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Does a mandatory reduction of standard working hours improve employees' health status?*

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Abstract

Most of the empirical evidence regarding the impact of reductions of standard working hours analyzes its effects on employment outcomes, family life balance and social networks, but there is no empirical evidence of its effects on health outcomes. This study uses panel data for France and Portugal and exploits the exogenous variation of working hours coming from labour regulation and estimate its impact on health outcomes (from 39 to 35 hours a week and from 44 to 40 hours a week respectively). Results suggest that the mandatory reduction of standard working hours decreased the working hours of treated individuals (and not the hours of individuals in the control group). Furthermore, results also suggest that the fact of being treated generated a negative (positive) effect on young males (females)' health in France. No effects on health outcomes were found for Portugal.

JEL Classification: I18, J08, J18, J22.
Key Words: Standard Working Hours, Labour Regulation, Health Outcomes, Promotions.

1 Introduction

It is important to understand how reductions in working hours affect workers' health, as several institutions have suggested this kind of policy. In particular, throughout the 20^{th} century, the International Labour Organization (ILO) strongly supported the reduction of working hours specifically because of its potential benefits to workers' health (International Labour Organization 1990). Similarly, in 1993 the European Union implemented the European Time Directive, which explicitly recommended that member countries reduce their weekly working hours to potentially improve their citizens' health.

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The rationale behind these recommendations is that longer working hours may be detrimental to workers' health because they disrupt workers' internal and external recovery (especially the latter).[1] In reference to internal recovery, Spurgeon et al. (1997) suggest that longer working hours negatively affect workers' health both directly and indirectly. Longer hours are directly harmful to workers' health because they cause stress as workers try to maintain performance levels while facing increasing fatigue, and they are indirectly harmful because they increase the length of time that a worker is exposed to other sources of workplace stress. Taris et al. (2006) suggest that internal recovery will depend mainly on the characteristics of each job and that longer working hours will affect external recovery mainly by shortening the periods when individuals rest.[2] Working longer hours will generate a spiral, since those workers who do not fully recover from a work day will have to invest additional effort to perform adequately during the following day, resulting in an increased intensity of negative load reactions that appeal even more strongly to the recovery process. These effects will accumulate over time, affecting health outcomes (Sluiter et al. 2003).

However, apart from these negative effects that support international organizations' claims, longer working hours may also have some positive effects, as they are positively associated with current and future earnings and with faster rates of career progression (Francesconi 2001); since health improves with earnings (Deaton 2003), higher earnings should increase individuals' health status. Furthermore, the literature on promotions supports these ideas. In particular, Lazear and Rosen (1981) and Rosen (1986) view promotion as a tournament in which promotions are allocated to those workers who rank higher than all other workers in a group in a given period. The probability of getting promoted provides an incentive to exert effort, and, as this effort or propensity to work hard is not directly observable, firms will use indicators, such as hours of work or overtime hours, to select workers for promotion. Thus, a mandatory reduction in working hours for treated individuals (relative to controls) will limit the scope for competition via hours for this group of workers.[3] This negative effect on the probability of promotions (which affects the future income pattern) may have a negative impact on health, as individuals may become concerned and stressed about their future career and income. This effect is in line with the implications derived from the effort-reward imbalance (ERI) model (Siegrist 1996). In general, the ERI model acknowledges the link between high-cost/low-gain conditions, which are considered particularly stressful. One example of this situation, is a high-effort situation associated

[1] Internal recovery is the worker's capacity to recover during working hours, and external recovery is the worker's capacity to recover outside office hours (Taris et al. 2006).

[2] This points to another open debate in organizational psychology: it is not even clear whether what causes negative health effects is the length or the organization of the working hours. The only consensus here is that something should be done, since countries like the United Kingdom face costs of around £1.24 billion a year in stress-related illnesses (Beswick and White 2003).

[3] Firms have two alternatives when a law that reduces standard working hours is imposed (if the employee is not fired). On the one hand, firms can reduce the treated workers' total number of hours. On the other hand, firms can maintain the total number of hours and pay overtime. In the former case, the probability of promotion is negatively affected by the reasons explained above, and this may negatively affect health. For the latter case, and with a heterogeneous pool of workers, firms will be more willing to pay overtime for the most productive workers, putting extra pressure on workers to show that they belong to this group, and in this way affecting their health status. Additionally, if workers foresee that they are likely to lose their jobs because of an increase in the marginal cost of employment relative to the marginal costs of hours, they will experience additional negative pressure on their health status.

with no prospects for promotion.

Therefore, reducing working hours may produce a trade-off between these two effects, thus having a theoretically ambiguous effect. For this reason, empirical evidence is needed.

To identify how reducing working hours affects health is complicated, as the number of working hours might be endogenous due to the so-called healthy worker effect (Frijters et al. 2009).[4] This is the main caveat of previous studies that analyze the link between health and working hours (see Beswick and White [2003] and van der Hulst [2003] for surveys). To overcome this problem I use a change in regulation as an exogenous reduction in the maximum amount of weekly working hours to analyze its effect on health outcomes. Moreover, we analyze two different countries, each with a different threshold of working hours: France, which reduced its standard weekly working hours from 39 to 35 in 1998, and Portugal, which reduced its hours from 44 to 40 in 1996.

It is important to acknowledge that we are studying only the short-term effects of a mandatory reduction in the standard working hours on health outcomes. We do not analyze the long-term effects, as we face some data constraints. For our analysis we use the eight waves of the European Community Household Panel (ECHP) for France and Portugal, which, despite the countries' institutional differences, enhances comparability since the countries use a common questionnaire. The empirical framework used is a difference-in-differences approach in a random-effects ordered-probit setup, which will allow us to control for individual heterogeneity as well as initial health status.

The structure of this study is as follows. Section 2 presents the existing empirical evidence. Section 3 describes the institutional background of labour regulation in France and Portugal. Section 4 presents the identification strategy, while section 5 presents the data and the summary statistics. Finally, section 6 presents the results and the sensitivity analysis, and section 7 concludes.

2 Empirical evidence on reductions of working hours

Despite the existence of studies that analyze the effect of mandatory reductions in standard working hours on labour market outcomes, welfare, family balance, and social networks, there is no evidence on the effect of this kind of policy on health outcomes. The only related evidence available is that which focuses on analyzing the relationship between health and working hours (see Beswick and White [2003] and van der Hulst [2003] for surveys and Yang et al. [2006] and Artazcoz et al. [2007] for some newer evidence). These studies have the caveat that working hours and health may be simultaneously determined because of the so-called healthy worker effect (see Frijters et al. [2009]). In a regression framework, with health as a dependent variable and working hours as a covariate, this implies that if working hours decrease, then that reduction in working hours may be endogenous; hence, some methods need to be applied in order to overcome this potential bias on the coefficient of working hours.

[4] The healthy worker effect states that individuals with better health will tend to work longer hours than those with worse health.

Among those studies that try to analyze the link between working hours and health are Bardasi and Francesconi (2000), Ulker (2006), and Llena-Nozal (2009). Bardasi and Francesconi (2000) use longitudinal data on male and female workers drawn from the first seven waves of the British Household Panel Survey, 1991-1997, to study how nonstandard employment affects mental health.[5] The authors use a mental health indicator as the dependent variable (derived from the General Health Questionnaire [GHQ]) and a two-period lagged first-difference model that yields estimates of the effect of nonstandard employment on psychological well-being under some strong orthogonality conditions on the process governing the dynamic path of unobservable inputs. They find that working long hours in Britain (>48 hours per week) has no impact on GHQ scores; nevertheless, and as they recognize, even these estimates must be taken with some caution because the imposed orthogonality conditions are strong.[6]

Further examples are Ulker (2006) and Llena-Nozal (2009), who use longitudinal data to empirically assess how changes in labour market status and working conditions affect health (measured as SF36 scores and GHQ scores, respectively) in Australia (Ulker) and in several countries (Llena-Nozal). The within-group estimators used in both studies eliminate the bias from the time-invariant individual unobserved heterogeneity, but that does not solve the healthy worker effect that biases their results. Llena-Nozal's results suggest that negative mental health effects result from working overtime hours for Australian, Canadian, and British men; no effects exist for Canadian women, Swiss men and women, or British women; and positive effects exist for Australian women. Ulker's results show a lower general health index for those men who work long hours.

