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Author(s): Bruno Crépon and Francis Kramarz

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Employed 40 Hours or Not Employed 39: Lessons from the 1982 Mandatory Reduction of the Workweek

Bruno Crépon

Centre de Recherche en Economique et Statistiques–Institut National de la Statistique et des Etudes Economiques

Francis Kramarz

Centre de Recherche en Economique et Statistiques–Institut National de la Statistique et des Etudes Economiques, Centre for Economic Policy Research, and Institute for the Study of Labor

We investigate the effects of the February 1, 1982, mandatory reduction of weekly working hours in France. Just after François Mitterrand's election in 1981, the minimum wage was increased by 5 percent. The workweek was then reduced from 40 to 39 hours. At the same time, stable monthly earnings for minimum-wage earners were mandated. We show that workers employed 40 hours and above in March 1981 were more likely to lose their jobs between 1981 and 1982 than workers employed 36–39 hours in March 1981. Moreover, many workers were still working 40 hours after February. These workers were also strongly affected by this reduction. Our estimates of the impact of this one-hour reduction on employment losses vary between 2 percent and 4 percent. Minimum-wage workers were most affected by the changes.

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I. Introduction

In 1998 the French government adopted a policy requiring firms to implement a 35-hour workweek by the year 2000.[1] Today, most French firms have negotiated a workweek reduction agreement with their unions or personnel delegates and have implemented it. Employer confederations claim that the reduction has had an adverse impact on their activity, in particular by creating short-term labor shortages in a booming period together with an increase in production costs. The representatives of small firms are pessimistic about the upcoming introduction of the mandatory reduction in 2002. On the other hand, this policy appears to be popular among unions and a fairly large part of the French working population. It is even considered successful by some politicians because of the sharp decrease in unemployment during the last three years. Some Italian and Spanish union leaders and politicians advocate the same set of policies that the French have adopted ("Italy: The Things Undone," 1998; "The Working Week: Fewer Hours, More Jobs," 1998). Germany's newly elected government also appears to support mandatory hours reductions, as shown in the famous Bavarian Motor Works agreement ("BMW's British Bruises," 1998; "Survey Germany: Could Be Worse," 1999).

Although the media are full of discussions of the effects of the hours-reduction laws, serious evaluation of the economic impact of the recent workweek reduction is difficult, even impossible, because the changes are too recent. Indeed, there are very few empirical assessments of the effects of this type of regulation, leaving the debate open to views based on political prejudices (see, however, Hunt [1999], who focuses mostly on the German case). This is ironic because France has already attempted to manipulate working hours in order to reduce unemployment. In 1982, a few months after François Mitterrand's election, the socialist government, as stated in its election platform, decided to shorten the workweek by cutting the maximum legal number of hours per week.[2] It is fair to say that the election of Mitterrand was not foreseen by most political analysts. At the beginning of 1981, the Paris correspondent of the *Economist* wrote: "For months French opinion polls have made President Giscard d'Estaing's reelection this spring seem a fore-

[1] The deadline was 2002 for firms with 20 employees or less.

[2] The legal workweek in France is determined by the maximum number of hours, as specified in national statutes and implemented in collective bargaining agreements. The firms are permitted to employ workers for more than the national statutory maximum hours under the following conditions: (1) overtime hours up to a statutory limit at a negotiated wage premium; (2) higher statutory limits in certain sectors, e.g., hotels and restaurants; and (3) exemptions for management and certain engineering positions (cadre) specified in the collective bargaining agreement. About 95 percent of the jobs in France are covered by collective bargaining agreements, even though a much smaller percentage of workers belong to unions (Card, Kramarz, and Lemieux 1999).

gone conclusion" ("An Ill Wind That Won't Necessarily Blow Giscard Out," 1981). Furthermore, the victory of the socialists at the parliamentary elections, which took place a few weeks after the presidential election, was an even longer shot in those first months of 1981. Therefore, even though the hours reduction was included in the socialists' platform, it was almost fully unexpected six months before the elections. The hours reduction took place at the beginning of 1982, in February, a few months after the 5 percent increase in the French minimum wage, the SMIC, of July 1981. In addition, the February 1982 decree stipulated the mandatory rigidity of monthly earnings for minimum-wage workers employed by the firm at that date.

In this article we investigate the effects of this reduction in the maximum workweek. We evaluate the effect of the workweek reduction on transitions from employment to nonemployment using two different approaches based on two natural experiments associated with the 1982 hours reduction. In the first one, we compare workers who worked 36–39 hours before 1982 with workers who worked exactly 40 hours and with those who worked overtime (up to 48 hours). In the second experiment, we take advantage of specific features of the implementation process of the reduction. As mentioned earlier, some firms were surprised by the February 1, 1982, decree. In April 1982, when the French Labor Force Survey took place, a sizable share of firms had not altered their hours to the new standard. To analyze these two issues, we use panel data from the French Labor Force Survey (Enquête Emploi) for the period 1977–87. Our results show that workers who were working exactly 40 hours per week in March 1981 as well as workers who were working overtime (41–48 hours per week) in March 1981 were less likely to be employed in 1982 than observationally identical workers who were working 36–39 hours per week in 1981. This first analysis uses differences-in-differences techniques by comparing transitions from 1981 to 1982, after implementation of the decree, with those prevailing between 1978 and 1981, before the election of Mitterrand. Our second analysis also demonstrates that workers still employed 40 hours in 1982 lost their jobs more often than those already employed under the new standard workweek. Indeed, all our results show that these job losses can be directly attributed to the reduction in the workweek. In our first analysis, the effects are significant and vary between 2.6 percent and 3.9 percent according to the technique considered. In our second analysis, the effects are also quite significant, and we estimate a lower bound for the induced additional job losses at 4.1 percent. Furthermore, we show that minimum-wage workers were much more affected than others. All such results are fully consistent with the predictions of most theoretical models of hours reduction, in particular since wage rigidity was binding for most low-wage workers, particularly after the 5 percent increase in the

TABLE 1
Hours Worked, 1976–81

Fraction of Employment Working:	1976	1977	1978	1979	1980	1981
36–39 hours	2.2	2.1	2.5	2.4	2.6	2.4
40 hours	46.6	53.6	55.6	58.6	60.9	65.9
41–43 hours	18.8	18.8	19.5	19.3	17.5	15.2
44 hours	4.2	4.4	3.9	2.3	2.7	2.0
45–48 hours	28.2	21.1	18.6	17.4	16.4	14.5
Observations	5,422	6,133	6,212	6,123	6,409	6,509

Source.—French Labor Force Survey, 1976–81.

minimum wage of July 1981. In particular, this wage rigidity should have generated simultaneous job destruction and creation. Given empirical relationships between employment destruction and worker flows (Abowd, Corbel, and Kramarz 1999), excess job destruction that is observed for low-wage workers, around 8 percent, corresponds to roughly 2 percent annual employment destruction, yielding an elasticity of employment to labor costs just below minus one, in the same ballpark as other estimates for this category (Abowd et al. 2000; Kramarz and Philippon 2001). In addition, our results show, also in conformity with the model, that better-compensated workers were less directly affected by the reduction of the workweek.

In Section II, we present the decree and the institutional context surrounding the 1982 reduction of the workweek. We briefly discuss the likely effects of this reduction in Section III. Section IV contains a description of the data sets that are used in the analysis. In Section V, we present our analysis of the direct natural experiment, and the analysis of the delayed reduction of hours is examined in Section VI. Finally, we present a conclusion in Section VII.

II. Institutional Context

A. *Principles and Legal Aspects of the 1982 Reduction of the Workweek*

1. Changes in Hours

The number of hours worked strongly decreased during the 1970s, from 48 hours in 1974 to just above 40 hours in 1981 (see table 1). During all this period, indeed since 1936, the standard workweek was 40 hours. Mitterrand's election in May 1981 induced a sudden decrease of the standard to 39 hours (January 16, 1982, ordinance). In fact, negotiations started just after May 1981, since the reduction was part of the Left's electoral platform. These negotiations should have ended before 1982. In a report to the president at the end of 1981, the prime minister mentioned that negotiations did not make real advances but nevertheless recommended letting firms' and workers' unions and delegates continue until the second quarter of 1982, the suggested date of application of the new standard. Against his prime minister's recommendations, Mitterrand imposed, by the January 16 ordinance, the new 39-hour standard, which took effect February 1, 1982. Collective agreements, specifying the terms of application of the decree, ensued, starting with the largest firms in the manufacturing industries and spreading to smaller firms and other industries (Marchand, Rault, and Turpin 1983).

Therefore, the law reducing the workweek became effective February 1, 1982. It mandated a maximum legal workweek of 39 hours, whereas it was 40 hours previously, and only slightly altered the prevailing regulation on overtime: the overtime premium remained 25 percent for the first four hours and 50 percent above, but the maximum of compensated hours was reduced from 50 to 48 per week (for more details, see Marchand et al. [1983]).

2. The Mandatory Nominal Wage Rigidity and Its Consequences

The government also recommended that monthly pay after the change in workweek remain unchanged for all workers, but no special arrangements were included in the law to enforce this recommendation *except* for workers paid the legal minimum wage (SMIC) and working 40 hours. For these workers, a special hourly minimum wage was prescribed in order to guarantee that their monthly earnings be unchanged after the change in hours.[3] Hence, a worker paid the SMIC and working 40 hours before February 1, 1982, received the same monthly earnings after February 1, even though the workweek was only 39 hours. However, any worker hired at the minimum-wage rate after February 1, 1982, received monthly pay corresponding to his or her exact number of hours. Therefore, newly hired workers were approximately 2.5 percent (100 francs a month or $20) cheaper than their more senior counterparts because of this special provision in the hours-reduction law. Furthermore, since a 5 percent increase in the hourly SMIC was one of the first decisions made by the newly elected government in mid 1981, the hourly cost of minimum-wage workers increased by 7.5 percent between mid 1981 and mid 1982. Finally, for all other categories of workers, the "recommendation" to leave monthly pay unchanged seems to have been followed by most firms. A survey conducted in September 1982 showed that more than 90 percent of all workers had their monthly pay unchanged after

[3] The minimum-wage legislation in France specifies an hourly wage.

implementation of the law reducing the length of the workweek (Marchand et al. 1983).[4]

III. Theory and Identification of the Impact of the Workweek Reduction

A. *Theory*

The theoretical consequences of changes in standard hours are now well known (see Rosen [1968], Ehrenberg [1971], and Calmfors and Hoel [1988], among many others). Consider a firm that operates with a production function $f(h, l, k)$, where h is weekly hours, l denotes total employment, and k denotes capital. The firm faces a cost function that comprises wages (w denotes the hourly wages, and θ denotes the overtime premium for all hours above h_s, the mandated standard hours), a fixed cost of employment f (see Rosen 1968), and a cost of capital r.

