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JEL Codes: J23; J62; J81.

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Working Hour Reform, Labor Demand and Productivity

Kentaro Asai*

July 19, 2022

Abstract

This paper examines the employment and productivity effects of the working hour reform in Portugal that reduced the standard hours from 44h to 40h in 1996-7. Using the variation across establishments in the intensity of treatment, I find that the establishments that were more treated experienced lower post-reform employment growth, although to a modest degree. Despite of the large reduction in the labor hour input, there is no statistically significant negative effect on sales, leading to a large improvement in labor efficiency measured by sales per hour. However, these overall effects mask substantial heterogeneity in responses: establishments in capital intensive sectors reduced employment without decline on sales, while those in labor intensive sector rather attempted to maintain employment, but their sales were negatively affected. These results provide indirect evidence consistent with the theories that highlight the role of scale effects and capital substitution effects.

Keywords: Working Hour; Labor Demand; Productivity; Labor Market.

JEL Classification: J23; J62; J81.

* Asai: Paris School of Economics, 48 Boulevard Jourdan, Paris 75014, kentaro.asai@psemail.eu. I would like to express my deepest gratitude to Thomas Breda for his tremendous support and guidance for this project. I am also deeply indebted to David Leite for his valuable suggestions and continuous assistance in obtaining the results in this paper. I am also thankful for Philippe Askenazy, Pedro S. Martins, Eric Maurin, Pedro Raposo, and José Varejão for their useful comments. I also thank participants at 7th Linked Employer-Employee Data Workshop (University of Porto), XV Labour Economics Meeting (Spanish Association of Labour Economics), and The Doctorissimes (Paris School of Economics).

1 Introduction

Many workers prefer to work less, and it is even better if working less creates more jobs for the others. The idea of “work sharing” that working hour reduction policies would generate more jobs has always been popular among policy makers, especially in the late 20th century in Europe. Even today, the reduction of working hours is one of the main agendas of workers and unions, and some societies and communities are experimenting “4-day work week” to test whether an alternative work arrangement with fewer working days is possible without resulting in any loss in production. These real-world policy questions are fundamentally rooted in the economic knowledge on the interaction of working hours, labor demand and productivity.

Much of the previous literature was devoted to uncover the impact of working hour reduction policies on the aggregate employment, given the goal of the policy as a potential tool to fight unemployment. On the contrary, the evaluation using establishment-level data with careful attention to identification is extremely scarce. As a consequence, little is empirically known about how employers absorb such negative shocks in labor hour input, including their margins of adjustments and how their responses vary. In this paper, I study the employment effect of the national working hour reform in Portugal in 1996-7 to examine the employment responses and adjustments at the establishment level. In this reform, standard hours were reduced from 44 to 40 hours. Since employers were not allowed to reduce monthly base remuneration for workers whose standard hours were reduced, this led to a significant increase in hourly labor cost. The fact that the reform kicked in across all sectors simultaneously and that there were no supplementary subsidies for job creation makes the reform an attractive case study for employment and productivity effects.

Using the difference-in-differences strategy comparing the evolution of employment across establishments with different treatment intensities, I find that the reduction in working hours led to a slower growth of employment, but to a modest degree with the estimated elasticity of 0.25. Interestingly, I do not find a significant negative impact on sales, leading to a large gain in efficiency of production, measured by sales-per-hour. I also show that there is substantial heterogeneity in terms of responses to the reform. Most notably, the establishments in capital intensive sectors responded by reducing employment more, without any negative effects on sales. Conversely, the negative employment effects are much smaller and insignificant for the establishments in labor intensive sectors, however they experienced a decline in sales. These are indirect but consistent evidence for the standard labor demand models that predict the negative employment effect of the working hour reduction through the scale effects and capital substitution effects, for which no previous work provided empirical support. In addition, I find an increased share of high skilled workers after the reform in the treated establishments, consistent with the improvement in labor efficiency. This also indicates the existence of alternative mechanisms available to employers that the standard labor demand models have not highlighted.

This article relates to the long-standing literature on working-hour regulations and reforms on working hours and employment. The employment effects of working hour reduc-

tion policies have been previously studied in different contexts. Much of the empirical work focuses on the total employment effect with the view to assessing the effectiveness of the policy as a job creation tool (for example, [Chemin and Wasmer \(2009\)](#); [Hunt \(1999\)](#); [Raposo and Van Ours \(2010\)](#); [Skuterud \(2007\)](#)), usually examined at the sector or regional level. Contrary to the expectations of the public and policy makers, they generally do not find a clear increase in employment or new job creation.¹ Some instead study at worker level, providing mixed evidence on the probability of job separation and destruction for the workers whose hours were reduced ([Crépon & Kramarz, 2002](#); [Raposo & van Ours, 2010](#)). Study on labor demand is also carried out using working hours themselves as an outcome to examine the labor hour demand elasticity (e.g., [Hamermesh and Trejo \(2000\)](#)).

While this paper builds upon these previous important contributions, it departs from the literature in several ways. First, I exploit a rich administrative employee-employer matched dataset to study establishment-level employment responses, in contrast to much of the previous work which focuses on aggregate employment trajectory, often relying on the survey data. This enables us to precisely track how the policy has affected key outcomes (e.g. working hours, employment, and sales) and to exploit finer variations in the treatment intensity at the establishment level for identification.² Examination at the establishment level is crucial because the economic theories and policy questions assume the labor demand adjustment at the firm level. This paper therefore studies such “direct” mechanism of employment effects in this paper. Moreover, I explore heterogeneity of employment effects across establishment characteristics to reveal differential responses to cope with the reduction in labor hour input, which has been given little attention.³ Leveraging rich information on workers and establishments in the dataset, I also explore the effect on some outcomes that may help explain the mechanisms behind employment effects and other adjustments that are available to employers. Lastly, I examine the causal effects of the reform carefully, by providing graphical evidence and a number of robustness checks.

The rest of the paper is organized as follows. Section 2 explains the institutional context of the Portuguese reform and the data used. Section 3 lays out my identification strategy and provides some visual evidence on how the policy affected hours and wages. I show my main results in Section 4. Section 5 shows further results on heterogeneity. Section 6 concludes.

¹[Crépon and Kramarz \(2002\)](#) finds job creation in the French reform in 1998-2000, but it is principally in the first phase (Aubry I) where financial incentives were provided for hours reduction and job creation.

²[Raposo and van Ours \(2010\)](#) also studies the Portuguese reform with the same data, but the scope of the paper is rather on the job separation probability between treated vs non-treated.

³A notable exception is [Kawaguchi, Naito, and Yokoyama \(2017\)](#) which uses regression discontinuity design to study the employment effect of the work-hour reduction in the context of Japan. On the other hand, the paper relies on a relatively small sample and studies heterogeneity only along the initial working hours.

2 Institutional Setting and Data

2.1 The Portuguese Working Hour Reform in 1996-1997

In July 1996, the Portuguese government passed a law that reduced the maximum standard workweek from 44 hours to 40 hours. This policy was implemented at once and at the national level and explicitly prohibits any reduction in monthly wages.⁴ Reducing unemployment was not the purpose of the reform, and instead it was to catch up with the European standard in terms of working hours (Varejao, 2006). For this reason, it was not accompanied by subsidies for job creation or any other instrument to compensate employers for the hours reduction, unlike the well studied French reforms around 2000. This makes the Portuguese reform an ideal setting to study how employers respond to working hour reductions without any directed incentives to increase employment.

All jobs had to meet the new standard hours in a two-step implementation. First, by 1st of December 1996 there should be a reduction of 2 hours in workweeks above 42 hours; the workweeks above 40 hours but below 42 hours should be set at the new standard of 40 hours of work. Second, by 1st of December 1997, all jobs should have a workweek at the new standard of 40 hours per week.

The maximum number of weekly working hours is set by the government, but it can be further reduced through collective bargaining between unions and associations of employers (if it is not detrimental for the worker relative to the standard set by the government). Before the reform, approximately 60% of the workers had a standard workweek of more than 40 hours and these workers were employed by approximately 2/3 of all establishments. One year after the reform, less than 1% of the workers reported a regular weekly working time of more than 40 hours per week.

It is worth noting that the reform did not change other aspects of working time, in particular overtime, which was kept at a maximum of 2 hours per day and 200 hours per year. The overtime premium was not changed either: 50% for the first hour above the standard workweek, and 75% from the second hour. On the other hand, the reform modified the calculation of overtime hours so that they are computed based on 4-month average rather than weekly. However, as shown later, the use of overtime was very rare even before the reform, reflecting the fact that the overtime premium is very high.

2.2 Data and Sample

Data. This paper uses *Quadros do Pessoal* (QP - “Lists of Personnel”), a yearly longitudinal administrative employer-employee dataset that covers the universe of Portuguese firms, establishments, and workers. All firms operating in the Portuguese territory with at least one wage-earner are required to fill the QP and send it electronically to the Portuguese Ministry

⁴More precisely, another law enacted long before the reform prevented employers from reducing monthly income when they reduce standard hours of their employees. This reform followed another one in 1991 that reduced the maximum number of weekly hours to 44 hours from 48 hours.

of Employment by April of the following year.⁵ The firms operating in public administration are exempt from filling QP. By and large, QP comprises 250 thousand firms and 2.5 million workers every year from 1992 to 2002.

QP collects detailed information at the firm, establishment, and worker levels. At the firm level, I have information on key firm characteristics, such as the activity sector (2-digit until 1994, 3-digit from 1995 for my dataset), region, sales, profits, total number of employees, capital structure, and ownership (share of national vs. share of foreign owners).⁶ At the establishment level, I have detailed information about all the personnel working in the establishment during the reference month (October). In particular, for each employee, information is collected on key demographic and labor market characteristics, including gender, age, education, occupation, professional rank, type of contract (temporary vs. permanent), wage-setting instrument (collective bargaining vs. other), standard weekly hours of work, total hours worked in the reference month, and overtime hours of work. I also have information on employee's remunerations, namely monthly wages (normal working hours), overtime payment, and compensation for particular objectives (bonuses, commissions, etc.).

I use information from 1993 to 2002. In 2001, the microdata relative to workers was not released, so I exclude this year from the analysis. The QP data are confidential data and of restricted access. It was made available by Statistics Portugal (INE - *Instituto Nacional de Estatística*) under a specific research protocol.⁷

Sample. For the main analyses, I use a balanced panel of 23,878 establishments that are observed in all sample years between 1993-2002 (except 2001) and have at least 5 employees in 1996.⁸ All sectors are included. I exclude establishments that exhibit rapid job growth or contraction (defined by the top and bottom 2.5th percentile) either before or after the reform. This was done to cope with a large fluctuation of the number of employees as I have many small establishments. Lastly, establishments belonging to firms that recorded zero sales in 1996 are not included in the sample. my study period starts in 1993 because 1992 is the first year since the previous working hour reform, which may confound my analyses.⁹ Ideally, I would like to avoid 2002 because it is January of this year when the use of EURO currency began in Portugal, which may have had a large economic impact, especially in tradable sectors. However, since I lack the 2001 worker file, I extend until 2002 to have a sufficient sample in the post-reform period. None of my sample construction procedures affect my results (are not shown in this version of the draft).

