Using Alsace-Moselle Local Laws to Build a Difference-in-Differences Estimation Strategy of the Employment Effects of the 35-Hour Workweek Regulation in France

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France's 1998 implementation of the 35-hour workweek has been one of the greatest regulatory shocks on labor markets. Few studies evaluate the impact of this regulation because of a lack of identification strategies. For historical reasons due to the way Alsace-Moselle was returned to France in 1918, the implementation of France's 35-hour workweek was less stringent in that region than in the rest of the country, which is confirmed by double and triple differences. Yet it shows no significant difference in employment with the rest of France, which casts doubt on the effectiveness of this regulation.

I. Introduction

The experience of working time reduction in France has been one of the most significant regulatory shocks imposed on any large economy.

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In 1998 and 2000, following laws known as Aubry I (which encouraged firms with over 20 employees to reduce working time) and Aubry II (which made working time reduction mandatory), France changed its working time regulation from the official 39 hours to 35 hours. This 10% reduction was implemented with no change to the net monthly wages of workers who were employed at this time. The implementation of this regulation constitutes a relatively far-reaching and unique experiment from which much can be learned about the underlying functioning of labor markets in general.[1] The experience also generated considerable debate in several European countries.

Despite the importance of this shock and the political controversy surrounding it, there have been few studies based on microeconomic evidence that evaluate the impact of a working time reduction in France. In our view, this is primarily due to the absence of proper identification strategies. Unlike the United States or Canada, France lacks a federal structure; therefore, most laws and decrees apply to the entire territory, which makes very problematic the identification of appropriate control groups involving firms or workers unaffected by the experiment. Moreover, a reduction in working time (hereafter RWT) has been accompanied by payroll tax exemptions, smoothing of hours over the year (*annualisation* in French), and a number of specific collective labor agreements that, together, add to the difficulty of disentangling the specific effect of RWT.

The goal of this paper is to provide a methodology that overcomes this identification problem by making use of a relatively unknown French specificity. France's territorial organization is less centralized and homogeneous than is generally assumed. For example, for historical reasons, the region of Alsace and the subregion, hereafter named *département*, of Moselle have laws that differ from those of the rest of the republic. Both areas belonged to Germany from 1870 to 1918 and, upon rejoining France, retained some favorable elements of the German legal system. In particular, two holidays that are unrecognized elsewhere in the country are preserved in these areas: *Saint-Etienne* (Saint Stephen's Day, December 26) and *Vendredi Saint* (Good Friday). However, when the RWT took effect in 1998 and 2000, firms in Alsace-Moselle decided that both holidays would be counted as part of the working time reduction. Therefore, the application of the RWT has been less favorable in Alsace-Moselle than in the rest of France, at least until employee recourses began to be examined by various legal courts.

[1] Despite the official obligation that wages not be cut in response to a working time reduction, theory indicates that newly hired workers must have faced a decline in monthly wages, attenuating the law's impact. Similarly, fringe benefits to already employed workers may have been reneged following the law's application in order to restore hourly wages.

This is the basis for our identification strategy. We will compare the evolution of hours, employment, and wages in France (by which we mean France without Alsace-Moselle) and in Alsace-Moselle. Using data from *Enquête Emploi*, a French labor force survey from 1996 to 2003, we are able to use a standard difference-in-differences approach and investigate the differential impact of RWT in France and in Alsace-Moselle.

However, this is not as straightforward as it seems: Alsace and Moselle have the particular distinction of being the only areas in France that share a border with Germany. This is a serious issue in our identification process: during the period under investigation, Germany faced a relatively strong recession that threatened to spill over into Alsace-Moselle. Thus, a simple comparison of these areas with the rest of France is likely to be spurious. In particular, it is possible that a rise in relative unemployment in Alsace-Moselle could simply be the result of Germany's recession disproportionately affecting northeastern France.

For this reason, we will mostly present triple difference (DDD) estimates, wherein the additional reference groups will consist of firms or occupations unaffected by the RWT (i.e., firms of fewer than 20 employees or independent workers). At this point, it is interesting to note that by using a DDD approach, we find that working hours in Alsace-Moselle rose relative to the rest of France by approximately the amount predicted by theory, despite the fact that Germany was concurrently experiencing a recession. This makes us reasonably confident that the difference in the number of working hours in Alsace-Moselle is exogenous to the German economic cycle and is instead attributable to legislative differences within France.

Our paper is organized into six sections. Section II presents France's RWT experiment and a selective literature review of ex post evaluations based on either microeconomic data or macroeconomic models. All such existing studies have ignored the France/Alsace-Moselle divide. We will then discuss the specificity of the Alsace-Moselle experiment. Section III presents a model with which we discuss a number of econometric issues, such as the selection of firms that entered into early RWT agreements. Section IV provides greater detail of our identification strategy. Since the existence of regional differences is typically ignored in examinations of French data, we devote Section V to providing detailed evidence that the strategy is valid. In particular, we show that workers in Alsace-Moselle worked more relative to the rest of France: the DDD coefficients are positive and significant over the relevant period. Further, the theoretical coefficient (16 hours per year, i.e., 0.35 hour per week worked) is always in the confidence interval. In Section VI, we employ the identification strategy to measure the effect of RWT on employment probability and unemployment incidence. We observe that the differential application of

the 35-hour law across regions did not have any relative employment impact.

II. Reduction in Working Time

A. 35 Hours: The Law

From a legal viewpoint, France's switch from a 39-hour to a 35-hour workweek in 2000 was a complex process, implemented after a lengthy negotiation process and several litigations. Two laws were proposed by the Jospin government, a coalition of socialists, communists, and the Green Party, and were voted on in Parliament. The first, on June 13, 1998 (known as the Loi Aubry I), was designed to provide firms with strong incentives to negotiate working time reductions of at least 10% at the firm or industry level. Incentives for such reductions were numerous. Foremost, firms obtained a subsidy for each worker in the firm if they raised employment by at least 6% following a decrease in working time and an even larger subsidy for an employment increase over 9%. This subsidy was quite large (between €800 and €1,500 per worker) and degressive over time. It was not applicable to agreements signed after the second semester of 1999 for firms with over 20 employees (a delay of 1–2 subsequent years was granted to smaller firms) in order to speed up the transition to the new legal working time. Finally, as an incentive for firms to reduce working time promptly, the Loi Aubry I specified that in 2000 (for firms with over 20 employees) or in 2002 (for smaller firms), the 35-hour workweek both would be irreversible and would be uniformly applied to all firms, even those not having signed an agreement. The second law, passed January 19, 2000 (known as the Loi Aubry II), enforced the Loi Aubry I by setting the official working time at 35 hours per week in all firms, modifying all relevant articles of the Code du Travail.

A decrease in working time from 39 hours to 35 hours represents an 11% reduction in hours worked per week. However, this does not necessarily imply that, at a fixed weekly wage and in the absence of subsidies, firms faced an 11% increase in hourly labor costs. Firms also had various adjustment mechanisms. For example, one such mechanism was overtime. Before the reform, firms were required to pay a compulsory overtime premium of 25% for the first 8 hours over 39 and then a 50% premium from then onward. Following the reform, the activation point for the overtime premium was shifted to 35 hours, and a second activation point was introduced at 43 hours. Figure 1 represents the wage profile before and after the 2000 reform in firms of more than 20 employees until 2003 (when the overtime premium was eventually reduced; see below).

For hourly wages earned by employees in these firms working 39 hours before and after the reform, labor costs increased by $(4 \times 0.25)/39 = 2.5\%$. For employees working 43 hours before and after the reform, labor

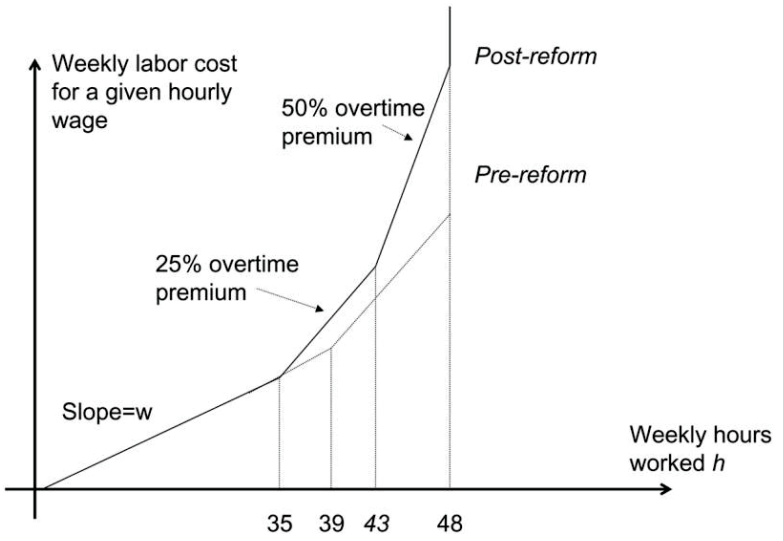

FIG. 1.—Weekly wage profile before and after the reform in firms with more than 20 employees. The slope of the wage is the base hourly wage (denoted by ω for convenience) before the overtime cutoff point (39h or 35h), $\omega \times 1.25$ between the cutoff point and the second cutoff point (48h or 43h), and $\omega \times 1.50$ between 43h and 48h in the postreform period.

costs increased by $(0.25 \times 8)/43 = 4.7\%$. Finally, for employees working 45 hours before and after the reform, the increase in labor costs amounted to $(0.25 \times 8 + 0.5 \times 2)/45 = 6.7\%$. In short, the reform made overtime more costly, especially overtime exceeding 43 hours.

The two laws have been widely debated. For example, the Conseil Constitutionnel (France's equivalent of the Supreme Court) nullified several items of the Aubry II law. There were also many different interpretations of how vacation days should be calculated, how overtime should be paid, and how collective agreements could bypass the law. As late as October 2006, more than $6\frac{1}{2}$ years after the reform, the Conseil d'Etat (another Supreme Court devoted to litigations in the public sector) invalidated a collective agreement in the public health sector as a result of a conflict with the main law.

