

Firm-Level Effects of Reductions in Working Hours

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Abstract

This paper explores how legislative reduction in working hours impact firms. We exploit the Portuguese transition to the 40-hour week in the 1990s, when Portugal gradually began to reduce the length of the standard working week by means of collective bargaining, and then suddenly decreased it to 40 at the national level. We show that firms that did not adjust voluntarily hours before the reform were experiencing higher labor demand in the pre-reform period, and then decreased both employment and output. We argue this is the result of increased labor cost, as nominal salaries did not adjust and hourly wages rose. The loss in output is partly offset by higher productivity, as firms are able to produce more for each unit of labour input.

JEL codes: J22, J23, J31

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1 Introduction

Working hours are a key economic variable; if workers and hours are not perfect substitutes, working hours (should) enter directly into the production function (Feldstein (1967)). This implies a direct link between working hours and first-order economic outcomes such as output, employment and productivity. Consequently, working hours *legislation*, i.e., those regulatory limits to the number of hours one can/should work, is bound to have significant economic repercussions: by setting limits to the number of hours a worker is allowed to work, the effects of regulations would also potentially ripple to output, employment and productivity. This type of legislation is pervasive across countries, particularly so in Europe, where there is a EU-level Directive regulating maximum working hours. Moreover, the topic is often passionately debated in the public discussion, with the debate of shorter working hours (or days) gaining significant traction after the Covid pandemic. Yet, empirical evidence of its effects is still very limited, and in particular little is known on how firms adjust to more stringent limits to weekly working hours.

The paper aims to fill this gap by looking at Portugal, a country that offers a unique case study in response to these questions. Using comprehensive matched employer-employee data, we estimate the effects, at the firm level, of a reduction in working hours that occurred between 1991 and 1998. Portugal has a system of labor relations that combines both national regulations and — usually more binding — sectoral agreements. Moreover, firms are free to bargain hours (wages) to lower (higher) levels than the thresholds specified by law or collective agreements. Portugal begun a significant reform of working hours legislation in 1990: a tripartite agreement was signed between unions, employers’ association and the government to gradually lower hours to 40 by 1995 through collective agreements. Few collective agreements complied, but many did not. In 1996, after national election, a new government decided to unilaterally implement the change to a 40-hour week by national legislation. This legislative change provide us with an important source of variation to estimate the firm-level impacts of reducing working hours. In our main analysis, we compare firms that were less or not affected by this process — i.e., firms with hours closer to 40 hours — with those that move to the 40-hour week either as a result of the national reform, in a difference-in-differences framework. We show that firms impacted by the national reform imposing the 40-hour week experienced a moderate

decrease in both employment and output, likely because hourly wages increased significantly after the reform as nominal salaries did not adjust.

Partly because of its salience in the policy debate, the employment effects of a reduction in working hours have received the most attention in the literature. We summarize these findings in a concise way in Table A1 in the Online Appendix, by reform and level of analysis (workers, firms, sectors, regions). Overall, while there are well-identified but conflicting estimates of the impact of a reduction in working hours on *incumbent* workers, evidence of the effect on labor demand is scarce. Crépon and Kramarz (2002) find that, in France, a reduction in standard time from 40 to 39 hours in 1982 increased the probability of *incumbent* workers being fired.¹ Using the same approach, Gonzaga et al. (2003) look at the effect of a reduction from 48 to 44 hours in Brazil in 1988, and estimate no effect on job loss, while Raposo and van Ours (2010) find ambiguous effects on the separation rate when Portugal reduced standard working hours from 44 to 40.² Between 2001 and 2005 in Chile, Sánchez (2013) finds no impact on employment transitions for a reform that allowed for a 4-year adjustment period. Estevão and Sá (2008) look at aggregate employment in large versus small firms in France, which, while impacted by the reduction to 35 hours at different times, have no visible difference in their employment dynamics.

Empirical evidence at the firm level, which should allow us to capture the overall effect on labor demand short of general equilibrium effects, is scarce and less convincing. 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

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

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

cost. In this paper, we argue that Portugal, which also experienced a large and sudden reduction in standard time but without compensating measures, provides a much cleaner case study. The only paper to look at standard hours reduction in Portugal impacted on firms' employment dynamics was the preliminary work by Varejao (2005), finding zero or negative employment effects. As we underline throughout the paper, the legislative process that brought Portugal to the transition to the 40-hour week was much more complex than put forward by the previous literature on this reform (Lepinteur, 2019; Varejao, 2005; Raposo and Van Ours, 2010; Raposo and van Ours, 2010). The national reform was not an out-of-the-blue change in the length of the working week, but the end of a process of 7 years of reform in working hours, occurring through several legislative instruments. In this paper, we try to shed light on the full process that brought Portugal to the 40-hour working week, and incorporate it within the empirical strategy through which we study how reductions through different legislative instruments impacted firms.

Lastly, some studies have focused on the sector and/or regional level, in an attempt to capture aggregate equilibrium effects not limited to labor demand (firms) or incumbent workers. Hunt (1999) shows that in Germany, in the late 1980s and early 1990s, sectors that adopted agreements regulating working time experienced a *relative* decrease in employment. Both Skuterud (2007) and Chemin and Wasmer (2009) use regional legislative specificities to capture the effect of a reduction in working hours. Skuterud (2007) shows that, while 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.

Furthermore, there is no evidence, to our knowledge, about how a nation-wide reduction in standard hours impacts output at the firm level and workers' (and hours) productivity. Do we observe a negative scale effect on output? Do workers become more productive (per hour worked) as working hours decrease? In the literature, evidence on the relationship between reductions in standard hours and productivity is virtually absent. Some studies have examined how working hours and productivity are related in very specific occupations: Brachet et al. (2012) look at the performance of paramedics with respect to shift-length, Pencavel (2014) examines this in the context of munition workers in the UK in the 1950s, while Collewet and Sauermann (2017) focus on the case of call center workers in the Netherlands. All these studies find evidence of a linear relationship between hours and output up to a certain point, followed by diminishing marginal returns above said level. Other papers have looked at how part-time employment affects firms' productivity, finding that firms with a higher share of part-time workers tend to be more productive in specific sectors (e.g. pharmacies, Künn-Nelen et al. (2013)), or when part-time employees work more than a certain level of hours (Garnero et al., 2014). All in all, these results would suggest a shorter working week should be beneficial to productivity. In this paper, we are able to provide evidence that reductions in working hours strongly increase output per hour at the firm level. However, we argue that the step from nominal output per hour to productivity is not straightforward, as the role of price effects makes it difficult to quantify the real increase in productivity.

The paper proceeds as follows. Section 2 provides an overview of the working-time legislation and describes its chronological evolution in Portugal. Section 3 discusses how the theoretical predictions around a reduction in hours differ. Section 4 presents the data that is used in the analysis. Section 5 describes the empirical strategy based on a difference-in-differences approach, as well as the different specifications employed in the paper, while Section 6 presents the results. Section 7 concludes.

2 Working Time Legislation in Portugal

2.1 General Aspects of Working Time Legislation

Working time regulations include all the legislation that limits the number of working hours a worker can work, and regulate the organization of the working week (but also day, year..). There are many aspects to working time legislation, including, for example, regulations on night-shifts, weekend work, paid leave, national holidays etc. Arguably, the most relevant to determine 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, i.e. how many hours a worker usually works or the hours specified in his/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 limit at which overtime hours begin. Overtime hours are the hours worked on top of standard hours, and are usually also limited (for example, maximum weekly hours are capped at 48 hours in the European Union), at either the daily, weekly, or yearly level, or a combination of the three. Overtime hours are paid at a higher rate, referred to as the *overtime rate*, which sets the wage increase a worker should earn on each extra hour. It is clear that the combination of the policies then affects how many workers actually work. For example, in the United States, standard hours are set at 40 similarly to many European countries, but overtime limits and premiums are much less stringent, which leads to actual hours that are much higher, on average. The opposite is true for Portugal: overtime is strictly capped and expensive, such that it is rarely used by firms, as we will show later.

2.2 Working Hours Legislation in Portugal over Time

Working time in Portugal is regulated by both the national legislation and collective agreements that can vary by sector and location, covering approximately 80% of workers. Generally, national legislation sets the upper bound, while collective agreements usually specify either lower levels and/or exceptions. The national legislation concerning working time in Portugal dates back to 1971, when the *Decreto-lei 409/71* set standard working time at 8 hours daily, and 48 hours weekly, and at 7 hours per day and 42 hours per week for office workers, with one day of mandatory rest. The law also

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 using the national legislation. *Year* refers to when the law was published in the official gazette, not the date of effective implementation. *Standard Hours* indicates the maximum usual hours specified in the national legislation, both at the daily and weekly levels. *Overtime* refers to the maximum number of hours that can be worked on top of standard hours, by paying the *overtime premium*: the first number refers to the first hour of overtime, and the second to the second hour. All reforms are in bold, see text for some remaining aspects of working time legislation not covered in the Table. The law specifies different working hours for office workers (7 hour day, and 42 hour week).

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 9 hours for other workers, with, respectively, a working week of 40 and 44 hours.³ As shown in Figure A1 in the Appendix, most workers were already on a Monday-to-Friday working week in the early 1990s, although an 8-hour maximum and a 44-hour week would have required them to work at least half a day on the weekend.⁴ However, around 80% of full-time workers were not working weekends already in 1992, which implies they were either on a 9 hour day (8 hour day if office workers) and 44 hour working week (40 hour if office workers.) We summarize the main changes in the national legislation that have occurred in Table 1.

The law also allowed for 2 daily hours of overtime, and a maximum of 240 per year. The first hour of overtime was paid a 25% premium, and the second a 50% premium. Yearly overtime was first reduced from 240 to 160 hours in 1983, and the premium increased to 50% for the first hour, and 75%

³Because employers had to give an additional day of rest, the standard weekly hours decrease to 45 (from 48) for workers, and to 40 (from 42) for office workers.

⁴Unfortunately, we are unable to get the same statistic for the late 80s, but it is likely to be very similar: by looking at the distribution of hours in the labor force surveys (Figure A2, we see that prior to 1991 most workers were indeed at 45 hours week, and only a few were working 48 hours.

for the second. It is interesting to note that the text of this law explicitly mentioned “work-sharing as the rationale” for this reform.⁵

Reforms in the length of the working week in Portugal began in 1990, when the government, unions, and employers’ associations signed a tripartite agreement lowering the length of the working week to 44h, effective immediately, with a memorandum of understanding that the working week would be lowered further, to 40 hours, by 1995, but by means of collective agreement. While formally the national legislation prior to 1991 specified a usual working week of 48h, *de facto* the usual working week lasted 45 hours, because employers could increase daily hours up to 9 (instead of 8), but by giving an additional rest day. So the usual length was a 5-day week, with a 9-hour day, and 45 weekly hours. As from 1991, this was decreased to 44 hours. The non-binding memorandum of understanding to lower hours to 40 by 1995 had only a limited efficacy. As we will show later, some collective agreements reformed hours but most did not, such that, when elections took place in Portugal in 1996 and the socialist party came into government, most workers were still working above 40 hours per week. The decision was then taken by the government to unilaterally decrease hours to 40 took place in 1996, through national legislation and not collective agreements. The reform allowed for an adjustment period of 1 year; the limit was first lowered to 42 hours in 1997 and then to 40 in 1998. This time the reform did not allow for any increase in overtime, but only for a longer reference period over which to average standard hours. The law had no specific provision for what should happen to salaries and wages as hours fell, but, as we will see later, monthly salaries did not adjust and this resulted in significant wage increases. Lastly, it is key to reiterate that, contrary to the French reform, no compensating measures were put in place (such as cuts in social security contributions.) This allows for, we argue, a much cleaner interpretation of the effects of reductions in standard hours at the firm-level.

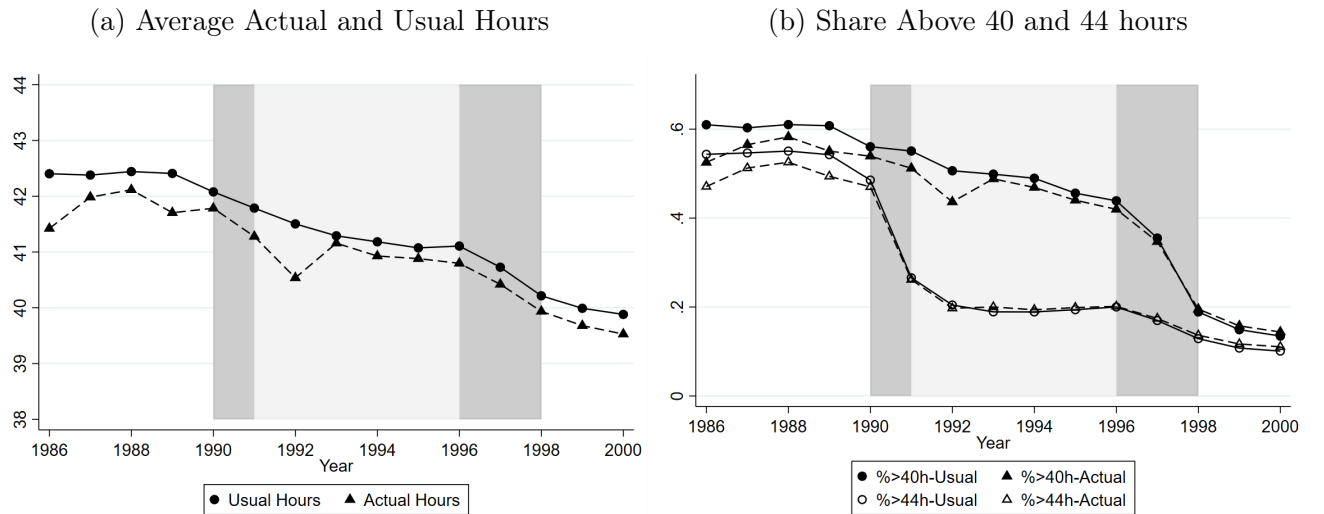
The effects of the period of reform are clearly visible in Figure 1, where average hours and the share of full-time workers above 40 and 44 hours a week are plotted over the period 1986 to 2004. Hours appear relatively stable before 1990, when, as expected, a decline begins. This decline continues

⁵“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).

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 A2 in the Online Appendix). The distribution of hours that peaked at 45 hours before 1990 shifts to 44 and lower hours. After 1998, when the second national reform is fully implemented, the peak is clearly at 40 hours.

We observe a similar trend in working hours recorded in the administrative dataset used throughout this paper.⁶ 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 1: Average Actual and Usual Hours of Full-Time Workers and Share Above 40 and 44 hours, 1986 to 2004, EU Labor Force Survey

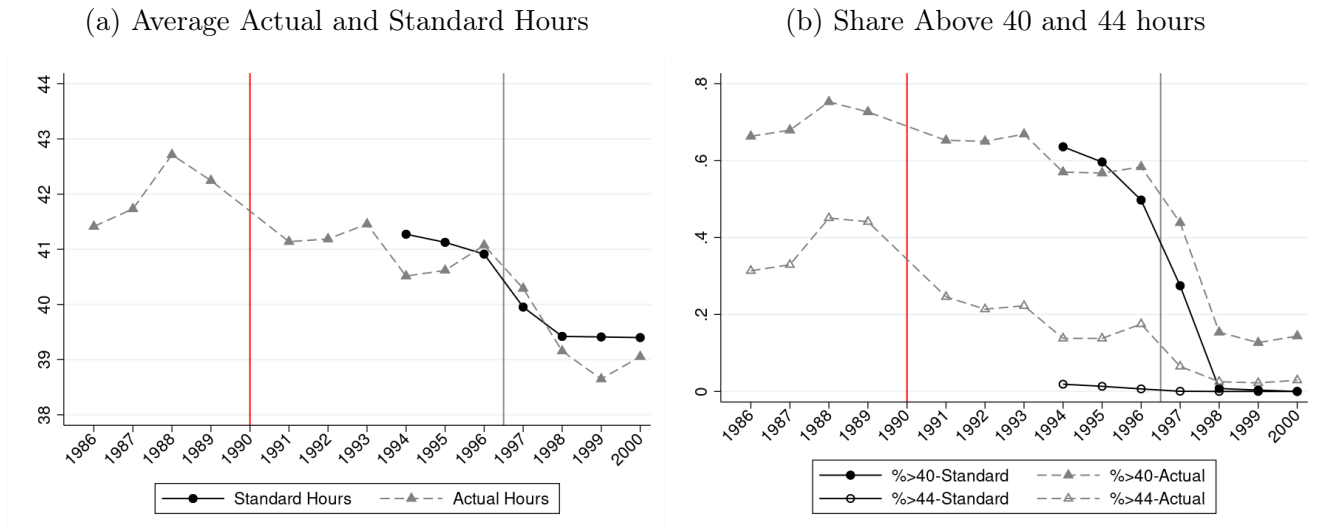


Note: The darker shaded area indicates the period of the national reform, while the lighter gray indicates the period of reform of collective agreements. The LFS measures both weekly hours usually worked throughout the year, and actual hours worked in the past week. The sampling structure of the LFS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

Source: Authors' calculations on EU-LFS data.

