

“Labor Market Outcomes and the Enforcement of the Employment Protection Legislation”

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Abstract

This paper assesses the impact of labor market regulations on labor market outcomes, using measures of individual labor disputes that end up in labor courts. We use an original and exhaustive data set of the individual labor disputes that were brought to the 264 labor courts spread over the French territory over the 1990-2004 period. Using the institutional setting and the legal environment of the labor courts as sources of identification, the number of labor judges, the outcomes of the cases, and the use of judicial expertise are found to influence the job creation and destruction rates. We find that a higher density of judges causes lower job creation, in particular on the extensive margin. Large out-of-court settlement rates, a small rate of dropped cases, or an intensive use of lawyers by workers cause lower job destruction rates. Hence, the various Employment Protection Legislations all attenuate labor flows, but on different margins (creation or destruction, extensive or intensive).

JEL classification: J32, J53, J63, K31

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1 Introduction

In a provocative NBER working paper on labor market flexibility, Richard Freeman (2004) states that -- after more than one decade spent by labor economists and international organizations to convince themselves through inconclusive aggregate data analyses and cross-country comparisons that a negative relationship between employment protection legislation (EPL hereafter) and labor market performance should prevail -- it was time to switch to micro-analysis of workers and firms and experimental methods. In a seminal paper, Lazear (1990), who used the unemployed benefit and severance payment given to a blue collar with 10 years of service as a proxy for labor market flexibility, acknowledges that high overall labor market flexibility can prevail without being captured by any of those two variables. Hence, one might conclude with Freeman that cross-country analyses are hardly convincing since “with only 30 or so advanced countries, highly correlated outcomes, and infrequent changes in institutions, the number of configurations can easily exceed the number of independent data points”. Another point of criticism to these cross-country analyses is that EPL indices typically used in these analyses are deemed exogenous and imperfectly capture how the behaviors of unions, employer federations, or governing regulators change over time.

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Taking stock of these critiques, a recent strand of literature has assessed the impact of EPL within countries. It is typically done by 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 different timing in the introduction of a new EPL in 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, 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 (2003), 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 alii (2007) find no effect of the change in the German EPL exemption for small firms on worker turnover.

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 interest and are they actually binding for the 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 and to which 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 would lead to an approximate number of 1,000 for the whole year in the entire state² (that is about 80 cases for one million of workers).³ In comparison, for France with a very population and GDP similar to those of California, the number of cases in any given year is approximately 200,000. 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 could vary over time. As pointed out by *OECD 2004 Employment Outlook*, even if an employer can 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”. In addition, methodologically, the timing of introduction of a new EPL can substantially alter the results. Indeed, Miles (2000) - using a different classification of cases in identifying the adoption dates - finds no significant effects. Third, problems of endogeneity remain: court interpretation with the ensuing impact might not be exogenous as market

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

³ These figures are taken from Dertouzos (1986).

conditions could have an impact on the leniency of the courts, the introduction of new laws, or workers' propensity to go to court. Ichino et alii (2003), using micro data on labor court cases, focus on this institutional endogeneity of EPL enforcement. They show that in the case of an Italian bank of approximately 20,000 employees among which 409 workers were fired and 86 of them went to trial over more than 20 years, a higher unemployment rate increases the worker's probability of winning. In contrast, Marinescu (2006) - using data from a 1992 survey of Employment Tribunal Applications in Great Britain - finds that a higher unemployment rate leads to more severe decisions against the worker, in particular if the worker already found another job.

Our contribution to the literature on EPL impacts on labor market outcomes is threefold. First, we consider measures of EPL varying across space and time based on the various measures of enforcement of the legislation. 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 1990-2004 period (2 millions of cases) to compute, at the local level of the court and for each year, various EPL indicators characterizing the enforcement of the labor laws: fraction of cases leading to a conciliation between parties, a trial, a worker's victory, dismissed by the court or the worker, fraction of cases where workers and firms 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 (distinguishing in particular the extensive and the intensive margins). 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, even though there is enough variation between as well as within local labor courts. Third, thanks to the precise French institutional setting and those local measures of legal environment, we use various empirical strategies (regression discontinuities, exogenously set environment, instrumentals) to correct for the endogeneity from which estimation of the relation between economic conditions, including labor flows, and enforcement of labor laws might suffer.

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 recommendations⁴. In contrast with the existing empirical literature, our EPL indicators capture some dimensions of the quality of labor relations which according to Blanchard and Philippon (2004) or Algan and Cahuc (2007) are related to the evolution of labor market conditions.

⁴ See Bertola (2003) for a theoretical model considering risk-averse workers and potential positive effect of EPL on welfare.

Section 2 describes the French labor courts institutional setting. Section 3 presents a simple theoretical model relating the enforcement of labor laws to firing costs. Section 4 describes our data sets and provides some descriptive statistics. Section 5 explains our empirical methodology to capture EPL causal effects and presents our regression results on labor flows and employment rates.

2 Labor Courts in France: the Institutional Setting

2.1 French Firing Laws

Three types of events may trigger the firm's decision to fire a worker: a grave misconduct, a lay-off due to a slowdown in the business activity, or an insufficient level of competence. Under the current French law, the separation should be declared as a redundancy (or economic dismissal). However, 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 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 as "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 totally 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).

As pointed out by Galdon-Sanchez and Guel (2003), EPL legislation in European countries may give rise to a double moral hazard problem: a worker fired for misconduct has an incentive to sue for unfair dismissal and a firm has an incentive to label "misconduct" a separation which, in reality, is a redundancy. Thus, even if this phenomenon is obviously hard to detect in the data, the proportion in France of dismissals for economic reasons decreased from 61% in 1993 to 24% in 2004.

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. There are several labor courts in each Prud’homme. 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 can be seen 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. All French establishments are allocated to one Prud’homme. 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. Unemployed can also vote but have to enroll on the list by themselves. On the employer side, in addition to employers and business owners, employees entitled to take firing or hiring decisions can also vote for employer representatives.

Prud’hommes are supposedly not very formal and should be seen as conciliation boards. Prud’hommes 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. If they do not, the case is judged. If 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.

In the 90’s, 264 Prud’hommes were spread all over metropolitan France, a labor court being at most within a radius of 30 miles from any establishment. Even though a majority of plaintiffs were represented by a lawyer, going to labor court was not necessarily expensive. For instance, local administration provides a list of benevolent specialists (former labor inspectors, for example) who are willing to assist workers. Furthermore, low-income workers are eligible for financial help.

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".⁵

In case of an emergency, a summary judgment can be made. However, these judgments are only temporary and might be overruled afterwards. In this paper, we do not consider these summary judgments.

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. This crossing out is less severe than a "null and void" classification. The worker can reinstate her case at the point it has been crossed out and does not have to restart the whole process. This crossing out can be decided by judges but it can also be the outcome of the plaintiff's initiative.⁶ 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%).⁷ 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.

2.3 Recent Changes

The legal environment did not change substantially during our sample period (1990-2004).⁸ In the relatively recent past of an institution officially founded in 1806⁹, a 1979 bill radically changed the institutional settings of the Prud'hommes. First, it extended the number of Prud'hommes across France in order to guarantee an equal access among workers. Second, it ended the majority rule for electing representatives which resulted in a more diverse composition of each Prud'homme. Third, it funded the Prud'hommes by the central administration (Ministry of Justice), an important feature in our analysis since national principles ruled funding which entailed much less sensitivity to local changes in the economic environment.

⁵ Munoz-Perrez and Serverin (2006).

⁶ In 2004, only 27% of crossed out cases were reinstated.

⁷ In a very vast majority of the cases won by the worker, the worker is not reinstated but receives a compensatory award.

⁸ Apart from minor changes related to the application of the working time reduction and the 35 hours workweek.

⁹ Prud'hommes can be traced back to the Middle Ages.

3 Litigation and Firing Cost: a Simple Theoretical Model

We do not study here the theoretical impact of firing costs on labor market variables. This has been extensively examined elsewhere (see Bertola and Bentilola, 1992). We just try to illustrate how enforcement of labor laws is related to firing costs. Thus, we need to model the incentives for an employer to commit an unfair dismissal and the incentives for a worker to challenge it. We depart from the traditional model of litigation proposed by Priest and Klein (1984) or Bebchuk (1984) 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. The employer can deliberately choose either lawful or unlawful behavior in firing a worker. In the latter case, he incurs a lower cost (c_U) if the dismissal remains unchallenged by the worker. This cost c_U is lower than the cost of a lawful dismissal c_L . Yet the firm has to take into account the probability that the worker files a suit, p_f , and the probability that the worker wins, p_w . Given the numerous outcomes a case might have, we could broadly define p_w as the probability of the worker to extract something from the judicial process whether it will be through a formal agreement in front of the court or through a conciliation step in the “shadow of the law” after the case having been filed. For clarity, we define p_w as the worker’s probability of winning at trial.

There is an uncertainty surrounding the decision of the judge because the firm can disguise the truth - possibly with the help of legal counseling- or because of the potential time-inconsistency of the labor court decisions or the novelty of the case. In case of unlawful behavior, we write the expected firing cost as:

$$E(c) = p_f [p_w (c_L + F) + (1 - p_w) c_U + l] + (1 - p_f) c_U$$

Where F is a compensatory award for the worker and l is the firm’s litigation cost. The marginal benefit of unfair dismissal of the firm is:

$$MB_{UD} = p_f [p_w (c_L + F) + (1 - p_w) c_U + l] + (1 - p_f) c_U - c_L$$

As for the worker, she chooses to challenge her unfair dismissal if her expected gain at trial is larger than the unlawful severance payment:

$$p_w (c_L + F) + (1 - p_w) c_U - k - c_U > 0$$

k being the cost of litigation for the worker.

