The Employment Effects of Working Time Reductions in Europe*

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Abstract

PRELIMINARY DRAFT

Working time legislation is a key labour market regulation and the subject of heated and recurrent debates. A first-order concern is how this legislation may impact employment. 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 five national reforms that took place over this period and on initial differences across sectors in the share of workers exposed to the reforms. We show that, on average, the number of hours worked in more affected sectors fell relative to less affected sectors but employment did not increase, while the impact on wages and productivity appears to be positive but insignificant.

JEL codes: J20, J30, J80
Keywords: working time, work sharing, employment, productivity

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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 national legislations 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 whose economic impacts are still poorly understood.[1]

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 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 and are not easily comparable between each other.[2]

[1]The debate on shorter working hours 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. During the COVID-19 pandemic, the idea of a four-day working week was flagged by the Prime Minister of New Zealand Jacinda Ardern as well as by the Finnish Prime Minister Sanna Marin and the junior party in the Spanish government coalition. In 2021, the majority party in Japan recommended the adoption of the four-day week, in particular to help people requiring time off to acquire new skills. The private sector too has started experimenting shorter working weeks: in 2019, Microsoft tested a four-day working week in Japan and, allegedly, reported a 40 % increase in productivity. In 2020, Unilever, a consumer good company, also announced the intention to test a four-day working week in New Zealand.

[2]When looking at the previous works in the field, in fact, 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 (hirings and separations), iii) while sectors/regions analysis may capture - under some assumptions - general equilibrium effects like a positive effect on labour supply. 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 studies use firm-level data to try capturing the total effect on labor 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],

In this paper, we try to provide a contribution to this debate by reassessing the impact of working time reductions on employment by jointly analysing several reforms that took place in Europe over a relative short period of time (1995–2007), partly upon the impulse of the European Union's Working Time Directive[5], and, partly, driven by the resonance of the French debate on the 35 hours which led to a landmark reform in 2000.[6] In order to identify the causal effect of 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 compare similar reforms in similar contexts, over a short period, while also being able to recover an average impact resulting from several legislative changes. We find that, on average, more exposed sectors reduce the number of working hours by 6% 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 wages and productivity.

The contribution of this paper to this literature is threefold. First, by lumping several reforms together 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 and minimize the idiosyncrasies linked to specific national reforms. Second, by jointly analyzing

to a null effect in the case of the Portuguese reform (Varejao, 2005; Tondini and Lopes, 2021) 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 labor markets (sector×region) to the others (Raposo and van Ours, 2010).

[5]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.

[6]The idea of a 35-hour week, down from 40 hours per week, was already part of the "110 proposals for France" by François Mitterrand in 1981. But it was legislated only in the late Nineties. The French debate had an influence in many other European countries. In Italy, for instance, the centre-left coalition government led by Romano Prodi risked to fall in 1997 over a row between coalition partners on the introduction of the 35 hours.

the effect on employment, output 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 are able to capture the net effect on employment – potentially positive effects on labour supply and other general equilibrium effects cannot be captured when comparing workers and/or firms –, while still controlling for country and industry specific effects.

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 working hours reductions are well understood in the literature. In a basic labour demand model with exogenous wages, where workers and hours are perfect substitutes,[7] 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 usual 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.

[7] This type of model can be found in standard textbooks such as Hart and Sharot (1978), and Hamermesh (1996).

By assuming exogenous wages, these predictions rely on 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 the nominal salary 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; Tondini and Lopes, 2021), 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).

However, in general equilibrium, the negative effect on labour demand does not necessarily imply a net negative effect on employment. Indeed, predictions on labour demand by firm 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 extent, this is one of the key advantages of our empirical framework: by using sectors as the level of the analysis, we can hope to account for these general equilibrium effects that cannot be identified when looking at firms or workers only.

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, 2013). In this case, a reduction in working hours associated to an increase in hourly wages would lead to an increase in employment (at least, temporarily).

Finally, if firms have other margins of adjustment to compensate for the higher labour cost – such as, for example, prices or work intensification (Askenazy, 2004) –, then the employment effect remains ambiguous.

3 Data

3.1 Sector-level Data

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 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 on `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 derived from the EU Labour Force Survey (LFS) and matched with KLEMS at the 2-digit level (NACE Revision 2)[8]: the share of workers working more than a certain threshold of hours (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 we detail later. The remaining variables provide important controls when running our estimations.

[8]The data from the EU LFS are based on a *ad hoc* extraction by Eurostat since that publicly available EU-LFS microdata only contains information on industry at the 1-digit level.

From our working sample, we drop agriculture, mining, education, health & social work and arts & entertainment. Moreover, we exclude sectors where value-added and working hours are usually poorly measured such as finance and real estate. We limit our sample to 2007 to avoid any overlap with the financial crisis and the subsequent recession. 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.[9]

3.2 Reforms of working time legislation

We collect the information on reforms of working time legislation using multiple sources. We start with the information included in the CBR Labour Regulation Index (Adams et al., 2010) and we complement and cross-check it with information available in the ILO Travail Database and the EU Commission LABour market REForm (LABREF) database. In our analysis, we focus only on reforms of usual working hours, coded as a binary variable (1 in the year when the reform enters into force) to avoid some inevitable measurement error if we were to use the exact provisions. As we detail below, the nominal decrease specified in the reform does not necessarily reflect its real size: focusing on reforms in a binary way allows us to circumvent this issue.

Over the period of interest, we identify six reforms of usual working hours in Europe (see Table 1 for a quick overview).

The first reform in order of time took place in 1996 in Portugal[10] and it reduced 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. Not all workers had to adjust their hours: as

[9]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.