3 Institutional background: The cases of Portugal and France

In Portugal, a law was introduced on December 1, 1996, to gradually reduce the maximum number of weekly working hours from 44 to 40. The law was passed because the newly elected government wanted to speed up convergence of the "traditionally long hours of work" in Portugal to the European average (Varejao 2005). This was done in two rounds and only for private-sector workers. The first one applied immediately (i.e., from December, 1, 1996) and mandated a reduction of two hours for all workers who were currently working 42 hours a week or more and a reduction for all employees who were working 40 to 42 hours per week. The second round started on December 1, 1997, and mandated that all workweeks should meet the new standard of 40 hours. With respect to overtime pay, the first hour had a premium of 50%, and the premium increased to 75% for additional overtime hours. This was not changed by the new law (although the activation point for overtime premiums was changed to 40 hours); nevertheless, some flexibility was introduced with the new law. The reduction took into account that the normal workweek could be defined on a four-month average. The maximum number of hours was allowed to increase by two hours per day if the total did not exceed 10 hours per day

[5] Notice that they analyze the effect of several types of nonstandard employment on health outcomes, including long working hours.

[6] Apart from the strong restrictions on the process governing the temporal path of the unobserved variables that affect health outcomes, one further caveat is that their methodology assumes that changes in working hours occur two periods before the change in mental health, which in their own words is "arguably a long period of time".

and 50 hours per week (Raposo and van Ours 2010). The law explicitly stated that the monthly wage could not decrease.

In France, in June 1998 the government passed the Aubry I law. This law had two parts. First, it established a weekly 35-hour limit in the private sector (from a previous limit of 39 hours), to begin January 1, 2000, for firms with more than 20 employees. For firms with fewer than 20 employees, the deadline was January 1, 2002. Besides excluding workers in the public sector, it also excluded independent workers. Before and after the reform, overtime was paid at a higher rate—25% for the first eight hours above the limit of 39 hours, and 50% for any additional overtime. The law did not change these rates, but it did shift the activation point for the overtime premium to 35 hours. Second, the Aubry I law also established financial incentives for firms (payroll tax subsidies).[7] Then, during 2000, a second Aubry law was passed (called Aubry II) in order to introduce more detailed legal provisions regarding overtime (e.g., it introduced flexibility to the adjustment to the 35-hour limit) and in order to confirm the limit of 35 hours per week established in the Aubry I law. As in Portugal, the law explicitly forbade a decrease in the monthly wage.

4 Empirical strategy and estimation

4.1 Effect on Hours

Before we analyze the health effect of the mentioned regulation we briefly investigate if the regulation affected or not the working hours of treated workers. This is important because we want to evaluate whether the regulation has further effects on health mediated through changes in work hours. In the following section we investigate (more extensively, as it is our main goal) the indirect effect of the regulation on health. Thus, in order to study the effect of the regulation on working hours we estimate the following regression (as in Sánchez [2013]):

$$Hours_{it} = \beta' x_{it} + \alpha_1' g_{1i} + \alpha_2' g_{2i} + \gamma' d_t + \delta_1' g_{1i} d_t + \delta_2' g_{2i} d_t + \varepsilon_{it} \tag{1}$$

Where $Hours$ refers to weekly working hours for individual i at period t, x_{it} is a vector of covariates and it does not include a constant. $g_{ki}d_t$ represents the interaction variable between the time dummy (d_t) and the group dummy (g_{ki}) where $k = 1, 2$ representing the control and treatment group respectively. In this way we will focus on $\delta_1' and\ \delta_2'$ which will give us the effect of the regulation on the control and treatment group' weekly working hours. The next step is to present the model used to estimate the effect of the regulation on health outcomes.

[7]By granting financial incentives to alleviate labour costs, the law encouraged firms to reduce hours by 10% and increase the number of employees by 6% before legal deadlines were set (Askenazy 2008).

4.2 Effect on Health

To analyze the effect of the regulation on health outcomes, we use a difference-in-differences approach in a random-effect ordered probit. The treated group is defined as those individuals whose hours are just below the old threshold and above the new one, and the control group is defined as those individuals whose hours are just below the new threshold. All these were defined regarding their working hours prior the policy change (more detail below).

It is crucial to include individual unobservable heterogeneity in these kind of studies since, as Adams et al. (2003) suggest, the apparent significant causation of some covariates on health outcomes may be due to an unobservable persistence that is correlated with covariates and health outcomes. Also, a sequence of repeated observations on the same individuals makes it possible to allow for unobservable but persistent differences in the way that individuals translate their perceptions of health into survey responses. Unfortunately, it will not be correct to include a lagged dependent variable as a covariate when a difference-in-differences approach is used, as we would be analyzing the change of the probability of reporting a given level of health in period t conditional on the previous health status for the pre- and post-treatment periods for the control and treatment groups. That would force the previous health status to be the same in post-treatment periods between the treatment and control groups.[8] Thus we estimate:

$$P_{it,j} = P(h_{it} = j) = \Phi\left\{\mu_j - \eta' x_{it} - \lambda' z_{it_1} - \delta' g_i - \zeta' r_t - \beta'(g_i * r_t) - \psi'(z_{it_1} * r_t) - \alpha_i\right\} \quad (2)$$

$$-\Phi\left\{\mu_{j-1} - \eta' x_{it} - \lambda' z_{it_1} - \delta' g_i - \zeta' r_t - \beta'(g_i * r_t) - \psi'(z_{it_1} * r_t) - \alpha_i\right\}$$

where $\Phi\{.\}$ is the standard normal distribution function. x_{it} is a set of observed variables for individual i at period t which may be associated with health and z_{it_1} is a vector of dummies for the individual's health status in their first year t_1 (i.e. 1994), $\eta, \lambda, \delta, \zeta, \psi$ and β are parameters to be estimated, α_i is an individual-specific and time-invariant random component which is assumed to be distributed as $N(0, \sigma_\alpha^2)$. As is typical in a difference-in-difference approach, we include a group effect (g_i) equal to one for those individuals treated, time effects (r_t) and interactions between these two ($g_i * r_t$) and whose parameter reflects the difference-in-difference effect. Our approach would imply

[8] In particular, if we include a one-period lagged dependent variable instead of initial health status, under this latter setup the difference-in-differences coefficient would not capture the effect of the policy change on health outcomes of period t, as we would be doing a ceteris paribus analysis on the change of $\mathrm{P}(y_{it} = j|\ y_{it-1}, x_{it}, \ldots)$, which conditions on $y_{i,t-1}$for the pre- and post-treatment periods for the control and the treatment group. That would not be correct, as the lagged dependent variable will be forced to be the same in post-treatment periods between the treatment and control groups. Therefore, if the policy change had any effect, this latter approach would be incorrect. In the "Robustness of the results" section, we also present the coefficients obtained when a lagged dependent variable is included as a control instead of the initial health status. The results do not change significantly for Portugal, but there are some differences for France (shown below).

that the health status of the first period would have the same impact on the health status of the second, third, and so on periods. To allow for different impacts of the initial health status by year, we add interactive terms ($z_{it_1} * r_t$) between the time effect (r_t) and the health status of the initial period (z_{it_1}).Thus, our interest lies in the coefficient β.[9] x_{it} and z_{it_1} are assumed uncorrelated with ε_{is} for all t and s. The error term ε_{it} is assumed to be normally distributed with mean 0 and variance σ^2 and uncorrelated across individuals and waves and uncorrelated with α.

Before the actual estimation, we need to deal with two challenges. Firstly, the random effect ordered probit, assumes that there is no correlation between the individual effect (α_i) and the covariates. This seems to be very restrictive in our setting since individual unobserved heterogeneity is likely to be correlated with the covariates (e.g. unobservable psychological characteristics that make individuals respond in a particular way to the health survey might be correlated with covariates as age or gender). Since the coefficients estimated by the random effect estimator are in general inconsistent under this setting (especially when T is not very large) we can use Mundlak's (1978) parameterization. This captures the correlation between the individual effect (α_i) and the average of the regressors. Thus, we use:

$$\alpha_i = \alpha_0 + \alpha_1' \overline{x_i} + u_i \tag{3}$$

where $\overline{x_i}$ is the average over the sample period of the observations on the exogenous time variant variables. By construction u_i is distributed $N(0; \sigma_u^2)$ and independent of the x_{it} variables and the idiosyncratic error term (ε_{it}).[10] Thus, α_i in equation (2) is replaced by equation (3).[11]

Finally, we follow the approach adopted by Contoyannis et al. (2004b) which is to split the sample by gender and age groups before estimating our models to see if there are heterogeneous effects of the covariates by subgroups. Evidence of very different results could indicate heterogeneity with respect to cut off points (van Doorslaer and Jones 2003).

5 Data

5.1 Data description

We use the European Community Household Panel (ECHP), which is a standardized panel survey used to interview a sample of households and persons every year in the European Union. These

[9] This assumes homogeneous effects of the treatment, which represents the change in the intercept between the treated and control groups. To allow for heterogeneous effects, one could include interactions between the group dummy, the time effect, and x_{it}, although that would have a degrees-of-freedom cost. Therefore, we maintain the assumption of homogeneous effects. This seems reasonable, as the interactive terms added to capture the heterogeneous effects are not significant in the cases of France and Portugal.

[10] By construction refers to the fact that once the distribution of α_i is defined, u_i has the same distribution.

