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Access to Labor Courts & Unemployment: Evidence from French Labor Courts

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PRELIMINARY WORK

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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 some workers that have now to go further to challenge their employers' decisions. We use this reform to identify how the distance to labor courts impacts job entries and exits on the labor market. Our empirical approach relies on regression adjusted difference-in-difference matching estimations. We use several matching algorithms (Nearest-neighbor, Kernel, CBPS). Our results show that the removal of labor courts increased job outflows. The overall effect on unemployment is however not clear since the reform also increased the number of new enterprises. We also investigate the conditional impact of the reform according to the increased distance to labor courts.

JEL codes: K31, K41
Keywords: Labor courts, employment, matching.

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 the institutions aiming at enforcing employment contracts, i.e. labor courts, and on their organization. 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 other one -and only one- court. The law 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 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 reform reducing the number of labor courts impacts employment. 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.

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: 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. 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.
As a consequence, our paper is 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 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.[1] 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.

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

(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. 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.
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.[2]
Let us precise that 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.[3] 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.[4]
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

[2]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.

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

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

be large over the territory: some cities have unemployment rates higher than 33% while others are below 8%. This creates useful sources of variations.
Last, our focus is on labor courts. By enforcing labor contracts, these 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.[5] 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[6], 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, ...).[7] 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.[8]
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 (respectively 5.4 and 5.8 months on average).[9]
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.[10] From a law passed on July

[5] 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.

[6] 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"*).

[7] 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.

[8] 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, 18th 2014.

[9] 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)).

[10] 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

13th, 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.[11]

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.[12] 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)).[13] 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*[14] had 14 labor courts, while some others had only one (Sénat (2012)). The reform was enacted by decree n^0 2008-514 of May 29th, 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 3rd, 2008.[15] 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*"[16], 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.
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

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).

[11] 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).

[12] 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.

[13] 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.

[14] *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 29th, 2008 and decree n^0 2014-899 of August 18th, 2014.)

[15] 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)).

[16] 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.

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 (and 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. Litigation costs are part of firing costs, so that the employers' decisions to hire and fire workers are indirectly impacted by the reform. However, the reform could impact employment through different channels. The final impact is then difficult to determine. We briefly discuss here some of the potential effects of the reform on employment.
To begin with, the reform has changed the distance to go to court and potentially the delays to be heard. This impacts firing costs through the following ways:

- 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 antic-

ipated (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.

- 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.

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.[17]
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/removable/receiving and unaffected 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 (*removal-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 (*untreated cities*).
- Cities that were precluded from treatment, because there was only one labor court prior to the reform in the *département* (*non-treatable cities*);

Table 6 displays the summary statistics for data in 2006 of our set of variables for the four categories of cities. As one can see, groups are relatively heterogeneous. The stars indicate that the treated groups' sample means are statistically different from the untreated group's. Removed-treated cities were the least populated cities, their working age groups were among the smallest, they had the highest unemployment rate, and they had the shortest distance to their labor court. Their associated labor courts were extremely different from the other courts: they dealt with much fewer cases, they had low départage rates, and shorter delays. These findings are consistent with the previous results of Espinosa et al. (2015): they show that the government targeted low-activity courts in high unemployment areas when deciding to remove courts.
On the contrary, the receiving-treated cities were very similar to the untreated cities: they have comparable courts in terms of duration, claims, and départage. Regarding the cities themselves, they are comparable in terms of population, unemployment, creation of new firms, and distance to labor courts.
On a different perspective, figure 1 depicts the evolution of the unemployment rate in the four groups of cities we have distinguished. The X-axis is the number of terms between 2006 and 2011. The y-axis is the average unemployment rate in the cities belonging to each group.
Last, figure 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 "removal-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.

[17] 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: Evolution of the unemployment rate per group of cities

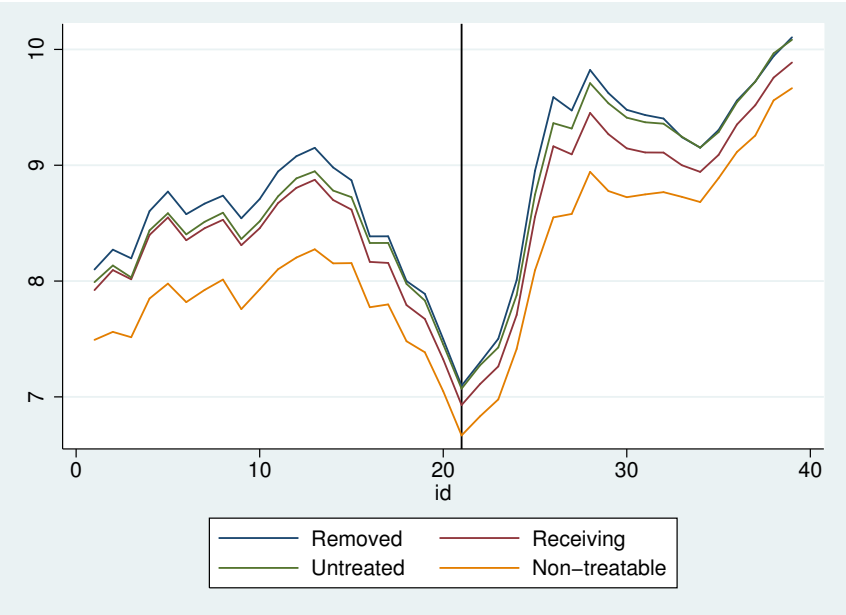
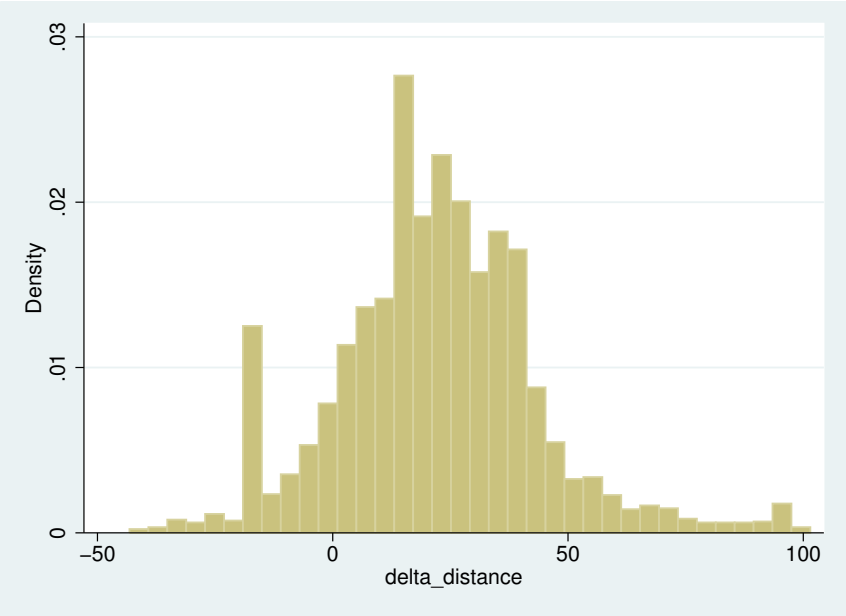


Figure 2: Distribution of changes in distance after the reform for removed cities



5 Empirical Strategy

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 aim of evaluation of public policies is to estimate the average reaction of treated units to a treatment. The two techniques differ however on the assumptions they make about reaction functions, and treatment assignment.

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, conditional 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 unconditional on the observables.

Second, the PSM estimations also assume that the treatment does not create any general equilibrium effect (Stable Unit Treatment Value Assumption, SUTVA). The DiD estimations share this same assumption.

As far as the DiD is concerned, the estimations assume that treated and non-treated units would have had similar trends if treated units would not have been treated (Common Trend Assumption, CTA). In other words, the CTA states that the difference between the two groups would have been stable over time if treatment did not occur. Since PSM does not use variations over time, the assumption is not relevant.

As far as the 2008 reform is concerned, both the CIA and the CTA are unlikely to hold. The descriptive statistics displayed in section **??** showed that the *receiving* courts were associated with higher unemployment rates than the *unaffected treatable* courts. In other words, the selection has been done on the outcome prior to the reform, which is a violation of the CIA. Moreover, Espinosa et al. 2015 have shown that removal decisions were done based on observables: *removed* courts were dealing with fewer cases and were closer to other labor courts.