Facing an unfair dismissal, the worker sues as soon as:

$$p_w > p_w^* = \frac{k}{(c_L + F - c_U)}$$

Hence, the employer dismisses the worker unfairly if:

$$p_w(c_L + F) + (1 - p_w)c_U + l - c_L < 0$$

That is:

$$p_w < p_w^{**} = \frac{c_L - c_U - l}{c_L + F - c_U}$$

As soon as the sum of litigation costs is below the difference in “direct” firing costs (that is $c_L - c_U$), there may be 3 equilibria: a low probability equilibrium where the firm does not comply as the employer will not be sued in doing so, a medium range p_w equilibrium where the firm does not comply even if the employer is sued since unlawful behavior remains less costly, and a high range p_w equilibrium where the firm complies given the cost of a lawsuit. An increase in p_w is associated with a shift from one equilibrium to another along the curve of increasing firing costs. The filing rate should decrease for high levels of p_w since the firm has an incentive to lawful behavior.

We consider a cumulative distribution function G for the probability of worker’s victory. We assume that this distribution is the same in each Prud’hommes area but that the truncation of this distribution varies over the areas according to institutional factors. A firm willing to fire L^* workers will face the expected firing cost:

$$E(fc) = [G(p_w < p_w^*)c_U + G(p_w^* \leq p_w < p_w^{**})c(p_w) + G(p_w^{**} \leq p_w)c_L]L^*$$

Where $c(p_w)$ is an increasing function of p_w bounded by c_U and c_L : $c_U \leq c\left(\overset{+}{p_w}\right) \leq c_L$.

An increase in the worker’s litigation cost increases p_w^* since fewer workers are likely to file an unfair dismissal case. An increase in the firm’s litigation cost will encourage the firm to adopt a lawful behavior while the firing cost is at its upper bound. An increase in F pushes downward p_w^* . Cases of lower quality (e.g. low p_w) might be filed. At the same time, a larger award at trial might encourage employers to adopt lawful behavior.

The parameters of the model can be related to the endogeneity issues that we will face when estimating the impact of labor regulation enforcement indicators on labor market characteristics. For instance, reverse causation from labor market characteristics to unfair dismissal outcomes might appear and the deterioration of labor market conditions can influence our EPL indicators. First, according to the legislator, F compensates the worker for past and future potential wages loss taking into account the difficulty to find a comparable new job. F is likely to be countercyclical (vis-à-vis labor market tightness). An economic downturn pushes p_w^* downwards and p_w^{**} upwards which

results, other things being equal, in higher firing costs¹⁰. Second, drawing on an efficiency wage story, we might on the contrary suppose that, facing a higher penalty when they lose their jobs, workers put more effort on their job during a downturn and those who are fired are in better position to dispute the firing such as the overall distribution of p_w shifts upward. 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).

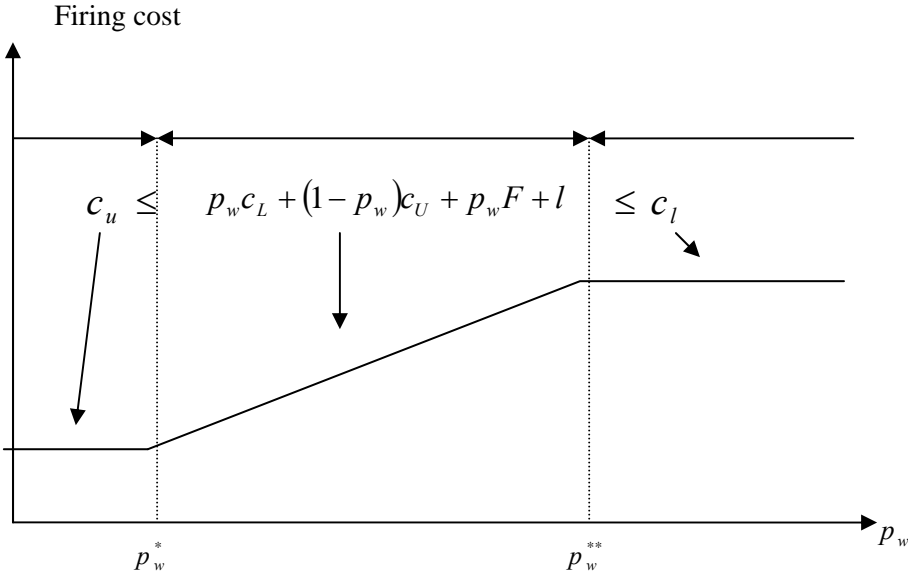


Fig. 1 : Firing cost and enforcement of the labor laws

In our instrumental approach we search for sources of changes in the distribution of p_w unrelated to current local labor market conditions. Aggregating labor flows at the level of the Prud'hommes area, we interpret differences in local institutional settings and local characteristics as variations over litigation costs (k and l) across areas and time. Labor judges and judicial clerks involved in labor disputes are unequally distributed over the French territory leading to differentially congested labor courts. This implies an increasing marginal cost of challenging the dismissal following Buchanan's club theory of public goods and thus a decrease in the firing cost through a higher k . The choice of legal representation influences the expected gain from the judicial process for both parties. Once a case has been filed, the worker and the firm trade off higher probability to prevail for a larger cost of

¹⁰ Empirically, Siegelman and Donohue (1995) find that cases of employment discrimination rise in downturns and are more likely to be lost. In our model, it will mean that the decrease in p_w^* more than offset the decrease in p_w^{**} when computing the average worker's victory rate.

representation, by hiring a lawyer or not. One might plausibly assume that a larger lawyer density induces a stronger competition among them and a higher rate of return for the use of legal services. When the worker takes a lawyer, it increases his chance of success p_w and this has an unambiguous impact on the firing costs. Judges at labor court in France are union members and their behaviors affecting p_w are likely to be shaped by a long tradition of industrial relations adopted by their union at the national level and loosely related to local economic developments. We will discuss more in depth the exogeneity of these indicators in section 5.

4 Data Set and Descriptive Statistics

4.1 Individual Cases Data Set

4.1.1 Firms' and Workers' Characteristics

Our data source on individual cases comes from administrative records made at the level of each Prud'homme and collected by the statistical department of the French Ministry of Justice. Their primary goal is to monitor labor courts' activities with an emphasis on speed of treatment. The data source is exhaustive for the period 1990 to 2004. It includes approximately 2 millions of individual cases.¹¹

Apart from years 1993, 1994 and 1995, 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 1 and Burgess, 1999).

For each case, the sex and age of the employee-plaintiff is recorded. There is no precise information on her skill-level in the firm. Nevertheless, the "managers" section of the Prud'hommes only deals with high-skill employees and managers. Similarly, since low-income workers are eligible for financial help (13% of the cases), eligibility can be used as a low-income indicator. Approximately one half of these cases are susceptible of appeal, which implies that the sums at stake are larger than 5,000 euros (in 2005). 53% of the employees are represented by a lawyer.

As for firms' characteristics, we know the industry, the size, and the Prud'hommes jurisdiction of the employing firm (i.e. the industry broadly defined). However, we can only differentiate between firms with more and firms with less than 10 workers. The size of the firm has to be known by labor court judges because labor laws differ for small firms; in particular, they are less stringent and try to ease the

¹¹ We will not consider the 2% of cases involving employers as plaintiffs.

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.

For each case, the starting date, the ending date, the motives for dismissal, and the court decision are recorded. An average case takes one year (343 days) with a standard deviation of 9 months.

4.1.2 EPL Enforcement Indicators

Using the individual cases data set, we are able to compute several EPL indicators for each Prud'homme. A case can follow various paths and every grouping is somewhat arbitrary. For example, as what we call "agreement" is registered by the court under the acceptance of both parties, one can argue that it can be interpreted as a winning case, a rational plaintiff stopping the judicial process only if her own interests have been satisfied. On the contrary, she might realize the low quality of her case in terms of probability of winning and then renounce her claims. We start by computing the following EPL indicators using the most disaggregated classification: "winning" (resp. "losing", "null and void", "crossed out", "conciliated", "agreement" and "tied") is computed as the ratio of the cases classified as worker's victory (resp. defeat at trial, null and void, crossed out, conciliated, having led to an agreement, having been judged by a professional judge) in year t over the number of cases disposed in year t . We then group cases in "agreed" (cases conciliated or having led to an agreement), "dropped" ("null and void" or "crossed out") and "trial" (cases having reached trial), see Table 1. We also compute the worker's victory rate at trial ("victory"). A simple logistic analysis using individual data is presented in Table A.1.

Then, we can rank a priori our EPL indicators according to the cost to the firm if each outcome. A case being dismissed by the court ("Null and Void") or by the worker ("crossed out") is the most favorable outcome for the firm. Even if one can argue that these "dropped" cases might correspond to agreements not reported to the court, it is safer judiciary speaking for both parties to report the deal to the Prud'homme. A case reaching trial and being won by the worker is the least favorable outcome. A case leading to a firm's victory at trial is not necessarily less costly than one classified after conciliation or agreement since it implies additional litigation costs. In the theoretical literature, the litigation process is usually considered as economically inefficient as a similar outcome could have been reached otherwise by both parties at lower costs by relying on a bargaining process. Hence we can consider that a case reaching trial is likely to be more costly than a case conciliated or dismissed. Beyond the outcome of the case, we use the fraction of firms and workers that are represented by a lawyer during the judicial process as an additional -- and more direct evaluation of the costs related to the suits -- EPL indicator (see again Table 1).

About 60% of cases ended by a trial, among which 75% led to a worker's victory (see Table 2). Despite the mandatory step of conciliation, only 11% of the cases ended at this stage. Taking into account cases that led to an agreement notified to the court or to a withdrawal on the worker's side, at least 20% of the filed cases led to an agreement. 20% is also the proportion of cases having been dropped. All EPL indicators display a very strong variance over time and across Prud'hommes. In comparison with what is observed in a country such as the UK, a large fraction of workers and firms are represented by a lawyer despite other available means of representation. Despite the conciliation step which promotes a quick and costless resolution of the cases, labor disputes seem to induce important litigation costs.