[11] This results in a likelihood that can be easily maximized using common software (e.g. Gllamm in STATA).

interviews cover a wide range of topics concerning living conditions, including the interviewees' income information, financial situation in a wider sense, working life, housing situation, and health, among other things. The sample size is 170,000 individuals in the initial wave for the 12 countries included. The ECHP had a total duration of eight years, running from 1994 to 2001 (eight waves). The main advantage is that information is homogeneous among countries since the questionnaire is similar in each case. This source of data is coordinated by the European Commission's Statistical Office (EUROSTAT).

5.2 Measures of health: SAH

One of the first concerns in studies that analyze health is how to measure it. In our case, given that we are using regulation of working hours as one of the covariates, we needed data that contains both the labour and health information of individuals. Because of its wide use in health economics models, SAH is a natural choice. Several socioeconomic surveys measure SAH and, despite some differences, it has a common frame. In the ECHP, it is generally defined by a response to the following question: would you say that your health has on the whole been excellent/good/fair/poor/very poor? SAH is measured as a categorical variable indicator from 1 (higher category) to 5 (lower category). Since SAH is a subjective measure of health, it is subject to criticism, as it may be affected by measurement error. Furthermore, it has been argued that the mapping of "true health" into SAH categories may vary with respondent characteristics. This happens when population subgroups use systematically different cutoff-points levels when reporting their SAH, despite having the same level of "true health".[12] Despite these caveats, SAH has been used widely in previous studies of the relationships between health and socioeconomic status (e.g., Adams et al. 2003) and between health and lifestyle (e.g., Contoyannis et al. 2004a). Moreover, SAH has been shown to be a powerful predictor of subsequent mortality (e.g., Idler and Benyamini 1997) and a good predictor of subsequent use of medical care.[13] It has also been shown that inequalities in SAH predict inequalities in mortality (e.g., van Doorslaer and Gerdtham 2003). Furthermore, an appealing characteristic of general health measures such as SAH is their ability to encapsulate and summarize a multitude of health conditions. This latter point is important since, in general, objective measures of health status are rare in survey data, and where they do exist they are often too specific to particular health conditions (Hernandez-Quevedo et al. 2005). Therefore, in our study we use SAH as a measure of health for Portugal and France for waves 1 through 8. Also, we merge the worst two categories due to the small sample size of these categories (hence we end with 4 categories).

Finally, it is important to mention that because of the econometric method described above, which assumes that the top category is the one with better health, we invert SAH. In this way, SAH goes from 1 (worse health) to 4 (better health).

[12] This source of heterogeneity with respect to cutoff points has been termed "state dependent reporting bias" (Kerkhofs and Lindeboom 1995), "scale of reference bias" (Groot 2000), and "response category cut-point shift" (Murray et al. 2001).

[13] Its predictive power does not appear to vary across socioeconomic groups (see, e.g., Burström and Fredlund 2001).

5.3 Covariates used

The included covariates can be divided into three groups: socioeconomic variables, occupational and firm-related variables, and other variables. For the first group we include dummy variables representing marital status (married, widowed, or divorced/separated), with single as the reference category; size of the household (the number of people living in the same household); and two dummies to account for the number of children at different ages (<12 and <16). The income variable is the logarithm of equivalised annual household income, equivalised by the OECD-modified scale to adjust for household size and composition.[14] We include two dummies for the highest level of educational qualification completed (second stage [ISCED 3] and third level [ISCED 5-7]); less than second stage (ISCED 0-2) is the base group.[15]

For the occupational and firm-related variables, we use three dummies for the type of work contracts (fixed-term or short-term, casual work with no contract, and some other working arrangements), where permanent employment is the base group; occupational dummies; and industry dummies (following the ILO categories). We include dummies to control for the level of job satisfaction, which is in line with Datta Gupta and Kristensen (2008), who use this variable as a proxy for job stressors.[16] For the other variables, we include age as a second-order polynomial (i.e., age and $\frac{age^2}{100}$).

5.4 Summary statistics and the evolution of health outcomes and weekly working hours

5.4.1 Summary Statistics

For the French case, the sample excludes those individuals who are employed in paid apprenticeships or training schemes, are self-employed, or are classified as unpaid family workers. We also exclude those who do not work in the private sector, those who work in firms that have fewer than 20 employees, or those younger than 20 or older than 60. The difference-in-differences approach described above requires the definition of a control and a treatment group and a pre- and post-treatment period. For the French case, as the interviews were conducted almost entirely in October of each year, the pre-treatment period was defined as 1994-1997 and the post-treatment period as 1998 onwards. Therefore, we defined the control group as those individuals who were working 30-35 hours a week prior the policy change (i.e. october 1997) and the treatment group as those who were working 36-39 hours per week before the policy change (i.e. october 1997).[17]

[14] The OEDC-modified scale gives a weight of 1 to the first adult, 0.5 to other persons age 14 or over, and 0.3 to each child younger than 14. For each person, the "equivalised total net income" is calculated as its household total net income divided by equivalised household size. In this case, we use the logarithm of household income (OEDC-modified scale), taking into account the concavity in the health-income relationship.

[15] The ISCED classification comes from the International Standard Classification of Education from UNESCO. It is a seven-level scale that allows for the comparison of educational levels in different countries.

[16] Dummies for job satisfaction levels might be endogenous. Nevertheless, we include them in the model since in some cases they are significant and also because their exclusion does not affect the coefficient of interest.

[17] It could be the case that the policy change may affect differently employees who were working 39 hours just before the policy change relative to those who were working 36 hours (even though both are treated, but in a different magnitude).

For the Portuguese case, the estimation excludes those individuals who are employed in paid apprenticeships or training schemes, are self-employed, or are classified as unpaid family workers. We also exclude those individuals who work in the public administration and defense sectors and those who are younger than 20 or older than 60. For the Portuguese case, we defined our pre-treatment period as 1994-1996 and our post-treatment period as 1997 onwards.[18] Also, we defined our control group as those workers who were working 37-40 hours a week before the policy change (i.e. october 1996) and our treatment group as those who were working 41-44 hours a week just before the policy change occurred (i.e. october 1996).

In **Table I** we present the summary statistics of the variables under analysis and the covariates separately for each country in the moment just before the policy was in place.[19] This table suggests that most of the covariate averages are very similar when the control and treatment groups are compared within each country. This suggests that most variables are well balanced between the control and treatment group. This is also true when we compare the distribution of each covariate (see, e.g., the age situation in Portugal, presented in **Figure** 1)[20]. The main differences between the control and treatment groups are in occupation and industry. These can be observed, for example, by looking at **Figure** 2 and **Figure** 3 for the Portuguese case.[21] These results were expected, as the number of job hours differs across occupations and industries, which suggests that controlling for occupation and industry is important for our purposes.

5.4.2 Evolution of Working Hours and Health

As working hours and health status are crucial in our analysis we present a further description of both variables by treatment group and year. Regarding weekly working hours we find the following: (a) In the Frech case we observe that until 1997 the average of weekly working hours stays right around 39 hours for the treated group (see **Figure 4**)); nevertheless, from 1998, when the policy was implemented, weekly working hours for treated individuals decreased significantly to almost 37 hours in 2000, which implies a reduction of around 2 working hours per week (on average). The declining trend increased in 1999 probably in order to meet the January 2000 deadline that the French government set when they announced the policy. Despite this behaviour on treated individuals it is possible to observe no significant changes on individuals belonging to the control group.

In the same line, results of **Table II** suggest that working hours regulation reduced in around 2 the weekly working hours for treated individuals in France. Furthermore, we observe that the regulation did not have almost any effect on those individuals who belong to the control group. These results are similar to the ones found by Goux, Maurin, and Petrongolo (2011). The decline in weekly working

We decided to keep the definition of treated to those employees who were working 36-39 because if we narrow it the sample size will became too small. Hence we are capturing the average effect on treated individuals. The same idea applies to the Portuguese case.

[18] We used these groupings because all the interviews were carried out in October 1996 and the policy change was in place from December 1, 1996.

[19] In each case, we discuss the final sample data.

[20] Because of the large number of figures, the rest of the distributions are not presented but are available on request.

[21] Similar situations occur for France. These figures are not included but they are available on request.

hours of the treated group (1998-2000) coincides with a break on the declining trend observed in SAH with a positive and not significant effect in 1999 (see **Figure 5**). On the other hand, for the control group we observe a declining trend in SAH during the complete period analyzed.

In the case of Portugal and as shown in **Figure 6**, we observe a drop of around 2,5 weekly working hours for treated individuals from 1996 to 1998, period when legislated reductions in working hours took place. Similarly to the French case, we do not observe any significant change of working hours for those individuals who belong to the control group. These observational findings coincide with what we found in **Table II** were a reduction of around 2,5 hours is found for the treated group and no significant variation is found for the control group. Regarding the average SAH for treated individuals in Portugal, we observe a declining trend in the SAH measure across time until 1997, then a slight increase in 1998, and a bit further increase in 1999 (**Figure 6**). Afterwards, it started to decrease marginally. This behaviour might suggest a lagged effect on health. For individuals belonging to the control group we do not observe significant changes.