Considering these issues, we propose to use a combination of the propensity score matching and the diff-in-diff methods, namely *conditional diff-in-diff* estimations. This estimation method has been proposed by Heckman et al. (1998). The process is the following. First, we estimate the probability of treated and control units to be treated. Second, we use a matching algorithm to define weights for control units. Third, we estimate the following equation using a weighted OLS estimation:

$$y_{it} = \alpha_0 + \alpha_1 D_{it} + \alpha_2 T_{it} + \alpha_3 (D_{it} \times T_{it}) + u_{it} \tag{1}$$

where D_{it} is equal to one for units of the treated group and 0 for the units of the control group, T_{it} is equal to one for the post-treatment period and to 0 for the pre-treatment period. The coefficient α_1 captures the pre-treatment heterogeneity between the treated and non-treated units, α_2 measures the common trend, and α_3 assesses the treatment's effect on the treated.

Treatment and control units As far as the 2008 reform is concerned, we want to estimate the impact of two treatments: the impact of court removal (*removal effect*) and the impact of court expansion (*enlarging effect*) on the economic activity at the city level. Because we are facing two treatments, we have two groups of treated units (*removal-treated cities* and *receiving treated cities*), one group of control (*untreated cities*). Note that the PSM estimation also require that all units of the control group have a positive probability of being treated (*overlapping assumption*). We therefore exclude from our analysis the *non-treatable* cities.

Removal treatment As far as the *removal effect* is concerned, all cities belonging to the same labor courts were not treated in the same way. Indeed, litigants of the former courts have been forced to bring their ongoing and future claims to the new court. The distance between the cities

and their associated labor court might have increased dramatically for some cities, while it may have increased at the margin and may even have decreased for some others. The above descriptive statistics showed that the average increase in distance is x and its standard deviation is x.

In this respect, the estimation of equation 1 only yields an average effect: it captures the average increase in unemployment that have incur cities whose labor court has been removed. One can however believe that the effect is a function of the increase in the distance to the labor court: the further the new court compared to the previous court, the stronger the effect on unemployment. This framework is similar to the case of continuous treatment, where the intensity of treatment may vary from one treated observation to another. The main difference is that the intensity of treatment in our case is exogenous. The decision to remove courts was made on observables at the court's level.

$$y_{it} = \alpha_0 + \alpha_1 D_{it} + \alpha_2 T_{it} + \alpha_3 (D_{it} \times T_{it}) + \alpha_4 (D_{it} \times T_{it} \times \Delta dist_{it}) + u_{it} \tag{2}$$

where $\Delta dist_{it}$ is the increase in distance for *removal-treated* cities.

Estimation of treatment propensity The estimation of the probability of treatment is necessary to construct counterfactuals to treated units. The rationale of PSM techniques is to sort both treated and control units on a single dimension called *balancing score*[18], and to compare units with similar scores. The motivation is to say that treatment is independent from the observables for units with similar propensity scores, and that the difference of outcomes for units with similar propensity scores is an unbiased estimate of the treatment effect.[19] The major advantage of this procedure is that units with similar balancing scores are similar across observables. The literature has extensively used the probability of treatment as a balancing score.

It follows from this discussion that the propensity score does not seek to perfectly estimate the probability of treatment but to sort units to have comparable units when balancing scores are equal. A large part of the literature has showed that a too precise estimation of the probability of treatment might be damageable.[20]

In order to compare units with similar observables, we therefore estimate the probability of treatment including variables at two levels: (i) variables at the level of the labor court, that determined and (ii) variables at the city level. Both kinds of variables might have affected courts' removal, since Espinosa et al. (2015) showed that labor court in high unemployment areas were more likely to be removed. Note that because some variables are used at the labor court level, we need to cluster standard errors. Since no method exists for such correction, we will rely on cluster bootstrap estimations.

6 Estimations

6.1 Matching

The first step of our estimation consists in using matching algorithm to create a group of counterfactuals for the treated observations. First, we estimate the propensity score by estimating the

[18] Rosenbaum and Rubin (1983): "A balancing score, $b(x)$ is a function of the observed covariates x such that the conditional distribution of x given b(x) is the same for treated and control units."

[19] Rosenbaum and Rubin (1983): "At any value of a balancing score, the difference between the treatment and control means is an unbiased estimate of the average treatment effect at that value of the balancing score if treatment assignment is strongly ignorable."

[20] It decreases the common support, and decreases the quality of matching. Caliendo et al. (2008)

following equation (logit):

$$\begin{aligned} removal_i^* = \beta_0 + \beta_1 lnpop_i + \beta_2 lnpopAge_i + \beta_3 propCS_i^* + \beta_4 unemployment_{k(i)} \\ + \beta_5 durAff_{j(i)} + \beta_6 newAff_{j(i)} + \beta_7 depRate_{j(i)} + \beta_8 succRate_{j(i)} + \beta_9 settleRate_{j(i)} \\ + \beta_{10} propSal_i + \beta_{11} creaEntr_i + \beta_{12} unemployed_i + \beta_{13} activRate_i + \beta_{14} distance_i \end{aligned} \quad (3)$$

where $removal^*$ is the latent variable associated with $removal$ (equal to 1 if a CPH associated to city i was removed, to 0 if not removed, and missing if receiving). Observations i are at the city level, variables are at the associated court level ($j(i)$) and one variable is at *zone d'emploi* level.

The estimation of equation 3 corresponds to *Model 1*. We also estimate two alternative models. In *Model 2*, we generate interaction variables (including squared variables) and include in the propensity score estimation those that are statistically different across groups of treated and matched observations following the matching scores obtained using Model 1. In *Model 3*, we proceed similarly but we consider only squared variables.

Second, we consider several algorithms to compute weights using the propensity scores obtained above. First, we use a Epanechnikov Kernel. Using the three models, we obtain three specifications : *M1-EK*, *M2-EK* and *M3-EK*. Second, we consider a Gausian Kernel : *M1-GK*, *M2-GK* and *M3-GK*. Third, we compute the weights using a nearest neighbor algorithm with 3 neighbors (*M1-N3*, *M2-N3* and *M3-N3*).

Third, we also consider another matching method, namely the *Covariate Balancing Propensity Score* method. This technique relies on GMM estimations and estimates the propensity scores and the weights jointly in order to maximize the decrease in bias. We label this method *CBPS*.

Comparing Matching Techniques Table **??** 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%.[21] We present two sets of results: on the left-hand side, we present the estimations associated with the above models *excluding* non-treatable observations from the original control group. On the right-hand side, we display the results including the non-treatable observations.

First of all, the data show a very high level of heterogeneity: the group of treated units is statistically very different from the those of the control group. In the literature, a variable is accepted as *balanced* if its standardized bias is below 5%.

Second, regarding the inclusion or the exclusion of the non-treatable variables, one can see that the ASB is originally lower in the inclusion sample (*Before Matching*: 20.89 vs. 21.78). The ASB is on overall lower with the inclusion of the non-treatable units (except for *M1-EK* and *M3-N3*).

Third, we observe that *Model 2* performs relatively bad compared to *Model 1*. Except for one specification (*N3, including*), the ASB is lower in specifications *M1* than *M2*. On the contrary, specifications of *Model 3* outperform specifications of model *Model 1*.

Fourth, it appears that the CBPS specifications are the most efficient techniques to reduce the ASB. The two techniques clearly outperforms the other methods. They decrease the number of biased variables from 14 (before matching) to 4. The CBPS specification with non-treatable units yields the lowest ASB.

[21] The 5% threshold has been usually used in the literature since the original paper of Heckman.

Biased Variables with CBPS Table 16 shows the results of the *CBPS* matching method for both excluding and including samples. It displays two statistics for each variable : the standardized bias and the t-values associated with an OLS regression.[22] First of all, the table allows to identify which variables are above the commonly accepted 5% threshold for both samples. In the *excluding* sample the employment level, the 6th socio-economic category, the average duration of cases and the success rates at the CPH level are above the 5% level. Regarding the *including* sample, the total population, the unemployment level, the 6th socio-economic category and the average duration of cases are above the threshold. One can note that the two groups of treated and untreated units are fundamentally different regarding the unemployment level. Switching to the t-statistics, we observe however a complete different picture : none of the covariates is statistically different between the treated and the counterfactual groups. This suggests that the *CBPS* algorithms perform very well in constructing a counterfactual group.

6.2 Specifications

We now seek to estimate the reform's impact using *Difference-in-difference matching* estimators. We propose to use the two series of weights, obtained by the $CPBS$ algorithm, by excluding or including the non-removable courts. We present two series of results. First, we show the ATT estimates, that we compute with the following formula:

$$\widehat{ATT} = \frac{1}{n_1}[\sum_{i \in I_1} \Delta y_i - \sum_{j \in I_0} w_j \Delta y_j] \tag{4}$$

where n_1 is the number of treated (matched) units, Δy is the change in the dependent variable, I_1 is the set of treated (matched) units, I_0 is the set of control (matched) units, and w is the weight derived from the matching algorithm.