In this framework, usual results based on marginal cost considerations as developed in Rosen (1968) and more fully in Calmfors and Hoel (1989) hold (see also Hunt 1999). They can be summarized as follows. First, consider the case in which firms minimize costs with output fixed. The effect of a reduction in the normal working time depends on the initial situation. Obviously, there is no effect if the firm has optimal hours below the old standard. At the opposite extreme, if the initial situation includes overtime, then employment decreases and hours increase. Indeed, the marginal cost of hiring is increased when standard hours decrease, whereas the marginal cost of additional overtime does not change. Furthermore, the marginal cost of a separation decreases when standard hours decrease. Finally, under the assumption again that output is fixed, if the firm moves from the old standard to the new standard, employment unambiguously increases; when the firm moves from the old standard to an interior solution with overtime, employment may decrease or increase.

Next, assume that firms maximize profit instead of minimizing costs; a negative output effect arises when standard hours are reduced. This effect is larger, the larger the absolute value of the elasticity of demand for workers (Calmfors and Hoel 1988, p. 57). Hence, the likelihood of positive employment effects is smaller than when output is fixed. In fact, increasing employment may even be impossible.

Now, to understand the specifics of the French workweek reduction, we must remember that nominal monthly wage rigidity was mandated for workers employed at the date of the decree. It is therefore useful to have two periods in our model: one that precedes the reduction in

[4] Manipulations of the compensation package similar to those described in Trejo (1991) were ruled out.

the workweek ($d = 0$ and before) and one that is at, and follows, the reduction of the workweek ($d = 1$). In period 0, we have

$$C_0 = w_0 h_0 l_0 + w_0 \theta (h_0 - h_{s,0}) l_0 + f l_0 + r k_0,$$

with

$$l_0 = (1 - q) l_{-1} + e_0 - s_0,$$

where l_{-1} denotes employment at the previous date, q denotes the exogenous quit rate, e denotes the number of hires in the firm, and s denotes the number of separations (i.e., with quits excluded) when we assume that firms choose hours in excess of standard hours. Total costs are

$$C_0 = w_0 h_0 l_0 + f l_0 + r k_0$$

when optimal hours are set at the standard. The firm optimizes on capital, hours, hires, and separations. Importantly, since hires and separations both entail a cost because only the difference between hires and separations matters for production through employment, it is never optimal to hire and fire simultaneously. Hence, at least one of e or s is equal to zero.[5]

Consider what happened after the French reform of 1982. First, standard hours, $h_{s,1}$, decreased. Second, monthly wages did not decrease (by law for minimum-wage workers; this policy was only suggested for other workers, but firms followed the suggestion). Hence, hourly pay increased for those who were *already* employed in the firm but was left unchanged for new hires. Two wage rates prevailed at those dates for otherwise identical workers. Therefore, in period 1, the cost function that the firm faces is

$$\begin{aligned} C_1 = {} & w_1^1 h_1 e_1 + w_1^1 \theta (h_1 - h_{s,1}) e_1 + w_0^1 h_1 [(1 - q) l_0 - s_1] \\ & + w_0^1 \theta (h_1 - h_{s,1}) [(1 - q) l_0 - s_1] + f l_1 + r k_1, \end{aligned}$$

with $l_1 = (1 - q) l_0 + e_1 - s_1$; w_1^1 denotes the wage of the new hires, and w_0^1 denotes the wage of the workers that were present in the firm before period 1. The equivalent equation can be written when hours are set at the new standard. Because of the legal restrictions and the "suggestions" to the firms, the following equation holds:

$$w_0^1 h_{s,1} = w_0 h_{s,0} \quad \text{or} \quad w_0^1 = w_0 \frac{h_{s,0}}{h_{s,1}} > w_0 \quad \text{and} \quad w_1^1 = w_0.$$

It is straightforward to see that this double wage structure exacerbates

[5] The proof is straightforward. One must write the Lagrangian with the multipliers for $e_0 \geq 0$ and $s_0 \geq 0$ and look at the first-order conditions for hires and separations.

the negative employment effects of an hours reduction found in usual models discussed above.

There are also dramatic consequences for worker flow. In addition to the (negative) effects on employment that we have just presented, the French specifics of the 1982 change add another source of employment loss: it is often optimal for firms to separate workers constrained by the old standard, that is, workers paid 40 hours for a 39-hour workweek, and to hire new workers unconstrained by the old standard, that is, workers who will be paid 39 hours for a 39-hour workweek (see Crépon and Kramarz 2000, app. 1).

B. *Two Sources of Identification*

The process of reduction of the standard workweek from 40 to 39 hours was sudden and unexpected but, at the same time, took several years. In April 1982, the month in which the 1982 French Labor Force Survey took place, only a fraction of the firms had signed an agreement with their workers. The structure of hours in some firms in 1982 was identical to its structure before promulgation of the decree. Indeed, table 4 below shows that the fraction of individuals employed 40 hours in the population of workers employed 40 or 39 hours was equal to 28 percent in 1982 and fell to approximately 20 percent in 1983, 1984, and 1985. Hence, the passage to the new standard continued even after April 1982, the date of the survey. In addition, negotiations resulted in new and old workweeks of equal lengths for 20 percent of the workforce, one hour being counted as overtime after February 1, 1982.

These two characteristics of the process constitute our two sources of identification of the effects of the hours reduction. The reduction of the workweek was unexpected. In addition, some full-time workers were already employed 39 hours or less in 1981. Hence, it can be considered as a natural experiment. We evaluate the effect of the reduction of the workweek by comparing the employment transitions of workers employed 40 hours in 1981 with those of workers employed less than 40 hours at the same date. The identifying restriction is then that workers employed between 36 and 39 hours in 1981 are not affected by the reduction. Since most theoretical analyses also predict a negative impact on overtime workers, we also examine the employment transitions of workers employed 41–43 hours, exactly 44 hours (the kink in the overtime premium schedule), and 45–48 hours (the overtime premium jumps from 25 percent to 50 percent for all hours in excess of 44) in 1981, once again comparing them with those of the workers employed less than 40 hours at the same date.

The reduction of the workweek was also gradual, and this constitutes another source of identification. To see this, assume that once the reduction has been negotiated with a group of workers, all such workers are employed 39 hours exactly. Hence, all those who work 40 hours in 1982 are potentially affected by the forthcoming reduction, whereas all workers employed 39 hours at that date are not affected any more. Therefore, this last group is a potentially valid control group.

IV. The French Labor Force Survey

In this section, we describe the longitudinal data sets, the French Labor Force Surveys (LFS) (called the Enquête Emploi), that are used in the analysis. Our analysis uses data from 1977 to 1987.[6] Since the LFS questionnaire and survey structure changed between 1981 and 1982, we first describe the features of the survey that are common to the two subperiods. Then we describe the specifics for the years 1977–81, and finally, we describe the LFS for the period 1982–87.

Every year, approximately 60,000 domiciles are sampled from the stock of all houses and apartments (the sampling rate is exactly one in 300). In March of the survey year, each person in the sampled household is interviewed (in person if present at the time of the interview or by proxy if absent).[7] One-third of the domicile sample is replaced each year. Hence, all persons in the household are followed at most three times. We build our longitudinal data using this feature. In all years, most of the usual household and individual characteristics are available for all surveyed individuals. Sex, education categories (six categories), age at the end of schooling (from which we derive labor force experience), region (lives in Île-de-France or not), employment contract type (apprentice, short-term contract, or other) and status (employed or not employed), an indicator for part-time status, seniority, employer's industry (14 categories), and employer's size (four categories) are available in all years and are used in most of our analyses. The employment status of the individuals is defined according to International Labor Office criteria.

During the first subperiod (1976–81), two features are essential to note. First, there is no wage variable in the data. Second, the information does not pertain to "usual" hours but to hours worked during the reference week. If the individual works fewer than 45 hours, a second question is asked on possible reasons. Some are labeled temporary (strike, disease, etc.) whereas others are labeled durable, among which are distinguished part-time work and usual duration of the workweek. Hence, we have a potential way of approximating the "usual hours"

[6] For some descriptive statistics, we use data from 1976 onward.

[7] In 1979, 1980, and 1981, the LFS also took place in October. Each wave had the same size as those of March. Unfortunately, the October survey does not exist after 1981 and is therefore not usable for our purpose.

TABLE 2
Employment Losses, by Work Hours

Hours Employed	Panel t to $t+2$			
	1980–82	1977–79	1978–80	1979–81
36–39 hours	3.2	3.9	2.7	7.3
40 hours	6.2	4.3	5.0	5.5
41–43 hours	4.6	3.1	3.6	4.0
44 hours	6.0	5.0	2.1	5.8
45–48 hours	5.7	4.0	4.0	3.0
Observations	6,509	6,212	6,123	6,409

Source.—French Labor Force Survey, 1977–82.

concept. Unfortunately, responses to this second question appear to be frequently missing, and this possible measure is not usable.

