Worktime regulations and Spousal Labor Supply

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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:

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

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

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

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:

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

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	*Private sector*	*Private sector "protected contracts"*
	(1)	(2)	(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	*Private sector*	*Private sector "protected contracts"*
	(1)	(2)	(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.

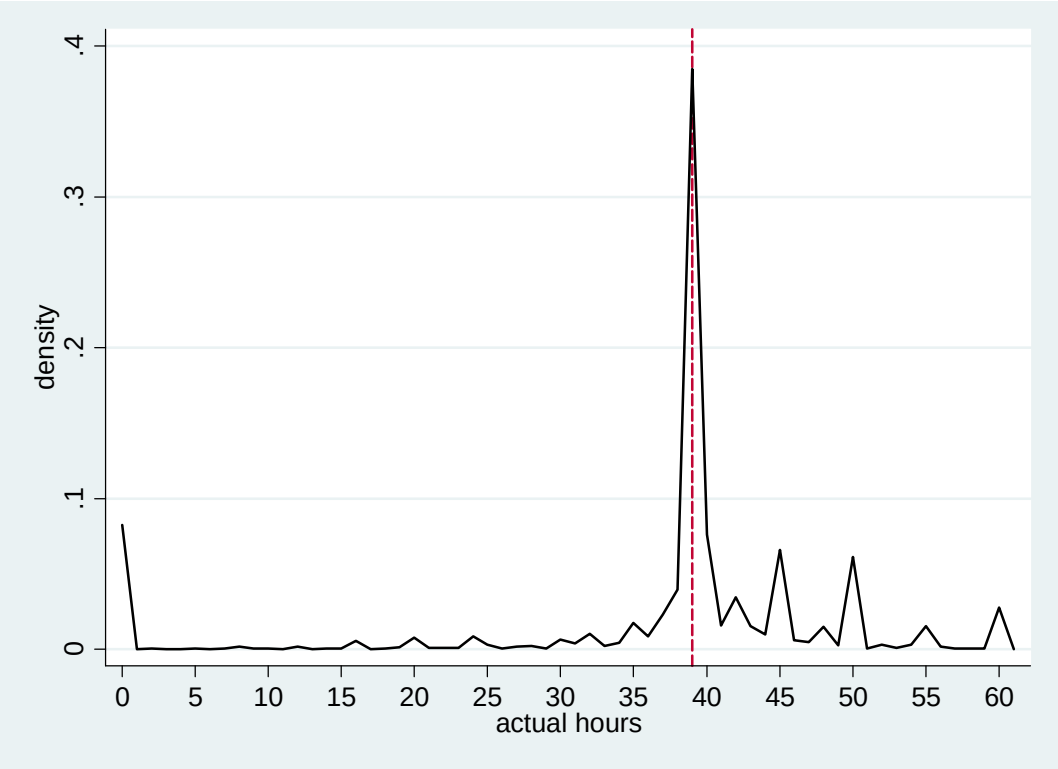
Table E5
Direct and Cross-effects of the Shorter Workweek:
Alternative Sources of Identification

