WORKING PAPER

Access to Justice & Unemployment: Evidence from French Labor Courts

Romain Espinosa, Claudine Desrieux and Marc Ferracci

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Abstract

In 2008, the French government enacted a reform to reduce the number of labor courts by 20%. This led to significant changes in the access to labor courts for many workers and employers who now have to go further to litigate their conflicts. We use this reform to identify how access to labor courts impacts the labor market. Our empirical approach mainly relies on regression adjusted conditional difference-in-difference estimations. Our results show that cities that experienced an increase of distance to their associated labor court suffered from a reduction in the growth rate of job inflows (-4 percentage points), job outflows (-4.6 pp) and creations of enterprises (-6.3 pp) between 2007 and 2012 compared to non-affected cities. We find partially opposite but non-significant effect for cities that experienced a decrease in the distance to the labor court. Our results are showed to be robust to changes of composition in the control group, changes in the matching algorithm, the inclusion of congestion effects, and to alternative econometric specifications (multilevel mixed-effect models).

JEL codes: K31, K41
Keywords: Labor courts, employment, firm creations, difference-in-difference, matching, multilevel mixed-effects.

1 Introduction

Reducing unemployment is one of the key challenges public authorities have dealt with over the past decades. Many determinants of job entries or exits have been explored in the literature up to now, such as education, training, or employment protection legislation. In this paper, we focus on a specific dimension of employment protection legislation, i.e. the organization of labor courts, defined as the institutions enforcing labor law. Does the geographical allocation of labor courts on a territory matter for employment? Does a longer distance to court impact entrepreneurs' decisions to hire or fire? Which consequences on employment can a reduction in the number of labor courts have?

To address these issues, we explore a reform enacted in 2008 in France to revise the judicial map. This reform reduced the number of labor courts by 20%. The conditions to challenge an employer's decision have then changed for many workers. Indeed, there is a geographical competency for each labor court in France: only one labor court is legitimate to hear a case happening on a given geographical area. Because of the reform, each area whose court has been removed has been reallocated to another court (and only one court). The decree implementing the reform identifies "receiving" courts that have to take in charge the judicial activity of suppressed courts. As a consequence, the distance to go to court has changed for many workers following the reform. This distance has increased for many workers but may also have decreased for some workers. This allows us to identify – without any causality problem- how geographical access to courts impacts employment on the labor market.

Our methodology relies on a combination of the propensity score matching and the diff-in-diff methods, namely conditional diff-in-diff estimations. Our goal is to estimate the impact of court removal on the employment activity at the city level. Building propensity scores, we use matching algorithms to create a group of counterfactuals for the treated observations (i.e. cities suffering from a suppressed court). Our results suggest that the reform slightly impacted job entries and exits.

Our analysis is - as far as we now- the first attempt to capture how a judicial reform reducing the

number of labor courts impacts employment and labor conditions. In a context of public debt, the organization of the judiciary - and of the number of courts- is yet at the center of many debates in western countries (World Bank (2011); Sénat (2012); ENCJ (2012)).
Our paper is structured as follows. Section 2 relates our work to the previous literature. Section 3 presents the French reform reducing the number of labor courts as well as the institutional context. Section 4 describes our data, and our empirical strategy is discussed in section 5. Our estimations follow in section 6, as well as a discussion in section 7.

2 Literature

Our paper is related to two main strands of the literature, namely the "law and economics" literature on the judiciary, and the literature on labor economics dealing with firing costs and employment.
First, several papers have investigated how market conditions influence court outputs, and more precisely decisions in labor courts (Ichino et al. (2003); Marinescu (2011)). The reverse impact of the judiciary on market outcomes has been explored in different contexts. For instance, Chemin (2009) shows that reforms in the organization of the judiciary to speed up the resolution of civil suits led to fewer breaches of contract, encouraged investment, and facilitated access to finance. Visaria (2009) and Von Lilienfeld-Toal et al. (2012) show similar results in the credit market.[1] Our paper is related to these previous contributions by linking courts' organization and market outcomes. However, it departs from them by focusing on the labor market and on the allocation of courts in a given territory.
Up to now, legal scholars (Gomes (2007); Mak (2008); Van Djik and Horatius (2013)) and international institutions (World Bank (2011); Sénat (2012); ENCJ (2012)) have shown concerns for access and geographical allocation of courts. However, these topics have been less investigated by the economic literature. Chappe and Obidzinski (2014) model how the distance to court impacts both the demand for litigation and the probability of accidents through the level of care chosen by people. When the probability of accidents depends on the level of care chosen by the parties, an increased distance to court may induce higher levels of care. Parties want to avoid accidents leading to potential costly litigation. With an empirical approach, Espinosa et al. (2015) analyze how the 2008 French reform that reduces the number of labor courts by 20% impacted the demand for litigation and the average case duration in the remaining courts. Their results show that case duration increased and the demand for litigation decreased more significantly in areas where courts received a high level of new claims coming from suppressed courts. We now go one step further to determine whether the reform influenced decisions on employment. By changing the distance to labor courts, the reform may have had consequences on the decisions to challenges dismissals and indirectly on firing costs.
Our paper considers changes in distance to labor courts as modifications in firing costs. Our work is then also related to another strand of the economic literature, focusing on the impacts of firing costs' variations on employment. Most of these studies suggest that a decrease in firing costs increases employment. For instance, Kugler and Pica (2008) use Italian panel data to study the impacts of a reform increasing unjust dismissal costs for businesses below 15 employees, while leaving dismissal costs unchanged for bigger businesses. The authors find that the increase in dismissal costs decreased accessions and separations for workers in small relative to large firms, especially in

[1] These papers show that an Indian reform introducing debt recovery tribunals to speed up the resolution of debt recovery claims reduced delinquency for the average loan and lowered the interest rates charged on larger loans, holding constant borrower quality. The reform reduces credit access for small borrowers and expand it for wealthy borrowers.

sectors with higher employment volatility. They also find some evidence suggesting that the reform reduced firms' entry rates and employment adjustments, but had no effect on exit rates.[2] Similar results have been replicated in different institutional environments. Hernanz et al. (2005) show that a reduction in dismissal costs for permanent contracts increased permanent employment probabilities and conversion of temporary into permanent jobs in Spain. Behaghel et al. (2008) study the reduction in the tax amount paid to the unemployment insurance in France for firms laying off workers aged 50 and above. The transition rate from unemployment to employment increased significantly for workers over 50 compared to workers under 50. However, the effect of this change on layoffs is less clear cut. Both theoretically and empirically, Kugler and Saint-Paul (2004) provide some evidence from U.S. data showing that firms increasingly prefer hiring employed workers (who are less likely to be lemons) as firing costs increase. With a different set-up, Gianfreda and Vallanti (2013) investigate the effect of the duration of labor trials on the composition of employment.[3]
Let us however mention that two other studies, Bauer et al. (2007) and von Below and Thoursie (2010), suggest that lower firing costs that may be applied to small firms have no significant impacts on hires and separations.[4]
The paper the most related to our study may be Fraisse et al. (2014). The authors analyze the French judicial process and its impact on the labor market. They use the lawyer density as a proxy for judicial fees and finds that a higher density leads to more litigation. This increased filing rate (increasing firing costs) causes a large decrease in employment fluctuations, especially for shrinking or exiting firms. However, it leads to a small positive effect on net employment growth. We depart from them by using a different identification strategy to measure the impact of firing costs on employment decisions, namely the 2008 reform of the judicial map of labor courts.
Last, from a methodological perspective, our paper borrows to the empirical literature on matching and regression-adjusted matching (Rosenbaum and Rubin (1983); Morgan and Harding (2006); Marcus (2014)).

3 The institutional context

3.1 The French labor market

According to the French National Institute (INSEE), 25.8 million people were working in 2013 in France.[5] Jobs (about three in four) are mainly in the service sectors, and most of the workers are salaried workers (9 workers out of 10). This explains why the enforcement of labor contracts is a real concern for the workers in France. More precisely, in 2013, 86,5% of these salaried workers had an open-ended contract (permanent/regular/long-term job, called *contrats à durée indéterminée* (CDI)), and 13,5% had a fixed term contract (temporary/short-term job). A good functioning of the labor market then implies a good regulation of the contractual employment relationship.

[2]Other studies show that the proportion of firms below 15 employees was reduced after the reform (Garibaldi et al. (2003); Schivardi and Torrini (2008)).

[3]They find that labor courts delays increase the probability of being employed for women and young people both in temporary and in permanent jobs, while they induce a switching from permanent to temporary jobs for middle age ranges of the working force.

[4]More precisely, Bauer et al. (2007) study the effects of changes in the threshold scale exempting small establishments from dismissal protection provision on worker flows. Using German data, their results indicate that there are no statistically significant effects of dismissal protection legislation on worker turnover. von Below and Thoursie (2010) study the seniority rules in the swedish legislation whereby a worker who was employed last has to go first when a firm downsizes. This rule is more lenient for small firms. Using a regression discontinuity approach, the authors do not find any significant impacts on hires and separations.

[5]The employment rate of people between 15 and 64 years old is at the European Union average, *i.e.* around 64%.

As many European countries, France is suffering from unemployment: the national average rate is estimated to 9,8% of the labor force (per ILO definition), *i.e.* 2.8 million people. Disparities can be large over the territory: some cities have unemployment rates higher than 33% while others are below 8%. This creates useful sources of variations for our study.
To have a comprehensive view of the labor market, let us add that 550 700 firms were created in France in 2014. Almost half of them were "auto-entrepreneurs", *i.e.* firms with a special status (enacted in 2009) for individual activity with a limited sales revenue. The other creations were public limited-liability companies (165 700 new companies in 2014) and individual enterprises (101 600 new enterprises others than "auto-entrepreneurs").[6]
By enforcing labor contracts, labor courts are key institutions for the employment protection. According to the OECD indicators, the employment protection legislation (EPL) in France is rather high: from a scale 0 (least restrictions) to 6 (most restrictions), the overall EPL indicator for France is worth 2.38, whereas the average for the OECD countries is 2.04.[7] A side effect of this stringent EPL is "to produce a large amount of legal procedures related to labour disputes" (Le Barbanchon and Malherbet (2013)). Those disputes are brought to the French labour courts called "conseils des prud'hommes".

3.2 The French Labor Courts

Labor courts are first-level tribunals[8], only dealing with individual disputes affecting labor relationships in the private sector (validity of employment contracts, nullification of a dismissal, monetary compensations, level of severance payments, ...).[9] There exist today 210 courts spread all over the territory. Each court is competent over a geographical area determined by the law. The territorial jurisdiction for a claim is then given by the location of the establishment in which the work is done and, if the work is not performed within an establishment, by the residence of the employee.
Each court is divided into 5 sections by activity (agriculture, commerce, industry, executives and diverse activities). Judges of labor courts are not professional judges but elected representatives (on a parity basis in each section) of employees and employers.[10]
Between 2004 and 2013, around 200 000 cases have been brought to labor courts each year in France (Guilloneau and Serverin (2015)). For most of the claims, the procedure is the following one. First, there is the "conciliation" stage: parties are invited to find a settled solution to their conflict. Only if they fail to find an agreement, they go to the "*bureau de jugement*" (ruling panel), comprising two employer lay-judges and two employee lay-judges. If the ruling panel does not make the decision (split votes inside the ruling panel, difficulties to interpret the law, ...), then a professional judge is asked to complete the jury in order to settle votes. In practice, the conciliation rate has kept on decreasing over the years to reach around 9% in 2013. Among cases that reach the "bureau de jugement", 15% go to "départage". Relatedly, labor courts suffer from long delays: cases need about 12 months to be terminated, while civil courts and commercial courts decide in half the time

[6] Source: $http://www.insee.fr/fr/themes/document.asp?ref_id=ip1534$ (Last Access: November 2015).

