

Online Appendix

Does merging small bankruptcy courts increase their efficiency?

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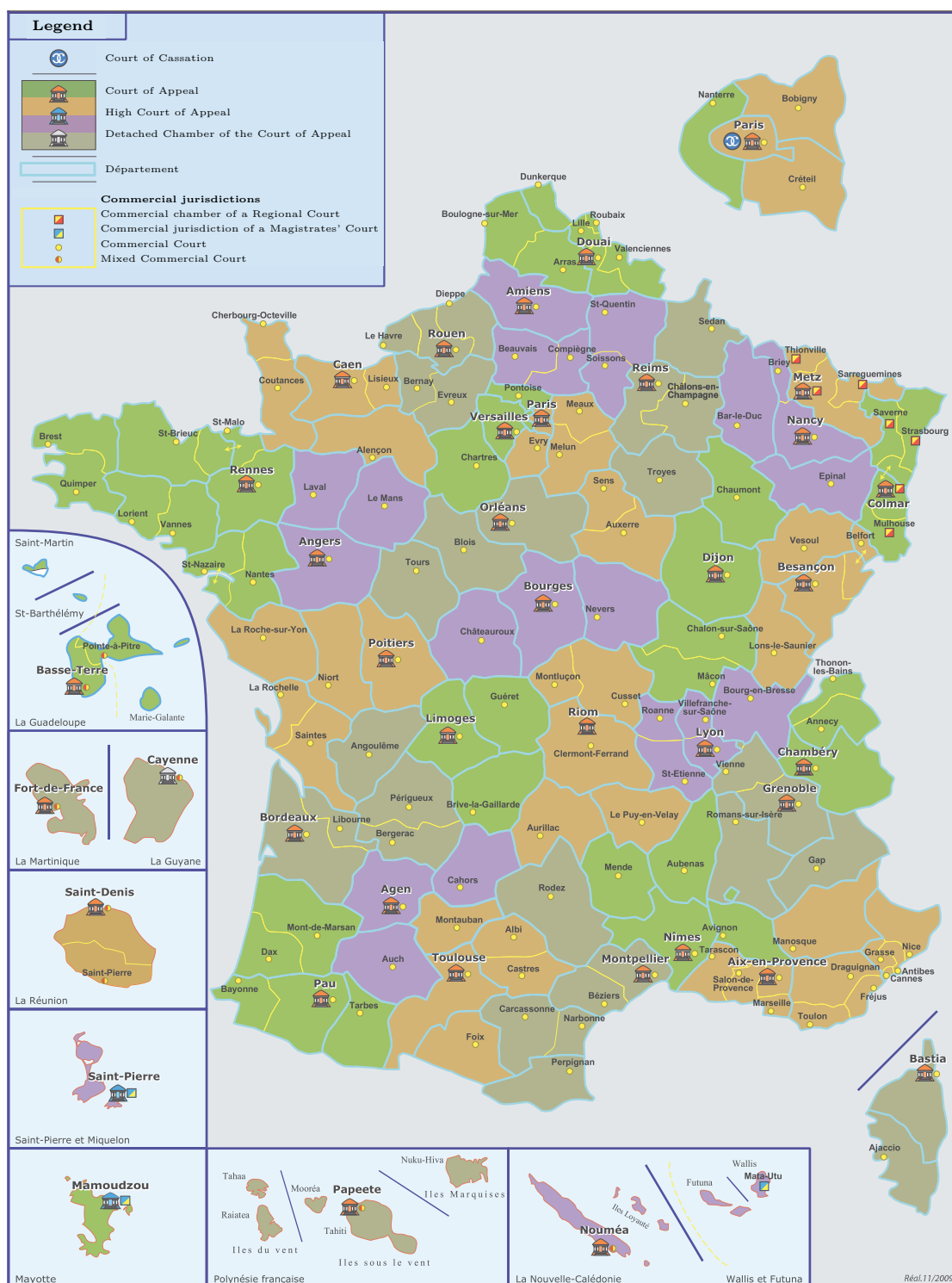
Appendix A Additional Materials and Robustness

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 by type of court

	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	23	20	10	11	53
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	6.3	6.1	2.3	2.4	12.6						
Absorbing	42	9.2	8.8	3.7	3.5	20.0						
New							42	12.5	11.9	3.1	7.4	19.1
Control	70	11.8	11.3	4.4	4.3	23.6	70	13.7	12.9	4.5	5.7	25.3

	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	13.2	5.2	2.1	27.5						
Absorbing	42	11.8	10.9	5.7	2.1	26.8						
New							42	18.1	18.0	7.5	2.5	33.4
Control	70	16.0	15.7	7.3	1.5	32.3	70	16.2	16.5	7.3	1.5	34.1

	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	207	203	35	144	297						
Absorbing	42	201	205	31	148	263						
New							42	233	234	31	166	306
Control	70	193	190	35	123	294	70	229	228	31	153	319