	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

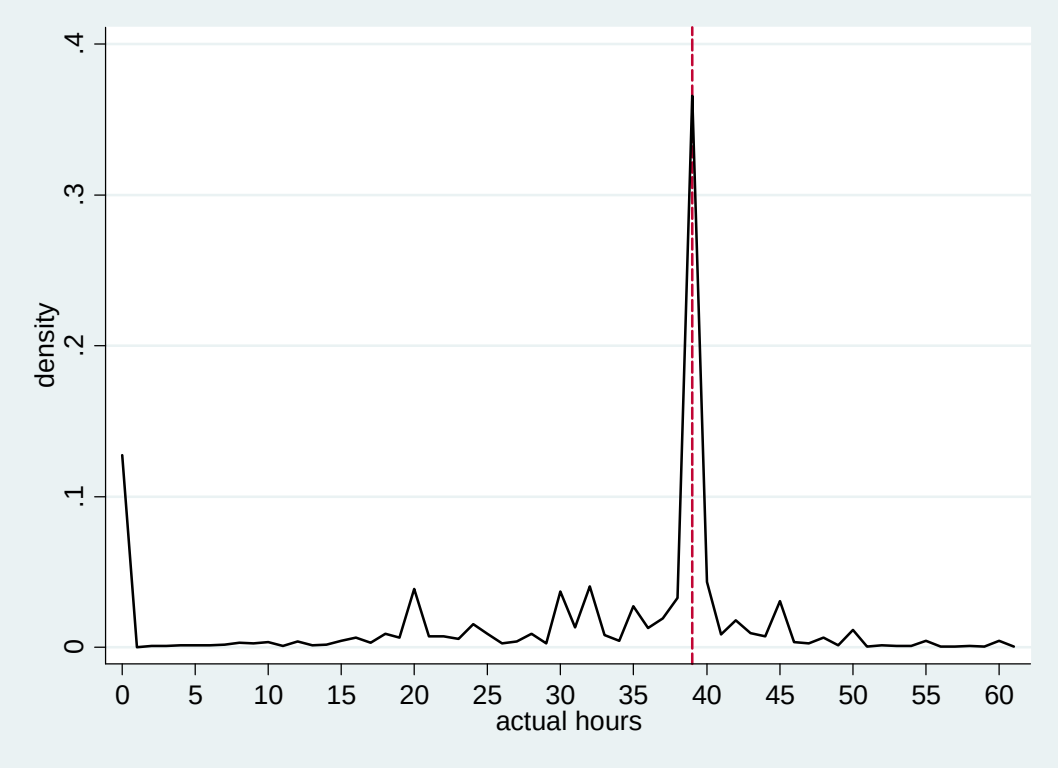
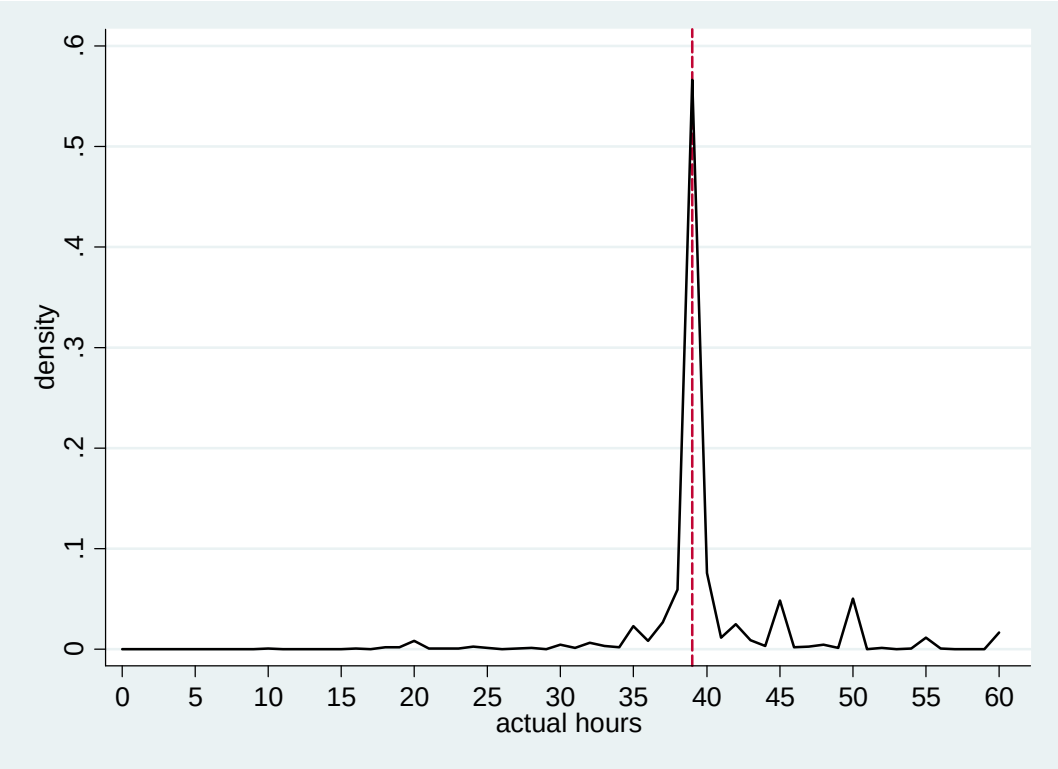


