respectively, 35h, 40h, 42h, and 44h for the corresponding hours in the reference month.

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

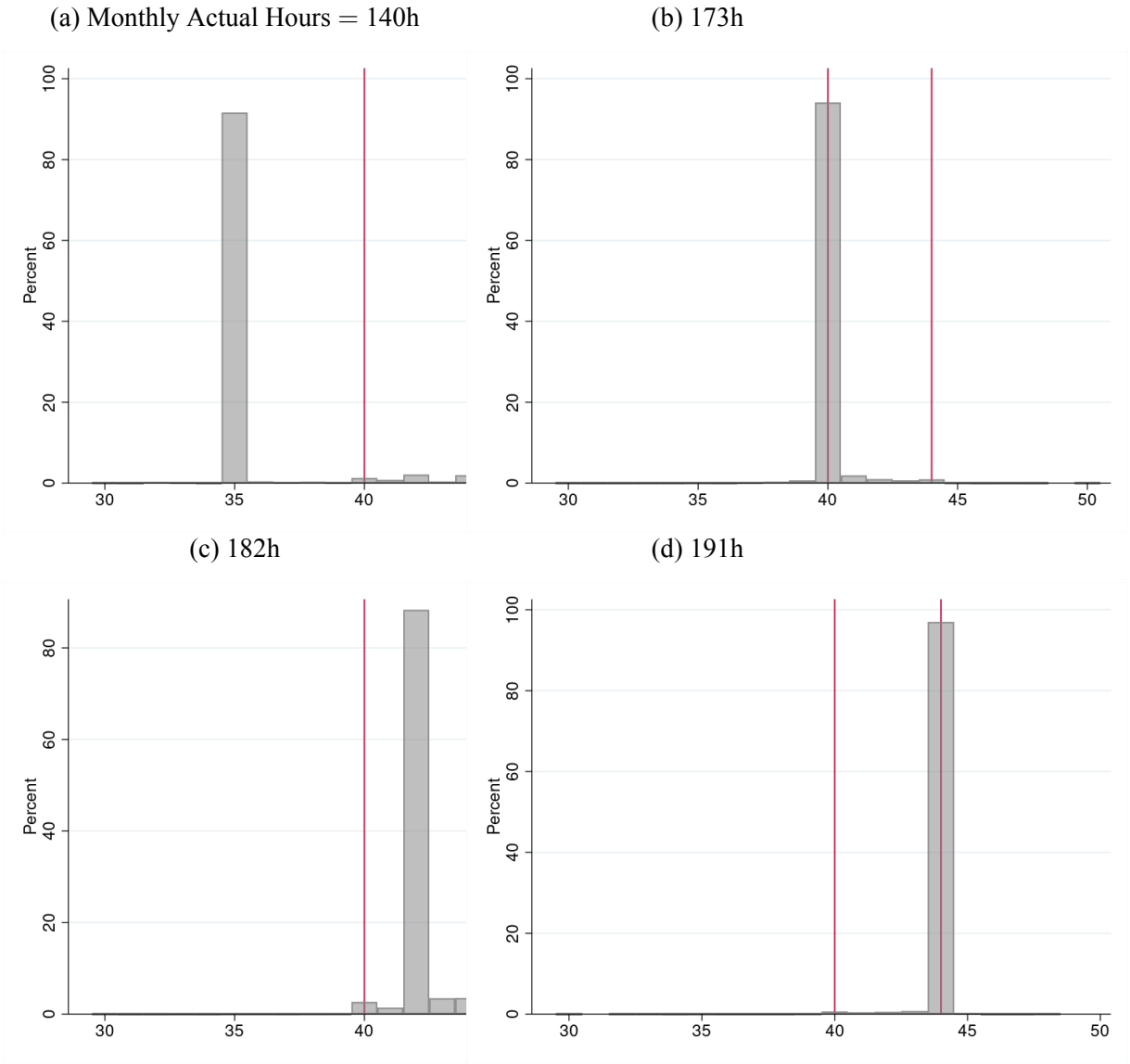


Note: The figure shows the distribution of actual hours worked per month in the QP for years 1994-1996. The distribution displays many spikes associated with particular contractual hours per week.

We run the procedure for each bin of monthly actual hours to create a full correspondence between monthly hours and weekly standard hours. Then, for years before 1994, we assign the corresponding imputed standard hours according to the worker's monthly hours worked.[42] Since the legal maximum standard hours was 45h until 1989, for years up to then we assign 45h (i) in the case of the monthly actual hours that are likely be associated with 45h per week (i.e. 180, 189, 192, 194, 195, 198, 207) or (ii) if the worker's monthly actual hours exceed 202.4 (i.e. maximum number of hours that can be achieved under 44h regime with maximum number of working days per month in the data, 23 days).

[42] 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.

A.2.4 Creating a Panel of Collective Agreement

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

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

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

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

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

(2) Collapse the data such that each row contains a pair of CA in t and CA in $t-1$ observed in the data (called CA_t and CA_{t-1} respectively hereafter), as well as the corresponding number

of workers in each pair.[43] 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 $CA_t = CA_{t-1}$.[44]

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

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

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

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

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

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

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

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

(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.[45]

(4) Merge it with the CW_{t-1} based on the CA_{t-1}.[46]

(5) Do the same step (2) and (3), using the pairs of CA_{t-1} and CA_{t-2}.[47]

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

At the end of this process, there are 341 distinctive new CA ID's containing 637 distinct original codes. Among the 341 new ID's, 55% have a unique corresponding CA (i.e. CA code that existed in all years, the code did not change, and not mixed with other CA's). 92% (97%) of the new ID's have maximum 3 (5) distinct CA's. The largest number of distinctive CA per new ID is 13. These numbers indicates that we successfully created a panel of CAs, avoiding to have a small number of new IDs containing many different CAs, by not using the weak links in the merging process.

[45] 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.

[46] 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.

[47] 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.

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.

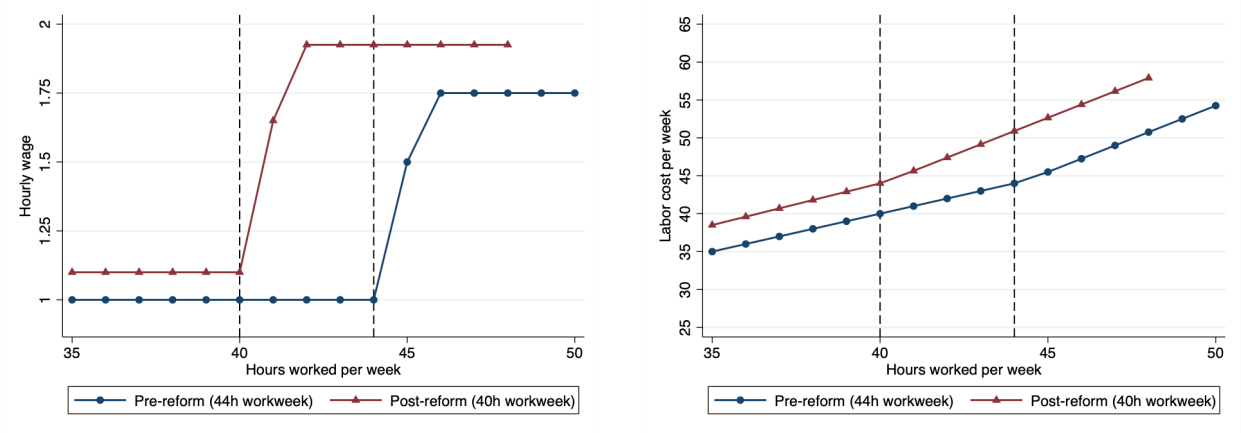
A.3 Supplementary Descriptive Statistics

A.3.1 Impact of the Hours Reduction on Monthly Salary and Hourly Wages

In theory, the reduction in standard hours by the reform essentially changes the threshold hour from which the overtime premium applies. Panel (A) in Figure A3 illustrates the expected change in the hourly wage schedule for workers who transitioned from a 44-hour workweek to a 40-hour workweek, assuming an initial base wage rate of 1. Prior to the reform, the wage rate remains at 1 until the 44th hour, but with the introduction of overtime premiums, it rises to 1.5 for the 45th hour and 1.75 for subsequent hours. Following the reform, the threshold shifts to 41 hours. A crucial aspect of the reform is that employers were not allowed to reduce the monthly salary for workers whose standard hours were reduced. This is depicted by the post-reform base wage rate of 1.1 (=44/40), indicating a 10% increase in hourly labor costs. Panel (B) portrays the corresponding monthly salary. Note that the monthly salary at 44 hours in the pre-reform schedule matches that at 40 hours in the post-reform schedule. To maintain a 44-hour workweek, the employer must pay a 16% higher monthly salary.

Figure A3: 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.

