Does Merging Small Bankruptcy Courts Increase Their Efficiency?

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ABSTRACT

We estimate the impact of a 2009 reform that merged 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 errors (restructuring a non-viable firm) and Type 2 errors (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 court behavior is determined more by that of the absorbing court than by that of the absorbed one.

Keywords: Corporate Bankruptcy, Commercial Courts.

JEL classification: G33, K22

NON-TECHNICAL SUMMARY

The modernization of judicial systems in European countries often involves merging courts. This was the case in Italy in 2013, France in 2009, and the Netherlands in 2008. Between 2010 and 2016, 10 European countries reduced the number of courts by 15% or more. Court mergers are intended to reduce costs, eliminate smaller and often less efficient entities, and break with inefficient local habits. However, they may also increase the distance to justice and risk overloading absorbing courts, at least in the short term.

What is the impact of court mergers on efficiency? This paper focuses on French bankruptcy courts and offers a novel approach to judicial efficiency—not measured by case speed, as often done, but by the quality of decisions. In bankruptcy proceedings, courts must decide whether a firm should be restructured or liquidated. They face two possible errors: restructuring a firm that cannot be saved (a "Type 1 error") or liquidating one that could have recovered (a "Type 2 error"). We treat courts as decision-makers that "screen" insolvent firms and examine whether the 2009 reform improved this screening ability.

To measure efficiency, we apply a difference-in-differences strategy, comparing bankruptcy outcomes of firms affected by the reform with those that were not. The analysis relies on an (almost) exhaustive dataset of 600,000 bankruptcy cases in France between 2000 and 2019. We study outcomes such as the probability of receivership, restructuring, and firm survival.

The results show that the reform reduced continuation bias (fewer Type 1 errors) in absorbed courts, while having no effect on the survival of viable firms (unchanged Type 2 errors). We show that at least part of the reduction in the continuation bias comes from a lower probability of putting non-viable firms to receivership. Figure below illustrates this result: the probability of receivership for firms from absorbed courts falls sharply after the reform.

We also find that the reform did not affect outcomes in absorbing courts, despite concerns at the time that these courts would struggle with heavier caseloads. In fact, the behavior of absorbing courts influenced the absorbed courts more than the reverse. Figure below further shows that the reform's effect was immediate and lasting, reflecting a break with local habits. From a policy perspective, this suggests that the effectiveness of absorbing courts, rather than their size, is key to the success of such reforms.

To summarize, the reform reduced continuation bias by spreading the better practices of absorbing courts, without increasing liquidation bias. Concerns about reduced access to justice or congested courts were not borne out. Overall, the reform's impact was positive.

The reform's impact on the probability of being put into receivership for firms in absorbed courts
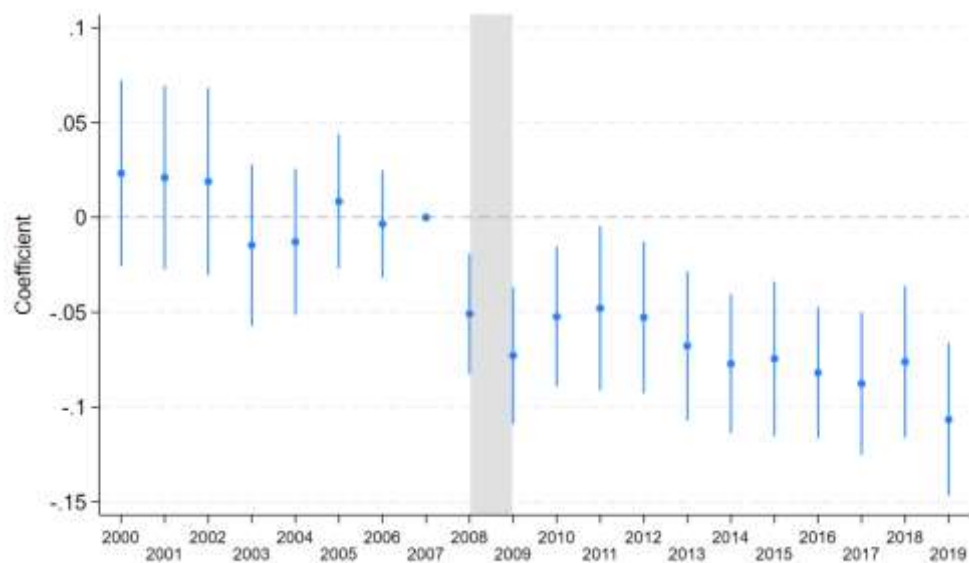
Note: This figure plots the reform's impact on the probability of entering receivership (instead of direct liquidation) in absorbed jurisdictions.

La fusion des petits tribunaux de commerce améliore-t-elle leur efficacité ?

RÉSUMÉ

Nous estimons l'impact d'une réforme de 2009 fusionnant les petits tribunaux de commerce sur la qualité de leurs décisions. Un cadre conceptuel nous permet de relier les estimations de différence en différence à l'impact de la réforme sur l'erreur de type 1 (restructuration d'une entreprise non viable) et l'erreur de type 2 (liquidation d'une entreprise viable). Nous appliquons ce cadre à un échantillon (presque) exhaustif de 600 000 cas de faillite en France qui ont débuté entre 2000 et 2019. La réforme réduit sans ambiguïté les erreurs de type 1 alors qu'elle n'a pas d'impact sur les erreurs de type 2. Le comportement des tribunaux post-fusion dépend plus de celui du tribunal absorbant que de celui du tribunal absorbé.

Mots-clés : défaillance d'entreprises, tribunaux de commerce.

1   Introduction

The reorganization and modernization of judicial systems in European countries often involves merging courts.[1] This was the case in Italy in 2013, France in 2008-2009, and the Netherlands in 2008. The European Commission for the Efficiency of Justice (CEPEJ (2018)) counts that between 2010 and 2016, 10 European countries reduced the number of courts by 15% or more. Several reasons are put forward for merging courts. It is intended to reduce operating costs, eliminate smaller entities – often deemed inefficient and prone to continuation bias – and help break bad local habits, where necessary. There are drawbacks to merging courts as well. By increasing the distance to courts, it could reduce access to justice (Chappe and Obidzinski (2014)), and the rapid growth of the absorbing courts could lead to congestion, at least in the short term.

A few empirical studies analyze the impact of these mergers on court efficiency.[2] In most of them, court efficiency is measured either by the speed at which the court processes[3] or by access to justice. We depart from this approach. Looking at bankruptcy courts, we measure the gain in efficiency by the change in the quality of the their rulings, an approach also considered by Iverson (2018), and Giné and Love (2010). More precisely, following White (1994), we consider bankruptcy courts to be screening devices and define a Type 1 error as restructuring a non-viable firm and a Type 2 error as liquidating a viable firm. In this context, the Type 1 error is equivalent to continuation bias, while the Type 2 error corresponds to liquidation bias.

Our first contribution is to develop a conceptual framework that shows that the reform's impact on Type 1 and Type 2 errors 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 errors 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 capture : it depends on both the survival and the restructuring rates of firms that enter a bankruptcy procedure. In addition, our conceptual framework provides a means of testing the robustness of the econometric results. This test is 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 (30% of courts) were absorbed.[4] We use an (almost) exhaustive sample of the 600,000 bankruptcy cases in France started during the period

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1. Inherited from a time when travel was difficult, the judicial maps of European countries are dense compared with those of more recent countries such as the United States, Canada or Australia.

