Does merging small bankruptcy courts increase their efficiency?

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Comments welcome

Abstract

We estimate the impact of a 2009 reform that merges small bankruptcy courts on the quality of their rulings. A conceptual framework enables us to link difference-in-difference estimates to the impact of the reform on Type 1 error (restructuring a non-viable firm) and Type 2 error (liquidating a viable firm). We apply this framework to an (almost) exhaustive sample of 600,000 bankruptcy cases in France that started between 2000 and 2019. The reform unambiguously reduces Type 1 errors while having no impact on Type 2 errors. Post-merger courts' behavior depends more on that of the absorbing court than on that of the absorbed one.

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1 Introduction

Inherited from a time when travel was difficult, the judicial maps of European countries are dense compared to those of more recent countries such as the United States, Canada or Australia. Thus, the reorganization and modernization of judicial systems in European countries often involves merging courts. This was the case of Italy in 2013, France in 2008 – 2009, The Netherlands in 2008. The European Commission for the efficiency of Justice (CEPJ) counts that between 2010 and 2016, 10 European countries reduced the number of courts location by 15% or more.

Several reasons are put forward for merging courts. They would reduce operating costs, eliminate smaller entities - often deemed inefficient -, and help break bad local habits. At the same time, judicial procedures' digitalization and better transport networks would mitigate negative impacts that local courts closures would have on access to justice.

A few empirical studies analyzes the impact of these merges on court efficiency. In most of them, court efficiency is measured by the speed at which the court discharges cases or some similar indicators. [1] We depart from this approach. Working on bankruptcy courts, we measure the gain in efficiency by the change in the quality of the their ruling, an approach also considered by Iverson (2018), and Giné and Love (2010). More precisely, following White (1994), we consider bankruptcy courts as screening devices and define Type 1 error as restructuring a non-viable firm and Type 2 error as liquidating a viable firm.

Our first contribution is to develop a conceptual framework that shows that the reform's impact on Type 1 and Type 2 error can be derived from the estimated coefficients of a set of difference-in-difference equations. The overall impact of merging commercial courts on Type 2 error depends on the sign of the reform's impact on the survival rate of firms that start a bankruptcy procedure. The impact on Type 1 is less straightforward to recover : it depends on both the survival and the restructuring rates of firms starting a bankruptcy procedure. The framework also provides robustness tests based on the consistency of estimates across equations on different bankruptcy outcomes.

Our second contribution is to apply this framework to the 2009 French reform of the judicial map in which 55 courts were absorbed. [2] We use an (almost) exhaustive sample of the 600,000 bankruptcy cases in France started over 2000 - 2019 and estimate a set of six standard difference-in-difference equations. We show that the reform unambiguously reduces Type 1 errors and did not impact Type 2 errors. [3] We find that the reform's impact shows entirely on bankruptcy outcomes of firms whose court was absorbed and have no impact on bankruptcy outcomes for firms in jurisdictions with absorbing courts. These results are robust but apply only to small firms. At the time of the reform, there was a fear that absorbing courts would not cope with their growth. We show that this risk did not materialize. In addition, we show that the behavior

1. Such as the clearance rate or backlog rate.

2. One might worry that the effects we measure are affected by the 2009 financial crisis. It might be the case if the characteristics of firms filing for bankruptcy are affected by the crisis differently across jurisdictions. We show in Appendix A.3 that this is not the case.

3. These results are consistent with Esquerré (2019), which measures the impact of the same reform on court bankruptcy decisions and bankruptcy outcomes.

of absorbing courts influences more the bankruptcy outcomes of firms in the jurisdiction of the court they absorbed than the opposite.

Our paper contributes to the literature on the impact of continuation and liquidation bias in bankruptcy on firm survival. In the US, Morrison (2007) show that, contrary to popular belief, Chapter 11's continuation bias (Type 1 error) for small firms is either absent or empirically insignificant, a result confirmed by Dou et al. (2021) for very large firms who also show that excess liquidation (Type 2 error) is also rare for very large firms. The continuation bias is however more likely in France, where the bankruptcy laws are more pro-debtor than in the US. Our results show that the 2009 French reform and merging of courts reduced the continuation bias.

The rest of the paper is organized as follows. Section 2 presents the 2009 reform of the judicial map. Section 3 presents the framework of our analysis and shows how to derive the efficiency impact of a reform on Type 1 and Type 2 errors from data observed by the econometrician. Section 4 presents data sources and statistics. Section 5 presents the main results of the reform's impact, and robustness tests. Section 6 provides evidence regarding the underlying mechanisms by testing for the transmission of courts' behavior. Section 7 concludes.

2 French Commercial Courts and the 2009 Reform

2.1 The French bankruptcy process

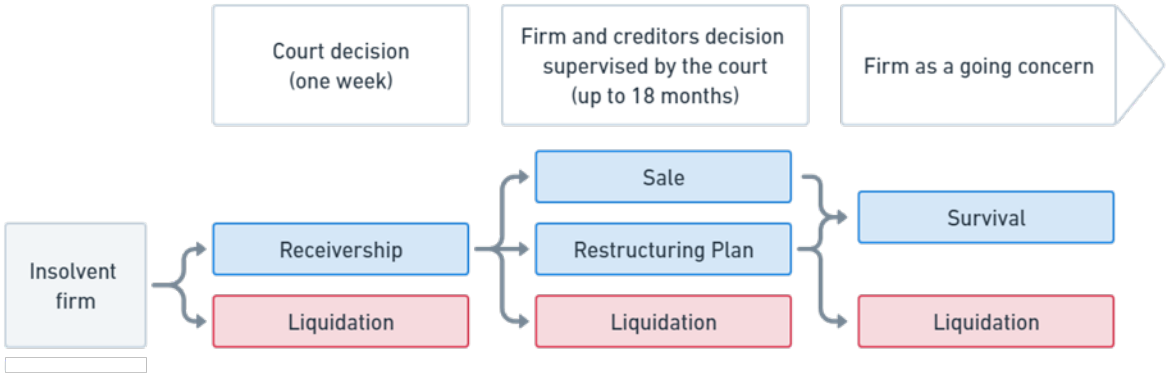
In France, commercial courts deal with corporate bankruptcies and commercial disputes. Each corporate bankruptcy case is assigned to a specific judge [4] (that we will refer to as "bankruptcy judge"), but decisions are taken by consensus amongst the judges in the chamber.

When a firm is insolvent (i.e., its available liquid assets do not cover its short-term debts) and without an informal agreement with one or more creditors, it must file for bankruptcy. [5] A deliberation hearing brings together the bankruptcy chamber judges (at least three) and the firm's management. They arbitrate between liquidation and receivership ; the decision is usually made within a week. Judicial liquidation is pronounced when it is deemed that the debtor has no apparent chance of pursuing a viable activity. Conversely, if the firm is considered potentially viable, receivership is chosen in an attempt to reorganize the business and the debt. In this case, an observation period is triggered for six months, renewable twice, up to a maximum of 18 months. The firm is protected from creditors, an administrator is appointed to advise or replace the manager, a judicial receiver defends creditors' rights, and the bankruptcy judge supervises the case.

At the end of the observation period, the judicial chamber deliberates on whether to approve or reject a restructuring plan. In case of rejection, the court pronounces the firm's liquidation. This decision is based on reports by the court-appointed administrator and bankruptcy judge and after consulting with the creditors. Creditors are brought together in a creditors' committee for firms exceeding a certain threshold (over 150 employees, with sales over 20 million euros). The final decision rests with the court. In practice, even if a debt-restructuring plan is approved,

4. In our database, we do not have judges' identities.
5. Creditors can also bring the case to the commercial court to trigger the bankruptcy filing.

FIGURE 1 – French bankruptcy process



Note: Figure 1 summarises the main stages of the bankruptcy process for insolvent firms in France. In our analysis, we consider the outcome "sale" as if the firm obtains a restructuring plan. It concerns only 3% of firms after receivership and their exclusion does not modify the results and their interpretation.

difficulties may worsen, and the plan may fail. The firm is then liquidated. Figure 1 summarizes the main stages of the bankruptcy process.

One specificity of French commercial courts [6] is that judges are not professionals but lay judges. There are currently over 3,000 lay commercial judges. Their election is a two-stage process : firm managers (legal entities or registered individuals who carry out commercial transactions) elect their representatives, who then elect the judges. Candidates must be registered in the Trade and Companies Register or have run a firm for at least five years. Each judge is initially elected for a two-year term and may then be re-elected three times for a four-year term (for a maximum duration of 14 years). They are unpaid volunteers, sitting only one or two half-days a week and pursuing their usual professional activities the rest of the time. They receive legal training after their election and during their term of office. For obvious reasons, they cannot work on cases relating to their firm. The judges elect from among themselves the President of the court and two vice presidents appointed for a non-renewable four-year term.

2.2 The 2009 reform beyond commercial justice

While the French judicial map set up in 1958 had been criticized for its obsolescence, it had not undergone any substantial change before 2009. The 2009 reform is ambitious and concerned the entire French judicial map and not only the organization of the commercial courts. Actually, reforming the commercial court map was not the authorities' main objective but rather a by-product of the overall reform. Official reports by the French National Assembly (AN (2008)), Senate (Sénat (2012)), and Court of Auditors (CdC (2015)) regarding the effect of the reform of the judicial map barely mention the impact it had on commercial courts and rather insisted on the impact on civil and high courts. The French Court of Auditors only regrets that the reform

6. That excludes the *département* of Moselle, Haut-Rhin, and Bas-Rhin that operate differently for historical reasons.

has insufficiently reorganized the commercial courts. [7]

The reform aimed to rationalize and adapt the judicial organization to the population dynamic. [8] One of the first objectives of the reform was thus to better distribute justice resources over the territory. Most of the reform consisted in choosing the civil and high courts that would be closed. [9] In most cases, the decision regarding these civil and high courts also applied to the local commercial courts [10]. The criteria in choosing which court to close (e.g., distance to a prison, distance to a psychiatric hospital, avoiding the isolation of magistrates, see Cahu (2015)) had nothing to do with commercial or bankruptcy laws. The low weight given to commercial courts in the selection process is linked to their low weight in the French judicial system : commercial courts represent only 15% of all French jurisdictions [11] and about 3% of civil justice's budget [12].

In cases where commercial courts were closed regardless of the decision made regarding the local high or civil court, the reform aimed for at least one commercial court per *département*. By default, the biggest court was set to be the absorbing one. This resulted in closing the smallest commercial court(s) in each *département*, regardless of their performance. [13]

In total, the reform closed 55 commercial courts [14] and created 5 others. [15] Discussed since May 2007, the details of the reform were announced in late 2007, and the reform law passed in February 2008. It became effective on January 1, 2009. As soon as the law was adopted, the actual reorganization of courts started. Figures A.1 and A.2 illustrate the suppression and creation of commercial courts. Figure 2 presents a close-up of the *départements* of Maine-et-Loire and Indre-et-Loire to describe the three types of commercial courts : absorbed, absorbing, and control courts. The Section 4 discusses a large set of descriptive statistics of the reform's impact on courts' activity.

7. "The Court reiterates the recommendation it made in a referendum of May 13, 2013, to deepen the reform of the commercial court map" – CdC (2015)

8. For instance, the number of high courts per inhabitant varied from 1 to 19 for territories of comparable size. (AN (2008)).

9. The choices were widely criticised (Cahu (2015)), leading to protests and resignations (CdC (2015)) in some instances. Some high courts closed in 2009 and reopened in 2013. By contrast, the reform of the commercial courts has been rather well-received (CdC (2015))

10. Out of the 23 high courts that were closed, 21 commercial courts in the same town were also closed.

11. There were roughly 1,200 jurisdictions before the reform. As a result of the reform, 29% of the commercial courts (55 out of 186) have closed, compared with 37% of the civil courts (*Tribunaux d'instance*, 178 out of 476), 11% of the high courts (*Tribunaux de Grande Instance*, 23 out of 182) and 23% of the labor courts (*Prudh'ommes*, 62 out of 271).

12. See `https://www.budget.gouv.fr/`.

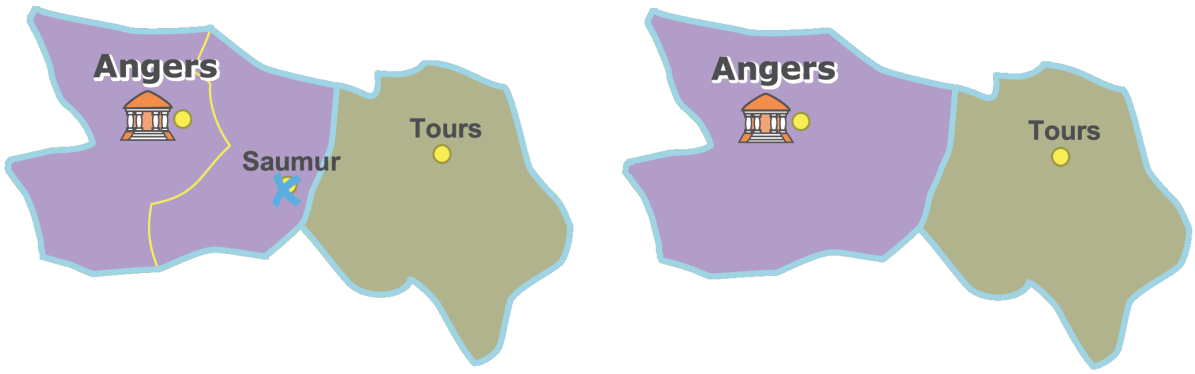
13. Only five absorbing commercial courts are slightly smaller than those they absorb. Three of them were in the same city as a closing high court or civil court. An individual study of these cases does not reveal clear political stakes in choosing which court to keep.

14. We exclude the particular case of Bernay/Pont-Audemer. Bernay was created, and Pont-Audemer closed without any change in the catching area. We thus retain 54 absorbed courts and 4 creations in our analysis.

15. In some of the *départements*, bankruptcy and commercial disputes were handled by high courts with commercial authority (e.g., in Mende, Lozère). After 2009, these courts were replaced by new commercial courts. Because of the different functioning of high court with commercial authority, we exclude from our analysis commercial courts that have absorbed or replaced high courts. Similarly, in the Grand-Est region and for historical reasons, commercial bankruptcy and disputes are dealt with by high courts. In overseas regions, commercial justice is handled by mixed commercial courts. The 2009 reform has not modified this organization. We also exclude these regions from our analysis.

FIGURE 2 – Close-up on the 2009 reform of the judicial map

(a) Before (b) After



Note: Figure 2, left image represents the absorbing (Angers), absorbed (Saumur) and control (Tour) courts before the reform. Right image represents the new (Angers) and control (Tours) courts after the reform. All firms in the absorbed court's catching area before reform are assigned to the absorbing court after reform.