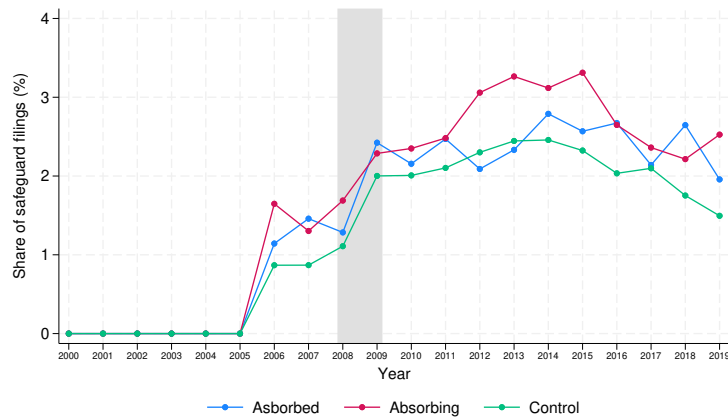
	Days in receivership when liquidated											
	Before (2000-2007)						After (2008-2019)					
	N	Mean	Median	St. Dev.	Min	Max	N	Mean	Median	St. Dev.	Min	Max
Absorbed	50	143	147	30	77	209						
Absorbing	42	140	140	27	101	203						
New							42	159	159	25	111	224
Control	70	133	134	27	71	207	70	154	152	25	100	231

	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	349	338	56	268	519						
Absorbing	42	356	345	56	275	509						
New							42	422	415	46	357	526
Control	70	350	346	50	260	492	70	416	414	36	305	497

A.1 The safeguard procedure

In 2006, the safeguard procedure was introduced into the French bankruptcy code. It aims to tackle financial difficulties at an early stage, allowing firms facing serious financial difficulties to file for court-supervised debt restructuring before reaching insolvency. In practice, safeguard filings represent less than 3% of filings per year (Figure A.3), and the procedure is taken up predominantly by larger, more sophisticated firms.¹ We show in Table A.2 that the inclusion of safeguard filings in our sample does not impact our results and their interpretations.

FIGURE A.3 – Share of safeguard filings



Note: Figure A.3 shows the share of safeguard filings over the total number of bankruptcy filings by type of court. The shaded area marks the reform period.

1. In [Epaulard and Zapha \(2022\)](#), the average safeguard-filing firm has 30 employees. By comparison, 94% of our sample consists of micro-enterprises with fewer than 10 employees.

TABLE A.2 – The reform’s impact – including safeguard filings

	RSA_{ijt}	P_{ijt}		S_{ijt}		
	(1) β^{RSA}	(2) β^P	(3) $\beta^{P RSA}$	(4) β^S	(5) $\beta^{S RSA}$	(6) $\beta^{S P}$
Absorbed $\times$ Post	-0.0671*** (-4.86)	-0.0352*** (-5.73)	-0.0188 (-1.42)	-0.00320 (-0.82)	0.0128* (1.83)	0.0326** (2.08)
Absorbing $\times$ Post	-0.0271* (-1.81)	-0.00731 (-1.13)	0.00317 (0.31)	-0.00253 (-0.80)	-0.000686 (-0.10)	0.0244* (1.82)
Unemployment rate	-0.0153* (-1.93)	-0.00598* (-1.96)	-0.000408 (-0.11)	-0.00228 (-1.27)	-0.000873 (-0.29)	-0.00211 (-0.43)
Year $\times$ Industry FE	✓	✓	✓	✓	✓	✓
Court FE	✓	✓	✓	✓	✓	✓
Firm size	✓	✓	✓	✓	✓	✓
Observations	591,432	591,432	207,400	459,769	166,558	55,934
Adj. R ²	0.157	0.093	0.071	0.077	0.075	0.039

t statistics in parentheses

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

Note: Table A.2 reproduces the baseline specification when the sample includes safeguard filings (SA). RSA = receivership or safeguard.

A.2 The reform’s (absence of) impact on procedure length

The French bankruptcy code fixes the length of the procedure by law. When a firm enters receivership, an observation period of six months begins ; it may be renewed twice at the request of the administrator, the debtor or the public prosecutor, for a maximum of eighteen months.² Therefore, we do not expect the length of the procedures to be greatly impacted by the reform. Table A.1 shows that average duration varies by only a few days across court types, with a standard deviation of about one month, which is precisely why the paper measures efficiency along the other margin, the quality of decisions. This section checks whether the reform did have an impact.

We note that the receivership procedure is twice as long when it leads to a restructuring plan, compared to that of a judicial liquidation (Table A.1). Thus, we consider the duration of the procedure according to the outcome of the proceedings. We estimate equation (2) 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.

The coefficients β (two first lines of Table A.3) are not significantly different from zero in any specification. They are also relatively small : the reform effect would be limited to an increase of 12 calendar days for restructured firms out of an average of 349 days (Table A.1). Therefore, these results suggest little to no impact of the reform on the duration of the procedure.