A.3.2 Evolution of Hours

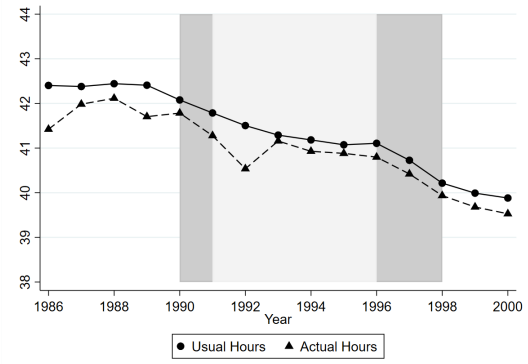
The effects of the reforms are clearly visible in LFS survey data, as shown in Figure A4, where average hours and the share of full-time workers above 40 and 44 hours a week are plotted over the period 1986 to 2004. Hours appear relatively stable before 1990, when, as expected, a decline begins. This 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 A6). 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 through-

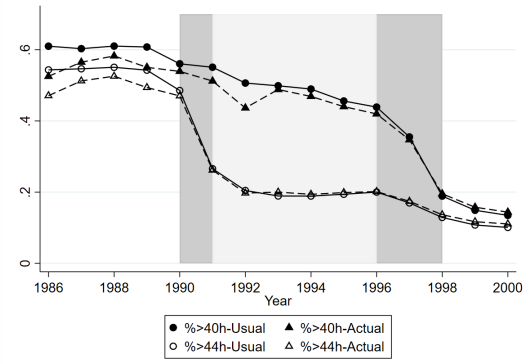
out this paper in Figure A5 (see Figure 2 that use imputed standard hours for years before 1994). In panel (a), there is a progressive decline in the average actual weekly working hours observed throughout the early- and mid-1990s, subsequently leading to a significant reduction following the reform implemented in 1996. Similarly, the proportion of workers performing 44 hours or more demonstrates a corresponding trend, as depicted in panel (b).

Figure A4: 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.

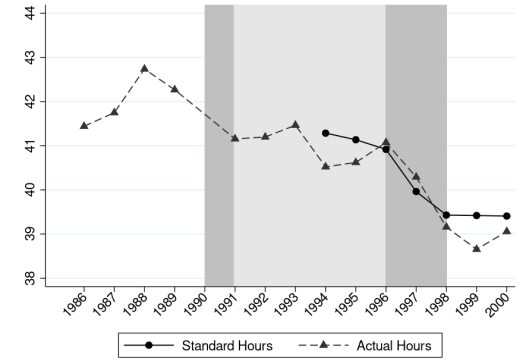
A.3.3 Distribution of Hours

Figure A6 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.[48] From panel (b), in the early- to mid-1990s, working hours shifted to 44h, given the 1990 reduction of standard hours from 45h to 44h. However, the figure

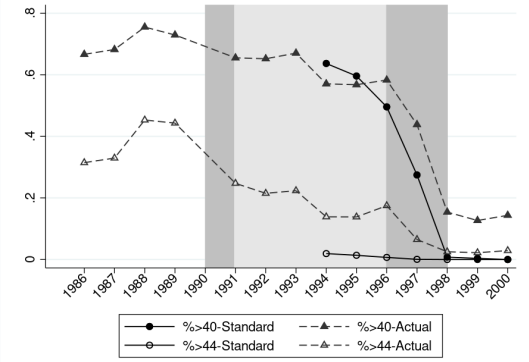
[48] Specifically, the law specified 7 hours per day and 42 hours per week for office workers. The law also specified that employers could increase the daily limit by one hour if an additional half-day or day of rest was provided. Therefore, in practice the daily limit could be increased at 8 hours per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

Figure A5: Average Standard and Actual Hours of Full-Time Workers and Share Above 40 and 44 hours, 1986 to 2000, QP

(a) Average Actual and Standard Hours



(b) Share Above 40 and 44 hours



Note: The panel (a) shows the average standard and actual hours. The panel (b) shows the share of workers with standard hours and actual hours above 40h and 44h, respectively. Standard hours are available in the QP only from 1994. See Figure 2 which replicates the figures with imputed standard hours. Actual working hours were derived by dividing the actual hours worked in the reference month by 21.625, which is the average number of working days per month. The darker shaded area indicates the period of the national reform, while the lighter gray indicates the period of reform of collective agreements.
Source: Authors' calculations based on the QP.

indicates that many workers responded to work 45h. It is perhaps due to misreporting or overtime hours (which were calculated over the average of 3 months – thus some workers could have worked more than 44h in some weeks). Note also that there is more mass at 40h because some collective agreements reduced standard hours during this period. Lastly, panel (c) indicates that most workers moved to 40h following the reform in 1996.

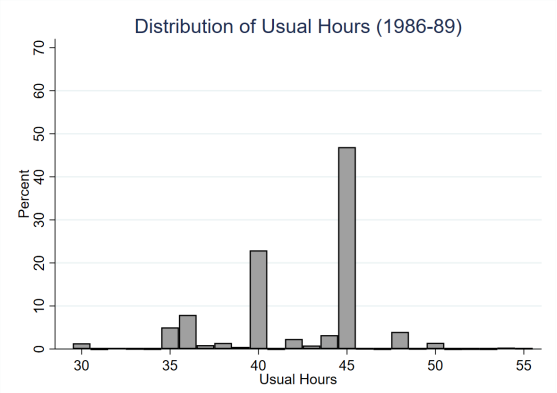
Figure A7 shows the distribution of standard hours recorded in the QP, the dataset used in our estimations. The standard hours before 1994 are imputed. The figure also exhibit similar shift in the hours distribution as in the Labor Force Survey. The distribution of actual hours worked per week in the QP provide similar shift as is clear from the Figure A8. Since actual hours fluctuate more and are influenced by the number of working days in the reference month in each year's QP, the distribution is fuzzier than the standard hours.

Figure 2 is supplementary to Figure A5, where the evolution of standard hours are extend to pre-1993 years with the imputed standard hours by the methodology outlined in Section A.2.3. Figure A12 shows the evolution of the share of workers working on weekends between 1992 and

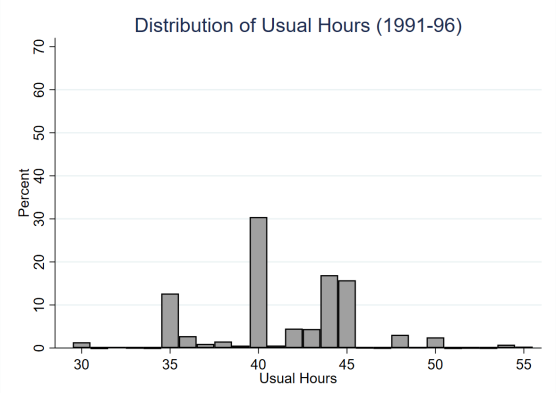
2001.Figure A9 shows the differences in number of working days in the reference month of the QP across difference years. The maximum is 23 days and the minimum is 20 days.

Figure A6: Distribution of Usual Working Hours for Full-Time Employees, Labor Force Survey, 1986-89, 1991-96, & 1998-04

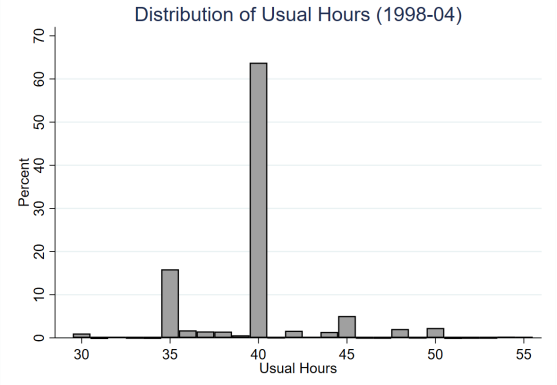
(a) 1986-89



(b) 1991-96



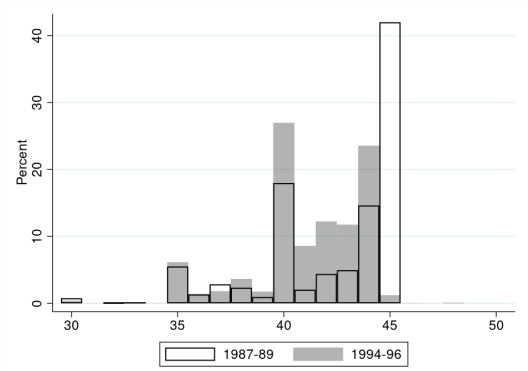
(c) 1998-04



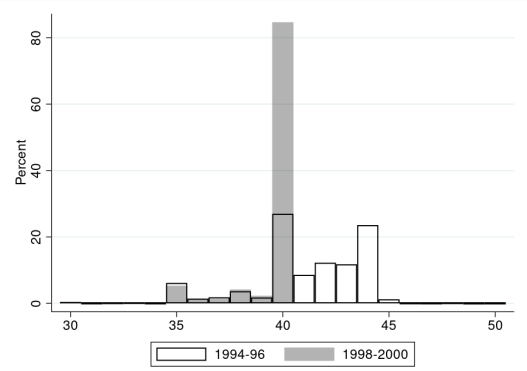
Note: These graphs plot the distribution of usual working hours for full-time employees over the three different periods in which the legislation was different. In the period 1986 to 1989, standard hours were set at 48, or 45 over 5 days. In the period 1991-96, they were set at 44 hours per week. As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

