Worktime Regulations and Spousal Labor Supply[1]

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Abstract

Interdependencies in spousal labor supply have long been identified as a key question in the study of household behavior, but progress in this area has been limited by the lack of independent variation in working hours of one's spouse, as well as variation in hours that is not systematically accompanied by income changes. To overcome these issues, we exploit the unique design of the 1998 French workweek reduction, which introduced exogenous variation in one's spouse's labor supply, while keeping earnings constant. Men and women exposed to the shorter legal workweek reduce their weekly labor supply by about 2 hours, and do not experience any reduction in earnings. While wives of treated men do not adjust their working time at either the intensive or extensive margins, husbands of treated wives respond by cutting their labor supply by about half an hour to one hour per week. Husbands' labor supply response does not entail the renegotiation of usual hours with employers, but involves instead a reduction in non-usual hours, whether within a given day, or through an increase in the take-up rate of paid vacation and/or sick leave. The estimated cross-hour effects are consistent with the presence of spousal leisure complementarity for husbands, though not for wives.

Keywords: Spousal labor supply, Cross-hour effects, Workweek reduction.

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I. Introduction

Interdependencies in spousal labor supply have long been identified as a key question in the study of household behavior (see e.g. Ashenfelter and Heckman, 1974). Complementarities in labor supply and leisure within or beyond the household are also a key policy issue, as they represent a channel through which reforms targeted at specific segments of the population can ultimately affect a wider set of individuals. When the value of leisure time for an individual depends on the amount of leisure enjoyed by her family members, co-workers, neighbors, social contacts, etc., reforms of the welfare state, or tax reforms, or changes in workweek regulations aimed at some segments of the workforce may impact individual behavior well beyond the targeted population (Alesina, Glaeser and Sacerdote, 2005).

While spillovers in work and leisure represent an important and controversial issue, there is still little micro-level evidence on the actual magnitude of these effects. Progress in this direction has been limited by the difficulty of finding independent variation in the labor supply of one's peers, as individuals within the same social or family network may be subject to the same reforms, or more in general to correlated labor supply shocks. Another major challenge is that changes in leisure time and working hours for individuals are in most cases associated with important changes in their earnings. Thus the labor supply responses of peers cannot be interpreted as reflecting pure cross-hour effects, as they may also encompass income effects.

In this paper we exploit the specific design of the workweek reduction policy implemented in France in the late 1990s to overcome these issues and provide one of the very first micro estimates of the effect of an exogenous change in individuals' number of hours worked on the labor supply behavior of their spouses.

In June 1998 the French socialist government mandated a reduction of the legal workweek, from 39 to 35 hours, to be implemented in large firms by January 2000 and in small firms by January 2002, without an associated reduction in monthly earnings. This made the legal workweek in France (by far) the shortest among OECD countries (Lee, McCann and Messenger, 2007, Table 2.4). In order to attenuate the impact of higher hourly wages on profitability, employers who would implement the shorter workweek before the relevant deadline would benefit from significant cuts in their payroll taxes. In January 2000 the Government pushed back the deadline for full adoption of the shorter workweek, and only about 300,000 firms had implemented it before the comeback of the conservative party to power in April 2002 and the interruption of the original workweek reform. The reform implied a noticeable change in the workweek of at least one spouse in over one third of French households, with no direct impact on family income. Both within-household variation in the workweek reduction, and the absence of income effects, make the French worksharing reform a unique scenario for assessing cross-hour effects within the household.

In general, it is theoretically ambiguous whether a fall in working hours and thus an increase in 'non-market' time of one spouse would generate a fall or a rise in working hours of the other spouse. Substitutability in non-market time of husbands and wives could be driven by substitutable spouse efforts in home production. A reduction in the workweek of one spouse would shift some of her time endowment from market to home production, thus freeing up some home production time of the other spouse, who could devote more time to market work. Conversely, if one detects complementarity in non-market time of spouses, this would rather be consistent with complementarity of their leisure time. A reduction in the workweek of one spouse would increase her leisure time and thus raise the value of leisure of the other spouse if spouses enjoy spending time together. Alternative explanations for complementarity of non-market time could rest on other forms of social interactions, such as for example spouses influencing each other's perceptions and adjusting accordingly their preferences about work, leisure and work-life balance.

This paper uses a unique matched worker-firm dataset obtained by combining the French Labor Force Survey with firm level information on the implementation of the shorter workweek, in order to estimate the labor supply response of men and women to a reduction in the legal workweek in their spouses' workplaces. We detect an average reduction of about 2 hours in the workweek of both male and female employees whose employers signed a workweek reduction agreement.[2] When looking at spousal responses, we find that women do not adjust their labor supply, whether at the intensive or extensive margin, when their husbands become subject to the shorter workweek. Men, by contrast, tend to work about 0.5 hours less per week when their wives become treated, independent of whether or not a man's own employer signed a workweek reduction agreement at the same time as his wife's employer.

Further tests reveal that men's labor supply response to wife treatment is not associated with a reduction in their usual working hours, but with a reduction in the 'non-usual' component of their workweek. Moreover, such response does not seem to have a detrimental impact on their earnings, suggesting that men manage to cut on some form of unpaid work involvement, whether within a given day, or through an increase in the take-up rate of paid vacation and/or sick leave. If employees do not use their whole paid leave entitlement, or simply spend some unproductive time at work, they have some leeway in cutting their hours while avoiding earnings losses, and it is mostly by adjusting around these unpaid work margins that men respond to shorter workweek agreements in their wives' firms.

Our estimates are all the more suggestive as the direct (first-stage) effect of shorter workweek agreements on treated wives is estimated to be only about 2 hours. Assuming that the workweek reduction in wives' firms affects their husbands only via wives' labor supply, we provide an instrumental variable estimate of the average cross-hour effect for husbands of

[2] As discussed below, there are various reasons why the average effect of the shorter legal workweek on actual weekly hours is lower than the legal workweek reduction, including the fact that the shorter legal workweek may not have been binding for employees initially working below 35 hours.

0.24, rising to 0.38 for managers and professionals, and to 0.58 for fathers of young children. The interpretation is that managers and professionals work relatively longer hours and have much closer control on their actual hours than employees in less-skilled occupations. For fathers, the leisure complementarity motive is plausibly stronger than for the childless, if children play the role of household public good that parents would enjoy together (Lundberg, 1988).

By looking at interdependencies in labor supply within the household, our paper relates to a long strand of literature on family labor supply, dating back (at least) to seminal work by Ashenfelter and Heckman (1974). This literature typically investigates the response of an individual's labor supply to independent changes in her spouse's income and/or hours of work. These changes may in turn be driven by events as diverse as retirement, job loss, fiscal reforms, etc. There is a fairly large literature documenting a positive correlation between husbands' and wives' retirement decisions, over and above what would be predicted by correlation in age and incentives in the retirement system (see, among others, Blau, 1998, and Gustman and Steinmeier, 2000). Conversely, the added worker effect literature detects a mild substitutability between spousal labor supply, as married women tend to increase their working hours following husband's job loss (Lundberg, 1985, Cullen and Gruber, 2000). More recently, Gelber (2011) exploits the Swedish tax reform of 1990-91 to examine the response of husbands' and wives' earnings to a change in the marginal tax rate for the other spouse, and shows that as net-of-tax earnings of one's spouse rise, own earnings rise too. Insofar earnings responses reflect labor supply responses, these findings suggest complementarity in spousal leisure. Complementarity is also detected by Hamermesh (2002), who finds that spouses' daily work schedules are more synchronized than would occur randomly. While building on very different sources of variation, these papers tend to agree in documenting important spillovers in the labor supply behavior of spouses.

Our contribution to this literature is threefold. First, we exploit independent variation in spousal hours of work, while keeping monthly earnings constant. This allows us to abstract from income effects of changes in spousal labor supply, and focus on pure cross-hour effects. In particular, under the assumption that an employee's workweek regulations affect their spouses only via their labor supply, we can recover an estimate of the structural parameter capturing leisure complementarity in the utility functions of spouses. Secondly, while most of the existing literature has focused on the labor supply response of secondary earners, we show in this paper that it is in fact men who significantly cut their working hours following the adoption of the shorter workweek in their wives' workplaces, while the corresponding women's response is close to zero. This may in turn be due to different degrees of leisure complementarities in spouses' utility functions, or a greater ability of men to control their working schedules. Thirdly, we provide evidence on specific adjustment margins in labor supply spillovers, and in particular we find that it is mostly husbands' unpaid involvement in their workplace that is affected when their wives' workweek is reduced.

Our paper is also related to the literature on work-sharing policies in developed countries. The study which is closest to ours is Hunt (1998), who shows that the gradual decline in standard working hours of male, German employees between 1984 and 1994 was not accompanied by changes in their wives' employment rates, but nevertheless produced a small decline in their hours of work. These results, while consistent with complementarity in spousal leisure, may also reflect underlying trends in female labor supply in Germany over this period, including wives' own gradual exposure to shorter standard workweeks. Finally, our paper adds to the literature evaluating the effects of workweek reduction reforms in France (see e.g. Crépon and Kramarz, 2002, Askenazy, 2008, Estevao and Sa, 2008, Chemin and Wasmer, 2009). Existing evaluations typically focus on the employment effects of such reforms by comparing employment levels across large and small firms (or across regions) before and after the introduction of the shorter workweek. The focus and methodology of our paper are different in that we exploit variation in the exact dates of implementation of the workweek reduction across firms to investigate the labor supply response of individuals to their spouses' reduction in working hours.

The paper proceeds as follows. Section II gives an overview of the workweek reduction reform. Section III describes the data used and provides some graphic analysis of the effect of the workweek reduction on treated individuals and their spouses. Section IV presents our main regression results. Section V shows robustness of our results and addresses a number of caveats to a causal interpretation of our estimates. Section VI provides instrumental variable estimates of cross-hour effects, using mandated workweek reductions as instruments for spousal labor supply. Section VII finally concludes.

II. Historical and institutional context

Since the early 1980s, the legal workweek in France has been 39 hours. The overtime wage premium was 25%, and the maximum number of overtime hours per worker was set at 130 per year. In 1993, the French economy went through one of the most severe recessions of the post-war period, accompanied by a rapid increase in unemployment, reaching the peak rate of 12% in 1996. In this highly depressed context, the French conservative government passed a law offering private firms fiscal incentives to expand employment levels through a workweek reduction (*Robien* law). The impact of the *Robien* law was very limited, with less than 3,000 workweek reduction agreements signed with unions, affecting less than 2% of the workforce (see Fiole and Roger, 2002). The law did not modify the legal workweek, which stayed at 39 hours.

In April 1997, the French president Jacques Chirac dissolved the parliament and called general elections one year ahead of the end of the legislature. This decision was highly unexpected and the electoral campaign that followed was very short. The socialist party proposed a program whose main axis was the reduction of unemployment through worksharing, with two basic slogans: "*travailler moins pour travailler tous*" (work less in order to work all) and "*35 heures payées 39*" (35 worked hours paid 39). The left coalition won the election in June 1997.

The workweek reduction was implemented in two steps (see Askenazy, 2008, for a detailed description of the reform). The first law (*Aubry* I, named after the then labor secretary Martine Aubry) was passed in June 1998. It set the legal workweek at 35 hours in the private sector and mandated that the new workweek be implemented by January 2000 in firms with more than 20 employees, and by January 2002 in smaller firms.[3] Hours worked beyond the 35th hour would be treated as overtime hours, subject to a 25% hourly wage premium and to a maximum of 130 overtime hours per employee per year. Also, the law stipulated that firms who would implement the shorter workweek through a collective agreement with unions before the relevant deadline would benefit from substantial cuts in payroll taxes,[4] provided that they commited to maintain employment levels.

Finally, the law required that workers should not experience a drop in their monthly earnings following the legal workweek reduction. In particular, firms who signed a 35-hours agreement had to grant a specific (4 hours) bonus to workers paid the monthly minimum wage. The general purpose of the law was to induce firms to raise employment levels by worksharing, while offering them fiscal advantages to attenuate detrimental impacts of this reform on profitability.

In January 2000, the second law (*Aubry* II) introduced additional regulations in order to limit the cost of the shorter workweek to employers. In particular, it became possible to implement the shorter workweek via slightly modified definitions of working time, without losing eligibility for fiscal aids. For example, it became possible for employers to exclude 'unproductive breaks' from the definition of working time. Also, firms could introduce

[3] There were no explicit deadlines set for firms in the public sector.

[4] For workers paid at the minimum wage, the tax cut corresponds to a reduction of about 8% in total labor cost for 5 years.

shorter working hours on an annual – rather than weekly – basis, with a cap on annual hours being set at 1600. In practice it means that a collective agreement could be signed, and fiscal advantages obtained, even with actual reductions of working hours below 10%. Most importantly, the new law introduced a two-year transitional phase during which it became possible for employers to keep the 39-hour workweek by using overtime at a reduced 10% rate. [5]

Two years later, in summer 2002, the conservative party came back to power and, while the Aubry laws remained formally in place, the whole transition to the shorter workweek was discontinued in practice. The new conservative government raised the maximum number of overtime hours from 130 to 220, and extended fiscal incentives to all firms, including those that did not sign workweek reduction agreements. In this new scenario firms could effectively have employees working 39 hours weekly, at no extra hourly cost with respect to the pre-reform period. As a consequence of these political changes, the 35-hour was never fully implemented, especially in small private firms. Nevertheless, the *Aubry* laws have had a very large impact on the French economy, with roughly 350,000 agreements signed, covering about 10 million workers.

