The Effect of Working Hours on Health*

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Abstract

Does working time affect workers' health behavior and health? We study this question in the context of a French reform that reduced the standard workweek from 39 to 35 hours, at constant earnings. Our empirical analysis exploits arguably exogenous variation in the reduction of working time across employers due to the reform. We find that the shorter workweek reduced smoking by six percentage points, corresponding to 16 percent of the baseline mean. The reform also appears to have lowered BMI and increased self-reported health, but these effects are imprecisely estimated in the overall sample. A heterogeneity analysis provides suggestive evidence that while the impact on smoking was concentrated among blue-collar workers, body mass index decreased only among white-collar workers. These results suggest that policies which reduce working time could potentially lead to important health benefits.

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1. Introduction

Does working time affect workers' health? Data from employee surveys suggest so: for example, in a recent study of European workers, the share of respondents who stated that their work negatively affects their health rose monotonically from 19% for those working less than 30 hours per week to 30% for those working at least 40 hours per week.[1] Perceived negative health impacts from work also motivated the change to a 6-hour workday, at constant earnings, by some Swedish employers, a decision that received extensive international media coverage.[2] From a theoretical point of view, working time may affect health because of potential direct impacts on the job, such as physically strenuous work leading to exhaustion, or because of potential indirect impacts due to the effects of working hours on income and the time available for health production at home.

Empirical studies of the effect of working time on health face two fundamental challenges. First, working hours are not randomly assigned, introducing bias into any naive regression estimate of the impact of hours. This bias may be due to omitted unobserved factors that influence both hours and health, or due to reverse causality, whereby health affects hours rather than the other way around. Second, estimates of the impact of working time are usually confounded by the influence of hours on income, which has an important independent effect on health (e.g. Frijters, Haisken-DeNew, and Shields, 2005; Lindahl, 2005). Both for determining the importance of working time as an input into health production and from a policy perspective, however, the effect of working hours on health keeping income constant is particularly relevant.

In this paper, we study the impact of working hours on health behavior and health in the context of a French workweek reform which allows us to

[1]These figures are for EU-27 respondents in the 2015 European Survey of Working Conditions. Shares of respondents who perceived negative health impacts from their work were: 19% (respondents working <30 hours per week), 26% (30-34 hours per week), 28% (35-39 hours per week), and 30% (40+ hours per week).

[2]For example, the switch to a 6-hour workday by a Gothenburg retirement home in 2015 was covered in *The New York Times*, *The Guardian*, and *Die Zeit*, among many other media outlets. Other Swedish employers who reduced or plan to reduce weekly working time at constant earnings include a Toyota production plant, several technology start-ups, and the municipal administration of Malmö, Sweden's third largest city.

address both of these challenges. Introduced by the socialist government in 1998, the reform reduced the standard workweek from 39 to 35 hours, at constant earnings. Importantly, the laws mandating this reduction included different deadlines for implementation for firms of different sizes, which led to substantial employer-level variation in working time in subsequent years. These policy-driven, exogenous changes in working time, together with the absence of income effects, make the French context uniquely suited to study the impact of working hours on health.

Our empirical analysis draws on data from a longitudinal health survey, which allows us to follow a sample of male workers from the pre-reform to the post-reform period, namely from 1998 to 2002. For each worker, we observe whether his employer had implemented the shorter workweek by the year 2002, and we use this information to create our binary treatment variable. Our main outcome variables are self-reported measures of smoking behavior, body mass index (BMI), and health status. Notably, smoking and high BMI are among the leading preventable causes of death, and both outcomes have been widely studied in the medical literature on the impacts of working time, yielding mixed results (e.g. Lallukka et al., 2008).

We first estimate the impacts of the workweek reform in a difference-in-differences framework, comparing the evolution of health outcomes of workers in treated and control firms. Our regressions control for individual fixed effects and assume that working in a treated firm in 2002 is orthogonal to changes in other determinants of health between 1998 and 2002. As a complementary strategy, we also present results from lagged dependent variable models, which directly exploit the longitudinal dimension of the data. These regressions instead rely on the assumption that conditional on pre-reform health and controls, the treatment is as good as randomly assigned. Finally, we also run difference-in-differences and lagged dependent variable regressions in which we instrument actual hours worked with our treatment variable; under the additional assumption that the workweek reform affected health only via its impact on working time, these specifications identify the causal effect of working hours on health.

The results show that the reform reduced smoking among treated workers by six percentage points, corresponding to 16 percent of the baseline mean. Under the exclusion restriction mentioned in the previous paragraph, this translates to an increase in smoking by 1.6-2.4 percentage points per additional hour of work in our instrumental variable estimates. The results further suggest that the reform slightly lowered treated workers' BMI and improved their self-reported health, but these effects are imprecisely estimated in the overall sample. Finally, a heterogeneity analysis provides suggestive evidence that while the impact on smoking is concentrated among blue-collar workers, the reform significantly lowered BMI only among white-collar workers.

A potential concern with our results is that they might be due to selection of healthier workers into treated firms or that they are otherwise confounded by unobserved differences between treated and non-treated workers. We address this issue in two main ways. First, we show that our estimates are similar when we concentrate on a matched sample of workers with comparable socio-demographic and job characteristics. Second, using the method developed by Oster (2017), we show that selection based on unobserved factors would need to be at least eight times as large as selection based on observed control variables to explain away the impact on smoking. The results from these sensitivity checks thus support a causal interpretation of our estimates.

The paper proceeds as follows. The next section reviews the related literature and discusses the contribution of our study. Section 3 describes the institutional background. Section 4 presents the data and Section 5 outlines our empirical strategy. Our main results are discussed in Section 6, with robustness analyses presented in Section 7. Section 8 concludes.

2. Related literature and contribution

Our paper is related to four different strands of literature in economics and medicine. First, there is a large body of medical research on the health impacts of working time. This research has mostly found negative associations between working hours, especially overtime, and health outcomes such as cardiovascular disease, diabetes, and indicators of mental health (e.g. Sparks and Cooper, 1997; van der Hulst, 2003; Kivimäki et al., 2015). In one of the few experimental studies in this literature, Åkerstedt et al. (2001) found that a reduction in working hours at constant earnings improved sleep quality, mental fatigue, and heart and respiratory symptoms among female health care and day care workers in Sweden. Other studies have also looked at associations between working time and health behaviors, such as smoking, and obesity, and generated more mixed results (e.g. Lallukka et al., 2008; Angrave, Charlwood, and Wooden, 2014). However, the bulk of this research has failed to adequately address the empirical challenges described above.

Second, a recent and growing literature in economics seeks to overcome these challenges to estimate causal effects of working hours on health behavior and health. Cygan-Rehm and Wunder (2018) exploit variation in working time in the range of 38.5 to 42 hours due to changes in statutory workweek regulations in the German public sector. They find that hours reduce self-assessed health and raise the number of visits to the doctor, but they do not find any statistically significant impacts on smoking and BMI. In contrast, Ahn (2016) uses the reduction of the standard workweek from 44 to 40 hours in South Korea and shows that working time increases smoking and reduces physical exercise. Our paper complements these studies by providing evidence from a different country (France) and at a different margin (35 to 39 hours) for both private and public sector workers.

Within this same strand of literature, two further studies exploit the same workweek reduction in France as we do to study the impact of working time on health. Costa-Font and de Miera Juarez (2018) use data from a single large company and show that consistent with our results, the reform increased BMI among blue-collar workers. We complement their work by using a more representative dataset and by also studying other health-related outcomes. Moreover, Sánchez (2017) uses panel data and a random-effects specification and finds that the workweek reduction decreased young males' self-reported health. Unlike our difference-in-differences fixed-effects models, these estimates cannot account for unobserved individual heterogeneity, and it appears that they might be partly driven by pre-existing trends in the sample (see Figure 5 in Sánchez (2017)).