[7] Figures are relative to the indicator "Strictness of employment protection - individual and collective dismissals (regular contracts)" and come from the OECD website: $http://stats.oecd.org/Index.aspx?DataSetCode=EPL\_R$ (Last access: October 2015). Let us note that the indicator for the strictness of employment protection regarding temporary contracts is worth 3.63 for France in 2013, and 1.72 on average for the OECD countries.

[8] Appeals are brought before the *"Cour d'Appel"* (*"Chambre sociale"*), and appeals against *"cour d'appel"*s decisions are lodged in the *"Cour de cassation"* (*"Chambre sociale"*).

[9] These courts only deal with individual disputes, since disputes affecting collective labor relationships (such as strikes) are dealt with by ordinary civil courts ("*Tribunal de grande instance*"). However, if people individually challenge their dismissal that is part of a collective dismissal, they do it in the labor courts.

[10] The last election was held in 2008. From 2018, the nomination conditions of the lay judges will change, according to the law $n^0 2014 - 1528$ of December, 18^{th} 2014.

(respectively 5.4 and 5.8 months on average).[11]
Labor courts mainly deal with dismissals. In 2013, 8 plaintiffs out of 10 opened a claim to challenge the breach of their employment contract (Guilloneau and Serverin (2015)). Most of the time (76% of the claims), the plaintiff contests his dismissal for personal reasons.[12] From a law passed on July 13^{th}, 1973, the firm has to prove a real and serious cause of termination ("*cause réelle et sérieuse*") to dismiss a worker. The French Labor Code does not provide for either a definition of the real and serious cause or a list of situations considered as such. The content and scope of this notion has rather been defined by French case law, leading to many difficulties in interpretation.[13]

3.3 Overview of the 2008 Reform

A reform project to reduce the number of courts in France was discussed in 2008. The reasons exposed by the government to support this reform were (*i*) the inadequacy between demographical evolution and the allocation of courts in the country, and (*ii*) the need to rationalize the management of courts.[14] The total cost of this reform is today evaluated to 413M €, and the savings on administrative expenditures are estimated to 9,1 M € per year (Cour des comptes (2015)).[15] Before the reform, there were 1,206 courts in France, among which 271 were first-level labor courts. Strong inequalities of access could be observed: some *départements*[16] had 14 labor courts, while some others had only one (Sénat (2012)). The reform was enacted by decree n^0 2008-514 of May 29^{th}, 2008, and removed 62 labor courts, *i.e.* more than 20% of the 271 former labor courts. One court was created, so that the total number of labor courts became 210 after the reform. The judicial map was redrawn: areas with removed courts were affected to other labor courts. This reform was effective on December 3^{rd}, 2008.[17] Two main criteria were announced as determining the choice of removed courts: first, public authorities wanted to maintain at least one labor court per "*département*"[18], and second, to remove low-activity courts (*i.e.* fewer than 500 new cases each year). Figures 1 and 2 in the appendix show the judicial map of French labor courts before and after the reform.

[11] Statistics come from both the Ministry of Justice (*www.justice.gouv.fr/statistiques.html*) and a report ordered by the Minister of Justice in 2014 (Lacabarats (2014)).

[12] To put it differently, around 30% of dismissals are challenged at court (Tresor-Eco (2014)), and one dismissal for personal reason out of four is brought to court. Pursuant to Article L. 1233-3 of the French Labor Code, a dismissal can only be considered as "economic" if it is based on a reason unrelated to the employee and caused by economic difficulties or technical changes. On the contrary, dismissals for personal reasons may come from disciplinary problems (e.g. refusal to follow work instructions) or not (professional inability or repeated errors for instance).

[13] As an illustration, companies cannot fire employees (for economic reasons) to "improve their competitiveness" but can do it to "safeguard" their competitiveness, which leads to many difficulties in interpretation. See Cahuc and Carcillo (2007).

[14] The last general reform regarding the number of courts in France dated back to 1958. Another smaller reform targeting only labor courts was implemented in 1992: 11 labor courts were removed.

[15] These figures come from the institution in charge of evaluating the public organizations and public services in France (*Cour des Comptes*). They are relative to the whole reform. Let us recall that this reform concerned not only labor courts but also civil and commercial courts. A total of 341 courts were removed, among which 62 were labor courts.

[16] *Départements* are French administrative subdivisions of the territory. Metropolitan France is made up of 95 *Départements*. *Départements* are themselves divided by "cantons" that serve as constituencies for the election of the members of the representative assembly in each department. Each labor court is competent on several identified "cantons" defined by the law (Decree n^0 2008-514 of May 29^{th}, 2008 and decree n^0 2014-899 of August 18^{th}, 2014.)

[17] Judges of removed labor courts were reallocated to other courts. Some 114 civil servants were working in removed labor courts: most of them have been reallocated to other jurisdictions, and 26 positions have been removed between 2008 and 2010 (Sénat (2012)).

[18] The exact criterion was to keep one labor court per *"département"*, and one on the geographical area of each civil court. These two geographical areas are more or less the same.

The reduction of the number of courts has lead to a redefinition of the territorial competency of some remaining courts. Following the decree n^0 2008-514, we distinguish between four types of courts:

- Courts that were removed at the end of 2008 (*removed courts*);
- Courts that managed claims of removed courts after 2008 (i.e. courts receiving cases). The competency of these courts was extended after 2008 to cover the geographical areas of the removed courts. In the following, we refer to this category as *receiving courts*. All (present and future) claims from a removed court were transferred to only one receiving court, identified in the decree n^0 2008-514.
- Courts that could not be removed during the reform because they were the unique court of their *Département* before 2008. They are denoted "non-treatble" courts as the reform aims to keep at least one court per *Département*;
- Courts that were not affected by the reform (*unaffected courts*): this group gathers all courts that were not removed (but could have been removed because they were in *Départements* with several courts), and whose geographical competency was unchanged by the reform.

As previously mentioned, litigants from a removed court were transferred to a new (receiving) court. Most of the time, this means that the distance to bring a claim to court for these litigants has increased after the reform (to reach the new receiving court). However, in some cases, the distance may have been reduced: if some litigants were geographically located near the frontier of a former jurisdiction, the distance to the court before the reform could have been longer than the distance to the new receiving court.

3.4 Potential impacts of the reform

The reform may have impacted job entries and exits on the labor market by changing the cost to litigate to challenge dismissals. We briefly discuss here the different channels through which the reform could impact labor conditions.

To begin with, the reform has changed the distance to go to court and potentially the delays to be heard. This may impact *the decision* to open a claim as well as the *bargaining positions*:

- First, employees can get fewer incentives to contest their dismissal when facing higher delays and increased distance to go to court. Anticipating this, employers could hope for lower firing costs as the likelihood to go to court decreases. This could increase job exits and entries on the labor market.
- Secondly, firing costs could also be smaller because pre-court negotiations could be more frequent with the reform. Indeed, during these negotiations, the outside option if parties fail to find an agreement is to bring the claim to court. If this strategy becomes more costly for the employees (because of an increased distance and/or more congestion), they will get more incentives to accept negotiations (and possibly, even for smaller settlement amounts).
- Thirdly, settlement during conciliation could increase for the same reasons. Conciliation is the first step of any conflict resolution in labor courts. Parties are formally invited to find an agreement by themselves. Any failure to agree implies that the parties have to go back to court for an hearing and may suffer from long delays to get a decision. To avoid such

a situation, disputants have higher incentives to conciliate right from the beginning of the procedure. This should decrease total litigation costs that are part of the firing costs.

The reform can also have consequences on *the nature* of the claims brought to court and their issues.

- As previously described, informal negotiation and conciliation are likely to increase to avoid hearings at court. This can be particularly true for claims whose issues can be easily anticipated (i.e. rejection or acceptation by the judges). Claims with low or high winning probabilities should be less often brought to court. On the contrary, claims with mixed evidence are more difficult to settled *ex ante* so that courts could mainly deal with these claims.

- Not only should low-winning probabilities claims be more frequently settled, but they should also be less often opened. The worker's decision to open a claim can be determined by a cost/benefit analysis: for a given anticipated benefit, the cost increase caused by the reform should diminish the probability to open claim with low-winning probabilities.

- If more mixed-evidence claims are brought to labor courts, the probability of *départage* increases. This increases the delay to get a final decision and increases congestion. Because they anticipate these longer delays, plaintiffs could get fewer incentives to open a claim.

However, other reform's effects could lead to an increase in firing costs, and make job entries or exits on the labor market less flexible.

- First, whenever claims are brought to court, employers also face higher litigation costs because of the courts' delays and the potential increased distance. These constraints can be particularly strong for small-sized firms that could perceive the reform as an indirect increase in firing costs. This could lead to lower job offers and separations.

- A "feedback" effect could also be observed: assuming that the reform has impacted firing or hiring decisions, the unemployment rate will be affected. Following Ichino et al. (2003) and Marinescu (2011), decisions at court are significantly influenced by the unemployment rate. Exploiting U.K data and controlling for case selection, Marinescu (2011) finds that when a dismissed worker has found a new job, higher unemployment decreases the worker's probability of prevailing at trial. Symmetrically, lower unemployment should lead to more claims' acceptations in court. This would give more incentives to open claims and then increase firing costs.

Last, beyond unemployment, the reform could also impact job offers: if firing costs (for permanent jobs) are modified, then the decision to propose short-term (temporary) contracts or long-term (permanent) positions can also change. More broadly, business creations (or destructions) could also be impacted: if fewer jobs are offered, people could get more incentives to create their own business.

This short description illustrates how difficult it is to determine the final impact of the reform on the labor market. This calls for an empirical analysis to identify the realized consequences of the access to court on employment.

4 Data

4.1 Information and units of observation

We build our dataset gathering information from the French Ministry of Justice and from the National Institute for Statistics (INSEE). The decree n^0 2008-514 of May 29th, 2008 lists the courts that were suppressed. The Ministry of Justice delivered us with the precise composition of each jurisdiction at the municipality's level (i.e. the geographical competency of each court) before and after the 2008 reform. This allows us to conduct our analysis at the municipality level. We also collect information on the INSEE website on the French metropolitan municipalities both in 2006 (two years before the reform) and in 2011 (three years after the reform). We then know socio-economic characteristics such as population, unemployment rate, working population, proportions of each social category, the number of firms created each year. We also calculate the distance between each municipality and its competent labor court, before and after the reform.[19]
Last, we also use the data of the Ministry of Justice regarding the average case duration, the number of new claims, the acceptance rate of the plaintiffs, and the probability to go to départage at each court's level (still in 2006 and 2011).