Starting in 1982, monthly wages (grouped by cells of 500 francs, roughly $100) and usual weekly hours are also available. Although the usual survey date is March, because of the 1982 Census of Population, the 1982 LFS took place in April of that year. Hence, the 1982 LFS took place just after the legal reduction in the workweek to 39 hours took effect on February 1, 1982.

For our first analysis, we construct four three-year panel data sets in which individuals are followed from 1977 to 1979, from 1978 to 1980, from 1979 to 1981, and from 1980 to 1982. This allows us to characterize workers' situations before, at, and just after implementation of the legal change in the standard workweek.

For our second analysis, we also construct four three-year panel data sets in which individuals are followed from 1982 to 1984, from 1983 to 1985, from 1984 to 1986, and from 1985 to 1987. This allows us to understand the implementation process of the reduced workweek that took place in 1982 and just after and contrast this period with the ones immediately after.

V. The Reduction of the Workweek: A Natural Experiment

A. *Principle and Descriptive Analysis*

Table 1 shows the proportion of full-time workers employed 36–39 hours within the population of all full-time workers employed 36–48 hours in 1981. This fraction is small, 2.4 percent, but is increasing across time. The fraction of workers employed 40 hours is also increasing, whereas the fraction of overtime workers constantly decreases between 1976 and 1981. Furthermore, the number of observations also increases during the period, reflecting a decrease in the number of workers employed more than 48 hours. Table 2 shows the transition rates from employment to nonemployment for these various categories of workers. Between

1981 and 1982, transitions from employment to nonemployment are more intense for workers employed 40 hours than for those employed 36–39 hours. Of all workers employed 40 hours in 1981, 6.2 percent have no employment in 1982, whereas 3.2 percent of those employed less than 40 hours are in the same situation, a difference of three percentage points. However, the size of the control group that we use to evaluate the impact of the reduction of the workweek is small: 2.4 percent of the workers with hours between 36 and 48 were employed less than 40 hours in March 1981.

B. *Selectivity Bias*

1. Motivation and Statistical Model

We now discuss the identification conditions under which we can isolate a causal effect of the workweek reduction on employment. Two hypotheses are necessary for our analysis. First, workers employed 36–39 hours must not have been affected by the reduction of the workweek. Second, the employment to nonemployment transitions of these workers identify what the transitions of workers employed 40 hours or more would have been *in the absence* of a reduction of the workweek. Therefore, we classify individuals into two categories: those affected (1) and those not affected (0) by the reduction. We consider the two corresponding labor market situations $NE(0)$ and $NE(1)$ equal to one if the individual is not employed and equal to zero otherwise. The effective situation of any individual is

$$NE_i = NE_i(0) + D_i[NE_i(1) - NE_i(0)],$$

where $D = 1$ if the individual is employed 40 hours or more a week in 1981. The employment loss probability is $E(NE_i)$, where $E(\cdot)$ denotes the expectation of the random variable between parentheses. We focus on the quantity $NE_i(1) - NE_i(0)$. Such quantities measure, for each individual, the difference between the labor market outcome when affected by the reduction of the workweek and what would have been the outcome if they had not been affected by this reduction. We follow Rubin (1974) in his definition of a causal effect. These quantities, different for different individuals, are unobservable since any given individual is in one and exactly one state among the two possible ones. Only some parameters of the distribution of $NE_i(1) - NE_i(0)$ can be identified, under some hypotheses. For instance, one can identify the expectation of the effect, conditional on changing hours from 40 to 39 (average treatment on the treated in Heckman, LaLonde, and Smith [1999] terminology), defined as $E(NE_i(1) - NE_i(0)|D_i = 1)$. To measure this last quantity, we assume (H_{0A}) that, conditional on observable variables, the *potential* outcome, in which workers are not affected by the reduction, $NE_i(0)$, is independent of actually being affected by the reduction to 39 hours (independence conditional on observable variables [Rubin 1977]). The corresponding equation is

$$\mathrm{H}_{0A}: E(NE_i(0)|x_i, D_i = 1) = E(NE_i(0)|x_i, D_i = 0).$$

Therefore, the following relation holds:

$$\begin{aligned} E(NE_i|x_i, D_i) &= E(NE_i(0)|x_i) + D_i E(NE_i(1) - NE_i(0)|x_i, D_i = 1) \\ &= g_0(x_i) + D_i e_D(x_i), \end{aligned}$$

where the function $e_D(x_i)$ represents the average effect of the workweek reduction when applied to individuals with characteristics x_i.

There are several available methods for estimating the effect of the workweek reduction that are compatible with the hypotheses above. A simple and transparent method is based on the differences in the transition rates from employment to nonemployment between workers affected and workers not affected by the change in the standard workweek, with all their observable characteristics controlled for. To analyze the 1982 employment losses of individuals employed in 1981 with observable characteristics x_{81} and duration D_{81} we estimate a regression using a linear probability model based on the following relation:

$$E(NE_{82}|x_{81}, D_{81}) = x_{81}\beta + \alpha_{81} I(D_{81} = 40), \tag{1}$$

where the i index has been omitted for simplicity, $I(A)$ equals one if A is true and zero otherwise, and NE_{82} corresponds to the nonemployment situation in 1982. The impact of the workweek reduction on employment to nonemployment transitions is given by the coefficient α_{81}. Note that other techniques, such as matching methods (Heckman et al. 1998), could have been used. We selected a linear probability model for simplicity and transparency.

To include workers working overtime hours, Rubin's model can be extended using the framework with multiple treatments as it was recently developed in Brodaty, Crépon, and Fougère (1999), Imbens (1999), and Lechner (1999). In 1981, before the change in the standard workweek, the overtime premium was 25 percent for all hours between 41 and 44 and 50 percent for all hours strictly above 44. Hence, because of the structure of the overtime premium schedule, in addition to workers employed 40 hours, we distinguish among workers employed 41–43 hours, workers employed exactly 44 hours (for whom the marginal cost of employment increases the most when the standard workweek is reduced by one hour), and workers employed 45–48 hours. We estimate the following equation:

$$E(NE_{82}|x_{81}, D_{81}) = x_{81}\beta + \alpha_{81}^{40}I(D_{81} = 40) + \alpha_{81}^{41}I(41 \leq D_{81} \leq 43)$$
$$+ \alpha_{81}^{44}I(D_{81} = 44) + \alpha_{81}^{45}I(45 \leq D_{81} \leq 48). \quad (2)$$

2. Results

The results presented in this subsection are based on the 1980–82 panel. Since our analysis uses workers employed 36–39 hours as a control group, we first checked that their observed characteristics were similar to those of workers employed exactly 40 hours and above. We estimated two logistic regressions in which the dependent variable was employed 40 hours versus employed more than 40 hours in years 1980 and 1981.[8] It appears that very few individual characteristics matter. More specifically, differences in education, experience, or seniority are not associated with strong differences in the hours category. Most differences stem from the employing firm. Short hours are found not only in the service sectors but also in some manufacturing industries, such as those producing intermediary or consumption goods. Hence, violations of our hypothesis H_{0A} are more likely to come from unobserved firm heterogeneity, which we cannot control for, than from unobserved individual heterogeneity.

Column 1 of panel A of table 3 presents the estimates of equation (2). The independent variables that we use are sex, region, education (four categories), labor market experience (four categories), seniority (four categories), the two-digit industry of the employing firm, and information on hours worked in the entry year of the panel, that is, 1980. The inclusion of variables on the past of the individual may render the independence assumption more plausible (Heckman et al. 1998). Finally, as mentioned above, estimates are based on a linear probability model. Resulting estimates confirm figures from table 2: workers employed 40 hours in 1981 lose their job more often. The point estimate is equal to 2.6 percent, significant at the 10 percent level, for workers employed exactly 40 hours; whereas coefficients for overtime workers, that is, employed strictly more than 40 hours, are all positive but not significantly different from zero.

[8] Those regressions are not reported here but can be found in Crépon and Kramarz (2000).

TABLE 3
NONEMPLOYMENT AND CHANGES IN HOURS FOR WORKERS EMPLOYED IN YEAR $t+1$

	PANEL t TO $t+2$				
	1980–82 (1)	1977–79 (2)	1978–80 (3)	1979–81 (4)	Pooled (5)
	A. Nonemployment at $t+2$				
Hours = 40 indicator	2.60	−1.26	1.29	−3.85	3.90
	(1.44)	(1.86)	(1.48)	(2.30)	(1.82)
41 ≤ hours ≤ 43 indicator	1.32	−2.40	1.06	−5.09	3.49
	(1.60)	(1.91)	(1.57)	(2.39)	(1.97)
Hours = 44 indicator	2.50	−.73	−.32	−4.57	4.20
	(2.50)	(2.35)	(1.91)	(2.92)	(2.88)
45 ≤ hours ≤ 48 indicator	2.12	−2.14	1.50	−6.52	4.52
	(1.66)	(1.96)	(1.61)	(2.39)	(2.03)
Observations	6,509	6,212	6,123	6,409	25,253
	B. Hours Change, $t+1$ to $t+2$				
36 ≤ hours ≤ 39 indicator	1.57	1.82	1.90	2.00	−.31
	(.40)	(.27)	(.27)	(.31)	(.40)
Hours = 40 indicator	−.94	.16	.10	.08	−1.02
	(.22)	(.14)	(.15)	(.15)	(.22)
41 ≤ hours ≤ 43 indicator	−2.64	−1.31	−1.14	−1.30	−1.35
	(.25)	(.16)	(.17)	(.18)	(.25)
Hours = 44 indicator	−4.07	−2.53	−2.31	−2.76	−1.48
	(.34)	(.22)	(.25)	(.25)	(.34)
45 ≤ hours ≤ 48 indicator	−5.17	−3.42	−3.57	−3.65	−1.60
	(.27)	(.18)	(.20)	(.20)	(.27)
Observations	4,475	5,287	5,227	5,303	20,292

NOTE.—Regressions for the LFS panels of 1977–79, 1978–80, 1979–81, and 1980–82 (linear probability models for panel A, ordinary least squares for panel B). The dependent variable is nonemployment in the exit year of the panel (1979, 1980, 1981, and 1982, respectively) for panel A and the change in hours between the median year (1978, 1979, 1980, and 1981, respectively) and the exit year (1979, 1980, 1981, and 1982, respectively) for panel B. Independent variables are indicator for the "hours" categories (only reported coefficients), industry, region (Île-de-France or other), skill level (three categories), sex, diploma (six categories), experience (four categories), seniority (four categories), labor market status (apprentice or not), and "hours in first year of the panel strictly 40" and "hours in first year of the panel strictly above 40." The population includes all full-time workers in the private sector working between 36 and 48 hours in the median year of each panel (1978, 1979, 1980, and 1981, respectively). Col. 5 reports pooled estimates in which all variables are interacted with the relevant year indicator except for the "hours" categories for which we introduce pooled coefficients and coefficients specific to year 1981 (panel 1980–82). These last coefficients are those reported in col. 5 (pooled). Robust standard errors are given in parentheses.