Third, our work connects to research in economics that examines the health impacts of job displacement (e.g. Sullivan and von Wachter, 2009; Marcus, 2014; Black, Devereux, and Salvanes, 2015; Schaller and Stevens, 2015), retirement (e.g. Coe and Zamarro, 2011), and recessions (e.g. Ruhm, 2000, 2005). Whereas those papers estimate the combined effects of reduced hours and everything else changing with these shocks, we focus more specifically on the impact of working time.

Finally, our paper relates to a body of research examining the non-health impacts of the French workweek reform. Estevão and Sá (2008) and Chemin and Wasmer (2009) show that the reform did not affect overall employment. Goux, Maurin, and Petrongolo (2014) exploit the reform to study interdependencies in spousal labor supply and document that husbands of treated women reduced their working hours, consistent with leisure complementarity. Lepinteur (2019) shows that the reform increased job and leisure satisfaction among affected workers.[3] Moreover, Saffer and Lamiraud (2012) find that the hours reduction did not lead to an increase in time spent on social interaction.

3. Institutional background

Until the late 1990s, the standard workweek in France was set at 39 hours, with a legal maximum of 130 overtime hours per year and a 25% overtime wage premium. This situation changed considerably in 1998, when the newly elected left-wing government launched the reform that provides the backdrop for our study. The coalition of socialists and several smaller parties had campaigned on a program of reducing unemployment via work-sharing; in particular, the standard workweek was to be shortened from 39 to 35 hours, at constant earnings. Once in government, the coalition implemented this reduction via two distinct laws, known as Aubry I and Aubry II after the then Minister of Labor Martine Aubry. We now describe the provisions of these laws which are relevant for our analysis.[4]

Aubry I was passed in June 1998 and set the standard workweek at 35 hours in the private sector, with deadlines for implementation in January 2000 for large firms with more than 20 employees and in January 2002 for smaller firms. The reduction in hours was to be achieved through bargained agreements between employers and employee representatives at the firm level. Employers' incentives to sign such 35-hours agreements

[3] In related work, Hamermesh, Kawaguchi, and Lee (2017) show that life satisfaction improved in Korea and Japan after an exogenous reduction in the standard workweek.

[4] This section draws heavily on Estevão and Sá (2008), Askenazy (2013), and Goux, Maurin, and Petrongolo (2014).

were threefold. First, after the relevant deadline, hours worked beyond the thirty-fifth hour were subject to the overtime wage premium, increasing labor costs. Second, the law introduced generous payroll tax cuts for firms which implemented the shorter workweek before these deadlines. Third, the negotiated agreements could allow for more flexible work schedules, the possibility of which had been very limited until then. Importantly, because workers should not bear the full costs of the reform, Aubry I required all agreements to keep the earnings of minimum-wage workers constant. In practice, previous studies have found near-zero effects of the reform on earnings also for higher-wage workers (Estevão and Sá, 2008; Goux, Maurin, and Petrongolo, 2014), a result that we further corroborate in the empirical analysis below.

Aubry II was passed in January 2000 and amended some of the rules regarding the implementation of the 35-hour workweek. Thus, it introduced a transitional period with reduced overtime payments for small firms, allowing them to employ workers for 39 hours per week at almost no additional cost until 2005. The law also made it possible to achieve some nominal reduction in hours by simply re-defining working time to exclude 'unproductive breaks' (Askenazy, 2013).[5] Moreover, firms could now implement the shorter hours on an annual basis, with a cap of 1,600 hours per worker and year. Finally, both Aubry I and Aubry II included special provisions for managers and other professionals with 'genuine autonomy' in their work: depending on their rank, these workers either could sign agreements restricting the number of days (but not hours) worked, or even were fully exempt from the new working time regulations.

In the general elections of June 2002, the conservative parties came back to power and almost immediately started to remove the incentives for employers to sign 35-hours agreements, meaning that the implementation of the reform was discontinued in practice. By that time, however, many firms had already switched to the shorter workweek. As could be expected, this group disproportionately included large firms, which faced the earlier deadline for implementation (see Estevão and Sá, 2008). But it also en-

[5]For example, supermarkets started excluding the usual three-minute breaks per hour for cashiers from calculations of paid work, see Askenazy (2013).

compassed the majority of public sector institutions, which reduced their employees' working time even though they were not formally bound by the Aubry laws. Taken together, the different deadlines for implementation and the abrupt discontinuation of the reform led to substantial employer-level variation in working time in the year 2002. Below, we exploit this variation to estimate the impact of working hours on health.

4. Data

We draw on data from the Enquête sur la Santé et la Protection Sociale (ESPS), a longitudinal survey of health, health insurance, and health care utilization. Around the time of the workweek reduction, the survey followed a representative sample of individuals in Metropolitan France, who were interviewed every four years. An important feature of ESPS is that it allows us to identify which workers were actually affected by the reform. In particular, the 2002 wave of the survey asked respondents whether the 35-hours workweek had been implemented by their current employer, and we construct our treatment variable based on the answers to this question. In the remainder of this section, we summarize our data construction and measurement, with many more details provided in the Data Appendix.

Our analysis uses individual-level data from the 1998 and 2002 waves of ESPS. Specifically, we focus on the subsample of employees interviewed in both 2002, when information on treatment was collected, and 1998, giving us one pre- and one post-treatment observation per individual.[6] In the main regressions, we moreover concentrate on workers whose hours were in all likelihood reduced if treated: we select all male individuals aged 18-61 and working more than 35 hours in 1998 (but any number of hours in 2002), but exclude managers who either were not covered by the Aubry laws or were subject to a different treatment. In Section 6, we also present results for a wider sample that includes part-time workers, managers, and women.

[6]Due to sample attrition and sample refreshments, not all individuals surveyed in 1998 were also surveyed in 2002 and vice versa. Unfortunately, the sampling method of the survey changed in 1998, such that only a small and unrepresentative subsample of 27% of workers is observed also in 1994. We therefore decided not to use the data from this earlier wave. We argue that 1998 belongs to the pre-treatment period as only very few employers signed a 35-hours agreement before 1999, see Figure 1 in Goux, Maurin, and Petrongolo (2014).

For reasons discussed there, the first-stage effect of the reform on hours is substantially smaller in this sample, and correspondingly the impacts on health outcomes are more muted but qualitatively similar.

We focus on three health-related outcomes based on self-reported data. First, we study effects on an indicator for smoking. The effect of working time on smoking has been widely studied in the medical literature and has yielded mixed results (e.g. Lallukka et al., 2008; Angrave, Charlwood, and Wooden, 2014). The proposed mechanism tying hours to smoking in these studies is usually job-related stress. Second, we examine impacts on BMI. Working time may influence BMI either directly via altering the amount of calories burned on the job, or indirectly via changing diets or the time spent on physical exercise. Our main specifications focus on continuously measured BMI, with alternative regressions using dummies for being overweight (BMI>25) or obese (BMI>30) instead. Third, we use information on self-reported health, which can be affected by working time via a large number of physical and psychological channels. Self-reported health is measured on a scale from 0-10 in the survey. For ease of interpretation, our main specifications focus on a dummy for being in good health, defined as health status 9 or 10. In alternative regressions, we also use other measures of health constructed from this scale as outcomes.[7]

The treatment variable in our regressions is an indicator for working for an employer who had implemented the 35-hours workweek. While the exact dates that these hours reductions were carried out are not observed in the data, Goux, Maurin, and Petrongolo (2014) show that only very few firms switched to the shorter hours before the year 2000. Thus, the treatment captures exposure to the 35-hours workweek for at most 2–3 years.

