Worktime Regulations and Spousal Labor Supply

By DOMINIQUE GOUX, ERIC MAURIN AND BARBARA PETRONGOLO*

We study interdependencies in spousal labor supply by exploiting the design of the French workweek reduction, which introduced exogenous variation in one's spouse's labor supply, at constant earnings. Treated employees work on average two hours less per week. Husbands of treated women respond by reducing their labor supply by about half an hour, consistent with substantial leisure complementarity, and specifically cut the non-usual component of their workweek, leaving usual hours unchanged. Women's response to their husband's treatment is instead weak and rarely statistically significant, possibly due to heavier constraints in the organization of their workweek.

* Goux: CREST, 60 rue Etienne Dolet 92240 Malakoff, France (dominique.goux@ensae.fr); Maurin: Paris School of Economics, 48 Boulevard Jourdan, 75014 Paris, France (eric.maurin@ens.fr); Petrongolo: Queen Mary University, Mile End Road, London E1 4NS, U.K. and CEP, LSE, Houghton Street, London WC2A 2AE, U.K. (b.petrongolo@lse.ac.uk). We thank Philippe Askenazy, François Bourguignon, Shelly Lundberg and Thomas Piketty for helpful discussions as well as seminar participants at London School of Economics, Paris School of Economics, CREST Malinvaud Seminar, Bocconi University, University of Bologna, UC3M (Madrid), CEMFI (Madrid), ISER-Essex, University of Maastricht, Greqam (University of Aix-Marseille), the OECD, the CEPR conference on the Economics of Family Policy (Bergen 2010) and the CHILD Conference (Modena 2012) for valuable comments. The authors declare that they have no relevant or material financial interests that relate to the research described in this paper.

Interdependencies in spousal labor supply have long been identified as a key question in the study of household behavior (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 spouse, 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 interdependencies 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 family or social 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 are in most cases associated with important changes in 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 unique 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' working hours on the labor supply 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 at constant 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, firms who would implement the shorter workweek before the relevant deadline would benefit from significant payroll tax cuts. Only about 300,000 firms had implemented the shorter workweek before the comeback of the conservative party to power in April 2002 and the interruption of the original reform. Nevertheless, 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 may 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 the 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.

This paper uses a 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 employees whose employers signed a workweek reduction agreement.[1] When looking at spousal responses, we find that men tend to work about half an hour less per week when their wives become treated, while women's response to their husbands' treatment is generally weak and rarely significantly different from zero.

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 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 do some unpaid overtime, 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. Under the assumption 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 0.23, rising to 0.34 for managers and professionals, and to 0.59 for fathers of young children.

Our paper builds on a long strand of literature on family labor supply, investigating the response of an individual's labor supply to independent changes in her spouse's income

[1] We will discuss below various reasons why the average effect of the shorter legal workweek on actual weekly hours is lower than the legal workweek reduction.

and/or hours of work. These changes may in turn be driven by events as diverse as retirement, job loss, or fiscal reforms. Several studies document the positive association between husbands' and wives' retirement decisions, over and above what would be predicted by correlation in age and incentives in the retirement system (Blau, 1998; Gustman and Steinmeier, 2000). Conversely, the added worker effect literature detects mild substitutability between the labor supply of spouses, as married women tend to increase their working hours following husbands' job loss (Lundberg, 1985, Cullen and Gruber, 2000). More recently, Gelber (2012) exploits the Swedish tax reform of 1990-91 to examine own earnings' responses to changes in the marginal tax rate for one's spouse, and shows that as spousal earnings 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 agree in documenting important spillovers in the labor supply of spouses.

Our contribution to this literature is threefold. First, independent variation in spousal hours of work at constant earnings allows us to obtain cross-hour effects that are not confounded by income effects. In particular, under the assumption that an employee's workweek regulations affect their spouses only via their labor supply, we can identify the presence of leisure complementarity in the utility functions of spouses. Second, while previous work has mostly focused on the labor supply response of secondary earners, we find that it is in fact men who more strongly respond to their wives' treatment, while the corresponding women's response is much weaker. This may in turn be due to different degrees of leisure complementarities in the utility functions of spouses, or a to greater ability of men to control their working schedules. While we do not find compelling evidence on different preferences, the fact that women work shorter hours in the first place, and are less likely than men to hold managerial positions, suggests that they face relatively more binding constraints in the organization of their working time. Third, we provide evidence on specific adjustment margins in labor supply spillovers, and in particular we find that it is mostly men's unusual, rather than usual, hours that are affected when their wives' workweek is reduced.

In addition to the literature on household labor supply, our paper relates to another strand of the labor supply literature, investigating differences between micro and macro labor supply elasticities. Macroeconomic calibrations typically imply much larger labor supply elasticities than microeconometric estimates (Chetty et al. 2011a,b), and the recent literature has investigated two main channels potentially driving such gap. First, work on social multipliers illustrates how social interactions would magnify aggregate responses relative to individual behavior in a range of contexts, including labor supply (Glaeser, Sacerdote and Scheinkman, 2003; Maurin and Moschion, 2009). Second, recent studies on optimization frictions have shown that costs of adjusting working hours at the intensive margin attenuate micro elasticities relative to aggregate responses (Chetty et al., 2011a; Chetty, 2012).

Our work contributes to the understanding of mechanisms underlying either channel and the interaction between them. Specifically, labor supply spillovers are substantially shaped in nature and magnitude by optimization frictions, insofar the cost of adjusting working hours restricts spousal labor supply responses to workers who have fewer constraints in organizing their workweek, and to the nonusual component of their workweek. The resulting labor supply spillovers are thus strongly asymmetric, whereby women's treatment affects male labor supply but not viceversa (with very few exceptions), and independent changes in usual working hours produce spillovers on nonusual hours, but not viceversa. Spillovers on nonusual hours may in turn have an impact on productivity and profitability, while the absence of spillovers on usual hours would in most cases rule out an impact on current earnings. As optimization frictions in working hours are likely to bind in a variety of institutional contexts, the French case study considered here can shed light on the nature and magnitude of labor supply spillovers in other scenarios.

Finally, our paper contributes to the literature on work-sharing policies in developed countries.[2] The study which is closest to ours is Hunt (1998), who shows that the gradual decline in standard working hours of male employees between 1985 and 1995 in Germany was not accompanied by changes in their wives' employment rates, but by a small decline in their hours of work. This result, while consistent with complementarity in spousal leisure, may also reflect wives' own gradual exposure to shorter standard workweeks.

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

[2] The employment effects of workweek reduction reforms in France are studied by Crépon and Kramarz, 2002, Askenazy, 2008, Estevao and Sa, 2008, and Chemin and Wasmer, 2009 – among others.

I. Historical and Institutional Context

Since the early 1980s, the legal workweek duration in France has been 39 hours, accompanied by a 25% overtime wage premium and a 130 overtime hour limit per worker per year. This scenario was substantially changed in the late 1990s. 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, after the then labor secretary Martine Aubry), passed in June 1998, set the legal workweek at 35 hours in the private sector and mandated its implementation 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. Firms who would implement the shorter workweek through collective agreements 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.[5] 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 worksharing on profitability.

In January 2000, the second law (*Aubry* II) introduced a few amendments in order to limit the burden of the shorter workweek on employers. Specifically, with a slight redefinition of working time, it made it possible for employers to exclude "unproductive breaks" from the hours count, and thus achieve some reduction in the measure of working hours without changing work schedules. Also, it allowed firms to implement shorter hours on an annual – rather than weekly – basis, with a 1600 annual hour cap. This means that fiscal advantages could be obtained even with actual workweek reductions below 10%. Finally, the *Aubry II*

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

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

[5] As in principle there might be an income effect through overtime pay, we will illustrate in Section III the (lack of) earnings effects of the shorter workweek.

law introduced a two-year transitional phase during which it was possible for employers to keep the 39-hour workweek by using overtime at a reduced 10% rate.

Two years later, in summer 2002, the conservative party came back to power and, while the Aubry laws remained formally in place, the transition to the shorter workweek was discontinued in practice. The new 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 scenario. Following these political changes, the 35-hour workweek was never fully implemented, especially in small private firms. Nevertheless, the *Aubry* laws have had a very large impact on the French economy, covering about 10 million workers by 2002.

In a nutshell, 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. We build on these features of the reform in order to evaluate the effect of an exogenous variation in an employee's workweek on the labor supply of her spouse.

II. Data and Descriptive Evidence

A. The dataset

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. Individual level information comes from the French Labor Force Survey, which is conducted by the French Statistical Office, 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). Since 2003, 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 1994-2009, 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, education, industry, monthly earnings and hours worked. We exploit information on both actual hours worked during the reference week (typically the week before the survey), and usual hours worked in a typical week. [6] Crucial for our purposes, our restricted use version of the LFS also provides coded employer identifiers.[7] These allow 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 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. 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.

In our analysis we select all married or cohabiting individuals aged 18-65, whose spouse is a wage-earner, and we focus on the labor supply response of these individuals to their spouses' exposure to the shorter workweek. We define treatment as working for an employer who has signed a workweek reduction agreement,[8] and we drop the small number of individuals whose spouses were treated either before 1996 or after 2002, since these early and late agreements may not correspond to the reform implemented in the late 1990s. Our working sample includes 189,894 males and 236,802 females. Descriptive statistics on these samples are provided in Table A1 and Figures A1 and A2 of the online appendix.

To illustrate the timing of treatment, Figure 1 shows the gradual implementation of the shorter workweek on our sample. While only about 40% of employees are eventually treated,

---

[6]According to the official ILO (2002) definition, usual hours 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), and does not include irregular or unusual overtime, nor unusual absences or rest. French labor laws require contracts to be explicit about hours, pay, tasks and paid leaves, and as a consequence interviewees would know precisely their normal hours as well as contractual changes in these. Moreover information in the LFS is collected through face-to-face interviews during which INSEE interviewers attempt to make sure that respondents understand questions and answer in a consistent way. This procedure considerably reduces measurement error on hours of work relative to self-filled questionnaires (Baum-Snow and Neal, 2009).

[7] 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. Most cases of 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).

[8] Note that we never use hours reported in the LFS to assign treatment status, but administrative information collected independently by the Ministry of Labour. This prevents us from generating an artificial correlation between our indicator of treatment status and weekly hours.

there is substantial variation in treatment dates between 1998 and 2002. Table 1 reports the distribution of own and spousal treatment for employed respondents, 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. While there is some assortative mating along the treatment dimension, 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. Information on exact agreement dates 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.

B. Graphical Evidence: Direct and Cross-effect of Treatment

Before moving on to regression analysis, we provide simple graphical evidence on the direct and cross-effects of the workweek reform. Figure 2 plots actual hours worked during the survey week by wives who are wage earners, by treatment status. 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 drop 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 are observed.[9] Their weekly hours follow a gradually rising trend throughout the sample period, with no break at time zero. 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 who 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. Figure B1 (Panel A) in the online appendix plots treatment-control differences in these series, together with the corresponding confidence intervals, and shows flat pre-treatment differences, followed by a permanent, 2-hour drop in correspondence of treatment.

The observed drop in weekly hours for treated wives relative to the non-treated is a first-stage effect for the cross-hour effect on men 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 (39-35=4), and this may be explained by a number of factors, including slight redefinitions of

[9]For each treated individual i, we obtain the average number of hours worked by never treated individuals observed in the LFS in the same year as i, denoted by $H_{c(i)}$. For each $D = -5, -4, \dots, +6$, the dotted line in Figure 2 shows the average of $H_{c(i)}$ over the population of treated individuals i observed at a distance D from treatment (where $D =$ year of observation-year of treatment).

working time and/or the possibility to implement the worktime regulation at the annual rather than weekly level[10] (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 incidence of relatively short workweeks among French employees in the pre-reform period. Specifically, about 39% of females and 16% of males usually worked less than 39 hours per week before treatment[11]. The estimated 2-hour drop in working hours can thus be interpreted as an average of a higher drop for women initially working 39 hours or more, and a smaller drop for those initially working less than 39 hours.

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 3 shows flat and virtually identical employment patterns of husbands of treated and non-treated wives. Figure 4 then addresses corresponding variations at the intensive margin, by showing the impact on hours worked by the subsample of employed men, and reveals a sizeable drop in hours worked by husbands of treated women, relative to husbands of non-treated women, at time of treatment. Specifically, the difference in working hours is close to zero during the five pre-treatment years, and rises to 40 minutes on average during the five post-treatment years. The difference between the two series shows no evidence of differential pre-trends, and jumps permanently upon treatment (Figure B2, Panel A).

As the observed cross-effects might be partly induced by cases of simultaneous treatment of spouses, we replicate the corresponding trends on a sub-sample that excludes men treated at the same date as their wives, and on a subsample that excludes men ever treated, respectively. Reassuringly, Figures B3 and B4 in the online appendix provide a very similar picture of cross-hour effects as Figure 4. In the regression analysis that follows we pool all households and control for own and spouse treatment separately.

Figures 5 to 7 repeat a similar analysis for female respondents and their husbands. Again we observe a clear first-stage effect for husbands (Figure 5), whose magnitude is very close to that observed for wives in Figure 2 (differences in these series are plotted in Panel B

[10]For example, an employer could cut the usual workweek to 37 hours and grant 12.5 additional days of annual leave. In treated firms, about 38% of male employees and 23% of female employees declare having usual workweeks longer than 35 hours after treatment.

[11]Note that for some employees the reform was not even binding, as about 6.5% and 31% of men and women, respectively, had usual hours below or at 35 in the pre-treatment period. For women, short usual workweeks mostly correspond to part-time work. For men, they correspond mostly to specific jobs and working conditions (e.g. night work, evening work, Sunday work, rotating shift patterns, etc.).

of Figure B1). However, we find no evidence of spillover effects on their wives' labor supply, either at the extensive margin (Figure 6), or the intensive margin (Figure 7). The difference between these series is essentially flat, and does not display any permanent jump upon treatment (Figure B2, Panel B).

The descriptive evidence presented is thus suggestive of labor supply spillovers at the intensive margin for men, but no spillovers at either margin for women. The next sections will show estimates of these effects that control for observable characteristics of the individuals, and explore further the nature of these spillovers.

III. Regression Results

A. 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 the first-stage effect of treatment on spouses. 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:

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

where $APost_{it}^{S}$ indicates the period following the introduction of the shorter workweek 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 2 shows the regression results for specification (1) for wives (Panel A) and husbands (Panel B). 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 cut their labor supply by about 1.81 hours per week once the shorter workweek is implemented, as it was also evident from Figure 2. Turning to husbands, column 1 in Panel B shows 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 (column 2), suggesting that the implementation of the shorter workweek was largely orthogonal to these job and worker characteristics. 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.