2. The 2019 *PACTE* law capped the observation period at twelve months for the safeguard procedure. The eighteen-month maximum still applies in receivership. In any case, the ordinance entered into force on 1 October 2021 and governs only filings opened from that date, so it does not concern our sample which end in 2019.

TABLE A.3 – The reform’s impact on procedure length

	Days in receivership		
	All (1)	Restructured (2)	Liquidated (3)
Absorbed $\times$ Post	7.644 (1.44)	12.82 (1.45)	5.225 (1.14)
Absorbing $\times$ Post	3.358 (0.59)	8.342 (0.97)	0.229 (0.05)
Unemployment rate	6.237** (2.18)	7.606 (1.54)	4.652* (1.73)
Year $\times$ 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.3 reports the result of equation (2) 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. Standard errors are clustered at the pre-reform court level.

A.3 Judges' caseload

The reform had an impact on the workload of judges. As discussed above, the total number of judges dropped in the court affected by the reform, while the number of bankruptcies increased (see Table 9). As a result, the number of cases per judge increased in the jurisdictions impacted by the reform. As reported in Table A.1, for firms in absorbed jurisdictions, the average caseload of judges doubled from a median of 6 cases per judge per year to more than 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 the workload of judges 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 induce a 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 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 (2)). We also study the impact of judges' workload on procedure length. The results are reported in Table A.4.

Firstly, the main coefficients of interest β (first two lines) are not affected by the introduction of the control variable : all bankruptcy outcomes present the same sign and are as significant as in the baseline specification.

Secondly, the procedure length is unaffected by the reform, but the associated procedure length (column (2), bottom panel) is correlated with judge caseloads.

The increase in judges' workload by a standard deviation (5.3 cases at the firm level) 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 1.03 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. For firms that restructure, the observation period is, on average, of one year. An increase of 10 days is thus negligible.

TABLE A.4 – 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 $\times$ Post	-0.0687*** (-4.31)	-0.0348*** (-5.43)	-0.00895 (-0.71)	-0.00207 (-0.52)	0.0164* (1.95)	0.0272* (1.66)
Absorbing $\times$ Post	-0.0238 (-1.56)	-0.0106* (-1.72)	0.00285 (0.30)	-0.00233 (-0.74)	-0.00155 (-0.22)	0.00792 (0.62)
Judges' caseload	0.00128 (0.81)	-0.000417 (-1.02)	-0.00192** (-2.34)	-0.000170 (-0.49)	-0.000822 (-0.91)	0.000593 (0.47)
Unemployment rate	-0.0272*** (-4.00)	-0.00962*** (-4.53)	-0.00182 (-0.45)	-0.00415*** (-3.10)	-0.00110 (-0.31)	-0.000607 (-0.12)
Year $\times$ 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 (1)	Restructured (2)	Liquidated (3)
Absorbed $\times$ Post	6.168 (1.13)	5.996 (0.69)	3.653 (0.79)
Absorbing $\times$ Post	3.219 (0.57)	6.995 (0.83)	0.161 (0.04)
Judges' caseload	0.423 (0.86)	1.991** (2.30)	0.451 (1.29)
Unemployment rate	5.833** (2.24)	5.798 (1.46)	4.207* (1.66)
Year $\times$ 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.4 reproduce the baseline specification 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. Standard errors are clustered at the pre-reform court level.

A.4 Impact of distance on bankruptcy outcomes

The reform led to an overall increase in the average distance between firms from absorbed jurisdictions³ and their newly assigned courts. This distance rose by an average of 24.5 km (Table A.5), from 13 km to 37 km. However, this effect is heterogeneous : among the 39,959 firms located in absorbed jurisdictions post-reform, 2,391 firms (6%) actually became closer to their newly assigned court, with an average reduction in distance of -7 km.

TABLE A.5 – Change in distance to court for firms in absorbed jurisdictions

N	Mean	St. Dev.	Change in distance (km)						
			Min	p5	p25	Median	p75	p95	Max
39,959	24.5	16.6	-38.8	-2.7	13.1	24.7	33.4	54.9	77.8

How did this change in distance influence bankruptcy outcomes? The expected effect is ambiguous. On the one hand, proximity may facilitate better information about the firm and a deeper understanding of its local economic environment. On the other hand, proximity may increase the risk of conflicts of interest.

To investigate this, we estimate the following equation :

$$Y_{ijt} = \alpha + \sum_{k=g,d} \beta_k^Y (Reform_{ij} \times Post_t) + \gamma_1 ChangeInDistance_i + \gamma_2 X_i + \gamma_3 u_{jt} + \theta_j + \theta_{st} + \epsilon_{ijt} \quad (A.1)$$

where $ChangeInDistance_i$ is defined for firms in absorbed jurisdictions after reform as the difference between the distance to their newly assigned court and the distance to their court before the reform. For firms in absorbing and control jurisdictions, $ChangeInDistance_i = 0$. The vector X_i includes the distance to court before reform, allowing the coefficient γ_1 to capture the exogenous change in distance on bankruptcy outcomes relative to the pre-reform distance.