Table 1 reports means and standard deviations of key variables in 1998

[7]ESPS also asks respondents which health conditions they are currently suffering from, with answers coded according to the International Classification of Diseases. Unfortunately, due to the small sample size, estimates of the impact of the shortened workweek on even broad groups of diseases were always very imprecise and thus little informative. This motivates our focus on smoking, BMI, and self-reported health, which have relatively high incidence or variation in the sample, see Table 1. Furthermore, while the 2002 wave of ESPS contains information on other health behaviors such as frequency of drinking and exercising, the lack of data for 1998 means that we cannot use these behaviors as outcomes in our analysis.

separately for the 588 treated and 156 control workers in the main analysis sample. While the two groups appear similar regarding age, marital status, and household income, treated workers tend to have higher levels of education. Interestingly, treated workers also work fewer hours on average already before the introduction of the 35-hours week, and they are more likely to be employed in the public sector. In contrast, there are no statistically significant differences in terms of smoking, body mass index, and self-reported health between the two groups.[8] Below, we explain in detail how our regressions account for these observable as well as for unobservable differences between treated and control workers.

5. Empirical strategy

Two fundamental challenges arise when trying to estimate the effect of working hours on health. First, working time is not randomly assigned, introducing bias into any naive regression estimate of the impact of hours. Second, even if working time were randomly assigned, the estimate would still be confounded by the usual impact of hours on income, which has an important independent effect on health (e.g. Frijters, Haisken-DeNew, and Shields, 2005; Lindahl, 2005). The French workweek reform allows us to address both of these challenges. In particular, it generated policy-driven, employer-level variation in working time that was arguably exogenous from an individual worker's perspective. Moreover, since income was unaffected by the reform, the hours effect can be disentangled from the income effect under some additional assumptions set out below.

Our first identification strategy leverages these features in a regression framework similar to the difference-in-differences model used by Goux, Maurin, and Petrongolo (2014). We estimate:

$$Y_{it} = \alpha_{\mathbf{i}} + \beta_1 Post_t + \beta_2 Treated_i * Post_t + \varepsilon_{it}, \tag{1}$$

where Y_{it} is a health-related outcome for individual i at time t, $\alpha_{\mathbf{i}}$ is a

[8]To further explore differences between treated and control workers, we also ran a regression of treatment status on the variables included in Table 1 and conducted a test for their joint significance. In line with the group differences observed in the table, the p value from this test was below 0.001.

vector of individual fixed effects, $Post_t$ is an indicator taking value 1 for $t = 2002$ and value 0 for $t = 1998$, and $Treated_i$ is an indicator for whether i's employer in 2002 adopted the 35-hours workweek. Note that unlike a classical difference-in-differences model based on cross-sectional data, the specification in Equation 1 exploits the longitudinal nature of our data to include individual fixed effects. Importantly, controlling for individual fixed effects does not change the point estimates compared to a classical difference-in-differences model, but it slightly improves their precision.

Equation 1 is a difference-in-differences specification with two groups and two periods. Under the assumption that differences in health between treated and untreated individuals would have been stable in absence of the workweek reform ("parallel trends"), it identifies the causal effect of adopting the 35-hours workweek. A drawback of having only a single pre-treatment period is that we cannot provide evidence in support of this assumption, for example by showing that trends in health for the two groups were parallel before the reform. Moreover, unlike in a classical difference-in-differences setting, we do not observe treatment status before the reform. This opens up the possibility that the workers in our sample switched between treated and control firms between 1998 and 2002. In Section 7 below, we provide evidence that such switching is not driving our effects.

To lend additional credibility to our results, we also present estimates of the following lagged dependent variable specification:

$$Y_{i,2002} = \gamma_1 Treated_i + \gamma_2 Y_{i,1998} + \mathbf{X}'_{i,1998}\gamma_3 + \varepsilon_{i,2002}, \tag{2}$$

where all variables are defined as above and $\mathbf{X}_{i,1998}$ is a vector of individual-level control variables measured in 1998, which includes all the socio-demographic and job characteristics shown in Table 1. Unlike the regression in equation 1, which accounts for selection into treatment based on fixed group and worker characteristics, the specification in equation 2 relies on the (arguably stronger) assumption of unconfoundedness given past outcomes and controls for identification. Thus, the two specifications are not nested, and we can gain some confidence in a causal interpretation of our results if they yield similar estimates (see Angrist and Pischke, 2008).

The regression models considered so far aim at identifying the overall, reduced-form effect of the workweek reform on workers' health. As described in Section 3, the Aubry laws mainly mandated a shortening of the standard workweek from 39 to 35 hours, but also introduced some other changes such as flexible work schedules. Under the assumption that the reform influenced health only via its effect on working time, we can use the treatment variable as an instrument for hours to provide a direct estimate of the impact of working hours on health. Accordingly, Section 6 below presents estimates from both the reduced-form specifications in equations 1 and 2 and the corresponding instrumental variable regressions.

Throughout the paper, we report estimates from regressions which weight observations using the sampling weights provided by ESPS, although in practice this makes little difference. Furthermore, in order to maximize sample size, we always show results for the full set of workers observed with a particular outcome. In a robustness check, we show that estimates are very similar if we instead restrict the sample to workers who are observed with all outcomes.

Finally, we note that from the description of the workweek reform in Section 3, one could devise at least two alternative identification strategies which are not used here. First, one may want to directly exploit variation in firm size in conjunction with the different deadlines for small and large firms. Unfortunately, this strategy is not feasible here because the ESPS data do not contain any information on firms. Second, one may be tempted to use part-time workers as an alternative control group. However, Oliveira and Ulrich (2002) show that part-time workers in treated firms actually *increased* their hours slightly in response to the reform, a result which we confirmed in our data. Thus, part-time workers were also affected by the reform, rendering them a bad control group.[9] In contrast, we present results from two complementary specifications which rely on distinct (untestable) assumptions for identification. Comparing the estimates from these models allows us to assess the robustness of our results.

[9] Similarly, managers are unlikely to be a valid control group, as they were also partly affected by the reform. Moreover, because the Aubry laws were vague on who actually could be considered a manager, it is impossible to cleanly identify this group in the data.

6. Results

6.1. Effects of the reform on hours and income

Figure 1 shows the distributions of hours in 1998 and 2002 separately for the treatment and control groups. In both groups, the distribution peaks at 39 hours in 1998, with about half the workers reporting this amount of weekly working time. In the treatment group, this peak shifts to 35 hours in 2002, whereas the mode stays at 39 hours in the control group, pointing to a strong negative impact of the reform on working time.

Column 1 of Table 2 quantifies this first-stage effect. Panel A reports an estimate of a 2.5-hour decrease for treated workers based on equation 1, and panel B shows a corresponding estimate of 3.4 hours based on equation 2. The two regressions thus yield roughly similar results; however, both estimates fall short of the nominal 4-hour reduction in the standard workweek. Potential reasons for this difference include re-definitions of working time, implementation of the shorter hours at the annual rather than weekly level (see Section 3), or simply an increased use of overtime work by employers who implemented the 35-hours workweek.[10]

Column 2 of Table 2 reports estimates of the effect of the reform on monthly household income, which is a rough proxy for individual earnings. In line with the findings from previous studies of the French workweek reduction (Estevão and Sá, 2008; Goux, Maurin, and Petrongolo, 2014), the results indicate an economically and statistically insignificant effect of the shorter workweek on income. Overall, the estimates in Table 2 thus confirm the expected impacts of the reform: it reduced weekly working hours at constant earnings.