Second, we also display results using regression adjustment. More particularly, we seek to estimate the following model:

$$\Delta y = \alpha_0 + \alpha_1 removal + \alpha_2 \Delta X + u \tag{5}$$

Our set of controls ΔX includes the growth rates in the population, the population in working age, the categories of social category, and the associated labor court's variables.

Finally, we estimate a treatment effect conditional on the distance. The associated equation is:

$$\Delta y = \alpha_0 + \alpha_1 removal + \alpha_2 \Delta X + \alpha_3 \Delta distance + u \tag{6}$$

Note that $\Delta distance = \Delta distance \times removal$, because $\Delta distance \neq 0$ iff $removal = 1$. The treatment's effect is therefore equal to $\alpha_1 + \alpha_3 \Delta distance$.

6.3 Results

We display the results of the three estimations for the two dependent variables presented above: unemployment rates and new unemployed. Results are displayed in table 1/

[22] For each variable Y, we compute the t-statistics associated with the OLS regression of Y on a dummy variable that accounts for the treatment status. Regressions are weighted by their matching weights.

Table 1: Estimations of the Average Treatment Effect using CSBP techniques. REMOVED **With Duration**

		Excluding			Including		
Variable		ATT	Regression		ATT	Regression	
Unemployed Workers	Removal	-.04***	-.039**	-.044**	-.034**	-.033*	-.038**
		(-2.931)	(-2.139)	(-2.236)	(-2.46)	(-1.869)	(-1.993)
	ΔDistance	.	.	0	.	.	0
		.	.	(.619)	.	.	(.691)
Inscriptions Pole Emploi	Removal	.005	.023	.037*	.002	.018	.032
		(.214)	(1.203)	(1.753)	(.073)	(1.026)	(1.585)
	ΔDistance	.	.	-.001*	.	.	-.001*
		.	.	(-1.814)	.	.	(-1.77)
New Firms	Removal	.131**	.151**	.181**	.143**	.164**	.194**
		(2.121)	(1.993)	(2.152)	(2.378)	(2.221)	(2.355)
	ΔDistance	.	.	-.002	.	.	-.002
		.	.	(-.906)	.	.	(-.915)
New Firms (Net)	Removal	.017	.036	.063	.047	.072	.099*
		(.368)	(.643)	(1.02)	(1.231)	(1.568)	(1.863)
	ΔDistance	.	.	-.001	.	.	-.001
		.	.	(-1.19)	.	.	(-1.165)
Activity Rate	Removal	.004***	.004***	.005***	.005***	.004***	.006***
		(2.808)	(2.962)	(3.741)	(3.158)	(3.323)	(4.074)
	ΔDistance	.	.	0***	.	.	0***
		.	.	(-2.625)	.	.	(-2.677)
propSal	Removal	0	0	.001	.001	.001	.001
		(.005)	(-.044)	(.16)	(.368)	(.276)	(.425)
	ΔDistance	.	.	0	.	.	0
		.	.	(-.491)	.	.	(-.434)

Table 2: Estimations of the Average Treatment Effect using CSBP techniques. REMOVED **Robustness 1: Without Duration**

		Excluding			Including		
Variable		ATT	Regression		ATT	Regression	
Unemployed Workers	Removal	-.04***	-.031**	-.038**	-.034**	-.027*	-.034*
		(2.931)	(-1.972)	(-2.115)	(-2.46)	(-1.726)	(-1.9)
	ΔDistance	.	.	0*	.	.	0*
		.	.	(-1.66)	.	.	(-1.685)
Inscriptions Pole Emploi	Removal	.005	.029	.042**	.002	.022	.035*
		(.214)	(1.614)	(2.084)	(.073)	(1.241)	(1.779)
	ΔDistance	.	.	-.001	.	.	-.001
		.	.	(-1.814)	.	.	(-1.77)
New Firms	Removal	.131**	.134*	.168**	.143**	.147**	.181**
		(2.121)	(1.902)	(2.088)	(2.378)	(2.117)	(2.28)
	ΔDistance	.	.	-.002	.	.	-.002
		.	.	(-.981)	.	.	(-.986)
New Firms (Net)	Removal	.017	.025	.054	.047	.063	.091*
		(.368)	(.464)	(.891)	(1.231)	(1.42)	(1.756)
	ΔDistance	.	.	-.001	.	.	-.001
		.	.	(-1.232)	.	.	(-1.202)
Activity Rate	Removal	.004***	.004***	.006***	.005***	.004***	.006***
		(2.808)	(3.4)	(4.123)	(3.158)	(3.828)	(4.51)
	ΔDistance	.	.	0***	.	.	0***
		.	.	(-2.53)	.	.	(-2.548)
propSal	Removal	0	-.001	0	.001	0	.001
		(.005)	(-.214)	(.068)	(.368)	(.115)	(.331)
	ΔDistance	.	.	0	.	.	0
		.	.	(-.551)	.	.	(-.495)

Table 3: Estimations of the Average Treatment Effect using CSBP techniques.REMOVED **Robustness 2: 3GK matching algorithm**

		Excluding			Including		
Variable		ATT	Regression		ATT	Regression	
Unemployment	Removal	-.063***	-.051**	-.052**	-.049***	-.036*	-.037
		(-3.845)	(-2.308)	(-2.188)	(-3.009)	(-1.7)	(-1.606)
	ΔDistance	.	.	0	.	.	0
		.	.	(.105)	.	.	(.067)
Pole Emploi	Removal	.038	.006	.031	.014	-.004	.021
		(1.555)	(.115)	(.635)	(.56)	(-.073)	(.449)
	ΔDistance	.	.	-.013*	.	.	-.013*
		.	.	(-1.823)	.	.	(-1.799)
New enterprises	Removal	.102	.118	.121	.142**	.161**	.162**
		(1.528)	(1.516)	(1.433)	(2.223)	(2.164)	(2.012)
	ΔDistance	.	.	-.001	.	.	-.001
		.	.	(-.076)	.	.	(-.051)

Table 4: Estimations of the Average Treatment Effect using CSBP techniques. **RECEIVING With Duration**

		Excluding		Including	
Variable		ATT	Regression	ATT	Regression
Unemployment	Receiving	-.018**	.004	.001	.007
		(-2.482)	(.439)	(.086)	(.823)
Pole Emploi	Receiving	.005	.014	.01	.019
		(.41)	(1.168)	(.897)	(1.603)
New enterprises	Receiving	-.078***	-.067**	-.066**	-.06**
		(-2.764)	(-2.213)	(-2.435)	(-2.023)

Table 5: Estimations of the Average Treatment Effect using CSBP techniques. **RECEIVING Without Duration**

		Excluding		Including	
Variable		ATT	Regression	ATT	Regression
Unemployment	Receiving	-.018**	.005	.001	.007
		(-2.482)	(.547)	(.086)	(.912)
Pole Emploi	Receiving	.005	.014	.01	.018
		(.41)	(1.172)	(.897)	(1.537)
New enterprises	Receiving	-.078***	-.063**	-.066***	-.056*
		(-2.764)	(-2.105)	(-2.435)	(-1.902)

7 Conclusion

We provide here the first analysis -to our knowledge- that investigates how changes in distance to labor courts impact employment. Using the french reform in 2008 that suppressed 20% of the number of labor courts, we run regression adjusted difference-in-difference matching estimations. Our first results suggest that the distance to labor courts has no large impact on employment. Cities depending on a labor court that has been removed do not suffer from an unemployment rate significantly different from what they could have without the reform. Yet, the number of people registered on unemployment agencies has slightly increased, as well as the number of new firms.