Estimation results are reported in Table A.6. The coefficient γ_1 is statistically not different from zero, suggesting that the change in distance to court has no systematic effect on bankruptcy outcomes.

3. The reform did not affect the distance between firms in absorbing or control jurisdictions and their assigned court.

TABLE A.6 – The impact of distance on bankruptcy outcomes

	R_{ijt}	P_{ijt}		S_{ijt}		
	(1)	(2)	(3)	(4)	(5)	(6)
Absorbed $\times$ Post	-0.0598*** (-4.25)	-0.0255*** (-3.46)	-0.00530 (-0.34)	0.00121 (0.28)	0.0169** (2.25)	0.0367** (2.08)
Absorbing $\times$ Post	-0.0260* (-1.82)	-0.00802 (-1.42)	-0.000793 (-0.08)	-0.00128 (-0.48)	-0.000696 (-0.10)	0.0211 (1.56)
Change in distance (km)	0.000117 (0.36)	-0.000189 (-1.27)	-0.000547 (-1.58)	-0.000105 (-0.89)	-0.000143 (-0.58)	-0.000167 (-0.36)
Unemployment rate	-0.0168** (-2.08)	-0.00724** (-2.31)	-0.00158 (-0.44)	-0.00320* (-1.79)	-0.00182 (-0.59)	-0.00399 (-0.78)
Year $\times$ Industry FE	✓	✓	✓	✓	✓	✓
Court FE	✓	✓	✓	✓	✓	✓
Firm controls	✓	✓	✓	✓	✓	✓
Observations	577,931	577,931	197,379	449,892	157,273	50,087
Adj. R ²	0.151	0.090	0.073	0.071	0.072	0.035

t statistics in parentheses

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

Note: Table A.6 reproduces the baseline specification, adding the change in distance to court for firms in absorbed jurisdictions as a control variable. Standard errors are clustered at the pre-reform court level.

A.5 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 (i) the characteristics of firms filing for bankruptcy are affected by the crisis differently across jurisdictions, and (ii) the recovery plan launched by the French government in response to the crisis differentially targeted firms across jurisdictions. We discuss these two concerns in the following subsections.

A.5.1 Pre-trends in firm characteristics and local conditions

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.

As suggested by [Iverson \(2018\)](#), we directly examine how firm characteristics evolved around the reform in the different jurisdictions. We estimate the following regression :

$$X_i = \alpha + \sum_{k=g,d} \left(\sum_{t \neq 2007} \phi_{kt} (\mathbb{1}_t \times Reform_{ij}) \right) + \theta_j + \theta_{st} + \epsilon_i \quad (\text{A.2})$$

We are interested in the coefficient ϕ_{kt} , which significance informs us about the reform's impact on firms' characteristics X_i . The results are displayed in Figure [A.4](#). They show mostly no correlation between firms' characteristics (firm size measured with the log of its total assets, leverage and debt composition⁴) and the reform.

The ϕ coefficients are overall not significantly different from zero. 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.

A further concern is that the financial crisis may have altered local economic conditions in ways that could affect bankruptcy outcomes across jurisdictions — either directly, by changing firm behavior, or indirectly, by influencing how judges evaluate cases. As noted by [Iverson \(2018\)](#), this could bias our results if those economic conditions vary systematically with treatment.

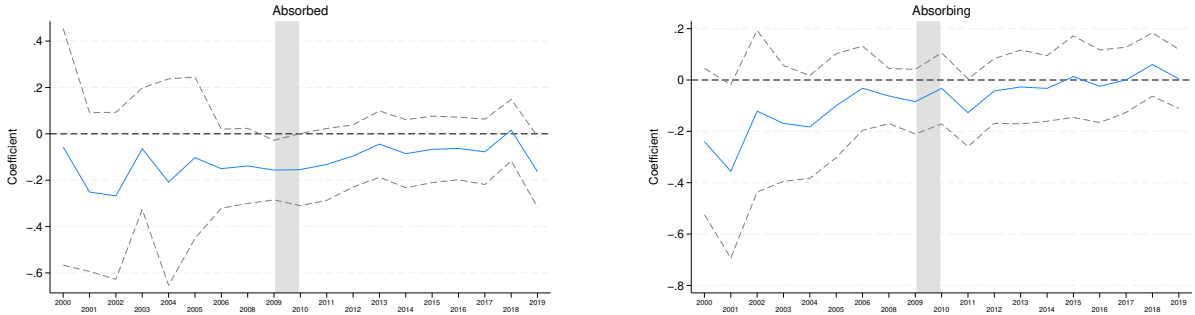
We test for a significant correlation between the unemployment rate and the reform by using the unemployment rate as a dependent variable in regression similar to equation (A.2). Compared to control courts, the coefficients ϕ are rather negative before reform and positive afterward in both absorbed and absorbing jurisdictions. We include the unemployment rate in all our main regressions to control for this heterogeneity between jurisdictions.