6.2. Effects of the reform on smoking, BMI, and self-reported health

Table 3 presents estimates of the effects of the reform on smoking, BMI, and self-reported health. Considering first the effect on smoking, column 1 shows that working for a treated firm leads to a six percentage point decrease in smoking, independently of the identification strategy used. This

[10] Previous studies have also found that workers who were affected by the reform reduced their labor supply by less than 4 hours; see Estevão and Sá (2008), Saffer and Lamiraud (2012), and Goux, Maurin, and Petrongolo (2014).

corresponds to a reduction of 16 percent of the baseline mean. Columns 2 and 3 show impacts on smoking separately for individuals who did versus did not smoke in 1998 and reveal that the negative effect in the overall sample is driven primarily by quitting among baseline smokers, rather than non-initiation among baseline non-smokers.[11]

Turning to BMI, column 4 reports a small negative impact of the workweek reform on continuous BMI, which is however imprecisely estimated. Alternative specifications which instead use indicators for being overweight or obese as outcomes similarly yield small point estimates that are not statistically significant at conventional levels, see Appendix Table 1.

Finally, column 5 of Table 3 reports estimates of the impact on self-reported health. The results suggest that the shorter hours raise the likelihood of being in good health by 2-3 percentage points, although this effect is imprecisely estimated. Using dummies for good health based on different cutoff values gives qualitatively similar results, see Appendix Table 1. To further investigate this potential impact, Figure 2 shows marginal effects from an ordered logit model based on the lagged dependent variable specification in which the outcome is the original self-reported health scale from 0-10.[12] The results indicate that working for a treated firm significantly increases the probability of reporting the highest two levels of health while reducing the likelihood of reporting medium levels of health, thus corroborating the results in Table 3 and Appendix Table 1.

6.3. Instrumental variable estimates

The estimates so far identify the effect of the workweek reform on workers' health. In contrast, Table 4 presents results from instrumental variable regressions, which identify pure hours effects under the assumption that the reform affected health only via its impact on working time. This is a relatively strong assumption since the reform also led to other changes, such as allowing for more flexible work schedules, which themselves might have affected health. We nevertheless think that the instrumental variable esti-

[11]Table 3 reports estimates for smoking based on linear probability models. Results from logit specifications are qualitatively similar and are available on request.

[12]We use the lagged dependent variable specification because the interpretation of nonlinear difference-in-differences estimates is not trivial, see Lechner (2011).

mates are interesting because they address a key question of interest in the literature, namely how working hours affect health.

The results in Table 4 suggest that each additional hour of weekly work leads to a 1.6-2.4 percentage point increase in smoking (column 1), which is mainly driven by non-quitting (columns 2 and 3). They further suggest that working time might raise BMI (column 4) and lower self-reported health (column 5), but these effects are small and imprecisely estimated. Taken together, these findings point toward a negative impact of working hours on health behavior and health.

6.4. Results for different groups of workers

In Table 5, we separate workers into blue-collar and white-collar occupations and report estimates of the effects of the reform on hours and health for each of the two groups. Even though both types of workers experience the same reduction in hours, there appear to be important differences in the impacts on health behavior and health. In particular, whereas treatment decreases smoking by 8-10 percentage points (19-24 percent of the baseline mean) for blue-collar workers, the estimated effect for white-collar workers is close to zero and not statistically significant at conventional levels. In contrast, BMI decreases by 0.4-0.5 (1.7-2.1 percent of the baseline mean) among white-collar workers but, if anything, increases among blue-collar workers. Note, however, that these estimates are based on small samples, and that the observed differences are not always statistically significant.[13] Because of these limitations, these results should be interpreted as suggestive.

The analysis so far focuses on workers whose hours are in all likelihood reduced if treated. Specifically, the sample excludes individuals who worked less than 35 hours already before the reform and managers, who were subject to a different treatment. Moreover, we found that women in the control group are more likely to switch to part-time work, potentially because a 39-hour workweek is harder to combine with caring for children than the reduced 35-hour workweek (see Berniell and Bietenbeck (2017) for

[13] We tested the hypothesis that treatment effects are equal for blue-collar and white-collar workers using the difference-in-differences specification. The resulting p values were: 0.207 (smoking), 0.074 (BMI), and 0.087 (good health).

details). As a result, the differential reduction of hours for treated women is quite small, which is why we also exclude them from the main sample. For completeness, Appendix Table 2 shows results for the unrestricted sample including part-time workers, managers, and women. As would be expected from the discussion above, the first-stage effect on hours is smaller in this sample. Correspondingly, the impacts of the reform on health outcomes are also more muted, but qualitatively similar to our main results.

6.5. Comparison with previous studies and discussion of mechanisms

We now compare our results to the existing literature and discuss potential mechanisms. Focusing first on smoking, Ahn (2016) shows that a one-hour increase in weekly working time raises smoking among South Korean men by 1.3 percentage points. In contrast, Cygan-Rehm and Wunder (2018) find no statistically significant effect on smoking among public sector workers in Germany. However, the upper bound of their 95 percent confidence interval corresponds to a rise of 2.3 percentage points. Thus, our instrumental variable estimate of a 1.6-2.4 percentage point increase per additional hour worked is broadly in line with previous economic research on the impacts of working time. As for the mechanism behind this effect, the medical literature has highlighted stress as a likely channel: to the extent that longer working hours increase stress, workers might want to seek relieve via smoking (Angrave, Charlwood, and Wooden, 2014; Lallukka et al., 2008). While our data do not allow us to measure impacts on stress directly, this channel appears plausible also in our context.[14]

Turning to BMI, Costa-Font and de Miera Juarez (2018) find that the French workweek reform increased BMI by 0.17 among blue-collar workers. This is similar to our difference-in-differences estimate of 0.14 shown in Panel A of Table 5. While Costa-Font and de Miera Juarez (2018) do not find any effect for white-collar workers, their estimate is also not statisti-

[14]Our estimates are not immediately comparable to those from most medical research, which typically studies the effect of working overtime rather than the marginal effect of working one more hour. We also note that while our point estimates are consistent with the previous literature in economics, the incidence of smoking differs across settings: 60 percent of men smoke in the sample of Ahn (2016) and 31 percent of public sector workers smoke in the sample of Cygan-Rehm and Wunder (2018), compared to 36 percent in our sample.

cally different from the negative impact that we document. A potential explanation for this pattern of results is that treated white-collar workers use part of the additional free time to exercise, thus lowering their BMI. Instead, blue-collar workers likely burn more calories on the job, and they fail make up for the decrease in work-related physical activity due to the shorter workweek, thus increasing their BMI.[15]

Finally, Cygan-Rehm and Wunder (2018) find that one additional hour of work reduces self-assessed health among male public sector workers by 0.7 percent of the mean, although this effect is imprecisely estimated. Our corresponding instrumental variable estimates show that the likelihood to be in good health decreases by (a statistically insignificant) 0.8-0.9 percentage points, or 1.5-1.6 percent of the baseline mean, which is broadly similar. A variety of channels could explain this potential effect of hours on self-reported health. First, if work itself is strenuous and detrimental to health, the shorter workweek will mechanically reduce job strain and raise health. Second, the shorter hours could improve health via reducing time pressure outside of the workplace (Cygan-Rehm and Wunder, 2018). Third, an improvement in self-reported health could reflect an increase in leisure time spent on health-promoting activities. Fourth, the improvement could capture improved mental health due to spending more time with the partner (Goux, Maurin, and Petrongolo, 2014), or improvements in life satisfaction (Lepinteur, 2019).