Therefore, by looking at the summary statistics of treated and control individuals, before and after the policy change, we observe a reduction of weekly working hours for the treated group while there were no significant reductions for individuals in the control group. Hence, from the preliminar data analysis it seems that the mandatory reduction had an effect on working hours. Results on health are less obvious from the figures, hence we procceed in the next section to the estimation of our model in order to see what the data suggests.

6 Results

6.1 The case of Portugal

Before going to our main set of results, we briefly discuss our results of equation (1) that focuses on the effect of the regulation on working hours. In **Table III,** we see that on average the legal reduction of working hours did not affect individuals in the control group. However, the legislation indeed reduced the working hours of treated individuals, although only in an average of around 2.5 hours a week. Results for the treated group are significantly different from zero while results for the control group are non significantly different from zero. These results point to the same direction than those presented in **Table II** previously discussed.

Regarding the results for health outcomes, we compute the average partial effects (APEs) for each of the four categories of SAH, but since the total changes of these probabilities should add up to zero (and to save space), we present the results for the best two probabilities (very good health and good health) in **Table IV** for men and women. In each case, we also separate the results by age range.[22]

From columns (1) and (2) of **Table IV**, we observed that for men younger than the average age (37 years), the top two health categories (very good and good health, respectively) present negative APEs for the treated group (41-44 hours) in 1997 and 1998. This would mean that in 1997, after

[22] To define the age ranges, we used the average age of the Portuguese sample, 37 years.

controlling for several covariates, those treated individuals have a lower probability of being in the top two categories (i.e., having very good or good health), but as the β coefficient in column (3) is not significant, these variations are not statistically different from zero. The same results are obtained for men above the average age (columns [4] and [5]). These results imply that the policy did not affect the probability of reporting very good or good health for this group.

These previous analyses assume that the impact of the reduction in hours is contemporaneous, but it could be the case that the impact of the policy takes time to show up in terms of health, as might be implied by **Figure 6.** For this reason we repeated the previous estimation but replaced the group dummy in year t by its one-year lagged value. In terms of equation (2), This implies the replacement of g_i defined at year t by g_i defined at year $t-1$. By doing this, we expect to capture any effect due to the implementation of the policy one year later. The results can be seen in **Table V** for men and women, each separated by age range. In all these cases, the effects are not statistically significant and are similar to the contemporaneous case.

6.2 The case of France

Similarly to our Portuguese case, we first discuss our results of equation (1) that focuses on the effect of the regulation on working hours. We see in **Table III** that the legal reduction of 4 hours in France did not affect individuals in the control group. However, the legislation indeed reduced the working hours of treated individuals, although only in an average of around 2 hours a week. Again, results for the treated group are significantly different from zero while results for the control group are non significantly different from zero. This points in the same line than results of **Table II** previously discussed and to the results of Goux, Maurin, and Petrongolo (2011).

Regarding health outcomes, the results for men below the average age of 39 years (columns [1] and [2] of **Table VI**) show negative APEs for the top two health categories for the treated group (e.g., 36-39 hours) in 1998 and 1999. The β coefficients are significant at 1% and 5%, respectively. In particular, the treatment reduces the probability of reporting very good or good health by 23 percentage points (13 and 10 percentage points, respectively) in 1998 and by 17.5 percentage points (11 and 6.5 percentage points, respectively) in 1999. The estimated APE across all periods results in a detriment in the probability of reporting very good health and good health by 6.0 and 4.2 percentage points, respectively.[23] As the probability of reporting very good or good health in 1997 is around 67%, the effect of the policy reduces the probability of reporting very good or good health by 14.9% on average. The results for men above the average age (columns [4] and [5] of **Table VI**) show no significant effects due to the policy change.

For women, results are exactly the opposite of those obtained for men. In particular, for women below the average age, the coefficient is significant at 5% and positive in 1998 (column [9] of **Table VI**). This result implies that the policy change increased the probability of reporting very good health

[23] This average is calculated by adding 13.1 percentage points (in 1998) plus 11 (in 1999) plus zero (in 2000 and 2001) and dividing by 4, which equals 6.0 percentage points. A similar calculation is used to get 4.2 percentage points.

by 15 percentage points and caused a combined increase of 13 percentage points in the probability of reporting very good or good health. The estimated APE across all periods results in an average increase of 3.8 percentage points in the probability of reporting very good health. As the pre-treatment probability of reporting very good or good health for this group is 64% in 1997, our result suggests that the effect of the policy change generated an average increase of around 5.9% in the probability of reporting very good health.[24]

6.3 Discussion of the results

The discussion of the results obtained above is complicated, as there are several dimensions to consider—in particular, comparisons between the treatment and control group by country, age range, and gender. Here we present some hypotheses that might explain our results. But as we will see, more research on this topic should be done in the future.

6.3.1 The Trade-off

The non-zero effect of the policy change in France and the zero effect in Portugal may be explained by drawing on both the literature on promotions and the literature on psychological health effects. On the one hand, the psychological literature suggests that a reduction in working hours might have positive effects on health since individuals will have more time to recover (the so-called external recovery; Taris et al. 2006). On the other hand, the probability of getting promoted provides an incentive for the worker to exert effort without the need for any formal contract with the firm (Francesconi 2001). As this effort or propensity to work hard is not directly observable, firms will use indicators, such as hours of work or overtime hours, in selecting workers for promotion. Thus, a mandatory reduction in working hours for treated individuals (relative to controls) will limit the scope for competition via hours for this group of workers. This negative effect on the probability of promotions (which affects the future income pattern) may have a negative impact on health, as individuals may become concerned and stressed about their future career and income. This effect is in line with the implications derived from the effort-reward imbalance (ERI) model (Siegrist 1996). Therefore, we may have a trade-off between two effects.

6.3.2 The differences in the case of Males and different age ranges

As individuals in Portugal already work longer hours than those in France, it would be more difficult for them to use overtime work as a way of increasing their chances of promotion. In other words, the scope for competition through hours is lower in Portugal than in France. Therefore, a mandatory reduction in working hours might have more negative effects on treated relative to control men in France than in Portugal.

[24] This includes coefficients that are significant at 5% at least.

Furthermore, the promotions explanation also seems to be useful to help us explain the difference between the effects found for men at different age ranges—in particular, for those below versus those above the average age (i.e., those 20-38 years old versus those 39-60 years old). This is because, if the hypothesis about the effect on promotions is true, we should expect a more negative effect on those individuals who are in the beginning or early stages of their careers relative to those who are more settled in their jobs.

6.3.3 The differences in the case of Females and different age ranges

What is more difficult to explain with the promotions hypothesis are the effects of the reduction in working hours on the health status of women in France. This is because, as it was stated, our hypothesis would suggest a negative effect; however, we find a positive one. A potential extension to our hypothesis would be that women, and especially those below the average age (i.e., those younger than 38 years), have already internalized that the probability of promotions in the future might be undermined by the loss of human capital due to pregnancy. Because of that, the negative effect on the probability of promotion for those treated women relative to the control group might be smaller than the potential positive effect on health, which may give an overall positive effect.

The above hypothesis is in line with what Booth and Francesconi (2000) found. They analyzed the difference of promotion predictors by gender and found that, by comparing the effects by gender, there are "striking similarities and important differences". In particular, they found that working longer overtime hours is positively associated with the probability of promotion. As an example, they point out that the probability of promotion for men working part-time decreases by 6 percentage points as compared to men working full-time, while the effect for women working part-time is much smaller relative to those women who work full-time. This implies that fewer hours of work for women have a weaker negative effect on the probability of promotions, which is in line with our hypothesis above.

6.4 Sensitivity Analysis

6.4.1 Attrition

In health studies (as ours), attrition is likely to be endogenous since healthier individuals should last longer in our panel. Due to this, Jones et al. (2006) studied the health-related non-response in the first 11 waves of the British Household Panel Survey and the full eight waves of the European Community Household Panel (ECHP) and explored its consequences for dynamic models of the association between socioeconomic status and self-assessed health. They use the Verbeek and Nijman (1992) test and correct for non-response (with the inverse probability weights method) in empirical models of the effect of socioeconomic status on self-assessed health. Jones et al. (2006) found that there is health-related non-response in the data, with those in very poor initial health more likely to drop out; nevertheless, as they point out, "a comparison of estimates—based on the balanced sample, the unbalanced sample and corrected for non-response by using inverse probability weights—shows that, on the whole, there are not substantive differences in the average partial effects of the variables of interest".