⁶Details on the data are provided in the following Data and Descriptive Statistics section.

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



Note: The panel (a) shows the average standard and actual hours for non-agricultural workers working between 30h and 55h. The panel (b) shows the share of workers with standard hours and actual hours above 40h and 44h, respectively. Standard hours are available in the QP only from 1994. See Figure A5 which replicates the figures with imputed standard hours. Actual working hours were derived by dividing the actual hours worked in the reference month by 21.626, which is the average number of working days per month. The red vertical line indicates the reform reducing the standard hours from 45h to 44h and the commencement of the tripartite agreements encouraging the reduction of hours through collective agreement. The gray vertical line indicates the national reform that reduced the standard hours from 44h to 40h.

Source: Authors' calculations based on the QP.

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 firms made use of paid overtime, and this share did not change even as standard hours were reduced.

3 Conceptual Framework

The theoretical underpinnings of the effects of reductions in standard hours are well-documented. In its simplest version, without fixed costs of employment and exogenous wages, a standard labor demand model would predict that a reduction in hours would unambiguously increase employment.⁷ Profit-maximizing firms would substitute hours for workers, such that a reduction in hours would certainly result in a higher number of workers. This argument is what is known as “work-sharing”, and its simplicity and intuitiveness can probably explain its appeal in the public and political debate. However, it is enough to add a fixed cost component to employment and firms’ endogenous overtime response to significantly complicate the predictions. This was shown by Calmfors and Hoel (1988), a seminal paper in the literature. The authors argue that, if overtime and the overtime premium are added in the model alongside a fixed cost for each worker, then the predictions of the effect on labor demand become much more ambiguous. The effect on employment will depend on hours before the reform, and whether firms are cost-minimizers, hence facing a fixed demand, or profit-maximizers, which can adjust output accordingly. The cost-minimization case, where output is fixed, is more likely to give rise to work-sharing. However, even in this scenario, the effect on employment

⁷As 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).

is ambiguous once overtime is considered. This is because lowering standard hours reduces the cost of an overtime hour relative to the cost per worker, with the perverse effect that firms may actually substitute overtime for workers. This implies that a reduction in standard hours could result in being counterproductive and lead to workers working more actual hours, as firms increase overtime and employ their workers more intensively. However, given the high overtime premium and the scarcity of overtime in Portugal, we argue that this channel is unlikely to be important in our setting. On the contrary, the high overtime premium makes work-sharing more likely, as firms would be more prone to hiring new workers. The other main take-away from Calmfors and Hoel (1988) is that, if firms are profit-maximizing and output is not fixed, then reducing working hours produces a negative scale effect on output, which in turn makes the employment effect even more ambiguous, and likely to be negative. This is because the cost per worker, which includes a fixed cost, increases even if hourly wages remain fixed.

Up until now we have considered models where the hourly wage is exogenous, and does not change as a result of the change in hours, but this does not need to be the case. Trejo (1991) proposes a “fixed-job” model as opposed to a “fixed-wage” model. In this setting, firms are able to fully adjust hourly wages and overtime such that the monthly salary of workers does not change. In this model, a legislative reform in hours would have no real effects, not even on the level of hours worked. But this is not what we observe in the case of the European reforms, where, on the contrary, hours were usually lowered at constant earnings, such that the hourly wage increased significantly. As we will see later, this is also clearly the case of the Portuguese reform in 1996 and of most reforms to standard hours, with the exception of the Canadian case (Skuterud, 2007), where monthly salaries could and did adjust to compensate for the lower hours. Formally, the scenario in which nominal monthly wages do not adjust is treated by Crépon and Kramarz (2002). When nominal monthly salaries do not adjust, and hourly wages go up, this further exacerbates the negative scale effect on employment shown by Calmfors and Hoel (1988). Moreover, as Crépon and Kramarz (2002) point out, it also has an impact on worker flows, as firms have the incentive to fire workers hired with the old standard, and hire new workers at lower wages with the new standard. This last point reiterates the importance of studying employment at the firm level: an increase in the separation rate for incumbent workers,

which is what some studies have shown, does not necessarily mean an overall negative effect on labor demand if the hiring rate also increases.

As already pointed out by Boeri and Van Ours (2013) and Raposo and Van Ours (2010), monopsonistic power in the labor market could provide a justification for legislative reductions in standard hours, as wages are less than the marginal product of labor and working hours are longer than what would be optimal for the worker.⁸ Moreover, the predictions of standard models could again change drastically if we move away from perfect labor markets and perfectly competitive product markets. This relates to the debate about the “elusive employment effect of the minimum wage” (Manning, 2021).⁹ In other words, if we move away from perfectly competitive labor markets, and take into account the potentially positive effects on labor supply, the effects of an increase in the hourly wage due to lower working hours become even less obvious. To this logic, we add that the models cited above assumed perfectly competitive goods and services markets where firms are price-takers, which made the negative scale effect on output unambiguously negative. If, instead, firms enjoy some market power and are able to adjust their prices to compensate for the higher labor costs, then the predicted effects again become ambiguous.

To sum up, the theoretical predictions of the effects of reductions in standard hours are less than clear-cut and necessarily depend on many assumptions. The work-sharing argument, while theoretically possible, holds only under very strong conditions. A negative employment effect, on the other hand, seems likely in competitive markets but more dubious otherwise. In short, this is an empirical question, which we try to address in the paper. This is even more the case for predicted effects on productivity. The effect on workers’ productivity will crucially depend on assumptions about the shape of the production function, and on whether and where marginal returns to hours are increasing or diminishing.

⁸“Another reason for a mandatory reduction of working hours arises when employers have monopsony power. Manning (2003) argues that in a monopsony not only the wage rate is less than the value of marginal product but the firm can also induce the worker to work more than would be optimal for the worker given the monopsony wage. In the same way as a minimum wage can be welfare improving in case of a monopsony, working hours reduction can be welfare improving.” (Raposo and Van Ours, 2010)

⁹“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)

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 covers the period from 1986 to 2016. The QP is collected in a specific month every year; until 1994, this snapshot took place in the month of March, since then, it has changed to October. We call this month as “reference month” in this paper. The full information available in the QP is specified in the Online Appendix A.2: it includes hours, wages, firm and establishment code, sales at the firm level, and several characteristics of the worker and the firm.¹⁰ Importantly, it also collects information on which collective agreement covers a given worker. The data at our disposal has two gaps, in 1990 and 2001.¹¹ 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. As discussed before, standard hours are defined as hours normally worked, by contract, in a week, which can be averaged over a certain period, called the reference period. However, the QP measures actual hours, i.e. the hours actually worked during the entire reference month (i.e., March until 1993, October since 1994). While there is certainly a strong correlation between the two, they are not identical. Moreover, actual hours can (and do) fluctuate much more than standard hours due to business cycles and idiosyncratic factors.¹² The fluctuation in the actual hours in the QP also comes from the difference in number of working days (excluding Saturdays, Sundays and national holidays) in the reference month of each year. As shown in the Appendix Figure A6, this can vary between 20 and 23 days per month, depending on the number of weekends and national holidays that fall within a given month.

This distinction between actual hours and standard hours will be important in our definition

¹⁰Sales in the QP refers to annual sales in the previous year. Therefore, we created an alternative sales variable containing the sales of the corresponding year. In what follows, analyses based on sales are based on this variable.

¹¹Specifically, there are no worker files for these years.

¹²For 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.

of firms’ treatment status later in the paper. In particular, defining a treatment status based on actual hours can be problematic because they may correlate with the economic cycle, which can be a potential source of endogeneity bias in the econometric estimations. For this reason, for years prior to 1994, we imputed standard hours from the actual hours worked in the month, to obtain a more stable measure. Appendix section A.2.3 outlines the method to impute the standard hours. The data also contains information about overtime hours, meaning that we know the number of hours paid at an overtime premium rate, and whether firms increase overtime after the reduction in standard hours.

For descriptive purposes, we also make use of the *Labor Force Survey*, in the harmonized version available through Eurostat. Contrary to the QP, this data has information on usual hours worked in a reference week, and a measure of *self-reported* actual hours worked. In the LFS, workers directly report how many hours they work, so it is a different measure than administrative hour, which also takes into account any informal or unaccounted overtime hours. This allows us to show that the reform reflected in “real” hours, and not only in those reported in administrative datasets (contract or actual hours, and paid overtime hours). All relevant variables across our data sources are outlined in the Online Appendix A.2.

4.2 General Sample Selection

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

More importantly, we limit our analysis to the whole Manufacturing sector and Service sector (i.e., Retail, Wholesale, and Hotel and restaurant). These sectors account for 64% of the QP sample in terms of workers, and 63% in terms of firms. The reason is that the QP is not representative of firms outside of these sectors, such as public administration, education and health, electricity and gas, that have a strong public component. We also exclude the Finance & Real Estate sectors, which tend to have lower hours since the beginning of the data period, have little variation in terms of hours within

the sector, and within which the concept of working hours are 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 either at the top 1% or the bottom 1%. Excluding these extreme outliers is necessary to account for the strong dynamics in some firms that would otherwise increase significantly the noise in the estimations.

5 Empirical Strategy

5.1 Main Estimation

The main objective in this paper is to estimate the impact of the working hour reductions on firm-level outcomes. As noted earlier, the reduction in working hours in Portugal from 44h to 40h was realized in the form of collective agreements and the nationwide reform in 1996. The main focus of our estimation is the national 1996 reform for multiple reasons: first, the majority of firms reduced working hours through the national reform; second, the reform created a sharp shock in the working hours that applied to all the firms nationwide; importantly, the timing of reform was unanticipated until the election that brought the change in government in 1996. Therefore, the treatment and its timing can be considered exogenous, providing an ideal case study from the viewpoint of identification. Studying the effects of the collective agreement is more challenging because precise information on the timing and the content of the collective agreement is likely.¹³ This is further complicated by the lack of coherent collective agreement codes across years and unavailability of contractual hours before 1993 in our data. Nonetheless, we do our best to also provide estimated effects of the working hour reductions through collective agreement. The main difference of this treatment from the national reform is that the reduction in hours occurred gradually over the longer period and, in some cases,

¹³We tried to identify which collective agreement agreed on reducing the hours and its timing from the published labor and employment bulletins, available at: <https://bte.gep.msess.gov.pt/>. However, we did not succeeded in extracting exhaustive (and reliable) information on the agreements related to working hours and in linking each signed agreement to the collective agreement code in the QP.

not fully (e.g. 44h to 42h). We take the unified approach with the same control group for the both types of the treatment to allow the possible comparison of the treatment effects.

5.1.1 Treatment Definition

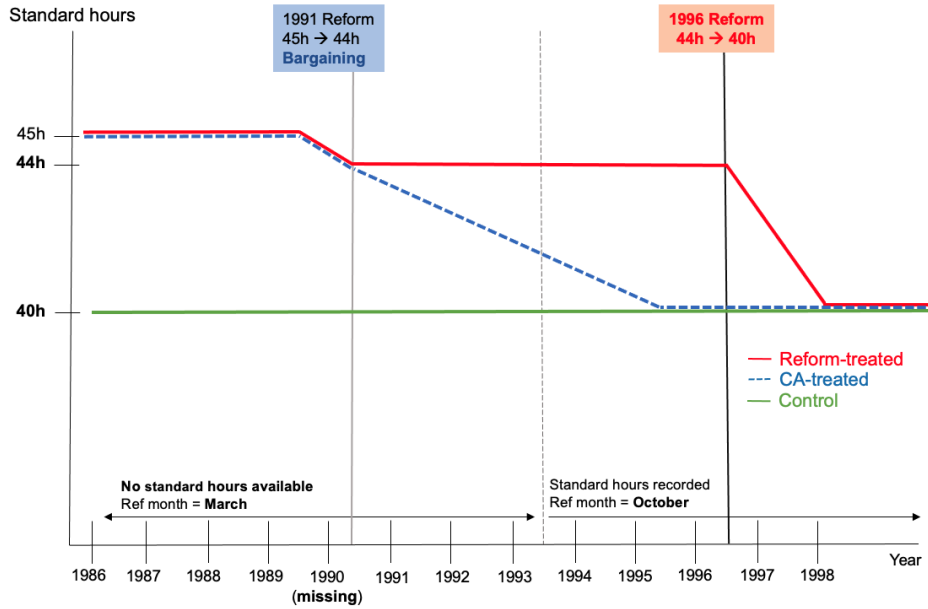
Our main estimation studies the treatment through the national reform (Ref-T), as well as through bargaining (CA-T). We plot in Figure 3 in a stylized way the main intuition behind our main empirical strategy. The idea is to use the firms that always have lower hours that were not or less affected by the legislative changes as the control group in the estimation. Then, we apply the difference-in-differences to compare the outcomes of the firms treated by the national reform to that of the control group to estimate the effects of the 1996 reform. Using the same control group, we also provide the effects of the treatment through collective agreement. We identify the three different groups following these steps:

1. **Control group** (in green in Figure 3): we consider firms that were not affected by regulatory changes or collective agreements due to having fewer working hours since before the beginning of the reform period. We define these firms as the ones in the lowest quartile in terms of the average standard hours between 1986 and 1996. In general, this group includes two types of firms: i) firms that already had a 40-hour workweek due to their existing collective agreements before the changes; ii) firms that had fewer hours than specified by the collective agreement. We use the bottom quartile of firms as our threshold (instead of the fixed 40-hour mark) in order to expand the size of the control group, which allows us to have more precision in estimates and include sector-year fixed effects in the estimation.¹⁴ As a result, our control group essentially consists of firms that *less* affected by the reform (rather than completely unaffected). Furthermore, we base our analysis on the average standard hours from 1986 to 1996, covering a longer time span rather than just the years before the initial reform in 1991, in order to limit the issue of mean reversion.¹⁵

¹⁴Although the choice of the lowest fourth cutoff is arbitrary, we show that the results are robust to alternative thresholds such as the bottom fifth or the bottom third.

¹⁵Since our standard hours prior to 1994 are imputed from actual hours worked, which tend to fluctuate due to economic cycles, year-to-year economic fluctuations, and the count of working days in the reference month, mean reversion is an

Figure 3: Stylized Evolution of Working Hours According to Different Groups



Note: The figure illustrates the stylized evolution of standard hours from 1986 to 2000. Two vertical lines in the graph mark the following events: (i) the first line corresponds to the 1991 reform when the maximum standard hours were reduced from 45 hours to 44 hours. This reform laid the foundation for further reductions through collective agreements; and (ii) the second line marks the nationwide reform in 1996, which reduced the maximum standard hours from 44 to 40 hours. The green solid line represents the hypothetical scenario where firms consistently maintained lower working hours, thus remaining unaffected by these reforms (Control group). The red line illustrates the typical evolution of hours for firms treated by the 1996 national reform. These firms kept their standard hours at 44 throughout the 1990s before the reform took effect (Reform-treated group). Lastly, the blue dashed line shows the evolution of standard hours reduced through collective agreement (CA-treated group). It is important to note that in the QP, standard hours are available only starting from 1994, and the data collection month also changed from March to October in 1994. Additionally, worker files containing hours for the year 1990 is missing.

2. **CA-treated group** (in blue in Figure 3) : This refers to the group of firms that were treated because their collective agreement signed on the reduction in working hours.¹⁶ It is important to note that is the upper-level bargaining process that negotiated the reduction on the working hours, rather than individual firms and workers. However, once the collective agreement is signed, it applied to all workers covered by that agreement. We define Collective agreement treated (CA-T) firms as those not in Control group that are covered by collective agreements

issue. Because of this, if we were to define the control group based solely on the average standard hours over 4 years from 1986 to 1989, the hours (and economic outcomes) would display a sudden jump right after 1989 due to a tendency to return to the mean. This is why we opt for calculating averages over a longer period. We have tested that our findings remain consistent when we consider different time frames.