Figure A1. Pre-policy Distribution of Actual Hours

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

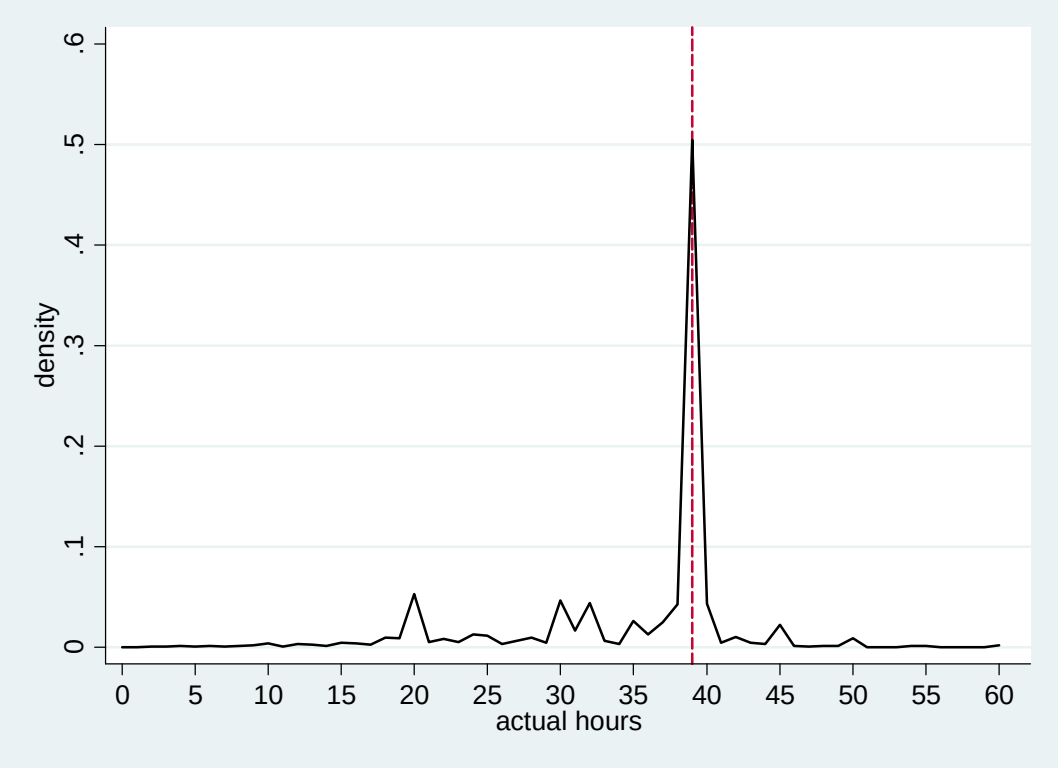
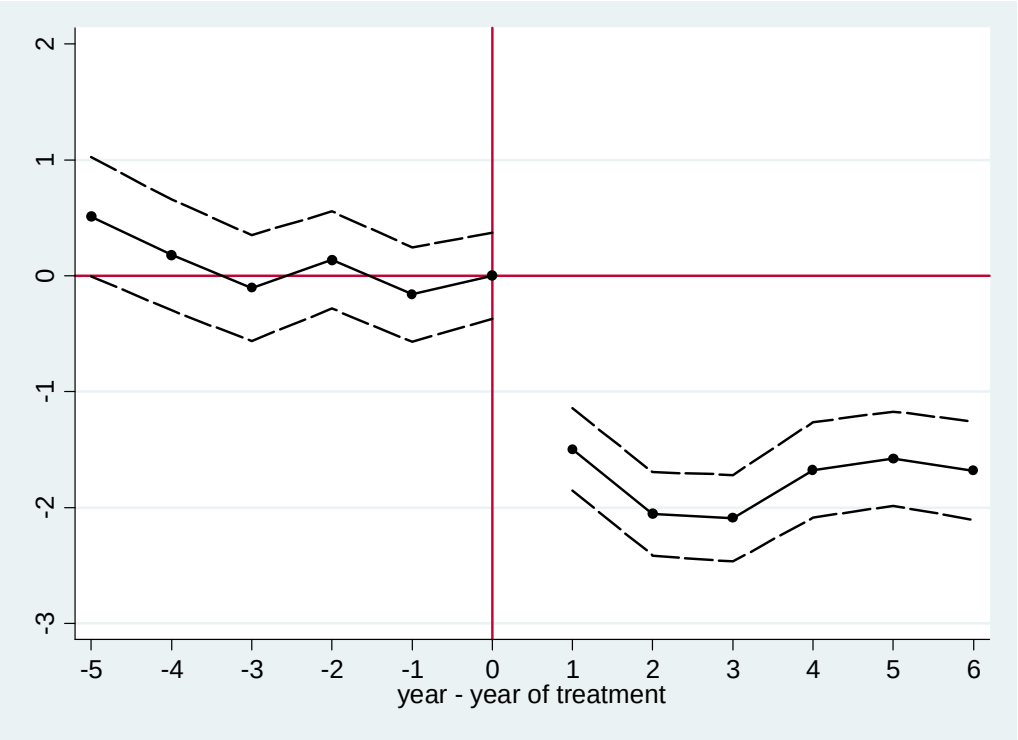