Jospin's left-wing coalition was defeated in the 2002 presidential elections. In 2003 and 2004, the new right-wing Parliament voted for two additional laws that imposed constraints on overtime wages. In particular, the overtime costs were reduced from 25% to 10% in 2003; during the following year, the legal working time (1,600 annual hours) was augmented to 1,607 hours with an additional day of work for all French firms and administrations (usually on Pentecost) in order to finance health spending for the elderly.

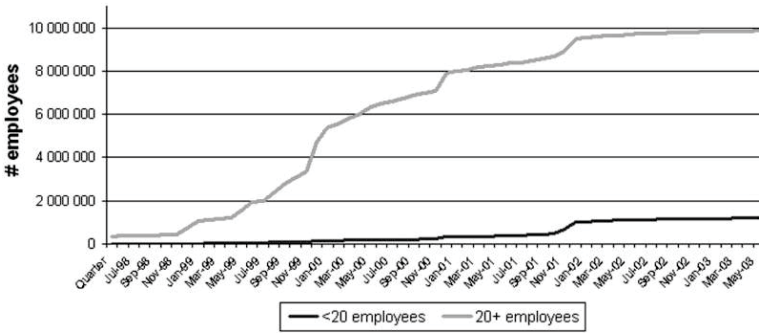

FIG. 2.—Timing of the 35-hour reform for firms with greater than and less than 20 employees.

In short, the timing of the RWT was not simple and can be broken down as follows:

1. Between 1998 and 2000, incentives were provided in order to reduce working time by 10% or greater (e.g., subsidy increases in cases in which working time was reduced by 15% and employment expanded by 9%; subsidies were also given to firms that claimed that they would be required to fire workers in the absence of an agreement on working time reduction; finally, after a period of time, all subsidies would disappear, giving incentives to reduce working time as early as possible following the first law).
2. In 2000, the RWT was almost uniformly enforced by law throughout the territory. It was applied differently according to activity sectors and depending on renegotiations with the various *conventions de branches* and the timing of the agreement.
3. In 2003–4, recourse to overtime was eased and its cost decreased.

The number of firms and employees that switched to the 35-hour regime was recorded on a monthly basis by the administration. Here we report data that show the progressive switch for larger firms between 1998 and 2000, the sudden increase in 2000, and the same process, but with a lag of 2 years, for firms with fewer than 20 employees. Note that the firms with fewer than 10 employees have not yet been subject to any working time reduction. Figures 2 and 3 provide an overview of the timing of the change for the firms with more than and fewer than 20 employees (fig. 3 uses a logarithmic scale).

B. A Selective Literature Review

The literature on work sharing is vast and has typically established a negative employment effect (Rosen 1968; Ehrenberg 1971; Calmfors and

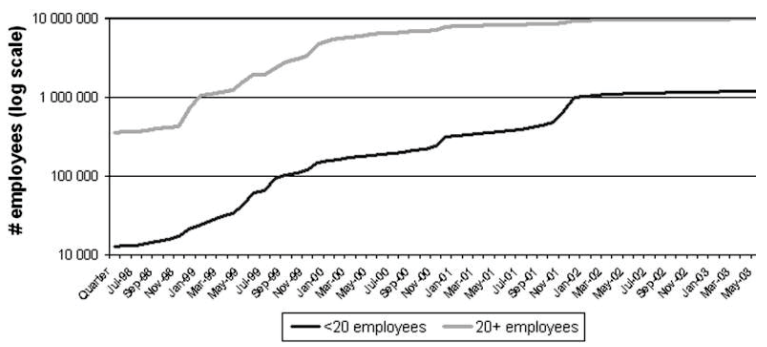

FIG. 3.—Timing of the 35-hour reform for firms with greater than and less than 20 employees (logarithmic scale).

Hoel 1988; Hunt 1999; Crépon and Kramarz 2002; see also Marimon and Zilibotti [2000] for a detailed theoretical analysis). The purpose of this paper is not to systematically survey the theoretical and empirical arguments for work sharing. Rather, we aim to survey various evaluations of the French experiment. The three most comprehensive surveys on the 35-hour experience are completed by Cahuc (2001), Askenazy (2005), and Kramarz et al. (2008). Cahuc and Askenazy classify the various available estimates of the 35-hour experience into two categories: approaches based on macroeconomic models and approaches based on microeconometric evidence. Those based on macroeconomic models depend on Keynesian effects. Their estimates of the employment effect typically depend on time-series evaluations and have been severely criticized. Further, both authors argue that microeconomic data are the most informative and may be the only rigorous methodology to employ in such a context. The objective is to compare the employment evolution of firms that switched to the 35-hour regime (treatment group) with that of firms that did not switch (control group). Unfortunately, it is likely that the treatment of the treated group differs from the treatment of the control group; thus, the key difficulty consists in dealing with the endogeneity of the agreement at the firm level on the 35-hour regime. Matching methods are a potentially powerful methodology with which to build a control group; however, as explained by Askenazy (2005), these methods account for only selection in a 35-hour agreement based on observables. This raises the question as to why some similar firms selected into the 35-hour reform but others did not.

In the latter survey by Kramarz et al. (2008), the authors explore the impact of the 35-hour reform with a similar methodology. They recognize that their analysis precludes causality since it is difficult to find a model that explains when and why firms decided to switch to the 35-hour regime

and sign agreements with their workers. Crépon, Leclair, and Roux (2004) attempt to find an instrumental variable that would affect the decision to reduce working time without having an effect on economic outcomes. They use the predicted amount of Aubry II subsidies that firms would have obtained per worker had they switched to the 35-hour workweek. However, the source of variation in the amount of Aubry II subsidies per worker across firms is correlated with the share of low- and high-wage workers. Comparing firms with more or less Aubry II subsidies may be equivalent to comparing firms with more or fewer low-wage workers. Firms with more or fewer low-wage workers differ systematically in unobservable ways. For example, one might argue that firms with a great number of low-wage workers are more likely to be unionized and to switch to the 35-hour reform. Additionally, a considerable amount of economic literature argues that unions have an independent impact on employment and productivity. Therefore, the amount of Aubry II subsidies a firm receives per worker may not be exogenous and then may not be a valid instrumental variable, although the direction of the bias is unclear.

An alternative strategy was implemented in a recent paper by Estevão and Sa (2006). They investigate the impact of a 35-hour workweek on several dimensions (stress, dual job holding, share of employment in firms, average pay of newly hired workers, employment) by exploiting the distinction between small and large firms regarding the timing of the implementation of the reform. As we argue in the paper, their methodology is a useful gateway for future research since it considerably improves on previous methodologies that were based solely on macroeconomic models and data. However, in order to draw conclusions on the impact of a working time reduction on the economy and to infer causality, their identification strategy also requires that the time effects for small and large firms are identical over the period of interest and that the responses of small and large firms to a regulatory shock on hours are the same. In our paper, we nest this identification strategy in offering triple differences. In addition to the previous methodology, we compare large firms affected by the 35-hour workweek with large firms affected in a milder way by the regulation (i.e., large firms in Alsace-Moselle). Compared to the methodology employed by Estevão and Sa, we do not focus on the various dimensions (stress, dual job holding, etc.), although our methodology can easily be adapted to address those questions.

III. A Model of Working Time Reduction

A. Setup

Our goal in this section is to provide a simple analytical model of a working time reduction applicable to the French case. In particular, our

setup includes both extra overtime costs for employers (weekly wages are convex in hours) and overtime costs affected by legislation changes. Other aspects of working hour regulations, such as the impact of labor costs on prices and on subsequent demand for produced goods, are ignored. In order to provide the simplest illustration of the empirical challenge facing most evaluations of the 35-hour law, we take a proponent's viewpoint and assume the most favorable case in which firms face a fixed demand for their output and, thus, must supply a fixed number of hours, denoted by H. It would be very easy to relax such an assumption and assume that H depends negatively on the cost of labor due to the elasticity of the demand for goods. Richer specifications can be found in Calmfors and Hoel (1988).

We denote by h the number of hours per worker and by N employment in a given firm. Thus, the firm chooses hours and employment subject to

$$hN = H.$$

The firm's objective is to minimize its total production cost $C(h, N)$. To account for the fact that employment and hours are not perfect substitutes, this cost can be broken down as follows:

$$C(h, N) = w(h)N + \delta_j N,$$

where δ_j is the cost per worker in firm j (which could be interpreted as a coordination cost), and the function $w(h)$ is an increasing, convex weekly wage profile. Denote by $\varepsilon(h) = hw'(h)/w(h) \geq 1$ the elasticity of the wage profile to wages.