4.2 Descriptive Statistics

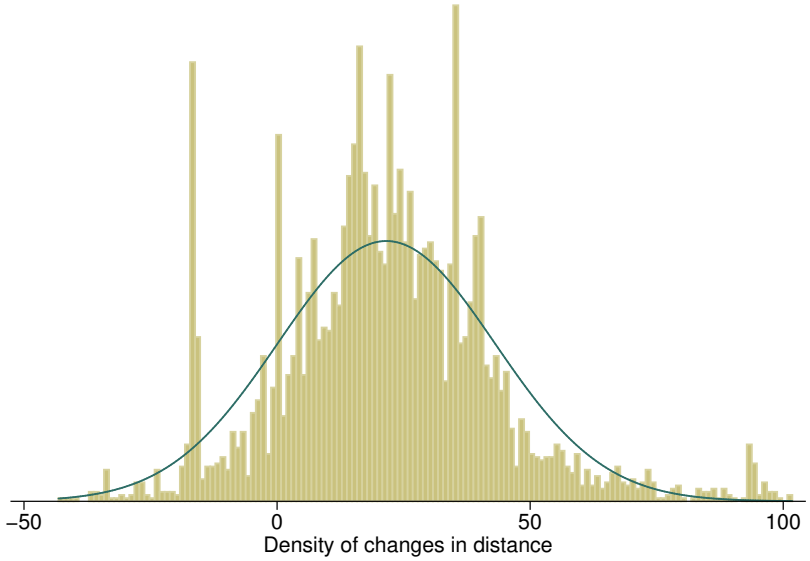
Building on the distinction between removed/receiving/unaffected and non-treatable courts described in subsection 3.3, we define four categories of cities:

- Cities whose labor court has been removed and that were assigned to a new labor court (*removed-treated cities*);
- Cities whose labor court has expanded its geographical competency (*receiving-treated cities*);
- Cities whose labor court was potentially removable but was not removed (*unaffected cities*).
- Cities that were precluded from treatment, because there was only one labor court prior to the reform in the *département* (*non-treatable cities*);

Last, figure 4.2 illustrates the distribution of changes in distances before and after the reform for removal-treated cities. Two remarks are in order: first, workers in most of the "removed-treated cities" have to go on average 25 kilometers further to reach their receiving labor court. Second, for some workers, the change in distance is negative, meaning that the distance has become shorter. The new labor court is closer than the older removed court. This is for instance the case when people work in cities located at the frontier of a zone. The distance to reach the court within the zone could be longer than the distance to go to another court located in another zone but close to the frontier. We then benefit from an interesting situation where an exogenous shock (the reform) makes the distance to court either shorter, longer or the same.

[19] Our calculations were made in June 2015 using google map and represent the number of kilometers to go to the labor court by car.

Figure 1: Distribution of changes in distance after the reform for removed cities.



5 Empirical Investigation

The main objective of this paper is to assess the effect of the removal of labor courts on the economic activity at the city level. Because cities have been treated in different ways, we propose to estimate two kinds of effects. First, we propose to estimate the *average treatment on the treated* (ATT), i.e. the reform's average effect on cities whose labor court has been removed. Second, we seek to estimate a effect conditionally on the increase or decrease of distance to the associated labor court.

Estimation Method The evaluation of public policies in non-randomized experiments is usually achieved either by propensity score matching (PSM) or by difference-in-difference (DiD) estimations. The goal of these methods is to estimate the average reaction of treated units to a specific treatment. The two techniques differ, however, on their assumptions about reaction functions and treatment assignment.

The main assumption supporting DiD estimations states that treated and non-treated units would have had similar trends if treated units would not have been treated (Common Trend Assumption, CTA). They further assume that treatment does not generate any general equilibrium effect (Stable Unit Treatment Value Assumption, SUTVA). If these two assumptions hold, DiD methods aim at assessing the treatment effect by comparing the change of trends between the treated and non-treated units once the treatment is introduced.

Alternatively, PSM estimations rely on two assumptions. First, the Conditional Independence Assumption (CIA) requires that a treated unit would have had the same outcome as non-treated units if it had not been treated, conditionally on the observables. This assumption ensures that one can take outcome of similar untreated units to build counterfactuals of the treated units. The DiD estimations make a stronger assumption since they assume that both treated and non-treated units have the same reaction function (un)conditional on the observables (depending on whether DiD are controlled by regression adjustment).

As far as the 2008 reform is concerned, Espinosa, Desrieux and Wan 2015 have shown that removal decisions were not random.[20] Nevertheless, the graphical discussion of the outcome variables in the previous section suggest that the CTA is likely to hold: evolutions between 2006 and 2007 were similar in the treated and control groups.

Considering these issues, we proceed in two steps. First, we estimate a DiD model including the treatment effect together with covariates that might also impact the considered outcome (regression adjustment). Given this first set of results, we discuss whether the CTA is likely to hold. Second, we use matching methods on the trends of the outcomes to generate control and treated samples who have common trends prior the reform. We show that the CTA is more likely to hold once the correction is applied, and discuss the new set of results.

5.1 Difference-in-Difference

5.1.1 Method

We first exploit the panel structure of the data to estimate the reform's effects in a *difference-in-difference* framework with regression adjustment. To assess the ATT, we estimate the following specification on the sample that includes cities which could have been affected by the reform and cities which have been affected by the reform. Our specification writes:

$$y_{it} = \beta X_{it} + \mu_t + \alpha_i + \sum_{s \neq 2007} \phi_s (D_i \times \mathbb{1}_{s=t}) + u_{it} \tag{1}$$

where X is the set of covariates discussed in the descriptive statistics[21], μ_t is the set of year fixed effects, α_i is the set of city fixed effects, and u_{it} is a random term. The variable D_i is equal to 1 for *removed treated* cities. The set of coefficients $\phi = \{\phi_{2006}, \phi_{2008}, \phi_{2009}, \phi_{2010}, \phi_{2011}, \phi_{2012}\}$ represents the ATT for each year. Year 2007 serves as a reference: the reform was discussed, voted and ratified in 2008. To counter anticipation effects, we take 2007 as reference. A non-significant coefficient ϕ_{2006} implies that treated and non-treated cities have had a similar trend between 2006 and 2007, making the CTA more likely to hold. The outcomes y_{it} considered here correspond to the normalized scores of job inflows, job outflows, unemployed workers, and creations of new enterprises. Given the normalization, marginal effects are to be interpreted as a difference of growth rate between the treated and non-treated cities between 2007 and year t.

Second, we also explore the reform's effect conditionally on the change of distance to the new labor court. We thus run the above model for two subsets of treated cities: those for which the distance to the associated labor court increased, and those for which this distance decreased.

5.1.2 Results

Table 2 in the appendix shows the estimates of the coefficients associated with the difference between treated and non-treated cities. It appears that cities affected by the reform have had a significant decrease of the job outflows and job inflows, together with a decrease in the number of new companies. However, this first estimation also detects a significant higher trend of firm creation for treated cities prior to the reform ($\phi_{2006} > 0$). This casts some doubts on the validity of the CTA for firm

[20] *Removed* courts were dealing with fewer cases and were closer to other labor courts.

[21] We exclude the duration of cases at the court level, since data are missing in 2008. Results including them are qualitatively equivalent. We present them as robustness check.

creation. On the one hand, it might be that the estimated negative coefficients associated with the reform after 2008 underestimate its impact: treated cities were benefiting from a positive trend in firm creation which was so strongly impacted that it even became negative after the reform. On the other hand, it is possible that this estimation overestimates the reform's impact on firm creations: the boom of company creations between 2006 and 2007 would have potentially been offset by a negative trend after 2007 even without the reform.

Second, we estimate our econometric model separately for cities that experienced an increase in distance to the labor court from those which experienced a decrease in distance. Results are displayed in table 3 for cities that experienced an increase in distance, and in table 4 for cities that experienced a decrease in distance. These results are also displayed graphically on figures 2 and 3 respectively.

A few comments can be made in the light of this new set of estimates. As far as the cities whose distance to the labor court increased are concerned, we observe similar but stronger effects compared to the average effects estimated for the entire set of treated cities: job inflows, job outflows and the flow of new enterprises significantly decreased in these cities compared to cities that were not affected by the reform. The growth rate of job outflows between 2007 and 2012 were about 4 percentage points (pp) smaller for cities with an increased distance to their labor courts, 4.6 pp smaller for job inflows and 6.3 pp for the flow of new enterprises. In other terms, the well-functioning of the labor market was significantly degraded by the reform in these areas. Employers are less likely to hire new employees, to fire existing employees, and less likely to create new firms. The overall effect on the overall number of unemployed workers in not statistically significant, but is estimated as positive. Regarding the cities which experienced a decrease in the distance to their labor court, we observe, if any, the opposite effects. Although none of the effects are significant, we observe a small increase in job outflows and job inflows.

Robustness In order to confirm the robustness of these results, we first estimate these models including the non-treatable cities in the control group. Results are similar and are displayed in tables A1 to A3 of the online appendix.

Second, we estimate the above specifications for the two types of treated cities using a multilevel mixed effects model. We introduce random effects at the city and the labor court $\times$ year level ($\phi_{c(i),t}$). The vector of covariates X_{it} is decomposed between covariates that are defined at the city level ($X_{1,it}$) and those that are defined at the court level ($X_{2,c(i)t}$) The general model writes:

$$y_{it} = \beta X_{1,it} + \mu_t + \alpha_i + \phi_{c(i),t} + \sum_{s \neq 2007} \phi_s (D_i \times \mathbb{1}_{s=t}) + u_{it} \tag{2}$$

$$\alpha_i = \gamma_0 + e_i$$

$$\phi_{c(i),t} = \lambda X_{2,c(i)t} + \epsilon_{c(i),t}$$

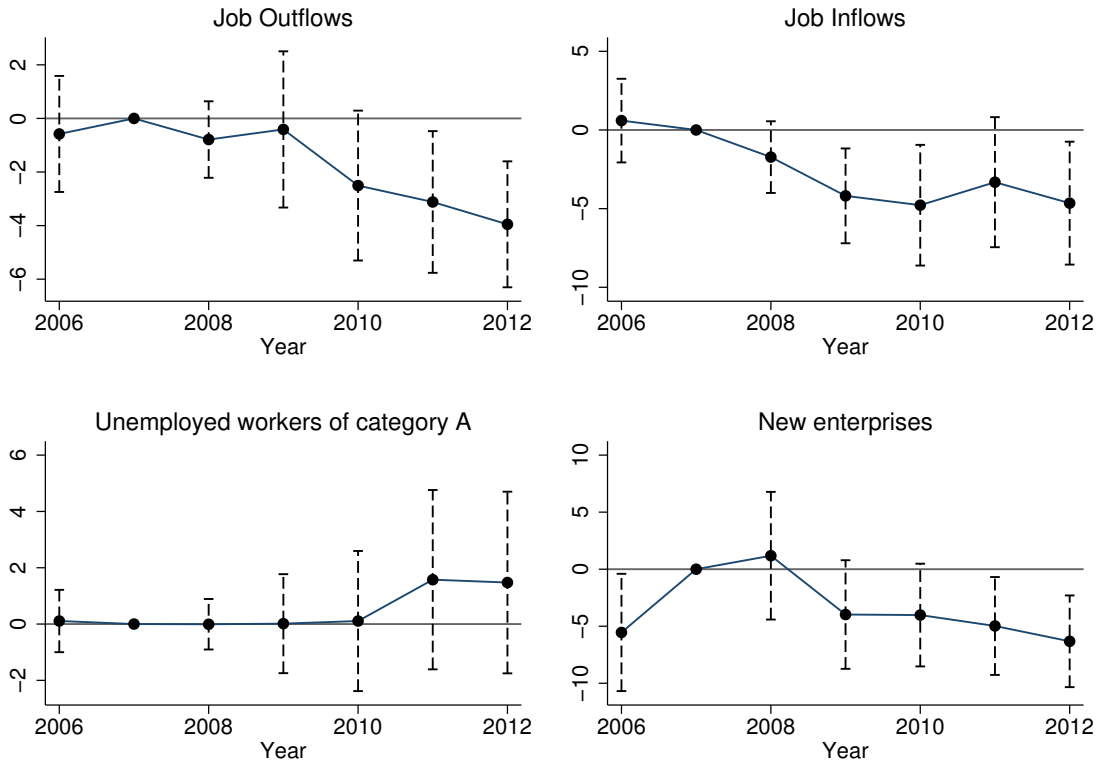
where λ includes an intercept.

