The 35-hour week

SUMMARY

Workweek reduction laws may be beneficial if market interactions do not fully take into account the preferences reflected in declining secular trends in working hours. The most recent law in France shortened the workweek from 39 to 35 hours in 2000 for large firms, and in 2002 for small firms. Analysing differences between large and small firm employees before and after the law, we find that aggregate employment was unaffected but labour turnover increased, as firms shed workers who became more expensive. Survey responses indicate that the welfare impact of the law was different across groups of workers: women but not men may have benefited from coordination to a shorter workweek, and there is also evidence of negative welfare effects for managers, possibly due to the law's administrative burden.

— Marcello Estevão and Filipa Sá

The 35-hour workweek in France: Straightjacket or welfare improvement?

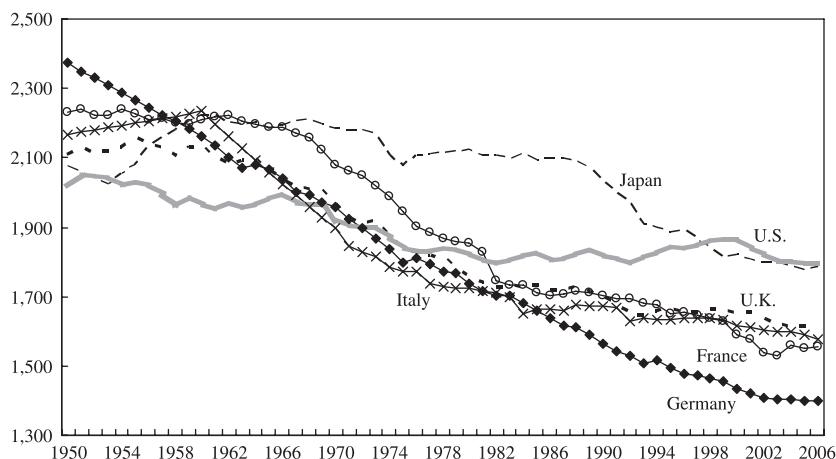Marcello Estevão and Filipa Sá

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1. INTRODUCTION

Since the industrial revolution, the length of the workweek has taken a central place in labour relations. In the nineteenth century, wage earners usually worked 12–16 hours per day, 6–7 days per week; a situation that began changing as the labour movement grew in force and organization. Indeed, in many countries across the world Labour Day is celebrated on 1 May, the starting date of a strike in 1886 Chicago in favour of an 8-hour working day, when several workers were killed and wounded. The evolution of working time since those early days has been driven by disputes on how to split productivity increases into profits, wages and leisure time,

Figure 1. Annual hours worked per person employed

Source: Groningen Growth and Development Centre and the Conference Board, Total Economy Database, November 2007, http://www.conference-board.org/economics/database.cfm

with legislation playing a significant role. Now, average working time across industrialized economies varies between 30 and 40 hours per week. In several countries, strict laws determine the length of standard workweeks (i.e. the length of time remunerated at the 'normal' wage rate) and overtime premium, while in others there is broader space for negotiation between employers and employees on these dimensions.

Even though the secular trend of reduction in working time is broad based, the last 50 years have seen large changes in working time across industrial economies. For instance, hours of work have declined steadily in Europe compared with the United States and Japan since the 1970s (Figure 1). While in the early-1950s annual hours per person employed in continental Europe were significantly larger than in the United States, they are now much smaller, with the average American working 300 hours more per year than the average European. The reasons for this gap in working hours have been a topic of recent discussion, with some studies attributing it to differences in tax rates and others highlighting differences in preferences for work and leisure or in labour market regulations and unionization between the two continents.[1]

One dimension behind cross-country differences in the evolution of working time has been the legislated cuts in the standard workweek enacted in several European countries. Although these policy actions can represent preferences for shorter working hours, they have often been presented as measures to fight high unemployment rates in the continent. The recent experience in France is a case in point. The socialist coalition government of Lionel Jospin (1997–2002) announced in June 1998 the reduction of the standard workweek from 39 to 35 hours as its flagship programme to reduce an unemployment rate around 12 per cent. The policy recognized possible pressures on

[1] See, among other studies, Blanchard (2004), Prescott (2004), and Alesina *et al.* (2005).

firms' profitability (and the strong opposition of business leaders) and included measures to alleviate its costs. In addition, the new workweek would be phased in slowly, with large firms adopting it in February 2000 and smaller firms doing so only in January 2002. The combination of a mandated reduction in the workweek with measures to alleviate its possible deleterious cost effects, make the French experience quite unique, raising the hopes that this law could indeed have produced a boost to employment.

Seven years later the unemployment rate in France remained relatively high, which forced contenders of the 2007 presidential campaign into a hot debate about the merits of the 35-hour workweek law. The socialist candidate, Ségolène Royal, criticized the law because she thought that employers had too much power and flexibility in implementing the shorter workweek. On the other hand, the victorious centre-right candidate, Nicolas Sarkozy, called the 35-hour workweek 'the worst mistake France has ever made'. During the campaign, he promised to relax the policy by exempting all overtime from payroll charges and income tax in order to encourage workers to work longer hours and increase their income. He believes that the short workweek has hurt France's competitiveness, making it less attractive to foreign investors.

The purpose of this paper is to analyse the effect of the 35-hour workweek on employment and workers' welfare – a broader mandate than previous research on workweek reductions – and to distil general lessons for future policy changes. After presenting detailed aspects of the 35-hour workweek law in Section 2, Section 3 discusses possible effects of legislated reductions in working hours on employment and welfare. Section 4 goes over previous evaluations of workweek reductions in France and elsewhere in the world. Section 5 explains the research strategy used to evaluate the effects of the 35-hour workweek and presents our empirical estimates. Section 6 summarizes the results as well as the policy lessons we derive from them.

2. THE 35-HOUR WORKWEEK IN FRANCE: BACKGROUND

Historically, working-time reduction laws have had two main objectives: freeing time for non-work activities, thus improving workers' health and overall well-being, and sharing available work more equitably. While the first motivation has played an important role in the secular downward trend in the workweek, the latter has been the main motivation for recent regulations. In the late 1970s, as economic activity slowed and unemployment rose in France, the public debate about working-time reduction as a way to restore full employment intensified. As a result, François Mitterrand's socialist government reduced the length of the workweek from 40 to 39 hours in 1982, without loss in workers' pay, and introduced a fifth week of paid vacation. The government had the intention to deepen the workweek reduction to 35 hours by 1985 but unfavourable economic conditions forced a change in policy to restore business confidence, maintain competitiveness, and halt capital flight.[2]

[2] Hayden (2007) describes in more detail the political developments surrounding the debate on working-time reduction in France in the 1980s.

The debate about workweek reductions remained alive in France since the experiment of the early 1980s. In 1986, the socialist party issued the Taddei Report, which focused on enhancing flexibility of scheduled working hours to reduce cost pressures from workweek reductions. Also, the public debate incorporated the notion that workweek reductions could lower unemployment, thus freeing public resources previously directed to unemployment insurance and some training programmes, which could then be used to offset part of the increase in production costs. These ideas were first implemented in 1993 by a centre-right government in a law providing financial incentives to working-time reductions. The same political coalition expanded these incentives in the 1996 Robien law, which lowered payroll contributions for firms that reduced working-time and increased employment by at least 10 per cent.

In June 1998, Lionel Jospin's socialist coalition passed a law reducing the workweek to 35 hours (named after Martine Aubry, the Labour Minister, as Aubry I). The law set the length of the workweek at 35 hours beginning in February 2000 for firms employing more than 20 people and in January 2002 for smaller firms. In January 2000 a second law (Aubry II) introduced more detailed legal provisions regarding the implementation of the reduced workweek. Employees were expected to bear only a small part of the cost of the working-time reduction, continuing to earn roughly the same monthly income – in line with the unions' slogan '35 hours pays 39'. However, explicit legislation keeping workers' monthly income unchanged was introduced only for individuals earning the hourly minimum wage ('salaire minimum interprofessionnel de croissance' or SMIC).

The lessons from the Mitterrand experience and the focus on limiting the negative cost effects of workweek reductions were clear in the way the 35-hour workweek laws were designed. First, the laws gave small firms more time to reduce the workweek as it was understood that it would be more difficult for them to put into practice the necessary changes to implement a shorter workweek. To ease that transition, the law reduced the overtime premium for small firms and increased their annual limit on overtime work compared with large firms. This way, small firms could continue operating on a 39-hour basis paying the difference with a reduced overtime premium. Second, since the purpose of the workweek reduction was to create more jobs during a period of high unemployment, and in accordance to the increased view in France that flexibility in hours and financial incentives were necessary to curb cost increases, the government offered social security rebates to firms signing 35-hour workweek contracts with the unions. The rebates declined with an employee's monthly income and were largest for workers receiving the minimum wage. Unlike the financial incentives of the Robien Law, the rebates did not depend on a firm actually hiring more employees. Third, unions accepted a more flexible accounting of overtime work from a weekly to an annual basis and productivity increases were expected from the shorter workweek and changes in the organization of production. French authorities expected that productivity increases together with cuts in social security contributions might even lead to a reduction in labour costs, so that firms would not need to cut monthly wages for the policy to be sustainable.

Another noteworthy feature of these laws is the treatment given to managers. Recognizing the autonomy of their work, the Aubry laws gave them more flexibility in the negotiation of hours. Aubry II classified managers in three categories: managers integrated in a team, autonomous managers (including researchers, engineers, investment bankers, etc.), and directors. The first two categories of managers could sign an agreement with their employers to establish a regular work length in hours or days on a weekly, monthly or annual basis, with additional hours or days being paid at an overtime premium. Directors were fully exempt from the 35-hour workweek.

Support for the 35-hour workweek begun to waver once the socialist government lost the general elections in 2002 and was replaced by a centre-right coalition. In January 2003, the Fillon law reduced incentives for companies still at 39 hours to reduce the workweek. In early 2005, the law was weakened further as it became easier for firms to pay for extra hours of work instead of giving employees actual time off. In addition, individuals were allowed to choose higher workweeks beyond the annual overtime limit of 220 hours per employee up to 48 hours in any week or 44 hours averaged over 12 weeks. Finally, in the past few years several high-profile cases of companies undermining the 35-hour workweek have also hit the press (for instance, Hewlett-Packard and the French subsidiary of Bosch). Overall, although the 35-hour workweek has become enshrined in France, it seems that the parameters pushing its adoption have weakened considerably since 2002.

3. THE RATIONALE BEHIND HOURS REDUCTIONS

The 35-hour workweek legislation was drafted to maximize the positive effect on employment and implicitly assumed that workers would be better off working fewer hours. Indeed, these are the two arguments commonly used to justify reductions in working hours: (i) reducing working hours may create more jobs through work sharing; and (ii) it may improve workers' welfare by increasing non-work time. This section discusses theoretical arguments on these effects. It also provides predictions for how key economic decisions may be affected by institutionally driven restrictions in working hours. These predictions are tested in the following section using the 35-hour workweek in France as a case study.

3.1. Job creation through work sharing

The idea of work sharing as an employment-creation policy is simple: if the production of goods and services in an economy is fixed, then a reduction in hours can redistribute the fixed amount of work across more people, increasing employment. In spite of its intuitive appeal, economists are sceptical about the success of work sharing as it is rooted in the so-called 'lump-of-labour fallacy': the false premise that the amount of output in the economy is fixed.

Contrary to the popular belief, the theoretical literature on work sharing suggests that the employment effect of a reduction in hours is ambiguous and may actually be negative. In a partial equilibrium model of labour demand where average hours of work and employment are perfect substitutes and the only relevant labour cost is the hourly wage, a reduction in the standard workweek reduces average hours and raises employment. However, as made clear by Calmfors and Hoel (1988) and the literature following them, institutional and cost features may hinder this simple result. First, firms face overhead costs associated with hiring, training, employment taxes, and so on, that do not vary with hours of operation, inducing differences between the price of a worker and the price of an extra hour – the hourly wage. Second, longer hours are remunerated differently according to an overtime premium. With only these two simple modifications, work sharing is not the necessary outcome of legislated reductions in the workweek. That is, if firms are hiring overtime hours, a reduction in the standard workweek leaves the marginal cost of an extra hour of overtime unchanged, but raises the marginal cost of hiring an additional worker. In this situation, firms hire more hours and fewer people for a given level of production.[3] In addition, if firms are allowed to vary output, there is a negative scale effect: facing a higher cost of production (because the fixed cost of labour is diluted over a smaller number of hours) firms may decide to produce less, decreasing both employment and hours. The net effect on employment becomes even more ambiguous if we introduce another production input in the analysis, say 'capital', which would raise the possibility of increased labour costs causing a substitution away from labour toward this other input. The parameters of the production function also affect the extent to which employment responds to variations in the workweek for a given level of production.[4]

Papers using general equilibrium models have found positive effects of workweek reduction laws on employment only in very particular specifications. For instance, Marimon and Zilibotti (2000) use a general equilibrium framework with search-matching frictions and show that to raise the effectiveness of the policy, productive factors complementing labour, such as capital, should not be able to adjust to the policy intervention. That might explain why some proponents of legislated workweek reductions would like these policies to be implemented at the widest possible scale (e.g., at the European Union level). Moreover, the authors show that the output losses

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[3] The marginal cost of an extra hour of overtime can be written as $w(1+o)N$, where w, o, and N represent the hourly wage, the overtime premium, and the level of employment, respectively. The marginal cost of an extra employee is $a + wH^s + w(1 + o)(H - H^s)$, where, a, H^s, and H represent overhead costs, the standard workweek, and the actual workweek, respectively. The marginal cost of an extra hour of work does not depend on the standard workweek but the marginal cost of an extra worker rises when the standard workweek is reduced. If firms hire exactly the standard workweek, thus making $H^s = H$, employment will increase for a given level of output, although, depending on production requirements, firms may begin hiring overtime hours instead.