3 Measuring Bankruptcy Court Efficiency

Most empirical papers aiming at explaining the efficiency of commercial courts and its impact on firms (their size, their access to bank loans) measure efficiency by the speed at which the court discharges cases or some similar indicators such as the clearance rate or backlog rate [16]. There is indeed some indication that in Italy, faster judicial procedures are associated with better access to finance (Jappelli et al. (2005)) and larger firms (Giacomelli and Menon (2017)). Pezone (2023) exploits the mergers of Italian courts to measure that a reduction in average trial length has a large, positive effect on firm employment. Similarly, Müller (2022) shows that a drop in court congestion increases firms' leverage. However, these efficiency measures might not be helpful in the cases of French bankruptcy procedures as the law strictly sets the time frame, and there is little heterogeneity between commercial courts in this regard (see Section 4). Moreover, even if speed is essential for a well-functioning judicial system, the quality of judicial decisions also matters. Measuring the quality of judicial decisions is difficult. In the case of bankruptcy courts, the quality of their decisions is probably easier to assess than that of other judicial courts. Indeed, few recent papers try to measure the impact of some reforms affecting commercial courts on the quality of their ruling. This is the case of Iverson (2018), which tests the impact of an exogenous drop in caseload in bankruptcy courts on recidivism. Giné and Love (2010) analyzes the impact of bankruptcy reform in Colombia on efficiency by studying its effect not only in reorganization duration, but also on firm survival after reorganization. Antill (2022) tries to measure whether the right firm survives bankruptcy. Our approach follows the same vein, but we propose a more formal framework to measure gains in court quality. This conceptual framework provides a way to interpret our empirical results regarding the impact of judicial reform on court efficiency.

Our starting point is that an efficient commercial court is the one that separates firms that

16. Backlog rate is defined as the number of pending cases in a court at the beginning of the year over the number of judges working in that court over the year.

are economically viable from those that are not early in the bankruptcy process, and ensures that the firms that reorganize are those with a good chance of surviving through well-designed restructuring plans. [17]

In this section, we develop a simple conceptual framework and a set of empirical tests based on information available to the statistician that together allow us to retrieve the impact of the reform on Type 1 error (restructuring a non-viable firm) and Type 2 error (not restructuring a viable firm). We start with the conceptual framework, then present the set of equations to be estimated and the parameters of interest, and finally show how estimates of these parameters help us recover the reform's impact on Type 1 and Type 2 errors.

3.1 Conceptual framework

3.1.1 Notations and simple arithmetic

Let us consider that there are two types of firms filing for bankruptcy : viable firms (i.e., the ones with survival chances if restructured) noted with a "h," and non-viable firms (i.e., the ones with no survival chances even if restructured) noted with a "l." We note x_j the share of viable firms at the onset of the procedure in the catching area of court j (the statistician does not observe x_j).

The probability that a firm in the catching area of court j is sent to receivership by the court is noted R_j and is observed by the statistician. However, the quality of the initial sorting by the court is not observed. It depends on R_j^h, the unobserved probability of receivership for a viable firm in court j, and R_j^l, the unobserved probability of receivership of a non-viable firm in court j.

$$R_j = x_j R_j^h + (1 - x_j) R_j^l \tag{1}$$

Let us note P_j^{Rh} the (unobserved) probability of restructuring of a viable firm after having been sent to receivership at the first stage of the bankruptcy procedure and P_j^{Rl} that of non-viable firms. The statistician observes P_j, the restructuring rate of firms starting a bankruptcy procedure, and P_j^R, the overall restructuring rate of firms sent to receivership.

$$\begin{cases} P_j = x_j R_j^h P_j^{Rh} + (1 - x_j) R_j^l P_j^{Rl} \\ P_j^R = \dfrac{x_j R_j^h P_j^{Rh} + (1 - x_j) R_j^l P_j^{Rl}}{R_j} = \dfrac{P_j}{R_j} \end{cases} \tag{2}$$

Let us note s_j^{Ph} the survival rate of viable firms that restructure. By definition, the survival rate of non-viable firms that restructure is null. We can then write the survival rate of firms after filing for bankruptcy S_j, the survival rate after receivership S_j^R, and the survival rate after restructuring S_j^P.

17. Another approach consists in measuring efficiency by creditor recovery rates, as in Antill (2022). However, we do not have access to creditor recovery rates.

$$\begin{cases} S_j = s_j^{Ph}\ x_j R_j^h P_j^{Rh} \\ S_j^R = s_j^{Ph} \dfrac{x_j R_j^h P_j^{Rh}}{R_j} = \dfrac{S_j}{R_j} \\ S_j^P = s_j^{Ph} \dfrac{x_j R_j^h P_j^{Rh}}{x_j R_j^h P_j^{Rh} + (1-x_j) R_j^l P_j^{Rl}} = \dfrac{S_j}{P_j} \end{cases} \tag{3}$$

3.1.2 Type 1 and Type 2 errors at different stages of the bankruptcy procedure

There are two crucial moments in the procedure. At the onset of the procedure, the court decides whether to send a firm to receivership or to liquidate it. At this stage, the court faces two types of errors. It can send a non-viable firm to receivership and liquidate a viable firm. We note this two errors T_{1R} and T_{2R}, with :

- $T_{1R} = R_j^l$, sending a non viable firm to receivership,
- $T_{2R} = 1 - R_j^h$, not sending a viable firm to receivership (= liquidating a viable firm at the first stage of the procedure).

During the observation period, there are again two types of errors : restructuring a non-viable firm and liquidating a viable firm. Taking into account the first-stage Type 1 and Type 2 errors and the second-stage errors, the overall Type 1 and Type 2 errors are the following :

- $T_1 = R_j^l P_j^{Rl}$, restructuring a non viable firm,
- $T_2 = 1 - R_j^h P_j^{Rh}$, not restructuring a viable firm.

Because T_1 and T_2 errors encompass the whole bankruptcy process, they are more meaningful than T_{1R} and T_{2R} errors that concentrate only on the first stage of the bankruptcy procedure. The lower T_{1R} and T_{2R}, the more efficient the sorting of firms at the onset of the procedure. The lower T_1 and T_2, the more efficient the overall sorting of firms. A reform that decreases both T_1 and T_2 unambiguously increases court efficiency. A reform that would also decrease T_{1R} and T_{2R} would lead to an even more efficient system as it would better sort firms at the earlier stage of the procedure. However a reform that would reduce T_{1R} and T_{2R} but increase T_1 and T_2 would not lead to an overall more efficient system.

3.2 Empirical framework

In the previous section, we did not need to introduce heterogeneity between firms within a given group when considering two types of firms (viable firms if restructured and non-viable firms even if restructured). When bringing the framework to the data, we need to consider the fact that viable firms (resp. non-viable) are not identical and control for firm characteristics.

We want to measure the 2009 reform's impact on bankruptcy outcomes. One can observe three non-independent outcomes for each firm starting a bankruptcy procedure. All these outcomes can be coded as binary variables.

1. The firm is sent to receivership (as opposed to being liquidated).
2. The firm restructures and continues as a going concern.
3. The firm survives a given number of years [18] after filing for bankruptcy.

18. We consider the survival 7 years after filing for bankruptcy.

In terms of empirical analysis, we thus have three potential equations to measure the impact of the reform. These have the same general form :

$$Y_{ijt} = \alpha + \sum_{k=g,d} \beta_k \left(Reform_k \times Post\right) + \gamma X_i + u_j + \theta_j + \theta_{s\times t} + \epsilon_{ijt} \tag{4}$$

where Y_{ijt} is a dummy variable (taking the value of 1 or 0) for each of the three above-mentioned outcomes. The index j refers to the jurisdiction based on the pre-reform division. X_i is a vector of firm observable characteristics, θ_j a pre-reform jurisdiction fixed effect that controls for the non-observable characteristics of the firms within the jurisdiction, and $\theta_{s\times t}$ an industry $\times$ year fixed effect. t refers to the year the firm started the bankruptcy procedure. $Post$ equals 1 after 2008, and $Reform_k$ takes the following values : *Absorbed* if the firm i is located in a jurisdiction whose court is absorbed (indexed by $k = d$), and *Absorbing* if the firm i is in a jurisdiction j whose court absorbed another court (indexed by $k = g$). The control sample is made of firms located in a jurisdiction j whose court is unaffected by the reform. In addition, we include the local annual unemployment rate (u_j) to control for economic conditions at the (pre-reform) jurisdiction level. [19].

Having firm characteristics as well as pre-reform court fixed effects, industry $\times$ year fixed effects and the unemployment rate at the pre-reform jurisdiction level on the right-hand side of the equation allows us to take into account the fact that firms are not identical (within each group of firms) and that bankruptcy outcomes can vary according to industry and year.

We are interested in coefficient β. We can estimate the equation on three different samples : the whole sample of firms filling for bankruptcy, the sample of firms admitted to receivership, or the sample of firms that restructure. We then have six equations to estimate corresponding to the six ratios of the conceptual framework :

1. $\boldsymbol{\beta_R}$ the coefficients of the reform's impact when the left-hand variable is the dummy for the outcome at the first stage of the procedure (1 if the firm is sent to receivership and 0 if it is liquidated right away). It is estimated on the whole sample of firms filing for bankruptcy.
2. $\boldsymbol{\beta_P}$ the coefficients of the reform's impact when the left-hand variable is a dummy for restructuring (1 if the firm manages to restructure and 0 otherwise). It is estimated on the whole sample of firms filing for bankruptcy.
3. $\boldsymbol{\beta_{P|R}}$ the same coefficients as in 2. estimated on the restricted sample of firms sent to receivership (restructuring after receivership).
4. $\boldsymbol{\beta_S}$ the coefficients of the reform's impact when the left-hand variable is a dummy for the firm's survival (1 if the firm survives a given number of years and 0 otherwise). It is estimated on the whole sample of firms filing for bankruptcy.
5. $\boldsymbol{\beta_{S|R}}$ the same coefficients as in 4. estimated on the restricted sample of firms sent to receivership (survival after receivership).

19. As pointed out by Iverson (2018), this control is all the more critical if the different jurisdictions face economic conditions that evolve differently over time (Appendix A.3).

6. $\boldsymbol{\beta_{S|P}}$ the same coefficients as in 4. estimated on the restricted sample of firms that restructure (survival after restructuring).

For each of these $\boldsymbol{\beta}$, we have two estimates : the one for firms in jurisdictions that were absorbed ($k = d$) and the one for firms in absorbing jurisdictions ($k = g$). In this setting, the $\boldsymbol{\beta}$ measures the reform's impact at each stage of the procedure, conditional (or not) on what happened at the previous stage.

We estimate the reform's impact by OLS in a regular difference-in-difference fashion with clustered standard errors at the pre-reform court level. [20]

3.3 From empirical equations to Type 1 and Type 2 errors

With additional assumptions discussed below, it is possible to go from estimating equation (4) for different outcomes to the impact of the reform on Type 1 and Type 2 errors (T_1, T_2, T_{1R}, T_{2R}).

We make the two following identification assumptions :

1. The share of viable firms x_j – adjusted for industry × year fixed effects, pre-reform court fixed effects, and firms characteristics – is not impacted by the reform,
2. The survival rate of viable firms that restructure s_j^{Ph} – adjusted for industry × year fixed effects, pre-reform court fixed effects, and firms characteristics – is not impacted by the reform.

These two assumptions do not mean that the share of viable firms and the survival rate of viable firms that restructure is the same before and after the reform, but that changes that may occur after the reform for other reasons (including the 2008-2009 financial crisis) are the same in the jurisdictions concerned by the reform as in the jurisdiction not concerned by the reform. Appendix A.3 provides empirical evidence supporting these assumptions.

We show in the Appendix B that estimates for β_S (the reform's impact on the survival rate after filing for bankruptcy) and β_P (the reform's impact on the restructuring rate after filing for bankruptcy) allow us to recover how the reform impacts T_1 and T_2 errors formally. Below, we explain the intuition.

With these assumptions, the sign of the coefficient β_S shows directly the reform's impact on the overall Type 2 error. A positive β_S means that the reform increased the overall survival rate of firms that start a bankruptcy procedure. As only viable firms survive, it also means that the proportion of viable firms that restructure is higher. So, the overall Type 2 error decreases with the reform. The same reasoning applies to $\beta_S < 0$. A negative β_S means the reform reduces the overall survival rate, implying that less viable firms restructure (i.e., Type 2 error increases).

The reform's impact on Type 1 errors is less straightforward to recover. The estimate for β_P together with that of β_S allows for recovery change in Type 1 error in some specific cases. We already show that a positive β_S means that more viable firms restructure after the reform (a lower T_2), then if β_P is negative (fewer firms restructure after the reform), one can conclude that less non-viable firms restructure, meaning a reduction in Type 1 error as well. The same can be conclude if $\beta_S > 0$ and $\beta_P = 0$. A reform that reduces the restructuring of viable firms

20. The outcomes of interest are dummy variables with outcomes that are not rare events ; we chose to use regular OLS and not logit model that is more suitable in case of the model with rare events.

($\beta_S < 0$) while increasing the overall restructuring rate ($\beta_P > 0$) is increasing the restructuring of non-viable firms. In this case, both Type 1 and Type 2 errors increase.

These results are summarized in the matrix Table 1. With this matrix, we can go directly from estimating the impact of the reform on survival and restructuring rates after filing for bankruptcy to Type 1 and Type 2 errors.

TABLE 1 – Type 1 and Type 2 errors matrix

		β_P		
		> 0	$= 0$	< 0
β_S	> 0	$?T_1 \downarrow T_2$	$\downarrow T_1 \downarrow T_2$	$\downarrow T_1 \downarrow T_2$
	$= 0$	$\uparrow T_1 = T_2$	$= T_1 = T_2$	$\downarrow T_1 = T_2$
	< 0	$\uparrow T_1 \uparrow T_2$	$\uparrow T_1 \uparrow T_2$	$?T_1 \uparrow T_2$

The next question we want to answer is the quality of firms' initial sorting at the procedure's onset. For example, in case where T_1 is reduced, we would like to know whether this reduction in the restructuring of non-viable firms results from less of these firms being sent to receivership (lower T_{1R}) and/or less of them restructuring after being sent to receivership. In the same way, in cases where the reform reduces T_2, we would like to know whether this comes from less viable firms being liquidated at the onset of the procedure (lower T_{2R}) and/or less of them restructuring once being sent to restructuring.

Estimates of the impact of the reform on the probability of being sent to recovery (coefficient β_R) can inform us of the change in Type 1 and Type 2 errors at the beginning of the procedure if we add two additional assumptions :

1. If the reform reduces the overall probability of a firm to be sent to receivership ($\beta_R < 0$), it does not increase the probability of a non-viable firm to be sent to receivership ($\Delta R_j^l \leq 0$).
2. If the reform increases the overall probability of a firm to be sent to receivership ($\beta_R > 0$), it does not decrease the probability of a non-viable firm to be sent to receivership ($\Delta R_j^h \geq 0$).

These two assumptions are plausible. The first one says that if the reform results in an overall more severe court at the onset of the procedure, it does not become less severe with non-viable firms. The second assumption says that if the reform results in an overall less severe court at the onset of the procedure, it does not become more severe with viable firms.

