

# “Labor Disputes and Labor Flows: Identifying Real EPL”

Henri Fraise, Banque de France<sup>1</sup>  
Francis Kramarz, CREST-INSEE, CEPR, and IZA  
Corinne Prost, CREST-INSEE

This Version: 19/11/2010, in Progress

## Abstract

Using a data set of individual labor disputes brought to court over the years 1996 to 2003 in France, we examine the impact of labor court activity on labor market flows. First, we present a simple theoretical model showing that judicial case outcomes cannot be directly interpreted in terms of EPL. When a large fraction of cases go to trial, it may well be a sign of low firing costs with firms facing low litigation costs and being therefore willing to go to court or, conversely, a sign of high firing costs of workers facing low litigation costs and being therefore willing to sue the firm. Second, we exploit our model as well as the French institutional setting to generate instruments for these endogenous outcomes. Using these instruments, we show that labor court decisions have a causal effect on labor flows. More trials and more cases won by the workers cause more job destructions: more trials indeed are a sign of lower separation costs. More settlements, higher filing rates, and a larger fraction of workers represented by a lawyer dampen job destruction. Whatever the judicial indicator used, the effects on job creations are small or not significant; thus the effects on net job creations are always the opposite of those on job destructions.

**JEL classification:** J32, J53, J63, K31

**Keywords:** employment protection legislation, labor flows, labor judges, unfair dismissal, France

## 1 Introduction

Following the seminal paper by Lazear (1990), the effects of employment protection legislation (EPL, hereafter) on labor markets have been extensively examined through cross-country analyses, using indicators supposed to capture the national strictness of EPL (see Freeman, 2004, for a critical review). A recent strand of literature assesses the impact of EPL within countries, using changes in the legislation. This strategy typically involves measuring the impact of a change in legislation targeted to a specific category within a whole country or -- in the case of the US -- the impact of the differential timing in the introduction of a new EPL across different states. Wrongful-discharge protections were adopted by US state courts during the last three decades. Autor, Donohue, and Schwab (2004) take advantage of the between-state variation in the timing of the introduction of these labor laws and estimate their impact using difference-in-difference estimators. The “implied-contract” exception law,

---

<sup>1</sup> Corresponding author: [hf42@cornell.edu](mailto:hf42@cornell.edu). +33 (0)2 .45.26.29.86. Banque de France-31 Rue Croix-des-Petits Champs 75001 Paris, France. We are grateful to John Abowd, David Autor, Jed DeVaro, Marc Ferracci, Larry Kahn, and Iona Marinescu for useful comments. We also thank seminar participants at the Banque de France, Crest, MIT, New York University, University of Chicago, Yale University as well as participants at the EDHEC-GAIN-ADRES conference, IZA summer school, EALE conference, and ESSLE conference. The opinions expressed here are the authors’ own and do not necessarily reflect the views of the Banque de France and Insee.

meaning that the employer implicitly promised not to terminate a worker without good cause, is found to have reduced state employment rate by 0.8 to 1.6%. Boeri and Jimeno (2005), using the 1990s tightening of the Italian regulation for firms with less than 15 employees, find that the threshold does matter in conditioning layoff and hiring probabilities but find no significant impact on employment growth. Bauer et al. (2007) find no effect of the change in the German EPL exemption for small firms on worker turnover. Martins (2009) reaches the same conclusion using Portuguese matched employer-employee data. Marinescu (2009) finds that the 1999 change in employment seniority before being eligible to go to court in the UK – it went from 200 days to 100 – had a substantial decreasing impact on the firing hazard of the workers with one to two years of tenure.

Unfortunately, this last strand of research suffers from several downsides as well. First, these studies do not provide information concerning the degree of **enforcement** of labor regulations. To which extent these regulations were used by workers to defend their own interests? Are these regulations actually binding for employers? For instance, in the case of the US, even if judicial breaches to the employment-at-will doctrine have been judged by some state courts, we have little evidence on the extent to which they are used or even known by the workers. Therefore, we do not know if they act as a credible threat to the employment-at-will policy. The state of California recognized the application of the covenant of good faith and fair dealing to employment relationships in 1980. In March and April 1986, about 100 cases were filed in Los Angeles which implies an approximate number of 1,000 for the entire year in the entire state<sup>2</sup> (hence, about 80 cases per million workers).<sup>3</sup> In comparison, for France, with a population and GDP close to those of California, 160,000 cases take place in any given year. Of course, one could argue that the law can act on the employer in a pre-emptive way but, to capture any effect in the data, this impact should be very strong (or conversely the impact on employment of labor courts should be extreme in France). Second, labor laws are subject to court interpretation and tend to vary over time and space. As pointed out by *The OECD 2004 Employment Outlook*, even if an employer may be sanctioned in case of non-respect of EPL, “these provisions are subject to court interpretation and this may constitute a major (but often hidden) source of variation in EPL strictness both across countries and over time”.

Therefore, opening the black box of the labor courts seems a promising path for assessing the impact of EPL and above all its enforcement. Enforcement of the law may indeed matter more than its content (see for example Bhattacharya and Daouk, 2002 who find that insider trading laws decrease the cost of equity only when a case has been prosecuted). Yet, empirically, problems of endogeneity abound: court interpretation with the ensuing impact might not be exogenous as market conditions are likely to have an impact on the leniency of the courts, the introduction of new laws, or workers’ propensity to

---

<sup>2</sup> In 1986, civil case filings in Los Angeles represented about 60% of all civil activity in the state of California.

<sup>3</sup> These figures are taken from Dertouzos (1988).

go to court. Ichino et al. (2003), using micro data on labor court cases, focus on this institutional endogeneity of EPL enforcement. Studying the case of an Italian bank with roughly 20,000 employees among which 409 workers were fired and 86 of them went to trial over more than 20 years, they show that a higher unemployment rate increases worker's probability of winning her case. In contrast, Marinescu (2008) - using data from a 1992 survey of Employment Tribunal Applications in Great Britain - finds that a higher unemployment rate leads to more decisions against the workers, in particular when they were already re-employed.

In this paper, we take advantage of the French system of labor courts to analyze the judicial process and measure how it can be interpreted as EPL. The French EPL system characterized by large separation costs, high coverage by collective bargaining agreements, powerful unions is usually considered by international organizations as one of the strictest. It also produces every year a large amount of legal procedures related to individual labor disputes (roughly 160,000 new cases every year, as we will see). About 1 over 4 dismissed persons indeed challenges dismissals in front of the labor courts. These procedures are complex, costly, and can last for years. It is these *legal procedures* that we analyze in this paper. We examine their impact on employment flows. However, in contrast with virtually all of the literature, we do not focus on legislation changes but on the judicial process itself. We measure case outcomes – trial rates, conciliation rates... -- directly using all cases that took place in France between 1996 and 2003. We relate these outcomes to the legal environment of each French geographical jurisdiction. We then try to understand how these legal outcomes affect job creation and job destruction within the jurisdiction of each court, using flows measured at those establishments that are within the jurisdiction of each court.

This strategy is, we believe, better suited to measuring and identifying *real* EPL since the mere wording of a new legislative text often tells little about its *real* impact. We build a model relating firing costs to judicial activities of the courts. The model as well as data will help us show how changes in the legislative environment that directly affect the legal costs of going to court transform the whole structure of potential labor court outcomes. Because we want to relate judicial case outcomes with EPL, our contribution to the literature has to be both methodological and empirical.

On the *methodology* side, we define here increasing EPL uniquely by **increasing separations costs**. Endowed with this definition, and using Bentolila and Bertola (1990)'s very general result that an increase in separation costs decreases labor flows (both at entry and at exit), we are in position to **identify those legal outcomes of labor courts that constitute *real measures of EPL*** as those that decrease labor flows.<sup>4</sup> Indeed, we show theoretically *and* empirically that some features of legislation

---

<sup>4</sup> Under conditions that we discuss later.

that are, apparently, helping workers have the exact opposite effect and decrease separations costs. The main reason for such a surprising result comes from *composition* effects. These composition effects have direct consequences on final separation costs: for instance, if only “expensive” cases come to court, on one side the firm economizes on the extensive margin (less cases) but pays more on the intensive margin (more expensive). Therefore, measured case outcomes do not have a simple and univocal interpretation in terms of separation costs: more trials may sometimes mean larger separation costs, but it may also mean lower separation costs. This result does not only apply to labor courts but also to divorce or more generally to any legislation that alters the decisions of workers, couples, firms when they contract, sue, or indeed go to court.<sup>5</sup>

On the *empirical* side, our contribution is fourfold. First, we consider measures of EPL varying across space and time based on the various measures of judicial case outcomes directly coming from legislation enforcement. In France, workers can contest the conditions of a firing by filing a case to one of the 264 local labor courts. We use information collected by the French Ministry of Justice on all cases that were filed over the 1996-2004 period to compute, for each geographical jurisdiction and each year, various EPL indicators characterizing the enforcement of the labor laws: the percentage of dismissed workers who litigate in employment tribunals, the fraction of cases leading to a conciliation between parties, to a trial, resulting in a worker’s victory, or the fraction of cases in which workers were legally represented. We match these local indicators with local measures of the legal environment (judges, administrative staff, lawyers) as well as local measures of job flows à la Davis and Haltiwanger (1992).

Second, as we work at the level of France, a country in which many institutions are centralized and do not vary across the French territory (minimum wage, unemployment benefits, wage bargaining...) we are able to “control” for most of the French labor market institutions. Third, thanks to the precise French institutional setting and those local measures of legal environment, we use an instrumental variable strategy to correct for the endogeneity from which estimation of the relation between economic conditions, including labor flows, and application of the labor laws might suffer. These instruments rely on administrative features presiding over the allocation of judges and judicial clerks across French territory, and on the location of universities training lawyers. All these features are shown to be disconnected from local business conditions. Then, we measure the effect of various case outcomes on employment flows, identifying in so doing those that are valid measures of increasing EPL and those that are not. Fourth, because labor laws in France do not vary across local areas (see however Chemin and Wasmer, 2009, on the noticeable exception of the working time reduction laws in one French region, Alsace-Moselle), a difference in difference approach on changes in the

---

<sup>5</sup> This has not escaped some analysts; see for instance Stevenson (2007) on legislation and divorce rates.

legislation is essentially impossible to implement in a French setting. Our local enforcement indicators approach (paired with the appropriate instruments) offers one of the few credible substitutes to this classic identification strategy, in addition one that has never been used, to analyze France.

As in all of the empirical papers we are aware of, our paper focuses on the impact of labor regulations on labor market characteristics and leave aside the welfare gains from job stability which must be taken into account for policy recommendations.<sup>6</sup> However, and in contrast with the existing empirical literature, our labor court outcomes capture some dimensions of the quality of labor relations which according to Blanchard and Philippon (2004) or Algan and Cahuc (2009) are related to the evolution of labor market conditions.

To summarize our results, we show that labor court decisions have a causal effect on labor flows. More trials and more cases won by the workers cause more job destructions: more trials indeed are a sign of lower separation costs. More settlements, higher filing rates and a larger fraction of workers represented by a lawyer dampen job destructions.

Section 2 describes the French labor courts institutional setting. Section 3 presents a simple theoretical model relating the case outcomes that result from the enforcement of labor laws to firing costs. Section 4 describes our data sets and the instrumental variables. Section 5 explains our empirical methodology to capture EPL causal effects and presents our regression results on labor flows. We briefly conclude.

## **2 Labor Courts in France: the Institutional Setting**

### ***2.1 French Firing Laws***

Under the current French law, separations are classified in two types: dismissals for a personal motive or for an economic motive. Economic dismissals are redundancies due to a slowdown in the business activity. Individual dismissals occur when the firm's decision to fire a worker is triggered by a grave misconduct of the worker or an insufficient level of competencies. In France as in many European countries an economic dismissal may entail a more complicated and time consuming process as well as the payment of large severance fees. On the contrary, a dismissal for misconduct is a faster process - if not challenged by the worker or if confirmed by the labor court. Thus the dismissal for "just" cause implies a lower firing cost than a redundancy. When fired, a French worker might sue the firm. Since a bill passed in 1973, every individual dismissal must be justified by a "real and serious cause" and the firm has the burden of proof. Without delving deep into 30 years of jurisprudence that have made this concept simultaneously blurred and precise, "real" means that the wrongdoing justifying the dismissal

---

<sup>6</sup> See Bertola (2003) for a theoretical model considering risk-averse workers and potential positive effect of EPL on welfare.

must be objectively defined, accurate, and in line with the mandatory firing notification letter. For example, being ten minutes late does not mean being seventy minutes late; a lack of performance or a lack of trust is not considered “real” if it is not objectively measured. The cause is considered as “serious” only if it is related to the professional activity of the worker and if it makes the labor relation impossible to continue. There are various degrees of “seriousness”. Some lead to “grave misconduct” (for example brawl or thievery) which allows the employer to fully deprive the worker of severance payment (in this case, the employee may lose her unemployment benefits).

