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ABSTRACT

The Employment Effects of Working Time Reductions: Sector-Level Evidence from European Reforms*

In this paper, we exploit a panel of industry-level data in European countries to study the economic impact of national reductions in usual weekly working hours between 1995 and 2007. Our identification strategy relies on the five national reforms that took place over this period and on initial differences across sectors in the share of workers exposed to the reforms. On average, the number of hours worked in more affected sectors fell, hourly wages rose, while employment did not increase. The effect on value-added per hour worked appears to be positive but non-significant.

JEL Classification: J20, J30, J80

Keywords: working time, work sharing, employment, wages, value-added

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1 Introduction

Over the last century, the total number of hours worked over the year has fallen dramatically in most OECD countries, as, along rising standard of living and higher demand for leisure, national legislation and/or collective agreements introduced caps to daily and weekly working hours, annual paid leave increased and part-time (and seasonal) work developed. However, the regulation of working time still displays notable differences across OECD countries (OECD, 2021) and it remains an issue of heated and recurrent debates.[1] In particular, its economic impact is still poorly understood.

Similarly to the controversies around the minimum wage, a first-order concern when it comes to working-time legislation is its impact on employment. Opinions differ regarding how reducing the number of hours usually worked through stricter legislation may impact the level of employment. The more optimistic view sees cuts in standard working hours not only as not harmful to employment but as potentially beneficial as work is redistributed among a larger group of people. This concept has traditionally been referred to as "work-sharing" (Dreze, 1986). However, previous theoretical and empirical works have provided little to no backing for this view, even if the results in the literature tend to vary significantly across reforms and level of analysis, which makes them not easily comparable.

In this paper, we provide new evidence on the impact of reductions in standard working time on employment by jointly analysing several reforms that took place in Europe between 1995 and 2007 under the umbrella of the European Union's Working Time Directive[2] and upon the impulse of the French debate on the 35 hours. In order to identify the causal effect of standard working time reductions, we rely on industry-level data and on a difference-in-difference approach that exploits the initial variation in the share of workers exposed to the reforms across sectors. This allows us to leverage variation across several reforms in similar contexts, over a short period, and to recover an average impact resulting from multiple legislative changes, net of national and sectoral trends. We

[1] The debate around the regulation of the working week has gained new force in response to the challenges brought by the global financial crisis and the COVID-19 pandemic as well as the impact of new technologies on the labour market.

[2] In 1993, the Council of the European Union issued a Directive which regulated various aspects of the working time regulation such as minimum rest periods, annual leave, night work, shift work and patterns of work. Most importantly, the EU set a limit to weekly working hours: according to Directive, the average working time for each seven-day period must not exceed 48 hours, including overtime. The Directive was later updated in 2000 and 2003 but the 48-hour weekly limited was confirmed. Depending on national legislation and/or collective agreements, the 48-hour average is calculated over a reference period of up to 4, 6 or 12 months.

find that, on average, more exposed sectors reduce the number of working hours relatively to less exposed sectors, but they do not symmetrically increase employment, such that the total number of hours worked falls. We estimate positive but insignificant effects on hourly wages and value-added per hour worked. Our results are robust to a large number of sensitivity analyses against alternative specifications, samples and estimators. In particular, our estimates are robust to the issues underlined in the presence of a staggered timing, dynamic and heterogeneous treatment effects, and results remain valid even when using the latest estimators proposed in this literature.

When looking at the previous literature in the field, it is important to carefully consider the level of analysis (worker, firm, or sectoral/regional), as this is strictly related to what the estimates can hope to recover: i) worker level studies can estimate only the effect on the separation rate, ii) firm level can recover the effect on labour demand of existing firms only (hirings and separations only, hence excluding effects on labour supply, and firm entry and exit), iii) while sectors/regions analysis may capture - under some assumptions - general equilibrium effects such as a positive effect on labour supply driven by an increase in hourly wages or the possibility to work shorter hours. Some earlier studies use worker-level data and find effects on the separation rate of affected workers that vary from a clear increase (Crépon and Kramarz, 2002), to null (Gonzaga et al., 2003; Sánchez, 2013), to a decrease for those directly affected (Raposo and Van Ours, 2010). Other studies use firm-level data to try capturing the total effect on labour demand (both on the separation and hiring rate), but also find very different results, ranging from a positive effect in the case of the French 35-hour reform (Crépon et al., 2004)[3], to a null effect in the case of the Portuguese reform (Varejao, 2005; Lopes and Tondini, 2022) and a negative effect in the case of the Japanese reform (Kawaguchi et al., 2017). Finally, a last set of studies investigates employment growth in sectors or regions more affected by reductions in working hours. Again, the range of estimates varies significantly: from a negative effect in Germany (Hunt, 1999)[4], to null in more affected regions in France (Chemin and Wasmer, 2009) and Canada (Skuterud, 2007), to a positive estimate in the case of Portugal when comparing more affected labour markets (sector×region) to the others (Raposo and van Ours, 2010).

[3]In this study, the authors argue that the positive effect on employment is the result of the decrease in hiring credits that accompany the reduction in standard working hours.

[4]Hunt (1999) analyses the impact of reductions in standard working hours as defined in industry-level collective agreements.

The contribution of this paper to this literature is threefold. First, going beyond the two specific European reforms analysed until now (France and Portugal), we jointly study all legislative changes in the duration of the working week that took place after the approval of the European Working Time Directive. By analysing several reforms in a relatively short time period (1995–2007), in countries with a similar legislative framework (the EU Working Time Directive) and relatively similar societal preferences, we are able to present an average effect, net of national and sectoral trends, and minimize the idiosyncrasies linked to specific national settings. Second, by also looking at the effect on employment, value-added and wages (which is not possible in studies using worker-level data), we go beyond most existing work and we try to uncover the possible channels of adjustment. Finally, by exploiting cross-country/cross-industry variation over time, we can more closely approximate the net effect on employment – potentially positive effects on labour supply and other general equilibrium effects which cannot be captured when comparing workers and/or firms –, while still controlling for country and industry specific effects. Overall, our view is that a sector-level analysis is the most appropriate approach to try to get a comprehensive picture of what happens to employment, as it allows not to have to rely exclusively on cross-country variation, while also hoping to realistically pick up more aggregate effects on employment that would not be possible in firm and worker-level studies. Generally, because of the limitations listed above, the literature has been largely inconclusive on the effects of shortening the working week on employment, and labour market outcomes more generally. With this wider approach (both in terms of countries/year covered and level of analysis), our aim is to bring additional empirical evidence to this debate.

The rest of this paper is organised as follows: Section 2 provides a short conceptual framework. Section 3 describes the data and the reforms. Section 4 presents the identification strategy, and Section 5 the results. Section 6 concludes.

2 Conceptual framework

The theoretical predictions of the employment effects of reductions in standard working hours are well understood in the literature. In a basic labour demand model with exogenous wages, where workers

and hours are perfect substitutes,[5] the concept of work-sharing finds a theoretical justification. Indeed, in this set-up, firms would simply substitute hours for workers to compensate for a decrease in the average hours, such that the total labour input stays constant, and employment increases. In a seminal paper in the literature, Calmfors and Hoel (1988) have shown that it is sufficient to add firms' endogenous overtime response and a fixed cost per worker to overturn these predictions. With a fixed cost per worker, reductions in working hours increase the labour cost, creating a negative scale effect on output and employment. Moreover, decreasing standard working hours decreases the relative price of overtime to workers, such that it may be optimal for the firm to have less workers working more intensively through overtime. In short, in the Calmfors and Hoel (1988) model, a positive employment effect will only emerge as a corner solution and with a fixed level of output, with many more scenarios giving an ambiguous or likely negative effect.

By assuming exogenous wages, these predictions rely on the assumption that nominal monthly salaries adjust to the shorter working week. However, the change in legislation might prescribe for the cut in hours to be salary-neutral, or nominal salaries might be rigid and hard to adjust. Previous country-specific studies have shown that nominal salaries almost never adjust (Crépon and Kramarz, 2002; Raposo and van Ours, 2010; Lopes and Tondini, 2022), with the exception of the Canadian reform (Skuterud, 2007). In a classical model of labour demand, an increase in the hourly wage as a result of the reduction in working hours exacerbates the negative scale effect, and leads to an unambiguously negative effect on the number of employed workers (Crépon and Kramarz, 2002). Moreover, the negative effect on labour demand could be mitigated by increases in hourly productivity: the magnitude of this effect would depend on the shape of relation between hours and output at the level of hours affected by the reform.

In general equilibrium, the negative effect on labour demand does not necessarily imply a net negative effect on employment. Indeed, predictions based only on the effect on labour demand ignore potentially positive labour supply responses, whereby individuals may be more willing to work at the lower hours level and the higher wage per hour. If labour supply increases as a response to the reform, even if wages increase, the net employment effect becomes again ambiguous. To a certain

[5]This type of model can be found in standard textbooks such as Hart and Sharot (1978), and Hamermesh (1993).

extent, this is a relative advantage of our empirical framework: by using sectors as the level of the analysis, we can hope to account for those aggregate effects that occur within sector (e.g, increase in labour supply towards one sector as people are more willing to work at lower hours), which cannot be identified when looking at firms or workers only. However, by comparing more and less affected sectors within the same country, our results are mute with respect to any general equilibrium effect that might occur economy-wide at the national level. This is an important caveat to keep in mind while interpreting our results.