We next assess labor supply spillovers by looking at the reduced-form effects of one's spouse's workweek reduction on *own* employment status and 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 2. Our reduced-form specification for hours is

$$H_{it} = \gamma_1 A_{it}^S + \gamma_2 APost_{it}^S + \gamma_3 A_{it} + \gamma_4 APost_{it} + \gamma_5 X_{it} + D_t + \varepsilon_{it}, \qquad (2)$$

where H_{it} denotes own weekly hours, A_{it} is a dummy variable denoting whether one's employer has ever implemented a shorter workweek agreement, whereas $APost_{it}$ indicates the period following this agreement. The main coefficient of interest is γ_2. Note that this specification 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. A similar linear specification to model (2) is used for the extensive margin, where the dependent variable is a dummy for the respondent's employment status, and clearly A_{it} and $APost_{it}$ are not defined.

The regression results are reported in Table 3. Columns 1 and 2 refer to employment, and columns 3-6 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 3. For women, the cross-effect on employment becomes marginally significant when further controls are included in column 2, but its magnitude is negligible. As we find virtually no impact on employment, we next look at hours worked for those who are employed. In column 3 of Panel A we regress men's hours on own treatment (A_{it} and $APost_{it}$), and on their wives' treatment (A_{it}^S and $APost_{it}^S$). The own treatment effect is about -2, and the cross-effect is -0.44 and highly significant, showing that when their wives become subject to the shorter workweek, men reduce their weekly labor supply by nearly half an hour. The magnitude of the cross-effect stays unchanged when we control for individual characteristics (column 4), and when we exclude men who are treated in the same year as their wives (column 5) or men who are ever treated (column 6). We next let the effect of treatment to vary over time, and in particular we estimate a reduced-form specification that includes all controls as in column 4 of Table 3, having interacted A_{it}^{S} with a full set of pre- and post-treatment dummies. The associated estimates are reported in Figure B5 (Panel A) of the online appendix, and show no pre-treatment effects, together with a permanent drop at time of treatment. In other words, post-treatment estimates are stable and all quite close to the overall treatment effect of -0.44.

Panel B of Table 3 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 spousal spillovers in the labor supply of women.

We further explore cross-effects by estimating reduced-form specifications across the whole hours distribution. Specifically, for each k between 15 and 49, we estimate reduced-form equations for the probability of working longer than k hours. These coefficients are reported in Figure 8, together with the corresponding 95% confidence interval. For men, cross effects on hours feature among the whole hours distribution, but most heavily for men working $35 - 38$ hours, and this result replicates very closely on a subsample that excludes men ever treated (graph not reported). For women, cross effects are much weaker and typically not statistically significant across the entire distribution, but if anything they involve a slight reduction in the incidence of long workweeks ($40 \leq H \leq 45$).

B. *Further Estimates: Cross-effects on Usual and Non-usual Working Hours*

We next investigate the nature of labor supply spillovers in further detail by combining information on actual hours (H) with information – also contained in the LFS – on usual hours (H_u), defined as the number of hours worked in a typical week. Actual hours H are the sum of the usual workweek H_u and a non-usual labor supply component $H - H_u$, which may be either positive or negative, depending on whether overtime hours exceed various forms of "undertime" hours (e.g. unusually short working days, sickness absence, paid or unpaid leaves, etc.) in a given week.[12] A worker may reduce weekly hours H by either negotiating a new contract with her employer, involving lower H_u, or keeping her contract unchanged, together with the associated H_u, but cutting on $H - H_u$, and namely some form of work

[12] 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 undertime. Descriptive statistics on overtime and undertime are reported in Section D of the online appendix.

involvement that is typically not specified in a contract. This may imply a reduction in overtime work or an increase in the take-up rate of leaves or in absenteeism. It is reasonable to expect that cross-effects mostly occur through reductions in $H - H_u$, since these would not require the renegotiation of one's labor contract, and are more easily under an employee's individual control than adjustments in H_u. On the other hand, the direct effect of the law is expected to bite on H_u, consistently with the collective nature of these agreements.

Estimates reported in Table 4 shed light on these adjustment margins. The sample period is now restricted to 1994-2002, as information on usual hours is unavailable from 2003 onwards. Estimates in Panel A refer to men. Columns 1 and 2 show that, as anticipated, the first-stage effect of the workweek reduction in their wives' firms mostly bites on usual hours (-1.75), while the effect on nonusual hours is much weaker (-0.54). By contrast, columns 3 and 4 show that the reduced-form effect of the reform on own hours works entirely via a reduction in nonusual hours (-0.62), with no cross-effect on usual hours (-0.05), and thus no need to renegotiate own work schedules for men responding to their wives' work schedules. For women (Panel B), we detect very similar first-stage effects as for men, but a small, albeit positive, cross-effect on H_u (0.17).

Changes in nonusual hours and earnings are further explored in Table 5. Columns 1 and 2 report cross-hour effects on overtime hours and undertime hours separately. These are defined as $(H - H_u)^+ = \max(H - H_u, 0)$ and $(H - H_u)^- = \max(H_u - H, 0)$, respectively. Cross-hour effects feature strongly on undertime hours (0.54), while overtime hours are hardly affected (-0.07). Cross-effects on undertime hours in turn involve an increase in the frequency of both unworked weeks ($H = 0$, column 3) and unusually short workweeks ($0 < H < H_u$, column 4), but no change at all in full-time status (column 5). For cases in which $H < H_u$, respondents are asked whether they worked less than usual in the reference week due to holidays and absence for personal reasons, sickness leave, maternity leave, continuous training, unusual workload, strike, or lock-out. While we detected significant cross-hour effects for holidays and sickness leaves, which are margins on which employees have closer control, we found no evidence of cross-effects on any other margin (results not reported).[13]

Finally, we do not find any detrimental cross-effect on male earnings (column 6), consistently with evidence on the contribution of various components of actual hours (usual,

[13] Information on the take-up rate of paid leaves and paid and unpaid overtime work contained in later waves of the LFS (2003-2009) confirms that there exists significant leeway for most employees, and especially for the high-skilled, in reducing their unpaid involvement at work.

overtime and undertime, respectively) to monthly earnings, as illustrated in Table C1 in the online appendix. Interestingly, undertime hours turn out to be the sole component of labor supply that men may cut unilaterally without earning losses.

No hours margin is significantly affected for women (Panel B), except the incidence of part-time work, which falls by nearly 1 percentage point. The slight increase in the usual workweek and the corresponding change in full-time status are accompanied by an increase in earnings (2%), in line with the fact that usual hours are the labor supply component that best predicts earnings (Table C1).

In summary, we detect substantial differences in both the magnitude and nature of spillover effects across genders. Specifically, cross-effects do not entail the renegotiation of usual hours with employers or changes in earnings for men, but involve instead a reduction in their unusual work involvement, whether within a given day, or through an increase in the take-up rate of paid vacation or sick leave, with no detrimental impact on (current) earnings. A reason why men may work some unpaid 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 mechanism, 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.

Women, by contrast, are more often working part-time and less often spending unpaid, nonusual hours at work. Compared to men, it is on average more costly for women to adjust hours downward, insofar they have lower nonusual hours margins than men, but less costly to adjust hours upward, as in the public sector and large private sector firms employees can easily shift from part-time to full-time status, and only among women is the incidence of part-time work substantial. The French reform thus provides a clean example of the role of optimization frictions in shaping the magnitude and nature of social spillovers.

C. Heterogeneous Cross-hour Effects

As working hours, constraints and preferences may vary widely across individuals, cross-hour effects may differ across occupations and the household composition of workers. Workers in high-skill occupations (managers, professionals and associate occupations) on average work longer hours than the less-skilled and typically have higher control over the organization of their workweek, while the less-skilled are more likely to work the legal workweek and thus would only be able to cut their working hours via new contractual agreements.

Panels A and B in Table 6 replicate our previous analysis on actual hours for employees in high-skill occupations and other employees, respectively. First-stage effects reported in column 1 have conventional magnitude and significance. For men, the associated cross-effect on hours is about three times larger for high-skill occupations (column 2) than for other occupations (column 3). Similar conclusions can be drawn by looking at the probability of working more than 45 hours weekly (columns 3 and 6). Spillover effects on men's labor supply thus seem much stronger for the high-skilled than for the less-skilled.[14] For women, we do not find significant cross-effects on overall working hours, but we do find a negative and significant impact on the probability that females in high-skill occupations work long weeks. This is the only subsample and only outcome variable for which we detect symmetric cross-effects for men and women. We found in Section III.A that women are slightly less likely to work very long hours when their husbands are treated (Figure 8), and we note here that for female managers and professionals this effect is as strong as for men, suggesting that when women have enough leeway to cut their hours – either because they work very long hours in the first place or they have managerial control – their labor supply response is qualitatively similar to that of men. However, the subsample of such women is too small, and their labor supply response too weak, for this effect to be discernible on the full sample.

We further explore spousal labor supply responses across household types. It has been argued that interdependences in spousal labor supply may be stronger in the presence of young children, as children appear to play the role of a jointly-consumed commodity for husbands and wives (Lundberg, 1988). Panels C and D of Table 6 cover households with at least one child aged 0-6, and other households separately. We find weaker first-stage effects for mothers of young children than for other women, in line with higher incidence of part-time work among mothers, as for part-timers the mandatory workweek reduction is not necessarily binding. Reduced-form regressions show a much stronger labor supply reaction for fathers of young kids than other men, despite a weaker first stage. For women, cross-effects are somewhat mixed, as we detect a positive, rather than negative, cross-hour effect for mothers of young kids, and a negative cross-effect on the probability to work long weeks for other women.

[14] In the online appendix, we also show that cross-effects for men are stronger in the public than in the private sector, consistently with the presumption that public employees in France tend to have, other things equal, greater control than private employees in organizing their working time (see Table D1, Panel A).

IV. Robustness Analysis

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 may think of scenarios in which this assumption is potentially violated, and we perform a number of robustness tests that should address various caveats to a causal interpretation of our estimates.

First, one should worry about the existence of differential pre-existing trends in working hours between treatment and control groups. However, the event-study type of evidence presented in Figures 2-7 clearly shows that this is not the case, as pre-trends are in all cases parallel or even flat. This is also confirmed by estimates of reduced-form specifications that control for treatment-specific trends, reported in columns 1 and 4 of Table E1 in the online appendix. Columns 3 and 6 in Table E1 further control for region*year interactions, capturing the effect of local shocks, and show virtually unchanged estimates from columns 1 and 4, respectively.

Second, our identifying assumption would be violated if spouses of employees in firms adopting the shorter workweek were subject to systematically different shocks or changes in unobservables around time of adoption, versus spouses of employees in non-adopting firms. If changes in unobservables of treatment and control groups would generate spurious changes to their labor supply, one would possibly expect to observe some change in some of their observables as well around the time of treatment. But Table E2 shows no evidence of any significant change in such characteristics upon treatment. Third, we take into account concerns of reverse causality, namely the possibility that changes in own labor supply behavior may affect spousal job mobility between adopting and nonadopting firms, and replicate our reduced-form specifications on a subsample of spouses of job-stayers (online appendix, page 5).

Finally, one may worry that in general employees in adopting (or early-adopting) firms would have systematically different spouses from employees in nonadopting (or late-adopting) firms. To address this concern, we provide fixed-effect estimates of the effects of interest, based on a (limited) rotating panel component of the LFS (Table E4). This last robustness test confirms our main estimates[15].

[15]We also checked that our estimates are very similar whether identification only relies on variation in hours across treated and nontreated spouses, or across early and late-treated spouses (Section E.2 in the online appendix).

V. Instrumental Variable Estimates of Cross-hour Effects

There is a long standing tradition of labor supply models in which the decisions of each spouse depend 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 as 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. In such scenario the French workweek reform helps identify the effects of interest by generating exogenous variation in the labor supply of one's 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. Under the exclusion restriction that workweek regulations affect spousal labor supply only via their effect on the labor supply of directly treated employees, IV estimates provide the parameter of interest for measuring how labor supply responds to independent changes in labor supply of one's spouse, and may be generalized to a variety of scenarios.

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, 2011), 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. 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 for men and women stem from the same set of parameters in spouses' utility functions 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 (see detailed discussion in Section F of the online appendix).

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 7 for men (Panel A) and women (Panel B), using the same samples and specifications as in Tables 3 and 6. Among men, the average cross-hour effect in labor supply is 0.23, but about twice as large for managers and professionals than for other occupations. 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 other households. The quantitative response for fathers is about 35 minutes for each extra hour spent at home by their wives, 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. For women we detect no significant cross-hour effect on the whole sample or across the occupational divide, but we do find a negative, marginally significant cross-effect for mothers of young kids.

These estimates can be used to quantitatively evaluate the social multiplier, i.e. the gap between aggregate and individual effects of a labor supply shock. Macroeconomic calibrations existing in the literature typically imply much higher labor supply elasticities than individual-level estimates (Chetty et al. 2011a,b), and spousal labor supply complementarities represent an important channel for such gap. Our estimates reveal a strongly asymmetric structure of spillovers, whereby women's treatment affects male labor supply but not viceversa (with very few exceptions). Specifically, an average cross-hour effects of 0.23 for husbands and a negligible one for wives means that a unit change in individual hours translates into a change in household labor supply of 2.23. This implies a macro response that is 2.23/2-1=11.5% higher than the micro response for the average household. 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 workweek reform provides a clean experiment to identify the multiplier in labor supply at the household level.

Finally, our findings on specific margins of adjustments of weekly hours reveal that, due to search frictions and hours constraints, it is mostly nonusual hours that respond to spouse treatment, leaving usual hours largely unchanged. Thus the above estimates of the social multiplier are likely attenuated by optimization frictions, and may be interpreted as a lower bound for macro elasticities that one would observe absent frictions (Chetty et al., 2011a, Chetty, 2012).

VI. Conclusions

We have investigated cross-hour effects in the labor supply of couples using independent variation in spousal hours generated by changes in worktime regulations. 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. While wives of treated men hardly adjust their working time, 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 women's labor supply elasticity is typically higher than men's (Blundell and MaCurdy, 1999). These results suggest significant spousal complementarities in leisure time for men. While we do not find strong evidence on different preferences by gender, insofar women work shorter hours in the first place and are less likely than men to have managerial control, they may be more heavily constrained in the organization of their working time.

Our results on cross-hour effects are noteworthy as they show that neglecting spousal responses may give a misleading view of the overall impact of labor supply shocks. In particular, evaluations restricted to the direct impact of policy on the targeted population are likely to underestimate its overall effect on labor supply. A simple back-of-envelope calculation suggests a social multiplier around 1.11, thus neglecting spillovers within the household would yield an underestimate of the overall policy impact on labor supply by about 11%. 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.