In addition to the cause of the dismissal, the employee can sue the employer if the latter did not follow the mandatory legal steps of the firing process (for example the employer must notify one week in advance that the employer intends to meet the worker in order to discuss his firing).

## ***2.2 French Labor Courts***

The French labor justice is mainly dispensed by the “Prud’hommes” which is the relevant jurisdiction to every labor dispute arising at the individual level in France. In the 90’s, 264 Prud’hommes jurisdictions were spread all over metropolitan France, a tribunal being at most within a radius of 30 miles from any establishment.

There are several sections in each Prud’hommes. As the legislators wanted to take into account industry characteristics of the cases brought to court, each Prud’homme is divided into 4 sections according to the main activity of the firm: Agriculture, Retail Trade, Manufacturing, and Other Activities (mainly Services). A fifth section is dedicated to cases involving “managers, engineers, and professionals” irrespective of the activity of the firm.

The judges in the Prud’hommes are not professional judges and are seen by some as performing a public duty. Each labor court comprises judges representing employers and judges representing employees in equal number. These judges are elected every five years within lists established by workers unions and employer federations. All employees are entitled to vote. They select judges in the union lists. Similarly, employers vote and select judges within the federation lists. Hence, in every section of every Prud’homme, the number of judges is even, at least four on each side. All French establishments are allocated to one jurisdiction. On the employee side, the electoral body includes all private sector workers with a labor contract. They are enrolled on the electoral list based on a mandatory administrative report from their employer.

Prud'hommes are supposedly not very formal and should be seen as conciliation boards. They were designed to foster agreements rather than trials. Therefore a first and mandatory step in each trial is a conciliation audience where plaintiffs and defenders explain their grievance and judges try to push for an agreement.<sup>7</sup> If they do not, the case is judged. If, in the end, an equal number of judges decides in favor of a worker and against her, there is a tie ("solution de départage"). In this case, a single professional judge decides the outcome of the trial.<sup>8</sup>

A majority of plaintiffs are represented by a lawyer. The plaintiff or the defender can appeal the decision of the court if the stake is larger than a given threshold (about 5,000 euros in 2006). It is worth noting that 60% of the decisions were appealed in 2004. Among them, 55% of these appeals did not overrule the Prud'hommes' decision, 30% confirmed it "partially".<sup>9</sup>

For any given case filed in a labor court, the range of outcomes is wide. A case can lead to a full tribunal hearing and be lost or won. It can be classified as null and void if the plaintiff has not shown due diligence in the conduct of her case. The case can also be crossed out. Finally, a case can either be conciliated during the conciliation step or outside the tribunal with a formal agreement sent to the court.

The motives for suing are multiple. The nullification of a dismissal is asked in the majority of cases (58%).<sup>10</sup> 21% of plaintiffs ask for some compensation that was not paid by their former employer whereas 9% of plaintiffs do not agree with the level of their severance payment. In this paper, we do not distinguish between these different motives.

### **3 Litigation and Firing Cost: a Simple Theoretical Model**

We develop a simple analytical model determining the relationships between litigation costs and case outcomes, taking specifically into account the conciliation step in the judicial process. Our intention is not to break new theoretical ground but rather to focus ideas. We do not study here the theoretical impact of firing costs on labor market variables. This has been extensively examined elsewhere (see Bentalila and Bertola, 1990 and Bertola, 1992). We rely on these authors to say that larger firing costs entail slower and smaller adjustments than separations inducing no costs.

---

<sup>7</sup> It is worth noting that to this respect, the French setting is close to almost every OECD country, where courts usually attempt to reach a compromise solution at the start of formal legal proceedings (see Venn, 2009).

<sup>8</sup> Moreover, in case of an emergency, a summary judgment can be made. However, such judgments are only temporary and might be overruled afterwards. In this paper, we do not consider these summary judgments.

<sup>9</sup> Munoz-Perrez and Serverin (2006). Unfortunately, current available data sets do not allow us to track the cases across the levels of jurisdictions; whether the decision is appealed by the worker or the firm is unknown.

<sup>10</sup> In the very vast majority of the cases won by the workers, they are not reinstated but receive a compensatory award.

To illustrate how firing costs are related to case outcomes, we depart from the traditional model of litigation proposed by Priest and Klein (1984) or Bebchuk (1984) or more recently Card and McCall (2009) to run a cost-benefit analysis similar to the one proposed by Flanagan (1989) for disputes related to the compliance to the National Labor Relations Act in the US. For simplicity, the model that we describe below has no uncertainty, no asymmetric information that would explain why trials take place; everything is known and predictable; we will come back later on this topic and discuss how our results are affected by asymmetric information.

In our analysis, the employer can deliberately choose to pay a minimal firing cost with the risk to be sued by the worker; or to pay a larger amount, which corresponds to the payment a plaintiff would accept in order to give up any further possibility of lawsuit. Important to note here that this last sum is not negotiated between the firm and the worker, but is directly coming from legal precedents (jurisprudence). In France, it amounts to one to two years of earnings. Another way of understanding the model is as follows: a firm chooses to dismiss the worker either for a personal motive, paying a small severance payment, or to dismiss the worker for an economic motive (redundancy) with larger severance payments. Our hypothesis, then, is that when firms pay the severance payment corresponding to a redundancy, the workers never choose to sue the firm (indeed, 97.5% cases in our data come from dismissals for personal motive rather than redundancies). When the worker goes to court after a dismissal, the firm has to prove that the case is a legitimate dismissal for personal motive rather than a redundancy.

In the case of a dismissal for personal motive, the firm incurs a minimum cost ( $c_m$ ) if the dismissal remains unchallenged by the worker. This cost  $c_m$  is lower than the maximum cost  $c_M$ , which leads the worker not to sue the firm. Yet the firm has to take into account the facts that the worker can file a suit ( $p_f=1$  if he does,  $p_f=0$  otherwise) and that he can then end the case with a formal agreement in front of the judge ( $p_c=1$  if he does,  $p_c=0$  otherwise). The firm also knows the probability that the worker wins if the trial occurs,  $p_w$ . We assume that during the conciliation step, the judge tries to reach an agreement using an “intermediary” cost  $c_c$ , given by the jurisprudence, always lower than  $c_M$ . Note that in order to simply introduce the co-existence of a conciliation stage and a trial stage we consider  $c_c$  to be constant. The firm cannot increase  $c_c$  in order to avoid the trial.

Uncertainty of the entire process is summarized through  $p_w$ . The firm and the employee share this value. In this cost-benefit analysis, we assume that the quality of each case is known by both parties and is related to observed characteristics of the workers and of the firms.<sup>11</sup> For instance, union or personnel delegates or pregnant women are very well protected by the law, and the judges are very

---

<sup>11</sup> As mentioned above, this assumption is discussed below.



strict against dismissals of such individuals. Several past statements of judgments also show that judges demand more stringent evidence when a firm has had large positive profits in the years preceding the trial.<sup>12</sup>

At this point, the model has no uncertainty, no asymmetric information that explains why trials take place. Theoretically, firms and workers should agree on a payment in order to avoid the litigation costs. Two features could be added to the model in order to explain why firms and workers go first to the Prud'hommes and then, if ever, to trial. First, costs for reaching an agreement without any mediator like the judge could be larger than the costs at the conciliation stage. This seems plausible since the Prud'hommes institution is seen as a public good and the conciliation stage is essentially free. Second, in line with the literature in which trial is an equilibrium outcome, we can assume that the worker and the firm have different and irreconcilable expectations on the outcome of the trial. This assumption would lead to a "contract zone" where a settlement amount can be found (see Bebchuk, 1984). When the expectations are not in the contract zone, the trial takes place; else an agreement can be found at the conciliation stage. Because there is a need to model expectations, computations become much less tractable. The model loses its simplicity without gaining much insight for our purpose. In addition, as underlined by Spier (2007) such a model does not fully solve the litigation puzzle since the conciliation stage should help the expectations to narrow. Again, it is possible to assume two stages of narrowing expectations, one leading to conciliation, one to the trial.<sup>13</sup> From this discussion, it is however interesting to note that workers employed in large firms are virtually absent from the Prud'hommes. In line with the above discussion, the various probabilities should be better known by the human resources management and union delegates that are always present in the larger firms. Hence, they should escape trials and easily agree on separation payments, as is observed. In small firms, conflicts become often personal and difficult to solve without the help of a neutral third party, a role apparently played by the Prud'hommes.

Now, let us go back to our model. If choosing  $c_m$  the expected firing cost for the firm is:

$$E(c) = p_f \{ p_c (c_c + l_c) + (1 - p_c) [p_w (c_m + F) + (1 - p_w) c_m + l_t] \} + (1 - p_f) c_m$$

where  $F$  is a compensatory award for the worker,  $l_c$  is the firm's litigation cost when the parties reach an agreement at the conciliation stage, and  $l_t$  is the firm's litigation cost when the parties go to trial. Otherwise, the firing cost is  $c_M$ .

---

<sup>12</sup> Unfortunately, the data do not contain a firm identifier. Hence, it is not possible to directly relate firm and worker behavior.

<sup>13</sup> Computations not included in the Appendix are available from the authors.

As for the worker, if the firm chooses  $c_m$ , the expected severance payment is:

$$E(s) = p_f \{ p_c (c_c - k_c) + (1 - p_c) [p_w (c_m + F) + (1 - p_w) c_m - k_t] \} + (1 - p_f) c_m$$

$k_c$  being the cost of litigation for the worker at the conciliation stage,  $k_t$  being the cost at the trial stage. Otherwise, the received payment is  $c_M$ .

The parameters  $p_f$  and  $p_c$  result from the optimization from the firm and the worker and equal one or zero. The worker chooses to go to court or not; and in that case to accept the conciliation or not. The firm chooses to pay  $c_m$  or  $c_M$ ; and, if at the court, to accept the conciliation or not.

We define:  $\overline{p_w} = \frac{k_t}{F}$ ,  $\overline{\overline{p_w}} = \frac{c_c - c_m + k_t - k_c}{F}$ ,  $p_w^* = \frac{c_c - c_m - l_t + l_c}{F}$ , and  $p_w^{**} = \frac{c_M - c_m - l_t}{F}$

Under several assumptions (see Appendix), there are four equilibria, depending on the value of  $p_w$ :

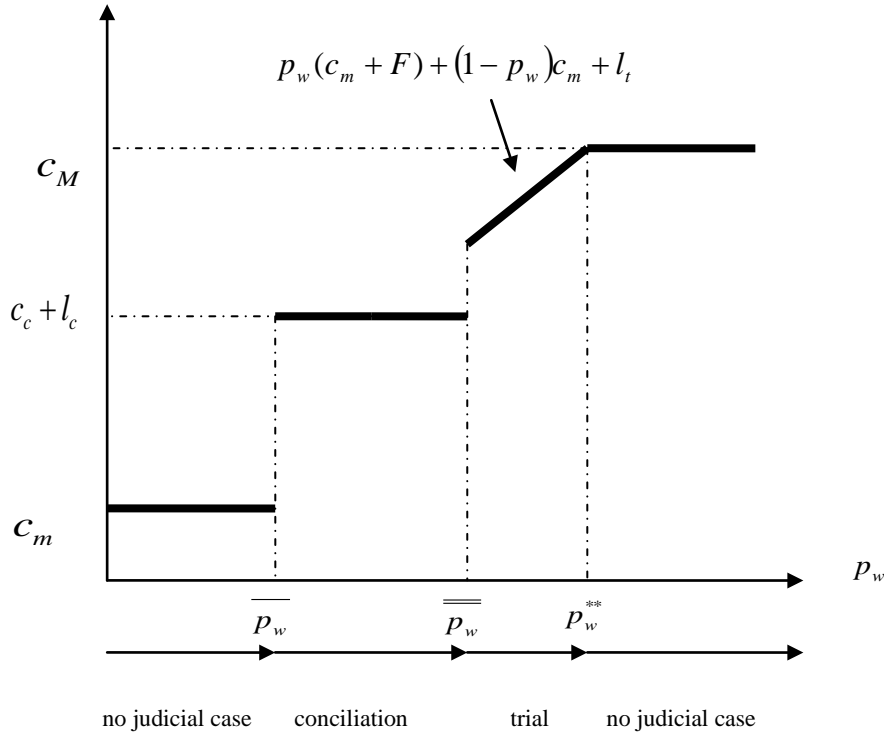
*Equilibrium (1):* for small probabilities  $p_w < \overline{p_w}$ , the firm pays  $c_m$  and the worker does not go to court ( $p_f = 0$  and  $p_c = 0$ ) since the firm would refuse the conciliation and the gain at trial would be negative for the worker.