[4] For instance, it seems reasonable to assume that as workdays lengthen, production indivisibilities become less relevant and individuals are more productive. After some level, fatigue effects kick in and longer work days lower the marginal productivity of a worker. These effects link the elasticity of production with respect to average hours of work to the length of the work day, and determine how employment levels vary when average hours change. For a thorough discussion of these issues, see Hart (1984). Estevão (1996) discusses how to estimate the elasticity of hours and employment in aggregate production functions.

associated with legislated workweek reductions can be quite large. Rocheteau (2002) finds a more positive role for work-sharing policies by introducing an efficiency-wage aspect in Marimon and Zilibotti's model. In his setup, workweek reductions have the potential to raise employment in high-unemployment countries but not otherwise. However, even in high-unemployment countries, monthly wages would have to decline for the positive effect on employment to materialize.

The importance of wage behaviour to determine the employment effects of work-week reductions has been documented in Houpis (1993), Kapteyn *et al.* (2000, 2004), and the references cited by them. At the same hourly wage, a reduction in hours would decrease monthly income. If workers demand higher hourly wages as compensation for the reduction in hours, possibly keeping monthly income unchanged, the increase in costs may offset any potential positive effect of work sharing on employment. Kapteyn *et al.* (2000, 2004) test this hypothesis using aggregate data for the OECD. They find that a reduction in working hours has a positive direct effect on employment. However, taking into account the increase in wages, the long-run effect becomes small and insignificant.

The reaction of wages to a legislated reduction in the workweek is indeed crucial in the French case. As discussed before, the 35-hour workweek law guaranteed the monthly earnings of workers receiving the minimum wage. For this group of workers, hourly wages increased to compensate them for the reduction in hours and keep their monthly income unchanged. Unions have also sought higher hourly compensation after the law. These wage responses may have reduced the potential of the workweek reduction to increase employment.

3.2. The welfare effects of a work-time reduction

The reaction of wages to reductions in the workweek brings to the fore how labour-supply factors shape the welfare effects of legislated workweek reductions. Even in the presence of negative consequences on employment levels, these laws could have a positive role by correcting market imperfections associated to labour market frictions, externalities, and coordination failures, although heterogeneity across different social groups could curb the effectiveness of these laws in the aggregate.

3.2.1. Labour market frictions.   A simple modification to the standard assumption in labour economics that firms face a horizontal labour supply curve as a result of perfect competition in labour markets may provide a rationale for governments to intervene in setting the length of the workweek. If there are frictions in labour markets that generate rents to jobs, the labour supply curve becomes positively sloped, giving employers some power to set wages and working conditions. As discussed in Manning (2003) and de Meza *et al.* (1998), in this situation workers are likely to be overworked, that is, to be forced to work more hours than they would like to at their current wage. Thus, legislated reductions in the workweek may make workers better

off as long as their desire for a shorter workweek is increasing the level of income (i.e., leisure is a 'normal' good) and regulation is not too onerous. However, the final effect of workweek reductions on workers' utility and aggregate welfare is still an empirical question.

3.2.2. Externalities and coordination failures.   The general equilibrium models used to analyse legislated workweek reductions do not take into account social coordination problems or the fact that workers may like restrictions on working time as a way to prevent employers from exploiting some type of competition mechanism to induce them to overwork. We believe, though, that these externalities could, in principle, provide a beneficial role to policy action. For instance, mandated reductions in the workweek may improve workers' welfare if there are positive spillovers in leisure, that is, interdependencies between the choices of hours of different workers, with each individual being better off when others are working fewer hours. This could happen if there is a 'rat race' in the workplace and individuals want to be viewed as hard working. In this case, workers are better off when their colleagues work fewer hours because they gain a comparative advantage in the 'rat race' and can improve their career prospects. Thus, the decentralized equilibrium, in which each worker chooses hours taking the other workers' choices as given, is characterized by inefficiently high hours. If workers could coordinate their actions and collectively choose shorter working hours, they would achieve a better, cooperative, equilibrium. A law imposing an upper bound on hours may provide this type of coordination.

This idea is discussed in Landers *et al.* (1996), who describe an organizational setting with positive spillovers in leisure. They consider a situation in which there are two types of workers: those who prefer to work short hours and those who prefer to work long hours. Workers participate in the labour market for two periods. They may be promoted or not in the second period depending on their performance in the first period. Firms cannot observe a worker's type, but take hours as an indicator and promote in the second period those workers who have worked long hours in the first period. This generates a 'rat race' in which workers with a preference for short hours may have an incentive to work long hours in the first period in order to be perceived as hard-working types. To reduce this adverse selection and ensure that they promote workers who really prefer long hours, firms may find it optimal to increase hours of work, raising the cost that individuals with a preference for short hours have to bear when disguising themselves as hard-working types. The authors show that this type of equilibrium is characterized by inefficiently long hours and find evidence in support of this conclusion using data from two large law firms. Indeed, law firms use long hours as an indicator when deciding whether to promote associates to partners. As a result, associates have a tendency to work long hours to stay ahead of their colleagues. This situation is inefficient and workers could be better off with a coordinated reduction in hours.

The simple notion of an externality produced by the free-market equilibrium workweek provides an interesting twist on the labour-supply side of the economy, adding

a testable complexity to the labour demand-oriented discussion of the previous subsection. For instance, it is possible that, in the absence of strong externalities, individuals decide to work overtime or look for a second job to avoid a reduction in income that could be associated with a reduced workweek, an effect not studied in previous empirical papers. Also, they may need to be compensated with an increase in hourly wages for the lower utility derived from shorter working hours. If present in the data, such an effort to circumvent the intent of the law will certainly limit its effect on employment. These mechanisms are key motivations for the empirical work in Section 5.

The idea of leisure complementarities and other externalities in workweek setting provides insights on what to expect from data analysis but other labour supply effects can also be at play. There might be direct complementarities between leisure and income such that workers may not be able to enjoy longer leisure hours if they are cash constrained. Thus, even in the presence of leisure externalities, workers who face a reduction in their workweek may get an additional job to supplement their income or bargain for increased hourly wages, instead of enjoying the extra leisure. It also suggests that workweek reductions could affect people differently depending on their level of income.

3.2.3. Heterogeneity across different population groups. The benefits and costs of workweek-reduction laws probably vary across different population groups. For instance, mostly for cultural reasons, women have been more attached to raising a family and to household tasks than men, and the externality of having to work longer hours in the free market equilibrium may be more burdensome to them. If this perception is right, compared to men, more women would be working 'too much' in their main job, and a coordinated reduction in the workweek would allow them to spend more time at home without suffering dire consequences in their professional life. Thus, relative to men, workweek reduction laws could benefit women more.

More generally, looking at the behaviour of groups likely to be affected by the law in different ways would enhance our view of how appropriate workweek-reduction policies are. Using the arguments presented here, while men and low wage-earners may want to circumvent the law by bargaining for higher hourly wages, women and high-wage earners may be happier with the increased time away from their main jobs. Also, as discussed in Cette *et al.* (2004), some category of employees may be able to better adapt to the shorter workweek because of the type of work they do. The authors report that individuals with more control over how to organize their working time (in general people with more formal education and in managerial or professional occupations) seemed happier with the 35-hour workweek in 2001 than other individuals. Some groups of individuals may also be more affected than others depending on the reorganization of production needed to adapt firms to the new work schedule. In this sense, managers could have suffered with the new workweek as they try to re-organize work schedules and procedures to minimize cost increases after the law.

The notion that the workweek reduction law may trigger a series of group-specific effects, making it harder to infer generalizations about its final welfare effect, is crucial in our analysis. Section 5 will provide evidence of these group-specific effects, looking for a consistent story based on the simple framework presented here: some groups will welcome the coordinated reduction in hours while others will try to circumvent it. The resulting movements in hourly wages to compensate losers will also be mirrored by firms' hiring and firing decisions. By matching the movement in wages with changes in employment flows we can have a more precise picture of the impact of the law.

Finally, the reduction in the workweek may lower the benefits of being a part-time worker vis-à-vis being a full-time worker. In this case, a legislated workweek reduction could induce some part-time workers to become full-timers, which would limit the positive employment effects of the law. This effect could have been particularly important in France, as authorities ended a social security rebate associated with the hiring of part-time workers when the 35-hour workweek law was enacted. Oliveira and Ulrich (2002) look at the effect of this measure on the incentives of working part-time after the reduction in the standard workweek. They find that part-time employees working between 20 and 30 hours in firms that reduced working time, saw their chances of getting a full-time position increase.

4. PREVIOUS EVALUATIONS OF LEGISLATED WORKWEEK REDUCTIONS

The literature evaluating the success of work-sharing experiments is not very extensive and available studies have focused mostly on employment effects in Germany, Sweden, Canada, Portugal, the French workweek reduction in 1982, and Brazil. Table 1 provides a summary of the methodologies and previous results, with a focus on studies closely related to the approach used here.

Germany started reducing standard hours in 1985 on an industry-by-industry basis, with the purpose of raising employment. Hunt (1999) exploits the cross-industry variation in standard hours' reductions to study their impact on actual hours worked, wages and employment. She finds that the reductions in hours decreased employment among men in the period 1984–94. The author explains the results by the large increase in hourly wages, which was enough to offset the decline in actual hours worked and keep monthly incomes unchanged.

Sweden reduced working time gradually by 5% for a particular class of shift workers in the mid-1980s. Skans (2004) evaluates this policy comparing workers affected by the reduction in hours to workers unaffected by it. He finds that there was little implementation of the working-time reduction, with actual hours falling by only about 35% of the reduction in standard hours. Hourly wages increased sharply, almost enough to offset the loss in earnings due to the decline in actual hours. He does not look at the effects on employment.

In Canada, there was a mandated reduction of the workweek from 44 to 40 hours in Quebec between 1997 and 2000 with the aim of increasing employment. This

Table 1. Closely-related work on the effect of working-time reductions

Paper	Country	Workweek reduction	Data type	Methodology	Results
Crépon and Kramarz (2002)	France	Mandatory from 40 to 39 hours beginning on 1 February 1982	Longitudinal data for households (*Enquête Emploi*)	Use employees working more than 39 hours before the law as the treated group and employees working 39 or fewer hours as the control group.	Two to four percent of workers directly affected by the law lost their jobs, and categories with higher hourly-wage increases account for more of these losses.
Gonzaga *et al.* (2003)	Brazil	Mandatory from 48 to 44 hours in 1988	Longitudinal data for households (Monthly Employment Survey)	Treated group formed by employees working more than 44 hours before the law and control group comprising individuals working fewer that 44 hours before the law.	No significant effect on employment, although it increased real hourly wages of affected workers.
Hunt (1999)	Germany	Negotiated between firms and employees beginning in 1985	Individual-level German Socio-Economic Panel (GSOEP) data to analyse impact on actual hours of work and wages. Industry-level data from Mikrozensus cross-section survey of individuals to analyse employment effects.	Exploits the cross-industry variation in standard hours reduction to examine their impact on actual hours worked, wages, and employment.	Employment of affected industries was reduced, as actual working hours declined but hourly wages increased by about the same amount.
Skans (2004)	Sweden	Five percent negotiated reduction in the standard workweek for one category of shift workers in manufacturing and mining	Individual-level register-based panel data set from firms' payroll records.	Because only some workers within each industry (and firm) are affected, other workers are used as control group.	Paper does not study effect on employment. Actual hours declined only by 35 percent and hourly wages rose, leaving monthly wages about unaffected.

Table 1 *Continued*

Paper	Country	Workweek reduction	Data type	Methodology	Results
Skuterud (2007)	Canada	Canadian province of Quebec reduced the standard workweek gradually from 44 to 40 hours between October 1997 and October 2000.	Monthly individual-level data from the Canadian Labour Force Survey.	In practice, working-time reduction applied only to non-unionized, hourly paid employees. Author uses hourly-paid, nonunionized workers in neighboring Ontario and salaried nonunionized workers in Quebec as control groups.	Employment remained basically unchanged while aggregate hours receded a bit.
Varejão (2006)	Portugal	Mandated reduction from 44 to 40 hours in 1996	Matched employer-employee dataset, which allows better control for scale effects.	Treated group formed by employees working more than 40 hours before the law and control group comprising individuals working fewer than 40 hours before the law. The same establishment could employ different hours from otherwise equivalent workers.	The law produced a negative scale effect even though hourly wage increases were quite small, which reduced overtime. Employment increased marginally.

Note: The papers listed here use microeconomic data and some identifying strategy to evaluate the effect of legislated workweek reductions mainly on working hours and employment.

reduction applied only to non-unionized hourly paid workers. Skuterud (2007) looks at the effect of this policy on employment. He uses both a differences-in-differences, DD, and a triple-difference, DDD, estimation approach. In the DD estimation he compares non-unionized hourly paid workers in Quebec (treatment group) to non-unionized hourly paid workers in Ontario, where there was no reduction in the workweek (control group). In the DDD estimation he exploits another source of variation to control for province-specific factors unrelated to the work-sharing policy. He compares non-unionized hourly paid workers in Quebec (treatment group) to non-unionized salaried workers (control group) in Quebec (experimental province) and in Ontario (non-experimental province). In other specifications, he also exploits cross-industry variation, comparing industries where worked hours were affected relatively more to those affected relatively less. His findings suggest that the policy failed to increase employment either at the provincial level or within industries that were affected relatively more.

Skuterud's evidence for Canada is particularly striking because its work-sharing experiment has a set of conditions which are particularly suitable for it to succeed in creating more jobs. First, Canada has a less regulated labour market than most European countries. Therefore, it is less likely that either unions or the government will impose full-wage compensation for the reduction in hours. Second, the reduction in hours was applied to non-unionized hourly paid workers, who are disproportionately unskilled, have high rates of unemployment, have little bargaining power in wage negotiations, and, thus, are unlikely to be able to obtain wage compensations. Third, fixed costs of employment for these workers (training costs, benefits, etc.) are low, reducing the importance of the scale effect of the reduction in hours. Fourth, the difference in skills between these workers and the unemployed is small, facilitating the substitution between hours and jobs. Finally, survey evidence suggests that these workers have a preference for shorter hours. So, they are less likely to look for a second job. Even with all these suitable characteristics, work sharing failed to create more jobs.