To sum up, the French workweek reform had several important features: it was largely unexpected; it has been interrupted, with only a fraction of workers being affected; it did not affect monthly earnings; and given its gradual implementation it would likely not treat spouses in a given household at the same time. In the remainder of this paper, we will build on these features of the reform in order to evaluate the effect of an exogenous variation in an employee's workweek on the number of hours worked by her spouse.

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[5] Furthermore, employers were no longer required to commit to maintain employment levels in order to be eligible for payroll tax cuts.

III. Data and descriptive evidence

III.1 Data

We combine individual level information on worker characteristics and working hours with firm level information on collective agreements signed by employers who adopted the shorter workweek.

We use individual records from the French Labor Force Surveys (hereafter, LFS), which is conducted each year by the French Statistical Office (*Institut National de la Statistique et des Etudes Economiques*, herafter, INSEE). Before 2003, the LFS was conducted in March of every year, and covered a representative sample of about 100,000 households each year (with a 1/300 sampling rate). From 2003 onward, the survey is conducted each quarter and covers a representative sample of about 55,000 households each quarter. Our main analysis will be based on all repeated cross-sections from 1994-2002, namely all annual surveys 1994-2002, and all first quarter surveys for 2003-2009.

For each household member aged 15 or above, the LFS provides information on gender, marital status, employment status, occupation, educational level, industry, monthly earnings and hours worked during the previous week.

Crucial for our purposes, our restricted use version of the LFS also provides employer identifiers. Specifically, each employee is asked to report the name and address of her employer, and this information is coded by INSEE. The coded employer identifier is available for just over 80% of the employees in the LFS.[6] This information allows us to match worker level information with firm level information from the DARES-URSSAF dataset, an administrative database collected by the French Ministry of Labor, which provides detailed information on all firms who signed a workweek reduction agreement, including the signing and implementation dates. We thus obtain a matched employer-employee dataset containing

[6] Most cases with missing employer ID correspond to very small firms. For a detailed description of the coding procedure, see Abowd and Kramarz (1999) or Goux and Maurin (1999).

information on working hours of respondents and their spouses, as well as information on when, if ever, their employers implemented the shorter workweek.

The matched employer-employee dataset used has some clear advantages compared to the non-matched LFS. First, it allows us to identify which workers were actually treated, and not simply the intention to treat based on the number of employees in their firms and the proximity to the law deadlines.[7] Also, the information on the exact date of treatment makes it possible to exploit the gradual implementation of the shorter workweek, thus avoiding to solely rely on the announced 2000 and 2002 deadlines.

III.2 Descriptive statistics

In what follows we focus on a sample of married or cohabiting respondents, whose spouse is a wage-earner, and we focus on the labor supply response of main respondents to spousal exposure to the shorter workweek. We restrict our analysis to respondents aged 18-65, and drop the small number of those whose spouses' employers signed an agreement either before 1996 or after 2002, since it is not clear whether these early and late agreements really correspond to the reforms implemented in the late 1990s. Our working sample includes 189,894 males and 236,802 females.

Table I provides some basic descriptive statistics on our sample, distinguishing between male and female respondents, and by the treatment status of their spouses. Throughout the paper we define as treated all spouses whose employers ever implement a workweek reduction agreement. Both men and women are less likely to work in the public sector when they have a treated spouse, which is consistent with the reform having mostly affected the private sector. But the age and years of education of both men and women are nonetheless very similar whether or not their spouses are treated.

[7] For example, a recent contribution by Ahmed (2009) compares wives' labor market transitions before and after 1998 using the small fraction of wives whose husbands are part-timers in small firms as a control group.

Table II reports the distribution of own and spousal legal workweek status in the employed sample, and shows that about 54% of husbands of treated women are not treated themselves by the workweek reduction (Panel A, column 2), while about 29% of husbands of non-treated women are treated. Thus there is some assortative mating along the treatment dimension, but spouses have nonetheless different treatment status in a large proportion of cases. Furthermore, even when both spouses are treated, the timing of treatment differs for about half of the couples. Panel B shows a very similar picture for wives of treated and non-treated men. To further illustrate timing of treatment, Figure I graphically shows the gradual implementation of the shorter workweek for spouses of employed respondents, i.e. on the same sample described in Table II. While only about 40% of employees are eventually treated in this sample, there is substantial variation in treatment dates between 1998 and 2002. Information on exact dates of treatment thus allows us to separately identify the direct and cross-effects of shorter workweeks across spouses, as in the majority of cases the year of treatment differs across spouses.

III. 3 Graphical evidence: Direct and cross-effects of treatment

Before moving on to regression analysis, below we provide some simple graphical evidence on the direct and cross-effects of the workweek reform on the number of hours worked by spouses in our sample. Figure II plots hours worked during the survey week by wives who are wage earners, by treatment status (189,894 observations in total). The solid line refers to treated wives, and time zero refers to the year in which a shorter workweek agreement is implemented at their workplace. Their weekly hours are stable, if anything slightly rising during the pre-treatment years, and drops by about 2 upon treatment. The dotted line refers to non-treated wives, and reports their working hours for the same dates at which treated wives were observed. Their weekly hours follow a gradually rising trend throughout the sample period, with no break at time 0. Thus we observe a decline of about 2 hours in working hours of treated wives relative to control wives at time of treatment. Interestingly, wives that become treated have longer weekly hours initially, and their hours converge almost exactly to hours of non-treated wives when their employers adopt the shorter workweek.

This observed drop in weekly hours for treated wives relative to the non-treated is a first-stage effect for the cross-hour effect that we intend to analyse next. A first-stage effect of about 2 hours is equivalent to roughly half the reduction in the legal workweek (i.e. the intended direct effect), and this may be explained by a number of factors. In particular, part of the implementation of the worktime regulation may have taken place with slight modifications of working time definition (for example excluding unproductive breaks from the hours count) or reducing the number of weeks worked per year rather than the number of hours worked per week, keeping usual weekly hours constant (see also Askenazy, 2008). This would deliver a mitigated effect of the workweek reduction on mean actual hours in the LFS, as the survey week falls in March of each year, and thus tends not to coincide with popular holiday seasons. Finally, the effect of the introduction of the 35-hour workweek has also been mitigated by the fact that about 30% of French female employees work part-time, and for them the shorter workweek would not be binding. The estimated 2-hour drop in working hours can be interpreted as an average of a higher drop for women initially working more than 35 hours, and a smaller drop for women initially working less than 35 hours.[8]

Given the behavior of treated wives, the next question is whether we observe a variation in either the employment rate or the number of hours worked by their husbands. Figure III shows flat and virtually identical employment patterns of husbands of treated and non-treated wives. When focusing on a [-3, +3] years window around time of treatment, the difference in employment rates between husbands of treated and non-treated wives is very

[8] This specific explanation, however, would not work for men, as the proportion of part-timers among male employees is negligible.

small (about 1.5 percentage points) and almost exactly the same before and after the time of the treatment. Thus we detect no spillover effects at the extensive margin.

Figure IV then addresses corresponding variations at the intensive margin, by showing the impact on hours worked by the subsample of employed husbands, and reveals a sizeable drop in average working hours of husbands of treated women, relative to husbands of non-treated women. Specifically, the difference in working hours is close to zero during the five pre-treatment years, and rises to nearly 1 during the five post-treatment years. Panel A in Figure A1 plots the difference between the two series, as well as the associated 95% confidence interval, and shows clearly the absence of any pre-treatment trend, as well as the drop in the post-treatment period.

These spillover effects may in part reflect the fact that some husbands were themselves employed in firms who adopted the shorter workweek, and thus became treated at the same date as their wives. To purge this effect out, we replicate the same trends on a sub-sample that excludes households in which spouses became treated at the same date, and still observe a clear change in the relative number of hours worked by husbands of treated wives at the time of treatment. The same result holds when we further restrict the sample to households in which the husband was never treated (graph not reported). In the regression analysis that follows we will pool all households and control for own and spouse treatment separately.

Figures V to VII repeat a similar analysis for female respondents and their husbands. Again we observe a clear drop in working hours of treated relative to non-treated husbands (see Figure V), whose magnitude is very close to that we observed for wives in Figure II. However, we find no evidence of spillover effects on their wives' labor supply, either at the extensive margin (Figure VI), or the intensive margin (Figure VII). Panel B in Figure A1 plots the difference between the two series of Figure VII, and shows that they are never significantly different from zero, except in one year of the post-treatment period.

To summarize, our descriptive evidence is suggestive of labor supply spillovers at the intensive margin for husbands of treated wives, but no spillovers at the extensive margins or for wives of treated husbands. The next section will show estimates of these effects that control for observable characteristics of the individuals, and explore further the nature of these spillovers.

IV. Regression results

IV.1 Main estimates

As in the previous descriptive analysis, we focus on two main outcome variables for each individual i in our sample, namely her employment status and her weekly hours worked, and assess how each is affected by the implementation of a shorter workweek agreement by her spouse's employer. This would work via an effect on the spouse's labor supply, and thus we start by estimating a first-stage specification that regresses spouse working hours on treatment variables and other covariates. We denote by H_{it}^{S} the actual weekly hours worked by the spouse, and introduce a dummy variable A_{it}^{S} indicating whether at time t she works for a firm who has ever adopted the shorter workweek. Our first-stage regression is the following difference-in-differences specification:

$$H_{it}^{S} = \alpha_1 A_{it}^{S} + \alpha_2 APost_{it}^{S} + \alpha_3 X_{it}^{S} + D_t + u_{it} \tag{1}$$

where $APost_{it}^{S}$ indicates the period following a workweek reduction in the spouse's firm, D_t denotes a set of year fixed effects, and X_{it}^{S} are relevant individual covariates, including a constant term. The α_2 coefficient shows the direct (first-stage) effect of workweek regulations on labor supply.

Table III shows the regression results for specification (1) for wives (Panel A) and husbands (Panel B) of main respondents. All reported standard errors in this and later tables are clustered at the year*treatment level (32 clusters). Column (1) in Panel A shows that wives working in firms who implemented a workweek reduction agreement were working about 1.36 hours more than wives in other firms in the pre-reform period, but then cut their labor supply by about 1.81 hours per week once the shorter workweek was implemented. This pattern of working hours was also evident from Figure II, and the only difference here is that we control for aggregate time effects and a public sector dummy. Turning to husbands, column (1) in Panel B shows small pre-treatment differences (-0.28 hours), but again strong and significant effects of the workweek reduction (-1.95 hours). All these estimates are robust to the introduction of controls for age, education and industry effects in column (2), suggesting that the implementation of the shorter workweek was largely orthogonal to these job and worker characteristics.[9]

Columns (3) and (4) in each panel report estimates of a similar specification for (the log of) monthly earnings, and once extra controls are included these show near zero effects of the workweek reduction on the earnings of wives and husbands. These first-stage results are clearly in line with the reform's intended outcome to shorten the workweek without cutting monthly earnings of treated employees. If anything, the effect of the shorter workweek on the monthly earnings of husbands is positive rather than negative, albeit tiny, and only significant at the 10% level.

We next assess labor supply spillovers by looking at the reduced-form effects of the workweek reduction in the spouse firm on the employment status of the main respondent and her weekly hours. Note that we can interpret such cross-effects as stemming from the sole reduction in the amount of time spent at work by the spouse once we have ruled out the presence of income effects, as shown in columns (3) and (4) of Table III. Our reduced-form specification for employment is

[9] Among related work, Gelber and Mitchell (2012) study the impact of changes in US taxes and transfers on single men's and single women's own labor supply and housework, while Lee, Kawaguchi and Hamermesh (2011) look at the impact of changes in Japanese and Korean legal standard hours on individuals' own housework and leisure time.

$$E_{it} = \beta_1 A_{it}^S + \beta_2 APost_{it}^S + \beta_3 X_{it} + D_t + v_{it}, \quad (2)$$

where E_{it} is a dummy variable that is equal to 1 for the employed and 0 for the nonemployed. For hours worked, we restrict the sample to employed individuals and estimate

$$H_{it} = \gamma_1 A_{it}^S + \gamma_2 APost_{it}^S + \gamma_3 A_{it} + \gamma_4 APost_{it} + \gamma_5 X_{it} + D_t + \varepsilon_{it}, \quad (3)$$

where H_{it} denotes weekly hours conditional on working, A_{it} is a dummy variable denoting whether the current employer of the main respondent has ever implemented a shorter workweek agreement, whereas $APost_{it}$ indicates the period following this agreement. The main coefficients of interest are β_2 in model (2) and γ_2 in model (3). Note that these specifications allow us to estimate cross-effects in labor supply (captured by $APost_{it}^S$), over and above the direct effect of own treatment (captured by $APost_{it}$). These two effects can be separately identified insofar treatment is not simultaneous for all spouses.