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 filed cases by unemployed workers hovered around five percent over the same period.

Cross-country analyses of EPL are built using aggregate indices ranking countries in terms of labor market flexibility as defined using the written legislation. This approach leaves aside the degree of law enforcement, conditional on the rules as well as cyclical variation. Numbers in Table 2 as well as the very large filing rate (around 25% of dismissals are contested in France) should lead us to conclude that the degree of enforcement of labor regulations is very high in France. A regression of the different indicators of outcomes on local measures of the business cycle shows that the enforcement behavior of these regulations is strongly correlated with the cycle (see Table A.2) and that traditional labor regulation indices are highly imperfect in this respect. A high unemployment rate is associated with a high trial rate and a small number of "agreed, conciliated, or dropped" cases. By contrast, the worker's victory rate seems to be less cyclical. If the number of unemployed workers is correlated with the number of fired workers, we can compute a filing rate. The same table shows that downturns are characterized by a low filing rate but a high litigiousness, workers being less willing to give up their case.

4.2 Local Employment Data Set

4.2.1 Labor Court 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 within a "département") for each establishment. We compute a set of Davis and Haltiwanger (1992) indicators

over the 1990-2004 period: job creation (both at the extensive and the intensive margin), job destruction (both at the extensive and the intensive margin), and net job creation variables over the 1990-2004 period (using Haltiwanger (1989)'s definitions). These measures are aggregated by industry (service, trade, manufacturing) and size of the establishments (more or less than 10 employees) at the city level as well as at the Prud'homme level, using a 1999 correspondence between cities and Prud'hommes provided by the Ministry of Justice. In comparison with cross-country analyses, these indicators also show a high heterogeneity across periods and across the 264 areas.

To measure local unemployment, we use the number of unemployed as registered at the National Labor Agency (ANPE) for each city as well as the city labor force as measured at the 1999 Census. Finally, from 1997 on, we are able to distinguish the reasons for losing one's job (economic or personal dismissal, entry into the labor force, end of temporary contract...).

4.3 Election Data Set

The elections for the Prud'hommes are crucial in France - at least for the trade unions - as they are the only way to assess unions' representative character at the national level. Over our sample period, 4 rounds of elections took place, in 1987, 1992, 1997, and 2002. For each round, we collected the share of judges affiliated with each union as well as the number of judges by section at the Prud'homme level. Union shares of votes are rather stable over time but display a great deal of heterogeneity across Prud'hommes (see Table 3). The number of judges did not change from 1993 to 2002 (see Table 4). Changes took place in 1992 and after 2002 (see Table 5). For the 1992, 1997 and 2002 rounds, we have the turnout rates and the number of workers who were enrolled in the electoral lists for each Prud'homme.

4.4 Additional Judicial Data

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 (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). From our micro data set on Prud'hommes cases, we are able to compute the number of workers who were represented by a lawyer by labor court and year. We observe a very high correlation (0.68) between the lawyers' densities

computed from these two different sources. Lawyer data cover a shorter period than other variables (1996-2004).

In addition, we obtained two other measures of labor laws enforcement: the number of “greffiers” (clerks) employed by the Ministry of Justice attached to tribunals in the area of each “Tribunal d’instance”¹², closest to the labor court (“Staff” hereafter) over the 1992-2004 period¹³ and the number of greffiers directly employed at the local labor court but over a shorter period (1997-2004). “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. Their allocation is centrally set (by the Ministry of Justice).

5 Identification Strategies

5.1 Using Courts Minimum Size

5.1.1 Institutional Principle

Prud’hommes’ judges are unequally spread over French territory. Before 1979 when the “Réforme Boulin” took place, the 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 administration. For instance, in those years (before 1979), 6 “départements”¹⁴ 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 with 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, employers’ federations, local, and 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 since 1979; every bargaining party preferring the status-quo.

Figures 2 and 3 illustrate the dispersion of the Prud’hommes across French territory. We compare the proportion of the judges working at the local Prud’hommes with the size of the local labor market in

¹² As there is more “tribunal d’instance” than Prud’hommes (460 versus 264), we use again orthodromic distance for the matching.

¹³ Data linearly interpolated for 1993 and 1994.

¹⁴ As mentioned above, a French “département” is equivalent to an American county.

1992 (Figure 2, with Figure 3 eliminating the 6 largest Prud'hommes). For similar labor market sizes, the number of judges in some Prud'hommes is twice that found in other Prud'hommes. Turning to labor court activity, we plot in Figure 4 the average number of cases disposed every year by judges, a measure of their productivity. Hence, in some Prud'hommes, judges deal with 10 times more cases than judges in other Prud'hommes.

Judges are elected in December. Some changes took place in 1992 (in comparison with 1987) and in 2002 (see Tables 4 and 5). 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.¹⁵ 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 5 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 the change in the number of judges that occurred in 1993.¹⁶ We try to give a sense of what might go on in the next lines. 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), see Table 6. We essentially do not examine Agricultural sections. Labor court elections in France are the only way to gauge the representative character of a union and are critical for them. 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.¹⁷ 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 Table 7). We clearly see that there is no significant link between these shares in the manufacturing sector and that the service industry is locally under-represented.

Judges are allocated to the different sections of their courts (see above). Each section has an even number of judges, at least two representing employees and, therefore, at least two representing employers. This minimum number of judges in any section, four as obvious from the above description, is in fact quite a lot for many small labor courts, and sometimes smaller sections (see Figure 6). Therefore, a fraction of labor courts have four judges in many of their sections, often in all. However, because it is a minimum, the number of judges is likely to increase in larger labor courts. Indeed, the above Figures show that the allocation of judges appears to obey to principles that are slightly less clear. However, we believe that we can use this minimum rule to identify some effects,

¹⁵ 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.

¹⁶ Results available from the authors.

¹⁷ However, some judges were reallocated from a section to another in 2002, mostly from "Agriculture" and "Manufacturing" to "Trade" and "Services".

which should be local and apply to the smallest zones. Because the number of judges, as shown above, did not change at all between 1993 and 2002, this first analysis will focus exclusively on this period.

5.1.2 Estimation and Results

Because there is no variation in the number of judges among these sections or courts, we use the ratio of judges to the (active) population depending from this labor court at the 1999 Census.¹⁸ We restrict attention to those sections having exactly 4 judges and estimate the following equation:

$$Flows_{p,s,t} = \alpha(Judges_{p,c} / Pop_{p,1999}) + \beta x_{p,90} + \gamma z_{p,t} + \varepsilon_{p,s,t} \quad (1)$$

where flows are job creation, job destruction, net job creation (both at the intensive and the extensive margins) measured across all cities depending from labor court p , in section s , at date t , where x captures the initial conditions before our analysis period 1993-2002, and ε is a statistical residual. Results are presented in Table 8. Each column corresponds to a flow, and each panel to a different section of Prud'hommes (as mentioned in the Data Section, it is not possible to compute flows for those workers depending on the "Cadres" section). The flows are computed using those establishments that depend on each different section, i.e. "manufacturing" only comprises those establishments that belong to the relevant industries. The control variables mentioned just above include year and local business cycle indicators, 1999 labor force and 1999 labor force squared, 1991 employment composition by industry and firm size, and 1991 caseload composition by industry and firm size. These control variables try to capture the initial conditions and the specificities of each labor court, observed as well as unobserved, in the spirit of a matching framework. Most coefficients are negative, some being significantly so. A higher density of judges tends to attenuate job creation and job destruction, a large fraction coming from the extensive margin (creation or destruction of establishments). The effect is particularly strong in the trade sections and is present in the services section.

A similar analysis is performed for labor courts that have exactly 16 judges (four times four sections, manufacturing, trade, services, and cadres). The period and the control variables are exactly similar to those presented just above. Results are presented in Table 9. The estimates are very similar to those for the analysis by section, with negative coefficients for all flows. There is no net impact but a clear impact on the creation (extensive) margin and a slightly smaller, and less significant, effect on the destruction (extensive) margin.

We performed various robustness checks, restricting the time period by focusing on the last years of the sample period or by including (for the first analysis) sections with 6 judges, with essentially similar results.

¹⁸ All results are robust to using other measures of the size of the region.

5.2 A Regression Discontinuity Approach

5.2.1 Matching Cities

The first analysis, just above, shows that the judge density can be interpreted as a measure of the strictness of employment protection legislation since it decreases labor flows. We will see other elements in support of such a statement in the following subsections. However, as the number of judges does not change over most of the period under review, and did not change between 1993 and 2002, we were unable to fully deal with unobserved Prud'homme heterogeneity using fixed effect regressions, other than by controlling for the most relevant observables measured during the period preceding our analysis. However, in the next paragraphs we take advantage of the geography of France to implement a regression discontinuity that should help us deal with potential endogeneity bias that might arise if the allocation of judges depended on local economic conditions.

France has more cities than the rest of the European community, added together. Hence, many cities are close one from another and, in most countries they would have merged at some point. For many complex political reasons, this is not so in France. Hence, very close cities may well depend on different labor courts, either between “départements” or within “départements” (a département is an administrative unit roughly corresponding to an American county, there are 100 départements in metropolitan France). Among the population of the 36,562 cities of metropolitan France, we match each city with its closest and second closest neighbors according to the orthodromic distance (shortest distance). We select the matches where both cities do not belong to the same Prud'hommes jurisdictions (3,993 cities). We focus on the period over which there has been absolutely no change in the number of judges (1993-2002). We match our database of selected cities with our INSEE Sirene files that provide labor flows at the city level. About 14% of the selected cities did not experience any labor flows over the period because (private) total employment was nil. 76% are present over the whole period. Hence our final sample comprises 3,109 cities. The selected cities are very close: the last centile being at 7,8 km and the average distance being equal to 3,6 km. In 1999, the median population of these cities is approximately 400 inhabitants. Each Prud'homme is at least represented by one match in our data set, the maximum being for the Prud'homme of the city of Tours represented by 24 matches.