Different firms may have different workplace organization and hence a larger or a smaller δ_j. Replacing h by H/N in the cost function, we have the objective of the firm as follows:

$$\min_N C(H/N, N) = \min_N w(H/N)N + \delta_j N,$$

which leads to a simple first-order condition:

$$w(h)(\varepsilon - 1) = \delta_j.$$

Figure 4 illustrates the determination of the equilibrium hours as the intersection between the curve $w(h)(\varepsilon - 1)$ and the horizontal line δ_j in a point h_A. The higher the cost per employee, the higher the choice of hours h_A and thus the lower employment $N_A = H/h_A$. As figure 4 further illustrates, a reduction in legal working time that makes overtime more costly will raise the wage profile w to w^+ after a given threshold point h_0, which can be thought of as the new legal working time, for example, $35h$. With this new wage profile, overtime above the legal threshold is

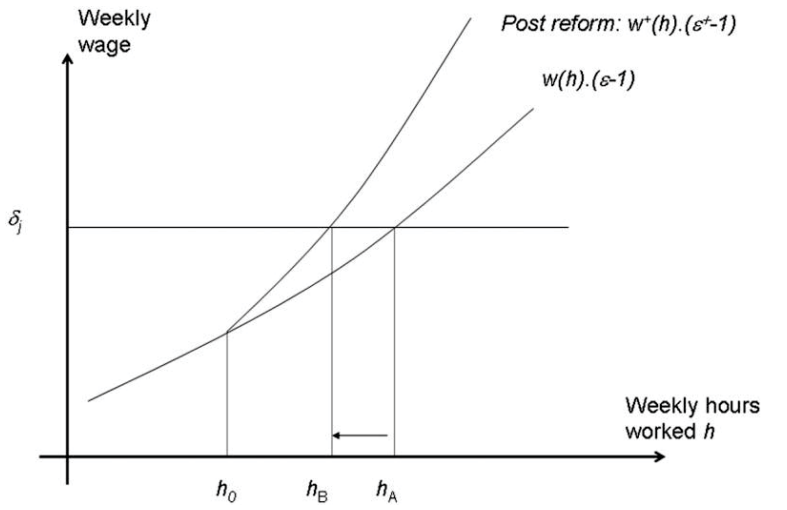

FIG. 4.—Equilibrium determination of hours worked per employee before and after the reform, in the absence of other government intervention.

more costly, and thus the firm optimally raises employment and reduces hours worked, to a point h_B.

However, the employment effect may not be large enough. First, even if we assume that H does not vary with the increase in labor costs, h_B may still be above the target h_0. Second, the total demand for hours at the firm level may decline: if labor costs raise production prices and the demand for goods is elastic, H itself will decline. This is why, in the process of reducing working time, the government added a subsidy to firms that was approximately proportional to hirings. Thus the cost function of the firm is augmented by a (negative) term $-(a + SN)$, where S is the per-worker subsidy and a is a possibly lump-sum transfer. After taking the first-order conditions, this subsidy simply moves the horizontal line (cost per employee) to a lower level (see fig. 5).

In summary, (1) even with a constant volume of hours H, the employment effect can be lower than expected if firms prefer to choose an intermediate level of hours (e.g., a number between $h_0 = 35$ and $h_A = 39$). (2) The value of H itself may decrease as a result of the rise in labor costs. In this case, the government needs to subsidize employment with a positive S per employee.

B. Other Issues: Heterogeneity of Workers and Self-Selection of Firms

It is possible that workers are heterogeneous in terms of their preferences for leisure. Here we simply modeled the labor demand side, but there might be interesting effects of the reform on the supply side as well.

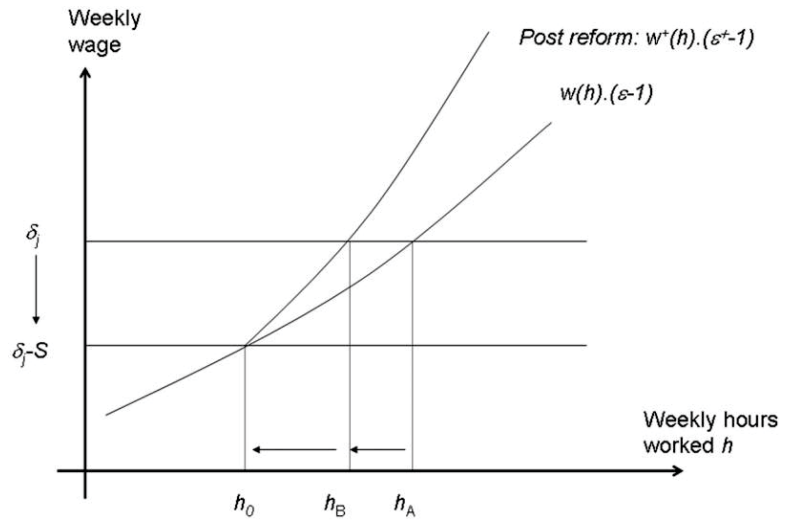

FIG. 5.—Equilibrium determination of hours worked per employee after the reform and with government subsidies *S*.

In particular, in a competitive market, firms and workers would sort according to preferences: workers preferring low work hours would go to firms offering lower work hours and, similarly, for those workers preferring a greater number of work hours. However, by later investigating the sample of only full-time workers, we limit the importance of variations in workers' preferences. Therefore, in a first order, our model without workers' heterogeneity captures the actual effects in the data. Of course, it may also be the case that workers within the full-time group also have different preferences. Some may want to move to a 35-hour workweek, whereas others may prefer to maintain their 39-hour workweek. Our view is that the preferences over working time for firms and for workers are subject to an externality: if everyone works 39 hours, it is more costly for the firm to deviate for one worker. In this way, firms do not want to have too many different contracts with different hours worked. With such an externality, the possibility of a "compensating differential" of different working hours in different firms is limited in practice. Similarly, if everyone works 35 hours, it may be more difficult for a worker to be the only one to work 39 hours. Burda and Weil (2005) have a nice theory that addresses such externalities and implications for the regulation of working time.[2]

[2] In a model with heterogeneous workers, firms would indeed fix different hours, corresponding to a tangency point between iso-utility, a concave curve in the space (w, h), and an iso-cost line, a convex curve in that space. A legal reduction in working hours again makes the iso-cost curve more convex since $w(h)$ is more curved. Hence, optimal hours are reduced for each given worker. The main message of the model is therefore unchanged. The net effect of the reform on hours may be lower than in the absence of workers' heterogeneity, but this is not necessarily always the case.

The model also points out some dimensions of heterogeneity at the firm level, illustrated by the fact that δ_j depends on the firm. This simply means that firms have a different hours-employment trade-off and, thus, react differently to changes in overtime regulations. For the employment impact of a policy change to be the same across all firms despite different δ_j, we require that the profile $w(h)(\varepsilon - 1)$ be linear, a condition unlikely to be met. As we will discuss below, our identification strategy is immune to these problems.

IV. Identification Strategy: The 35-Hour Reform in Alsace-Moselle

A. Details of the Alsace-Moselle Experience

As noted earlier, the implementation of the 35-hour reform was, to a large extent, nationwide, making the identification of the causal effect difficult at the macroeconomic level. Simply comparing unemployment rates before and after the reform would confound the impact of the reform with the ongoing macroeconomic trend. In contrast, an identification based on regional differences is able to capture the reform's causal effect.

A historical accident provides a regional difference in the implementation of the reform between Alsace-Moselle and the rest of France. Figure 6 represents France, where Alsace-Moselle corresponds to three *départements* in the northeastern part of France, those with numbers 67, 68 (Haut-Rhin and Bas-Rhin), and 57 (Moselle). As noted previously, Alsace-Moselle has had two supplementary public holidays (December 26 and Good Friday) in effect since the adoption of the German legal code in 1890. Insofar as they were opportunistically converted by employers into the RWT, the amount of work supplied in firms decreased less in Alsace-Moselle than elsewhere.

There is clear anecdotal evidence confirming this phenomenon. For example, Laurence Grisey-Martinez, a lawyer at the Institut du Droit Local Alsacien-Mosellan, an institute in charge of raising awareness about the specificities of local Alsace-Moselle laws, writes about this phenomenon in the *Revue du Droit Local* (no. 44, June 2005). In this instance, a case was brought to the attention of the courts in 2002 in which the letters RTT (the French translation of RWT) were written on the calendar of a firm for the following December 26.

The employees, having been stripped of an "RWT day," questioned the legitimacy of employers "using" the RWT on a day that is, according to Alsace-Moselle's local laws, deemed a statutory holiday. Finally, on Oc-

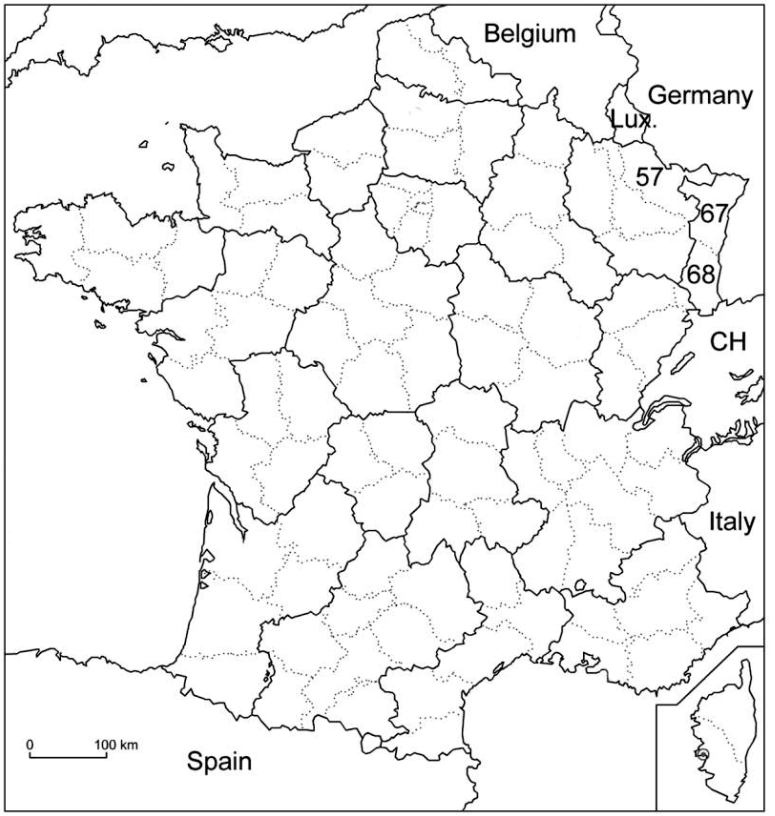

FIG. 6.—Map of the 95 French *départements*. Alsace-Moselle are *départements* numbers 57, 67, and 68 in the northeastern part of France.

tober 23, 2002, the local council (called *prud'hommes de Metz*) stated that "December 26 must be considered a bank holiday as per the special dispositions of the local laws in Alsace-Moselle, this day cannot be counted as a reduction of working time (RWT)" (Grisey-Martinez 2005, 2).