More generally, predictions of the negative effect on labour demand rely on the assumptions of perfect competition. In a monopsonistic framework, where firms hold some market power, workers might work longer hours than what might be optimal in the absence of regulation (Boeri and Van Ours, 2021).[6] In the presence of monopsony power, a reduction in working hours associated to an increase in hourly wages would lead to an increase in employment (at least, temporarily).[7] Finally, if firms have other margins of adjustment to compensate for the higher labour cost – such as, for example, prices (Lopes and Tondini, 2022) or work intensification (Askenazy, 2004) –, then the employment effect remains ambiguous.

3 Data

3.1 Sector-level data

In order to identify the impact of reforms of standard working time exploiting variation across sectors, we need relatively granular and comparable data on the share of workers affected by the reforms, hours worked, employment, wages and value-added at the industry level.

[6]For instance, Marimon and Zilibotti (2000) show that, in the presence of search and matching frictions, (some degree of) regulation restricting working time benefits workers, both unemployed and employed, by strengthening their bargaining power given that the hours limit established in the law, is lower than that which would emerge from free negotiation.

[7]However, there are some differences with the minimum wage that make standard working time legislation a more limited tool to fight against monopsony than the minimum wage or collective bargaining: reductions in standard working time cannot be repeated indefinitely over time (in the case of working time regulation there is a lower bound at zero at the very least) and employers can restore the pre-reform monopsonistic equilibrium simply by not increasing (real) wages if the structural sources of monopsonistic power are not addressed. Also, differently from the minimum wage, there are no "aggregate demand"-type of general equilibrium effects that may explain the lack of employment losses as monthly/annual wages tend to stay the same.

Our main data source is from the EU KLEMS project (EU level analysis of capital (K), labour (L), energy (E), materials (M), and service (S) inputs). This initiative goes back to the late 1990s and was undertaken primarily to develop productivity measures at the industry level for the European Union (Van Ark and Jäger, 2017). The original EU KLEMS database, which was published in 2008, covers long-term series of output, input (including wages and hours worked) and productivity measures at the industry level, based on official national accounts supplemented by other secondary sources. The original data series ran up to 2005 and included 72 industries and 15 countries. Since then, it has been updated on several occasions and its last series (accessible at `https://euklems.eu/`) provides detailed data for all EU Member States and various country aggregates, Japan, the United Kingdom and the United States over the period 1995–2017 (though coverage differs across countries) and for 40 detailed industries.

EU KLEMS data are particularly well suited for industry-level analysis as the information is derived from national accounts, and are among the most reliable cross-country comparable sources for industry-level data. However, the set of worker-level information is limited and, therefore, we add the following variables based on an *ad hoc* extraction from the EU Labour Force Survey (LFS) by Eurostat and matched with KLEMS at the 2-digit level (NACE Revision 1.1)[8]: the share of workers working more than a certain threshold of hours of work (35, 38, 40), the share of women, workers below 30 and above 50, those with tenure longer than 24 months and low, mid, or high education, as well as the share of blue collar workers, part-time and workers on open-ended contracts. The first piece of information (i.e. the share of workers potentially impacted by the reforms) is key for our identification strategy, as it gives us the portion of workers affected in each sector. We use the three threshold set by the five reforms: France 35; Belgium 38; Italy, Slovenia, and Portugal 40 (see the next sub-section for a description of the reforms). From the Eurostat extraction, we obtain both the share of workers usually working above the threshold set by each reform, and also the share of workers actually working above the threshold. The first is the one that better reflects standard working hours set by the legislation, but we test the sensitivity of our estimates to both measures. The remaining variables provide important controls when running our estimations.

[8]Publicly available EU-LFS microdata only contains information on industry at the 1-digit level.

From our working sample, we drop agriculture, education, health & social work and arts & entertainment either because they usually have a high share of self-employed or of public-sector workers. By definition, these groups are either not (self-employed) or significantly less impacted (public sector) by reduction in standard hours, as they are usually governed by specific legislation or collective agreements. Furthermore, it is well known that for the public sector it is particularly complicated to measure value added.). In general, we exclude sectors where value-added and working hours are usually poorly measured such as mining, finance and real estate. We limit our sample to 2007 to avoid any overlap with the financial crisis and the subsequent recession as well as breaks in the industry classification in the KLEMS series.[9] The final sample, after matching with the EU LFS, consists of 23 countries and 32 industries between 1995 and 2007 for a total of 7,345 industry-country-year observations.[10]

3.2 Reforms of working time legislation

We collect the information on reforms of standard working time legislation using multiple sources. We start with the information included in the CBR Labour Regulation Index (Armour et al., 2016) and we complement and cross-check it with information available in the ILO Travail Database and the EU Commission LABour market REForm (LABREF) database (European Commission, 2021). In our analysis, we focus only on reforms of standard working hours[11] and, over the period of interest (1995–2007), we identify five reforms[12] of standard working hours in Europe (see Table 1 for a quick overview).[13]

[9] Also, there were no more reforms of standard working hours after 2007.

[10] The panel is unbalanced as not all countries are available in all years (Hungary and Slovenia are available only starting in 1996, the Czech republic and Estonia in 1997 and Latvia, Lithuania and Slovakia in 1998 while data for Portugal and Slovenia are available only up to 2006). Moreover, only about 20 industries are available for Luxembourg.

[11] The number of standard working hours determines the point at which overtime pay rates, i.e. higher pay rates, start being paid, and in some cases, where a different tax treatment applies.

[12] Six if we include also Poland which, however, is not used the analysis because of lack of LFS data (see below) in the relevant years.

[13] In a recent work, Rasmussen (2021) made available a dataset on working-time regulation for 197 territories between 1789 and 2021. In addition to the reforms of standard working hours identified in this paper, Rasmussen (2021) also lists reform events in the United Kingdom in 1998, in Greece in 2000, in Slovakia in 2001, in Denmark in 2004, in Hungary in 2004 and in the Netherlands in 2005. However, in the initial stages of our analysis, when the list of relevant reforms was assembled, a careful crosscheck suggested that these reforms are not reforms of statutory standard working hours in the sense that is pertinent to our analysis. In the United Kingdom, what Rasmussen (2021) identifies as a reform is the adoption of the EU Working Time Directive that led to the introduction of a maximum weekly hour limit of 48 hours but no limit to standard working hours. In Greece, a law in 2000 introduced more flexibility in

Table 1: Overview of the reforms of standard working hours in Europe, 1995–2007

Country	Adoption*	Implementation*	Change	Monthly wage	Compensations
Portugal	1996	1997-98	44h → 40h	=	none
Italy	1997	1998	48h → 40h	No specific adj.	none
France	1998	2000	39h → 35h	=	Lower SSC**
Belgium	2001	2002	40h → 38h	=	Lower SSC**
Slovenia	2002	2003	42h → 40h	=	none

**Adoption* refers to the year of adoption of the national legislation, while *Implementation* refers to the year in which the legislation was actually implemented.

** SSC stands for social security contributions, compulsory payments paid to general government that confer entitlement to receive a future social benefit (e.g. unemployment insurance, sickness benefits, pensions, etc.).

Note: An additional reform took place in Poland in 1997 reducing weekly working hours from 46 to 42. This reform, however, is not used the analysis because of lack of LFS data (see text) in the relevant years and therefore not included in the Table. A second reform in Poland took place in 2002 and brought the hours of work from 42 to 40.

The first reform in order of time took place in 1996 in Portugal[14] and it reduced standard weekly working time from 44 to 40 hours while keeping monthly wages constant – hence with an increase in hourly wages (Raposo and van Ours, 2010) – without any specific compensation for firms. On paper the reform affected more than 60% of the Portuguese employees (see Table OA5). However, not all workers had to adjust their hours: as Lopes and Tondini (2022) show, around half of the workforce was already at or below the new limit before the reform entered into force, due to the stricter constraints imposed by sectoral and regional collective agreements.

A second reform took place in Italy in 1997 as part of a more general labour market reform (the so-called "*Pacchetto Treu*"). The reform (law 196/1997) reduced the standard weekly working hours to 40 hours, down from 48. While very large on paper, the Italian reform essentially adapted the labour code to the provisions already foreseen by most collective agreements where standard working

working time management but made no change to standard working hours (European Commission, 2021). The reform in Slovakia in 2001 reflects a change in how breaks are considered: prior to the reform, weekly working hours were 42.5 hours, including breaks at work for food and rest. Upon request of the International Labour Organization, breaks were excluded from the count and standard working hours reduced to 40 without any reduction in actual working hours. The same happened in the Czech Republic. In the Netherlands and Denmark rules governing normal hours are not set by law but negotiated in sector-level agreements between unions and employers (OECD, 2021). Changes therefore happen gradually over time. Finally, the CBR Labour Regulation Index reports no reform of standard working time in Hungary in 2004. A 40-hour working week was already introduced in 1992, successive amendments to the Labour Code introduced some flexibility to allow derogations to work longer hours under certain conditions.

[14]We refer here to the year of adoption of the legislation. In the analysis, we will use the year of implementation.

hours were already well below 48 hours/week. Only 18% of Italian employees were affected by the reform (see Table OA5). The Italian reform did not foresee any specific adjustment to monthly wages nor any compensation for firms.