3. Working 39 Hours, 40 Hours, or More before the Reduction

Of course, independent of the reduction of the workweek, it is possible that workers employed exactly 40 hours or workers employed overtime lose their jobs more (respectively, less) often than other workers, even after one controls for observable individual characteristics. In such a case, our preceding estimates would be biased. Table 2 shows that the probability of job loss for workers employed 36–39 hours fluctuates from year to year, in contrast to that for workers employed exactly 40 hours. This probability is, in general, lower than the one observed for workers employed 40 hours (1979, 1980, and 1982), but it is greater in 1981. The biggest difference is observed in 1982 (3 percent), whereas the difference is equal to 1.4 percent in 1979, 2.3 percent in 1979, and −1.8 percent in 1981. Workers employed overtime also tend to lose their jobs more often than workers employed 36–39 hours except in 1981; furthermore, workers employed 41–43 hours lose their job less often than those employed exactly 40 hours. Columns 1–4 of panel A of table 3 show the probability of nonemployment for our four panels, with observable characteristics of the workers controlled for, expressed as a difference compared to the control group, workers employed 36–39 hours. No obvious pattern is present.

To identify the specific effect of the 1981 reduction of the workweek, we estimate the following double difference equation using all four panels:

$$\begin{aligned} E(NE_t|x_t, D_t, t) = {} & x_t\beta_t + \alpha_0^{40} I(D_t = 40) + \tilde{\alpha}_{81}^{40} I(D_{81} = 40) \\ & + \alpha_0^{41} I(41 \leq D_t \leq 43) + \tilde{\alpha}_{81}^{41} I(41 \leq D_{81} \leq 43) \\ & + \alpha_0^{44} I(D_t = 44) + \tilde{\alpha}_{81}^{44} I(D_{81} = 44) \\ & + \alpha_0^{45} I(45 \leq D_t \leq 48) + \tilde{\alpha}_{81}^{45} I(45 \leq D_{81} \leq 48), \\ t = {} & 1978,\ 1979,\ 1980,\ 1981. \end{aligned} \tag{3}$$

The impact of the reduction of the workweek is now given by the coefficients $\tilde{\alpha}_{81}^{40}$, $\tilde{\alpha}_{81}^{41}$, $\tilde{\alpha}_{81}^{44}$, and $\tilde{\alpha}_{81}^{45}$. The estimates of these four coefficients are presented in column 5 of panel A of table 3. For workers employed exactly 40 hours, the resulting estimate from this approach is larger, around 4 percent, than those obtained with a simple difference method. In addition, for overtime workers, all estimated coefficients are large: some are even larger than those obtained for workers employed 40 hours as the theory predicts; two of them are marginally significant and one strongly significant (for workers employed 45–48 hours).[9]

The double-difference analysis may lead to overestimating the impact of the workweek reduction. As described in Section IV, hours are measured from the number of hours worked in the week that precedes the interview. There is no satisfactory information on usual hours. Hence, we may capture the prevailing economic conditions rather than the specific effects of the mandatory reduction in hours. For instance, in 1981, economic conditions were bad, and workers employed 36–39 hours may well be those working in adversely affected firms. Therefore, such workers may lose their job more often than other types of workers. This type of measurement error results in an upward bias of the esti-

[9] Even though the differences between the various coefficients are not statistically significant. See also Andrews, Schank, and Simmons (1999) for results with the same flavor based on German plant-level data.

mated impact of the reduction of the workweek, if assessed with a double-difference approach.

One way of checking the validity of our control group is as follows. If workers employed 36–39 hours constitute a valid control group, they should not be affected by the reduction in hours. In particular, if we examine changes in hours between t and $t+1$ for workers employed 36–39 hours at date t who are also employed at date $t+1$, measured changes for $t = 1981$ should not be different from changes in the years before. Of course, this is not true of workers employed 40 hours or more. Therefore, we first estimate

$$\begin{aligned} E(\Delta D_t^{t+1} \mid x_t, D_t, t) = {} & x_t\beta_t + \tilde{\alpha}_t^{39} I(36 \le D_t \le 39) + \alpha_t^{40} I(D_t = 40) \\ & + \alpha_t^{41} I(41 \le D_t \le 43) + \alpha_T^{44} I(D_t = 44) \\ & + \alpha_t^{45} I(45 \le D_t \le 48), \\ & t = 1978,\ 1979,\ 1980,\ 1981, \end{aligned} \tag{4}$$

in which we do not include a constant. The resulting estimates are presented in columns 1–4 of panel B of table 3. We are interested in the effects that are specific to the year 1981, that is, to changes between 1981 and 1982 that cannot be found in other years. The corresponding equation is

$$\begin{aligned} E(\Delta D_t^{t+1} \mid x_t, D_t, t) = {} & x_t\beta_t + \tilde{\alpha}_{81}^{39} I(36 \le D_{81} \le 39) + \alpha_0^{40} I(D_t = 40) \\ & + \tilde{\alpha}_{81}^{40} I(D_{81} = 40) + \alpha_0^{41} I(41 \le D_t \le 43) \\ & + \tilde{\alpha}_{81}^{41} I(41 \le D_{81} \le 43) + \alpha_0^{44} I(D_t = 44) \\ & + \tilde{\alpha}_{81}^{44} I(D_{81} = 44) + \alpha_0^{45} I(45 \le D_t \le 48) \\ & + \tilde{\alpha}_{81}^{45} I(45 \le D_{81} \le 48), \\ & t = 1978,\ 1979,\ 1980,\ 1981. \end{aligned} \tag{5}$$

In equation (5), the reference group comprises workers employed 36–39 hours in the years preceding 1981. The estimates of these five coefficients, $\tilde{\alpha}_{81}^{39}$, $\tilde{\alpha}_{81}^{40}$, $\tilde{\alpha}_{81}^{41}$, $\tilde{\alpha}_{81}^{44}$, and $\tilde{\alpha}_{81}^{45}$, are presented in column 5 of panel B of table 3. Consistent with our hypothesis, $\tilde{\alpha}_{81}^{39}$ is not significantly different from zero. In this regression, workers employed 36–39 hours in the years before 1981 constitute our reference group. Therefore, workers employed 36–39 hours in 1981 had hours changes that were similar to those of workers employed 36–39 hours in the years before 1981. Hence, their hours were not affected by the workweek reduction. We conclude that workers employed 36–39 hours in 1981 constitute a valid control group. In addition, workers employed 40 hours or more in 1981 saw their hours decrease. For instance, workers employed 40 hours in 1981 had a change of −0.71 hours per week (= −1.02 − [−0.31]) in comparison with workers employed 36–39 hours in 1981. Similarly, workers employed 45–48 hours had a decrease in their hours of 1.29 hours per week. Results for workers employed 40 hours in 1981 are therefore consistent with the predictions of theoretical models. However, results for overtime workers are apparently less in line with such predictions—estimated changes show a decrease instead of an increase—but the estimated changes in hours are not precise enough to give a definitive answer.

4. Discussion

Our first analysis has several limitations. First, the size of the control group is small—less than 3 percent of the observed population—but increasing. This is obviously the main reason for the lack of precision of some of our estimates.[10] Furthermore, even though it is small, this control group could also be heterogeneous. To test the robustness of our results, we estimated the same equations as those presented in table 3 but with two different, alternative, control groups: workers employed 35–39 hours and workers employed 37–39 hours. We also estimated the same equations using data from 1975–82 (instead of 1977–82). All the estimated results were similar to those already discussed.

It is also possible that unobserved differences in transition rates between workers employed 36–39 hours and those employed 40 hours, even after all observed characteristics are controlled for, still exist. In particular, we do not have pay data. The minimum wage strongly increased in July 1981 and may have generated job losses quite apart from those associated with the hours reduction. If workers employed 40 hours were more affected by the SMIC increase than those working 39 hours or less, we may put the blame on the hours reduction, whereas the job losses actually resulted from the SMIC increase. It is likely that this source of estimation bias is small. Wages are strongly correlated with skills, education, diploma, experience, or seniority.[11] As mentioned previously, the introduction of all these characteristics in our regressions did not modify the estimated effects. The residual component of wages should not cause a bias.

[10] Nevertheless, the diffusion of the reduction of hours to workers employed 39 hours or less in 1981 leads to underestimating the effect of interest. In the extreme case of a complete diffusion of the reduction of the workweek to all workers employed less than 40 hours a week, we should find no effect of the hours worked in 1981 on employment losses in our regressions.

[11] This is so more strongly in France than in the United States (see Abowd, Kramarz, and Margolis 1999).

We tried to examine the possible heterogeneity of the impact of the reduction of the workweek in the population. In particular, we estimated effects by skill levels, by experience or seniority groups, and by employment status (apprentice or not). As expected, we could not isolate any significant effect. This does not mean that the reduction affects all groups similarly, but simply that the control group is too small to measure such effects.