*Equilibrium (2):* when  $\overline{\overline{p_w}} < p_w < \overline{p_w}$ , the expected gain of the worker at trial is positive. She can credibly threat the firm to go to a full hearing. Since  $p_w^* < \overline{p_w}$  the firm accepts to settle with the worker. The settlement amount is lower than the expected loss of the firm at trial but larger than the expected gain of the worker. ( $p_f = 1$  and  $p_c = 1$ ).

*Equilibrium (3):* when  $\overline{\overline{p_w}} < p_w < p_w^{**}$ , the worker is better off at the trial stage and refuses to conciliate anymore ( $p_f = 1$  and  $p_c = 0$ ).

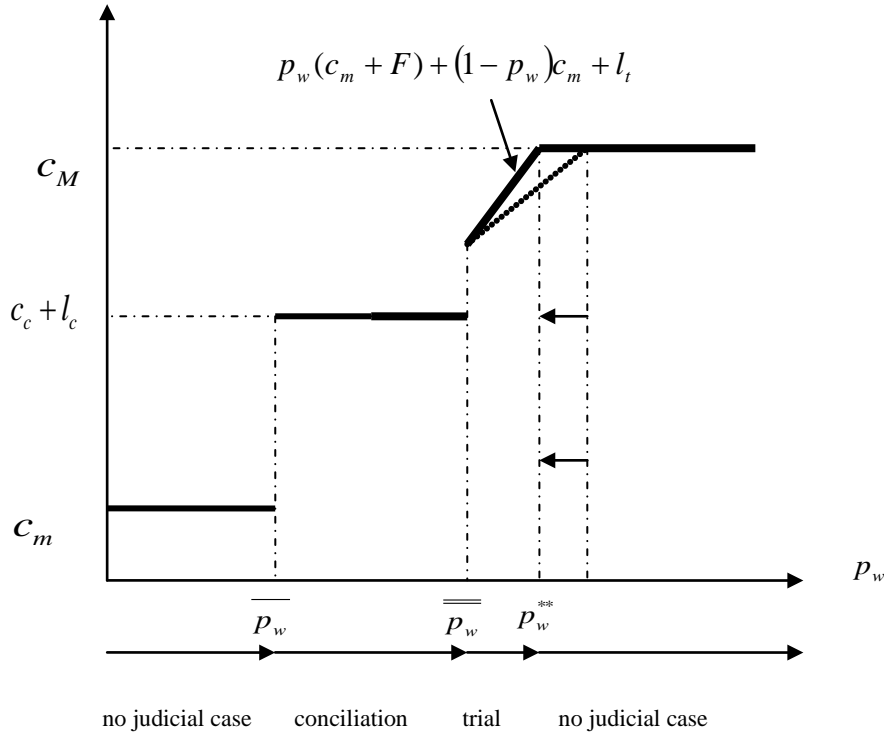
*Equilibrium (4):* when  $p_w > p_w^{**}$ , the firm pays  $c_M$  since it would be too costly to go to court.

The firing cost, depending on the probability of winning of the case, is shown on Figure 1.



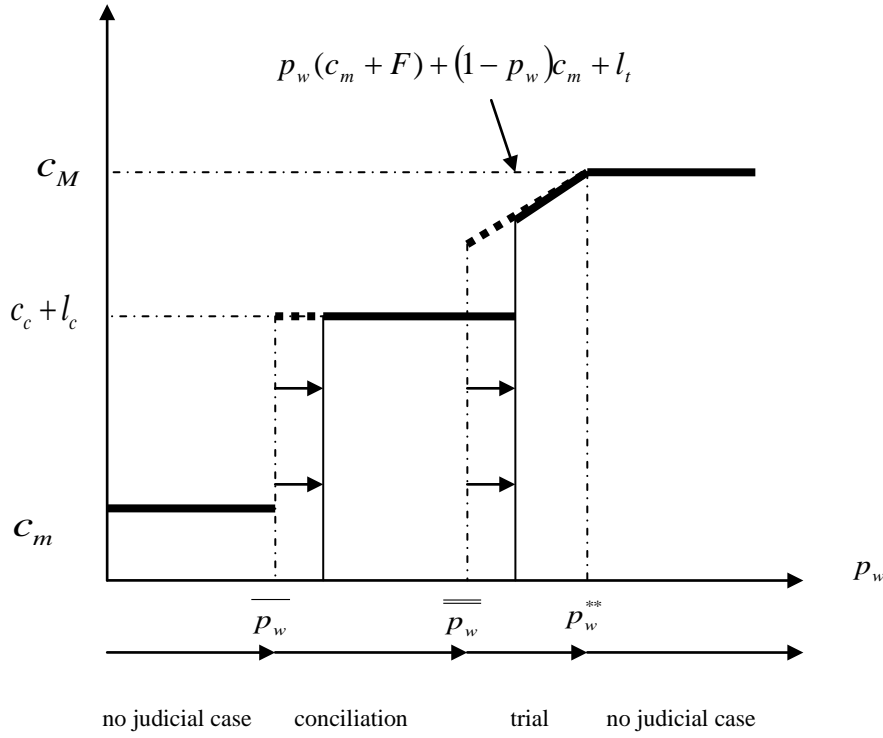
**Fig. 1: Firing Cost**

**Discussion:** We consider that firms are facing an invariant distribution of case quality. This model shows that changes in the litigation costs have intuitive impacts on the global firing costs; yet the results on the case outcomes are ambiguous. For instance, an increase in the firm's litigation cost at trial  $l_t$  implies a decrease in  $p_w^{**}$  (Figure 2). The firm will be more likely to fire high probability workers with an economic motive to avoid lawsuits. It will also increase the relative cost of a trial with respect to conciliation. The expected firm's cost at trial will rise. All these changes lead to an increase in the firing cost, a decrease in the trial and filing rates. Hence, a *smaller* number of trials and filing cases appear to be associated (in some cases at least) with *more* EPL.



**Fig. 2: Firing Cost, Case Outcomes and an Increase in the Litigation Costs for the Firm**

Let us study the following case that stands in stark contrast with the previous one (Figure 3). An increase in workers' litigation costs  $k_t$  will induce a decreased probability for the workers to file a case (through a higher  $\overline{p_w}$ ) as well as more workers that prefer to conciliate (through a higher  $\overline{\overline{p_w}}$ ). In this situation (contrasting with the previous one), a smaller number of trials is associated with lower total firing costs. In fact, our model shows that firing costs directly depend on variation in input costs; yet the link with case outcomes is ambiguous.



**Fig. 3: Firing Cost, Case Outcomes and an Increase in the Litigation Costs for the Worker**

## 4 Data and Methodology

### 4.1 Judicial Cases Data

Our data source on individual cases comes from administrative records made at the level of each geographical jurisdiction and collected by the statistical department of the French Ministry of Justice. The primary goal of these data is to monitor the activity of labor courts with an emphasis on speed of treatment. The data source is exhaustive for the period 1996 to 2003. It includes approximately 1.5 millions individual cases.<sup>14</sup>

For each case, the starting date, the ending date, the motives for dismissal, and the court decision are recorded. An average case takes approximately one year (343 days) with a standard deviation of 9 months.<sup>15</sup> For each case, we know the legal representation chosen by the firm and the plaintiff. Few characteristics of the employee-plaintiff are available: mainly gender and age. As for firms' characteristics, we know the industry, the size (more or less than 10 workers), and the Prud'hommes

<sup>14</sup> We will not consider the 2% of cases involving employers as plaintiffs.

<sup>15</sup> Because we use jurisdiction-level information for our analysis, rather than case-level information, our Tables will report jurisdiction-year statistics. All case-level statistics are available from the authors on request.

section of the employing firm (i.e. the industry broadly defined). The size of the firm has to be known by labor court judges because labor laws differ for small firms; more specifically, they are less stringent and try to ease the financial costs of firing that could hurt them irreversibly. Small firms are overrepresented with 56% of the filed cases whereas they comprise 25% of the labor force.

These data on individual cases are used to compute several aggregate measures of the cases examined in each jurisdiction-year pair. The first indicator relates to litigiousness: the filing rate, number of cases over the number of dismissed persons.<sup>16</sup> The second indicator relates to representation: the worker lawyer rate, number of cases where the worker is represented by a lawyer over the number of cases. The three other indicators describe the main outcomes of the cases, which are: the worker and the firm manage to conciliate, or they go to trial, and in that case, either the worker wins or not. Thus we build three indicators: the conciliation rate, number of cases conciliated or having led to an agreement over the number of cases; the trial rate, number of cases having reached trial over the number of cases; the winning rate, number of cases having led to a victory for the worker over the number of cases (see Table 1 for the definition of the indicators).

Admittedly starting from a high base, we do not observe in France a strong increase in the number of cases brought to the labor courts. In absolute terms, the number of filed cases increased by 10% over the 1990 to 2004 period. The number of cases treated by labor courts appears to be stable over the period, in stark contrast with what happened in some countries such as the UK where a sharp increase took place (see Figure 4 and Burgess, 2001). It is important to note that the percentage of filed cases among the dismissed persons is large (mean of 22%, see Table 2). Almost one dismissal over four ends at the labor court.

Despite the conciliation step which is mandatory, and promotes a quick and costless resolution of the cases, about 60% of cases end by a trial, among which 75% lead to a worker's victory (see Table 2). Only 20% of the filed cases end at the conciliation stage or lead to an agreement notified to the court or to a withdrawal on the worker's side. All outcome indicators display a very strong variance over time and across jurisdictions. In comparison with what is observed in a country such as the UK, almost half of the workers are represented by a lawyer despite other available means of representation.

## ***4.2 Labor Flows Data***

---

<sup>16</sup> No exhaustive statistics give the number of dismissed persons in France; we are thus obliged to have a proxy through the number of registered unemployed who declare being unemployed because of a dismissal. These data come from a data set on the number of unemployed registered at the national employment service (ANPE at this time) in each city, distinguishing the reasons for being unemployed (dismissal, entry into the labor force, end of temporary contract...). As for labor flows (see below), we can aggregate these data at the jurisdiction level.

Local employment flows at the establishment level are computed from the SIRENE files, maintained at the French statistical institute (INSEE). These files give the precise location (city) for each establishment. We compute a set of Davis and Haltiwanger (1992) indicators over the 1996-2004 period: job creation, job destruction, and net job creation rates. Job creation is positive for an expanding or new business, and null for other establishments. On the opposite, job destruction is positive for a shrinking or exiting business, and null for others.

These measures are aggregated at the jurisdiction level, using a 1999 correspondence between cities and jurisdictions provided by the Ministry of Justice. The rates of job creations and job destructions from year  $t$  to year  $t+1$  are computed relative to average employment in the two years. In comparison with cross-country analyses, these indicators also show a high heterogeneity across periods and across the 264 geographical jurisdictions. The job creation rate and the job destruction rate evolve around an average of 16%; thus the mean of the net job creation rate is zero (see Table 2).

To measure local unemployment, we use the number of unemployed as registered at the public employment office (ANPE) for each city as well as the city labor force as measured at the 1999 Census. Unfortunately, there is no data set giving us, at the local level of the city, the size of the temporary help service industry. Hence, we cannot perform an Autor's type analysis. However, in contrast with other European countries (such as Spain), the fraction of temporary workers in French total private employment is low (about 2.5% in 2009).

### ***4.3 Instrumental Variables: Discussion and First Stage***

We want to assess the causal effect of our indicators describing labor disputes on labor flows. Yet the judicial activity is likely to be endogenous. Our model can be used to discuss the endogeneity problems that we will face when estimating the relations between case outcomes and labor market characteristics. For instance, an adverse shock on the labor market conditions can affect case outcomes. First, according to the legislator,  $F$  compensates the worker for past and future potential wage losses, in particular by taking into account the difficulty of finding a new and comparable job.