Varejão (2006) studies the consequences of the reduction in the standard workweek from 44 to 40 hours mandated by the Portuguese government in 1996. He uses a matched employee-employer dataset that allows the study of how the proportion of individuals working more than 40 hours in a particular establishment affects the adjustment of employment, wages, and hours of work to the law. His results indicate that the reduced standard workweek produced a negative scale effect (i.e. the total number of hours hired was reduced by declines in overtime) despite the wage moderation observed in the data. He finds that the law had a slightly positive effect on total employment, although the establishment-specific results depend on how much the number of working hours had to be reduced after the enactment of the law.

Crépon and Kramarz (2002) analyse the effect on transitions from employment to unemployment of the earlier French law reducing the workweek from 40 to 39 hours in 1982. Some analysts have taken their results as a benchmark for what could have happened in the context of the 35-hour workweek. The authors explore the variation

in hours worked to design a 'natural experiment', comparing workers who were already working 39 or fewer hours before the law (control group) with those who were working 40 hours or more (treatment group). They find that the reduction in hours increased the probability of making a transition from employment to unemployment between 2.3 and 3.9 percentage points. This is an indication that the reduction in hours may have reduced employment, even though the authors do not attempt to estimate the effect on the *level* of employment, that is, taking into account also possible transitions from unemployment to employment. Gonzaga *et al.* (2003) applied the same methodology to evaluate the effect of Brazil's standard workweek reduction in 1988 from 44 to 40 hours and found no effect on employment in the short-run (12 months after the law was enacted), although hourly wages increased somewhat.

In principle, the approach developed in Crépon and Kramarz (2002) could be applied mechanically in the evaluation of the 35-hour workweek law by defining the treatment group as individuals working more than 35 hours per week before the law was announced and the control group as individuals working 35 or fewer hours before the law was announced. However, while the difference between having a workweek of 40 hours or more or a workweek of 39 or fewer hours in 1982 could be viewed as marginal in terms of the type of individuals who would choose one schedule over the other, the same cannot be argued for the most recent law. Indeed, individuals working fewer than 35 hours per week can be significantly different from those working 39 hours or more. In particular, they may have a stronger desire to work part-time or have a more flexible schedule.

The general lesson to be learned from previous research is that the effect on employment and hours varies from country to country, but, in general, one can predict that the employment effects will be null or negative if hourly wages increase significantly after standard workweek reductions. While previous work has presented some evidence that the effects of workweek reductions differed across the population, that has not been the focus so far.

4.1. The French 35-hour workweek law

Compared to the international literature discussed above, studies of the impact of the 35-hour workweek in France are based on weaker empirical frameworks. Also, the few studies on the recent French experiment have focused almost exclusively on the employment effect of the 35-hour workweek. Only a couple of descriptive studies analyse individuals' satisfaction with the law.

Available studies on the employment effect can be categorized in two groups: *ex-ante* simulations, which predict the effect of the 35-hour workweek before it was implemented; and *ex-post* evaluations. A survey of some of these studies can be found in Gubian *et al.* (2004).

Ex-ante simulations are based on either macro or micro models. Macro models make assumptions about the evolution of productivity, hourly wages, and capital

usage following the reduction in hours and predict the evolution of employment under these circumstances. An example of this type of study is Dares-BDF-OFCE (1998), which found that a reduction in the workweek from 39 to 35 hours could potentially generate 700,000 additional jobs. Micro models specify a functional form for the production function and endogenize the evolution of wages using hedonic models, which look at the different characteristics of jobs, including hours of work. Gaps between the workers' desired and actual hours lead to a compensating increase in wages. Micro models tend to predict more modest effects on employment than macroeconometric simulations, because wages would need to increase to compensate workers for working shorter hours. This increase in labour costs reduces the positive impact of the reduction in hours on employment.

Ex-post evaluations compare firms that reduced hours (treatment group) with those that did not (control group). Some evaluations have methodological flaws, though, as they do not control for differences in characteristics between the two groups. Others apply econometric techniques to account for those differences, which is important to ensure that the control group is a good counterfactual for the behaviour of the treatment group in the absence of the reduction in hours.[5] Most of these studies find a strong positive effect on employment. However, they restrict the analysis to large firms and compare those that reduced hours earlier with those that did it later. By leaving out small firms, these studies ignore potentially useful information. Moreover, by dividing large firms into two groups depending on the timing of implementation of the shorter workweek, these studies generate a bias: both groups of large firms were affected by the law and their decision to implement it sooner or later could be related to unobservable variables (e.g. productivity). The results would be capturing the effect of those unobservable differences rather than the effect of the shorter workweek.

The effect of the 35-hour workweek on workers' welfare has received much less attention than the effect on employment. Most studies are based on surveys asking employees affected by the law whether they consider that their situation has improved as a result of the reduction in hours. Typically, the majority of employees say that their situation has improved. However, because these studies do not compare the outcomes of treated individuals to the outcomes of a control group, they fail to capture any causal effect.[6]

In this paper we take a broad approach and look at the effects of the workweek reduction on several margins closely related to workers' welfare. We use information on wages, dual-job holdings, transitions out and into employment, and workers' satisfaction with their working hours to have a fuller assessment of the effects of the law.

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[5] Crépon *et al.* (2004) is an example of this type of *ex post* evaluation.

[6] An example of this type of study is Cette *et al.* (2004), who, nonetheless, unearth interesting anecdotal evidence of how some different groups of individuals view the reduced workweek.

5. MEASURING THE EFFECTS OF THE 35-HOUR WORKWEEK IN FRANCE

We are interested in studying the effect of the 35-hour workweek along several dimensions, motivated by the theoretical arguments, the institutional background, and the available international literature on the issue discussed so far. As we have argued, theory does not make clear-cut predictions about the effects of reductions in hours on employment and welfare, although every model shows that positive employment effects are dependent upon limited hourly wage increases following the adoption of the law. In any case, whether the 35-hour workweek succeeded in creating more jobs and in making French workers happier is ultimately an empirical question.

We will use evidence across outcomes and characteristics to shed light on the effects of the law. In particular, we will break down the results for men and women, since anecdotal evidence and theory suggest that women may have benefited more from the reduction in the workweek.

To evaluate the 35-workweek law, we design a quasi-experiment using the fact that the law was applied earlier in large firms. Firms with more than 20 employees had to implement the 35-hour workweek by February 2000, while firms with fewer than 20 employees had until January 2002 to do so. Thus, we can use workers in large firms as the treatment group and workers in small firms as the control group. The effect of interest is captured by the difference between the outcome of the treatment group after the law and before the law, and the corresponding difference for the control group – the DD estimator.

This strategy requires that there are no contemporaneous shocks, other than the treatment, affecting the outcomes of the control (workers in small firms) and treatment (workers in large firms) groups, after controlling for workers' observable characteristics, during the period we analyse. If this assumption holds, the behaviour of individuals employed in smaller firms gives information about how individuals in large firms would have behaved if they had not been 'treated' by the law.

Several other factors need to be taken into account before results based on this general strategy can be interpreted as outcomes of the law. First, individuals and firms may have adjusted to the law gradually. As we discussed earlier, policies aiming at reducing the workweek in France have been in place since the 1980s and mechanisms to encourage firms to do so have been introduced incrementally, including in 1996 with the Robien Law. Thus, it is possible that workers in large firms had already been adapting to a smaller workweek well before 2000, while workers in smaller firms, which face tougher constraints to implement shorter workweeks, lagged behind. This differential behaviour would cause a trend difference in the workweek for both groups that needs to be accounted for. Second, small firms were aware that they would need to adopt the shorter workweek by 2002, and may have started adjusting even before the deadline, in which case the difference between large and small firms may not be so large and our estimates would be capturing a lower bound of the effect of the law. Third, size-specific technologic or cyclical shocks could also drive wages, hours and

employment within both groups of firms. In that case our 'before treatment'/'post treatment' analysis would erroneously attribute such firm size-specific cyclical effects to the 35-hour workweek law.

To account for such trends and gradual effects, we compare the outcomes of both groups of workers in a period immediately before the reform was implemented and in each post-reform year.[7] By doing so we can observe whether the effects for each post-law period were merely a repetition of what has been happening before the law was announced due to some trend or anticipation effects.

To account for the fact that the business cycle may affect small and large firms differently we follow the strategy in Kugler and Pica (2005) and control for interactions between real GDP growth and the relative outcomes for individuals in large firms.[8] This is potentially important in the case of the 35-hour workweek because it was implemented during a period when the French economy was booming. If the boom affects the control and treatment groups differently, the DD estimator will be a biased measure of the effect of the law.

5.1. Data and descriptive statistics

To implement our empirical strategy, we need information on labour market outcomes and on firm size, for a period of time spanning the implementation of the 35-hour workweek in large and small firms. We use data from the French labour force survey (Enquête Emploi) from 1993 to 2002, thus focusing on the period when the 35-hour workweek had the highest take-up: 1999 to 2002. This dataset is matched with firm-level data from the French Registry of Firms (SIRENE), containing information on firm size. The experiment stops in 2002, as after this date, employees in

[7] Formally, we use the following specification to estimate the effect of the treatment on the outcome variable y_i for individual i:

$$y_{it} = \alpha + \gamma d_t + \beta X_{it} + \delta_0 Treat_i + \delta_1(Treat * 1997\text{--}1998)_{it} + \delta_2(Treat * 1999)_{it} + \delta_3(Treat * 2000)_{it} + \delta_4(Treat * 2001)_{it} + \delta_5(Treat * 2002)_{it} + \varepsilon_{it}$$

where y_{it} is the outcome of interest for individual i at year t. X_{it} is a set of controls capturing observable differences in the characteristics of the control and treatment groups that affect the outcome of interest. d_t is a vector of year effects controlling for time-specific changes in y_{it}. $Treat_i$ is an indicator equal to 1 if the individual belongs to the treatment group and 0 if she belongs to the control group. This variable captures time-invariant differences between the two groups. The interactions of the indicator for treatment with the post-reform years – 2000, 2001, and 2002 – are the DD estimators measuring the causal effect of the law on the outcome variable. The results for 2001 and 2002 may serve as a lower bound for the effects of the law as smaller firms may have begun to adopt the 35-hour workweek. The interaction with 1999 captures possible anticipation effects. The interaction with the period 1997–8 accounts for possible trend effects affecting the relative behaviour of workers in large firms vis-à-vis workers in small firms. For a more technical and comprehensive discussion of strategies for identifying causal effects, see Angrist and Krueger (1999).

[8] The final specification is thus:

$$y_{it} = \alpha + \gamma d_t + \beta X_{it} + \delta_0 Treat_i + \delta_1(Treat * 1997\text{--}1998)_{it} + \mu(GDP_t * Treat_i) + \delta_2(Treat * 1999)_{it} + \delta_3(Treat * 2000)_{it} + \delta_4(Treat * 2001)_{it} + \delta_5(Treat * 2002)_{it} + \varepsilon_{it},$$ in which GDP_t is the growth rate of real GDP.

Throughout the paper, we report least-squares estimates, but probit or logit estimates give similar results. In all estimations, standard errors are clustered by year × treatment group cells. By clustering the standard errors we correct for the fact that there may be common errors within these groups. Failure to take this into account would lead to underestimation of the standard errors and overestimation of the effect of interest, as shown in Moulton (1990).

small firms cannot be used as part of the control group any more because the law also applied to them (even if in a more flexible manner). This survey is conducted in March of each year, with the exception of 1999 when it was done in January. It has information on several demographic characteristics, as well as on labour market status, wages, hours of work, tenure, etc. The sample is renewed by a third every year, so the same individual can be followed for three consecutive years.

Table 2 reports descriptive statistics by hours worked, firm size, and year for key individual characteristics. The sample is limited to employees aged 15 to 64 with positive net monthly income, and excludes self-employed individuals. Small firms have fewer than 20 employees. Large firms have between 20 and 49 employees. We impose a cut-off at 49 to ensure more homogeneity between the two groups, so we can be more confident that the behaviour of small firms, after controlling for individuals' observable characteristics, can be taken as the counterfactual for the behaviour of large firms in the absence of the law. We experimented with different cut-off levels and the results in the paper are not sensitive to the cut-off choice. The table shows some differences between workers in large and small firms: workers in large firms earn higher wages and work slightly fewer hours. There are also differences between individuals working longer and shorter hours: individuals working longer hours tend to be male, earn higher wages, and have longer tenure than those working shorter hours. Our estimations control for these differences in observable characteristics.

5.2. Results

5.2.1. Hours distribution and wages.   The introduction of the 35-hour workweek had a clear impact on weekly hours of work, as shown in Figure 2 and Table 3. After the introduction of the law, the percentage of individuals working exactly 39 hours declined significantly in large firms and less so in small firms. By the same token, a spike in 35 hours is clear for large and, to a lesser extent, small firms. The percentage of employees working 39 hours or more in large firms has trended down in the 1990s, but a clear discontinuity can be observed in 2000. In small firms, the percentage of individuals working 39 hours or more is more stable during the 1990s and a larger drop is observed only around 2001–2. The mass of employees working exactly 35 hours in large firms has also trended up slightly before surging after 1999–2000 to reach 43% in 2002. In small firms, the increase in the proportion of employees working exactly 35 hours has been more gradual and suggests some anticipation effect for this group of workers.