To test for the possibility of endogenous attrition, we follow the same approach. That is, we use a variable addition test as proposed by Verbeek and Nijman (1992), which tests the significance of an indicator that counts the number of waves observed for each individual. The reasoning behind this test is that if non-response is random, indicators of an individual's pattern of survey responses (e.g., number of waves [nw]) should not be associated with the outcome of interest (health) after controlling for the observed covariates x.[25] The results of this test for the ECHP suggest that attrition is not endogenous for France, although for Portugal it is significant for men and women at 10% (see **Table VII**). These differ from the results of Jones et al. (2006), who find that the indicator is significant at 1%. An explanation for this difference might be that Jones et al. (2006) use the entire sample of individuals (i.e., those older than 16 who are employed, those who are unemployed, etc.), while in our case, and since we are interested in the effect of a reduction in hours on health outcomes, we use only those who are employed and age 20-60. This subgroup is healthier on average than, for example, unemployed adults; therefore, the ECHP is expected to have a weaker dropout rate due to health-related reasons.[26]

We also compare the coefficients of the model with the indicator relative to the coefficients of the model without the indicator. We find that the estimates are similar, which is the same result obtained by Contoyannis et al. (2004a) and Jones et al. (2006) (see **Table VIII** for the Portuguese case, where columns [1] and [2] show the results for men and columns [3] and [4] for women, and similarly **Table IX** for the French case). This supports the idea that, even with endogenous attrition, its existence does not affect the estimates of the variables of interests. These results are corroborated when we compare the coefficients of the balanced and unbalanced panels for men and women in Portugal (see **Table X**) and France (see **Table XI**). Therefore, and similarly to previous studies, we do find some support for no significant effects of attrition bias on the estimates of the variables of interest.[27]

6.4.2 Alternative definition of control groups

We present the results when slight modifications are introduced to the definition of the control group range, which can be increased or decreased and where either option has benefits and costs. In our case, the control group definition is modified by increasing and reducing its range for each country. Results are presented in **Table XII** and **Table XIII** for Portugal and France, respectively, for some of the verified cases. For Portugal we present the case when the control group is defined as age 36-40 instead of the 37-40 age group used above. For the French case we present the results when the control

[25] This means that it tests a conditional independence condition $E\ (health \mid x,\ nw){=}E(health \mid x)$.

[26] Another reason for the difference between our result and the one presented by Jones et al. (2006) could be that our model includes the initial health status and its interaction with the time effect as covariates, while Jones et al. (2006) use instead a lagged dependent variable as a covariate.

[27] This can also be done by comparing the coefficients with a Hausman test. We did not, however, make such a comparison, since the coefficients are almost the same with or without the indicator and the Hausman test also has low power (see Jones et al. 2006, 14).

group is defined as age 31-35 instead of the 30-35 age group used above. As can be seen, results are very similar to those presented above in each case; therefore, conclusions do not change with changes in the definition of the control group.

6.4.3 Flexibility

In Portugal, greater flexibility was introduced along with the reduction in hours. Greater flexibility may involve the reorganization of work; this reorganization, rather than the reduction in hours, may affect the health of individuals. This generally depends on the type of industry and occupation, and given that we control for both, it is less likely to be the case.[28] In France, flexibility was introduced only in 2000; hence, it should not affect our results for 1998 and 1999.

6.4.4 Common macro trend and common support

Results, when the difference-in-differences approach is used, rely on the crucial assumption of a common macro trend. In practice, this means that interactions between the group dummies and the time effects in the absence of the policy change should be insignificant, because if they were significant it would imply that control and treatment groups behave differently when there is no policy in place.

For the Portuguese case we can observe the interaction in 1996, which is not significant (see **Table IV**). For the French case we observe the interactions for 1996 and 1997. As can be seen for both men and women, they are not significant (see **Table VI**). All these results suggest that there are no significant differences in trajectories between the control and treatment groups before the policy takes place.

As the difference-in-differences approach compares the treatment group with a control group, the latter should represent what would have happened with those treated if they had been not been treated. In order for this to be true, one should contrast comparable individuals. This condition is also known as "full common support". In our case, this is likely since both the treatment and control group have full common support (see, e.g., **Figures 1-3**) and also are very similar in observables.[29] Therefore, it is reasonable to assume that they are also similar in unobservable characteristics.

6.4.5 Alternative Specification

In models where health is the dependent variable, it is typical to include a lagged dependent variable in order to try to capture health state dependence. Unfortunately, because we are exploiting a policy change, the difference-in-differences coefficient would not capture the effect of the policy change on

[28] To further check If more flexibility affects my identification I have dropped from the sample those jobs with more volatile hours. Results do not change in a significant way. Hence, higher flexibility doesn´t seem to be an important problem in the case of Portugal.

[29] See summary statistics above. For the Average Treatment on the Treated (ATT), full common support means that given X, the probability of being treated is less than one, that is, $P(D = 1|\ X) < 1$. This implies that for a covariate X there are treated and control individuals, not only treated individuals.

health outcomes of period t. This is because we would be doing a ceteris paribus analysis on the change of P($y_{it} = j|\ y_{it-1}, x_{it,....}$), which conditions on $y_{i,t-1}$for the pre- and post-treatment periods for the control as well as for the treatment group. That would not be correct since, for post-treatment periods, the lagged dependent variable would be forced to be the same between the treatment and control groups, which may not be the case. Therefore, if the policy change had any effect, this latter approach would be incorrect. In any case, since in the health economics literature it is common to include a lagged dependent variable as a covariate, we present in **Table XIV** the coefficients for France obtained when a lagged dependent variable is included as a control instead of the initial health status. As can be seen, results are similar with respect to those in **Table VI**, which uses the initial health condition as well as its interactions with the time effect, although there are some differences in the significance level for France. As expected, results are not significantly different for Portugal.[30]

6.4.6 Measurement Error

The ECHP data used in our study, as all survey data, might be subject to measurement error. This is especially important for hours of work, since measurement error in this variable could lead to misclassification of individuals into hours groups and thereby to a dilution of the estimated effect on health outcomes. As the ECHP does not include a direct question about overtime, we might have misclassified individuals into hours groups.[31] To test how important this effect is, we exclude those sectors with higher probability of working overtime in France and Portugal just before the policy was in place (from the LABORSTA database).[32] This probability is mainly affected by occupation (economic activity). In particular, workers in wholesale retail trade, restaurants, and hotels and in community, social, and personal services sectors have a higher probability of working overtime in Portugal in 1996. For France, sectors with a higher probability of working overtime in 1997 are wholesale retail trade, restaurants, and hotels, as well as real estate, renting, and business activities. Therefore, if we do have an important misclassification due to the lack of a direct question about overtime, we should expect our results to change by excluding workers in these categories, since they should have higher probabilities of misclassification. Results suggest that estimates do not change when we exclude workers in the mentioned categories; hence, it seems that misclassification is not a significant problem in our case.This is reinforced when we see **Tables XII** and **XIII**, where our results do not change when we modify the definition of treatment and control groups. This is because with a wider definition of groups we should expect a lower measurement error between the two groups.

[30] Also, and to relax the particular functional form of the probit used in our Difference in Difference, we estimate our model with the linear probability model. Results point to the same direction of the ones reported here for the ordered probit. These results are not included but are available upon request.

[31] For example, someone in Portugal who reports 44 hours a week of usual hours might imply (a) 44 normal hours and zero overtime or (b) 40 hours plus 4 hours of overtime. This is important since, without further information, in the first case the individual will be categorized in the treatment group and in the latter case in the control group. The same kind of problem might arise in the French case.

[32] See http://laborsta.ilo.org/.

6.4.7 Effect on Earnings

Another robustness check is to analyze the effect of the legislation on monthly wages (i.e. earnings). This is because someone may think that the regulation may do nothing to work hours, but just increase earnings and via earnings it may affects health. To analyze this concern we did two exercises: firstly, we can go back to our previous result were we show that working hours indeed were reduced for treated workers and not for control workers (**Tables II and III**). Secondly, because working hours decreased hourly wages increased. Thus, we should check what happened to monthly wages (i.e. earnings). If monthly wages remain constant any impact on health outcomes can be attributed to working hours regulation and not higher earnings. To study this potential effect we re-estimate equation (1) but replacing the dependent variable for ln(hourly wage). The results are presented in the second column of **Table III**. We observe that the reduction of weekly working hours is linked to higher hourly wages only for treated workers. In the case of Portugal, treated individuals (with a reduction of around 2.5 weekly hours, i.e. a 5.7% reduction in weekly hours) have a 5.4% increment in hourly wage. In the case of France an average decrease of around 2 hours per week from the original 39 hours (i.e. a 5.1% reduction) generated a 4.9% increment in hourly wage for treated individuals. Thus, for treated workers hourly wages increased in the same proportion that working hours decrease on average, which means that there are wage compensation in both countries. This result implies that monthly wages (i.e. earnings) for treated individuals remained close to the same after the reduction in hours.