Such limits—size of the control groups or absence of some important variables (wage or usual hours, as opposed to hours in the week preceding the interview)—motivated us to examine more carefully the situation that prevailed from 1982 onward, since these two data limitations do not exist after that date. Nevertheless, such data limitations do not invalidate the results of this first analysis: the reduction of the workweek from 40 to 39 hours is directly responsible for the increased transition rates from employment to nonemployment of affected workers, those who worked 40 hours as well as those who worked overtime before the reduction in 1981.

VI. Late Changes to the New Standard Workweek

A. *Motivation*

A first examination of the proportion of workers employed 40 hours between 1982 and 1987, after the decree mandating the workweek reduction, confirms that the passage to 39 hours was progressive (table 4). The fraction of workers employed exactly 40 hours became stable only in 1983, at around 20 percent of workers employed either 39 or 40 hours. This progressive transition to 39 hours is another potential source of identification of the effect of the workweek reduction. Examination of table 4 also shows that differences in probabilities of job loss between workers employed 39 hours and workers employed 40 hours exist. In 1982, this difference amounts to 1.6 points for year-to-year job losses and to 3.9 points for cumulated losses over two years. These differential losses are much greater in 1982 than in any of the following years.

The persistence of a large fraction of workers employed 40 hours (usual hours) after 1982, 20 percent, demonstrates that negotiations led many firms to maintain hours as they were before 1982. Thus a fraction of the workers declaring usual hours equal to 40 in 1982 may well work for firms that implemented the new standard before April 1982, the date of the survey, one hour being paid as overtime. The proportion of workers employed exactly 40 hours in a firm that had implemented the 39-hour workweek before April 1982 is not known but

TABLE 4
TRANSITIONS FROM EMPLOYMENT TO NONEMPLOYMENT FOR WORKERS EMPLOYED 39 OR 40 HOURS IN 1982, 1983, 1984, AND 1985

	Proportion	Employment Losses Cumulated over: One Year	Employment Losses Cumulated over: Two Years	Observations
		1982		
40 hours	27.9	8.2	16.5	1,700
39 hours	72.1	6.6	12.6	4,397
Difference		1.6	3.9	
		1983		
40 hours	21.9	6.9	12.6	1,214
39 hours	78.1	6.4	11.8	4,331
Difference		.5	.8	
		1984		
40 hours	20.3	7.9	14.8	1,112
39 hours	79.7	6.6	11.9	4,371
Difference		1.3	2.9	
		1985		
40 hours	19.1	5.7	11.9	996
39 hours	80.9	6.3	12.1	4,225
Difference		−.6	−.2	

NOTE.—LFS panels for the years 1982–84, 1983–85, 1984–86, and 1985–87. Statistics were computed using the nonemployment variables in the median and in the final year of each panel. Observations: All full-time workers employed 39 or 40 hours in the first year of the panel.

can be estimated under various hypotheses using the proportion of workers employed 40 hours at various dates.

When we use as a parallel our first analysis, in which the control group was composed of workers employed between 36 and 39 hours in 1981, it seems that, on the basis of the discussion above, a potential control group for this second analysis could be all workers employed 39 hours in April 1982, that is, workers for whom the new standard was applied immediately after the publication of the February 1 decree and who were still employed two months after this date. The associated treated group consists of all workers employed exactly 40 hours in April 1982, that is, workers who were also employed after February 1 in firms that had completed negotiations and kept the old standard as well as in firms that had not yet implemented the new standard. We discuss in subsection *B* the main ideas that are sufficient for this potential control/treatment comparison *to detect and to measure* the impact of the workweek reduction on employment.[12] We discuss in subsection *E* the economic validity of these hypotheses. In particular, on the basis of a survey per-

[12] The full set of statistical hypotheses and results are contained in the appendices of Crépon and Kramarz (2000).

formed in October 1982, we show that firms that completed negotiations after April 1982 did so for reasons unrelated to their economic situation, but for reasons that were related to a weaker and less active tradition of bargaining that prevailed in their industry; those that signed earlier mostly belonged to industries in which negotiations were more commonplace.

B. Statistical Model

The statistical model that we use to evaluate the effect of the reduction of the workweek is an extension to multiwave treatments of the model used in our first analysis. Let us consider three potential states in 1983 and 1984: not affected by the reduction to 39 hours (0), affected before April 1982 (1), and affected after April 1982 (2). Let us denote T_{0i}, T_{1i}, and T_{2i} as the respective events and $NE_{it}(0)$, $NE_{it}(1)$, and $NE_{it}(2)$ *equal to one if individual* i *was ever nonemployed between 1982 and* t, for $t =$ 1982, 1983, and 1984. Therefore, *NE* summarizes the employment history of each individual in the years following the workweek reduction. Denote also NE_i as the resulting vector of labor market history at the various dates of interest. Notice that the three potential states are mutually exclusive; hence $T_{0i} + T_{1i} + T_{2i} = 1$. In addition, since all workers are eventually affected by the reduction of the workweek, we have $T_{0i} = 0$.

Our quantities of interest are $NE_{it}(1) - NE_{it}(0)$ and $NE_{it}(2) - NE_{it}(0)$, that is, the impact of the reduction on employment for those workers affected before April 1982 or after April 1982. However, these quantities are not directly observable. Therefore, identifying hypotheses are necessary, as they were in our first analysis, in order to recover some of the parameters of the distribution of these quantities and in order to give an evaluation of the reduction of the workweek. The basic elements of the model as well as their consequences are presented in the Appendix. All formal details and proofs of the results are presented in appendix 2 of Crépon and Kramarz (2000). We summarize their economic content and the resulting equations in the next paragraphs.

The first hypothesis (H_A) implies that, *for workers employed 40 hours in 1982,* and conditionally on observable variables, the labor market state associated with being affected before April 1982, $NE_i(1)$, is independent of the date at which the new standard was implemented, T_{1i}. This is the analogue of hypothesis H_{0A} of our previous model. The empirical plausibility of this hypothesis is discussed in subsection *E.*

In addition, we assume—as hypothesis H_B—that the effect of the reduction of the workweek is independent of the outcome, 39 or 40 hours, of the negotiation surrounding implementation of the new standard workweek. This last hypothesis is not very demanding since it amounts to neglecting the additional cost induced by one overtime hour when the outcome is 40 (overtime adds 25 percent to a normal hour). Notice that our first analysis tends to support this hypothesis.

Proposition 1 in the Appendix shows that under these hypotheses, the potential control group (i.e., workers employed 39 hours in 1982) is a valid control group for *detecting* an impact of the workweek reduction on employment. In particular, it implies that *as soon as* the reduction of the workweek has no employment effect, that is, $NE_i(0) = NE_i(1) = NE_i(2)$, a regression trying to explain nonemployment in 1983 or 1984 should not include an indicator for working 40 hours in 1982, denoted $I(D_{82i} = 40)$. Even if the interpretation of the resulting estimates of such an equation is complex, a significant coefficient on the variable $I(D_{82i} = 40)$ would demonstrate that the reduction had an impact on employment.

Not only are we trying to detect the existence of an effect, we want to measure this effect. To do so, we need additional assumptions. First, we assume that the effects of the reduction last only a limited number of periods, specifically *two years*. For instance, in 1984, only workers employed in firms having completed negotiations after April 1982 are susceptible to losing their job *because* of the reduction. And, in 1983, job losses *because* of the reduction may come from job losses of workers employed 40 hours in 1982 (first-year effect) but also from job losses of workers employed 39 hours in 1982 (second-year effect). More precisely, consider workers employed 40 hours in 1982 with characteristics x_i. Their probability of job loss between 1982 and 1983 is the sum of two components: one general to workers with characteristics x_i and one due to the *first-year* effect of the reduction of the workweek, $\pi_1(x_i)$. Consider now workers employed 39 hours in 1982 with characteristics x_i. Their probability of job loss is also the sum of two components: one general to workers with characteristics x_i and one due to the *second-year* effect of the reduction of the workweek, $\pi_2(x_i)$. As a consequence, in 1983, the difference in probabilities of job loss between workers employed 40 hours in 1982 and those employed 39 hours in 1982 (and employed 40 hours in 1981) is exactly $\pi_1(x_i) - \pi_2(x_i)$. The same reasoning applies to transitions between 1982 and 1984 and identifies $\pi_1(x_i)$.

However, 20 percent of the workers were employed 40 hours several years after completion of the negotiations (table 4). This makes more difficult the comparison of workers employed 40 hours with those employed 39 hours and introduces a nuisance parameter in our estimating framework, the fraction of workers employed 40 hours in 1982 that completed their negotiations after April 1982.

Our proposition 2 summarizes this discussion (see the Appendix).[13]

[13] All hypotheses and proofs are presented in app. 2 of Crépon and Kramarz (2000).

We prove that the potential control group (i.e., workers employed 39 hours in 1982) is a valid control group for *measuring* the impact of the workweek reduction on employment and that

$$E(NE_{83i}|x_i, D_{82i}) = g_{83}(x_i) + P(T_{2i} = 1|x_i, D_{82i} = 40) \times [\pi_1(x_i) - \pi_2(x_i)]I(D_{82i} = 40),$$
$$E(NE_{84i}|x_i, D_{82i} = g_{84}(x_i) + P(T_{2i} = 1|x_i, D_{82i} = 40) \times \pi_1(x_i)I(D_{82i} = 40),$$

where, as mentioned above, $\pi_k(x_i)$ denotes the change in the probability of employment loss that can be directly attributed to the reduction of the workweek for individuals with characteristics x_i in the kth period following negotiations, and $g(\cdot)$ denotes any function ($NE_{84} = 1$ for a worker not employed in 1983 *or* in 1984 since we consider only workers employed in 1982). The total effect of the reduction is therefore equal to $\pi_1 + \pi_2$. As noted above, there is a *nuisance parameter,* $P(T_{2i} = 1|x_i, D_{82i} = 40)$, in these equations.