The magnitude of  $F$  is therefore likely to be countercyclical.<sup>17</sup> An economic downturn pushes  $\overline{p_w}$ ,  $\overline{p_w^*}$ , and  $\overline{p_w^{**}}$  downwards which results, other things being equal, in higher firing costs. Moreover, if

---

<sup>17</sup> Regressions of our indicators of judicial activity on local unemployment rates show that they are strongly correlated with the cycle (see Table A.1). The cyclical behaviors of collective conflicts have been extensively studied in the literature (see Harrison and Stewart, 1994, or Devereux and Hart, 2010). The evidence about individual disputes is less prevalent (see however Donohue and Siegelman, 1993).

workers exert more effort on the job during a downturn (with an efficiency wage story in mind), the overall distribution of  $p_w$  will shift upwards. Third, economic conditions might also alter the overall distribution of  $p_w$  through judges' behavior. Judges showing a pro-worker bias when labor market conditions deteriorate increase the firing costs faced by the firms (see Ichino et alii, 2003).

Thus we need suitable instruments which explain the average outcomes observed at the level of the jurisdiction and be exogenous to current labor market developments. We claim that the institutional settings of the jurisdictions themselves and the local legal environment provide convincing instruments because, as our model discussed, they are related to case outcomes by affecting various costs of litigation and because their variations **within** each jurisdiction are essentially random.

#### *a) Lawyers*

Our main instrument is the number of lawyers enrolled at the local bar – lawyers of all specialties, not only those specializing in labor disputes, a small fraction of the total -- scaled by total employment of the jurisdiction (“lawyer density” hereafter). In France, each lawyer has to get licensed and registered at the Bar (“barreau”) in order to be entitled to practice. We know the number of lawyers registered at each “barreau” from 1996 to 2006. It allows us to have a local estimate of the number of lawyers by employed worker. As there are fewer bars in France than Prud’hommes jurisdictions (181 versus 264), we match each Prud’homme to the closest bar using shortest route distance and compute the number of lawyers available to employees depending on one single Prud’homme. Using the 1999 Census, the national average is 77 lawyers per 10,000 employees, going from a minimum of 14 (Creuse) to a maximum of 868 (Paris).

A high lawyer density is likely to reduce legal fees thanks to a higher competition (see Siegelman and Donohue, 1995 for a similar argument). It also helps to disseminate legal expertise and judicial knowledge of labor disputes among the population of workers. It should correspond to a lower cost of litigation for the worker ( $k_t$  and  $k_c$  in our model) and hence influence the judicial activity and the case outcomes. This result of our model is true even without assuming that being represented by a lawyer increases the probability of winning.

One could argue that the lawyer’s choice of location depends on local economic conditions. First, labor disputes are only a small amount of the total number of civil cases (11% at the national level<sup>18</sup>). Second, in order to get a license to practice, a lawyer must enroll the local bar which jurisdiction the Prud’homme belongs to. This requirement and the building of a reputation and a clientele induce a low

---

<sup>18</sup> See available on line Info Stat justice (2005) « Une évaluation de l’activité des juridictions en 2004 » n° 80.



mobility of lawyers from one region to another. Moreover, a lawyer typically enrolled the bar the city where she studied and her location preference is likely to be unrelated to the incidence of labor disputes litigation. To see this, first note that there are only twelve law schools spread over French territory (see Figure 5). Then, observe the strong overlap between these areas where lawyers are trained and those that see the strongest increase in lawyer density over time (see Figure 6). Further supporting the identifying assumption that local labor market conditions are disconnected from the increase in lawyer density, lagged labor flows are found to have no predicting power on lawyer density including jurisdiction fixed effects and year dummies (see Appendix Table A.2). Thus the lawyers' density influences judicial outcomes through the cost and the efficiency of the litigation process but are likely to be random *within* a Prud'homme with respect to current labor market developments, therefore making it a plausible instrument.

#### *b) Judges and Clerks*

We also consider as instruments the number of judges and staff in charge of dealing with judicial cases (scaled by the local 1999 labor force). Both categories obviously have an impact on judicial decisions as well. Judicial activity can be modeled as a production function for the case disposition. Beenstock and Haitovsky (2004) using a panel data on Israeli courts find that judges complete more cases as their caseloads grow and complete fewer cases when new judges are appointed to their court. In the case of Prud'homme, the sociological literature<sup>19</sup> supports this result and states that facing an increasing number of cases and having to meet some productivity requirements, judges tend to be more meddlesome implying crossing out more cases for administrative reasons to speed up the process and lighten their burden. More judges or staff would imply a decreasing marginal cost of challenging the dismissal following Buchanan's club theory of public goods and thus increase the firing costs through a smaller  $k_t$ .<sup>20</sup>

#### *Judges*

Prud'hommes' judges are unequally spread over the French territory. Before 1979 when the "Réforme Boulin" took place, the administrative cost of the Prud'hommes was borne by the local administration and their creation mostly depended on a bargaining process between unions, firms, and this local

---

<sup>19</sup> See Bonaffé-Schmidt (1987).

<sup>20</sup> In our approach, the congestion of the labor courts increases the cost of litigation which deters workers either to file or to refuse the settlement and therefore decreases the firing cost. Two recent papers by Besancenot and Vranceanu (2008) and Stahler (2008) relate labor courts to labor markets through the existence of "judicial mistakes". The congestion of the court pushes upward the number of judicial mistakes. Bad type workers – that is workers that have rightly been fired for a personal motive – have an incentive to go to court gambling on a judicial mistake. This is found under some assumptions – and contrasting with our view – to have an increasing impact on the firing costs.

administration. For instance, in those years (before 1979), 6 “départements”<sup>21</sup> out of 95 did not have a single labor court. In 1979, a legislation strongly supported by the Minister of Labor Robert Boulin transformed the financing and made it depend exclusively on central government resources. In addition, at least one labor court had to be present in every zone that also had a civil tribunal (“Tribunal de Grande Instance”). Since then, every additional change in the number of judges within a labor court or the opening of a new labor court depends on the outcome of a bargaining between the unions, the employers’ federations, the local, and the national government. The process is supervised by a national agency (“Conseil national de la Prud’homme”). This system generated strong rigidities with the consequence of essentially freezing the number of judges. This number stayed roughly the same from 1979 until 2008; every bargaining party preferring the status-quo. In 2008, the reform of the judicial map leading to the closing of 66 Prud’hommes out of 264 took stocks of the misallocation of judges across the French territory.

Figures 7 and 8 illustrate the dispersion of the jurisdictions across French territory. We compare the proportion of the judges working at the local jurisdiction with the size of the local labor market in 1991. For similar labor market sizes, the number of judges in some jurisdictions is twice that found in other jurisdictions. Turning to labor court activity, we plot in Figure 9 the average number of cases disposed every year by judges, a measure of their productivity. Hence, in some jurisdictions, judges deal with 10 times more cases than judges in other jurisdictions.

The elections for the Prud’hommes are crucial in France - at least for trade unions - as they are the only way to assess unions’ representativeness at the national level. Over our sample period, 4 rounds of elections took place, in 1992, 1997, and 2002. For each round, we collected from the Ministry of Labor the share of judges affiliated with each union as well as the number of judges by section at the jurisdiction level. The number of judges did not change from 1993 to 2002 (see Table 3). Changes took place in 1992 and after 2002 (see Table 4).

Digging into administrative archives of the French Ministry of Labor, the number of cases brought to labor courts seems to be the main apparent quantitative indicator used to decide these changes.<sup>22</sup> Thus, nine labor courts were closed in 1992 because less than 100 cases were examined in a year. However, not all labor courts with less than 100 cases a year were closed. Figure 10 also shows that, along a very wide range of judges’ productivity, no change took place (30 cases a year per judge being a rough threshold for an increase in the number of judges). Besides, lags of labor flows are unable to predict

---

<sup>21</sup> A French “département” is equivalent to an American county.

<sup>22</sup> In the US, the Administrative Office of the United States Court uses statistics over the average time spent by judges to handle a case of a given type to give an appraisal of judge allocation.

the number of judges (see Table A.2). We try to give a sense of what might go on in the next paragraph.

As explained before, labor courts are divided into 4 “sections” according to the industry of the firm (Agriculture, Retail Trade, Manufacturing, Services) and a fifth section for the “Cadres” (engineers, managers, and executives). CGT, the most important union in France, is traditionally well represented in manufacturing and is reluctant to accept a reduction in the number of judges allocated to the manufacturing section, even if the share of workers employed in the manufacturing industry has declined in the geographical area.<sup>23</sup> To illustrate this point, we regress (using 1993 data) the local share of judges in a given section on the corresponding share of local employment (see Appendix Table A.3). We clearly see that there is no significant association between these shares in manufacturing and that the service industry is locally under-represented.

### *Clerks*

In the vein of our lawyer density indicator, we consider the total number of civil servants from the Ministry of Justice working at the civil court independently of the type of cases they deal with. We obtained the number of “greffiers” (clerks) employed by the Ministry of Justice attached to tribunals in the area of each “Tribunal d’instance”<sup>24</sup>, closest to the labor court (“Staff” hereafter) over the 1992-2004 period. “Greffiers” are civil servants in charge of all the administrative tasks, which include assisting the workers in filing their cases as well as writing the judgment terms.

The allocation of these clerks is centrally set (by the Ministry of Justice). Their allocation planned at the national level responds to budget constraints and changes in the local caseload. As stressed before, Prud’hommes’ cases represent a small share of the total civil case load and their steady number across the years is unlikely to have driven massive reallocations of judicial personnel. We check again that the clerks’ density cannot be predicted by lagged labor flows (see again Table A.2).

The judge and staff densities influence the disposition of the cases through a congestion effect. Their allocation depends on institutional settings which generate outcomes that seem largely disconnected from local economic developments and let us think that they offer the characteristics of good instruments.

---

<sup>23</sup> However, some judges were reallocated from a section to another in 2002, mostly from “Agriculture” and “Manufacturing” to “Trade” and “Services”.

<sup>24</sup> As there are more “tribunal d’instance” than Prud’hommes (460 versus 264), we use again orthodromic distance for the matching.

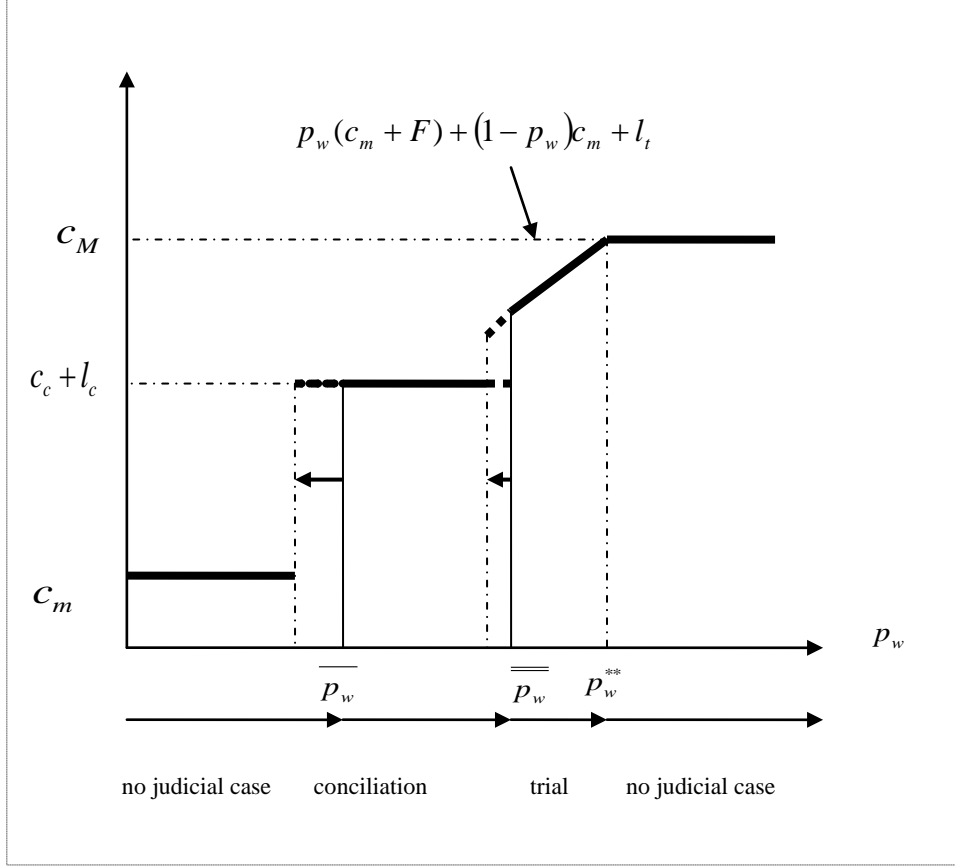
Notice the variety of origins of our instruments: “staff” comes from the allocation by the central government of civil servants into local jurisdictions, “judges” are set by the institutional settings of the Prud’hommes, “lawyers” is related to location preferences of the lawyers.