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A Maps of Judicial System

Figure 3: French courts before the reform and removals during the reform

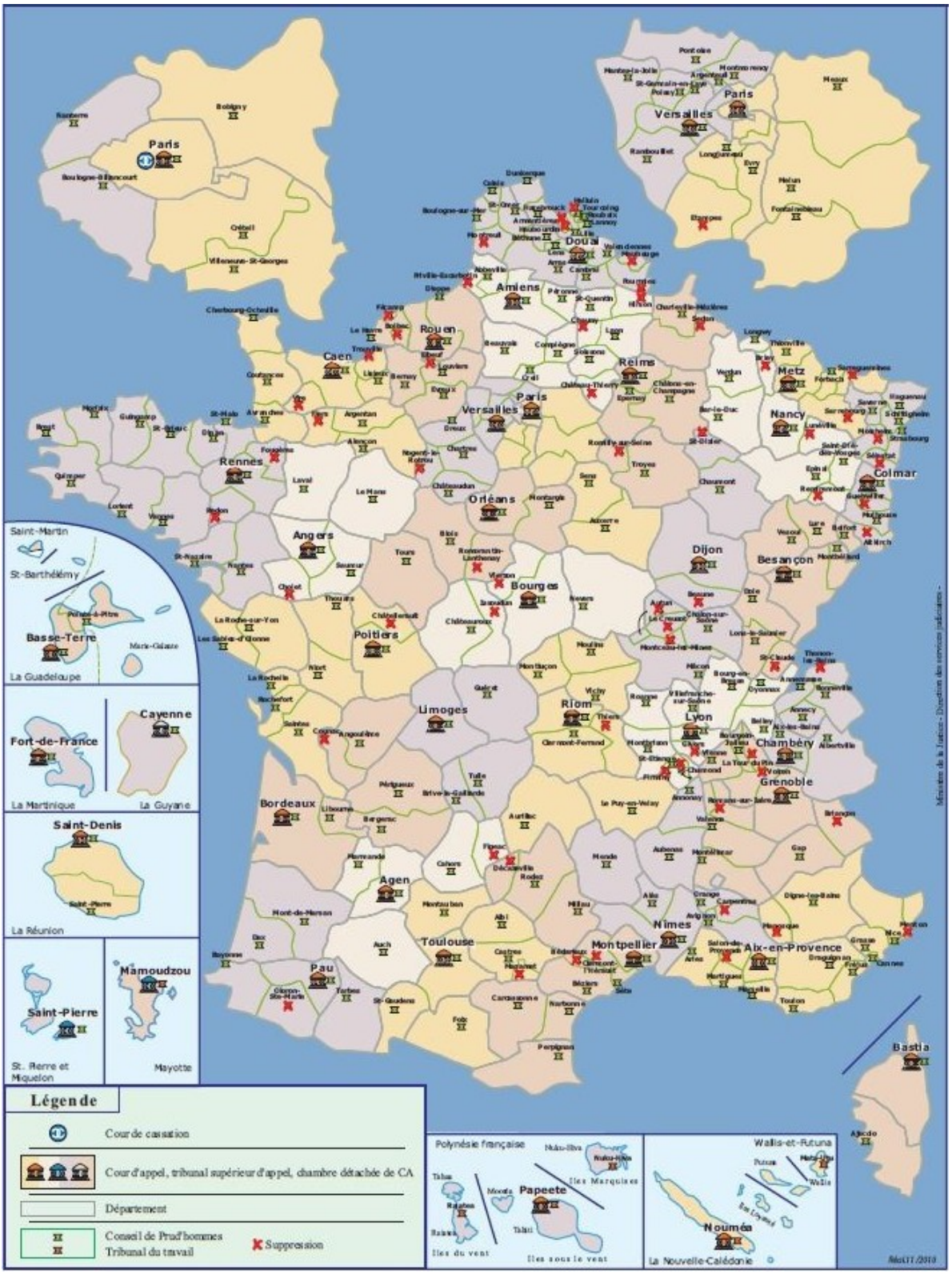
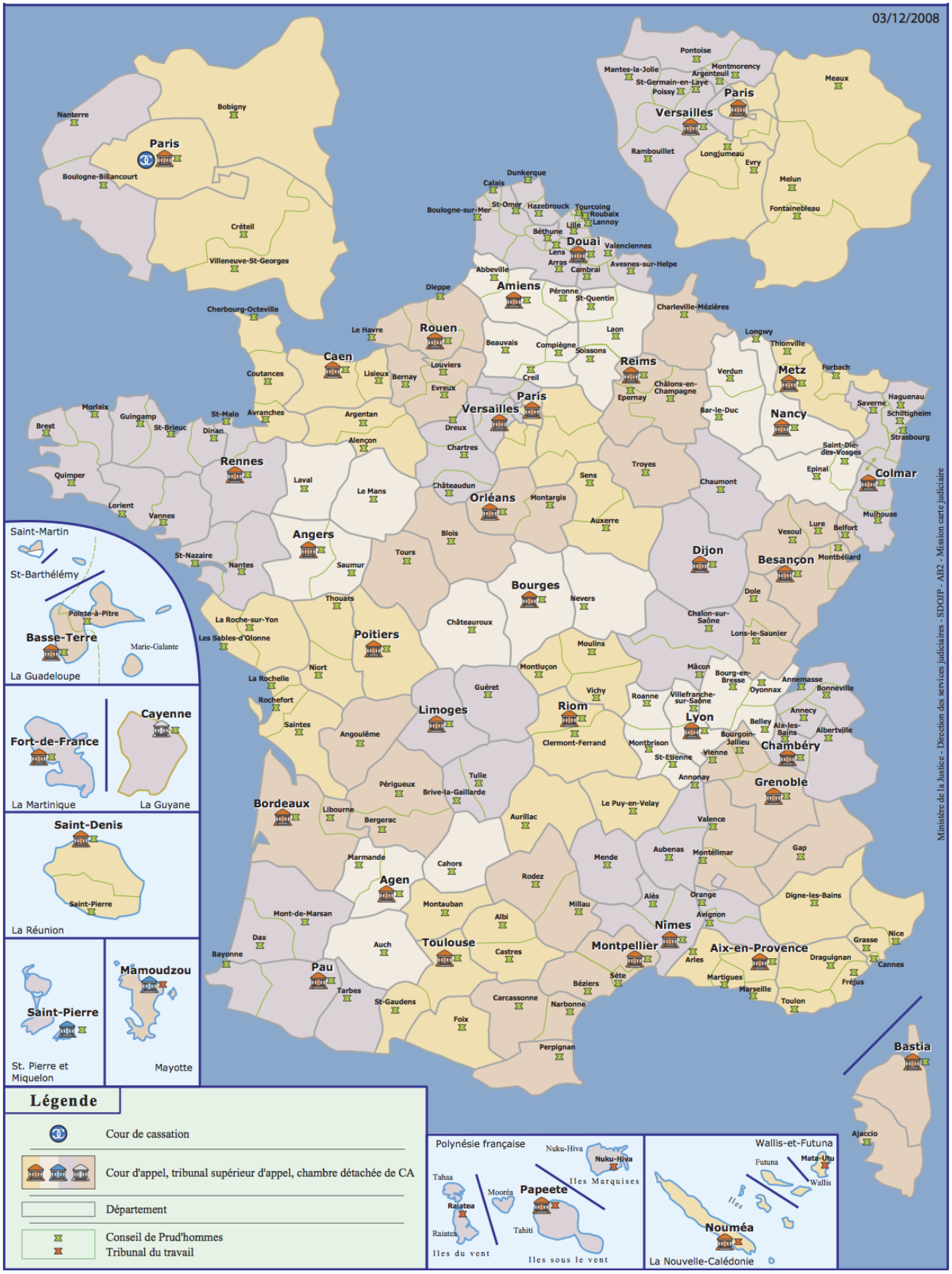


Figure 4: French courts after the reform



B Tables

Table 6: Summary Statistics per city in 2006 (prior to the reform). Means and standard deviations (in parentheses). Variables with stars are reported at the labor court's level. Stars indicate that the sample mean is statistically different from the untreated cities' sample mean at 5%.

Variable	Label	Non-treatable	Removal-treated	Receiving-treated	Untreated
pop	Population (log)	5.797	6.08*	6.045*	6.294
		(1.332)	(1.209)	(1.322)	(1.347)
popAge	Working age population (log)	5.298	5.615*	5.582*	5.833
		(1.352)	(1.221)	(1.338)	(1.361)
unempl	Unemployment	8.274	9.15	8.874*	8.948*
		(1.539)	(2.301)	(1.797)	(1.9)
propCS1	Proportion of individuals in the 1st social category	.066	.043*	.044*	.041
		(.073)	(.055)	(.06)	(.056)
propCS2	Proportion of individuals in the 2nd social category	.04	.036*	.038*	.039
		(.037)	(.032)	(.034)	(.031)
propCS3	Proportion of individuals in the 3rd social category	.037	.045*	.053	.053
		(.036)	(.04)	(.047)	(.048)
propCS4	Proportion of individuals in the 4th social category	.102	.118*	.124	.124
		(.064)	(.06)	(.064)	(.061)
propCS5	Proportion of individuals in the 5th social category	.141	.15*	.152	.152
		(.065)	(.057)	(.06)	(.056)
propCS6	Proportion of individuals in the 6th social category	.139	.178*	.162*	.166
		(.077)	(.073)	(.075)	(.072)
propCS7	Proportion of individuals in the 7th social category	.338	.282	.285*	.28
		(.121)	(.095)	(.104)	(.101)
ratioSal	Proportion of salaried jobs among the entire set of jobs.	.55	.643*	.639*	.656
		(.237)	(.212)	(.215)	(.205)
creaEntr	Number of firms created per year	5.344	3.677*	6.123*	7.644
		(32.693)	(11.45)	(49.573)	(42.494)
distance	Distance between the city and its labor court (km)	40.932	24.797*	32.417	32.081
		(22.77)	(17.513)	(21.153)	(38.749)
durAff*	Average duration of terminated cases in month (log)	10.388	8.543*	10.821*	10.151
		(2.403)	(2.654)	(2.478)	(2.728)
newAff*	Number of new claims per year	732.735	182.859*	872.656*	738.669
		(1392.107)	(84.129)	(833.81)	(799.378)
depRate*	Rate of *départage*	16.613	8.884*	16.797*	14.005
		(11.529)	(8.874)	(14.663)	(9.741)
succRate*	Success rate for plaintiffs	.714	.717*	.724*	.709
		(.068)	(.108)	(.086)	(.084)