To examine more carefully the effect of the law on hours worked, we use the DD estimator with employees in small firms as the control group and employees in large firms as the treatment group. We report the results in Table 4 separately for men and women, excluding part-time workers, and controlling for possible interactive business-cycle effects. The DD estimates are negative for the period 1999–2002 for men and show a clear break with past trend, as represented by the parameter for the interaction

Table 2. Summary statistics

	35 hours or less				More than 35 hours			
	Large firms		Small firms		Large firms		Small firms	
	1993–1998	1999–2002	1993–1998	1999–2002	1993–1998	1999–2002	1993–1998	1999–2002
Percentage female	66.409	68.201	70.623	68.768	39.793	40.305	36.403	34.414
	(47.232)	(46.571)	(45.551)	(46.348)	(48.947)	(49.051)	(48.117)	(47.511)
Percentage married	77.641	76.618	79.422	79.598	77.87	74.587	75.815	74.718
	(41.666)	(42.327)	(40.429)	(40.302)	(41.512)	(43.538)	(42.822)	(43.465)
Average age	40.671	41.616	40.669	41.653	40.787	41.385	38.782	39.453
	(10.349)	(10.312)	(10.291)	(10.174)	(9.59)	(9.91)	(10.133)	(10.242)
Average net monthly income (francs)	7838.94	7976.079	5862.345	5990.688	9577.388	9899.455	8520.081	8316.608
	(29 972.31)	(26 410.08)	(17 248.56)	(4755.371)	(27 021.92)	(22 124.7)	(27 871.54)	(9986.894)
Average hours in main job	22.41	23.021	22.416	23.082	40.317	39.276	41.081	40.345
	(7.444)	(7.661)	(7.092)	(7.088)	(4.93)	(5.241)	(5.698)	(5.546)
Percentage with tenure less than 1 year	18.374	17.526	14.372	12.679	8.47	11.106	11.009	12.11
	(38.728)	(38.019)	(35.083)	(33.277)	(27.843)	(31.421)	(31.301)	(32.625)

Notes: Table reports group means by hours worked, firm size and time period. Standard errors are reported in parentheses. The sample is limited to employees under age 64 with positive net monthly income and excludes self-employed individuals.

Source: French labour force survey (*Enquête Emploi*), 1993–2002.
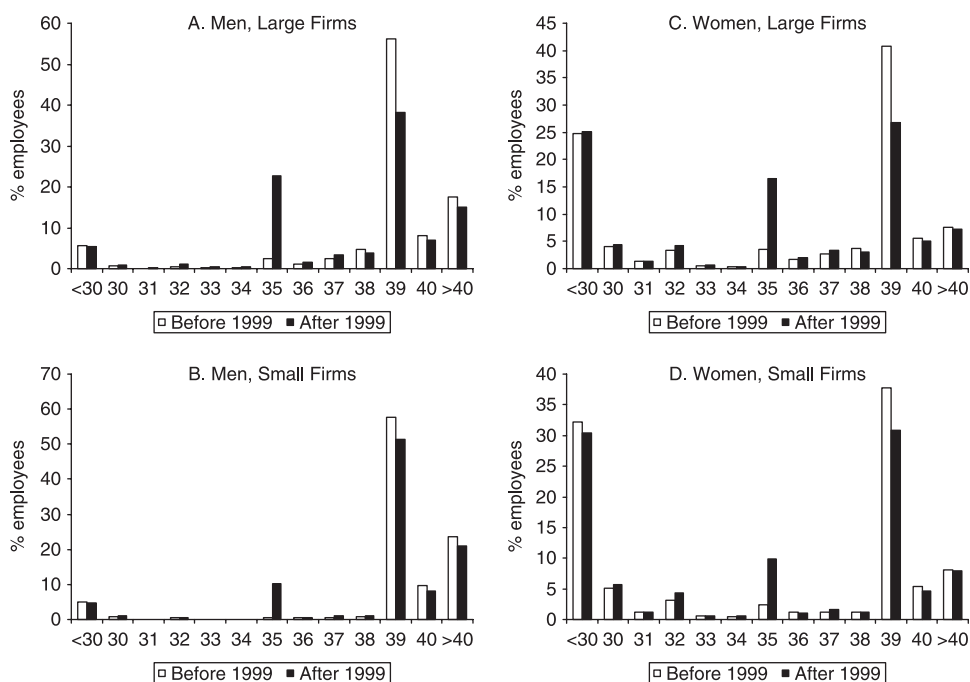
Figure 2. Distribution of usual weekly hours in the main job

Sources: Enquête Emploi, 1993–2002; authors' calculations.

Table 3. Percentage of employees working different intervals of usual weekly hours

	Small firms					Large firms				
	Above 39	39	35–39	35	Below 35	Above 39	39	35–39	35	Below 35
1993	27.5	45.6	2.4	1.3	23.2	21.4	49.3	8.3	2.6	18.4
1994	25.3	47	2.6	1.4	23.7	20.5	49.9	8	2.5	19.1
1995	25.4	47.1	2.7	1.4	23.4	20.5	48.9	8.3	2.7	19.6
1996	22.6	48.5	2.5	1.4	25	19.6	49.4	8	2.7	20.3
1997	21.8	50	2.7	1.3	24.2	18.7	48.7	8	3	21.6
1998	22.4	48.6	2.7	1.8	24.5	18.6	47.2	8.3	4	21.9
1999	22	48	2.9	2.8	24.3	18.4	45.5	8.5	5.3	22.3
2000	21.6	44.3	2.7	7.6	23.8	16.4	31.7	8.3	21	22.6
2001	22.5	39.6	3.6	13.5	20.8	17.4	21.9	8.6	31.6	20.5
2002	17.9	19.9	4.9	35.6	21.7	15.5	10.9	9.2	42.9	21.5

Source: Enquête Emploi, 1993–2002.

between the treatment group and the period immediately before the law was announced (1997–8). For women, decreases in working hours in large firms during 2000–1 add to effects already observed before the law. In both groups, the relative reduction in working hours in large firms were less pronounced in 2002 but that is

Table 4. Usual weekly hours

	Men	Women
Treat	−0.597***	−0.382***
	(0.114)	(0.071)
Treat * (1997–1998)	0.167*	−0.989***
	(0.089)	(0.069)
Treat * 1999	−0.061	−0.690***
	(0.097)	(0.071)
Treat * 2000	−0.610***	−1.697***
	(0.125)	(0.087)
Treat * 2001	−1.225***	−1.347***
	(0.070)	(0.050)
Treat * 2002	−0.890***	−0.297***
	(0.079)	(0.051)
Married	0.482***	−0.324***
	(0.087)	(0.075)
Children under 6	0.064	−0.616***
	(0.052)	(0.104)
Age	0.137***	−0.041*
	(0.021)	(0.024)
Tenure less than 1 year	−0.153	−0.315**
	(0.121)	(0.122)
N	32 307	18 871

Notes: Robust standard errors adjusted for clustering by year × Treat. Regressions include year dummies, region effects, education, age-squared, and GDP × Treat. Sample excludes part time workers.
*** significant at the 1% level, ** significant at the 5% level, * significant at the 10% level.
Estimated equation:

$$y_{it} = \alpha + \gamma d_t + \beta X_{it} + \delta_0 Treat_i + \mu(Treat_i \times GDP_t) + \delta_1(Treat \times (1997{-}1998))_{it} + \delta_2(Treat \times 1999)_{it} + \delta_3(Treat \times 2000)_{it}$$
$$+ \; \delta_4(Treat \times 2001)_{it} + \delta_5(Treat \times 2002)_{it} + \varepsilon_{it}$$

y_{it} is usual weekly hours. d_t is a set of time dummies. X_{it} is a vector of control variables. $Treat_i$ is an indicator equal to 1 if the worker works at a large firm at time t and 0 if he works at a small firm. Least squares estimates.
Sources: Enquête Emploi, 1993–2002; authors' calculations.

likely associated to the jump in the use of the 35-hour workweek in smaller firms after the deadline of January 2002, as reported in Table 3.

Table 5 shows the results of applying the same technique to measure the effect on hourly wages and monthly incomes in the main job, again studying the separate effects on men and women. We present the estimates separately for employees receiving between 10% below and 10% above the minimum wage and workers receiving between 10% and 80% above the minimum wage.[9] We are interested in this distinction because the law mandated that the monthly earnings of workers receiving the minimum wage should stay constant. The results suggest that the law led to larger increases in hourly wage for men earning around the minimum wage (Table 5, panel A, columns 1 and 2), which has caused a small increase in monthly wages after the law (Table 5, panel A, columns 3 and 4). Among workers earning more than the

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[9] We exclude workers with earnings more than 80% above the minimum wage as we want to ensure more homogeneity within the group.

Table 5. Wages and monthly income

	Log hourly wage		Log monthly income	
	Men	Women	Men	Women
A. Workers between 10% below and 10% above minimum wage				
Treat	0.011***	0.014**	−0.002	−0.001
	(0.001)	(0.005)	(0.002)	(0.002)
Treat * (1997–1998)	0.009***	−0.004	0.002	−0.004
	(0.002)	(0.006)	(0.002)	(0.003)
Treat * 1999	0.021***	−0.017***	0.006***	0.002
	(0.001)	(0.005)	(0.002)	(0.002)
Treat * 2000	0.034***	0.013**	0.005*	−0.004*
	(0.002)	(0.007)	(0.003)	(0.003)
Treat * 2001	0.037***	0.020***	0.011***	−0.008***
	(0.001)	(0.004)	(0.001)	(0.001)
Treat * 2002	0.030***	0.0002	0.003**	0.001
	(0.001)	(0.004)	(0.001)	(0.002)
Married	0.009***	0.004	0.007***	0.004
	(0.002)	(0.004)	(0.002)	(0.003)
Children under 6	−0.006**	0.007*	−0.002*	0.0003
	(0.002)	(0.004)	(0.001)	(0.002)
Age	0.005***	0.004***	0.004***	0.003***
	(0.001)	(0.001)	(0.0004)	(0.001)
Tenure less than 1 year	−0.006	−0.004	−0.004**	−0.001
	(0.005)	(0.007)	(0.002)	(0.003)
N	9769	4661	9770	4661
B. Workers between 10% and 80% above minimum wage				
Treat	0.020***	−0.011***	0.007***	−0.006***
	(0.001)	(0.003)	(0.002)	(0.002)
Treat * (1997–1998)	−0.009***	0.046***	−0.008***	0.001
	(0.002)	(0.004)	(0.002)	(0.004)
Treat * 1999	0.002	0.010***	−0.004**	−0.011***
	(0.002)	(0.002)	(0.002)	(0.002)
Treat * 2000	0.023***	0.028***	−0.003	−0.008***
	(0.003)	(0.003)	(0.002)	(0.003)
Treat * 2001	0.040***	0.010***	0.002	−0.014***
	(0.002)	(0.003)	(0.001)	(0.001)
Treat * 2002	0.017***	−0.002	−0.009***	−0.026***
	(0.001)	(0.002)	(0.001)	(0.001)
Married	0.022***	0.021***	0.027***	0.008**
	(0.005)	(0.006)	(0.004)	(0.004)
Children under 6	−0.002	0.013*	−0.004**	−0.005
	(0.002)	(0.007)	(0.001)	(0.003)
Age	0.018***	0.031***	0.011***	0.021***
	(0.001)	(0.002)	(0.001)	(0.002)
Tenure less than 1 year	−0.024***	−0.032***	−0.013**	−0.022***
	(0.007)	(0.012)	(0.005)	(0.008)
N	13 129	6529	13 130	6530

Notes: Robust standard errors adjusted for clustering by year × Treat. Hourly wage is monthly wage/(4.33 × usual weekly hours). Section A shows the results for a sample of employees under age 64 with net monthly income between 10% below and 10% above the minimum wage. Section B shows the results for a sample of employees under age 64 with net monthly income between 10% and 80% above the minimum wage. Regressions include year dummies, region effects, education, age-squared, and GDP × Treat. Sample excludes part time workers. *** significant at the 1% level, ** significant at the 5% level, * significant at the 10% level.
Estimated equation:

$$y_{it} = \alpha + \gamma d_t + \beta X_{it} + \delta_0 Treat_i + \mu(Treat_i \times GDP_t) + \delta_1(Treat \times (1997-1998))_{it} + \delta_2(Treat \times 1999)_{it} + \delta_3(Treat \times 2000)_{it} + \delta_4(Treat \times 2001)_{it} + \delta_5(Treat \times 2002)_{it} + \varepsilon_{it}$$

y_{it} is log hourly wage or log monthly income. d_t is a set of time dummies. X_{it} is a vector of control variables. $Treat_i$ is an indicator equal to 1 if the worker works at a large firm at time t and 0 if he works at a small firm. Least squares estimates.
Sources: Enquête Emploi, 1993–2002; authors' calculations.

minimum wage, men's hourly wages were raised more than women's in 2001–2, although the effects were about the same in 2000 (Table 5, panel B, columns 1 and 2). In addition, the hourly wage of women in large firms had been moving ahead of the wage of women working in smaller firms in the period immediately before the law was enacted (coefficient of Treat*(1997–8)), hinting that the effects observed in 1999–2002 could be a simple continuation of a previous trend and not the effect of the law. During the whole 1999–2002 period, women working in large firms saw their monthly wages decline vis-à-vis women working in smaller firms but the same did not happen among men (Table 5, panel B, columns 3 and 4).[10]

The higher increases in hourly wages for men as a result of the law, but not for women, suggest that men were more attached to their monthly wages and could have seen the 35-hour workweek as a constraint to having more income. That is consistent with men being the main breadwinner in a family. In addition, women seem to have asked for less compensation for working fewer hours, a development consistent with our hypothesis that women would benefit more from coordination away from the 'rat-race' equilibrium.

5.2.2. Dual-job holdings. A margin of adjustment which sheds light both on the work-sharing hypothesis and on the different supply-side hypotheses about workweek choice (externality in leisure or complementarity between income and leisure) is the proportion of workers with more than one job. If individuals respond to the reduction in hours by working in a second job, it is less likely that unemployment will decline, because some jobs will be filled by individuals who are already employed. At the same time, the fact that workers are looking for a second job suggests that they have a stronger preference for income relative to leisure. In that case the reduction in hours would not be working, for instance, as a coordination mechanism to encourage workers to increase their leisure time.