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

Table 1: Overview of working time reforms in Europe, 1995–2007

Country	Year	Implementation	Change	Monthly wage	Compensations
Portugal	1996	1997-98	44h → 40h	=	none
Italy	1997	1998	48h → 40h	No specific adj.	none
*(Poland**	*1997*		*46h → 42h*	=	*none)*
France	1998	2000	39h → 35h	=	Decrease in SSC
Belgium	2001	2002	40h → 38h	=	Decrease in SSC
Slovenia	2002	2003	42h → 40h	=	none

* Not used in the analysis because of lack of LFS data (see text) in the relevant years. Note also that a second reform in Poland took place in 2002 and brought the hours of work from 42 to 40. *Year* refers to the year of adoption of the national legislation, while *Implementation* refers to the year in which the legislation was actually implemented.

Tondini and Lopes (2021) 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 usual 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 usual working hours were already well below 48 hours/week. 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 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*).[11]

In Poland, a reform reducing working time from 46 to 42 hours/week was introduced in 2001 (Labour Code Amendment 1 March 2001). 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.[12]

Belgium reduced usual 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 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 working hours to 38 hours with no compensation.

Finally, in 2002, Slovenia reduced 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.

In the empirical analysis, we always use the year of implementation, rather than the year of adoption of the law. As shown in Table 1, there can be a significant gap between the law and its implementation. In a robustness check, we show that the results do not change significantly when using the year of passing of the law instead.

[11] 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.

[12] 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.

4 Empirical Strategy

Given our empirical setting and data, the most direct way to identify the effect of reductions in 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 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;[13] $u_{i,c,t}$ 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 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:

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

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

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[14] 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 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 by the change in working hours legislation. This also has important implications for the "statistical power" of our estimation: it will be determined by how much hours

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

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.

In our preferred specification, we opt for a binary *treated* variable (1 if above the median, 0 if below), for several reasons: first, this makes the intuition of the underlying parallel trend assumption easier to understand and visualize. Indeed, this allows for more intuitive graphs where we can plot the relative evolution of the more and less exposed sectors. However, we also test for an alternative 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* indicates the share of workers above the reform level in each sector. Descriptive statistics of the main variables by less and more exposed sectors (i.e. sectors where the share of workers above the reform threshold is below/above the median in the pre-reform period) are reported in Table A3. The share of workers used to identify less and more exposed sectors is shown in Table A4, for countries with reforms only, as these are the relevant ones for the identification of β.

Finally, as shown by de Chaisemartin and d'Haultfoeuille (2020, 2021), the estimation of equations 2 and 2b may suffer from the issues highlighted in the presence of heterogeneous and intertemporal treatment effects (Goodman-Bacon, 2021). This is not an issue in our estimation, as no negative weights arise. However, in the robustness checks, we also present the results with the estimator proposed by de Chaisemartin and d'Haultfoeuille (2020, 2021), and show that they are qualitatively similar.

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.[15] 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.

[15] I.e., 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)
		Log of:				
	Share>x	Hours/emp	Hours	Emp	VA/hour	Comp/hour
Panel A: Discrete Treatment Variable						
Treated × Post	-4.863***	-0.014***	-0.040**	-0.026	0.011	0.015
without controls	(1.369)	(0.004)	(0.018)	(0.017)	(0.023)	(0.012)
Treated × Post	-4.773***	-0.013***	-0.036**	-0.023	0.012	0.18
with controls	(1.381)	(0.004)	(0.017)	(0.027)	(0.022)	(0.011)
Observations	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	0.165	0.071
without controls	(10.939)	(0.018)	(0.093)	(0.086)	(0.112)	(0.062)
Exposure × Post	-33.909***	-0.059***	-0.172**	-0.113	0.169	0.066
with controls	(10.933)	(0.019)	(0.088)	(0.080)	(0.119)	(0.055)
Observations	7,345	7,345	7,345	7,345	7,345	7,345

Note: 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 Appendix (Tables A5, A6, A8).

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. When looking at Panel A, the specification with the discrete treatment variable, we observe that reforms reduced the share of workers with usual weekly hours above the threshold by around 5 percentage points and the yearly hours worked per employed person by 1.4%, relative to sector 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. The reduction of hours worked implied by our estimates is smaller that what one might expect given the nominal changes in hours of the reforms listed in Table 1. A relatively small effect on hours worked is not surprising given that in many sectors, even before the reform, collective agreements at sectoral or firm-level already fixed lower working hours than those established in the law. Moreover, as mentioned before, the coefficients of equation 2 and 2b only recover a relative effect (i.e. the extra exposure of treated sectors relative to control). Finally, firms may have compensated lower usual hours via higher overtime.[16] This could explain why the yearly hours do not drop as much as expected. Columns (3) and (4) present the results on employment and the labor input, i.e. the total number of hours worked within each sector (=employment $\times$ average hours). Our main coefficient of interest, the one on employment, is negative and insignificant. We cannot reject

[16]This potentially perverse mechanism was already underlined by Calmfors and Hoel (1988), where cut in usual hours might actually result in fewer workers working longer hours through overtime.

that this coefficient is statistically different from zero, but we can reject that it is statistically equal to 0.014, which would be the coefficient implied by a full-work sharing scenario, where the fall in average hours worked is entirely offset by an 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 labor input stays constant. In the discrete specification of panel A, we observe a relative drop of around 4% for sectors above the median, while when using the continuous exposure variable, we see that having all of the workers exposed relative to none implies a drop in the labour input of about 17%. Again, even in this specification that leverages the full initial, sectoral variation in exposure to the reform, no work-sharing scenario emerges: more exposed sectors do not experience a relative increase in employment. 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 "naive" estimation of equation 1, presented in Table A2, suggesting that indeed controlling for country$\times$year variation drastically changes the results.