We can also infer from **Table III** that effects are non-significantly different from zero for the control group, which suggests that there is no spillover effect on hourly wages on the control group. These results are in line with the legislation (in Portugal and France), as the law explicitly stated that the monthly wage (i.e. earnings) could not decrease. What we also found is that monthly wages (i.e. earnings) did not increase either. This result is in line with Goux, Maurin, and Petrongolo (2011) who provide empirical evidence showing that the Aubry's Law did not have any effect on workers' monthly earnings. Therefore, the effect on health seems to be explained by the mandated reduction of working hours and not due to higher earnings.

7 Conclusion

The overall theoretical effect of reductions in working hours on health outcomes is ambiguous, and therefore empirical evidence is needed. Until now there has been no such empirical evidence. To our knowledge, we provide the first of such evidence. The identification of the health effects of reductions in working hours in a regression framework becomes complicated, as working hours may be endogenous due to the so-called healthy worker effect. This refers to the fact that workers with good mental and physical health are generally more likely to work longer hours than those with fair or bad health.

To overcome these caveats, we exploit exogenous reductions in working hours coming from labour regulation for two countries with two different levels of working hours (France and Portugal, with 35 and 40 weekly hours, respectively). To enhance comparability between these two countries, we use the European Community Household Panel dataset (ECHP) between 1994 and 2001. One of the advantages of the ECHP is that it is a homogeneous questionnaire that includes health as well as labour information for individuals. We find that the policy change has different effects by country, gender, and age range. In particular, we find no significant effects for Portugal and a significant one for France. In France, the effect is negative for men and positive for women, and in both cases the effect is significant only for younger individuals. Furthermore, and by keeping in mind institutional differences, for men (women) the results by country may imply that the relationship between hours and health may not be monotonic, as the country with a lower threshold of working hours (i.e., France) presents negative (positive) effects of the reduction in weekly hours, while the country with the higher threshold (i.e., Portugal) presents no significant effects (although further research on this should be done in the future).

These results may be explained by the trade-off between the psychological and promotions hypotheses, where the latter seems to have stronger effects than the former for young men in France, while the reverse is true for young women. This opposite result may be found because the promotion channel has weaker effects on young women, who already internalize the effect of pregnancy on their promotion pattern. From our results, we conclude that mandated reductions in standard working hours need to be more carefully applied with heterogeneous employees since some groups may be worse off. Finally, it could be the case that the relationship between the psychological and promotions hypotheses might behave differently when higher thresholds of working hours are investigated. That may be the case represented by Portugal, where no effects were found. However, as an extension to further test this latter hypothesis, researchers could analyze mandated reductions in standard working hours in countries with higher thresholds of working hours.

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Appendix

Figure 1: Distribution of Age (Portugal)

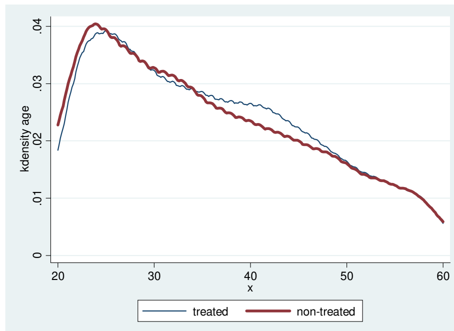

Figure 2: Distribution of Occupation (Portugal)

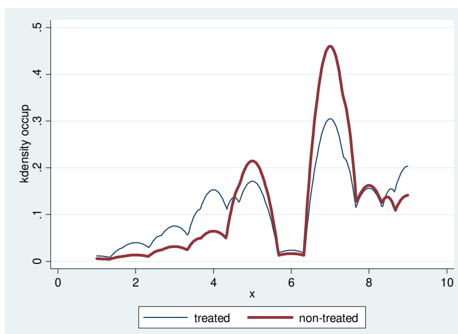

Figure 3: Distribution of Industry (Portugal)

Figure 4: Average weekly working hours in France 1994-2001

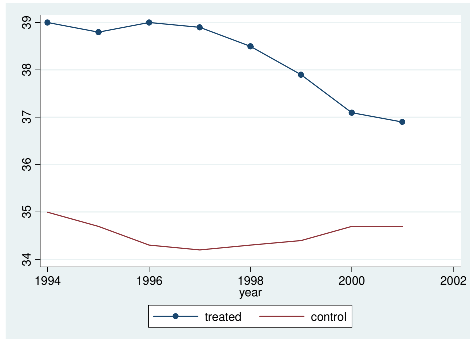

Figure 5: Average Self-Assessed Health in France 1994-2001

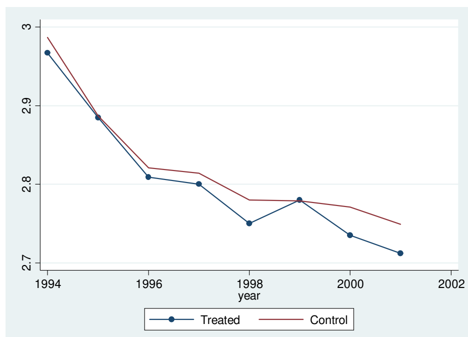

Figure 6: Average weekly working hours in Portugal 1994-2001

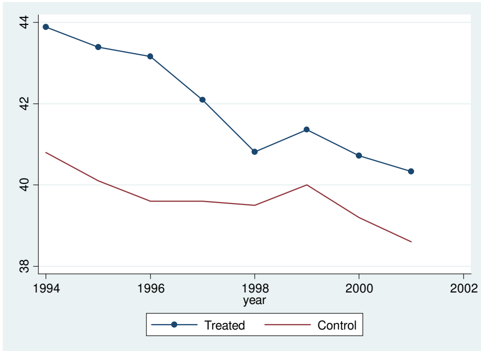

Figure 7: Average Self-Assessed Health in Portugal 1994-2001

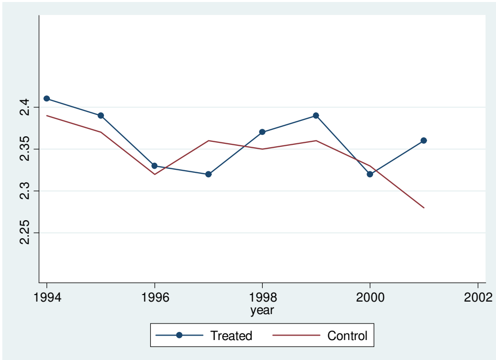

Table I

Summary Statistics for Portugal and France

	ECHP					
	Portugal			France		
	Control	Treated	Difference	Control	Treated	Difference
	(1)	(2)	(3)	(4)	(5)	(6)
SAH 1-4 (from Bad & Very Bad to Very Good)	2.33	2.33	0.00	2.71	2.81	-0.10
Weekly Hours	39.6	43.2	-3.60***	34.2	38.9	-4.70***
ln(Household income)	14.6	14.5	0.01	12.5	12.5	0.00
Age (between 20-60 years old)	36.9	34.7	2.20	39.2	38.6	0.65
Female (male=0, female=1)	0.42	0.44	-0.02	0.79	0.38	0.40***
Children <12	1.64	1.60	0.04	1.53	1.58	-0.05
Children <16	1.80	1.79	0.01	1.83	1.83	0.00
Job satisfaction 1 (Not at all satisfied)	0.01	0.01	0.00	0.03	0.02	0.01
Job satisfaction 2 (Largely unsatisfied)	0.04	0.05	-0.01	0.03	0.03	0.00
Job satisfaction 3 (Mildly unsatisfied)	0.20	0.24	-0.04	0.10	0.14	-0.04
Job satisfaction 4 (Mildly satisfied)	0.51	0.53	-0.03	0.36	0.33	0.03
Job satisfaction 5 (Largely satisfied)	0.20	0.15	0.05	0.41	0.43	-0.02
Job satisfaction 6 (Fully satisfied)	0.04	0.01	0.03	0.04	0.01	0.03
Educ 1 (3rd level = ISCED 5-7)	0.05	0.02	0.03	0.17	0.18	-0.01
Educ 2 (2nd stage = ISCED 3)	0.18	0.07	0.11***	0.41	0.45	-0.04
Educ 3 (Less than 2nd stage = ISCED 0-2)	0.77	0.91	-0.14***	0.42	0.37	0.05
Household size	3.81	3.89	-0.08	3.37	3.21	0.16
M. Status 1 (Married)	0.68	0.65	0.03	0.70	0.60	0.10***
M. Status 2 (separated, divorced & widowed)	0.06	0.05	0.01	0.07	0.08	-0.01
M. Status 3 (single)	0.26	0.30	-0.04	0.23	0.32	-0.09***
Type of Contract 1 (permanent employment)	0.78	0.85	-0.07***	0.80	0.86	-0.06
Type of Contract 2 (fixed-term or short term)	0.13	0.11	0.02	0.14	0.09	0.05
Type of Contract 3 (work with no contract)	0.04	0.02	0.02	0.03	0.02	0.01
Type of Contract 4 (other)	0.05	0.02	0.03	0.03	0.02	0.01