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

(a) 1987-89 and 1994-96



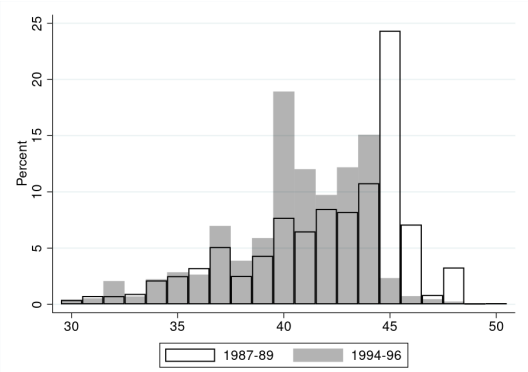
(b) 1994-96 and 1998-2000



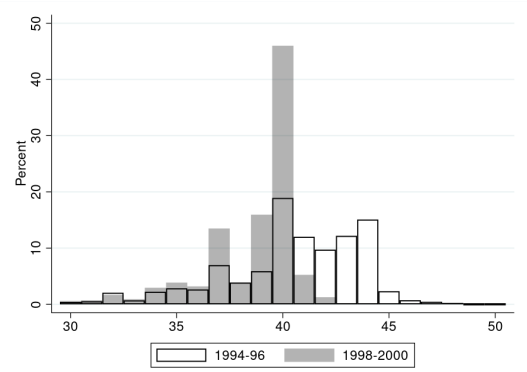
Note: These graphs plot the distribution of weekly standard hours for full-time employees, as recorded in the QP. In the period 1986 to 1989, standard hours were set at 48, or 45 over 5 days. In the period 1991-96, they were set at 44 hours per week. As from 1998, a 40 hour working week was introduced.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

Figure A8: Distribution of Actual Hours Worked for Full-Time Employees, QP

(a) 1987-89 and 1994-96



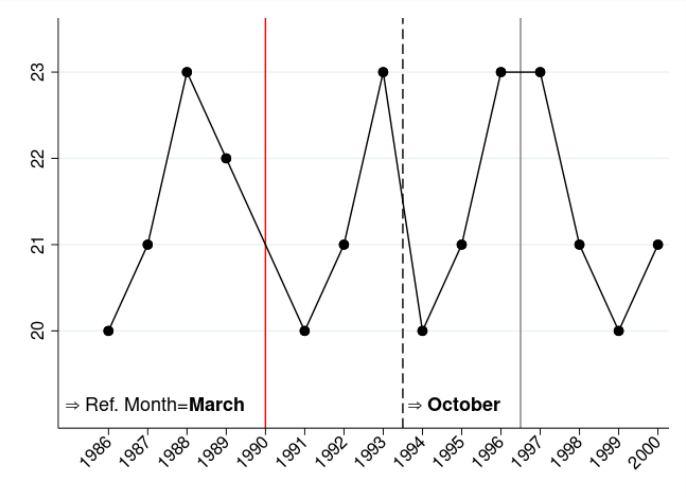
(b) 1994-96 and 1998-2000



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

A.3.4 Number of Working Days in the Reference Month

Figure A9: 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.3.5 Variation of Standard Hours

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 of various dimensions. We restrict the analysis to the 1996 sample for full-time workers with standard hours ranging from 30 to 50 hours.

In Table A2 Column (1), we find that collective agreements account for 56% of variation in standard hours. In Column (2), we observe that 3-digit industry fixed effects alone explain nearly half of the variation, while in Column (3), municipality fixed effects capture only 20% of the variation. When we consider the fixed effects of the combination of industry and municipality in Column (4), we see these two factors jointly explaining slightly over 60% of the variation.

In Column (5), when we introduce collective agreement fixed effects alongside industry-municipality fixed effects, R^2 only marginally improves, suggesting that collective agreements already largely

reflect a combination of sector and geographical dimensions. Further, in Column (6), adding occupation fixed effects increases the R^2 by 0.1. Occupation alone does not account for a significant portion of the variation.

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

Table 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.4 Supplementary Figures and Tables for the Empirical Analysis

A.4.1 Supplementary tables

Table A3 provides the regression table for Figure 4. Similarly, Table A4 provides the regression table corresponding to the Figure 5, showing the effects of the reduction in working hours on the share of workers separated and newly hired.

Table A4: 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; * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

A.4.2 Additional Results

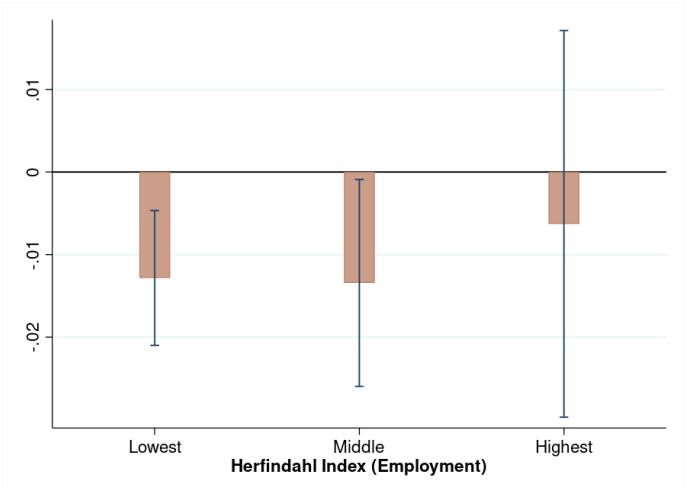
Role of market concentration Under imperfect competition, the impact of increased labor costs on labor demand may not necessarily be negative (Manning, 2003). To investigate this, we examine the heterogeneity in employment effects across labor markets, which we define by a combination of municipality and 2-digit economic sector.[49] For each labor market, we compute the Herfindahl-Hirschman Index (HHI) based on employment, utilizing each firms' employment share in the market, and categorized the markets into three groups (bottom, middle, and top third) according to

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

the index.[50] We then estimate the heterogeneous employment effects by interacting the treatment variable with dummies representing each of the three market groups.

Figure A10 illustrates the heterogeneous employment effects across HHI, showing that the negative employment effects were least pronounced among firms operating in more concentrated labor markets. While there are substantial standard errors, the point estimate is the smallest in markets with the highest concentration.[51] This is suggestive evidence that monopsony power might also play a role in the context of working hour reforms. However, even in the most concentrated labor markets, we can rule out a positive work-sharing effect.

Figure A10: Heterogeneity in the Employment Effects, by Market Concentration



Note:The figure shows the heterogeneity in employment effects of the 1996 reform across labor markets with differing levels of market concentration. The estimation is based on Equation 3, where we further interact the treatment variable with dummy variables indicating each group of labor market concentration, and add firm-specific trends. Labor markets are defined by a combination of municipality and 2-digit sector, and their concentration is measured using the Herfindahl-Hirschman Index based on each firms' employment share within each labor market (averaged over the 1994-1996 period). We categorize the markets into three groups (bottom, middle, and top third) according to the index. The heterogeneous coefficients are obtained by interacting the treatment variable with dummies indicating three groups of markets.

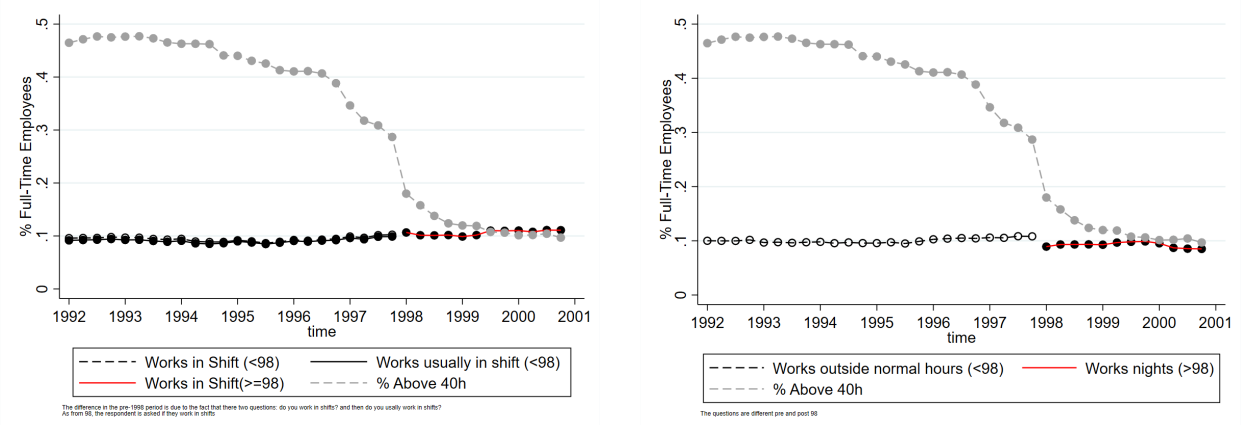
[50]The Herfindahl-Hirschman Index (HHI) is computed as follows: $HHI_m = \sum s_{jm}^2$, where sjm represents the share of firm j's employment in market m.

[51]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.

Weekend, night and shift work. 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.