Table 7: Results of Matching with Unemployment data: Average Standardized Bias (percentages) and number of biased variables. (For unemployment) **REMOVED**

	Excluding Non-treatable		Including Non-treatable	
Algorithm	Average Bias	# Biased Variables	Average Bias	# Biased Variables
Before Matching	21.816	15	22.214	17
M1-EK	8.315	13	7.076	11
M1-GK	6.999	15	7.266	13
M1-N3	10.863	14	10.719	10
M2-EK	13.651	17	12.17	18
M2-GK	9.592	17	7.146	14
M2-N3	8.361	10	9.795	11
M3-EK	8.173	8	8.948	14
M3-GK	7.214	10	7.091	9
M3-N3	8.684	8	7.496	12
CBPS	3.154	2	3.582	6

Table 8: Results of Matching with Unemployment data: Average Standardized Bias (percentages) and number of biased variables. (For Pôle Emploi) **REMOVED**

	Excluding Non-treatable		Including Non-treatable	
Algorithm	Average Bias	# Biased Variables	Average Bias	# Biased Variables
Before Matching	20.714	17	20.19	17
M1-EK	8.449	12	6.393	9
M1-GK	6.61	13	6.599	11
M1-N3	11.172	11	10.229	11
M2-EK	10.407	17	12.984	16
M2-GK	9.438	16	9.975	16
M2-N3	10.009	11	9.415	13
M3-EK	7.561	7	7.357	5
M3-GK	6.288	7	6.971	8
M3-N3	7.73	8	7.565	5
CBPS	3.507	2	3.75	7

Table 9: Results of Matching with Unemployment data: Average Standardized Bias (percentages) and number of biased variables. (For New Firms) **REMOVED**

	Excluding Non-treatable		Including Non-treatable	
Algorithm	Average Bias	# Biased Variables	Average Bias	# Biased Variables
Before Matching	21.214	15	21.67	17
M1-EK	7.709	12	5.093	6
M1-GK	5.891	11	5.104	9
M1-N3	7.604	12	7.945	10
M2-EK	10.287	14	10.979	14
M2-GK	8.97	17	8.862	17
M2-N3	9.571	16	7.762	13
M3-EK	7.486	7	7.416	8
M3-GK	6.513	9	5.837	6
M3-N3	7.462	9	8.502	10
CBPS	4.057	4	3.777	7

Table 10: Results of Matching with Unemployment data: Average Standardized Bias (percentages) and number of biased variables. (For New Firms (Net)) **REMOVED**

	Excluding Non-treatable		Including Non-treatable	
Algorithm	Average Bias	# Biased Variables	Average Bias	# Biased Variables
Before Matching	24.15	17	23.566	15
M1-EK	7.467	11	4.997	8
M1-GK	5.573	10	5.235	10
M1-N3	8.737	11	9.08	13
M2-EK	7.498	9	7.047	10
M2-GK	7.322	13	5.373	8
M2-N3	7.279	10	10.677	14
M3-EK	7.993	9	7.253	7
M3-GK	6.811	8	5.858	8
M3-N3	8.706	9	9.354	11
CBPS	1.493	0	3.896	7

Table 11: Results of Matching with Unemployment data: Average Standardized Bias (percentages) and number of biased variables. (For Activity Rate) **REMOVED**

	Excluding Non-treatable		Including Non-treatable	
Algorithm	Average Bias	# Biased Variables	Average Bias	# Biased Variables
Before Matching	20.566	17	20.791	17
M1-EK	8.613	12	6.492	11
M1-GK	6.929	13	6.822	12
M1-N3	11.763	14	10.346	11
M2-EK	9.656	15	9.409	12
M2-GK	10.09	17	9.691	17
M2-N3	10.044	11	8.823	13
M3-EK	7.424	7	7.427	6
M3-GK	6.191	6	7.065	8
M3-N3	7.348	6	7.147	9
CBPS	3.284	3	3.765	7

Table 12: Results of Matching with Unemployment data: Average Standardized Bias (percentages) and number of biased variables. (For Proportion of Salaried Jobs) **REMOVED**

	Excluding Non-treatable		Including Non-treatable	
Algorithm	Average Bias	# Biased Variables	Average Bias	# Biased Variables
Before Matching	20.626	17	20.815	17
M1-EK	8.64	12	6.507	11
M1-GK	6.946	13	6.834	12
M1-N3	10.97	13	10.527	11
M2-EK	9.663	15	9.875	13
M2-GK	10.058	17	9.532	17
M2-N3	7.413	11	8.763	13
M3-EK	7.427	7	7.432	6
M3-GK	6.195	6	7.071	8
M3-N3	7.489	8	7.989	8
CBPS	3.283	2	3.762	7

Table 13: Results of Matching with Unemployment data: Average Standardized Bias (percentages) and number of biased variables. (For unemployment) **RECEIVING**

	Excluding Non-treatable		Including Non-treatable	
Algorithm	Average Bias	# Biased Variables	Average Bias	# Biased Variables
Before Matching	6.494	8	7.596	12
M1-EK	1.885	2	1.327	2
M1-GK	3.826	3	4.273	3
M1-N3	1.12	0	.975	0
M2-EK	3.182	3	2.761	2
M2-GK	4.8	9	4.09	6
M2-N3	4.998	7	5.573	3
M3-EK	9.317	4	2.45	3
M3-GK	7.148	6	9.155	6
M3-N3	5.803	4	6.295	3
CBPS				

Table 14: Results of Matching with Unemployment data: Average Standardized Bias (percentages) and number of biased variables. (For Pôle Emploi) **RECEIVING**

	Excluding Non-treatable		Including Non-treatable	
Algorithm	Average Bias	# Biased Variables	Average Bias	# Biased Variables
Before Matching	8.255	8	7.625	10
M1-EK	1.767	1	1.145	1
M1-GK	2.977	4	2.949	4
M1-N3	1.986	1	1.583	1
M2-EK	9.496	16	3.96	4
M2-GK	3.739	4	3.456	5
M2-N3	10.77	14	10.926	9
M3-EK	21.115	8	11.2	5
M3-GK	23.057	11	26.06	7
M3-N3	8.422	10	15.453	10
CBPS				

Table 15: Results of Matching with Unemployment data: Average Standardized Bias (percentages) and number of biased variables. (For New Firms) **RECEIVING**

	Excluding Non-treatable		Including Non-treatable	
Algorithm	Average Bias	# Biased Variables	Average Bias	# Biased Variables
Before Matching	6.869	7	7.245	11
M1-EK	1.629	1	1.349	1
M1-GK	3.397	3	3.974	5
M1-N3	1.563	0	1.251	0
M2-EK	4.086	5	2.148	2
M2-GK	4.931	10	4.294	7
M2-N3	5.127	8	4.669	4
M3-EK	9.906	6	3.735	5
M3-GK	10.349	6	10.883	6
M3-N3	6.082	3	5.924	4
CBPS				

Table 16: Matching diagnosis for unemployment data: Means of both treated and control matched units. Standardized Biases (%) and t-values. (For Unemployement)