I also performed an individual-level analysis to supplement my main empirical result from the establishment-level sample. In constructing a worker panel over the sample period, I

⁵From 2010 onwards, the QP was renamed RU (*Relatório Único*), but the information collected remained essentially the same. The forms filled by the employer's can be found [here](#).

⁶However, I have not obtained profit and capital structure that are available in the linked accounting information for my sample years. I am working to get it.

⁷INE protocol reference PED-512101417.

⁸I also performed the analyses at firm level and obtained very similar results.

⁹The previous reform reduced standard hours from 48h to 44h in 1991. However, there were few workers affected by the reform because the vast majority of workers already had 44h contract when the reform took place, as confirmed in my data.

drop workers with an erroneous worker identifier or when base characteristics such as gender and age are missing.¹⁰ In the case of multiple job holding, I keep her main job that pays the highest monthly remuneration.

2.3 Descriptive Statistics

Figure (1) compares the distribution of weekly standard hours before and after the reform. There is a mass of workers with a 40h contract after the full implementation of the reform, corresponding to approximately 80% of all workers in my sample. This confirms that the policy was well enforced. Roughly 20% of the workers had standard hours of the previous ceiling, that is, 44h. Note that almost a third of the workers were already in the 40h regime prior to the reform.

Actual working hours were also significantly altered. Figure (2) shows clear bunching of working hours in the 39-40h bin in the post-reform period, while it was more flatly distributed between 38h and 44h before. Given that overtime is expensive (50% premium for the first hour and 75% premium after the second hour), the figure is consistent with the optimization behavior of employers to adjust their labor hour demand at the threshold standard hour. On the other hand, the standard labor supply theory would rather predict a presence of a “dip” at 40h. This implies that the working hours cannot be easily adjusted as desired by the workers’ side. In total, there are less than 10% of workers who worked longer than 40h in the 1998-2000 period.¹¹ Appendix provides further evidence from the Labor Force Survey and EU KLEMS that the actual hours decrease in other data sources, not only in the administrative data.

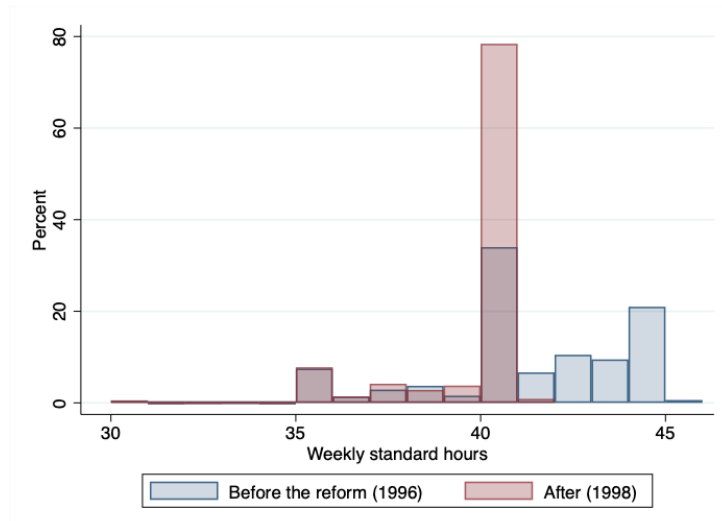
By law, employers were not allowed to cut the remuneration for workers when the standard hours were reduced. Figure (3) plots the mean monthly base remuneration (excluding overtime remuneration) relative to 1996 among “treated workers” whose standard hours exceeded 40h in 1996. There is no disruption in the monthly base income growth for these workers, confirming that the maintenance of monthly income was respected. Base remunerations did not deviate from the trajectory in 1998 for those who performed overtime in 1998 either (but they are paid extra for their overtime work, although not reflected in this figure). This also confirms that the employers did not reduce base remuneration (by reducing wage rate) as a means to avoid the total payments while letting workers work overtime to keep same working hours, as predicted in Trejo (1991).

Overall, these figures confirm that the reform was well enforced: employers were obliged to reduce standard hours, leading to a reduction in actual working hours, whereas the monthly remunerations to workers were not cut back.

¹⁰Sometimes workers are assigned a wrong individual identifier by error in the original data. I detect such cases by looking for the identifiers that have multiple workers of different gender or age in the same year. I remove those with such worker ID from the sample.

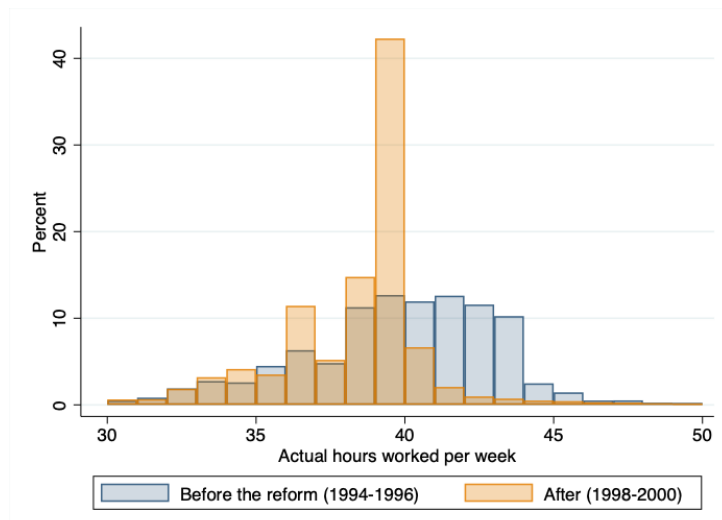
¹¹There is a concern of misreporting of working hours, or more specifically unpaid work, which does not show up the administrative data. However, Labor Force Survey confirms that actual hours worked declined when the workers themselves report.

Figure 1: Distribution of weekly standard hours



Note: The figure shows the distribution of the standard weekly hours for all workers recorded in the QP. The blue bins correspond to the distribution of standard hours in October 1996, before the first phase of the reform became binding in December 1996. The red bins are of October 1998, after the reform was fully put in place (in December 1997). In my data, standard hours are usually multiples of 1, but in some cases multiples of 0.5 (e.g. 38.5h per week). The figure confirms that after the reform, standard hours were changed to 40h for those whose contractual hours used to be above 40h.

Figure 2: Distribution of weekly actual hours worked



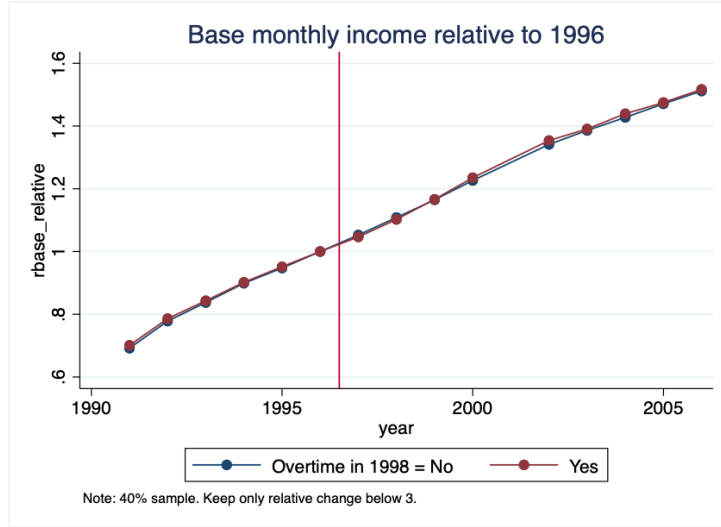
Note: The figure shows the distribution of actual hours worked per week for all workers recorded in the data, separately for 1994-1996 (pre-reform period) and 1998-2000 (post-reform period). The distribution is capped at 30h and 50h. Weekly hours are imputed from total hours worked in a reference month (October) recorded in the data, correcting for number of business days in the reference month in each year, using the method described in Appendix A.1. The figure shows a bunching of working hours in the bin of 39h-40h after the reform.

3 Empirical Strategy and Policy Impact

3.1 Empirical Strategy

Source of variation. To estimate the employment effects of the working hour reduction policy, I compare the evolution of the outcomes across establishments that differ in the intensity of treatment. Specifically, I measure the treatment intensity by the *hour share of treatment* defined

Figure 3: Evolution of monthly income relative to 1996



Note: The figure plots the average value of monthly base income (relative to 1996) for workers whose standard hours were above 40h in 1996, i.e., the treated workers. This excludes remunerations paid for overtime hours. The figure confirms that employers did not reduce the base monthly payment to workers, as it was prohibited. This is the case regardless of whether a worker performs overtime or not, as the monthly pay does not decline in 1998 for those who performed overtime in 1998, contrary to the prediction from Trejo (1991).

by the following expression:

$$HourShareTreat_j = \frac{\sum_i \max\{Hour_{ij,1996} - 40, 0\}}{\sum_i Hour_{ij,1996}} \quad (1)$$

where $Hour_{ij,1996}$ denotes the standard hours of a worker i working at an establishment j . This measure captures the share of total hour input (standard hours) that was subject to reduction following the reform. For example, for an establishment with 10 workers composed of 5 employees with 40h regime and 5 employees with 44h regime in 1996, the treatment intensity is 0.048 (=20h/420h). The concept follows closely the idea of measuring “bite” of the policy used in the minimum wage literature, which often uses the wage bill share of workers earning below the new minimum wage. In robustness checks, I also use alternative definitions of treatment intensity, such as share of treated workers or wage bill share of treated hours, and show that my findings are not sensitive to the measurement of treatment intensity.

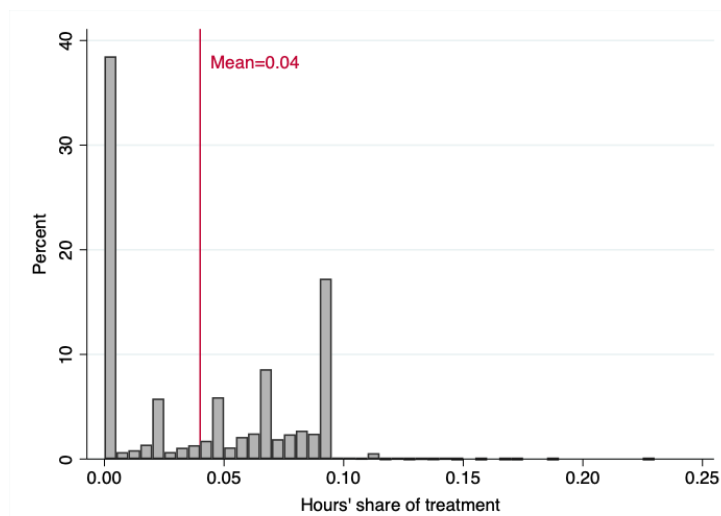
Importantly, Weuse standard hours in 1996 in computing the treatment intensity, instead of the actual hours worked. It is natural to use standard hours as they were the legal target of the reform. More importantly, while the actual hours worked are seemingly suited to gauge an actual bite of the reform, it is sensitive to the business cycle and temporary demand shocks (either at macro-level or establishment-specific). There are establishments where actual working hours temporarily increased (decreased) just before the policy due to short-term demand fluctuations and I will erroneously measure their treatment intensity when using actual hours. To the extent that it is correlated with the employment adjustment in the post-reform, my estimation of the employment effect will be biased.¹² In contrast, standard hours capture

¹²Confirming my concern, the actual working hours of more treated establishments jump from 1995 to 1996 when I use actual hours worked to define treatment intensity. This is because I overestimate treatment intensity for some establishments facing a short-term labor demand in 1996. This is purely mechanical because the same jump

a more stable and permanent component of the establishments' production process and performance. For a very small subset of workers, standard hours are recorded as zero in the data, either by error or due to their work arrangement. We use the actual hours worked only for these workers.