5.2.2 Estimation and Results

We estimate the following equation:

$$Flows_{c,p,t} - Flows_{c',p',t} = \alpha \left(Judges_p / Emp_{1991,p} - Judges_{p'} / Emp_{1991,p'} \right) + \delta_t + \varepsilon_{cc',t} \quad (2)$$

where c and c' are the matched cities and “judges” is the judge density of the jurisdiction of the Prud'homme they belong to. δ_t are year dummies. We do not apply any weight and cluster the

observation at the level of the match. Tables 10 and 11 present the estimates. In Table 10, we restrict attention to cities with less than 5,000 inhabitants. Table 11 includes all cities. Consistent with our previous analysis, a higher judge density reduces job creation, in particular on the extensive margin. The net outcome is also negative.

5.3 An Instrumental Approach

Until now, we essentially focused on judges, an indirect measure of EPL enforcement. Indeed, judges by their decisions “implement” employment protection. We presented in Table 1 our measures of these decisions, properly aggregated to have a simple enough view of a complex sequence of outcomes. As clearly appears, these decisions – dropping cases, workers’ victories or losses, settling cases, going to trial, or the duration of a case – are all depending on various factors. In what follows, we try to examine the causal impact of these EPL measures on employment flows. We essentially rely on tentative instruments, based on various institutional arrangements, which we describe in the next subsections after having presented the simple estimating framework that we adopt.

5.3.1 Estimating Equations

We intend to estimate the following econometric model:

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

where $EPL_{p,t}$ is an enforcement of EPL indicator at the unit of observation p and for the year t . $BC_{p,t}$ is a business cycle indicator. The unit of observation is the Prud’homme. Our labor market variables $Flows_{p,t}$ are the labor flows at the Prud’homme level. Δ_p is a Prud’homme fixed effect; γ_t is the year effect and $\varepsilon_{p,t}$ is the statistical residual. In each regression, observations are clustered at the local labor market area level. The labor market areas defined by the jurisdiction of the Prud’hommes display a large heterogeneity in size (measured by active population or employment). Half of the Prud’hommes account for about 80% of the 1991 total employment. We weight our regressions by the 1999 active population of the Prud’hommes area (results are unchanged when using total employment at the start of the period under review, 1991). Table 12 presents results of regression (3) with and without fixed effects using OLS. No clear pattern is apparent in this Table and, again, it is impossible to know if an increased number of conciliations causes job destructions or job destructions cause more conciliations in court.

Therefore, to estimate the parameter β measuring the causal impact of the judicial activity on the labor market flows, we adopt an instrumental approach by projecting our EPL indicators on instruments Z , business cycle indicators, year dummies and local labor market fixed effects:

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

Clearly, the business cycle BC is endogenous and also needs to be instrumented: unobserved economic shocks might simultaneously impact the quality of the cases brought to labor court, bias the judges in their decisions, and affect the labor flows. 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 or Blanchard and Katz) 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} .

5.3.2 Instruments

Suitable instruments for estimating the parameter β must explain the average outcomes observed at the level of the labor court and be exogenous to current labor market developments. We claim that the institutional settings of the Prud'homme itself and the local legal environment provide convincing instruments because their variations **within** each Prud'homme are essentially random.

Lawyers

One of our instruments is the number of lawyers enrolled at the local bar scaled by total employment of the Prud'homme area or the département in 1991 (“lawyer density” hereafter). 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 and hence influences the outcome of the case.¹⁹ 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 level²⁰). Second, in order to get a license to practice, a

¹⁹ Logit regressions using our data set of individual cases reaching the trial stage shows that hiring a lawyer against a unrepresented firm increases the probability of worker victory by about 4%.

²⁰ See available on line Info Stat justice (2005) « Une évaluation de l’activité des juridictions en 2004 » Numéro 80.

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 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. Supporting this, lagged labor flows are found to have no predicting power on lawyer density including fixed effects and yearly dummies (see Table 13). 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.

Clerks and Judges

We also consider as instruments the number of judges and staff in charge of dealing with judicial cases (scaled by the local 1991 employment or by the local 1999 active population). They are likely to have an impact on judicial decisions as well. Judicial activity can be modeled as a production function for the case disposition. Benstock 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 literature²¹ supports this result and state 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. Less judges or staff would imply more dismissed cases which clearly diminish the firing costs of the firm.

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. 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 Table 13).

As mentioned before the number of judges of Prud'hommes changed only twice over our analysis period and hence the variation in the judge density is mainly cross-sectional. This stability and the allocation of judges across Prud'hommes allow us to claim that the change in their density within a Prud'homme is barely related to local economic development. Again, Table 14 confirms this view.

²¹ See Bonaffé-Schmidt (1987).

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.

Union shares of votes

We also use as instrument the share of judges working at the local labor court and affiliated to one of the unions running for the Prud'hommes elections. The union color of the Prud'homme is likely to influence the judicial outcomes. Prud'hommes elections in France are keys to determine the representative character of each union among the labor force. A large share of votes increases bargaining power at the firm and the national level. For instance, over the period under review, a share larger than five percent at the national level was a necessary condition to allow a union to take office in a firm with more than 50 employees and this irrespectively of the representative nature of the union at the firm level. The political platforms of the unions for these elections are their bargaining behaviors. CGT is often perceived as a hard line union with a strong political left ideology and is less prone to negotiate: the CGT leader was a member of the central committee of the communist party until 2001. From 1995 to 2004, the CGT signed on average one third of the collective agreements at the industry level against around seventy percent for the CFDT sixty percent for the CFTC²². CFDT and CFTC are known as softer and more likely to conciliate. One could argue that facing a higher probability to lose their jobs workers would tend to vote for hard line unions. First, it would imply that every worker's opinion is to favour clash over dialogue to obtain what they want. Second, as shown by Andolfatto (1988) the map of union votes overlaps the map of political votes and is more related to traditional culture and local industrial history than to current economic condition. The moderate Western France is characterized by a high turnout in favor of CFDT and CFTC and contrasts with left territories from the south west and the north voting for CGT. Third, we showed that the institutional setting of the Prud'homme generates some discrepancies (putting it mildly) between the local Prud'homme characteristics and the local economy characteristics. Judges' behavior determined by their union affiliation and a share of local votes independent of current local economic developments – remember that an election takes place only every five years -- let us think that the union color can instrument convincingly the judicial activity, again *within* a Prud'homme.

²² See page 63 « La négociation interprofessionnelle en 2004» part 2, page 63. Rapport du Ministère du Travail. Available on line at http://www.travail-solidarite.gouv.fr/IMG/pdf/NC_2004_-_2_La_negociation-2.pdf.

We can note the variety of origins in our instruments: “staff” comes from the allocation by the central government of civil servants into local jurisdictions, “judges” and the union colors at the Prud’homme are set by the institutional settings of the Prud’hommes and industrial relations, “lawyers” is related to location preferences of the lawyers.

5.3.3 Results

Tables 14 and 15 present the instrumental regressions (first stage) for each of our EPL indicators – worker’s victory, going to trial, settlement, dropping the case, duration of the case, worker’s use of a lawyer, firm’s use of a lawyer – on the set of potential instruments, controls (year and business cycle indicators, appropriately transformed as described above), and Prud’hommes fixed effects. In Table 14, we focus on the whole period (1990-2003) with a restricted set of instruments (nothing on legal environment). In Table 15, we focus on a more recent period for which we can use an extended set of instruments (on the legal environment as measured by lawyers and ministry of justice staff in the area, see above). 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 rows of the Tables. Note that no instrument appears to be valid for the victory indicator. When more than one instrument is selected, we will test the (statistical) validity of our instruments using the Sargan-Hansen’s tests of over-identifying restrictions.

On substance, a larger judge density implies less dropped cases; when judges are “overworked” they tend to select the “best” cases and push the rest of plaintiffs (mostly workers) to abandon. Hence, dropping cases seems to work in favor of firms. By contrast, more judges (within a Prud’hommes court) are associated with more lawyers “helping” workers. This is another proof that judge density implies stricter EPL. The legal environment is captured by the population of lawyers in the area and by the legal staff. More lawyers (all specialties, not only labor) imply less trials and more settlements, a proof that a strong legal environment (supply of legal services) favors out-of-court resolution of conflicts. However, it seems that settlements work in favor of workers when trials do not. Interestingly, more lawyers are also associated with more lawyers representing workers, but not more lawyers representing firms. It not necessarily surprising since firms appear to use lawyers when judicial staff is missing (see last column of Table 15). Finally, the type of union that is strong in the Prud’hommes has some effect on court outcomes. Increases in votes for Force Ouvrière (FO) tend to favor the use of lawyers by workers and act against dropping cases when increases in votes for CFDT, a modernist union, appears to decrease the duration of cases.

5.3.3.1 Labor Flows and Court Outcomes

The results from our IV procedure are given in Table 16. The first panel presents estimates for the full period whereas the second panel presents estimates for the last years, 1996-2003. The EPL indicators differ examined according to the period since available and valid instruments also differ by period.

Consistent with previous results on judges, our EPL indicators which capture decisions that favor workers tend to dampen job destruction when those that favor firms tend to increase job destruction. As analyzed just before, dropped cases tend to favor firms. And, indeed, more dropped cases mean more job destructions as well as less net creation since dropping cases does not affect job creation. Again, we saw that settlements were apparently working in favor of workers when trials appeared to help firms (see Tables 14 and 15). This is confirmed by the signs in Table 16: more settlements dampen job destruction when more trials increase job destruction. But, 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. Clearly, when one compares the various measures of EPL, it is difficult to come up with a simple view.