Table 6 shows that the law increased dual job holdings by 0.7 percentage point among men in 2000, although this effect converged down to zero in the two subsequent years. These estimates exclude part-time workers to avoid the effect of increased incentives to work full-time in one job when the workweek is reduced. The magnitude of this increase is large, since only 2.7% of employees in our sample hold a second job.

The increased dual-job holdings in 2000 for men whose main job is in large firms vis-à-vis employees of small firms corroborates the view that men could have been more constrained in their labour supply by the reduced workweek than women.

5.2.3. Employment. To test the work-sharing hypothesis directly, we are interested in the effect of the 35-hour workweek on employment. We look both at flows in and out of employment and at the level of employment. For that, we take advantage of the

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[10] The results are insensitive to estimation with individual fixed effects to account for unobservable differences among individuals.

Table 6. Dual job holdings

	Men	Women
Treat	−0.005***	0.002
	(0.002)	(0.003)
Treat * (1997–1998)	0.001	−0.011***
	(0.002)	(0.003)
Treat * 1999	−0.001	0
	(0.001)	(0.004)
Treat * 2000	0.007***	−0.0003
	(0.002)	(0.005)
Treat * 2001	−0.001*	−0.002
	(0.0006)	(0.002)
Treat * 2002	0.0004	−0.003
	(0.001)	(0.002)
Married	0.002	−0.007***
	(0.002)	(0.002)
Children under 6	−0.001	0.002**
	(0.001)	(0.001)
Age	0.001**	0.0007
	(0.0006)	(0.0006)
Tenure less than 1 year	−0.003*	0.002
	(0.001)	(0.003)
N	32 670	18 202

Notes: Robust standard errors adjusted for clustering by year × Treat. Regression includes year dummies, region effects, hours in the main job, education, net monthly income, age-squared and GDP × Treat. Sample excludes part-time workers.
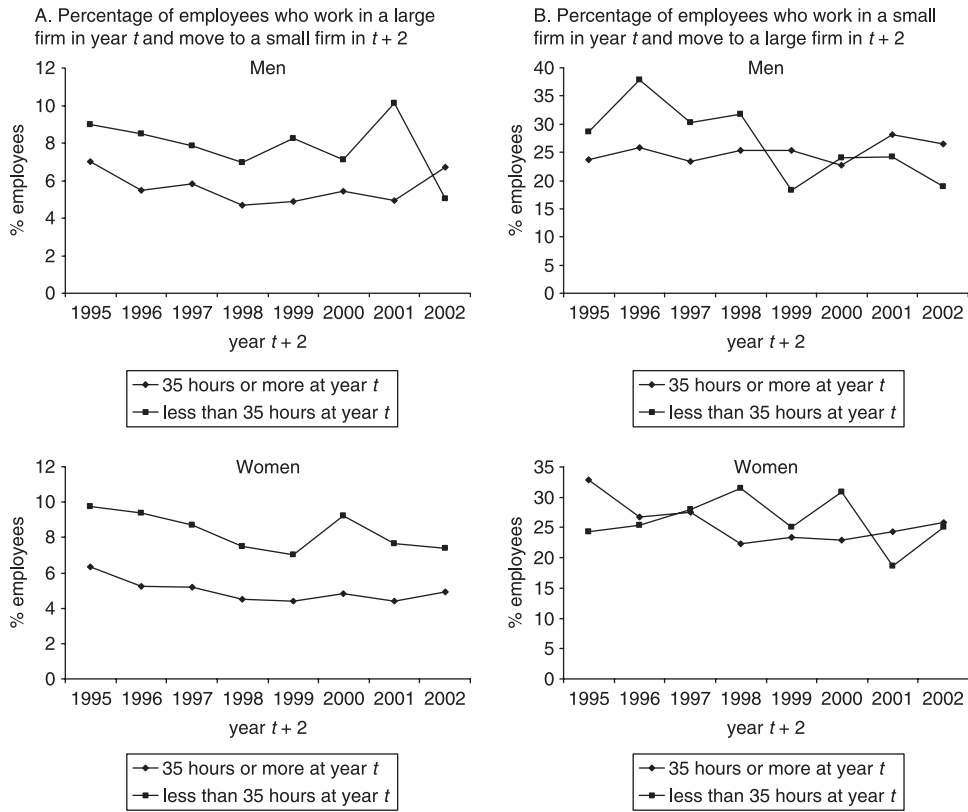*** significant at the 1% level, ** significant at the 5% level, * significant at the 10% level.
Estimated equation:

$$y_{it} = \alpha + \gamma d_t + \beta X_{it} + \delta_0 Treat_i + \mu(Treat_i \times GDP_t) + \delta_1(Treat \times (1997-1998))_{it} + \delta_2(Treat \times 1999)_{it} + \delta_3(Treat \times 2000)_{it} + \delta_4(Treat \times 2001)_{it} + \delta_5(Treat \times 2002)_{it} + \varepsilon_{it}$$

y_{it} is an indicator equal to 1 if the worker has a second job and 0 otherwise. d_t is a set of time dummies. X_{it} is a vector of control variables. $Treat_i$ is an indicator equal to 1 if the worker works at a large firm at time t and 0 if he works at a small firm. Least squares estimates.

Sources: Enquête Emploi, 1993–2002; authors' calculations.

panel aspect of the survey: using the fact that the same individual can be followed for three consecutive years, we construct several three-year panels: 1993–5, 1994–6, 1995–7, 1996–8, 1997–9, 1998–2000, 1999–2001, and 2000–2. The period before the law covers the years 1993 to 1998 (since the law was announced in June 1998 and the *Enquête Emploi* was conducted in March 1998) and the period after covers 1999 to 2002. Individuals are classified in the base year, which falls within the period before the law was enacted for all periods except the last two, 1999–2001 and 2000–2. Their labour market status is then observed two years later. By analysing the relative flows of employees in larger firms into unemployment in 1999–2002 vis-à-vis the equivalent flows of their counterparts in smaller firms, we can judge how the law affected the employment levels of workers directly affected by it. The inclusion of 1999 allows accounting for anticipation effects while, following the empirical strategy so far, we will look at transitions from employment to unemployment in 1997–8 as a

A. Percentage of employees who work in a large firm in year *t* and move to a small firm in *t* + 2

Men

B. Percentage of employees who work in a small firm in year *t* and move to a large firm in *t* + 2

Men

Women

Women

Figure 3. Transition probabilities

Sources: Enquête Emploi, 1993–2002; authors' calculations

way to check for previous trend in the relative flows of the two groups of workers that could be influencing the post-law estimates.

The key three-year panel is 1998–2000 when individuals are classified in the treatment and control groups before the law was announced, but subsequent panels can also be instructive. The later panels, 1999–2001 and 2000–2, will be more informative if there were little change in the trend pattern of workers leaving large firms and going into small firms, that is, if individuals in large firms did not react significantly to the law in 1999 and 2000 by moving to smaller firms to avoid having to work fewer hours. Otherwise, we would be underestimating severely the effect of the law on labour outcomes as a possibly important escape valve had already been used. Figure 3 shows that, indeed, transitions from large to small firms are not that common (Panel A) and they did not increase significantly in 1999 and 2000 for individuals working more than 35 hours per week in large firms.[11]

[11] The only more significant change in transition probabilities between the two types of firms before and after 1999 was an overall decline in the flow of men working fewer than 35 hours in smaller firms to large firms in 1999 and 2000 (Figure 3, panel B), which suggests some law-anticipation effect from workers in small firms.

Table 7. Transitions from employment to unemployment

	Men	Women
Treat	−0.013***	0.008
	(0.005)	(0.007)
Treat * (1997–1998)	0.008*	0.001
	(0.004)	(0.006)
Treat * 1999	0.039***	−0.005
	(0.004)	(0.007)
Treat * 2000	0.027***	0.006
	(0.005)	(0.009)
Treat * 2001	0.010***	0.021***
	(0.002)	(0.003)
Treat * 2002	0.014***	−0.012***
	(0.003)	(0.003)
Married	−0.015**	−0.005
	(0.006)	(0.006)
Tenure less than 1 year	0.080***	0.075***
	(0.007)	(0.012)
Seasonal contract	−0.010	0.174***
	(0.040)	(0.051)
N	10 296	5662

Notes: Robust standard errors adjusted for clustering by year × Treat. Regression includes year dummies, region effects, education, age, age-squared, an indicator for having children under age 6, and GDP × Treat. The sample is limited to employees working more than 35 hours at time t (the first year in each panel) and excludes part time workers.
*** significant at the 1% level, ** significant at the 5% level, * significant at the 10% level.
Estimated equation:

$$y_{it+2} = \alpha + \gamma d_{t+2} + \beta X_{it} + \delta_0 Treat_i + \mu(Treat_i \times GDP_{t+2}) + \delta_1(Treat \times (1997 - 1998))_{it} + \delta_2(Treat \times 1999)_{it+2} + \delta_3(Treat \times 2000)_{it+2} + \delta_4(Treat \times 2001)_{it+2} + \delta_5(Treat \times 2002)_{it+2} + \varepsilon_{it+2}$$

y_{it+2} is an indicator equal to 1 if the individual is unemployed at time $t + 2$ (the last year in each panel). d_{t+2} is a set of time dummies. X_{it} is a vector of control variables. $Treat_i$ is an indicator equal to 1 if the worker works at a large firm at time t and 0 if he works at a small firm.
Least squares estimates.

Sources: *Enquête Emploi*, 1993–2002; authors' calculations.

To measure the effect of the law on transitions from employment to unemployment, we restrict the sample to employees working more than 35 hours before the law as those were the ones for whom the law was binding. We compare the probability of becoming unemployed for workers initially working at large firms (treatment group) relative to workers initially working at small firms (control group). The results in Table 7 show that the law increased transitions from employment to unemployment for men by 3.9 percentage points in 1999, 2.7 percentage points in 2000, 1 percentage point in 2001, and 1.4 percentage points in 2002. Women working in large firms more than 35 hours per week in the base year were relatively unaffected by the law, with an increased probability of going into unemployment only in 2001, which was partially reversed in 2002. All these movements are departures from the previous trend. This is consistent with the results found so far: men demanded higher hourly wages than women and got more second jobs, which suggests that they were less

happy with the 35-hour workweek. They also flowed more into unemployment either because as a group they became more expensive to firms (creating incentives for firms to fire the least productive employees within this group of workers) or as an intermediate step to finding jobs with fewer hours. Women, on the other hand, seem to have been less adverse to the law, including by accepting the same (or even a slightly reduced) hourly wage, which did not put additional cost pressures on firms.

This novel evidence on the law's negative effect on employment of people directly affected by it can be complemented by investigating the law's effect on transitions in the reverse direction, that is, from unemployment to employment. We test whether unemployed workers are more likely to find a job in large firms or in small firms as a result of the reduction in hours. The first and third columns of Table 8 report the log of the probability of working at a large firm at $t + 2$ relative to being unemployed for men and women. The second and fourth columns report the equivalent relative probability for small firms. We are interested in the interactive terms between being unemployed in the base year and the years following the announcement of the law. By comparing the coefficients for these terms in the equations for small and large firms, we can evaluate whether unemployed individuals are more likely to find a job in a large firm or in a small firm after the 35-hour workweek was implemented.

The results show that large firms tended to hire men from the unemployment pool more often than small firms after the law was announced. That was particularly true in 1999 and 2001 (the p-value of the difference between the coefficients on *Unemployed * 1999 and Unemployed * 2001* is 0). For women, in some years (2000 and 2002) they left unemployment to find jobs more easily in small firms. However, in 2001, when there was a significant flow of women from employment in large firms into unemployment (see the previously discussed result in Table 7), large firms also hired women from unemployment more often than small firms. This is consistent with higher turnover rates for men in large firms – as we saw earlier that men in large firms also flowed more toward unemployment after the 35-hour workweek – and women in large firms in 2001.

To measure the impact of the law on employment it is important to look not only at transitions in and out of employment but also at the *level* of employment. Unfortunately, the data do not allow measuring this effect in a clear way. If we were to apply the DD strategy to study the effect of the law on the level of employment, we would like to have treatment and control groups which are composed of both employed and unemployed individuals. Then, we would test whether the fraction of employed individuals increased by more for the treatment group than for the control group after the law. The problem is that the source of variation that we can explore to define the treatment and control groups (variation in firm size) implies that, by definition, all individuals in the two groups are employed. Thus, we do not have suitable treatment and control groups.

In spite of this technical difficulty, we can still shed light on the effect of the 35-hour workweek on the level of employment by comparing the evolution of employment in

Table 8. Transitions from unemployment to employment

	Men		Women	
	log[P(large)/ P(unemployed)]	log[P(small)/ P(unemployed)]	log[P(large)/ P(unemployed)]	log[P(small)/ P(unemployed)]
Unemployed	−2.762***	−2.514***	−2.293***	−2.191***
	(0.073)	(0.012)	(0.079)	(0.040)
Unemployed * (1997–1998)	−0.104	0.017	−0.172*	−0.075
	(0.071)	(0.078)	(0.101)	(0.081)
Unemployed * 1999	0.018	−0.182***	−0.085	−0.017
	(0.070)	(0.011)	(0.082)	(0.043)
Unemployed * 2000	−0.162**	−0.157***	−0.500***	−0.238***
	(0.071)	(0.012)	(0.082)	(0.044)
Unemployed * 2001	0.606***	0.150***	0.353***	0.167***
	(0.071)	(0.019)	(0.082)	(0.043)
Unemployed * 2002	−0.223***	−0.202***	−0.167**	0.173***
	(0.070)	(0.013)	(0.080)	(0.043)
Married	0.885***	0.837***	0.206***	0.250***
	(0.045)	(0.049)	(0.063)	(0.049)
Children under 6	−0.135***	−0.094*	−0.463***	−0.452***
	(0.054)	(0.054)	(0.082)	(0.045)
Age	0.164***	0.099***	0.080***	0.066***
	(0.026)	(0.021)	(0.019)	(0.021)
p-value for equality of coeff. on unemployed*(1997–1998)	0.214		0.465	
p-value for equality of coeff. on unemployed*1999	0.021		0.096	
p-value for equality of coeff. on unemployed*2000	0.944		0	
p-value for equality of coeff. on unemployed*2001	0		0	
p-value for equality of coeff. on unemployed*2002	0.764		0	
N	17 812		16 812	

Notes: Robust standard errors adjusted for clustering by year × unemployed in base year. Comparison group is the unemployed. Estimates give the change in the log odds ratio for a one-unit change in the independent variable. Regression includes year dummies, region effects, education and age-squared.
*** significant at the 1% level, ** significant at the 5% level, * significant at the 10% level.
Estimated equation:

$$P(status_{it+2} = k) = \Phi(\alpha + \gamma d_{t+2} + \beta X_{it} + \delta_0 unemp_{it} + \delta_1(unemp_{it} \times (1997 - 1998)_{t+2}) + \delta_2(unemp_{it} \times 1999_{t+2}) + \delta_3(unemp_{it} \times 2000_{t+2}) + \delta_4(unemp_{it} \times 2001_{t+2}) + \delta_5(unemp_{it} \times 2002_{t+2}))$$

status$_{it+2}$ takes three values (k = 1, 2, 3): 1 if the individual is unemployed, 2 if he is employed at a large firm, and 3 if he is employed at a small firm at time t + 2 (the last year in each panel). d_{t+2} is a set of time dummies. X_{it} is a vector of control variables. $unemp_{it}$ is an indicator equal to 1 if the individual is unemployed at time t (the first year in each panel) and 0 otherwise. Multinomial logit estimates.