2. See Espinosa et al. (2017), Esquerré (2019), Arcuri et al. (2023), Pezone (2023), Belarouci et al. (2024).

3. Or similar indicators such as the clearance rate or backlog rate.

4. We discuss the exogeneity of the reform to the ex-ante outcomes of absorbed and absorbing courts. We also discuss the potential impact of the 2009 financial crisis.

2000-2019 and estimate a set of six standard difference-in-difference equations. Firms in jurisdictions not impacted by the reform serve as our control group. Our five main results are as follows : (i) the reform unambiguously reduces Type 1 errors (continuation bias) and did not impact Type 2 errors (liquidation bias). (ii) The reform's impact appears entirely in the bankruptcy outcomes of firms whose court was absorbed. (iii) The behavior of absorbing courts influences the bankruptcy outcomes of firms in the jurisdiction of the court they absorbed more than the other way round. (iv) The speed at which the impact appears is consistent with a break with bad local habits. (v) The fear that absorbing courts would not cope with their growth was not borne out. To summarize, the reform reduced the overall continuation bias by spreading the somehow better practices of absorbing courts. It did not increase the liquidation bias, where there was any. Fears of reduced access to justice and crowded courts were not borne out.

The rest of the paper is organized as follows. Section 2 presents the 2009 reform of the judicial map. Section 3 sets out 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 sets out the main results of the reform's impact, and robustness tests. Section 6 provides evidence regarding the underlying mechanisms. 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[5] (that we will refer to as the "bankruptcy judge"), but decisions are taken by consensus amongst the judges in the chamber.

When a firm becomes insolvent[6] and without an informal agreement with one or more creditors, it must file for bankruptcy[7] with its assigned court based on the location of its head office, so forum shopping is ruled out by law.[8] 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 firm has no apparent chance of pursuing a viable activity. Conversely, if the firm is considered potentially viable,

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5. In our database, we do not have judges' identities and gathering such nominative data on judges in illegal in France.

6. i.e. its available liquid assets do not cover its short-term debts.

7. Creditors can also bring the case to the commercial court to trigger the bankruptcy filing.

8. In addition, Epaulard and Zapha (2022) show that, over the period 2010-2016, firms do not move their head office in the five years preceding bankruptcy filing.

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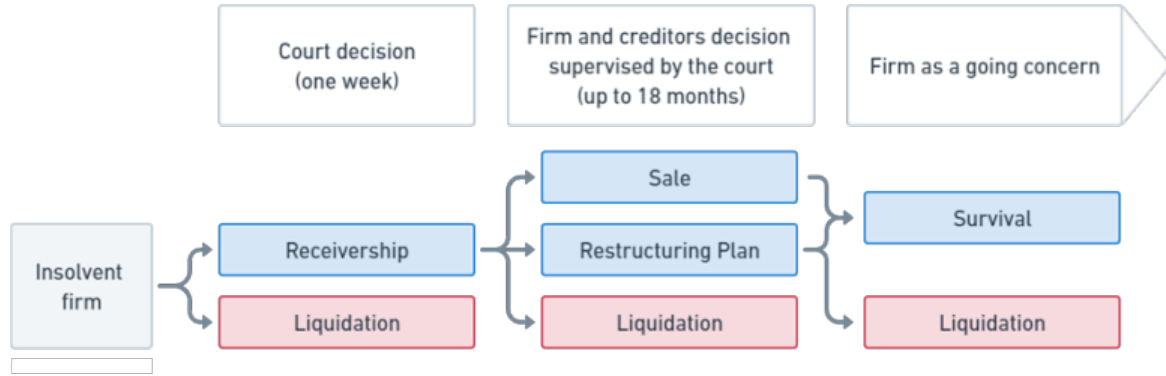
Note: Figure 1 summarises the main stages of the bankruptcy process for insolvent firms in France. In our analysis, we consider the outcome "sale" as if the firm obtains a restructuring plan. It concerns only 3% of firms after receivership and their exclusion does not modify the results and their interpretation.

receivership is chosen in an attempt to reorganize the business and the debt. In this case, an observation period of six months is triggered, which is 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. The creditors are brought together in a creditors' committee for firms exceeding a certain threshold (over 150 employees, with turnover over €20 million). The final decision rests with the court. In practice, even if a debt-restructuring plan is approved, 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 [9] 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 (with 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

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9. With the exception of the Moselle, Haut-Rhin, and Bas-Rhin *départements*, which operate differently for historical reasons.

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. However, the selection mechanism might create a closed circle of influence in smaller jurisdictions. 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 drawn up in 1958 had been criticized for its obsolescence, it had not undergone any substantial changes before 2009. The 2009 reform was ambitious and concerned the entire French judicial map and not only the organization of commercial courts. In fact, 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)) about the effect of the reform of the judicial map barely mention the impact it had on commercial courts and rather focused on the impact on civil and high courts. In 2015, the French Court of Auditors simply regretted that the reform had insufficiently reorganized commercial courts.[10]

The reform aimed to rationalize and adapt the judicial organization to take account of demographic dynamics.[11] One of the first objectives of the reform was thus to better distribute judicial resources across the country. The bulk of the reform consisted in choosing the civil and high courts that would be closed.[12] In most cases, the decision regarding these civil and high courts also applied to local commercial courts.[13] The criteria used to decide 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 priority given to commercial courts in the selection process is linked to their relative low importance in the French judicial system : commercial courts represent only 15% of all French courts[14] and about 3% of the civil justice budget.[15]

In cases where commercial courts were closed regardless of the decision made regar-

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10. "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)

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

12. The choices were widely criticized (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 commercial courts has been rather well received (CdC (2015))

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

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

15. See https://www.budget.gouv.fr/.

ding 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.[16]

In total, the reform closed 55 commercial courts[17] and created five others.[18] Discussed from 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 in the online appendix report the closure and creation of commercial courts. Section 4 discusses a large set of descriptive statistics on the reform's impact on courts' activity.

To further assess whether the selection of absorbing and absorbed courts was influenced by their propensity to initiate receivership proceedings, we conduct two empirical tests. The first examines the determinants of being an absorbed court; the second focuses on the determinants of being an absorbing court. To account for the constraint that each *département* must retain at least one commercial court, we proceed as follows. For each *département*, we identify : (i) the largest commercial court (based on the average annual number of insolvency cases prior to the reform), (ii) the smallest court, (iii) the court with the highest propensity to grant judicial reorganization (measured by the reorganization-to-insolvency ratio), (iv) the court of appeal, and (v) the court located in the prefecture. These criteria give us five binary indicators for each commercial court.

We then regress the probability of being an absorbed court on these five indicators, first separately, and then jointly. The results are reported in Table 7, Panel A. Among the 112 courts in *départements* affected by the reform, the strongest predictor of being absorbed is being the smallest court in the *département*. This single indicator yields the highest R^2 (0.610) across all specifications. When all the indicators are included simultaneously, only the "smallest court" variable remains statistically significant ($R^2 = 0.653$). Importantly, the court's propensity to put firms into receivership (rather than direct liquidation) is not a significant predictor. We replicate the analysis for absorbing courts, with the results presented in Table 7, Panel B. Once again, court size is the dominant factor : being the

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16. 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 considerations in choosing which court to keep.