Figure A2. Pre-policy Distribution of Usual Hours

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

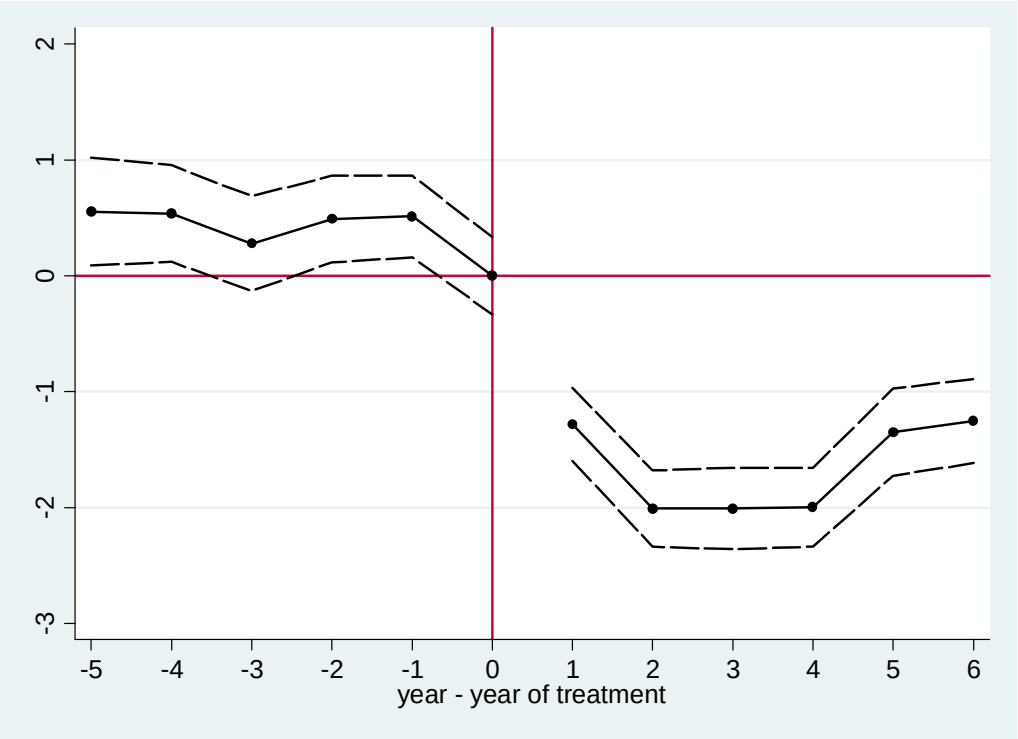
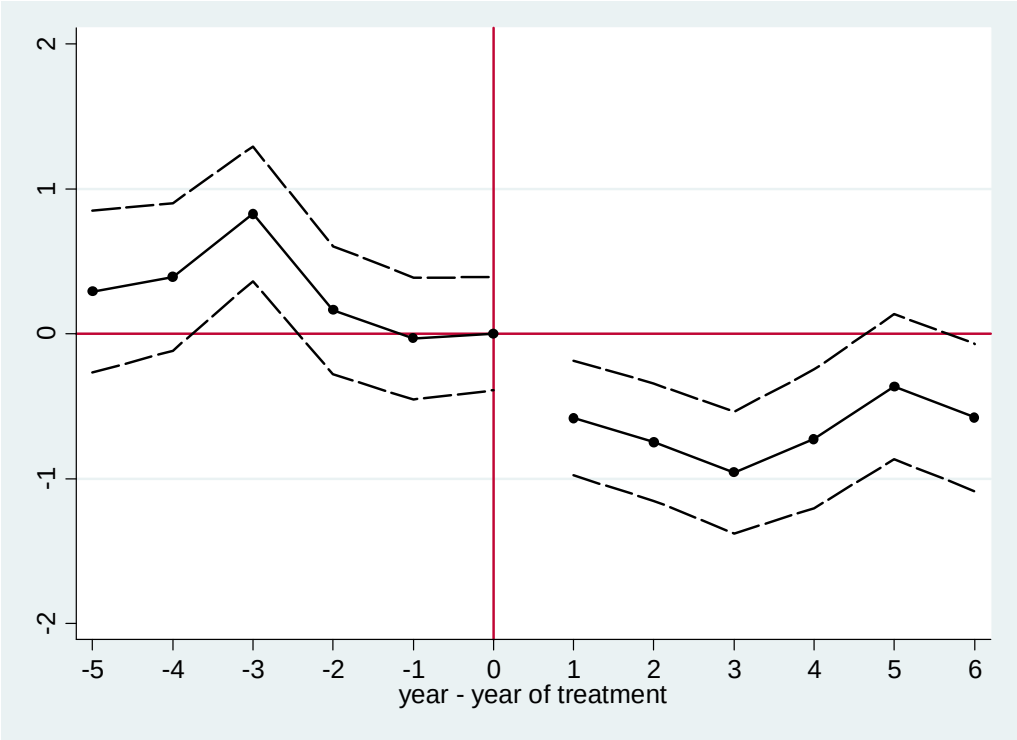


Figure B1. Differences in Hours Worked, by Own Treatment.