The French reform is arguably the most well-known in the public debate and in the literature. Following the election of a Socialist government in 1997, France cut standard working time from 39 to 35 hours with no change to the net monthly wages of workers who were employed at the time of the reform. In exchange, firms received a fairly generous reduction in social security contributions, targeted to low-skilled workers. The French law was passed in 1998 (*Loi Aubry I*) but, initially, it essentially worked through economic incentives and collective agreements between employers and unions. It was only in 2000 that the reduction in working time was uniformly enforced by law throughout the territory (*Loi Aubry II*).[15] Overall, 80% of French employees were affected by the reform.

Belgium reduced standard working hours to 38 in 2001 (*Loi relative à la conciliation entre l'emploi et la qualité de vie*), in a similar way, and with similar timing, to the French reform. Until December 2002, the reduction was voluntary and companies were free to determine the modalities (for example, either by effectively reducing standard working hours to 38 in any given week or averaging 38 hours per week over a determined reference period). In order to encourage employers to reduce working time, a one-off reduction in employers' social security contributions was granted. As of January 2003, all companies were mandated by law to reduce standard working hours to 38 hours with no compensation. Overall, 33% of Belgian employees were affected by the reform.

Finally, in 2002, Slovenia reduced standard working time from 42 hours/week to 40 (Employment Relations Act). The law did not specify anything with respect to wages, but the pay policy agreement for 2002–2003 ensured that workers did not get any cut in their wage (Banerjee et al., 2013). At the same time, companies did not receive any compensation or subsidy. Overall, 21% of Slovenian employees were affected by the reform.

As mentioned in the note below Table 1, an additional reform took place in 1997 in Poland but it

[15] Chemin and Wasmer (2009) show that the number of firms (and employees) that switched to the 35-hour regime was limited before 2000 while it jumped afterwards.

is not used in the analysis because of lack of LFS data in the relevant years.[16]

In the empirical analysis, we use the year of implementation, rather than the year of adoption of the law. As shown in Table 1, there is always a gap between the adoption of the law and its implementation. Alternatively, in a robustness check, we show the results when using the year of passing of the law instead.

4 Empirical Strategy

Given our empirical setting and data, the most direct way to identify the effect of reductions in standard working hours on the outcomes of interest would be to rely on the staggered implementation of reforms across countries. Under the standard common trend assumption, one could recover the effects of reductions in standard working hours by running the following estimation:

$$Y_{i,c,t} = \gamma_{i,c} + \beta Post_{c,t} + \theta_{i,t} + X'_{i,c,t} + u_{i,c,t} \tag{1}$$

where $Y_{i,c,t}$ is a selected outcome (e.g. total employment) in sector i, country c and year t; $\gamma_{i,c}$ are sector $\times$ country fixed effects, which take out the outcome average for every sector in every country; $\theta_{i,t}$ are sector $\times$ year fixed effects, hence controlling for the common evolution of outcomes across countries for a given sector in a given year; $X'_{i,c,t}$ is a vector of time-changing covariates at the country-sector level;[17] $u_{i,c,t}$ is the error term. As mentioned before, for this estimation to recover a consistent estimate of β, a common trend assumption would need to hold. In this setting, this would imply that a sector i in a country with a reform would have evolved in the same way as the same sector in countries without reforms in the absence of working-time reductions. We believe that this crucial assumption is unlikely to hold: countries, even if within the European Union, might be on

[16] In Poland, a reform reducing standard working time from 46 to 42 hours/week was introduced in 1997. The new working time standards did not result in a reduction of the remuneration paid to the employee as the law explicitly foresaw that every employee should get a remuneration not lower than the one received before. Moreover, companies did not receive any specific compensation for the increase in the hourly labour cost. However, one should note that these were years of strong economic growth in Poland and firms might have absorbed more easily the effect of the reform in a relatively short period.

[17] These include: share of self-employed, female, part-time, temporary contract, blue collar, share of high and low educated, and median age.

very different paths, in terms of growth for example, which would make it difficult for common trends to be verified. With this issue in mind, in our preferred specification we augment equation (1) in the following way:

$$Y_{i,c,t} = \gamma^*_{i,c} + \beta^* Treated_{i,c} \times Post_{c,t} + \theta^*_{c,t} + \theta^*_{i,t} + X^{'}_{i,c,t} + \varepsilon_{i,c,t} \quad (2)$$

where $Treated_{i,c}$ is a binary variable indicating whether a sector is above the median of the share of affected workers in the pre-reform years[18] interacted with $Post_{c,t}$, which, as in equation (1), indicates the staggered implementation of the reform across countries. Importantly, this second specification allows us to introduce $\theta^*_{c,t}$ in the regression, i.e. country×year fixed effects. By doing this, we exclude any country-year variation from the estimation and only exploit within-country variation over time. Our coefficient of interest, β^* is identified by the evolution of *more*-affected sectors relative to *less*-affected sectors in reforming countries at the moment of the reform. Identification relies on the weaker assumption that more and less affected sector *within the same country*, controlling for general time trends for each sector and time-varying controls at the country-sector level, would have evolved in the same way in the absence of standard working hours reductions.

We find the identification assumption of equation (2) to be more likely to hold than the one of equation (1), for two main reasons: i) this estimation does not rely on country×year variation, and hence is not subject to bias from country-specific shocks; moreover, ii) as we still allow for a general sector×year fixed effect, the estimation also controls for potentially diverging trends between sectors within country (for example, due to technology shocks). β^* is only identified by how much treated sectors in reforming countries diverge from their general sectoral trends at the moment of the reform and from control sectors within the same country.

There are two important caveats to point out about β^*: first, this coefficient is identified only through variation within reforming countries, hence non-reforming countries play a role only in the estimation of the set of sector×year fixed effects; second, contrary to equation (1), this coefficient only recovers a relative effect, i.e. we only identify the effect of more treated sectors relative to less treated sectors. This will only recover the total effect of the reform if *less*-exposed sectors are unaffected

[18] Affected workers are the workers usually working more hours than the threshold specified by the reform or the national legislation.

by the change in working hours legislation. This also has important implications for the "statistical power" of our estimation which will be determined by how much hours drop in more-affected sectors relative to less-affected sectors: the larger the relative drop, the more statistical power we will have to estimate the effect on our outcomes of interest, such as employment.

In our specification, we use a binary *post* variable (1 if after the reform, 0 if before) and not a measure of the magnitude of the reform, as the nominal decrease specified in the reform does not necessarily reflect its real size[19]. Focusing on reforms in a binary way allows us to circumvent this issue. In our preferred specification, we have opted for a binary *treated* variable (1 if above the median, 0 if below). However, this specification discards a significant amount of information and variation in the intensity of treatment. We made this choice for two reasons: i) setting up the specification this way makes the intuition of the underlying parallel trend assumption easier to understand and visualize. more importantly, ii) as shown in Callaway et al. (2021), the drawback of a diff-in-diff with a continuous treatment variables is that it requires stronger assumptions; namely, that the average change in outcomes over time across all units if they had been assigned a given amount of treatment is the same as the average change in outcomes over time for all units that experienced that amount. This assumption is stronger than the standard parallel trend hypothesis. Nonetheless, we also estimate a specification where we introduce a continuous measure of sectoral exposure to the reform (i.e. the pre-reform share of workers above the threshold) linearly into the regression. This also allows to recover a relative effect, leveraging the full variation in exposure to the reform, at the price of assuming a linear relation between the effect and the measure of exposure. We rewrite equation 2 as follows:

$$Y_{i,c,t} = \gamma^*_{i,c} + \beta^* Exposure_{i,c} \times Post_{c,t} + \theta^*_{c,t} + \theta^*_{i,t} + X'_{i,c,t} + \varepsilon_{i,c,t} \tag{2b}$$

where $Exposure_{i,c}$ indicates the share of workers above the reform level in each sector as defined by the distribution of work hours before the reform in each country. Descriptive statistics of the main variables by less and more exposed sectors (i.e. sectors where the share of workers above the reform

[19] At national level, the official "bite" of a reform may not reflect its actual one. For instance, as shown in Table 1, the 1997 reform in Italy, on paper, implied a reduction of 8 hours per week while the French one reduced standard working hours by 4 hours per week. Hence, on paper, the Italian reform was twice as large as the French one. However, in practice, the Italian reform adapted the labour code to the prevailing practices and it was not very binding while the French affected many more workers and companies. See Table OA5.

threshold is below/above the median in the pre-reform period) are reported in Table OA4. The share of workers used to identify less and more exposed sectors is shown in Table OA5, for countries with reforms only, as these are the relevant ones for the identification of β^*. Because specification 2 relies on weaker assumptions but discards a lot of information, while 2b relies on stronger assumptions but leveraging the full information, there is value in showing the results of both.

Finally, while the estimation of equation 1 might also suffer from the issues highlighted in the presence of heterogeneous and intertemporal treatment effects when the treatment implementation is staggered (Goodman-Bacon, 2021), this is not an issue in equation 2 and 2b, where the estimation only relies on within-country comparison (hence with common reform timing within country, not staggered). Indeed, in the robustness checks, we show that results produced by the recent estimator proposed by de Chaisemartin and d'Haultfoeuille (2020, 2022) are qualitatively similar to those obtained through OLS estimation of equation 2b (the marginal differences are attributable to the aggregation of each within-country coefficient between the two estimators).