REFERENCES

Abowd, John M. and Francis Kramarz. 1999. "The Analysis of Labor Markets using Matched Employer-Employee Data." In *Handbook of Labor Economics*. Vol. 3B, edited by Orley C. Ashenfelter and David E. Card, 2629-2710. Amsterdam: North-Holland.

Alesina, Alberto, Edward L. Glaeser, and Bruce Sacerdote. 2005. "Work and Leisure in the U.S. and Europe: Why So Different?", *NBER Macroeconomics Annual* 20, 1-100.

Apps, Patricia and Ray Rees. 1988. "Taxation and the Household." *Journal of Public Economics* 35 (3): 355-369.

Ashenfelter, Orley and James J. Heckman. 1974. "The Estimation of Income and Substitution Effects in a Model of Family Labor Supply." *Econometrica* 42 (1): 73-85.

Askenazy, Philippe. 2008. "A Primer on the 35-Hour Reduction in France: 1997-2007." *IZA* Working Paper No. 3402.

Baum-Snow, Nathaniel and Derek Neal. 2009. "Mismeasurement of Usual Hours Worked in the Census and ACS." *Economics Letters* 102 (1): 39-41.

Blau, David M. 1998. "Labor Force Dynamics of Older Married Couples." *Journal of Labor Economics* 16 (3): 595-629.

Blundell, Richard and Thomas E. MaCurdy. 1999. "Labor Supply: A Review of Alternative Approaches." In *Handbook of Labor Economics*. Vol. 3A, edited by Orley C. Ashenfelter and David E. Card , 1560-1695. Amsterdam: North-Holland.

Bourguignon, Francois. 1984. "Rationalité Individuelle ou Rationalité Stratégique: Le Cas de l'Offre Familiale de Travail." *Revue Economique* 35 (1): 147-162.

Chemin, Matthieu, and Etienne Wasmer. 2009. "The Employment Effects of 35-Hour Workweek Regulation in France: Using Alsace-Moselle Local Laws to Build a Diff-in-Diff." *Journal of Labor Economics* 27 (4): 487-524.

Chen, Zhiqi and Frances Woolley. 2001. "A Cournot-Nash Model of Family Decision Making." *Economic Journal* 111 (October): 722-748.

Chetty, Raj. 2012. "Bounds on Elasticities with Optimization Frictions: A Synthesis of Micro and Macro Evidence on Labor Supply." *Econometrica* 80 (3): 969-1018.

Chetty, Raj, John N. Friedman, Tore Olsen, and Luigi Pistaferri. 2011a. "Adjustment Costs, Firm Responses, and Micro vs. Macro Labor Supply Elasticities: Evidence from Danish Tax Records." *Quarterly Journal of Economics* 126 (2): 749-804.

Chetty, Raj, Adam Guren, Day Manoli, and Andrea Weber. 2011b. "Are Micro and Macro Labor Supply Elasticities Consistent? A Review of Evidence on the Intensive and Extensive Margins." *American Economic Review Papers and Proceedings* 101 (3): 471-475.

Chiappori, Pierre-André. 1988. "Rational Household Labor Supply." *Econometrica* 56 (1): 63-90.

Crépon, Bruno, and Francis Kramarz. 2002. "Employed 40 Hours or Not-Employed 39: Lessons from the 1981 Mandatory Reduction of the Weekly Working Hours." *Journal of Political Economy* 110 (6): 1355-1389.

Cullen, Julie B. and Jonathan Gruber. 2000. "Does Unemployment Insurance Crowd Out Spousal Labor Supply?" *Journal of Labor Economics* 18 (3): 546-572.

Estevao, Marcelo, and Filipa Sa. 2008. "Are the French Happy with the 35-Hour Workweek?" *Economic Policy* 55: 417-446.

Gelber, Alexander M. 2012. "Taxation and the Earnings of Husbands and Wives: Evidence from Sweden." Forthcoming, *Review of Economics and Statistics.*

Glaeser, Edward L., Bruce I. Sacerdote and Jose A. Scheinkman. 2003. "The Social Multiplier." *Journal of the European Economic Association* 1 (2-3): 345-353.

Goux, Dominique, and Eric Maurin. 1999. "The Persistence of Inter-industry Wage Differentials: A Reexamination on Matched Worker-firm Panel Data." *Journal of Labor Economics* 17 (3): 492-533.

Gustman, Alan L. and Thomas L. Steinmeier. 2000. "Retirement in Dual-Career Families: A Structural Model." *Journal of Labor Economics* 18 (3): 503-545.

Hamermesh, Daniel S. 2002. "Timing, Togetherness and Time Windfalls." *Journal of Population Economics* 15 (4): 601-623.

Hunt, Jennifer. 1998. "Hours Reduction as Work Sharing." *Brookings Papers on Economic Activity* 1: 339-381.

International Labor Organisation. 2002. *Key Indicators of the Labor Market: 2001-2002*, Geneva.

Lechene, Valérie and Ian Preston. 2011. "Non Cooperative Household Demand." *Journal of Economic Theory* 146 (2): 504-527.

Lee, Sangheon, Deirdre McCann and Jon C. Messenger. 2007. *Working Times Around the World*, London and New York: Routledge.

Lundberg, Shelly J. 1985. "The Added Worker Effect." *Journal of Labor Economics* 3 (1): 11-37.

Lundberg, Shelly J. 1988. "Labor Supply of Husbands and Wives: A Simultaneous Equation Model." *The Review of Economics and Statistics*, 70 (2): 224-34.

Maurin, Eric and Julie Moschion. 2009. "The Social Multiplier and the Labor Market Pariticipation of Mothers." *American Economic Journal: Applied Economics* 1 (1): 251-272.

McElroy, Marjorie B. and Marie Jean Horney. 1981. "Nash Bargained Household Decisions: Toward a Generalization of the Theory of Demand." *International Economic Review* 22 (2): 333-349.

Table 1
Distribution of Own Treatment, by Spouse's 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. The sample includes employed 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.

Source: French LFS, 1994-2009, Insee.

Table 2
First-stage Regressions
Direct Effects of the Shorter Workweek on Hours and Earnings

Panel A	*Men*			
	Wives' hours		Wives' (log) earnings	
	(1)	(2)	(3)	(4)
$APost^S$	-1.81**	-1.91**	0.002	-0.002
	(0.13)	(0.10)	(0.010)	(0.006)
Additional controls	no	Yes	no	yes
Mean dep. variable	30.05	30.05	8.658	8.658
No. Observations	189,894	189,894	160,046	160,046
Panel B	*Women*			
	Husband's hours		Husband's (log) earnings	
	(1)	(2)	(3)	(4)
$APost^S$	-1.95**	-1.92**	0.017*	0.007
	(0.13)	(0.14)	(0.008)	(0.004)
Additional controls	no	Yes	no	yes
Mean dep. variable	37.07	37.07	9.011	9.011
No. Observations	236,802	236,802	201,559	201,559

Notes. The table shows results from first-stage regressions for hours and earnings of spouses. Columns 1 and 2 refer to the full sample (married or cohabiting respondents whose spouse in an employee). Columns 3 and 4 refer to the subsample whose spouses have nonmissing earnings (from 2003 onwards, information on earnings is collected on one third of the LFS sample). Baseline controls include A^S, 15 year dummies and a dummy for public sector. Additional controls include 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.

.

Table 3
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)	(6)
$APost^S$	-0.0037	-0.0028	-0.44**	-0.45**	-0.50**	-0.44**
	(0.0027)	(0.0022)	(0.09)	(0.09)	(0.09)	(0.10)
$APost$	-	-	-1.96**	-1.96**	-2.02**	-
			(0.14)	(0.14)	(0.13)	
Further controls	no	yes	No	yes	yes	yes
Mean dep. variable	0.8819	0.8819	38.89	38.89	38.97	39.55
No. observations	189,894	189,894	167,460	167,460	156,392	115,445
Panel B	*Women*					
	Own employment		Own hours (conditional on employment)			
	(1)	(2)	(3)	(4)	(5)	(6)
$APost^S$	-0.0032	-0.0041	0.12	0.05	0.06	0.07
	(0.0023)	(0.0022)	(0.10)	(0.11)	(0.11)	(0.11)
$APost$			-1.86**	-1.88**	-1.86**	-
			(0.17)	(0.15)	(0.18)	
Further controls	no	yes	no	yes	yes	yes
Mean dep. variable	0.6786	0.6786	30.32	30.32	30.25	30.04
No. observations	236,802	236,802	160,689	160,689	150,371	116,596

Notes. The table shows results from reduced-form regressions in which own employment status and hours are regressed on spousal treatment (A^S and $APost^S$), as well as on own treatment (A and $APost$). Columns 1 and 2 refer to the full sample. Columns 3 and 4 refer to the subsample of employed respondents. Column 5 refers to employed respondents who were not treated at the same time as their spouses. Column 6 refers to employed respondents who were never treated. Baseline controls in columns 1 and 2 include A^S, 15 year dummies and spouse's public sector dummy. Additional controls in column 2 are own years of education, age and age square, and spouse's years of education, age and age square. Baseline controls in columns 3-6 include A, A^S, 15 year dummies, own public sector and wage-earner dummies, and a spouse's public sector dummy. Additional controls in columns 4-6 include own years of education, age, age square and 16 industry dummies, and spouse'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 4
First-stage and Reduced-form Regressions
Direct and Cross-effects of the Shorter Workweek on Usual and Nonusual Hours

Panel A	*Men*			
	First stage		Reduced form	
	Wife's usual Hours H_u	Wife's actual–usual hours $H - H_u$	Own usual hours H_u	Own actual–usual hours $H - H_u$
	(1)	(2)	(3)	(4)
$APost^S$	-1.75**	-0.54**	-0.05	-0.62**
	(0.15)	(0.16)	(0.05)	(0.14)
Mean dep. var.	33.79	-4.46	39.24	-3.17
No. obs.	97,470	97,470	97,470	97,470

Panel B	*Women*			
	First stage		Reduced form	
	Husband's usual Hours H_u	Husband's actual–usual hours $H - H_u$	Own usual hours H_u	Own actual–usual hours $H - H_u$
	(1)	(2)	(3)	(4)
$APost^S$	-2.02**	-0.46	0.17*	0.06
	(0.12)	(0.23)	(0.08)	(0.10)
Mean dep. var.	39.17	-3.18	33.33	-4.28
No. obs.	102,123	102,123	102,123	102,123

Notes. Regressions refer to the employed subsample with nonmissing own and spouse's usual hours. Control variables in columns 1 and 2 are the same as in column 2 of Table 2, and in columns 3-4 they are the same as in column 4 of Table 3. 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, Insee

Table 5
Reduced-form Regressions
Cross-effects of the Shorter Workweek on Types of Hours Worked and Earnings

Panel A	Men					
	Own overtime hours $(H-H_U)^+$	Own undertime hours $(H-H_U)^-$	Own unworked weeks $H=0$	Own unusually short workweeks $0<H<H_U$	Own part-time	Own (log) earnings
	(1)	(2)	(3)	(4)	(5)	(6)
$APost^S$	-0.07 (0.03)	-0.54** (0.11)	0.012** (0.003)	0.006* (0.003)	-0.002 (0.002)	0.002 (0.003)
Mean dep. var.	0.86	-4.03	0.088	0.065	0.031	9.004
No. obs.	97,470	97,470	97,470	97,470	97,470	97,470

Panel B	*Women*					
	Own overtime hours $(H-H_U)^+$	Own undertime hours $(H-H_U)^-$	Own unworked weeks $H=0$	Own unusually short workweeks $0<H<H_U$	Own part-time	Own (log) earnings
	(1)	(2)	(3)	(4)	(5)	(6)
$APost^S$	-0.06 (0.03)	0.12 (0.10)	-0.005 (0.003)	0.005 (0.003)	-0.012** (0.004)	0.020** (0.004)
Mean dep. var.	0.56	-4.84	0.129	0.061	0.323	8.587
No. obs.	102,123	102,123	102,123	102,123	102,123	102,123

Notes. Regressions refer to the employed subsample with nonmissing own and spouse's usual hours. Control variables are the same as in column 4 of Table 3. In column 2, the interpretation of positive coefficients is that the fall in labor supply is now picked up by an *increase* in undertime hours. 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, Insee

Table 6
Heterogeneous Effects of the Shorter Workweek, by Occupation and Family Type

Panel A	*Men*					
	Managers, profs. and kindred occup.			*Other occupations*		
	First stage	Reduced form		First stage	Reduced form	
	Wife's hours	Own hours	Own hours ≥ 45	Wife's hours	Own hours	Own hours ≥ 45
	(1)	(2)	(3)	(4)	(5)	(6)
$APost^S$	-2.32**	-0.81**	-0.033**	-1.72**	-0.32**	-0.006**
	(0.30)	(0.27)	(0.009)	(0.11)	(0.09)	(0.002)
Mean dep. var	29.80	40.91	0.447	30.20	38.44	0.217
No. obs.	30,432	30,432	30,432	137,028	137,028	137,028
Panel B	*Women*					
	Managers, profs. and kindred occup.			*Other occupations*		
	First stage	Reduced form		First stage	Reduced form	
	Husband's hours	Own hours	Own hours ≥ 45	Husband's hours	Own hours	Own hours ≥ 45
	(1)	(2)	(3)	(4)	(5)	(6)
$APost^S$	-2.51**	-0.17	-0.034**	-2.03**	0.15	-0.001
	(0.40)	(0.34)	(0.008)	(0.13)	(0.11)	(0.002)
Mean dep. var	38.51	32.03	0.196	36.91	30.14	0.069
No. obs.	15,217	15,217	15,217	145,472	145,472	145,472
Panel C	*Men*					
	At least one child aged 0-6			*No children aged 0-6*		
	First stage	Reduced form		First stage	Reduced form	
	Wife's hours	Own hours	Own hours ≥ 45	Wife's hours	Own hours	Own hours ≥ 45
	(1)	(2)	(3)	(4)	(5)	(6)
$APost^S$	-1.30**	-0.81**	-0.028**	-2.08**	-0.34**	-0.003
	(0.23)	(0.28)	(0.008)	(0.13)	(0.12)	(0.003)
Mean dep. var	27.53	38.85	0.260	30.93	38.91	0.259
No. obs.	39,468	39,468	39,468	127,992	127,992	127,992
Panel D	*Women*					
	At least one child aged 0-6			*No children aged 0-6*		
	First stage	Reduced form		First stage	Reduced form	
	Husband's hours	Own hours	Own hours ≥ 45	Husband's hours	Own hours	Own hours ≥ 45
	(1)	(2)	(3)	(4)	(5)	(6)
$APost^S$	-2.25**	0.49	-0.001	-2.04**	-0.08	-0.005*
	(0.24)	(0.25)	(0.003)	(0.11)	(0.10)	(0.002)
Mean dep. var	37.16	27.94	0.063	37.03	31.03	0.086
No. obs.	36,959	36,959	36,959	123,730	123,730	123,730

Notes. Regressions refer to the employed subsample. In columns 1 and 4, control variables are the same as in column 4 of Table 3, and in columns 2, 3, 5, 6 they are the same as in column 4 of Table 4. 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 7
IV Estimates of Cross-hour Effects

Panel A	*Employed men*				
	Own hours				
	All	High-skilled	Other occupation	One or more child 0-6	Other households
	(1)	(2)	(3)	(4)	(5)
Wife's hours	0.23**	0.34**	0.18**	0.59**	0.16**
	(0.05)	(0.12)	(0.05)	(0.21)	(0.06)
Mean dep. variable	38.89	40.91	38.44	38.85	38.91
No. observations	167,460	30,432	137,028	39,468	127,992
Panel B	*Employed women*				
	Own hours				
	All	High-skilled	Other occupation	One or more child 0-6	Other households
	(1)	(2)	(3)	(4)	(5)
Husband's hours	-0.02	0.08	-0.07	-0.23	0.04
	(0.05)	(0.13)	(0.06)	(0.12)	(0.05)
Mean dep. variable	30.32	32.03	30.14	27.94	31.03
No. observations	160,689	15,217	145,472	36,959	123,730

Notes. Regressions refers to the employed subsample. Estimates reported show the effect of spousal hours (H^S) on own hours (H), using spousal treatment ($APost^S$) as an instrument. The corresponding reduced-form results are reported in Tables 3 and 6. Further controls include A, $APost$, A^S, 15 year dummies, a wage-earner dummy and the following variables for each spouse: a public sector dummy, 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.