*c) First Stages*

Table 6 presents the instrumental regressions (first stage) for each of our indicators of judicial activity on the set of potential instruments, controls (year and business cycle indicators, appropriately transformed as will be described later), and jurisdiction fixed effects. For each regression, the variables that are significant (in bold) are used in the IV regressions. The F-statistics (test of the null of the equality to zero of the selected instruments) and its associated p-value are given in the last row of the Table. The instruments are not very strong, except for the filing rate. The effects of the lawyer density on the judicial indicators can be analyzed thanks to our model.

Consider the number of lawyers and assume that an increase in the number of lawyers induces a decrease in the costs of litigation for the worker ( $k_t$  and  $k_c$ ), the decrease being larger for the cost at the trial stage than at the conciliation stage. We assume that the impact on the costs of litigation for the firm is negligible.

Under those assumptions, the model shows that  $\overline{p_w}$  decreases more than  $\overline{\overline{p_w}}$ : more workers file a case since it is less costly, and as a proportion more end the case at the conciliation stage than at the trial stage. Finally, the firing cost increases for the firms (Figure 11). This is consistent with the results of the first stage in Table 6: more lawyers imply a higher filing rate, a higher conciliation rate, and a lower trial rate. It also implies a lower winning rate since the new workers who litigate have smaller probabilities of winning.



**Fig. 11: Firing Cost, Case Outcomes and a Decrease in the Lawyers' Costs**

Judges and staff are not often significant. Staff can be kept as an instrument variable for explaining the conciliation rate, without deteriorating the F-test. We see that more staff decreases the conciliation rate. Judge density is the only valid instrument for the worker lawyer rate.

#### d) Reduced Form Regressions

To check that our instruments are well correlated with the labor flows, we compute reduced-form regressions. The results are presented in Table 7. First, judge and lawyer densities have a strong negative effect on job destructions, resulting in a clear positive effect on net job creation since job creation is less affected. The correlations of staff density on flows are not significant; still it is significant on net job creations.

## 5 Empirical Results

We intend to estimate the following econometric model:

$$Flows_{p,t} = \alpha_1 BC_{p,t} + \alpha_2 BC_{p,t-1} + \beta JudicialInd_{p,t} + \delta_p + \gamma_t + \varepsilon_{p,t} \quad (3)$$

where  $JudIndicator_{p,t}$  is an indicator of judicial activity at the unit of observation  $p$  and for year  $t$ .  $BC_{p,t}$  is a business cycle indicator. The unit of observation is the jurisdiction. Our labor market variables  $Flows_{p,t}$  are the labor flows at the jurisdiction level.  $\delta_p$  is a jurisdiction fixed effect;  $\gamma_t$  is the year effect and  $\varepsilon_{p,t}$  is the residual. In each regression, observations are clustered at the local jurisdiction level. The jurisdiction areas display a large heterogeneity in size (measured by labor force or employment). Half of the jurisdictions account for about 80% of the 1991 total employment. We weight our regressions by the 1999 labor force of the jurisdiction area (results are unchanged when using total employment at the start of the period under review, 1991). Table 5 presents results of regression (3) using OLS. All indicators except the filing rate are significantly correlated with the job destructions; trial rate and winning rate positively correlated; worker lawyer rate and conciliation rate negatively correlated. None of the indicators are significantly correlated with job creations. Finally, worker lawyer rate and conciliation rate are positively correlated with net job creations.

To estimate the parameter  $\beta$  measuring the causal impact of the judicial activity on the labor market flows, we adopt an instrumental approach by projecting our outcome indicators on instruments  $Z$ , business cycle indicators, year dummies and local labor market fixed effects:

$$Outcome_{p,t} = \mu_1 BC_{p,t} + \mu_2 BC_{p,t-1} + \lambda Z_{p,t} + \delta_p + \gamma_t + \nu_{p,t} \quad (4)$$

We cannot use a business indicator such as the local unemployment rate, which is clearly too much correlated to the labor flows. Local unemployment rate probably reflects unobserved economic shocks which impact simultaneously the quality of the cases brought to labor court, bias the judges in their decisions, and affect the labor flows. Thus we build an indicator of business cycle, which takes into account the initial differences and reflects the national business cycle. To do this, we instrument the measure of local business cycles (number of unemployed registered at the local employment agency on the 1999 local labor force) by the *national* unemployment rate (in the spirit of Bartik, 1991 or Blanchard and Katz, 1992) using the following relation:

$$U_{p,t} = \delta_p + \gamma_t + \mu_p U_{aggregate} + \eta_{p,t} \quad (5)$$

Then, we use the *predicted* value  $\hat{U}$  of  $U$  by (3) to compute our exogenous measure of cycle  $BC$  as  $(\bar{U} - \hat{U})/\bar{U}$  where  $\bar{U}$  is the average of the *predicted* local unemployment rate  $\hat{U}$ .

The results from our IV procedure are given in Table 8. Through the results on job flows, we can induce which outcomes reflect increasing EPL, and which ones reflect decreasing EPL. Using Bentolila and Bertola (1990)'s very general result that an increase in separation costs decreases labor

flows; we can deduce that outcomes which are negatively related to flows reflect increasing EPL (see below for a discussion of the assumptions needed for such a conclusion).

Table 8 shows that a larger conciliation rate dampens job destruction when a larger trial rate increases job destruction: settlements are apparently working in favor of workers when trials appear to help firms. Because these two EPL measures have no impact on job creation, the effect on net job creation is of the opposite sign. More trials destroy (net) jobs when more settlements create (net) jobs.

Workers are more often represented by a lawyer when the legal environment is dense (judges, lawyers of all specialties, or legal staff in the court). This in turn dampens job destruction, and the effect on net creation is positive. In unreported results (available from the authors), the fraction of firms represented by a lawyer has no impact on labor flows. Finally, lower cost of legal representations encourages the workers to file a case (last column of Table 8) and a high filing rate is found to deter firms from destroying jobs (last column of Table 9, second panel).

Finally, our results can be summarized as: the fraction of workers represented by a lawyer, the part of settlements, the filing rate are indicators of EPL; on the opposite, the part of trials are negative indicators of EPL. Whatever indicator is used, EPL seem to protect jobs: more EPL increases net job creation, essentially through a decrease of job destruction.

### ***Robustness Checks***

The effects of our judicial outcomes on job flows are large. In order to assess their plausibility we exploit a natural experiment ran at the local level in the jurisdiction of Grenoble. Grenoble is a city situated at the foot of the French Alps in southeastern France. The jurisdiction of the labor court of Grenoble is the 15<sup>th</sup> largest jurisdiction in term of 1999 labor force (254,567). In 1996, in order to facilitate dispute resolution the French Parliament passed a law empowering the judges to mandate a mediator. This law went unheeded since labor courts were already supposed to be a place where the parties were invited to conciliate during the mandatory stage of conciliation. In 1995, the judge Blohorn-Brenneur was appointed at the Circuit court of appeals of Grenoble and decided to exploit the possibilities offered by this law in order to enhance the conciliation process. Starting in 1998, it was done by: sending out an information letter and a questionnaire to the parties in order to increase parties' awareness of mediation, offering mediation and conflict management training to the judges of

Grenoble and organizing specific hearing where mediation services were proposed to the parties<sup>25</sup>. This experiment leads to a strong increase in the conciliation rate from 1998 onwards at the jurisdiction of Grenoble. In order to assess its impact on job flows, we run a simple difference in difference regression of the form:

$$Flows_{p,t} = \alpha_1 BC_{p,t} + \alpha_2 BC_{p,t-1} + \beta \times Grenoble \times Post1998 + \delta_p + \gamma_t + \varepsilon_{p,t} \quad (5)$$

Where Grenoble is a dummy equaling one for the jurisdiction of Grenoble interacted with a dummy equaling one on the treatment period (1998-2003). We estimate in Table XX equation (5) considering firstly the rest of French jurisdictions as the control group. In order to check if local specific shocks might jeopardize the identifying assumption of the difference-in-difference method, we consider as additional control groups jurisdiction of similar sizes (that is displaying a 1999 labor force between 150,000 and 400,000) and the jurisdictions surrounding the jurisdiction of Grenoble: the other jurisdictions in Isère, the ‘département’ where is located Grenoble and the jurisdictions belonging to ‘départements’ contiguous to Isere. The first column of the table displays the parameter  $\beta$  associated to an equation similar to (5) where the conciliation rate has been substituted to our flow variables. The Grenoble experiment increases substantially the conciliation rate making it larger than the national average by around 8 percentage points that is about one standard deviation of the conciliation rate measured across years and jurisdictions. The difference is similar when Grenoble is compared to contiguous jurisdictions and jurisdictions of similar size. Turning to the impact of the experiment on labor flows, as in our instrumental regressions a higher conciliation rate is found to depress job destructions. Although obtained on different time periods and through different identification strategies that can be both interpreted as local average treatment effect as suggested by Imbens and Angrist (1994), the causal impacts of the conciliation rate on job destruction evolves within similar ranges: a one standard deviation increase of the conciliation stage leads to an increase in the job destruction rate of 2-4 percentage points using the difference-in-difference estimates and of 6 percentage point using the instrumental regressions.

As an additional check, we propose a falsification test based on the different provisions protecting from unfair dismissals across types of workers. By law, firing constraints are reduced for workers who have less than two years of seniority. In that case, the severance payment due for the absence of “real and serious cause” is not subject to the minimum amount of 6 months of wage but is limited to a compensatory award that can be nil if the worker found a new job. Therefore, one can assume that the

---

<sup>25</sup> See « La Médiation Prud’homale » by Béatrice Blohorn-Brenneur in « Refondation du droit social, concilier protection des travailleurs et efficacité économique » Gilbert Cette et Jean Barthélémy, Rapport du Conseil Economique et Social. The summary in English of the whole report is to be found page 191-197. The Judge Blohorn-Brenneur founded with others the European Association of Judges for Mediation in 2003.



impact of the judiciary is more limited on the flows of these workers. Using the waves of the Insee French labor force surveys at the ‘département’ level, we compute the ratio of workers that have been dismissed within the year and that had a seniority of less than two years over the number of employees in the private sector. As we are at a more aggregate level and on a shorter period, our instruments lose some of their explanatory powers. However, lawyer density indicator at the ‘département’ level keeps its strong explanatory power of the worker’s legal representation at the labor court. One can see that the negative causal impact of legal representation on job flows remains but as expected is more limited on the inflows of short tenure workers into unemployment.

## 6 Conclusion

This article examines the impact of the enforcement of the Employment Protection Legislation on labour markets for France using an original data set of individual labour disputes brought to court over the years 1996 to 2003. First, we present a simple theoretical model showing that judicial case outcomes are difficult to interpret in terms of EPL. The clearest example is the fraction of cases that go to trials. Firms faced with low litigation costs may well prefer to fire their personnel for personal motive and risk litigation rather than to fire them for economic motives, avoid lawsuits but pay a large separation cost. In this situation, trials are induced by lower firing costs. Workers faced with low litigation costs are more likely to sue the firm. Trials are now induced by higher firing costs.

Second, we exploit our model as well as the French institutional setting and the local legal environment to generate instruments for these endogenous outcomes. For instance, because lawyers tend to open their practice close to the university they went to, changes in their number are unrelated to the number of cases in each labor court except through the litigation costs. Using these instruments, we show that judicial activities have a causal effect on labor flows. More trials cause more job destructions: more trials indeed are a sign of lower separation costs. More settlements, higher filing rates, a larger fraction of workers represented at trial dampen job destruction.

Hence, the web of legislations and the variety of local situations affect the enforcement of legislation and legal outcomes. The nature of real EPL is likely to be the product (or the sum) of multiple effects. It is therefore not surprising that analyses ignoring this dimension yield inconclusive outcomes.