5.3.3.2 Labor Flows and Legal Representation

The results for worker representation are given in the last column of Table 16. 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, dampens job creation (second panel), but because the former effect is very strong, the net effect is positive. Hence, when EPL is measured by the fraction of workers represented by a lawyer, EPL protects jobs. Finally, in unreported results (available from the authors), the fraction of firms represented by a lawyer has no impact on labor flows.

6 Conclusion

The impact of the EPL on labor markets has most often been assessed through cross-country analyses which make it hard to control for all various potential interactions between the labor market institutions. Within country analyses have most often used difference-in-difference estimators and tended to ignore the extent to which EPL was enforced and was acting as a binding constraint for the firm or the worker. Both of these strands do not address the problem of EPL endogeneity and enforcement of the EPL. By contrast, in order to measure EPL in France --a country with a highly

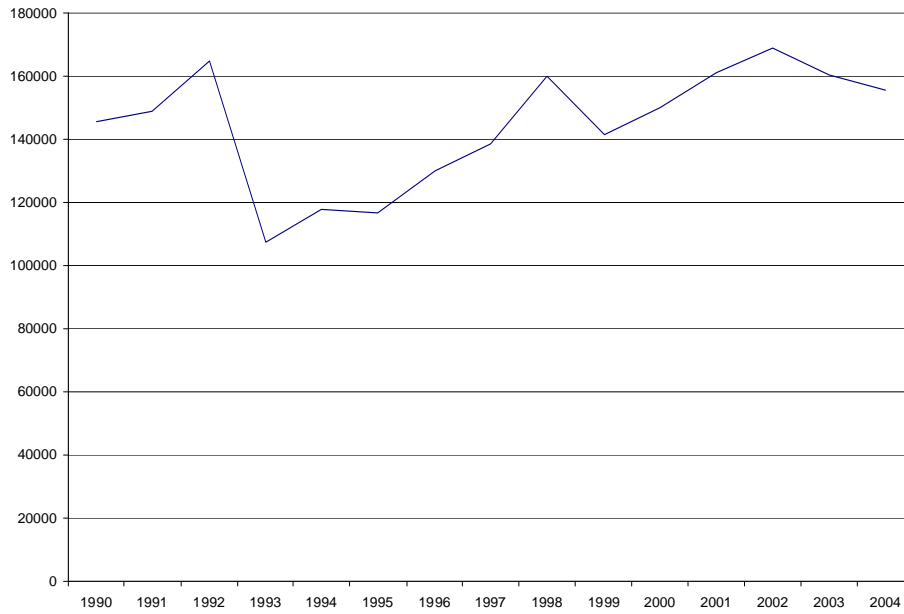
regulated labor market—we use various institutional features and time-varying judicial measures of activity and outcomes of the local courts in charge of ruling individual labor disputes (Prud’hommes). Our results show that labor flows are affected by labor courts. More judges tend to affect job creation, in particular on the extensive margin: more judges means less job creation. Labor courts decisions that have the effect of protecting workers, such as providing them with a lawyer, tend to prevent job destruction. Labor courts decisions that have the effect of favoring firms tend to increase job destruction. Interestingly, the net outcome of more EPL is sometimes positive (i.e. net job creation for lawyers assisting workers) and sometimes negative (i.e. net job destruction for a higher density of judges). Indeed, it appears that EP legislations tend to have an effect on either the creation margin or the destruction margin, rarely on both. Hence, because a specific country mixes various legislations and because local situations depend on a variety of specifics, the final outcome is likely to be the product (or the sum) of multiple effects. It is therefore not surprising that aggregate analyses yield inconclusive outcomes.

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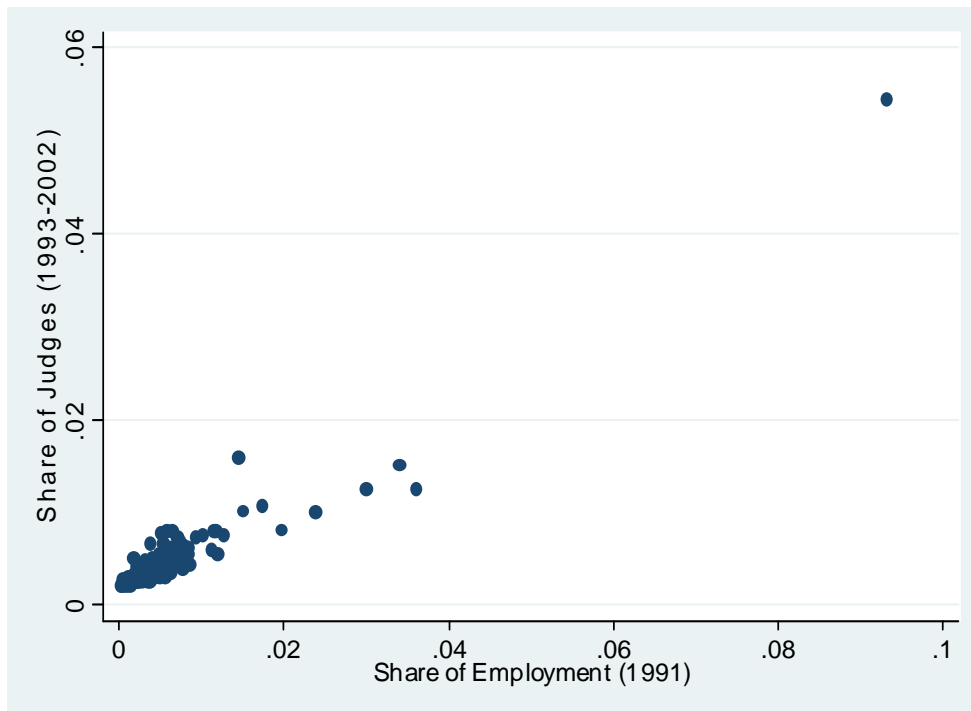
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Figure 1: Number of filed cases



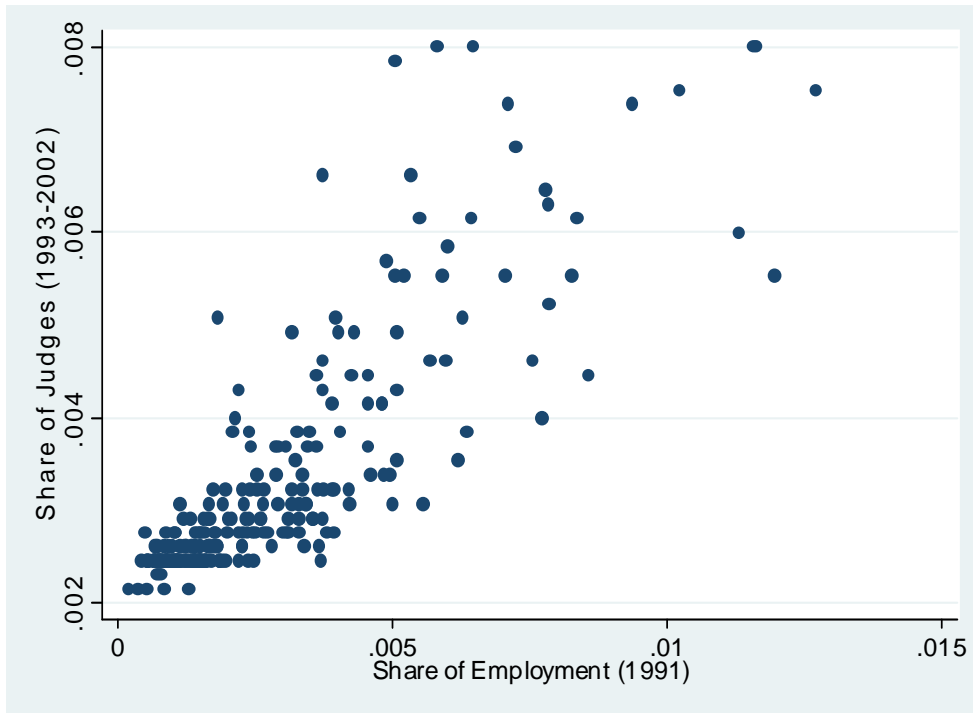
Sources: Prud'hommes data from Ministry of Justice.

Figure 2: Allocation of judges



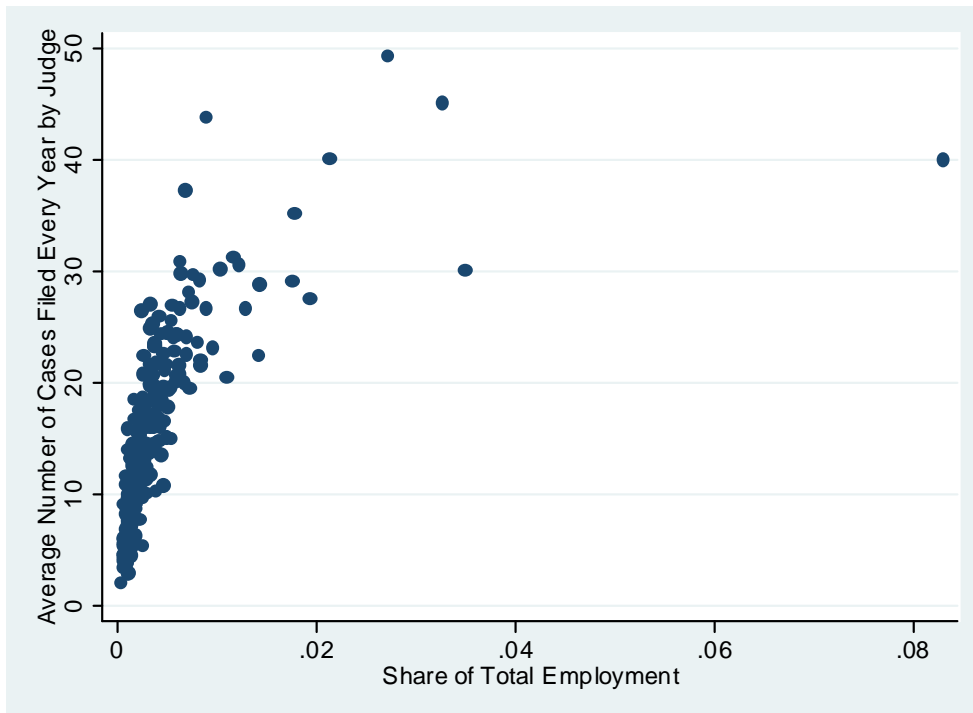
Sources: Election data from Ministry of Labor. Employment data from the Insee Sirene files on establishments.