It is immediate to see that under the first hypothesis :

$$\beta_R < 0 \Rightarrow \Delta T_{1R} \leq 0 \tag{5}$$

And that under the second hypothesis :

$$\beta_R > 0 \Rightarrow \Delta T_{2R} \leq 0 \tag{6}$$

These two measures are summarized in Table 2.

TABLE 2 – Type 1 and Type 2 errors at the onset of the bankruptcy procedure

β_R		
> 0	$= 0$	< 0
$?T_{1R} \downarrow = T_{2R}$	$?T_{1R}\ ?T_{2R}$	$\downarrow = T_{1R}\ ?T_{2R}$

3.4 A robustness test : Checking the consistency of the estimates across equations

In the previous section, we have seen how to judge the qualitative impact of the reform on Type 1 and Type 2 errors from estimates of the impact of the reform on the probabilities of being sent to receivership, restructuring, and surviving after filing. We used only the estimates on the whole sample of firms stating a bankruptcy procedure (β_R, β_P and β_S). Here, we show how the three other estimates of the impact of the reform ($\beta_{P|R}$, $\beta_{S|R}$ and $\beta_{S|P}$) can be used to verify that our econometric estimations lead to consistent results.

For example, the equation of the survival rate after restructuring ($\beta_{S|P}$) helps check the robustness of the result. The reasoning is the following : a reform that reduces Type 1 errors ($\beta_P > 0$) and reduces or leaves unchanged Type 2 errors ($\beta_S \geq 0$) also increases the share of viable firms within the firms that restructure. One would then expect the survival rate after restructuring to increase (i.e., $\beta_{S|P} >0$). The same reasoning applies if the reform increases Type 1 errors while increasing or leaving unchanged Type 2 errors. In this case, one would expect $\beta_{S|P} < 0$. This is shown in the bottom part of Table 3. The same reasoning applies to finding conditions for $\beta_{P|R}$, $\beta_{S|R}$. A formal proof is given in Appendix B. If coefficients derived from conditional equations do not meet the conditions set in Table 3, one could argue that the results derived from outcomes on the whole sample of firms filing for bankruptcy lack robustness.

4 Data and Descriptive Statistics

4.1 Data sources and firms characteristics

Our data include all corporate bankruptcies and their outcome in France from 2000 [21] to 2019. We use data from FIBEN (*Fichier Bancaire des Entreprises*, the information system of Banque de France). Bankruptcy data come from the commercial court registries. These data provide the date and nature of the judgments at each stage of the bankruptcy procedures. For each case, it is possible to retrieve the date of the bankruptcy filing, the date of restructuring, the date of sale, the duration of the procedure, the firm's survival if it emerges from bankruptcy, and the date

21. We start our sample in 2000 not to be impacted by the 1999 reform that started modifications to the judicial map, although not to the same extent as the 2009 reform.

TABLE 3 – Robustness matrix

		β_R			β_P		
		> 0	$= 0$	< 0	> 0	$= 0$	< 0
β_S	> 0	?	$\beta_{S\|R} > 0$	$\beta_{S\|R} > 0$	?	$\beta_{S\|P} > 0$	$\beta_{S\|P} > 0$
	$= 0$	$\beta_{S\|R} < 0$	$\beta_{S\|R} = 0$	$\beta_{S\|R} > 0$	$\beta_{S\|P} < 0$	$\beta_{S\|P} = 0$	$\beta_{S\|P} > 0$
	< 0	$\beta_{S\|R} < 0$	$\beta_{S\|R} < 0$	?	$\beta_{S\|P} < 0$	$\beta_{S\|P} < 0$	?
β_P	> 0	?	$\beta_{P\|R} > 0$	$\beta_{P\|R} > 0$			
	$= 0$	$\beta_{P\|R} < 0$	$\beta_{P\|R} = 0$	$\beta_{P\|R} > 0$			
	< 0	$\beta_{P\|R} < 0$	$\beta_{P\|R} < 0$	?			

of liquidation at any stage of the process. From FIBEN, we also have information on the firm's location (postal code).

The assignment of firms to courts depends on the firm's location. For filings starting after 2008, we have the information regarding the commercial court the firm is assigned to via BODACC (*Bulletin Officiel d'Annonce Civile et Commerciale*). For filings before 2008, that information was not available. Based on the firm's postal code and the division of the former commercial courts' jurisdictions, we have reconstructed the pre-reform commercial courts' catching areas for all the firms that filed for bankruptcy before 2008.

For almost every firm in our sample, FIBEN provides information on its size and sector of activity. [22] The final sample includes 580,227 unique firms whose bankruptcy case was handled by commercial courts between 2000 and 2019. We observe the survival of these firms up to 2022. Our analysis uses firm data as cross-sectional data, with each firm being observed only once when filing for bankruptcy.

Table 4 describes the composition of firms entering bankruptcy. 92,4% of firms are very small, "micro"-enterprises (less than 10 employees and less than €2 million in annual turnover). 36% of the sample firms are in the service industry, 25% in the construction and 24% in trade, the remaining in the manufacturing and transport industries. [23]

4.2 A snapshot of commercial courts' activity before and after the reform

Absorbed courts are different from other courts. First, they are much smaller, as shown by Figure 3, panel (a). Absorbed courts handled 49 cases over the pre-reform period against nearly 128 in the absorbing courts and 154 in the control courts (Table 5, first panel). The new courts resulting from the reform handle as many cases as the control courts. We illustrate the court size distribution shift before and after reform Figure 3, panel (b). After the reform, courts are,

22. Complete balance sheet data are only available for a small proportion of the sample. Financial information on firms that have filed for bankruptcy is scarce, not only because the vast majority of these firms are very small firms, for which data are less accessible than for large firms, but also because these firms, being in difficulty, generally provide less information on their accounts than healthy firms.

23. We removed agricultural firms from the sample because their bankruptcy process differs from other sectors.

TABLE 4 – Firms summary statistics

	All	Before (2000-2007)			After (2008-2019)	
		Control	Absorbed	Absorbing	Control	New
Full sample	580,227	95,104	12,778	29,686	303,455	139,204
Bankruptcy outcome						
Receivership rate	198,951	0.374	0.553	0.506	0.294	0.374
Restructuring rate after filing	59,457	0.353	0.401	0.342	0.275	0.277
Restructuring rate after receivership	59,457	0.132	0.222	0.173	0.081	0.103
Survival 7 years after filing	25,715	0.071	0.098	0.082	0.047	0.052
Survival 7 years after receivership	25,715	0.190	0.176	0.163	0.159	0.137
Survival 5 years after restructuring	22,682	0.458	0.434	0.429	0.455	0.440
By size						
Micro-entreprises	546,147	0.924	0.932	0.934	0.944	0.950
Others	34,080	0.076	0.068	0.066	0.056	0.050
By industry						
Construction	153,062	0.249	0.242	0.261	0.270	0.266
Trade	136,398	0.243	0.244	0.242	0.229	0.243
Services	216,253	0.357	0.324	0.341	0.387	0.366
Others	73,054	0.151	0.190	0.155	0.114	0.124

Note: Table 4 presents summary statistics on our cross-sectional firm data. Size and industry are the one the year before the firm filed for bankruptcy. First row and first column report the number of firms for each category. The rest of the Table displays percent calculated on the first row. Micro-enterprises are those that have less than 10 employees and less than €2 million annual turnover. Other industry includes manufacturing and transport industries.

on average, bigger and more concentrated (the red line compared to the blue line). This effect does not come from a cyclical effect, as evidenced by the red dotted line that illustrates the size the former jurisdictions would have had after 2009. Given these figures, the objective of the reform to harmonize the size of the courts across the territory is reached. However, there is still a considerable heterogeneity in court size after the reform.

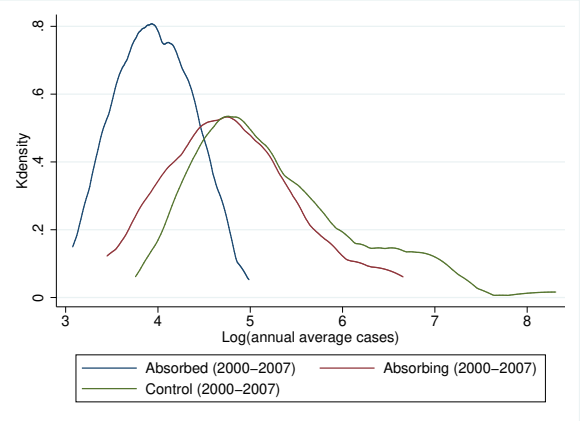
Commercial courts are also heterogeneous regarding bankruptcy outcomes (reported in Table 5). The average absorbed court granted receiverships to 49.5% firms. This compares to 44.9% for absorbing courts and 42.3% for control courts. After the reform, the receivership rate of the average new jurisdiction dropped to 36.4%, on par with that of the average controlling court (34.8%). The higher rate of receivership in the absorbed jurisdictions is also illustrated in Figure 4. This graph reveals that, before the reform, trends over time are parallel across types of jurisdictions ; this point is essential for our identification strategy that will rely on the parallel trends hypothesis. There is much less divergence when looking at the other outcomes.

Other descriptive statistics are reported in the appendix Table A.1 and illustrate the differences in composition between courts. Notably, the reform impacted the number of judges. 25% of the control courts gained 2 judges or more (up to 8 in the case of Nanterre) at the time of the reform, while the remaining 75% remained unchanged. The story differs for absorbed and absorbing courts : the absorption reduced the total number of judges. In median, there was a loss of 5 judges in the new court compared with the sum of the judges in the former courts. [24] Let us take the

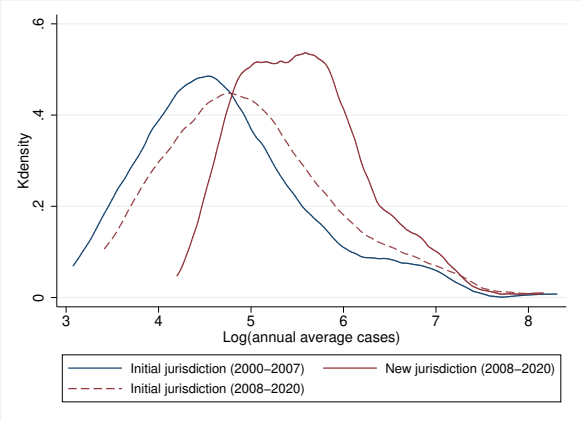
24. This analysis excludes courts that have absorbed a high court. Because High courts operate differently,

FIGURE 3 – Court size distribution

(a) Before reform, by jurisdiction type

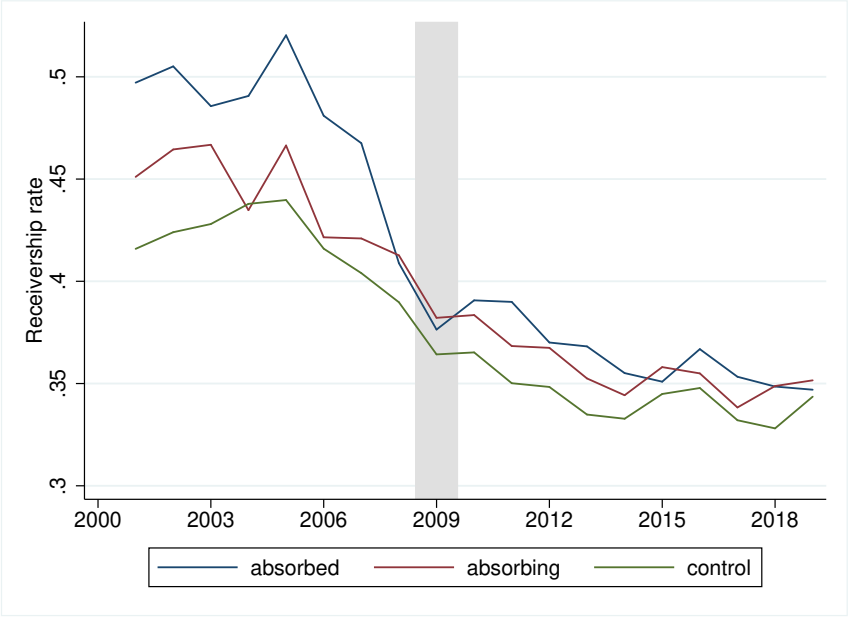


(b) Before and after reform



Note: Figure 3 panel (a) reports the distribution of each type of jurisdiction before reform. Figure 3 panel (b) shows the court size distribution before and after reform (resp. blue and red line). Court size is measured as the log of the average number of cases per year. To ensure that the change in distribution is not due to a cyclical effect, the dashed red line represents what the distribution of court size would have been in the absence of a reform (i.e., for the former jurisdictions).

FIGURE 4 – Receivership rate per jurisdiction



Note: Figure 4 represents the annual rates of receivership by jurisdiction type (absorbed, absorbing and control), on average.

example of the Vosges department : the former court of Épinal had 15 judges, Mirecourt 8 and Saint-Dies-Dès-Vosges 9, for a total of 32 judges in office before reform. After the reform, the new Épinal court had only 20 judges for the whole department. Only 10% of the new courts retained at least as many judges as the sum of merging courts.

When the reform was discussed, because of the overall reduction in the number of judges, the

counting the number of judges dedicated to corporate bankruptcy is impossible.

fear was that absorption would result in a substantial increase in judges' caseload and procedure length and a deterioration in the quality of rulings. The judge's caseload indeed increased from an average of 6.0 cases per judge per year in absorbed courts and 8.8 in absorbing courts to 13.4 in the new court. For firms in absorbed courts, this leads to facing a judge with a caseload twice as significant as the one before the reform. However, the caseload also increased in control courts (from 11 to 13 cases per judge per year on average). We show in Appendix A that the increase in judge's caseload seems to have little to no impact on bankruptcy outcomes, and that the reform had no impact on procedure length.

The length of receivership (which varies by a factor of two between a receivership that leads to liquidation and one that leads to restructuring) increases from 2000 to 2019 but is very homogeneous between absorbed, absorbing, and control courts. This homogeneity in procedure length comes from the law strictly setting it.

The reform also impacts the physical distance between firms and their assigned court, but only marginally so : for firms in the absorbed courts, the distance to the new court increases by an average of 10km. In some instances (6% of the cases), the reform reduces the distance between firms and their newly assigned court.

5 The reform's impact

5.1 Main Results

Table 7 shows the reform's impact on the probability of the different bankruptcy outcomes for firms in the district whose court was absorbed ($Absorbed \times Post$) and for firms in the district whose court absorbed another court ($Absorbing \times Post$). The control group comprises firms in the district whose court was unaffected by the reform.

Whatever the outcome, we can see that the bankruptcy of firms in absorbing districts is not impacted by the reform (second line of Table 7, coefficients are not significantly different from zero). On the contrary, as shown by coefficients reported on the first line of Table 7, the bankruptcy outcomes of firms in absorbed districts are affected by the reform : they are less likely to be sent to receivership (-6.43 percentage points). Their probability of restructuring after filing is also lower. The probability of survival seven years after filing for bankruptcy is unaffected, but the probability of surviving seven after receivership and five years after restructuring are significantly higher. According to the model developed Section 3, results in columns (2) and (4) mean that the overall Type 2 error for firms in absorbed jurisdictions is unchanged (the probability of a viable firm to be liquidated). In contrast, the overall Type 1 error (the probability for a non-viable firm to be restructured) is reduced (see Table 1). These results are comforted by the positive survival rate after restructuring (column (6)). This is indeed the sign that the composition of restructured firms has changed with relatively fewer non-viable firms (this is also what was expected ; see upper right part of Table 3).