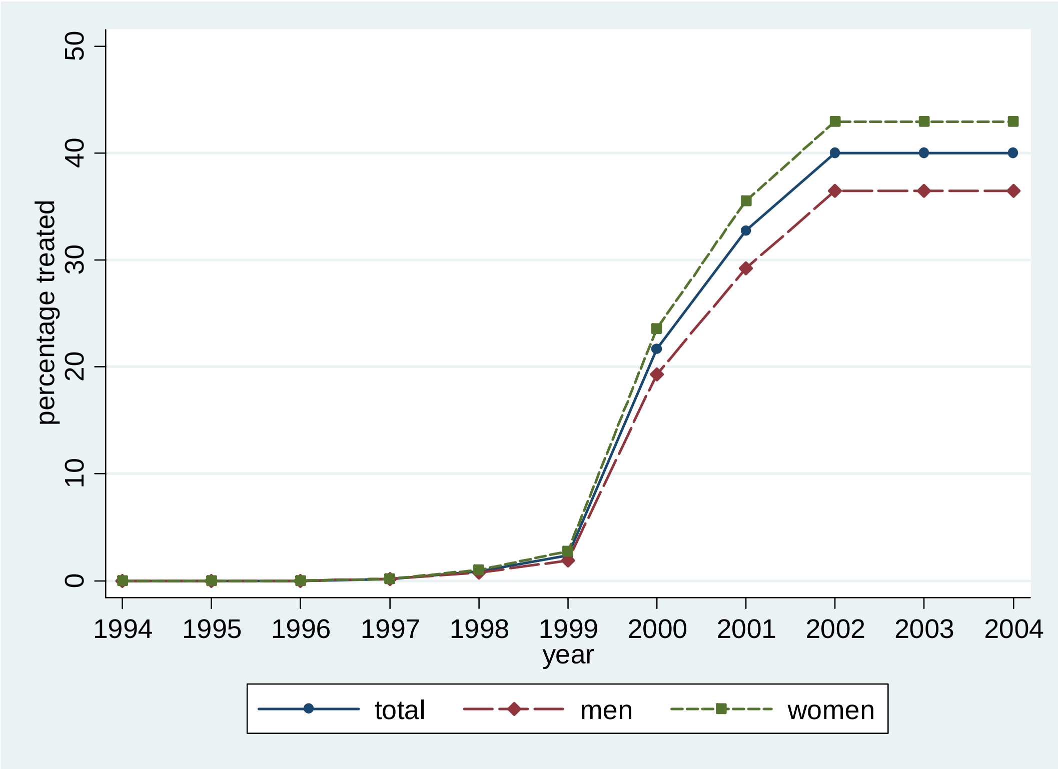


Figure 1. Timing of Implementation of the Shorter Workweek:
Percentage of Employees Treated

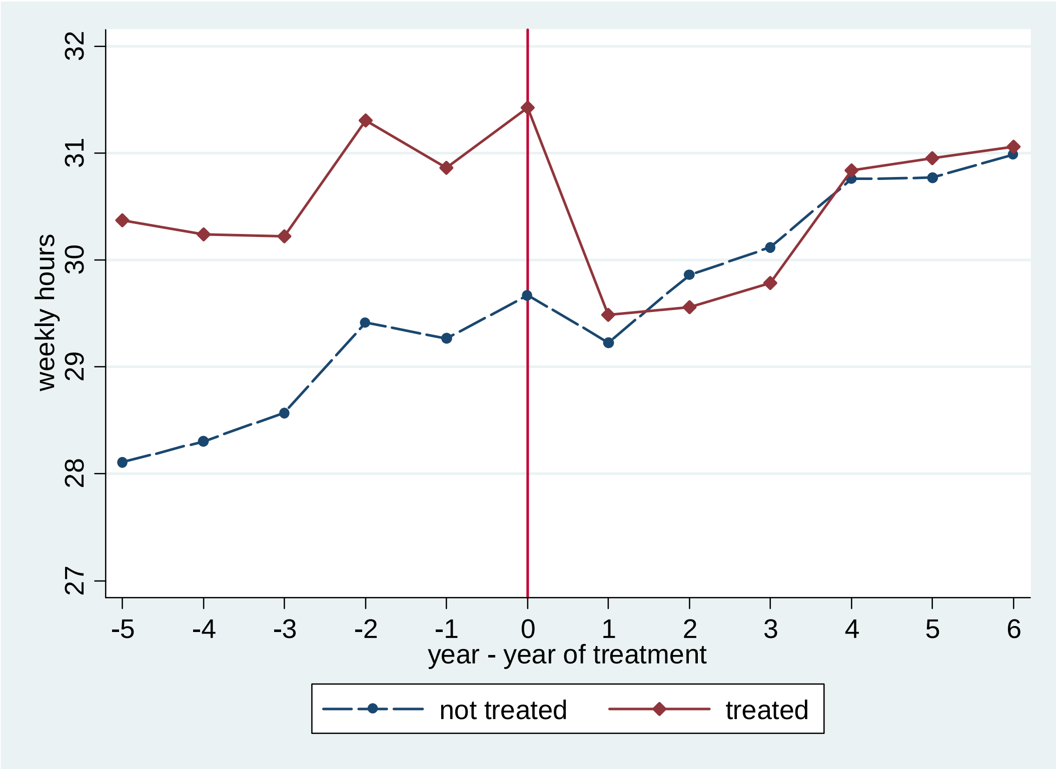


Figure 2. Wives' Hours Worked, by Own Treatment

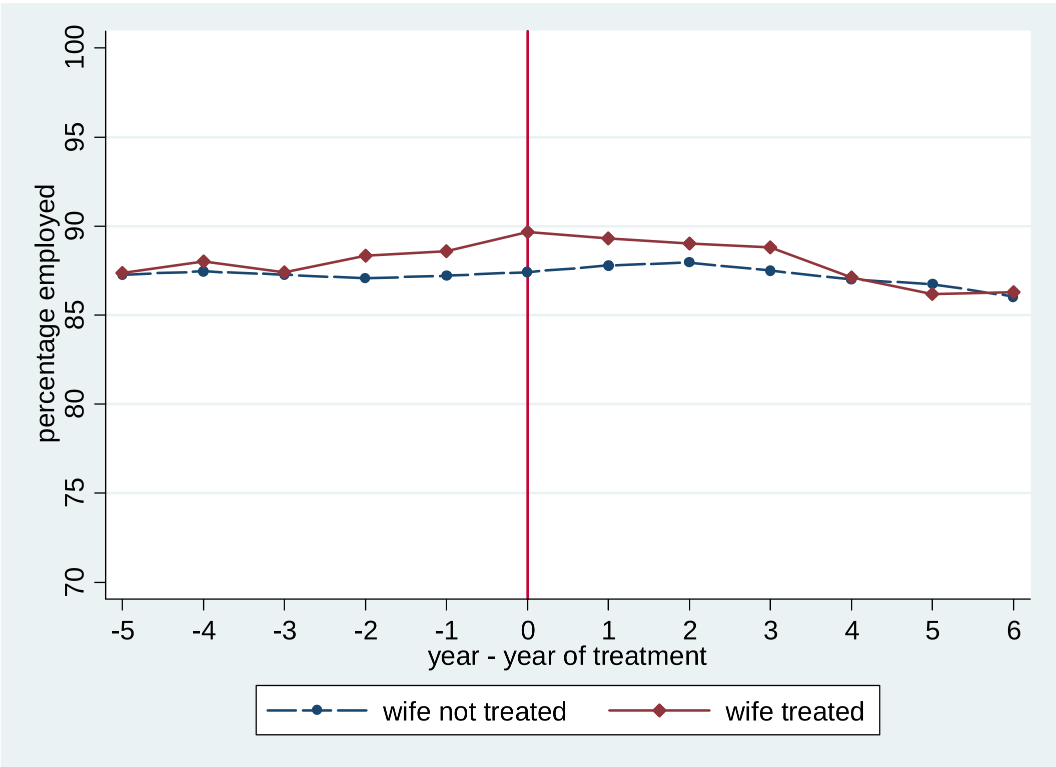


Figure 3. Men's Employment Rates, by Wife’s Treatment

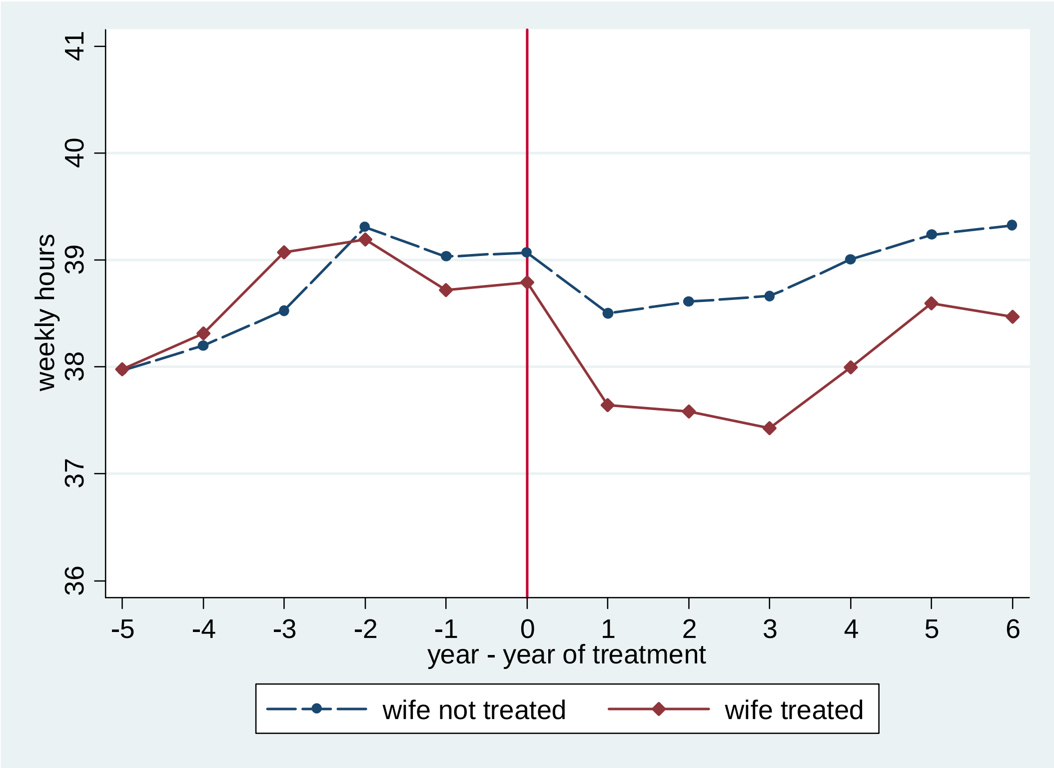


Figure 4. Men's Hours Worked, by Wife’s Treatment.

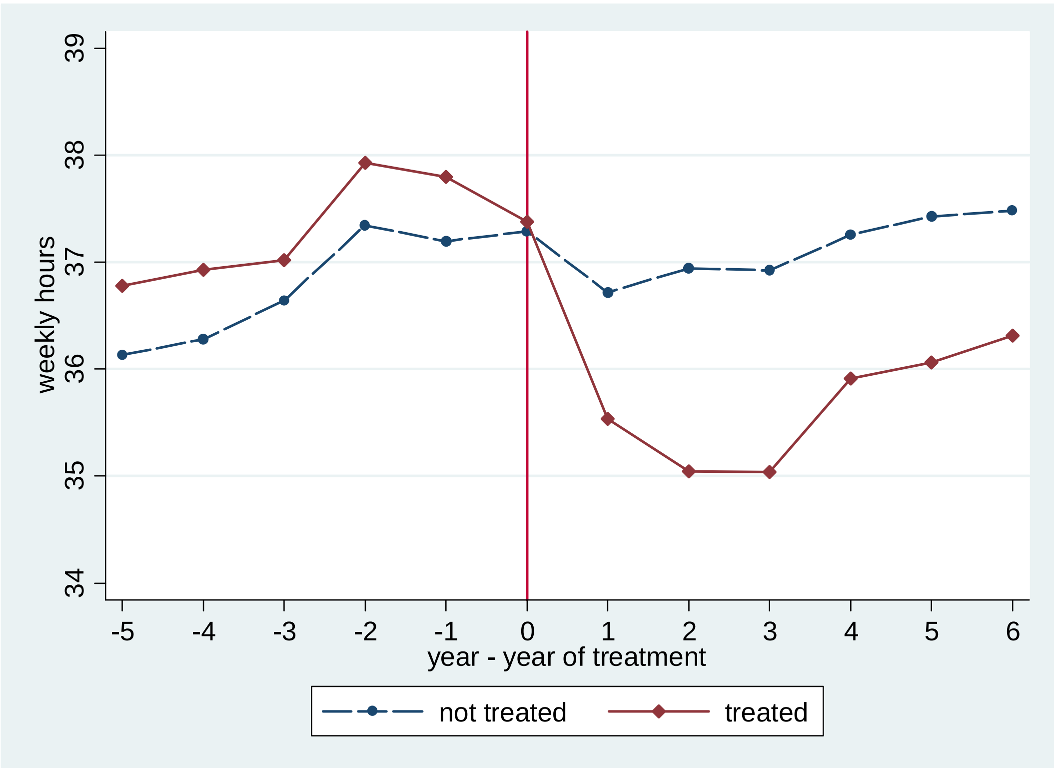


Figure 5. Husbands' Hours Worked, by Own Treatment.