Figure A11: Evolution of Shift and Night Work Over Time

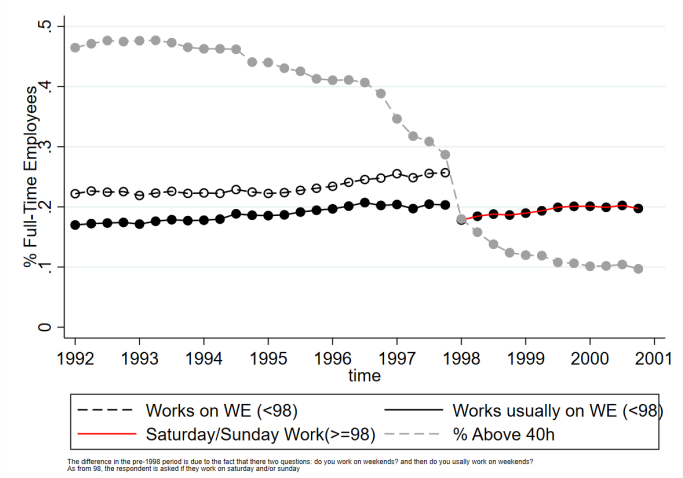
(A) % of Workers Working in Shifts (B) % of Workers Working Nights



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.

Role of prices. The positive impact of the 1996 reform on sales per hour may potentially reflect that firms increased the prices of their goods to compensate for the loss of sales. To test this hypothesis, we examine how the reduction in working hours affected sales in different types of markets. In theory, a firm's ability to raise prices to cover the increased labor costs depends on how much competition it faces in the product market. This leads to a testable prediction that firms in less competitive market should experience less negative effects on nominal sales. To test this hypothesis, we quantify the level of product market concentration by using firms' sales data. Following Autor, Dorn, Katz, Patterson, and Van Reenen (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

Figure A12: % Workers Working of Week-Ends



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

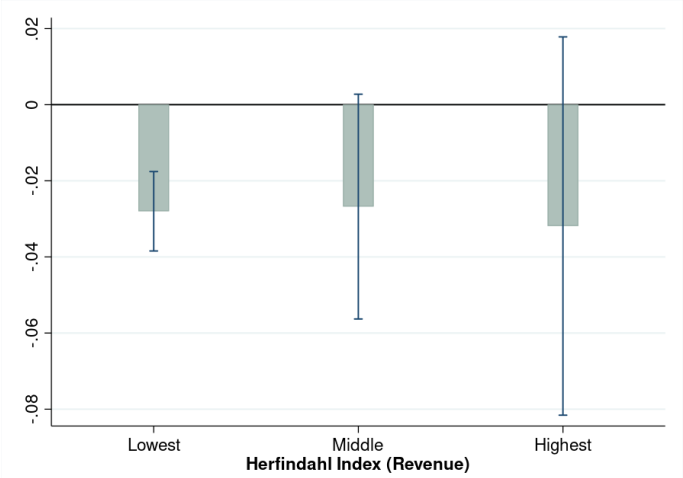
the heterogeneity in the effects on firms' sales across these three groups. The result is shown in Figure A13. The estimated effects on sales are similar across firms operating in product markets with different levels of sales concentration.[52] This provides indirect evidence that price was not a major mechanism for firms to compensate for the loss in output.

Capital intensity. Increasing the capital use in the short run is likely more feasible for firms operating with higher capital intensity in their production processes. Conversely, firms that heavily rely on labor may face difficulties in reducing their labor input. To explore the heterogeneity effects on employment and productivity, we consider the capital intensity of the sector. Using data from the EU KLEMS database that provide capital compensation measures at the 2-digit sector level, we compute the sector-level capital intensity.[53] The results are presented in Figure A14. In Panel (A), we observe that the negative employment effects are more pronounced for firms in sectors with

[52] 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.

[53] 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 A13: Heterogeneity in Sales Effects, by Product Market Concentration

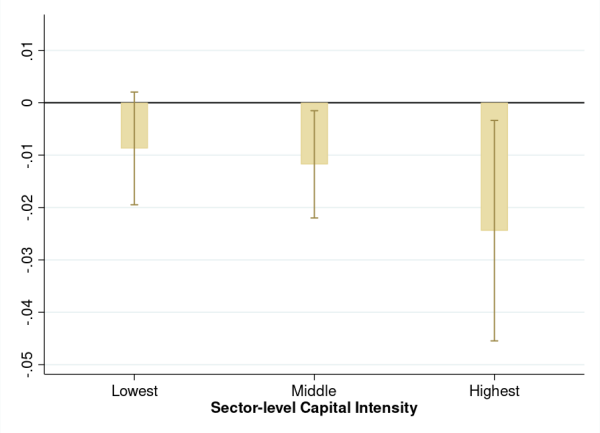


Note: The figure shows the heterogeneity in sales effects of the 1996 reform across 4-digit industries with differing levels of sales concentration. The estimation is based on Equation 3, where we further interact the treatment variable with dummy variables indicating each group of product market concentration, and add firm-specific trends. The concentration is measured using the Herfindahl-Hirschman Index based on each firm's sales share within each industry (averaged over the 1994-1996 period). We categorize the industries into three groups (bottom, middle, and top third) according to the index. The heterogeneous coefficients are obtained by interacting the treatment variable with dummies indicating three groups.

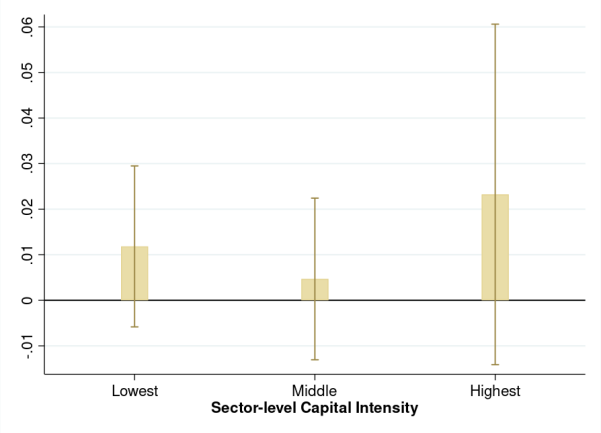
high capital intensity. This is consistent with the notion that firms increased their use of capital in response to reduced work hours and higher labor costs. Furthermore, in line with this, Panel (B) demonstrates that in these sectors, the increase in hourly labor productivity exhibits the most significant 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.

Figure A14: Treatment Effects by Capital Intensity

(A) Employment Effects



(B) Productivity Effects



Note: The figure shows the heterogeneity in employment and productivity effects of the 1996 reform across 2-digit sectors differing in the pre-reform capital intensity. The estimation is based on Equation 3, where we further interact the treatment variable with dummy variables indicating each group of capital intensity, and add firm-specific trends. Capital intensity is defined as a fraction of capital compensation and total value added in each sector and averaged over the 1994-1996 period. Both information is obtained from EU KLEMS database, Sectors are subsequently categorized into three groups (bottom, middle, and top third) based on their capital intensity scores. To estimate the coefficients for each group, we interact the group dummy variables with the treatment variable. Note that the standard errors are larger the high capital intensity group because there are smaller number of firms in this group.

ICT and non-ICT capital. Table A5 supplements the sector-level analysis of capital in Table 4 by breaking down capital into ICT and non-ICT categories and examining the capital service per labor hour. The results indicate that 2-digit sectors with greater reductions in working hours also experienced increased growth in capital services and capital service use per hour. These effects are more pronounced for ICT capital.

Firm survival. Table A6 investigates if the probability of firm survival was affected by the 1996 reform. We analyze firms separately depending on their first appearance in the pre-treatment years (1994–1996). We fill the data so that each firm is tracked until the end of the sample period (i.e., 2000) following its initial appearance. We then create a variable indicating firm survival, assigned a value of 1 if the firm continues to operate and 0 for all years after its exit. Our findings indicate that the reform has no significant effect on the probability of firms remaining operational.

Table A6: The Effect of the 1996 Reform on the Firm Exit Probability

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

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

Table A3: The Effects of the 1996 National Reform

(a) Hours and Labor Cost

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

(b) Labor Input and Sales

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

Note: The tables display the regression results corresponding to Figure 4, estimating the effects of the 1996 reform in Equation 3. All outcomes are in log, except for the hour in columns (1)-(3). The outcomes in columns (1)-(5) represent firm-level average values for full-time equivalent workers who worked at least 30 hours per week. The variable $Treat$ is 1 for treated firms and 0 for control firms, and it is interacted with year dummies. There is no coefficient for the reference year of 1996. Standard errors are clustered at the firm level. Standard errors in parentheses; * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

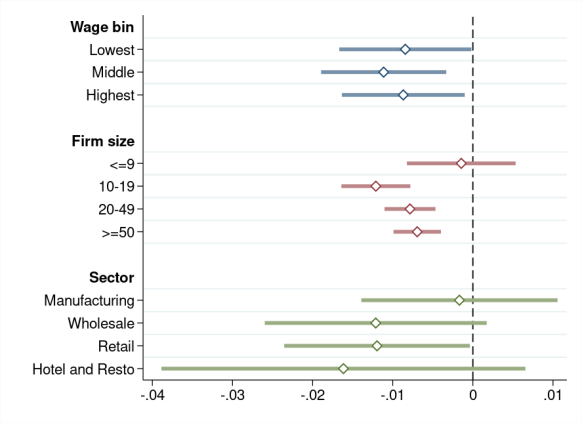
Table A5: Disaggregation of Capital Effects