7. Robustness

7.1. Differences between treated and control firms

As described in detail in Section 2, firms of different sizes were incentivized to implement the 35-hours workweek at different points of time. Therefore, the bulk of the variation in treatment status observed in 2002 is likely coming from differences in firm size (see also Estevão and Sá (2008), who directly exploit differences in firm size for identification). One might

[15]Cygan-Rehm and Wunder (2018) do not find an impact of working hours on BMI. However, the 95 percent confidence interval around their estimate includes our instrumental variable estimate of a 0.04 rise per hour worked. Ahn (2016) does not study impacts on BMI, but shows that in line with our explanation, working hours reduce physical exercise.

nevertheless be concerned that employers who did versus did not operate on a 35-hour schedule differ in ways related to workers' health, and that these differences are not constant over time (and thus not accounted for by the difference-in-differences models) and not fully captured by observable differences in baseline health (which are accounted for by the lagged dependent variable models). Here, we present two pieces of evidence that this is not the case.

First, we show results for a matched sample of workers with comparable socio-demographic and job characteristics. Intuitively, if workers are very similar on these characteristics, they are less likely to be on differential trends in health-related variables; put differently, the parallel trends assumption underlying our difference-in-differences specifications is more likely to be fulfilled. Therefore, following the suggestion by Crump et al. (2009), we estimated workers' propensity to be treated using a logit regression, and restricted the sample to individuals with estimated propensity scores between 0.1 and 0.9.[16] As Appendix Table 3 shows, workers in this sample appear much more similar in terms of their socio-demographic and job characteristics compared to the unrestricted sample. Importantly, the difference-in-differences estimates for the matched sample, which are shown in Appendix Table 4, are similar to the ones reported above. This suggests that differential trends are not driving the improvements in health.

Second, we address the specific concern that employers who operate on a 35-hour schedule might be disproportionately located in areas where the local economy is trending upwards, a trend that itself might be related to improvements in health. To rule this explanation out, we estimate specifications which control for the regional unemployment rate as a proxy for economic activity. The results from these regressions, which are shown in Appendix Table 5, are again very similar to those reported above. Overall, there is thus no evidence that endogenous implementation of the shorter workweek is driving our results.

[16]The characteristics used to predict treatment are the ones used in the lagged dependent variable specifications.

7.2. Judging the importance of selection on unobservables

A general worry might be that our results are driven by selection of firms and workers into treatment based on unobserved characteristics. In this subsection, we ask how large such selection would need to be to explain away our main effects. Our analysis builds on the methodology presented in Altonji, Elder, and Taber (2005) and recently refined by Oster (2017), which relies on comparing the coefficient of interest and the R-squared between regressions with and without control variables to gain insights into the importance of omitted variable bias. Here, we focus on the calculation of δ, which is the ratio of the impact of unobservables to the impact of observable controls that would drive the coefficient on the treatment variable to zero. As a point of reference, Oster (2017) suggests that effects for which $\delta > 1$ can be considered robust.

Table 6 shows the results from our analysis. We concentrate on the lagged dependent variable specification, which explicitly relies on the assumption that selection effects can be captured by observable control variables, and present estimates only for smoking, for which we find statistically significant effects in the overall sample. The estimates show that in a regression of smoking on the treatment dummy, adding controls reduces the coefficient in absolute value from -0.073 to -0.057, while increasing the R-squared from 0.004 to 0.596. The corresponding δ indicates that selection on unobservables would have to be eight times as large as the selection on observed controls to make the effect in column 2 go to zero, a value well beyond the threshold of one. These results strongly suggests that omitted variable bias is not driving our results.

8. Conclusion

In this paper, we study whether working time causally affects workers' health, a question that is important both for learning about the health production function and for informing labor market policy. To overcome problems of non-random assignment of hours and confounding income effects, our empirical analysis exploits a French reform that shortened the standard workweek from 39 to 35 hours, at constant earnings. Our difference-in-differences and lagged dependent variable models use variation in the adoption of this shorter workweek across employers that is arguably exogenous from an individual worker's perspective.

Our estimates show that working time negatively affects health behaviors and health: four years after the reform was initiated, treated workers who saw their hours reduced were 6 percentage points less likely to smoke, corresponding to a reduction of 16 percent of the pre-reform mean. The reform also appears to have lowered BMI and increased self-reported health, but these effects are imprecisely estimated in the overall sample. A heterogeneity analysis suggests that the impact on smoking was concentrated among blue-collar workers, whereas BMI decreased only among white-collar workers. All these results are very similar across our different identification strategies, and they survive a series of robustness checks which address potential concerns about time-varying differences between treated and control workers as well as sorting of workers across firms. This consistency across specifications makes us confident that our estimates reflect causal effects.

Our results add to a growing literature in economics that documents negative impacts of working hours on health behavior and health. An implication of these results is that policies which reduce working time, such as shortening the statutory workweek, could potentially lead to important health benefits. At the same time, such policies would most likely be costly: for example, output per worker would probably decrease and firms would have to hire additional workers to make up for this shortfall. These additional costs will have to be weighed against the benefits of improved health behavior and health in any policy decision regarding working time.

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Data Appendix

Merging the 1998 and 2002 waves of ESPS

The empirical analysis is based on the 1998 and 2002 waves of the Enquête sur la Santé et la Protection Sociale (ESPS). The survey draws a random sample of individuals from an administrative database of the three main public health insurance funds in France. The selected individuals, who are referred to as “assurés principaux” (APs, “main insured”), as well as all members of their households are then interviewed for the survey. APs interviewed in 1998 were contacted again to participate in the 2002 wave of ESPS, and also in that wave, the current (i.e. 2002) members of their households were asked to participate. As usual, there was some attrition such that not all APs surveyed in 1998 are observed also in 2002; moreover, the sample was refreshed with some individuals not surveyed in the earlier years. The resulting sample is representative of 95% of the households in Metropolitan France. In our analysis, we weight observations using the sampling weights provided with the 1998 data.[17]

The data contain unique household identifiers that are consistent across all waves of ESPS. Moreover, there is an indicator for whether an individual is an AP. Together, these variables let us uniquely identify APs across the two waves of our sample. In order to identify non-AP household members across the two waves, we matched individuals on their relationship to the AP (partner, child, father or mother, brother or sister), gender, and age within households, keeping only unique matches. In principal, these matches could still be "false positives," e.g. when the AP changes partner between 1998 and 2002 and the new partner has the same gender and age as the old partner. To get a sense of the magnitude of this problem, we exploited the fact that in 1994 and 1998 (but not in 2002), the first five letters of individuals' first names are available in the data. In our final sample of males used in the empirical analysis, only two out of the 220 individuals who are observed also in 1994 did not have the same first name in 1994 and 1998 (and results are robust to excluding them from the sample).[18] This suggests that our within-household matching procedure works very well.

Construction of variables

The data contain information on individuals' age, gender, and education. For the latter variable, we collapse the available six categories into three education levels: lower secondary or less, upper secondary, and tertiary. We also use information on household size and household income. The latter is only available as a categorical variable, with different intervals in 1998 and 2002. For our analysis, we construct a continuous variable by imputing household income at the midpoint of each interval and converting the values to 1998 euros.[19] Finally, we use information on the region of

[17]Results are qualitatively and quantitatively very similar if no sampling weights are used. For detailed information on ESPS sampling procedures, questionnaires, etc. (in French), see the ESPS website: http://www.irdes.fr/recherche/enquetes/esps-enquete-sur-la-sante-et-la-protection-sociale/questionnaires.html.