¹⁶Note that, in Portugal, most workers are covered by a collective agreement. In the vast majority of cases, employees within the same firm share the same collective agreement due to the substantial sectoral and geographical alignment of these agreements. Therefore, we use the most frequent collective agreement among workers as the collective agreement applicable to each firm.

that reduced the mode of standard hours by at least 2h between the period 1991-1996.¹⁷

We provide details in the Online Appendix about how we merge various collective agreements codes (recorded in the QP) over time, aiming at creating a balanced panel of collective agreements. Then, we identify if a collective agreement reforms, and the year in which it reforms, by measuring when the mode of hours (i.e. the most frequent value within that collective agreement) changes by at least 1 hours. Note that some collective agreements within this category did not entirely reduce the hours to 40 hours, and consequently, they still experienced partial treatment from the subsequent 1996 reform.

3. **Reform-treated group** (in red in Figure 3): this is defined as the group of firms that had long working hours until 1996 and was forced to reduce standard hours by the 1996 national reform. We define reform treated (Ref-T) as those neither in group (1) nor group (2). Note that firms whose collective agreement's mode of standard hours decreased only by an hour between 1991 and 1996 are included in this group.

5.1.2 Specification

Our estimation strategy is difference-in-differences. We use firms in the Control group as a common comparison group against the Ref-T group and the CA-T group. After defining treatment status in the way described above, we run the following regression:

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

¹⁷A collective agreement is viewed as treated if the overall change in mode of the standard hours covered the agreement decreased by at least 2 hours between 1991 and 1996. Then we identify the year in which the mode decreased for the first time or the share of workers at the mode decreased by 10 percentage point as the first year of the treatment. We also take into account the fact that some collective agreement could have started negotiations in 1990 and the CA-treatment had started already in 1991. To identify these collective agreements, we first compute the mode of standard hours before 1989 (not using only 1989 to avoid mean reversion). Then, if pre-1989 mode of hours is 45h, then we assume that the treatment by collective agreement started in 1991 if the mode change by 2h or more by 1991. We impose a 2h reduction to distinguish from the first reform's effect on changing the maximum standard hours from 45h to 44h; we also consider as treated in 1991 if the pre-1989 mode is 44h or below and the mode declines by at least 1h by 1991. In effect, a small number of collective agreements were identified as started to be treated by 1991.

where Y_{jt} are the outcomes of interest for firm j at year t ; γ_j and $\delta_{s(j)t}$ are firm fixed effects and sector-year fixed effects, respectively.¹⁸ T_j can indicate two different types of treatment: i) reform-treated (Ref-T), again compared to not affected firms, i) collective agreement treated (CA-T), compared to not affected firms. We run this in two separate estimations, thereby estimating two separate β_t for reform-treated firms, and for CA-treated firms. We cluster standard errors at the firm level, in order to account for panel nature of the data. β_t are the coefficients of interest, that identify the dynamic effects of the treatment. β_t is normalized as zero for one year before the start of the treatment (i.e., β_{1996} for the Reform-treatment and β_{1989} for CA-treatment, respectively).

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

¹⁸Sector refers to the four large sectors of Manufacturing, Wholesale, Retail, and Hotel and restaurants.

time.¹⁹ In the case of employment, for example, it is important to determine whether we expect the same employment change in percentage or in number of workers. In addition to the case of hours, where the absence of a trend allows us to estimate the effect both on units or logs, we put all our outcomes in logarithmic form, since the analysis of the pretrend shows that the relative evolution is more likely to hold for firms starting from different levels²⁰ More importantly, we will show that in some comparisons the parallel trend assumption is unlikely to hold in our setting: treated firms experience different trends relative to those with low hours since the beginning of the period. In these cases, we graphically examine if the pre-existing trend exhibits a significant break following to the treatment, and identify the net-of-trend effects by introducing firm-specific linear trend in the estimation, the approach taken in Dustmann et al. (2022). Moreover, we also provide the results with alternative specification described later, on different samples, with which we can reasonably argue parallel trends and show similar results.

5.1.3 Sample Definition

On top of the general sample selection described in Section 4, we limit to sample of firms that we observe in the beginning of the sample, that is 1986. We estimate the treatment effects over the whole period of our data from 1986 to 2000.²¹

5.1.4 Descriptive Statistics

Table 2 provides the characteristics of the firms that belong to each of the Control, CA-treated, and Reform-treated groups. In general, the firms in the control group are relatively more productive: their mean firm size is larger, pay higher wages and record more sales, relative to the CA-treated or Reform-treated firms. On the other hand, these characteristics are comparable between Ref-T firms and CA-T firms. Nearly half of the CA-T firms are located in the Lisbon metropolitan area, which is similar to the Control, but much higher than the Reform-treated. In terms of the sector

¹⁹The only case in which this is not true is if there is no trend, i.e. the evolution is flat.

²⁰Taking the log also has the advantage of helping us to deal with outliers due to large firms, which are particularly troublesome for wages and sales, without having to significantly trim the sample.

²¹We stop the estimation in the year 2000 due to the data gap in 2001. Moreover, given that treatment has to be defined prior to 1991, firm exit from the labor market makes it difficult to estimate precise effects long after.

Table 2: Comparison of Treatment Groups (1986-1989)

	Control	CA-treated	Reform-treated
<i>Firm characteristics</i>			
Average firm size	25.3	15.8	17.9
Mean wage (in euro)	3.1	2.5	2.3
Median sales (in euro)	392,242	255,960	238,601
<i>Location</i>			
Lisbon metropolitan area	0.43	0.48	0.24
<i>Sector composition</i>			
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: 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.

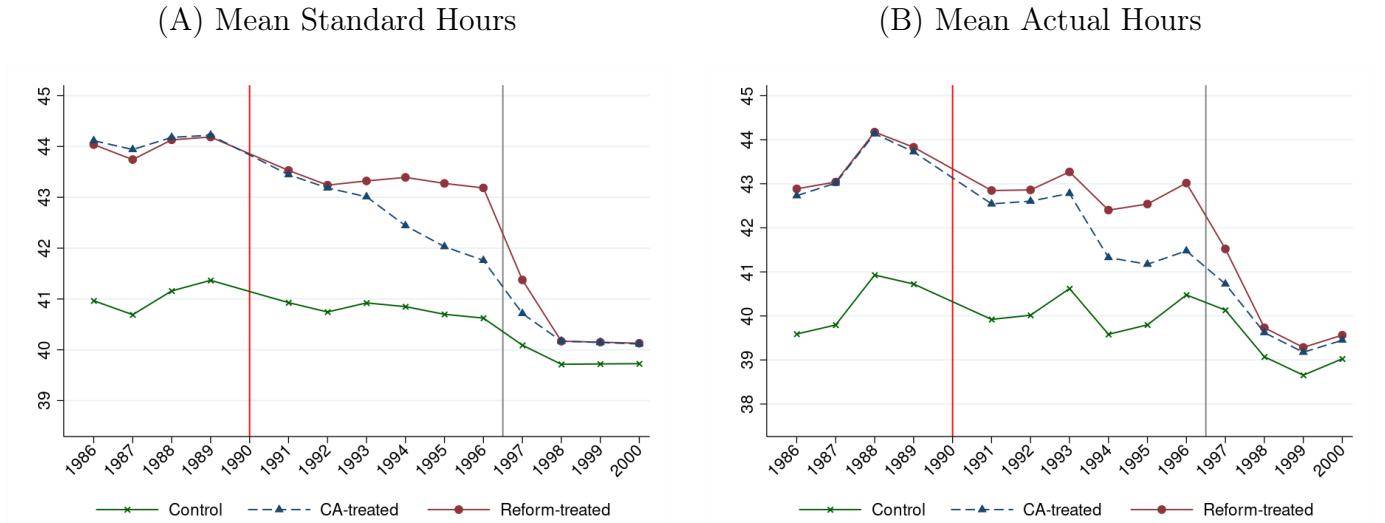
distributions, in the Control group, sectors are relatively evenly distributed apart from the hotel and restaurant sector. The CA-T group has a higher share in the manufacturing and wholesale sectors. Compared to CA-T, Ref-T firms have a higher share of hotel and restaurant sector, but a lower share of manufacturing. Appendix Figure A9 shows graphically the distribution of the treatment groups across sectors.

Figure 4 shows clear distinction in the evolution of hours across the three groups. In Panel (A), the mean of the firm-level average standard hours of the Control group firms was stably around 41h before 1996. Note that it is because we defined the control group as the bottom one-fourth in terms of the mean standard hours over the 1986-1996 period that their level of hours is higher than 40h. Some firms in this group are also affected by the regulatory changes from 1989 to 1991 (45h to 44h) and from 1996 to 1998 (44h to 40h), appearing as a decline in hours in the figure.²² The red line shows

²²Note that the change in hours from 1989 to 1991 observed across all groups also reflect the change in number of working

that the Ref-T firms reduced hours from 1989 to 1991 due to the change in the maximum standard hours from 45h to 44h (and the change in the number of working days). After the stability in the hours from 1991 to 1996, this group was strongly hit by the national reform, going from just above 43h to 40h by 1998. Finally, the blue dashed line shows the evolution for the CA-T firms. After the stability in the 1980s, the average of the mean standard hours declined from 44h to slightly below 42h from 1989 to 1996; it reached close to 40h only in 1998, because some collective agreements reduced the hours only partially (e.g. 44h to 42h). Panel (B) in the figure shows the counterpart of the actual hours. While there are more fluctuations because of the differences in number of working days in the survey month across years, the evolution of actual hours is similar to the one observed in the mean standard hours.

Figure 4: Evolution of Hours Across Treatment Groups



Note: The figure shows the evolution of the average of firm-level mean hours for the three groups: Control, CA-treated, and Reform-treated. Panel (A) shows the evolution of standard hours, while panel (B) shows the evolution of actual hours worked. These mean hours are computed from workers who had at least 30 hours in the respective hour category. The large fluctuations in actual hours are due to the variations in the number of working days within the reference month across different years, as shown in Appendix Figure A6.

6 Results

6.1 Results of 1996 reform

6.1.1 Dynamic effects

Our first results are the dynamic treatment effects of the 1996 reform estimated in the difference-in-differences as specified in the equation 1. Dependent variables are expressed in log, with the exception of hour outcomes, which are expressed in the absolute terms. The firm-level outcomes that are computed from the worker-level information (i.e. hours, wages and salaries) are based only on workers who performed at least 30 hours of work. We dealt with the missing values by assigning the median value of each firm. Note again that 1990 is omitted from the analyses due to the lack of a worker file for this year.

Figure 5 illustrates the effects of the 1996 national reform on hours, wages, and monthly salaries. Given our 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 departure from the pre-existing trend. To provide a visual representation of this, we also present dynamic effect results adjusted for the pre-existing trend, borrowing the method from Dustmann et al. (2022). We fit a linear trend based on the estimated dynamic coefficients from 1986 to 1996, and use the estimated trend coefficient to predict outcomes values across the entire time period. These predicted values are then subtracted from the initial dynamic coefficients, with the re-centering at the year 1996. We retain the standard errors from the original regression, since the purpose here is solely to visualize the deviations from the trend in the post-intervention period. To provide a formal estimate, we estimate the treatment effects based on the regression that control for the firm-specific trend. The estimates provide consistent results implied by the detrended figures in most of the cases.

Hours, wages and salaries. Panel (A1) in the figure shows the effect on average weekly standard hours. The coefficients leading up to 1996 are close to zero, indicating a similar evolution in standard hours between the reform-treated firms and the control firms prior the reform. The effect of the 1996 reform is evident: between 1996 and 1998, the Ref-T firms experienced an average reduction of 2

hours in mean standard hours compared to the control group. In relative terms, this amounts to roughly a 5% decline in mean standard hours. Similarly, Panel (B1) exhibits a comparable result for mean weekly actual hours worked, demonstrating a sharp decrease of 2 hours following the reform.

As mentioned earlier in the institutional context, firms cannot reduce a worker's monthly earnings when their standard hours are reduced. Consequently, the reduction in working hours results in an increase in the hourly wage rate, a trend corroborated by Panel (C1) which outlines the effects on mean hourly wage. After a gradual decline in the mean hourly wage rate for Ref-T firms in contrast to the Control group until 1996, the wage rate experiences a sharp increase following the reform's implementation. In comparison to the pre-trend, there is nearly a 6% rise in the mean wage rate after the completion of the reform. Consistently, Panel (D) demonstrates that the mean monthly salary remains unaffected, and if anything, there is a positive impact on monthly salary relative to a long-term pre-trend.²³ A small positive effect on mean monthly salary might be linked to the change in the composition of workers after the reform, caused by a negative impact on employment, as shown later. In summary, these findings highlight that the effects of the reform align with the institutional context: working hours reduced from 1996 to 1998 without any adverse impact on monthly remunerations. Consequently, labor costs, measured through hourly wage rates, experienced a corresponding increase.

²³However, this observation remains inconclusive because the deviation from the pre-trend depends on the starting point of the pre-treatment period. Just before the 1996 reform, from 1994 to 1996, the differences in trends are minimal, and the effects in the post-treatment period do not appear to significantly diverge from these trends.