	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

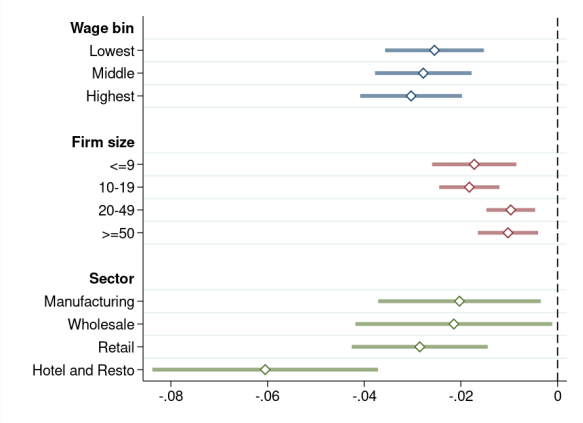
Note: This table complements Table 4 by examining the impact on capital, with a breakdown into non-ICT capital and ICT capital, as well as their values per labor hour. The unit of analysis is the 2-digit sector, and all information regarding capital is sourced from the EU KLEMS database. Treated sectors are those above the median in the average of firm's treated hour shares, as defined in Equation 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 A15: Heterogeneous Effects of Working Hour Reductions by National Reform

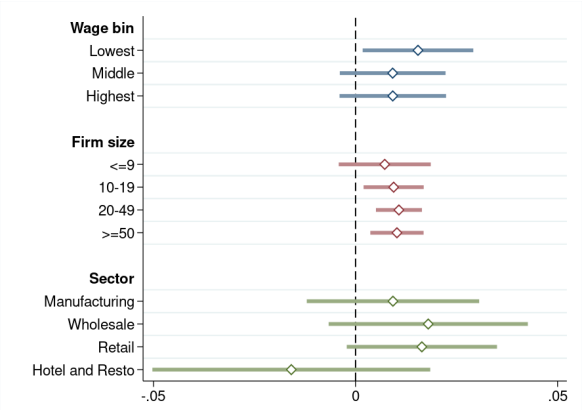
(a) Employment



(b) Sales



(c) Sales per hour



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

A.4.3 Robustness checks

In this subsection, we provide the results of several robustness checks for the results presented in Table 3.

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

Robustness to cut-offs In the main specification we use firms with no treated workers as our control group. To examine the robustness of our findings under different treatment definitions, Table A8 compares the effect on the key outcomes using different treatment definitions, with the inclusion of firm-specific linear trends. The first row replicates the main results (with firm-specific trends) using only firms with no treated workers as a control group. In the second and third rows, we use firms in the bottom third and firms below the median in terms of share of treated hours as criteria to define control and treated firms. The magnitudes of the coefficients decrease as firms categorized as control and treated become more similar. Nevertheless, our findings remain consistent across the alternative treatment definitions.

All firms The results are based on a sample of firms where the mode of standard hours did not change by more than one hour between 1994 and 1996. This is to prevent contamination from firms that might have been undergoing a reduction in working hours through collective agreements before the 1996 reform. Therefore, our findings rely on a sample of firms operating at "equilibrium hours". To assess whether our results are influenced by this specific sample selection, Table A9 presents the results without excluding any firms. In other words, all firms that existed between 1994 and 1996 (but after the common sample selection such as on sectors) are included in the sample. In the most conservative specification with firm-specific linear trends, the results remain largely consistent.

Sample selection Finally, we show that our results are robust to the exclusion of firms born just before 1996. Table A10 shows the estimated effects on key outcomes by excluding firms by restricting firms born before 1995 (second row) and by 1994 (third row). The coefficients point to the similar conclusion, that is, hours are reduced, employment and sales were decreased, and hourly labor productivity increased. The estimated effects on sales are substantially smaller when we exclude recently-created firms, indicating a sudden reduction in hours and increase in labor cost was particularly large for newly-born firms.

Table 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 display 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 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 A8: Robustness: Using Alternative Definition for Control Group

(a) Hours and Labor Cost

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

(b) Labor Input and Sales

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

Note: The tables display the results of the working hour reduction the 1996 reform analogous to Table A7, using alternative definitions of treatment groups. Instead of using the bottom quarter in terms of the share of hours treated (where the share of hours treated is effectively zero), we use the bottom third and below median. Standard errors in parentheses; * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

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Table A9: 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 display 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 A10: Robustness: Exclusion of Firms Created Just Before the Reform

(a) Hours and Labor Cost

	Hours			Labor Cost	
	Standard	Actual	Overtime	Monthly salary	Wage
	(1)	(2)	(3)	(4)	(5)
Sample:					
All firms	-1.986***	-1.585***	0.011	0.011***	0.049***
(Baseline)	(0.014)	(0.025)	(0.022)	(0.004)	(0.004)
Firms created by 1995	-1.980***	-1.572***	0.005	-0.012***	0.049***
	(0.015)	(0.026)	(0.022)	(0.004)	(0.004)
Firms created by 1994	-1.970***	-1.547***	0.013	0.012***	0.048***
	(0.015)	(0.027)	(0.022)	(0.004)	(0.004)

(b) Labor Input and Sales

	Labor Input		Sales		
	Employment	Total Hour	Total	Per Worker	Per Hour
	(6)	(7)	(8)	(9)	(10)
Sample:					
All firms	-0.013***	-0.056***	-0.032***	-0.021***	0.019***
(Baseline)	(0.004)	(0.006)	(0.005)	(0.006)	(0.007)
Firms created by 1995	-0.012***	-0.052***	-0.018***	-0.008	0.031***
	(0.004)	(0.006)	(0.005)	(0.006)	(0.007)
Firms created by 1994	0.009**	-0.049***	-0.015***	-0.007	0.031***
	(0.004)	(0.006)	(0.005)	(0.006)	(0.007)

Note: The tables test the robustness of the results of the working hour reduction from the 1996 reform, analogous to Table A7, by excluding firms created just before the reform. The first column presents the baseline results, the second column restricts the sample to firms created by 1995, and the third column also restricts the sample to firms created by 1995. Standard errors in parentheses; * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

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A.5 Supplementary Figures and Tables for Long Term Estimation: 1996 Reform

In this subsection of the Appendix, we provide alternative estimates of the results of the national reform by running an estimation over the full period 1986 to 2010, and defining treated and control groups on firms existing before the reform process began in 1990.

A.5.1 Descriptive Statistics of the Three Treatment Groups

Table A11: 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.5.2 Sectoral Distribution of the Three Groups

Figure A16 provides the distribution of the treated groups (as defined in Section 6) across sectors. Panel (A) shows the distribution across large sectors, while Panel (B) shows the sectors at the two digit level that disaggregates manufacturing sector further. Figure A17 similarly provides the distribution across districts.

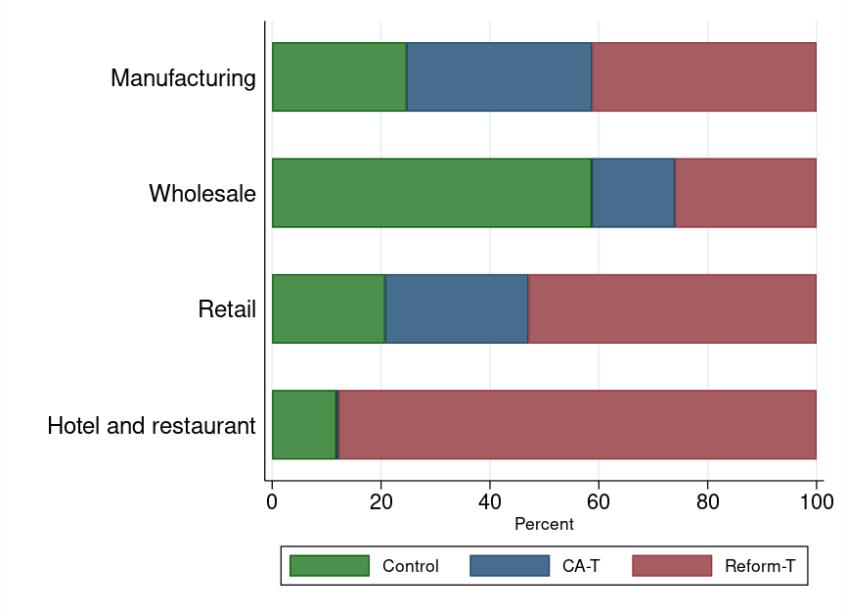
A.5.3 Effects of 1996 national reform (long-term estimation)

Figure A18 illustrates the effects of the 1996 national reform on hours, wages, and monthly salaries. Given the long pre-treatment period in this analysis, differential pre-trends emerge between the Ref-T group and the control group for certain outcomes. In such cases, the reform's effect becomes apparent as a deviation from the pre-existing trend. To provide a clearer visual representation of the effects, we also present dynamic effect results adjusted for the pre-existing trend, in an identical fashion to Figure VII of Dustmann, Lindner, Schönberg, Umkehrer, and Vom Berge (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 A12.