[18]We allowed for some differences in the spelling of names; for example, we would not count "JJacq" (which likely stands for Jean-Jacques) and "Jean-" as different names.

[19]The highest income intervals in 1998 and 2002 are not bounded from above. In our newly-constructed variable, we set household income to missing for these intervals.

residence (eight different regions) of the respondent.

We construct our hours variable from the answers to the question "Combien d'heures travaille-t-elle par semaine hors trajet?," which translates as "How many hours do you work per week, not counting commuting time?" We discard the top 1% of values, corresponding to working more than 70 hours, as many of these values are likely misreported (e.g., some individuals report working 160 hours per week).

Regarding occupation type, the data contain information on whether an employee works in the public or private sector as well as information about her occupation from two questions. The first of these questions asks employees about their perceived occupation type, with possible answers "unskilled worker / specialized worker," "qualified worker," "employee," "technician, foreman," and "engineer, professional" ("cadre" in French). The second question asks about employees' profession, with answers coded into 19 different categories. As described in the main text, managers and high-level professionals were subject to special rules under the Aubry laws and are therefore excluded from our analysis. Unfortunately, the laws were not very specific regarding the definition of these managers. In our analysis, we consider employees with the following profession to be managers or high-level professionals: artists, traders, business and executive managers, and liberal and intellectual professionals.[20] We experimented with a host of alternative definitions of managers and found that our results were robust to using any of them (details are available upon request). Finally, we considered employees with perceived occupation "unskilled worker / specialized worker" or "qualified worker" as blue-collar workers, and all other employees as white-collar workers. Again, we experimented with using alternative definitions and found that our results were robust to this.

Our three main outcome variables are an indicator for whether an individual is a current smoker, self-reported health on a scale from 0 to 10, and body mass index (BMI). For the latter variable, we exclude extreme values above 65 which are likely misreported (a BMI of 65 corresponds, for

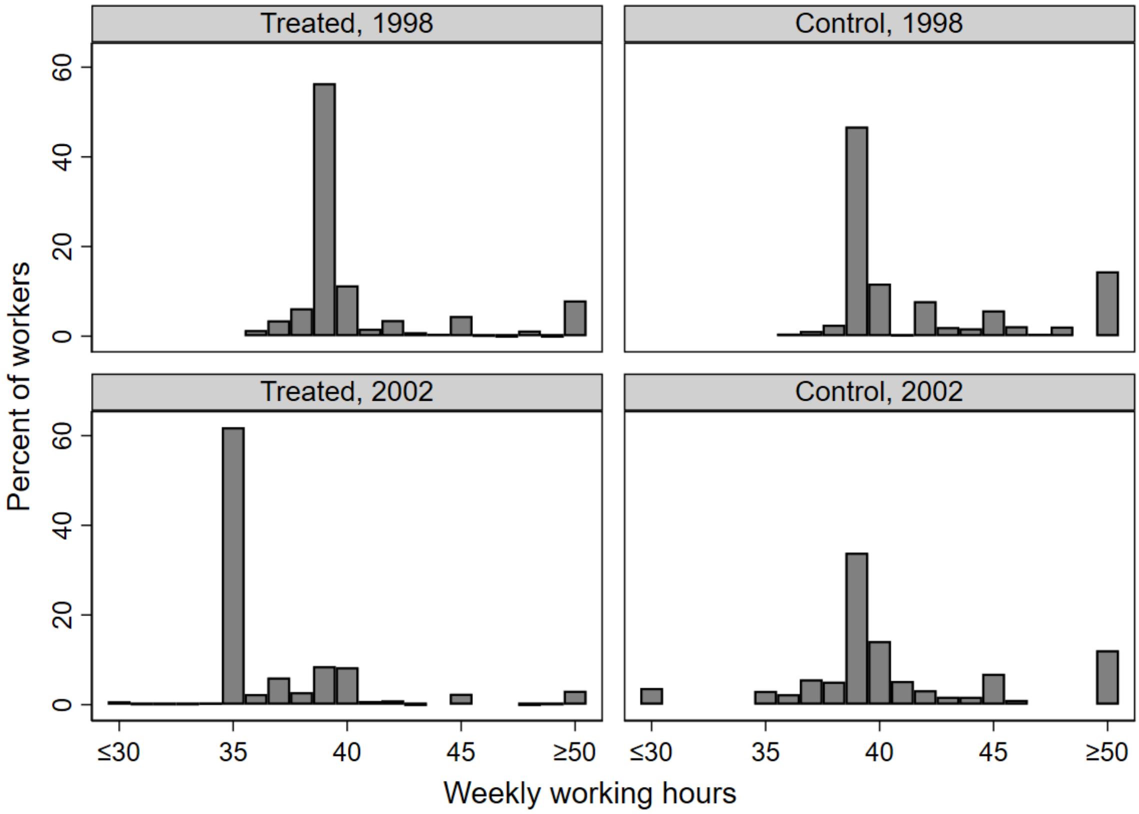
[20]In French, the categories are: "artisan," "commerçant et assimilé," "chef d'entreprise de 10 salariés et plus," "profession libérale," "profession intellectuelle, artiste, cadre fonction publique," and "cadre d'entreprise."

example, to a person measuring 175cm and weighing 200kg).

Sample restrictions

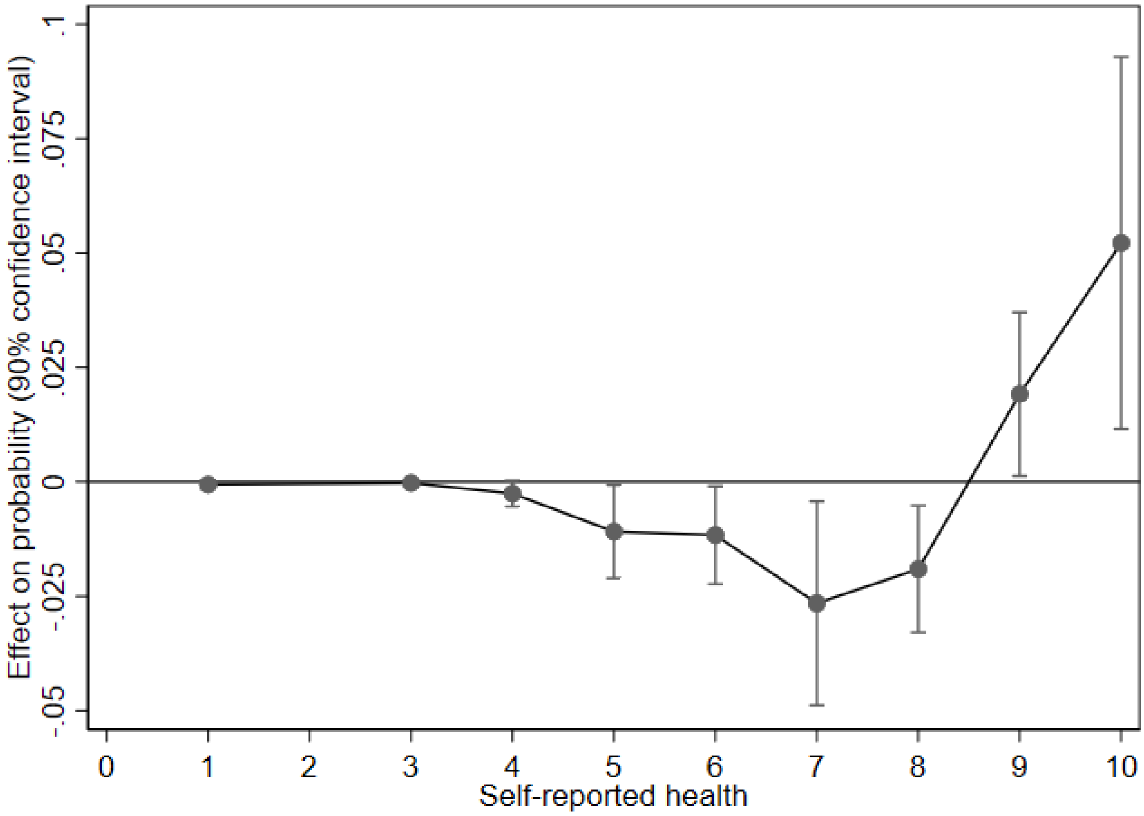
As described in the main text, we focus on a sample of male workers who are aged 18-61 in 1998 and who are employed in both 1998 and 2002. We drop individuals without information on treatment status or on the health-related outcomes used in our analysis. We further drop individuals working less than 35 hours in 1998 as well as managers and professionals, who received special treatment under the Aubry laws.