Figure 5: Effects of 1996 Reform on Hours, Wages and Salaries

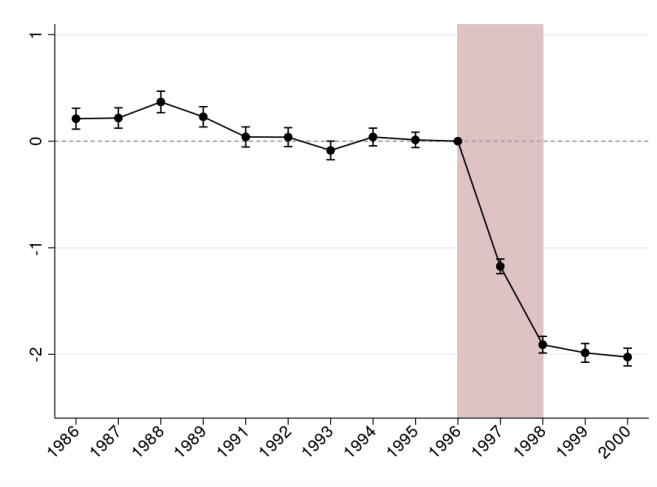
(A1) Mean Standard Hours



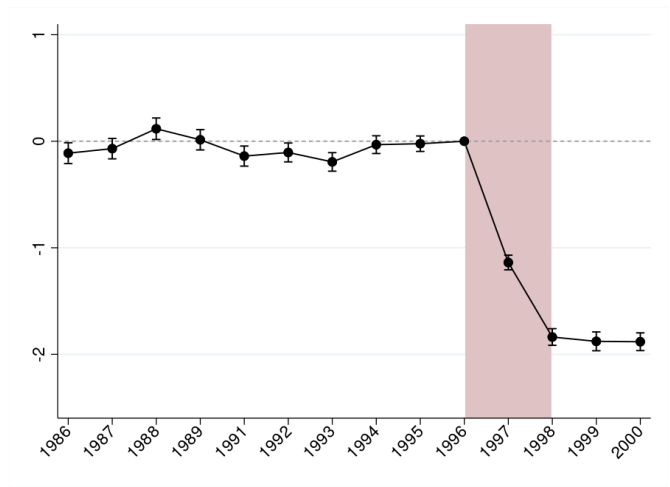
(A2) detrended



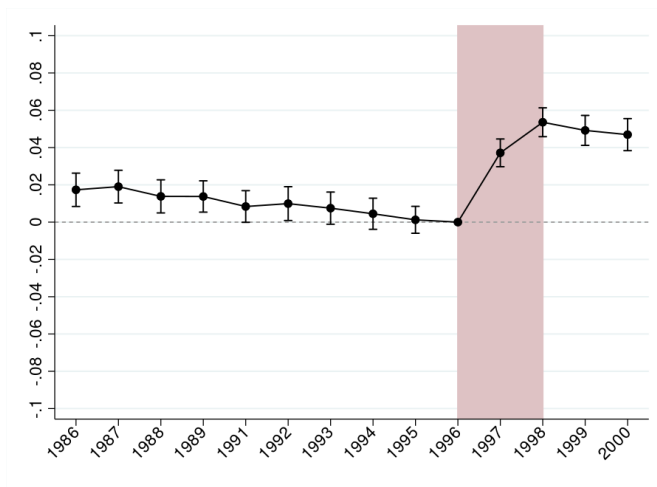
(B1) Mean Actual Hours



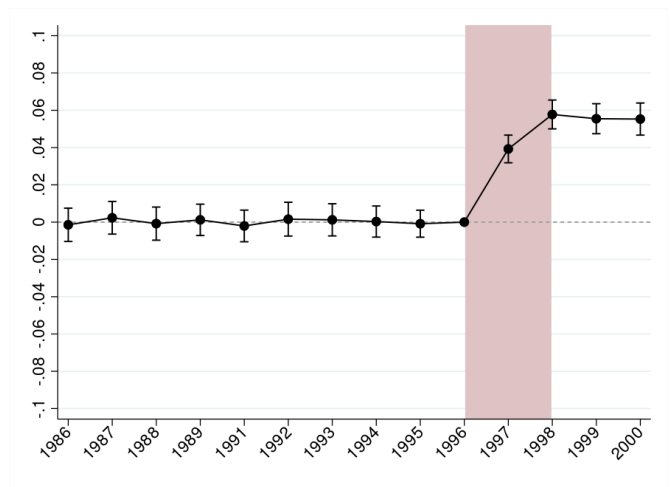
(B2) detrended



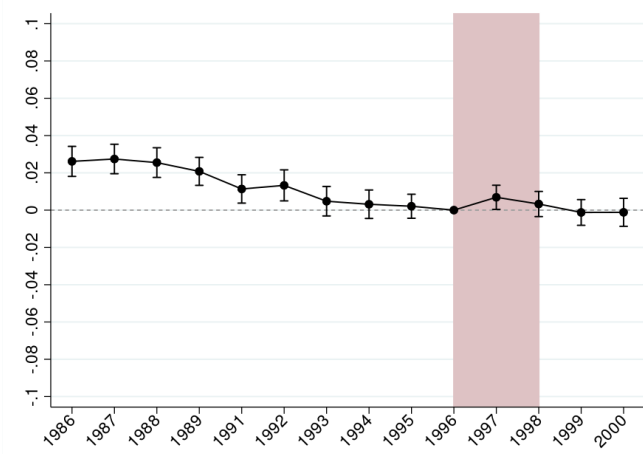
(C1) Mean Wage



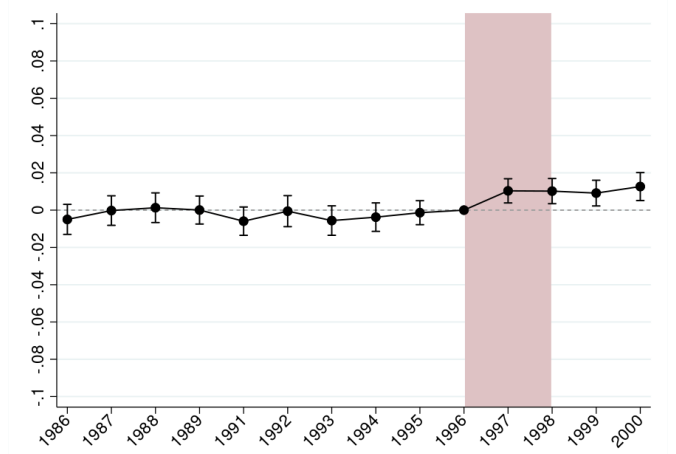
(C2) detrended



(D1) Mean Monthly Salary



(D2) detrended



Note: The left-panel figures show the dynamic effects of the reduction in working hours through the national reform in 1996 using the difference-in-differences estimation specified in the equation 1. 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 6 shows the effects of the reform on our main outcomes. In panel (A1) of the figure, the evolution of employment in pre-treatment periods were different between the reform-treated firms and the control firms. Specifically, Ref-T firms displayed relatively higher and consistent growth in employment. This could help explain why these firms were not treated through collective agreements. Reducing labor input through the working hour reductions might have been more difficult to be agreed on when the collective agreements, which have strong sector-geography dimensions, are experiencing an increase in product demand. Importantly, the figure also displays a marked shift in the trend starting from the year 1997, precisely coinciding with the implementation of the reform. The previously stable trend of employment growth vanished from this point onward or even exhibited a slight reversal. This impact becomes more 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. We confirm this reform's negative and significant effect on employment in a formal regression where we account for firm-specific trends. As discussed before, the existing theories predict that firms may respond to the forced reduction in working hours by reducing employment. This is in particular true

if it comes with an increase in per-hour labor cost, which indeed is case in the Portuguese context. One potential mechanism of labor cost affecting negatively the employment is the down-scaling of the output.²⁴ The QP allows us to test it with the firm-level annual sales information.

Panel (B1) provides the effects of the reform on sales. The coefficients in the post-reform are constantly negative and mostly significant, suggesting the output down-scaling for the firms treated by the reform. However, the ambiguity remains as panel (B2) shows that the coefficients become closer to zero, once adjusted for the pre-existing trend from 1986 to 1996. 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), as shown later in Table 3.²⁵ Nonetheless, we show later in Section 6.3 that, when we employ an alternative estimation strategy that exploits more precise and larger variation in the treatment with larger sample, we find strong evidence for the negative effect on total annual sales. Thus, both the direct effect of reducing hours and an indirect effect of the increased labor cost likely led firms to adjust their output size downward.

The implication for the productivity can differ from that for annual sale because the effects on sales, although tending to be negative, are of small magnitude. As seen in Panel (C1) and (C2), per-worker productivity doesn't seem to be significantly impacted by the reform.²⁶ On the other hand, what is evident from Panel (D1) and (D2) is that sales per hour has noticeably increased since the reform. It has increased by approximately 6% toward the end of the data period, relative to the pre-existing trend. This demonstrates that labor productivity levels were able to keep pace with the increase in wage rates.

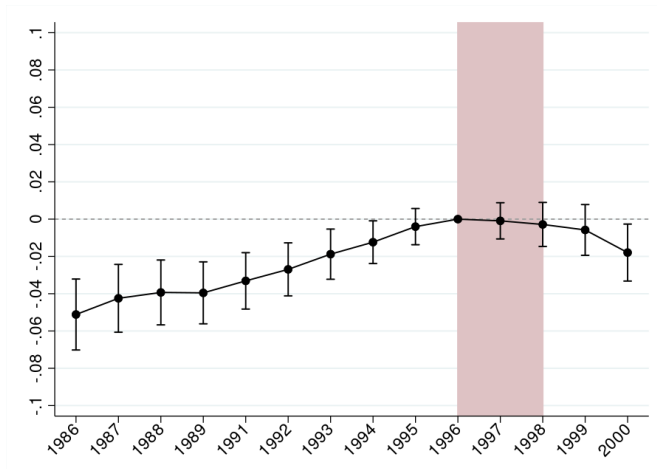
²⁴The other possibility is substitution of labor with capital in the production, if capital can adjust.

²⁵However, the null effect under the specification with firm-specific linear trend is strongly influenced by the two large coefficients in 1986 and 1987 (which we believe is related to the early turmoil from the Portugal's entry into the EU market in January 1986), that fit a strong negative trend in the estimation. If we restrict to the sample only starting from 1988, the effect on sale is actually negative and statistically significant.

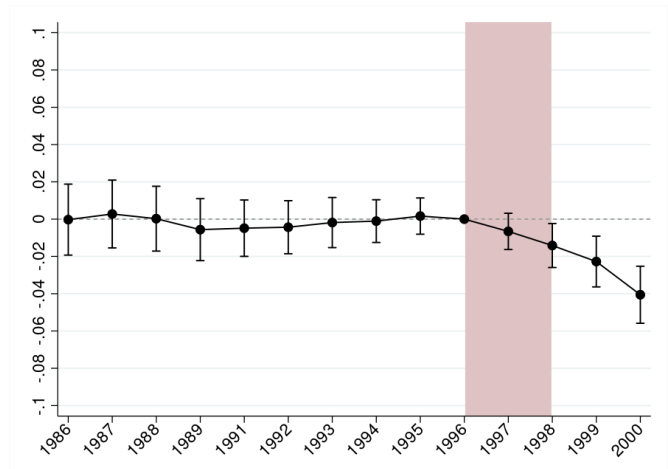
²⁶If anything, the coefficients are somewhat positive when we account for the pre-existing trend. However, when we analyze the results with firm-specific trends, there is no statistically significant effect on per-worker productivity.

Figure 6: Effects of 1996 Reform on Employment, Sales, and Productivity

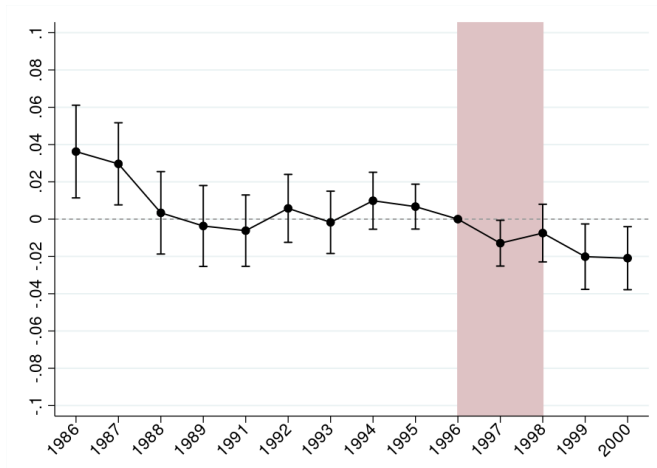
(A1) Employment



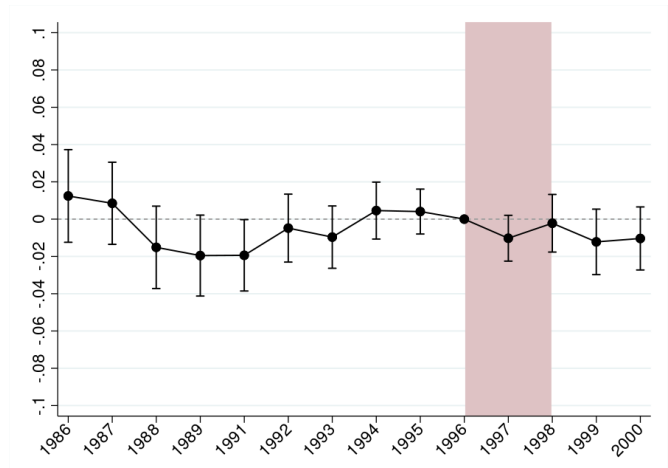
(A2) detrended



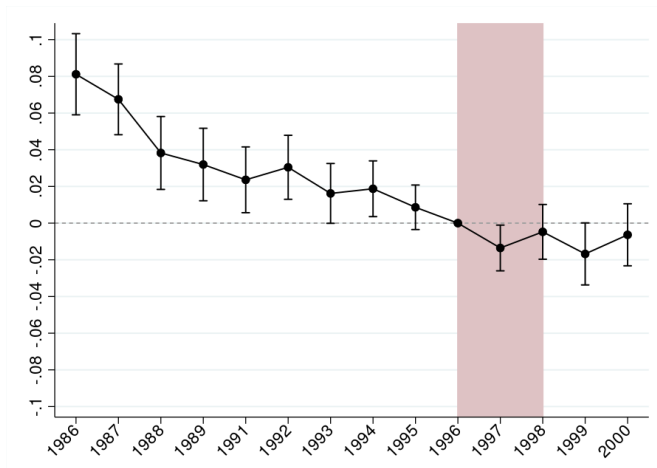
(B1) Sales



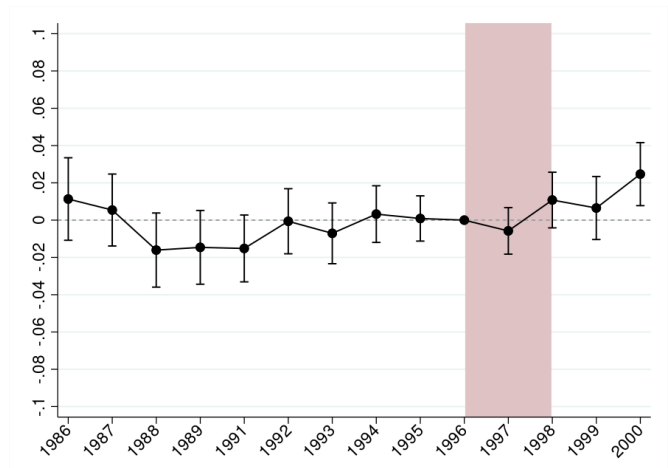
(B2) detrended



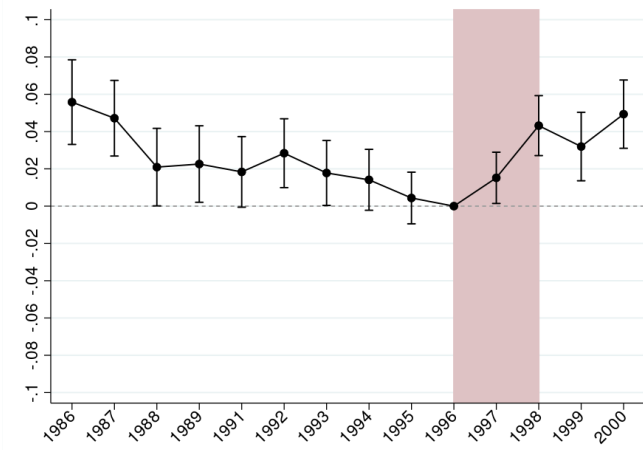
(C1) Sales per Worker



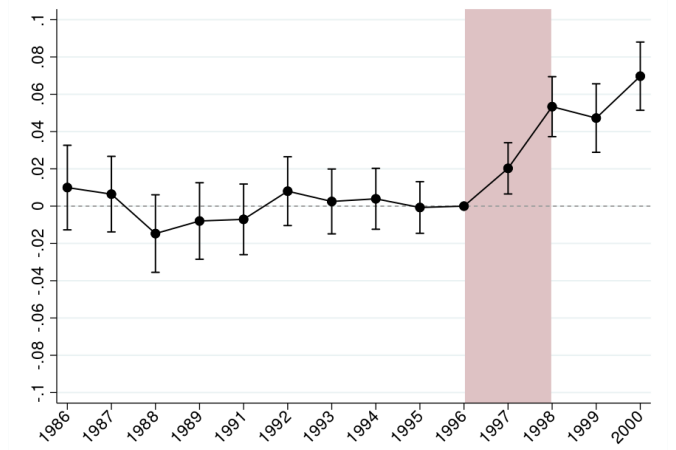
(C2) detrended



(D1) Sales per Hour



(D2) detrended



Note: The left-panel figures show the dynamic effects of the reduction in working hours through the national reform in 1996 using the difference-in-differences estimation specified in the equation 1. 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.

6.1.2 Regression Results of Reform Treatment

Table 3 presents the results of the difference-in-differences regressions, estimating the overall impact of the 1996 reform. To deal with the potential differential pre-trends, as observed in the dynamic effects for some outcomes, we control for firm-specific linear trends. Columns (1) and (2) show that, on average, mean standard hours and mean actual hours decrease by approximately 1.5 hours for the Ref-T firms in comparison to the control firms. Column (3) shows that employers did not respond to the reform by increasing overtime hours to compensate for the reduction in regular hours. This is likely due to the high cost of overtime in Portugal at that time, with a premium of 50% for the first hour and 75% for subsequent hours. The effects on the mean wage rate, as demonstrated in column (4), are positive and statistically significant, indicating a 5% increase in hourly labor costs. Column (5) indicates a modest yet significant increase of 1.3% in the average monthly salary. We believe this may be linked to changes in worker composition following the reform, driven by the negative employment effects.

Column (6) confirms the overall negative impact of the reform on employment, aligning with the graphical results in panel (A2) of Figure 6. However, the coefficient suggests a relatively minor

negative effect, resulting in a 1.4% reduction in employment in response to a 5% decrease in mean hours and an equivalent increase in hourly wage rates. After the reform, firm's total labor input decreased by 5% for the Ref-T group compared to the control group. Columns (8)-(10) present the results for sales and productivity. Column (8) indicates an absence of negative effects on sales despite the reduction in total labor input.²⁷ Worker productivity, as measured by sales-per-hour, did not exhibit a significant increase, whereas hourly productivity notably improved. Sales per hour saw a 4.3% increase, justifying the rise in wage rates.

6.2 Results of Collective Agreement Treatment

A part of the reduction in working hours occurred earlier in the 1990s through collective agreement, that had been neglected in the previous literature. We also estimate its effects in the same analytical framework as we study the 1996 reform. The effects of the collective agreement treatment may differ from the national reform in 1996 because the reduction in hours occurred gradually throughout the early-1990s and sometimes only partially.