Does this reduction in the continuation bias of absorbed courts come from a better sorting at the onset of the procedure or at the restructuring stage (restructuring vs. liquidation after receivership) ? Coefficient β_R of column (1), Table 7 is negative. This is also what is shown in

TABLE 5 – Summary statistics per jurisdiction

	Average number of procedures per year											
	Before (2000-2007)						After (2008-2019)					
	N	Mean	Median	St. Dev.	Min	Max	N	Mean	Median	St. Dev.	Min	Max
Absorbed	50	55	50	25	21	145						
Absorbing	42	158	112	148	29	772						
New							42	294	249	189	101	985
Control	70	318	154	526	42	4,020	70	383	209	500	62	3,490
	Receivership											
	Before (2000-2007)						After (2008-2019)					
	N	Mean	Median	St. Dev.	Min	Max	N	Mean	Median	St. Dev.	Min	Max
Absorbed	50	0.495	0.497	0.105	0.281	0.790						
Absorbing	42	0.449	0.456	0.095	0.227	0.709						
New							42	0.364	0.358	0.062	0.206	0.526
Control	70	0.423	0.423	0.125	0.102	0.861	70	0.348	0.356	0.085	0.125	0.585
	Restructuring rate after filing											
	Before (2000-2007)						After (2008-2019)					
	N	Mean	Median	St. Dev.	Min	Max	N	Mean	Median	St. Dev.	Min	Max
Absorbed	50	0.160	0.159	0.043	0.075	0.242						
Absorbing	42	0.134	0.129	0.043	0.041	0.253						
New							42	0.105	0.099	0.028	0.048	0.186
Control	70	0.122	0.113	0.045	0.032	0.234	70	0.100	0.096	0.032	0.030	0.200
	Restructuring after receivership											
	Before (2000-2007)						After (2008-2019)					
	N	Mean	Median	St. Dev.	Min	Max	N	Mean	Median	St. Dev.	Min	Max
Absorbed	50	0.330	0.343	0.085	0.140	0.523						
Absorbing	42	0.297	0.295	0.072	0.108	0.425						
New							42	0.288	0.293	0.058	0.142	0.404
Control	70	0.291	0.285	0.071	0.146	0.458	70	0.288	0.292	0.056	0.144	0.387

Figure 5 : the effect of the reform is direct and significant at the 1% level. As soon as the reform was passed in 2008 and became effective in 2009, firms' probability of entering receivership decrease by 6 percentage points on average in the absorbed jurisdictions. The effect varies from -5 p.p. in 2009 to more than -10 p.p. in 2019, but remains globally stable over time. This graph also provides evidence towards to validity of our method with respect to the parallel trends hypothesis. Prior to 2008, there is no significant difference in treatment between absorbed and control jurisdictions, nor between absorbing and control jurisdictions. As discussed in the conceptual framework section, we interpret that as the fact that the Type 1 error at the onset of the procedure (T_{1R}) has also been reduced (Table 2). Our experiment does not allow us to conclude regarding the Type 2 error at the onset of the procedure (T_{2R}).

Finally, given the sign of β_R, β_P, and β_S, we can check the consistency of the β estimates across specifications. As expected, β_R^S is positive. β_R^P is not significantly different from zero, but

TABLE 6 – Summary statistics per jurisdiction – continued

	Survival 7 years after filing											
	Before (2000-2007)						After (2008-2019)					
	N	Mean	Median	St. Dev.	Min	Max	N	Mean	Median	St. Dev.	Min	Max
Absorbed	50	0.077	0.072	0.030	0.024	0.152						
Absorbing	42	0.065	0.064	0.020	0.038	0.126						
New							42	0.036	0.036	0.009	0.018	0.059
Control	70	0.067	0.063	0.027	0.024	0.161	70	0.035	0.033	0.009	0.015	0.056
	Survival 7 years after receivership											
	Before (2000-2007)						After (2008-2019)					
	N	Mean	Median	St. Dev.	Min	Max	N	Mean	Median	St. Dev.	Min	Max
Absorbed	50	0.157	0.148	0.055	0.070	0.326						
Absorbing	42	0.147	0.141	0.039	0.070	0.251						
New							42	0.097	0.096	0.020	0.055	0.135
Control	70	0.164	0.160	0.055	0.069	0.359	70	0.104	0.100	0.027	0.056	0.198
	Survival 5 years after restructuring											
	Before (2000-2007)						After (2008-2019)					
	N	Mean	Median	St. Dev.	Min	Max	N	Mean	Median	St. Dev.	Min	Max
Absorbed	50	0.460	0.453	0.102	0.303	0.712						
Absorbing	42	0.447	0.433	0.087	0.333	0.665						
New							42	0.328	0.330	0.041	0.239	0.430
Control	70	0.479	0.471	0.081	0.265	0.804	70	0.333	0.327	0.037	0.272	0.417

Note: Table 5 presents summary statistics at the court level. All statistics are annual averages per type of jurisdiction. The right-hand side of the table presents the statistics for the pre-reform period, and the left-hand side for post-reform period for which new = absorbed + absorbing.

our conceptual framework does predict the sign of this particular coefficient given the sign of β_P and β_R.

One concern with the empirical experiment we just conducted is that our control group includes firms in jurisdictions of various sizes, including very large ones. The absorbed courts are among the smallest, as discussed in Section 2.2. Also, even if, following Iverson (2018), we check in the Appendix A.3 that the 2009 financial crisis did not impact the composition of firms in different jurisdictions, there could be a suspicion that firms in smaller jurisdictions were more impacted by it [25]. To ensure that our results do not come from a size effect of control courts compared to absorbed ones, we reduce our control group to courts with a size comparable to that of absorbed courts [26] and run the same set of equations. Results shown in Table 8 are qualitatively identical

25. Boekwa et al. (forthcoming) show that the most affected commuting zones in terms of private employment by the 2009 financial crises were usually the smallest.

26. The largest absorbed courts had up to 15 judges (Annonay, Argentan, Charleville-Mézières, Dole) whereas the smallest of the control courts had only 8 judges before the reform (Foix, Gap). These courts, albeit small, were not concerned by the reform because they were the only commercial court in the *département*. We keep in the control group firms in jurisdictions with only one commercial chamber and 15 judges or less. It corresponds to the maximum number of judges in absorbed courts and the median number of judges in control courts (Table A.1).

TABLE 7 – The reform's impact

Bankruptcy outcome	R_{ijt}	P_{ijt}		S_{ijt}		
Coefficient of interest	β_R	β_P	$\beta_{P\|R}$	β_S	$\beta_{S\|R}$	$\beta_{S\|P}$
	(1)	(2)	(3)	(4)	(5)	(6)
Absorbed × Post	-0.0643***	-0.0346***	-0.0156	-0.00265	0.0147**	0.0322**
	(-4.51)	(-5.66)	(-1.22)	(-0.67)	(2.08)	(1.98)
Absorbing × Post	-0.0234	-0.00746	0.00221	-0.00240	-0.000710	0.0223*
	(-1.56)	(-1.21)	(0.23)	(-0.77)	(-0.11)	(1.66)
Unemployment rate	-0.0254***	-0.0100***	-0.00366	-0.00440***	-0.00229	-0.000397
	(-3.99)	(-4.12)	(-0.85)	(-2.92)	(-0.56)	(-0.08)
Year × Industry FE	✓	✓	✓	✓	✓	✓
Court FE	✓	✓	✓	✓	✓	✓
Firm size	✓	✓	✓	✓	✓	✓
Observations	580,227	580,227	198,950	451,820	158,607	50,522
Adj. R^2	0.152	0.090	0.071	0.071	0.072	0.035

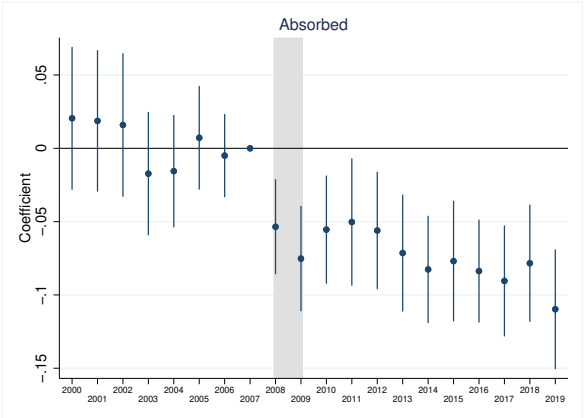
t statistics in parentheses

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

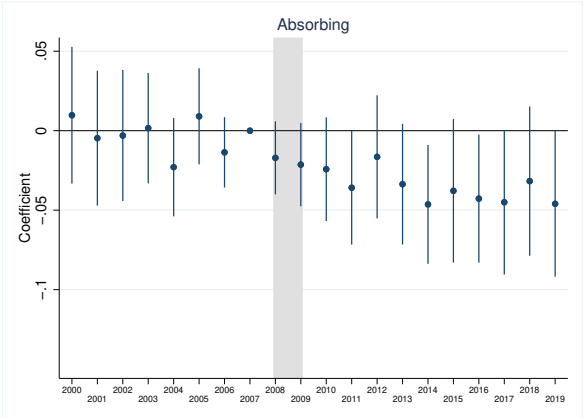
Note: Table 7 presents the results of equation (4). Dependent variables are : column (1) the probability of receivership, column (2) the probability of restructuring after filing, column (3) the probability of restructuring after receivership, column (4) the survival 7 years after filing, column (5) the survival 7 years after receivership, and column (6) the survival 5 years after restructuring. Standards errors are clustered at the pre-reform court level.

FIGURE 5 – Reform's impact on the probability of receivership

(a) β_{dt} for firms in absorbed jurisdictions



(b) β_{gt} for firms in absorbing jurisdictions



Note: In its dynamic form, equation (4) can be written :

$$Y_{ijt} = \alpha + \sum_{k=g,d} \sum_{t \neq 2007} \beta_{kt} \cdot (\mathbb{1}_t \times Reform_k) + \gamma X_i + u_j + \theta_j + \theta_{s \times t} + \epsilon_{ijt} \tag{7}$$

where $\mathbb{1}_t$ are year dummies. Figure 5 plots the coefficients β_{kt} on the probability of entering receivership (compared to direct liquidation). Left hand side shows the β_t for firms in absorbed jurisdictions ($k = d$), and right hand side shows the β_t for firms in absorbing jurisdictions ($k = g$). Standards errors are clustered at the pre-reform court level.

to previously reported results. The impact is slightly stronger for survival after receivership and restructuring (columns (5) and (6)) and slightly smaller (but still significant) for receivership and the probability of restructuring after filing (columns (1) and (2)). The check for internal consistency of estimates across equation are also the same.

TABLE 8 – The reform's impact – controlling by small control courts only

	R_{ijt}	P_{ijt}		S_{ijt}		
	(1)	(2)	(3)	(4)	(5)	(6)
	β_R	β_P	$\beta_{P\|R}$	β_S	$\beta_{S\|R}$	$\beta_{S\|P}$
Absorbed × Post	-0.0400***	-0.0260***	-0.0247	0.00415	0.0208**	0.0583***
	(-2.94)	(-3.19)	(-1.64)	(0.74)	(2.12)	(2.93)
Absorbing × Post	0.00272	0.00137	-0.00653	0.00433	0.00540	0.0481***
	(0.21)	(0.17)	(-0.52)	(0.84)	(0.56)	(2.71)
Unemployment rate	0.00188	0.000380	-0.00206	0.00294	0.00421	0.00420
	(0.24)	(0.13)	(-0.31)	(1.16)	(0.94)	(0.51)
Year × Industry FE	✓	✓	✓	✓	✓	✓
Court FE	✓	✓	✓	✓	✓	✓
Observations	251,483	251,483	103,517	197,643	83,622	27,534
Adj. R^2	0.121	0.090	0.073	0.073	0.066	0.037

t statistics in parentheses

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Note: Table 8 is the same as Table 7 with a reduced control group. To ensure that our result does not come from a size effect of control courts – on average much bigger compared to absorbed courts – we reduce our control group to courts with only one commercial chamber and 15 judges or less. This leaves us with 32 control courts (out of 70) that handled 69,815 cases over the period (out of 398,559, or 17.5%). Standards errors are clustered at the pre-reform court level.

In summary, these results point to a positive impact of the reform : it reduced the continuation bias of absorbed courts while having no impact on the survival chances of viable firms. At least part of the reduction in the continuation bias comes from a lower probability of sending non-viable firms to receivership. The reform also seems to have no impact on bankruptcy outcomes for firms in districts with absorbing courts. This is an important result since, at the time of the reform, there were fears that absorbing courts would not cope with their growth.

5.2 Heterogeneity of the reform's impact according to firm size

In France, most bankruptcy procedures concern small firms. In our sample, over the 2000-2019 period, 92,4% of firms have less than 10 employees and an annual turnover of less than €2 million ("micro-enterprises"). One would expect that the continuation bias is bigger for these firms than for larger ones for which financial stakes are of greater consequences, and thus courts (and creditors) decisions more likely to be rational [27] Consequently, one would expect the reform to have little or no impact on larger firms. In order to check that, we split our sample according to firm size. The

27. Bernstein et al. (2019) also make this point, stating that "presumably the stakes are large enough in these cases that judicial preferences are of less consequence."

first sample comprises micro-enterprises. It has 546,147 bankruptcy cases. The second sample include all other firms, bigger than micro-enterprises. It has 34,067 bankruptcy cases. We then run the same set of estimations.

Results are presented in Table 9, panel A for small enterprises and Table 9, panel B for other firms. The results are clear-cut. The reform does not impact larger firms' bankruptcy outcomes. The impact on small firms is the same as the whole sample. The continuation bias is thus reduced for small firms. It is not reduced for larger ones, maybe because there is no continuation bias to begin with for these firms. In terms of macroeconomic impact, reducing the continuation bias for small firms is a good thing. Even if each firm is small because there are so many of them, reducing the continuation bias can have a significant economic impact. Indeed, the 546,147 small firms' bankruptcy cases concentrate almost as many employees (737,000) as larger firms (953,876).