Figure 1
Weekly working hours by treatment status and year



Notes: The figure shows the distribution of weekly working hours separately for workers in treated and control firms in 1998 and 2002.

Figure 2
Marginal effects on self-reported health based on ordered logit estimates



Notes: The figure shows estimated marginal effects of working for an employer who implemented the 35-hour workweek on self-reported health, measured on a scale from 0 to 10. The results are based on an ordered logit regression which follows the lagged dependent variable specification, see the notes to Table 3 for details.

Table 1
Means and standard deviations in 1998 by treatment status

	Treated	Control	Difference [p-value]
Socio-demographic characteristics			
Age	38.16	37.23	0.93
	(8.21)	(8.32)	[0.21]
Education			
Lower secondary	0.66	0.79	-0.13
	(0.47)	(0.41)	[<0.01]
Upper secondary	0.17	0.12	0.05
	(0.38)	(0.33)	[0.14]
Tertiary	0.17	0.09	0.08
	(0.37)	(0.29)	[0.02]
Married	0.84	0.87	-0.02
	(0.36)	(0.34)	[0.45]
Household size	3.32	3.52	-0.19
	(1.31)	(1.31)	[0.10]
Household income	2033	1932	101.35
	(790)	(763)	[0.16]
Job characteristics			
Hours	40.76	42.45	-1.69
	(4.62)	(5.97)	[<0.01]
Blue collar	0.44	0.64	-0.19
	(0.50)	(0.48)	[<0.01]
Public sector	0.21	0.15	0.06
	(0.41)	(0.35)	[0.08]
Health-related outcomes			
Current smoker	0.36	0.37	-0.02
	(0.48)	(0.48)	[0.71]
Body mass index	24.81	25.18	-0.31
	(3.17)	(4.03)	[0.33]
Good health	0.54	0.55	-0.01
	(0.50)	(0.50)	[0.90]
No. of workers	588	156	

Notes: The table reports means and standard deviations (in parentheses) of key variables separately for the 588 treated and the 156 control workers in the sample. Household income measures monthly income in euros. Good health is an indicator for reporting health status greater than 8 on a scale from 0-10. For further details regarding all variables used in the empirical analysis, see the Data Appendix.

Table 2
Effects on hours and household income

	Hours	Household income
	(1)	(2)
Panel A: difference-in-differences estimates		
Treated × post	–2.516***	–22.333
	(0.516)	(75.355)
No. of workers	744	613
Panel B: lagged dependent variable estimates		
Treated	–3.439***	–4.947
	(0.506)	(71.063)
No. of workers	744	613

Notes: The table reports estimates of the effect of working for an employer who implemented the 35-hours workweek on working hours and household income. Specifications in panel A control for individual fixed effects and a dummy for post. Specifications in panel B control for the dependent variable measured in 1998 as well as for age, age squared, education, marital status, household size, five occupation-type dummies, eleven profession dummies, a public-sector dummy, and eight region dummies, all measured in 1998. Standard errors in parentheses are clustered at the individual level. * p<0.10, ** p<0.05, *** p<0.01.

Table 3

Effects on smoking, BMI, and self-reported health

	Current smoker			BMI	Good health
	All workers	1998=yes	1998=no		
	(1)	(2)	(3)	(4)	(5)
Panel A: difference-in-differences estimates					
Treated × post	–0.059**	–0.115**	–0.033	–0.106	0.021
	(0.029)	(0.045)	(0.036)	(0.154)	(0.052)
No. of workers	734	265	469	725	705
Panel B: lagged dependent variable estimates					
Treated	–0.056*	–0.103**	–0.023	–0.155	0.030
	(0.029)	(0.048)	(0.038)	(0.154)	(0.046)
No. of workers	734	265	469	725	705

Notes: The table reports estimates of the effect of working for an employer who implemented the 35-hours workweek on smoking behavior, BMI, and self-reported health. Specifications in panel A control for individual fixed effects and a dummy for post. Specifications in panel B control for the dependent variable measured in 1998 as well as for age, age squared, education, marital status, household size, working hours, five occupation-type dummies, eleven profession dummies, a public-sector dummy, and eight region dummies, all measured in 1998. Standard errors in parentheses are clustered at the individual level. * $p<0.10$, ** $p<0.05$, *** $p<0.01$.

Table 4
Instrumental variable estimates

	Current smoker			BMI	Good health
	All workers	1998=yes	1998=no		
	(1)	(2)	(3)	(4)	(5)
Panel A: difference-in-differences instrumental variable estimates					
Hours	0.024*	0.031**	0.018	0.041	–0.008
	(0.013)	(0.013)	(0.021)	(0.061)	(0.020)
First-stage F	23.2	25.1	6.9	23.8	23.2
No. of workers	734	265	469	725	705
Panel B: lagged dependent variable instrumental variable estimates					
Hours	0.016*	0.027**	0.007	0.045	–0.009
	(0.009)	(0.014)	(0.012)	(0.045)	(0.014)
First-stage F	45.6	20.2	24.5	44.0	42.8
No. of workers	734	265	469	725	705

Notes: The table reports instrumental variable estimates of the effect of working hours on smoking behavior, BMI, and self-reported health. Specifications follow those in Table 3, with the difference that the treatment dummy is used as an instrumental variable for actual hours worked. Standard errors in parentheses are clustered at the individual level. * $p<0.10$, ** $p<0.05$, *** $p<0.01$.