This is the basis of our identification strategy: between 2000 and 2002, some employers in Alsace-Moselle attempted to attenuate the impact of RWT by including holidays as part of the reduction in working time, thereby attempting to integrate them into the less favorable regime of France's common law, Code du Travail. As these two additional holidays are not mentioned in the Code du Travail, this was a relatively easy task for Alsace-Moselle employers, at least up until the local council established a jurisprudence.

It follows that we should observe a weaker impact of the 35-hour reform in Alsace-Moselle: 2 days represent 16 hours of work per year. The 35-hour reform corresponds to a reduction of 4 hours per week throughout the 46-week work year, for a total reduction of 184 hours. Therefore, there is a 9% variation in the impact of the 35-hour reform in Alsace-Moselle as opposed to the rest of France. As 16 hours per year correspond to 0.35 hour per week, workers in Alsace-Moselle were expected to work 0.35 hour more than workers in the rest of France in 2001 relative to 1999.

Also note that one expects to find two different types of firms and workers in the data: those with a strict "per-week" application of the 35-hour reform and those with an annualization of the hours worked. For those with a strict per-week application of the reform, we expect workers in Alsace-Moselle to work the same number of hours as in the rest of France every week except on the two relevant weeks (December 26 and Good Friday) and except in 2001–2. For those who tabulate hours annually, we expect to observe fewer hours worked in Alsace-Moselle for all weeks on average except in 2001–2. Therefore, to account for the disparity between these two groups, the best measure of hours to be considered is the usual number of hours worked. An alternative measure (number of hours worked last week) will be a more noisy measure.

Finally, note that, given that firms were receiving subsidies, our paper is about the effect of the combination of 35-hour reform and the subsidies, like most previous empirical studies of the 35-hour reform. However, subsidies in both France and Alsace-Moselle were calculated in the same way. Therefore, a difference-in-differences approach cancels out the effect of subsidies and measures only the differential application of the reduction in working time. This is an additional justification of our strategy.[3]

B. The Pros and Cons of This Identification Strategy

In previous sections of this paper, we extensively discussed the various firm-level selection issues that make the evaluation more difficult. However, our difference-in-differences approach addresses these concerns by comparing firms that switched to the 35-hour regime in Alsace-Moselle to firms that switched regimes in the rest of France. Therefore, the only difference between these firms is that, for historical reasons, the 35-hour reform has been implemented in a milder way in Alsace-Moselle. Of course, firms in Alsace-Moselle might have a systematically different δ_j compared to firms in France, but the outcomes of firms in Alsace-Moselle are differenced before and after the reform so as to preclude any of these systematic differences. Another problematic assumption with a difference-in-differences approach is the "common time effects" assumption: in the absence of the reform, firms in Alsace-Moselle may have evolved differently compared to firms in the rest of France. To address this concern,

[3] We thank a referee for this point.

we will present DDD estimates, wherein the additional reference groups will be based on those occupations or firms unaffected by the RWT (i.e., independent workers or firms with fewer than 20 employees).

A last concern is that our identification strategy is based on an impact that has thus far not been documented, namely, the differential application of the 35-hour reform in a particular region of France. This is why we now check that the shock could indeed be detected in hours actually worked.

V. Robustness and Falsification of the Identification Method

A. Sample

Our sample is based on France's Labor Force Survey, or *Enquête Emploi*. We used the annual representative cross sections of the population between 1996 and 2002, to which we added data from the 2003 cross section. The latter has a different design but similar questions, thus making it compatible with the previous data waves. As focusing solely on the data for the years 1996–2002 did not change our results (available on request), we present only the results obtained from the most exhaustive data set of 1996–2003.

Appendix table A1 reports summary statistics. It is also important to note that, in contrast to other countries in which common law applies and contracts are less specific, French labor laws require labor contracts to be very explicit regarding the hours, pay, and tasks performed by employees. As a consequence, when they are interviewed, employees have precise knowledge regarding the number of hours they usually work and the number of hours they worked during the previous week.[4]

B. First Check: A Double Difference Approach

For this first check, a simple difference-in-differences (DD) strategy is used according to the following framework:

[4] We thank John Abowd for making this point to us. In confirmation of this notion, we examined the survey from 2003, in which the data are continuous throughout the entire year, whereas in earlier years, interviews were conducted in March. In 2003, we found that workers reported having worked 27.43 hours on average during the previous week in Alsace-Moselle when that week included either Good Friday or Saint Stephen's Day. In those same weeks, they reported 35.48 hours on average during that same week for workers in the rest of France. This is reported in app. table A1. These results are indicative of the precision of hours reporting by employees: the reported difference in this example is exactly 8 hours, or an average working day.

$$\begin{aligned}\text{hours}_{ijt} = {} & \alpha_j + \beta_t + \gamma_1(\text{Alsace-Moselle}) \times (2003)_{ijt} \\ & + \gamma_2(\text{Alsace-Moselle}) \times (2001\text{–}2)_{ijt} \\ & + \gamma_3(\text{Alsace-Moselle}) \times (1999\text{–}2000)_{ijt} \\ & + \theta X_{ijt} + \eta \text{job}_{ijt} + u_{it}, \end{aligned} \tag{1}$$

where i corresponds to individual i, j to department j, and t to year t; the dependent variable hours_{ijt} is the number of hours usually worked per week by individual i (the sample is restricted to employed full-time individuals);[5] α_j are department fixed effects (95); β_t are year fixed effects (seven); $(\text{Alsace-Moselle}) \times (2003)_{ijt}$ is a variable that takes the value one if individual i works in Alsace-Moselle and is interviewed in 2003; $(\text{Alsace-Moselle}) \times (2001\text{–}2)_{ijt}$ is a variable that takes the value one if individual i works in Alsace-Moselle and is interviewed in 2001 or 2002; and $(\text{Alsace-Moselle}) \times (1999\text{–}2000)_{ijt}$ is a variable that takes the value one if individual i works in Alsace-Moselle and is interviewed in 1999 or 2000. Therefore, the reference period is between 1996 and 1998. The coefficient of interest is γ_2, which measures the relative increase in hours worked by individuals in Alsace-Moselle after the reform. If the identification strategy is correct, this coefficient should be equal to 0.35, as stated above: 2 days, or 16 hours, per year correspond to 0.35 hour per week over the year. A coefficient γ_1 not significantly different from zero would be consistent with the fact that the 2002 decision by the local council (*prud'homme de Metz*) effectively forbade the practice of converting public holidays into RWT days and effectively canceled the regional disparity in the implementation of the 35-hour reform. The introduction of the variable $(\text{Alsace-Moselle}) \times (1999\text{–}2000)_{ijt}$ allows us to test the common time effects assumption. This assumption states that treated individuals, had they not been treated, should evolve in the same way as nontreated individuals. A coefficient γ_3 not significantly different from zero means that prior to the reform there was no significant difference in the evolution of individuals within and outside of Alsace-Moselle, confirming the common time effects assumption. Additionally, 14 control variables (five age dummies, sex, size of the household, and seven diploma dummies) and 30 occupation fixed effects are included in the analysis.

[5] We considered only full-time workers because the theoretical effect of an increase of 0.35 hour for Alsace-Moselle workers is calculated for full-time workers. There are 1.4 million individuals in the database. Of these, only 627,820 individuals are employed, with 466,742 employed as full-time workers. There are 111,215 part-time workers; a further 49,863 individuals have an unreported work duration and were thus dropped from the analysis. We repeated all analyses with the full sample of full-time and part-time workers, and results do not vary, probably because of the low number of part-time workers.

Standard errors are clustered at the level of the department to take into account issues of serial correlation within a department, the unit at which the reform is implemented (Moulton 1990) that may arise in a DD estimation (Bertrand, Duflo, and Mullainathan 2004). Conley and Taber (forthcoming) further discuss the issues arising in a DD estimation when the treated group and the number of policy changes are small. They show that the difference between the estimated and true coefficients is equal to the difference of the sum of the disturbance terms for the treated unit before and after the implementation of the policy. This may not tend toward zero when the treated group is small, especially if there are time-varying department-specific effects from 1996 to 2003, the time period of our sample.[6] In addition to clustering at the level of the department, we also apply the Conley-Taber methodology to find confidence intervals for our results. This methodology basically builds a confidence interval for the DD coefficient by estimating the empirical distribution, in the control group, of the difference of the sum of the disturbance terms for the unit before and after the implementation of the policy. We will systematically report the confidence intervals in the text.

Table 1 presents the results of this DD approach. In column 1, the sample is restricted to workers in occupations affected by the 35-hour reform. Indeed, not all occupations are affected by the regulation: typically self-employed workers were left unaffected by the 35-hour law. The results are fairly similar to what we expected: γ_2, the coefficient in front of (Alsace-Moselle) × $(2001\text{–}2)_{ijt}$, is equal to 0.38 and is statistically significant. Note that this corresponds closely to the predicted value of 0.35 (see the *p*-value in table 1). This means that affected workers in Alsace-

[6] Note that we will mostly present triple differences, which control for the presence of time-varying department-specific effects from 1996 to 2003. Additionally, we cluster the standard errors at a finer level, the sampling stratum, i.e., the well-defined neighborhoods in which all households are interviewed. There are 238 such clusters in Alsace-Moselle. Results do not change and are available on request. The occupations affected by the 35-hour reform are cadres de la fonction publique; professeurs; professions scientifiques; professions de l'information, des arts, et des spectacles; cadres administratifs et commerciaux d'entreprises; ingénieurs et cadres techniques d'entreprises; instituteurs et assimilés; professions intermédiaires de la santé et du travail social; professions intermédiaires administratives de la fonction publique; professions intermédiaires administratives et commerciales des entreprises; techniciens; contrematres; agents de matrise; employés civils et agents de service de la fonction publique; policiers et militaires; employés administratifs d'entreprises; employés de commerce; ouvriers qualifiés industriels; ouvriers qualifiés de type artisanal; chauffeurs; ouvriers qualifiés de la manutention, du magasinage, et du transport; ouvriers non qualifiés de type industriel; ouvriers non qualifiés de type artisanal; and ouvriers agricoles. The occupations unaffected by the 35-hour reform are agriculteurs, artisans, commerçants et assimilés, chefs d'entreprise de 10 salariés ou plus; and professions libérales.