Our equations can be restated as the following regressions:

$$E(NE_{83}|D_{82}, x_i) = x_ic + x_i\lambda I(D_{82} = 40),$$
$$E(NE_{84}|D_{82}, x_i) = x_id + x_i\mu I(D_{82} = 40),$$

which yield parameters $\pi_1(x_i) - \pi_2(x_i) = x_i\lambda$ and $\pi_1(x_i) = x_i\mu$ for any given value of the nuisance parameter. Once again, for simplicity and transparency, we estimate all models in this section using linear probability models. We examine the case of a homogeneous effect within the population, and we also estimate a specific effect for low-wage workers. As in Section V, we must also check that the effects that are found, if any, are specific to 1982. One cannot ignore the possibility that, starting after 1982, workers employed 40 hours lose their job more often than those employed 39 hours. Indeed, a common reason—specific to these jobs but unobserved by the econometrician—could explain 40 hours as both the outcome of the negotiation and the destruction of these jobs, a destruction that would have taken place even without any mandatory hours reduction. Therefore, we also estimate the following equations:[14]

$$E(NE_{t+1}|D_t, x_i) = \delta_1 I(t = 82) + x_i c + x_i\tilde{\lambda}I(D_t = 40) + x_i\lambda I(D_{82} = 40),$$

$$E(NE_{t+2}|D_t, x_i, z_i) = \delta_2 I(t = 82) + x_i d + x_i\tilde{\mu}I(D_t = 40) + x_i\mu I(D_{82} = 40).$$

Identification of the nuisance parameter $P(T_{2i} = 1|x_i, D_{82i} = 40)$ is discussed at length in Crépon and Kramarz (2000). The solution adopted here assumes that the nuisance parameter is equal to one for all individuals. This corresponds to the following hypothesis: all workers employed in firms having implemented the workweek reduction before April 1982 actually work 39 hours after the reduction. The hypothesis implies that all firms in which a 40-hour workweek prevailed, with one hour being compensated as overtime (on the basis of the French LFS of 1983–87, such firms constitute 20 percent of total employment), implemented the reduction to 39 hours after April 1982. Since the nuisance parameter lies between zero and one, this hypothesis provides us with a lower bound on the parameter of interest.[15]

C. *Results*

Our analysis is based on four panel data sets spanning three years each, 1982–84, 1983–85, 1984–86, and 1985–87. The regressions presented in the tables explain nonemployment at date $t + 1$, NE_{t+1}, given employment at date t or nonemployment at date $t + 1$, *or* at date $t + 2$, NE_{t+2}, given employment at date t, as functions of the number of workweek hours in year t. We consider only full-time employees working either 39 or 40 hours. Our additional explanatory variables are the industry (two-digit classification), size of the employing firm, region (Île-de-France or not), sex, diploma (six categories), labor market experience (four categories), seniority (four categories), wage level (five categories defined with respect to the minimum wage, the SMIC; a low wage corresponds to wages between 0.95 and 1.10 times the SMIC), and

[14] We also estimate the year-by-year regressions:

$$E(NE_{t+1}|D_t, x_i, z_i) = x_i c_t + z_i\lambda_t I(D_t = 40),$$

$$E(NE_{t+2}|D_t, x_i, z_i) = x_i d_t + z_i\mu_t I(D_t = 40).$$

[15] Results using other values of this nuisance parameter are given in Crépon and Kramarz (2000).

TABLE 5
Job Losses and Hours Worked: Late Reduction Total Effects and Effect for Low-Wage Workers Panel by Panel Estimation

	Hours = 40 Indicator (1)	Hours = 40 Indicator (2)	Hours = 40 and Low-Wage Indicator (3)
	A. 1982–84		
NE_{83}	1.28	.53	8.49
	(.77)	(.77)	(3.66)
NE_{84}	2.71	1.75	10.87
	(1.02)	(1.04)	(4.50)
$2NE_{84} - NE_{83}$	4.13	2.96	13.24
	(1.63)	(1.66)	(7.09)
	B. 1983–85		
NE_{84}	−.08	−.06	−.14
	(.81)	(.79)	(3.02)
NE_{85}	−.36	−.53	1.21
	(1.04)	(1.05)	(3.72)
$2NE_{85} - NE_{84}$	−.64	−1.01	2.56
	(1.62)	(1.66)	(5.70)
	C. 1984–86		
NE_{85}	.91	.93	−.38
	(.89)	(.90)	(3.98)
NE_{86}	2.05	1.81	2.94
	(1.16)	(1.18)	(5.22)
$2NE_{86} - NE_{85}$	3.20	2.68	6.28
	(1.82)	(1.84)	(8.32)
	D. 1985–87		
NE_{86}	−.94	−1.38	3.30
	(.85)	(.82)	(3.42)
NE_{87}	−.53	−1.80	9.48
	(1.15)	(1.14)	(4.34)
$2NE_{87} - NE_{86}$	−.12	−2.21	15.66
	(1.83)	(1.84)	(6.81)

Note.—Panel by panel regressions (linear probability models). The dependent variable is employment loss in the median year or in the last year of the panel. Independent variables are industry, region (Île-de-France or other), skill level (three categories), sex, diploma (six categories), experience (four categories), seniority (four categories), labor market status (apprentice, short-term contract, or long-term contract), wage level (five categories defined with respect to the minimum wage in the relevant year), an indicator for hours = 40 (only reported coefficient in col. 1), and an indicator for hours = 40 and its interaction with an indicator for a low-wage worker, i.e., with wage between 0.95 and 1.1 times the minimum wage (both are the only reported coefficients in col. 2). Observations are full-time workers of the private sector employed either 39 or 40 hours in the entry year of the panel. Robust standard errors are in parentheses.

labor market status (apprentice, on short-term contract, or on long-term contract).

Table 5 presents estimation results in which the indicator function for usual hours = 40 is directly included (col. 1). In columns 2 and 3, we show the results when this indicator function and its interaction with a "low-wage" indicator are both included. Results for the 1982–84 panel demonstrate that those workers employed 40 hours in 1982 are more

TABLE 6
JOB LOSSES AND HOURS WORKED: LATE REDUCTION
(Total Effects and Effect for Low-Wage Workers, Pooled Estimates) (N=22,345)

	Specification 1		Specification 2			
	Hours = 40 Indicator (1)	Hours = 40 and Year = 1982 Indicator (2)	Hours = 40 Indicator (3)	Hours = 40 and Low-Wage Indicator (4)	Hours = 40 and Year = 1982 Indicator (5)	Hours = 40, Low-Wage, and Year = 1982 Indicator (6)
NE_{t+1}	.00	1.29	−.10	.79	.64	7.70
	(.49)	(.91)	(.49)	(1.97)	(.91)	(4.14)
NE_{t+2}	.40	2.30	−.10	4.06	1.84	6.80
	(2.04)	(1.20)	(.49)	(2.49)	(1.22)	(5.15)

NOTE.—Pooled regressions (linear probability models). The dependent variable is employment loss in the median year or in the last year of each panel. Independent variables: industry, region (Île-de-France or other), skill level (three categories), sex, diploma (six categories), experience (four categories), seniority (four categories), labor market status (apprentice, short-term contract, or long-term contract), wage level (five categories defined with respect to the minimum wage in the relevant year), and an indicator for hours=40 and its interaction with a year indicator for 1982 (both are the only reported coefficients in col. 1), an indicator for hours=40, its interaction with a year indicator for 1982, an indicator for hours=40 interacted with an indicator for a low-wage worker (i.e. with wage between 0.95 and 1.1 times the minimum wage), and its interaction with a year indicator for 1982 (all four are the only reported coefficients in col. 2). Robust standard errors are in parentheses.

likely to lose their jobs. After one year, the effect amounts to 1.3 percent. On the basis of proposition 2, this number measures the difference between the first-year and the second-year probabilities of employment loss. The two-year effect, 2.7 percent, is significantly different from zero. It is roughly twice the one-year effect, demonstrating that the effect is still present after one year. The total effect over the two years is equal to 4.1 percent ($= 2 \times 2.7 - 1.3$; see proposition 2).

For a value of the nuisance parameter equal to one, we estimate that the effect of the reduction of the workweek was a 4.1-percentage-point increase in the probability of becoming nonemployed (other values of the nuisance parameter would yield larger estimates of the impact). More important, for all possible values of the nuisance parameter, we always reject the null hypothesis of no impact of the reduction of the workweek on the transition rates from employment to nonemployment.

Table 5 also shows that, if estimated panel by panel, the coefficient of the hours = 40 indicator fluctuates across years. It is negative and close to zero for the 1983 and 1985 panels; it is positive and close to significance for the 1984 panel. Given the standard errors, such values are not mutually incompatible. Still, they leave open the possibility that workers employed 40 hours lose their job more often than those working 39 hours for reasons other than the reduction of the workweek. Table 6 presents estimation results for four pooled regressions. The coefficient on the hours = 40 variable interacted with the 1982 year indicator is equal to 1.3 for the one-year effect and to 2.3 for the two-year effect. Here again, this coefficient is significantly different from zero. Hence, we once more reject the null of no effect of the reduction of the standard workweek on employment. These later estimates lead to slightly lower effects of the reduction of hours on employment losses than before. The total effect is equal to 3.3 percent ($= 2 \times 2.3 - 1.3$) in this case. Therefore, the effect of the reduction is also equal to 3.3 percent. All these results are robust to the introduction of individual heterogeneity in the nuisance parameter.[16]

D. *Low-Wage Workers*

For the data collected since 1982, the size of the sample and the availability of the wage variable allow us to focus on the effects of the reduction of the workweek on various subgroups. We focus on the low-wage population, trying to isolate a specific effect of the hours reduction on this group. Results based on the 1982–84 panel show a specific effect on the low-wage group employed 40 hours (table 6). This effect is present in 1982 but not in 1983 or in 1984. However, the effect on low-wage workers also shows up for 1985 (transitions from 1985 to 1987). If all panels are pooled, we observe a significant effect for the low-wage group that is specific to year 1982 in addition to the common effect. The estimated effect is equal to 7.7 percent after one year (and to 6.8 percent, not significantly different from zero, after two years). From these results, we infer that the reduction first affects low-wage workers, and the rest of the population is also affected after two years. These very large differences in transition rates for minimum-wage workers appear to indicate that firms replaced their low-wage workers with workers from the pool of applicants, consistent with results of the theoretical model, in the year following the decree. This excess destruction and creation of jobs was entirely due to the mandatory rigidity of monthly earnings and not to the July 1981 increase in the SMIC. Indeed, the minimum wage continued to increase by 4 percent in 1982, 1983, and 1984. But the effects that are discussed in tables 5 and 6 are present in 1982, the date at which the constraint on monthly earnings stability was binding.