TABLE 9 – The reform's impact – by firms' size

	Panel A: Micro-enterprises					
Bankruptcy outcome	R_{ijt}	P_{ijt}		S_{ijt}		
Coefficient of interest	β_R	β_P	$\beta_{P\|R}$	β_S	$\beta_{S\|R}$	$\beta_{S\|P}$
	(1)	(2)	(3)	(4)	(5)	(6)
Absorbed × Post	-0.0658***	-0.0357***	-0.0183	-0.00265	0.0189**	0.0376**
	(-4.57)	(-5.65)	(-1.36)	(-0.70)	(2.58)	(2.28)
Absorbing × Post	-0.0241	-0.00802	0.00186	-0.000719	0.00510	0.0264*
	(-1.58)	(-1.30)	(0.19)	(-0.25)	(0.77)	(1.90)
Unemployment rate	-0.0252***	-0.00991***	-0.00491	-0.00407***	-0.00176	-0.00358
	(-4.13)	(-4.20)	(-1.04)	(-3.17)	(-0.48)	(-0.72)
Year × Industry FE	✓	✓	✓	✓	✓	✓
Court FE	✓	✓	✓	✓	✓	✓
Observations	546,147	546,147	176,909	423,628	140,469	44,408
Adj. R^2	0.128	0.083	0.074	0.044	0.054	0.036
	Panel B: Larger than micro-enterprises					
Bankruptcy outcome	R_{ijt}	P_{ijt}		S_{ijt}		
Coefficient of interest	β_R	β_P	$\beta_{P\|R}$	β_S	$\beta_{S\|R}$	$\beta_{S\|P}$
	(1)	(2)	(3)	(4)	(5)	(6)
Absorbed × Post	-0.0183	0.00191	0.0158	-0.00185	-0.0164	-0.0212
	(-0.79)	(0.08)	(0.57)	(-0.09)	(-0.69)	(-0.45)
Absorbing × Post	0.000449	0.00296	0.00403	-0.0260	-0.0456**	-0.0122
	(0.02)	(0.21)	(0.20)	(-1.59)	(-2.41)	(-0.38)
Unemployment rate	-0.0178**	-0.0000477	0.0107	0.00228	0.00615	0.0251*
	(-2.26)	(-0.01)	(1.59)	(0.40)	(0.75)	(1.84)
Year × Industry FE	✓	✓	✓	✓	✓	✓
Court FE	✓	✓	✓	✓	✓	✓
Observations	34,067	34,067	22,031	29,816	19,215	6,099
Adj. R^2	0.186	0.111	0.080	0.108	0.081	0.042

t statistics in parentheses

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Note: Table 8 reproduces Table 7 on two sub-samples according to firms' size. Panel A is restricted to "micro-enterprises", aka firms with less than 10 employees and €2 million in annual turnover. Panel B includes all other firms. Standards errors are clustered at the pre-reform court level.

6 Transmission of Courts' Behavior

What are the channels that lead to the reduction in the overall Type 1 error (T_1 and T_{1R}) for firms in absorbed courts ? To answer this question, we develop a simple empirical method to measure how absorbing courts transmit their behavior to the firms in the catching area of the court they absorb. We also leave the possibility of an absorbing court influencing the behavior of its absorbing court. Indeed, because some judges were transferred from the absorbed court to the absorbed one, it is reasonable that they also influence absorbing court behavior. In a sense, this experiment is close to the work by Abrams et al. (2022) that shows there is a convergence in judges' decisions within a commercial court that reflects a process of acquiring local practices by new judges. [28]

The empirical test we conduct is the following. Before the reform, we observe the various bankruptcy outcomes at each court : the receivership, restructuring and survival rates. We calculate the difference in outcome between the two courts before the reform for each pair of courts that merged and each possible outcome. We then run a regression that includes this difference as an explanatory variable and expect these differences to have no impact whatsoever on bankruptcy outcomes before the reform, but an impact after the reform. Of course, these variables are null for the control firms (those whose court is unaffected by the reform). The value of the coefficient indicates the intensity with which the past absorbing court behavior influences the bankruptcy outcome for firms that were initially in the absorbed jurisdiction. Symmetrically, we also measure the intensity with which bankruptcy outcomes in the absorbing court are influenced by the absorbed court's past behavior.

More precisely, we construct a measure of court behavior, $ShareY_j$ (see details in Appendix C). For absorbing jurisdiction (indexed by g), we measure $\overline{\Delta ShareY_{jg}}$ as the average difference for outcome Y between the absorbed and the absorbing court before the reform. Symmetrically, for absorbed jurisdictions (indexed by d), $\overline{\Delta ShareY_{jd}}$ is the average difference in behavior for outcome Y between the absorbing court and the absorbed one prior to the reform. By definition, $\Delta ShareY_j = 0$ for control courts. We include these measures as covariates of our baseline equation, which becomes :

$$\begin{aligned} Y_{ijt} = \alpha + \sum_{k=d,g} \beta_k (Reform_k \times Post) \\ + \sum_{k=d,g} \delta_k \left(\overline{\Delta ShareY_{jk}} \times Post\right) + \gamma X_i + u_j + \theta_j + \theta_{s\times t} + \epsilon_{ijt} \end{aligned} \tag{8}$$

We are interested in the δ_k coefficients. We expect them to be positive or null and below one. Results are presented in Table 10. They are consistent across outcomes. The past behavior of the absorbing court always has a more considerable influence on bankruptcy outcomes for firms in the absorbed jurisdiction than the absorbed court on that of the absorbing. The survival rate after filing in the absorbing court is not impacted by the past behavior of the absorbed court (column (4)).

A potential problem with this experiment is that the differences in bankruptcy outcomes

28. However, whereas their study is based on judges' rotation, our experiment is set at the court level.

TABLE 10 – Transmission of courts' behavior

	R_{ijt}	P_{ijt}		S_{ijt}		
	(1)	(2)	(3)	(4)	(5)	(6)
	β_R	β_P	$\beta_{P\|R}$	β_S	$\beta_{S\|R}$	$\beta_{S\|P}$
Absorbed × Post	-0.0450***	-0.0253***	0.00205	0.00296	0.0174**	0.0357**
	(-3.37)	(-3.78)	(0.19)	(0.77)	(2.46)	(2.52)
Absorbing × Post	-0.0359***	-0.0184**	-0.00502	-0.00455	-0.00624	0.00408
	(-3.14)	(-2.47)	(-0.49)	(-1.16)	(-0.91)	(0.31)
Post × $\Delta Share_{g\to d}$	0.424***	0.424***	0.659***	0.598***	0.573***	0.617***
	(5.84)	(3.53)	(7.14)	(4.47)	(5.11)	(6.09)
Post × $\Delta Share_{d\to g}$	0.282***	0.340***	0.120***	0.157	0.176***	0.179*
	(3.19)	(3.10)	(3.69)	(1.39)	(3.45)	(1.83)
Unemployment rate	-0.0261***	-0.00984***	-0.00298	-0.00436***	-0.00146	0.000308
	(-4.23)	(-3.94)	(-0.67)	(-2.86)	(-0.35)	(0.06)
Year × Industry FE	✓	✓	✓	✓	✓	✓
Court FE	✓	✓	✓	✓	✓	✓
Firm size	✓	✓	✓	✓	✓	✓
Observations	580,227	580,227	198,950	451,820	158,607	50,522
Adj. R^2	0.152	0.090	0.072	0.071	0.069	0.035

t statistics in parentheses

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Note: Table 10 presents the results of equation (8). We note $\Delta Share_{g\to d}$ (resp. $\Delta Share_{d\to q}$) the impact of absorbing court's (resp. absorbed) past behavior on firms from the absorbed (resp. absorbing) jurisdiction. Standards errors are clustered at the pre-reform court level.

between courts might reflect differences in the composition of firms in the two jurisdictions rather than that of their behavior. We thus use another measure of the outcomes by computing the part of the bankruptcy outcomes not explained by firms' characteristics : the court average residual of equation (4) without court fixed effect before the reform. [29] We then construct the difference in behavior between pairs of merging courts and estimate the equation (8). Results are shown in Table 11. They are qualitatively the same as the one reported in Table 10.

In equation (8), because of the court fixed effect, it is not possible to test that the difference in court behavior has no influence on bankruptcy outcome before the reform. We answer this issue with a dynamic difference-in-difference equation (9). Figure 6 displays the coefficients on three outcomes : receivership (panel (a)), restructuring (panel (b)), and survival 7 years after filing (panel (c)).

$$\begin{aligned} Y_{ijt} = \alpha + \sum_{k=g,d} \sum_{t\neq 2007} \beta_{kt}(\mathbb{1}_t \times Reform_k) + u_j + \gamma X_i + \theta_j + \theta_{s\times t} + \epsilon_{ijt} \\ + \sum_{k=g,d} \left(\sum_{t=2000}^{2007} \tau_{kt}(\mathbb{1}_t \times \Delta ShareY_{jkt}) + \sum_{t=2008}^{2019} \delta_{kt}(\mathbb{1}_t \times \overline{\Delta ShareY_{jk}}) \right) \end{aligned} \tag{9}$$

Before reform, we interact the annual measure $\Delta ShareY_{jt}$ with year dummy $\mathbb{1}_t$. The coefficient

29. See Appendix C for details.

TABLE 11 – Transmission of courts' behavior – controlling for firms composition in each jurisdiction

	R_{ijt}	P_{ijt}		S_{ijt}		
	(1)	(2)	(3)	(4)	(5)	(6)
	β_R	β_P	$\beta_{P\|R}$	β_S	$\beta_{S\|R}$	$\beta_{S\|P}$
Absorbed × Post	-0.0484***	-0.0265***	-0.00794	-0.00143	0.0131*	0.0345**
	(-3.71)	(-4.19)	(-0.66)	(-0.36)	(1.68)	(2.26)
Absorbing × Post	-0.0296***	-0.0144**	-0.00408	-0.00342	-0.00477	0.00378
	(-2.65)	(-2.40)	(-0.42)	(-1.06)	(-0.75)	(0.30)
Post × $\Delta Residuals_{g\to d}$	0.465***	0.280***	0.473***	0.199**	0.305***	0.411***
	(4.92)	(4.14)	(3.52)	(2.53)	(3.01)	(3.79)
Post × $\Delta Residuals_{d\to g}$	0.340***	0.200***	0.187**	0.106*	0.229***	0.158***
	(4.63)	(3.07)	(2.16)	(1.67)	(5.01)	(2.95)
Unemployment rate	-0.0264***	-0.0101***	-0.00343	-0.00439***	-0.00132	0.000301
	(-4.32)	(-4.26)	(-0.80)	(-2.90)	(-0.32)	(0.06)
Year × Industry FE	✓	✓	✓	✓	✓	✓
Court FE	✓	✓	✓	✓	✓	✓
Firm size	✓	✓	✓	✓	✓	✓
Observations	580,227	580,227	198,950	451,820	158,607	50,522
Adj. R^2	0.152	0.090	0.072	0.071	0.069	0.035

t statistics in parentheses

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Note: Table 11 presents the results of equation (8) with the measure for court behavior being $\Delta ResidualsY$. Standards errors are clustered at the pre-reform court level.

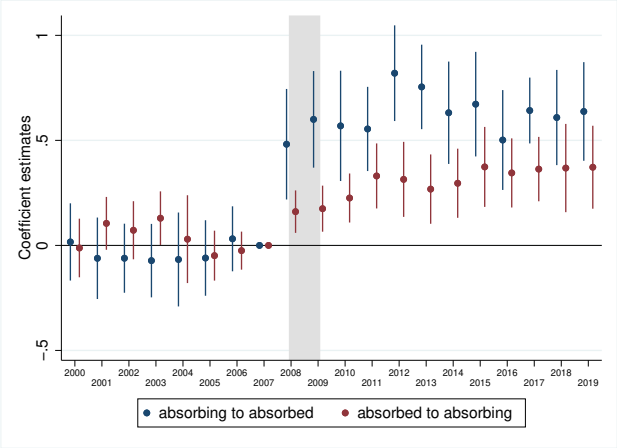
τ_{gt} (resp. and τ_{dt}) captures the proportion of the behavior of the absorbing (resp. absorbed) courts that has an impact on the firms in the absorbed (resp. absorbing) jurisdiction. After reform, we consider $\overline{\Delta ShareY_j}$, the pre-reform average behavior. The coefficient δ_{gt} (resp. and δ_{dt}) captures the proportion of the pre-reform behavior of the absorbing (resp. absorbed) court that has an impact on firms from the absorbed (resp. absorbing) jurisdiction.

In all panels of Figure 6, we see that before the reform, τ_{jdt} and τ_{jgt} are not significantly different from zero : for firms in absorbed and absorbing jurisdictions, the difference in behavior between the two courts had no impact on the bankruptcy outcomes. This result supports the parallel trends hypothesis.

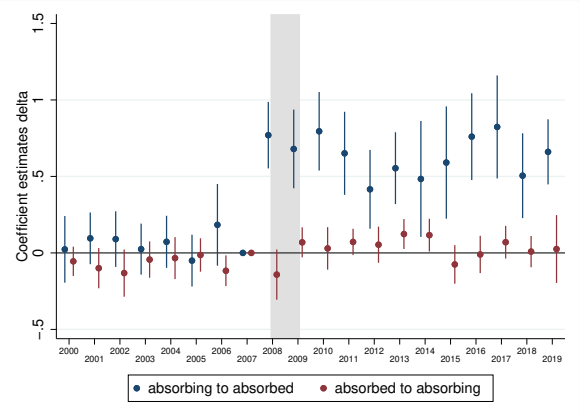
After the reform, the coefficients δ_{gt} and δ_{dt} suggest interesting sets of influences as mentioned above. The blue dots represent δ_{gt}, the influence of absorbing courts on firms from absorbed jurisdictions. For all three outcomes studied, the effect is largely positive and significantly different from zero : the behavior of absorbing courts is transmitted to firms. It explains between 50% and 75% of receivership and restructuring (panels (a) and (b), respectively), and up to 100% of firms' long-term survival (panel (c)). The effect is immediate and stable over time. Reciprocally, the red dots represent δ_{dt}, the influence of absorbed courts on firms from absorbing jurisdictions. Panel (a) shows that this influence is also significant : it explains around 25% of the entries into receivership for firms in the absorbing jurisdiction. Panels (b) and (c) imply that the behavior of the absorbed court explains, to a lesser extent, the restructuring and survival of firms after the reform. These results mean that the influence is reciprocal, even if the influence of the absorbing

FIGURE 6 – Transmission of courts' behavior

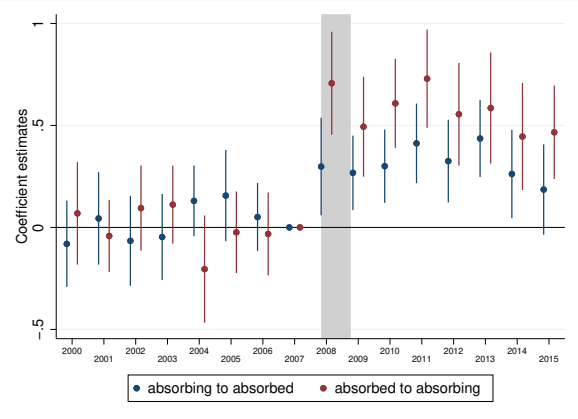
(a) Receivership



(b) Restructuring



(c) Survival



Note: Figure 6 shows the coefficients of equation (9) τ_{jg} and δ_{jg} in blue, and τ_{jd} and δ_{jd} in red. Panel (a), the dependent variable is the probability of entering receivership compared to direct liquidation, and $\Delta Share_{jt}$ is the difference in receivership rates between absorbing and absorbed jurisdiction. Panel (b), the dependent dependent variable is the probability of restructuring after receivership and $\Delta Share_{jt}$ the difference in restructuring rate between absorbing and absorbed jurisdiction. Panel (c), the dependent variable is the 7-year survival after filing and $\Delta Share_{jt}$ is the difference in the 7-year survival rates between absorbing and absorbed jurisdiction.
Reading : Panel (a) after the 2009 reform, the blue points represent the δ_{jg} coefficients. They are significantly positive and vary between 0.5 and 0.75. This means that 50% to 75% of the entry into receivership of firms from absorbed jurisdictions is explained by the pre-reform receivership rate of the court that absorbed them.

court is dominant and determines the aggregate effect of the reform.