Figure 3: Allocation of judges (without the 6 largest Prud'hommes)



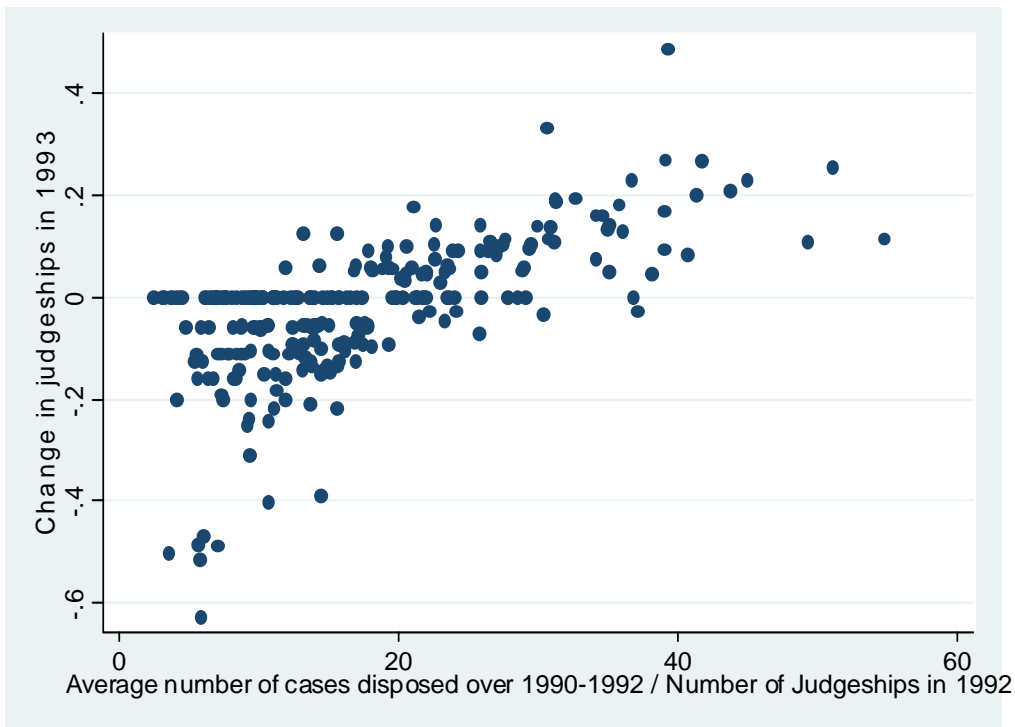
Sources: Election data from Ministry of Labor. Employment Data from the Insee Sirene files on establishments.

Figure 4: Productivity of judges across Prud'hommes



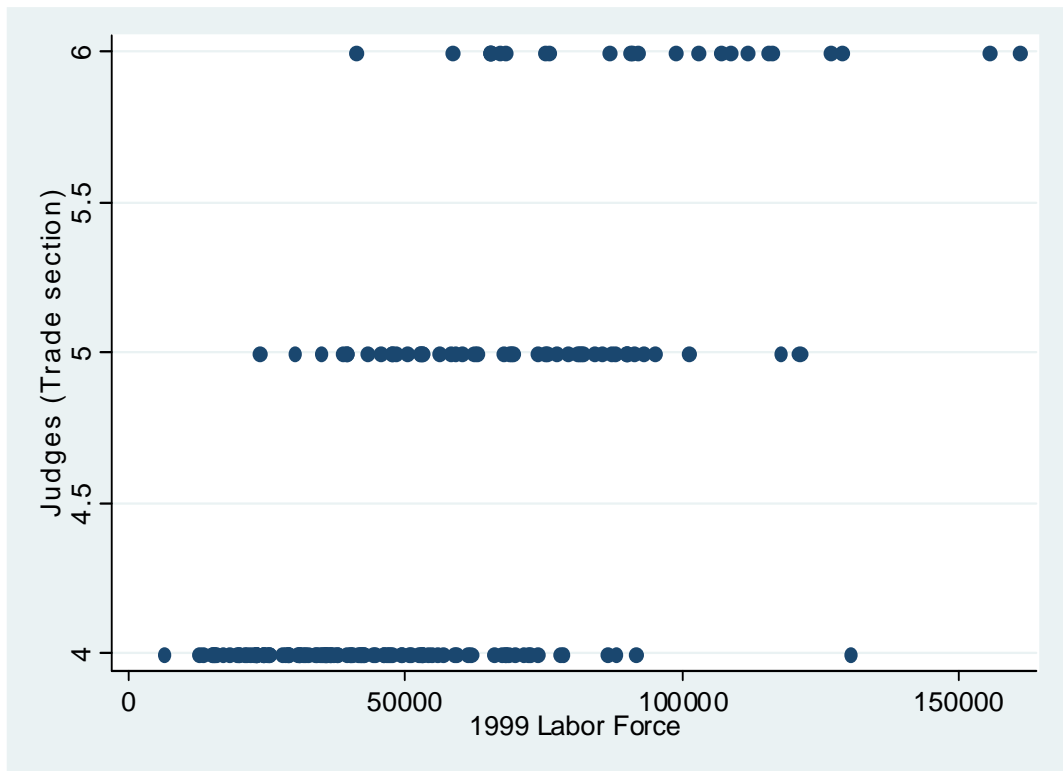
Sources: Election data from Ministry of Labor. Employment Data from the Insee Sirene files on establishments.

Figure 5: Change in judges in 1993 and productivity of judges



Sources: Election data from Ministry of Labor. Employment Data from the Insee Sirene files on establishments.

Figure 6: Number of Judges, the small Prud'hommes



Sources: Election data from Ministry of Labor. Employment Data from the Insee Sirene files on establishments.

Table 1 : Case outcomes: definition of variables

Names	Definition
Dropped	(Null and Void +Crossed Out)/(Total number of cases)
Settled	(Conciliation +Agreement)/(Total number of cases)
Trial	(Winning+Losing)/(Total Number of Cases)
Victory	(Winning)/(Winning +Losing)
Duration	Duration (in days) of the legal process from the filing of the case to the its classification
Lawyerf	Number of cases where the firm is represented by a lawyer over the total number of cases
Lawyer	Number of cases where the worker is represented by a lawyer over the total number of cases

Notes: These variables are computed at the labor court level (Prud'hommes)

Table 2: Summary statistics: case outcomes

Case Outcome	Mean*	Std.	Min	Max
Dropped	0.20	0.09	0.00	0.75
Agreed	0.20	0.09	0.00	0.81
Trial	0.60	0.10	0.13	0.95
Victory	0.75	0.09	0.00	1.00
Duration	258	81	48	1037
Lawyer	0.43	0.15	0.00	0.95
Lawyerf	0.58	0.15	0.00	0.95

Notes: we first compute the proportion of cases with outcomes i in year t at the Prud'hommes level using the data set of individual cases collected from 1990 to 2004 by the French Ministry of Justice. We then take the means of these proportions over the 264 Prud'hommes over the 1990-2004 period. Sources: Prud'hommes data from Ministry of Interior.

Table 3: Share of judges by unions

Union	Mean	Std	Min.	Max.
CGT	37%	11%	0%	71%
CFDT	28%	10%	0%	63%
FO	22%	7%	0%	50%
CFE-CGC	8%	4%	0%	21%
CFTC	4%	6%	0%	44%

Note: Number of observations: 1,056 (264 Prud'hommes over 4 electoral terms)

Sources: French Ministry of Labor

Table 4: Number of judges by section and change over the electoral terms

	Number of judges in 1987	Change in % between term t and term t-1 (t/t-1)		
		1992/1987	1997/1992	2002/1997
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

Sources: French Ministry of Labor.

Table 5: Breakdown of change in the number of judges across the 264 Prud'hommes

	1992 Election			2002 Election		
	Manufacturin	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 Prud'hommes that lost (or gained or no change) x judges in the election year t

Sources: French Ministry of Labor.

Table 6: Breakdown of judges and employment by industry

	1990-1992		1993-2002	
	Employment	Judges	Employment	Judges
Manufacturing	35%	41%	53%	37%
Trade	47%	33%	36%	36%
Service	18%	26%	11%	27%

Notes: A change in French classification of products occurred in 1993. Proportion are averaged out over the period under review. Number of observations: 264 Prud'hommes. Sources: Election data from Ministry of Labor. Employment Data from the Insee Sirene files on establishments.

Table 7: (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%.