17. We exclude the particular case of Bernay/Pont-Audemer. Bernay was created and Pont-Audemer closed without any change to the catchment area. We thus retain 54 absorbed courts and four newly-created ones in our analysis.

18. In some *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 courts 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.

largest court in the *département* is the most robust predictor of absorption, and not being the smallest court the only significant variable in the full specification. As before, the court's leniency – measured by its reorganization rate – is not significant.

3   Measuring bankruptcy court efficiency

Most empirical papers aiming to explain the efficiency of commercial courts and its impact on firms (their size, access to bank loans, etc.) measure efficiency by the speed at which the court processes cases or some similar indicators such as the clearance rate or backlog rate.[19] 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 length of procedure has a large, positive effect on firm employment. Similarly, Müller (2022) shows that, in the United States, a reduction in court congestion increases firms' leverage. However, these efficiency measures may not be helpful in the case 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, a few recent papers try to measure the impact of reforms affecting commercial courts on the quality of their ruling. This is the case of Iverson (2018), who tests the impact of an exogenous drop in caseload in bankruptcy courts on recidivism. Giné and Love (2010) analyse the impact of bankruptcy reform in Colombia on efficiency by studying its effect not only on the duration of reorganization, but also on the survival of the company after reorganization. Antill (2022) tries to measure whether the right firms survive bankruptcy. Our approach is in the same vein, but we propose a more formal framework to measure improvements 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 one that separates firms that are economically viable from those that are not early on in the bankruptcy process, and ensures that the firms that reorganize are those with a good chance of surviving with the help of well-designed restructuring plans.[20]

We develop a simple conceptual framework and a set of empirical tests based on

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19. The 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.

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

information available to the statistician that together allow us to capture the impact of the reform on Type 1 errors (restructuring a non-viable firm) and Type 2 errors (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 to capture the reform's impact on Type 1 and Type 2 errors.

3.1 Conceptual framework

3.1.1 Notation and simple arithmetic

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

The probability that a firm in the catchment area of court j is put into receivership by the court is denoted 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 denote P_j^{Rh} the (unobserved) probability of restructuring a viable firm after being put into 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 put into 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 denote 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 as S_j, the survival rate after receivership S_j^R, and the survival rate after restructuring S_j^P.

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

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 put a firm into receivership or to liquidate it. At this stage, the court faces two types of errors. It can put a non-viable firm into receivership and liquidate a viable firm. We denote these two errors T_{1R} and T_{2R}, by :

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

For firms put into receivership, there are again two types of error : 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 as follows :

- $T_1 = R_j^l P_j^{Rl}$, 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 which relate only to 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 reduces both T_1 and T_2 unambiguously increases court efficiency. A reform that also reduced 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 reduced T_{1R} and T_{2R} but increased 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 looking at two types of firms (viable firms if restructured and non-viable firms even if restructured). When applying 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. We 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 put into 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 [21] 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 :

$$Y_{ijt} = \alpha + \sum_{k=g,d} \beta_k^Y \left(Reform_{ij} \times Post_t \right) + \gamma_1 X_i + \gamma_2 u_{jt} + \theta_j + \theta_{st} + \epsilon_{ijt} \tag{4}$$

where Y_{ijt} is a dummy variable for each of the three above-mentioned outcomes. The index j refers to the jurisdiction based on the pre-reform division ; t is the year the firm started the bankruptcy procedure, and $Post_t$ equals 1 after 2008. $Reform_{ij}$ takes three values : *absorbed* if firm i is located in a jurisdiction j whose court is absorbed, *absorbing* if the firm is in a jurisdiction whose court absorbed another court, and *control* for firms located in jurisdictions unaffected by the reform. We are interested in the coefficient β_k^Y which measures, for bankruptcy outcome Y, the 2009 reform's average impact on firms in absorbed ($k = d$) and absorbing ($k = g$) courts, compared with control firms.

In addition, 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 firms within the jurisdiction, and θ_{st} an industry × year fixed effect. Finally, we include the local annual unemployment rate (u_{jt}) to control for economic conditions at the (pre-reform) jurisdiction level. [22] These controls allow us to take into account the fact that firms are not identical within jurisdictions and that bankruptcy outcomes may vary according to industry and year.

We can estimate the equation on three different samples : the whole sample of firms filing 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. β^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 put into receivership and 0 if it is liquidated right away). It is estimated on the whole sample of firms filing for bankruptcy.

2. β^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.

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21. We use survival seven years after filing for bankruptcy.

22. 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.

3. $\beta^{P|R}$ the same coefficients as in 2., estimated on the restricted sample of firms put into receivership ("restructuring after receivership").

4. β^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. $\beta^{S|R}$ the same coefficients as in 4., estimated on the restricted sample of firms put into receivership ("survival after receivership").

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

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. [23]

3.3   From empirical equations to Type 1 and Type 2 errors

With the 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 $\times$ 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 $\times$ year fixed effects, pre-reform court fixed effects, and firm 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 are 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 jurisdictions not affected by the reform. The online appendix provides empirical evidence supporting these assumptions.

We show in the online appendix 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 capture formally how the reform impacts T_1 and T_2 errors. The intuition is as follows. The sign of the coefficient β^S gives the reform's impact on the overall Type 2 error. A positive β^S means that the reform increases the overall survival rate of firms that enter a bankruptcy procedure. As only viable firms

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

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 that the reform reduces the overall survival rate, implying that less viable firms restructure (i.e. the Type 2 error increases). The reform's impact on Type 1 errors is less straightforward to capture. The estimate for β^P together with that of β^S allows for recovery change in Type 1 errors in some specific cases (but not all). 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), we can conclude that fewer non-viable firms restructure, meaning a reduction in Type 1 error as well. The same can be concluded if $\beta^S > 0$ and $\beta^P = 0$. A reform that reduces the restructuring of viable firms ($\beta^S < 0$) 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
	> 0	$T_1\,?,\,T_2 \downarrow$	$T_1 \downarrow,\ T_2 \downarrow$	$T_1 \downarrow,\ T_2 \downarrow$
β^S	$= 0$	$T_1 \uparrow,\ T_2 =$	$T_1 =,\ T_2 =$	$T_1 \downarrow,\ T_2 =$
	< 0	$T_1 \uparrow,\ T_2 \uparrow$	$T_1 \uparrow,\ T_2 \uparrow$	$T_1\,?,\,T_2 \uparrow$

Note : This table summarizes the results of the conceptual framework. A reform that reduces the probability of restructuring, $\beta^P < 0$, while increasing the probability of survival, $\beta^S > 0$, reduces both Type 1 and Type 2 errors.

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

Estimates of the impact of the reform on the probability of being put into receivership

(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 being put into receivership ($\beta^R < 0$), it does not increase the probability of a non-viable firm being put into receivership ($\Delta R_j^l \leq 0$).

2. If the reform increases the overall probability of a firm being put into receivership ($\beta^R > 0$), it does not decrease the probability of a non-viable firm to be put into 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.