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

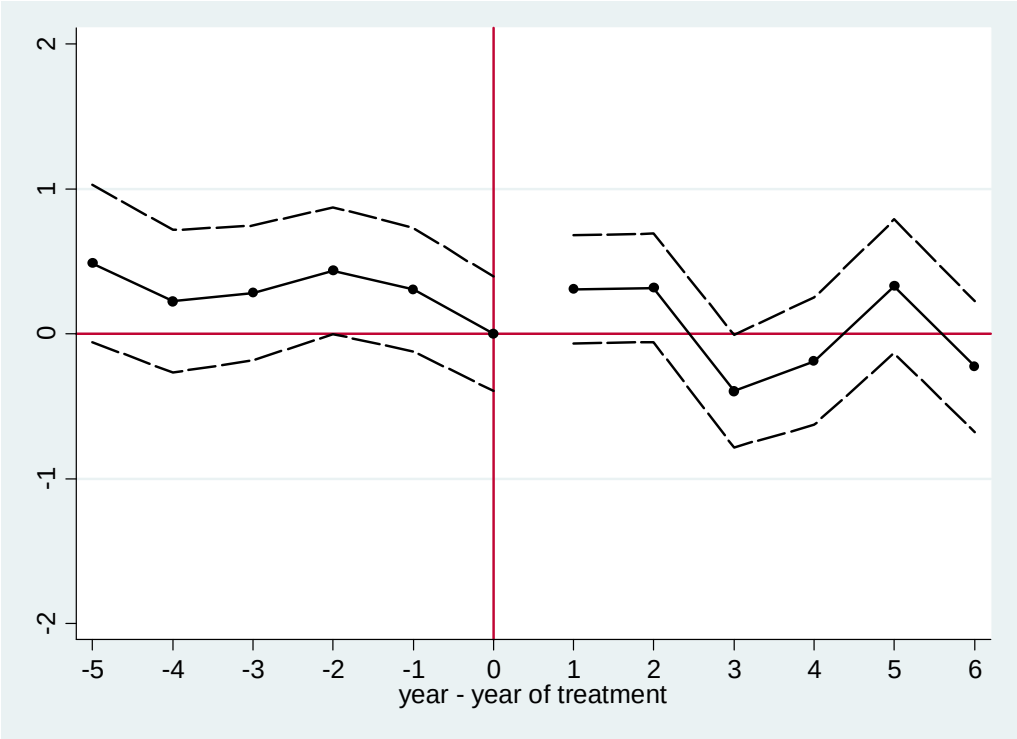


Figure B2. Differences in Hours Worked, by Spouse's Treatment.

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.

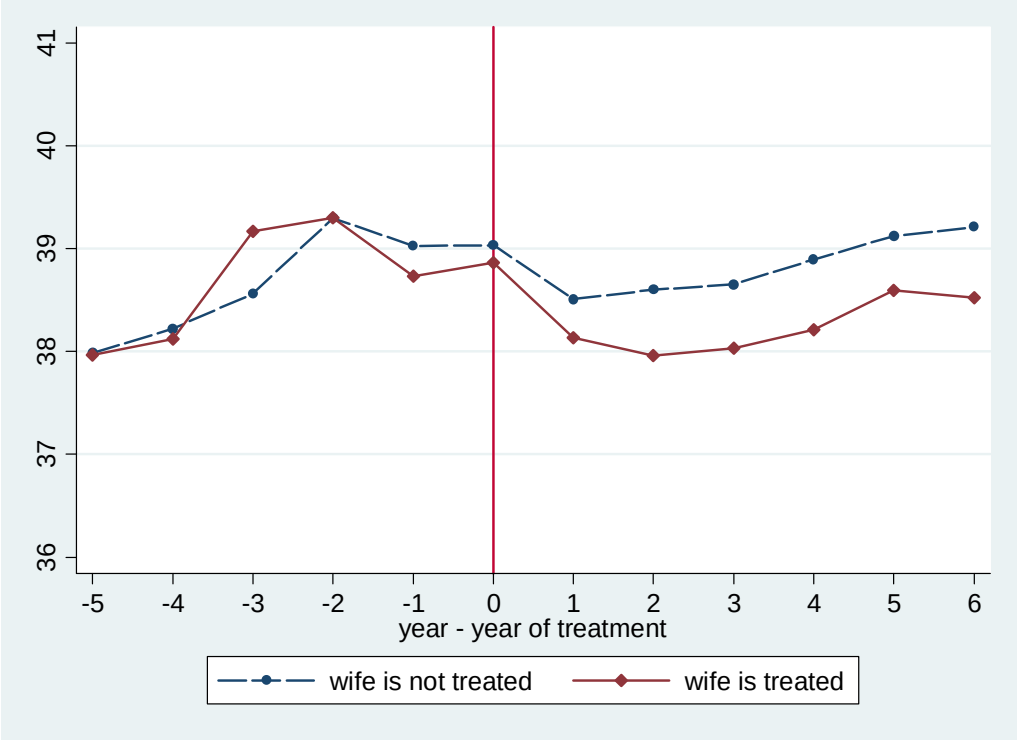


Figure B3. Men's Hours Worked, by Wife's Treatment
Excluding Men Treated at the Same Date as their Spouses