Columns (5) and (6) give instead the effects on productivity (value added per hour worked) and 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 power of our estimates due to the relatively small effect of working time reforms on hours worked themselves. 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 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 productivity 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.

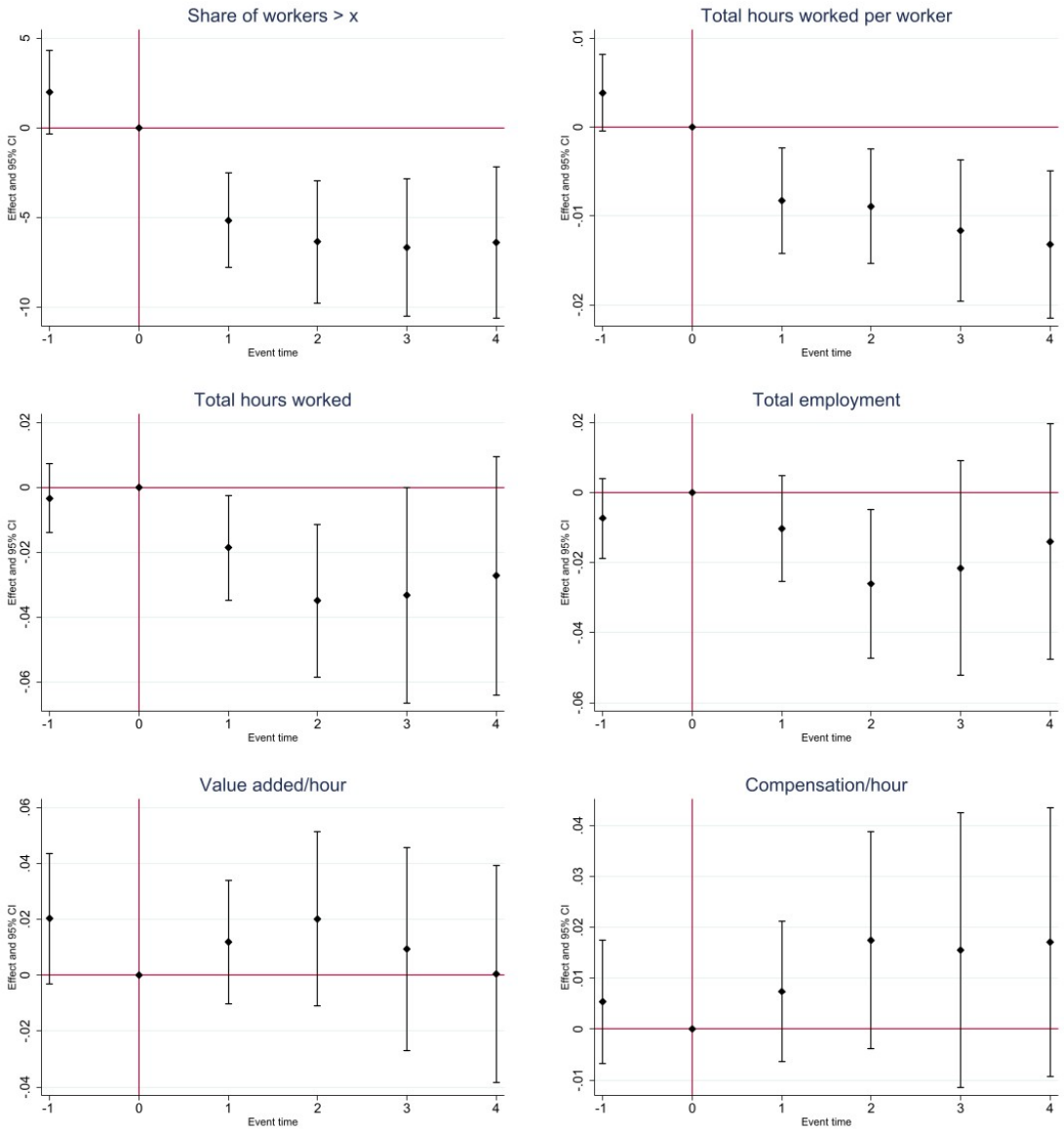
Table 3: Average Impact of Standard Hours Reductions, 1995–2007, Reforming countries only

		Log of:				
	(1)	(2)	(3)	(4)	(5)	(6)
	Share$> x$	Hours/Emp	Hours	Emp	VA/Hour	Comp/Hour
$Treated \times Post$	-4.753***	-0.015***	-0.036**	-0.011	0.009	0.016
	(1.136)	(0.004)	(0.016)	(0.015)	(0.021)	(0.011)
Observations	1,709	1,709	1,709	1,709	1,709	1,709

Note: This table gives the estimates of Equation 2 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. 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.*

All in all, taking our results at face value, we can conclude that reforms of usual 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 is somewhat limited by the relatively small bite of the reforms. However, when looking at the results, one cannot find any validation for the "work sharing theory" as there are no indications that reducing working time leads to a redistribution of work and an increase in

Figure 1: Graphical Representations of the Main Results (Discrete Treatment)



Note: The figure plots the estimated coefficients β_k and their 95% confidence intervals obtained by estimating this variant of equation 2: $Y_{i,c,t} = \gamma^*_{i,c} + \sum_{k=-1}^{4} \beta^*_k Treated_{i,c} \times \mathbb{1}\{t = k\} + \theta^*_{c,t} + \theta^*_{i,t} + X'_{i,c,t} + \varepsilon_{i,c,t}$. The vertical red line indicates the timing of the reform. *Share of workers* $> x$ indicates the share of workers with hours above the threshold specified by the reform.

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, and the coefficient on labor input is negative and significant. Instead, the signs of the estimated coefficients are compatible with a classical model of labour demand and supply where, in the absence of sufficient productivity increases, a reduction in working time with no proportional cut in weekly/monthly wages increases the labour cost and, therefore, leads to decrease in employment.