4. 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.

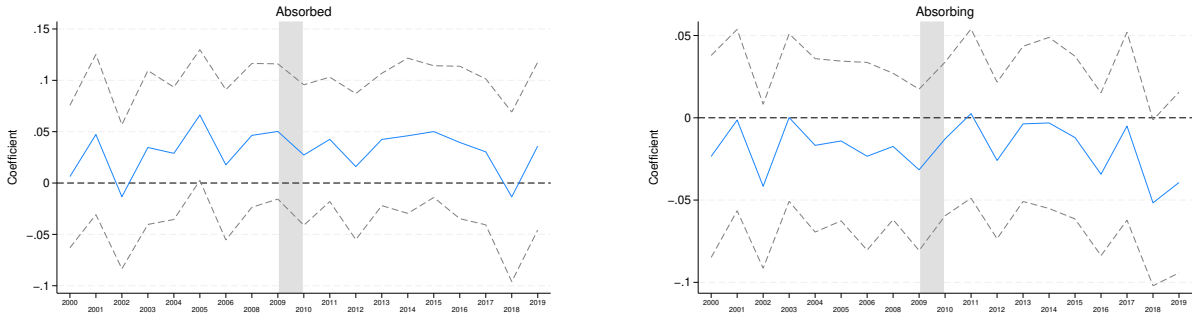
FIGURE A.4 – Dependent variables pre-trends

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 by running equation (A.2). Figure A.4 plots the coefficients ϕ and their 95% confidence intervals over time. The parallel trends assumption is satisfied if the coefficients are stable around the reform, indicating that it was not changing differently by type of jurisdiction prior to the reform.

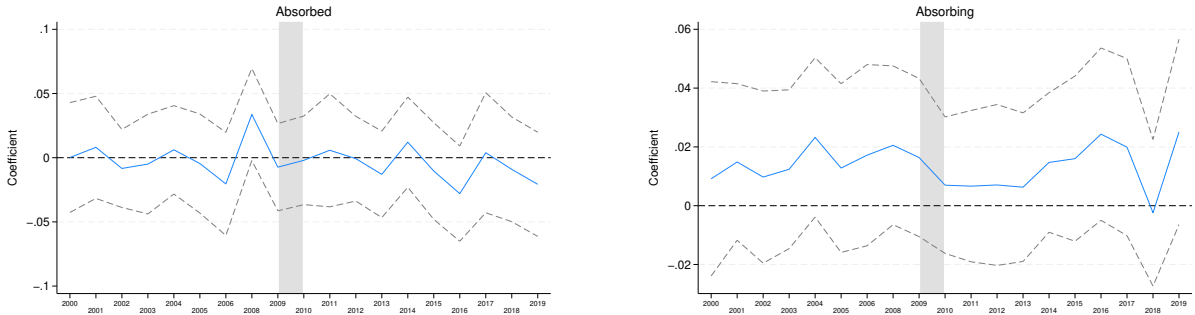
(a) Ln(Total Assets)