5 Results

5.1 Main results

Table 2 reports the estimates of equations 2 and 2b on our outcomes of interest. Panel A of Table 2 shows the results for a discrete treatment variable, as in equation 2, while Panel B shows the results with a continuous measure of exposure, as defined in equation 2b. Both estimations are presented with and without controls extracted at the sectoral level from the EU-LFS.[20] In all our estimations, standard errors are clustered at the country×sector level and sectors are weighted by the within-country share of employment in the pre-reform period. This weighting procedure allows us to account for the size of the sector, while still giving each country the same weight; as employment is potentially impacted by the changes in legislation, it is key to define these weights only in the pre-reform years.

[20]The controls are the following: share of workers under 30, share of workers over 50, share of low- and high-educated workers, share of female workers, share of self-employed, share of permanent contracts, share of part-time contracts, share of workers with tenure above 24 months and share of blue-collar workers.

Table 2: Average Impact of Standard Hours Reductions, 1995–2007

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
		Log of:					
	Share$> x$	$\frac{\text{Hours}}{\text{Employment}}$	Hours	Employment	Value-added	$\frac{\text{Value-added}}{\text{Hours}}$	$\frac{\text{Compensation}}{\text{Hours}}$
Panel A: Discrete Treatment Variable							
Treated × Post	-4.863***	-0.014***	-0.040**	-0.026	-0.029	0.011	0.015
without controls	(1.369)	(0.004)	(0.018)	(0.017)	(0.022)	(0.023)	(0.012)
R^2	0.974	0.979	0.997	0.997	0.996	0.993	0.995
Observations	7,345	7,345	7,345	7,345	7,345	7,345	7,345
Treated × Post	-4.773***	-0.013***	-0.036**	-0.023	-0.025	0.012	0.018
with controls	(1.381)	(0.004)	(0.017)	(0.027)	(0.022)	(0.022)	(0.011)
R^2	0.974	0.979	0.997	0.997	0.996	0.991	0.995
Observations	7,345	7,345	7,345	7,345	7,345	7,345	7,345
Panel B: Continuous Exposure Variable							
Exposure × Post	-34.124***	-0.063***	-0.184**	-0.120	-0.019	0.165	0.071
without controls	(10.939)	(0.018)	(0.093)	(0.086)	(0.112)	(0.112)	(0.062)
R^2	0.968	0.974	0.996	0.9969	0.995	0.991	0.993
Observations	7345	7345	7345	7345	7345	7345	7345
Exposure × Post	-33.909***	-0.059***	-0.172**	-0.113	-0.003	0.169	0.066
with controls	(10.933)	(0.019)	(0.088)	(0.080)	(0.107)	(0.119)	(0.055)
R^2	0.975	0.979	0.997	0.998	0.996	0.993	0.995
Observations	7345	7,345	7,345	7,345	7,345	7,345	7,345

Note: *** p<0.01, ** p<0.05, * p<0.1. This table gives the estimates of Equation 2 and 2b on the share of workers above the threshold, and the log of average hours per worker, employment, valued added per hour and compensation per hour. Share > x (0–100) indicates the share of workers working more than the value specified by the existing legislation (countries w/o reform) or introduced by the reform (country w. reform). Sectors are weighted by the within-country share of employment in the pre-reform period. Standard errors are clustered at the country×sector level. Panel A gives the results of equation 2 with a discrete treatment variable. Panel B presents the results of equation 2b, hence with a continuous measure of initial exposure (the share of workers above the threshold). *To be read as:* Panel A, the effect of being in a sector above the median of exposed workers before the reform; Panel B, the effect of going from 0 to 100% of workers exposed to the reform. Controls included are at the 2-digit Nace Rev. 1.1 from an *ad-hoc* extraction by Eurostat, and include the following: *share of workers under 30, workers over 50, low- and high-educated workers, female, self-employed, permanent and part-time contracts, workers with tenure above 24 months and blue-collar.* Full tables for each specification in the Online Appendix (Tables OA2 and OA3).

Columns (1) and (2) show the impact of the reform on the share of workers working more than the value specified by the legislation before the reform and on the number of annual hours per employed person. These first two columns can be considered as a first stage of our analysis: importantly, the reforms appear to significantly reduce the number of workers working more than the new threshold introduced by the reform and the yearly number of hours worked on average by workers. The results of Column (1) are qualitatively identical whether we use the share *usually* working above the threshold, or *actually* working above the threshold. When looking at Panel A, the specification with the discrete treatment variable, we observe that reforms reduced the share of workers with standard weekly hours above the threshold by around 5 percentage points and the yearly hours worked per employed person by 1.3%, relative to sectors below the median. Instead, in Panel B, we present the results of equation 2b using directly the sectoral share of exposed workers before the reform. As stated before, this specification leverages the full variation in initial exposure to the reform across sectors. The coefficients here have to be interpreted as the relative effect of going from 0 to 100% of exposed workers: in sectors where all workers are affected by the reduction in hours, hours drop by 6 % relative to those sectors where all workers were already working less than the reform threshold, and the share above the threshold decreases by 33 percentage points. A more meaningful interpretation of these coefficients is to scale them from pre-reform mean of the less-exposed group to the one of the more-exposed group (14 pp. points difference). In this way, we obtain coefficients that are in line with the magnitude of Panel A, suggesting that the way we input exposure into our specification (2 or 2b, binary treatment vs. continuous linear exposure) is not driving our results.[21] As specification 2b leverages more variation but requires stronger assumptions, it is reassuring to see that the two approaches convey the same results.

The reduction of hours worked implied by our estimates is smaller than what one might expect given the nominal changes in hours of the reforms listed in Table 1. However, it is important to recall that our estimates only recover a relative effect of standard working time reform (i.e. the extra exposure of treated sectors relative to control), not an absolute one. Nevertheless, if one compares our estimates to the average difference between more exposed sectors and less exposed ones before

[21] Share>x -4.8*** ; Hours/emp -0.01***; Hours -0.02**; Emp -0.015; VA/hour 0.02; Comp/hour 0.01.

the reform, it can be seen that a decrease of log hours per head of about 0.013 corresponds to about two-thirds of the initial difference of 0.020 between the two types of sectors. Moreover, looking at the share of those working more than the post-reform threshold, a decrease of 5 percentage points corresponds to one third of the initial average 15 points difference.

Columns (3) and (4) present the results on the labour input, i.e. the total number of hours worked within each sector (=employment × average hours) and employment. Our main coefficient of interest, the one on employment, is negative and insignificant. We cannot reject that the coefficients in column (4) are statistically different from zero, but we can reject that they are statistically equal to 0.013 (Panel A) or 0.059 (Panel B), which would be the coefficients implied by a full-work sharing scenario, where the fall in average hours worked is entirely offset by an increase in employment. Even in the specification that leverages the full sectoral variation in exposure to the reform (Panel B), no work-sharing scenario emerges: more exposed sectors do not experience a relative increase in employment. Indeed, in column (3) we see that the labour input — the total number of hours worked within a sector — falls significantly, a result that goes against the work sharing scenario, which would have implied a substitution of hours for workers such that the labour input stays constant. In column (5), we see that the coefficient on output, measured as the value added in a given sector, is also negative and insignificant, which is consistent with the sign and proportions of the fall in employment and labour input and with the theoretical prediction of a negative scale effect.

As one can see from Table 2, the point estimates are practically unchanged when introducing our wide set of controls, which reassures us as to the absence of simultaneous shocks that would differentially affect more and less exposed sectors. Moreover, we see that the results of these estimations are qualitatively very different from the estimation of equation 1, presented in Table OA2, suggesting that indeed controlling for country×year variation drastically changes the results.

Columns (6) and (7) give instead the effects on value added per hour worked and hourly wages. None of the coefficients is statistically significant as the standard errors are relatively large. As discussed above, this may be the result of the low precision of our estimates for these outcomes and the demanding specification. However, it is interesting to note that the sign of the coefficient on wages is positive and similar in magnitude to that for hours worked, as one would expect given that

Table 3: Average Impact of Standard Hours Reductions, 1995–2007, Reforming countries only

		Log of:					
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Share$> x$	$\frac{\text{Hours}}{\text{Employment}}$	Hours	Employment	Value-added	$\frac{\text{Value-added}}{\text{Hours}}$	$\frac{\text{Compensation}}{\text{Hours}}$
Panel A: Discrete Treatment Variable							
$Treated \times Post$	-5.739***	-0.015***	-0.035**	-0.019	-0.014	0.020	0.012
	(1.364)	(0.004)	(0.016)	(0.015)	(0.021)	(0.020)	(0.011)
R^2	0.967	0.994	0.999	0.999	0.999	0.993	0.998
Observations	1,709	1,709	1,709	1,709	1,709	1,709	1,709
Panel B: Continuous Treatment Variable							
$Exposure \times Post$	-35.671***	-0.074***	-0.192**	-0.118	-0.029	0.162	0.019
	(10.628)	(0.019)	(0.081)	(0.072)	(0.114)	(0.110)	(0.064)
R^2	0.967	0.994	0.999	0.999	0.999	0.993	0.998
Observations	1,709	1,709	1,709	1,709	1,709	1,709	1,709

Note: *** p<0.01, ** p<0.05, * p<0.1. This table gives the estimates of Equation 2 and 2b on the share of workers above the threshold, and the log of average hours per worker, employment, valued added per hour and compensation per hour, estimated on the sample of reforming countries only (FRA, BEL, ITA, SVN, PRT). Share$> x$ indicates the share of workers working more than the value introduced by the reform. Sectors are weighted by the within-country share of employment in the pre-reform period. Panel A gives the results of equation 2 with a discrete treatment variable. Panel B presents the results of equation 2b, hence with a continuous measure of initial exposure (the share of workers above the threshold). Standard errors are clustered at the country×sector level. Controls included are at the 2-digit Nace Rev. 1.1 from an ad-hoc extraction by Eurostat, and include the following: *share of workers under 30, workers over 50, low- and high-educated workers, female, self-employed, permanent and part-time contracts, workers with tenure above 24 months and blue-collar.*