Sources: Enquête Emploi, 1993–2002; authors' calculations.

small and large firms. To do so, we look at how the probability of working in large firms relative to being unemployed changed after the reduced workweek comparing with the equivalent probability for small firms. Table 9 shows that the probability of being employed relative to being unemployed increased after 1999 in large and small

Table 9. Employment

	Men		Women	
	log[P(large)/ P(unemployed)]	log[P(small)/ P(unemployed)]	log[P(large)/ P(unemployed)]	log[P(small)/ P(unemployed)]
1999	−0.009	−0.001	−0.056	−0.120***
	(0.037)	(0.032)	(0.038)	(0.031)
2000	0.151***	0.167***	0.013	−0.014
	(0.038)	(0.034)	(0.039)	(0.031)
2001	0.482***	0.519***	0.248***	0.215***
	(0.065)	(0.058)	(0.065)	(0.054)
2002	0.357***	0.409***	0.226***	0.250***
	(0.066)	(0.059)	(0.067)	(0.054)
Married	1.185***	1.106***	0.423***	0.472***
	(0.032)	(0.028)	(0.030)	(0.024)
Children under 6	−0.125***	−0.067***	−0.297***	−0.302***
	(0.025)	(0.022)	(0.026)	(0.021)
Age	0.169***	0.113***	0.099***	0.080***
	(0.009)	(0.007)	(0.009)	(0.007)
p-value for equality of coefficients on:				
1999	0.8		0.076	
2000	0.614		0.473	
2001	0.473		0.581	
2002	0.315		0.681	
N	58 595		55 434	

Notes: The comparison group is the unemployed. Estimates give the change in the log odds ratio for a one-unit change in the independent variable. Regression includes year dummies, region effects, education and age-squared. *** significant at the 1% level, ** significant at the 5% level, * significant at the 10% level.
Estimated equation:

$$P(status_{it} = k) = \Phi(\alpha + \gamma_1 1999_t + \gamma_2 2000_t + \gamma_3 2001_t + \gamma_4 2002_t + \beta X_{it})$$

$status_{it}$ takes three values ($k = 1, 2, 3$): 1 if the individual is unemployed, 2 if he is employed at a large firm, and 3 if he is employed at a small firm at time t. X_{it} is a vector of control variables.
Multinomial logit estimates.

Sources: Enquête Emploi, 1993–2002; authors' calculations.

firms by approximately the same amount, as the two coefficients on the years after 1999 are not statistically different for neither men nor women. Thus, even though employment increased after the law, it did not increase more in large firms relative to small firms, suggesting that the law was ineffective in creating employment.

An alternative way of making the same point is to plot the log of the probability of working in a large or in a small firm divided by the probability of being unemployed. Figure 4 shows that the log odds of employment by firm size are essentially parallel, suggesting that the 35-hour workweek had no effect on the level of employment.

5.2.4. Satisfaction with hours of work. The evidence produced so far suggests that the workweek-reduction law had a significant impact on society and it seems that
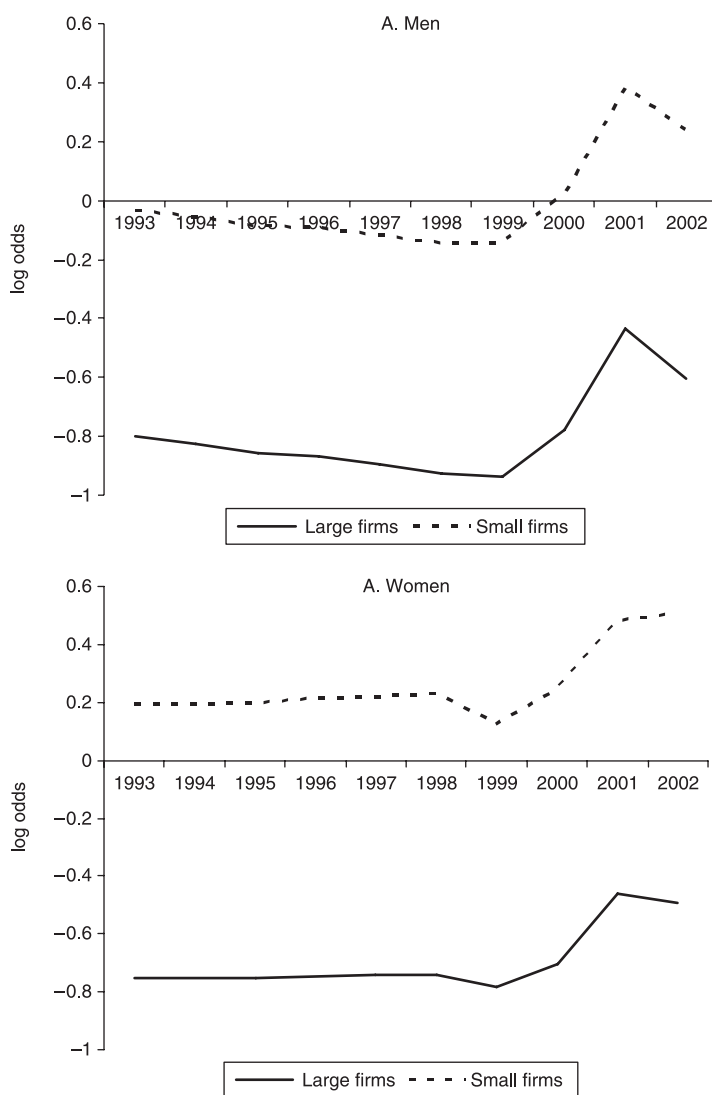
Figure 4. Log employment probabilites by firm size

Notes: The two series are log[*Prob*(*employed large firm*)/*Prob*(*unemployed*)] and log[*Prob*(*employed small firm*)/ *Prob*(*unemployed*)].

Sources: Enquête Emploi, 1993–2002; authors' calculations.

men may have suffered more from its negative consequences while women probably reaped more of its benefits. However, these are, to some extent, only indirect evidence of the effect of the law on people's satisfaction with their working hours. One way of bridging this gap is to look at subjective measures of satisfaction with hours of work. Previous studies (e.g. Cette *et al.*, 2004) have done that using data from a survey conducted by the French Ministry of Finance for 2001. However, we can go beyond

simply reporting individuals' average answers by using our identification strategy to uncover the causal effect of the law on satisfaction with the workweek.

Since information on satisfaction cannot be found in labour force surveys, we need to use an alternative data source. We use the French section of the ECHP, Waves 1 to 8, covering the years 1994 to 2001. This survey has a panel structure, following the same individuals over time. It has information on satisfaction with hours of work, with the amount of leisure time, and with working schedules.[12] An important aspect of this survey is that it has information on firm size, which allows us to apply our identification strategy by classifying individuals into the treatment group if they were working in a large firm in the last survey before the law was announced and into the control group if they were working in a small firm. As before, the outcome variable for each person in the panel is evaluated in the post-law-announcement period.

Before estimating the effects of the law on individuals' satisfaction it is important to recognize, again, possibly important differences within the affected population. For instance, on the basis of the evidence discussed so far women have benefited more from the 35-hour workweek than men. The reasons advanced in this paper are mostly cultural, as women are in general more severely bound by home production duties than men, and a free-market equilibrium of high working hours would hurt them more directly.

Other group-specific implications are also plausible. High-skill workers could have benefited more from the reduced workweek for several reasons: first, unlike other workers, the French press has reported that they were not subject to manipulation of working-time reporting, for example by squeezing lunch time or the time to change into uniforms; second, there is anecdotal evidence that low-skill employees began working more intensively through faster assembly lines to raise hourly productivity; third, high-skill employees could have more control over the organization of their time and production methods than low-skill employees. However, high-skill workers also earn higher hourly wages, thus making limitations on the amount of hours they work more costly to them. This effect could be minimized by the fact that for a given workweek size they would earn more overtime pay but that is equally true for low-skill employees. It is also important to consider that the law was applied more flexibly to managers, so it is possible that this category of workers were happier after the law. However, the law forced reorganization of production, changed schedules, and probably affected usual management procedures, which could have raised work stress among managers.

Unobservable differences across groups of individuals are also relevant. Those with greater preference for longer working hours will probably be more negatively affected by the imposition of a shorter workweek than others. However, we do not observe individuals' preferences but only workweek sizes before and after the law is announced.

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[12] Satisfaction with these variables is measured on a scale from 1 to 6. Satisfaction with working time refers to day times, night times, shifts, etc. There is some scepticism about the quality of subjective measures of happiness and satisfaction. But, in most cases, friends and colleagues of the individual give ratings which are strongly related to the way people rate themselves, as reported in Diener and Suh (1999). This is reassuring and suggests that these measures provide useful information on individuals' welfare.

For instance, instead of reflecting an individual's preference, a long workweek could be the result of institutional constraints or the imposition of firms (who could have significant power in setting the workweek due to labour market frictions, as discussed above in the review of relevant theories).

These issues can only be settled by appropriate empirical tests. An initial set of results is reported in Table 10, suggesting that, indeed, shorter working hours made men less happy with their working hours, leisure times, and working schedules, while women became less happy with working hours but not with the other two dimensions of work organization (panel A).[13] The discrepancy between men's and women's satisfaction with the new workweek becomes more dramatic when we control for trends in satisfaction that are not related to the law itself: as shown in Table 11, panel A, men's dissatisfaction with the new workweek is more marked when controlling for these trends, while women were better off after the 35-hour workweek in all dimensions available in the ECHP database.[14]

Other key individual characteristics do not offer a unified picture of how skill-intensity affects people's satisfaction with their new working time and gender differences seem to play a key role again.

- *Education*: As suggested above, more educated workers, in particular men, were not as hurt by the 35-hour workweek, although they seem less satisfied with their new shift times after the law (Table 10, panel B) and the results from a more complete specification to control for trends in satisfaction produce estimates with large standard errors (Table 11, panel B).

- *Occupation*: Managers were less satisfied with all aspects of their new working hours after the 35-hour workweek. In the basic specification without controlling for trends in satisfaction (Table 10, panel C), the only aspect of the new working week to appear somewhat beneficial to male managers was the workweek size, but this result is not robust to the inclusion of time dummies (Table 11, panel C). Overall, managers of both genders were less satisfied with their working hours, leisure time, and shifts after the introduction of the 35-hour workweek.

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[13] Formally, we used the following ordered probit to estimate the effect of treatment on the outcome variable for individual i:

$$P(y_{it} = k) = \Phi[\beta X_{it} + \delta_0 Treat_i + \delta_1 post_t + \delta_2 (Treat * post)_{it} + \delta_3 (Treat * Keyvar)_{it} + \delta_4 (Keyvar * post)_{it} + \delta_5 (Treat * post * Keyvar)_{it}]$$

y_{it} represents self-reported levels of satisfaction with hours worked, amount of leisure time, and working times (shifts), on a scale from 1 (not satisfied) to 6 (fully satisfied). $Treat_i$ is an indicator equal to 1 if the individual belongs to the treatment group (large firms) and 0 if she belongs to the control group (small firms). $post_t$ is an indicator variable that equals 1 for the period after the law was announced and zero otherwise. X_{it} is a vector of control variables, including region effects, age, net monthly income, an indicator for being married, an indicator for the existence of children under 12 in the household, an indicator for reported health problems, and $GDP * Treat$. $Keyvar_{it}$ refers to individual characteristics that could be relevant to explain the effect of the law. The relatively small sample size for this set of estimates limits the inclusion of interaction terms, which are done one at a time. For the same reason, year-specific interactions in the post-law period are avoided. An example of the problem is the higher standard deviations for the estimates in specifications including time-specific effects (see Table 11).