We can immediately see that under the first hypothesis :

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

And that under the second hypothesis :

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

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

In the previous section, we saw how to assess 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 entering a bankruptcy procedure (β^R, β^P and β^S). Here, we show how the other three 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 as follows : 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. We 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, we would expect $\beta^{S|P} < 0$. This is shown in the bottom part of Table 2. The same reasoning applies to finding conditions for $\beta^{P|R}$, $\beta^{S|R}$. A formal proof is given in the

online appendix.

Table 2 gives us a way to check the consistency of our results across estimates. This can be used as a robustness test.

TABLE 2 – 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	$\beta^{P\|R} < 0$	$\beta^{P\|R} < 0$	?			

Note : If $\beta^S > 0$ and $\beta^S < 0$, we would expect $\beta^{S|R} > 0$ to be positive.

4 Data and descriptive statistics

4.1 Data sources and firm characteristics

Our data include all corporate bankruptcies and their outcome in France from 2000 to 2019. We use data from FIBEN (*Fichier Bancaire des Entreprises*, the Banque de France's information system) that receives bankruptcy information from the commercial courts. These data provide the date and nature of the rulings at each stage of the bankruptcy procedure. For each case, it is possible to obtain 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 of liquidation at any stage of the process. From FIBEN, we also have information on the firm's location (post code). The assignment of firms to courts depends on the firm's location. [24]

For almost every firm in our sample, FIBEN provides information on its size and sector of activity. [25] The final sample includes 580,227 unique firms whose bankruptcy case was

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24. 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 post code and the division of the former commercial courts' jurisdictions, we have reconstructed the pre-reform commercial courts' catchment areas for all the firms that filed for bankruptcy before 2008.

25. Complete balance sheet data are only available for a small proportion of the sample. Financial

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 3 gives the composition of firms entering bankruptcy. 92,4% of firms are very small "micro"-enterprises (fewer than 10 employees and less than €2 million in annual turnover). 36% of the sample firms are in the service industry, 25% in construction and 24% in trade, the remainder being in the manufacturing and transport industries. [26]

TABLE 3 – Firm summary statistics

	All	Before (2000-2007)				After (2008-2019)	
		Control	Absorbed	Absorbing	Absorbed + Absorbing	Control	New
Full sample	580,227	95,104	12,778	29,686	42,464	303,455	139,204
Bankruptcy outcome							
Receivership rate	580,227	0.374	0.553	0.506	0.521	0.294	0.374
Restructuring rate after filing	580,227	0.132	0.222	0.173	0.188	0.081	0.103
Restructuring rate after receivership	198,951	0.353	0.401	0.342	0.361	0.275	0.277
Survival 7 years after filing	451,820	0.071	0.098	0.082	0.087	0.047	0.052
Survival 7 years after receivership	158,608	0.190	0.176	0.163	0.167	0.159	0.137
Survival 5 years after restructuring	50,533	0.458	0.434	0.429	0.431	0.455	0.440
By size							
Micro-entreprises	546,147	0.924	0.932	0.934	0.934	0.944	0.950
Others	34,080	0.076	0.068	0.066	0.066	0.056	0.050
By industry							
Construction	153,062	0.249	0.242	0.261	0.256	0.270	0.266
Trade	136,398	0.243	0.244	0.242	0.243	0.229	0.243
Services	216,253	0.357	0.324	0.341	0.336	0.387	0.366
Others	73,054	0.151	0.190	0.155	0.166	0.114	0.124

Note: Table 3 presents summary statistics on our cross-sectional firm data. Size and industry are those reported by the firm the year before it filed for bankruptcy. The first row and first column report the number of firms for each category. The rest of the Table displays percentages calculated on the first row. Micro-enterprises are those that have fewer than 10 employees and less than €2 million annual turnover. Other industry comprises the manufacturing and transport industries.

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 2, panel (a). Absorbed courts handled an average of 49 cases per year over the pre-reform period as against nearly 128 in the absorbing courts and 154 in the control courts (Table 8, first panel). The new courts resulting from the reform process as many

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information on firms that have filed for bankruptcy is scarce, not only because the vast majority of these are very small firms, for which data are less accessible than for large firms, but also because these firms, due to being in difficulty, generally provide less information in their accounts than healthy firms.

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

cases as the control courts. We illustrate the court size distribution shift before and after reform in Figure 2, panel (b). After the reform, courts are, on average, bigger and more concentrated (the red line compared with 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 courts would have been after 2009. However, there is still considerable heterogeneity in court size after the reform.

FIGURE 2 – Court size distribution

(a) Before reform, by court type

(b) Before and after reform

Absorbed (2000–2007) — Absorbing (2000–2007)
Control (2000–2007)

Initial jurisdiction (2000–2007) — New jurisdiction (2008–2020)
Initial jurisdiction (2008–2020)
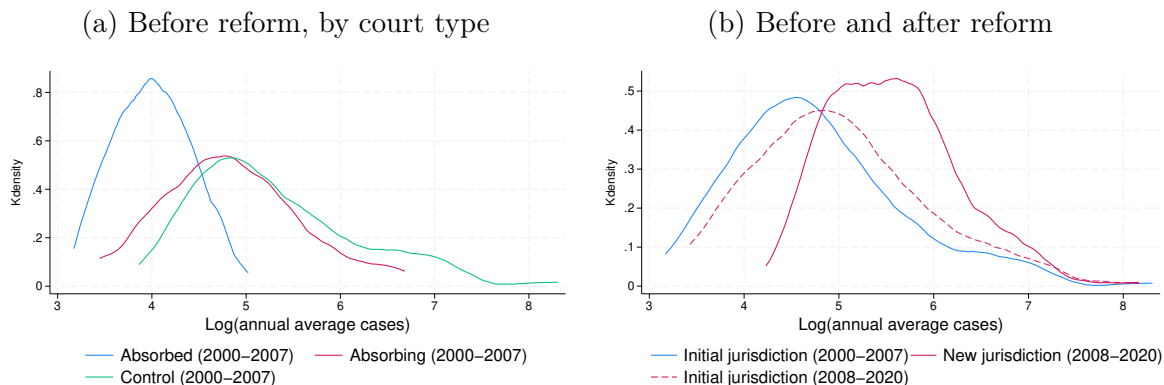
Note: Figure 2 panel (a) reports the distribution of each type of court before reform. Figure 2 panel (b) shows the court size distribution before and after reform (resp. blue and red line). Court size is measured as the log of the average number of cases per year. To ensure that the change in distribution 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 reform (i.e. for the former courts).

Commercial courts are also heterogeneous regarding bankruptcy outcomes (reported in Table 8). The average absorbed court granted receivership to 49.5% firms. This compares with 44.9% for absorbing courts and 42.3% for control courts. After the reform, the receivership rate of the average new court dropped to 36.4%, on a par with that of the average controlling court (34.8%).