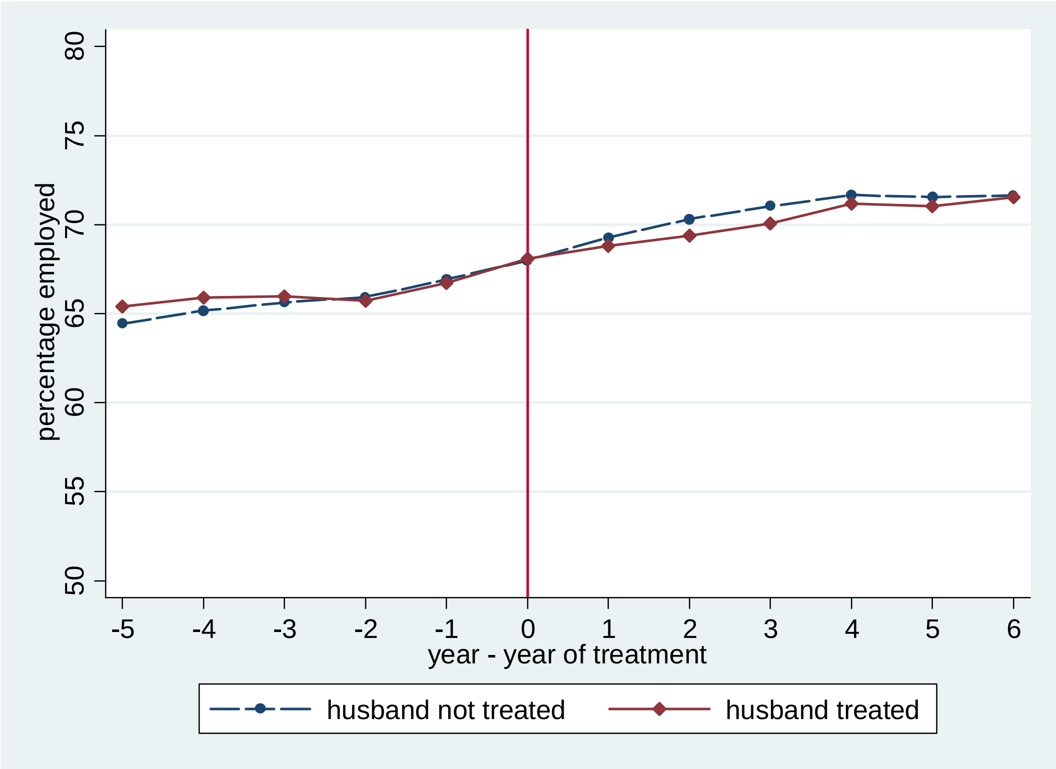


Figure 6. Women's Employment Rates, by Husband’s Treatment.

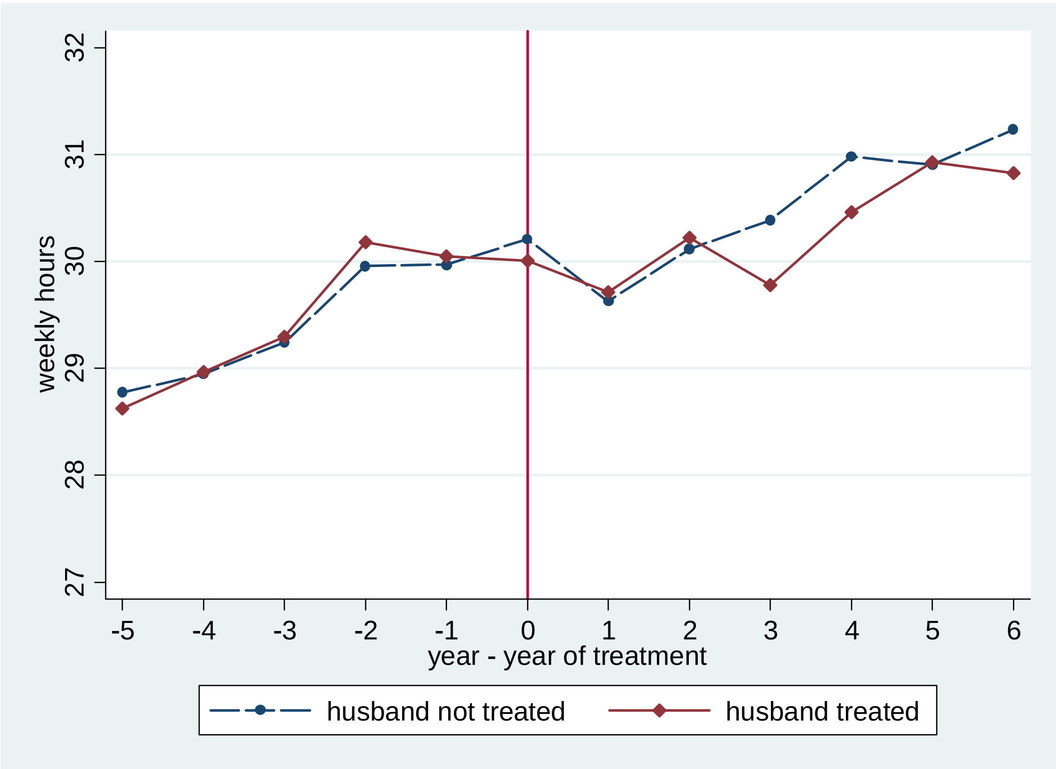
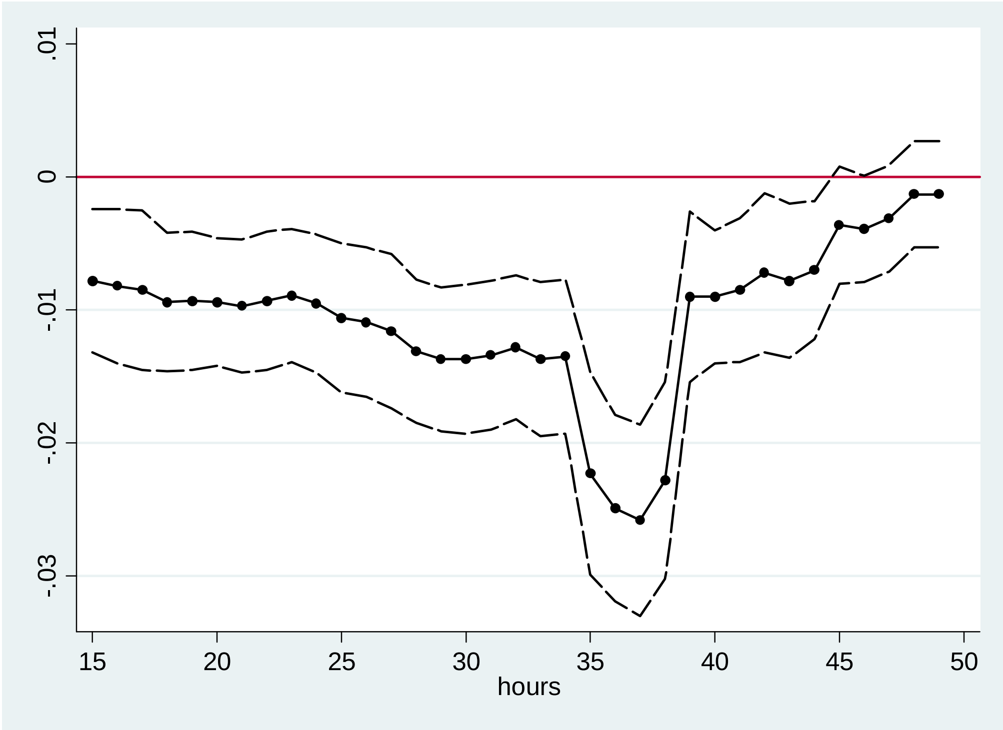


Figure 7. Women's Hours Worked, by Husband’s Treatment.

Panel A: Men



Panel B: Women

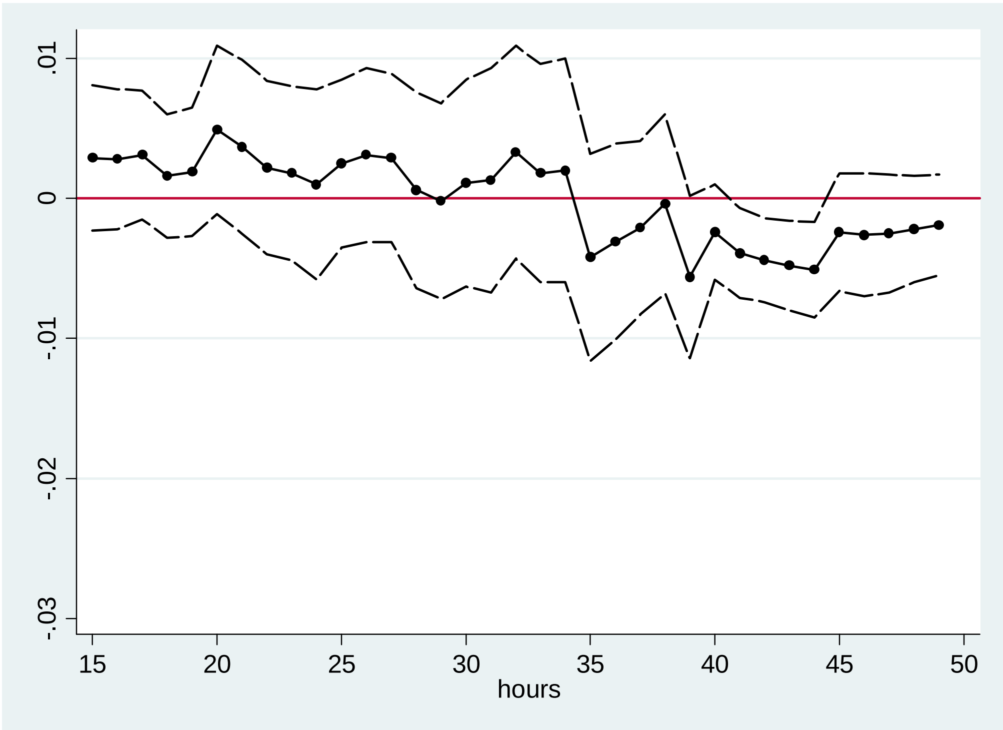


Figure 8. Estimated Cross-effects on the Cumulative Distribution of Hours

Notes: For each k between 15 and 49, the solid lines show the cross effect on the probability of working longer than k, i.e. $Pr(H > k)$. Dashed lines show the corresponding 95% confidence intervals.

Worktime regulations and Spousal Labor Supply

Dominique Goux, Eric Maurin, Barbara Petrongolo

Online Appendix

A. Descriptive Statistics

Table A1 provides some basic descriptive statistics on our sample, distinguishing between male and female respondents, and by the treatment status of their spouses. The age and years of education of both men and women are very similar whether or not their spouses are treated, although they are more likely to work in the private sector when their spouses are treated, consistent with stronger impact of the reform in the private sector and some degree of assortative mating.

Figures A1 and A2 show the distribution of actual and usual working hours, respectively, in the pre-policy period, i.e. for workers whose employers have not yet signed an agreement. Clear spikes in correspondence of 39 hours can be detected for both men and women in the pre-policy period, and as one would expect spikes are more marked in the distribution of usual than actual hours. Reassuringly, there is no evidence of "early" spikes in correspondence of $H_U = 35$ in the pre-treatment hours distribution of later treated firms. In fact, spikes at $H_U = 35$ appear (and spikes at $H_U = 39$ disappear) exactly upon treatment. For example, among firms treated in 2001, the density at $H_U = 35$ remains stable below 6% until 2001 and jumps above 47% in 2002 (and the density at $H_U = 39$ remains stable at about 50% until 2001 and falls to 12% in 2002)

B. Further Evidence on Cross-effects on Actual Hours

Figure B1 represents differences in hours for treated and non-treated employees by distance from treatment (i.e. the difference version of Figures 2 and 5), together with the corresponding 95% confidence interval, having normalized to zero such difference at time zero. The Figure highlights flat pre-treatment differences, followed by a permanent two hour drop in correspondence of treatment. Reduced-form effects on spouses are shown in Figure B2: while for men one can detect a permanent drop in hours worked, induced by wives' treatment, for women the difference in hours stays essentially flat, with no discernible change upon treatment.

As the observed cross-effects might be partly induced by cases of simultaneous treatment of spouses, Figures B3 and B4 complement evidence presented in Figure 4 in the paper by showing men's hours worked by wife's treatment status, excluding men treated at the same time as their wives, and men ever treated, respectively. These figures show a very similar pattern as Figure 4, i.e. upon their wife's treatment men on average cut their labor supply relative to men whose wives are not treated, and this result holds whether or not one includes men who are themselves treated. To the extent that treatments of spouses are correlated over time, the evidence presented in Figures B3 and B4 should alleviate concerns about our identification strategy.

Finally, we show in Figure B5 that adjusting the series for hours differences for all observables included in specification 4 of Table 3 leaves the main picture virtually unchanged from Figure B2.

C. Usual hours, Non-usual Hours and Earnings.

In our sample usual hours H_u are defined for about 85% of cases. For these, $H = H_u$ in 73% of cases, $H > H_u$ in 11.6% of cases, and $H < H_u$ in the remaining 15.4% of cases. Conditional on $H < H_u$, 57% of cases correspond to $H = 0$, and among them the average number of undertime hours is 38, and 43% of cases correspond to $0 < H < H_u$, and among them the average number of undertime hours is 10. Conditional on $H > H_u$, the average number of overtime hours is 7.4.

We have shown in Section III.B that cross-hour effects for men mostly happen through variations in $H - H_u$ rather than in H_u, and specifically through an increase in undertime hours $(H - H_u)^-$. For women, we detected a milder but positive cross-effect on H_u, associated to a rise in earnings. Here we relate our findings on cross-effects on hours and earnings to evidence from the decomposition of total earnings into a component explained by usual hours and a component explained by non-usual hours. Table C1 reports estimates from regressions of monthly earnings on H_u, $(H - H_u)^+$ and $(H - H_u)^-$ separately, and shows that earnings significantly respond to usual hours H_u for both men and women, while undertime hours $(H - H_u)^-$ have no discernible impact on male earnings. In other words, $(H - H_u)^-$ turns out to be the sole component of labor supply that men may cut without bearing losses in earnings, while increments in H_u do generate earnings gains. This evidence is in line with our estimated cross-effects on earnings.