Sources: French Ministry of Labor, Insee Sirene Files

Table 8 : Judges and labor flows in prudhommes with four judges by section

Dependent variable:				<i>Extensive margin</i>		
	Job Destruction	Job Creation	Net Job Creation	Job Destruction	Job Creation	Net Job Creation
Manufacturing:						
Judges	-64.493 (121.232)	18.031 (73.253)	82.524 (96.423)	-79.575 (107.581)	-56.727 (81.449)	22.848 (57.665)
Observations	1010	1010	1010	1010	1010	1010
R-square	0.30	0.26	0.33	0.31	0.11	0.33
Trade:						
Judges	-194.916*** (58.433)	-199.225*** (45.972)	-4.309 (60.087)	-115.394** (44.085)	-139.818*** (47.482)	-24.424 (48.228)
Observations	1060	1060	1060	1060	1060	1060
R-square	0.21	0.27	0.24	0.23	0.23	0.28
Service:						
Judges	-181.742 (244.481)	-321.249 (200.186)	-139.507 (169.938)	113.258 (190.872)	-130.720 (189.402)	-243.978* (147.275)
Observations	2118	2118	2118	2118	2118	2118
R-square	0.14	0.19	0.19	0.21	0.19	0.25

Notes: we regress labor flows of a particular industry on the corresponding judge density e.g. the number of judges allocated to this industry in one given prudhommes over the 1999 labor force of the prudhommes jurisdiction. We restrict the samples of prudhommes to the ones having 4 judges in the industry section. 4 being the minimum required. Each regression includes year and local business cycle indicators, 1999 labor force and 1999 labor force squared, 1991 employment composition by industry and firm size, 1991 caseload composition by industry and firm size. Prud'hommes' jurisdiction 1999 total labor force is used as weights. Clusters: Prud'hommes level. Robust standard errors are between parentheses. * significant at 10%; ** significant at 5%, ***significant at 1%. Sources: Prud'hommes data from Ministry of Labor. Job flows from Sirene files on establishments

Table 9: Judges and labor flows in prudhommes with sixteen judges

Dependent variable:				<i>Extensive margin</i>		
	Job Destruction	Job Creation	Net Job Creation	Job Destruction	Job Creation	Net Job Creation
Judges	-95.633 (67.222)	-140.585*** (49.106)	-44.952 (63.015)	-101.715* (51.558)	-131.723*** (40.147)	-30.008 (38.164)
Observations	780	780	780	780	780	780
R-square	0.33	0.38	0.42	0.34	0.25	0.40

Notes: we regress labor flows on the judge density ("Judges") e.g. the number of judges allocated to one prudhommes over the 1999 labor force of the prudhommes jurisdiction. We restrict the samples of prudhommes to the ones having 16 judges. 16 being the minimum required. Each regression includes year and local business cycle indicators, 1999 labor force and 1999 labor force squared, 1991 employment composition by industry and firm size, 1991 caseload composition by industry and firm size. Prud'hommes' jurisdiction 1999 total labor force is used as weights. Clusters: Prud'hommes level. Robust standard errors are between parentheses. * significant at 10%; ** significant at 5%, ***significant at 1%.

Sources: Prud'hommes data from Ministry of Labor. Job flows from Sirene files on establishments

**Table 10: Regression discontinuity; labor flows and judge density
(cities with less than 5,000 inhabitants)**

	<i>Extensive Margin</i>				
	Job Destruction	Job Creation	Net Job Creation	Job Destruction	Job Creation
Judges	-0.328 (4.93)	-17.03*** (5.60)	-16.70*** (6.02)	-4.107 (4.77)	-17.85*** (5.30)

Robust standard errors are between parentheses. * significant at 10%; ** significant at 5%, ***significant at 1%. Observations are for 3,109 pair of cities belonging to different Prud'hommes jurisdiction over the 1993-2003 period (17,873 observations). The sample is restricted to pairs where cities are populated with less than 5,000 inhabitants. Each regression includes year indicators. Clusters: city match. Sources: Prud'hommes data from Ministry of Labor. Job flows from Sirene files on establishments.

Table 11: Regression discontinuity; labor flows and judge density

	<i>Extensive Margin</i>				
	Job Destruction	Job Creation	Net Job Creation	Job Destruction	Job Creation
Judges	0.293 (4.52)	-13.64*** (5.16)	-13.93** (5.52)	-3.147 (4.37)	-14.88*** (4.87)

Robust standard errors are between parentheses. * significant at 10%; ** significant at 5%, ***significant at 1%. Observations are for 3,448 pair of cities belonging to different Prud'hommes jurisdiction over the 1993-2003 period (20,274 observations). Each regression includes year. Clusters: city match. Sources: Prud'hommes data from Ministry of Labor. Job flows from Sirene files on establishments.

Table 12: EPL indicators and job flows, OLS

Dependent variable:	Job destructions		Job creations		Net job creations	
drop	0.004 (0.014)	0.106*** (0.018)	-0.011 (0.011)	0.116*** (0.017)	-0.015 (0.017)	0.010 (0.011)
R ²	0.33	0.29	0.46	0.36	0.51	0.48
conci	-0.030* (0.018)	-0.097*** (0.022)	0.007 (0.012)	-0.105*** (0.022)	0.037** (0.017)	-0.008 (0.013)
R ²	0.34	0.28	0.46	0.35	0.52	0.48
trial	0.018 (0.013)	-0.015 (0.018)	0.003 (0.009)	-0.017 (0.015)	-0.015 (0.014)	-0.002 (0.010)
R ²	0.33	0.25	0.46	0.32	0.51	0.48
win	0.000 (0.012)	0.001 (0.017)	0.012 (0.009)	0.009 (0.017)	0.011 (0.013)	0.008 (0.011)
R ²	0.33	0.29	0.46	0.36	0.51	0.48
duration	-0.010 (0.008)	0.038*** (0.005)	-0.001 (0.004)	0.047*** (0.005)	0.009 (0.008)	0.010*** (0.004)
R ²	0.34	0.31	0.46	0.40	0.51	0.49
lawyer	-0.019 (0.012)	0.057*** (0.013)	-0.002 (0.008)	0.080*** (0.013)	0.018 (0.013)	0.022*** (0.008)
R ²	0.34	0.28	0.46	0.36	0.51	0.49
lawyerf	-0.019** (0.009)	-0.028*** (0.008)	0.004 (0.006)	-0.014 (0.009)	0.023** (0.011)	0.015* (0.009)
R ²	0.34	0.26	0.46	0.32	0.52	0.49
Fixed effects	Yes	No	Yes	No	Yes	No

Robust standard errors are between parentheses. * significant at 10%; ** significant at 5%, ***significant at 1%. Observations are for 264 Prud'hommes and for the years 1991-2004 (3, 432 obs.). Each regression includes year and local business cycle indicators. Prud'hommes jurisdiction 1999 labor force is used as weights. Clusters: Prud'hommes level.

Sources: Prud'hommes data from Ministry of Labor. Job flows from Sirene files on establishments.

Table 13: The Impact of past labor flows on judges, staff, lawyer densities and union share of votes

<i>Job Destructions</i>						
Flows=	Judges	Staff	Lawyer	pctfo	pctcfdt	pctcgt
Flows (-1)	-0.0000 (0.0000)	0.0000 (0.0001)	-0.0004 (0.0003)	-0.0196 (0.0162)	-0.0216 (0.0186)	0.0342* (0.0184)
Flows (-2)	-0.0000 (0.0000)	-0.0000 (0.0001)	-0.0002 (0.0002)	0.0076 (0.0157)	0.0260 (0.0192)	-0.0151 (0.0163)
R-squared	0.01	0.12	0.11	0.14	0.33	0.07
<i>Job Creations</i>						
Flows=	Judges	Staff	Lawyer	pctfo	pctcfdt	pctcgt
Flows (-1)	-0.0000 (0.0000)	0.0001 (0.0001)	0.0001 (0.0004)	0.0211 (0.0227)	0.0009 (0.0226)	0.0413 (0.0280)
Flows (-2)	0.0000 (0.0000)	-0.0000 (0.0001)	0.0006 (0.0006)	0.0287 (0.0209)	0.0095 (0.0228)	-0.0207 (0.0214)
R-squared	0.00	0.12	0.11	0.14	0.33	0.07
<i>Net Job Creations</i>						
Flows=	Judges	Staff	Lawyer	pctfo	pctcfdt	pctcgt
Flows (-1)	0.0000 (0.0000)	0.0000 (0.0001)	0.0003* (0.0002)	0.0262 (0.0164)	0.0122 (0.0164)	0.0019 (0.0218)
Flows (-2)	0.0000 (0.0000)	0.0000 (0.0001)	0.0005 (0.0003)	0.0162 (0.0130)	-0.0088 (0.0171)	-0.0042 (0.0141)
R-squared	0.00	0.12	0.11	0.14	0.33	0.07
Observations	2904	2860	2103	2904	2904	2904

Notes: Robust standard errors are between parentheses.*** p< 0.01, **p<0.05, *p<0.1. Each regression includes year and Prud'hommes and local business cycle indicators. Prud'hommes' jurisdiction 1999 labor force is used as weights.Clusters: Prud'hommes level.

Sources: Prud'hommes data from Ministry of Labor. Job flows from Sirene files on establishments.

Table 14: First stage regressions (1990-2003) without legal environment

Period: 1990-2003							
	victory	trial	settlement	drop	duration	lawyer	lawyerf
Judges	-16.973 (47.068)	93.737 (69.478)	30.897 (61.110)	-124.635** (57.379)	162.462 (167.446)	301.183*** (77.468)	26.946 (114.942)
Union share of votes:							
FO	-0.063 (0.062)	0.032 (0.068)	0.072 (0.058)	-0.104** (0.052)	-0.100 (0.175)	0.153* (0.084)	0.123 (0.123)
CFDT	-0.042 (0.060)	0.011 (0.055)	0.026 (0.048)	-0.037 (0.047)	-0.381*** (0.134)	-0.006 (0.084)	0.123 (0.111)
Others	0.024 (0.061)	0.021 (0.063)	-0.025 (0.038)	0.004 (0.071)	-0.191 (0.116)	0.075 (0.060)	0.068 (0.081)
R-squared	0.02	0.17	0.19	0.05	0.18	0.59	0.25
F				4.17	5.13	9.32	
p-values				0.0165	0.0243	0.0001	

Robust standard errors are between parentheses. * significant at 10%; ** significant at 5%, ***significant at 1%. Observations are for 264 prudhommes (3,696 observations for the 1990-2003 period and 2,112 observations for the 1996-2003 period). Each regression includes year Prud'hommes and local business cycle indicators. Prud'hommes jurisdiction 1999 labor force is used as weights. Clusters: Prud'hommes level. Sources: Prud'hommes data from Ministry of Labor. Sirene files on establishments.