Table 1
Impact of the Milder Version of the 35-Hour Reform in Alsace-Moselle on Hours Worked: Dependent Variable: Hours Usually Worked per Week

	Sample						
	Occupations Affected (DD) (1)	Occupations Unaffected (DD) (2)	Full (DDD: 1–2) (3)	Affected Individuals (DD) (4)	Unaffected Individuals (DD) (5)	Full (DDD: 4–5) (6)	Affected Individuals (DD) (7)
(Alsace-Moselle) × (2003)	−.2566	−1.0317	−1.0204	−.7025	−.4026	−.4055	−.6218
	(.23)	(.45)**	(.48)**	(.68)	(.20)**	(.20)**	(.67)
(Alsace-Moselle) × (2001–2)	**.3805**	−.4308	−.3675	**.9194**	.1523	.1513	**.9608**
	(.19)*	(.24)*	(.28)	**(.40)****	(.18)	(.18)	**(.40)****
(Alsace-Moselle) × (1999–2000)	.1427	−.1941	−.1593	.2142	.0735	.0726	.1747
	(.17)	(.65)	(.66)	(.11)*	(.29)	(.29)	(.12)
(Alsace-Moselle) × (2003) × (affected individuals)			.7717			−.2707	
			(.52)			(.66)	
(Alsace-Moselle) × (2001–2) × (affected individuals)			**.7538**			**.7512**	
			(.22)***			**(.27)*****	
(Alsace-Moselle) × (1999–2000) × (affected individuals)			.3084			.1638	
			(.51)			(.23)	
Border × GDP growth							5.2040
Department fixed effects (95)	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Year fixed effects (7)	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Control variables (14)	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Occupation fixed effects (30)	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year × (affected individuals dummy) fixed effects	No	No	Yes	No	No	Yes	No
Department × (affected individuals dummy) fixed effects	No	No	Yes	No	No	Yes	No
Occupation × (affected individuals dummy) fixed effects	No	No	Yes	No	No	Yes	No
p-value of the t-test coefficient of interest = .35	.84		.28	.06		.11	.06
Observations	370,473	88,074	458,547	53,285	405,262	458,547	53,286
R^2	.17	.41	.36	.20	.36	.36	.21

Note.—Ordinary least squares results. Robust standard errors are in parentheses, clustered at the level of department. We also report in the text the Conley-Taber confidence intervals to correct for the small number of treated departments. The dependent variable in all columns is the number of hours usually worked per week. In col. 1, the sample is restricted to occupations affected by the 35-hour reform (see n. 6 for the list of such occupations). In col. 2, the sample is restricted to occupations unaffected by the 35-hour reform (see n. 6 for the list of such occupations). In col. 3, the coefficient of interest is (Alsace-Moselle) × (2001–2) × (affected individuals), where affected individuals is a dichotomous variable equal to one if the individual has an occupation affected by the 35-hour reform, zero otherwise. In col. 4, the sample is restricted to the affected individuals (individuals working in occupations affected by the 35-hour reform, AND working in firms employing over 20 employees, AND in sectors affected by the 35-hour reform). In col. 5, the sample is restricted to the unaffected individuals. In all columns, 95 department fixed effects, eight year fixed effects, 14 control variables (five age dummies, sex, size of the household, and seven diploma dummies), and 30 occupation fixed effects are included. Column 7 adds border × GDP growth. In cols. 3 and 6, 95 interaction dummies between departments and the affected individuals dummy, seven interaction dummies between year dummies and the affected individuals dummy, and 30 interaction dummies between occupations and the affected individuals dummy are included. The p-value of the t-test coefficient of interest is = .35 is the p-value of the t-test (Alsace-Moselle) × (2001–2) = .35 in cols. 1, 4, and 7 and (Alsace-Moselle) × (2001–2) × (affected individuals) = .35 in cols. 3 and 6. The coefficients of interest are highlighted in bold.

* Significant at 10%.

** Significant at 5%.

*** Significant at 1%.

Moselle worked 0.38 hour more per week than their counterparts in France in 2001–2 compared to 1996–98. Thus, the 35-hour reform was indeed milder in Alsace-Moselle than in the rest of France. This difference disappears in 2003 since γ_1 is not significantly different from zero, which was expected because of the decision by the local council (*prud'homme de Metz*). The common time effects assumption is valid since γ_3 is not significantly different from zero.

C. Second Check: A Triple Difference Based on Affected Occupations

A falsification exercise is presented in column 2 of table 1. We should see no difference between Alsace-Moselle and the rest of France for individuals in occupations unaffected by the 35-hour reform. Indeed, column 2 of table 1 shows that the difference between Alsace-Moselle and the rest of France is not significant for individuals working in unaffected occupations.

As noted previously, Alsace and Moselle are the only French areas that share a border with Germany. Many French workers cross the border to find work in Germany. Thus, a particular macroeconomic cycle in Germany could significantly affect performance in Alsace-Moselle. As a consequence, we would then run the risk of confounding the impact of the reform with a particular event that might have taken place in Germany at the same time. The inclusion of $(\text{Alsace-Moselle}) \times (1999\text{–}2000)_{ijt}$ constitutes a first step in showing that there is no systematic difference in the evolution of Alsace-Moselle and the rest of France over time. All the information can be collapsed in a DDD approach, which compares individuals within Alsace-Moselle who are more or less affected by the 35-hour reform while at the same time exposed to the same German macroeconomic trend. This strategy relies on the assumption that all occupations in Alsace-Moselle are equally affected by the German macroeconomic cycle.

Column 3 of table 1 presents the DDD results. The "affected individual" dummy variable, equal to one if the individual has an occupation affected by the 35-hour reform, zero otherwise, is interacted with department fixed effects, year fixed effects, and $(\text{Alsace-Moselle}) \times (2003)_{ijt}$, $(\text{Alsace-Moselle}) \times (2001\text{–}2)_{ijt}$, and $(\text{Alsace-Moselle}) \times (1999\text{–}2000)_{ijt}$. The coefficient of interest is the one in front of $(\text{Alsace-Moselle}) \times (2001\text{–}2) \times (\text{affected individuals})_{ijt}$. This coefficient is significantly positive and is approximately 0.75, although the theoretical coefficient 0.35 falls within the confidence interval (see the p-value in the table). This is a confirmation that the impact of the 35-hour reform was indeed milder in Alsace-Moselle than in the rest of France.

Standard errors are clustered at the department level. Results hold when standard errors are clustered at the less aggregated level of the sampling

stratum. Following the Conley-Taber methodology, we find that the DDD coefficient is 0.73 (close to the coefficient of 0.75 in the table) and that the confidence interval for this coefficient is [0.18, 1.73] at the 90% level and [0.09, 2.14] at the 95% level. We may thus be confident that the significance of our results is not driven by the inadequacy of the asymptotic assumption in our study.

D. Robustness III: A Triple Difference Based on Affected Occupations, the Size of Firms, and Sectoral Collective Agreements

In this subsection, we refine the triple differences by further restricting the treated group according to the size of the firm and the sector of activity. As noted above, the Aubry I law specified that in 2000 (for firms with over 20 employees) or in 2002 (for smaller firms), the 35-hour workweek would both be irreversible and be uniformly applied to all firms, even those not having signed an agreement. Therefore, in order to capture the causal effect of the 35-hour reform, we could compare individuals working in firms with more than 20 employees to individuals working in firms with fewer than 20 employees, in Alsace-Moselle compared to the rest of France, and in 2001–2 compared to 1996–98. This strategy would rely on the assumption that firms with more or fewer than 20 employees in Alsace-Moselle are equally affected by the German macroeconomic trend. Appendix table A2 replicates the analysis performed in table 1 and finds similar results when undertaking this DDD strategy.

There are also important differences across sectors of the economy, stemming from local collective labor agreements (*conventions collectives*) that regulate all aspects of work. A collective agreement is an agreement negotiated between employers and trade unions in order to clarify general dispositions from the Code du Travail. There are 528 national, regional, and departmental collective agreements in France. After we examine Lower Rhine's (*département du Bas-Rhin*, one of the two subregions in Alsace) 10 collective agreements, Upper Rhine's (the other *département* in Alsace) one, Alsace's four regionwide agreements, Moselle's six, and Lorraine's one, it is interesting to note that only four sectors (retailing and repairs, metallurgy and metal transformation, construction, and hotels and restaurants) have collective agreements that do not explicitly mention the two supplementary public holidays. We argue that it has been easier for employers to substitute public holidays for RWT days in these four sectors since workers in these sectors seem to be less aware of their rights. In other sectors, however, it may have been less easy to do so. Therefore, we deemed the four aforementioned sectors, which experienced a milder application of the 35-hour reform, as the "treated group," and all other sectors were included as the "control group." As in table 1, we present the DDD results associated with these affected firms, or affected sectors, in appendix table A2. We may also combine these three triple differences based on affected occupations, affected firms, and affected sectors in a single triple difference.