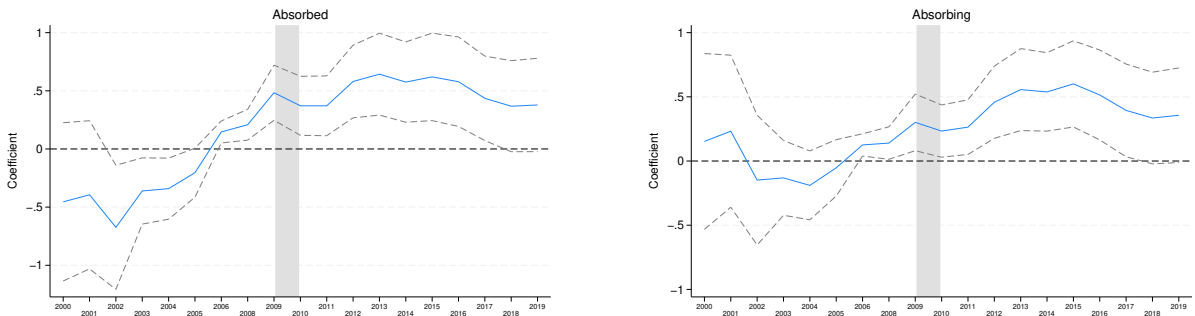
(b) Total Debt / Total Asset



(c) Supplier Debt / Total Debt



(d) Unemployment rate

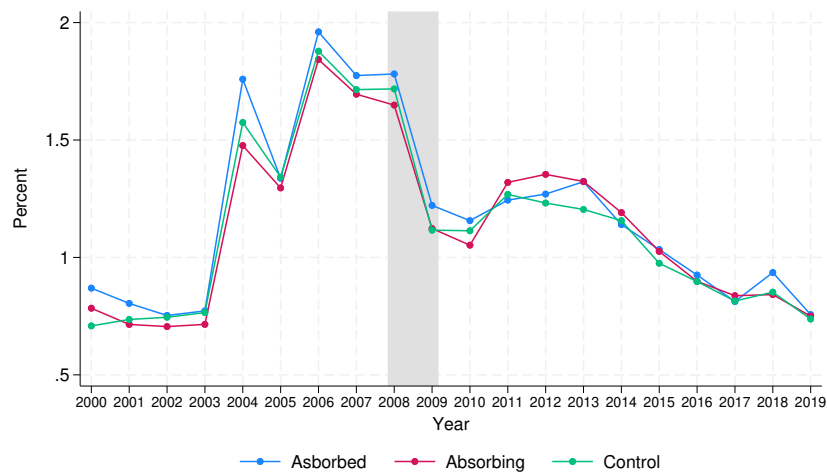


A.5.2 The policy response to the financial crisis

A remaining concern is that the crisis' support policies⁵ may have differentially affected firm viability across jurisdiction types. In 2008-2010, the French government's recovery plan notably included an €11 billion package of cash flow support measures to firms. While national in design, their effective impact on firm liquidity could have differed across jurisdictions if the composition of firms in terms of tax credit exposure varied systematically between court types.

To evaluate whether this measure had different impact on firms across jurisdictions, we study the evolution of firms' payment incidents around the time of the financial crisis. Payment incidents are drawn from the French Credit Register⁶. Figure A.5 plots the fraction of firms with at least one payment incident each year. The changes in 2004-2006 are concomitant with a change in the Banque de France's rating methodology, while the decrease in 2009 is consistent with the effect of the support measures. Importantly, the trajectories of payment incidents do not diverge across jurisdiction types and throughout the period. Our test cannot distinguish between a different impact of the crisis or of the recovery plan on firms' liquidity. This supports the view that firms' liquidity was affected in the same way across jurisdictions around the financial crisis.

FIGURE A.5 – Payment incidents by type of court



Note: Figure A.5 shows the fraction of firms by type of court with at least one payment incident per year between 2000 and 2019. The population of firms is drawn from the French Credit Register, with on average 1.5 million firms per year. The shaded area marks the reform period.

5. The main measures of the recovery plan were the following : (i) €11 billion aimed at easing cash flow constraints ; (ii) expansion of the short-time work scheme (*chômage partiel*) ; (iii) the national credit mediation scheme was launched to help firms renegotiate bank credit ; (iv) small firms benefited from a temporary payroll subsidy ; (v) expansion of the public investment bank Bpifrance state-guaranteed loan schemes for SMEs.

6. The French Credit Register provides the credit ratings of firms with at least €25K bank credit exposure. The rating values 7, 8 and 9 indicate whether a firm has experienced one payment incident or more on bills of exchange of an individual amount equal to or greater than €1,524.

Appendix B Conceptual Framework : Full exposition, derivations and proofs

This appendix provides the complete version of the conceptual framework presented in Section 3 of the paper. Section B.1 sets out the notation and the accounting identities linking the observed bankruptcy outcomes to the unobserved type-specific probabilities. Section B.2 defines Type 1 and Type 2 errors at each stage of the procedure. Section B.3 derives the impact of the reform on the observed outcomes under our identification assumptions and maps it to the difference-in-difference coefficients. Section B.4 derives the results of the error matrix (Table 1 in the paper). Section B.5 derives the results on first-stage errors. Section B.6 proves the cross-equation restrictions used as a robustness test (Table 2 in the paper).

B.1 Setup, notation and accounting identities

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 , nor the type of any individual firm.

The procedure has two stages. At the onset, the court either liquidates the firm or puts it into receivership ; for firms in receivership, the procedure ends either with a court-approved restructuring plan or with liquidation. We denote R_j^h (resp. R_j^l) the unobserved probability that a viable (resp. non-viable) firm is put into receivership by court j , and P_j^{Rh} (resp. P_j^{Rl}) the unobserved probability that a viable (resp. non-viable) firm restructures once in receivership. Finally, we denote s_j^{Ph} the survival rate of viable firms that restructure. By definition of viability, the survival rate of non-viable firms that restructure is null.