The regression results are reported in Table IV. Columns (1) and (2) refer to employment, and columns (3)-(5) refer to weekly hours. Estimates show no evidence of any significant cross-effects on employment for men, and the associated point estimate is always very close to zero, in line with the trends reported in Figure III. For women, the cross-effect on employment becomes marginally significant when further controls are included in column (2), but its magnitude stays very close to zero. As we find virtually no impact on employment, we next look at hours worked for those who are employed.[10] In column (3) of Panel A we regress men's hours on own treatment variables (A_{it} and $APost_{it}$), and on their wives' treatment variables (A_{it}^S and $APost_{it}^S$). The own treatment effect is about -2,[11] and the cross-effect is -0.44 and highly significant, showing that when their wife becomes treated by the

[10] We also estimated first-stage regressions like (1) for the spouses of employed respondents, and the results are reported in Table A1 in the Appendix. The own effect of the shorter workweek is -1.8 hours for wives, and -2.1 hours for husbands, and again we find near zero effects on monthly earnings.

[11] Note that this first-stage effect is virtually identical to that estimated in first-stage regressions reported in Panel B of Table III. However the two samples differ as Panel B of Table III includes male employees, and Panel A of Table IV includes husbands of female employees.

shorter workweek, working men reduce their weekly labor supply by nearly half an hour. The magnitude of this effect does not change when we control for individual characteristics of respondents (column (4)), or when we exclude own treatment variables A_{it} and $APost_{it}$ (results not reported). Finally, the estimated cross-effect stays largely unchanged when we exclude men who are themselves treated at some point during the sample period (column (5)).

Panel B reports corresponding estimates for women. While the own effect of workweek regulations is negative and significant, the cross effect is positive, small, and not significantly different from zero. We thus detect no evidence of any spousal spillover in the labor supply of women.

Our estimates provide one of the first pieces of evidence showing that changes in the workweek of a subsample of employees may have a very significant impact well beyond the targeted population. A simple back-of-envelope calculation can help quantify overall spillover effects. In particular, the adoption of the shorter workweek implies a reduction of nearly two hours in the labor supply of married women, and nearly half an hour in the labor supply of their husbands. Assuming for simplicity that the same probability of treatment (0.4) and the same first stage effect (-2 hours) would apply to all categories of workers, the average direct effect of the reform on the male labor supply would be $-0.4 \times 2 = -0.8$ hours, whereas the average cross-effect would be $-0.4 \times 0.61 \times 0.5 = -0.12$ hours, where 0.61 represents the proportion of male workers who are married to a female wage earner. Thus neglecting spillover effects would lead to an underestimate of the overall impact of the workweek reduction on male labor supply of about $0.12/(0.12 + 0.8) = 13\%$. Given that men represent about half of the overall employed population, this implies underestimating its impact on the overall population by about $0.12/(0.12 + 0.8 + 0.8) = 7\%$.

IV.2 Further estimates: Cross-effects on usual and non-usual working hours

We have shown that, following a workweek reduction at their workplaces, women work about 1.9 hours less per week at constant earnings, and their husbands respond by cutting their labor supply by about half an hour. The aim of this section is to assess the nature of these labor supply spillovers. How did respondents manage to cut their actual hours just after an agreement was signed in their spouses' firm?

One simple way to shed light on this question is to exploit information provided by the LFS on the difference between actual hours (that we denoted by H) and usual hours (that we will denote by H_u), defined as the number of hours worked in a typical week.[12] Using this information, actual hours H can be conceptualized as the sum of a usual number of hours H_u and a non-usual component $H - H_u$, which may be either positive or negative, depending on whether overtime hours exceed various forms of unworked hours (e.g. unusually short working days, sickness absence, paid or unpaid leaves, etc.) in a given week.

Based on this distinction, there are two ways of reducing weekly hours H. The first one is to negotiate a new contract with one's employer, involving a smaller number of usual hours H_u. The second way is to keep one's contract unchanged, together with the associated usual hours H_u, but to reduce $H - H_u$, and namely some form of work involvement that is typically not specified in the contract. It may involve a reduction in overtime work or an increase in the take-up rate of leaves or an increase in absenteeism. It would be reasonable to expect that the observed cross-effects mostly occur through reductions in $H - H_u$, since such reductions would not require the renegotiation of one's labor contract, and are more easily under an employee's individual control than adjustments in usual hours H_u. By contrast, the

[12]According to the official ILO (2002) definition, usual hours per week represent "the modal value of the number of hours actually worked per week over a long period of time". This definition is applicable to workers with regular schedules only (about 85% of cases in the LFS). It does not include irregular or unusual overtime (whether worked for a premium pay or not compensated at all) nor unusual absence or rest. As also noted by Chemin and Wasmer (2009), French labor laws require labor contracts to be explicit about hours, pay, tasks and paid leaves, and as a consequence interviewees would know precisely their normal weekly hours as well as contractual changes in these.

direct effect of the law should be expected to bite on H_u, consistently with the collective nature of these agreements.

To look into possible adjustment margins, columns 1 and 2 of Table V re-estimate our reduced-form specification (3), using H_u and $H - H_u$ as dependent variables in turn. The sample period is now restricted to 1994-2002, as the information on usual hours is only available until 2002 in the LFS. These regressions show a sizeable (-0.59) cross-hour effect for non-usual hours $H - H_u$ only, but no cross-effect on usual hours H_u. This finding confirms that cross-effects on husbands' hours did not occur through a renegotiation of usual working schedules, but mostly through a reduction in non-usual hours. As anticipated, the direct effect of the shorter workweek mostly happens on usual hours (-1.99). For women, we did not detect significant cross-effects on either usual or non-usual hours (results not reported).

We next test whether cross-effects in male hours were accompanied by earnings losses. Column 3 of Table V shows that it is clearly not the case: using the same reduced-form specification as in Table III, we find that if anything a workweek reduction agreement in the wife's workplace is associated with a 0.8% increase in husband's earnings, and this effect is marginally significant. Column 4 re-estimates the same specification on the 1994-2002 sample period, consistently with columns 1 and 2, and the results show lack of earnings effects at any conventional significant level.

As for how adjustments in non-usual hours may be achieved in practice, recent waves of the LFS confirm that there exists significant leeway for most employees, and especially for the high-skilled, in reducing their individual involvement in the workplace. From 2003 onwards the LFS provides information on the take-up rate of paid leaves, as well as on paid and unpaid overtime work. During the 2003-2009 period, about 12% of male employees declare that their paid holiday entitlement exceeded the amount of paid leave actually taken by one week or more. Also, about 23% declare that they have worked overtime in the survey week, and that over 61% of their overtime hours were unpaid. For employees in high-skill occupations, about 37% have been working overtime, and about 84% of their overtime hours were not remunerated (see Table A2).

Adjustments via these margins are further explored in Table VI, where we report cross-hour effects on overtime hours and unworked hours, separately. These are defined as $(H - H_u)^+ = \max(H - H_u, 0)$ and $(H - H_u)^- = \max(H_u - H, 0)$, respectively. Table VI shows that cross-hour effects feature strongly on unworked hours (column (2)),[13] while overtime hours are hardly affected (column (1)). Further tests show that cross-effects on unworked hours involve an increase in the frequency of both unworked weeks (i.e. $H = 0$, see column (3)), and unusually short workweeks (i.e. $0 < H < H_u$, see column (4)). Interestingly $(H - H_u)^-$ turns out to be the sole component of labor supply that employees may cut unilaterally without earning losses, as discussed in Appendix A.

Finally, for cases in which $H < H_u$, respondents are asked to state the main reason why they worked less than usual in the reference week. Possible answers include: holidays and absence for personal reasons,[14] sickness leave, maternity leave, continuous training, unusual workload, strike, and lock-out. While we detected significant cross-hour effects for holidays and sickness leaves, which are margins on which employees have closer control, we find no evidence of cross-effects on any other margin (results not reported).

In summary, we find evidence that cross-effects did not entail the renegotiation of usual hours with employers or changes in earnings, but involved instead a simple reduction in (unpaid) work involvement, whether within a given day, or through an increase in the take-up rate of paid vacation and/or sick leave. These margins of adjustment typically have no detrimental impact on (current) earnings. An explanation for why men may work some unpaid

[13] Note that the coefficient on $APost^S$ is now positive, because the fall in labor supply is now picked up by an *increase* in unworked hours.

[14] The exact wording is «*congé annuel*, *congé ou absence pour convenance personnelle*, *jour férié*, *pont*, *récupération*».

hours in the first place is that these may have an impact on future, as opposite to current, earnings, to the extent that someone who is more absent from work may lose on prospects of promotion and/or earnings growth. Another possible explanation is that some individuals may derive utility from work *per se*. Regardless of the underlying explanation, our results show that men decide to cut on such unpaid hours following their wives' treatment, as increased spousal nonmarket time would raise the utility of their own nonmarket time relative to the utility of being at work. This mechanism will be illustrated in a simple mode of spousal labor supply in Section IV and Appendix B.

IV.3 Heterogeneous Cross-Hour Effects

As Table A2 shows, workers in high-skill occupations typically work longer hours than the less-skilled and are also more likely work overtime. High-skill occupations here include managers, professionals and engineers at various levels (*cadres* in the French classification of occupations), and cover about 20% of the employed workforce. About 51% of males in these occupations work more than 45 hours weekly, while only 24% of those in less-skilled occupations do so. For women, the proportions are 21% and 8% respectively. Moreover, high-skill workers typically have higher control over the organization of their workweek, and thus should be expected to respond more to changes in their spouses' workweek than the less-skilled, who are instead more likely to work the legal workweek and therefore would only be able to cut their working hours via new contractual agreements.

To test this assumption, we replicate our previous analysis for men in high-skill occupations and other men separately. The results are reported in Table VII, which shows in column (1) a strong and significant first-stage effect of the shorter workweek on wives' working hours, which is somewhat higher for wives of managers and professionals than for other wives. Interestingly, the associated cross-effect on hours is about three times larger for managers and professionals than for other workers (column 2). In particular, managers and professionals cut their working time by nearly one hour when their wives are treated, while the corresponding figure for workers in other occupations is about 19 minutes. One can draw similar conclusions by looking at the probability of working more than 45 hours weekly in column (3), which shows that wives' treatment lowers the likelihood of a long workweek for men in managerial or professional jobs by 3.3 percentage points, representing a 6.5% reduction on the baseline proportion of 51%. Spillover effects on men's labor supply thus seem a lot stronger for the high-skilled than for the less-skilled.

We further explore spousal labor supply responses in households with young children, as compared to other households. It has been argued that the interdependence of spousal labor supply may be stronger when there are young children in the household, as children appear to play the role of a "jointly-consumed commodity" for husbands and wives (Lundberg, 1988). To test this assumption, Table VIII replicates previous regressions for households with at least one child aged 0-6 and for other households separately. In column 1, we find weaker first-stage effects for mothers of young children (-1.30) than for other women (-2.08). This difference is at least partly explained by higher incidence of part-time work among mothers, as for part-timers the mandatory workweek reduction would not necessarily be binding (Bloch-London and al., 2003). Moving to reduced-form regressions in column 2, the reaction of husbands to their wives' treatment is noticeably stronger in households with young children than in other households, despite a weaker first-stage effect. The presence of at least one young child thus clearly increases the reaction of men's labor supply to their wives working hours. Again we did not detect any similar spillovers for women.

As the presence of young kids in the household is systematically related to the age of respondents, one may worry that the above estimates would be driven by age effects, rather than parenthood. To investigate this we further split the sample along the age dimension of respondents (18-29, 30-39, 40-49, 50+ years old; with or without kids aged 0-6). We find that for all age groups the cross-hour effect in the sample with children is higher than the corresponding effect estimated on the childless sample (especially in the younger age group), thus we do find evidence of genuine parenthood effects. By contrast, in the childless sample, we do not find any significant age effects (results not reported).

V Robustness checks

V.1 Alternative sources of identification

The whole previous analysis uses two sources of identification for the cross-hour effects of the shorter workweek, and namely the fact some spouses are treated whereas others are not, and the fact that not all treated spouses are treated at the same date. In order to check the robustness of our estimates, Table IX replicates the previous regressions using these two sources of identification separately. Specifically, the first-stage regression is based the following specification,

$$H_{it}^S = \alpha_{11} A_{it}^S + \alpha_{12} A_{it}^S * (1998 \leq t \leq 2002) + \alpha_{21} A_{it}^S * (t > 2002) + \\ + \alpha_{22} APost_{it}^S * (t \leq 2002) + \alpha_3 X_{it}^S + D_t + u_{it}. \quad (4)$$

The parameters of interest are α_{21} and α_{22}. The α_{21} coefficient compares differences in hours between those ever treated and the nontreated after 2002 and before 1998. By contrast, the α_{22} coefficient compares hours worked by those treated later to hours worked by those treated earlier.[15]

The corresponding reduced-form equation for the impact of spouse treatment on the main respondent's labor supply is thus

$$H_{it} = \gamma_{11} A_{it}^S + \gamma_{12} A_{it}^S * (1998 \leq t \leq 2002) + \gamma_{21} A_{it}^S * (t > 2002) \\ + \gamma_{22} APost_{it}^S * (t \leq 2002) + \gamma_3 A_{it} + \gamma_4 APost_{it} + \gamma_5 X_{it} + D_t + \varepsilon_{it}, \quad (5)$$

---

[15]Note that $APost_{it}^S = APost_{it}^S * (t \leq 2002) + A_{it}^S * (t > 2002)$, so that specifications (3) and (4) are nested. In particular, specification (3) is a special case of specification (4), in which we impose $\alpha_{11} = \alpha_{12}$ and $\alpha_{21} = \alpha_{22}$.

where γ_{21} and γ_{22} are the parameters of interest.