To summarize, in this section, we show that the behavior of absorbing courts influences more the bankruptcy outcomes of firms in the catching area of the court they absorbed than the opposite. From a policy point of view, one may be tempted to conclude that, rather than the size of the commercial court, the absorbing court's quality matters for the success of a reform that merges courts.

7 Conclusion

For many reasons, there are suspicions that small commercial courts have a continuation bias : too often, they would allow small, non-viable firms to survive. This is detrimental to most of the firms' stakeholders : creditors (whose recovery rates are higher the faster the liquidation, see Blazy et al. (2018)), employees, and suppliers. One could also argue that damages associated with this continuation bias go beyond the fragile firms and their stakeholders and harm the economic dynamism of firms in these small jurisdictions. These damages can be all the greater when employment is held captive in non-viable firms at times when the labor market is tight. Despite the potentially large implications of the continuation bias, proving its existence is difficult. In the US, Morrison (2007) concludes that there is no such continuation bias for small firms.

In this paper, we take advantage of a reform implemented in 2008 that resulted in the absorption of 55 small commercial courts by larger ones (while keeping some other commercial courts unchanged). Because it was part of an extensive reform of the whole judicial map, commercial court efficiency played no role in deciding which court would be absorbed, absorbing or left unchanged. We show that small absorbed court had a continuation bias for small firms, which was reduced thanks to the reform. We also show that the reform resulted in better sorting at the onset of the bankruptcy procedure (less non-viable firms are allowed to engage in restructuring discussions with their creditors and are more often liquidated right away). This impact shows entirely on bankruptcy outcomes of firms whose court was absorbed. The reform also has no impact on bankruptcy outcomes for firms in districts with absorbing courts : risks that absorbing courts would not cope with their growth did not materialize. Finally, we show that this reduction in the continuation bias did not reduce the chances of fragile but viable firms to restructure and survive.

These results are robust but apply only to bankruptcy cases of small firms. When restricting the sample to micro-enterprises with more than 10 employees (and with turnover above €2 million), we cannot detect any impact of the reform on the restructuring chances of firms (viable or non-viable). In addition, we show that the behavior of absorbing courts influences more the bankruptcy outcomes of firms in the catching area of the court they absorbed than the opposite.

Our empirical analysis is conducted within a conceptual framework that allows a direct interpretation of the estimates of the impact of the reform (in a standard difference-in-difference strategy) in terms of Type 1 errors (restructuring a non-viable firm) and Type 2 errors (liquidating a viable firm). This is an important contribution to the literature. Notably, it complements the analysis of commercial court reforms based on their impact on procedure lengths.

In its annual rapport, the French Court of Auditors recommends further reorganizing the commercial court map, as the 2009 reform was insufficient (CdC (2015)). If this were to be the case, our results suggest that what matters might be not so much the court's final size, but the absorbing court's quality and that this criterion should orient future reorganization schemes.

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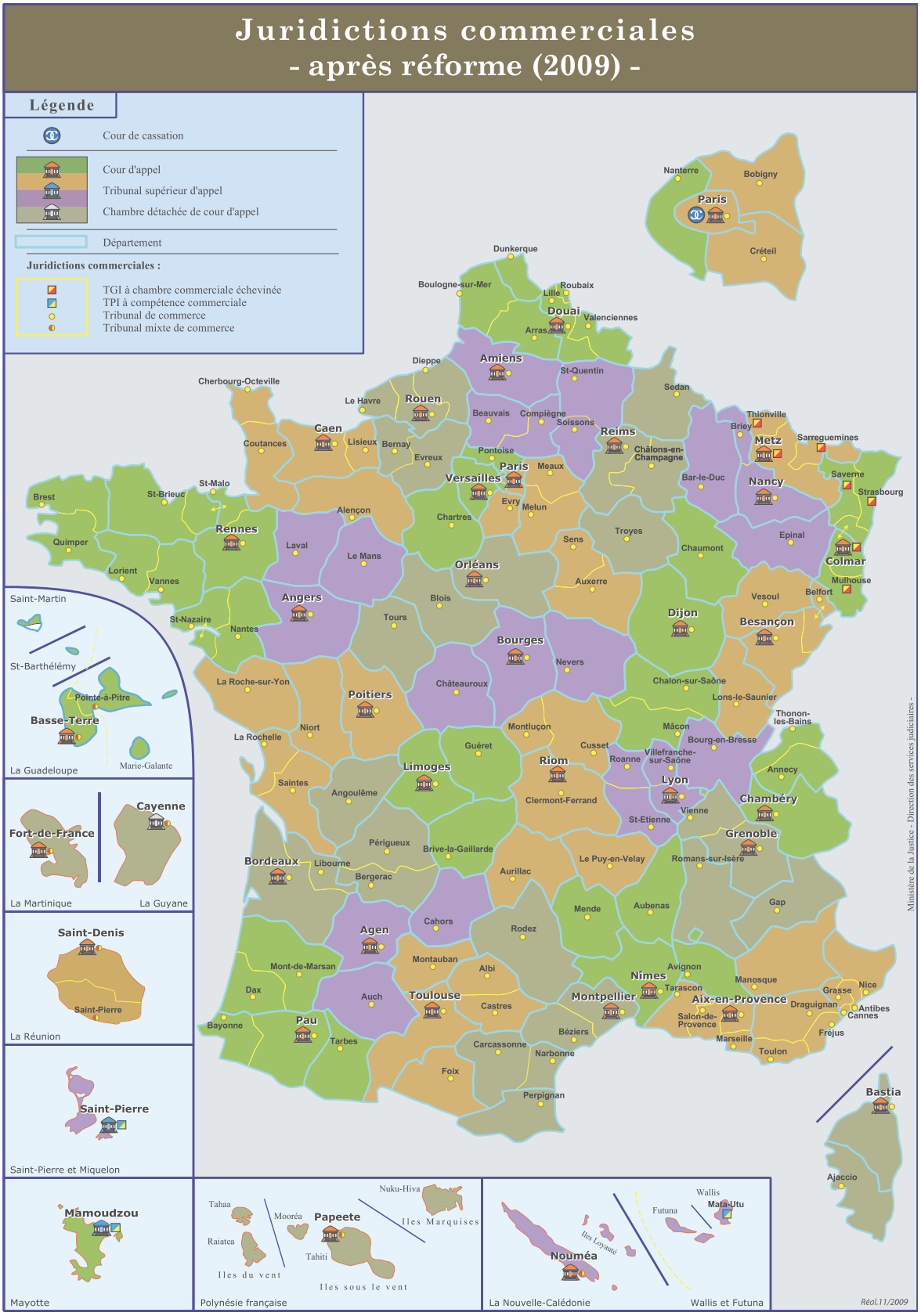
Appendix A Additional Materials and Robustness

FIGURE A.1 – French judicial map before reform

Source : French Ministry of Justice

FIGURE A.2 – French judicial map after reform



Source : French Ministry of Justice

TABLE A.1 – Additional summary statistics : average per jurisdiction

	Number of chambers											
	Before (2000-2007)						After (2008-2019)					
	N	Mean	Median	St. Dev.	Min	Max	N	Mean	Median	St. Dev.	Min	Max
Absorbed	50	1	1	0	1	2						
Absorbing	42	2	1	1	1	7						
New							42	4	3	1	2	8
Control	70	3	2	4	1	22	70	4	3	3	2	25
	Number of judges											
	Before (2000-2007)						After (2008-2019)					
	N	Mean	Median	St. Dev.	Min	Max	N	Mean	Median	St. Dev.	Min	Max
Absorbed	50	9	9	2	6	15						
Absorbing	42	16	13	9	7	45						
New							42	22	20	9	11	52
Control	70	23	15	23	8	172	70	25	16	24	10	173
	Judge caseload											
	Before (2000-2007)						After (2008-2019)					
	N	Mean	Median	St. Dev.	Min	Max	N	Mean	Median	St. Dev.	Min	Max
Absorbed	50	6.0	5.6	2.2	2.4	12.2						
Absorbing	42	8.8	8.3	3.6	3.4	19.8						
New							42	13.4	12.4	4.1	7.5	28.6
Control	70	11.3	10.7	4.3	3.9	23.6	70	13.5	12.7	4.5	5.5	24.8
	Distance to assigned court											
	Before (2000-2007)						After (2008-2019)					
	N	Mean	Median	St. Dev.	Min	Max	N	Mean	Median	St. Dev.	Min	Max
Absorbed	50	13.2	13.1	5.2	2.1	27.9						
Absorbing	42	11.8	10.9	5.8	2.0	27.2						
New							42	17.7	17.8	7.1	2.5	31.7
Control	70	16.0	15.6	7.3	1.5	32.1	70	16.1	16.4	7.3	1.4	34.3
	Days in receivership											
	Before (2000-2007)						After (2008-2019)					
	N	Mean	Median	St. Dev.	Min	Max	N	Mean	Median	St. Dev.	Min	Max
Absorbed	50	205	202	36	140	301						
Absorbing	42	199	202	30	153	257						
New							42	232	234	31	166	305
Control	70	191	186	36	118	295	70	227	227	31	151	315
	Days in receivership when liquidated											
	Before (2000-2007)						After (2008-2019)					
	N	Mean	Median	St. Dev.	Min	Max	N	Mean	Median	St. Dev.	Min	Max
Absorbed	50	140	143	31	73	216						
Absorbing	42	139	136	26	100	196						
New							42	159	159	25	108	227
Control	70	131	129	29	66	209	70	154	151	25	99	229
	Days in receivership when restructured											
	Before (2000-2007)						After (2008-2019)					
	N	Mean	Median	St. Dev.	Min	Max	N	Mean	Median	St. Dev.	Min	Max
Absorbed	50	345	329	59	256	534						
Absorbing	42	350	336	58	267	511						
New							42	421	412	46	360	525
Control	70	344	335	52	260	489	70	415	413	36	304	497

A.1 The reform's (absence of) impact on procedure length

In this section, we estimate the impact of the reform on the duration of procedures. In the paper, we have chosen not to focus on this efficiency measure because, as mentioned, the duration of procedures is strictly regulated by law in France. When a firm enters receivership, an observation period of six months begins, renewable twice for a maximum of 18 months. This observation period is relatively uniform across the country ; Table A.1 shows that, on average, the length of proceedings only varies by a few days between jurisdictions, and the standard deviation is relatively small.

We note that the receivership procedure is twice as long when it leads to a restructuring plan, compared with a judicial liquidation (Table A.1). This leads us to break down the length of the procedure according to the outcome of the proceedings. We estimate equation (4) where Y_{ijt} is the days spent in receivership for (1) the total sample, (2) firms that reach a restructuring plan, and (3) firms that end up liquidated. The coefficient β informs us about the impact of the reform.

Coefficients β Table A.2 are not significantly different from zero in any specification. They are also relatively small : the reform's effect would be limited to an increase of 12 calendar days for restructured firms out of an average of 345 days (Table A.1). These results, therefore, suggest little to no impact of the reform on procedure length.

Table A.2 – Reform's impact on procedure length

	Days in receivership		
	All	Restructured	Liquidated
	(1)	(2)	(3)
Absorbed × Post	7.644	12.82	5.225
	(1.44)	(1.45)	(1.14)
Absorbing × Post	3.358	8.342	0.229
	(0.59)	(0.97)	(0.05)
Unemployment rate	6.237**	7.606	4.652*
	(2.18)	(1.54)	(1.73)
Year × Industry FE	✓	✓	✓
Court FE	✓	✓	✓
Firm size	✓	✓	✓
Observations	198,950	59,150	139,792
Adj. R^2	0.102	0.286	0.091

t statistics in parentheses – * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Note: Table A.2 reports the result of equation (4) with as dependent variables the procedure length in days. The sample includes all firms that started a receivership procedure. By definition, there is no procedure length for direct liquidation. Column (1) includes all firms that started a receivership, column (2) includes only the sample of firms that obtained a restructuring plan, and column (3) firms that did not obtain a restructuring plan and were liquidated. Standards errors are clustered at the pre-reform court level.

A.2 Judges' caseload

The reform has had an impact on judges' workloads. As discussed before, the total number of judges has dropped in the jurisdictions affected by the reform. The number of bankruptcies, on the other hand, has increased (see Table 5). Mechanically, this means that the number of cases per judge has increased in the jurisdictions impacted by the reform. As reported in Table A.1, for firms in absorbed jurisdiction, the average caseload of judges doubled from a median of 6 cases per judge per year to over 12. A relatively smaller increase in judges' workload is also observed for firms in absorbing courts (from 8 to 12 cases per year) and control courts (from 11 to 13 cases per year).

The literature expects an impact of judges' workload on the outcome of judgments, even if the effect is difficult to predict (Iverson (2018), Müller (2022)). One possibility is that the increased workload slows the processing of cases, thus prolonging the time spent in bankruptcy. Another option would be to observe a more systematic, less in-depth processing of files, whose processing time would then be reduced. In both cases, the impact on the bankruptcy outcome is uncertain. Two scenarios can be expected. Busy judges may increase the acceptance rate of firms in receivership for fear of liquidating viable ones ; they would then take advantage of the observation period to gauge the firm's viability. Conversely, as there are more receivership cases, judges may accept fewer firms and liquidate them more systematically at the onset of the procedure.

In this section, we propose to include judges' workload as a control variable in our main specification (equation (4)). We also study the impact of judges' workload on procedure length. The results are reported in Table A.3.

First, we can see that the main coefficients of interest β are not affected by the introduction of the control variable. The procedure length is still unaffected by the reform, and all bankruptcy outcomes present the same sign as in the baseline specification.

We find that the variables correlated with judge caseloads are the probability of obtaining a restructuring plan (columns (3), top panel) and the associated procedure length (column (2), bottom panel). The increase in judges' workload by a standard deviation (5.3) increases the observation period of firms in receivership by around 10 days in cases where the firm reaches a restructuring plan. At the same time, it reduces the probability of obtaining a restructuring plan by 0.2 percent point. These results are significant at the 5% level and robust to the log of the number of cases per judge (unreported results).

The magnitude of these results needs to be put into perspective. The observation period is generally renewed when the firm obtains a restructuring plan and lasts, on average, one year. In this context, an increase of 10 days remains relatively small. Conversely, the 0.2 percent point drop represents a 0.4% reduction in the chance of obtaining a restructuring plan. In summary, although statistically different from zero, these correlations remain small. Here again, the reform's impact seems marginal.