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 for the CA-T firms from the first year of treatment in 1991 until 1996. The overall reduction is approximately 1.5h, equivalent to roughly a 5% reduction. A further reduction occurs from 1996 to 1997 because some collective agreements did not fully reduce hours to 40h and were also affected by the 1996 reform. The effect on actual hours worked per week shown in panel (B) displays similar effects on actual hours worked. Note that the estimated reduction in hours is not 4h for two reasons: first, not all collective agreements fully reduced hours to 40 but only, for example, going from 44 to 42 hours. Second, in our definition, a part of the control group firms are “less treated”, hence firms that were also partly reduced hours but to a smaller degree.

In Panel (C), we observe that the reduction in working hours resulted in a sharp increase in the labor cost. Given that firms were not allowed to adjust monthly remuneration in response to the reduction in working hours, it created a mechanical increase in hourly wages. The wage rate of the

²⁷As noted earlier, it is due to the way the linear trend fits because of the two large positive coefficients in the first two period of the sample; if we restrict to the years since 1988, the effect changes to be significant.

Table 3: The Effects of the 1996 Reform, Controlling for Firm-specific Trend

	Hours			Labor Cost		Labor Input		Sales		
	Standard (1)	Actual (2)	Overtime (3)	Wage (4)	Monthly salary (5)	Employment (6)	Total Hour (7)	Total (8)	Per Worker (9)	Per Hour (10)
$Treat \times Post$	-1.530*** (0.031)	-1.559*** (0.039)	-0.006 (0.031)	0.050*** (0.003)	0.013*** (0.003)	-0.014** (0.006)	-0.053*** (0.007)	-0.003 (0.008)	0.011 (0.008)	0.042*** (0.009)
Mean Outcome	42.2	41.6	0.3	1.0	6.2	2.0	6.9	12.8	10.8	5.9
Year FE	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Firm FE	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Sector-Year FE	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Firm-trend	No	No	No	No	No	No	No	No	No	No
R-squared	0.73	0.70	0.69	0.86	0.86	0.97	0.96	0.96	0.90	0.89
Observations	396,136	396,136	396,136	396,136	396,136	396,136	396,136	396,136	396,136	396,136

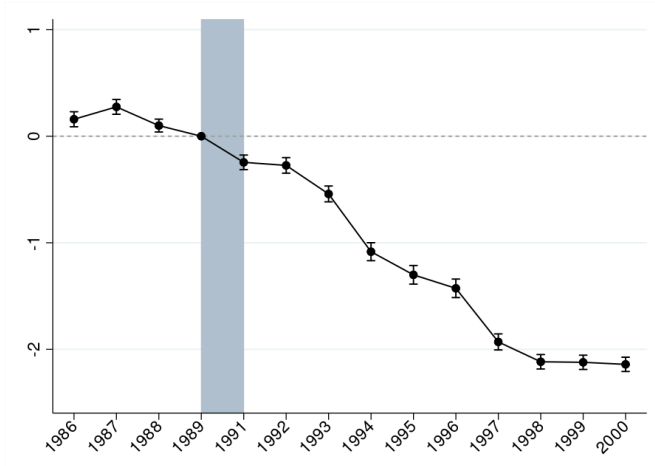
Note: The tables displays the results of the working hour reduction the 1996-reform. All outcomes are in log, except for the hour measures in the columns (1)-(3). The outcomes in the columns (1)-(5) are of the firm-level average values of the fulltime-equivalent workers who worked at least 30h per week. The variable $Treat$ takes 1 for firms in the Reform-treatment group and 0 for the firms in the Control group. $Post$ takes 1 the years after 1997 and 0 otherwise. The outcome variables are regressed on the interaction of $Treat$ and $Post$ to provide the difference-in-differences estimate of the effects of the working hour reductions. Standard errors are clustered at the firm level. Standard errors in parentheses; * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

CA-T firms gradually increased by about 4% from 1989 to 1996 relative to the control group. Panel (D) confirms that the monthly remuneration of workers at CA-T firms were not reduced. If anything, we see a significant increase at least toward 1994. We believe that this is likely due to the (periodical) negotiation on financial condition were simultaneously discussed in the collective bargaining process. However, this effect gradually decays after 1995. Overall, Figure 7 provides a picture consistent with the institutional context: working hours were gradually reduced as the collective agreement reduced, and the labor cost increased as the regulation was respected to maintain the monthly remuneration.

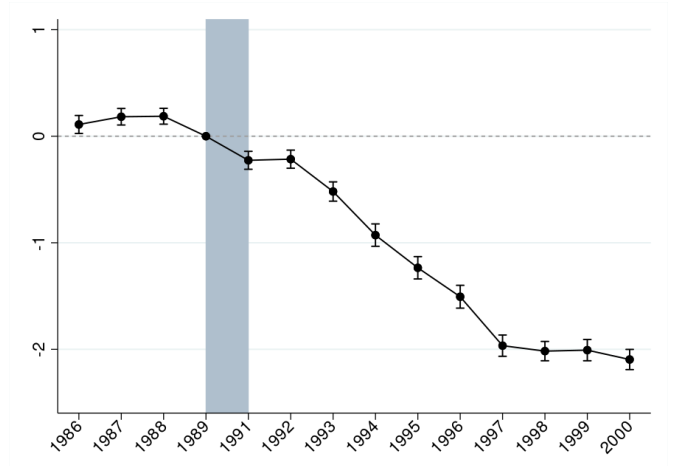
Figure 8 shows the results on our main outcomes of employment, sales and productivity. In contrast to the sharp reduction in working hours and increased labor cost, panel (A) displays no significant difference in employment trend between the CA-T firms and the control firms. The output measured by sales in panel (B) is not significantly impacted by the treatment through collective agreement. Absent of the price adjustment in the competitive product market, this would indicate that firms could maintain the level of production with smaller number of working hours, which has significant implication for productivity. panel (C) confirms that per-worker productivity did not alter significantly after the treatment, consistent with the previous panels. While it is difficult to assess the impact of the hours reduction on sales per hour in panel (D), the regression estimates indicate an positive effect as shown later.

Figure 7: Effect on Hours, Wages and Salaries

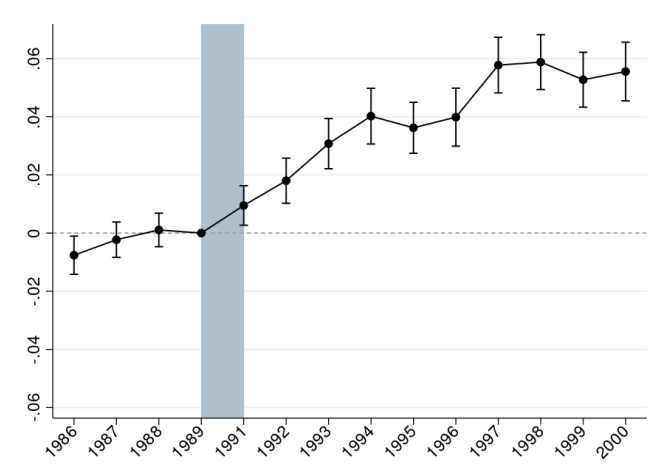
(A) Mean Standard Hours



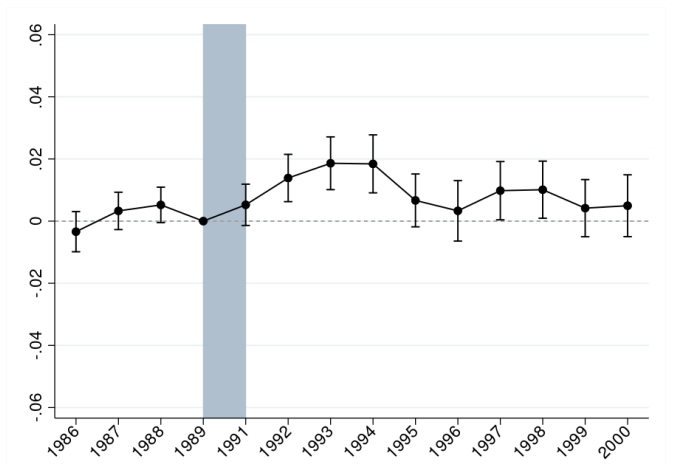
(B) Mean Actual Hours



(C) Mean Wage

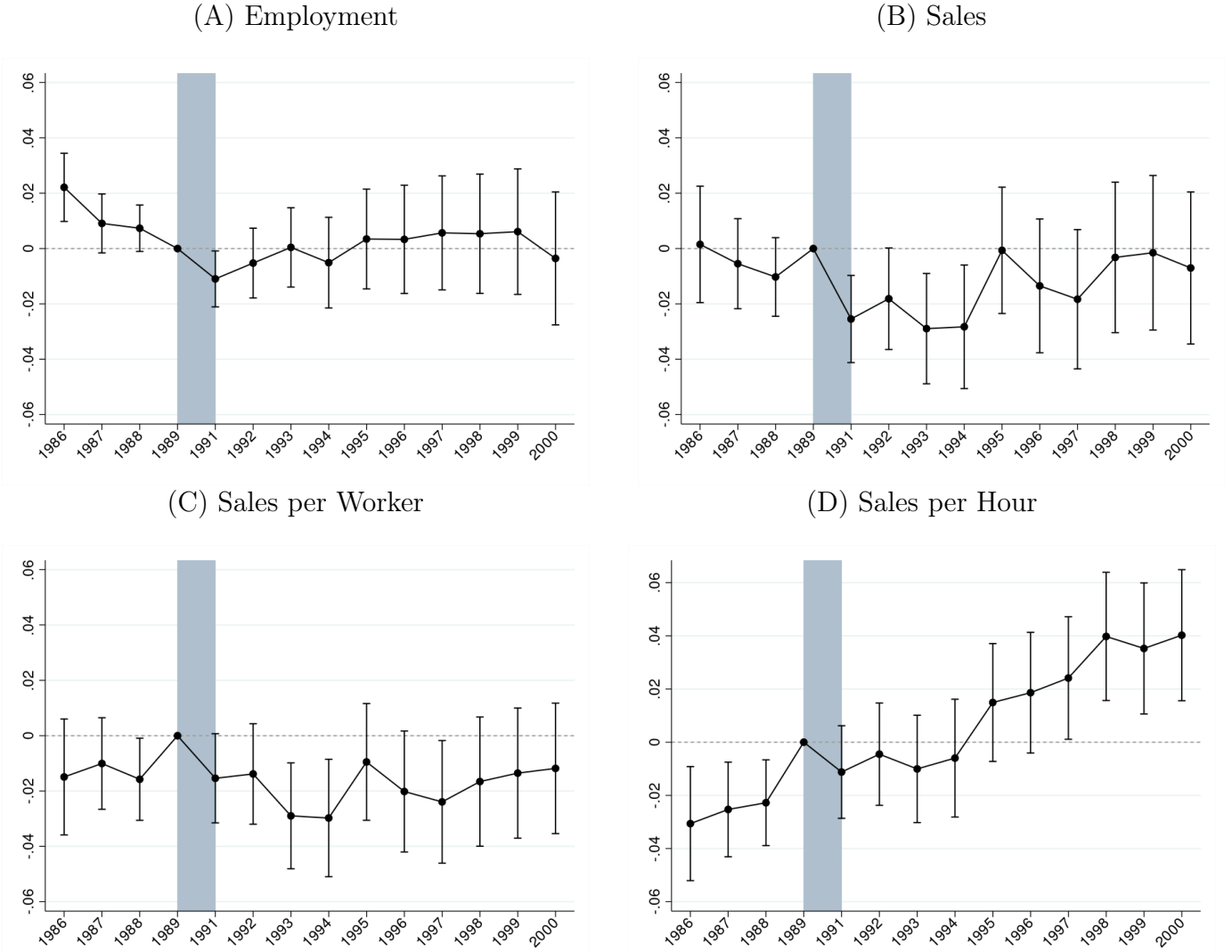


(D) Mean Monthly Salary



Note: The figure shows the dynamic effects of the reduction in working hours through collective agreement as specified in the equation 1, where the control group are firms at the bottom fourth of the distribution of the mean standard hours over 1986-1996. Years after the blue vertical lines are the treatment periods. The gray line indicates the national reform in 1996. There is no 1990 because of the missing QP worker file. Standard errors are clustered at the firm level.

Figure 8: Effect on Employment, Sales and Productivity



Note: The figure shows the dynamic effects of the reduction in working hours through collective agreement as specified in the equation 1, where the control group are firms at the bottom fourth of the distribution of the mean standard hours over 1986-1996. Years after the blue vertical lines are the treatment periods. The gray line indicates the national reform in 1996. There is no 1990 because of the missing QP worker file. Standard errors are clustered at the firm level.

6.2.1 Regression results for Collective Agreement Treatment

In Table 4, we quantify the overall effects of the collective agreement treatment. In this specification, we do not control for the firm-specific trend. This is because the treatment was gradual over a long time, therefore the gradual effects on the outcomes are often erroneously “controlled for” by the firm-specific trend (for example, the effects on the hours become no longer negative).

On average, the CA-treatment has led to the reduction of standard hours by -1.29 and of actual

hours by -1.23, as shown in the columns (1) and (2). Note that, due to the gradual implementation process, the average effects on the hours is less than the maximum reduction after the full implementation. In column (3), we observe little effects on the overtime hours. Column (4) shows that the Wage rate increased by 3.7%. In column (5), average monthly salary was not largely affected by the treatment, but there is a small increase due most likely to the negotiation of the remuneration in the collective bargaining processes.

Column (6) shows that the coefficient for the employment is small and not statistically significant, indicating that the reduction in hours through collective agreement did neither created more jobs nor destroying them, at least at the firm level. As a consequence, the total working hour input at firm level has declined by nearly 5% (column 7). Despite this, there is no statistically significant effect on sales, as shown in column (8). Consistent with these findings, per-worker productivity were not impacted, while the per-hour productivity increased significantly (columns 9 and 10).

6.3 Alternative Specification for 1996 National Reform

We also run an alternative specification in order to estimate the effect of the 1996 national reform, which only focuses on the period 1994–2000. The advantage of this approach is that, as from 1994, the data at our disposal measures standard hours (i.e. the usual hours worked in a given week as per the employment contract), which allows for a significantly more precise measured of what firm is impacted by the reform, as well as being less subject to the business cycle fluctuations and changes in the number of working days. Contrary to the main estimation outlined above, in this estimation the 1996 reform is considered as an independent event, i.e. ignoring the legislative changes (or lack thereof), both national and sectoral, that occurred since 1991. The advantage of this narrower approach to estimate the effect of the 1996 reform is twofold: i) first, by using contract hours, we can exactly identify affected and unaffected firms just before the reform; ii) second, by defining treatment in the years just prior to the reform, we can estimate effect on the full sample of affected and not only those alive before 1991, which allows to get a fuller picture of the effects of the reform on existing firm. The main issue with this approach is that we need to take into account that some firms/collective agreements were reducing hours over the period, as a result of the normative push started in 1991. For this reason, we

Table 4: The Effects of Collective Agreement Treatment

	Hours			Labor Cost		Labor Input		Sales		
	Standard (1)	Actual (2)	Overtime (3)	Wage (4)	Monthly salary (5)	Employment (6)	Total Hour (7)	Total (8)	Per Worker (9)	Per Hour (10)
$Treat \times Post$	-1.289*** (0.025)	-1.228*** (0.032)	-0.024 (0.021)	0.039*** (0.003)	0.009*** (0.003)	-0.011 (0.008)	-0.048*** (0.008)	-0.014 (0.009)	-0.009 (0.008)	0.029*** (0.008)
Mean Outcome	41.7	41.0	0.2	1.1	6.3	2.1	6.9	13.0	10.9	6.0
Year FE	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Firm FE	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Sector-year FE	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Firm-trend	No	No	No	No	No	No	No	No	No	No
R-squared	0.59	0.57	0.49	0.80	0.80	0.92	0.91	0.92	0.83	0.82
Observations	259,543	259,543	259,543	259,543	259,543	259,543	252556	259,543	259,543	259,543

Note: The tables displays the results of the working hour reduction by collective agreement. All outcomes are in log, except for the hour measures in the columns (1)-(3). The outcomes in the columns (1)-(5) are of the firm-level average values of the fulltime-equivalent workers who worked at least 30h per week. The variable $Treat$ takes 1 for firms in the CA-treatment group and 0 for the firms in the Control group. $Post$ takes 1 for the years after 1991 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; * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

need to exclude firms that are changing hours over the period. We elaborate on this further below in the sample definition subsection.