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. Figure 1 plots the estimated coefficient over the window around the reforms, and their 95% confidence intervals. As one can see in Figure 1, the pre-treatment coefficients are not significantly different from zero. Unfortunately, given that our dataset starts in 1995 and the first reform in our sample took place in Portugal in 1996 (implemented in 1997), we cannot show more pre-treatment coefficients on a balanced sample.[17] 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). By defining the timing of the reforms in this way, the coefficients are all quite close to zero.

Moreover, in Table A7, we show the results when using the year of adoption of

[17]The reference coefficient for 1996 set at 0 relative to the one in 1995, all the others are post-treatment.

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 better capture when the adjustment (in hours) occurs.

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

		Log of:				
	(1)	(2)	(3)	(4)	(5)	(6)
	Share$> x$	Hours/Emp	Hours	Emp	VA/Hour	Comp/Hour
T×Post	-1.102	-0.001	0.004	0.006	-0.023	-0.011
	(0.744)	(0.003)	(0.013)	(0.012)	(0.019)	(0.010)
Obs	6,308	6,308	6,308	6,308	6,308	6,308

Note: 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 briefly discussed in Section 4, the estimation of a difference-in-difference with differential timing with two-way fixed effects (TWFE) may be biased since the final coefficient is a weighted average of all 2x2 DiD coefficients, and groups at the middle of the panel weight more than those at the beginning or the end (Goodman-Bacon, 2021). Therefore, the results can change by simply adding or substracting years to the panel. Moreover, by using all possible 2x2 combinations, a TWFE estimation will inevitably use past treated units as controls for future treated units ("late to early 2x2"). Insofar there is substantial weight given to "late to early" units, the presence of differential timing and heterogeneous effects may bias the results and even flip the sign (de Chaisemartin and d'Haultfoeuille, 2020). To test the robustness of our results, we re-run the analysis using the estimator proposed by de Chaisemartin and d'Haultfoeuille (2020), and de Chaisemartin and d'Haultfoeuille (2021). To make this comparable with sets of fixed effects of equation 2, we allow for a non-parametric country trend, hence only comparing within country, after taking out

the sector×year variation. In Figure 2, we show that our results are robust to this alternative estimator. The qualitative conclusions are similar: we estimate a negative and clear effect on the share of workers above the threshold, a negative effect on the average hours worked, and no other significant effect. Importantly, even with this estimator, the coefficient on employment is close to zero. With this estimator, we can also relax the restriction to estimate the effect only over the window for which there is balanced sample (-1 to +4). In Figure A4 in the Appendix, we show the results with a window using all the pre- and post-periods available (even if not all countries are at the extremes of the windows). This allows us to observe more clearly the dynamics of the effect, and, importantly, test the pre-trends over many more years. Qualitatively, the results are very similar: we estimate a negative effect only on the share of workers above the threshold, and the average hours per worker, a positive but insignificant effect on wages, and a zero effect on employment.

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 assigned sectors a value for the variable $Treated_{i,c}$ and re-run our main estimations 500 times. We then compare the original estimate to the resulted distribution of pseudo estimates of the effect of working time reduction reforms on hours worked in Figure A1. The estimated coefficient of Table 2 is a clear outlier in the distribution of coefficients generated by randomly assigning treatment status.

Finally, we replicate the baseline results excluding one country and one industry at the time, to ensure that the relationship is not driven by a specific sector or country. The results in Figures A2 and A3 in the Appendix show that this is not the case.

6 Conclusion

In this paper, we use a panel of industry-level data in European countries between 1995 and 2007 to evaluate the impact of national working time reductions on hours worked, employment and productivity. 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 wages or productivity. These results are robust to an extended set of robustness checks.

By jointly estimating the effect of several reforms and allowing for 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 the "work-sharing" scenario, where lower hours are fully substituted by more workers. The results are rather in line with a more standard "neo-classical" model, where higher labor costs translate into lower employment: if anything, our estimates point to a (relatively mild) negative scale effect on output and employment.

In conclusion, while the results in this paper do not provide any evidence for the argument that shorter working weeks or days would also boost employment, it is important to point out that workers' well-being and productivity considerations are increasingly part of the debate on working time. Investigating to what extent shorter working hours (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 specific identification strategies.