Table 5
Heterogeneity by occupation type

	Hours	Household income	Current smoker	BMI	Good health
	(1)	(2)	(3)	(4)	(5)
Panel A: blue-collar workers, difference-in-differences estimates					
Treated × post	–2.632***	–90.226	–0.097***	0.144	0.096
	(0.638)	(102.101)	(0.035)	(0.209)	(0.070)
No. of workers	370	305	365	360	350
Panel B: blue-collar workers, lagged dependent variable estimates					
Treated	–3.691***	–118.310	–0.079**	0.030	0.092
	(0.592)	(100.003)	(0.035)	(0.213)	(0.060)
No. of workers	370	305	365	360	350
Panel C: white-collar workers, difference-in-differences estimates					
Treated × post	–2.469***	14.150	–0.015	–0.421*	–0.085
	(0.869)	(101.866)	(0.055)	(0.237)	(0.079)
No. of workers	374	308	369	365	355
Panel D: white-collar workers, lagged dependent variable estimates					
Treated	–3.385***	45.662	–0.004	–0.531**	–0.053
	(0.896)	(100.885)	(0.052)	(0.239)	(0.076)
No. of workers	374	308	369	365	355
Means of dependent variable in 1998					
Blue-collar workers	40.74	1878	0.41	25.03	0.52
White-collar workers	41.46	2133	0.30	24.85	0.56

Notes: The table reports estimates of the effect of working for an employer who implemented the 35-hours workweek on working hours, household income, smoking behavior, BMI, and self-reported health, separately for workers in blue-collar occupations and workers in white-collar occupations in 1998. For information on the categorization of occupations into these two groups, see the Data Appendix. For further details on the specifications, see the notes to Tables 2 and 3. Standard errors in parentheses are clustered at the individual level. * $p<0.10$, ** $p<0.05$, *** $p<0.01$.

Table 6
Judging the importance of selection on unobservables

	Current smoker	
	no controls	with controls
	(1)	(2)
Treated	–0.073	–0.056*
	(0.047)	(0.029)
No. of workers	734	734
R^2	0.004	0.596
δ		7.869

Notes: Estimates based on the lagged dependent variable specification. Column 1 reports estimates from a regression of smoking behavior on the treatment dummy without further controls. Column 2 adds controls as in panel B of Table 3. The final row shows the amount of selection on unobservables that is necessary, relative to the amount of selection on observable controls, to explain away the coefficient in the respective column. For the calculation of this δ, we use the Stata command `-psacalc-`. Following the recommendation in Oster (2017), we set $Rmax$ to 1.3 times the R^2 in the respective column. For further details, see text and Oster (2017). * p<0.10, ** p<0.05, *** p<0.01.

Appendix Table 1
Effects on alternative outcome variables

	Dependent variable is a dummy for...			
	BMI greater than		self-reported health greater than	
	25	30	7	9
	(1)	(2)	(3)	(4)
Panel A: difference-in-differences estimates				
Treated × post	–0.005	0.025	0.018	0.048
	(0.038)	(0.016)	(0.043)	(0.048)
No. of workers	725	725	705	705
Panel B: lagged dependent variable estimates				
Treated	–0.015	0.013	0.043	0.035
	(0.036)	(0.018)	(0.039)	(0.039)
No. of workers	725	725	705	705
Mean of dep. var.	0.43	0.07	0.81	0.28

Notes: The table reports estimates of the effect of working for an employer who implemented the 35-hours workweek on working indicators for having a BMI higher an 25 or 30 (columns 1 and 2) and indicators for having self-reported health higher than 7 or 9 (columns 3 and 4). Means of these dependent variables in 1998 are reported in the last row of the table. For details on the specifications, see the notes to Tables 2 and 3. Standard errors in parentheses are clustered at the individual level. * $p<0.10$, ** $p<0.05$, *** $p<0.01$.

Appendix Table 2

Effects for the unrestricted sample of workers

	Hours	Household income	Current smoker	BMI	Good health
	(1)	(2)	(3)	(4)	(5)
Panel A: difference-in-differences estimates					
Treated × post	–1.924***	–26.305	–0.026	0.006	0.037
	(0.585)	(51.271)	(0.019)	(0.093)	(0.030)
No. of workers	2,033	1,518	2,011	1,979	1,926
Panel B: lagged dependent variable estimates					
Treated	–1.414***	3.076	–0.018	0.022	0.018
	(0.485)	(47.071)	(0.018)	(0.099)	(0.027)
No. of workers	2,033	1,518	2,011	1,979	1,926

Notes: The table reports estimates of the effect of workplace implementation of the 35-hours workweek on working hours, household income, smoking behavior, BMI, and self-reported health. Compared to the regressions in Tables 2 and 3, the samples in this table also include part-time workers, managers, and women. See the notes to Tables 2 and 3 for controls included in each regression. Standard errors in parentheses are clustered at the individual level. * $p<0.10$, ** $p<0.05$, *** $p<0.01$.

Appendix Table 3

Means and standard deviations in 1998 by treatment status (matched sample)

	Treated	Control	Difference [p-value]
Socio-demographic characteristics			
Age	37.64	37.03	0.61
	(7.88)	(8.29)	[0.42]
Education			
Lower secondary	0.75	0.80	-0.05
	(0.44)	(0.40)	[0.18]
Upper secondary	0.13	0.11	0.03
	(0.34)	(0.31)	[0.38]
Tertiary	0.12	0.09	0.03
	(0.32)	(0.29)	[0.38]
Married	0.86	0.87	0
	(0.34)	(0.34)	[0.92]
Household size	3.46	3.54	-0.08
	(1.29)	(1.33)	[0.50]
Household income	1967	1900	67.22
	(753)	(747)	[0.36]
Job characteristics			
Hours	40.79	42.24	-1.45
	(4.63)	(5.24)	[<0.01]
Blue collar	0.56	0.68	-0.11
	(0.50)	(0.47)	[0.01]
Public sector	0.19	0.13	0.05
	(0.39)	(0.34)	[0.14]
Health-related outcomes			
Current smoker	0.38	0.37	0.01
	(0.49)	(0.48)	[0.80]
Body mass index	24.80	25.27	-0.39
	(3.10)	(4.10)	[0.24]
Good health	0.53	0.55	-0.02
	(0.50)	(0.50)	[0.57]
No. of workers	464	148	

Notes: For details on the variables, see the Notes to Table 1 and the Data Appendix. For details on the construction of the matched sample, see text.

Appendix Table 4
Regression results for the matched sample

	Hours	Household income	Current smoker	BMI	Good health
	(1)	(2)	(3)	(4)	(5)
Panel A: difference-in-differences estimates					
Treated × post	–2.470***	–38.769	–0.080**	–0.084	0.027
	(0.548)	(78.274)	(0.031)	(0.163)	(0.054)
No. of workers	612	500	604	594	578
Panel B: lagged dependent variable estimates					
Treated	–3.387***	4.788	–0.069**	–0.136	0.005
	(0.490)	(73.382)	(0.030)	(0.157)	(0.047)
No. of workers	612	493	604	594	578

Notes: The table reports estimates of the effect of working for an employer who implemented the 35-hours workweek on working hours, household income, smoking behavior, BMI, and self-reported health for the matched sample. For details on the specifications, see the notes to Tables 2 and 3. For details on the construction of the matched sample, see text. Standard errors in parentheses are clustered at the individual level. * p<0.10, ** p<0.05, *** p<0.01.

Appendix Table 5
Controlling for regional unemployment

	Hours	Household income	Current smoker	BMI	Good health
	(1)	(2)	(3)	(4)	(5)
Panel A: difference-in-differences estimates					
Treated × post	–2.515***	–25.746	–0.058**	–0.105	0.021
	(0.516)	(75.223)	(0.029)	(0.155)	(0.052)
No. of workers	744	613	734	725	705
Panel B: lagged dependent variable estimates					
Treated	–3.439***	–4.973	–0.056*	–0.154	0.030
	(0.506)	(71.134)	(0.029)	(0.154)	(0.046)
No. of workers	744	613	734	725	705

Notes: The table reports estimates of the effect of working for an employer who implemented the 35-hours workweek on working hours, smoking behavior, BMI, and self-reported health for the matched sample. The specifications follow the regressions in Tables 2 and 3, but additionally control for unemployment at the level of eight NUTS 1 regions. Standard errors in parentheses are clustered at the individual level. * p<0.10, ** p<0.05, *** p<0.01.