Columns 1 and 2 in Table IX report the estimated first-stage effects on wives' hours and earnings. Reassuringly, the estimates for first-stage effects α_{21} and α_{22} are both negative, highly significant, very similar to each other and very close to the overall effect obtained with the basic specification (see Table III). Column 3 reports reduced-form effects for their husbands. The estimates obtained for γ_{21} and γ_{22} are again negative, significant, close to each other and to the overall reduced-form effect reported in Table III.

For females, the estimated cross effects were still negative, but very small in magnitude and not significantly different from zero at standard levels, regardless of the source of identification (results not reported).

V.2 Unobserved Heterogeneity

The identifying assumption underlying our main estimates is that a respondent's unobserved characteristics be uncorrelated to the timing of adoption of the shorter workweek in his or her spouse's firm. One could think of a number of scenarios in which this identifying assumption may be potentially violated, and this subsection provides results of robustness tests that should address various caveats to a causal interpretation of our estimates.

First, our identifying assumption would be violated if employees in firms signing an agreement were subject to systematically different shocks or changes in unobservables around time of signing, versus employees in non-signing firms. As the time of signing and policy adoption is staggered across firms, one may be less worried about aggregate trends affecting various outcomes differently at signing versus non-signing firms, than in the case of simultaneous treatment. Nevertheless, the timing of treatment may be endogenous from a firm's point of view (though not probably from an individual employee's point of view, or even less from his/her spouse's point of view), and more in general there could be differential labor supply movements in the treatment and control groups that are unrelated to the adoption of the shorter workweek.

As a preliminary response to this concern one may argue that, if time of policy adoption is endogenous to unobserved shocks to local labor markets, and this mechanism were driving spurious cross-hour effects, one should probably find evidence of these for both men and women, while we systematically rule out cross-effects for women in all specifications. Also, unobservable shocks to local labor markets would likely feature in both usual and non-usual hours, while we detect direct effects on usual hours and cross-effects on non-usual hours.

Beyond this general argument, if changes in unobservables of treament and control groups would generate spurious changes to their labor supply, one would expect to observe some change in some of their observables as well around the time of treatment. But we show in Table X that while there are significant pre-treatment differences in the age, education, public sector status, and industry of treatment and control groups (see coefficients on A^S and A variables), there is no evidence of any significant change in such characteristics upon treatment (see coefficients on $APost^S$ and $APost$ variables).

Also, we estimate first-stage and reduced-form specifications, having controlled for region*year interactions and treatment-specific trends, capturing the effects local shocks and trends in treatment-specific unobservables, respectively. The results are reported in Table XI and show a first-stage effect of the workweek reduction that is virtually identical to that reported in Table III. The corresponding reduced-form effect is very similar to that reported in Table IV, albeit slightly less precise, but still significant at the 5% level.[16]

[16] We also run typical placebo tests by estimating first-stage and cross-effects on the 1994-1998 pre-reform period and the 2002-2006 post-reform period, having created artificial treatment dates four years before and four years after actual treatment dates, and found no significant coefficients on the newly created $APost$ and $APost^S$ interaction terms.

Second, we take into account concerns of reverse causality, and namely the possibility that changes in the labor supply behavior of the main respondent may affect his or her spouse's job mobility from signing to non-signing firms and viceversa. To do this we exploit information on job tenure with the current employer to select a subsample of workers whose spouses did not change employer during the adoption period 1998-2002. When estimating our usual reduced-form specification on the subsample of spouses of job-stayers, we find a cross-effect of -0.46 (s.e. 0.21) for men, and a cross-effect of 0.16 (s.e. 0.15) for women, and both estimates as well as their level of significance are very close to those found on the main sample in Table IV.

Finally, one may worry that in general employees in early-signing firms would have systematically different spouses from employees in late-signing firms. To address these concerns, we complement the above results with fixed-effect estimates of the effects of interest. The French LFS has a rotating panel dimension, with one third of the sample being replaced each year, and each household staying in the sample for at most three survey years. When focusing on the 1998-2002 period,[17] about 10% of respondents surveyed are observed both before and after the implementation of the shorter workweek in their spouses' firms (see Table XII). These include about 23,000 male and 29,000 female respondents.

Table XIII reports fixed-effect estimates of all parameters on interest, controlling for individual fixed-effects. Employment and earnings effects of the shorter workweek are again nil. The first-stage effect on hours is negative and significant for both men (-1.22) and women (-1.21), although this is somewhat smaller than the effect detected in cross-section estimates of Table III. As fixed-effect estimates focus by construction on short-term effects of worktime agreements, while cross-sectional estimates exploit a longer horizon, one may think that the difference between the two may be due to some gradual implementation of the shorter

[17]Households surveyed either before 1998 or after 2002 did not experience any changes in working time regulations while in our panel, and thus cannot contribute to the identification of the effect of these changes on spousal labor supply. Our panel estimates thus focus on the 1998-2002 period.

workweek. Figures II and V show that this may be the case for husbands, though not for wives. Another possible interpretation is that fixed-effect estimates may be more seriously affected by measurement error in the actual date of implementation of the shorter workweek, which would generate a stronger attenuation bias than in cross-section estimates.

The cross-hour effect for husbands is negative (-0.40), although this only becomes significant when one looks at the difference between actual and usual hours (-0.76), and again it is the amount of unworked hours that is adjusted following wives' shorter workweeks (0.80). For wives, the cross-hour effect is either positive or close to zero, but never statistically significant. Overall, our main findings are robust to the introduction of individual fixed-effects, although as it is to be expected the significance of some of the coefficients of interest is reduced in this smaller sample.

VI. Instrumental variable estimates of cross-hour effects

There is a long standing tradition of labor supply models in which the labor supply decisions of each spouse depend not only on own potential wage or unearned income, but also on the number of hours spent at work by the other spouse (see Lundberg, 1988, for a seminal example). These models are hard to estimate since they involve a system of two simultaneous equations in which wives' hours feature in the husbands' labor supply equation and vice versa, and good instruments for independent variation in the labor supply of one of the spouses are typically hard to find. This is precisely where the French workweek reform could help identify the effects of interest, by generating exogenous variation in the labor supply of one spouse.

While the previous sections have highlighted the reduced-form effect of workweek regulations on spousal labor supply, in this section we use workweek regulations in an individual's firm as an instrument for her working hours in her spouse's labor supply equation. As well known, both reduced form parameters and IV estimates are of interest in their own right. The former is most policy relevant, as the Government can directly control the adoption of the shorter workweek, but of course cannot directly control spousal labor supply. Also, reduced-form estimates would not require as an exclusion restriction that workweek regulations affect spousal labor supply only via their effect on the labor supply of directly treated employees. However, if one is willing to accept this (reasonable) exclusion restriction, IV estimates provide the parameter of interest for measuring how labor supply responds to independent changes in labor supply of one's spouse.

The structural interpretation of this parameter, as well as of its variation across genders, relies on the underlying model of intra-household interactions, and in particular on whether one assumes the household decision making process to be cooperative or non-cooperative. In non-cooperative models (see for instance Bourguignon, 1984, Chen and Woolley, 2001, Lechene and Preston, 2012), each spouse maximizes an individual utility function, taking the decisions of the other spouse as given. The arguments of such utility functions may include own as well as spousal use of time. In this framework cross-hour effects represent the effect of spousal labor supply on the marginal utility of substituting time spent at work with leisure (see proof in Appendix B). Asymmetric cross-hour effects can be easily generated in this context by different utility functions for men and women, such that men's utility of leisure would respond to wives' leisure, but not viceversa.

In cooperative household models (see, among others, McElroy and Horney, 1981, Chiappori, 1988, Apps and Rees, 1988), the household jointly maximizes a utility function, strictly increasing in the utility of each spouse. In this case it can be shown that estimated cross-hour effects may stem from leisure complementarities in both own and spouse's utility function (see Appendix B), and consequently strongly asymmetric cross-hour effects for men and women are less straightforward to rationalize, unless women are initially trapped at a corner solution characterized by zero unpaid time at work, and consequently have much less leeway in adjusting their labor supply following a shock to their husbands' leisure.

Below we report estimates of the impact of spousal hours on own hours, having instrumented spousal hours by $APost^S$. The regression results are reported in Table XIV for both men (Panel A) and women (Panel B), using the same samples and specifications as in Tables IV, VII and VIII. Unsurprisingly, while we detect no significant cross-hour effect for wives on either the whole sample or any of the subsamples used, for husbands the cross-hour effect is always positive and significant. Among husbands the cross-hour effect in labor supply is about twice as large for managers and professionals than for other occupations, and in particular when their wives cut their labor supply by one hour, men in high occupations respond to by cutting their own labor supply by about 20 minutes. Also, cross-effects are three times larger in the presence of young children, relative to childless households. The quantitative response for fathers is about 35 minutes for each extra hour spent at home by their wives, thus suggesting that worktime policy evaluations restricted to direct labor supply effects may strongly underestimate its impact on the time spent by fathers with their young children.

An average cross-hour effects of 0.23 for husbands and a negligible one for wives would translate into a social multiplier of household labor supply of about $1 + 0.23/2 = 1.1$, implying that the equilibrium labor supply response to an exogenous shock is about 10% larger than the initial impact. This figure rises to about 30% for families with young kids. As discussed by Glaeser, Sacerdote and Scheinkman (2003), the role of social interactions and social multipliers may vary widely across demographic groups and levels of aggregation, and the French reform provides a clean experiment to identify the multiplier in labor supply at the household level.

VII. Conclusions

This paper has investigated cross-hour effects in the labor supply of couples using independent variation in spousal hours that generated by the introduction of the shorter workweek in France in the late 1990s. In particular we exploit independent variation in spousal hours at constant monthly earnings, which allows us to abstract from income effects of changes in spousal labor supply, and focus on pure cross-hour effects.

We found that both female and male employees treated by the shorter legal workweek reduce their weekly labor supply by about 2 hours, and do not experience any reduction in their monthly earnings. While wives of treated men do not seem to adjust their working time at either the intensive or extensive margins, husbands of treated women respond by cutting their workweek by about half an hour to one hour, according to specifications and samples. Such gender differences in cross-hour effects are remarkable, especially insofar overall labor supply elasticity of women's is typically higher than men's (see, among others, Blundell and MaCurdy, 1999).

The estimated labor supply response of men takes place by cutting actual hours below usual weekly hours, and is not associated to an earnings' loss. It seems thus that husbands cut their labor supply by adjusting unpaid work margins such as taking more paid vacation or sick leaves. These results suggest significant spousal complementarities in leisure time for husbands, and namely when a wife's workweek is reduced, the increase in her leisure time raises the value of leisure for her husband and reduces his labor supply.

Our results on cross-hour effects are noteworthy as they show that neglecting labor supply reactions of spouses may give a misleading view of the overall impact of labor supply shocks. In particular, by focusing on the direct impact of policy on the targeted population, most existing evaluations of workweek reduction reforms are likely to underestimate the effect of these reforms on labor supply. A simple back-of-envelope calculation suggests that neglecting spillovers within the household would likely yield an underestimate of the overall policy impact on male labor supply by about 13%, and the estimated spillovers would translate into a social multiplier around 1.1.

Finally, cross-hour effects vary widely across household types, and tend to be strongest in the presence of young children, with policy relevant effects on the time spent by fathers with their offspring. This opens a call for further research on the impact of parents' labor supply shocks on children's outcomes such as schooling and non-cognitive skills.

Appendix A: Non-usual hours and earnings

In our sample usual hours H_u are defined for about 85% of individuals. For these individuals, $H = H_u$ in 73% of cases, $H > H_u$, in 11.6% of cases and $H < H_u$, in the remaining 15.4% of cases. Note that H and H_u represent weekly-aggregated measures, thus someone who works one hour longer than the typical workday for three days in a week and one hour shorter for the remaining two days would have $H > H_u$. For simplicity, we will refer to cases in which $H > H_u$ as cases of overtime work, and to cases in which $H < H_u$ as cases of unworked hours. Conditional on $H < H_u$, 57% of cases correspond to workers who did not work at all during the survey week ($H = 0$), and among them the average number of unworked hours is 38, and 43% of cases correspond to workers who worked positive hours but still below their usual workweek ($0 < H < H_u$), and among them the average number of unworked hours is 10. Conditional on $H > H_u$, the average number of overtime hours is 7.4 hours.

We have shown in Section IV.2 that cross hour effects mostly happen through variations in $H - H_u$ rather than in H_u, and specifically through an increase in unworked hours $(H - H_u)^-$. Here we show that $(H - H_u)^-$ turns out to be the sole component of labor supply that employees may cut without earning losses. To check this, Table A3 reports estimates from regressions of monthly earnings on H_u, $(H - H_u)^+$ and $(H - H_u)^-$ separately. Column 2 shows that earnings only respond significantly to usual hours H_u and overtime hours $(H - H_u)^+$, whereas unworked hours $(H - H_u)^-$ have no discernible impact, and columns 3-6 show that this result holds true within both the treated and the control sample.