Other descriptive statistics are reported in the online appendix and illustrate the differences in composition between courts. Notably, the reform impacted the number of flight jurisdiction judges. 25% of the control courts gained two judges or more (up to eight 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. The new courts lost a median of five judges compared with the sum of the judges in the former courts.[27] 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 fear was that the mergers would result in a substantial increase in judges'

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27. This analysis excludes courts that have absorbed a high court. Because high courts operate differently, counting the number of judges dedicated to corporate bankruptcy is impossible.

caseload and procedure length and a deterioration in the quality of rulings. The judge's caseload did indeed increase, 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 courts. For firms in absorbed courts, the judge handling their cases has twice the workload as before the reform. We show in the online appendix that the increase in judges' caseload seems to have little or 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 cases), the reform reduces the distance between firms and their assigned court. Change in the distance to courts associated with the reform has no impact on bankruptcy outcomes (see online appendix).

5 The reform's impact

5.1 Main results

Table 4 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.

In a nutshell, the reform appears to be positive as it reduces the continuation bias of absorbed courts and has no impact on their liquidation bias. Firms in absorbing jurisdictions are not impacted. Indeed, whatever the outcome considered, the bankruptcy of firms in absorbing districts is not impacted by the reform (second line of Table 4, coefficients are not significantly different from zero). The impact of the reform is clearly significant for firms in absorbed jurisdiction : they are less likely to be sent to receivership (-6.43 percentage points, see first line of Table 4). Their probability of restructuring after receivership is also lower (column (3)). The probability of survival seven years after filing for bankruptcy is unaffected (column (4)), but the probability of surviving seven after receivership and five years after restructuring are significantly higher (columns (5) and (6)). This is a sign that the composition of restructured firms has changed with relatively fewer non-viable firms. This is consistent with an overall reduction in Type 1 errors, i.e. a reduction in the continuation bias.

The signs of β^R, β^P, and β^S reported in Table 4 allow us to check for the consistency

TABLE 4 – 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 4 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) survival seven years after filing, column (5) survival seven years after receivership, and column (6) survival five years after restructuring. Standard errors are clustered at the pre-reform court level.

of the β estimates across specifications as exposed in Table 2. $\beta^{S|R}$ is positive as it should be ; $\beta^{P|R}$ is not significantly different from zero, but our conceptual framework does not 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.[28] To ensure that our results do not come from a size effect of control courts compared with absorbed ones, we reduce our control group to courts with a size comparable to that of absorbed courts[29] and run the same set of equations. The results shown in Table 5 are qualitatively identical to those reported in Table 4. The impact of the reform 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)). Table **??** in the online appendix gives an additional robustness check in which control courts are restricted to the ones that do

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28. As discussed in Section 2.2.

29. The largest absorbed courts had up to 15 judges (Annonay, Argentan, Charleville-Mézières, Dole) whereas the smallest of the control courts had only eight judges before the reform (Foix, Gap). These courts, albeit small, were not affected 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 fewer. This corresponds to the maximum number of judges in absorbed courts and the median number of judges in control courts (Table A.1).

not gain any judge after the reform.

TABLE 5 – The reform's impact – controlling by small control courts only

	R_{ijt}	P_{ijt}		S_{ijt}		
	(1) β^R	(2) β^P	(3) $\beta^{P\|R}$	(4) β^S	(5) $\beta^{S\|R}$	(6) $\beta^{S\|P}$
Absorbed × Post	-0.0324**	-0.0249***	-0.0289**	0.00278	0.0201**	0.0582***
	(-2.60)	(-3.54)	(-2.05)	(0.62)	(2.07)	(2.91)
Absorbing × Post	0.00256	-0.00133	-0.0110	0.00330	0.00562	0.0471***
	(0.21)	(-0.19)	(-0.92)	(0.80)	(0.59)	(2.66)
Unemployment rate	0.00164	0.000169	-0.00221	0.00272	0.00404	0.00390
	(0.21)	(0.06)	(-0.34)	(1.11)	(0.91)	(0.47)
Year × Industry FE	✓	✓	✓	✓	✓	✓
Court FE	✓	✓	✓	✓	✓	✓
Firm size	✓	✓	✓	✓	✓	✓
Observations	251,482	251,482	103,517	197,640	83,620	27,550
Adj. R²	0.121	0.090	0.073	0.072	0.065	0.036

t statistics in parentheses

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

Note: Table 5 is the same as Table 4 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 with absorbed courts – we reduce our control group to courts with only one commercial chamber and 15 judges or fewer. This leaves us with 32 control courts (out of 70) that processed 69,815 cases over the period (out of 504,766, or 13.8%). Standard 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 be able to not cope with their growth.

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

In France, most bankruptcy procedures involve small firms. In our sample, over the 2000-2019 period, 92,4% of firms have fewer than 10 employees and an annual turnover of less than €2 million. We would expect the continuation bias to be greater for these firms than for larger ones for which the financial stakes are higher, and thus courts' (and creditors') decisions are more likely to be rational. [30] Consequently, we would expect the

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30. 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."

reform to have little or no impact on larger firms. In order to check that, we split our sample according to firm size. The first sample comprises small firms, with 546,147 bankruptcy cases. The second sample includes all other firms, with 34,067 bankruptcy cases. We then run the same set of estimations.

The results are presented in Table 6, panel (a) for small firms and Table 6, 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 for the whole sample. The continuation bias is thus reduced for small firms. It is not reduced for larger ones, maybe because, for these firms, there is no continuation bias to begin with. 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 represent almost as many employees (737,000) as the 34,067 other firms (953,876).

TABLE 6 – The reform's impact – by firm size

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)
Panel (a) - small firms						
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) - large firms						
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 5 reproduces Table 4 on two sub-samples according to firms' size. Panel (a) is restricted to small firms with fewer than 10 employees and €2 million in annual turnover. Panel (b) includes all other firms. Standard errors are clustered at the pre-reform court level.

6 Mechanisms

How can we explain the reduction in the Type 1 error for firms in absorbed courts ? We are interested in two elements : first, the speed at which the impact of the reform appears. Second, the transmission of behavior between the absorbing courts and the absorbed courts.

6.1 Speed of the reform's impact

Concerning the speed, we would like to identify whether the impact corresponds mainly to a slow convergence of practices, in which case the reform's impact builds up gradually, or a break with local habits, in which case the impact is more immediate. Note that the two channels can be at play at the same time. This will be the case if we detect a significant impact right after the reform that increases over time.

In its dynamic form, equation (4) becomes :

$$Y_{ijt} = \alpha + \sum_{k=g,d} \sum_{t \neq 2007} \beta_{kt}^Y \cdot (\mathbb{1}_t \times Reform_{ij}) + \gamma_1 X_i + \gamma_2 u_{jt} + \theta_j + \theta_{st} + \epsilon_{ijt} \qquad (7)$$

where $\mathbb{1}_t$ are year fixed effects.

Figure 3 plots the estimated coefficients β_{dt}^Y for firms in absorbed jurisdictions. Panels (a) and (b) show that the effects on the probability of receivership and restructuring, β_{dt}^R and β_{dt}^P respectively, are both immediate and persistent after the reform. The speed at which the impact of the reform appears reflects a break with habits. In addition, at least for receivership (panel (a)), the downward trend seems compatible with a slow transmission of behavior (however, the 2019 coefficient is below but not significantly different from that of 2009). Panel (b) is less conclusive : one cannot prove nor rule out the break with bad local habits and the slow transmission of behavior.