Figure (4) shows the distribution of the treatment intensity across establishments. Roughly one third of the establishments was not at all affected by the reform (i.e., none of their workers' standard hours exceeded 40h in 1996). On the other hand, nearly 20% of establishments had the hour share of treatment of around 0.091 corresponding to the situation where all workers had a 44h contract before the reform. On average, the hour share of treatment is 0.04, i.e. 4% of the total labor hour input had to be reduced.¹³

Figure 4: Distribution of hour share of treatment



Note: The figure shows the distribution of the treatment intensity at establishment level as defined in Equation 1. Note that some establishment exhibits the intensity higher than the theoretical maximum (that is, $0.91=4h/44h$) because actual hours were used for some workers when contractual hours were not available.

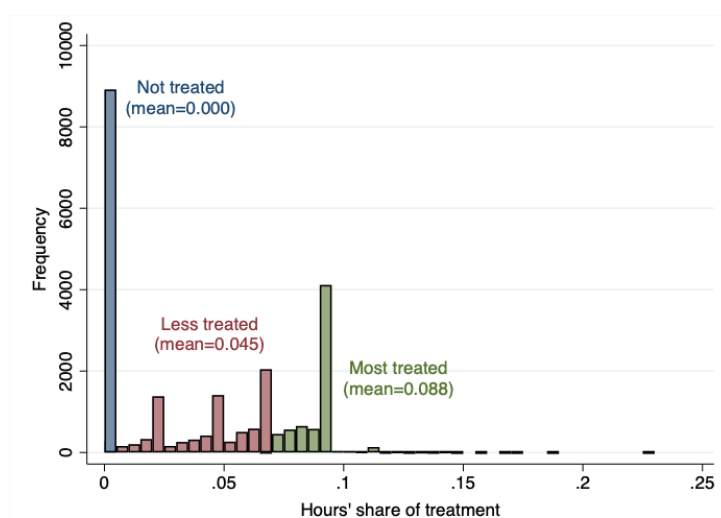
We split these establishments into 3 groups as in Figure (5) when making aggregate comparisons graphically: non-treated group (mean treatment intensity 0), less treated group (mean 0.045), and more treated group (mean 0.088). Non-treated group constitutes of establishments where no workers were treated. Less treatment group constitutes establishments whose treatment intensity was below the median among establishments with at least one worker treated. The rest is classified as the more treated group. However, the continuous version of the treatment intensity is used throughout the regression estimation later.

Table (1) provides the mean values and standard deviation of some of the establishment characteristics separately for the 3 comparison groups. In general, more treated establishments are on average smaller and low-paying relative to non-treated and less treated establishments.

occurs when I use another year (e.g., 1995) as a reference pre-treatment period. If these establishments increase employment in the post-1996 period to cope with the demand, my biased estimation points to a large positive employment effect; the opposite will happen if these establishments significantly reduce employment following the end of temporary demand.

¹³Some establishments having higher treatment intensity than the theoretical maximum of 0.09 ($=4h/44h$) is due to the mis-recording of standard hours above 44h in 1996 and to the use of actual working hours for a very small number of workers whose standard hours are recorded zero in 1996.

Figure 5: Distribution of hour share of treatment



Note: The figure shows how I divide establishments into 3 groups according to the treatment intensity defined in Equation 1. The non-treated group corresponds to the establishments with hour share of treatment equal to zero, i.e. with no treated workers. Less (more) treated establishments are those whose hour share of treatment is below (above) the median among establishments with at least one worker treated

Manufacturing sector (service sector) establishments are more represented (less represented) in the less treated group, compared to non-treated or most-treated groups. In the Appendix, Figure (A5) shows more detailed description of sectoral composition of these groups. All in all, more treated establishments tend to be more from low-paying service sector such as wholesale, retail and trade, and hotels and restaurants.

3.2 Policy Effects on Hours

Figure (6) shows the evolution of the mean standard hours by treatment group. Mean standard hours of more treated workers sharply dropped from 41h to slightly below 38h between 1996 and 1998, while it was reduced from 39h to 37.5h for the less treated group. No corresponding reduction was observed for the non-treated group. Importantly, an almost identical picture emerges for the actual hours worked in Figure (7), where the actual hours worked are corrected for the number of business days in a reference month each year, as explained in the Appendix A.1. These confirm that the reform was well enforced and had an actual impact on the production process. Figure (8) shows that overtime use did not see a sharp increase after the reform for the treated group. Interestingly, overtime was more used by non-treated establishments, perhaps reflecting the lower standard hours and the volatility of weekly labor demand. Figure (9) shows that on average, overtime hour is a small component of total working hours (for the non-treated group, the average is 0.4h, meaning only 1% of standard hours), mostly likely because overtime is expensive for employers (50% premium for a first hour and 75% from a second hour). Employers did not use overtime to respond to the hour reform, as there is no marked increase in overtime hours.¹⁴

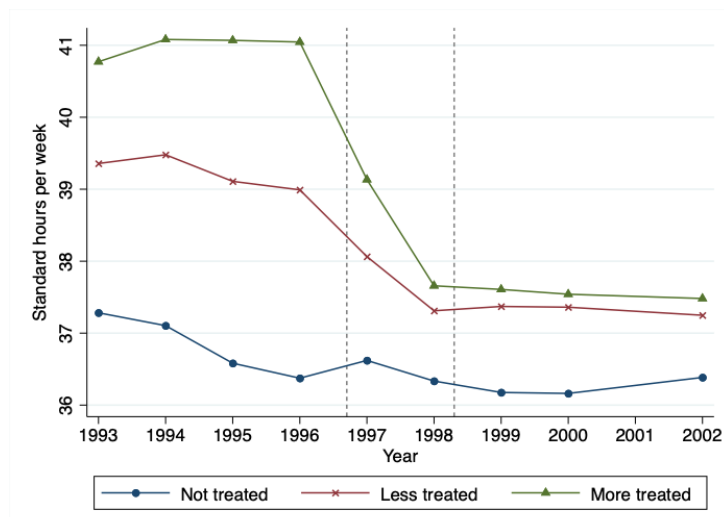
¹⁴Some noisily jumps toward the end of sample period is due to shocks to some establishments in specific sectors (in real estate after 1999 and construction after 2000).

Table 1: Summary Statistics

	All	Non-treated	Less treated	More treated
<i>Basic Characteristics</i>				
Number of employees	24.8 (91.9)	24.6 (117.6)	30.3 (86.6)	18.0 (48.5)
Mean wage	4.58 (3.1)	6.05 (3.7)	3.93 (2.4)	3.40 (1.6)
Mean age	35.5 (6.7)	36.7 (6.6)	34.9 (6.6)	34.6 (6.6)
Share of female	0.36 (0.3)	0.35 (0.3)	0.34 (0.3)	0.41 (0.3)
Share of college grads	0.03 (0.1)	0.06 (0.1)	0.02 (0.1)	0.01 (0.0)
<i>Sectoral composition</i>				
Sector I	0.02	0.03	0.02	0.02
Sector II	0.39	0.32	0.58	0.23
Sector III	0.59	0.65	0.40	0.75
Number of establishments	23,873	8,913	8,305	6,655

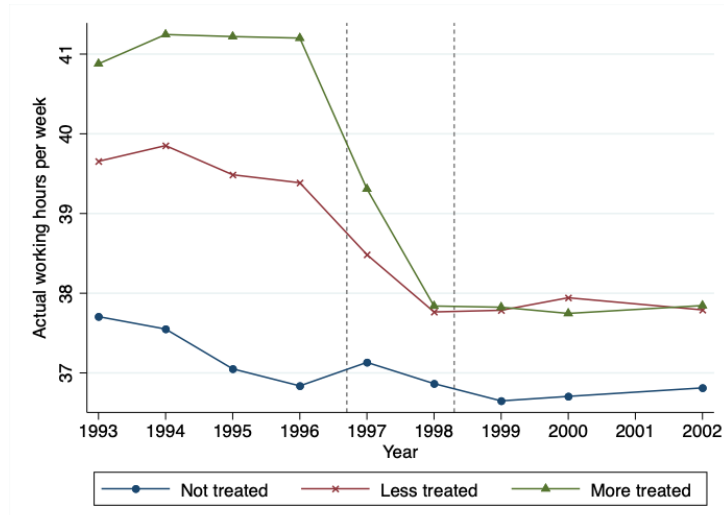
Note: The table shows the mean and standard deviation of some establishment characteristics in our sample. Sector classification is as follows: Sector I (Agriculture, livestock, hunting and forestry, Fishing, and Mining and quarrying); Sector II (Manufacturing industries, Electricity, gas and water supply, and Construction); Sector III (Wholesale, retail trade and repairing, Hotels and restaurants, Transport, storage and communications, Financial activities, Real estate, renting and business, Education, Health and social work, and Community, social and personal service).

Figure 6: Evolution of standard hours by treatment groups



Note: The figure plots mean weekly standard hours of workers belonging to each treatment group defined in Figure 5. Two gray vertical indicates the beginning of the first phase of the reform and the completion of the second phase. The figure shows that average contractual hours reduced from 41h to below 38h due to the reform for the more treated establishments, while there is no discernible change for the non-treated establishments.

Figure 7: Evolution of actual hours worked by treatment groups



Note: The figure plots the mean actual hours worked of workers belonging to each treatment group defined in Figure 5. Actual weekly hours are imputed from monthly hours as described in Appendix A.1. The actual hours worked also show a very similar pattern as standard hours described in Figure 6.