## References

- Algan, Y. and P. Cahuc, (2009), "Civic Virtue and Labor Market Institutions," *American Economic Journal: Macroeconomics*, 1(1): 111-45.
- Andolfatto D., (1992), *L'univers des élections professionnelles. Travail et société au crible des urnes*. Paris, Editions de l'Atelier.
- Autor, D. H. (2003), "Outsourcing at Will: The Contribution of Unjust Dismissal Doctrine to the Growth of Employment Outsourcing," *Journal of Labor Economics*, 21(1), January, 1 – 42.
- Autor D. H., J. J. Donohue and S. J. Schwab, (2006), "The Costs of Wrongful-Discharge Laws," *The Review of Economics and Statistics*, vol. 88(2), 211-231.
- Autor, D. H., A. R. Kuegler, and W. D. Kerr, (2007), "Do Employment Protections Reduce Productivity? Evidence from U.S. States," *The Economic Journal*, 117, June, 189-219.
- Bartik, T. J. (1991), *Who Benefits from State and Local Economic Development Policies?* Kalamazoo, Mich.: W. E. Upjohn Institute for Employment Research.
- Bauer, T., S. Bender, and H. Bonin, (2007), "Dismissal Protection and Worker Flows in Small Establishments," *Economica*, 74 (296), 804-821.
- Bebchuk L. A., (1984), "Litigation and Settlement under Imperfect Information," *RAND Journal of Economics*, 15(3), 404-415.
- Bentolila S. and G. Bertola, (1990), "Firing Costs and Labor Demand: How Bad is Eurosclerosis?," *The Review of Economic Studies*, 57, 3, 381-402.
- Bertola, G. (1992), "Labor Turnover Costs and Average Labor Demand," *Journal of Labor Economics*, 10, 389-411.
- Beenstock M., and Y. Haitovsky, (2004), "Does the appointment of judges increase the output of the judiciary?," *International Review of Law and Economics*, 24, 351-369.
- Bhattacharya, U. and H. Daouk, (2002), "The World Price of Insider Trading," *Journal of Finance*, 2002, vol 57, 75-108
- Blanchard O., and L. F. Katz (1992), "Regional Evolutions," *Brookings Papers on Economic Activity*, 1, 1-75.
- Blanchard, O. and T. Philippon. (2004), "The Quality of Labor Relations and Unemployment", NBER Working Paper 10590.
- Boeri, T., and J. F. Jimeno, (2005), "The effects of employment protection: Learning from variable enforcement," *European Economic Review*, 49(8), 2057-2077.
- Bonafé-Schmidt J. P., (1987), "Les prud'hommes: du conseil de discipline à la juridiction de droit commun du travail," *Le Mouvement social*, 141, Les Prud'hommes XIXe-XXe Siècle, 121-148.
- Burgess, S., Popper, C. and D. Wilson, (2001) "Explaining the growth in the number of applications to employment tribunals 1972-1997." Department of Trade and Industry.
- Card, D., and B. P. McCall, (2009), "When to Start a Fight and When to Fight Back: Liability Disputes in the Workers' Compensation System," *Journal of Labor Economics*, 27 (2), 149-178.
- Chemin, M and E. Wasmer, (2009). "The employment effects of 35-hour workweek regulation in France: using Alsace-Moselle local laws to build a diff-in-diff", *Journal of Labor Economics*, 27 (4); 487-524.

Davis S., and J. C. Haltiwanger, (1992), "Gross Job Creation, Gross Job Destruction, and Employment Reallocation," *Quarterly Journal of Economics*, 819-864.

Dertouzos, J. N., (1988), "The End of Employment-at-Will: Legal and Economic Costs," Report P-7441. Santa Monica, Calif.: Rand Corp.

Devereux P. and R. Hart (2010), "A good time to stay out? Strikes and the business cycle," *British Journal of Industrial Relations* (forthcoming).

Flanagan, M. (1989), "Compliance and Enforcement Decision under the National Labor Relations Act," *Journal of Labor Economics*, 7(3), 257-280.

Freeman R. B., (2004), "Labor Market Institutions Without Blinders: The Debate over Flexibility and Labor Market Performance," NBER Working Paper 11286.

Galdón-Sánchez J. E. and M. Güell, (2003), "Dismissal Conflicts and Unemployment," *European Economic Review*, 47, 2, 127-139.

Harrison A. and M. Stewart, (1994), "Is Strike Behavior Cyclical ?," *Journal of Labor Economics*, 12(4), 524-553.

Ichino, A., M. Polo and E. Rettore, (2003), "Are Judges Biased by Labor Market Conditions ?," *European Economic Review*, 47(5), 913-944.

Lazear E. P., (1990), "Job Security Provision and Employment". *The Quarterly Journal of Economics*, 105 (3), 699-726.

Marinescu, I., (2008), "Are Judges Sensitive to Economic Conditions? Evidence from UK Employment Tribunals," working paper.

Marinescu, I., (2009), "Job Security Legislation and Job Duration ? Evidence from the UK," *Journal of Labor Economics*, 27, 3, 475-486.

Martins, P. S., (2009), "Dismissals for Cause: The Difference That Just Eight Paragraphs Can Make," *Journal of Labor Economics*, 27 (2), 257-280.

Miles, T. J., (2000), "Common Law Exceptions to Employment At Will and U.S. Labor Markets," *Journal of Law, Economics, and Organization*, 16, 1, 74-101.

Munoz-Perez B., and E. Serverin, (2006), "Le sort des demandes prud'homales en 2004," *Bulletin d'information statistique*, Ministère de la Justice. n° 87.

OECD, (2004), *Employment Outlook*, OECD Publication, Paris.

Priest, G. and B. Klein, (1984), "The selection of disputes for litigation," *Journal of Legal Studies*, 13, 1-55.

Siegelman, P., and J. J. III, Donohue, (1995), "The Selection of Employment Discrimination Disputes for Litigation: Using Business Cycle Effects to Test the Priest-Klein Hypothesis," *Journal of Legal Studies*, 24(2), 427-62.

Spier, K. (2007), "Litigation" in *The Handbook of Law and Economics* (A. M. Polinsky and S. Shavell, eds., North Holland).

Stevenson, B. (2007), "The Impact of Divorce Laws on Investment in Marriage Specific Capital," *Journal of Labor Economics*, 25(1), 75-94.

Venn, D. (2009), "Legislation, Collective Bargaining and Enforcement: Updating the OECD Employment Protection Indicators", *OECD Social, Employment and Migration Working Papers*, 89.

### Model Appendix:

The employer dismisses the worker at the minimum cost, instead of paying the maximum severance payments, if:

$$p_f \{p_c(c_c + l_c) + (1 - p_c)[p_w(c_m + F) + (1 - p_w)c_m + l_t]\} + (1 - p_f)c_m < c_M$$

As for the worker, she chooses to challenge her dismissal ( $p_f = 1$ ) if her expected gain at trial or at the conciliation stage is larger than the minimum severance payment:

$$p_w(c_m + F) + (1 - p_w)c_m - k_t > c_m \text{ or } c_c - k_c > c_m$$

Thus the worker chooses to go to court if the gain at trial is large enough ( $p_w(c_m + F) + (1 - p_w)c_m - k_t > c_m$ , that is  $p_w > \overline{p_w} = \frac{k_t}{F}$ ). The worker would prefer the agreement ( $p_c = 1$ ) than the trial when  $p_w(c_m + F) + (1 - p_w)c_m - k_t < c_c - k_c$ , i.e.

$$p_w < \overline{p_w} = \frac{c_c - c_m + k_t - k_c}{F}$$

Yet the firm can refuse the agreement.

On the firm side, the firm dismisses the worker offering the minimum cost if:

$$p_w(c_m + F) + (1 - p_w)c_m + l_t < c_M$$

that is:

$$p_w < p_w^{**} = \frac{c_M - c_m - l_t}{F}$$

We assume that the compensatory award  $F$  is large enough so that when the firm is certain to lose at trial, it is less costly to pay the maximum severance payment. That is:  $c_M < c_m + F + l_t$  and thus  $p_w^{**} < 1$ .

In addition, the firm accepts the conciliation only if it is less costly than going to trial, that is:

$$p_w(c_m + F) + (1 - p_w)c_m + l_t > c_c + l_c$$

which means:

$$p_w > p_w^* = \frac{c_c - c_m - l_t + l_c}{F}$$

In order a conciliation to exist, suing must be a credible threat to the employer. Therefore, we impose that  $p_w^* < \overline{p_w}$  that is  $c_c - c_m + l_c < k_t + l_t$ . In addition, there must a probability range where the worker is better off to conciliate than going to trial. We must have  $\overline{p_w} < \overline{p_w}$  that is  $c_m < c_c - k_c$ . Finally, for the trial stage to exist, the firm must be better off in some probability range to go to trial rather than giving the compensatory award  $c_M$  that protects against any suing :  $\overline{p_w} < p_w^{**}$ .

To summarize, we have five assumptions:

**Assumptions:**

*Condition (1):*  $k_t > c_c - c_m + l_c - l_t$  : the cost of trial is sufficiently large (  $p_w^* < \overline{p_w}$  )

*Condition (2):*  $c_c - k_c > c_m$  : the gain for the worker at the conciliation stage is larger than the severance payment she receives in case of firing for a personal motive (  $\overline{p_w} < \overline{\overline{p_w}}$  ).

*Condition (3):*  $c_c + l_c < c_M$  : the cost for the firm at the conciliation stage is smaller than the severance payment received by the worker in case of firing for an economic motive.

Conditions (1), (2) and (3) taken together allow for the possibility of a **conciliation** stage.

*Condition (4):* The compensatory award  $F$  is large enough so that when the firm is certain to lose at trial, it is less costly to pay the maximum severance payment. That is:  $c_M < c_m + F + l_t$  . It implies  $p_w^{**} < 1$  and excludes an equilibrium in which the law has no deterrent effect, every worker being fired for a personal motive.

*Condition (5):*  $c_c - k_c + k_t + l_t < c_M$  : there is a probability range for a trial to exist. The firm is better off at trial than paying  $c_M$ .

### **Result:**

Under these assumptions we end up with four equilibria:

- $p_f = 0$  and  $p_c = 0$  if  $p_w < \overline{p_w}$
- $p_f = 1$  and  $p_c = 1$  if  $\overline{p_w} < p_w < \overline{\overline{p_w}}$  (with  $p_w^* < \overline{p_w}$  )
- $p_f = 1$  and  $p_c = 0$  if  $\overline{\overline{p_w}} < p_w < p_w^{**}$
- the firm pays  $c_M$  if  $p_w > p_w^{**}$