Appendix B: A simple theoretical framework

Consider a married worker, working H hours and enjoying l hours of leisure, where H and l satisfy the usual (normalized) constraint $H + l = 1$. We assume that H can be conceptualized as the sum of paid working hours L and unpaid hours M, where only M is chosen by the worker, whereas L is defined by a formal contract, depending on the institutional setting. As a result, earnings Y are constant, as the duration of paid work is exogenously set, and the only work margin under the worker's control is unpaid. These assumptions are meant to capture in the simplest form the main institutional features of the French workweek regulations.

Preferences can be represented by a well-behaved utility function

$$U(l, M, H^S, C) = U(l, 1 - l - L, H^S, C), \tag{6}$$

where H^S represents the number of hours worked by the spouse (with $H^S = L^S + M^S$), and C represents consumption, which we assume to be a household public good. The number of unpaid hours M may enter the utility function either because investment at work is an intrinsic source of utility for the worker or because it is expected to increase the probability of professional success in the future. Spousal labor supply H^S enters the utility function because the value of own leisure may depend on how many hours one's spouse spends at work or, conversely, in the household.

We consider first a non-cooperative household model in which each individual chooses M and l in order to maximize own utility U, taking H^S as given, and subject to the usual budget constraints $L + M + l = 1$ and $C = Y + Y^S$, where Y^S denotes spouse income.

This problem is a special case of the more general set-up introduced by Pollak (1969) to describe "conditional demand functions", i.e. consumer's behavior when the quantity of one or more goods is rationed. In our specific case, the optimal l^* represents the conditional demand for leisure by a worker whose paid hours are institutionally set. Optimal choices l^* and M^* are functions of H^S and household income $Y + Y^S$, and optimal labor supply is simply $H^* = L + M^*$.

Using this notation, the first-stage effect of the workweek reduction is $\partial H^{S*}/\partial L^S = 1 + \partial M^{S*}/\partial L^S$ and the cross-effect is $\partial H^*/\partial L^S = \partial M^*/\partial L^S$. In our empirical context, the worktime regulation reform provides a source of variation in L^S, which is independent of households' earnings, and makes it possible to estimate this cross-hour effect.

The relationship between the cross-hour effect and the characteristics of the utility function can be recovered by first obtaining first-order conditions of this maximization problem for l and M, and then differentiating with respect to M and L^S:

$$\frac{\partial H^*}{\partial L^S} = \frac{\partial M^*}{\partial L^S} = \frac{1}{u^2} \frac{\partial (U_1 - U_2)}{\partial H^S} \frac{\partial H^{S*}}{\partial L^S} = \frac{U_{23} - U_{13}}{u^2} \frac{\partial H^{S*}}{\partial L^S}, \tag{8}$$

where U_i denotes the partial derivative of U with respect to its ith argument, U_{ij} denotes cross-derivatives, and $u^2 = -U_{11} + 2U_{12} - U_{22}$ is positive due to the concavity of U. Conditional on positive direct effects, $\partial H^{S*}/\partial L^S > 0$, one would detect positive cross-hour effects if $U_{23} > U_{13}$, i.e. if spouse working time reduces the utility of leisure time more than it raises the utility of unpaid time spent at work. In other words, $U_{23} > U_{13}$ implies that an individual is willing to substitute time at work with time in the household when his or her spouse works less, consistent with complementarity in spousal leisure. In this context, positive cross-hour effects for men but not for women can be easily rationalized by $U_{23} - U_{13} > 0$ and $U_{23}^S - U_{13}^S = 0$, where S indexes women's utility functions. Another possible explanation could be that women are initially at a corner solution with $M^{S*} = 0$, and thus cannot reduce voluntary involvement at work any further.

If intra-household interactions are instead cooperative, spouses would jointly maximize a utility function that is increasing in the utility of each spouse. In this case it can be shown that positive cross-hour effects for the husband may be driven by both complementarity of leisure in his utility function, and complementarity of leisure in his wife's utility function. Thus one could now detect positive cross-effects for a husband not only because he may enjoy leisure more at higher wife's leisure, but also because his wife may enjoy leisure more at higher husband leisure, and this mechanism is taken into account by the cooperative nature of household decisions. Given this result, it is not straightforward to generate positive cross-effects for men but zero cross-effects for women, unless women are initially at a corner solution with $M^{S*} = 0$.

Assume for simplicity a linear household welfare function of the type

$$aU(l, M, H^S, C) + bU^S(l^S, M^S, H, C) \tag{9}$$

where a and b are spouse-specific Pareto weights.[18] In the special case with $M^{S*} = 0$, cross-effects for husband are given by:

$$\frac{\partial H^*}{\partial L^S} = \frac{\partial M^*}{\partial L^S} = \frac{a(U_{23} - U_{13}) - bU_{13}^S}{au^2 - bU_{33}^S}\frac{\partial M^{S*}}{\partial L^S}, \tag{10}$$

where $au^2 - bU_{33}^S > 0$ due to the concavity of U and U^S. In this context, cross-hour effects for men capture leisure complementarities in both their own utility function ($U_{23} - U_{13} > 0$)

[18] Given that the natural experiment that we exploit does not affect spouses' relative income, we do not need to make assumptions on whether a and b are constant (as in the unitary model) or vary with spouses' relative income (as in a typical collective model).

and their wife's utility function ($-U_{13} > 0$). But such complementarities, if any, would not show up in cross-hour effects for women if $M^{S*} = 0$.

Note finally that in this framework we have implicitly interpreted all nonmarket time as leisure, while in reality it can include both leisure and home production. We believe, however, that allowing for home production would not substantially alter the interpretation of the estimated cross-hour effect. In this case positive cross-hour effects would imply complementarity of spousal nonmarket time, while negative cross-hour effects would imply substitutability of nonmarket time, where complementarity would be plausibly driven by the leisure component of nonmarket time, while substitutability would be driven by the home production component. As we find positive cross-hour effects, we should conclude that complementarity of leisure dominates substitutability of home production.

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Table I
Descriptive statistics

Panel A	*Men*			
	Full sample		Employed	
	Wife not treated	Wife treated	Wife not treated	Wife treated
Years of education	12.7	12.4	12.9	12.5
Age	42.6	41.9	41.7	41.0
High-skill occupation (%)	17.7	14.2	19.4	15.4
Private sector (%)	57.1	66.2	64.9	74.6
Spouse's year of educ.	13.1	12.7	13.2	12.8
Spouse's age	40.5	39.7	39.7	39.0
Spouse in high-skill occ. (%)	11.1	8.1	11.3	8.3
Spouse in private sector (%)	54.3	90.2	54.4	90.4
No. observations	130,468	59,426	114,705	52,755
Panel B	*Women*			
	Full sample		Employed	
	Husband not treated	Husband treated	Husband not treated	Husband treated
Years of education	12.6	12.4	13.0	12.8
Age	39.4	39.5	39.5	39.5
High-skill occupation (%)	7.4	5.7	10.4	7.8
Private sector (%)	42.7	47.5	63.0	69.9
Spouse's year of educ.	12.5	12.2	12.7	12.4
Spouse's age	41.5	41.6	41.4	41.5
Spouse in high-skill occ. (%)	18.7	16.7	19.3	16.6
Spouse in private sector (%)	72.4	93.6	70.1	92.9
No. observations	150,371	86,431	101,923	58,766

Notes. The full sample includes married or cohabiting respondents, whose spouse is an employee. The employed subsample is restricted to those classified as employed according to the ILO definition of employment. The interpretation of figures is as follows: The average number of years of education for men whose wife is not treated is 12.7, and the average number of years of education for their wives is 13.1. High-skill occupations include managers, professionals, engineers or associate occupations (*cadres*). Source: French LFS, 1994-2009, Insee.

Table II
Distribution of own treatment, by spouse treatment (%).

Panel A	*Employed men*	
	Wife not treated	Wife treated
Own firm never adopted shorter workweek	71.0	54.2
Own firm adopted shorter workweek	29.0	45.8
- not same year as wife's firm	*29.0*	*22.8*
- same year as wife's firm	-	*23.0*
Total	100	100
Panel B	*Employed women*	
	Husband not treated	Husband treated
Own firm never adopted shorter workweek	73.2	58.1
Own firm adopted shorter workweek	26.8	41.9
- not same year as wife's firm	*26.8*	*21.3*
- same year as wife's firm	-	*20.6*
Total	100	100

Notes. Panel A : Employed subsample restricted to male respondents. Panel B: Employed subsample restricted to female respondents. The interpretation of figures is as follows: among employed males whose spouse works in a treated firm, 45.8% are working in a treated firm.

Table III
First stage regressions
Direct effects of the shorter workweek on hours and earnings.

Panel A	*Men*			
	Wives' hours		Wives' earnings	
	(1)	(2)	(3)	(4)
AS	1.36**	1.01**	0.088**	0.064**
	(0.14)	(0.13)	(0.008)	(0.005)
$ASPost$	-1.81**	-1.91**	0.002	-0.002
	(0.13)	(0.10)	(0.010)	(0.006)
Additional controls	no	yes	no	yes
No. Observations	189,894	189,894	160,046	160,046

Panel B	*Women*			
	Husband's hours		Husband's earnings	
	(1)	(2)	(3)	(4)
AS	-0.28*	-0.34**	0.042**	0.013**
	(0.12)	(0.12)	(0.004)	(0.003)
$ASPost$	-1.95**	-1.92**	0.017*	0.007
	(0.13)	(0.14)	(0.008)	(0.004)
Additional controls	no	yes	no	yes
No. Observations	236,802	236,802	201,559	201,559

Notes. The table shows first stage regressions for hours and earnings of spouses of main respondents. Columns 1 and 2 refer to the full sample. Columns 3 and 4 refer to the subsample of respondents whose spouses have nonmissing earnings (from 2003 onwards, information on earnings is collected on one third of the LFS sample). Baseline controls include 15 year dummies and a dummy indicating whether the spouse works in public sector. Additional controls include spouse's years of education, age, age squared and 16 industry dummies. Standard errors clustered at the treatment*year level are reported in brackets. ** and * denote significance at the 1% and 5% levels, respectively. Source: French LFS, 1994-2009, Insee.

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Table IV
Reduced form regressions
Cross-effects of the shorter workweek on employment and hours.

Panel A			*Men*		
	Own employment		Own hours (conditional on employment)		
	(1)	(2)	(3)	(4)	(5)
A^S	0.0062**	0.0021	-0.11	-0.11	-0.16
	(0.0021)	(0.0018)	(0.10)	(0.10)	(0.10)
$APost^S$	-0.0037	-0.0028	-0.44**	-0.45**	-0.50**
	(0.0027)	(0.0022)	(0.09)	(0.09)	(0.09)
A	-	-	-0.05	-0.09	-0.17
			(0.10)	(0.12)	(0.13)
$APost$	-	-	-1.96**	-1.96**	-2.02**
			(0.14)	(0.14)	(0.13)
Further controls	no	yes	No	yes	yes
No. observations	189,894	189,894	167,460	167,460	156,392

Panel B			*Women*		
	Own employment		Own hours (conditional on employment)		
	(1)	(2)	(3)	(4)	(5)
A^S	0.0164**	0.0146**	-0.24**	-0.25**	-0.27**
	(0.0016)	(0.0016)	(0.07)	(0.07)	(0.08)
$APost^S$	-0.0032	-0.0041	0.12	0.05	0.06
	(0.0023)	(0.0022)	(0.10)	(0.11)	(0.11)
A			1.76**	1.22**	1.22**
			(0.15)	(0.11)	(0.13)
$APost$			-1.86**	-1.88**	-1.86**
			(0.17)	(0.15)	(0.18)
Further controls	no	yes	No	yes	yes
No. observations	236,802	236,802	160,689	160,689	150,371

Notes. The table shows reduced-form regressions for main respondents, and regresses their employment status and hours on their spouses' treatment variables (A^S and $APost^S$), as well as on their own treatment (A and $APost$). Columns 1 and 2 refer to the full sample. Columns 3 and 4 refer to the employed subsample. Column 5 refers to employed respondents who were not treated at the same time as their spouses. Baseline controls in columns 1 and 2 include include 15 year dummies and a dummy indicating whether the spouse works in the public sector. Additional controls in column 2 are spouse's years of education, age and age square, and respondent's years of education, age and age square. Baseline controls in columns 3-5 include 15 year dummies, a public sector dummy, a wage-earner dummy and a dummy indicating whether spouse works in the public sector. Additional controls in columns 4 and 5 include spouse's years of education, age, age square and 16 industry dummies, and respondent's years of education, age, age square and 16 industry dummies. Standard errors clustered at the treatment*year level are reported in brackets. ** and * denote significance at the 1% and 5% levels, respectively. Source: French LFS, 1994-2009, Insee.

Table V
Reduced-form regressions
Cross-effects of the shorter workweek on types of hours worked and earnings.