D. Cross-hour Effects in the Public and the Private Sector

We provide further evidence on heterogeneous effects by showing in Table D1 separate results for the public and the private sector. Estimates reported in Panel A imply a cross-effect for males in the public sector of 36 minutes (column 1), while the corresponding figure for men in the private sector is only 15 minutes (column 2). Interestingly, when one selects private employees with open-ended contracts and tenure longer than two years,[1] the estimated labor supply response rises to about 22 minutes (column 3). In line with our main estimates of Table 3, Panel B shows lack of cross-effect for women in either the public or the private sector.

E. Robustness Tests

E.1 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, one should worry about the existence of differential pre-existing trends in working hours between treatment and control groups, and about the impact of local shocks, which would affect spouses in a similar way. To address these concerns, we estimate first-stage and reduced-form specifications that control for treatment-specific trends and region*year interactions. The results are reported in Table E1 and show a first-stage effect of the workweek reduction that is virtually identical to that reported in Table 2. The corresponding reduced-form effect is very similar to that reported in Table 3, albeit slightly less precise, but still significant at the 5% level.[2]

Second, our identifying assumption would be violated if spouses of employees in firms adopting the shorter workweek were subject to systematically different changes in unobservables around time of adopting, versus spouses of employees in non- adopting firms.

[1] Within two years of tenure there are no mandated severance payments and the advance notice for dismissal is one month instead of two.

[2] 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.

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 as much from an individual employee's point of view, and 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.

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 E2 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).

Third, we take into account concerns of reverse causality, and namely the possibility that changes in own labor supply behavior may affect spousal job mobility between signing and non-signing firms. 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 3.

Finally, one may worry that in general employees in adopting (or early-adopting) firms would have systematically different spouses from employees in nonadopting (or late-adopting) 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,[3] about 10% of respondents surveyed are observed both before and after the implementation of the shorter workweek in their spouses' firms (see Table E3).

[3]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.

Table E4 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 2. 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 workweek. Figures 2 and 5 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 undertime 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.

E.2 Alternative Sources of Identification

The whole analysis of our paper uses two sources of identification for estimating cross-hour effects of the shorter workweek, and namely variation in hours between treated and nontreated spouses, as well as variation across the early and the late treated. In principle the two sources of variation should trigger the same type of labor supply responses and one may worry in case our main results were driven by one type of variation but not the other. In order to check the robustness of our estimates, this section replicates our main specifications using these two sources of identification separately. The first-stage regression is based the following specification,

$$H_{it}^{S} = \alpha_{11} A_{it}^{S} + \alpha_{12} A_{it}^{S} * (1998 \leq t \leq 2002) + \alpha_{21} A_{it}^{S} * (t > 2002) + \\ + \alpha_{22} APost_{it}^{S} * (1998 \leq t \leq 2002) + \alpha_{3} X_{it}^{S} + D_t + u_{it}. \quad \text{(E1)}$$

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. By contrast, the α_{22} coefficient compares hours worked by those treated later to hours worked by those treated earlier.[4] The corresponding reduced-form equation is

$$H_{it} = \gamma_{11}A_{it}^S + \gamma_{12}A_{it}^S * (1998 \leq t \leq 2002) + \gamma_{21}A_{it}^S * (t > 2002)$$
$$+\gamma_{22}APost_{it}^S * (1998 \leq t \leq 2002) + \gamma_3 A_{it} + \gamma_4 APost_{it} + \gamma_5 X_{it} + D_t + \varepsilon_{it}, \quad \text{(E2)}$$

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

Columns 1 and 2 in Table E5 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 our main specification (see Table 2). 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 3.

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).

F. Simple Interpretative Models

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

$$U(l, M, H^S, C) = U(l, 1 - l - L, H^S, C), \quad \text{(F1)}$$

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

[4] Note that $APost_{it}^S = APost_{it}^S * (1998 \leq t \leq 2002) + A_{it}^S * (t > 2002)$, so that specifications (1) and (E1) are nested. In particular, specification (1) is a special case of (E1), in which one imposes $\alpha_{11} = \alpha_{12}$ and $\alpha_{21} = \alpha_{22}$.

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:

$$\frac{\partial H^*}{\partial L^S} = \frac{\partial M^*}{\partial L^S} = \frac{1}{u^2}\frac{\partial(U_1 - U_2)}{\partial H^S}\frac{\partial H^{S*}}{\partial L^S} = \frac{U_{23} - U_{13}}{u^2}\frac{\partial H^{S*}}{\partial L^S}, \tag{F2}$$

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 $U^S_{23} - U^S_{13} = 0$, 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

$$aU(l, M, H^S, C) + bU^S(l^S, M^S, H, C) \qquad \text{(F3)}$$

where a and b are spouse-specific Pareto weights.[5] In the special case with $M^{S*} = 0$, cross-effects for husband are given by:

$$\frac{\partial H^*}{\partial L^S} = \frac{\partial M^*}{\partial L^S} = \frac{a(U_{23} - U_{13}) - bU_{13}^S}{au^2 - bU_{33}^S} \frac{\partial M^{S*}}{\partial L^S}, \qquad \text{(F4)}$$

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 ($U_{23} - U_{13} > 0$) 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.

---

[5] 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).

REFERENCES

Pollack, Robert. 1969. “Conditional Demand Functions and Consumption Theory.” *Quarterly Journal of Economics* 83: 60-78.

Table A1
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. 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* in the French classification of occupations).

Source: French LFS, 1994-2009, Insee.

Table C1
Usual Hours, Actual Hours and Monthly Earnings

	Monthly earnings			
	Men		Women	
	(1)	(2)	(3)	(4)
Usual hours (H_u)	6.63** (0.35)	6.65** (0.34)	5.38** (0.22)	5.39** (0.22)
Actual–usual hours $(H - H_u)$	0.26 (0.18)		-0.15 (0.11)	
Overtime hours $(H - H_u)^+$		2.61** (0.40)		1.62** (0.35)
Undertime hours $(H - H_u)^-$		-0.00 (0.19)		-0.27* (0.11)
Mean dep. variable	325.91	325.91	228.74	228.74
No. Observations	97,470	97,470	102,123	102,123

Notes. The sample includes employed persons for which usual hours are defined. All regressions include controls as column (4) in Table 3. 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.

Table D1
Cross-effects on Hours Worked, by Sector

Panel A	*Men*		
	Public sector (1)	*Private sector* (2)	*Private sector "protected contracts"* (3)
A^S	0.22 (0.28)	0.23* (0.10)	-0.18* (0.08)
$APost^S$	-0.60** (0.24)	-0.26* (0.12)	-0.37* (0.15)
A	1.12* (0.49)	-0.49** (0.10)	-0.60** (0.11)
$APost$	-2.14** (0.61)	-1.53** (0.17)	-1.56** (0.17)
Mean dep. variable	34.77	37.86	37.84
No. observations	33,170	113,834	90,194

Panel B	*Women*		
	Public sector (1)	*Private sector* (2)	*Private sector "protected contracts"* (3)
A^S	-0.30 (0.20)	-0.13 (0.09)	-0.32** (0.09)
$APost^S$	0.15 (0.22)	-0.03 (0.11)	0.17 (0.15)
A	0.05 (0.39)	1.34** (0.12)	1.03** (0.13)
$APost$	-1.47* (0.58)	-1.65** (0.16)	-1.61** (0.21)
Mean dep. variable	29.24	30.23	30.90
No. observations	49,321	105,331	75,156

Notes. Estimates refer to the employed subsample. Column 1 refers to employees in the public sector, column 2 to employees in the private sector, and column 3 to employees in the private sector who hold an open-ended contract, with tenure longer than 2 years. Control variables are as in column 4 of Table 3. 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 E1
Direct and Cross-effects of the Shorter Workweek:
Additional Controls for Treatment-specific Shocks and Local Shocks

	Men					
	First stage			Reduced form		
	Wife's hours			Own hours		
	(1)	(2)	(3)	(4)	(5)	(6)
A^S	0.73**	1.17**	0.90**	-0.28	-0.07	-0.24
	(0.17)	(0.13)	(0.18)	(0.15)	(0.09)	(0.15)
$APost^S$	-1.97**	-1.93**	-2.03**	-0.37*	-0.45**	-0.38*
	(0.16)	(0.13)	(0.16)	(0.18)	(0.10)	(0.18)
A	-	-	-	-0.09	-0.03	-0.03
				(0.12)	(0.13)	(0.13)
$APost$	-	-	-	-1.96**	-1.98**	-1.98**
				(0.13)	(0.13)	(0.13)
A^S * year	yes	no	yes	yes	no	yes
Regions * year dummies	no	yes	yes	no	yes	yes
Mean dep. variable	30.05	30.05	30.05	38.89	38.89	38.89
No. observations	189,894	189,894	189,894	167,460	167,460	167,460
	Women					
	First stage			Reduced form		
	Husband's hours			Own hours		
	(1)	(2)	(3)	(4)	(5)	(6)
A^S	-0.80**	-0.25**	-0.74**	-0.28**	-0.13*	-0.16
	(0.19)	(0.12)	(0.19)	(0.08)	(0.06)	(0.10)
$APost^S$	-1.83**	-1.95**	-1.86**	0.12	0.03	0.10
	(0.20)	(0.14)	(0.19)	(0.15)	(0.11)	(0.18)
A	-	-	-	1.22**	1.33**	1.33**
				(0.11)	(0.10)	(0.10)
$APost$	-	-	-	-1.88**	-1.94**	-1.94**
				(0.15)	(0.15)	(0.15)
A^S * year	yes	no	yes	yes	no	yes
Regions * year dummies	no	yes	yes	no	yes	yes
Mean dep. variable	37.07	37.07	37.07	30.32	30.32	30.32
No. observations	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 2 for first-stage regressions, and as in column 4 of Table 3 for reduced-form regressions. Specifications 2, 3, 5 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 E2
Falsification Tests on Further Outcomes

	Men			
	Years of Schooling	Age	Private sector	Manufacturing
	(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	0.009	0.000	-0.001
	(0024)	(0.040)	(0.002)	(0.003)
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)
Mean dep. variable	12.78	41.45	0.680	0.357
No. observations	167,460	167,460	167,460	167,460
	Women			
	Years of Schooling	Age	Private sector	Manufacturing
	(1)	(2)	(3)	(4)
A^S	-0.022	-0.044	-0.018**	-0.021**
	(0.012)	(0.024)	(0.002)	(0.002)
$APost^S$	0.021	0.079	-0.002	-0.001
	(0.018)	(0.039)	(0.003)	(0.003)
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)
Mean dep. variable	12.91	39.49	0.655	0.145
No. observations	160,689	160,689	160,689	160,689

Notes. The sample and specifications are the same as in column 4 of Table 3, 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 E3
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. The table refers to the 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 E4
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 $(H-H_U)^+$	Undertime 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)
Mean dep. var.	0.891	36.88	9.033	38.79	-2.64	0.91	3.55
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 $(H-H_U)^+$	Undertime 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)
Mean dep. var.	0.686	29.60	8.596	33.05	-3.61	0.62	4.23
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 4-7 refer to the employed subsample for which usual hours are defined. Controls include individual fixed effects as well as the same baseline and additional control variables as in Table 3. 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.

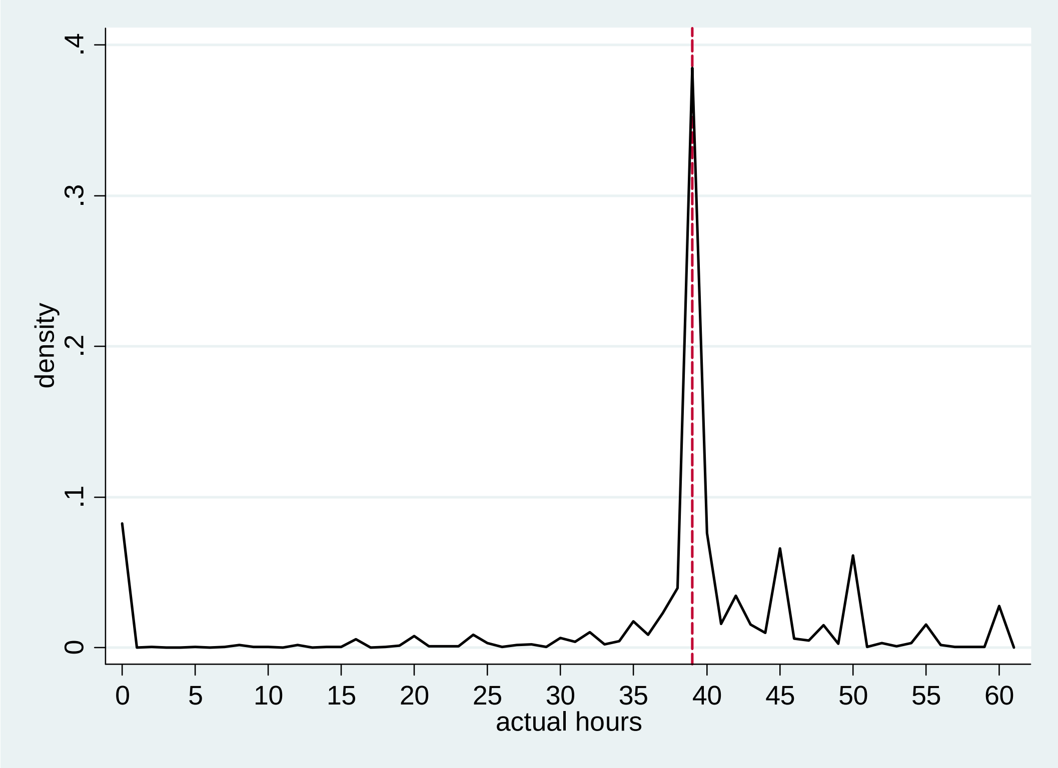
Table E5
Direct and Cross-effects of the Shorter Workweek:
Alternative Sources of Identification

	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)
Mean dep. variable	30.13	8.668	38.89
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 spouse's 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 2. In column 3, control variables are the same as in column 4 of Table 3. 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