Estimates of this model for the two separate samples (e.g. increase or decreasing distance) are given in tables A4 and A5. Results obtained by this alternative model are similar, i.e. a significant decrease in the number of job outflows, job inflows, and new enterprises for cities which experienced an increase of distance to their labor court.

CTA Among the above regressions, two variables may be suspected to violate the common trend assumption. First, the number of new enterprises seems to have experienced a greater increase between 2006 and 2007 in treated cities compared to the control group (2). This effect is mainly

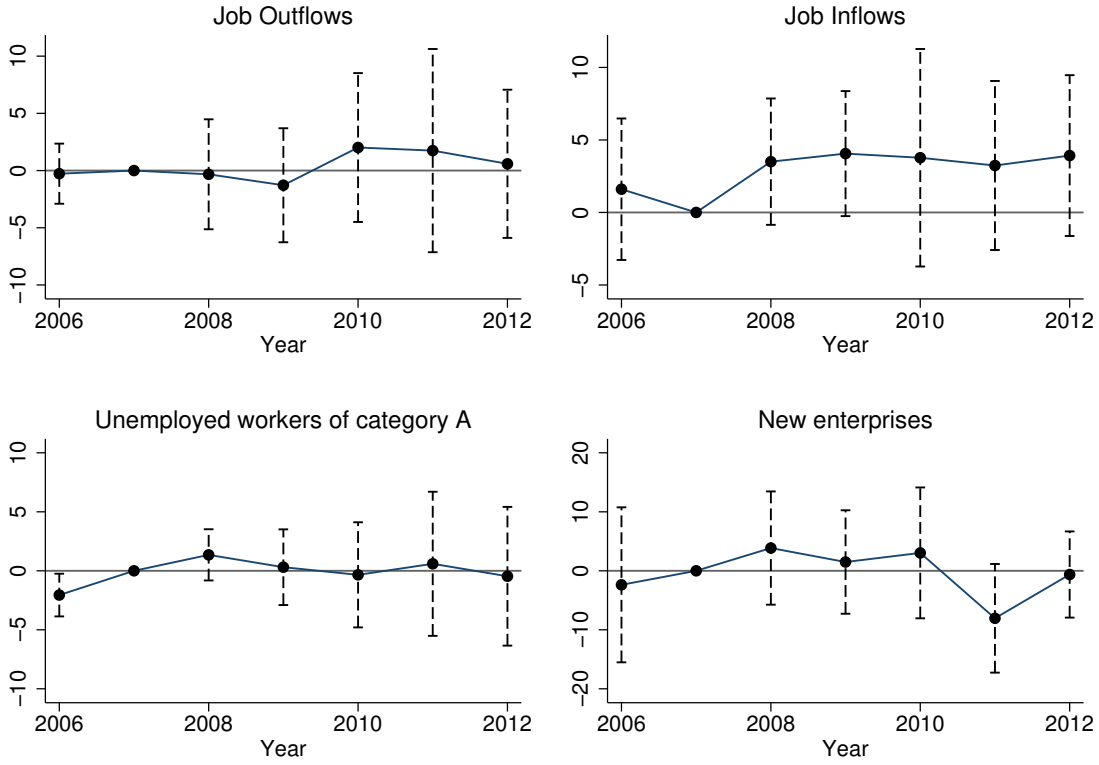
due to cities whose distance to the labor court increased (3). Second, the number of unemployed workers has experienced a significantly stronger increase for cities whose distance to the associated labor court decreased compared to the control group (table 4).

Figure 2: Marginal effect of the reform for cities that experienced an increase in the distance to their labor court. (Difference-in-difference)



Note: Confidence intervals at 95% confidence level. Outcomes normalized to 100 in 2007, regression-adjusted difference-in-difference, standard errors clustered at the court level.

Figure 3: Marginal effect of the reform for cities that experienced a decrease in the distance to their labor court. (Difference-in-difference)



Note: Confidence intervals at 95% confidence level. Outcomes normalized to 100 in 2007, regression-adjusted difference-in-difference, standard errors clustered at the court level.

5.2 Conditional Difference-in-Difference

The above results show that the CTA is potentially violated for some outcomes. We therefore propose to correct our estimations by weighting the sample of the control units to obtain a sample of non-treated cities whose pre-reform trend is similar to the treated cities'. We first estimate a propensity score, and then run the above regressions with the weights we derived from matching processes.

5.3 Matching

In order to obtain units with similar pre-reform trends, we estimate the propensity score at the city level including variations between 2006 and 2007 of all dependent and independent variables.[22] Matching on pre-reform trends aims at correcting for pre-trend differences that the DiD estimates detected in the above subsection. The latent utility model writes:

$$removal_i^* = \gamma \Delta Y_i + u_i \tag{3}$$

where $removal^*$ is the latent variable associated with *removal* (equal to 1 if the labor court associated to city i was removed, to 0 if not, and missing if the labor court expanded its activity). ΔY_i represents the changes in the outcomes between 2006 and 2007.

We consider several matching algorithms to compute weights using the propensity score obtained with the above equation. First, we use a Epanechnikov Kernel (EK). Second, we consider a Gaussian Kernel (GK). Third, we compute the weights using a nearest neighbor algorithm with 3 neighbors (N3). Fourth, we also use the the *Covariate Balancing Propensity Score* (CBPS) method. This technique relies on GMM estimations and estimates the propensity scores and the weights jointly in order to maximize the decrease in bias.

Comparing Matching Techniques Table 5 displays the average standardized bias (ASB) associated with each matching model. It also shows, for each estimation, the number of variables whose standardized bias is above 5%.[23] We present two sets of results: on the left-hand side, we display the ASB for the matching procedure dealing with cities whose distance to the labor court increased. On the right-hand side, we show results for cities whose distance to their labor court decreased.

First of all, the data show a relatively small level of heterogeneity regarding the pre-reform trends: the average standardized bias is below the 5% for cities with an increased distance and below the 10% threshold for the second sample. We note however that several variables can considered as biased, *i.e.* as statistically different across treatment groups: 2 in both samples. In other words, for these variables, treated and control units experienced different trends between 2006 and 2007. This justifies the use of matching techniques to obtain a control group whose trend is similar to those of the treated cities.

Second, as far as the matching algorithm is concerned, it appears that the CBPS is the most efficient technique to reduce the ASB. Indeed, it decreases the ASB from 4.44 (resp. 7.25) in the unmatched sample to 0.06 (resp. 0.03) in the matched sample for the cities with an increase (resp.

[22] Matching is done on changes between 2006 and 2007 rather than between 2007 and 2008 to avoid potential anticipation effects, since the reform was discussed and voted in 2008.

[23] The 5% threshold has been usually used in the literature since the original paper of Heckman (1998).

decrease) of distance. The CBPS algorithm also minimizes the number of biased variables. The Nearest Neighbor Algorithm (N3) is the second-best matching algorithm regarding the ASB.

5.4 Results of the weighted DiD regressions

We now estimate the above DiD models using with the weights obtained from the matching process to correct the CTA.[24] Given that the reform seems to have had different effects considering the nature of the change of distance (increase or decrease), we estimate an ATT for each type of affected courts.

Table 6 and figure 4 display the results of this new estimation for cities that experienced an increase in the distance to their labor court. First, one can observe substantive improvement in the CTA, which hold for all outcomes considered here. Second, this new set of results confirm the above findings: cities that experienced an increase in the distance to their labor court have suffered from a decrease in job outflows (about -3.8 percentage points in 2012), job inflows (-5.8 pp) and in the creation of new enterprises (-5.4 pp).

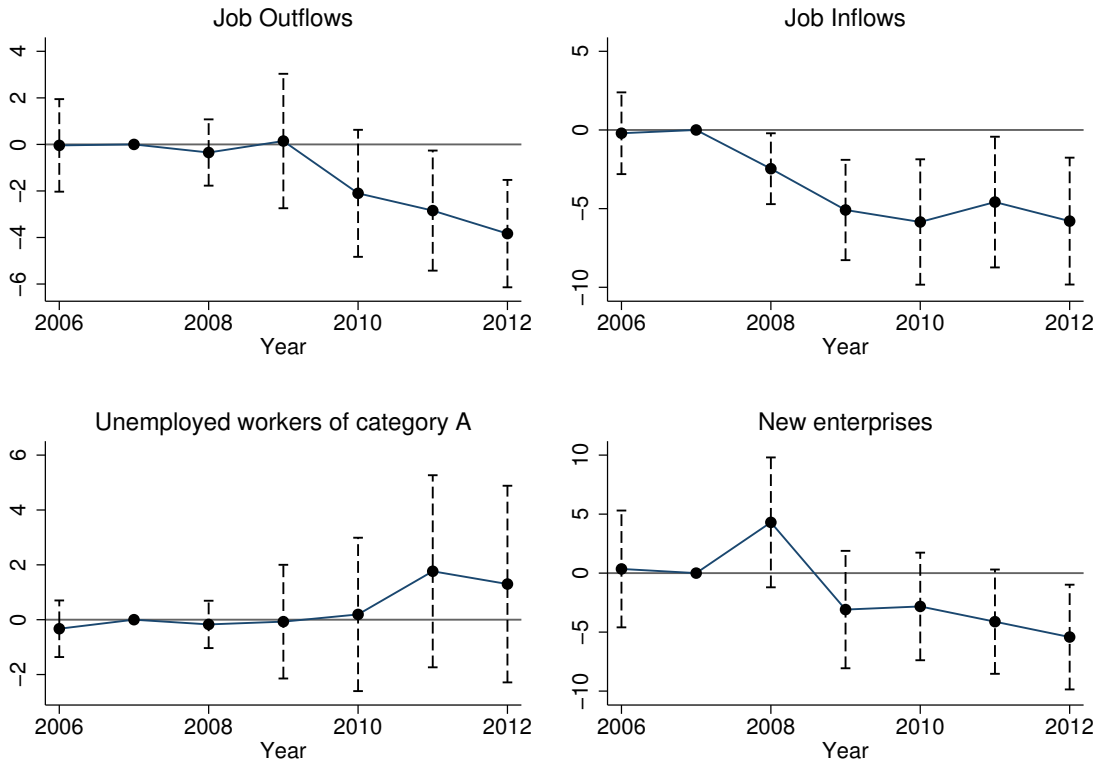
As far as cities that experienced a decrease in the distance are concerned (table 7 and figure 7), we do not observe any significant effect of the reform. This absence of effect might result from the low number of cities that benefited from this decrease compared to the pool of cities that suffered from an increase of distance, or/and from the relatively small amount of distance variation (here again, compared to the cities which experienced an increase).