6.3.1 Treatment Definition

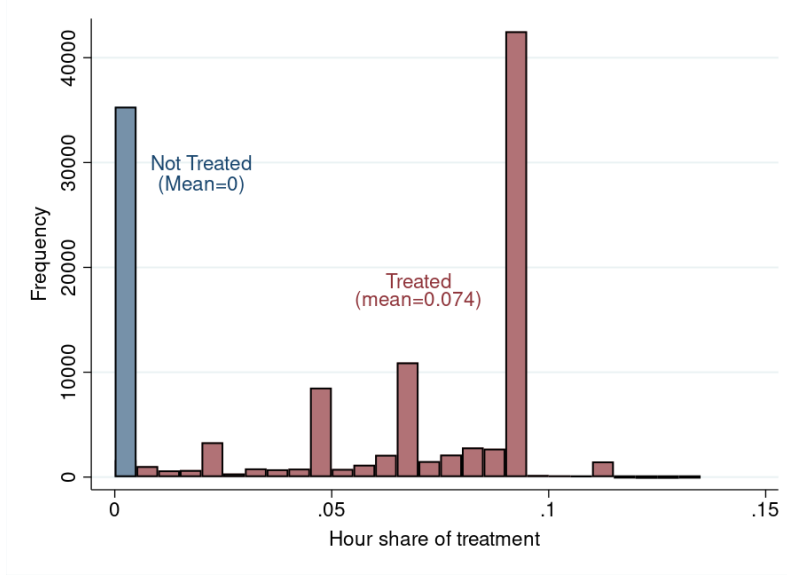
To define affected firms, we construct the measure of the exposure to the treatment for each firm. We compute the share of the treated hours for each firm, that is:

$$HourShareTreat_j = \frac{\sum_j \max(Hour_{ij} - 40, 0)}{\sum_j Hour_{ij}} \quad (2)$$

where $Hour_{ij}$ is the standard hours of worker i at firm j . We use pre-treatment years between 1994–1996 to construct this measure for all firms. As is the case in our main analysis, we take workers at the bottom fourth in terms of this treatment exposure as the control group. This is shown in Figure 9, which plots, per firms alive in the 1994–1996 period, the share of total treated hours before the reform. At the two extremes of the distribution, there are firms with all and no workers treated (two groups of roughly equal size); in-between, there are firms with some workers above 40 hours, but not all. In effect, the threshold for the lowest quartile in terms of the hour share of treatment is zero, that is, we use firms with all workers with contractual hours of 40h or below as the control group.²⁸ Here, the treated group includes Reform-treated firms from the previous analyses, but also includes firms born after 1991 with hours above the reform threshold and excluding firms that do not exist after 1994. Similarly, the control group is a subsample of the Control group, firms that had low hours since the beginning in the sample, part of group CA-treated firms, those early treated to 40 hours before 1994, and new firms born after 1991 with hours at or below 40. Moreover, the treated group here is defined based on the firm-level variation in hours, rather than based on the mode in each collective agreement. This enables us to precisely measure each firm’s treatment exposure to the 1996 reform, considering that firm-level hours may deviate from the normative hours specified in the collective agreement.

²⁸As robustness checks, we also tried using the bottom one-third or the bottom half of the treatment exposure as our control group and obtained consistent results, indicating that our estimated effects come largely from the comparison of the least exposed firms and strongly exposed firms.

Figure 9: Firm-level share of treated hours



6.3.2 Sample Definition

Differently from our main estimation, the sample is limited to those firms in the sample at least once between 1994 and 1996, which is a necessary condition in order to measure exposure to the reform. As mentioned above, because firms might be decreasing hours during the period because of a change in the collective agreement or an autonomous decrease in hours, only firms with hours that are stable during the period are kept in the estimation. Stability is defined as having the same mode of hours, at the firm-level, over the period 1994 to 1996. Unstable firms are therefore those that experience a change in the mode of hours at the firm level in the years between 1994 and 1996. Including this restriction leads us to drop two thirds of the sample over the period.

6.3.3 Specification

After defining treatment status in the way described above, we run the following regression, which is a similar difference-in-differences specification similar to the main one:

$$Y_{jt} = \gamma_j + \delta_{s(j)t} + T_j \sum_{t=1994}^{2000} \beta_t \mathbb{1}\{year = t\} + \varepsilon_{jt} \quad (3)$$

where Y_{jt} are the outcomes of interest at the firm-level; γ_j and $\delta_{s(j)t}$ are firm and year fixed effects respectively. $\zeta_{s(j)t}$ is sector-year fixed effects. T_j indicates whether the firm has any worker exposed to the reform, as opposed to having already all the workers at a 40 hour week. β_t identifies the dynamic effects of the treatment, where the coefficient is normalized in 1996. We cluster standard errors at the firm level, in order to account for panel nature of the data.

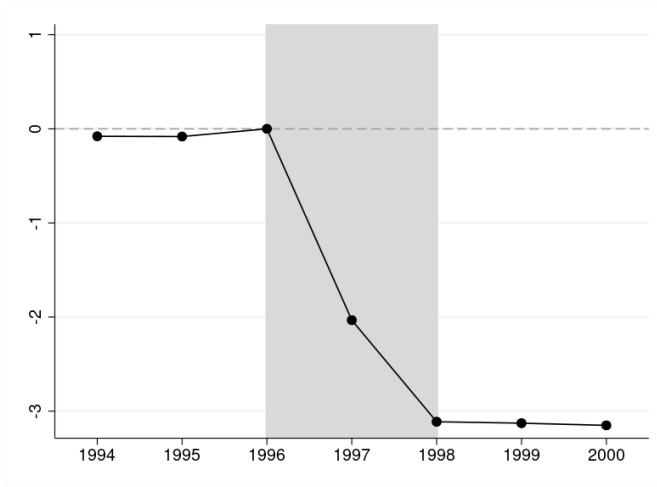
6.3.4 Results

Figure 10 shows the results. Panel (A) shows that the reform led to a reduction in mean standard hours of 3 hours from 1996 to 1998 in the treated firms. Thanks to defining the treatment groups at the firm level just before the treatment with the recorded contractual hours, the coefficients are more precisely estimated with very small standard errors. The absence of no differential pre-trend in hours is in part by construction because we only kept firms with the same mode of hours in the pre-reform periods. Panel (B) displays a concomitant decrease in mean actual hours. Since mean remuneration did not adjust, the mean wage rate increased sharply, over 6%, after the full implementation of the reform, as depicted in panel (C) and (D).

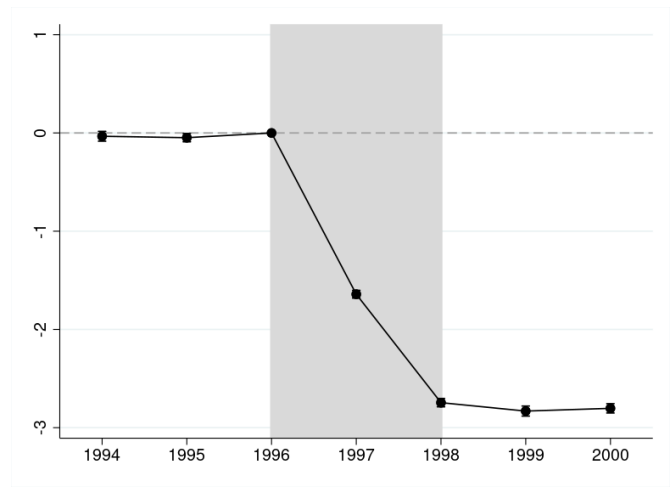
Consistent with our main analysis with the long-run approach, Panel (E) shows that there was a negative effect on employment. While the coefficients in the pre-treatment periods are not exactly around zero, the coefficients are smaller in magnitude and, if anything, was an increasing trend. Panel (F) shows a clear negative effects on sales starting from from 1997 and the magnitude is relatively large, cumulative 6% toward 2000. Given the large negative effects on sales than that on the employment, the per-worker productivity was still negatively affected, as shown in panel (G). On the contrary, the decrease in sales is not as large as the decrease in total labor hour input (i.e. number of workers times mean hours). Therefore, as suggested in panel (H), there is an increase in labor efficiency measured by sales per hour. Taken together, the results indicate that, due to a sudden reduction in working hours, firms have moved to a new equilibrium where both input and output levels are lower compared to the pre-reform period, which are however more efficiency in terms of labor use.

Figure 10: Effects of the 1996 reform, Alternative Approach

(A) Mean Standard Hours



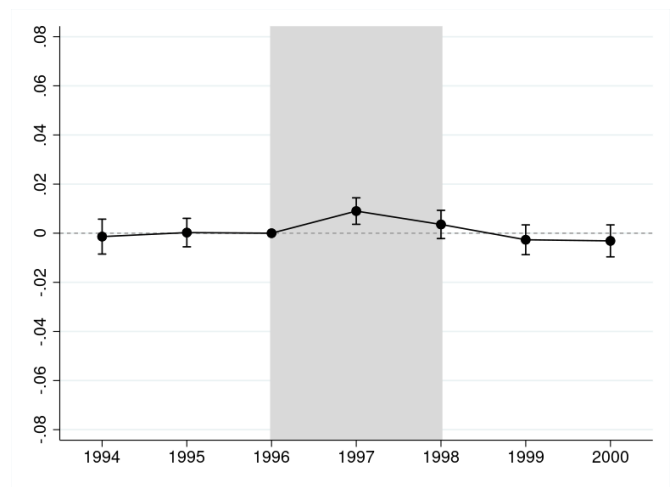
(B) Mean Actual Hours



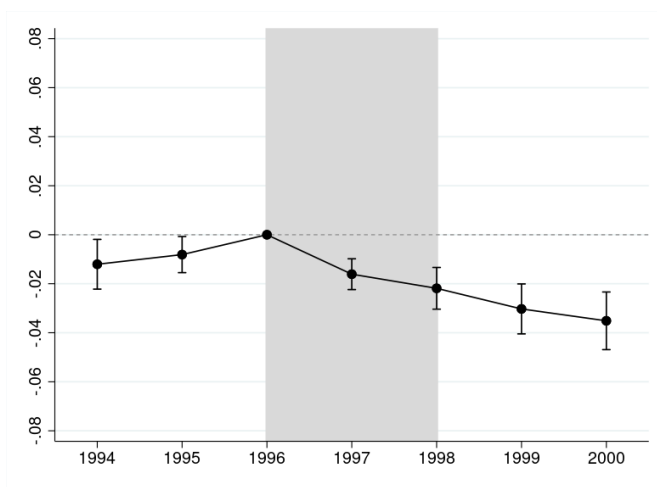
(C) Mean Wage



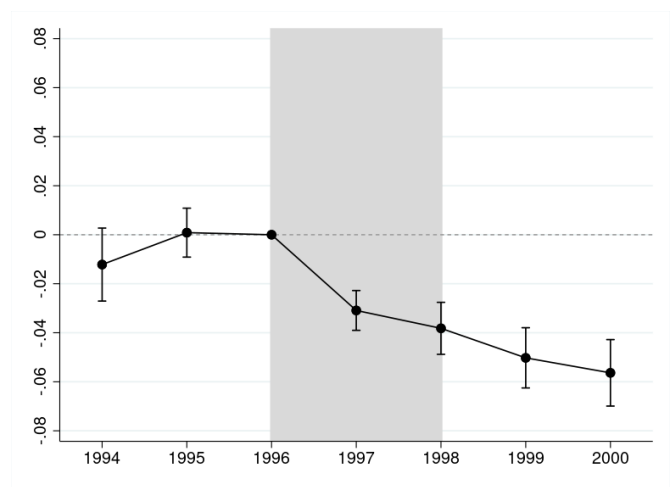
(D) Mean Monthly Salary



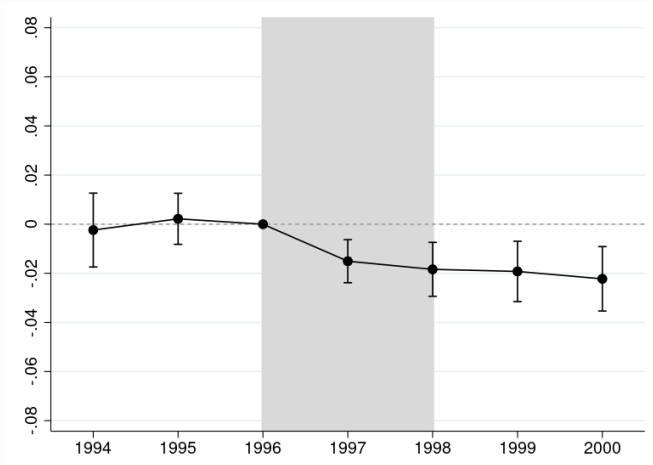
(E) Employment



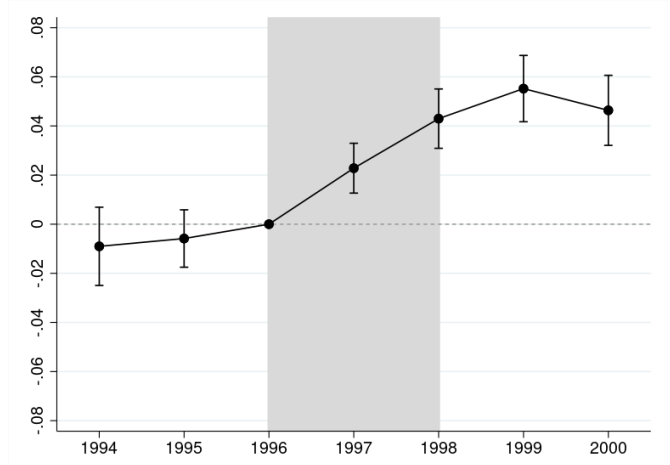
(F) Sales



(G) Sales per Worker



(H) Sales per Hour



Note: The figure shows the dynamic effects of the reform in 1996 on the firm outcomes as estimated in the equation 3. The outcomes are in log for employment and sale. Sales refer to the annual sales of the previous year. The red horizontal line separates the pre- and post-treatment periods. The control group constitutes firms that are at the bottom fourth in terms of the share of hours treated as defined in the equation 2, whereas the rest is considered treated firms Panel (A) and (B) show the effects on the mean standard and actual hours, respectively; Panel (C) display the employment effect; Panel (D) shows the effects on sales. All regressions include FE in year, firm and sector-year combination. Standard errors are clustered at the firm level.

7 Conclusion

This paper studies how a reduction in working hours at constant salaries impact firms. Specifically, we study the Portuguese transition to the 40-hour working week in the 1990s as an ideal case study of a policy shock without compensating measures put in place for firms. In the early 1990s, Portugal embarked on a process of reduction of working hours through collective agreements, which partly failed. As a result, a sudden policy decision resulting from a change in government was taken to force firms to adjust to a 40-hour working week within a short timeframe. In this paper, we show that affected firms, relative to unaffected firms (so firms that had lower hours either by choice or because of their collective agreement), experience both employment and output decline as a result of the reform. However, labour input (employment \times hours) of treated firms decreases proportionally more than output, such that productivity per hour significantly increases. Moreover, we find that firms that instead reduced hours through more gradual collective agreement reforms did not experience employment nor output losses, while still experiencing similar decrease (increase) in hours (wages), although smaller in magnitude.

With these findings, we provide the first set of clearly identified results of how standard hours

reductions impact firms. Our results show that, by increasing the labour cost, reducing working hours discourages labour demand. However, this does not mean the total employment effect in general equilibrium is negative, as it could be that workers in affected firms relocate to other firms. On the other hand, the positive productivity effect, i.e., that output per hour worked increases significantly, suggests that reduction in working hours might also lead to significant efficiency gains for firms. This question also needs to be explored further, as the lack of a “real” measure of productivity still leaves doubt on the extent to which this effect might mingle in an increase in prices. This paper also tackles the questions of the different legislative instruments that can be used to lower working hours: in our results, those sectors that decreased through collective agreements do not experience negative economic repercussions. Yet, this does not imply that collective agreements are a better way to reduce working hours, as the different treatments (national vs. collective bargaining) were applied to different sectors, and, as such, are impossible to compare. Moreover, the inability of collective bargaining institutions to autonomously lower hours as agreed upon was the main reason behind the national reform of 1996. In short, the main contribution of this paper has been to provide firm-level estimates of an exogenous reduction in working hours, showing that, while potentially harmful for employment, they can be beneficial to productivity. Many questions remain open to further research, including potential effects of prices, general equilibrium effects, and question regarding the most appropriate legislative instrument to lower hours.