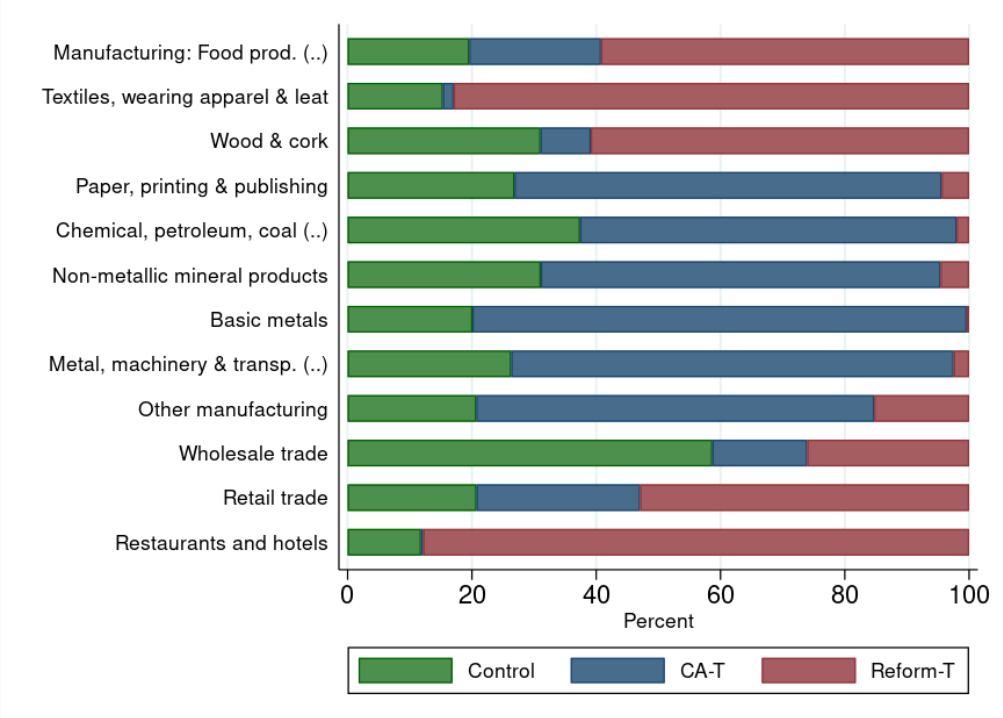
Hours, wages and salaries. Panel (A1) in the figure shows the effect on average weekly standard hours. The coefficients leading up to 1996 are close to zero, indicating a similar evolution in standard hours between the reform-treated firms and control firms prior the reform. The effect of the 1996 reform is evident: between 1996 and 1998, Ref-T firms experienced an average reduction of 2 hours in mean standard hours compared to the control group. In relative terms, this amounts to roughly a 5% decline in mean standard hours. Similarly, Panel (B1) exhibits a comparable result

Figure A16: Sector Distribution of Treatment Groups

(A) Aggregated Sector

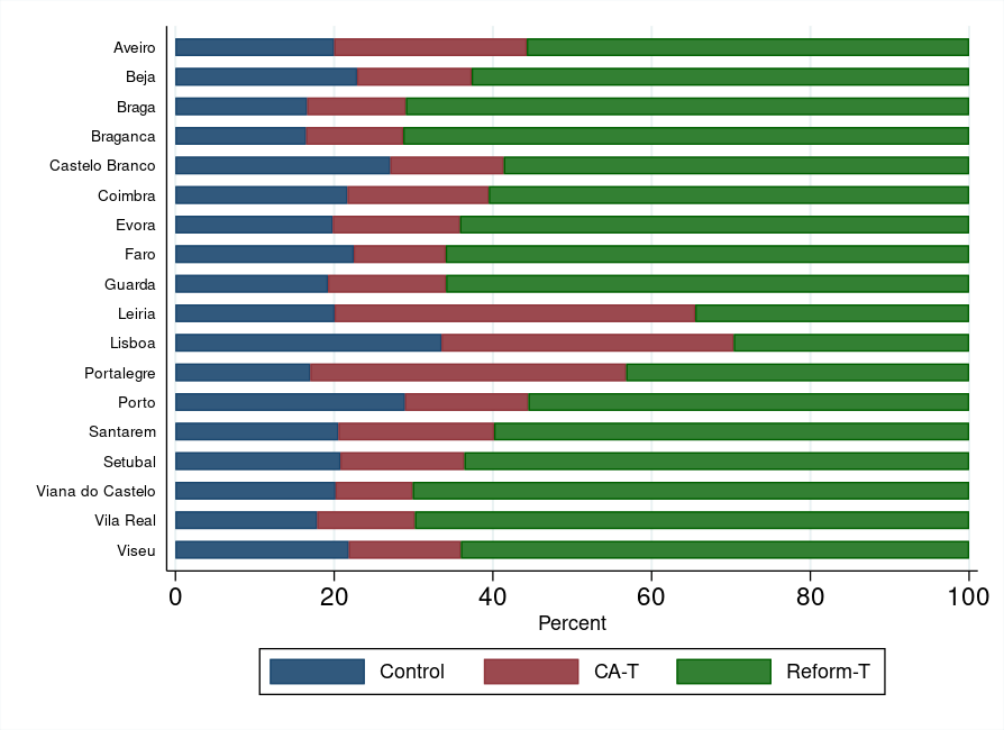


(B) Two-digit Sector



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 A17: Geographical Distribution of Treatment Groups



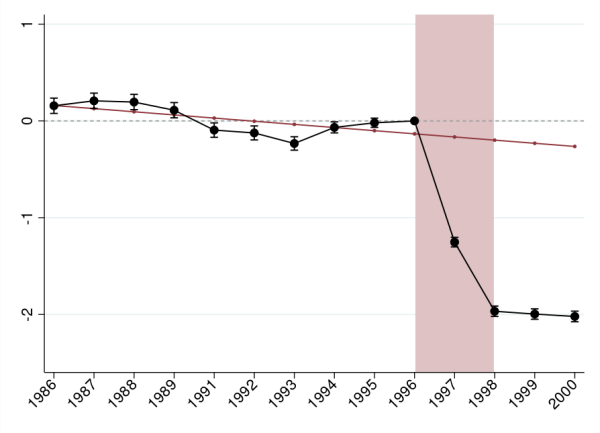
Note: The figures show the distribution of the treatment groups across districts.

for mean weekly actual hours worked.

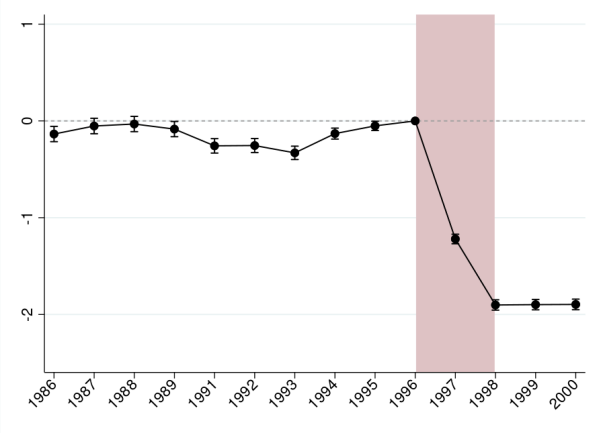
In Panel (C), we observe that there is no adjustment in the mean monthly salary after 1996. Consequently, the reduction in working hours resulted in an increase in the cost of labor, a trend corroborated by Panel (D1) which outlines the effects on mean hourly wage. In comparison to the pre-trend, there is nearly a 6% rise in the mean wage rate after the completion of the reform. Columns (1) to (5) of Table A12 show the same results in regression form, with the difference that a firm-specific trend is fitted for each firm.