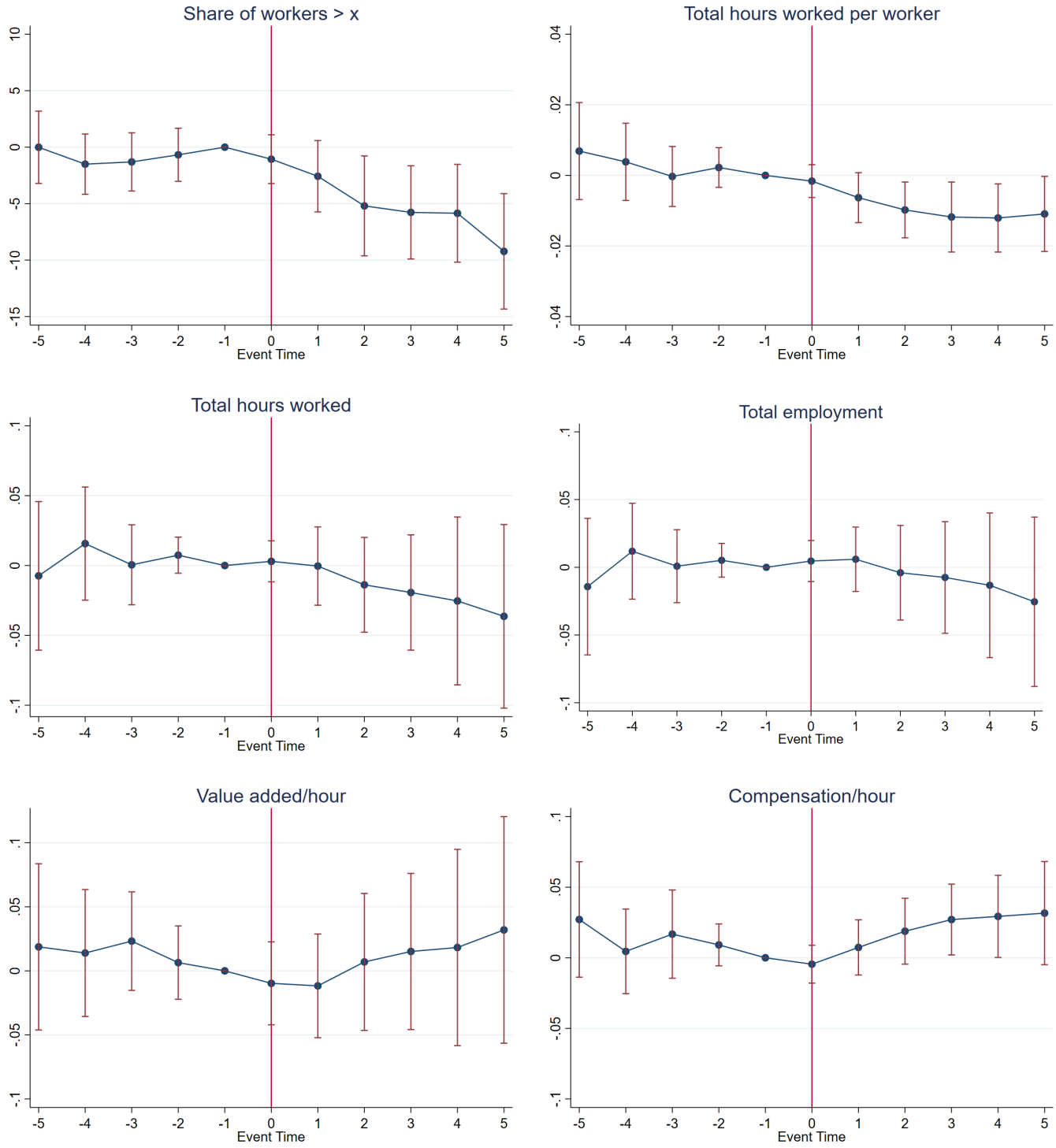
most reforms tried to preserve the purchasing power of workers and hence their monthly salaries, therefore resulting in an increase in the hourly rate. The coefficients for value-added is also of similar magnitude, but again very imprecisely estimated, both in the discrete and continuous specification. This result goes in the direction of a positive productivity effect, as one would expect with diminishing marginal returns (Pencavel, 2014), but again not statistically different from zero.

In Table 3, we show that the results do not vary if we run our estimation only on the sample of countries with a reform (i.e. Belgium, France, Italy, Slovenia and Portugal). This is reassuring, as non-reforming countries enter the estimation of our coefficient of interest only through the estimation of the set of sector×year fixed effects, hence the coefficient β^* is only identified by variation within reforming countries.[22]

All in all, taking our results at face value, we can conclude that reforms of standard working hours contributed to reducing working hours and the share of workers usually working above the threshold specified by the reform, but did not lead to more employment. Our estimates only allow us to recover a relative effect (the difference between more and less exposed sectors), and their power may be somewhat limited. However, when looking at the results, one cannot find any validation for the "work sharing theory" as there are no indications that reducing standard working time leads to a redistribution of work and an increase in total employment. Even in the specification with a continuous measure of exposure, where we leverage the full variation in the share of exposed workers across sectors, the coefficient on employment is negative and insignificant while the coefficient on labour input is negative and significant. As we discuss more in details in the conclusions, there can be several explanations for our results. The first is that between 1995 and 2007, European countries experienced relatively strong growth and the reduction of standard working time and the increase in labour cost per hour worked were quickly absorbed with no sizeable effect on employment. An alternative explanation is that the reductions in standard working time, in fact, offset the effects of the asymmetry in market power between employers and workers, very much like the increase in the minimum wage in a monopsony model. Our results do not allow to reject any of these two hypotheses.

[22]Also, the results are not affected when focusing only on reforms in Italy, Portugal and Slovenia and therefore excluding Belgium and France where the reforms of standard working hours were accompanied by a reduction in social security contributions (see Table 1) which may have mitigated the negative effect of an increase in hourly wages (results available upon request).

Figure 1: de Chaisemartin and d'Haultfoeuille (2020, 2022) Estimates over the Reform Window



Note: The figure plots the estimated coefficients and their 95% confidence using the estimator proposed by de Chaisemartin and d'Haultfoeuille (2020, 2022). To make this directly comparable to equation 2, we allow for a country non-parametric trend while controlling for sector×year fixed effects. The reference point (t=-1) is the year before the reform is passed (see Table 1). Note that the sample of countries is not balanced over the full period, as we do not observe all countries from t=-5 to t=5 over the period of the reform; all reforming countries are present over the period t=-1 to t=4.

5.2 Robustness Checks

We perform a number of sensitivity analyses to check the robustness of our main findings against alternative specifications, samples and estimators.

The key assumption in any difference-in-difference analysis is the "parallel trends" assumption, i.e. that there are no time-variant group specific unobservables correlated to the outcome of interest. Specifically, with the sets of sector and country×year fixed effects, our estimation relies on the assumption that, conditional on general sectoral and country trends, more-exposed sectors would have evolved in the same way as less-exposed sectors within the same country in the absence of the reforms. In Table 4 we show the results of a placebo test where we artificially set the reforming year at t-2 (except for Portugal, at t-1) and exclude post-reform years. By defining the timing of the reforms in this way, the coefficients are all insignificant and close to zero, which reassures as to the fact that the estimation we are proposing is not picking up effects where there should not be.

Table 4: Placebo test: Reform at t-2 (PRT: t-1) and post-reform years excluded

		Log of:					
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Share$> x$	$\frac{\text{Hours}}{\text{Employment}}$	Hours	Employment	Value-added	$\frac{\text{Value-added}}{\text{Hours}}$	$\frac{\text{Compensation}}{\text{Hours}}$
T×Post	-0.042	-0.004	-0.002	0.002	-0.022	-0.020	-0.015
	(0.900)	(0.003)	(0.013)	(0.012)	(0.019)	(0.019)	(0.009)
R^2	0.975	0.972	0.996	0.996	0.995	0.991	0.993
Observations	6308	6308	6308	6308	6308	6308	6308

Note: *** p<0.01, ** p<0.05, * p<0.1. This table gives the estimates of Equation 2 on the share of workers above the threshold, average hours per worker, employment, valued added per hour and compensation per hour. Reform years are anticipated by 2 years (1 in the case of PRT), and post-reform years are excluded. Share$> x$ indicates the share of workers working more than the value specified by the existing legislation (countries w/o reform) or introduced by the reform (country w. reform). Controls included are at the 2-digit Nace Rev. 1.1 from an ad-hoc extraction by Eurostat, and include the following: *share of workers under 30, workers over 50, low- and high-educated workers, female, self-employed, permanent and part-time contracts, workers with tenure above 24 months and blue-collar.*

As our dataset starts in 1995 and the first reform in our sample took place in Portugal in 1996 (but implemented in 1997), we cannot estimate longer lags using a balanced sample of countries to check whether sectors were on a similar pre-trend over a longer period. To estimate dynamic coefficients over a longer period, we make use of the recent estimator proposed in de Chaisemartin

and d'Haultfoeuille (2020) and de Chaisemartin and d'Haultfoeuille (2022). As briefly discussed in Section 4, the estimation of a two-way fixed effect specification may be biased in the presence of differential treatment timing, given that the final coefficient is a weighted average of all 2x2 DiD coefficients (Goodman-Bacon, 2021). Insofar there is substantial weight given to "late to early" units, the presence of differential timing and heterogeneous and dynamic effects may bias the results and even flip the sign (de Chaisemartin and d'Haultfoeuille, 2020). This is not the case of the specification proposed in equation 2, which, by focusing only on the within-country variation, does not have differential timing. Nonetheless, the estimator proposed in de Chaisemartin and d'Haultfoeuille (2020) and de Chaisemartin and d'Haultfoeuille (2022) is more apt to estimate dynamics effect, in particular over an unbalanced panel of country× sector.[23] We show the results of this estimator in Figure 1. To make this directly comparable with equation 2, we allow for a non-parametric country trend and control for the sector×year variation. Qualitatively, the results we obtain are in line with what presented before: we estimate a negative effect only on the share of workers above the threshold and the average hours per worker, a positive effect on wages, significant in some years, and a positive and insignificant effect on value added per hour. The effect on labour input and employment, as in the results presented in Table 2, is negative but insignificant, although very imprecisely estimated. Most importantly, Figure 1 reassures us further as to the absence of pre-trends in the pre-reform years.