[14] Formally, the model includes a vector of year dummies, d_t, instead of the variable $post_t$ and can be written as:

$$P(y_{it} = k) = \Phi[\beta X_{it} + \delta_0 Treat_i + \delta_1 d_t + \delta_2 (Treat * post)_{it} + \delta_3 (Treat * Keyvar)_{it} + \delta_4 (Keyvar * post)_{it} + \delta_5 (Treat * post * Keyvar)_{it}]$$

Table 10. Satisfaction with hours of work, amount of leisure time, and working times (without year dummies)

	Men			Women		
	Working hours	Amount of leisure time	Working times	Working hours	Amount of leisure time	Working times
A. No interactions						
Treat	−2.345**	0.213***	0.240***	−3.059***	0.064	−0.335
	(1.205)	(0.035)	(0.061)	(1.006)	(0.194)	(0.236)
Post	0.927***	0.098***	0.028***	0.730***	0.050***	−0.077***
	(0.054)	(0.008)	(0.007)	(0.057)	(0.013)	(0.008)
Treat*Post	**−1.589***	**−0.102***	**−0.108***	**−1.786***	**0.075**	**0.131**
	(0.647)	(0.014)	(0.044)	(0.572)	(0.117)	(0.167)
B. Interaction with education						
Treat	−2.359**	0.214***	0.213***	−3.067***	0.040	−0.342
	(1.192)	(0.052)	(0.066)	(1.000)	(0.206)	(0.234)
Post	0.961***	0.052***	−0.011	0.737***	−0.050***	−0.211***
	(0.058)	(0.010)	(0.015)	(0.061)	(0.012)	(0.015)
Treat*Post	**−1.688***	**−0.173***	**−0.051**	**−1.816***	**0.054**	**0.265***
	(0.654)	(0.040)	(0.068)	(0.595)	(0.152)	(0.162)
High education	−0.063**	−0.220***	0.039	0.087**	−0.386***	−0.116*
	(0.026)	(0.073)	(0.050)	(0.037)	(0.083)	(0.063)
High education*Treat	0.023	−0.036	0.096***	0.004	−0.007	0.016
	(0.018)	(0.057)	(0.028)	(0.006)	(0.042)	(0.033)
High education*Post	−0.093***	0.237***	0.171***	−0.015	0.229***	0.295***
	(0.013)	(0.024)	(0.025)	(0.020)	(0.046)	(0.024)
High education*Treat*Post	**0.242***	**0.060**	**−0.248***	**0.068**	**0.067**	**−0.292***
	(0.041)	(0.031)	(0.036)	(0.076)	(0.070)	(0.033)
C. Interaction with occupation						
Treat	−2.338**	0.162***	0.185***	−3.036***	0.037	−0.340
	(1.210)	(0.023)	(0.047)	(1.017)	(0.222)	(0.241)
Post	0.943***	0.043***	0.050***	0.674***	−0.047***	−0.127***
	(0.056)	(0.008)	(0.014)	(0.060)	(0.014)	(0.010)
Treat*Post	**−1.629***	**−0.012**	**−0.046**	**−1.775***	**0.199**	**0.164**
	(0.662)	(0.026)	(0.052)	(0.575)	(0.153)	(0.177)
Manager	−0.180***	−0.623***	−0.180***	0.130***	0.021	−0.084**
	(0.040)	(0.092)	(0.051)	(0.024)	(0.041)	(0.040)
Manager*Treat	−1.000***	0.366***	0.480***	−0.199***	−0.004	−0.006
	(0.027)	(0.043)	(0.020)	(0.067)	(0.044)	(0.035)

Table 10. *Continued*

	Men			Women		
	Working hours	Amount of leisure time	Working times	Working hours	Amount of leisure time	Working times
Manager*Post	−0.123***	0.413***	−0.213***	0.397***	0.482***	0.323***
	(0.016)	(0.052)	(0.019)	(0.069)	(0.089)	(0.032)
Manager*Treat*Post	**0.259***	**−0.658***	**−0.381***	**−0.053**	**−0.749***	**−0.222***
	(0.079)	(0.041)	(0.028)	(0.098)	(0.098)	(0.062)
D. Interaction with family income						
Treat	−3.131***	2.059***	−1.299***	3.173***	2.706***	2.658***
	(0.799)	(0.267)	(0.153)	(0.932)	(1.046)	(0.754)
Post	0.630***	0.064	0.800***	2.788***	−3.571***	−1.599***
	(0.124)	(0.125)	(0.030)	(0.274)	(0.649)	(0.358)
Treat*Post	**−0.200**	**−1.401***	**1.918***	**−6.448***	**2.942***	**0.334**
	(1.162)	(0.367)	(0.513)	(0.460)	(0.730)	(0.420)
Log(Family income)	−0.139***	0.036	−0.059	0.387***	0.117***	0.113***
	(0.053)	(0.053)	(0.063)	(0.027)	(0.037)	(0.027)
Log(Family income)*Treat	0.083**	−0.197***	0.163***	−0.656***	−0.279***	−0.314***
	(0.042)	(0.023)	(0.020)	(0.059)	(0.105)	(0.072)
Log(Family income)*Post	0.033*	0.005	−0.081***	−0.219***	0.373***	0.156***
	(0.019)	(0.014)	(0.003)	(0.029)	(0.069)	(0.039)
Log(Family income)*Treat*Post	**−0.149**	**0.132***	**−0.213***	**0.488***	**−0.296***	**−0.019**
	(0.062)	(0.038)	(0.050)	(0.061)	(0.073)	(0.054)
N	993			544		

Notes: Robust standard errors adjusted for clustering by Treat × Post. Regressions include region effects, age, wage and salary earnings in year prior to the survey, an indicator for being married, an indicator for the existence of children under age 12 in the household, an indicator for reported health problems, and GDP × Treat. In addition, the regressions in panel A control for education and management occupations. High education is defined as tertiary education (undergraduate level) or higher. Management occupations include legislators, senior officials, managers, and professionals (science, health, and teaching professionals). The sample is limited to full time employees.
*** significant at the 1% level, ** significant at the 5% level, * significant at the 10% level.
Estimated equations:

$$P(y_{it} = k) = \Phi[\delta_0 Treat_i + \mu(GDP_t * Treat_i) + \delta_1 post_t + \delta_2(Treat \times post)_{it} + \beta X_{it}]$$

y_{it} represents self-reported levels of satisfaction with hours worked, amount of leisure time, and working times (shifts), on a scale from 1 (not satisfied) to 6 (fully satisfied). $post_t$ is an indicator equal to 1 if year t falls within the period after the law and 0 otherwise. X_{it} is a vector of control variables. $Treat_i$ is an indicator equal to 1 if the worker works in a large firm in 1997 and 0 if he works in a small firm. Ordered Probit estimates.

Sources: ECHP for France, Waves 1 to 8 (1994–2001); authors' calculations.

Table 11. Satisfaction with hours of work, amount of leisure time, and working times (with year dummies)

	Men			Women		
	Working hours	Amount of leisure time	Working times	Working hours	Amount of leisure time	Working times
A. No interactions						
Treat	0.051	0.089	0.063	0.246	0.354*	−0.438**
	(0.240)	(0.083)	(0.106)	(0.749)	(0.197)	(0.205)
Treat*Post	**−0.253***	**−0.190***	**−0.236***	**0.216**	**0.291***	**0.089**
	(0.143)	(0.115)	(0.139)	(0.377)	(0.178)	(0.187)
B. Interaction with education						
Treat	0.017	0.098	0.035	0.152	0.314	−0.471*
	(0.249)	(0.096)	(0.105)	(0.758)	(0.222)	(0.246)
Treat*Post	**−0.323***	**−0.255**	**−0.182**	**0.285**	**0.242**	**0.198**
	(0.190)	(0.158)	(0.197)	(0.364)	(0.221)	(0.271)
High education	−0.233***	−0.221**	0.032	−0.161***	−0.407***	−0.136
	(0.066)	(0.102)	(0.100)	(0.050)	(0.092)	(0.139)
High education*Treat	0.138	−0.035	0.102	0.346***	0.011	0.034
	(0.095)	(0.205)	(0.145)	(0.077)	(0.069)	(0.190)
High education*Post	0.068	0.236**	0.157	0.019	0.212*	0.305
	(0.158)	(0.105)	(0.208)	(0.107)	(0.122)	(0.299)
High education*Treat*Post	**0.090**	**0.067**	**−0.234**	**−0.182**	**0.120**	**−0.261**
	(0.234)	(0.274)	(0.273)	(0.137)	(0.223)	(0.364)
C. Interaction with occupation						
Treat	0.050	0.052	−0.016	0.205	0.306	−0.452**
	(0.240)	(0.071)	(0.113)	(0.760)	(0.247)	(0.212)
Treat*Post	**−0.249***	**−0.093**	**−0.198**	**0.254**	**0.404***	**0.122**
	(0.143)	(0.139)	(0.165)	(0.389)	(0.177)	(0.204)
Manager	−0.444***	−0.621***	−0.177**	−0.230***	−0.005	−0.105
	(0.054)	(0.080)	(0.080)	(0.071)	(0.221)	(0.075)
Manager*Treat	0.021	0.364***	0.478***	0.309	0.025	0.019
	(0.096)	(0.123)	(0.097)	(0.323)	(0.201)	(0.078)
Manager*Post	0.049	0.388***	−0.295	0.773***	0.519	0.373***
	(0.192)	(0.110)	(0.224)	(0.196)	(0.359)	(0.098)

Table 11. *Continued*

	Men			Women		
	Working hours	Amount of leisure time	Working times	Working hours	Amount of leisure time	Working times
Manager*Treat*Post	**−0.030**	**−0.640***	**−0.337**	**−0.407**	**−0.781****	**−0.253**
	(0.282)	(0.377)	(0.274)	(0.335)	(0.406)	(0.185)
D. Interaction with family income						
Treat	−1.199	1.969	−1.434	7.980***	2.925**	2.443
	(1.374)	(1.881)	(1.047)	(1.036)	(1.497)	(2.054)
Treat*Post	**1.564**	**−1.557**	**1.473**	**−6.372****	**3.115**	**0.313**
	(2.867)	(3.266)	(3.016)	(2.346)	(2.171)	(3.387)
Log(Family income)	−0.185***	0.038	−0.054	0.287***	0.096	0.089*
	(0.070)	(0.193)	(0.123)	(0.043)	(0.065)	(0.054)
Log(Family income)*Treat	0.135	−0.200	0.158	−0.081***	−0.271*	−0.300
	(0.140)	(0.199)	(0.114)	(0.079)	(0.141)	(0.208)
Log(Family income)*Post	0.064	−0.005	−0.146	−0.089	0.370***	0.159
	(0.252)	(0.247)	(0.296)	(0.181)	(0.120)	(0.261)
Log(Family income)*Treat*Post	**−0.194**	**0.140**	**−0.180**	**0.689****	**−0.292**	**−0.020**
	(0.300)	(0.340)	(0.312)	(0.230)	(0.234)	(0.344)
N	993			544		

Notes: Robust standard errors adjusted for clustering by Treat × year. Regressions include region effects, age, wage and salary earnings in year prior to the survey, an indicator for being married, an indicator for the existence of children under age 12 in the household, an indicator for reported health problems, and GDP × Treat. In addition, the regressions in panel A control for education and management occupations. High education is defined as tertiary education (undergraduate level) or higher. Management occupations include legislators, senior officials, managers, and professionals (science, health, and teaching professionals). The sample is limited to full time employees.
*** significant at the 1% level, ** significant at the 5% level, * significant at the 10% level.
Estimated equations:

$$P(y_{it} = k) = \Phi[\delta_0 Treat_i + \mu(GDP_t * Treat_i) + \delta_1 d_t + \delta_2(Treat \times post)_{it} + \beta X_{it}]$$

y_{it} represents self-reported levels of satisfaction with hours worked, amount of leisure time, and working times (shifts), on a scale from 1 (not satisfied) to 6 (fully satisfied). $post_t$ is an indicator equal to 1 if year t falls within the period after the law and 0 otherwise. d_t is a set of year dummies. X_{it} is a vector of control variables. $Treat_i$ is an indicator equal to 1 if the worker works in a large firm in 1997 and 0 if he works in a small firm. Ordered Probit estimates.

Sources: ECHP for France, Waves 1 to 8 (1994–2001); authors' calculations.

- *Income*: Taking the estimates for all aspects of workweek satisfaction together (Tables 10 and 11, panel D), the only consistently significant result is that women's satisfaction with their new working hours increased with their income level. The estimates for men seem to suggest the opposite, in general, although they often have large standard errors associated to them.

Overall, these results show heterogeneous reactions to the workweek reduction: more educated men and better paid women were happier, and managers were less happy after the introduction of the 35-hour workweek. The latter underscores hard-to-measure costs of the law in terms of administrative burden and decreased work quality.

The reduced workweek could also have affected people differently depending on how long they used to work before the law, an effect captured by the estimates in Table 12. The results show once more that women were more satisfied with their working hours after the law while men were not. However, the longer a woman worked before the law – probably a sign of her preference for longer hours of work – the less satisfied with the shorter workweek she was in the post-law period. The length of the workweek before the law was enacted was not a factor in explaining men's satisfaction with the new setup.

5.2.5. Summing up.   To close this section it is useful to take stock of the empirical evidence. We found that the 35-hours workweek had the following significant effects:

- Hours of work decreased after the introduction of the law. The decrease happened more rapidly in large firms, which were the first to introduce the shorter workweek.
- Hourly wages increased more for men than for women.
- Transitions out of employment increased for men, but, in general, not for women.
- After the law, large firms hired more people from unemployment during years when it sent more people into unemployment, suggesting increased job turnover.
- The overall level of employment does not seem to have been affected by the law.
- Dual-job holdings increased in 2000 for men, suggesting a desire to work more than the mandated number of hours.

Direct measures of satisfaction show that men were less happy with their workweek after the law while women were happier. However, women's positive reaction to the law was not homogeneous. Women's happiness with the workweek reduction increased with their income level and decreased with the number of hours they used to work before the law was enacted. The first effect suggests the existence of important complementarities between income and leisure. The second effect shows that even among women, individuals with strong preferences for longer hours were less happy with the reduced standard workweek (which also came associated to limitations on the amount of overtime work).