Figure A18: Effects of 1996 Reform on Hours, Wages and Salaries

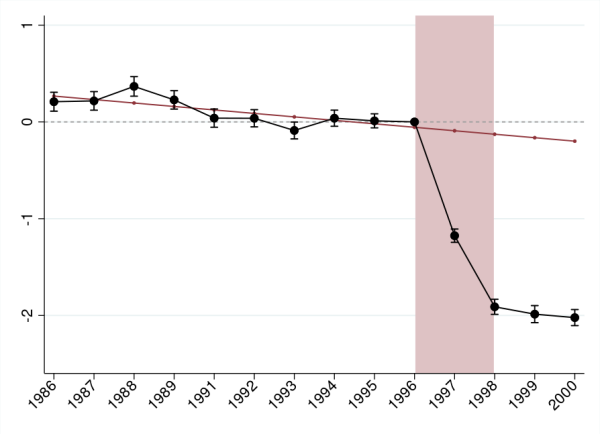
(A1) Mean Standard Hours



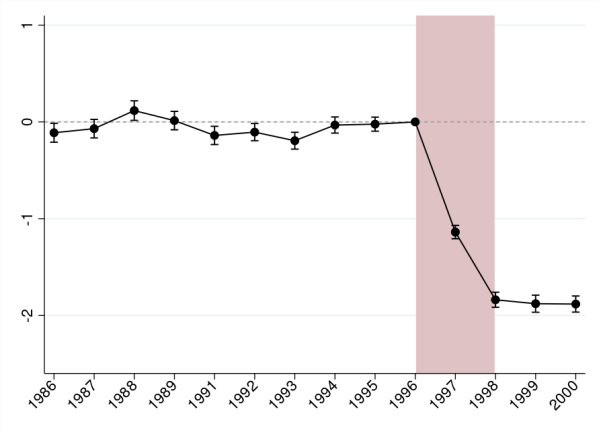
(A2) Detrended



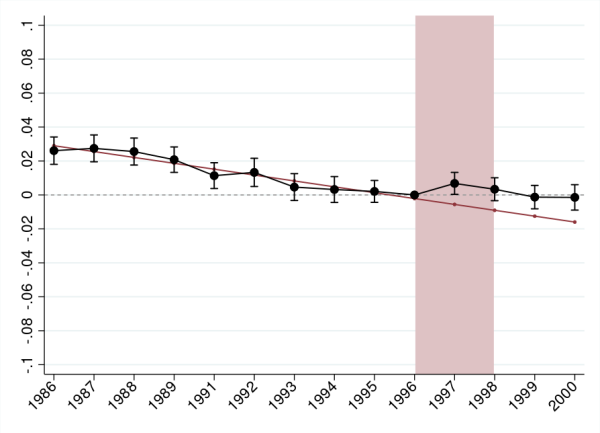
(B1) Mean Actual Hours



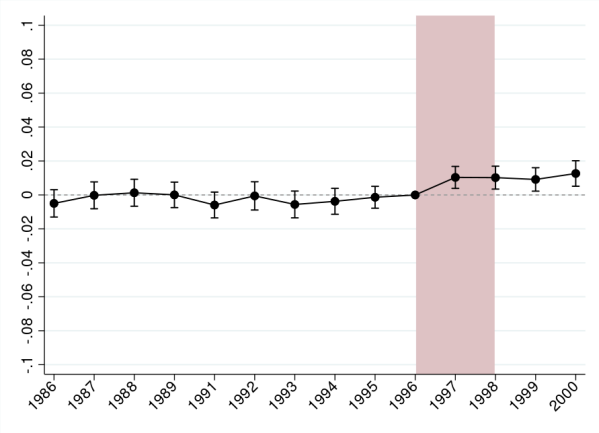
(B2) Detrended



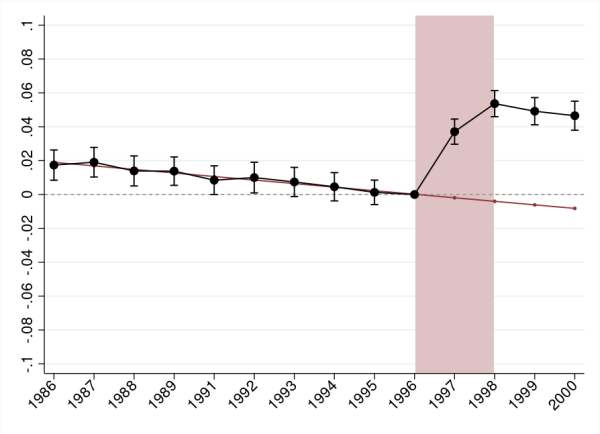
(C1) Mean Monthly Salary



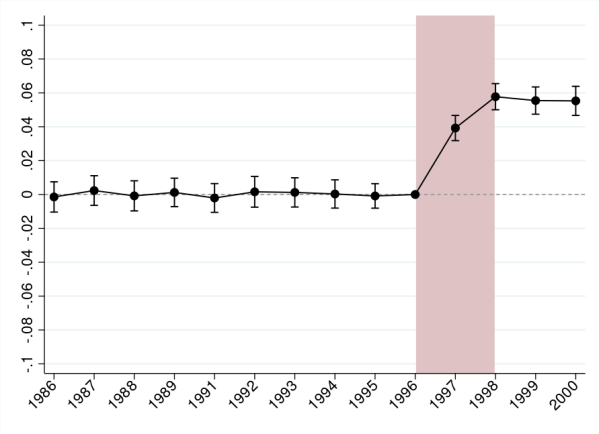
(C2) Detrended



(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. 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. Figure A19 shows the effects of the reform on employment, sales, and productivity (sales per worker and per hour). In panel (A1) of the figure, the evolution of employment in pre-treatment periods was significantly different between the reform-treated firms and control firms. Specifically, Ref-T firms displayed consistently higher growth in employment. This could help explain why these firms were not treated through collective agreements: in the face of growing labor (and potentially 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 A12. After the reform, firm's total labor input decreased by 5% for the Ref-T group compared to

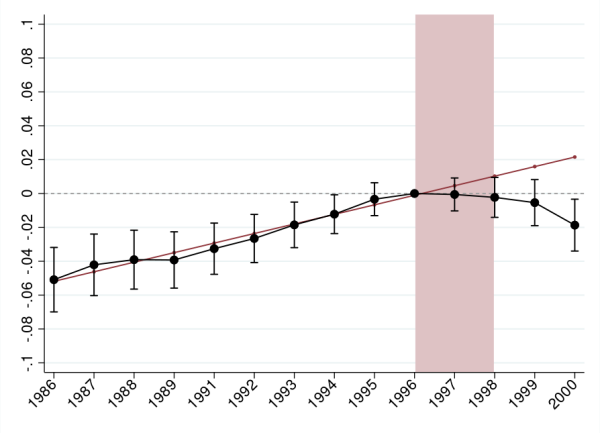
the control group.

Panel (B1) provides the effects of the reform on sales. The effect is ambiguous: the coefficients in the post-reform years (panel B1) are constantly negative and significant, hinting to the output down-scaling for the firms treated by the reform. However, controlling for the pre-reform trend (B2), shows a much more ambiguous picture: the post-reform coefficients are small and close to zero. As shown in Table A12, 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).

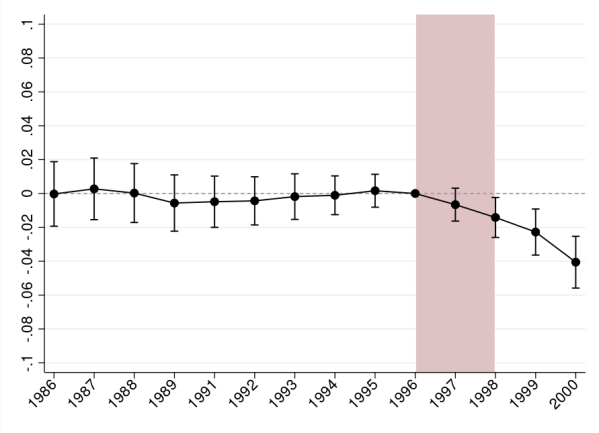
In terms of productivity, as seen in Panel (C1) and Table A12, per-worker sales were not significantly altered by the reform. The coefficients are positive in the graphical analysis presented in panel (C2). When we analyze the results with firm-specific trends, the effect on per-worker productivity is around 1% after the reform, but not statistically significant. This indicates a relatively similar magnitude of the reduction in sales and employment, leaving per-worker productivity unaffected. On the contrary, Panels (D1) and (D2) clearly illustrate a noticeable increase in sales per hour since the reform. Towards the end of the data period, this increase amounts to approximately 6%, relative to the pre-existing trend (or about +4% when estimated with firm-specific trends).