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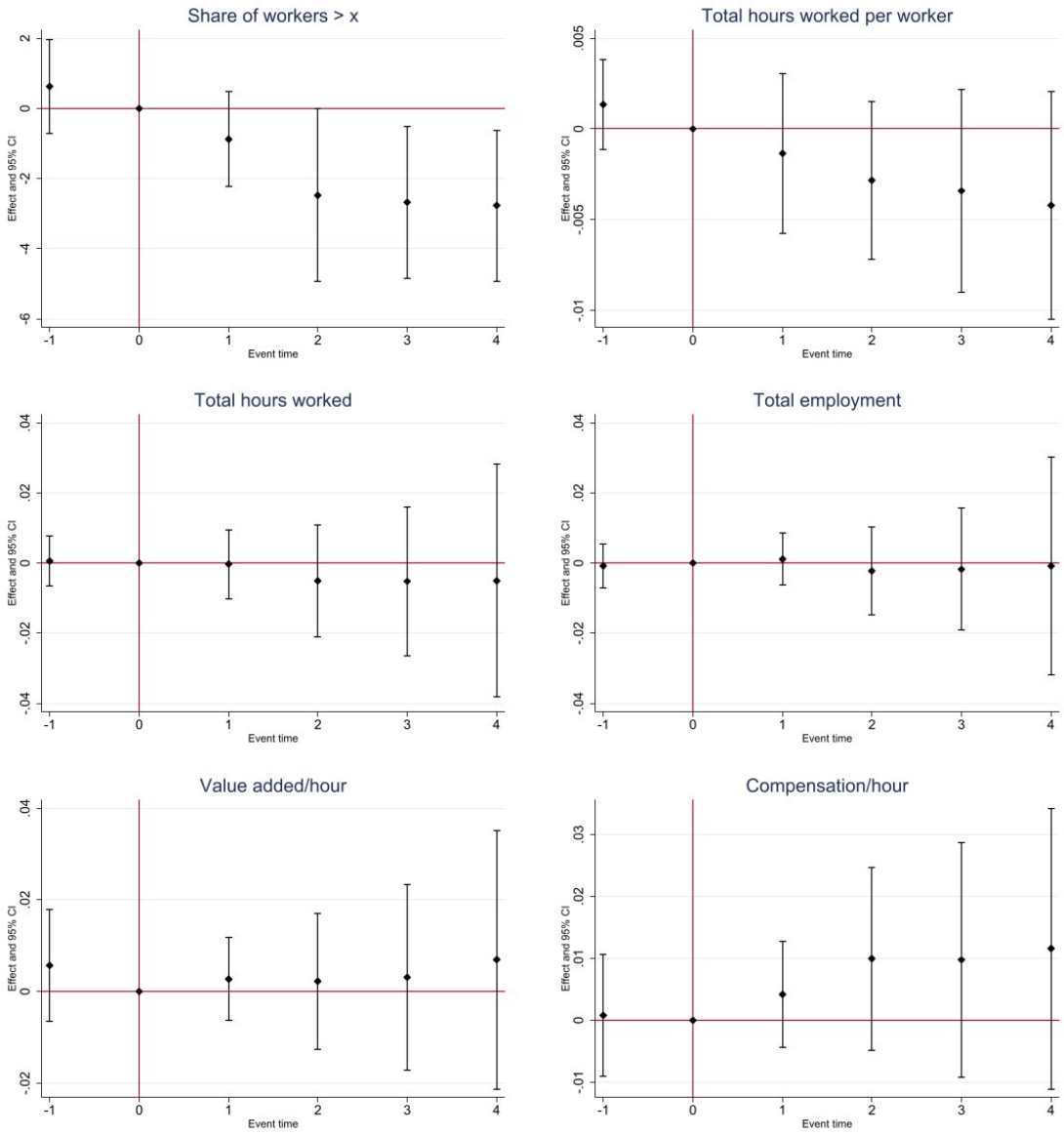
Figure 2: Results using de Chaisemartin and d'Haultfoeuille (2020) and de Chaisemartin and d'Haultfoeuille (2021)



Note: The figure plots the estimated coefficients and their 95% confidence using the estimator proposed by de Chaisemartin and d'Haultfoeuille (2020, 2021). To make this directly comparable to equation 1, we allow for a country non-parametric trend, after taking out the sector×yearvarition.

Appendix

Table A1: 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 39	Worker	Higher separation (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
Tondini and Lopes (2021)	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 35	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 A2: Sector-Level Effects of Reductions in Length of the Working Week ("Naive" Specification)

		Log of:								
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	Share$> x$	Hours/Emp	Hours	Emp	VA	VA/Emp	VA/Hours	Comp/Emp	Comp/Hours	Comp/VA
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)
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)
Obs	7,345	7,345	7,345	7,345	7,345	7,345	7,345	7,345	7,345	7,345

Note: This table presents the results of equation (1) on outcomes at the sectoral level. Controls in the bottom panel 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.

Table A3: Descriptive statistics

	Less exposed sectors			*More exposed sectors*		
	Obs	Mean	Std. Dev.	Obs	Mean	Std. Dev.
Share >x	4,047	14.11	18.41	3,298	28.79	26.08
Hours/employee (log)	4,047	7.48	0.13	3,298	7.50	0.13
Total nb of employees (lo	4,047	3.46	1.75	3,298	3.94	1.82
Value-Added/hour (log)	4,047	-3.46	1.43	3,298	-3.17	1.33
Compensation/hour (log)	4,047	9.19	0.85	3,298	9.30	0.92
Share of self-employed	4,047	0.09	0.09	3,298	0.11	0.12
Share of women	4,047	0.35	0.23	3,298	0.29	0.19
Share of low educated	4,047	0.32	0.22	3,298	0.28	0.21
Share of high educated	4,047	0.16	0.15	3,298	0.15	0.13
Tenure >24 months	4,047	0.78	0.12	3,298	0.76	0.12
Share of permanent contract	4,047	0.92	0.09	3,298	0.92	0.08
Share of <30 year old	4,047	0.26	0.13	3,298	0.27	0.12
Share of 50+ year old	4,047	0.18	0.11	3,298	0.18	0.09
Share of part-time	4,047	0.07	0.11	3,298	0.07	0.08
Share of blue collar	4,047	0.61	0.24	3,298	0.53	0.25

Note: *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 A4: Share of workers working more than the threshold introduced by the reform

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**

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.