Figure 8: Evolution of use of overtime by treatment groups

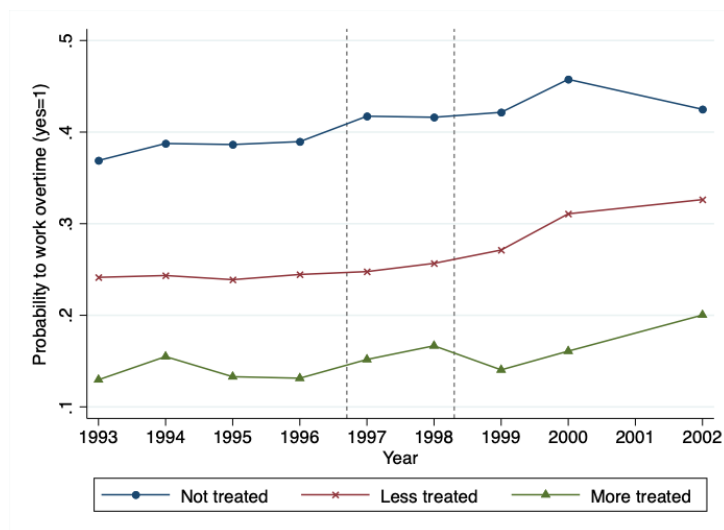
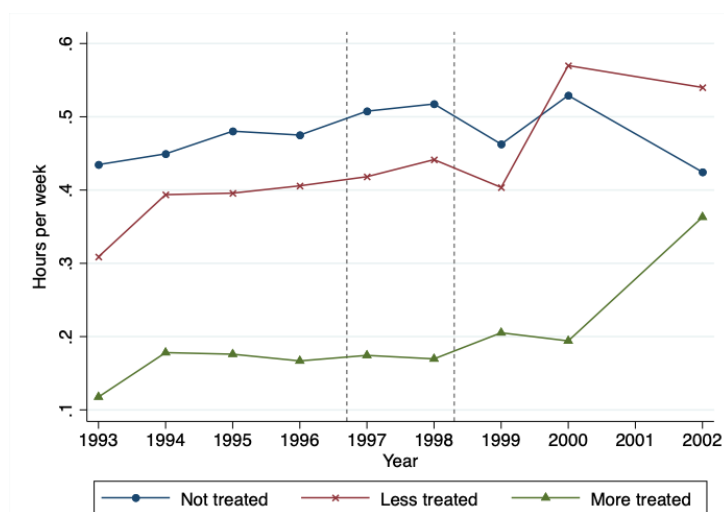


Figure 9: Evolution of overtime hours by treatment groups



Note: The figure shows the mean overtime hours of workers belonging to each treatment group defined in Figure 5. The figure shows that overtime hour is not a large share of total hours (nearly 1% of total hours even for the non-treated establishment which used the longest mean overtime hours), nor it increased after the reform. The anomalous jumps in the figure are due to shocks to some establishments in specific sectors (real estate after 1999 and construction after 2000). Removing of these sectors results in a stable trend from the previous years.

4 Main Results

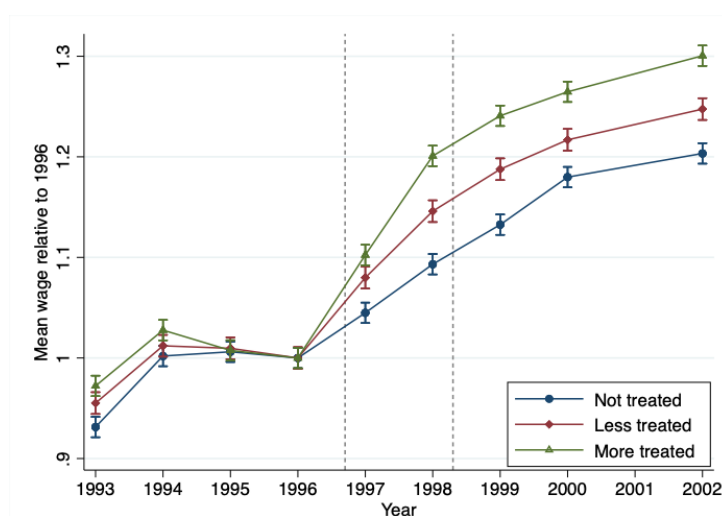
4.1 Graphical Evidence of the Reform Impact

I first provide graphical evidence on the effects of the reform by comparing the evolution of key outcomes across establishments. The assumption of parallel trend across establishments is also visually examined.

Labor cost (base hourly wage). Figure (10) follows the evolution of mean base hourly wage (not total hourly wage including the overtime premium) relative to 1996 among three groups of establishments with different treatment intensity. Clearly, the reform created a significant jump in mean hourly wage rate for more treated establishments, because of the legal obligation to maintain monthly income while reducing standard work hours. In 1998 when the reform was fully put in place, the average mean wage (relative to 1996) of most treated establishments becomes nearly 10 point above the non-treated establishments. This is numerically consistent with the 0.088 reduction in total working hour input for the more treated establishments. Importantly, the gap is stable until 2002, implying that the employers did not adjust (or suppress) wages dynamically to fully shift incidence on incumbent workers over time, or impose lower wages on the newly hired workers. This is evidence against the prediction of the fixed job model proposed in Trejo (1991), which predicts that contractual wage rates will fall so that total working hours and total monthly income remain completely unchanged. Note also that there is no concerning differential trends before the reform. Employers therefore faced both the reduction in working hours and the increase in unit labor cost.¹⁵

¹⁵A stagnation in wage for 1994-1995 is due to some establishments whose wage declined largely before 1996. Excluding them results in an increasing pre-trends that are similar across three groups.

Figure 10: Evolution of mean labor cost (hourly wage) (1996 = 1)



Note: The figure shows, for each of the three treatment group, the average of per-establishment mean base hourly wage relative to 1996 and its confidence interval. Hourly labor cost increases sharply for treated establishments due to the reduced working hours with fixed monthly remuneration. Average wage rate of the non-treated establishments also increased, but this is driven by a small number of establishments whose wage increased substantially in 1994 then decreased sharply until 1996. Such are present in other treated groups as well. Removing these establishments result in a stable and parallel pre-trend in wage rate for all three groups until the reform kicks in.

Employment. I examine the employment effects visually in Figure (11). While the growth rate of employment is comparable across establishments until 1996, it diverges following the reform. Specifically, the employment growth of more treated group stagnates and decreases since the implementation of the reform, staying at 3-4 point below the non-treated and less-treated groups consistently until 2002. This is not due to the economic cycle in a specific sectors or establishments, as the economic slump (higher unemployment rate) started around 2001, 111 after the divergence I observe in the figure. No visible decline of employment for less treated group relative to non-treated group is consistent with the heterogeneity examined later. Essentially, this group contains more establishments from manufacturing and middle-sized firms, for which I find smaller negative employment effect. The figure provides a clear negative effect on the employment of the working hour reduction reform.

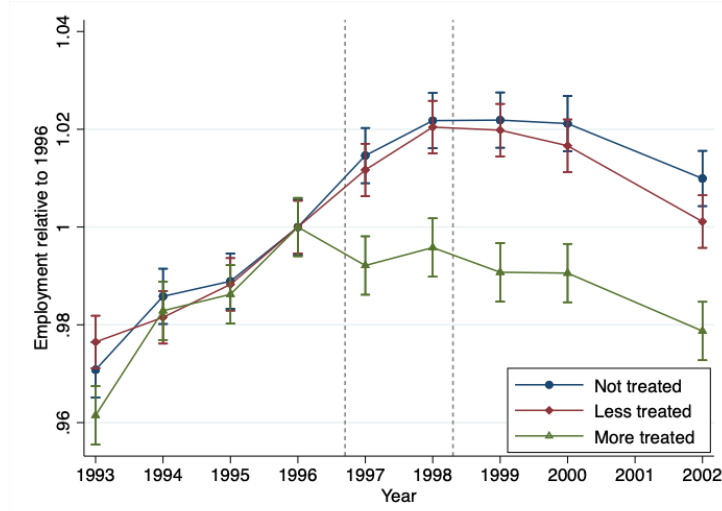
Sales. Figure (12) repeats the same comparison for sales.¹⁶ Knowing that the working hours were reduced, it is natural to study if and how production was affected. While the literature theoretically recognizes the key role played by production size adjustment (i.e., scale effect) as a channel of employment effects, it falls short of empirical evidence.¹⁷ Unfortunately, a direct measure of production is not available in my data. Instead, I use sales as a proxy measure for production. One caveat is that sales are also affected by price.¹⁸ Since sales information is available only at firm level, all analyses on sales use the inverse of the number of establishments per firm as a weight. From Figure (12), in the post-reform period, sales (relative to 1996)

¹⁶Since sales are volatile and their values relative to 1996 can be extremely large, I take a log difference of sales of each year and 1996 and add 1 to deal with these issues (instead of simply taking the ratio of sales relative to 1996).

¹⁷An exception is Crépon, Leclair, and Roux (2004) in the case of French working hour reform.

¹⁸Some recent evidence in the minimum wage literature highlights the shifting of labor cost increase to consumers through price (e.g. Harasztosi and Lindner, 2019, for example).

Figure 11: Evolution of employment (1996 = 1)



Note: The figure shows, for each three treatment group, the mean and confidence interval of employment relative to 1996. While the employment growth is relatively stable and comparable across the groups, but the employment of the more treated establishment declines when the reform started in 1997. Employment declines from 2000 due to the business cycle of that time in Portugal. The reason for no discernible decline in employment for less treated group is due to the fact that this group contains more establishments for which I find less negative effect in the heterogeneity analyses.

are lower, the more establishments are treated. Interpretation, however, requires caution as pre-trends are not necessarily similar across groups. I investigate more carefully the effect of sales in the regression framework including the differential trends across establishments (we find no statistically significant effect on sales). In the Appendix (A.3), I repeated the same analysis for 2 groups.

4.2 Differences-in-differences Estimation

Graphical evidence provides a first sketch of the effects of the reform. More treated employers faced higher labor cost and their employment growth were negatively affected, while the effect on sales is less clear. Here, I formally tests these results in a regression framework using a standard difference-in-differences with continuous treatment.

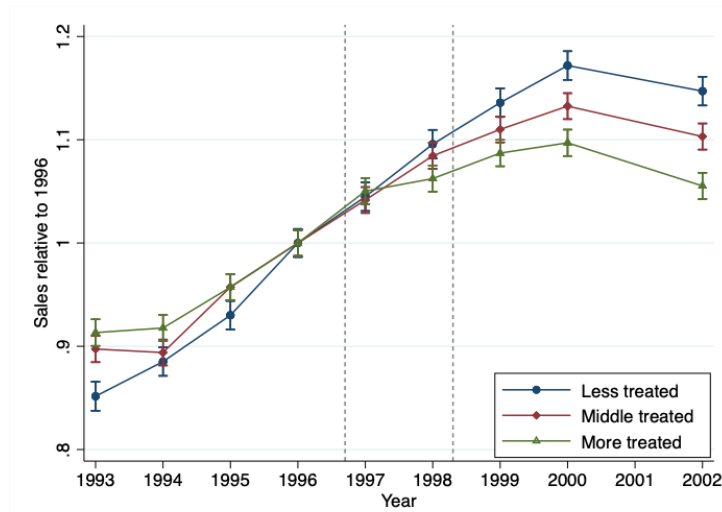
The baseline specification takes the following form:

$$Y_{jt} = \alpha + \beta \cdot (HourShareTreat_j \times Post_t) + \gamma X_{jt} + FE + \epsilon_{jt}, \quad (2)$$

where Y_{jt} is the outcome (relative to 1996) of establishment j at year t , $HourShareTreat_j$ the hour share of treatment as defined in the Equation (1), $Post_t$ the dummy that takes 1 for post-intervention periods (i.e., 1997-2002) and 0 otherwise, X_{jt} the time-varying establishment characteristics, FE's the fixed effects and ϵ_{jt} the error term. For X_{jt} , I include mean age, number of establishments per firm and foreign capital share. For the fixed effects, I include fixed effects in establishment, year, region, and industry.¹⁹ I later include establishment-specific linear trends to control for potential differences in trends of the outcomes. Note that my treatment variable

¹⁹Region and industry fixed effects are not perfectly linear with establishment fixed effects due to cross-region relocation of the establishments and change of industry in which an establishment operates.