Table 15: First stage regressions (1996-2003) with legal environment

Period: 1996-2003							
EPL	victory	trial	settlement	drop	duration	lawyer	lawyerf
Judges	268.440 (171.574)	360.177 (250.752)	-86.030 (260.322)	-274.147 (207.048)	1,190.945** (574.365)	603.166*** (218.819)	129.189 (340.770)
Lawyers	2.080 (2.077)	-8.558*** (2.795)	8.083*** (2.043)	0.475 (2.258)	1.878 (7.233)	5.655** (2.745)	-2.328 (4.953)
Staff	5.830 (6.477)	9.755 (11.254)	-10.059 (6.962)	0.304 (6.143)	9.276 (17.676)	19.746* (10.235)	-50.686*** (14.797)
Union share of votes:							
FO	-0.017 (0.075)	0.013 (0.093)	-0.047 (0.083)	0.034 (0.065)	0.079 (0.191)	0.096 (0.094)	0.186 (0.151)
CFDT	0.071 (0.082)	0.004 (0.074)	-0.063 (0.068)	0.059 (0.066)	-0.241 (0.168)	-0.057 (0.080)	0.043 (0.135)
Others	-0.034 (0.048)	-0.004 (0.053)	-0.105*** (0.040)	0.109** (0.047)	-0.194 (0.182)	-0.007 (0.057)	0.201 (0.154)
R-squared	0.03	0.23	0.28	0.04	0.17	0.25	0.30
F		8.11	12.49			6.39	4.32
p-values		0.0048	0.0000			0.0003	0.0387

Robust standard errors are between parentheses. * significant at 10%; ** significant at 5%, ***significant at 1%. Observations are for 264 prudhommes (3,696 observations for the 1990-2003 period and 2,112 observations for the 1996-2003 period). Each regression includes year Prud'hommes and local business cycle indicators. Prud'hommes jurisdiction 1999 labor force is used as weights. Clusters: Prud'hommes level. Sources: Prud'hommes data from Ministry of Labor. Sirene files on establishments.

Table 16: Instrumental Variables Regressions

<i>Period: 1990-2003</i>			
EPL indicators:	drop	duration	Lawyer w
Dependent variable: Job Destructions			
EPL	0.540** (0.25)	0.145 (0.11)	-0.275** (0.11)
P-value Hansen J statistic	0.903		0.563
Dependent variable: Job Creations			
EPL	0.0204 (0.069)	0.0984 (0.081)	0.0297 (0.086)
P-value Hansen J statistic	0.933		0.972
Dependent variable: Net Job Creation			
EPL	-0.595** (0.27)	-0.0469 (0.070)	0.305** (0.13)
P-value Hansen J statistic	0.877		0.575
Instruments	Judges,FO	CFDT	Judge,FO
Test of excluded instruments F	4.17	5.31	9.32
<i>Period: 1996-2003</i>			
EPL indicators:	trial	settlement	Lawyer w
Dependent variable: Job Destructions			
EPL	0.834** (0.35)	-0.631*** (0.17)	-0.610*** (0.22)
P-value Hansen J statistic		0.447	0.302
Dependent variable: Job Creations			
EPL	0.137 (0.17)	-0.0515 (0.10)	-0.145* (0.075)
P-value Hansen J statistic		0.347	0.360
Dependent variable: Net Job Creation			
EPL	-0.697** (0.28)	0.579*** (0.19)	0.464** (0.21)
P-value Hansen J statistic		0.606	0.356
Instruments	Lawyer	Lawyer, Staff, Others	Judge, Lawyer, Staff
Test of excluded instruments F	8.11	12.49	6.39

Robust standard errors are between parentheses. * significant at 10%; ** significant at 5%, ***significant at 1%. Observations are for 264 Prud'hommes and for the years 199-2003 (3,432 obs.) or 1996-2003 (2,112 obs.). Each regression includes year and local business cycle indicators. Prud'hommes jurisdiction 1999 labor force is used as weights. Clusters: Prud'hommes level. Sources: Prud'hommes data from Ministry of Labor. Job flows from Sirene files on establishments

Table A.1: Logit model for case outcomes

	Case outcomes					Case outcomes			
	(2)	(3)	(4)	(5)		(2)	(3)	(4)	(5)
	Settlement	Trial	Dropped	Victory		Settlement	Trial	Dropped	Victory
Judges	-49.77*** (16.5)	39.62** (16.3)	-15.26 (15.3)	-4.646 (11.3)	Representative (Lawyer):				
Age (37-49):					Missing	0.314*** (0.061)	-1.856*** (0.14)	1.627*** (0.12)	0.208*** (0.059)
Missing	-27.28*** (4.90)	10.91*** (2.73)	-1.249 (0.82)	-11.61*** (0.028)	Union	0.194*** (0.039)	-0.0652 (0.064)	-0.0942 (0.061)	-0.107*** (0.027)
15-24	0.0560*** (0.016)	-0.0923*** (0.016)	0.00155 (0.016)	0.295*** (0.017)	Others	0.405*** (0.056)	-0.246** (0.10)	-0.262** (0.11)	-0.0820 (0.056)
25-36	-0.00527 (0.014)	-0.0116 (0.012)	0.00389 (0.013)	0.130*** (0.012)	No representative	0.189*** (0.050)	-1.234*** (0.14)	1.067*** (0.10)	-0.161*** (0.029)
50+	0.0274* (0.016)	0.0125 (0.011)	-0.0313** (0.014)	-0.0495** (0.024)	Jurisdictions (Unfair Dismissal):				
Duration	-0.00726*** (0.00048)	0.00244*** (0.00068)	-0.0000960 (0.00023)	-0.000330*** (0.000071)	Redundancy	-0.0866* (0.046)	0.142*** (0.045)	-0.114** (0.049)	-0.0700** (0.034)
Judicial Assistance Benefit	-0.195*** (0.044)	0.541*** (0.033)	-0.554*** (0.040)	-0.0125 (0.021)	Deduction of wages	0.132*** (0.044)	-0.258*** (0.046)	0.163*** (0.039)	0.00509 (0.033)
Firm Size (Small):					Disciplinary	0.302*** (0.042)	-0.316*** (0.050)	0.114** (0.055)	-0.330*** (0.042)
Missing	0.0242 (0.062)	-0.0889** (0.042)	0.0571* (0.030)	-0.0612** (0.028)	Bankruptcy	-1.102*** (0.068)	0.571*** (0.082)	-0.000624 (0.086)	0.300*** (0.037)
Large	0.0396 (0.043)	-0.0218 (0.032)	-0.0181 (0.029)	-0.265*** (0.051)	Missing	-0.141** (0.068)	-0.253*** (0.060)	0.344*** (0.093)	-0.00970 (0.099)
Gender (Male):					Union Share of votes (CGT):				
Female	0.220*** (0.016)	-0.102*** (0.015)	-0.0509*** (0.013)	0.0613*** (0.013)	FO	0.526* (0.32)	-0.150 (0.28)	-0.302 (0.27)	-0.407** (0.17)
Job Destructions	-0.430 (0.39)	0.0319 (0.38)	0.604*** (0.20)	0.0307 (0.23)	CFDT	1.362*** (0.28)	-0.815*** (0.22)	-0.268 (0.24)	0.0996 (0.17)
					CFTC	0.205 (0.44)	-0.968** (0.39)	0.803** (0.37)	0.0934 (0.29)
Observations	1055359	1055359	1055359	628396	Observations	1055359	1055359	1055359	628396

Sources: Prud'hommes data from French Ministry of Justice. Job Destructions from the Sirene files on establishments.

*Column (2) to (5) display results from logistic regressions at the individual level where the dependent variable is the case outcome. "Judges" is the ratio of the judges at the Prud'hommes level over the local employment. Year indicators are added. The omitted category for each polytomial variable is in parenthesis. Standard errors are in parenthesis. * significant at 10%; ** significant at 5%, ***significant at 1%. Cluster: Prud'hommes level

Table A.2: Case outcomes and the business cycle

Outcome variable:	Local Unemployment Rate	Local Unemployment Rate (-1)	R-squared
Winning	0.184 (0.160)	0.856*** (0.160)	0.39
Losing	0.157 (0.100)	0.0506 (0.100)	0.28
Null and Void	0.236*** (0.073)	0.613*** (0.074)	0.37
Crossed Out	0.301** (0.120)	0.331** (0.130)	0.53
Conciliation	0.457*** (0.095)	0.150* (0.082)	0.54
Agreement	0.180* (0.100)	0.774*** (0.120)	0.31
Tied	0.404*** (0.095)	0.370*** (0.099)	0.33
Dropped	0.0648 (0.130)	0.283** (0.140)	0.5
Agreed	0.276* (0.150)	0.624*** (0.150)	0.46
Trial	0.341** (0.160)	0.907*** (0.170)	0.41
Victory	0.143 (0.150)	0.305** (0.150)	0.31
Filerate	0.589*** (0.170)	1.079*** (0.210)	0.59

Sources: Prud'hommes data from Ministry of Justice. Others from Insee.

Each row displays the regression of an outcome variable on the current and lagged local unemployment rate and Prud'hommes fixed effects. The local unemployment rate is defined as the number of unemployed enrolled at the local branch of the National Employment Agency (ANPE) over the 1999 census local workforce. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1