Table A5: Full set of results (without controls)

		Log of:								
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	Share$> x$	Hours/Emp	Hours	Emp	VA	VA/Emp	VA/Hours	Comp/Emp	Comp/Hours	Comp/VA
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)
Obs	7,345	7,345	7,345	7,345	7,345	7,345	7,345	7,345	7,345	7,345
R-squared	0.974	0.979	0.997	0.997	0.996	0.993	0.993	0.994	0.995	0.995

Note: This table presents the results of equation (2) on outcomes at the sectoral level. 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 A6: Full set of results (with controls)

		Log of:								
	Share$> x$	Hours/emp	Hours	Emp	VA	VA/emp	VA/hour	Comp/emp	Comp/hour	Comp/VA
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-squared	0.97	0.97	1.00	1.00	1.00	0.99	0.99	0.99	0.99	0.99
Observations	7345	7345	7345	7345	7345	7345	7345	7345	7345	7345

Note: This table presents the results of equation (2) on outcomes at the sectoral level. 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 A7: Full set of results — Year of Adoption of the Law instead of Implementation

		Log of:								
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	Share$> x$	Hours/Emp	Hours	Emp	VA	VA/Emp	VA/Hours	Comp/Emp	Comp/Hours	Comp/VA
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)
Obs	7,345	7,345	7,345	7,345	7,345	7,345	7,345	7,345	7,345	7,345
R-squared	0.974	0.979	0.997	0.998	0.996	0.993	0.993	0.994	0.995	0.995

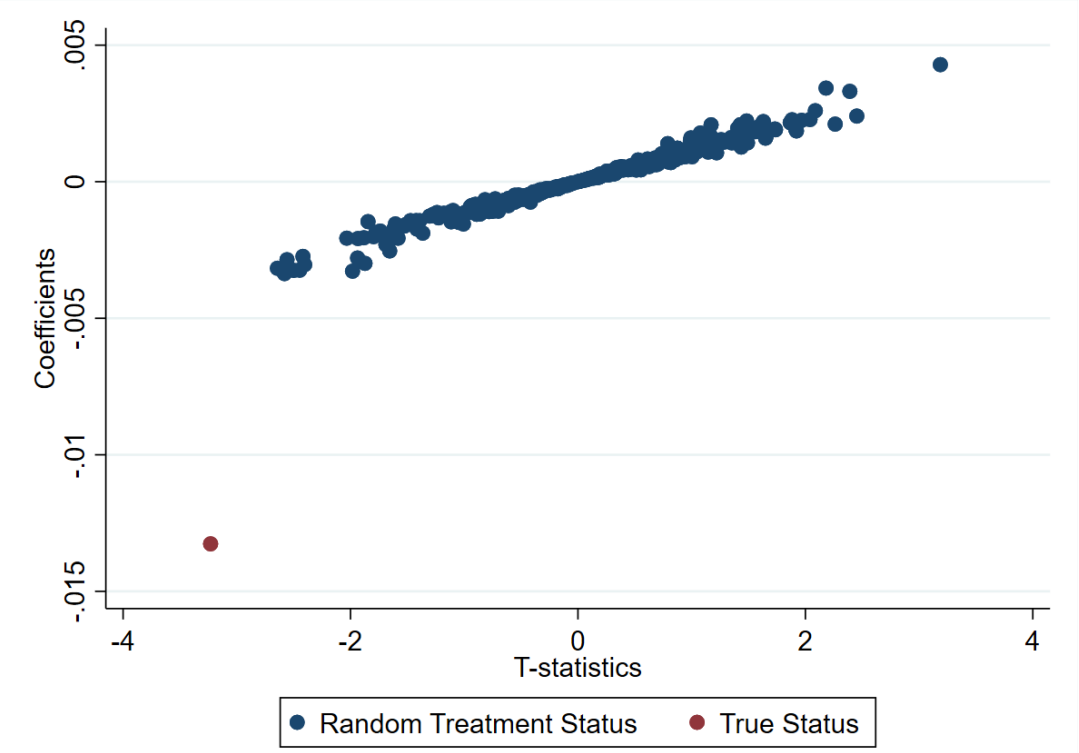
Note: This table presents the results of equation (2) on outcomes at the sectoral level. From Table 1, we use the year of adoption of the law instead of the year of implementation. Sectors are weighted by the within-country share of employment in the pre-reform period. Standard errors are clustered at the country*sector level.

Table A8: Full set of results — Continuous Exposure Variable

		Log of:								
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	Share$> x$	Hours/Emp	Hours	Emp	VA	VA/Emp	VA/Hours	Comp/Emp	Comp/Hours	Comp/VA
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)
Obs	7,345	7,345	7,345	7,345	7,345	7,345	7,345	7,345	7,345	7,345
R-squared	0.975	0.979	0.997	0.998	0.996	0.993	0.993	0.994	0.995	0.995