	Excluding				Including			
	Mean of matched...		SB	t-value	Mean of matched...		SB	t-value
	treated	control			treated	control		
pop	6.0799	6.0213	4.6	0.05	6.0799	6.0154	5.0	0.05
popAge	5.615	5.5594	4.3	0.05	5.615	5.5541	4.7	0.05
unempl	9.1504	8.8901	12.3	0.11	9.1504	8.8273	15.5	0.14
propCS1	.04339	.0419	2.7	0.03	.04339	.04208	2.3	0.02
propCS2	.03624	.03542	2.6	0.03	.03624	.03561	2.0	0.02
propCS3	.04518	.04643	-2.8	-0.03	.04518	.04621	-2.4	-0.03
propCS4	.11813	.11732	1.3	0.01	.11813	.11699	1.9	0.02
propCS5	.14959	.14916	0.8	0.01	.14959	.1493	0.5	0.01
propCS6	.17826	.18215	-5.3	-0.05	.17826	.18367	-7.3	-0.07
propCS7	.28151	.27964	1.9	0.02	.28151	.27843	3.0	0.03
ratioSal	.64349	.65181	-4.0	-0.04	.64349	.65267	-4.3	-0.04
creaEntr	3.6765	3.6564	0.1	0.00	3.6765	3.6091	0.2	0.01
distance	24.797	24.768	0.1	0.00	24.797	25.983	-4.2	-0.07
durAff	8.5425	8.8334	-10.8	-0.11	8.5425	8.681	-5.2	-0.05
newAff	182.86	199.37	-2.9	-0.20	182.86	198.43	-2.3	-0.19
depRate	8.8839	9.21	-3.5	-0.04	8.8839	9.0008	-1.2	-0.01
succRate	.71703	.72338	-6.6	-0.06	.71703	.71781	-0.8	-0.01

C Figures

Figure 5: ATT (with duration) (1/2)

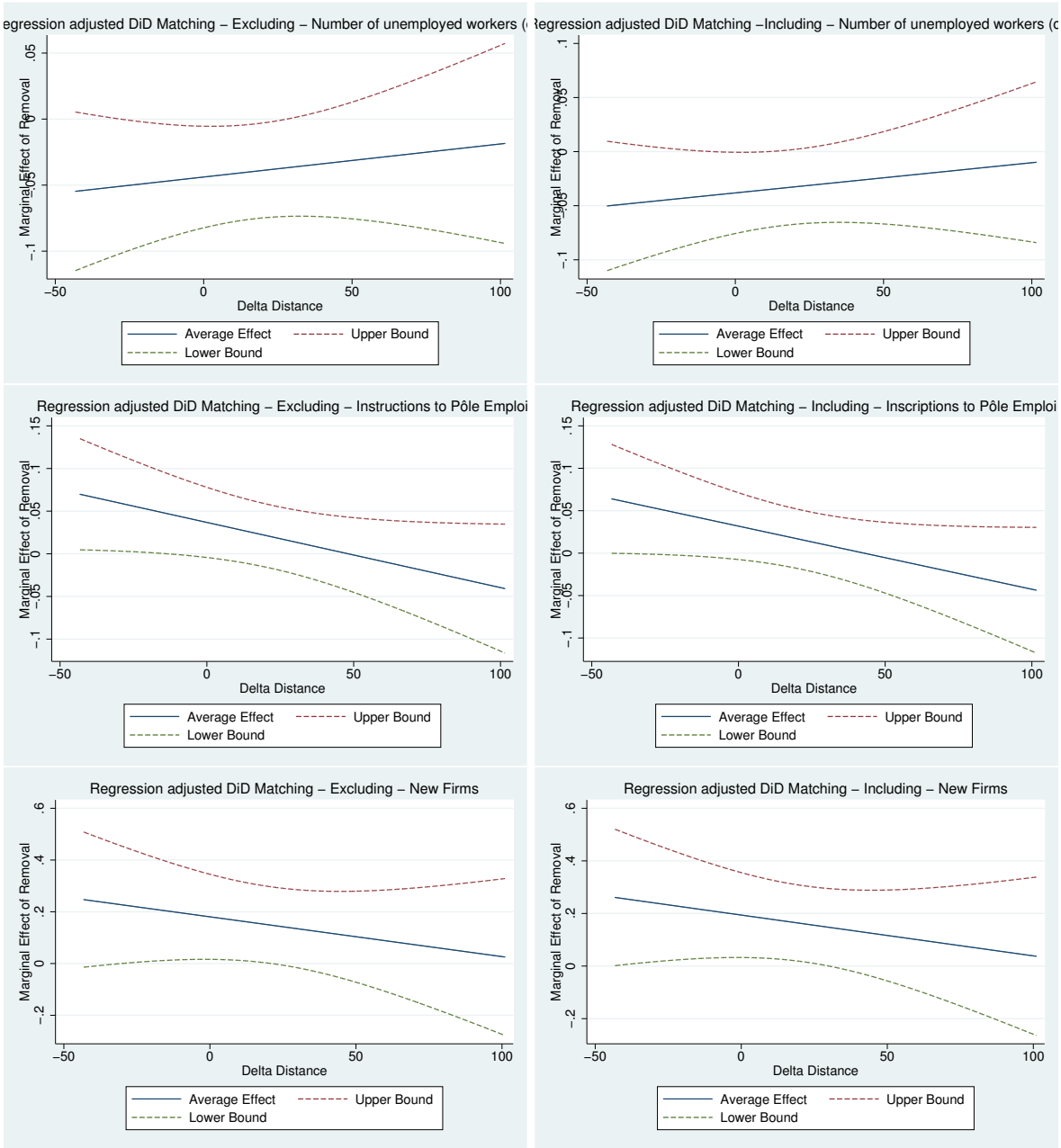


Figure 6: ATT (with duration) (2/2)

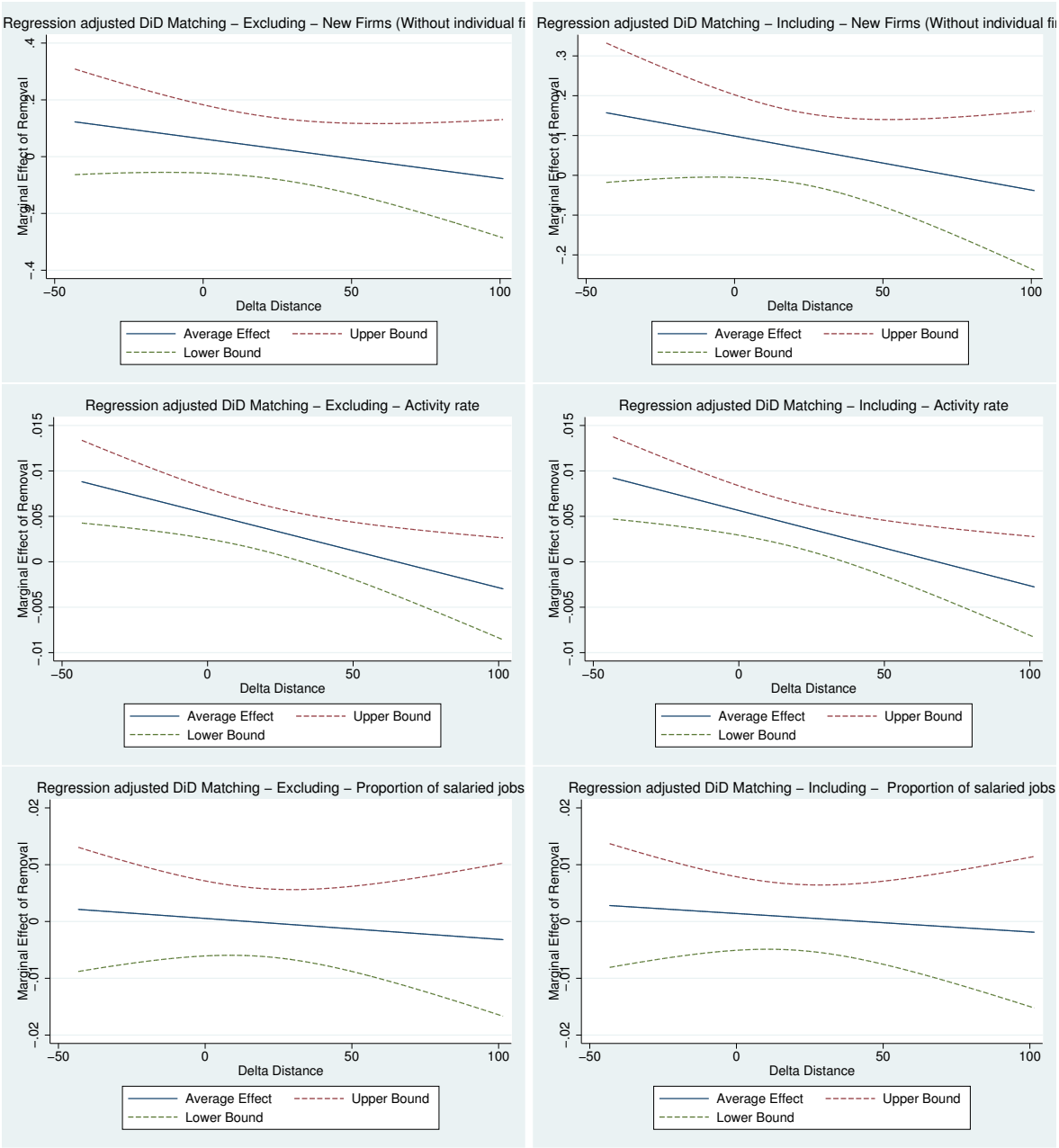


Figure 7: ATT (without duration) (1/2)