TABLE A.3 – The impact of judges' caseload on bankruptcy outcomes and procedure length

Bankruptcy outcome	R_{ijt}	P_{ijt}		S_{ijt}		
Coefficient of interest	β_R	β_P	$\beta_{P\|R}$	β_S	$\beta_{S\|R}$	$\beta_{S\|P}$
	(1)	(2)	(3)	(4)	(5)	(6)
Absorbed × Post	-0.0687***	-0.0348***	-0.00895	-0.00207	0.0164*	0.0272*
	(-4.31)	(-5.43)	(-0.71)	(-0.52)	(1.95)	(1.66)
Absorbing × Post	-0.0238	-0.0106*	0.00285	-0.00233	-0.00155	0.00792
	(-1.56)	(-1.72)	(0.30)	(-0.74)	(-0.22)	(0.62)
Judges' caseload	0.00128	-0.000417	-0.00192**	-0.000170	-0.000822	0.000593
	(0.81)	(-1.02)	(-2.34)	(-0.49)	(-0.91)	(0.47)
Unemployment rate	-0.0272***	-0.00962***	-0.00182	-0.00415***	-0.00110	-0.000607
	(-4.00)	(-4.53)	(-0.45)	(-3.10)	(-0.31)	(-0.12)
Year × Industry FE	✓	✓	✓	✓	✓	✓
Court FE	✓	✓	✓	✓	✓	✓
Firm size	✓	✓	✓	✓	✓	✓
Observations	580,227	580,227	198,950	451,820	158,607	50,522
Adj. R^2	0.152	0.090	0.071	0.071	0.069	0.035

	Days in receivership		
	All	Restructured	Liquidated
	(1)	(2)	(3)
Absorbed × Post	6.168	5.996	3.653
	(1.13)	(0.69)	(0.79)
Absorbing × Post	3.219	6.995	0.161
	(0.57)	(0.83)	(0.04)
Judges' caseload	0.423	1.991**	0.451
	(0.86)	(2.30)	(1.29)
Unemployment rate	5.833**	5.798	4.207*
	(2.24)	(1.46)	(1.66)
Year × Industry FE	✓	✓	✓
Court FE	✓	✓	✓
Firm size	✓	✓	✓
Observations	198,950	59,150	139,792
Adj. R^2	0.102	0.288	0.091

t statistics in parentheses

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Note: Table A.3 reproduce the baseline specification (equation (4)) on firms' bankruptcy outcomes and procedure length with as an additional control variable the judges' caseloads. The judges' caseload variable is measured by the average number of cases per judge, per court, and per year. Because we have court fixed effects, we measure the impact of a change in judge caseload within courts. Standards errors are clustered at the pre-reform court level.

A.3 Robustness to the 2009 financial crisis

One might worry that the effects we measure are affected by the 2009 financial crisis. It might be the case if the characteristics of firms filing for bankruptcy are affected by the crisis differently across jurisdictions. According to Fougère et al. (2013), the proportion of firms filing for bankruptcy in France between 2008 and 2010 attributable to the crisis is not negligible. This has no impact on our identification strategy as long as all jurisdictions are impacted in the same way and the parallel trends assumption holds.

To answer this concern and as suggested by Iverson (2018), we directly examine how firm characteristics evolved around the reform in the different jurisdictions. We estimate the following regression :

$$X_i = \alpha + \sum_{k=g,d} \left(\sum_{t \neq 2007} \beta_{kt} (\mathbb{1}_t \times Reform_k) \right) + \theta_j + \theta_{s \times t} + \epsilon_i \tag{10}$$

We are interested in the coefficient β_{kt}, which significance informs us about the reform's impact on firms' characteristics X_i. Results are displayed in Figure A.3. They show mostly no correlation between firms' characteristics (firm size measured with the log of its total assets, leverage and debt composition [30]) and the reform.

We propose the same test on the sectoral composition of each jurisdiction. We create dummies equal to 1 if the firm is in the service sector for example, 0 otherwise. We consider four sectors : construction, services, trade, and other (agriculture, manufacturing, and transport). The results are also reported in Figure A.3. [31] The β coefficients are overall not significantly different from zero, although some trends suggest that the sectoral composition of jurisdictions may have varied over time (in the services sector, for example, where β is relatively positive in absorbed jurisdictions compared to control jurisdictions). To account for any differences along these dimensions, we include in all the specifications of our paper firms characteristics and Industry $\times$ Year fixed effects.

Our results would also be biased if, because of the financial crisis, economic conditions in different jurisdictions varied differently from one another after reform in a way that altered the outcomes of bankruptcies in those jurisdictions, "either because the firms themselves are different or because judges treat them differently because economic conditions have changed" (Iverson (2018)). We test for a significant correlation between the unemployment rate and the reform by using the unemployment rate as a dependent variable in regression similar to equation (10). Compared to control courts, the coefficients β_t are rather negative before reform and positive afterward in both absorbed and absorbing jurisdictions. We include the unemployment rate in all our main regressions to control for this heterogeneity between jurisdictions.

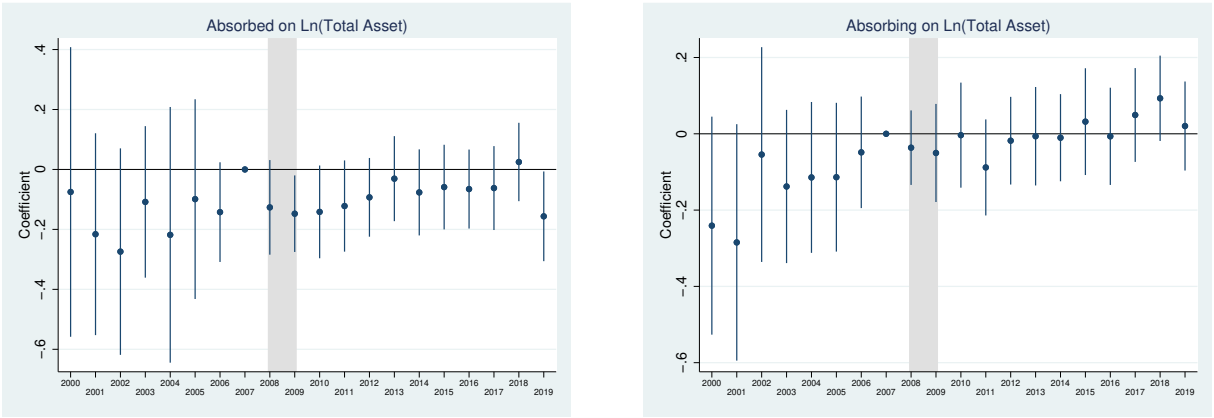
30. Such financial information is only available for about 8% of our full sample via FIBEN. It concerns mainly bigger firms and might therefore not be fully representative of the full sample.

31. Regressions on industry do not include industry $\times$ year fixed effects, but only year fixed effects.

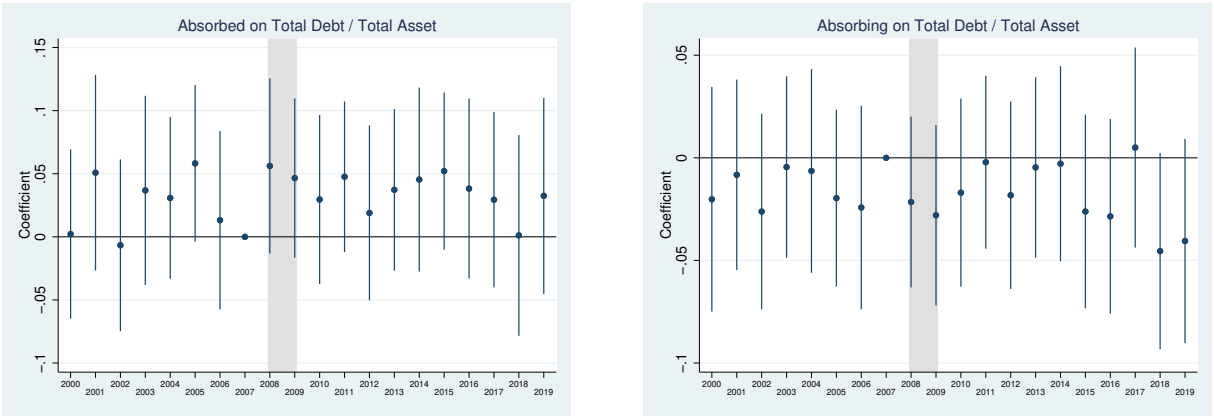
FIGURE A.3 – Dependent variables pre-trends

In order for the difference-in-differences regressions to detect a causal effect of the reform, the parallel trends assumption must hold. We test this assumption in Figure A.3 by running equation (10). Figure A.3 plots the coefficients β and their 95% confidence intervals over time. The parallel trends assumption is satisfied if the coefficients are stable before the reform, indicating that it was not changing differently by type of jurisdiction prior to the reform shock.

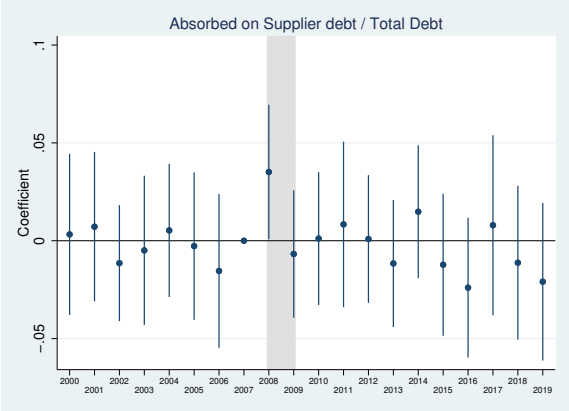
Ln(Total Assets)