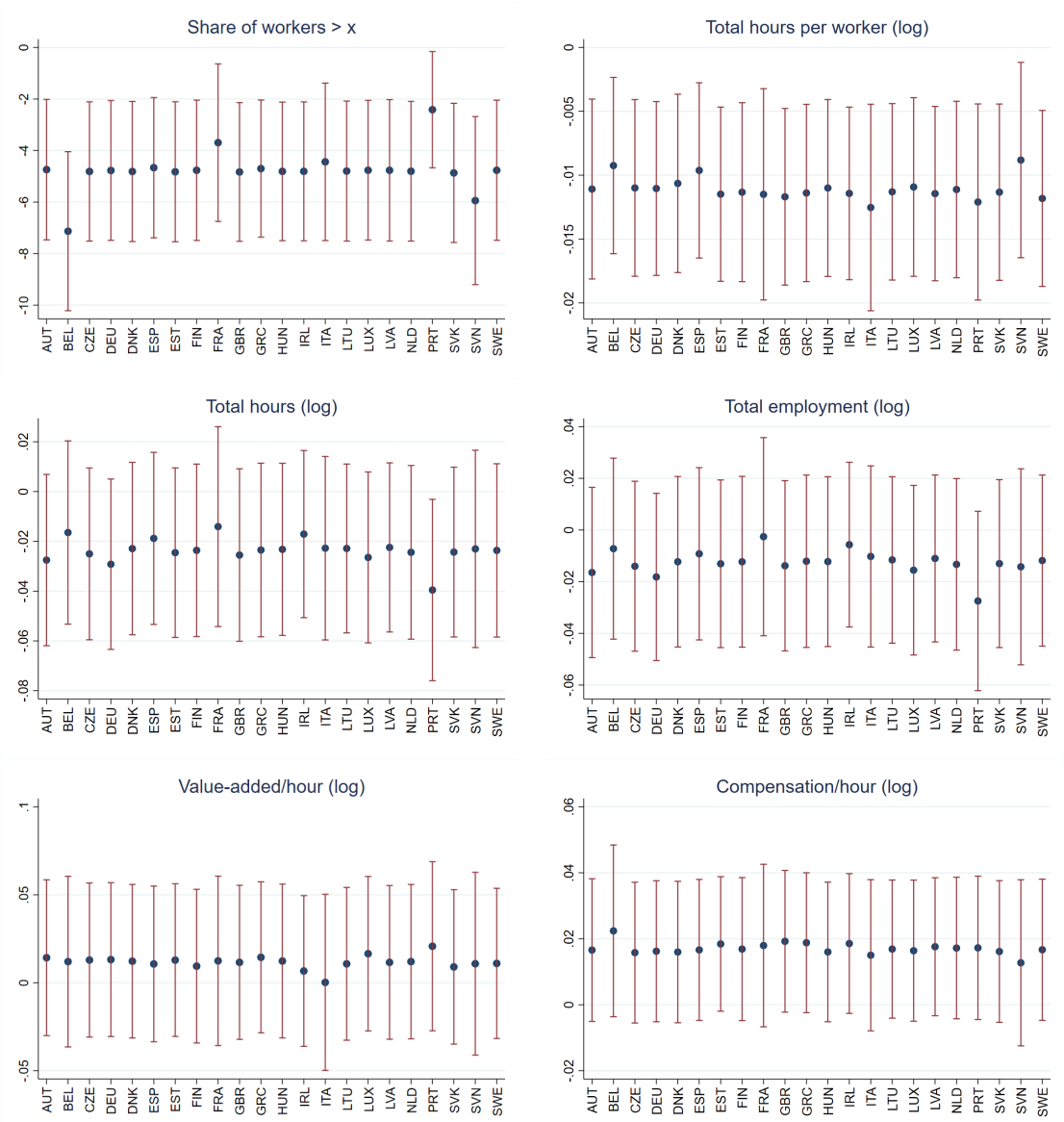
Note: This table presents the results of equation 2b on outcomes at the sectoral level. Sectors are weighted by the within-country share of employment in the pre-reform period. Standard errors are clustered at the country*sector level. Controls included are at the 2-digit Nace Rev. 1.1 from an ad-hoc 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.*

Figure A1: Randomization inference: distribution of pseudo estimates of the effect of working time reduction reforms on hours worked



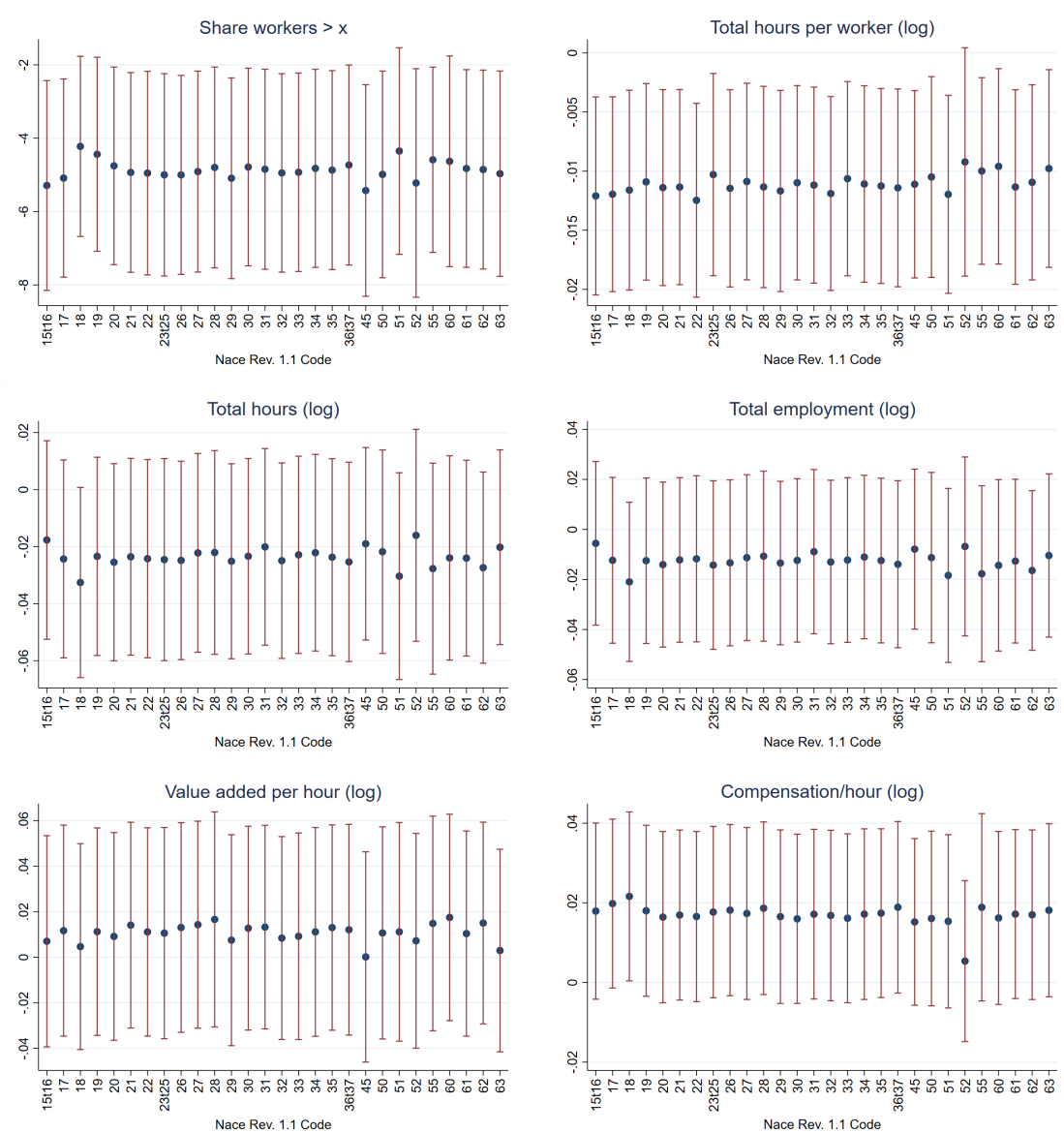
Note: the blue dots show the distribution of estimates of equation 2 when we randomly assigned sectors a value for the variable $Treated_{i,c}$ in multiple draws. The red dot marks for comparison our chosen estimate (with the true value of $Treated_{i,c}$ by sector). Controls included are at the 2-digit 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 A2: Robustness test to varying the country sample