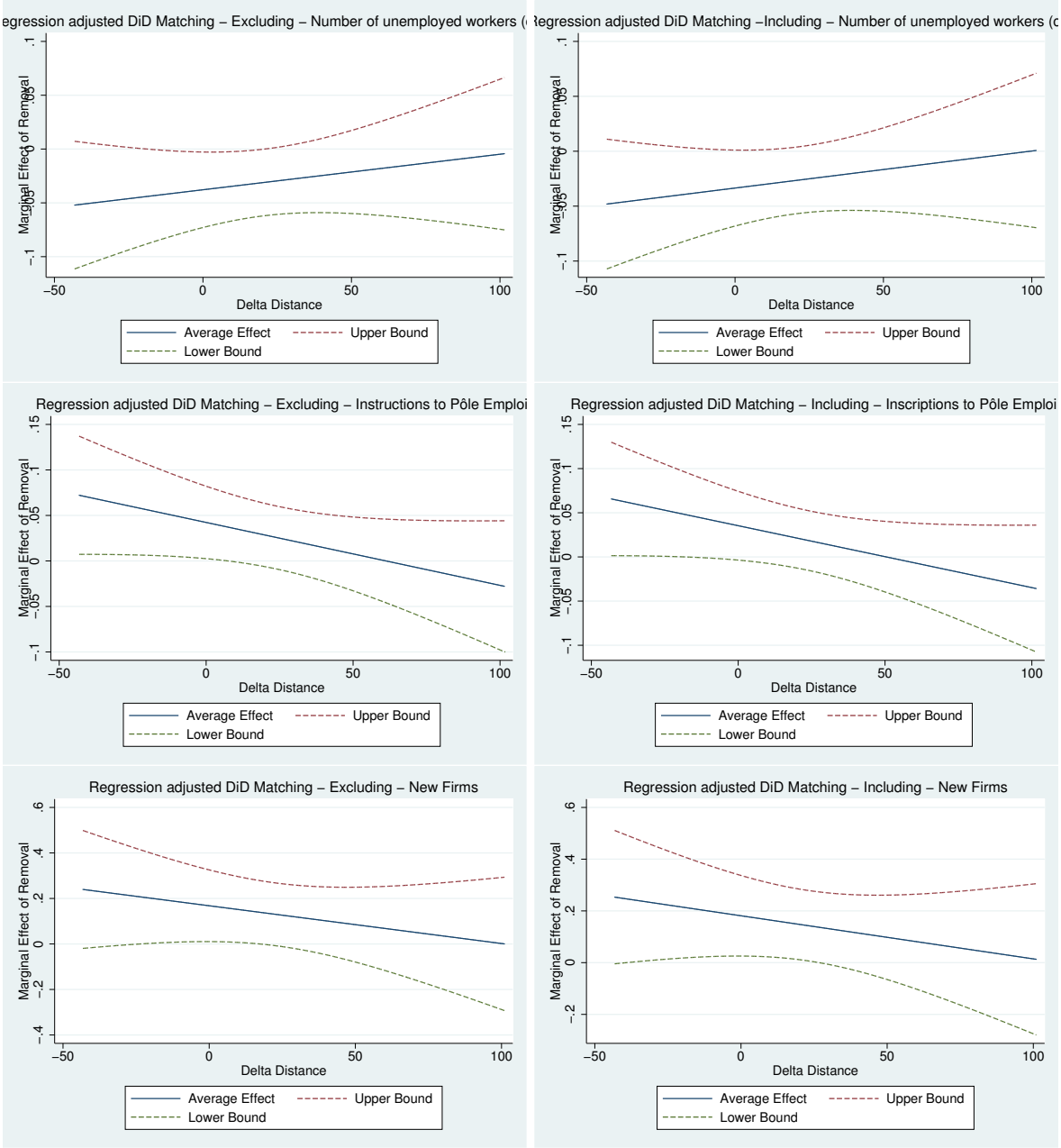


Figure 8: ATT (without duration) (2/2)