	Men			
	Usual Hours H_u	Actual–usual hours $H - H_u$	Earnings	
	(1)	(2)	(3)	(4)
A^S	-0.14**	0.04	-0.018**	-0.016**
	(0.03)	(0.10)	(0.003)	(0.003)
$APost^S$	-0.01	-0.59**	0.008*	0.003
	(0.06)	(0.10)	(0.004)	(0.004)
A	0.02	-0.14*	0.008**`	0.008**
	(0.07)	(0.07)	(0.003)	(0.003)
$APost$	-1.99**	-0.33*	0.005	0.007
	(0.13)	(0.16)	(0.004)	(0.004)
No. Observations	101 139	101 139	124 417	101 139

Notes. Columns 1, 2 and 4 refer to the employed subsample for which usual hours are defined. Columns 3 and 4 refer to the employed subsample with nonmissing earnings (from 2003 onwards, information on earnings is collected on one third of the LFS sample). Regressions include the same set of control variables as in specification (4) of Table IV. Standard errors clustered at the treatment*year level are reported in brackets. ** and * denote significance at the 1% and 5% levels, respectively. Source: French LFS, 1994-2002 (columns 1, 2 and 4) and 1994-2009 (column 3), Insee.

Table VI
Reduced-form regressions
Cross-effects of the shorter workweek on overtime hours and unworked hours.

	Men			
	Overtime hours $(H - H_U)^+$ (1)	Unworked hours $(H - H_U)^-$ (2)	Unworked weeks $H = 0$ (3)	Unusually short workweeks $0 < H < H_U$ (4)
A^S	0.00	-0.04	0.000	-0.001
	(0.02)	(0.09)	(0.003)	(0.002)
$APost^S$	-0.06	0.53**	0.012**	0.006*
	(0.03)	(0.08)	(0.003)	(0.003)
A	-0.01	0.13*	0.001	0.001
	(0.02)	(0.07)	(0.002)	(0.003)
$APost$	0.09*	0.43**	0.016**	0.008**
	(0.05)	(0.13)	(0.004)	(0.003)
No. Observations	101 138	101 138	101 138	101 138

Notes. The Table refers to the employed subsample for which usual hours are defined. Regressions include the same set of control variables as in specification (4) of Table IV. Standard errors clustered at the treatment*year level are reported in brackets. ** and * denote significance at the 1% and 5% levels, respectively.Source: French LFS, 1994-2002 (columns 1, 2 and 4) and 1994-2009 (column 3), Insee.

Table VII
Direct and cross-effects of the shorter workweek:
By men's occupation.

Panel A	*Employed men: Managers, professionals and kindred occ.*		
	First stage	Reduced form	
	Wife's hours	Own hours	Own hours ≥ 45
	(1)	(2)	(3)
A^S	1.26**	0.30	0.006
	(0.35)	(0.19)	(0.008)
$APost^S$	-2.32**	-0.81**	-0.033**
	(0.30)	(0.27)	(0.009)
A		0.16	0.002
		(0.30)	(0.009)
$APost$		-1.66**	-0.042**
		(0.38)	(0.012)
No. observations	30,432	30,432	30,432
Panel B	*Employed men: Other occupations*		
	First stage	Reduced form	
	Wife's hours	Own hours	Own hours ≥ 45
	(1)	(2)	(3)
A^S	0.94**	-0.10	-0.003
	(0.12)	(0.08)	(0.002)
$APost^S$	-1.72**	-0.32**	-0.006**
	(0.11)	(0.09)	(0.002)
A		-0.10	-0.022**
		(0.11)	(0.002)
$APost$		-2.06**	-0.030**
		(0.13)	(0.005)
No. observations	137,028	137,028	137,028

Notes. Panel A refers to men who are managers, professionals, engineers or associate occupations (*cadres*). Panel B refers to other occupations.. In column 1, control variables are as in column 4 of Table III. In columns 2 and 3, control variables are as in column 4 of Table IV. Standard errors clustered at the treatment*year level are reported in brackets. ** and * denote significance at the 1% and 5% levels, respectively. Source: French LFS, 1994-2009, Insee.

Table VIII
Direct and cross-effects of the shorter workweek:
By family type.

Panel A	*Employed men: Managers, professionals and kindred occ.*		
	First stage	Reduced form	
	Wife's hours	Own hours	Own hours ≥ 45
	(1)	(2)	(3)
A^S	0.48*	-0.01	0.002
	(0.19)	(0.15)	(0.007)
$APost^S$	-1.30**	-0.81**	-0.028**
	(0.23)	(0.28)	(0.008)
A		-0.25	-0.023**
		(0.28)	(0.008)
$APost$		-2.23**	-0.038**
		(0.32)	(0.011)
No. observations	39,468	39,468	39,468

Panel B	*Employed men: Other occupations*		
	First stage	Reduced form	
	Wife's hours	Own hours	Own hours ≥ 45
	(1)	(2)	(3)
A^S	1.17**	-0.14	-0.010**
	(0.16)	(0.11)	(0.002)
$APost^S$	-2.08**	-0.34**	-0.003
	(0.13)	(0.12)	(0.003)
A		-0.05	-0.021**
		(0.09)	(0.002)
$APost$		-1.89**	-0.025**
		(0.13)	(0.005)
No. observations	127,992	127,992	127,992

Notes. Panel A refers to employed men in households with at least on child aged 0-6. Panel B refers to employed men in households without children aged 0-6. Control variables are the same as in Table VI. Standard errors clustered at the treatment*year level are reported in brackets. ** and * denote significance at the 1% and 5% levels, respectively. Source: French LFS, 1994-2009, Insee.

Table IX
Direct and cross-effects of the shorter workweek:
Alternative sources of identification.

	Employed men		
	First stage		Reduced form
	Wife's hours	Wife's earnings	Own hours
	(1)	(2)	(3)
$A^S * (t > 2002)$	-1.87**	0.009	-0.47**
	(0.17)	(0.009)	(0.14)
$APost^S * (t \leq 2002)$	-1.85**	-0.005	-0.40**
	(0.12)	(0.011)	(0.10)
A^S	1.19**	0.064**	0.00
	(0.17)	(0.004)	(0.13)
$A^S * (1998 \leq t \leq 2002)$	-0.47**	-0.002	-0.26
	(0.17)	(0.008)	(0.17)
$APost$	-	-	-1.96**
			(0.14)
A	-	-	-0.09
			(0.12)
No. observations	167,460	141,623	167,460

Notes. Columns 1 and 3 refer to the employed subsample, and column 2 refers to the employed subsample with nonmissing earnings (from 2003 onwards, information on earnings is collected on one third of the LFS sample). In columns 1 and 2, control variables are the same as in columns 2 and 4 of Table III. In column 3, control variables are the same as in column 4 of Table IV. Standard errors clustered at the treatment*year level are reported in brackets. ** and * denote significance at the 1% and 5% levels, respectively. Source: French LFS, 1994-2009, Insee.

Table X
Falsification tests on further outcomes

	Men			
	Years of Schooling	Age	Private sector	Manuf. Industry
	(1)	(2)	(3)	(4)
A^S	-0.045**	-0.071*	-0.012**	-0.020**
	(0.014)	(0.029)	0.002)	(0.003)
$APost^S$	0.001	0.008	0.0004	-0.0011
	(0024)	(0.040)	(0.0024)	(0.0033)
A	-0.020	0.119**	0.054**	0.157**
	(0.018)	(0.031)	(0.008)	(0.010)
$APost$	0.025	-0.059	0.016	0.017
	(0.029)	(0.044)	(0.010)	(0.014)
No. observations	167 460	167 460	167 460	167 460
	Women			
	Years of Schooling	Age	Private sector	Manuf. Industry
	(1)	(2)	(3)	(4)
A^S	-0.022	-0.044	-0.017**	-0.021**
	(0.012)	(0.024)	(0.002)	(0.002)
$APost^S$	0.021	0.079	-0.0017	-0.0008
	(0.018)	(0.039)	(0.0026)	(0.0025)
A	0.003	0.061	0.199**	0.138**
	(0.022)	(0.039)	(0.011)	(0.009)
$APost$	-0.013	0.061	(0.002)	0.012
	(0.026)	(0.045)	(0.014)	(0.014)
No. observations	160 689	160 689	160 689	160 689

Notes. The sample and specification are the same as in column 4 of Table IV, using alternative dependent variables. Standard errors clustered at the treatment*year level are reported in brackets. ** and * denote significance at the 1% and 5% levels, respectively. Source: French LFS, 1994-2009, Insee.

Table XI
Direct and cross-effects of the shorter workweek:
Additional controls for local and treatment-specific shocks

	Men					
	First stage			Reduced form		
	Wife's hours			Own hours		
	(1)	(2)	(3)	(4)	(5)	(6)
A^S	1.17**	0.73**	0.90**	-0.07	-0.28	-0.24
	(0.13)	(0.17)	(0.18)	(0.09)	(0.15)	(0.15)
$APost^S$	-1.93**	-1.97**	-2.03**	-0.45**	-0.37*	-0.38*
	(0.13)	(0.16)	(0.16)	(0.10)	(0.18)	(0.18)
A	-	-	-	-0.03	-0.09	-0.03
				(0.13)	(0.12)	(0.13)
$APost$	-	-	-	-1.98**	-1.96**	-1.98**
				(0.13)	(0.13)	(0.13)
Regions * year dummies	yes	no	yes	yes	no	yes
A * year	no	yes	yes	no	yes	yes
Obs.	189 894	189 894	189 894	167 460	167 460	167 460
	Women					
	First stage			Reduced form		
	Wife's hours			Own hours		
	(1)	(2)	(3)	(4)	(5)	(6)
A^S	-0.25**	-0.80**	-0.74**	-0.13*	-0.28**	-0.16
	(0.12)	(0.19)	(0.19)	(0.06)	(0.08)	(0.10)
$APost^S$	-1.95**	-1.83**	-1.86**	0.03	0.12	0.10
	(0.14)	(0.20)	(0.19)	(0.11)	(0.15)	(0.18)
A	-	-	-	1.33**	1.22**	1.33**
				(0.10)	(0.11)	(0.10)
$APost$	-	-	-	-1.94**	-1.88**	-1.94**
				(0.15)	(0.15)	(0.15)
Regions * year dummies	yes	no	yes	yes	no	yes
A * year	no	yes	yes	no	yes	Yes
Obs.	236 802	236 802	236 802	160 689	160 689	160 689

Notes. The sample and specifications are the same as in column 2 of Table III for first-stage regressions, and as in column 4 of Table IV for reduced-form regressions. Specifications (1), (3), (4) and (6) include interactions for 22 regions * 15 years. Standard errors clustered at the treatment*year level are reported in brackets. ** and * denote significance at the 1% and 5% levels, respectively. Source: French LFS, 1994-2009, Insee.

Table XII
Number of observations per respondent and proportion of switchers

	Men		
Number of obs. per respondent	Total number of respondents	Total number of observations	Proportion of changes in spouses' firms
1	26 231	26 231	-
2	13 916	27 832	11.9%
3	9 073	27 219	17.9%
All	*49 220*	*81 282*	*10.1%*
	Women		
Number of obs. per respondent	Total number respondents	Total number observations	Proportion of changes in spouses' firms
1	31 110	31 110	-
2	17 292	34 584	14.1%
3	11 901	35 703	22.6%
All	*60 303*	*101 397*	*12.8%*

Notes. Sample: Employed subsample, 1998-2002. Interpretation of figures is as follows: 13,916 male respondents are observed at two dates and 11.9% have a spouse whose firm signed an agreement between these two dates. Source: French LFS, 1998-2002, Insee.

Table XIII
Reduced-form regressions
Cross-effects of the shorter workweek on employment and hours: Fixed-effect estimates

	Men						
				Type of hours			
	Employm.	Hours	Earnings	Usual hours H_U	Actual-usual $H-H_U$	Overtime hours $(H-H_U)^+$	Unworked hours $(H-H_U)^-$
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
A^S	0.005	0.45	0.005	-0.10	0.48	0.12	-0.36
	(0.006)	(0.47)	(0.009)	(0.15)	(0.47)	(0.12)	(0.44)
$APost^S$	-0.006	-0.40	-0.000	0.15	-0.76*	0.04	0.80*
	(0.004)	(0.35)	(0.006)	(0.11)	(0.34)	(0.09)	(0.32)
A	-	0.19	-0.005	0.61**	-0.26	-0.17	0.09
		(0.42)	(0.008)	(0.14)	(0.42)	(0.11)	(0.39)
$APost$	-	-1.22**	-0.009	-1.52**	0.33	0.19*	-0.13
		(0.34)	(0.006)	(0.11)	(0.34)	(0.09)	(0.31)
No. obs.	81,282	63,796	63,796	56,941	56,941	56,941	56,941
	Women						
				Type of hours			
	Employm.	Hours	Earnings	Usual hours H_U	Actual-usual $H-H_U$	Overtime hours $(H-H_U)^+$	Unworked hours $(H-H_U)^-$
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
A^S	-0.001	-0.24	-0.002	-0.25	0.11	-0.01	-0.12
	(0.006)	(0.41)	(0.009)	(0.16)	(0.40)	(0.09)	(0.38)
$APost^S$	-0.003	0.33	0.006	0.15	0.04	-0.07	-0.12
	(0.005)	(0.31)	(0.007)	(0.13)	(0.31)	(0.07)	(0.29)
A	-	0.28	0.013	0.89	-0.43	-0.11	0.33
		(0.45)	(0.010)	(0.18)	(0.44)	(0.10)	(0.42)
$APost$	-	-1.21**	-0.010	-1.50**	0.31**	0.04	-0.27
		(0.35)	(0.008)	(0.14)	(0.34)	(0.08)	(0.32)
No. obs.	101,397	67,133	67,133	63,236	63,236	63,236	63,236

Notes. Column 1 refers to the full sample, Columns 2 and 3 refer to the employed subsample, and Columns 4 to 7 refer to the employed subsample for which usual hours are defined. Controls include individuals fixed effects as well as the same baseline and additional control variables as in Table IV. Standard errors clustered at the treatment*year level are reported in brackets. ** and * denote significance at the 1% and 5% levels, respectively. Source: French LFS, 1998-2002, Insee.