The statistician observes six probabilities at the court level : the probability of receivership R_j , the probability of restructuring P_j and the survival probability S_j , all defined over the population of firms filing for bankruptcy, as well as the corresponding probabilities on the conditional samples : the restructuring rate of firms in receivership P_j^R , the survival rate of firms in receivership S_j^R , and the survival rate of restructured firms S_j^P . These observed quantities are linked to the unobserved parameters by the following six accounting identities :

$$\left\{ \begin{array}{l} R_j = x_j R_j^h + (1 - x_j) R_j^l \\ P_j = x_j R_j^h P_j^{Rh} + (1 - x_j) R_j^l P_j^{Rl} \\ P_j^R = \frac{x_j R_j^h P_j^{Rh} + (1 - x_j) R_j^l P_j^{Rl}}{R_j} = \frac{P_j}{R_j} \\ S_j = s_j^{Ph} x_j R_j^h P_j^{Rh} \\ S_j^R = \frac{S_j}{R_j} \\ S_j^P = \frac{S_j}{P_j} \end{array} \right. \quad (\text{B.1})$$

The first equation decomposes the observed receivership rate into the type-specific receivership rates. The second decomposes the observed restructuring rate : a firm restructures if it is first put into receivership and then obtains a plan. The fourth states that only viable firms that restructure survive, a fraction s_j^{Ph} of them. The remaining three equations are the conditional counterparts.

Note that $S_j^P = s_j^{Ph} \times \frac{x_j R_j^h P_j^{Rh}}{x_j R_j^h P_j^{Rh} + (1 - x_j) R_j^l P_j^{Rl}}$: the survival rate after restructuring is the product of the survival rate of viable restructured firms and of the share of viable firms among restructured firms. This composition interpretation is used in the robustness test of Section B.6.

B.2 Type 1 and Type 2 errors at each stage of the procedure

An efficient court is one that restructures viable firms and liquidates non-viable ones. At the onset of the procedure, when deciding whether to put a firm into receivership or to liquidate it, the court faces two types of errors :

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

For firms put into receivership, there are again two types of errors : restructuring a non-viable firm and liquidating a viable one. Combining the first-stage and second-stage decisions, the overall errors over the whole bankruptcy process are :

- $T_1 = R_j^l P_j^{Rl}$, 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 encompass the whole bankruptcy process, they are more meaningful than T_{1R} and T_{2R} , which relate only to the first stage of the 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.

B.3 Impact of the reform on observed outcomes

We denote ΔZ the impact of the reform on variable Z . We make the two following identification assumptions :

(A1) The share of viable firms is not impacted by the reform : $\Delta x_j = 0$,

(A2) The survival rate of viable firms that restructure is not impacted by the reform : $\Delta s_j^{Ph} = 0$,

where both x_j and s_j^{Ph} are understood as adjusted for industry $\times$ year fixed effects, pre-reform court fixed effects, and firm characteristics, as in the empirical specification (equation 2 in the paper). These assumptions do not mean that the share of viable firms and the survival rate of viable firms that restructure are constant 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 it. Empirical support for these assumptions is provided in Figure 5 of the paper and in Appendix A.5.

Differentiating the system (B.1) under (A1) and (A2) yields :

$$\left\{ \begin{array}{l} \Delta R_j = x_j \Delta R_j^h + (1 - x_j) \Delta R_j^l \\ \Delta P_j = x_j (\Delta R_j^h P_j^{Rh} + \Delta P_j^{Rh} R_j^h) + (1 - x_j) (\Delta R_j^l P_j^{Rl} + \Delta P_j^{Rl} R_j^l) \\ \Delta P_j^R = \frac{\Delta P_j R_j - \Delta R_j P_j}{(R_j)^2} \\ \Delta S_j = s_j^{Ph} x_j (\Delta R_j^h P_j^{Rh} + \Delta P_j^{Rh} R_j^h) \\ \Delta S_j^R = \frac{\Delta S_j R_j - \Delta R_j S_j}{(R_j)^2} \\ \Delta S_j^P = \frac{\Delta S_j P_j - \Delta P_j S_j}{(P_j)^2} \end{array} \right. \quad (\text{B.2})$$

Each of the six left-hand-side terms is the reform's impact on one of the six observed outcomes, which is precisely what the six difference-in-difference equations estimate :

$$\begin{cases} \Delta R_j \equiv \beta^R \\ \Delta P_j \equiv \beta^P \\ \Delta P_j^R \equiv \beta^{P|R} \\ \Delta S_j \equiv \beta^S \\ \Delta S_j^R \equiv \beta^{S|R} \\ \Delta S_j^P \equiv \beta^{S|P} \end{cases} \quad (\text{B.3})$$

B.4 From the estimates to the overall Type 1 and Type 2 errors

Type 2 error. From the definition of the overall Type 2 error and the fourth equation of system (B.2) :

$$T_2 = 1 - R_j^h P_j^{Rh} \Rightarrow \Delta T_2 = - \left(\Delta R_j^h P_j^{Rh} + R_j^h \Delta P_j^{Rh} \right) = - \frac{\Delta S_j}{x_j s_j^{Ph}} \equiv - \frac{\beta^S}{x_j s_j^{Ph}} \quad (\text{B.4})$$

Since $x_j > 0$ and $s_j^{Ph} > 0$, the impact of the reform on T_2 is of the opposite sign to β^S . If the survival rate of firms after filing is positively affected by the reform ($\beta^S > 0$), the reform reduces the Type 2 error : fewer viable firms are liquidated. When $\beta^S < 0$, one can conclude the opposite : more viable firms are liquidated and the Type 2 error is higher because of the reform.