Figure A19: Effects of 1996 Reform on Employment, Sales, and Productivity

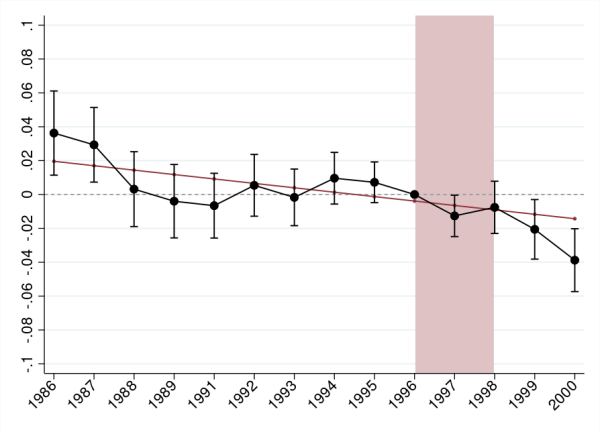
(A1) Employment



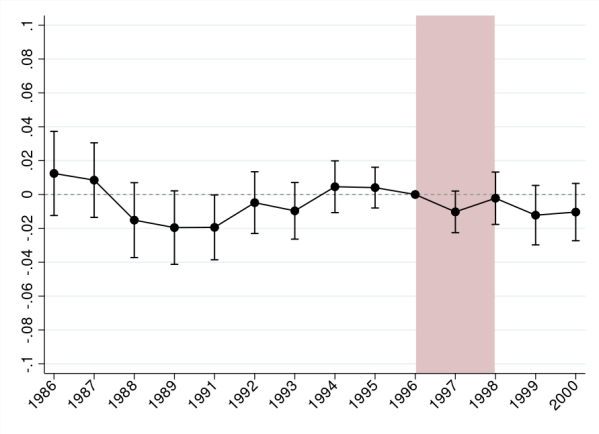
(A2) Detrended



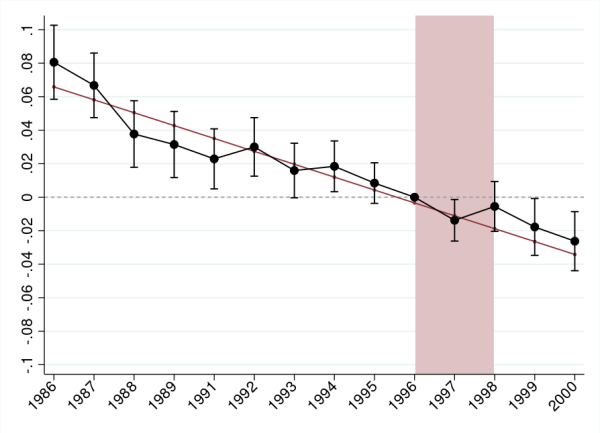
(B1) Sales



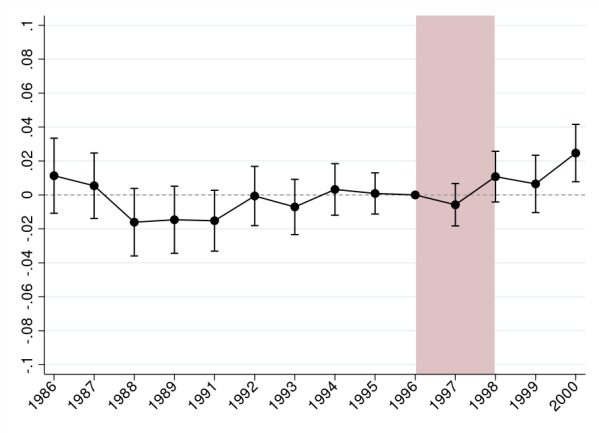
(B2) Detrended



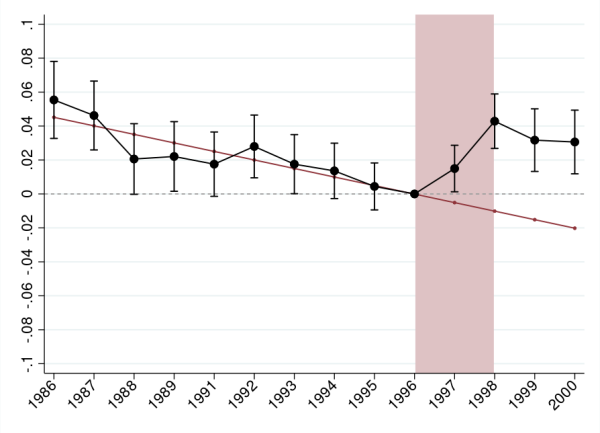
(C1) Sales per Worker



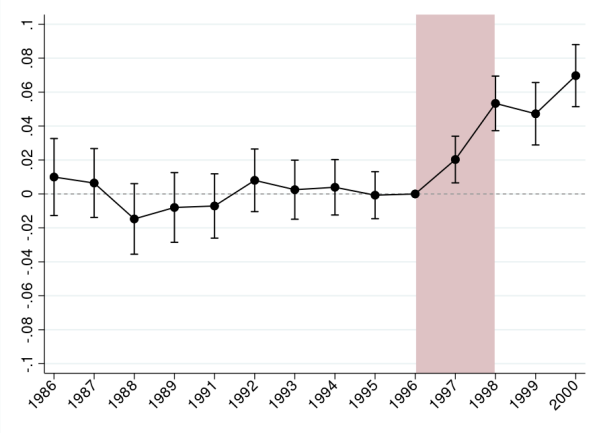
(C2) Detrended



(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. On the right, we show the detrended version of the same dynamic effects. Detrending was obtained by fitting a linear trend on the coefficients estimated between 1986-1996 and taking the difference of each coefficient from the predicted values, which are then re-centered at 1996 again while keeping the stand errors unchanged, as in Dustmann et al. (2022). The control group consists of firms situated in the lowest fourth of the distribution of mean standard hours spanning from 1986 to 1996. The red-shaded areas corresponds to the treatment period of the 1996 reform. The outcomes are in absolute terms for hours and in log otherwise. Standard errors are clustered at the firm level.

Table A12: The Effects of the 1996 Reform, Long-run Approach, with 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 display the results of the working hour reduction the 1996 reform. All outcomes are in log, except for the hour measures in the columns (1)-(3). The variable $Treat$ takes 1 for firms in the Reform-treatment group and 0 for the firms in the Control group. $Post$ takes 1 the years after 1997 and 0 otherwise. The outcome variables are regressed on the interaction of $Treat$ and $Post$ to provide the difference-in-differences estimate of the effects of the working hour reductions. Standard errors are clustered at the firm level. Standard errors in parentheses; * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

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A.6 Supplementary Figures and Tables for Collective Agreement Treatment

A.6.1 Supplementary Figures

Table A13 displays the number of collective agreements that reduced standard hours following the 1990 reform by the initial year of treatment. The first year of treatment is determined as the year in which the mode of standard hours in the collective agreement first decreased or as the first year in which the share of workers at the mode in the collective agreement declined by 10% for the first time. The majority of collective agreements and firms began reducing hours in either 1991, 1992, or 1993.

Table A13: 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.

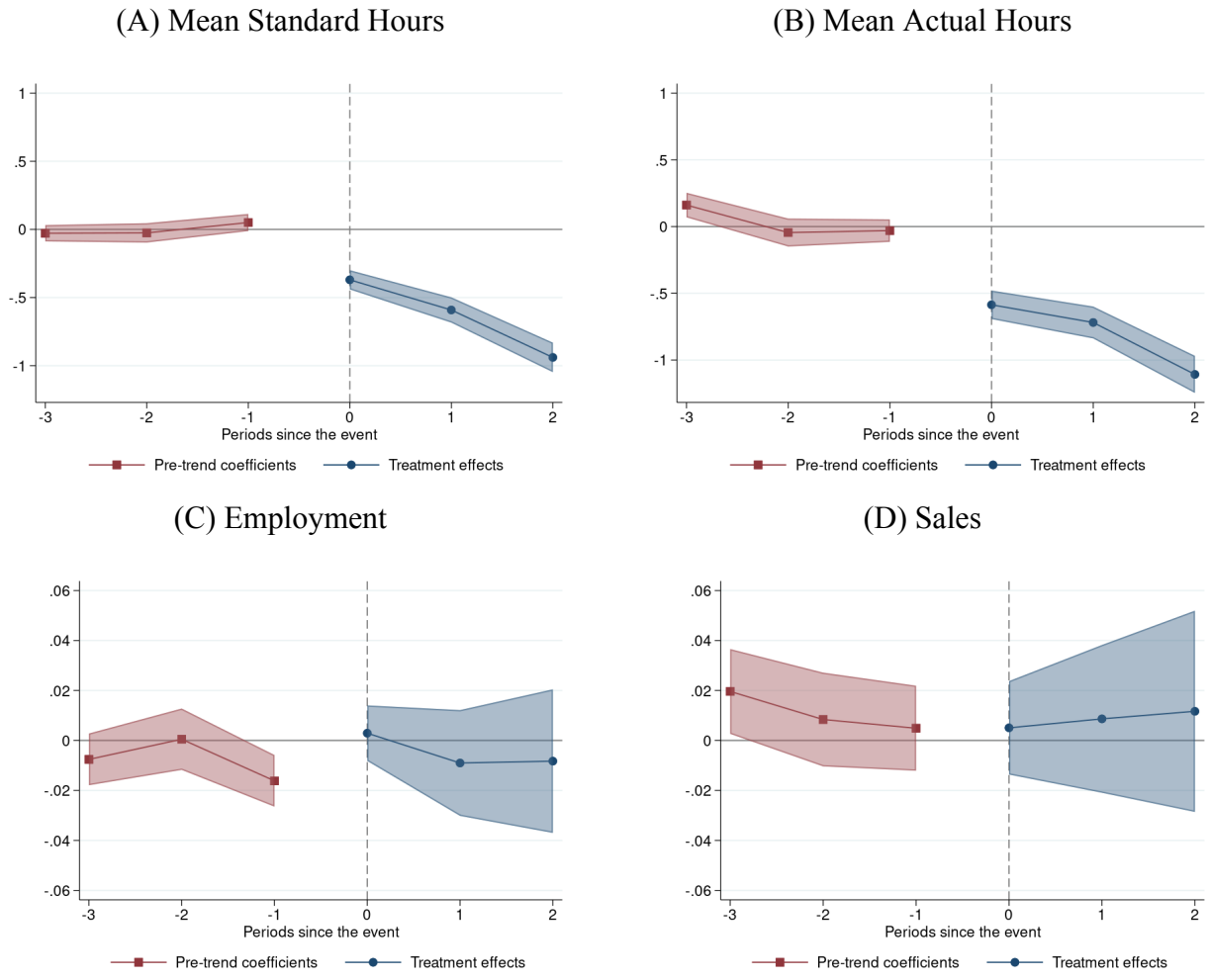
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A.6.2 Within-collective agreement comparisons.

Alternative to the results presented in Section 6, we run the estimation by focusing exclusively on firms treated by collective agreements that reduced hours over the period 1991-1996. We exploit only the variation in the *timing* of treatment. Relative to the previous approach, the advantage here is to compare only firms within the sample of collective agreements that autonomously decide to lower their hours. This specification exclusively compares firms treated by the collective agreements earlier to those treated later.

Figure A20 shows the results. Panel (A) and (B) show that both mean of standard hours and actual hours were reduced by 1 hour for the firms treated by collective agreement, relative to the firms treated by collective agreement later. Although the gradual reduction is consistent with the prediction, the total reduction is rather small. This is in part due to the firms that had already lower hours than the collective agreement mode, but also because of the difficulty to identify the precise timing of the first treatment year. Panel (C) shows that there is no significant change in the level of coefficients before and after the reduction in working has started, consistent with the finding from the main difference-in-differences results in our main results. Similarly, we detect no discernible effects on sales, as shown in panel (D). These results show that our main results are not driven by unobserved characteristics that endogenously divide CA-treated firms and the control firms.

Figure A20: 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.