6.2 Transmission of court behavior

We want to measure how absorbing courts transmit their behavior to the firms in the jurisdiction of the court they absorb. We also consider the possibility for an absorbed court to influence the behavior of its absorbing court as some judges were transferred from the absorbed court to the absorbing one. [31]

The intuition behind the empirical test is as follows. Before the reform, we observe the various bankruptcy outcomes for each court : the receivership, restructuring and survival

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31. Unfortunately, we do not have the judges' identity nor the number of judges that were actually transferred.

FIGURE 3 – The reform's dynamic impact for firms in absorbed jurisdiction

(a) Receivership - β_{dt}^R (b) Restructuring - β_{dt}^P
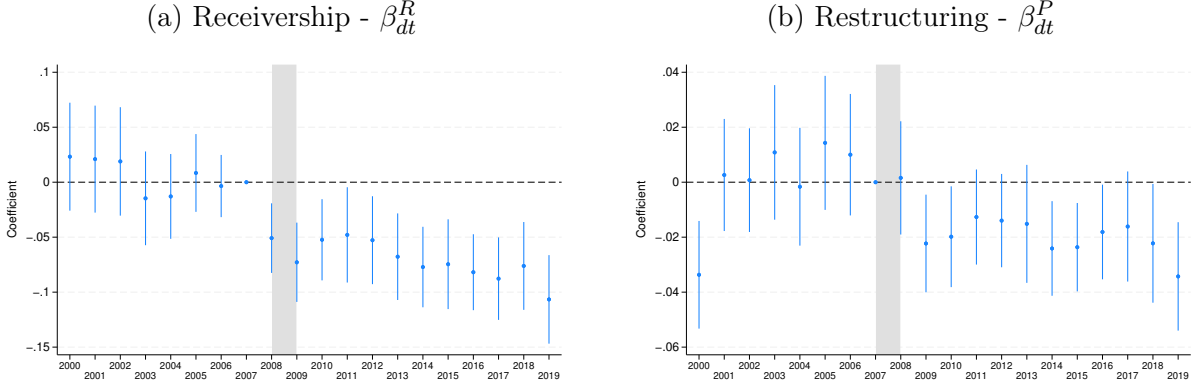
Note: Figure 3 shows the results of equation (7). Panel (a) plots β_{dt}^R : the reform's impact on the probability of entering receivership (rather than direct liquidation). Panel (b) plots β_{dt}^P : the reform's impact on the probability of restructuring after filing. Standard errors are clustered at the pre-reform court level.

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 the set of regressions and include this difference as an explanatory variable in addition to the other explanatory variables considered before. We expect these differences to have no impact whatsoever on bankruptcy outcomes before the reform, but an impact after the reform. By construction, these variables are null for the control firms.

More precisely, we create a measure of court behavior, ϵ_j^Y, that captures the component of bankruptcy outcomes that is not explained by firm characteristics. ϵ_j^Y is the average residual of equation (4), estimated without court fixed effects, using only pre-reform data (see appendix for details). We then construct the difference in behavior between pairs of merging courts and estimate equation (8). For absorbed jurisdiction (indexed by d), we measure $\overline{\Delta\epsilon_{jd}^Y}$ as the average difference for outcome Y between the absorbing and the absorbed court before the reform. Symmetrically, for absorbing jurisdictions (indexed by g), $\overline{\Delta\epsilon_{jg}^Y}$ is the average difference in behavior for outcome Y between the absorbing court and the absorbed one prior to the reform. By definition, $\Delta\epsilon_j^Y = 0$ for control courts.

Before reform, we interact the annual measure $\Delta\epsilon_{jt}^Y$ with year dummy $\mathbb{1}_t$. The coefficient τ_{gt}^Y (resp. and τ_{dt}^Y) 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 $\overline{\Delta\epsilon_j^Y}$, the pre-reform average behavior. The coefficient δ_{gt}^Y (resp. and δ_{dt}^Y) 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.

$$Y_{ijt} = \alpha + \sum_{k=g,d} \sum_{t \neq 2007} \beta_{kt}^Y (\mathbb{1}_t \times Reform_{ij}) + \gamma_1 X_i + \gamma_2 u_{jt} + \theta_j + \theta_{st}$$

$$+ \sum_{k=g,d} \left(\sum_{t=2000}^{2007} \tau_{kt}^Y (\mathbb{1}_t \times \Delta \epsilon_{jkt}^Y) + \sum_{t=2008}^{2019} \delta_{kt}^Y (\mathbb{1}_t \times \overline{\Delta \epsilon_{jk}^Y}) \right) + \epsilon_{ijt}' \tag{8}$$

Figure 4 displays the coefficients on two outcomes : receivership (panel (a)) and restructuring (panel (b)). In both panels, 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 bankruptcy outcomes. This result supports the parallel trends hypothesis.

FIGURE 4 – Transmission of court behavior

(a) Receivership

(b) Restructuring
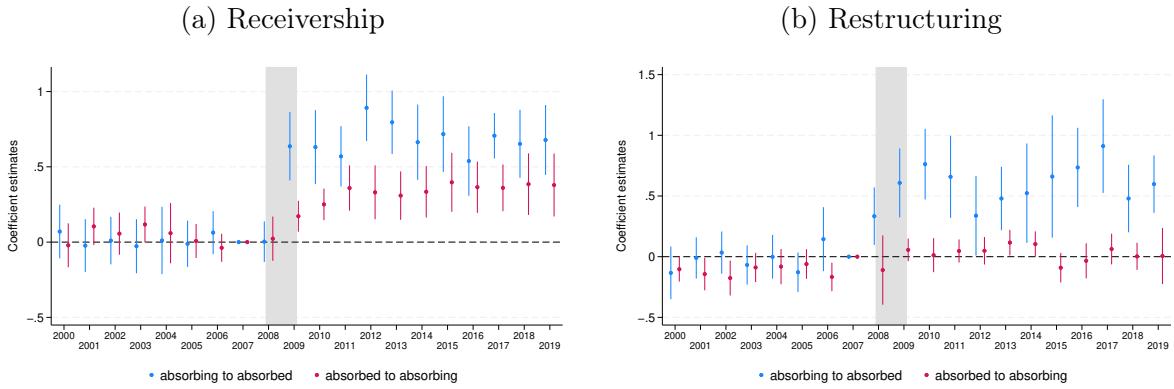
Note:

Figure 4 shows the coefficients of equation (8) τ_{gt} and δ_{gt} in blue, and τ_{dt} and δ_{dt} in red. Panel (a), the dependent variable is the probability of entering receivership compared with direct liquidation, and $\Delta \epsilon_{kt}^R$ is the difference in receivership rates between absorbing and absorbed jurisdictions. Panel (b), the dependent variable is the probability of restructuring after receivership and $\Delta \epsilon_{kt}^{P|R}$ the difference in restructuring rate between absorbing and absorbed jurisdictions.

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. Between 50% and 75% of the differences between absorbing and absorbed courts is transmitted to the firms of absorbed jurisdictions. The effect is immediate and stable over time. Conversely, 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 : 25% of the differences between absorbed and absorbing courts is transmitted to firms in the absorbing jurisdiction. Panel (b) indicates that the behavior of the absorbed court does not influence the restructuring of firms after the reform. These results mean that the influence is reciprocal, even if the influence of the absorbing court is dominant and determines the

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aggregate effect of the reform.