In Table OA2 (Panel C), we re-estimate equation 2 using the year of adoption of the law rather than the year of the implementation. The results are qualitatively identical, with a slightly smaller first stage, which is not surprising, as the year of implementation should more closely capture when the adjustment (in hours) occurs. In addition, we test how sensitive the results are to changes in the regression weights given to each sector: we obtain similar results regardless of whether sectors are unweighted, or whether they are weighted by the share of total employment in the pre-reform period, defined across all countries and not within-country (results available upon request).

As a supplementary robustness check of the statistical significance of the results, we adapt the randomized inference method suggested by Bertrand et al. (2004). Specifically, we randomly assign sectors a value for the variable $Treated_{i,c}$ and re-run our main estimations 500 times. We then compare

[23]The panel of reforming countries is balanced over the period t=-1 to t=4.

the original estimate to the resulted distribution of pseudo estimates of the effect of standard working time reduction reforms on hours worked in Figure OA1. The estimated coefficient of Table 2 is a clear outlier in the distribution of coefficients generated by randomly assigning treatment status. Finally, we estimate equation 2 excluding one industry at the time, to ensure that a coefficient is not driven only by a specific sector. The results in Figure OA2 in the Online Appendix show that this is not the case.

6 Conclusion

We use a panel of industry-level data in European countries between 1995 and 2007 to evaluate the impact of national standard working time reductions on hours worked, employment, hourly wages and value-added per hour worked. For identification, we exploit the time variation introduced by five national reforms in France, Italy, Belgium, Portugal and Slovenia and the initial differences in the share of affected workers by sector. Our results show that more affected sectors experienced, as expected, larger reductions in working hours, but lower working hours did not translate into higher employment. Alongside, we find positive but insignificant effect on hourly wages and value added per hour worked. These results are robust to an extended set of robustness checks.

By jointly estimating the effect of several reforms and more closely approximating general equilibrium effects, these findings contribute to the ongoing debate on the employment effects of working time legislation. In particular, our estimates do not provide support for a "work-sharing" scenario, where lower hours are fully substituted by more workers. However, our results also do not support the view that reforms of standard working hours, which do not also entail a cut in monthly/weekly wages, have a significant negative effect on employment as a classical model of labour demand and supply would suggest.

On the one hand, the years in which the reforms took place were a period of relatively robust GDP growth as well as productivity and wage growth (although with a lot of heterogeneity across sectors/countries). It is therefore possible that, even in a classical setting of supply and demand, the reduction of working time and the increase in labour cost per hour worked were quickly absorbed. Our

results, however, may also suggest the presence of an asymmetry in market power between employers and workers whereby a reduction in standard working time with no change in compensation acted very much like the increase in the minimum wage in a monopsony model.

In conclusion, while this paper has essentially focused only on the employment effects of standard working time reforms, it is important to point out that other outcomes such as workers' well-being and productivity are increasingly part of the public debate on working time (OECD, 2022). If reforms of working time do not hurt workers either on wages or on employment while freeing up more leisure time, one could argue that a shorter working week or day leads to an increase in well-being. In turn, if there are diminishing marginal returns to longer hours and workers' well-being increases, a shorter working week or day could also benefit companies in terms of higher productivity and higher ability to attract and retain workers. On these important dimensions, the evidence available remains very limited. Investigating to what extent shorter working days or weeks can benefit workers' well-being and productivity without significant employment costs is a key empirical issue, that, similarly to the literature on minimum wages, will require more granular data and appropriate identification strategies.

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Online Appendix

for *The Employment Effects of Working Time Reductions: Sector-Level Evidence from European Reforms*

by C. Batut, A. Garnero and A. Tondini

Table OA1: Working Time Reduction and Employment: Overview of the Literature

Paper	Country/Year	Reform	Level of Analysis	Sign on Employment
Crépon and Kramarz (2002)	France - 1982	40h to 39h	Worker	Higher separations (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
Lopes and Tondini (2022)	Portugal - 1996	44h to 40h	Firm	Null
Hunt (1999)	Germany - '84–'95	Various	Sector	Negative
Skuterud (2007)	Canada - '97–'00	44h to 40h	Sector/Region	Null
Raposo and van Ours (2010)	Portugal - 1996	44h to 40h	Sector × Region	Positive
Chemin and Wasmer (2009)	France - 1998	39h to 35h	Region	Null

*This does not, by definition, imply that the total employment effect in 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 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.

Table OA2: Sector-Level Effects of Reductions in Standard Weekly Working Hours

		Log of:								
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	Share$> x$	$\frac{\text{Hours}}{\text{Employment}}$	Hours	Employment	Value-added	$\frac{\text{Value-added}}{\text{Employment}}$	$\frac{\text{Value-added}}{\text{Hours}}$	$\frac{\text{Compensation}}{\text{Employment}}$	$\frac{\text{Compensation}}{\text{Hours}}$	$\frac{\text{Compensation}}{\text{Value-added}}$
Panel A: Sector-Level Effects of Reductions in Length of the Working Week (Equation 1)										
T×Post	-9.546***	-0.007*	-0.042***	-0.035***	-0.074***	-0.039***	-0.032**	-0.033*	-0.025	0.006
	(1.796)	(0.004)	(0.013)	(0.012)	(0.016)	(0.015)	(0.015)	(0.019)	(0.018)	(0.018)
R^2	0.861	0.967	0.996	0.996	0.995	0.991	0.991	0.977	0.981	0.992
Observations	7345	7345	7345	7345	7345	7345	7345	7345	7345	7345
w. 2d NACE controls:										
T×Post	-9.842***	-0.005	-0.037***	-0.032***	-0.064***	-0.032**	-0.027*	-0.021	-0.016	0.011
	(1.585)	(0.004)	(0.012)	(0.011)	(0.015)	(0.015)	(0.014)	(0.020)	(0.018)	(0.018)
R^2	0.871	0.968	0.997	0.997	0.995	0.991	0.991	0.980	0.983	0.992
Observations	7345	7345	7345	7345	7345	7345	7345	7345	7345	7345
Panel B: Full set of results (without controls) (Equation 2)										
T×Post	-4.863***	-0.014***	-0.040**	-0.026	-0.029	-0.003	0.011	0.001	0.015	0.004
	(1.369)	(0.004)	(0.018)	(0.017)	(0.022)	(0.023)	(0.023)	(0.011)	(0.012)	(0.023)
R^2	0.974	0.979	0.997	0.997	0.996	0.993	0.993	0.994	0.995	0.995
Observations	7345	7345	7345	7345	7345	7345	7345	7345	7345	7345

Table OA2: Sector-Level Effects of Reductions in Standard Weekly Working Hours (continued)

		Log of:								
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	Share$> x$	$\frac{\text{Hours}}{\text{Employment}}$	Hours	Employment	Value-added	$\frac{\text{Value-added}}{\text{Employment}}$	$\frac{\text{Value-added}}{\text{Hours}}$	$\frac{\text{Compensation}}{\text{Employment}}$	$\frac{\text{Compensation}}{\text{Hours}}$	$\frac{\text{Compensation}}{\text{Value-added}}$
Panel C: Full set of results — Year of Adoption of the Law instead of Implementation (Equation 2)										
T×Post	-3.899***	-0.014***	-0.040**	-0.026	-0.033	-0.008	0.006	-0.004	0.010	0.004
	(1.245)	(0.004)	(0.017)	(0.016)	(0.023)	(0.023)	(0.024)	(0.010)	(0.010)	(0.022)
R^2	0.974	0.979	0.997	0.998	0.996	0.993	0.993	0.994	0.995	0.995
Observations	7345	7345	7345	7345	7345	7345	7345	7345	7345	7345
Panel D: Full set of results — Continuous Exposure Variable (Equation 2b)										
T×Post	-33.909***	-0.059***	-0.172**	-0.113	-0.003	0.110	0.169	0.007	0.066	-0.103
	(10.933)	(0.019)	(0.088)	(0.080)	(0.107)	(0.115)	(0.119)	(0.057)	(0.055)	(0.129)
R^2	0.975	0.979	0.997	0.998	0.996	0.993	0.993	0.994	0.995	0.995
Observations	7345	7345	7345	7345	7345	7345	7345	7345	7345	7345

Note: *** p<0.01, ** p<0.05, * p<0.1. This table presents the results of equations (1) and (2) on outcomes at the sectoral level on several specifications. Panel A presents the results of the estimation of equation (1). Controls in the bottom panel A include the following: % part-time, female, self-employment, temporary contract, median age, blue collar, education at the 2-digit NACE Rev. 1.1 level from 1995 to 2007. Sectors are weighted by the within-country share of employment in the pre-reform period. Standard errors are clustered at the country×sector level. Panel B presents the results of equation (2) on outcomes at the sectoral level. Panel C presents the results of equation (2) on outcomes at the sectoral level using the year of adoption of the law instead of the year of implementation from Table 1. Panel D presents the results of equation 2b on outcomes at the sectoral level.