Figure 4: Number of Filed Cases

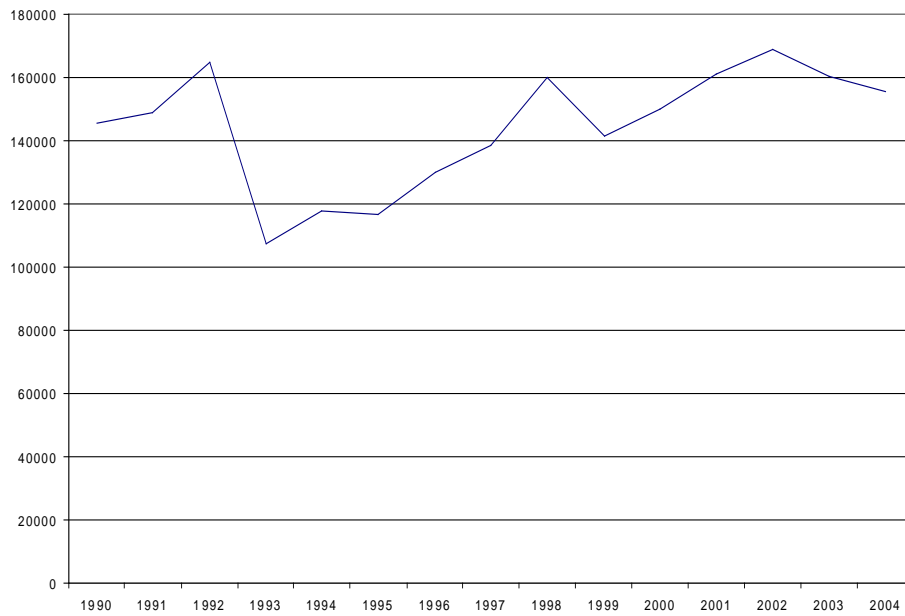


Figure 5: Map of the Universities Training Lawyers

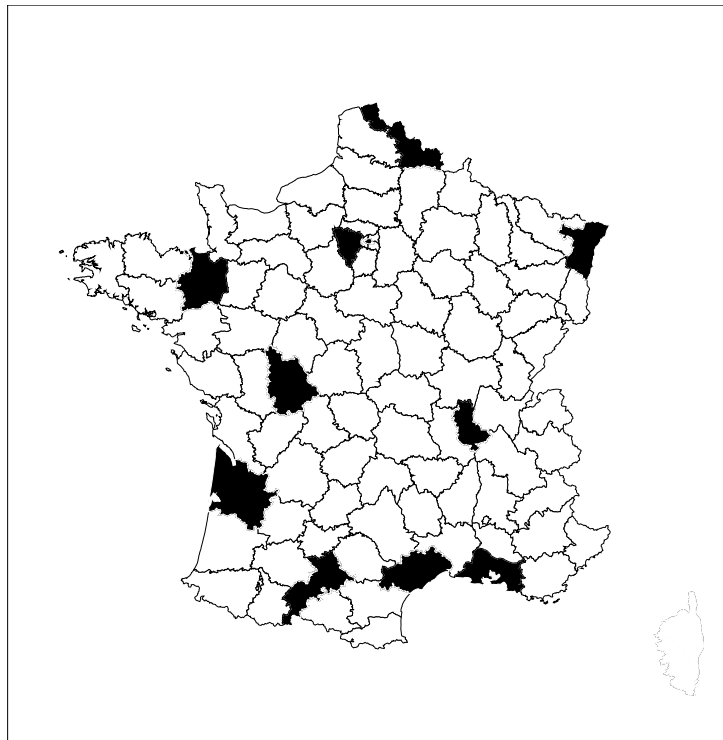


Figure 6: Map of the Changes in the Lawyer Density between 1996 and 2003

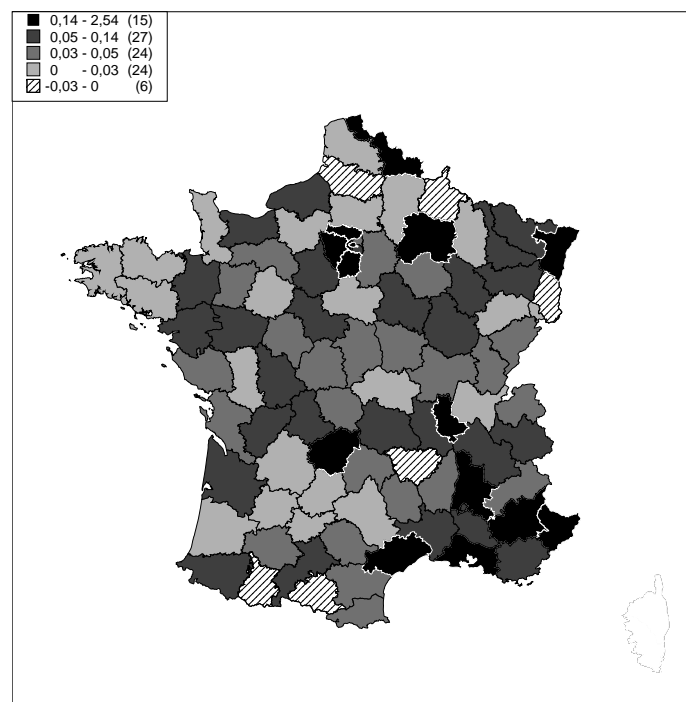


Figure 7: Allocation of Judges

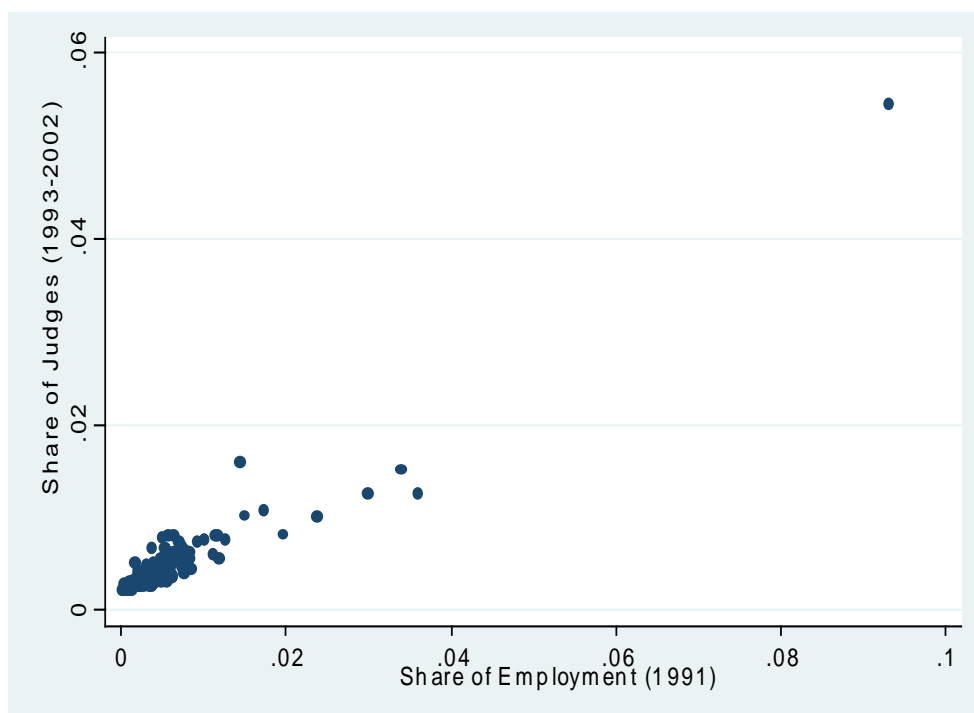


Figure 8: Allocation of Judges (without the 6 Largest Jurisdictions)

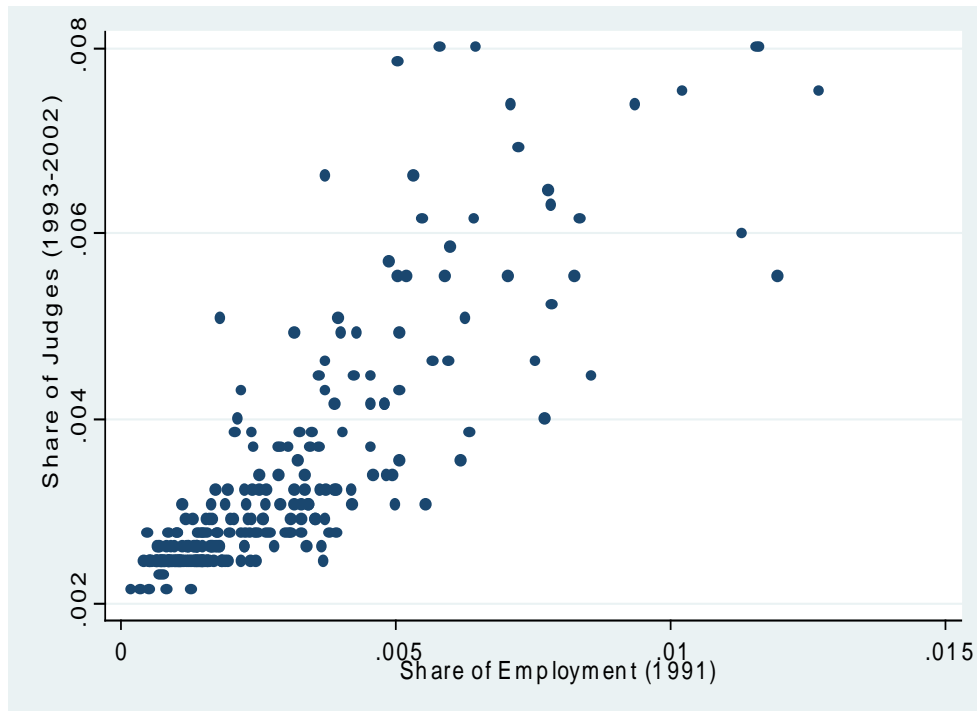


Figure 9: Productivity of Judges across Jurisdictions

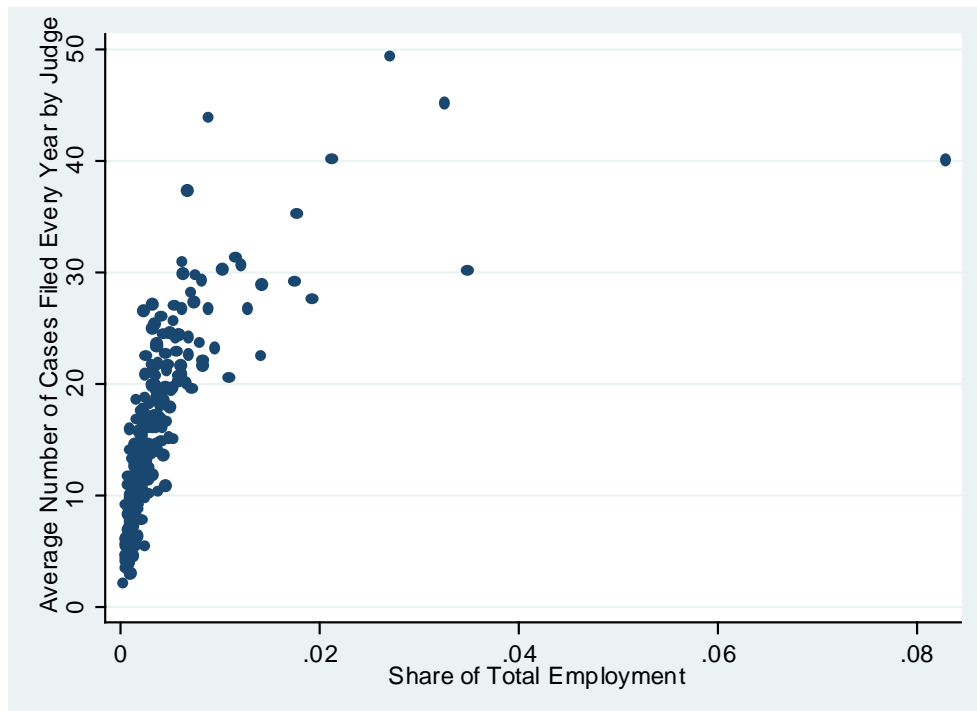




Figure 10: Change in Judges in 1992 and Productivity of Judges

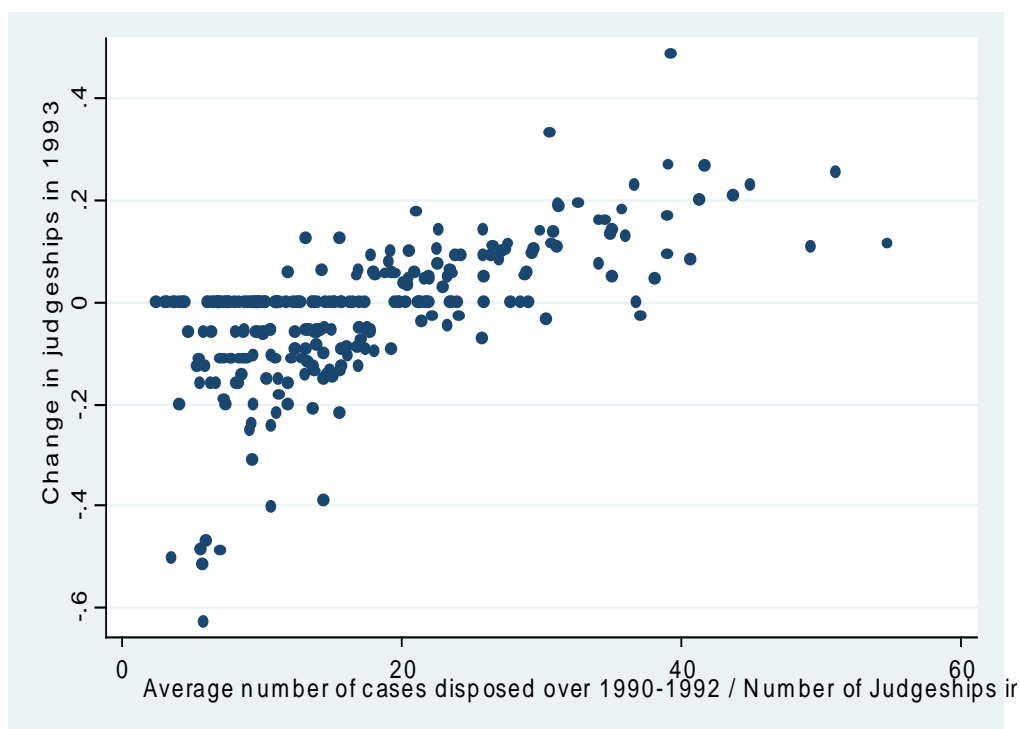


Table 1: Judicial Indicators: Definition of Variables

Names	Definition
Filing rate	Number of cases filed over number of dismissals
Worker Lawyer rate	Number of cases where the worker is represented by a lawyer over the total number of cases
Conciliation rate	Number of cases leading to a conciliation or an agreement between the parties over the total number of cases
Trial rate	Number of cases reaching the trial stage over the total number of cases
Winning rate	Number of cases won by the worker at trial over the total number of cases

Notes: These variables are computed at the jurisdiction level (jurisdiction\*year)

**Table 2: Summary Statistics: Judicial Indicators and Job Flows**

	Mean	Std.	Min	Max
<i>Judicial Indicators :</i>				
Filing rate	0,22	0,11	0,03	0,98
Worker Lawyer rate	0,48	0,15	0,00	0,95
Conciliation rate	0,20	0,09	0,00	0,77
Trial rate	0,61	0,10	0,19	0,95
Winning rate	0,45	0,09	0,09	0,93
<i>Job Flows :</i>				
Job Destructions	0,16	0,04	0,07	0,52
Job Creations	0,16	0,06	0,05	0,71
Net Job Creations	0,00	0,07	-0,63	0,43

Notes: Means of the jurisdiction\*year indicators, over the 264 jurisdictions and the years 1996-2003.