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Online Appendix

A.1 Extra Figures and Tables for Literature, Context and Descriptive

A.1.1 Literature

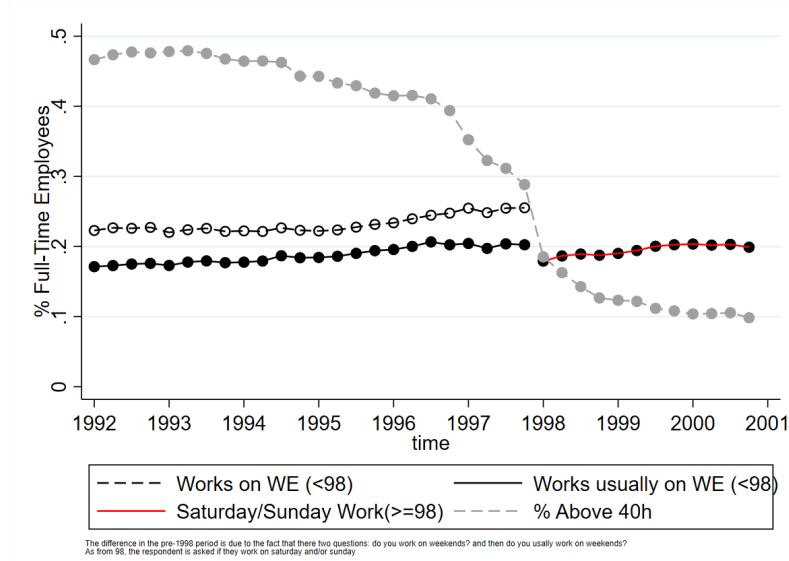
Table A1 provides the summary of the literature which studies the effect of the working hour reduction.

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 \times Region	Positive
Chemin and Wasmer (2009)	France - 1998	39h to 35	Region	Null
Batut et al. (2023)	EU - 1995-2002	Several	Country \times Sector	Null

Note: *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 table of what was published in Batut et al. (2023).

Figure A1: Share of Full-Time Workers Working on Weekends, 1992 to 2000, National Labor Force Survey



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

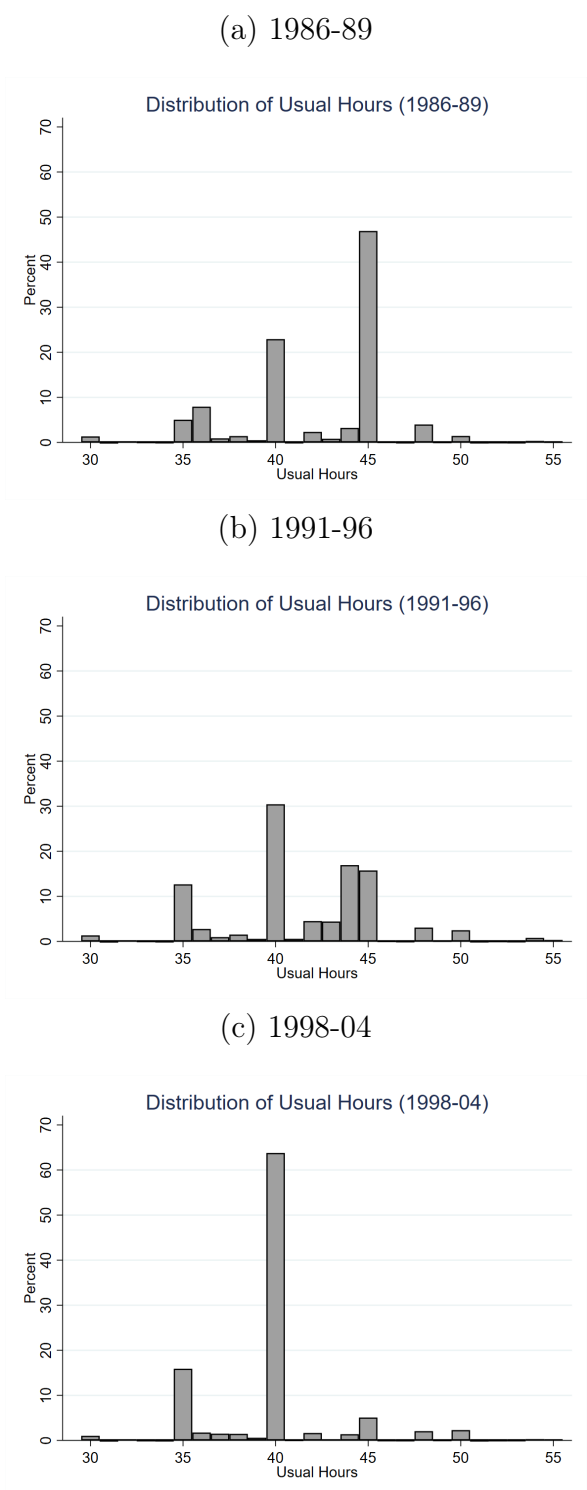
Source: Authors' calculations on national LFS data.

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 uses workers that are employees with standard hours between 30 and 50. The year is 1996. 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.

Figure A2: Distribution of Usual Working Hours for Full-Time Employees, Labor Force Survey, 1986-89, 1991-96, & 1998-04

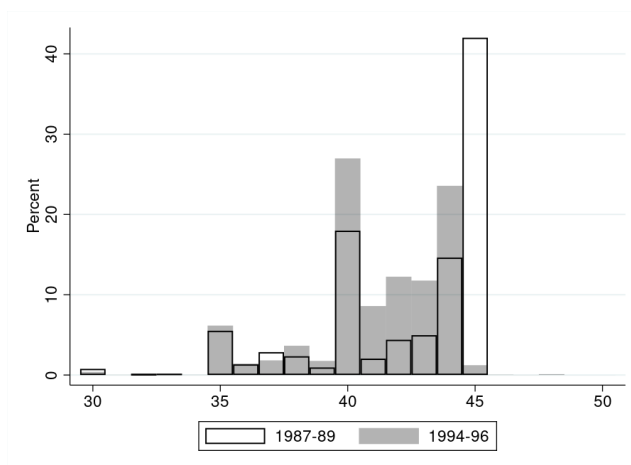


Note: These graphs plot the distribution of usual working hours for full-time employees over the three different periods in which the legislation was different. In the period 1986 to 1989, standard hours were set at 48, or 45 over 5 days. In the period 1991-96, they were set at 44 hours per week. As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

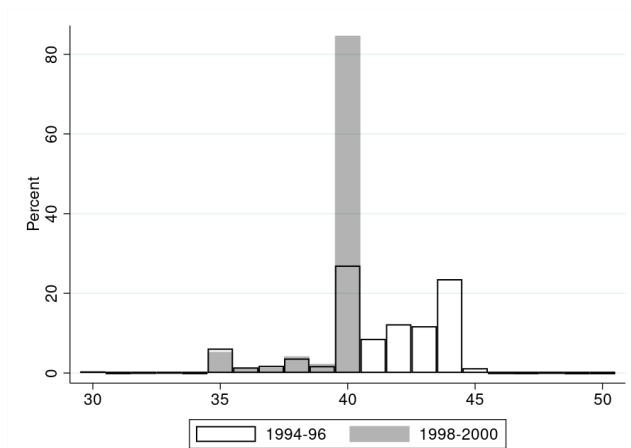
Source: EU- LFS

Figure A3: Distribution of Standard Hours for Full-Time Employees, QP

(a) 1987-89 and 1994-96



(b) 1994-96 and 1998-2000

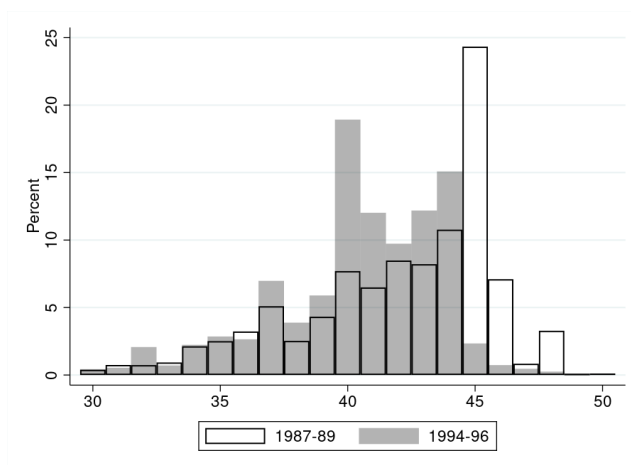


Note: These graphs plot the distribution of weekly standard hours for full-time employees, as recorded in the QP. In the period 1986 to 1989, standard hours were set at 48, or 45 over 5 days. In the period 1991-96, they were set at 44 hours per week. As from 1998, a 40 hour working week was introduced.

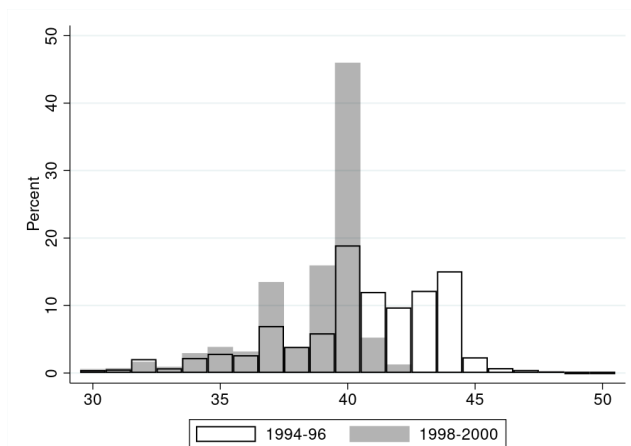
Source: QP

Figure A4: Distribution of Actual Hours Worked for Full-Time Employees, QP

(a) 1987-89 and 1994-96



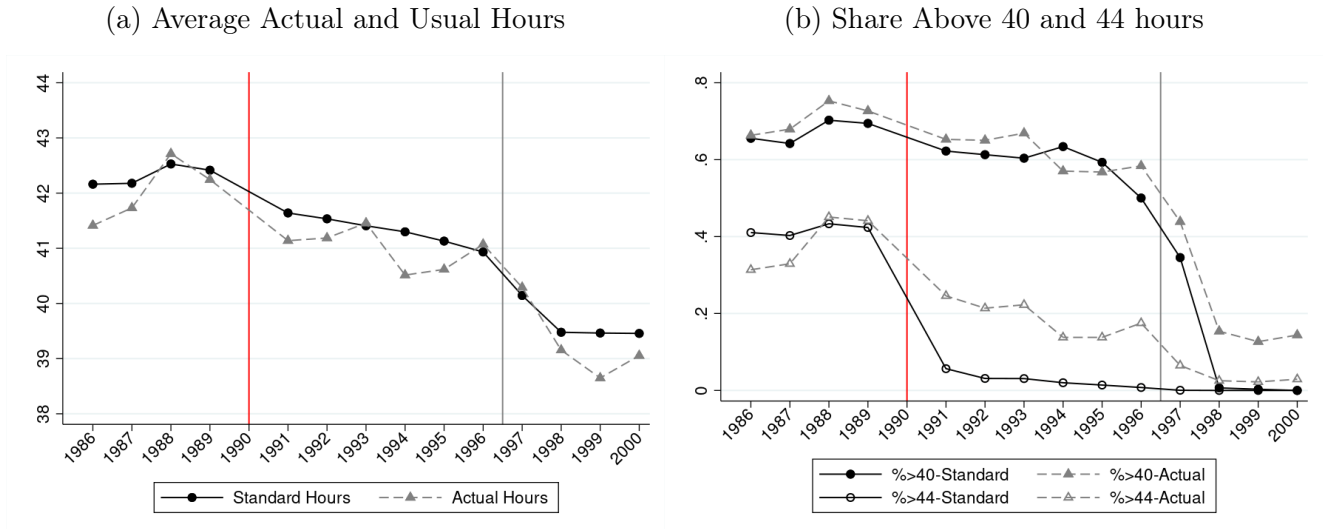
(b) 1994-96 and 1998-2000



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

Source: QP

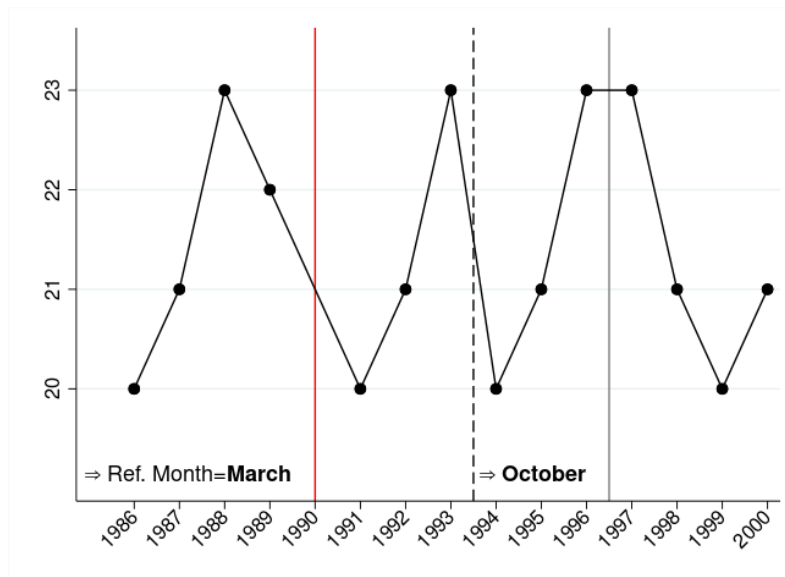
Figure A5: Average Standard and Actual Hours of Full-Time Workers and Share Above 40 and 44 hours, 1986 to 2000, QP



Note: The figure replicates Figure 2 after imputing standard hours for missing standard hours. The panel (a) shows the average standard and actual hours for non-agricultural workers working between 30h and 55h. The panel (b) shows the share of workers with standard hours and actual hours above 40h and 44h respectively. The red vertical line indicates the reform reducing the standard hours from 45h to 44h and the tripartite agreement encourage the reduction of hours by collective agreements started. The gray vertical line indicates the national reform that reduced the standard hours from 44h to 40h.

Source: Authors' calculations based on the QP.

Figure A6: 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.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; *capital-priv*: 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; *workers-est*: 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; *collective*: collective agreement covering the worker; *employment*: worker employment type (employee, employer, self-employed, family-worker); *contract*: worker’s contract type (fixed-term contract, permanent contract); *schedule*: worker’s schedule (part-time, full-time); *educ1 to 3*: worker’s education from 1 to 3 digits; *prof 1 to 6*: worker’s profession 1 to 6 digits of disaggregation; *wage*: worker’s monthly wage (base plus extra time, plus

bonus); *hours month*: worker's hours in March (before 1993), or October (after 1993); *hours extra*: worker's overtime hours in March (before 1993), or October (after 1993); *hours_week*: usual working hours (after 1993)

A.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.

A.2.3 Recovering standard hours in the QP before 1994

As mentioned in the Section 4, unfortunately, the information on the standard hours is available in the QP only since 1994. However, it is important to have standard hours for the entire period of our study, because, unlike actual hours worked, it is not affected by economic cycles and temporary shocks, and hence is less subject to endogeneity problems when defining treatment status of firms. Moreover, it is natural to use standard hours because it was the target of the two reforms. Therefore, for workers with missing information on standard hours, we impute them using the method outlined below.

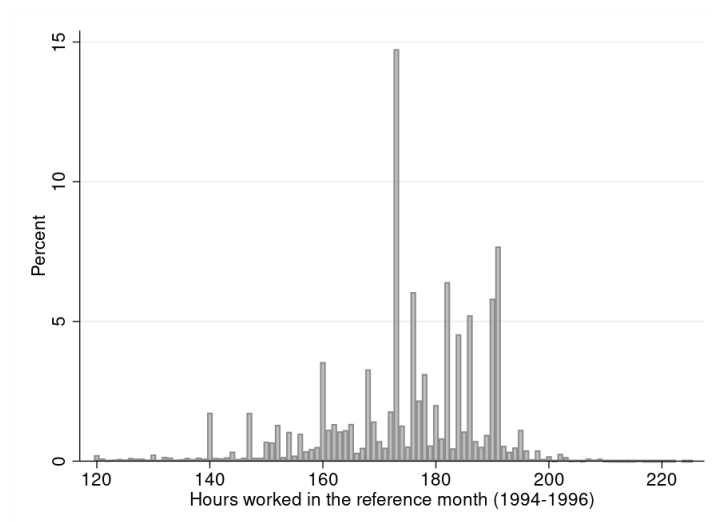
To recover the standard hours of the workers, we use the fact actual hours worked in the reference month recorded in the QP is strongly related to specific weekly standard hours. This is due to the fact that, in filling hours information, firms often calculate hours worked in the month based on the standard hours. For example, for a worker with standard hours of 40 per week and if the month has 22 work days, firms often report monthly actual hours as 176h (i.e. 8h per day multiplied by 22 days). In other cases, firms may multiply per-day standard hours by 21.626, the average number of working days per month, or simply by 20, corresponding to 4 weeks. In this example, these results in the monthly hours of 173h and 160h, respectively.