Table OA3: Full set of results (with controls)

		Log of:								
	Share> x	$\frac{\text{Hours}}{\text{Employment}}$	Hours	Employment	Value-added	$\frac{\text{Value-added}}{\text{Employment}}$	$\frac{\text{Value-added}}{\text{Hours}}$	$\frac{\text{Compensation}}{\text{Employment}}$	$\frac{\text{Compensation}}{\text{Hours}}$	$\frac{\text{Compensation}}{\text{Value-added}}$
T x Post	-4.773***	-0.013***	-0.036**	-0.023	-0.025	-0.002	0.012	0.004	0.018	0.006
	(1.381)	(0.004)	(0.017)	(0.017)	(0.022)	(0.022)	(0.022)	(0.010)	(0.011)	(0.022)
% self-employed	-5.945	0.047	-0.370**	-0.418**	-0.796***	-0.378*	-0.426**	-0.912***	-0.959***	-0.534**
	(5.980)	(0.038)	(0.183)	(0.191)	(0.251)	(0.214)	(0.215)	(0.162)	(0.157)	(0.225)
% men	5.257***	-0.004	-0.005	-0.001	0.074	0.075	0.079	-0.008	-0.004	-0.083*
	(1.377)	(0.008)	(0.035)	(0.034)	(0.048)	(0.053)	(0.053)	(0.030)	(0.031)	(0.047)
% primary education	-2.709*	0.005	0.090**	0.084**	0.144***	0.060	0.054	-0.008	-0.013	-0.068
	(1.577)	(0.012)	(0.038)	(0.037)	(0.047)	(0.046)	(0.045)	(0.029)	(0.031)	(0.048)
% tertiary education	0.912	-0.018**	-0.044	-0.025	-0.040	-0.015	0.003	0.023	0.041	0.038
	(1.636)	(0.008)	(0.037)	(0.038)	(0.066)	(0.061)	(0.059)	(0.040)	(0.038)	(0.057)
Tenure > 24 months	-2.638	-0.020	-0.025	-0.005	-0.003	0.002	0.022	0.037	0.057	0.036
	(1.869)	(0.015)	(0.051)	(0.049)	(0.072)	(0.073)	(0.071)	(0.052)	(0.050)	(0.062)
% permanent	-7.883**	-0.006	-0.004	0.002	0.140	0.138	0.144*	0.114**	0.120**	-0.024
	(3.168)	(0.015)	(0.068)	(0.066)	(0.088)	(0.084)	(0.082)	(0.058)	(0.057)	(0.082)
% young	-0.297	-0.014	0.180***	0.193***	0.060	-0.133**	-0.119*	0.014	0.028	0.147**
	(1.477)	(0.011)	(0.056)	(0.055)	(0.059)	(0.060)	(0.061)	(0.033)	(0.036)	(0.064)
% old	0.579	-0.002	-0.083*	-0.081	-0.101*	-0.020	-0.018	-0.017	-0.014	0.003
	(1.900)	(0.009)	(0.049)	(0.049)	(0.060)	(0.068)	(0.067)	(0.043)	(0.043)	(0.061)
% full-time	18.219***	0.088***	0.205**	0.117	0.072	-0.045	-0.132	0.090	0.003	0.135
	(3.577)	(0.021)	(0.084)	(0.078)	(0.103)	(0.090)	(0.090)	(0.061)	(0.061)	(0.104)
% blue collar	1.424	0.011	0.065*	0.053	0.001	-0.052	-0.063	0.015	0.004	0.067
	(1.355)	(0.008)	(0.037)	(0.036)	(0.045)	(0.047)	(0.046)	(0.028)	(0.027)	(0.046)
Constant	11.858***	7.425***	12.030***	4.605***	8.532***	3.927***	-3.498***	9.555***	9.037***	-1.280***
	(4.436)	(0.023)	(0.115)	(0.110)	(0.125)	(0.124)	(0.124)	(0.078)	(0.077)	(0.140)
R^2	0.974	0.979	0.997	0.997	0.996	0.991	0.991	0.993	0.995	0.993
Observations	7345	7345	7345	7345	7345	7345	7345	7345	7345	7345

Note: *** p<0.01, ** p<0.05, * p<0.1. This table presents the results of equation (2) on outcomes at the sectoral level, including the full list of coefficients of the control variables. Sectors are weighted by the within-country share of employment in the pre-reform period. Standard errors are clustered at the country×sector level.

Table OA4: Descriptive statistics by Discrete Treatment Status

	Less exposed sectors		*More exposed sectors*	
	Mean	Std. Dev.	Mean	Std. Dev.
Share >x	14.11	18.41	28.79	26.08
Hours/employee (log)	7.48	0.13	7.50	0.13
Total nb of employees (log)	3.46	1.75	3.94	1.82
Value-Added/hour (log)	-3.46	1.43	-3.17	1.33
Compensation/hour (log)	9.19	0.85	9.30	0.92
Share of self-employed	0.09	0.09	0.11	0.12
Share of women	0.35	0.23	0.29	0.19
Share of low educated	0.32	0.22	0.28	0.21
Share of high educated	0.16	0.15	0.15	0.13
Tenure >24 months	0.78	0.12	0.76	0.12
Share of permanent contract	0.92	0.09	0.92	0.08
Share of <30 year old	0.26	0.13	0.27	0.12
Share of 50+ year old	0.18	0.11	0.18	0.09
Share of part-time	0.07	0.11	0.07	0.08
Share of blue collar	0.61	0.24	0.53	0.25

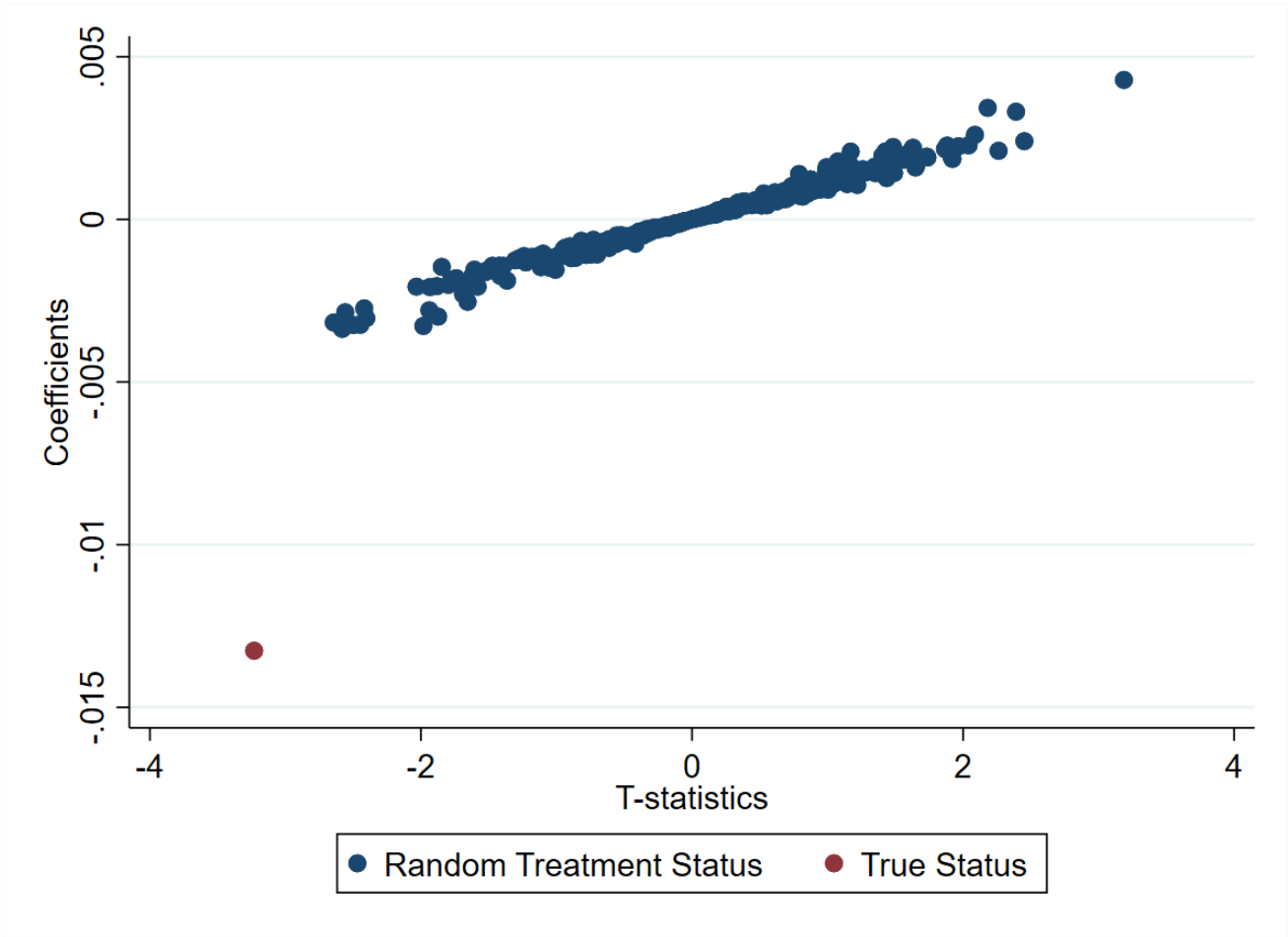
Note: There are 4047 less exposed country-sector-year observations and 3,298 more exposed country-sector-year observations. *Share*> x indicates the share of workers working more than the value specified by the existing legislation (countries w/o reform) or introduced by the reform (country w. reform). *More exposed sectors*: sectors where the share of workers above the reform threshold is above the median in the pre-reform period.