Robustness We explore further specifications to ensure the robustness of the above results. First, we include in the control groups non-treatable cities, i.e. cities whose labor court could not have been removed by the reform (online appendix, tables A7 and A8) Second, we include the average duration of terminated cases of the associated labor court in the control variables of the regressions (online appendix, tables A9 and A10). To do so, we are forced to exclude observations in 2008 since the data of the Ministry of Justice reports many missing values in 2008.[25] Third, we estimate the above models using the second-best matching algorithm in terms of bias reduction (N3) (online appendix, tables A11 and A12). Finally, we use the CBPS scores with the multilevel mixed-effects model with the same specification as in the previous section (online appendix, tables A13 and A14). All these alternative estimations yield the same results as the above main estimates: the reform has had no significant effect on cities that experienced a decrease of distance to their labor court, but had a negative effect on job inflows, job outflows, and creations of new companies for cities which are now further from their labor court.

[24] We used the Stata *xtreg* package to estimate the model with weights, clustering observations at pre-reform labor court level.

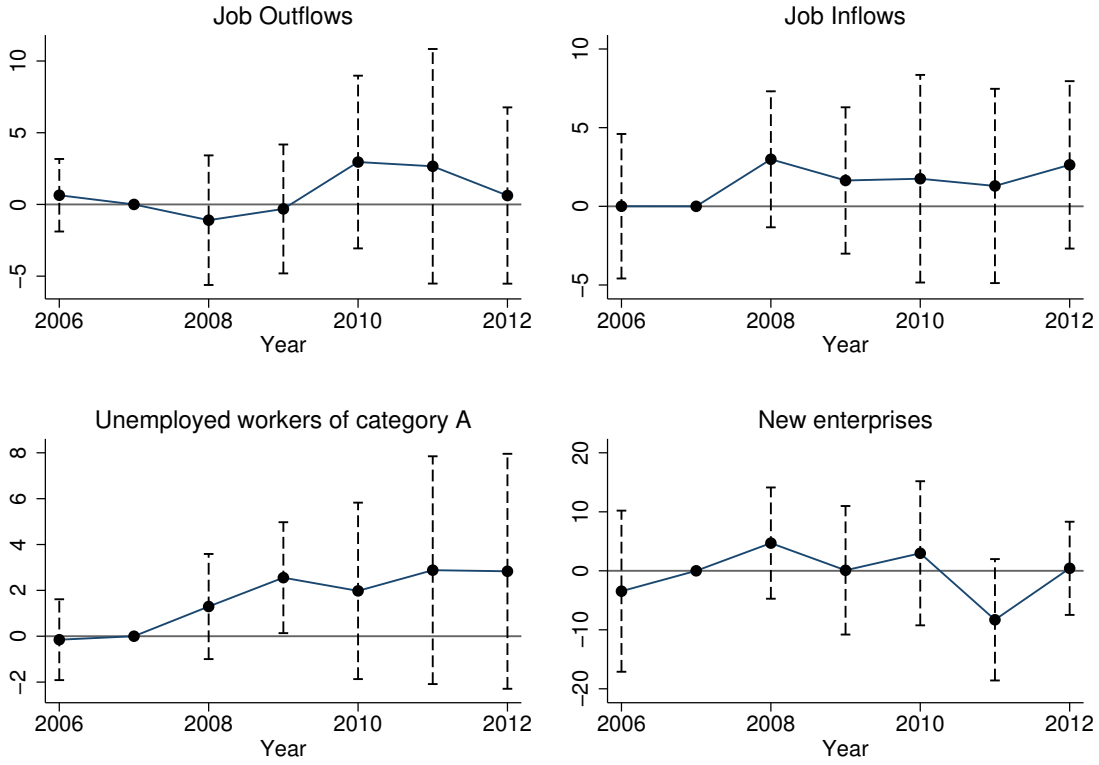
[25] We prefer to drop data in 2008 rather than only missing values, because the estimations are made using weights, which are computed with the entire sample. Dropping one year for every observation does not affect the validity of the matching process.

Figure 4: Marginal effect of the reform for cities that experienced an increase in the distance to their labor court. (Conditional Difference-in-difference)



Note: Confidence intervals at 95% confidence level. Outcomes normalized to 100 in 2007, regression-adjusted difference-in-difference, standard errors clustered at the court level.

Figure 5: Marginal effect of the reform for cities that experienced a decrease in the distance to their labor court. (Conditional Difference-in-difference)



Note: Confidence intervals at 95% confidence level. Outcomes normalized to 100 in 2007, regression-adjusted difference-in-difference, standard errors clustered at the court level.

6 Conclusion and Discussion

Since the financial crisis, many developed countries have faced a dual challenge: to reduce the amount of public expenditures without impacting the economic situation. A popular idea among public decision-makers has been to rationalize the public administration in order to reduce the associated costs. Several countries have then sought to decrease the number of courts over their territory. However, the impact of such reforms on the labor market has been hardly predicted -and even considered- by decision-makers given the lack of empirical evidence on this issue.

Our paper is the first empirical investigation that assesses the impact of the accessibility of labor courts on the labor market. To do so, we exploit the French reform of 2008 that removed more than 20% of the labor courts over the territory. We distinguish two types of cities affected by the reform: those that experienced an increase in the distance to the associated labor court, and those whose distance to the labor court decreased. Using (conditional) diff-in-diff estimation methods, we show that the increase of distance is associated with a degradation of the labor market. We observe indeed a significant decrease in the growth rate of job outflows (-4 percentage points), job inflows (-4.6 pp) and creations of enterprises (-6.3 pp) for cities which experienced this increase in distance compared to cities that were not affected by the reform. On the other hand, we find no significant change when the reform decreased the distance to the labor court.

These results show some doubts about the economic profitability of such reforms. Although the financial benefits of the reduction of the number of courts are easily quantifiable, the associated costs are harder to anticipate. Our estimated effects are far from negligible and may lead public decision-makers to reconsider such reforms. Labor courts are indeed responsible of the good enforcement of labor contracts: the decrease of accessibility to the authority that ensures this enforcement is very likely to weaken the use of such contracts. In addition, the costs associated with an increase in distance might be negligible for big companies but are likely to represent a real burden for small firms. Regions affected by the reform already had a weaker economic dynamic prior to the reform. The economic network of these regions is mainly composed of small employers for whom greater distance to labor courts might necessitate longer absence at work or even the use of a lawyer. Considering that the presence of parties is mandatory at several steps of the procedure –even when the party is represented at some steps–, the repeated costs of longer distance might translate into real burdens for employers. All in all, our estimation shows that the increased burden for both employees and employers leads to degraded labor market conditions.

Limitations Our empirical investigation intended to address most of the challenges evaluation of public policies generally face. However, two main difficulties were not tackled by this applied work because the necessary econometric tools have not been developed so far. First, we are aware that the estimation of the propensity score might generate some measurement errors, which are not taken into account here. Abadie and Imbens (2016) have indeed shown that the propensity score estimation affects the large sample distribution of the matching estimator. Their contribution is however so far limited to the ATT without regression adjustment. Further developments (especially to regression adjustment and conditional DiD) are required to correct for the potential biases in our case. Second, the matching algorithm might generate a need for additional clustering between matched units. Abadie and Spiess (Working Paper 2016) derive results for the K-nearest neighbors matching technique without replacement. This matching algorithm is the most simple case since cluster are exclusive, and we have no doubt that these results will be extended to more complex matching algorithms in the coming years.

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Appendix 1: Maps of Judicial System