To summarize, in this subsection we show that the behavior of absorbing courts influences the bankruptcy outcomes of firms in the jurisdiction of the court they absorbed more than the other way round. From a policy point of view, we may be tempted to conclude that, rather than the size of the commercial court, the absorbing court's effectiveness 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 appear to 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 Blazy et al. (2018)), employees, and suppliers. One could also argue that the damage associated with this continuation bias goes beyond the fragile firms and their stakeholders and harms the economic dynamism of firms in these small jurisdictions more generally. This damage can be even 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 United States, Morrison (2007) concludes that there is no such continuation bias for small firms.

In this paper, we take advantage of a reform implemented in 2009 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 courts 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 (fewer non-viable firms are allowed to engage in restructuring discussions with their creditors and are more often liquidated right away). This impact is seen entirely in the bankruptcy outcomes of firms whose court was absorbed. The reform also had 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 diminish the chances of fragile but viable firms to restructure and survive.

These results are robust but apply only to the bankruptcy cases of small firms. When we restrict the sample to firms 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 the bankruptcy outcomes of firms in the catchment area of the court they absorbed more

than the other way round.

Our empirical analysis is conducted within a conceptual framework that allows 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.

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Appendix

TABLE 7 – Determinants of being an absorbed or an absorbing court

	Panel A : absorbed (Y/N)				
	(1)	(2)	(3)	(4)	(5)
Is a prefecture	-0.598***				-0.162
	(-5.68)				(-1.57)
Is a court of appeal		-0.629***			-0.0194
		(-3.49)			(-0.14)
Largest court			-0.329***		-0.238**
			(-3.24)		(-2.16)
Smallest court			0.543***		0.495***
			(5.33)		(4.81)
Highest receivership rate				0.218	0.149
				(1.45)	(1.50)
Lowest receivership rate				-0.115	-0.0355
				(-0.77)	(-0.35)
Département FE	✓	✓	✓	✓	✓
Observations	112	112	112	112	112
R^2	0.355	0.201	0.610	0.147	0.653

	Panel B : absorbing (Y/N)				
	(1)	(2)	(3)	(4)	(5)
Is a prefecture	0.479***				0.149
	(4.30)				(1.13)
Is a court of appeal		0.634***			0.215
		(3.58)			(1.18)
Largest court			0.308**		0.172
			(2.41)		(1.21)
Smallest court			-0.333**		-0.290**
			(-2.61)		(-2.19)
Highest receivership rate				-0.0180	0.0312
				(-0.12)	(0.24)
Lowest receivership rate				0.213	0.126
				(1.41)	(0.96)
Département FE	✓	✓	✓	✓	✓
Observations	112	112	112	112	112
R^2	0.240	0.189	0.350	0.091	0.396

t statistics in parentheses

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

Note: Table 7 reports the determinants of a court being absorbed (panel A) or absorbing (panel B). The variable "smallest court" (resp. "largest court") is a dummy equal to one if the court had, on average, the lowest (resp. largest) number of registered procedures in the *département* prior to the reform. The variable "highest receivership rate" (resp. "lowest receivership rate") is a dummy equal to one if the court had, on average, the highest (resp. lowest) average receivership rate in the *département* before the reform. All specifications include *département* fixed effects and comprise all French *départements* affected by the reform, with some *départements* having more than two courts.

TABLE 8 – Summary statistics by type of court

Note: Table 8 presents summary statistics at the court level. All statistics are annual averages by type of court. 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.

| | 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

| | 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

TABLE 8 – Summary statistics by type of court – continued

| | | | 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

FIGURE 5 – Median bankruptcy rate by type of court
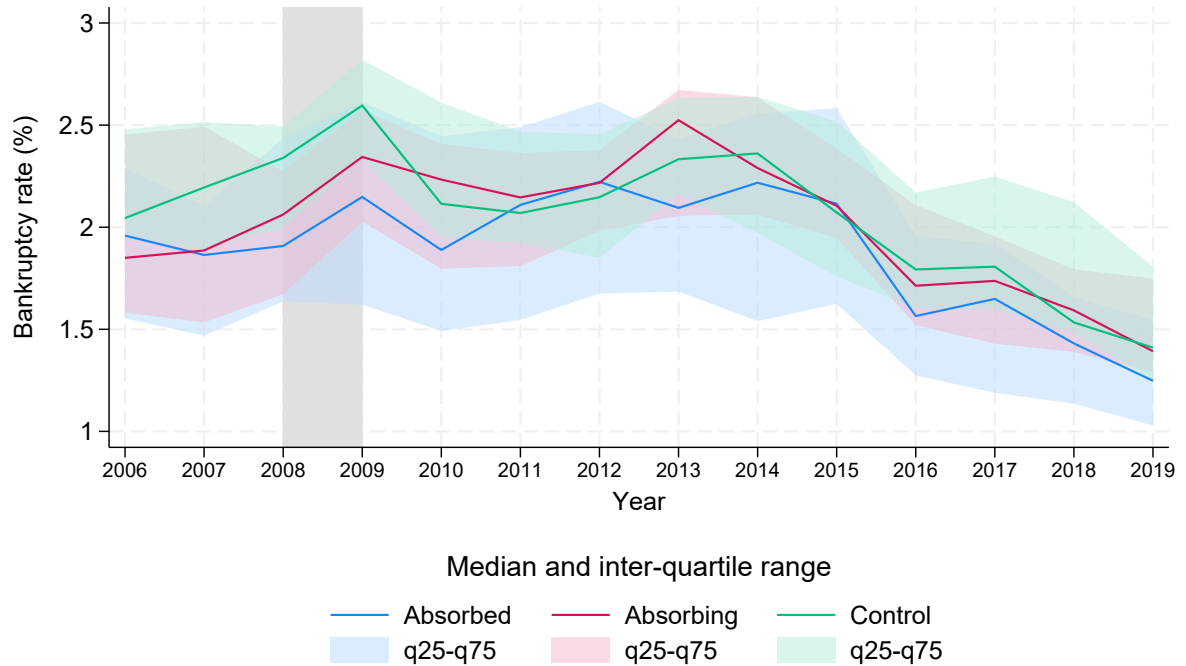
Note: Figure 5 shows the median bankruptcy rate and inter-quartile range by type of court. The bankruptcy rate is defined as the ratio of the number of bankruptcy filings to the total number of active firms. Data on the stock of active firms are available from 2006 onward (see https://www.insee.fr).