Table XIV
IV estimates of cross-hour effects

Panel A	*Employed men*				
	Hours				
	All	High-skilled	Other Occup.	1 or more child 0-6	Other households
	(1)	(2)	(3)	(4)	(5)
Wives' hours	0.23**	0.34**	0.18**	0.59**	0.16**
	(0.05)	(0.12)	(0.05)	(0.21)	(0.06)
No. observations	167,460	30,432	137,028	39,468	127,992
Panel B	*Employed women*				
	Hours				
	All	High-skilled	Other Occup.	1 or more child 0-6	Other households
	(1)	(2)	(3)	(4)	(5)
Husbands' hours	-0.02	0.08	-0.07	-0.23	0.04
	(0.05)	(0.13)	(0.06)	(0.12)	(0.05)
No. observations	160,689	15,217	145,472	36,959	123,730

Notes. The Table refers to the employed subsample. Estimates reported show the effect of spousal labor supply on the main respondent's labor supply, using treatment of spousal firms an instrument. The corresponding reduced-form results are reported in Tables IV, VII and VIII. Controls include a dummy variable for type of spouse firm (A^S), 15 year dummies, a wage-earner dummy, a public sector dummy, spouse's years of education, age, age square, 16 spouse's industry dummies and a dummy indicating whether spouse works in public sector. Standard errors clustered at the treatment*year level are reported in brackets. ** and * denote significance at the 1% and 5% levels, respectively. Source: French LFS, 1994-2009, Insee.

Figure I
Timing of implementation of shorter workweek: Percentage of employees treated

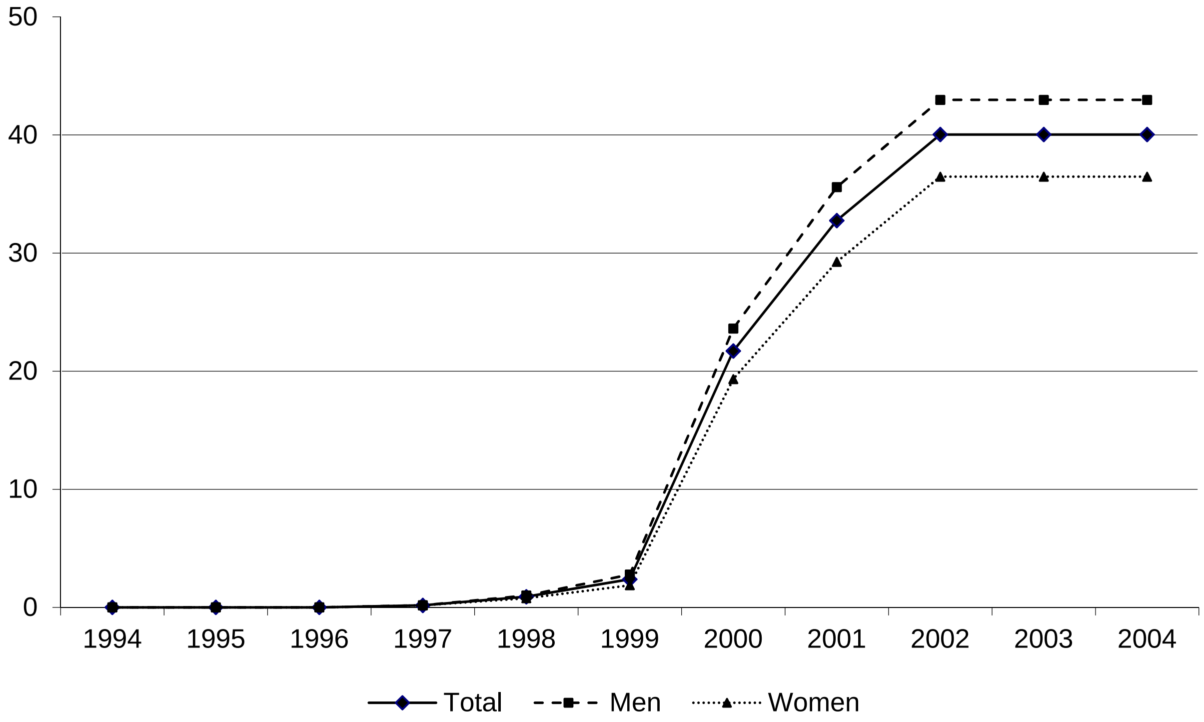


Figure II
Wives' hours worked, by own treatment.

Figure III
Men's employment rates, by wife's treatment

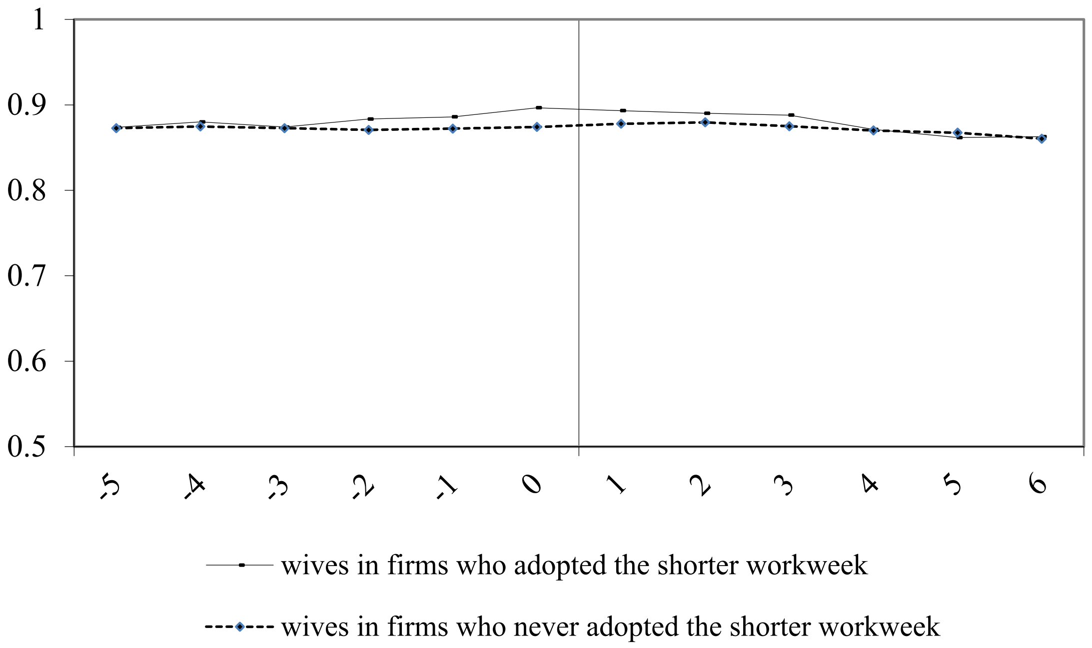


Figure IV
Men's hours worked, by wife's treatment.

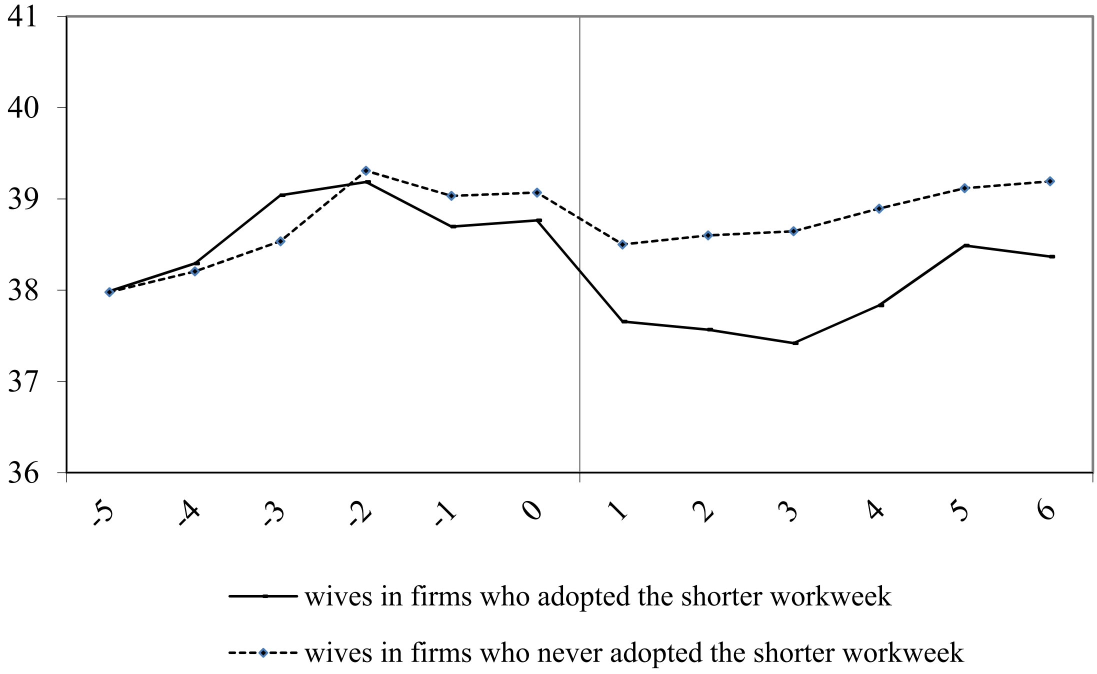


Figure V
Husbands' hours worked, by own treatment.

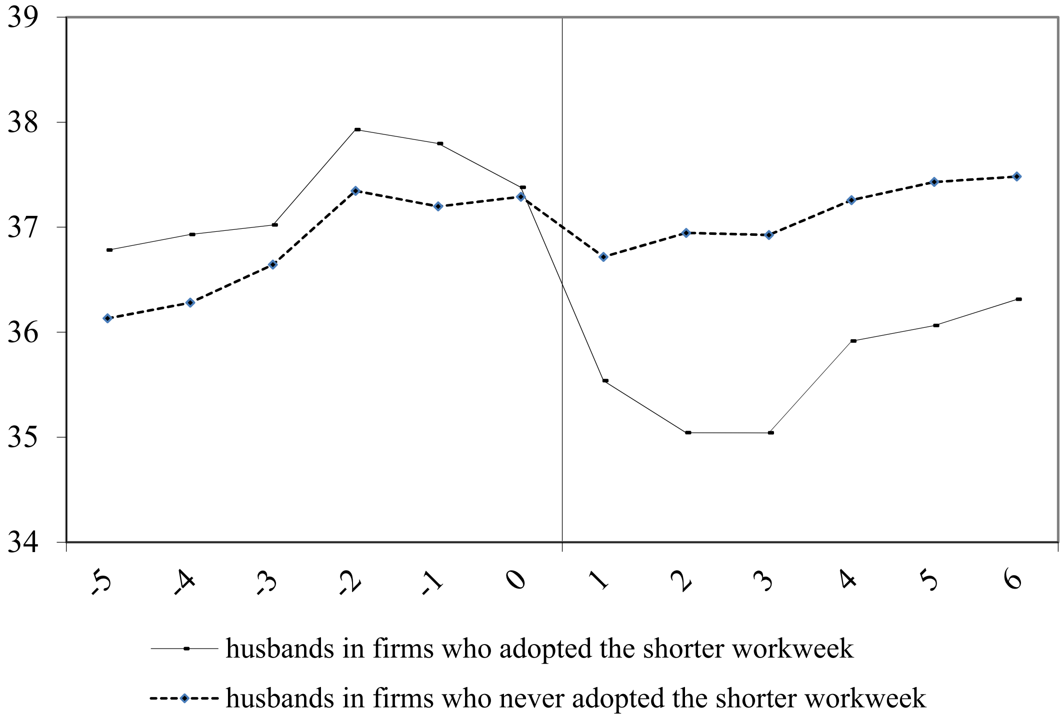


Figure VI
Women's employment probability, by husbands' treatment.