In column 4 of table 1, the sample is restricted to individuals employed in an affected occupation, in an affected firm, and in an affected sector. The coefficient in front of (Alsace-Moselle) × (2001–2)$_{ijt}$, γ_2, is equal to 0.92 and is statistically significant. This coefficient is slightly higher than expected. In column 5, the sample is restricted to individuals working in occupations unaffected by the 35-hour reform, OR working in firms employing fewer than 20 employees, OR working in sectors unaffected by the 35-hour reform. There is no significant effect of the reform, as expected. In column 6, the coefficient of interest is (Alsace-Moselle) × (2001–2) × (affected individuals)$_{ijt}$, a DDD coefficient. This coefficient measures the relative increase in hours worked for affected as opposed to unaffected individuals, in Alsace Moselle compared to the rest of France, and in 2001–2 compared to 1996–98. The coefficient in column 6 is significant and positive (0.75), and the theoretical coefficient 0.35 still falls within the confidence interval. We obtain the same high significance level for the coefficients of interest with alternative clusters ("sampling stratum") or without any cluster (results available on request). The DDD Conley-Taber coefficient is 0.66 (close to the coefficient of 0.75 in the table), and the Conley-Taber confidence interval for this coefficient is [0.007, 1.47] at the 90% level.

E. Robustness and Falsification IV

There is an alternative way to account for economic cycles from other countries (i.e., Germany). In particular, one could estimate a simple double difference by attempting to control for the GDP growth of the foreign country. To identify the coefficient, one would then take into account the GDP growth of all France's bordering countries. In our case, the two *départements* of Alsace border Germany, and Moselle shares a border with Germany and Luxembourg. The French *département du Nord* borders Belgium, and a few other *départements* in France share a border with Italy, Switzerland, and Spain. Including all these border effects (and counting half the border effect when a *département* has a border with two countries), we compute a border × GDP growth effect, which can be seen in table 1, where border is a dichotomous variable equal to one if the individual lives in a department bordering a country and zero otherwise.[7]

[7] Border is a dummy variable equal to one for all *départements* close to a border. *Department* 54 is close to Luxembourg; 57, 67, and 68 are close to Germany; 90, 25, 39, 1, and 74 are close to Switzerland; 73, 74, 4, 5, and 6 are close to Italy; and 66, 9, 31, 65, and 64 are close to Spain. Data on GDP were collected from the OECD Web site.

Column 7 includes the new border × GDP growth effect term. First, there is not a significant change in the coefficient of interest (hours worked), and second, the GDP growth effect coefficient is positive (although insignificant).[8]

The last falsification test of the identification strategy has been to apply the same DD analysis to all regions in France. Indeed, owing to sample size limitations in the labor force survey, we could have found spurious results indicating that the number of work hours declared in Alsace-Moselle increased relative to the rest of France in 2001–2. Replacing Alsace-Moselle with any other region should yield no significant results. If this is the case, we could be more confident that we are effectively picking up an "exogenous" regional specificity of Alsace-Moselle. Indeed, we replicated column 6 of table 1 for the 21 regions in France and obtained reassuring results. Out of the 21 regions,[9] only Alsace and Lorraine had positive and significant coefficients. Overall, these findings lend support to Alsace's specificity and the exogeneity of the relative increase in hours in triple differences.

VI. Impact of the 35-Hour Reform on Other Outcomes

In light of the previous section, we can be fairly confident that the application of working time reduction was different in Alsace-Moselle than in the rest of France. We can also be confident that this difference is exogenous and due to the specificity of labor laws. Considering that the reform was 10% less effective in Alsace-Moselle, the next step is to establish how various economic variables evolved in Alsace-Moselle relative to France in the period 2001–2. If it evolves positively, that is, if we obtain a positive coefficient for the variable (Alsace-Moselle) × (2001–2) × (treatment group), this means that the RWT had a negative impact on the specific variable under investigation. A negative coefficient implies a positive RWT effect on that variable, whereas insignificant coefficients imply that the effects of the reform are nonexistent, or at least not strong enough to be detected in the data.

A. The Employment Effect

To establish the individual employment effect, employment rates in Alsace-Moselle are compared to those in the rest of France in 2001–2 compared to 1996–98. While the 2003 *Enquête Emploi* provides information concerning workers' specific departments, a difficulty arises in that the data on unemployed, or inactive, individuals include only the

[8] We replicated the analysis with GDP per capita growth in place of GDP growth. The main coefficient on hours is not significantly affected.

[9] As Corsica has no regional code standing in the *Enquête Emploi*, 21 instead of 22 regions were accounted for.

regions (groupings of three to four departments) in which they live. Thus, we drop the year 2003 in this section.

Table 2 examines the relationship between the milder reform in Alsace-Moselle in 2000 and employment in the full sample. The dependent variable is a dichotomous variable taking the value one if the individual is employed and zero in any other case (i.e., the individual may be unemployed or inactive).

In column 1, the sample is restricted to occupations affected by the 35-hour reform.[10] Note that the sample cannot be restricted to firms with more than 20 employees, or to sectors affected by the reform, since we do not know the firm size or activity sector of unemployed or inactive individuals. The coefficient of (Alsace-Moselle) × (2001–2) is not significantly different from zero, indicating that the milder version of the reform had no effect on employment.

In column 2, the sample is restricted to occupations unaffected by the 35-hour reform. In column 3, the coefficient of interest is (Alsace-Moselle) × (2001–2) × (affected individuals), a DDD coefficient, where affected individuals is a dichotomous variable equal to one if the individual has an occupation affected by the 35-hour reform and zero otherwise.

As shown in table 2, the coefficient of interest is not significantly different from zero. This indicates that there is no difference in local employment in Alsace-Moselle relative to the rest of France as a result of the milder version of the 35-hour reform. This result is in concordance with the findings of Estevão and Sa (2006), who argued on the basis of firms' size that the employment effect is difficult to estimate and found it to be generally insignificant. As small and large firms may be differentially affected by macroeconomic shocks, our strategy may be considered especially robust in that our analysis is based on both regions and the occupations of individuals, regardless of the size of the firm in which they work.

Columns 4 and 5 investigate the differential effect of the 35-hour reform on various groups. In these columns, the sample is restricted, respectively, to high school graduates and non–high school graduates. Results are always insignificant.[11]

Column 6 introduces, as in table 1, an interacted term between a border dummy variable (taking the value one if the individual lives in a department sharing a border with a country, zero otherwise) and the GDP growth of this particular country. When foreign business cycles are accounted for, no effect is found on employment.

[10] We know the occupations of 73,875 unemployed individuals and 115,732 inactive individuals, making this estimation feasible.

[11] A similar test was conducted for individuals younger than 40 years old and individuals older than 40 years old, with insignificant results.

While columns 1–6 focus on the individual employment effect, a similar analysis may be led at a more aggregated level. We thus estimate the number of individuals employed in a certain department to test whether the milder version of the 35-hour reform in Alsace-Moselle had an influence on the total number of jobs. This is especially interesting since we may further discriminate between jobs likely affected, or not, by the 35-hour reform. In column 7, the dependent variable is the log of the number of individuals employed per department in occupations affected by the 35-hour reform, AND in firms employing more than 20 employees, AND in sectors affected by the 35-hour reform. No effect is found on the number of jobs theoretically affected by the 35-hour reform in Alsace-Moselle after the reform. However, this could be due to border effects with Germany. We thus look at other jobs not likely affected by the 35-hour reform. In column 8, the dependent variable is the log of the number of individuals employed in occupations not affected by the 35-hour reform, OR in firms employing fewer than 20 employees, OR in sectors not affected by the 35-hour reform. No significant effect is found on total employment in Alsace-Moselle after 2000. In column 9, the coefficient of interest is (Alsace-Moselle) × (2001–2) × (affected individuals), a DDD coefficient, where affected individuals is a dichotomous variable equal to one if the log of the number of individuals employed concerns individuals employed in occupations affected by the 35-hour reform, AND in firms employing more than 20 employees, AND in sectors affected by the 35-hour reform, zero otherwise. The DDD coefficient is insignificant, indicating that the effect of the reform on total employment is insignificantly different from zero. Conley-Taber confidence intervals were also computed and indicate no significant impact of the reform on employment.

B. The Unemployment Effect

Another outcome of interest is unemployment. Table 3 examines the relationship between the milder 2000 reform in Alsace-Moselle and unemployment. The dependent variable is an unemployment dummy (one if unemployed, zero if employed). This subsection is thus different from the previous one since the sample in Section VI.A included all individuals (employed, unemployed, and inactive), whereas the sample in this subsection includes only employed and unemployed individuals.

In column 1, the sample is restricted to occupations affected by the 35-hour reform. No effect is found of the milder reform in Alsace-Moselle in 2001 and 2002. In column 2, the sample is restricted to occupations unaffected by the 35-hour reform. In column 3, the coefficient of interest is (Alsace-Moselle) × (2001–2) × (occupations affected), a DDD coefficient. Again, we find that the coefficient of interest is not significantly different from zero.