Type 1 error. From the definition of the overall Type 1 error :

$$T_1 = R_j^l P_j^{Rl} \Rightarrow \Delta T_1 = \Delta R_j^l P_j^{Rl} + \Delta P_j^{Rl} R_j^l \quad (\text{B.5})$$

Substituting $x_j(\Delta R_j^h P_j^{Rh} + \Delta P_j^{Rh} R_j^h) = \Delta S_j / s_j^{Ph}$ into the second equation of system (B.2) gives an explicit expression for ΔT_1 as a function of the estimated coefficients :

$$\Delta T_1 = \frac{1}{1 - x_j} \left(\beta^P - \frac{\beta^S}{s_j^{Ph}} \right) \quad (\text{B.6})$$

The sign of ΔT_1 therefore follows from the signs of β^P and β^S in most configurations. When $\beta^P \leq 0$ and $\beta^S > 0$ (or $\beta^P < 0$ and $\beta^S \geq 0$), then $\Delta T_1 < 0$: fewer non-viable firms are restructured. Symmetrically, when $\beta^P \geq 0$ and $\beta^S < 0$ (or $\beta^P > 0$ and $\beta^S \leq 0$), then $\Delta T_1 > 0$: more non-viable firms are restructured. When β^P and β^S are both strictly positive or both strictly negative, the sign of ΔT_1 is ambiguous and depends on the relative magnitudes of the two coefficients (as well as on s_j^{Ph}).

These results are summarized in the matrix below, reported as Table 1 in the paper. With this

matrix, one can go directly from the estimated impact of the reform on survival and restructuring rates after filing for bankruptcy to the qualitative impact on Type 1 and Type 2 errors.

TABLE B.7 – 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 $\uparrow T_2$

B.5 From the estimates to first-stage errors

The first equation of system (B.2) links β^R to the changes in the type-specific receivership rates :

$$\beta^R = x_j \Delta R_j^h + (1 - x_j) \Delta R_j^l, \quad \text{with } \Delta T_{1R} = \Delta R_j^l \text{ and } \Delta T_{2R} = -\Delta R_j^h. \quad (\text{B.7})$$

The sign of β^R alone does not identify the signs of ΔR_j^h and ΔR_j^l separately. We therefore add two assumptions :

(A3) 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 ($\Delta R_j^l \leq 0$).

(A4) 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 viable firm being put into receivership ($\Delta R_j^h \geq 0$).

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

It follows immediately that under (A3) :

$$\beta^R < 0 \Rightarrow \Delta T_{1R} \leq 0 \quad (\text{B.8})$$

and that under (A4) :

$$\beta^R > 0 \Rightarrow \Delta T_{2R} \leq 0 \quad (\text{B.9})$$

Combined with the results of Section B.4, the estimate of β^R thus indicates whether a change in the overall errors originates, at least partly, in the initial sorting of firms. For example, when the reform reduces the overall Type 1 error ($\Delta T_1 < 0$) and $\beta^R < 0$, the reduction in the restructuring of non-viable firms comes at least partly from fewer of these firms being put into receivership (lower T_{1R}), rather than only from fewer of them restructuring once in receivership.

B.6 Cross-equation restrictions : derivation of the robustness matrix

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 2 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 .

The economic interpretation is most transparent for $\beta^{S|P}$. As shown in Section B.1, the survival rate after restructuring S_j^P is the product of s_j^{Ph} (unchanged by the reform under (A2)) and of the share of viable firms among restructured firms. A reform that reduces the Type 1 error while not increasing the Type 2 error (e.g. $\beta^P < 0$ and $\beta^S \geq 0$) increases the share of viable firms among the firms that restructure; we then expect the survival rate after restructuring to increase, $\beta^{S|P} > 0$. Conversely, a reform that increases the Type 1 error while not decreasing the Type 2 error (e.g. $\beta^P > 0$ and $\beta^S \leq 0$) dilutes the pool of restructured firms with non-viable ones, and we expect $\beta^{S|P} < 0$. The same reasoning applies to $\beta^{P|R}$ and $\beta^{S|R}$ with respect to the composition of firms put into receivership.

These restrictions are testable : the six coefficients are estimated from six separate regressions on different (nested) samples, and nothing in the estimation constrains them to be mutually consistent. Verifying that the estimated signs satisfy the matrix below is therefore a genuine specification check of the framework and of the econometric estimates. This verification is carried out in Section 5 of the paper.

TABLE B.8 – 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	$\beta^{P R} < 0$	$\beta^{P R} < 0$	?			

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