***p<1%,**p<5% and *p<10%

Table I(cont)

Summary Statistics for Portugal and France

	ECHP					
	Portugal			France		
	Control	Treated	Difference	Control	Treated	Difference
	(1)	(2)	(3)	(4)	(5)	(6)
Occup. 1 (Legislators/Senior Officers/Managers)	0.02	0.01	0.01	0.02	0.02	0.00
Occup. 2 (Professionals)	0.04	0.02	0.02***	0.05	0.02	0.03***
Occup. 3 (Technical and associate professionals)	0.09	0.03	0.06***	0.11	0.18	-0.07***
Occup. 4 (Clerks)	0.17	0.05	0.12***	0.30	0.18	0.12***
Occup. 5 (Service and shopping)	0.15	0.15	0.00	0.21	0.09	0.12***
Occup. 6 (Skilled agriculture and fisherman)	0.03	0.02	0.01	0.01	0.02	-0.01
Occup. 7 (Craft and related)	0.21	0.44	-0.23***	0.06	0.22	-0.16***
Occup. 8 (Operators and assemblers)	0.12	0.14	-0.02	0.09	0.21	-0.12***
Occup. 9 (Elementary occupations)	0.17	0.14	0.03	0.15	0.07	0.08***
Ind. 1 (Agric., hunting, forestry and fishing)	0.05	0.04	0.01	0.02	0.02	0.00
Ind. 2 (Mining and quarrying)	0.01	0.01	0.00	0.02	0.02	0.00
Ind. 3 (Electricity, gas and water)	0.03	0.01	0.02***	0.02	0.02	0.00
Ind. 4 (Manufacturing)	0.23	0.45	-0.22***	0.21	0.39	-0.18***
Ind. 5 (Construction)	0.14	0.11	0.03	0.02	0.10	-0.08***
Ind. 6 (wholesale and retail trade, etc)	0.12	0.18	-0.06***	0.20	0.19	0.01
Ind. 7 (Hotels and restaurants)	0.04	0.08	-0.04***	0.04	0.01	0.03**
Ind. 8 (Transport, storage and communication)	0.08	0.03	0.05***	0.03	0.02	0.01
Ind. 9 (Financial Intermediation)	0.02	0.01	0.01	0.04	0.04	0.00
Ind. 10 (R. state renting and business activities)	0.05	0.01	0.04***	0.09	0.09	0.00
Ind. 11 (Public admin. and defense)	0.00	0.00	0.00	0.00	0.00	0.00
Ind. 12 (Education)	0.10	0.01	0.09***	0.04	0.01	0.03***
Ind. 13 (Health and social work)	0.06	0.03	0.03***	0.13	0.06	0.07***
Ind. 14 (Other community and social activities)	0.07	0.02	0.05***	0.14	0.03	0.11***

***p<1%,**p<5% and *p<10%

Table II

Average weekly working hours by group before and after the policy change

France	**Before**	**After**
	(1)	(2)
Control Group	34.2	34.4
Treated Group	38.9	37.0
Portugal	**Before**	**After**
Control Group	39.6	39.5
Treated Group	43.2	40.8

Table III

Effects of working hours regulation on weekly working hours and hourly wages

France	**Hours**	**Ln(hourly wages)**
Control	0.2	-0.006
Treated	-1.8***	0.043***
Portugal	**Hours**	**Ln (hourly wages)**
Control	-0.1	0.005
Treated	-2.5***	0.049***

*p<10%, **p<5%, ***p<1%.

Table IV

Average partial effects in Portugal, by gender and age range, of the hours reduction in period t in individuals' self-assessed health in period t

	Men						Women					
	Age 20-36			Age 37-60			Age 20-36			Age 37-60		
Health	Very Good	Good	β^{RE}	Very Good	Good	β^{RE}	Very Good	Good	β^{RE}	Very Good	Good	β^{RE}
Year	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
1996	0.017	0.014	0.18	0.004	0.023	0.11	0.007	0.021	0.13	0.005	0.042	0.19
1997	-0.008	-0.010	-0.10	0.002	0.014	0.07	0.001	0.001	0.01	-0.004	-0.048	-0.21
1998	-0.021	-0.016	-0.21	0.001	0.007	0.03	-0.005	-0.019	-0.11	-0.003	-0.026	-0.11
1999	-0.008	-0.011	-0.10	0.005	0.031	0.15	-0.002	-0.007	-0.04	-0.001	-0.009	-0.04
2000	-0.016	-0.023	-0.20	-0.005	-0.042	-0.19	-0.001	-0.001	-0.01	-0.002	-0.025	-0.11
2001	-0.001	-0.002	-0.02	-0.006	-0.049	-0.22	-0.001	-0.005	-0.03	-0.004	-0.037	-0.16
Obs.		4,206			3,414			2,586			1,906	
ICC		0.72			0.88			0.83			0.85	

Cutoff points as well as the estimated coefficients of the rest of the covariates are not reported, but are available on request. ICC is the intraclass correlation which is equal to $\frac{\sigma_\alpha^2}{1+\sigma_\alpha^2}$. The estimation is carried out with clustered standard errors at the individual level. *p<10%, **p<5%, ***p<1%.

Table V

Average partial effects in Portugal, by gender and age range, of the hours reduction in period t in individuals' self-assessed health in period t+1

	Men						Women					
	Age 20-36			Age 37-60			Age 20-36			Age 37-60		
Health	Very Good	Good	β^{RE}	Very Good	Good	β^{RE}	Very Good	Good	β^{RE}	Very Good	Good	β^{RE}
Year	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
1996	0.017	0.016	0.19	0.001	0.001	0.01	0.011	0.033	0.20	0.010	0.072	0.34
1997	-0.008	-0.013	-0.11	0.002	0.010	0.05	0.003	0.012	0.07	-0.002	-0.020	-0.09
1998	-0.023	-0.019	-0.24	0.001	0.004	0.02	-0.040	-0.015	-0.08	-0.001	-0.004	-0.02
1999	-0.009	-0.013	-0.11	0.003	0.018	0.09	-0.001	-0.001	-0.01	-0.001	-0.001	-0.01
2000	-0.013	-0.021	-0.17	-0.008	-0.065	-0.30	-0.004	-0.017	-0.10	-0.003	-0.032	-0.15
2001	-0.008	-0.012	-0.10	-0.010	-0.078	-0.36	-0.002	-0.007	-0.04	-0.003	-0.030	-0.14
Obs.		3,639			3,159			2,098			1,733	
ICC		0.79			0.90			0.82			0.87	

Cutoff points as well as the estimated coefficients of the rest of the covariates are not reported, but are available on request. ICC is the intraclass correlation which is equal to $\frac{\sigma_\alpha^2}{1+\sigma_\alpha^2}$. The estimation is carried out with clustered standard errors at the individual level. *p<10%, **p<5%, ***p<1%.

Table VI

Average partial effects in France, by gender and age range, of the hours reduction in period t in individuals' self-assessed health in period t

	Men						Women					
	Age 20-38			Age 39-60			Age 20-38			Age 39-60		
Health	Very Good	Good	β^{RE}	Very Good	Good	β^{RE}	Very Good	Good	β^{RE}	Very Good	Good	β^{RE}
Year	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
1996	0.047	-0.003	0.22	0.093	0.052	0.61	0.043	0.005	0.24	0.014	0.019	0.13
1997	-0.030	-0.005	-0.16	0.022	0.023	0.18	0.073	0.001	0.39	0.011	0.012	0.10
1998	-0.131	-0.101	-0.93***	-0.009	-0.012	-0.08	0.153	-0.025	0.74**	0.016	0.017	0.14
1999	-0.110	-0.065	-0.72**	0.100	0.052	0.65	0.054	0.004	0.29	0.003	0.004	0.03
2000	-0.033	-0.060	-0.18	0.047	0.039	0.36	0.044	0.005	0.24	0.043	0.035	0.34
2001	-0.064	-0.020	-0.37	0.006	0.008	0.06	0.121	-0.012	0.61*	0.003	0.004	0.03
Obs.		3,876			4,585			2,214			2,204	
ICC		0.49			0.56			0.56			0.66	

Cutoff points as well as the estimated coefficients of the rest of the covariates are not reported, but are available on request. ICC is the intraclass correlation which is equal to $\frac{\sigma_\alpha^2}{1+\sigma_\alpha^2}$. The estimation is carried out with clustered standard errors at the individual level. *p<10%, **p<5%, ***p<1%.

Table VII

Test for endogenous attrition for Portugal and France

	Portugal		France	
	Men	Women	Men	Women
	(1)	(2)	(3)	(4)
Numwaves	0.020*	0.028*	-0.012	0.018
	(0.012)	(0.016)	(0.010)	(0.014)

Clustered standard errors are at the individual level. *p<10%, **p<5%, ***p<1%.