Table 12. Satisfaction with hours of work, amount of leisure time, and working times depending on hours worked in 1997

	Men			Women		
	Working hours	Amount of leisure time	Working times	Working hours	Amount of leisure time	Working times
Treat	−0.244	−0.003	−0.114	−0.042	0.076	−0.596**
	(0.311)	(0.141)	(0.147)	(0.843)	(0.319)	(0.263)
Treat*Post	**−0.526***	**−0.039**	**−0.243**	**0.829***	**0.486***	**0.134**
	(0.203)	(0.161)	(0.152)	(0.437)	(0.297)	(0.170)
Hours_1997−35	−0.036***	−0.037***	−0.038***	−0.051***	−0.041***	−0.018
	(0.011)	(0.009)	(0.005)	(0.006)	(0.014)	(0.012)
(Hours_1997−35)*Treat	−0.0001	0.007	0.012	0.041***	0.018	0.015
	(0.013)	(0.008)	(0.007)	(0.006)	(0.017)	(0.013)
(Hours_1997−35)*Post	0.010	0.019	0.013***	0.058***	0.029	−0.001
	(0.016)	(0.018)	(0.005)	(0.009)	(0.023)	(0.016)
(Hours_1997-35)*Treat*Post	**0.019**	**−0.005**	**0.004**	**−0.059***	**−0.010**	**−0.010**
	(0.017)	(0.019)	(0.007)	(0.016)	(0.028)	(0.016)
N	924			462		

Notes: Robust standard errors adjusted for clustering by Treat × year. Regressions include region effects, age, wage and salary earnings in year prior to the survey, an indicator for being married, an indicator for the existence of children under age 12 in the household, an indicator for reported health problems, GDP × Treat, and controls for education and management occupations. The sample is limited to full-time employees working 35 hours or more in 1997.

*** significant at the 1% level, ** significant at the 5% level, * significant at the 10% level.

Estimated equations:

$$P(y_{it} = k) = \Phi[\delta_0 Treat_i + \mu(GDP_t * Treat_i) + \delta_1 d_t + \delta_2(Treat \times post)_{it} + \delta_3(hours\_1997 - 35) + \delta_4(hours\_1997 - 35) * Treat) + \delta_5(hours\_1997 - 35) * Post) + \delta_6(hours\_1997 - 35) * Treat * post) + \beta X_{it}]$$

y_{it} represents self-reported levels of satisfaction with hours worked, amount of leisure time, and working times (shifts), on a scale from 1 (not satisfied) to 6 (fully satisfied). d_t is a set of year dummies. X_{it} is a vector of control variables. $Treat_i$ is an indicator equal to 1 if the worker works in a large firm in 1997 and 0 if he works in a small firm. ($hours\_1997 − 35$) is the difference between hours worked in 1997 and 35. Note that this difference is always positive since the sample is limited to individuals working 35 hours or more in 1997. Ordered Probit estimates.

Sources: ECHP for France, Waves 1 to 8 (1994–2001); authors' calculations.

Managers were less satisfied with their working time, leisure time and working shifts after the law. That could be the result of increased stress in this category of workers as firms reorganized production to adapt to the new law.

These findings tell a consistent story and provide evidence against the work-sharing hypothesis, while hinting that positive spillovers in leisure could be important for particular groups, although not in the aggregate. Complementarities between income and leisure may also exist for particular groups of people. The effect on hours of work shows that the law was indeed binding. The evidence on hourly wages and on dual job holdings suggests that there are other margins of adjustment, which reduced any potential positive impact of the reduction in hours on employment. In particular, the importance of wage reactions to determine the impact of workweek reduction laws mirrors what has been found in other studies: negative employment effects exist only for the group that obtained significant increases in hourly wages (men). The increase in dual-job holding among men suggests that some individuals looked for more hours of work (and, thus, income) at the margin.

The evidence on employment and on satisfaction with working hours provides a direct test of the work-sharing hypothesis and of the distributive effects of the law. The evidence on employment shows that the law changed the composition of the labour force, with larger firms letting more expensive employees (men) go, while hiring people from the unemployment pool, leaving the overall level of employment about unchanged. The presence of negative effects of a reduced workweek on men's employment (but not on women's) was also detected in the German case (Hunt, 1999), suggesting that the evidence and the framework developed here could extrapolate the French case. The evidence on satisfaction with working hours suggests that the group more exposed to bad market equilibriums (women) enjoyed the new workweek and the associated changes in work organization. Within this group, individuals earning lower income and the ones working longer hours were less happy with the law, which shows that complementarities between income and leisure are significant, and that the law was constraining to people with strong preferences for longer hours. Others (men), who may put higher priority on income than on having a shorter workweek, reported reduced levels of satisfaction with their working time. Managers also seem to have disliked the 35-hour workweek, a possible symptom of the stress associated to the reorganization of the production process triggered by the shorter workweek, but this is a subject for a future research project.

6. POLICY IMPLICATIONS

Our analysis suggests that the 35-hour workweek in France failed to create more jobs and raised job turnover. Previous work on the effects of the 35-hour workweek on employment emphasized that the law could have positive short-run effects on employment but that long-term cost pressures would undo them, at least partially. Our results show that the law did not even have short-run positive employment effects.

The importance of this result goes beyond the French case as it conforms to general economic principles, e.g. that employment effects will depend crucially on how hourly labour costs react to the reduced workweek. The same effect has been documented in other studies surveyed here. Groups of employees who react to the law by demanding higher hourly wages or who get the wage increase by law seem to lose jobs. Differently from other work, though, we have shown that the workweek-reduction laws could affect other adjustment margins beside flows to unemployment: firms may increase job turnover and employees could search for another job to compensate for lost income. These other margins of adjustment would decrease the potential of legislated workweek reductions to create more jobs. An aspect missing in our empirical analysis – a possible negative production-scale effect on total working hours after a reduction in the standard workweek – would boost our scepticism about the benefits of such policies on aggregate employment. To measure this effect we would need a matched employer-employee database and particular sources of hetero-geneity across firms. Evidence from Varejão (2006) using this type of data for Portugal shows that policy makers should be keenly aware of the scale effect as well.

Thus, the fallacy underlying the intuitive and simple idea of work sharing is that it fails to consider additional margins of adjustment other than hours and jobs. In particular, it seems too strong to assume that hourly wages will remain unchanged by the law. Or, as in the French case, to assume that, even if they increase, social security rebates and increases in labour productivity would annul cost pressures. In fact, what we can say is that, as people react differently to the same policy change, hourly wages will behave differently for each group of workers, which, given the same financial incentives and productivity increases in firms, will trigger redistributive effects. Unex-plained results in previous work for other countries (e.g. the negative impact of reduced standard workweek on men's employment in Germany as documented by Hunt, 1999) get a new light from our framework and estimates.

Reduced standard workweeks have the potential to benefit some groups at the expense of others, making aggregate evaluations of its welfare effects difficult. Our estimates show that the negative effects of a 'rat-race' equilibrium in the workplace seem to be relevant only for women, who probably value non-market activities more than men. In the same vein, our novel use of survey-based satisfaction measures underscores the importance of looking at group-specific reactions to the law to ana-lyse its impact on people's happiness. Some groups of high-skill employees seem to have benefited more from the reduced workweek and managers seem less happy with their working times and shifts than before the law. Workweek reduction laws will also impact people working very long hours differently from people working fewer hours. If the aim of the law is to avoid long workweeks, more targeted measures (for instance, higher overtime premiums after certain thresholds) seem more appropriate.

Overall, from the point of view of their effects on employment, workweek-reduction laws should be avoided. Substitution and scale effects seem generally to get in the way of positive employment responses. In addition, significant parts of the

population would probably feel severely constrained by the policy. However, other groups could be benefited by a reduced workweek. If a government wants to reach those groups, more targeted measures are probably the best way to proceed.

Discussion

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London School of Economics

While the benchmark neoclassical model of labour supply tells us that the worker is faced with a single hourly wage rate and can freely choose working hours to maximize utility, the vast majority of OECD countries has adopted working time regulations that typically restrict the length of the workweek. Moreover, there has been a generalized trend towards a shortening of the workweek in most countries during the past few decades. It is thus crucial to understand what kind of effects one should expect following the introduction of working time regulations, and in particular following an important reduction in the workweek. Such effects potentially concern a variety of outcomes, including employment, wages, worker welfare, profits and productivity.

This paper draws lessons from the recent reduction of the workweek in France, where the socialist coalition government cut the length of the workweek from 39 to 35 hours for large firms in 2000, and for small firms in 2002. Aggregate trends show that weekly working hours in France became substantially shorter than in a number of OECD countries, including the United States, the United Kingdom, Sweden or Italy, since the early 1980s, with a marked decline after 2001. At the same time, the French employment rate rose by four percentage points between 1999 and 2002. But this paper illustrates that appearances can be deceptive. The analysis throughout the paper is based on difference-in-differences (DD) estimates, where the control group is represented by workers employed in small firms, and the post-reform period is 2000–2. In fact we learn from these estimates that the 35-hour reform slightly decreased usual weekly hours, raised hourly wages (with ambiguous effects on total income), raised job turnover, and did not boost employment. One interpretation is that lack of employment effects are not surprising in light of the theoretical literature on work-sharing, which points at a number of reasons why hours and bodies may not be perfect substitutes in total labour demand, including mainly institutional constraints and the reaction in hourly wage. Another possible interpretation is that the shorter workweek was combined with more intensive use of overtime work (although at higher unit labour cost), without large adjustments on the extensive employment margin. To see how binding the reform has been, it would help to complement the results provided with a similar analysis of overtime work in small and large firms before and after the 35-hour law.

I find particularly novel in this paper the study of job satisfaction effects of the workweek reduction. If hours of work are kept unchanged after the workweek reduction,

and a wage premium is paid on overtime hours, worker welfare unambiguously increases. But firms may react to higher average labour costs by cutting the demand for hours per employee – plus there is a limit on the amount of total overtime hours – implying ambiguous effects on worker welfare. The results of this paper tend to suggest negative effects, at least for men, as signalled by a fall in the level of satisfaction with working hours, work shifts and leisure time. I find interesting the result that women from high-income households are more satisfied with their working hours as a consequence of the reform, suggesting some sort of complementarity between income and leisure for this group. But it would also be interesting to see other types of interactions as well. For example, individuals paid the monthly minimum income before and after the enactment of the 35-hour law should see their levels of satisfaction with working hours increase unambiguously, as their working hours would have fallen and their monthly income would have remained unaffected. This could almost be considered as a falsification test for the rest of the analysis on worker welfare. Also, interactions with levels of own hourly wages (as opposed to household income) may reveal the presence of costly substitution between work and leisure for high-wage workers.

I also have some general remarks concerning the identification strategy used throughout the paper. As the shorter workweek was introduced for different categories of firms at different points in time, the DD methodology comes as an obvious choice. At the same time, some features of the law may imply that the DD methodology has some limitations as well. First, DD results may at times read like a 'black-box', concealing important aspects of distribution dynamics. The distribution of working hours before and after 1999 certainly helps understanding some of the effects of the workweek reduction, but the analysis would be greatly enriched by transition matrices showing direct transitions among relevant hour brackets (0; 1–34; 35; 36–38; 39; 40+) in small and large firms over the reform period. As correctly noted by the authors, the DD estimation strategy cannot rely on a clear control group when looking at transitions between employment and non-employment, but complementary evidence on these can be precisely provided by transitions matrices.

Furthermore, falsification tests, based on interaction terms between the treatment group and the pre-treatment period, fail in several cases. This is likely to be due to the gradual adoption of the shorter workweek during the 1990s. Some incentives to worktime reductions were introduced in 1993, and expanded in 1996. The 35-hour law was then announced in 1998, to be implemented in 2000 in large firms and in 2002 in small firms, with substantial social security rebates to firms acting earlier than these dates. It is thus plausible to expect some anticipatory effects of the reform well prior to its enactment, due both to fiscal incentives and to necessary adjustment lags to reorganize production around shorter work shifts. But this may raise a concern: as the early adoption of the shorter workweek is voluntary, based on incentives and negotiations with the unions, there are issues of non-random selection of firms who shorten the workweek before or after 2000. The existence of systematic differences

between the unobservable characteristics of firms who cut their workweek before or after 2000 potentially complicates the interpretation of DD results.

Finally, the gradual adoption of the shorter workweek may pose problems for the choice of the control group. The underlying identifying assumption is that outcomes in the control group would not change as a result of the reform. Employees in small firms are used here as the control group, but anticipatory effects imply that indeed they may respond before 2002. And this is exactly what the distribution of working hours depicted in Figure 2 shows: after the announcement of the reform – but before the enactment of the shorter workweek for small firms – there is evidence of a clear spike at 35 hours in the distribution of working hours for small firms, while no spike at all existed in the pre-announcement period. This would in principle undermine the DD estimation strategy used in the paper – or at least should lead us to interpret the results provided as a lower bound for the true effects of interest.

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Panel discussion

Clemens Fuest pointed out that the paper's interesting results may not have broader policy implications, since the effects of the policy may be very different in firms much larger than the 50 employees threshold. Jean Imbs wondered whether a tendency of firms to bunch just below the threshold as the reform is implemented may affect the estimated effects. Clemens Fuest also commented on the relevance of work-sharing, which according to recent research was an important factor in Germany only before 1990 but not later on, and may more generally depend on economic conditions.

Several panel members discussed the policy implications of the paper. Philippe Martin noticed that in France the political debate on the reform during the presidential campaign was very polarized, but the paper indicates that the impact of the reform was much smaller than either candidate appeared to think. He and Georges de Ménil were intrigued by the analysis of differences in satisfaction with work, since in France some people think the law benefited mostly the better qualified and better paid employees. He added that the effects on satisfaction and on leisure are very interesting and appear far from negligible. Luigi Guiso pointed out that interactions between working hour regulation and income may be positive or negative, depending on whether high wages make people want to work more, or less though income effects. Barbara Petrongolo added that since managers and professionals were not covered by the reform, care is needed when looking at the relevance of income: it is not clear whether the substitution effect was predominant or not. Jorge Braga de Macedo recalled that firms' profitability also would have to be taken into account before concluding that the law had very small effects.

On the empirical approach, Pedro Portugal said that the estimated effects on flows out of employment are really very large in the paper, possibly reflecting only a

temporary impact on a small group of workers, with much smaller (if any) effects on steady state unemployment.

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