Additional results

TABLE 9 – The reform's impact – controlling by control courts that retain the same number of judges

	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	-0.0624***	-0.0314***	-0.0152	-0.00135	0.0164**	0.0370**
	(-3.92)	(-5.02)	(-1.13)	(-0.39)	(2.01)	(2.05)
Absorbing × Post	-0.0282*	-0.00757	0.00281	-0.000425	0.00227	0.0261*
	(-1.66)	(-1.22)	(0.25)	(-0.15)	(0.29)	(1.74)
Unemployment rate	-0.0231***	-0.0104***	-0.00804*	-0.00505***	-0.00561	-0.000755
	(-2.93)	(-3.80)	(-1.80)	(-3.21)	(-1.35)	(-0.13)
Year × Industry FE	✓	✓	✓	✓	✓	✓
Court FE	✓	✓	✓	✓	✓	✓
Observations	412,343	412,343	139,002	320,819	111,340	35,486
Adj. R^2	0.159	0.091	0.071	0.069	0.071	0.036

t statistics in parentheses

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

Note: Table 9 reports the main results with a reduced control group. The reform allocated new judges to some courts – mainly absorbing courts, but also some control courts. To ensure that our result is not impacted by the control courts that gain judges after the reform, we reduce our control group to courts that retain the same number of judges directly after the reform. This leaves us with 37 control courts (out of 70) that handled 295,415 cases over the period (out of 504,766, or 58.5%). The results are unaffected. Standard errors are clustered at the pre-reform court level.

Measuring courts' behavior

This appendix details the measures of court behavior, $\Delta \epsilon^Y$. 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, survival seven years after filing, survival seven years after receivership, and the survival five years after restructuring.

We measure $\Delta \epsilon^Y$ that captures average court behavior and control for the composition of firms that file for bankruptcy in each court. We include controls for firm size, industry, and the local unemployment rate. For $t \leq 2008$ we estimates :

$$Y_{ijt} = \alpha + \gamma_1 X_i + \gamma_2 u_{jt} + \theta_{st} + \epsilon_{ijt}^Y \qquad (9)$$

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 $t \leq 2008$, for absorbed jurisdictions :

$$\Delta \epsilon_{jdt}^Y = \epsilon_{gt}^Y - \epsilon_{dt}^Y$$

Similarly, for absorbing jurisdictions :

$$\Delta \epsilon_{jgt}^Y = \epsilon_{dt}^Y - \epsilon_{gt}^Y$$

For $t > 2008$ and $k = g, d$, we take the average over the pre-reform period :

$$\overline{\Delta \epsilon_{jk}^Y} = \frac{1}{8} \sum_{t=2000}^{2008} \Delta \epsilon_{jkt}^Y$$

Tables 10 summarizes the measures. The first row of each panel, $\Delta \epsilon_d^Y$, reports the average impact of the absorbing court's behavior on the absorbed court. The second row of each panel $\Delta \epsilon_g^Y$ reports the influence of absorbed courts on absorbing courts. By construction, they are of opposite sign. $\Delta \epsilon^Y$ is always equal to 0 for control courts. We use these measures in Section 6 of the paper.

TABLE 10 – Summary of $\Delta\epsilon_{kt}^{Y}$ (2000-2008)

	N	Mean	Median	St. Dev.	Min	Max
Receivership						
$\Delta\epsilon_d^R$	50	-0.033	-0.026	0.091	-0.297	0.151
$\Delta\epsilon_g^R$	42	0.030	0.026	0.092	-0.153	0.300
Restructuring after filing						
$\Delta\epsilon_d^P$	50	-0.020	-0.018	0.035	-0.104	0.042
$\Delta\epsilon_g^P$	42	0.017	0.018	0.033	-0.043	0.091
Restructuring after receivership						
$\Delta\epsilon_d^{P|R}$	50	-0.030	-0.017	0.074	-0.272	0.147
$\Delta\epsilon_g^{P|R}$	42	0.025	0.020	0.074	-0.146	0.279
Survival 7 years after filing						
$\Delta\epsilon_d^S$	50	-0.007	-0.005	0.017	-0.047	0.031
$\Delta\epsilon_g^S$	42	0.005	0.003	0.016	-0.025	0.045
Survival 7 years after receivership						
$\Delta\epsilon_d^{S|R}$	50	-0.006	-0.005	0.043	-0.147	0.075
$\Delta\epsilon_g^{S|R}$	42	0.002	0.004	0.043	-0.084	0.143
Survival 5 years after restructuring						
$\Delta\epsilon_d^{S|P}$	50	-0.003	-0.008	0.127	-0.306	0.298
$\Delta\epsilon_g^{S|P}$	42	-0.003	-0.009	0.126	-0.314	0.301

$$Y_{ijt} = \alpha + \sum_{k=d,g} \beta_k^Y (Reform_{ij} \times Post_t) + \gamma_1 X_i + \gamma_2 u_{jt} + \theta_j + \theta_{st}$$
$$+ \sum_{k=d,g} \delta_k^Y \left(\overline{\Delta\epsilon_j^Y} \times Post_t \right) + \epsilon'_{ijt} \tag{10}$$

We are interested in the δ_k^Y coefficients : δ_d^Y measures the average influence of absorbing courts on firms from absorbed courts, and δ_g^Y vice versa : the average influence of absorbed courts on firms from absorbing courts. We expect δ_k^Y to be positive or null and below one. The results are presented in Table 11. They are consistent across outcomes. The past behavior of the absorbing court always has a greater impact on bankruptcy outcomes for firms in the absorbed court than the absorbed court has on those of the absorbing one. Indeed, the coefficients associated with $Post \times \Delta\epsilon_d^Y$ are always positive and larger than those of $Post \times \Delta\epsilon_g^Y$.

TABLE 11 – Transmission of court behavior

Bankruptcy outcome	R_{ijt}	P_{ijt}		S_{ijt}		
	(1)	(2)	(3)	(4)	(5)	(6)
Coefficient of interest	δ^R	δ^P	$\delta^{P|R}$	δ^S	$\delta^{S|R}$	$\delta^{S|P}$
Absorbed × Post	-0.0406***	-0.0222***	0.00145	0.00209	0.0168**	0.0320**
	(-3.54)	(-3.72)	(0.14)	(0.67)	(2.59)	(2.05)
Absorbing × Post	-0.0315***	-0.0160**	-0.00808	-0.00158	-0.00253	0.0197
	(-2.98)	(-2.34)	(-0.87)	(-0.53)	(-0.36)	(1.38)
Post × $\Delta\epsilon_d^Y$	0.476***	0.486***	0.742***	0.632***	0.584***	0.363***
	(6.68)	(3.93)	(8.60)	(4.13)	(3.93)	(3.73)
Post × $\Delta\epsilon_g^Y$	0.301***	0.385***	0.149***	0.134	0.140**	0.105
	(3.76)	(3.35)	(4.00)	(1.17)	(2.01)	(1.00)
Unemployment rate	-0.0263***	-0.00998***	-0.00276	-0.00445***	-0.00197	-0.000610
	(-4.34)	(-4.01)	(-0.63)	(-2.93)	(-0.47)	(-0.12)
Year × Industry FE	✓	✓	✓	✓	✓	✓
Court FE	✓	✓	✓	✓	✓	✓
Firm size	✓	✓	✓	✓	✓	✓
Observations	580,233	580,233	198,958	451,821	158,610	50,547
Adj. R^2	0.152	0.090	0.072	0.070	0.071	0.035

t statistics in parentheses

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

Note: Table 11 presents the results of equation (10). We denote $\Delta\epsilon_d$ (resp. $\Delta\epsilon_g$) the impact of absorbing court's (resp. absorbed) past behavior on firms from the absorbed (resp. absorbing) court. Standard errors are clustered at the pre-reform court level.