Having established that low-wage workers were most affected and this effect was present in the first year, 1983, but not in the second year, 1984, following the change, we next examine more formally the possibility that the effects persisted longer for high-wage workers than for low-wage workers. We tested several combinations of restrictions based on the estimation results presented in table 6. All such tests are shown in table 7. The first row shows the unrestricted results, one coefficient for high- and low-wage workers one year and two years after the reduction of hours. Each subsequent row corresponds to a set of constraints.

[16] All such results are presented in app. 3 of Crépon and Kramarz (2000).

TABLE 7

SUMMARY AND TESTS OF POSSIBLE EFFECTS AND CONSTRAINTS

	One-Year Effect		Two-Year Effect		Degrees of Freedom	Test Statistics	*p*-Value
	Low-Wage (1)	High-Wage (2)	Low-Wage (3)	High-Wage (4)	(d) (5)	$\chi^2(d)$ (6)	(7)
Unconstrained	8.34 (4.06)	.64 (.91)	8.64 (5.00)	1.84 (1.22)	...	...	...
	Constrained						
Low-wage effects = high-wage effects	.99 (.88)		2.18 (1.19)		2	3.45	.18
1-year effects = 2-year effects	8.40 (3.99)	.75 (.90)	8.40 (3.99)	.75 (.90)	2	1.78	.41
1-year effects = 0	0	0	1.69 (3.69)	1.25 (.90)	2	4.68	.10
1-year effects (high-wage) = 0	8.31 (4.06)	0	8.62 (5.00)	1.25 (.90)	1	.49	.48
1-year = 0 (high-wage) and 1-year = 2-year (low-wage)	8.37 (3.99)	0	8.37 (3.99)	1.25 (.90)	2	.50	.78
1-year effects = 2-year effects (low-wage)	8.40 (3.99)	.64 (.91)	8.40 (3.99)	1.84 (1.22)	1	.006	.94
	Total Effects ($\pi_1 + \pi_2$) if:						
1-year effects = 2-year effects (low-wage)	8.40 (3.99)	3.03 (1.94)					

NOTE.—All computations are based on table 6 results. Cols. 5–7 present the basis for the test of the restrictions given in the corresponding row.

The test statistics for the validity of the set of constraints are given in columns 5–7. After examination of the various test statistics, the results given in the penultimate row—in which the second-year effect, π_2, for the low-wage workers is constrained to be zero; hence the one-year and the two-year effects are equal for this category, whereas the coefficients for the high-wage workers are unrestricted—appear to summarize nicely the structure of our results. Notice first that the χ^2 for this constraint is lowest among all tested restrictions. More important, the estimated effect for low-wage workers is large, 8.4 percent, and significant. On the other hand, the estimated effect for high-wage workers is only marginally significant. Finally, the last row presents estimates of the total effect of the reduction of the standard workweek, separately for low-wage and for high-wage workers, based on the previous restrictions. Effects of the reduction of the workweek on employment losses are very strong for low-wage workers, whereas effects for better-compensated workers are only marginally significant.

E. Back to the Control Group

All the results above rely on one important assumption, derived from apparently reasonable hypotheses: workers employed 39 hours in 1982 constitute a valid control group for, first, detecting and, second, measuring the impact of the reduction of the workweek. We discuss now the potential problems of this approach.

Obviously, the biggest concern comes from the assumptions of independence, conditional on observable variables, between employment at various dates if affected before April 1982, $NE_i(1)$, and the date at which the reduction was implemented after, sometimes, negotiations with the personnel delegates or the unions, T_{1i} (hypothesis H_A). Indeed, if all firms or sectors in good economic conditions implemented the new standard just before or at the date of publication of the decree whereas firms that implemented the new agreement later on were in worse health, the independence assumption would be violated and our estimates of the effects would all be upward biased. We believe that this situation is very unlikely. First, it is crucial to remember that all our estimates include firm-level variables, in particular the industry or the size of the firm. Furthermore, we attempted to use more detailed industry classification (three digits), with no change in the results. So, if any such problem arises, it comes from variation within the sector. Fortunately, a survey conducted in September and October 1982 by INSEE, the French national statistical institute (Marchand et al. 1983), describes the implementation of the new standard workweek in detail.

Marchand et al. first show that the new standard had not taken effect before February 1 since only 8.4 percent of workers had hours strictly below 40 hours in January 1982 (see their table 1). Then, their survey shows the timing and the diffusion of the new workweek:

> The diffusion of the agreements [on the reduction of the workweek] originated from the industries more accustomed to contractual negotiations and from the large nationalized companies to large industries such as the metal or the construction industries to the less concentrated industries in the trade or service sectors. … Most of the sector-level agreements were extended to all firms in the sector by a decision of the Minister of Labor. [Marchand et al. 1983, p. 4; our translation]

Furthermore, more than 70 percent of firms with more than 500 employees declared that the reduction was implemented by means of a sector-level collective agreement, whereas the proportion is 50 percent for manufacturing firms with fewer than 100 employees and 33 percent for the service and trade sectors.

These results show that the process of implementation of the new workweek was primarily based on industry and firm size considerations. In the early 1980s, the industry was the level at which many collectively negotiated agreements were executed. The reduction of the workweek was no exception. Note, however, that the industries recorded in the LFS do not correspond to the structure of negotiations: some industries negotiated simultaneously, whereas other four-digit industries bargained separately. In particular, these patterns were based on traditions of bargaining (Marchand et al. 1983), and not on a precise schedule vis-à-vis the reduction of the workweek. This gives us our *exogenous* source of random variation in the date of implementation of the new workweek, just before February 1 or just after this date (April, the date of the survey). Hence, the evidence does support our independence hypothesis, conditional on observable variables, *including the industry and the size of the employing firm* (hypothesis H_A).

In addition, one of our hypotheses implies that workers for whom the reduction took place before April and who were still employed at that date constitute a valid control group for our analysis; that is, the selection of the workers kept until April 1982 in firms having implemented the reduction before April is *random,* conditional on observables. On the firm's side, one may view the evidence above as supporting this hypothesis since the state vis-à-vis the reduction, affected before April (1) or affected after April (2), appears to be determined at some sector level with little scope for firm-specific decisions. On the worker's side, we measure most of their individual characteristics sufficiently precisely, and we included most available variables.[17] In particular, we included a firm-specific seniority effect and a nonlinear function of the wage, which captures much of the unobserved heterogeneity that often perturbs such analyses. Furthermore, the group of low-wage workers who appear to be the most affected by the reduction is a much less heterogeneous group than better-compensated workers.

VII. Conclusion

The election of François Mitterrand in 1981 ushered in a 5 percent increase in the French minimum wage in July 1981 and, on February 1, 1982, a mandatory reduction of workweek hours, from 40 to 39, mandatory stability of monthly earnings for minimum-wage workers, and recommendations—largely followed by firms—of stability of monthly earnings for all other workers.

Our two evaluation methods demonstrate that the effects of the reduction of the standard workweek were large. In our first analysis, the one-year effect is a 3.9-percentage-point increase in the probability of making a transition from employment to nonemployment (difference-in-difference estimate) for workers employed 40 hours in 1981 as well for workers employed overtime at this same date. Our second analysis yields a lower bound on this difference in transition rates of 2.3 percentage points. Our two methodologies give similar results even though the data and the assumptions are quite different. However, our estimated effect may seem large for a one-hour decrease and an associated increase in compensation costs of 2.5 percent ($[40 - 39]/40$). It is even larger, 8.4 percentage points, if one focuses on low-wage workers for whom the reduction in hours was associated with monthly pay rigidity. At this point, the reader must note that we measure employment losses of individuals and not job destruction. The two-tiered wage system was shown to be a mechanism that induced firms to both hire and fire. Empirical evaluations of this phenomenon are rare. For France, Abowd, Corbel, and Kramarz (1999) have shown that French establishments that decrease employment by one in any given year do so by hiring three persons and separating four (including all within-year entry and exit). Hence, by applying this ratio of 1/4 to our estimated effects, the associated net job destruction amounts to approximately 2 percent, yielding an elasticity of employment to labor costs just below minus one, a number consistent with recent French evaluations (see Kramarz and Philippon 2001).

Changes in the legal standard workweek led to employment losses,

[17] This is never a problem in this type of analysis since we are not estimating structural parameters of an economic model but the causal effect of a given public policy.