This fact is reflected as spikes in the distribution of monthly actual hours recorded in the QP,

shown in Figure A7. The largest spike is at 173h, amounting to approximately 15% of workers in the data between 1994 and 1996. The figure also indicates the presence of many other spikes in the distribution. We take advantage of these spikes in order to inversely extrapolate the standard hours from monthly actual hours worked. In particular, using the pooled sample of workers in the 1994-1996 period, during which the legal maximum weekly hours was still 44h and the QP records contractual hours, we identify the most frequent standard hours within each bin of monthly hours worked. Figure A8 shows some examples. In each panel, we show 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 these monthly hours. We do the procedure for each bin of monthly actual hours to create a full correspondence between monthly hours and weekly standard hours. Then, for years before 1994, we assign these imputed standard hours according to the worker's monthly hours worked.²⁹ Since the legal maximum standard hours was 45h until 1989, we assign 45h (i) in the case of the monthly actual hours that are likely be associated with 45h per week (i.e. 180, 189, 192, 194, 195, 198, 207) or (ii) if the worker's monthly actual hours exceed 202.4 (i.e. maximum number of hours that can be achieved under 44h regime with maximum number of working days per month in the data, 23 days) for years in the 1980s.

²⁹For 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.

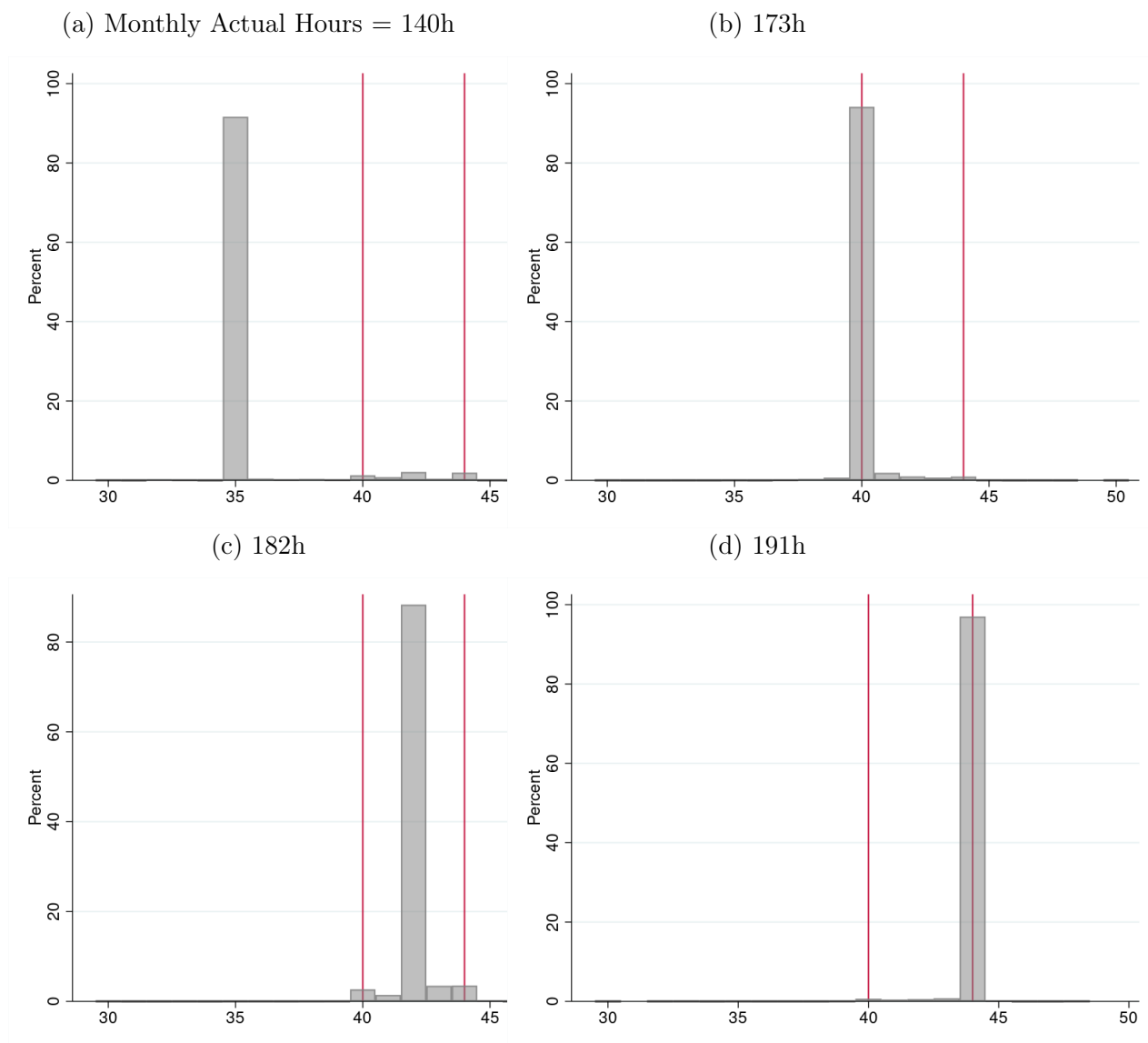
Figure A7: Distribution of Actual Hours Worked in the Reference Month



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

Source: Authors' calculations based on the QP.

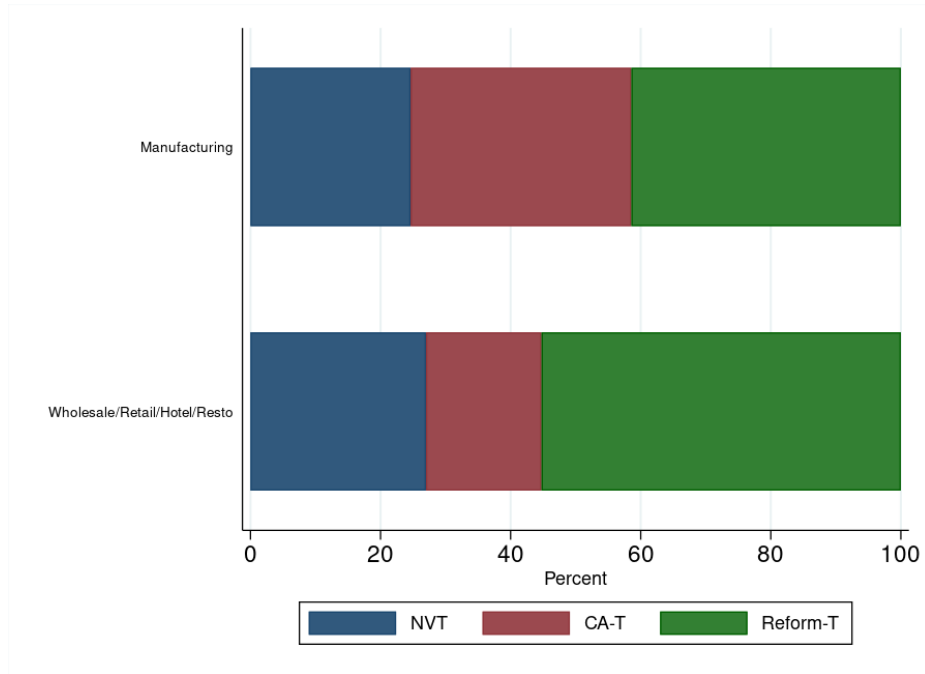
Figure A8: Examples of Distribution of Standard Hours According to Actual Hours per Month



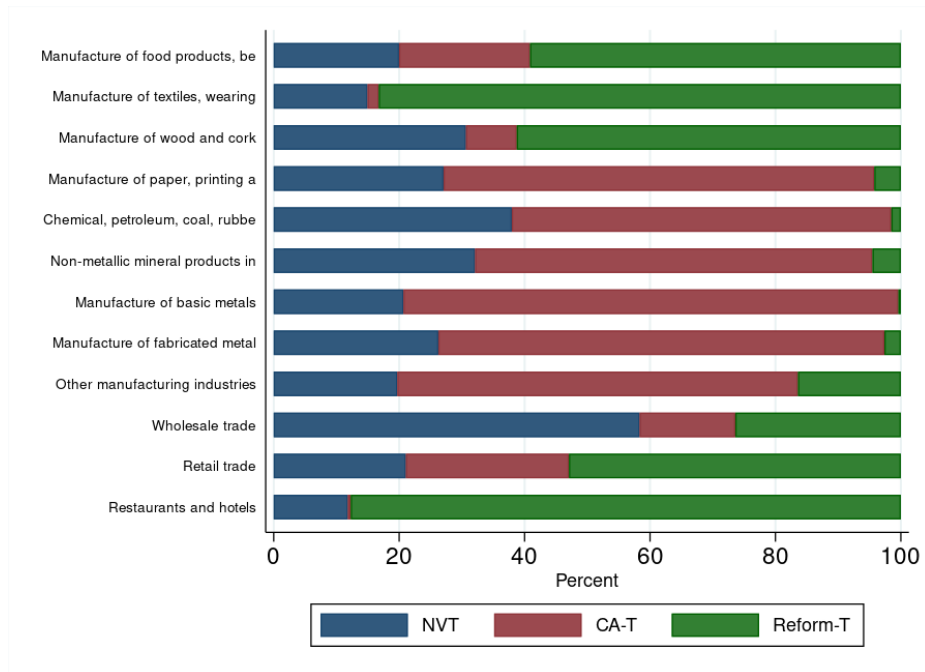
Note: The figure presents the distribution of recorded weekly contractual hours in relation to the recorded actual hours worked per month for the period 1994-1996, taking specific examples of 140 hours, 173 hours, 182 hours, and 191 hours. The figure indicates the presence of specific contractual hours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

Figure A9: Sectoral Distribution of Treatment Groups

(A) Aggregated Sector



(B) 2-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 disaggregated 2-digit sectors.

A.2.4 Creating a Panel of Collective Agreement

The QP records the unique collective agreement (CA) codes assigned to each individual worker. Every year, there are around 500-600 distinct CA codes, varying in terms of their sizes (i.e. number of workers covered by each CA). Working with these codes over time can be intricate for various reasons. There are instances where different CAs combine into one, workers under a specific agreement shift to another CA, or conversely, one CA could split into multiple new agreements. Furthermore, even without such changes, a CA might be renewed in a way identical to before but with a new code. These dynamics create challenges in accurately identifying firms and workers that experienced treatment through collective agreements between 1991 and 1995/6. To address this, we create a consistent panel of CA codes by leveraging the panel dimension of workers. This involves recursively identifying connected groups of collective agreements, a process explained in greater detail below.

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

- (1) Keep years t and $t - 1$ and workers who did not change firm between these two years.
- (2) Collapse the data such that each row contains a pair of CA in t and CA in $t - 1$ observed in the data (called CA_t and CA_{t-1} respectively hereafter), as well as the corresponding number of workers in each pair.³⁰ 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 $CA_t = CA_{t-1}$.³¹
- (4) Repeat the step (3) for CA_{t-1} against CA_t ,

³⁰For 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.

³¹In the vast majority of cases, the same CA does appear in the adjacent year. Among CA's in 2000, nearly 95% of them has at least one row satisfying $CA_t = CA_{t-1}$

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

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

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

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

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

- (1) Start with the CW_t . In effect, we use CW_{2000} as a starting point.
- (2) We would like to avoid small linkages (i.e. CA pairs with small number of workers) connecting many collective agreements, leading to a small number of extremely large new CA groups. Therefore, we identify the rows (i.e. CA-pairs) with at least 100 workers (called "strong link") and drop the rows that fall in one of the following conditions:
 - (i) If CA_t has multiple corresponding CA_{t-1} and at least one strong link, drop the rows of weak link (with below-100 workers)
 - (ii) If CA_t has multiple corresponding CA_{t-1} but none of the rows have a strong link, keep the row with the CA_{t-1} that has the largest number of workers
 - (iii) Same as (i) and (ii), but the other way around looking from CA_{t-1}
- (3) For any CA_t and CA_{t-1} that have multiple rows, we identify all the CA_t and CA_{t-1} that are linked with that CA. We then assign a new CA ID to all of them. For simplicity, we simply assign the highest value of the original CA code as a new ID. The dataset is reshaped such that

it contains two columns, one for the list of the new ID (possibly duplicated) and the other the (original) CA codes corresponding to each new ID. The second column (with the original code) are then named simply as CA_{t-1} for the merging process in the next step.³²

- (4) Merge it with the CW_{t-1} based on the CA_{t-1} .³³
- (5) Do the same step (2) and (3), using the pairs of CA_{t-1} and CA_{t-2} .³⁴
- (6) Repeat the steps until reaching to the first year of the data (i.e. 1986).

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

³²For 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.

³³We use the STATA command *joinby* so that all the possible combinations are created in case of multiple CA_{t-1} existing in both crosswalks.

³⁴Sometimes, after the step (3), there are still duplicates of the original CAs entering into multiple new codes. In this case, we repeat the grouping process so that the CA's connected to this CA will form again one new group with a new code.

A.3 Supplementary Analyses for 1996 Reform

A.3.1 Robustness to Alternative Definition of Control Group

In our main estimation, we defined the control group as the firms in the lowest quartile in terms of the average standard hours between 1986 and 1996. We show in Table A3 that our results are robust to the alternative definition of control groups.

Table A3: Robustness to Control Group Definition, 1996 reform Effects

	Actual Hours			Employment				Sales				Sales per Hour				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)	(16)
Bottom quarter	-1.559*** (0.039)				-0.014** (0.006)				-0.004 (0.008)				0.042*** (0.009)			
Bottom fifth		-1.523*** (0.035)				-0.012** (0.005)				-0.000 (0.007)				0.038*** (0.008)		
Bottom third			-1.522*** (0.044)				-0.014** (0.007)				-0.002 (0.009)				0.046*** (0.009)	
1986-1995 average				-1.394*** (0.040)				-0.013** (0.006)				-0.001 (0.008)				0.039*** (0.009)
Mean Outcome	41.6	41.6	41.6	41.6	2.0	2.0	2.0	2.0	12.8	12.9	12.8	12.8	5.9	5.9	5.9	5.9
R-squared	0.70	0.69	0.70	0.70	0.97	0.97	0.97	0.97	0.96	0.96	0.96	0.96	0.89	0.89	0.89	0.89
Observations	396130	420010	381430	392735	396130	420010	381430	392735	396130	420010	381430	392735	396130	420010	381430	392735

Note: The tables compares the results of the 1996 reform (Figure 3) using different control groups. The results in the first row replicates the main results, using the bottom quarter of the mean hours over 1986-1996 as our control group. The second row and third row use the bottom fifth and the bottom third, respectively. The last row uses the mean hours over 1986-1995 period to show that our results are not driven by mean reversion arising from using the year 1996 as the last year of the period to take an average over. All regressions include firm fixed effects, sector-year fixed effects, and firm-specific linear trend. Standard errors are clustered at the firm level. Standard errors in parentheses; * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

A.4 Supplementary Analyses for Collective Agreement Treatment

A.4.1 Alternative Specification

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

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

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

Specification Given the staggered nature of the changes in the collective agreement hours, we implement the methodology suggested by Callaway and Sant’Anna (2021) to avoid the issue of dynamic effects confounding the estimates. In short, this deals with the issue of negative weights and wrong

comparison arising from the standard two-way fixed effects specification by avoiding the comparison of late-treated units (in our case, collective agreements) with early-treated units.

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

Results Figure A10 shows the results. Panel (A) and (B) shows that both mean of standard hours and actual hours were reduced by 1 hour for the firms treated by collective agreement, relative to the firms treated by collective agreement later. Although the gradual reduction is consistent with the prediction, the total reduction is rather small. This is in part due to the firms that had already lower hours than the collective agreement mode, but also because of the difficulty to identify the precise timing of the first treatment year. Panel (C) shows that there is no significant change in the level of coefficients before and after the reduction in working has started, consistent with the finding from the main difference-in-differences results in our main results. Similarly, we detect no discernible effects on the sales, as shown in panel (D). These results show that our main results are not driven by unobserved characteristics that endogenously divide CA-treated firms and the control firms.

Figure A10: Effects of 1996 Reform on Employment, Sales, and Productivity

(A) Mean Standard Hours

(B) Mean Actual Hours



(C) Employment

(D) Sales



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.