Table 2
Impact of the 35-Hour Reform on Employment (Full Sample, Including Inactive Individuals)

	Dependent Variable: Individual Employment Dummy (1 If Employed, 0 If Unemployed or Inactive)						Dependent Variable: Log(Employed Individuals in the Department)		
	Occupations Affected (DD) (1)	Occupations Unaffected (DD) (2)	Full (DDD: 1–2) (3)	Skilled (DDD: 1–2) (4)	Unskilled (DDD: 1–2) (5)	Occupations Affected (DD) (6)	Affected Individuals (DD) (7)	Unaffected Individuals (DD) (8)	Full (DDD: 7–8) (9)
(Alsace-Moselle) × (2001–2)	**−.0028**	−.0036	−.0032	−.0086	.0116	**−.0028**	**.1617**	.1038	.1038
	(.007)	(.008)	(.013)	(.021)	(.015)	**(.007)**	**(.124)**	(.081)	(.081)
(Alsace-Moselle) × (1999–2000)	−.0032	−.0008	.0009	−.0057	.0217	−.0032	−.0185	.0524	.0524
	(.003)	(.012)	(.016)	(.022)	(.015)	(.003)	(.049)	(.021)**	(.021)**
(Alsace-Moselle) × (2001–2) × (affected individuals)			**.0007**	**−.0003**	**.0062**				**.0579**
			(.016)	**(.024)**	**(.014)**				**(.092)**
(Alsace-Moselle) × (1999–2000) × (affected individuals)			−.0039	−.0023	−.0111				−.0709
			(.015)	(.018)	(.026)				(.034)
Border × GDP growth						−.007			
						(.090)			
Department fixed effects (95)	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects (7)	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Control variables (14)	Yes	Yes	Yes	Yes	Yes	Yes	No	No	No
Occupation fixed effects (30)	Yes	Yes	Yes	Yes	Yes	Yes	No	No	No

Year × (affected individuals) fixed effects	No	No	Yes	Yes	Yes	No	No	No	Yes
Department × (affected individuals) fixed effects	No	No	Yes	Yes	Yes	No	No	No	Yes
Occupation × (affected individuals) fixed effects	No	No	Yes	Yes	Yes	No	No	No	No
Observations	454,999	113,396	568,395	441,557	126,838	454,999	657	658	1,315

NOTE.—Probit results with marginal effects at the mean in cols. 1–6 and OLS results in cols. 7–9. Robust standard errors are in parentheses, clustered at the level of department. We also report in the text the Conley-Taber confidence intervals to correct for the small number of treated departments. Zero always falls in the confidence interval for all reported results. The dependent variable in cols. 1–6 is an employment dummy (one if employed, zero if unemployed or inactive). We know the occupation of 189,607 unemployed or inactive individuals; this is why a DDD is possible. In col. 1, the sample is restricted to occupations affected by the 35-hour reform (see n. 6 for the exact list). In col. 2, the sample is restricted to occupations unaffected by the 35-hour reform. In col. 3, the coefficient of interest is (Alsace-Moselle) × (2001–2) × (affected individuals), a DDD coefficient, where affected individuals is a dichotomous variable equal to one if the individual has an occupation affected by the 35-hour reform, zero otherwise. In col. 4, the sample is restricted to skilled individuals (highest degree more than the baccalaureate). In col. 5, the sample is restricted to individuals with less than the baccalaureate. Column 6 adds border × GDP growth. In col. 7, the dependent variable is the log of the number of individuals employed per department in occupations affected by the 35-hour reform, AND in firms employing more than 20 employees, AND in sectors affected by the 35-hour reform. In col. 8, the dependent variable is the log of the number of individuals employed in occupations not affected by the 35-hour reform, OR in firms employing fewer than 20 employees, OR in sectors not affected by the 35-hour reform. In col. 9, the coefficient of interest is (Alsace-Moselle) × (2001–2) × (affected individuals), a DDD coefficient, where affected individuals is a dichotomous variable equal to one if the log of the number of individuals employed concerns individuals employed in occupations affected by the 35-hour reform, AND in firms employing more than 20 employees, AND in sectors affected by the 35-hour reform. In all columns, 95 department fixed effects and seven year fixed effects are included. In cols. 1–6, 14 control variables (five age dummies, sex, size of the household, and seven diploma dummies) and 30 occupation fixed effects are included. In cols. 3, 4, 5, 6, and 9, 95 interaction dummies between departments and the affected individuals dummy are included, as well as seven interaction dummies between year dummies and the affected individuals dummy. The coefficients of interest are highlighted in bold.

* Significant at 10%.

** Significant at 5%.

*** Significant at 1%.

Table 3
Impact of the 35-Hour Reform on Unemployment (Sample of Employed and Unemployed, Excluding Inactive Individuals)

	Dependent Variable: Individual Unemployment Dummy (1 If Unemployed, 0 If Employed)						Dependent Variable: Unemployment Rate
	Occupations Affected (DD) (1)	Occupations Unaffected (DD) (2)	Full (DDD: 1–2) (3)	Skilled (DDD: 1–2) (4)	Unskilled (DDD: 1–2) (5)	Occupations Affected (DD) (6)	Department Level (DD) (7)
(Alsace-Moselle) × (2001–2)	**.003**	.0057	.011	.0081	.0293	**.0029**	**.0088**
	(.007)	(.005)	(.009)	(.020)	(.043)	**(.043)**	**(.009)**
(Alsace-Moselle) × (1999–2000)	.0014	.0007	.0003	.0009	−.0005	.0014	.0053
	(.002)	(.008)	(.017)	(.025)	(.024)	(.002)	(.003)
(Alsace-Moselle) × (2001–2) × (occupations affected)			**−.0075**	**.0021**	**−.0265**		
			(.013)	**(.026)**	**(.017)**		
(Alsace-Moselle) × (1999–2000) × (occupations affected)			.0009	.0061	−.0139		
			(.017)	(.027)	(.017)		

Border × GDP growth						−.0057	−.1087
						(.104)	(.214)
Department fixed effects (95)	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects (7)	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Control variables (14)	Yes	Yes	Yes	Yes	Yes	Yes	No
Occupation fixed effects (30)	Yes	Yes	Yes	Yes	Yes	Yes	No
Year × (occupations affected dummy) fixed effects	No	No	Yes	Yes	Yes	No	No
Department × (occupations affected dummy) fixed effects	No	No	Yes	Yes	Yes	No	No
Observations	433,421	109,133	542,581	415,156	122,685	433,421	658

NOTE.—Probit results with marginal effects at the mean in cols. 1–6 and OLS results in col. 7. Robust standard errors are in parentheses, clustered at the level of department. We also report in the text the Conley-Taber confidence intervals to correct for the small number of treated departments. Zero always falls in the confidence interval for all reported results. The dependent variable in cols. 1–6 is an unemployment dummy (one if unemployed, zero if employed). The sample is restricted to employed and unemployed individuals. As such, it excludes inactive individuals. In col. 1, the sample is further restricted to occupations affected by the 35-hour reform (see n. 6 for the exact list). In col. 2, the sample is restricted to occupations unaffected by the 35-hour reform. In col. 3, the coefficient of interest is (Alsace-Moselle) × (2001–2) × (occupations affected), a DDD coefficient, where occupations affected is a dichotomous variable equal to one if the individual has an occupation affected by the 35-hour reform, zero otherwise. In col. 4, the sample is restricted to skilled individuals (highest degree more than the baccalaureate). In col. 5, the sample is restricted to individuals with less than the baccalaureate. Column 6 adds border × GDP growth. In col. 7, the dependent variable is the unemployment rate at the department level. In all columns, 95 department fixed effects and seven year fixed effects are included. In col. 1, 14 control variables (five age dummies, sex, size of the household, and seven diploma dummies) and 30 occupation fixed effects are included. In cols. 3–5, 95 interaction dummies between departments and the occupations affected dummy are included, as well as seven interaction dummies between year dummies and the occupations affected dummy. The coefficients of interest are highlighted in bold.

Table 4
Impact of the 35-Hour Reform on Other Outcomes

	A. Hourly Wage[a]				
	+20 Employees (DDD) (1)	+20 Employees (DDD Tobit) (2)	+20 Employees (DDD) (3)	Sectors (DDD) (4)	Occupations (DDD) (5)
(Alsace-Moselle) × (2001–2) × (+20 employees/sectors affected/occupations affected)	−2.0196	−1.5356	−2.0306	1.0589	1.6946
	(1.15)*	(.83)*	(1.11)*	(1.04)	(1.69)
	B. Log of Hourly Wage[a]				
	+20 Employees (DDD) (1)	+20 Employees (DDD Tobit) (2)	+20 Employees (DDD) (3)	Sectors (DDD) (4)	Occupations (DDD) (5)
(Alsace-Moselle) × (2001–2) × (+20 employees/sectors affected/occupations affected)	−.0257	−.0252	−.0266	.0132	.0289
	(.02)	(.02)	(.02)	(.08)	(.04)

	C. Other Outcomes				
	Would You Like to Work More? (1)	Exhibitions per Month? (2)	Books per Month? (3)	Times Watch TV per Day? (4)	National Newspapers Read per Month? (5)
(Alsace-Moselle) × (2001–2) × (occupations affected)	−.1820	−.4270	−1.2102	−.0194	−5.2186
	(.03)***	(.09)***	(.88)	(.06)	(1.18)***
Observations	6,693	6,759	6,759	6,759	6,759
R^2		.13	.16	.12	.23

NOTE.—OLS results. Robust standard errors are in parentheses, clustered at the level of department. We also report in the text the Conley-Taber confidence intervals to correct for the small number of treated departments. The dependent variable in all columns of panel A is monthly wage divided by hours worked last week (and divided by 4.3 to get the hourly wage). DDD results using the three control groups (occupations affected, sectors affected, and size of the enterprise) are shown in cols. 1–5. Only the coefficient of interest is shown, but all columns include 95 department fixed effects, eight year fixed effects, 14 control variables (five age dummies, sex, size of the household, and seven diploma dummies), 30 occupation fixed effects, 95 interaction dummies between departments and the treated dummy, seven interaction dummies between year dummies and the treated dummy, and 30 interaction dummies between occupations and the treated dummy are included. In col. 2, Tobit results are shown; there is bottom coding at 16 francs per hour and 83 francs per hour. The dependent variable is the log of hourly wage in panel B. OLS results are presented (Tobit in col. 2). Panel C looks at leisure in the data set *Enquêtes permanentes sur les conditions de vie, Indicateurs sociaux d'octobre—Fichier historique 1996–2003*.

[a] Monthly wage divided by hours worked last week (and divided by 4.3 to get the hourly wage).

* Significant at 10%.

** Significant at 5%.

*** Significant at 1%.

Columns 4 and 5 restrict the sample, respectively, to high school graduates and non–high school graduates. Results are always insignificant.[12] Column 6 controls for the potential impact of foreign business cycles.

The analysis may be undertaken at a more aggregated level. Unemployment rates may be calculated at the department level. However, the DDD analysis performed in columns 7, 8, and 9 of table 2 may not be undertaken in this section because the firm's size and the sector of activity of an unemployed individual are by definition not known. To control for border effects, we thus perform a simple DD analysis with total unemployment rates, controlling for the GDP growth of neighboring countries.