Figure 12: Evolution of sales (1996 = 1)



Note: The figure shows the average of sales relative to 1996 for the three treatment groups. I use the inverse of number of establishment per firm as a weight, since sales are available only at firm level. Moreover, since sales is extremely variant year by year, I construct the sales relative to 1996 by adding 1 to the log-difference in sales relative to 1996. Although the figures shows lower sales for more treated establishments, it is inconclusive due differential trends before the reform.

$HourShareTreat_j$ takes continuous values. I am interested in β .

Employment effects. The estimated employment effects are presented in the Table (2). In column (1), I first estimate the most parsimonious diff-in-diff specification with only establishment fixed effects and time fixed effects. The estimated coefficient is negative and statistically significant from zero at 1% level. I obtain a similar result from the baseline model in column (2), which adds time-varying establishment characteristics and fixed effects of location and industry. In terms of magnitude, the coefficient of -0.19 implies that establishments where all workers used to work at 44h grew approximately 2 points less in the post treatment period, compared to those with no workers working above 40h in 1996.²⁰ In column (3), I add establishment-specific linear trends to control for potential non-parallel trends; the estimate is similar. The remaining columns also include high dimensional fixed effects and find similar statistically significant negative employment effects, while the coefficient is reduced because I lose large cross-industry(-location) variation in treatment intensity. Overall, the negative employment effects estimated in the diff-in-diff framework are consistent with the graphical variance presented before. In the Appendix Table (B1), I show the robustness of the results to alternative measures of treatment intensity. Also, in the appendix Figure (B1), I provide time-varying coefficients that confirm validity of the parallel trend assumption.

To put the estimate into perspective, I use the fact that the wage bill share of treatment is actually similar to the hour share of treatment (so that the percentage increase in wage and hour are comparable). Thus, constant labor demand elasticity is estimated as roughly 0.25, if I posit that the higher wage rate caused the negative employment. This is relatively low but in the range of the own-wage elasticities estimated previously in the literature (mean -0.551 with

²⁰When all workers move from 44h to 40h, the hours' share of treatment is around 0.091. Thus, most treated establishments' employment growth is 1.7 (=0.091*0.19) point lower compared the non-treated.

Table 2: Diff-in-diff estimates for employment

	(1)	(2)	(3)	(4)	(5)
	Parsimonious	Baseline	Firm trend	High-dim-FE (i)	High-dim-FE (ii)
HourShareTreat \times Post	-0.25*** (0.069)	-0.19*** (0.061)	-0.21*** (0.057)	-0.14*** (0.049)	-0.11** (0.057)
Mean age		-0.0093*** (0.000)	-0.0090*** (0.000)	-0.0089*** (0.000)	-0.0088*** (0.000)
Number of establishments		-0.00017** (0.000)	0.000088 (0.000)	0.00011* (0.000)	0.00011* (0.000)
Foreign share capital		-0.00063 (0.000)	-0.00014 (0.000)	-0.000080 (0.000)	-0.000092 (0.000)
4 fixed effects	Year & Estab	All	All	All	All
Estab-trend	No	No	Yes	Yes	Yes
Interaction FE	No	No	No	Industry-year	Industry-year-region
Observations	214,902	214,901	214,901	214,803	213,328
Cluster	Industry \times year	Industry \times year	Industry \times year	Industry \times year	Industry \times year

Note: The table investigates the effect of the working hour reduction on the employment using the specification in Equation 2. The outcome is employment relative to 1996. The first row corresponds to the diff-in-diff estimates. Column (1) includes only year and establishment fixed effects. Column (2) adds industry and region fixed effects, as well as mean age, number of establishment per firm, and foreign capital share as controls. Column (3) further adds firm-specific trend to deal with potential differential trends across establishments. Column (4) and (5) add high-dimensional fixed effects, where industry-year fixed effects and industry-year-region fixed effects are added respectively in the specification. Standard errors are clustered at industry-year level, but the result is robust to other levels of clustering. Standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

a standard deviation of 0.747 according to the meta-analysis by [Lichter, Peichl, and Siegloch \(2015\)](#)). However, several factors can explain why it is relatively lower. First, firing is very costly in Portugal. Second, wages offered to (potential) new hires could be different from (or more explicitly, lower than) treated incumbents' wage rates. Third, negative employment was partially offset by the positive labor demand to maintain the same output.

Wage and productivity effects. The effects on other outcomes are summarized in Table (3), where the estimation is based on the baseline specification with establishment specific trend. The column (1) confirms the increase in mean wage rate. In the column (3), I estimate the effect on sales. The coefficient is not statistically significantly different from zero and actually positive (although very small). This indicates that the reduction in working hours did not lead to an immediate down-scaling of production and that the negative employment effects are not necessarily due to scale effect. Note again that the interpretation is complicated by a potential price adjustment. Consistently, per-worker sales and per-hour sales increased, as confirmed in the column (4) and (5).

Evidence from diff-in-diff hints the necessity of looking at the employers' adjustment process in more detail. On the one hand, employers responded by reducing employment. However, on the other hand, they successfully avoided their revenue to go down, achieving much higher efficient use of labor hours (sales per hour).

4.3 Theoretical explanation

How do these results square with the theoretical predictions? The likes of theories inspired by the standard labor demand model, such as [Calmfors and Hoel \(1989\)](#) and [Hunt \(1999\)](#), show that, under certain conditions, employers would respond by reducing number of workers. Ac-

Table 3: Diff-in-diff estimates for various outcomes (with firm trend)

	(1)	(2)	(3)	(4)	(5)
	Mean wage	Employment	Sales	Sales per worker	Sales per hour
HourShareTreat \times Post	0.83*** (0.129)	-0.21*** (0.057)	0.033 (0.108)	0.21 (0.131)	0.83*** (0.157)
Mean age	0.0079*** (0.000)	-0.0090*** (0.000)	-0.0015*** (0.000)	0.0076*** (0.000)	0.0060*** (0.001)
Number of establishments	0.000043 (0.000)	0.000088 (0.000)	0.0017* (0.001)	0.0054** (0.002)	0.0056** (0.002)
Foreign share capital	-0.00049 (0.000)	-0.00014 (0.000)	0.00037 (0.001)	0.00020 (0.001)	0.000060 (0.001)
4 fixed effect	Yes	Yes	Yes	Yes	Yes
Firm-trend	Yes	Yes	Yes	Yes	Yes
Observations	214,901	214,901	214,901	214,901	214,901
Cluster	Industry \times year	Industry \times year	Industry \times year	Industry \times year	Industry \times year

Note: The table shows the effect of the working hour reduction on labor cost and productivity productivity, using the diff-in-diff specification in Equation 2 with firm-specific trend. All outcomes are relative to 1996. Column (1) studies the impact on mean base hourly wage, calculated excluding overtime pay. Column (2) shows the employment effect from the previous regression in Table 2. Column (3) studies sales. Since sales information is available only at firm level, I weight the estimate by the inverse of number of establishment per firm. Column (4) and (5) investigate on sales per worker and sales per hour as measures of production efficiency. Standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

According to these models, potential channels through which the negative effects may result can be summarized as follows: (1) scale effect, i.e. cutting down the production size due to the increased labor cost, leading to the reduction in employment; (2) substitution of labor with capital, because of higher labor cost; (3) substitution of workers with hours (if overtime was already used, or when fixed cost of hiring is sufficiently high), due to the fact that the marginal cost of hours remains unchanged while hiring a new worker becomes more expensive. Note that scale effect channel is not supported in my results, as sales were little affected. Similarly, the third channel of substitutions of workers with more hours is not consistent with the fact that the actual hours worked declined. Hence, the only mechanisms in the previous theoretical models that is consistent with my results is substitution of labor service with capital (i.e., increase in capital input in production in the standard labor model of [Calmfors and Hoel \(1989\)](#), or increases in shiftwork in [Hunt \(1999\)](#)). In both models, the marginal effect of the increased wage rate that I observe is rather to increase employment, in the case of fixed output (which is the case in my estimate of no significant impact on sales) because fixed cost of hiring becomes less important as wage increases. In the subsequent section, I study the heterogeneous effects across sectors with different capital intensities in their production input to examine if the negative employment is observed more strongly in sectors that use capital. To foresee the result, I do not find a clear systematic result.

4.4 Alternative mechanisms

First, the existing theoretical models are not explicit about the nature of the production function. If the production is strongly concave in labor hour input, due for example to fatigue, then the reduction in working hours naturally does not result in a proportional loss of production. [Pencavel \(2015\)](#) provides empirical support for decreasing marginal output with respect

to working hours in the case of munition workers. To the extent that this result can be generalized to other tasks and occupations, my findings is consistent with a strong concavity of the production function. Second, labor market frictions can result in a non-optimal equilibrium of employment, such as the model by [Acemoglu \(2001\)](#), leading to too many low-productive jobs; when the wage increases, these jobs will be destroyed and productivity increase, as consistent with my results. Third, personnel economics models predict a long equilibrium working hours as a result of promotion competition across workers (e.g. tournament theory). Managers may not be able to precisely assess the link between tasks and output and base the promotion decision of their workers on the observed effort measured by hours worked. Workers might signal their effort through hours, resulting in an excessively long equilibrium hours with little output gain. Working hour reduction policies cap such promotion competition at lower equilibrium hours, while production barely affected.

Moreover, employers might be able to gain efficacy by inducing more effort from the workers ([Askenazy, 2004](#); [Lazear, Shaw, & Stanton, 2016](#)). Efficiency gains can also come from other mechanisms such as turnover and training ([Riley & Rosazza Bondibene, 2017](#)).²¹ Another mechanism to improve labor efficiency is to change the composition of workers: employers keep or hire relatively high skilled workers. This is also the case when employers introduce or more extensively use production technologies that are more complementary to high-skilled workers. I study the reform's effect on the composition of the workforce in [Table 4](#) in the same diff-in-diff framework.²² Column (1) shows that the share of workers with at least high school degree (which are relatively high-skilled that time in Portugal, constituting only 1/4 of the workers in my sample) slightly increased, consistent with the idea that the treated establishments increased the skilled workforce to raise efficiency. Column (2) shows that the college share did not increase statistically significantly. This is primarily because the share of workers with college degree was very low (only a few percent in my sample) in the context of my study back in the late 1990s and early 2000s. Column (3) shows that the share of female workers increased in the establishments where working hours were more reduced, which is consistent both with supply and demand explanation.²³ These evidence suggests that the changed workforce composition is one potential mechanism of increased labor efficiency.

²¹Not shown in this draft, but I estimated the effect on training participation comparing treated and non-treated workers in diff-in-diff setting using Labor Force Survey and do not find significant increase in training participation. Similarly, there is no statistically significant increase in night shift or shift work (to increase capital operation time per worker), predicted in the model of [Hunt \(1999\)](#). Note however that the study using LFS is complicated by the change in sampling methodology of Portuguese LFS from 1998 onward.