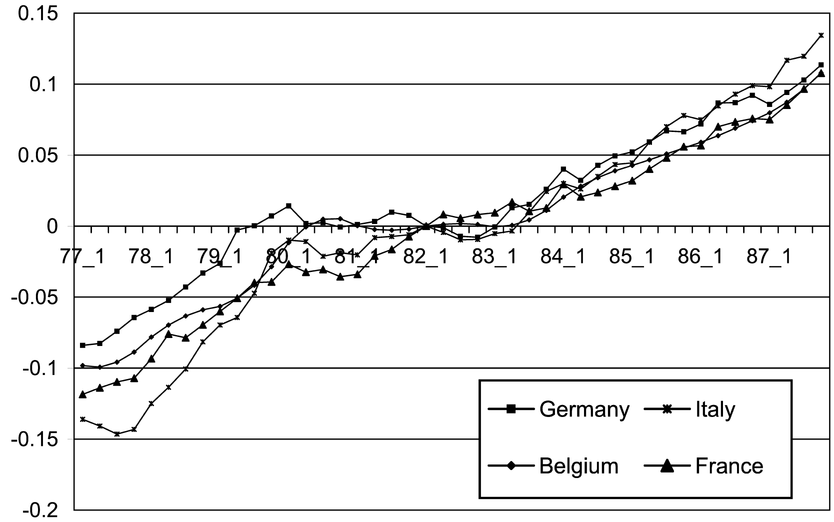

FIG. 1.—Quarterly GDP in France, Germany, Italy, and Belgium (1977–87, in differences from GDP in 1982, first quarter). Sources: OECD for Germany, Italy, and Belgium; quarterly accounts for France.

contrary to the initial goals of these policies. Gains in hourly productivity associated with the reduction of hours appear to have been insufficient to compensate firms for the increase in hourly pay. Furthermore, the policy was implemented at a particularly unfortunate moment. Figure 1 shows (log) gross domestic product for the years surrounding February 1982 for France, Germany, Italy, and Belgium (all expressed as the difference from the level of [log] GDP prevailing during the first quarter of 1982). In 1981, the business cycle in France differed from that of the other three countries. After Mitterrand's election, a Keynesian stimulation of the economy took place. The inflation that followed, aggravated by the increase in the SMIC followed by the reduction of the standard workweek, induced a loss of competitiveness for French firms. The result was a surge in imports. In turn, this induced restrictive policy measures in July 1982. The state of the economy was quite bad in the following years. The French recovery occurred one year after that in the other countries in the European Community. This difficult economic context must be kept in mind to understand our estimates.

Of course, we provide no direct evidence of potential substitution effects, where affected workers would be replaced by more efficient ones. We examine only employment losses, whereas our theoretical model tells us that the reduction in the standard workweek, when associated with monthly pay rigidity, induces both job destruction and creation.

In addition, we do not provide direct evidence on the possible substitution of part-time workers for full-time workers. However, there is no evidence of an increase in the fraction of part-time workers around these dates. Even if employment had remained stable, given the structure of French unemployment (the fraction of long-term unemployed is very large), the employment losses that have their origin in these institutional changes must have had large negative consequences on the affected workers' incomes. In particular, the workers most affected were precisely the minimum-wage workers whom such policies try to protect. Because of the mandatory stability of their monthly compensation, the burden fell almost entirely on minimum-wage workers employed exactly 40 hours in 1982. Given this mandatory stability, the decrease in hours, and the increase in the minimum wage, the most effective cost minimization strategy was to fire some of these workers and hire new ones. Such replacements could still be paid the minimum-wage rate but had monthly earnings that were based on actual hours worked. Our results show that firms did, indeed, follow this strategy.

The reader may legitimately wonder whether these conclusions apply to today's French or, more generally, European situation. Recall that French firms (with more than 20 employees) were required to reduce hours, from 39 to 35, in January 2000. Pending legislation for small firms will depend on the first evaluations of the new policy, the state of the economy, as well as political considerations. Indeed, the length, method, and moment of transition from 39 to 35 hours per week will determine how large firms, as well as smaller ones, accommodate current employees who are affected by the law. Although the 1981 changes were totally unexpected, the current process was fully anticipated. Nevertheless, low-wage workers, more precisely minimum-wage workers, are most likely to be adversely affected by these changes, as their predecessors were, since their monthly earnings were not allowed to decrease, inducing a hike in their effective real hourly wage rate of 11 percent. The extension of differential payroll tax subsidies for low-wage workers may well counteract the potentially major disemployment effects of this law. But the new law may also counteract the beneficial effects on low-wage labor demand due to recent payroll tax changes, as described in Kramarz and Philippon (2001), that provided more employment incentives by allowing an 18 percent decrease in employer-paid social contributions for workers paid the SMIC.[18]

[18] In 1996, employer-paid contributions—health insurance, pensions, etc.—decreased from roughly 40 percent of the wage to 20 percent of the wage.

Appendix

We first present our hypotheses. The first hypothesis (H_A) is the following:

$$H_A : E(NE_i(1)|x_i,\ D_{82i} = 40,\ NE_{82i}(1) = 0,\ T_{2i} = 1) = E(NE_i(1)|x_i,\ D_{82i} = 40,\ NE_{82i}(1) = 0,\ T_{1i} = 1).$$

This hypothesis implies that, *for workers employed 40 hours in 1982,* and conditionally on observable variables, the labor market state associated with being affected before April 1982, $NE_i(1)$, is independent of the date at which the new standard was implemented, T_{1i}.

In addition, we assume—hypothesis H_B—that the effect of the reduction of the workweek is independent of the outcome, 39 or 40 hours, of the negotiation surrounding implementation of the new standard workweek. Under these two hypotheses, the conditional expectation of NE_i is

$$\begin{aligned} E(NE_i|x_i, D_{82i}, NE_{82i} = 0) = {} & E(NE_i(1)|x_i, D_{82i} = 39, NE_{82i}(1) = 0, T_{1i} = 1) \\ & + [E(NE_i(2)|x_i, D_{82i} = 40, NE_{82i}(2) = 0, T_{2i} = 1) \\ & - E(NE_i(1)|x_i, D_{82i} = 40, NE_{82i}(1) = 0, T_{2i} = 1)] \\ & \times P(T_{2i} = 1|x_i, D_{82i} = 40, NE_{82i} = 0)I(D_{82i} = 40). \end{aligned} \tag{A1}$$

The following proposition shows that under these hypotheses, the potential control group (i.e., workers employed 39 hours in 1982) is a valid control group for *detecting* an impact of the workweek reduction on employment.

PROPOSITION 1. Under hypotheses H_A and H_B, when the reduction of the workweek has no effect on the labor market state,

$$NE_{it}(0) = NE_{it}(1) = NE_{it}(2) \quad \forall i \text{ and } \forall t, \quad t = 1982,\ 1983,\ 1984,$$

the following testable restriction holds:

$$E(NE_i|x_i,\ D_{82i},\ NE_{82i} = 0) = E(NE_i|x_i,\ NE_{82i} = 0).$$

Proof. See appendix 2 of Crépon and Kramarz (2000).

Therefore, equation (A1) should not include the term $I(D_{82i} = 40)$ *as soon as* the reduction of the workweek has no employment effect, that is, $NE_i(0) = NE_i(1) = NE_i(2)$.

To measure the effect of the reduction, we first note that equation (5) includes a vector of *parameters of the joint distribution of potential outcomes:*

$$\begin{aligned} & E(NE_i(2)|x_i,\ D_{82i} = 40,\ NE_{82i}(2) = 0,\ T_{2i} = 1) \\ & - E(NE_i(1)|x_i,\ D_{82i} = 40,\ NE_{82i}(1) = 0,\ T_{2i} = 1), \end{aligned}$$

which measures the difference between the effect of the reduction when implemented after April 1982—state 2—and what would have been the effect had the reduction been implemented before April 1982—state 1—and had workers not lost employment between 1981 and 1982, evaluated for individuals for whom the reduction actually took place after April 1982. Equation (5) also includes a *nuisance parameter* $P(T_2 = 1|x_i,\ D_{82} = 40)$.

To go from the parameters above to our *parameters of interest,*

$$\pi_1(x_i) = E(NE_{83i}(2) - NE_{83i}(0)|x_i, D_{82i} = 40, NE_{82i}(2) = 0, T_{2i} = 1),$$

$$\pi_2(x_i) = E(NE_{84i}(2) - NE_{84i}(0)|x_i, D_{82i} = 40, NE_{83i}(2) = 0, T_{2i} = 1),$$

we assume that these effects last only a limited number of periods, two years in our empirical application. Together with three other technical hypotheses (these three hypotheses, denoted H_C, H_D, and H_E, are presented in app. 2 of Crépon and Kramarz [2000]), we are able to recover the parameters of interest of our analysis, $\pi_1(x_i)$ and $\pi_2(x_i)$.

The following proposition shows that under these hypotheses, the potential control group (i.e., workers employed 39 hours in 1982) is a valid control group for *measuring* the impact of the workweek reduction on employment.

PROPOSITION 2. Under hypotheses H_A to H_E, transitions from employment to nonemployment between 1982 and 1983 and between 1982 and 1984 are

$$E(NE_{83i}(2)|x_i, NE_{82i}(2) = 0, T_{2i} = 1) - E(NE_{83i}(1)|x_i, NE_{82i}(1) = 0, T_{2i} = 1)$$
$$= \pi_1(x_i) - \pi_2(x_i),$$
$$E(NE_{84i}(2)|x_i, NE_{82i}(2) = 0, T_{2i} = 1) - E(NE_{84i}(1)|x_i, NE_{82i}(1) = 0, T_{2i} = 1)$$
$$= \pi_1(x_i).$$

Therefore,

$$E(NE_{83i}|x_i, D_{82i}) = g_{83}(x_i) + P(T_{2i} = 1|x_i, D_{82i} = 40)$$
$$\times [\pi_1(x_i) - \pi_2(x_i)]I(D_{82i} = 40),$$
$$E(NE_{84i}|x_i, D_{82i}) = g_{84}(x_i) + P(T_{2i} = 1|x_i, D_{82i} = 40)\pi_1(x_i)I(D_{82i} = 40),$$

where $\pi_k(x_i)$ denotes the change in probability of employment loss that can be directly attributed to the reduction of the workweek for individuals with characteristics x_i in the kth period following negotiations, and $g(\cdot)$ denotes any function ($NE_{84} = 1$ for a worker not employed in 1983 or in 1984 since we consider only workers employed in 1982). The total effect is $\pi_1 + \pi_2$.

Proof. See appendix 2 of Crépon and Kramarz (2000).

Remark.—If the impact lasts one year instead of two, we have $\pi_2 = 0$, a testable restriction.

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