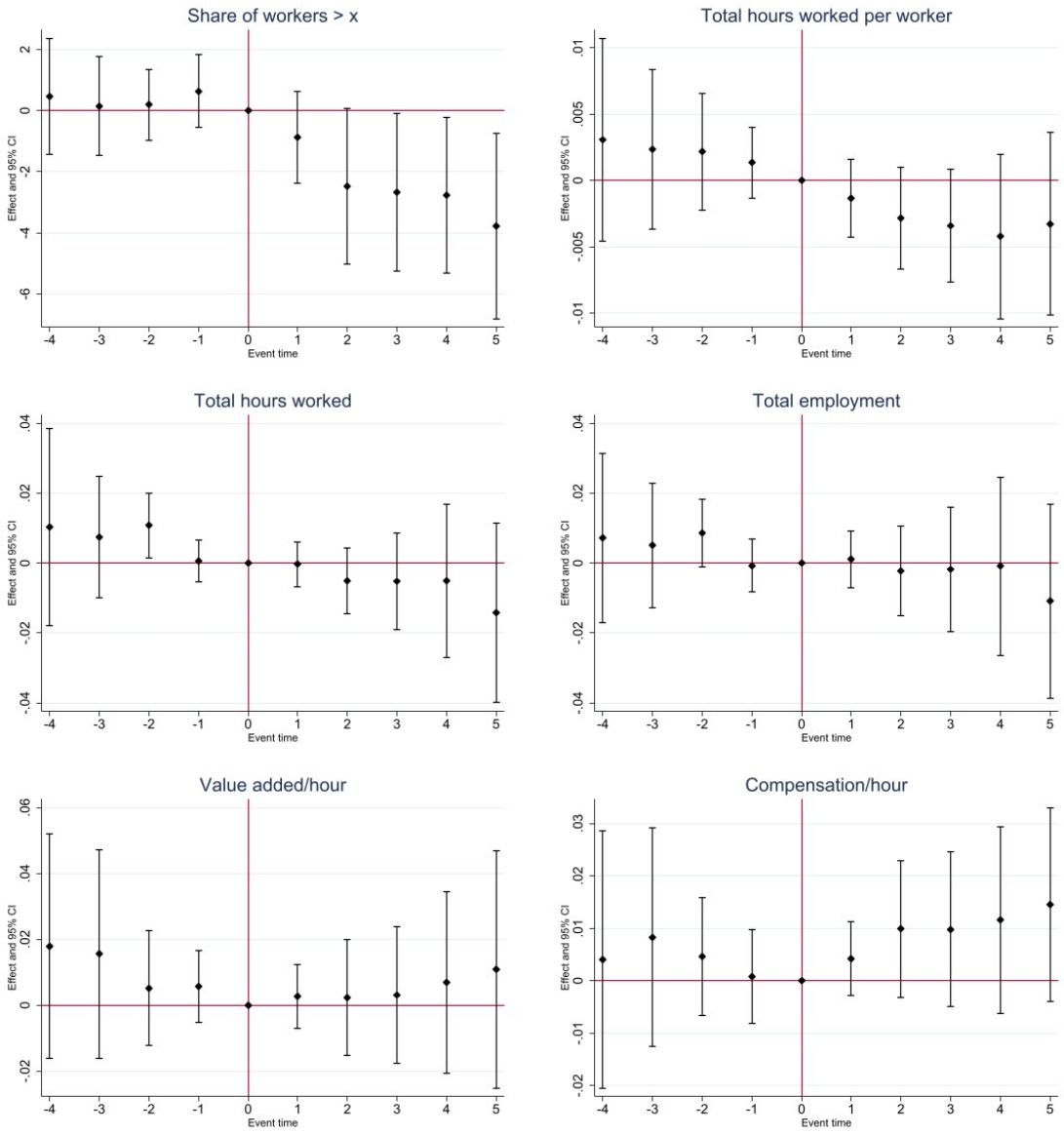
Note: The figure show the evolution of the coefficients from equation 2 for our different outcomes when we drop a given country from the sample.

Figure A3: Robustness test to varying the industry sample



Note: The figure show the evolution of the coefficients from equation 2 for our different outcomes when we drop a given sector from the sample.

Figure A4: de Chaisemartin and d'Haultfoeuille (2020, 2021) - Extended Window



Note: The figure plots the estimated coefficients and their 95% confidence using the estimator proposed by de Chaisemartin and d'Haultfoeuille (2020, 2021). To make this directly comparable to equation 1, we allow for a country non-parametric trend, after taking out the sector×year varition.