²²The results are obtained from the sample what removes workers with duplicated worker identifiers and keeps her first job which pays the highest income. For this reason, the number of observation is smaller than the previous diff-in-diff; but my total employment is unchanged with this sample.

²³This is consistent with the supply side explanation that women have stronger preferences for better working conditions (i.e. shorter work hours) and therefore their probability of joining or staying in the treated establishment increased. On the other hand, it is also consistent with the demand side story that employers attempt to reduce labor costs by increasing the portion of workers to whom they have more bargaining power (note, however, that the mean wage rate did not see a stagnation for treated establishments even after the reform).

Table 4: Diff-in-diff estimates for worker compositions

	(1)	(2)
	Share(high school +)	Share(college)
HourShareTreat \times Post	0.072***	0.0027
	(0.026)	(0.013)
4-way fixed effect	Yes	Yes
Estab-trend	Yes	Yes
Observations	150648	150648
Cluster	Industry \times year	Industry \times year

Note: The table shows the effect of the working hour reduction on worker compositions, using the diff-in-diff specification in Equation 2 with firm-specific trend. Worker compositions are calculated with the sample that removes duplicates worker identifiers and keeps only one highest paying job per worker (i.e., remove multiple job holdings). Column (1) studies the impact on the share of high school workers or above. Column (2) shows the effect on college graduate share. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

5 Heterogeneity results

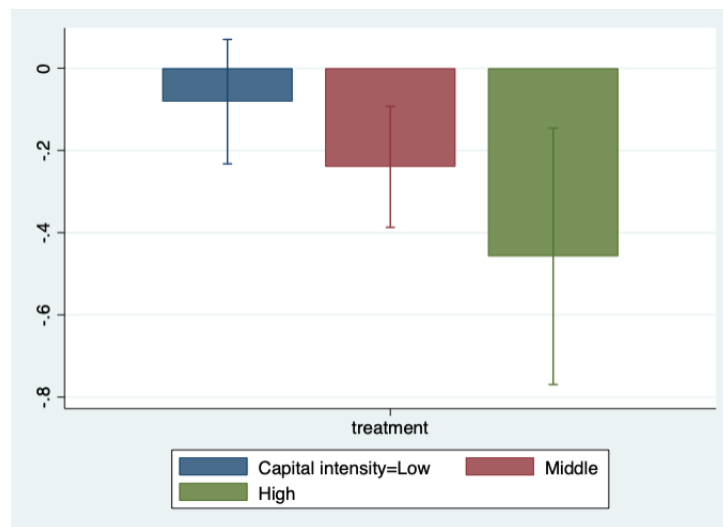
Employment responses to the working hour reform might not be homogeneous. In light of the theory, it can depend on the nature of the production function (e.g., capital input share) and cost structure (e.g. fixed cost). More broadly, it is natural to posit that better firms, measured by wage level or size, have more capacity to absorb negative shocks (more financial strength to endure full incidence on employers or reorganize production). Similarly, the competitiveness of the domestic product market or exposure to international competitors limit the ability for firms to pass the incidence onto consumers. Such heterogeneous response has not been given much empirical investigation previously. I study the heterogeneous effects by interacting diff-in-diff variable $HourShareTreat_j \times post$ with the categorical dummies.

Figure (13) shows the results for employment. The negative employment effects are stronger for the establishments in sectors with higher capital intensities. The negative coefficient is small and statistically insignificant from zero for the establishments in low capital intensity sector. If I posit that the substitutability between labor and capital is higher in more capital intensive sector, this implies that the establishments may have reduced employment by using more capital in the production (either by increasing capital stock or operation time of capital) when the labor hour inputs were reduced and the cost of labor increased. (14) explores the heterogeneous effects on sales across capital intensities. The reduction in working hours seems to have negative effects on sales in establishments in the labor intensive (i.e. low capital intensity) sector, with the coefficient significant at 10% level. Establishments in middle- and high-capital intensity sectors did not experience such negative effect.

These results on the heterogeneity effects demonstrate very different consequences of the working hour reduction across firms with different production structure. In more capital intensive sectors, firms may have increased capital use in production in order to compensate for the loss of working hours, substituting labor service with capital service given the highest cost of labor. This provides indirect but consistent evidence for the capital substitution mechanism in the theories that predict the reduction of employment as a result of the forced working hours reduction. Labor intensive sectors react differently. With the importance of labor in the production, the establishments have lower possibility of substituting labor with capital - in

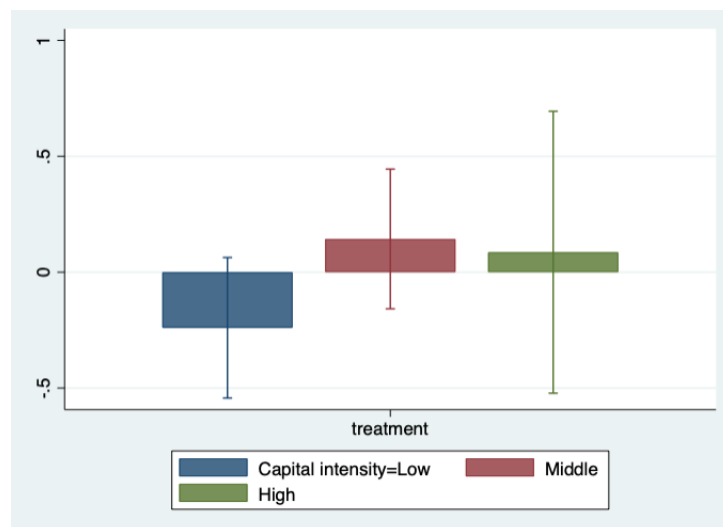
this case, employers rather attempt to keep the original employment, but the production goes down as a result of the working hour reduction.

Figure 13: Heterogeneous treatment effect by capital intensity: Employment



Note: The figure shows the employment effects by capital intensity in 1996 measured at 2-digit sector level using EU KLEMS database. Capital intensity is defined by total capital compensation divided by value added. I then split sectors into the 3 equal-sized categories according to their capital intensity and match with the main sample based on the establishments' sector.

Figure 14: Heterogeneous treatment effect by capital intensity: Sales

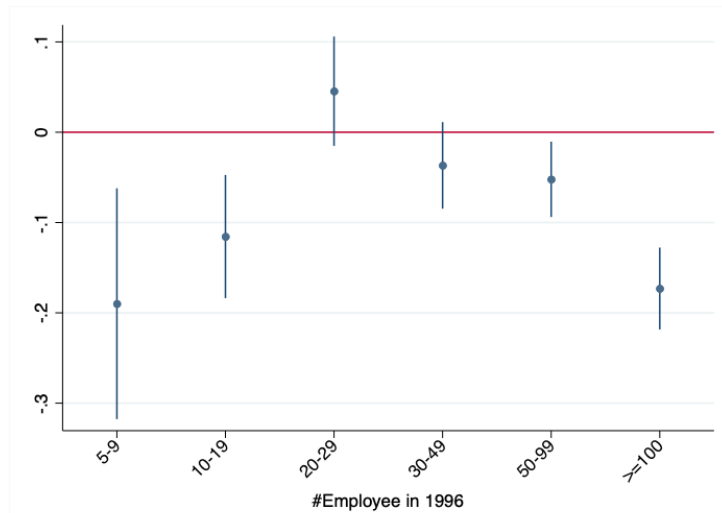


Note: The figure shows the effects on sales by capital intensity in 1996 measured at 2-digit sector level using EU KLEMS database. Capital intensity is defined by total capital compensation divided by value added. I then split sectors into the 3 equal-sized categories according to their capital intensity and match with the main sample based on the establishments' sector.

Firm quality and size. I further investigate heterogeneity alongside measures of firm quality in Figure (16). Firm quality is defined by mean wage quantile as of 1996, i.e. before the reform. The estimates are generally negative across firm quality, but tend to be slightly smaller at the upper end of the firm quality distribution. "Better" establishments might be able to absorb negative shocks without passing the incidence on workers, or they avoid putting negative

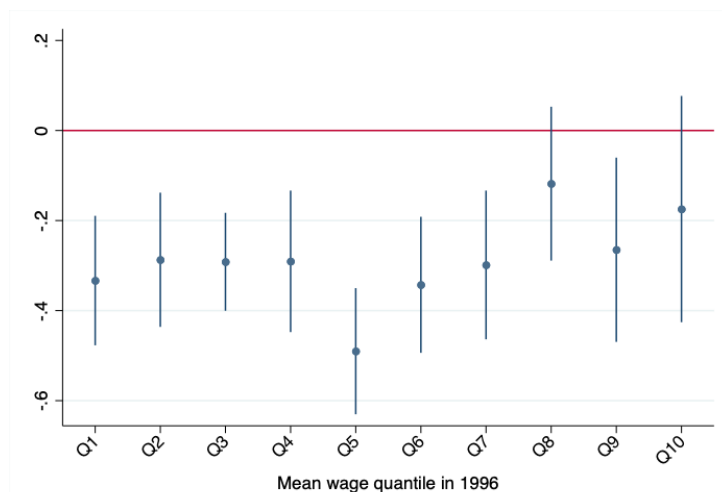
consequence for workers in order to induce loyalty from their employees. When I analyze it by firm size in 1996 in Figure (15), the negative employment effects are more observed in small firms (which constitute the majority of samples); but the effect is also negative in largest establishments with more than 100 employees.

Figure 15: Heterogeneous treatment effect: firm size in 1996



Note: The figure shows the employment effects by the number of employees, estimated by interaction sector dummies with $(HourShareTreat \times Post)$.

Figure 16: Heterogeneous treatment effect: firm quality (mean wage in 1996)



Note: Financial sector omitted from the analysis.

Note: The figure shows the employment effects by quality of establishment, estimated by interaction sector dummies with $(HourShareTreat \times Post)$. Establishment quality is measured by mean wage quantile in 1996.

6 Conclusion

In this paper, I investigated the impact of the working hour reform in Portugal that reduced the standard hours from 44h to 40h in 1996-7. Using the variation across establishments in terms of the intensity of treatment, I found that the establishments that were more treated experienced lower post-reform employment growth compared to less or non-treated counterparts. Such negative employment is relatively modest, hinting at the elasticity of roughly 0.25 with respect to own wages. On the other hand, I do not find a significant negative impact on sales, leading to a large gain in efficiency of production, measured by sales-per-hour. This masks substantial heterogeneity in the responses to the reform. The establishments in capital intensive sectors responded by reducing employment more, without any negative effects on sales. Conversely, the negative employment effects are much smaller and insignificant in labor intensive sectors, and the establishments in these sectors experienced a decline in sales. These are indirect but consistent evidence for the theories that predict negative employment effect of the working hour reduction through the scale effects and capital substitution effects, for which no previous work provided empirical support. In addition, I find an increased share of high skilled workers after the reform in the treated establishments, consistent with the improve in labor efficiency. This also indicates the existence of alternative mechanisms for employers that the previous standard labor demand models have not highlighted.