Figure 6: French courts before the reform and removals during the reform

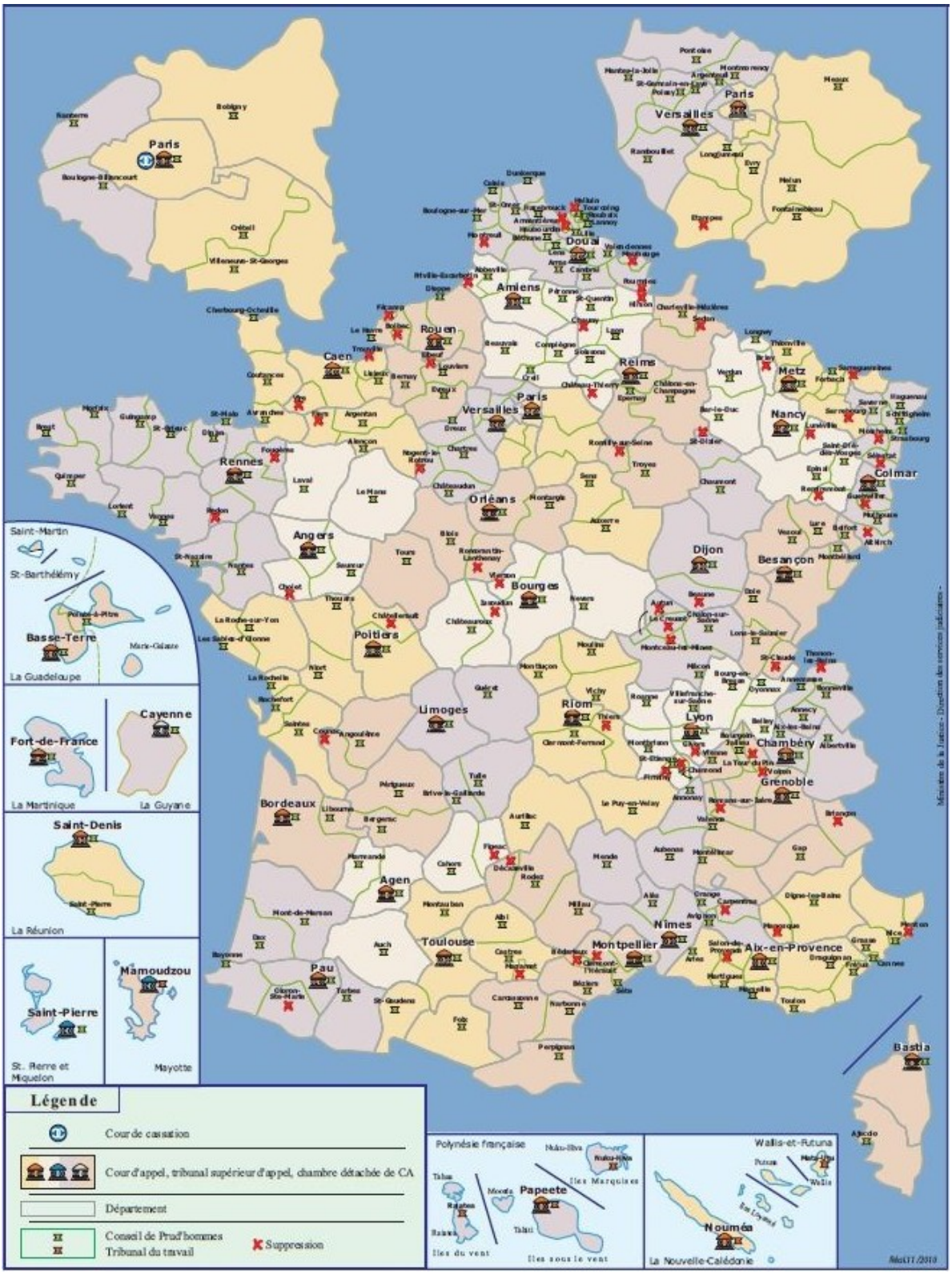
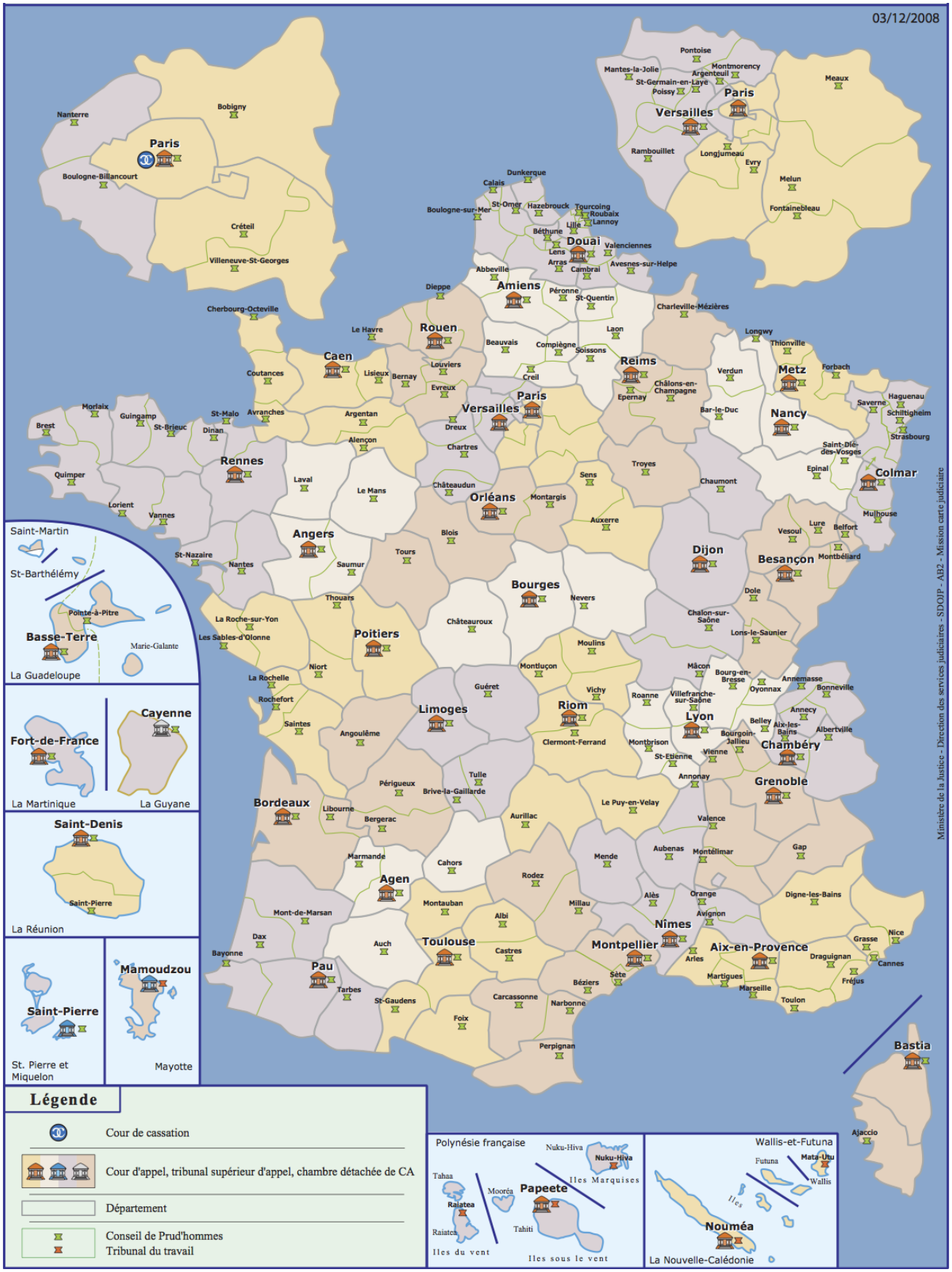


Figure 7: French courts after the reform



Appendix 2: Tables

Table 1: Summary statistics in 2007.

Variable	Label	Non-treatable	Removed	Receiving	Untreated
unemployed	Number of unemployed workers	100	100	100	100
jobOut	Number of job outflows (registration to unemployment benefits)	100	100	100	100
jobIn	Number of unemployed workers who left unemployment benefits after finding a job	100	100	100	100
newEntr	Number of firms created per year	100	100	100	100
distance	Distance between the city and its labor court (km)	28.986	17.178***	22.005***	28.618
		(23.082)	(13.215)	(21.791)	(54.593)
pop	Population (log)	7.728	7.614***	7.768	7.803
		(.95)	(.783)	(.926)	(.95)
popAge	Working age population (log)	7.253	7.159***	7.326	7.355
		(.962)	(.779)	(.931)	(.955)
propCS1	Proportion of individuals in the 1st social category	.02	.015	.012***	.014
		(.023)	(.019)	(.017)	(.019)
propCS2	Proportion of individuals in the 2nd social category	.039	.036***	.037***	.038
		(.016)	(.017)	(.016)	(.017)
propCS3	Proportion of individuals in the 3rd social category	.05	.054***	.072***	.068
		(.027)	(.03)	(.043)	(.047)
propCS4	Proportion of individuals in the 4th social category	.124	.13***	.145***	.14
		(.041)	(.04)	(.044)	(.043)
propCS5	Proportion of individuals in the 5th social category	.169	.162***	.168**	.166
		(.033)	(.035)	(.035)	(.036)
propCS6	Proportion of individuals in the 6th social category	.158	.179***	.152***	.158
		(.051)	(.054)	(.052)	(.056)
propCS7	Proportion of individuals in the 7th social category	.298	.274***	.262**	.266
		(.081)	(.062)	(.069)	(.074)
propCS8	Proportion of individuals in the 8th social category	.142	.15	.153**	.15
		(.042)	(.042)	(.042)	(.041)
newAff$^{\diamond}$	Number of new claims per year	991.7	196.2***	1266***	926.556
		(1108.0)	(72.3)	(1268)	(987.413)
succRate$^{\diamond}$	Success rate for plaintiffs	.712	.74***	.722***	.714
		(.092)	(.128)	(.081)	(.074)
concRate$^{\diamond}$	Conciliation rate	.107	.127***	.118***	.108
		(.052)	(.054)	(.047)	(.051)
depRate$^{\diamond}$	Rate of *départage*	16.59	8.28***	13.94	14.067
		(9.624)	(10.353)	(8.769)	(10.205)
durAff$^{\diamond}$	Average duration of terminated cases in month (log)	10.443	8.355***	11.099**	10.94
		(2.676)	(2.378)	(3.059)	(3.058)

Means and standard deviations (in parentheses). Variables with $\diamond$ are reported at the labor court's level.
Stars indicate that the sample mean is statistically different from the untreated cities' sample mean at 10%, 5% and 1%

Table 2: Estimation of the difference between cities not affected by the reform and cities whose labor court was removed (Difference-in-difference, regression adjusted, excluding non-treatable cities).

	outflows	inflows	unemployed	newEntr
ϕ_{2006}	-0.554	0.705	-0.0908	-5.252**
	(1.031)	(1.345)	(0.533)	(2.445)
ϕ_{2008}	-0.766	-1.250	0.0920	1.433
	(0.782)	(1.152)	(0.435)	(2.684)
ϕ_{2009}	-0.444	-3.456**	0.126	-3.481
	(1.462)	(1.504)	(0.829)	(2.403)
ϕ_{2010}	-2.076	-4.020**	0.140	-3.357
	(1.443)	(1.862)	(1.223)	(2.268)
ϕ_{2011}	-2.661*	-2.730	1.556	-5.236**
	(1.425)	(2.061)	(1.561)	(2.049)
ϕ_{2012}	-3.536***	-3.879**	1.364	-5.771***
	(1.222)	(1.919)	(1.589)	(1.973)
City FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Covariates	Yes	Yes	Yes	Yes
Observations	50,376	50,376	50,372	49,497
R-squared	0.255	0.151	0.341	0.279

Significance level: *** significant at 1% level; ** significant at 5% level; * significant at 10% level.
Robust standard errors clustered at the court level.

Table 3: Estimation of the difference between cities not affected by the reform and cities that experienced an increase of distance to their labor court (Difference-in-difference, regression adjusted, excluding non-treatable cities).

	outflows	inflows	unemployed	newEntr
$\phi_{I,2006}$	-0.579	0.596	0.108	-5.547**
	(1.105)	(1.356)	(0.565)	(2.624)
$\phi_{I,2008}$	-0.789	-1.723	-0.00714	1.181
	(0.729)	(1.163)	(0.458)	(2.855)
$\phi_{I,2009}$	-0.411	-4.187***	0.0137	-3.976
	(1.487)	(1.538)	(0.897)	(2.429)
$\phi_{I,2010}$	-2.506*	-4.781**	0.107	-4.021*
	(1.427)	(1.957)	(1.270)	(2.298)
$\phi_{I,2011}$	-3.118**	-3.319	1.576	-4.979**
	(1.349)	(2.109)	(1.625)	(2.188)
$\phi_{I,2012}$	-3.952***	-4.651**	1.475	-6.323***
	(1.200)	(1.993)	(1.646)	(2.051)
City FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Covariates	Yes	Yes	Yes	Yes
Observations	49,640	49,640	49,636	48,768
R-squared	0.255	0.152	0.340	0.279

Significance level: *** significant at 1% level; ** significant at 5% level; * significant at 10% level.
Robust standard errors clustered at the court level.