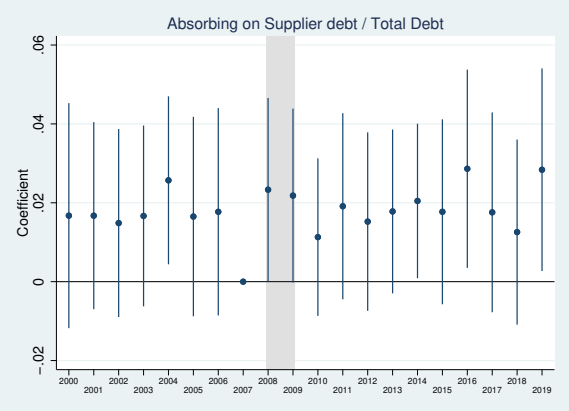


Total Debt / Total Asset

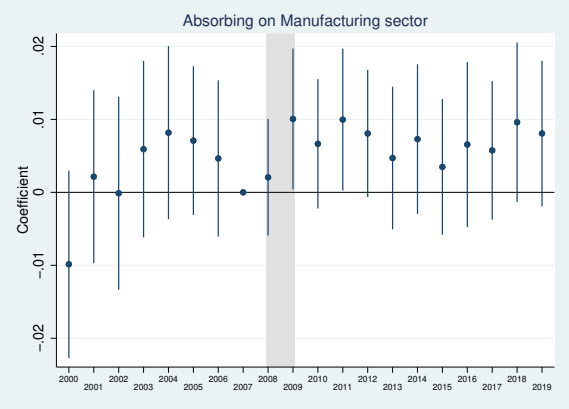


Supplier Debt / Total Debt

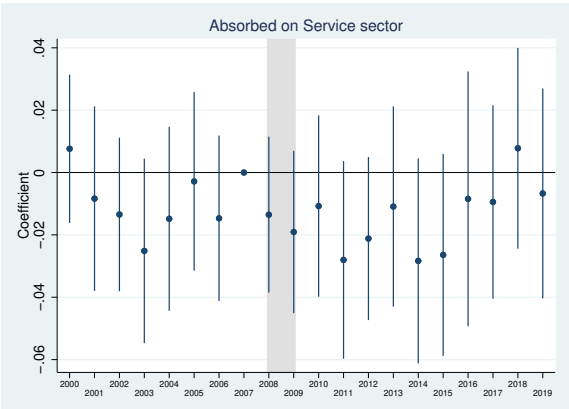


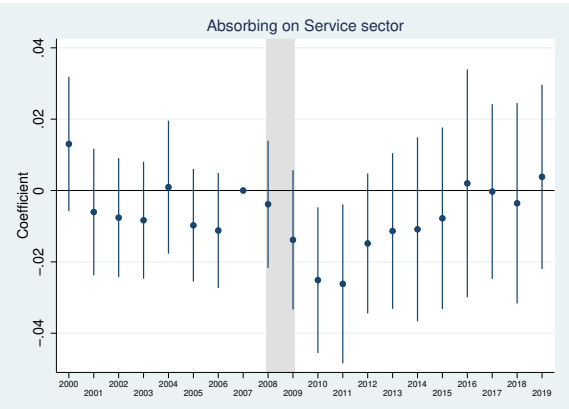


Manufacturing sector

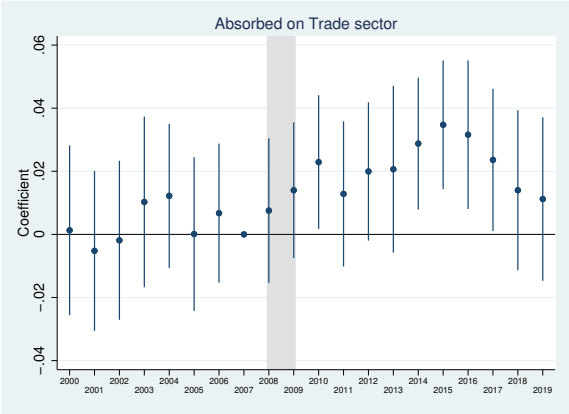


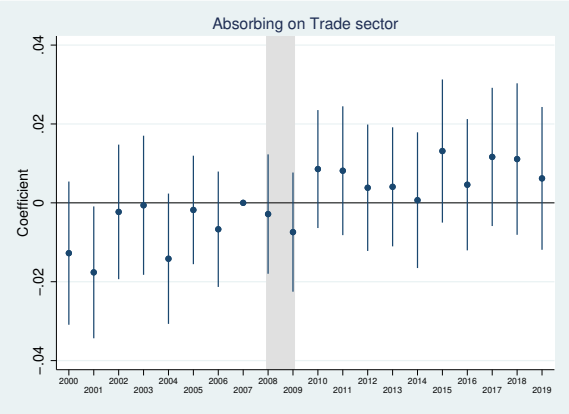
Service sector



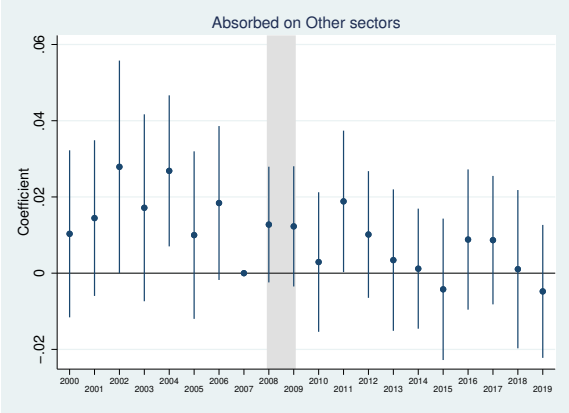


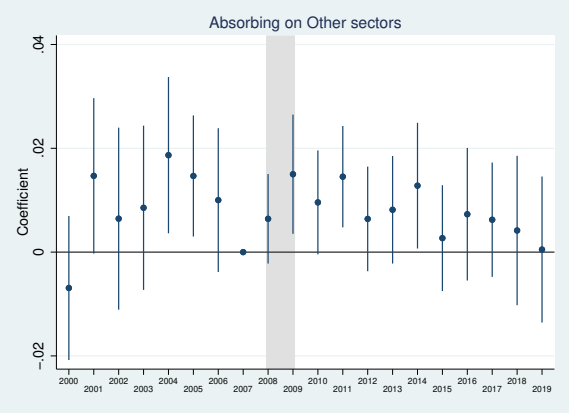
Trade sector



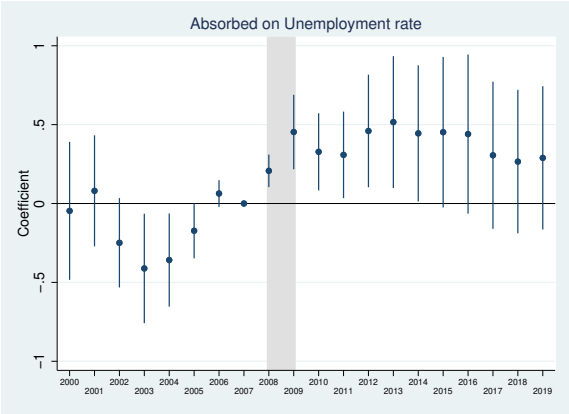


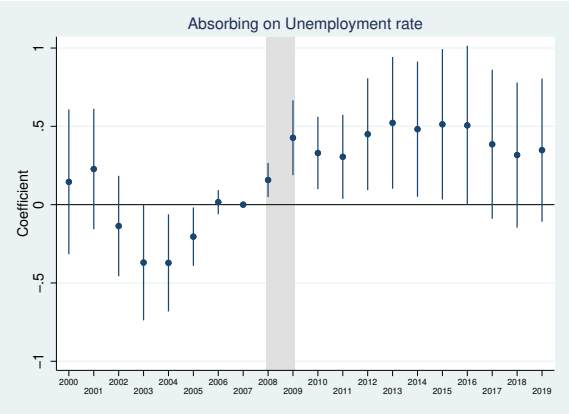
Other sectors





Unemployment rate





Appendix B Conceptual Framework : Derivation of results

Starting from the six initial equations :

$$\begin{cases} R_j = x_j R_j^h + (1 - x_j) R_j^l \\ P_j = x_j R_j^h P_j^h + (1 - x_j) R_j^l P_j^l \\ P_j^R = \dfrac{x_j R_j^h P_j^h + (1 - x_j) R^l P^l}{R_j} = \dfrac{P_j}{R_j} \\ S_j = s_j \; x_j R_j^h P_j^h \\ S_j^R = \dfrac{S_j}{R_j} \\ S_j^P = \dfrac{S_j}{P_j} \end{cases} \tag{11}$$

We want to recover the impact of the reform on R_j^l and R_j^h that inform us on the impact of the reform on T_{1R} and T_{2R}, and $R_j^l P_j^l$ and $R_j^h P_j^h$ that inform us on the impact of the reform on T_1 and T_2.

Noting ΔZ the impact of the reform on variable Z, we can write the following system under the the assumptions that $\Delta x_j = 0$ and $\Delta s_j^h = 0$.

$$\begin{cases} \Delta R_j = x_j \Delta R_j^h + (1 - x_j) \Delta R_j^l \\ \Delta P_j = x_j (\Delta R_j^h P_j^h + \Delta P_j^h R_j^h) + (1 - x_j)(\Delta R_j^l P_j^l + \Delta P_j^l R_j^l) \\ \Delta P_j^R = \dfrac{\Delta P_j R_j - \Delta R_j P_j}{(R_j)^2} \\ \Delta S_j = s_j \; x_j \; (\Delta R_j^h P_j^h + \Delta P_j^h R_j^h) \\ \Delta S_j^R = \dfrac{\Delta S_j R_j - \Delta R_j S_j}{(R_j)^2} \\ \Delta S_j^P = \dfrac{\Delta S_j P_j - \Delta P_j S_j}{(P_j)^2} \end{cases} \tag{12}$$

From the six empirical equations we can measure the impact of the reform, noting that :

$$\begin{cases} \Delta R_j \equiv \beta_R \\ \Delta P_j \equiv \beta_P \\ \Delta P_j^R \equiv \beta_{P|R} \\ \Delta S_j \equiv \beta_S \\ \Delta S_j^R \equiv \beta_{S|R} \\ \Delta S_j^P \equiv \beta_{S|P}. \end{cases} \tag{13}$$

From that, and with the expression of the overall Type 2 error, one can recover the impact of the reform on Type 2 error :

$$T_2 = 1 - R_j^h P_j^h \;\Rightarrow \Delta T_2 = -\Delta R_j^h P_j^h - R_j^h \Delta P_j^h \equiv -\frac{\beta_S}{(x_j s_j^h)} \tag{14}$$

The impact of the reform on T_2 is of opposite sign of β_S. This means that if the rate of survival of firms after filing is positively affected by the reform ($\beta_S > 0$), the reform reduces Type 2 error (less viable firms are liquidated). When $\beta_S < 0$, one can conclude the opposite : more viable firms are liquidated and Type 2 error is higher because of the reform.

Combining this first result with the sign of β_P helps recover the impact of the reform on the overall Type 1 error (restructuring a non-viable firm).

$$T_1 = R_j^l P_j^l \;\Rightarrow \Delta T_1 = \Delta R_j^l P_j^l + \Delta P_j^l R_j^l \tag{15}$$

It is straightforward to see that when $\beta_P < 0$ (i.e., $x_j(\Delta R_j^h P_j^h + \Delta P_j^h R_j^h) + (1 - x_j)(\Delta R_j^l P_j^l + \Delta P_j^l R_j^l) < 0$) and $\beta_S > 0$ (i.e., $\Delta R_j^h P_j^h > 0$) then $\Delta T_1 < 0$ (less non-viable firms are restructured), and when $\beta_P > 0$ and $\beta_S < 0$ then $\Delta T_1 > 0$ (more non-viable firms are restructured).

These first results are reported in the matrix below (Table 1 in the paper).

TABLE B.4 – Type 1 and Type 2 errors matrix

		β_P		
		> 0	$= 0$	< 0
β_S	> 0	$?T_1 \downarrow T_2$	$\downarrow T_1 \downarrow T_2$	$\downarrow T_1 \downarrow T_2$
	$= 0$	$\uparrow T_1 = T_2$	$= T_1 = T_2$	$\downarrow T_1 = T_2$
	< 0	$\uparrow T_1 \uparrow T_2$	$\uparrow T_1 \uparrow T_2$	$?T_1 \uparrow T_2$

Using equations for ΔP_j^R, ΔS_j^R and ΔS_j^P, and noting that $R_j > 0$, $P_j > 0$ and $S_j > 0$ one can derive the robustness matrix below (Table 3 in the paper) that gives the expected signs for $\beta_{S|R} > 0$, $\beta_{S|P} > 0$ and $\beta_{P|R} > 0$ in function of the signs of β_S, β_P and β_R.

TABLE B.5 – Robustness matrix

		β_R			β_P		
		> 0	$= 0$	< 0	> 0	$= 0$	< 0
β_S	> 0	?	$\beta_{S\|R} > 0$	$\beta_{S\|R} > 0$	?	$\beta_{S\|P} > 0$	$\beta_{S\|P} > 0$
	$= 0$	$\beta_{S\|R} < 0$	$\beta_{S\|R} = 0$	$\beta_{S\|R} > 0$	$\beta_{S\|P} < 0$	$\beta_{S\|P} = 0$	$\beta_{S\|P} > 0$
	< 0	$\beta_{S\|R} < 0$	$\beta_{S\|R} < 0$	?	$\beta_{S\|P} < 0$	$\beta_{S\|P} < 0$	?
β_P	> 0	?	$\beta_{P\|R} > 0$	$\beta_{P\|R} > 0$			
	$= 0$	$\beta_{P\|R} < 0$	$\beta_{P\|R} = 0$	$\beta_{P\|R} > 0$			
	< 0	$\beta_{P\|R} < 0$	$\beta_{P\|R} < 0$	?			

Appendix C Measuring Courts' Behavior

This appendix details the measures of court behavior, $\Delta ShareY$ and $\Delta ResidualsY$. The purpose is to measure the difference in bankruptcy outcomes between the absorbing court and its absorbed court(s), and vice versa. The variables are constructed for each of the six outcomes Y_{ijt} : the probability of receivership, the probability of restructuring after filing, the probability of restructuring after receivership, the survival 7 years after filing, the survival 7 years after receivership, and the survival 5 years after restructuring.

Formally, for $t \leq 2007$, for each year and for each court, we construct $\Delta ShareY$ as follow :

$$ShareY_{jt} = \frac{\sum_i Y_{ijt}}{\#cases_{jt}}$$

with $\#cases_{jt}$ being the number of filings (receiverships + direct liquidations), of receiverships, or of restructurings, depending on the outcome.
For absorbed jurisdictions (indexed by d), we take the difference between the absorbing court and its absorbed court :

$$\Delta ShareY_{jdt} = ShareY_{gt} - ShareY_{dt}$$

We follow the same logic for absorbing jurisdiction (indexed by d) :

$$\Delta ShareY_{jgt} = ShareY_{dt} - ShareY_{gt}$$

For $t > 2007$ and for $k = g, d$, we take the average over the pre-reform period :

$$\overline{\Delta ShareY_{jk}} = \frac{1}{8} \sum_{t=2000}^{2007} \Delta ShareY_{jkt}$$

Next, we measure $\Delta ResidualsY$ following the same logic. Here, in addition to capturing average court behavior, we also control for the composition of firms that file for bankruptcy in each court. We include controls for firm size, industry, and local unemployment rate. For $t \leq 2007$ we estimates :

$$Y_{ijt} = \alpha + \beta(Reform_j \times Post) + u_j + \gamma X_i + \theta_{s \times t} + \epsilon_{ijt} \tag{16}$$

where Y_{ijt} is defined for firm i in court j at time t for the six bankruptcy outcomes. This estimation is the same as equation (4) but without the court fixed effects, and estimated on the pre-reform period (2000-2007). From this and for each of the outcomes, we recover the residuals ϵ_{jt}, averaged per court j and year t, e.g., we recover for each court what is not explained by firm characteristics or local economic conditions. We then calculate the difference each year for $t \leq 2007$:

$$\Delta ResidualsY_{jdt} = \epsilon Y_{gt} - \epsilon Y_{dt}$$

In the same way for absorbing jurisdictions :

$$\Delta ResidualsY_{jgt} = \epsilon Y_{dt} - \epsilon Y_{gt}$$

For $t > 2007$ and $k = g, d$, we take the average over the pre-reform period :

$$\overline{\Delta ResidualsY_{jk}} = \frac{1}{8} \sum_{t=2000}^{2007} \Delta ResidualsY_{jkt}$$

Tables C.1 summarizes the measures. The first row of each panel $g \to d$ reports the measure of the impact of absorbing court's behavior on absorbed court. The second row of each panel $d \to g$ reports the influence of absorbed court on absorbing courts. By construction, they are of opposite sign. $\Delta Share_{jd}$ and $\Delta Residuals_{jd}$ are always equal to 0 for control courts. We observe that, when controlled for the characteristics of the firms entering the proceedings, the average is closer to 0. We use these measures in Section 6 of the paper.

TABLE C.1 – Summary of $\Delta ShareY$ and $\Delta ResidualsY$

		Receivership									
		$\Delta Share_{jt}$ (2000-2007)					$\Delta Residuals_{jt}$ (2000-2007)				
	N	Mean	Median	St. Dev.	Min	Max	Mean	Median	St. Dev.	Min	Max
$g \to d$	50	-0.047	-0.035	0.101	-0.342	0.153	-0.032	-0.021	0.090	-0.278	0.177
$d \to g$	42	0.043	0.040	0.101	-0.155	0.340	0.031	0.029	0.091	-0.176	0.267
		Restructuring after filing									
		$\Delta Share_{jt}$ (2000-2007)					$\Delta Residuals_{jt}$ (2000-2007)				
	N	Mean	Median	St. Dev.	Min	Max	Mean	Median	St. Dev.	Min	Max
$g \to d$	50	-0.027	-0.020	0.041	-0.129	0.042	-0.032	-0.031	0.066	-0.223	0.088
$d \to g$	42	0.022	0.018	0.038	-0.045	0.110	0.029	0.043	0.063	-0.082	0.166
		Restructuring after receivership									
		$\Delta Share_{jt}$ (2000-2007)					$\Delta Residuals_{jt}$ (2000-2007)				
	N	Mean	Median	St. Dev.	Min	Max	Mean	Median	St. Dev.	Min	Max
$g \to d$	50	-0.034	-0.023	0.086	-0.316	0.167	-0.025	-0.027	0.083	-0.253	0.144
$d \to g$	42	0.028	0.019	0.086	-0.167	0.321	0.019	0.017	0.073	-0.130	0.191
		Survival 7 years after filing									
		$\Delta Share_{jt}$ (2000-2007)					$\Delta Residuals_{jt}$ (2000-2007)				
	N	Mean	Median	St. Dev.	Min	Max	Mean	Median	St. Dev.	Min	Max
$g \to d$	50	-0.012	-0.009	0.023	-0.076	0.037	-0.010	-0.009	0.048	-0.130	0.094
$d \to g$	42	0.010	0.009	0.021	-0.032	0.071	0.004	0.002	0.045	-0.080	0.114
		Survival 7 years after receivership									
		$\Delta Share_{jt}$ (2000-2007)					$\Delta Residuals_{jt}$ (2000-2007)				
	N	Mean	Median	St. Dev.	Min	Max	Mean	Median	St. Dev.	Min	Max
$g \to d$	50	-0.011	-0.004	0.058	-0.182	0.121	-0.004	0.004	0.069	-0.137	0.200
$d \to g$	42	0.006	0.005	0.056	-0.110	0.178	-0.004	-0.005	0.064	-0.199	0.125
		Survival 5 years after restructuring									
		$\Delta Share_{jt}$ (2000-2007)					$\Delta Residuals_{jt}$ (2000-2007)				
	N	Mean	Median	St. Dev.	Min	Max	Mean	Median	St. Dev.	Min	Max
$g \to d$	50	-0.013	-0.025	0.113	-0.311	0.295	-0.010	-0.024	0.141	-0.320	0.394
$d \to g$	42	0.003	0.001	0.104	-0.288	0.174	0.003	0.031	0.141	-0.406	0.321