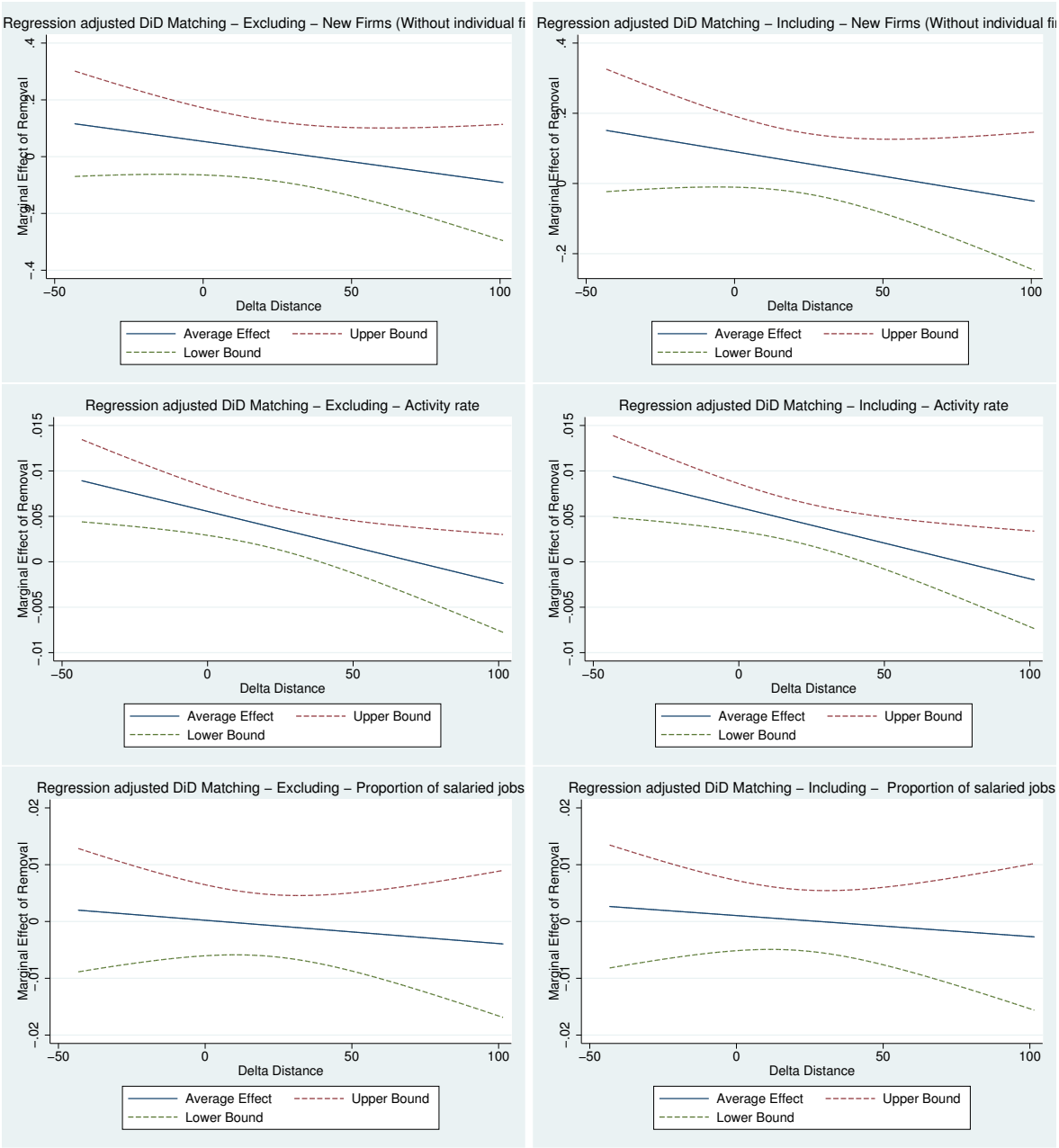


Figure 9: ATT (with duration) 3GK, per 10 kilometers

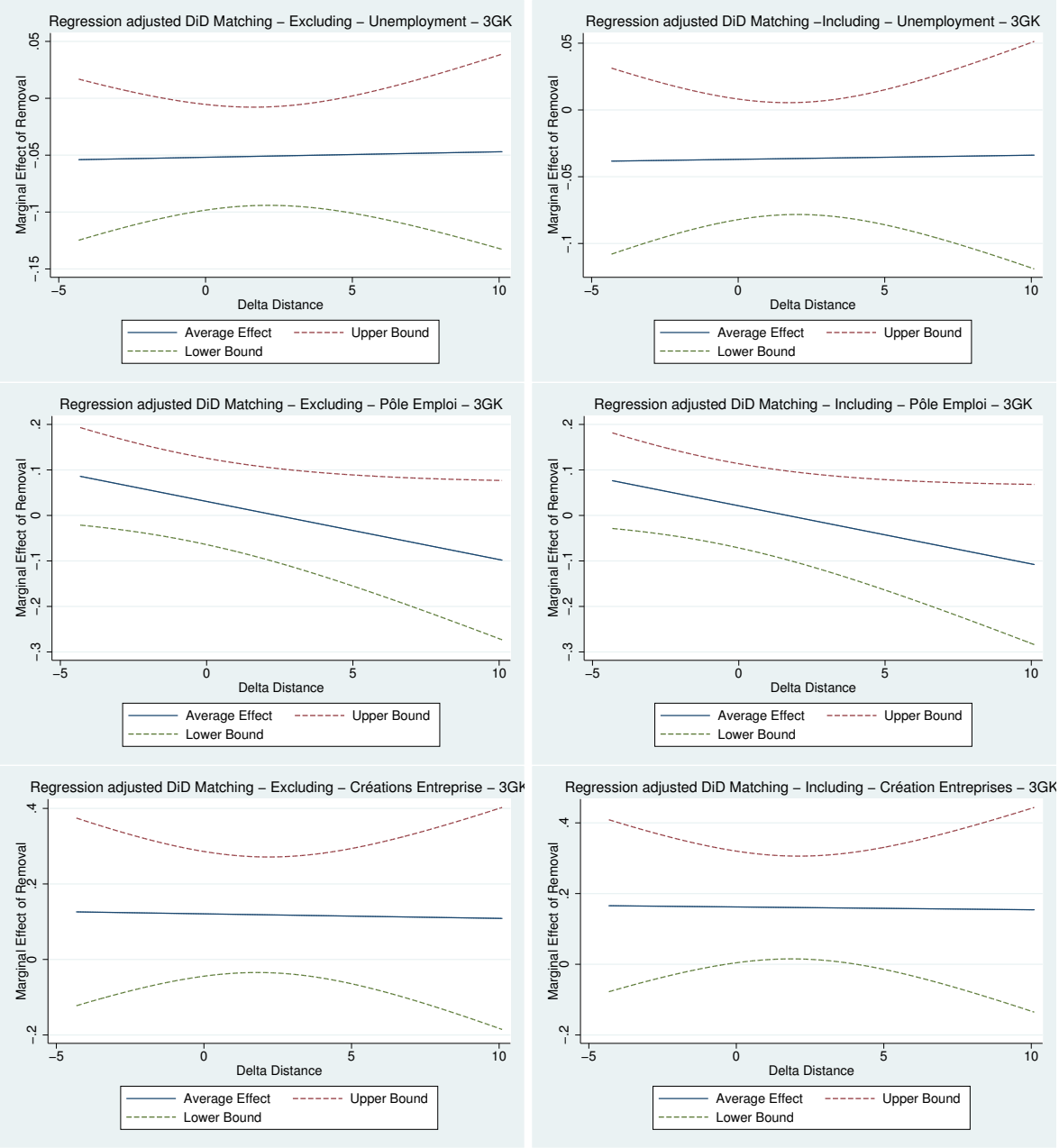
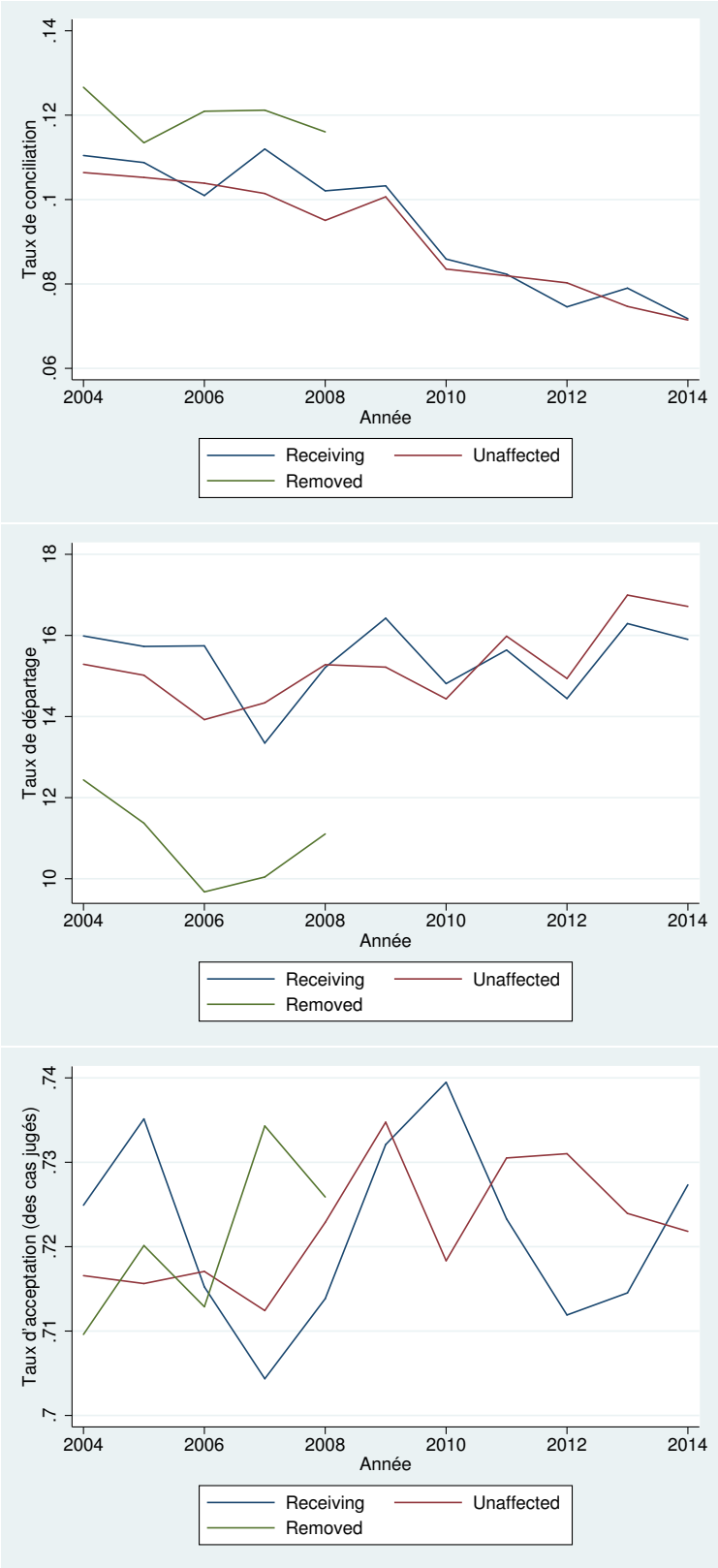


Figure 10: Evolution of outcomes at the court's level.



```
//ATT (Formule dans Marcus 2012)
//d_unemployment: change in unemployment
//weights1: matching weights

gen w_d_unemployment=weights1*d_unemployment

tab weights1 if treat_remov==1 & weights1!=0
local n1=`r(N)'
su w_d_unemployment if treat_remov==1 & weights1!=0
local sum1=`r(N)'*`r(mean)'
su w_d_unemployment if treat_remov==0 & weights1!=0
local sum2=`r(N)'*`r(mean)'
local att=1/`n1'*(`sum1'-`sum2')

//Estimated ATT
display `att'

su d_unemployment if treat_remov==1 & weights1!=0
local var1=`r(Var)'
su d_unemployment if treat_remov==0 & weights1!=0
local var0=`r(Var)'
gen w2=weights1^2
su w2 if treat_remov==0 & weights1!=0
local sum=`r(N)'*`r(mean)'
local varatt=1/`n1'*`var1'+`sum'/`n1'^2*`var0'

//Estimated variance of ATT
display `varatt'

//t-statistics
local t=`att'/sqrt(`varatt')

display `att'
display `t'
```