Table 4: Estimation of the difference between cities not affected by the reform and cities that experienced a decrease of distance to their labor court (Difference-in-difference, regression adjusted, excluding non-treatable cities).

	outflows	inflows	unemployed	newEntr
$\phi_{D,2006}$	-0.271	1.603	-2.057**	-2.375
	(1.340)	(2.488)	(0.923)	(6.703)
$\phi_{D,2008}$	-0.324	3.504	1.351	3.859
	(2.451)	(2.222)	(1.107)	(4.897)
$\phi_{D,2009}$	-1.282	4.058*	0.306	1.490
	(2.542)	(2.199)	(1.637)	(4.476)
$\phi_{D,2010}$	2.015	3.773	-0.343	3.033
	(3.320)	(3.825)	(2.273)	(5.659)
$\phi_{D,2011}$	1.746	3.238	0.592	-8.051*
	(4.530)	(2.973)	(3.116)	(4.697)
$\phi_{D,2012}$	0.591	3.922	-0.463	-0.630
	(3.303)	(2.829)	(3.000)	(3.727)
City FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Covariates	Yes	Yes	Yes	Yes
Observations	42,891	42,891	42,901	42,228
R-squared	0.267	0.138	0.348	0.278

Significance level: *** significant at 1% level; ** significant at 5% level; * significant at 10% level.
Robust standard errors clustered at the court level.

Table 5: Diagnosis of matching process: Average standardized bias and number of biased variables at 5% (excluding non-treatable cities).

	Increased Distance		Decreased Distance	
Algorithm	Average Bias	# Biased Var.	Average Bias	# Biased Var.
Before Matching	4.44	2	7.25	2
EK	2.82	0	7.28	2
GK	3.63	0	7.27	2
N3	1.28	0	4.92	2
CBPS	0.06	0	0.03	0

Table 6: Estimation of the difference between cities not affected by the reform and cities that experienced an increase of distance to their labor court (Conditional difference-in-difference, regression adjusted, excluding non-treatable cities).

	outflows	inflows	unemployed	newEntr
$\phi_{I,2006}$	-0.0423	-0.206	-0.330	0.354
	(1.015)	(1.326)	(0.526)	(2.525)
$\phi_{I,2008}$	-0.349	-2.461**	-0.172	4.300
	(0.727)	(1.149)	(0.440)	(2.809)
$\phi_{I,2009}$	0.145	-5.085***	-0.0715	-3.089
	(1.474)	(1.626)	(1.057)	(2.538)
$\phi_{I,2010}$	-2.102	-5.848***	0.192	-2.825
	(1.392)	(2.033)	(1.426)	(2.327)
$\phi_{I,2011}$	-2.845**	-4.582**	1.765	-4.116*
	(1.316)	(2.120)	(1.786)	(2.255)
$\phi_{I,2012}$	-3.834***	-5.792***	1.299	-5.420**
	(1.177)	(2.058)	(1.829)	(2.264)
City FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Covariates	Yes	Yes	Yes	Yes
Observations	48,752	48,752	48,755	48,754
R-squared	0.234	0.190	0.330	0.290

Significance level: *** significant at 1% level; ** significant at 5% level; * significant at 10% level.
Robust standard errors clustered at the court level.

Table 7: Estimation of the difference between cities not affected by the reform and cities that experienced a decrease of distance to their labor court (Conditional difference-in-difference, regression adjusted, excluding non-treatable cities).

	outflows	inflows	unemployed	newEntr
$\phi_{D,2006}$	0.641	0.00567	-0.151	-3.459
	(1.290)	(2.343)	(0.900)	(6.968)
$\phi_{D,2008}$	-1.097	2.990	1.296	4.696
	(2.305)	(2.206)	(1.170)	(4.812)
$\phi_{D,2009}$	-0.314	1.644	2.554**	0.0869
	(2.293)	(2.373)	(1.234)	(5.558)
$\phi_{D,2010}$	2.958	1.755	1.978	2.969
	(3.072)	(3.366)	(1.963)	(6.231)
$\phi_{D,2011}$	2.661	1.296	2.881	-8.300
	(4.172)	(3.150)	(2.535)	(5.249)
$\phi_{D,2012}$	0.622	2.636	2.833	0.421
	(3.136)	(2.715)	(2.615)	(4.030)
City FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Covariates	Yes	Yes	Yes	Yes
Observations	42,226	42,226	42,229	42,228
R-squared	0.272	0.130	0.381	0.282

Significance level: *** significant at 1% level; ** significant at 5% level; * significant at 10% level.
Robust standard errors clustered at the court level.

Online Appendix

Access to Justice & Unemployment: Evidence from French Labor Courts

Romain Espinosa, Claudine Desrieux and Marc Ferracci

Difference-in-Difference

Table A1: Estimation of the difference between cities not affected by the reform and cities whose labor court was removed (Difference-in-difference, regression adjusted, excluding non-treatable cities).

	outflows	inflows	unemployed	newEntr
ϕ_{2006}	-0.691	0.626	-0.109	-5.707**
	(1.040)	(1.315)	(0.529)	(2.403)
ϕ_{2008}	-0.610	-1.114	0.149	0.626
	(0.782)	(1.129)	(0.422)	(2.682)
ϕ_{2009}	0.107	-3.465**	0.184	-3.210
	(1.453)	(1.494)	(0.829)	(2.389)
ϕ_{2010}	-1.786	-3.698**	0.206	-3.054
	(1.437)	(1.836)	(1.216)	(2.243)
ϕ_{2011}	-2.485*	-2.468	1.677	-4.493**
	(1.411)	(2.035)	(1.539)	(2.033)
ϕ_{2012}	-3.463***	-3.668*	1.530	-5.394***
	(1.210)	(1.880)	(1.562)	(1.972)
City FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Covariates	Yes	Yes	Yes	Yes
Observations	57,124	57,121	57,120	56,147
R-squared	0.243	0.155	0.340	0.279

Significance level: *** significant at 1% level; ** significant at 5% level; * significant at 10% level.
Robust standard errors clustered at the court level.

Table A2: Estimation of the difference between cities not affected by the reform and cities that experienced an increase of distance to their labor court (Difference-in-difference, regression adjusted, including non-treatable cities).

	outflows	inflows	unemployed	newEntr
$\phi_{I,2006}$	-0.710	0.521	0.0904	-5.991**
	(0.525)	(0.695)	(0.872)	(0.0216)
$\phi_{I,2008}$	-0.632	-1.580	0.0515	0.375
	(0.387)	(0.168)	(0.908)	(0.895)
$\phi_{I,2009}$	0.142	-4.201***	0.0706	-3.706
	(0.923)	(0.00642)	(0.937)	(0.127)
$\phi_{I,2010}$	-2.215	-4.461**	0.172	-3.718
	(0.120)	(0.0220)	(0.892)	(0.103)
$\phi_{I,2011}$	-2.937**	-3.056	1.697	-4.227*
	(0.0281)	(0.144)	(0.291)	(0.0532)
$\phi_{I,2012}$	-3.874***	-4.442**	1.640	-5.934***
	(0.00122)	(0.0239)	(0.312)	(0.00419)
City FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Covariates	Yes	Yes	Yes	Yes
Observations	56,388	56,385	56,384	55,418
R-squared	0.243	0.157	0.340	0.279

Significance level: *** significant at 1% level; ** significant at 5% level; * significant at 10% level.
Robust standard errors clustered at the court level.

Table A3: Estimation of the difference between cities not affected by the reform and cities that experienced a decrease of distance to their labor court (Difference-in-difference, regression adjusted, including non-treatable cities).

	outflows	inflows	unemployed	newEntr
$\phi_{D,2006}$	-0.482	1.494	-2.078**	-2.944
	(0.726)	(0.544)	(0.0250)	(0.660)
$\phi_{D,2008}$	-0.158	3.600	1.373	3.101
	(0.949)	(0.104)	(0.213)	(0.534)
$\phi_{D,2009}$	-0.910	3.942*	0.405	1.735
	(0.723)	(0.0763)	(0.806)	(0.697)
$\phi_{D,2010}$	2.116	3.932	-0.241	3.359
	(0.536)	(0.295)	(0.916)	(0.557)
$\phi_{D,2011}$	1.693	3.312	0.729	-7.386
	(0.717)	(0.266)	(0.815)	(0.118)
$\phi_{D,2012}$	0.470	3.952	-0.257	-0.378
	(0.891)	(0.159)	(0.932)	(0.921)
City FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Covariates	Yes	Yes	Yes	Yes
Observations	49,639	49,636	49,649	48,878
R-squared	0.252	0.145	0.347	0.278

Significance level: *** significant at 1% level; ** significant at 5% level; * significant at 10% level.
Robust standard errors clustered at the court level.

Table A4: Estimation of the difference between cities not affected by the reform and cities that experienced an increase of distance to their labor court (Difference-in-difference, regression adjusted, excluding non-treatable cities, **Multilevel mixed effects**).

	outflows	inflows	unemployed	newEntr
$\phi_{I,2006}$	-0.443	1.788	-0.218	-5.569**
	(0.979)	(1.385)	(0.884)	(2.260)
$\phi_{I,2008}$	-0.522	-0.950	-1.185	1.428
	(0.992)	(1.402)	(0.896)	(2.292)
$\phi_{I,2009}$	-1.065	-4.647***	-0.644	-2.079
	(0.965)	(1.364)	(0.870)	(2.220)
$\phi_{I,2010}$	-2.778***	-4.839***	-0.821	-2.695
	(0.964)	(1.364)	(0.870)	(2.219)
$\phi_{I,2011}$	-2.916***	-4.535***	0.552	-3.648
	(0.964)	(1.363)	(0.869)	(2.219)
$\phi_{I,2012}$	-4.338***	-5.749***	0.248	-4.793**
	(0.965)	(1.364)	(0.870)	(2.221)
City RE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Court × year RE	Yes	Yes	Yes	Yes
Covariates	Yes	Yes	Yes	Yes
Observations	49,640	49,640	49,636	48,768

Significance level: *** significant at 1% level; ** significant at 5% level; * significant at 10% level.

Table A5: Estimation of the difference between cities not affected by the reform and cities that experienced a decrease of distance to their labor court (Difference-in-difference, regression adjusted, excluding non-treatable cities, **Multilevel mixed effects**).

	outflows	inflows	unemployed	newEntr
$\phi_{D,2006}$	-0.375	2.255	-3.312	-1.943
	(2.347)	(3.297)	(2.251)	(6.220)
$\phi_{D,2008}$	-1.281	3.565	0.0155	4.167
	(2.377)	(3.341)	(2.277)	(6.287)
$\phi_{D,2009}$	-0.513	2.648	-0.665	2.159
	(2.272)	(3.190)	(2.180)	(6.033)
$\phi_{D,2010}$	1.473	5.370*	-1.650	4.158
	(2.270)	(3.188)	(2.179)	(6.030)
$\phi_{D,2011}$	0.249	2.045	-0.967	-7.222
	(2.271)	(3.189)	(2.180)	(6.033)
$\phi_{D,2012}$	-0.823	4.029	-1.853	0.562
	(2.271)	(3.189)	(2.180)	(6.033)
City RE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Court × year RE	Yes	Yes	Yes	Yes
Covariates	Yes	Yes	Yes	Yes
Observations	42,891	42,891	42,901	42,228

Significance level: *** significant at 1% level; ** significant at 5% level; * significant at 10% level.

Conditional Difference-in-Difference

Table A6: Diagnosis of matching process: Average standardized bias and number of biased variables at 5% (including non-treatable cities).