Table VIII

Coefficients of the random-effect ordered probit for Portugal in the unbalanced case with and without the attrition indicators

	Men		Women	
	No Indicator	With Indicator	No Indicator	With Indicator
	(1)	(2)	(3)	(4)
1996	0.13	0.13	0.15	0.16
1997	0.01	0.01	-0.08	-0.08
1998	0.18	0.17	-0.11	-0.11
1999	0.04	0.04	0.04	0.05
2000	-0.14	-0.14	-0.05	-0.05
2001	-0.05	-0.05	-0.09	-0.09
ICC	0.77	0.78	0.84	0.86
Log Likelihood	-5,263	-5,263	-3,261	-3,261
Obs.	7,620		4,492	

ICC is the intraclass correlation which is equal to $\frac{\sigma_\alpha^2}{1+\sigma_\alpha^2}$. Clustered standard errors are at the individual level. *p<10%, **p<5%, ***p<1%.

Table IX

Coefficients of the random-effect ordered probit for France in the unbalanced case with and without the attrition indicators

	Men		Women	
	No Indicator (1)	With Indicator (2)	No Indicator (3)	With Indicator (4)
1996	0.43	0.43	0.02	0.03
1997	0.01	0.01	0.14	0.14
1998	-0.48**	-0.48**	0.33	0.33
1999	0.03	0.03	0.03	0.03
2000	0.14	0.15	0.19	0.19
2001	-0.15	-0.15	0.12	0.12
ICC	0.54	0.54	0.58	0.58
Log Likelihood	-7,283	-7,283	-3,707	-3,707
Obs.	8,461		4,418	

ICC is the intraclass correlation which is equal to $\frac{\sigma_\alpha^2}{1+\sigma_\alpha^2}$. Clustered standard errors are at the individual level. *p<10%, **p<5%, ***p<1%.

Table X

Comparison of coefficients between the balanced and unbalanced panel for Portugal

	Balanced		Unbalanced	
	Men	Women	Men	Women
	(1)	(2)	(3)	(4)
1996	0.19	0.24	0.13	0.15
1997	0.08	-0.19	0.01	-0.08
1998	0.27	-0.06	0.18	-0.11
1999	0.11	0.08	0.04	0.04
2000	-0.26	-0.17	-0.14	-0.05
2001	-0.13	-0.04	-0.05	-0.09
ICC	0.70	0.78	0.77	0.84
Log Likelihood	-2,726	-1,403	-5,263	-3,261
Obs.	2,989	2,069	7,620	4,492

ICC is the intraclass correlation which is equal to $\frac{\sigma_\alpha^2}{1+\sigma_\alpha^2}$. Clustered standard errors are at the individual level. *p<10%, **p<5%, ***p<1%.

Table XI

Comparison of coefficients between the balanced and unbalanced panel for France

	Group 1			
	Balanced		Unbalanced	
	Men	Women	Men	Women
	(1)	(2)	(3)	(4)
1996	0.32	0.08	0.43	0.02
1997	-0.03	0.18	0.01	0.14
1998	-0.39**	0.36	-0.48**	0.33
1999	0.07	0.15	0.03	0.03
2000	0.19	0.18	0.15	0.19
2001	-0.18	0.21	-0.15	0.12
ICC	0.52	0.54	0.54	0.58
Log Likelihood	-2,711	-1,316	-7,283	-3,707
Obs.	3,289	1,645	8,461	4,418

ICC is the intraclass correlation, which is equal to $\frac{\sigma_\alpha^2}{1+\sigma_\alpha^2}$. Clustered standard errors are at the individual level. *p<10%, **p<5%, ***p<1%.

Table XII

Average partial effects in Portugal, by age group and gender, of the hours reduction in period t in individuals' self-assessed health in period t (control group redefined as 36-40).

	Men						Women					
	Age 20-36			Age 37-60			Age 20-36			Age 37-60		
Health	Very Good	Good	β^{RE}	Very Good	Good	β^{RE}	Very Good	Good	β^{RE}	Very Good	Good	β^{RE}
Year	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
1996	0.013	0.011	0.13	0.002	0.014	0.07	-0.001	-0.001	-0.01	0.002	0.021	0.09
1997	-0.014	-0.019	-0.17	0.001	0.010	0.05	0.014	0.019	0.19	-0.007	-0.076	-0.34
1998	-0.023	-0.021	-0.24	0.002	0.007	0.04	-0.013	-0.031	-0.23	-0.005	-0.053	-0.24
1999	-0.016	-0.024	-0.20	0.006	0.032	0.16	-0.003	-0.004	-0.05	-0.002	-0.020	-0.09
2000	-0.016	-0.024	-0.21	-0.006	-0.044	-0.20	-0.002	-0.004	-0.04	-0.005	-0.054	-0.24
2001	-0.002	-0.002	-0.02	-0.007	-0.055	-0.26	-0.002	-0.006	-0.03	-0.005	-0.054	-0.24
Obs.		4,244			3,491			2,631			2,010	
ICC		0.70			0.87			0.87			0.89	

Cutoff points, as well as the estimated coefficients of the rest of the covariates, are not reported but are available on request. ICC is the intraclass correlation which is equal to $\frac{\sigma_\alpha^2}{1+\sigma_\alpha^2}$. The estimation is carried out with clustered standard errors at the individual level. *p<10%, **p<5%, ***p<1%.

Table XIII

Average partial effects in France, by age group and gender, of the hours reduction in period t in individuals' self-assessed health in period t (control group redefined as age 31-35)

	Men						Women					
	Age 20-38			Age 39-60			Age 20-38			Age 39-60		
Health	Very Good	Good	β^{RE}	Very Good	Good	β^{RE}	Very Good	Good	β^{RE}	Very Good	Good	β^{RE}
Year	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
1996	0.019	-0.006	0.79	0.113	0.054	0.72	-0.009	-0.003	-0.06	0.033	0.056	0.35
1997	0.044	-0.002	0.21	0.003	0.004	0.03	0.087	-0.001	0.45	0.006	0.008	0.06
1998	-0.091	-0.043	-0.56	-0.002	-0.010	-0.07	0.130	-0.015	0.64*	0.029	0.027	0.24
1999	-0.085	-0.037	-0.51	0.091	0.050	0.64	0.046	0.005	0.25	0.015	0.016	0.13
2000	-0.016	-0.002	-0.09	-0.015	-0.022	-0.14	0.008	0.002	0.05	0.031	0.028	0.25
2001	-0.037	-0.007	-0.20	0.061	0.163	0.81**	0.160	-0.027	0.77*	0.012	0.013	0.10
Obs.		3,842			4,559			2,118			2,085	
ICC		0.49			0.56			0.52			0.67	

Cutoff points, as well as the estimated coefficients of the rest of the covariates, are not reported but are available on request. ICC is the intraclass correlation which is equal to $\frac{\sigma_\alpha^2}{1+\sigma_\alpha^2}$. The estimation is carried out with clustered standard errors at the individual level. *p<10%, **p<5%, ***p<1%.

Table XIV

Average partial effects in France, by gender and age range, of the hours reduction in period t in individuals' self-assessed health in period t. Model with a lagged dependent variable as a covariate.

	Men						Women					
	Age 20-38			Age 39-60			Age 20-38			Age 39-60		
Health	Very Good	Good	β^{RE}	Very Good	Good	β^{RE}	Very Good	Good	β^{RE}	Very Good	Good	β^{RE}
Year	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
1996	0.035	-0.001	0.16	0.096	0.058	0.62	0.048	0.006	0.26	-0.002	-0.003	-0.02
1997	-0.012	-0.002	-0.06	0.034	0.035	0.26	0.066	0.004	0.34	0.017	0.018	0.14
1998	-0.127	-0.113	-0.88***	-0.010	-0.016	-0.10	0.147	-0.019	0.69**	0.025	0.025	0.20
1999	-0.081	-0.039	-0.48	0.110	0.059	0.69	0.024	0.005	0.13	0.013	0.015	0.11
2000	-0.033	-0.008	-0.17	0.032	0.034	0.24	0.036	0.006	0.19	0.050	0.039	0.37
2001	-0.077	-0.036	-0.45	-0.009	-0.014	-0.08	0.112	-0.006	0.54	0.001	0.002	0.01
Obs.		3,419			4,357			1,931			2,071	
ICC		0.32			0.33			0.45			0.38	

Cutoff points, as well as the estimated coefficients of the rest of the covariates, are not reported but are available on request. ICC is the intraclass correlation which is equal to $\frac{\sigma_\alpha^2}{1+\sigma_\alpha^2}$. The estimation is carried out with clustered standard errors at the individual level. *p<10%, **p<5%, ***p<1%.