Panel A: Men



Panel B: Women

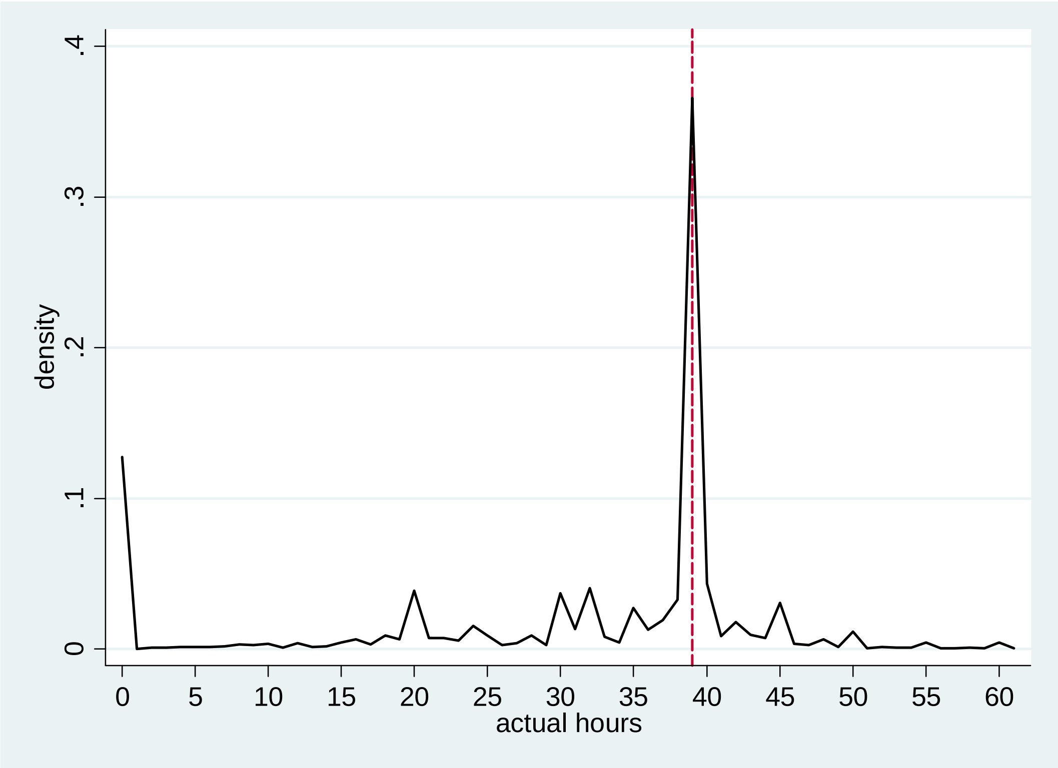
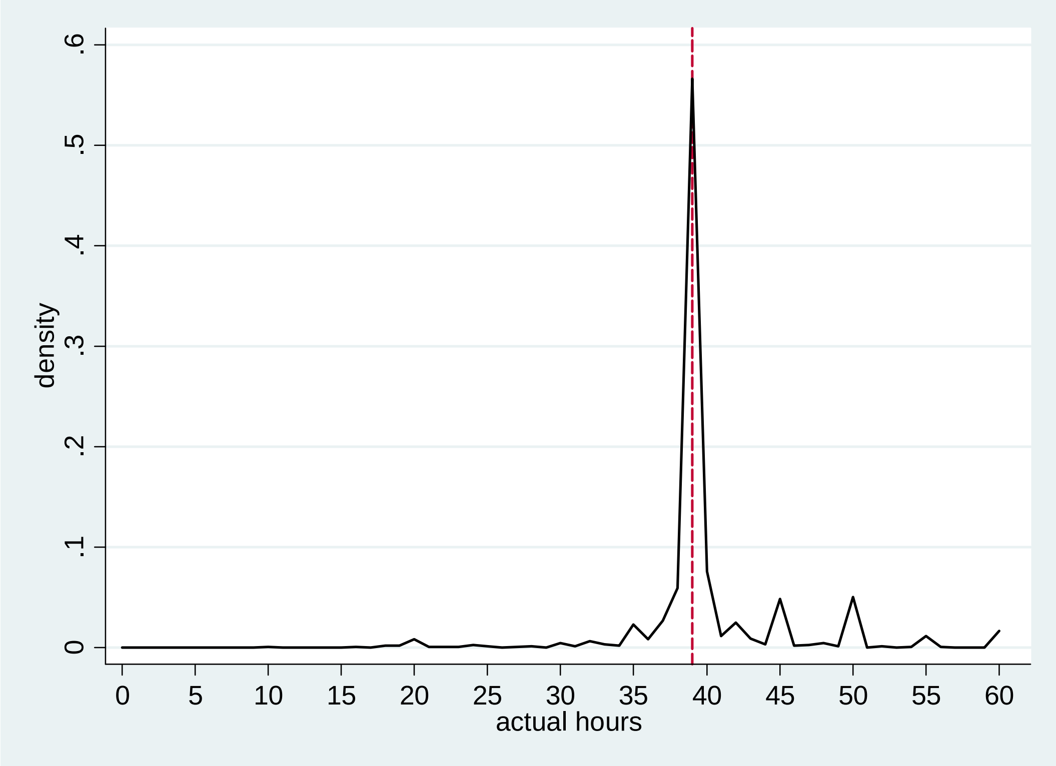


Figure A1. Pre-policy Distribution of Actual Hours

Notes. The distribution shown covers employees in firms that have not (yet) signed a workweek reduction agreement. The observed spikes are in correspondence of 39 hours.

Panel A: Men



Panel B: Women

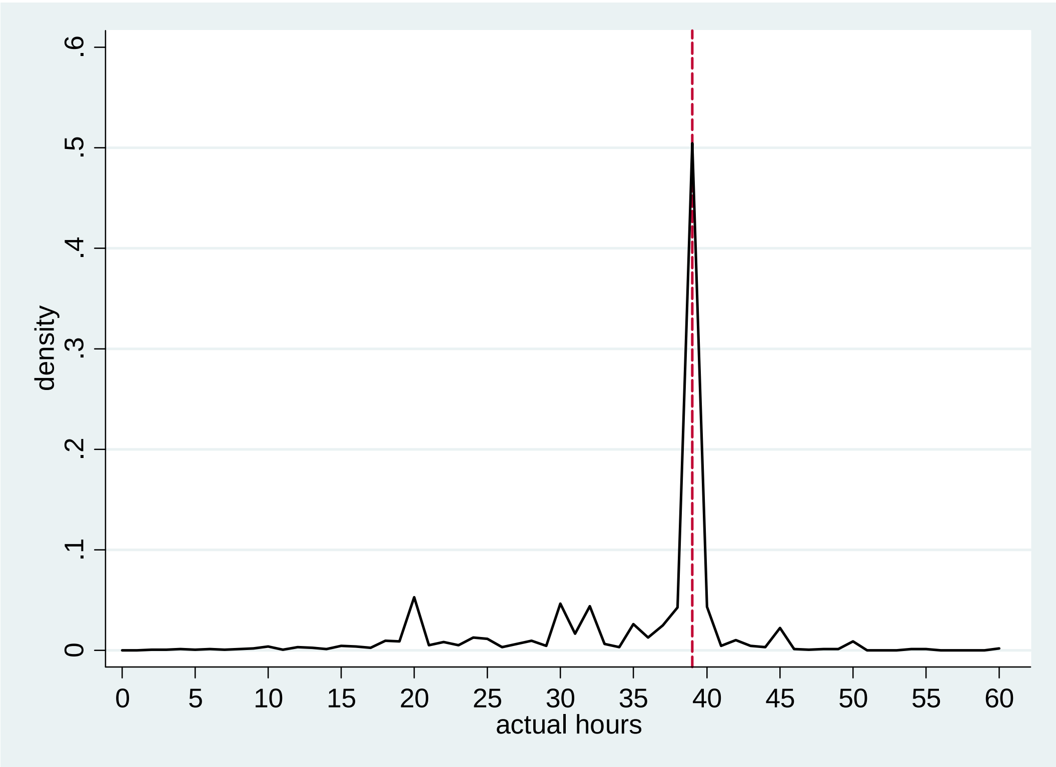
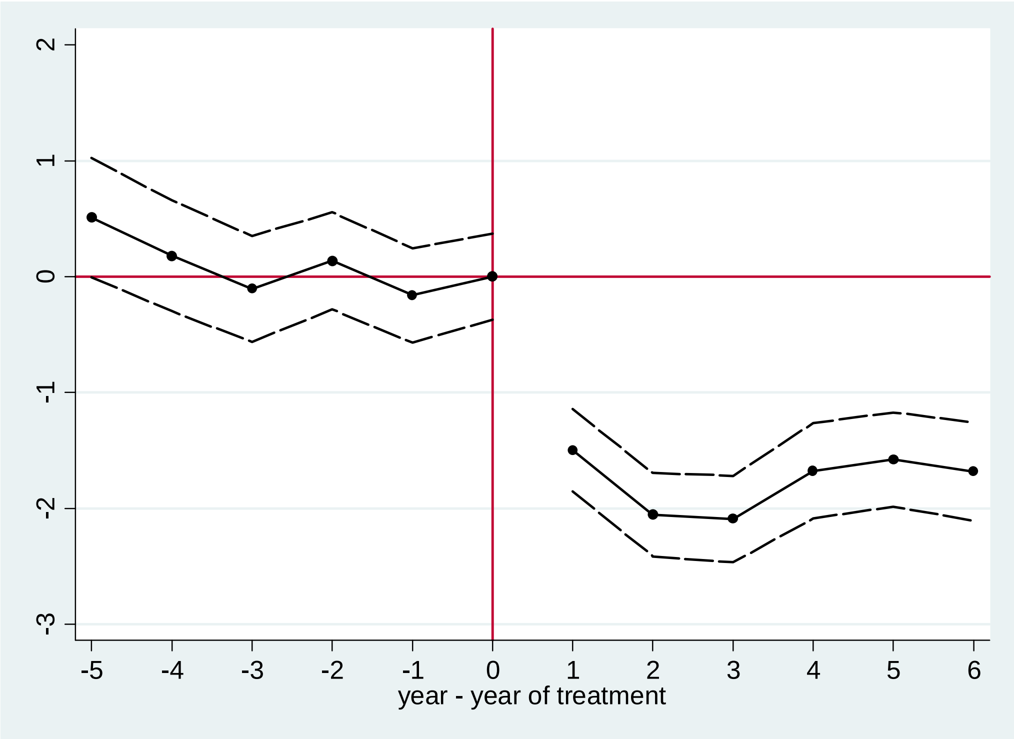


Figure A2. Pre-policy Distribution of Usual Hours

Notes. The distribution shown covers employees in firms that have not (yet) signed a workweek reduction agreement. The observed spikes are in correspondence of 39 hours.

Panel A: Wives



Panel B: Husbands

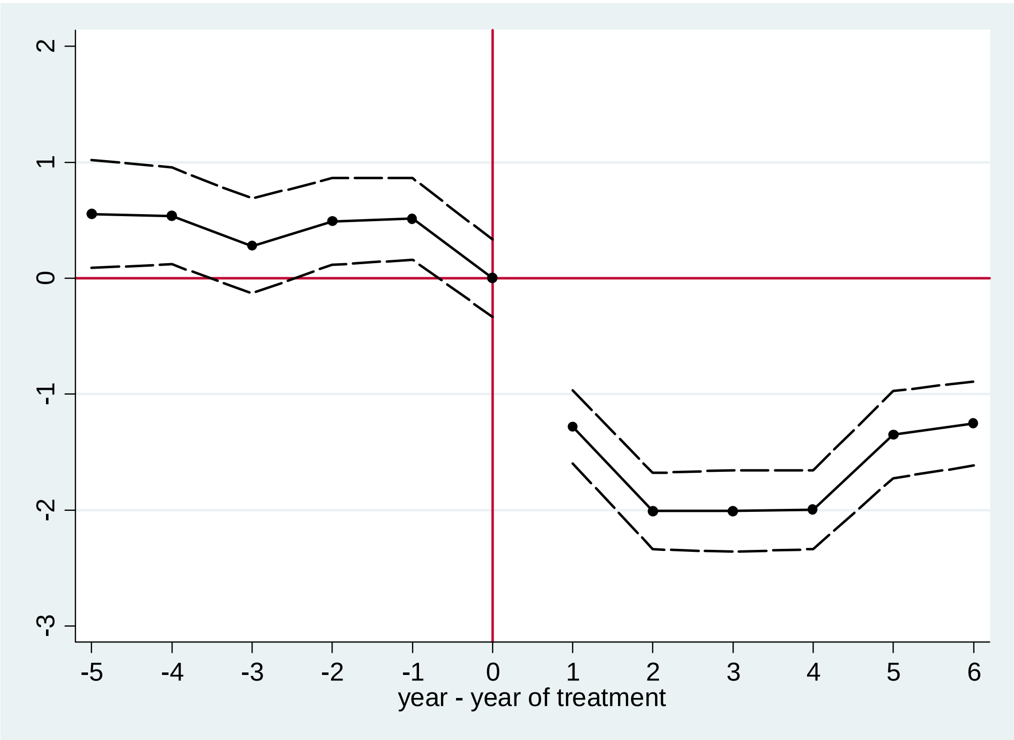
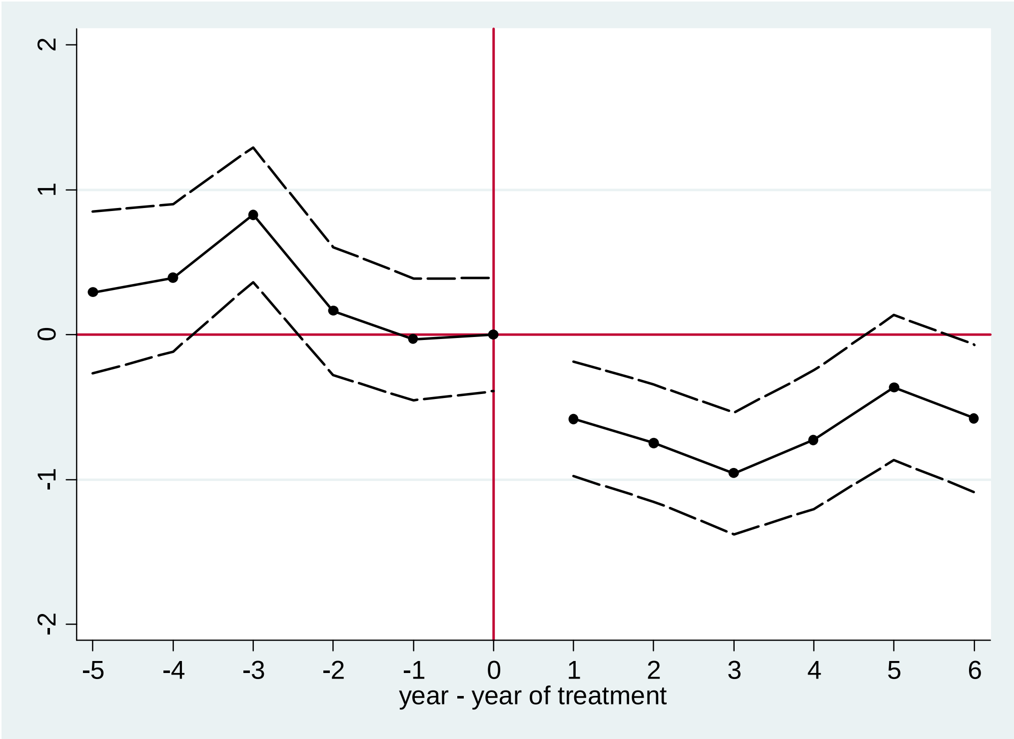


Figure B1. Differences in Hours Worked, by Own Treatment.