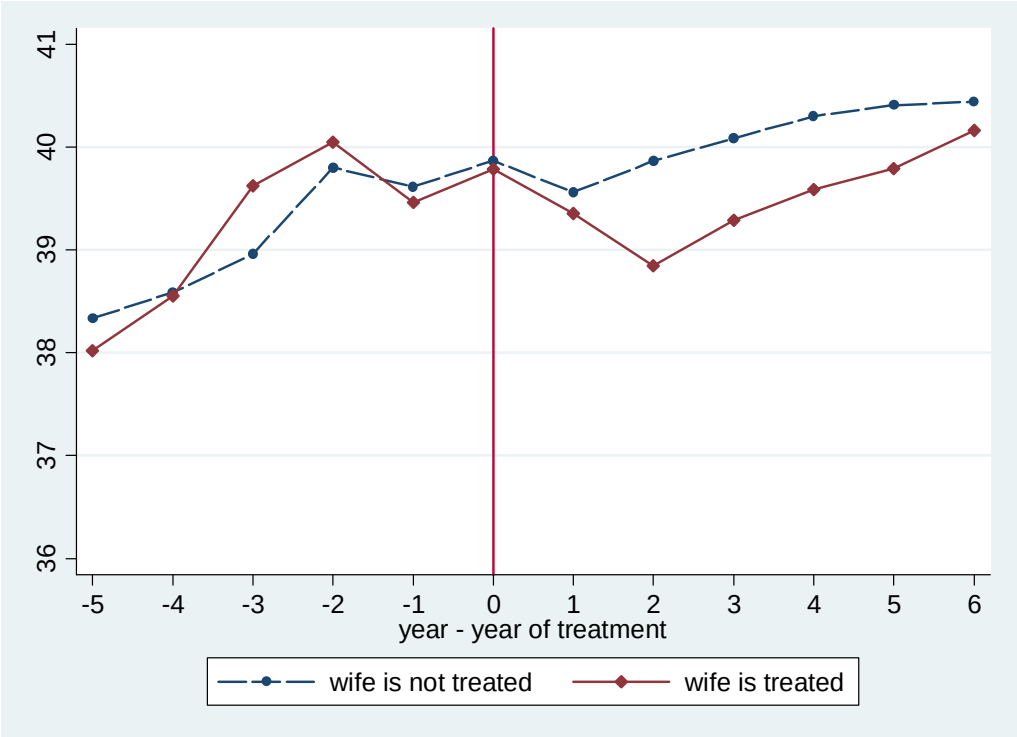
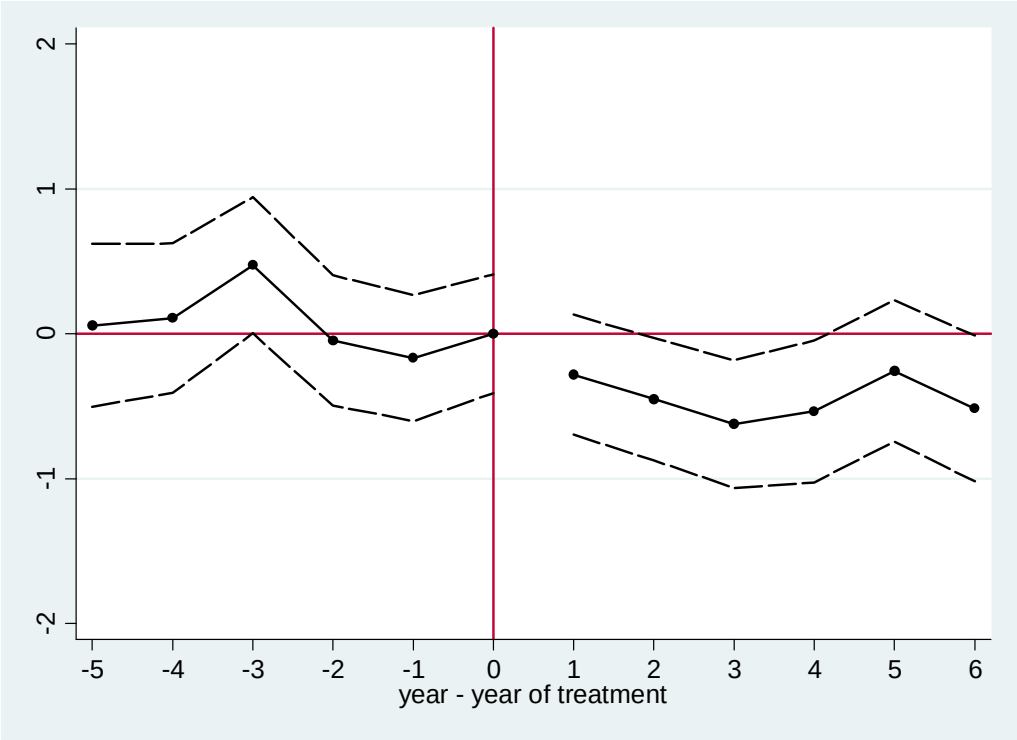