Robust standard errors clustered at the department level are presented. Conley-Taber confidence intervals were also computed and indicate no significant impact of the reform on unemployment.

C. Other Outcomes

Table 4 presents additional results based on variables such as wages and leisure activities. Although some of the coefficients do not reach significance, all significant coefficients provide confirmatory evidence for the identification strategy: there were fewer leisure activities and lower hourly wages in Alsace-Moselle than in the rest of France over the period 2001–2. In particular, we find that over the period 2001–2, individuals in Alsace-Moselle had a lower propensity to be willing to work more; went to exhibitions less frequently; read fewer books compared to the rest of France, although not significantly; and finally read fewer newspapers. However, they did not watch less television: this is indicative of the relative elasticities of various types of leisure with respect to available time.

VII. What We May Conclude

Our paper had three aims. The first was to verify, from a purely descriptive standpoint, that earlier findings by law scholars (e.g., Grisey-Martinez 2005) would be confirmed by the data. These works are legal analyses of the process of working time reduction in Alsace-Moselle and how the national Aubry laws interfered with the local laws of German origin. These works indicate that 2001–2 was an ambiguous period concerning the application of the Aubry laws, an ambiguity favorable to employers. In fact, in Section V, we did find that the number of weekly hours worked in Alsace-Moselle increased relative to the rest of France by 0.35 hour on average, which represents 16 hours (2 days) per year.

The second aim was to verify whether the 35-hour reform had a significant impact on employment growth, as claimed by its proponents. Our claim here is that a milder application of the 35-hour reform in

[12] A similar test was conducted for individuals younger than 40 years old and individuals older than 40 years old, with insignificant results.

Alsace-Moselle evidenced no impact on relative employment in that region. We cannot conclude with certainty that the aggregate impact of the 35-hour reform was close to zero. However, our results are certainly consistent with this interpretation.

How do our estimates compare to previous estimates of the impact of the 35-hour reduction? In earlier works, the most optimistic estimates about the effect were that approximately 500,000 jobs were created, whereas more conservative estimates were closer to 100,000–200,000 jobs. These estimates consistently fell within the range of our data, primarily because of the large standard errors. Despite the lack of precision of the coefficients in tables 2 and 3 (the employment and unemployment effects, respectively), we may want to say something about the gross numbers implied by these estimates. Indeed, we can obtain gross numbers in multiplying the marginal effect coefficients (Alsace-Moselle × 2001–2) by the total number of employees in France and by a factor of 10 to get the full effect of the working time reduction (since the difference in implementation is by 10% of the total effect). The mean effect of table 2 is 155,000 jobs created. However, this range is extremely sizable, going at least from −1 million to +1 million jobs (or about 5% of total employment). Moreover, the signs of the effect vary from one specification to the other. The conclusion is that previous estimates of the employment effect are not entirely inconsistent with our results, but rather that we cannot find any significant effect of the 35-hour reform using our empirical strategy.

The third aim was methodological: we have shown that interesting identification strategies based on local specificities in France were possible. Prior to our study, the belief was that most legal changes apply to the entire nation. In reality, France is a rather interesting aggregation of regional disparities. The example used in the present paper sheds light on one prominent example of regional disparities, and researchers may be well served to explore other such geographic disparities in future research.

Appendix

Table A1
Descriptive Statistics

	Alsace-Moselle			Rest of France		
	Mean	SD	*N*	Mean	SD	*N*
Average number of hours worked per week (all workers)	37.09	10.23	29,607	37.54	11.14	530,560
Average number of hours worked per week (only full-time workers)	39.93	7.83	24,793	40.53	9.08	439,499
Number of hours worked last week	36.67	13.69	24,262	37.43	14.20	426,565
Restricted sample:[a]						
Average number of hours worked per week (only full-time workers)	39.32	9.41	159	40.97	10.12	4,104
Number of hours worked last week	27.43	15.62	128	35.48	13.72	3,101
Triple differences on hours:[b]						
Proportion of workers in big firms (more than 20 employees)	.86	.34	24,879	.85	.36	441,600
Proportion of workers in affected sectors	.22	.41	24,879	.19	.39	441,600
Proportion of workers in affected occupations	.84	.37	24,879	.80	.40	441,600
Proportion of workers in affected group (big firms AND affected sectors AND affected occupations)	.15	.35	24,879	.12	.32	441,600
Control variables:						
Age:						
15–24 years	.11	.32	24,879	.10	.30	441,600
25–39 years	.36	.48	24,879	.32	.47	441,600
40–49 years	.05	.22	24,879	.07	.25	441,600
50–59 years	.29	.45	24,879	.30	.46	441,600
60+ years	.17	.38	24,879	.20	.40	441,600
Sex (0 = female, 1 = male)	.64	.48	24,879	.61	.49	441,600
Household size	2.98	1.21	24,879	2.99	1.22	441,600
Highest degree completed:						
Postsecondary education	.00	.01	24,879	.00	.01	441,600
High school + 2 years	.21	.41	24,879	.23	.42	441,600
High school	.05	.22	24,879	.07	.26	441,600
Technical degree	.37	.48	24,879	.31	.46	441,600
Secondary school	.14	.35	24,879	.14	.35	441,600
No degree	.12	.33	24,879	.13	.33	441,600
Labor market outcomes:						
Employed (1 if employed, 0 otherwise)	.47	.50	95,958	.45	.50	1,287,962
Unemployed (1 = unemployed, 0 = employed)	.09	.28	49,639	.11	.31	652,174
Hourly wage	55.64	29.76	18,426	54.79	36.82	301,612
Log of hourly wage	3.93	.41	18,426	3.90	.42	301,612

[a] The sample is restricted to the two weeks with public holidays only in Alsace-Moselle in 2003 (the only year in which data were collected throughout the year, as opposed to March for the previous rounds). The two weeks are those that include Good Friday (April 18, so April 14–20) and Saint Stephen's Day (December 26, so December 22–27).
[b] The sample is restricted to full-time workers (466,479 observations).

Table A2
Identification Strategy: Differential Impact by Firm Size, and Affected Sectors, of the 35-Hour Reform in Alsace-Moselle

	Dependent Variable: Hours Usually Worked per Week					
	+20 Employees (1)	< 20 Employees (2)	DDD (3)	Affected Sectors (4)	Unaffected Sectors (5)	DDD (6)
(Alsace-Moselle) × (2003)	−.4040	−.2199	−.2376	−.2880	−.5503	−.5571
	(.35)	(.29)	(.27)	(.47)	(.30)*	(.30)*
(Alsace-Moselle) × (2001–2)	**.3795**	.0930	.0704	**.5534**	.0951	.0956
	(.23)	(.21)	(.22)	**(.24)***	(.22)	(.22)
(Alsace-Moselle) × (1999–2000)	.0412	.0487	.0425	.2434	.0338	.0341
	(.24)	(.11)	(.08)	(.25)	(.24)	(.24)
(Alsace-Moselle) × (2003) × (affected individuals)			−.1637			.2568
			(.52)			(.60)
(Alsace-Moselle) × (2001–2) × (affected individuals)			**.3092**			**.4605**
			(.12)*			**(.26)***
(Alsace-Moselle) × (1999–2000) × (affected individuals)			.0423			.2155
			(.24)			(.09)**
Department fixed effects (95)	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects (7)	Yes	Yes	Yes	Yes	Yes	Yes
Control variables (14)	Yes	Yes	Yes	Yes	Yes	Yes
Occupation fixed effects (30)	Yes	Yes	Yes	Yes	Yes	Yes
Year × (affected individuals dummy) fixed effects	No	No	Yes	No	No	Yes

Table A2 (*Continued*)

	Dependent Variable: Hours Usually Worked per Week					
	+20 Employees (1)	< 20 Employees (2)	DDD (3)	Affected Sectors (4)	Unaffected Sectors (5)	DDD (6)
Department × (affected individuals dummy) fixed effects	No	No	Yes	No	No	Yes
Occupation × (affected individuals dummy) fixed effects	No	No	Yes	No	No	Yes
p-value of the *t*-test coefficient of interest = .35	.87		.87	.37		.65
Observations	387,794	70,759	458,553	86,283	372,270	458,553
R^2	.34	.39	.36	.43	.33	.36

NOTE.—OLS results. Robust standard errors are in parentheses, clustered at the level of department. We also report in the text the Conley-Taber confidence intervals to correct for the small number of treated departments. Contrary to table 1, zero falls in the confidence interval. The dependent variable in all columns is the number of hours usually worked per week. In col. 1, the sample is restricted to individuals working in firms with more than 20 employees. In col. 2, the sample is restricted to individuals working in firms with fewer than 20 employees. In col. 3, the coefficient of interest is (Alsace-Moselle) × (2001–2) × (affected individuals), a DDD coefficient. In this column, the affected individuals are individuals working in a firm with more than 20 employees. In col. 4, the sample is restricted to individuals working in sectors affected by the deduction of the two extra public holidays (commerce de détail, réparations; metallurgie et transformation des métaux; construction; hôtels et restaurants). In col. 5, the sample is restricted to individuals working in all other sectors. In col. 6, the coefficient of interest is (Alsace-Moselle) × (2001–2) × (affected individuals), a DDD coefficient. In this column, the affected individuals are individuals working in affected sectors. In all columns, 95 department fixed effects, eight year fixed effects, 14 control variables (five age dummies, sex, size of the household, and seven diploma dummies), and 30 occupation fixed effects are included. Additionally, in col. 3, 95 interaction dummies between departments and the +20 employees dummy are included, seven interaction dummies between year dummies and the +20 employees dummy, 30 interaction dummies between occupations, and the +20 employees dummy are added. In col. 6, 95 interaction dummies between departments and the affected sectors dummy, seven interaction dummies between year dummies and the affected sectors dummy, and 30 interaction dummies between occupations and the affected sectors dummy are included. The coefficients of interest are highlighted in bold.

* Significant at 10%.
** Significant at 5%.
*** Significant at 1%.

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