**Table 3: Number of Judges by Section and Change over the Electoral Terms**

		Change in % between term t and term t-1 (t/t-1)		
		1992/1987	1997/1992	2002/1997
	Number of judges in 1987			
Manufacturing	2 213	-15	0	-9
Service	1 266	0	0	11
Trade	1 831	5	0	1
Management	1 278	10	0	4
Total	6 588	-1	0	1

**Table 4: Breakdown of Change in the Number of Judges across the 264 jurisdictions**

	1992 Election			2002 Election		
	Manufacturing	Service	Trade	Manufacturing	Service	Trade
lost 3 judges or more	17	4	4	7	0	0
lost 2 judges	17	0	0	8	0	0
lost 1 judges	16	2	6	27	1	25
no change	44	85	58	56	79	58
gained 1 judges	3	5	17	1	9	9
gained 2 judges	1	2	10	1	5	3
gained 3 judges or more	1	2	6	0	6	4
	100	100	100	100	100	100

Note: read as % of jurisdictions that lost (or gained or no change) x judges in the election year  $t$

**Table 5: Judicial Indicators on Job Flows: OLS Estimates**

	Job Destructions	Job Creations	Net Job Creations
Filing rate	0.0169 (0.0188)	-0.00703 (0.0126)	-0.0239 (0.0212)
R-square	0.43	0.48	0.59
Worker Lawyer rate	-0.0469** (0.0182)	-0.00588 (0.0103)	0.0410** (0.0199)
R-square	0.41	0.47	0.56
Conciliation rate	-0.0439** (0.0222)	-0.00504 (0.0134)	0.0389* (0.0221)
R-square	0.40	0.47	0.56
Trial rate	0.0363** (0.0180)	0.00431 (0.0114)	-0.0320 (0.0209)
R-square	0.40	0.47	0.56
Winning rate	0.0382** (0.0185)	0.00704 (0.0117)	-0.0312 (0.0211)
R-square	0.40	0.47	0.56

Robust standard errors are between parentheses. \* significant at 10%; \*\* significant at 5%, \*\*\*significant at 1%. Observations are for 264 jurisdictions and for the years 1996-2003 (2,112 obs.). Each regression includes jurisdiction and year fixed effects, and local business cycle indicators. 1999 labor force of the jurisdictions is used as weights. Clusters: jurisdiction level.

**Table 6: First Stage Regressions: Effect of Legal Inputs on Judicial Indicators**

	Filing rate	Worker Lawyer rate	Conciliation rate	Trial rate	Winning rate
Lawyers	<b>10.88***</b> (1.661)	5.556** (2.704)	<b>7.897***</b> (2.101)	<b>-8.491***</b> (2.743)	<b>-4.112***</b> (1.434)
Judges	-154.1 (138.4)	<b>567.8***</b> (211.4)	-123.0 (278.4)	376.0 (257.7)	372.5* (220.6)
Staff	-0.204 (6.962)	19.25* (6.962)	<b>-10.76</b> (6.962)	9.847 (6.962)	10.16 (6.693)
R-squared	0.140	0.251	0.276	0.226	0.189
F-test of joint significance (p-value)	20,79 (0.000)	11,93 (0.0003)	12.39 (0.000)	8.11 (0.004)	8.21 (0.000)

Robust standard errors are between parentheses. \* significant at 10%; \*\* significant at 5%, \*\*\*significant at 1%. Observations are for 264 jurisdictions and for the years 1996-2003 (2,112 obs.). Each regression includes jurisdiction and year fixed effects, and local business cycle indicators. 1999 labor force of the jurisdictions is used as weights. Clusters: jurisdiction level. F is the F statistic of the joint significance of the variables corresponding to the parameters in bold.

**Table 7: Judicial Indicators on Job Flows: Reduced-form Regressions**

	Job Destructions	Job Creations	Net Job Creations
Lawyers	-5.734*** (1.181)	-0.832 (1.065)	4.902*** (1.650)
Judges	-511.4*** (139.8)	-158.7* (83.26)	352.7** (149.1)
Staff	6.863 (4.263)	-2.125 (2.037)	-8.989** (3.647)
R-square	0.433	0.457	0.565

Robust standard errors are between parentheses. \* significant at 10%; \*\* significant at 5%, \*\*\*significant at 1%. Observations are for 264 jurisdictions and for the years 1996-2003 (2,112 obs.). Each regression includes jurisdiction and year fixed effects, and local business cycle indicators. 1999 labor force of the jurisdictions is used as weights. Clusters: jurisdiction level.

**Table 8: Judicial Indicators on Job Flows: 2SLS Estimates**

	Job Destructions	Job Creations	Net Job Creations
Filing rate	-0.674*** (0.179)	-0.272** (0.131)	0.402* (0.214)
Instruments : Lawyers			
R-square	0.215	0.314	0.459
Worker Lawyer rate	-1.065*** (0.373)	-0.205* (0.116)	0.859** (0.371)
Instruments : Judges			
R-square	0.375	0.355	0.56
Conciliation rate	-0.772*** (0.216)	-0.0699 (0.129)	0.702*** (0.268)
Test of overidentifying restrictions (p-value)	0.805	0.151	0.856
Instruments : Lawyers and staff			
R-square	0.278	0.446	0.253
Trial rate	0.829** (0.344)	0.140 (0.168)	-0.689** (0.278)
Instruments : Lawyers			
R-square	0.735	0.401	0.132
Winning rate	1.617*** (0.608)	0.273 (0.305)	-1.345** (0.541)
Instruments : Lawyers			
R-square	0,31	0.281	0.191

Robust standard errors are between parentheses. \* significant at 10%; \*\* significant at 5%, \*\*\*significant at 1%. Observations are for 264 jurisdictions and for the years 1996-2003 (2,112 obs.). Each regression includes jurisdiction and year fixed effects, and local business cycle indicators. 1999 labor force of the jurisdictions is used as weights. Clusters: jurisdiction level.

**Table 9: 2SLS Estimates of the Effects of Worker Legal Representation: Falsification Test**

	Job Destructions	Job Creations	Net Job Creations	Dismissed persons with few seniority
Worker Lawyer rate	-0.225* (0.139)	-0.198* (0.117)	0.0273 (0.0948)	-0.0364 (0.0442)
Instruments: Lawyers				
R-square	0.306	0.460	0.508	0.382
F-test of joint significance (p-value) <sup>a</sup>	22,6			

Robust standard errors are between parentheses. \* significant at 10%; \*\* significant at 5%, \*\*\*significant at 1%. Observations are for 93 Départements and for the years 1996-2002 (651 obs.). Dismissed persons with few seniority is the ratio of workers laid-off within the year with a job tenure of less than 2 years. By law, these workers can not obtain the minimum of 6 months of severance payment but only compensatory awards. Each regression includes Département and year fixed effects, and local business cycle indicators. 1999 labor force of the Départements is used as weights. Clusters: Département level.

**Table 10: Impact of the Conciliation Rate: Difference-in-Difference Estimates of the Brenner Experiment**

	Job Destructions	Job Creations	Net Job Creations	Conciliation rate
Treatment Group: Jurisdiction of Grenoble				
Control Group: Rest of France				
Observations = 3393 (263 jurisdictions)				
Grenoble*Post1998	-0.0371*** (0.00185)	-0.0297*** (0.00171)	0.00732*** (0.00178)	0.0833*** (0.00389)
R-square	0.332	0.376	0.463	0.109
Control Group: Jurisdictions of Similar Size				
Observations = 494 (38 jurisdictions)				
Grenoble*Post1998	-0.0414*** (0.00335)	-0.0352*** (0.00376)	0.00624 (0.00388)	0.0642*** (0.00630)
R-square	0.384	0.499	0.560	0.297
Control Group : Jurisdictions within Contiguous Départements				
Observations = 416 (32 jurisdictions)				
Grenoble*Post1998	-0.0206*** (0.00377)	-0.0167*** (0.00282)	0.00384 (0.00409)	0.0711*** (0.00779)
R-square	0.408	0.619	0.604	0.180

Robust standard errors are between parentheses. \* significant at 10%; \*\* significant at 5%, \*\*\*significant at 1%. Each regression includes jurisdiction and year fixed effects. Clusters: jurisdiction level. Grenoble is a variable equal to 1 for the jurisdiction of Grenoble. Post1998 is a variable equal to 1 if the year of observation is after 1998. Grenoble\*Post1998 is a variable equal to 1 for the jurisdiction of Grenoble after 1998. This is the difference-in-difference variable of interest.

## Appendix Tables

**Table A.1: Judicial Indicators and the Business Cycle**

	Filing rate	Worker Lawyer rate	Conciliation rate	Trial rate	Winning rate
Unemployment rate	0.897*** (0.108)	-0.876*** (0.0880)	1.177*** (0.118)	-1.435*** (0.141)	-1.353*** (0.135)
R-squared	0.038	0.046	0.056	0.093	0.076

Robust standard errors are between parentheses. \* significant at 10%; \*\* significant at 5%, \*\*\*significant at 1%. Observations are for 264 jurisdictions and for the years 1996-2003 (2,112 obs.). Each regression includes jurisdiction and year fixed effects. 1999 labor force of the jurisdictions is used as weights. Clusters: jurisdiction level.

**Table A.2: The Impact of Past Labor Flows on Lawyer, Judge and Staff Densities**

	Lawyers	Judges	Staff
Job Destructions (-1)	-0.0004 (0.0003)	-0.0000 (0.0000)	0.0000 (0.0001)
Job Destructions (-2)	-0.0002 (0.0002)	-0.0000 (0.0000)	-0.0000 (0.0001)
R-squared	0.11	0.01	0.12
	Lawyer	Judges	Staff
Job Creations (-1)	0.0001 (0.0004)	-0.0000 (0.0000)	0.0001 (0.0001)
Job Creations (-2)	0.0006 (0.0006)	0.0000 (0.0000)	-0.0000 (0.0001)
R-squared	0.11	0.00	0.12
	Lawyer	Judges	Staff
Net Job Creations (-1)	0.0003* (0.0002)	0.0000 (0.0000)	0.0000 (0.0001)
Net Job Creations (-2)	0.0005 (0.0003)	0.0000 (0.0000)	0.0000 (0.0001)
R-squared	0.11	0.00	0.12
Observations	2103	2904	2860

Robust standard errors are between parentheses. \* significant at 10%; \*\* significant at 5%, \*\*\*significant at 1%. Each regression includes jurisdiction and year fixed effects. 1999 labor force of the jurisdictions is used as weights. Clusters: jurisdiction level.

**Table A.3: (Mis)allocation of Judges by Industry**

Dependent variable: 1993 local share of judges in the industry	Manufacturing	Trade	Service
1993 local share of employment in the industry	0.002 (0.014)	0.203*** (0.015)	-0.451*** (0.031)
Observations	264	264	264
R-squared	0.00	0.19	0.21

Notes: Columns (2) (3) and (4) display the regressions of the proportion of local number of judges allocated to industry  $i$  in the national aggregate on the corresponding proportion of employment. Standard errors in parentheses. \* significant at 10%; \*\* significant at 5%, \*\*\*significant at 1%.