	Increased Distance		Decreased Distance	
Algorithm	Average Bias	# Biased Var.	Average Bias	# Biased Var.
Before Matching	4.64	2	7.27	2
EK	2.89	0	7.28	2
GK	3.81	2	7.28	2
N3	0.74	0	5.33	2
CBPS	0.05	0	0.03	0

Table A7: Estimation of the difference between cities not affected by the reform and cities that experienced an increase of distance to their labor court (Conditional difference-in-difference, regression adjusted, including non-treatable cities).

	outflows	inflows	unemployed	newEntr
$\phi_{I,2006}$	-0.0567	-0.218	-0.326	0.382
	(1.018)	(1.301)	(0.521)	(2.495)
$\phi_{I,2008}$	-0.131	-2.357**	-0.129	3.771
	(0.722)	(1.124)	(0.428)	(2.797)
$\phi_{I,2009}$	0.889	-4.932***	-0.0434	-2.812
	(1.465)	(1.632)	(1.071)	(2.530)
$\phi_{I,2010}$	-1.615	-5.333***	0.239	-2.493
	(1.381)	(2.016)	(1.431)	(2.302)
$\phi_{I,2011}$	-2.490*	-4.132*	1.833	-3.329
	(1.301)	(2.109)	(1.778)	(2.243)
$\phi_{I,2012}$	-3.571***	-5.389***	1.426	-5.031**
	(1.172)	(2.039)	(1.814)	(2.260)
City FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Covariates	Yes	Yes	Yes	Yes
Observations	55,395	55,394	55,398	55,397
R-squared	0.227	0.193	0.329	0.291

Significance level: *** significant at 1% level; ** significant at 5% level; * significant at 10% level.
Robust standard errors clustered at the court level.

Table A8: Estimation of the difference between cities not affected by the reform and cities that experienced a decrease of distance to their labor court (Conditional difference-in-difference, regression adjusted, including non-treatable cities).

	outflows	inflows	unemployed	newEntr
$\phi_{D,2006}$	0.608	-0.00404	-0.154	-3.451
	(1.294)	(2.318)	(0.890)	(6.915)
$\phi_{D,2008}$	-0.894	3.100	1.294	4.066
	(2.298)	(2.186)	(1.160)	(4.833)
$\phi_{D,2009}$	0.376	1.658	2.727**	0.444
	(2.300)	(2.411)	(1.224)	(5.610)
$\phi_{D,2010}$	3.346	2.186	2.121	3.357
	(3.122)	(3.278)	(1.947)	(6.283)
$\phi_{D,2011}$	2.888	1.622	3.070	-7.466
	(4.237)	(3.175)	(2.500)	(5.316)
$\phi_{D,2012}$	0.695	2.950	3.071	0.885
	(3.194)	(2.703)	(2.576)	(4.083)
City FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Covariates	Yes	Yes	Yes	Yes
Observations	48,869	48,868	48,872	48,871
R-squared	0.266	0.133	0.380	0.282

Significance level: *** significant at 1% level; ** significant at 5% level; * significant at 10% level.
Robust standard errors clustered at the court level.

Table A9: Estimation of the difference between cities not affected by the reform and cities that experienced an increase of distance to their labor court (Conditional difference-in-difference, regression adjusted, excluding non-treatable cities, including delay of cases at the labor court).

	outflows	inflows	unemployed	newEntr
$\phi_{I,2006}$	-0.158	-0.226	-0.311	0.501
	(1.002)	(1.312)	(0.520)	(2.508)
$\phi_{I,2008}$	.	.	.	.
$\phi_{I,2009}$	0.305	-5.340***	0.263	-2.362
	(1.501)	(1.654)	(1.169)	(2.503)
$\phi_{I,2010}$	-1.943	-6.033***	0.492	-2.043
	(1.365)	(2.030)	(1.534)	(2.386)
$\phi_{I,2011}$	-2.706**	-4.770**	2.057	-3.400
	(1.315)	(2.211)	(1.874)	(2.293)
$\phi_{I,2012}$	-3.672***	-5.895***	1.555	-4.783**
	(1.175)	(2.048)	(1.898)	(2.212)
Delay of cases	Yes	Yes	Yes	Yes
City FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Covariates	Yes	Yes	Yes	Yes
Observations	41,837	41,837	41,840	41,839
R-squared	0.230	0.215	0.332	0.264

Significance level: *** significant at 1% level; ** significant at 5% level; * significant at 10% level.
Robust standard errors clustered at the court level.

Table A10: Estimation of the difference between cities not affected by the reform and cities that experienced a decrease of distance to their labor court (Conditional difference-in-difference, regression adjusted, excluding non-treatable cities, including delay of cases at the labor court).

	outflows	inflows	unemployed	newEntr
$\phi_{D,2006}$	0.645	0.0157	-0.0756	-3.203
	(1.305)	(2.415)	(0.917)	(6.917)
$\phi_{D,2008}$	.	.	.	.
$\phi_{D,2009}$	-0.183	0.642	2.945**	0.0520
	(2.379)	(2.809)	(1.363)	(6.062)
$\phi_{D,2010}$	3.270	0.835	2.293	3.124
	(3.094)	(3.275)	(1.935)	(6.209)
$\phi_{D,2011}$	3.121	0.395	3.231	-7.810
	(4.242)	(3.429)	(2.582)	(4.856)
$\phi_{D,2012}$	1.058	1.608	3.087	0.632
	(3.076)	(3.044)	(2.595)	(3.976)
Delay of cases	Yes	Yes	Yes	Yes
City FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Covariates	Yes	Yes	Yes	Yes
Observations	36,197	36,197	36,200	36,199
R-squared	0.275	0.145	0.391	0.234

Significance level: *** significant at 1% level; ** significant at 5% level; * significant at 10% level.
Robust standard errors clustered at the court level.

Table A11: Estimation of the difference between cities not affected by the reform and cities that experienced an increase of distance to their labor court (Conditional difference-in-difference, regression adjusted, excluding non-treatable cities, 3-nearest neighbor matching algorithm).

	outflows	inflows	unemployed	newEntr
$\phi_{I,2006}$	-0.340	-0.665	-0.159	0.989
	(1.064)	(1.448)	(0.542)	(2.796)
$\phi_{I,2008}$	-0.864	-2.254*	0.0661	4.751
	(0.794)	(1.232)	(0.461)	(3.230)
$\phi_{I,2009}$	-0.246	-4.952***	0.279	-2.290
	(1.526)	(1.758)	(1.096)	(2.646)
$\phi_{I,2010}$	-2.518*	-5.966***	0.239	-2.053
	(1.437)	(2.085)	(1.460)	(2.380)
$\phi_{I,2011}$	-3.662***	-5.147**	1.677	-4.047*
	(1.383)	(2.218)	(1.829)	(2.432)
$\phi_{I,2012}$	-4.225***	-6.560***	1.025	-5.363**
	(1.256)	(2.174)	(1.901)	(2.504)
City FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Covariates	Yes	Yes	Yes	Yes
Observations	24,142	24,142	24,142	24,142
R-squared	0.237	0.192	0.340	0.294

Significance level: *** significant at 1% level; ** significant at 5% level; * significant at 10% level.
Robust standard errors clustered at the court level.

Table A12: Estimation of the difference between cities not affected by the reform and cities that experienced a decrease of distance to their labor court (Conditional difference-in-difference, regression adjusted, excluding non-treatable cities, 3-nearest neighbor matching algorithm).

	outflows	inflows	unemployed	newEntr
$\phi_{D,2006}$	0.318	-2.345	-0.472	-7.530
	(1.766)	(3.052)	(0.976)	(8.456)
$\phi_{D,2008}$	-1.216	3.284	1.206	-1.972
	(2.643)	(2.628)	(1.328)	(7.785)
$\phi_{D,2009}$	-1.871	1.649	2.883**	-6.002
	(2.860)	(2.954)	(1.409)	(7.263)
$\phi_{D,2010}$	1.208	1.834	1.768	-4.766
	(3.599)	(3.932)	(2.132)	(8.135)
$\phi_{D,2011}$	1.251	0.288	1.104	-13.84*
	(4.756)	(3.723)	(2.647)	(7.535)
$\phi_{D,2012}$	-0.639	1.072	0.840	-9.122
	(3.954)	(3.185)	(2.874)	(5.897)
City FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Covariates	Yes	Yes	Yes	Yes
Observations	2,927	2,927	2,927	2,927
R-squared	0.292	0.142	0.429	0.254

Significance level: *** significant at 1% level; ** significant at 5% level; * significant at 10% level.

Robust standard errors clustered at the court level.

Table A13: Estimation of the difference between cities not affected by the reform and cities that experienced an increase of distance to their labor court (Conditional difference-in-difference, regression adjusted, excluding non-treatable cities, **Multilevel mixed effects**).

	outflows	inflows	unemployed	newEntr
$\phi_{I,2006}$	0.482	0.460	-0.448	0.479
	(0.888)	(1.393)	(0.602)	(2.684)
$\phi_{I,2008}$	-0.0243	-2.233**	-1.125**	4.586
	(0.799)	(1.114)	(0.457)	(2.877)
$\phi_{I,2009}$	-0.0778	-5.086***	-0.684	-0.637
	(1.269)	(1.310)	(0.647)	(2.049)
$\phi_{I,2010}$	-2.120*	-5.739***	-0.747	-0.767
	(1.278)	(1.725)	(1.098)	(1.905)
$\phi_{I,2011}$	-2.600**	-5.198***	0.624	-1.975
	(1.182)	(1.778)	(1.452)	(1.787)
$\phi_{I,2012}$	-3.927***	-6.528***	-0.0754	-3.358**
	(1.148)	(1.756)	(1.537)	(1.623)
City RE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Court × year RE	Yes	Yes	Yes	Yes
Covariates	Yes	Yes	Yes	Yes
Observations	48,752	48,752	48,755	48,754

Significance level: *** significant at 1% level; ** significant at 5% level; * significant at 10% level.

Table A14: Estimation of the difference between cities not affected by the reform and cities that experienced a decrease of distance to their labor court (Conditional difference-in-difference, regression adjusted, excluding non-treatable cities, **Multilevel mixed effects**).

	outflows	inflows	unemployed	newEntr
$\phi_{D,2006}$	0.400	0.332	-1.540	-2.149
	(1.407)	(2.670)	(1.677)	(6.788)
$\phi_{D,2008}$	-0.695	3.195	-0.370	6.541
	(2.217)	(2.453)	(1.083)	(4.771)
$\phi_{D,2009}$	-1.422	0.717	-0.583	2.556
	(2.405)	(2.228)	(1.229)	(3.641)
$\phi_{D,2010}$	2.087	1.099	-1.639	5.029
	(2.560)	(3.843)	(1.563)	(5.236)
$\phi_{D,2011}$	1.635	0.749	-0.834	-6.513
	(3.289)	(2.857)	(3.062)	(4.504)
$\phi_{D,2012}$	0.111	2.078	-1.116	1.995
	(2.431)	(2.711)	(3.026)	(3.394)
City RE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Court × year RE	Yes	Yes	Yes	Yes
Covariates	Yes	Yes	Yes	Yes
Observations	42,226	42,226	42,229	42,228

Significance level: *** significant at 1% level; ** significant at 5% level; * significant at 10% level.
Robust standard errors clustered at the court level.