Figure B4. Men's Hours Worked, by Wife's Treatment
Excluding Men Ever Treated

Panel A: Men



Panel B: Women

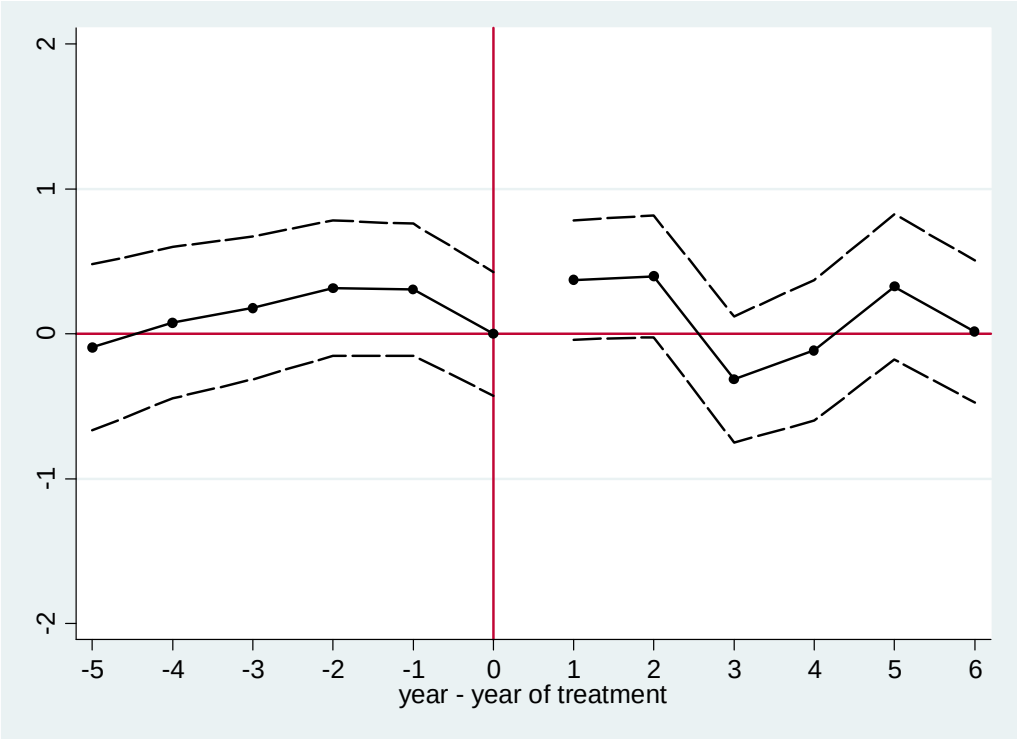


Figure B5. Differences in Hours Worked, by Spouse's Treatment Controlling for Characteristics.

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.