Table OA5: Share of Workers Above the Threshold Introduced by the Reform in Pre-Reform Years, by Country and Sector

Sector (Isic Rev. 3)	BEL	FRA	ITA	PRT	SVN
15t16 - Food, beverages and tobacco	23.62	80.94	**22.07**	57.61	20.93
17t19 - Textiles, wearing apparel, leather and related prodcuts	**33.61**	**90.65**	8.81	58.85	14.51
18 - Manufacture of wearing apparel; dressing and dyeing of fur	28.16	**84.72**	9.39	**83.94**	11.37
19 - Tanning and dressing of leather; manufacture of luggage, handbags, saddlery, harness and footwear	**35.07**	**86.32**	9.14	**88.61**	**26.47**
20 - Manufacture of wood and of products of wood and cork, except furniture; manufacture of articles of straw and plaiting materials	**45.73**	**89.53**	16.23	**79.24**	19.09
21 - Manufacture of paper and paper products	17.41	85.73	8.10	50.46	22.06
22 - Publishing, printing and reproduction of recorded media	17.75	76.79	14.05	46.07	**19.77**
23t25 - Chemical, rubber, plastic and fuel	**35.02**	**86.86**	11.39	34.90	14.58
26 - Manufacture of other non-metallic mineral products	**29.79**	**88.26**	14.85	59.49	12.87
27 - Manufacture of basic metals	31.62	**87.56**	12.24	**70.77**	17.00
28 - Manufacture of fabricated metal products, except machinery and equipment	**34.36**	**91.78**	10.18	**69.03**	13.68
29 - Manufacture of machinery and equipment n.e.c.	**26.13**	**88.69**	10.78	54.61	12.58
30 - Manufacture of office, accounting and computing machinery	**34.28**	76.42	12.66	0.00	28.08
31 - Manufacture of electrical machinery and apparatus n.e.c	**27.93**	**87.59**	9.73	44.56	8.62
32 - Manufacture of radio, television and communication equipment and apparatus	**25.31**	**87.02**	6.82	**64.86**	12.43
33 - Manufacture of medical, precision and optical instruments, watches and clocks	**27.20**	**82.45**	9.13	**76.49**	17.56
34 - Manufacture of motor vehicles, trailers and semi-trailers	**38.01**	**94.08**	8.97	45.08	17.41
35 - Manufacture of other transport equipment	29.60	**91.71**	5.77	32.70	17.11
36t37 - Manufacture of furniture and recycling	**29.22**	**88.10**	15.81	**76.36**	10.82
45 - Construction	**54.99**	**91.07**	**21.25**	**70.91**	**29.37**
50 - Sale, maintenance and repair of motor vehicles and motorcycles; retail sale of automotive fuel	36.20	**85.18**	**28.74**	59.17	**31.12**
62 - Air transport	25.18	64.83	8.62	12.72	24.40
51 - Wholesale trade and commission trade, except of motor vehicles and motorcycles, except of motor vehicles and motorcycles	**32.60**	78.34	**20.59**	46.47	**19.81**
52 - Retail trade, except of motor vehicles and motorcycles; repair of personal and household goods	24.03	66.31	**29.06**	61.29	**27.65**
55 - Hotels and restaurants	32.86	63.92	**40.50**	**76.57**	**38.02**
60 - Inland transport	**35.61**	73.78	18.47	58.19	**31.70**
61 - Water transport	**27.42**	56.14	**52.44**	58.08	**72.14**
63 - Supporting and auxiliary transport activities; activities of travel agencies	24.44	81.96	**21.47**	43.96	**25.40**
TOTAL	33.20	80.53	18.37	63.33	21.28

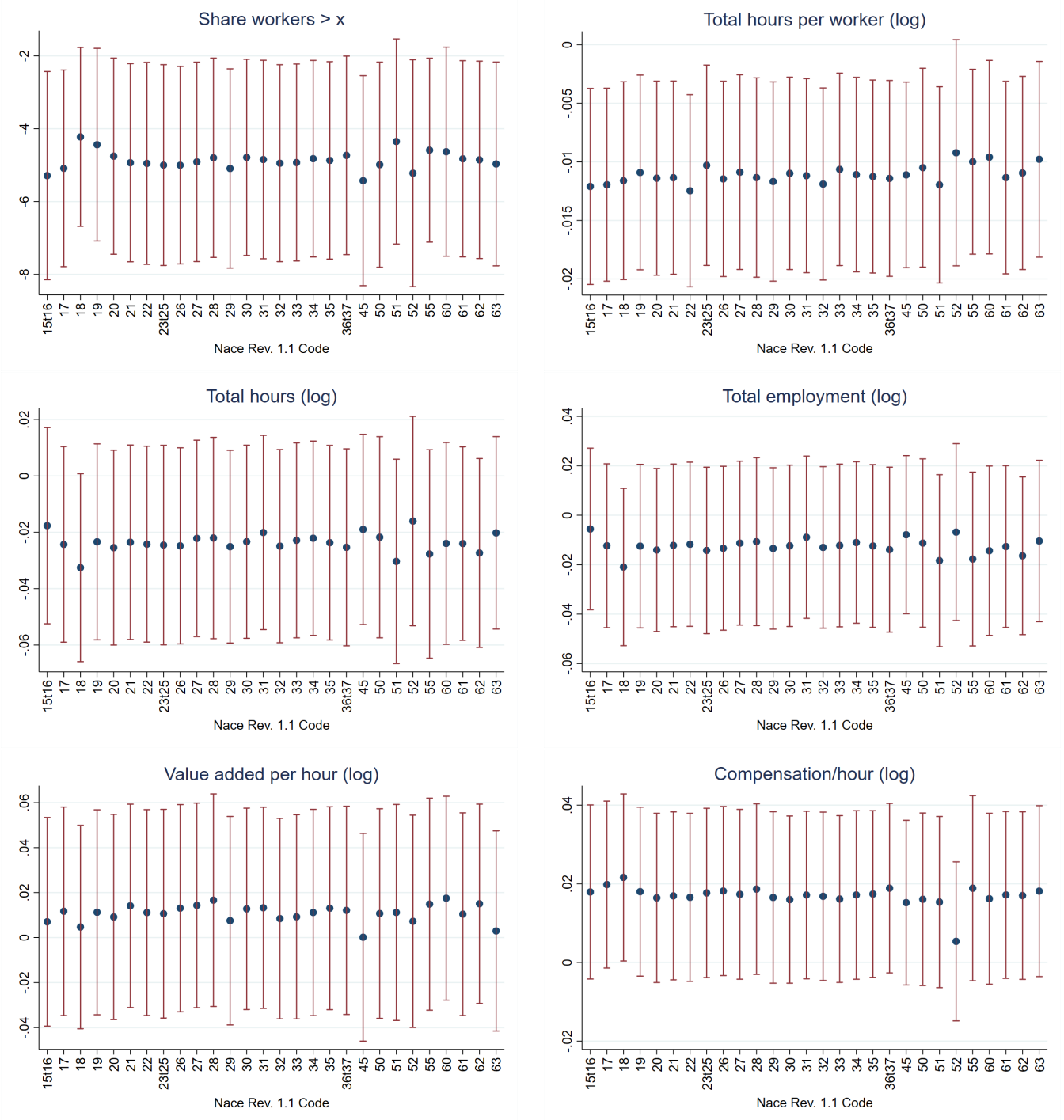
Note: In ***bold*** the *more exposed sectors*, i.e. the sectors where the share of workers above the reform threshold is above the median (weighted by employment) in the pre-reform period.

Figure OA1: Randomization Inference: Distribution of Pseudo Estimates of the Effect on Hours Worked



Note: the blue dots show the distribution of estimates of equation 2 when we randomly assigned sectors a value for the variable $Treated_{i,c}$ in multiple draws. The red dot marks for comparison our chosen estimate (with the true value of $Treated_{i,c}$ by sector). Controls included are at the 2-digit Nace Rev. 1.1 from an ad-hoc extraction by Eurostat, and include the following: *share of workers under 30, workers over 50, low- and high-educated workers, female, self-employed, permanent and part-time contracts, workers with tenure above 24 months and blue-collar.*

Figure OA2: Robustness test to varying the industry sample



Note: The figure show the evolution of the coefficients from equation 2 for the set of outcomes when each given NACE Rev. 1.1 2-digit sector is dropped from the sample.