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Appendices

Appendix A Data, sample, hours and supplementary descriptives

A.1 Data and sample

QP records the total hours worked during the reference month (March until 1993 and October since 1994). This means that the recorded hours worked depend on the number of business days in each year's reference month. In the raw data, working hours indeed increases when the reference month has less weekends or no national holiday, and vice versa (the maximum difference is 3 days in terms of business days between years with least and most business days in the reference month). This is another reason why defining treatment status based on actual hours worked in 1996 can be dangerous. None of the previous work addressed this issue to the best of my knowledge. On the contrary, standard hours are not affected by this because the data records contractual weekly hours. For actual hours worked, I correct for this issue in the following steps before the process of sample construction: first, I calculate the mean number of weekdays and holidays of the reference month across years and calculate each year's deviation from the means. Second, I then regress the total hours worked of individual workers on these deviations in terms of number of weekdays and holidays, worker characteristics (age, gender, and education), and fixed effects in worker, firm, industry and region. Using the obtained coefficients, I adjust each worker's recorded hours so that they represent the total working hours in a month with the number of business days. Note that in the regression, I use all available years to us (1988-2010). Besides, I separately run for two periods, i.e. until 1996 and after 1998. I did not include 1997 as the working hour reform was not fully implemented. I ignored the change in standard hours in 1991 since in practice it did not seem to have had a large impact. I use this corrected actual hours whenever presented in this paper. I do the same for the recorded overtime hours, separately.

A.2 Hours from different data source

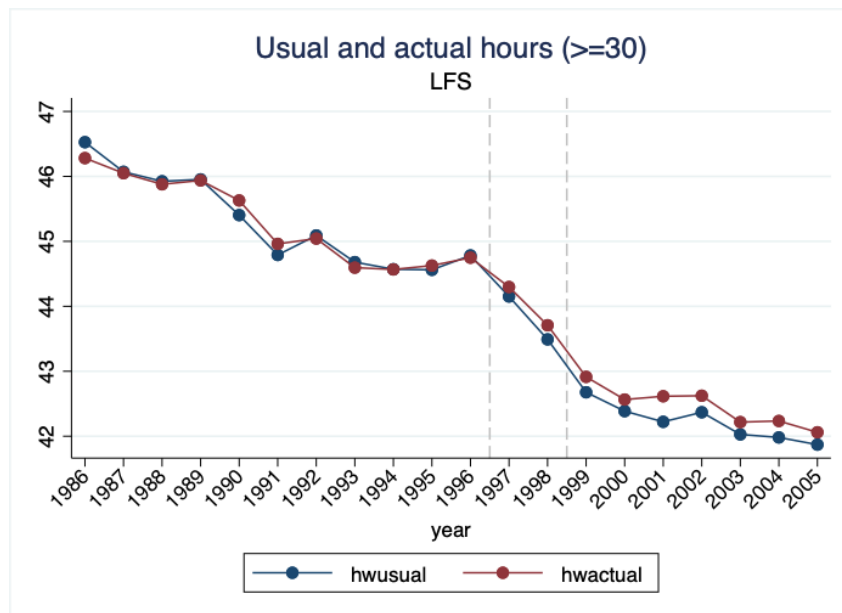
This section provides the evolution of working hours from different sources. Although the working hours in QP are "correct" by construction since it is administrative, it may not necessarily reflect the actual working hours.²⁴ In particular, there is a concern that the actual working hours did not drop, while it shows up as a decline in working hours both in contract and in declared actual hours in QP — indeed, this in fact can be perfectly consistent with my finding of an increase in labor efficiency if the decline in ours were simply "superficial".

Figure A1 shows the evolution of usual hours worked and actual hours worked per week in the Labor Force Survey. Since these hours are self-reported by workers themselves, rather than declared by employers, it allows us to check if hours worked indeed declined or not. The figure makes it clear that the working hours declined after the reform. I also provide the distribution of hours. Figure A2 shows the distribution of usual hours per week. While it is exactly the same as contractual hours, it looks fairly similar to the distribution of contractual hours in QP and there is an increase in mass of working hours between 39-40h in the post-reform periods. Figure A3 shows the distribution of actual working hours. While there are some discrepancies such as less hours between 41h-44h before the reform, the figure shows that many workers' actual working hours are moved to 40h in the post-reform period. Slight discrepancies may come from two things: first, QP records only the month of October, and second, I corrected actual working hours in QP to correct for number of business days in October for each year.

²⁴Note, however, that QP was created for the authorities to check if the collective agreements were respected by employers in the first place, and it is monitored to ensure the accuracy of the information.

The decline in hours are reflected in the aggregate series, too, such as in National Account available at EUKLEMS, as shown in Figure A4. Per-worker annual hours in this data is imputed from total hours in the economy and total number of workers, therefore has a very different procedure of measuring hours, often providing a discrepancy with micro-level information. Furthermore, they cover more types of workers such as self-employees, the impact of the reform can be blurred compared to QP. However, in this data too, per-worker annual hours also has a clear discontinuity before and after the reform. The evidence from LFS and EUKLEMS are consistent with the fact that the hours actually declined.

Figure A1: Evolution of in Labor Force Survey

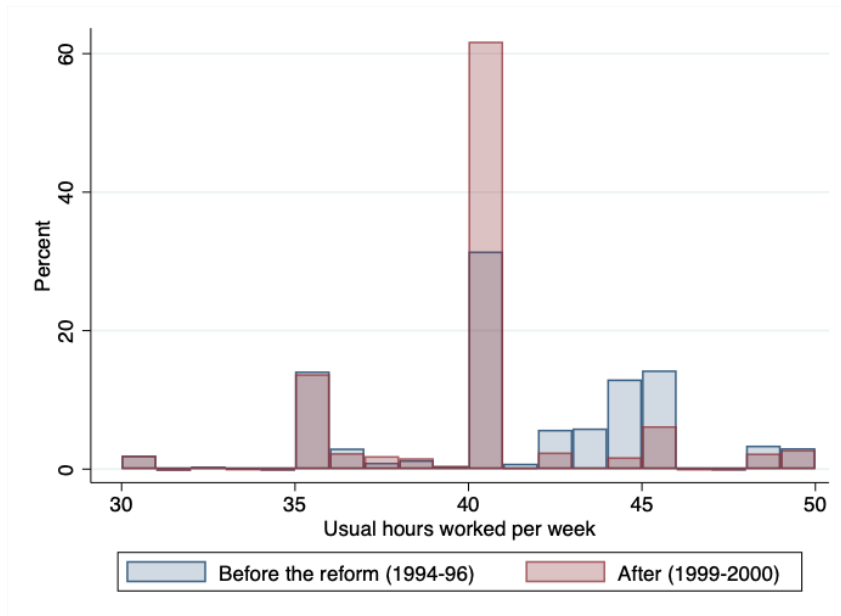


Note: The figure provides the average working hours (both usual and actual) for those who worked at least 30h in the survey week, as recorded in the Portuguese Labor Force Survey. The figure drops individuals whose working hours were different in the survey week due to holidays. Note that the sampling procedure for LFS in Portugal has changed in 1991 and 1998.

A.3 Supplementary descriptive statistics

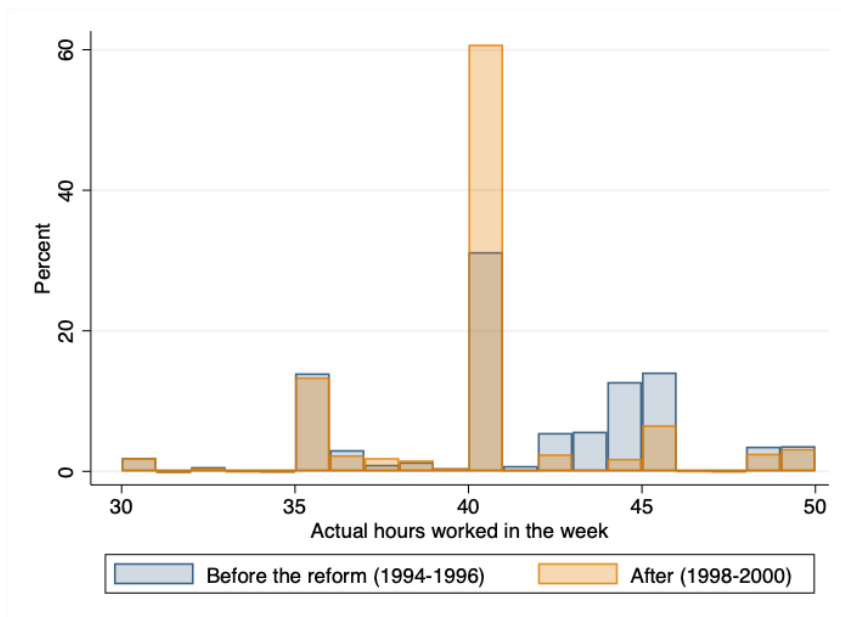
Figure A5 shows the share of each treatment groups across sectors (1-digit) in my sample. Note that the continuous version of the treatment intensity is in the regression estimations and this 3-group partition is for the graphical purpose only.

Figure A2: Distribution of usual hours in Labor Force Survey



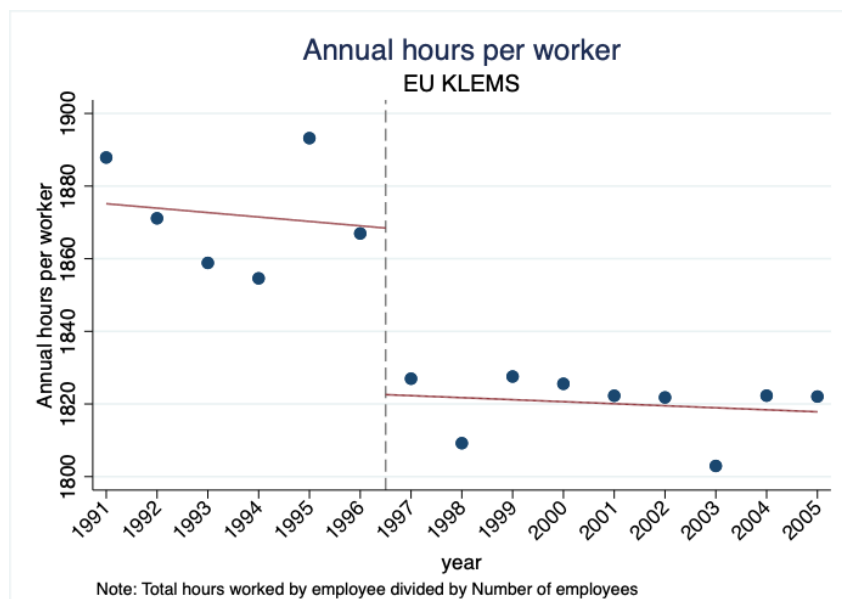
Note: The figure shows the distribution of usual hours per week as declared in the Portuguese Labor Force Survey.