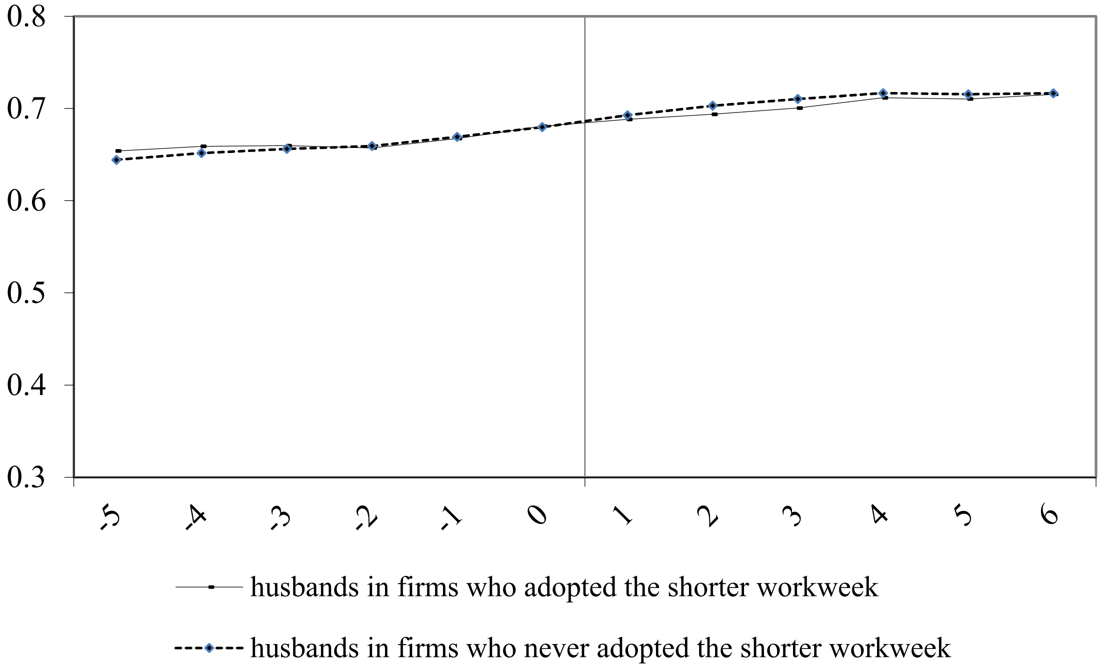
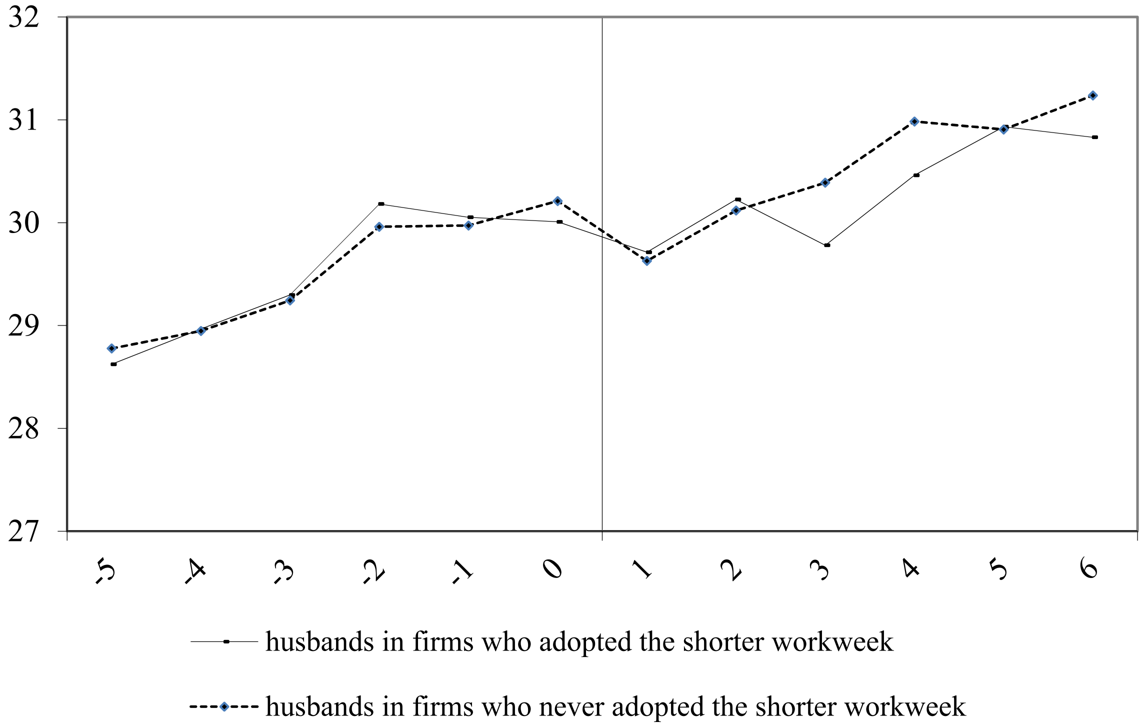


Figure VII
Women's hours worked, by husbands' treatment.



Appendix Tables and Figures

Table A1
First stage regressions.
Direct Effects of the Shorter Workweek on Hours Worked and Earnings.
Subsample of employed respondents.

Panel A	*Men*			
	Wives' hours		Wives' earnings	
	(1)	(2)	(3)	(4)
AS	1.34**	0.99**	0.086**	0.064**
	(0.15)	(0.14)	(0.008)	(0.005)
$ASPost$	-1.80**	-1.89**	0.002	-0.002
	(0.15)	(0.12)	(0.010)	(0.006)
Additional controls	no	yes	no	Yes
No. Observations	167 460	167 460	141 623	141 623
Panel B	*Women*			
	Husband's hours		Husband's earnings	
	(1)	(2)	(3)	(4)
AS	-0.28*	-0.32**	0.035**	0.009*
	(0.11)	(0.12)	(0.005)	(0.004)
$ASPost$	-2.12**	-2.08**	0.013*	0.004
	(0.11)	(0.12)	(0.007)	(0.004)
Additional controls	No	Yes	no	Yes
No. Observations	160 689	160 689	135 729	135 729

Notes. Columns 1 and 2: Employed subsample. Columns 3 and 4: Employed subsample restricted to respondents on which information on spouses' earnings is collected (from 2003 onward, information is collected on one third of the LFS sample). The table shows the results of regressing spouse's hours and earnings on the treatment status of spouse's firm (A^S) and on whether the shorter workweek is already adopted in the spouse's firm ($APost^S$). Baseline and additional control variables are as in Table III. Standard errors clustered at the treatment*year level are reported in brackets. **, and * denote significance at the 1% and 5% levels, respectively. Source: French LFS, 1994-2009, Insee.

Table A2
Overtime hours and paid holidays.

	One or more week of paid holidays not taken (%)	Positive overtime hours (%)	Number of overtime hours (conditional on overtime)	Fraction of overtime hours that are paid (%)
	(1)	(2)	(3)	(4)
		Men		
All men	12.4	23.0	7h06m	39.0
High-skill occupations	14.0	36.6	8h47m	16.4
Other occupations	11.9	20.4	6h31m	46.7
		Women		
All women	15.3	16.5	5h16m	26,5
High-skill occupations	13.4	31.9	6h53m	13.0
Other occupations	15.6	14.8	4h53m	29.7

Notes. Column 1: employed subsample. Columns 2, 3 and 4: employed subsample restricted to employees with usual hours. High-skill occupations include managers, professionals, engineers and associate occupations. Source: French LFS, 2003-2009, Insee.

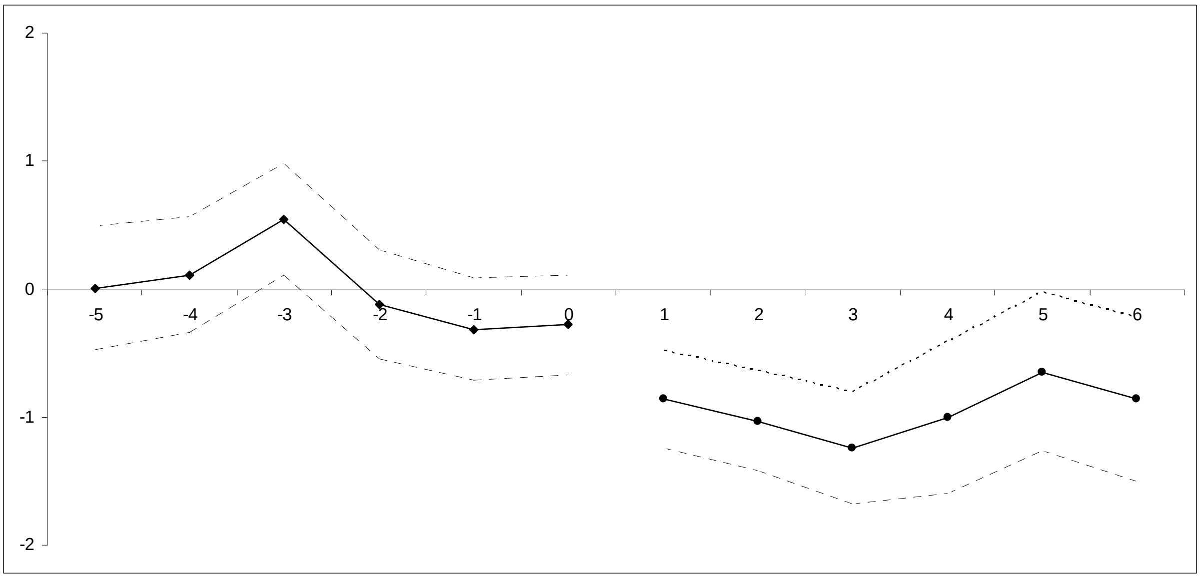
Table A3
Usual hours, actual hours and monthly earnings.

	Men					
	Monthly earnings					
	All		Pre-Reform		Post-reform	
	(1)	(2)	(3)	(4)	(5)	(6)
Usual hours (H_u)	6.48** (0.33)	6.50** (0.33)	6.29** (0.35)	6.31** (0.35)	9.16** (0.35)	9.25** (0.35)
Actual–usual hours ($H - H_u$)	0.30* (0.18)		0.29 (0.19)		0.43** (0.11)	
Overtime hours $(H - H_u)^+$		2.52** (0.39)		2.36** (0.39)		4.25** (0.26)
Unworked hours $(H - H_u)^-$		-0.05 (0.19)		-0.06 (0.21)		0.03 (0.08)
No. Observations	101 138	101 138	89 822	89 822	11 316	11 316

Notes. The sample includes employed men for which usual hours are defined. Columns 3 and 4 include observations in firms that have not (yet) adopted the shorter workweek. Columns 5 and 6 include observations in that have already adopted the shorter workweek. All regressions include controls as column (4) in Table IV. Standard errors clustered at the treatment*year level are reported in brackets. ** and * denote significance at the 1% and 5% levels respectively. Source: French LFS, 1994 to 2002, Insee.

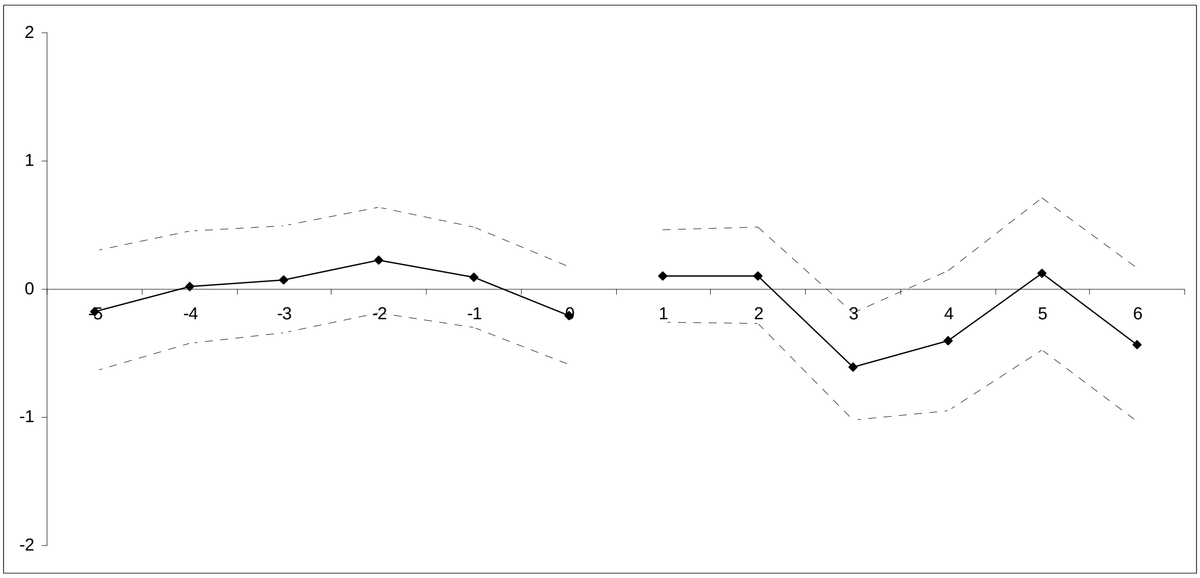
Figure A1
Differences in hours worked, by spouse's treatment.

Panel A – Men.



Notes. The solid line represents the difference between the hours levels plotted on Figure IV for husbands of treated and nontreated women, respectively, together with the 95% confidence interval.

Panel B – Women.



Notes. The solid line represents the difference between the hours levels plotted on Figure VII for wives of treated and nontreated men, respectively, together with the 95% confidence interval.