Notes. The solid line in Panel A represents the difference between the hours series plotted in Figure 2 for treated and nontreated wives, respectively. The solid line in Panel B represents the difference between the hours series plotted in Figure 5 for treated and nontreated husbands, respectively. All differences are normalized to zero in correspondence of time of treatment. The dashed lines show 95% confidence intervals.

Panel A: Men



Panel B: Women

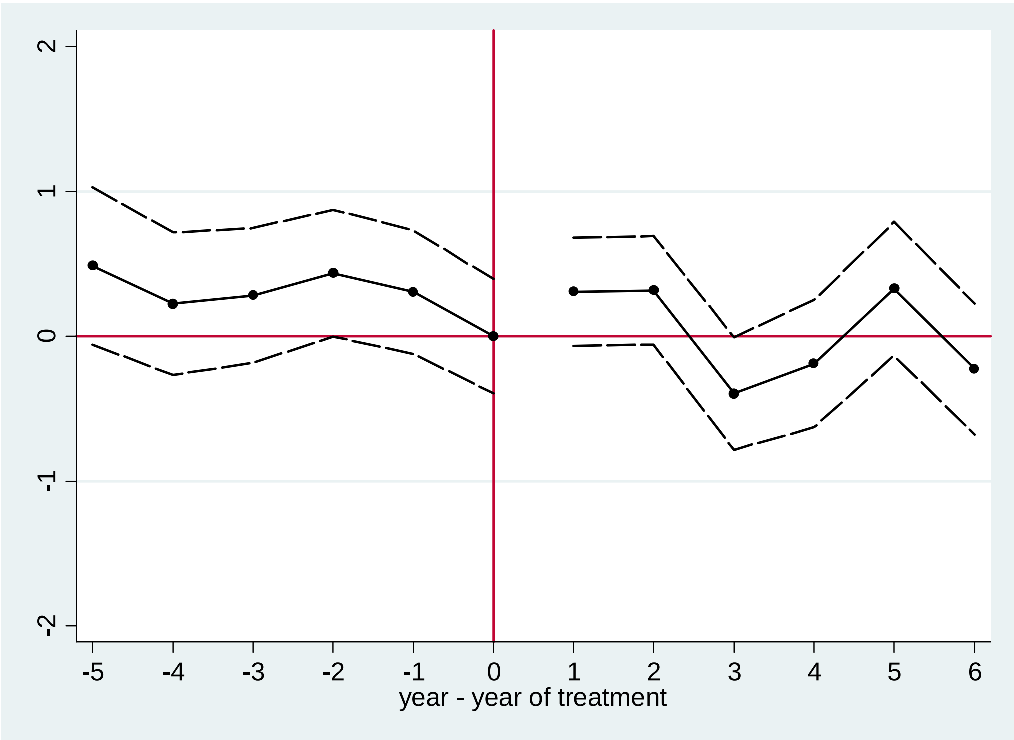


Figure B2. Differences in Hours Worked, by Spouse's Treatment.

Notes. The solid line in Panel A represents the difference between the hours series plotted in Figure 4 for husbands of treated and nontreated women, respectively. The solid line in Panel B represents the difference between the hours series plotted in Figure 7 for wives of treated and nontreated men, respectively. All differences are normalized to zero in correspondence of time of treatment. The dashed lines show 95% confidence intervals.

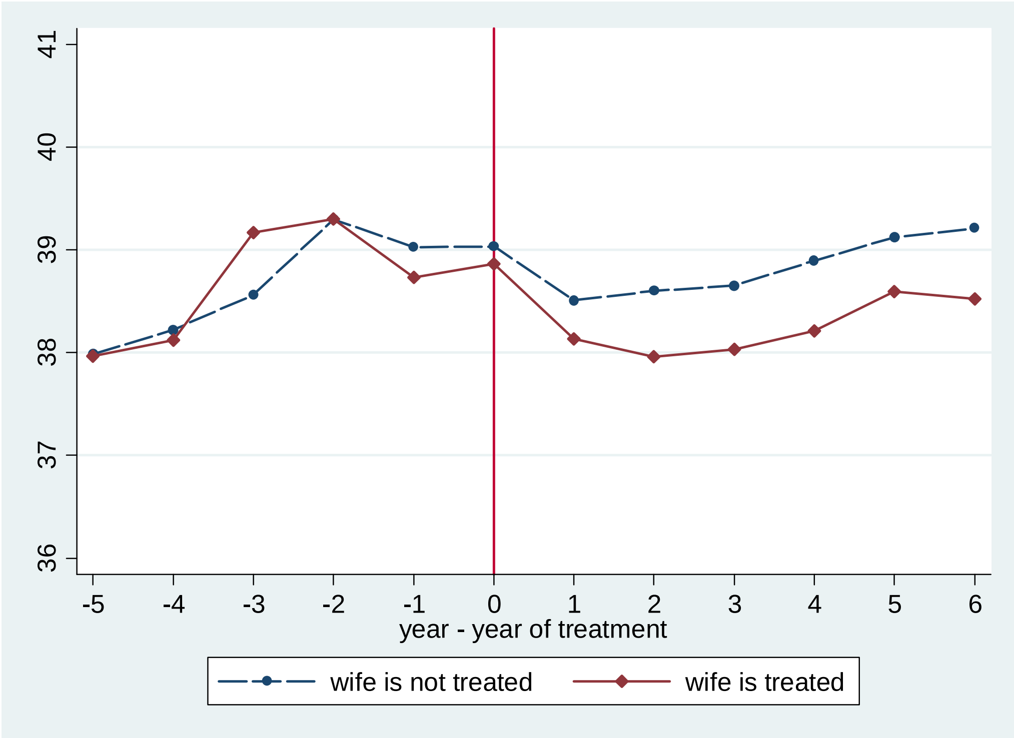


Figure B3. Men's Hours Worked, by Wife’s Treatment
Excluding Men Treated at the Same Date as their Spouses

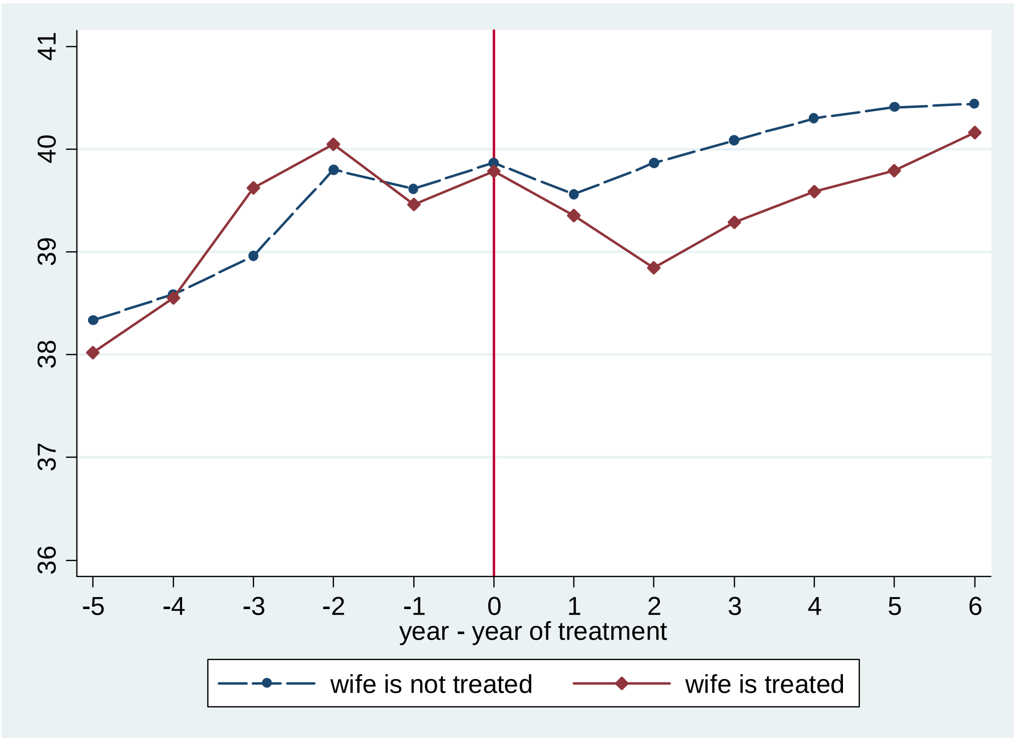
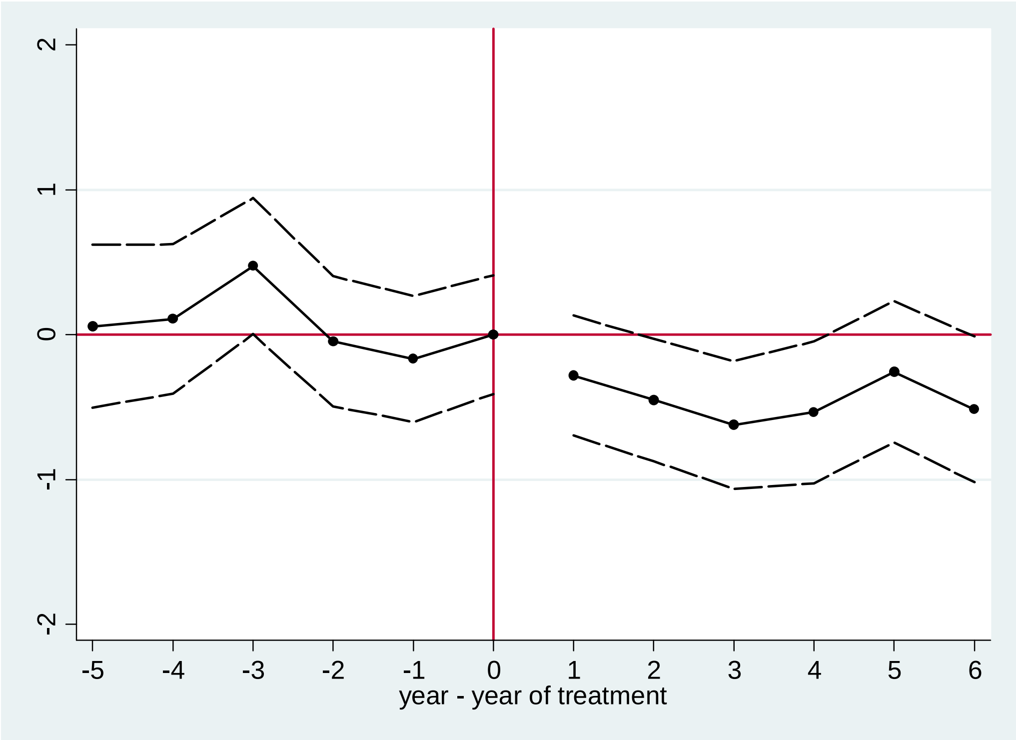


Figure B4. Men's Hours Worked, by Wife’s Treatment
Excluding Men Ever Treated

Panel A: Men



Panel B: Women

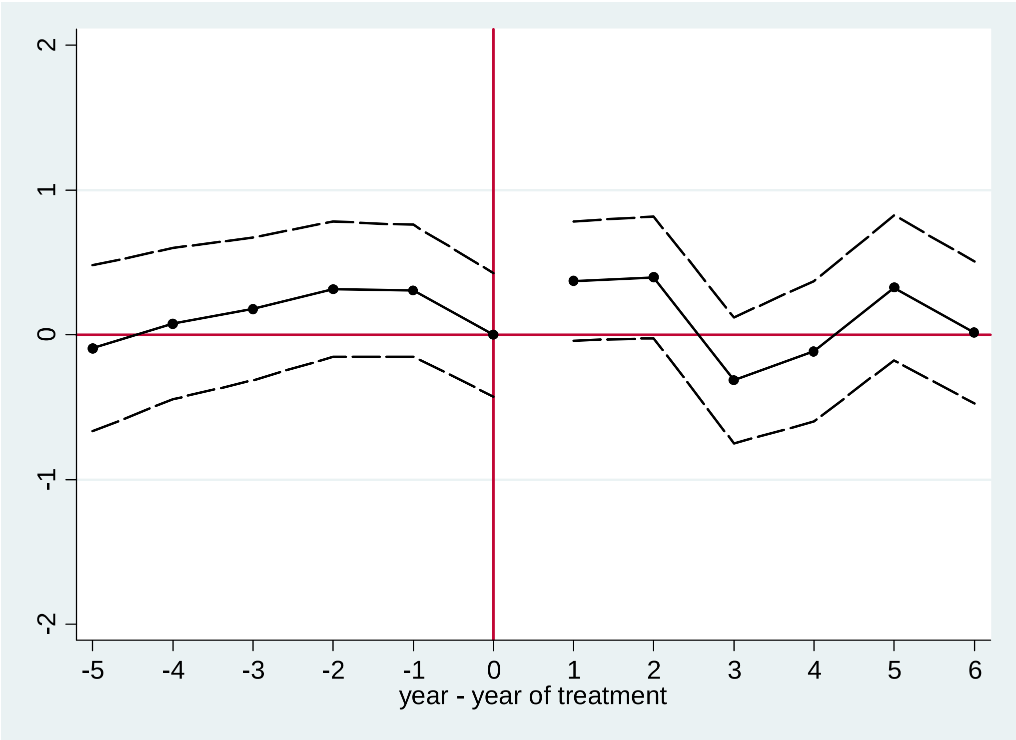


Figure B5. Differences in Hours Worked, by Spouse's Treatment
Controlling for Characteristics.

Notes. The solid line in Panel A represents the estimated difference in hours for husbands of treated and nontreated women, respectively. Estimates are obtained on a reduced-form specification that includes all controls as in column 4 of Table 3, having interacted treatment status with pre- and post-treatment year dummies. The solid line in Panel B represents the corresponding difference in hours for wives of treated and nontreated men, respectively. The dashed lines show 95% confidence intervals.