Figure A3: Distribution of actual hours in Labor Force Survey



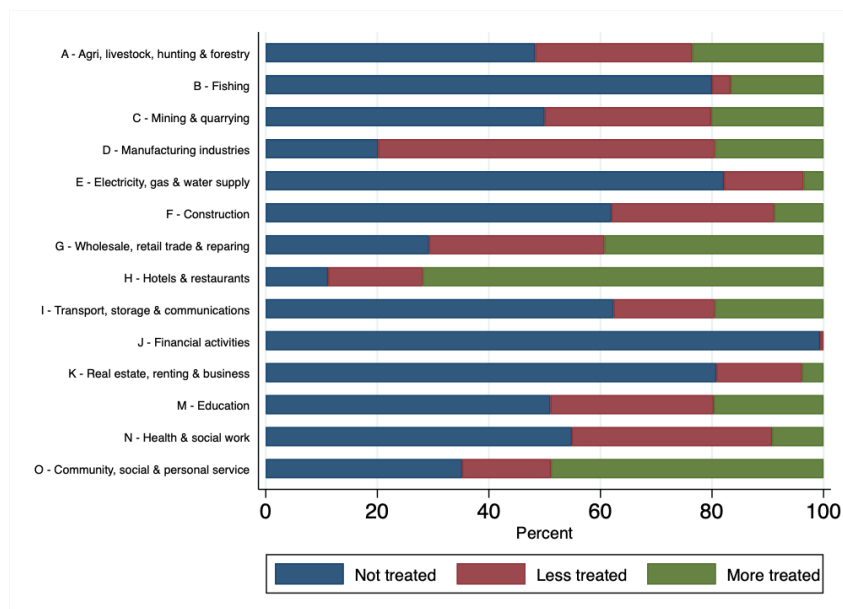
Note: The figure shows the distribution of actual hours worked in the survey as declared in the Portuguese Labor Force Survey.

Figure A4: Distribution of hours in EUKLEMS National Account



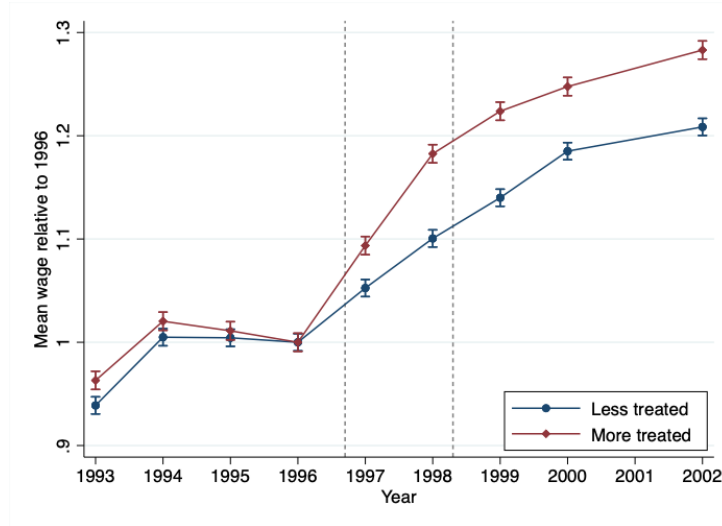
Note: The figure shows the distribution of actual hours worked in the survey as declared in the Portuguese Labor Force Survey.

Figure A5: Sectoral distribution of treatment intensity



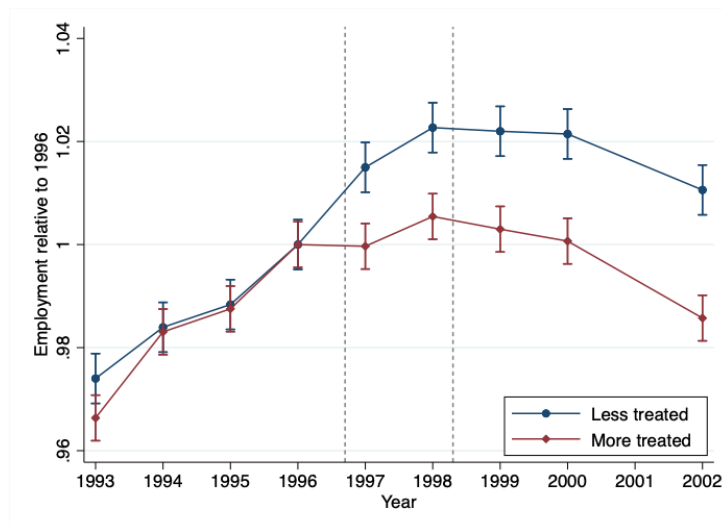
Note: The figure provides the share of establishments belonging each of the 3 treatment groups as defined in the Figure 5 for each 1-digit sector. More treated establishments are more represented in low-wage service sectors, while it is least represented in high-wage service sectors. In Appendix B, I defined more treated establishments within each sector so that my findings are not driven by the differences in sector compositions.

Figure A6: Evolution of mean labor cost (hourly wage) (1996 = 1) - 2 groups



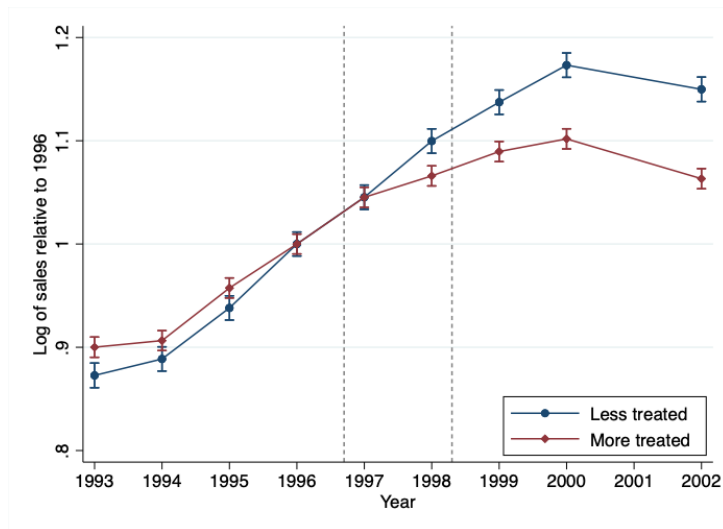
Note: The figure repeats the same as Figure 10 using two groups.

Figure A7: Evolution of employment (1996 = 1) - 2 groups



Note: The figure repeats the same as Figure 11 using two groups.

Figure A8: Evolution of Sales (1996 = 1) - 2 groups



Note: The figure repeats the same as Figure 12 using two groups.

Appendix B Robustness checks

In the main analysis, I use the hour share of treatment as defined by equation 1 to study the employment effects. To show that the estimated results are not specific to the measurement of treatment intensity, Table B1 uses alternative measures. The specification corresponds to column (3) of Figure 2, which adds the establishment-specific linear time trend to the diff-in-diff estimation specified in the equation 2.

Table B1: Robustness of employment effect (different treatment measures)

	(1)	(2)	(3)	(4)	(5)	(6)
	Emp	Emp	Emp	Emp	Emp	Emp
Hours share	-0.21*** (0.057)					
Share of treated workers		-0.0098** (0.005)				
Wagebill share			-0.21*** (0.057)			
Within-2d-sector (2groups)				-0.012*** (0.004)		
Within-3d-sector (2groups)					-0.012*** (0.004)	
Within-2d-sector (3groups)						-0.0063*** (0.002)
4-way fixed effect	Yes	Yes	Yes	Yes	Yes	Yes
Estab-trend	Yes	Yes	Yes	Yes	Yes	Yes
Observations	214,901	214,901	214,901	214,901	214,901	214,901
Cluster	Industry×year	Industry×year	Industry×year	Industry×year	Industry×year	Industry×year

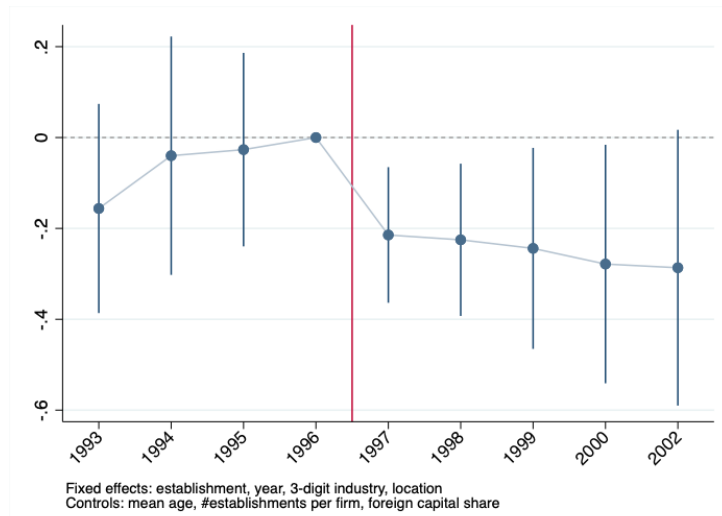
Note: The table provides robustness checks of the employment effect found in Table 2, by using alternative measures of treatment intensity. All regressions controls for firm-specific trends. Column (1) repeats the finding in Table 2. Column (2) uses share of treated workers, i.e., share of workers whose contractual hours in 1996 were above 40. Column (3) uses wage bill share of treated hours which essentially weights the hour share treatment with each worker's base wage rate. Column (4) to (6) exploit within-sector variation in treatment intensity to make sure that the employment effect estimate is not driven by having more treated workers represented in some sectors. In column (4), the treatment variable takes 1 for the establishments whose hour share of treatment is above within-2-digit-sector median, and 0 if below this sector-specific median. Column (5) repeats the same but with 3-digit sector. Finally, Column (6) splits into 3 groups rather than 2 groups within 2-digit sector. Standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

For each row, I use different measures of treatment intensity. Column (1) repeats the hour share of the treatment. Column (2) uses the share of treated workers per establishment, rendering the negative significant estimate. Column (3) uses the wage bill share, instead of the hour share, gaining an almost identical estimate (this is due to the presence of many establishments where all or none of their workers were treated, leading to a similar variation as the hour share of treatment). One concern for identification is that the employment effect is potentially confounded by differential trends or shocks specific to sectors (although they are by and large already controlled for by firm-specific trend or the interacted fixed effects shown in Table 2). To alleviate this concern, I construct treatment groups based on *within*-sector variation. In Column (4), I split establishments to two groups (i.e., less treated vs more treated) based on their hour share of treatment being below or above the median within each 2-digit sector. The result again shows a negative employment effect. Column (5) repeats the same procedure but using finer 3-digit sectors and obtains the similar estimate. Lastly, I split into 3 groups (rather than 2 groups) and again the result points to negative employment effects. Marginal effects (or elasticities) of hour reduction on employment calculated from these alternative measures are also very much comparable (not shown).

Figure B1 shows the time-varying coefficients estimated from the interaction of year dummies (which replace the post-intervention dummy in the previous specification in the equation 2) with $HourShareTreat_j$. The figure confirms no statistically significant differential trends. The negative employment effect kicks in 1997 and continues until the end of the sample period.

While not shown for the moment, the employment estimate is also robust to different

Figure B1: Coefficients from dynamic DID



Note: The figure shows the time-varying coefficients and confidence interval estimated by interacting time dummies with $HourShareTreat_j \times Post_t$ in the main specification expressed in the equation 2, by using 1996 as the reference year.

samples. Specifically, it is robust to: (1) extending sample years; (2) using unbalanced panel; (3) not excluding establishments with rapid job growth and contraction; (4) exclusion of construction sector (infrastructure demand from 1997) or financial sector (few treated establishments and affected largely by the Portugal's EURO adaption).