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Research Article

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Posted Date: November 24th, 2026

DOI: https://doi.org/10.21203/rs.3.rs-11760989/v1

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Additional Declarations: No competing interests reported.

Does harmonising insolvency rules harmonise their application? Evidence from French commercial courts

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Abstract

When insolvency law leaves wide discretion to decentralised courts, what determines how it is applied—the rules, the territory or the jurisdiction? And which lever can make its application more uniform? European Union law has twice harmonised insolvency rules since 2019, leaving court organisation to Member States. We study France, which over 2008–2023 reformed its insolvency rules several times without changing its courts, and redrew the map of its commercial courts without changing the rules. Using every insolvency judgment published in the official gazette, we follow the 160,000 reorganisation proceedings opened in 2008–2023 and separate true differences between courts from sampling noise with independent split samples. True differences in plan adoption have a standard deviation of 5 to 7 percentage points around a mean of 21 to 28 percent; case composition and local conditions explain one sixth of their variance, and they persist within *départements* and for cases handled by the same insolvency practitioner. Their dispersion was no lower in 2017–2023 than before 2014, despite a major reform of the rules, compulsory training of judges and the transposition of Directive (EU) 2019/1023; a difference-in-differences based on past court rates wrongly suggests convergence. By contrast, when the 2009 reform of the judicial map transferred the territories of abolished courts to neighbouring courts, these territories immediately adopted their new court's screening of cases at opening. Practices belong to jurisdictions, not to territories: harmonising rules does not harmonise their application, and the organisation of courts is a policy lever in its own right.

Keywords: Bankruptcy courts, Judicial heterogeneity, Implementation of legal reforms, Lay judges, Court mergers, Regression to the mean

JEL Classification: K22 , G33 , K41 , K40

1 Introduction

When the law leaves wide discretion to the courts that apply it, what determines how it is applied: the rules, the territory in which a case arises, or the jurisdiction that hears it? And which of these can a legislator act upon to make the application of the law more uniform? The gap between the law in books and the law in action is an old theme of legal scholarship (Pound, 1910). It becomes a pressing policy question when a legislator seeks convergence of outcomes by harmonising rules that decentralised courts will apply.

Insolvency law in the European Union (EU) is a case in point. Directive (EU) 2019/1023 on preventive restructuring frameworks set minimum standards that Member States transposed from 2021, and Directive (EU) 2026/799, published in April 2026, harmonises further aspects of insolvency law, from avoidance actions to pre-packaged sales and creditors' committees. Both instruments harmonise rules. The organisation of the courts that apply them is left to Member States: Article 25 of the 2019 Directive only requires that judges dealing with these procedures receive suitable training and have the necessary expertise, "without prejudice to judicial independence and to any differences in the organisation of the judiciary across the Union", and does not require that they specialise. Commentators have accordingly expected its harmonising effect to be limited (Garrido et al., 2021). Whether harmonised rules produce harmonised outcomes depends on what drives differences in application, which is an empirical question.

The economics literature shows that insolvency rules matter—creditor rights and the trade-off between liquidation and reorganisation shape the supply of credit and the reallocation of capital (Davydenko & Franks, 2008; Djankov et al., 2007; La Porta et al., 1997)—but also gives reasons to expect that courts matter as much as rules. The effect of a reform depends on the courts that implement it: a Brazilian bankruptcy reform increased secured lending and investment more where courts were less congested (Ponticelli & Alencar, 2016), and lighter caseloads change the outcomes of US Chapter 11 cases (Iverson, 2018). Across countries, the effectiveness of law depends more on how it was received than on the legal family it comes from (Berkowitz et al., 2003), more formalistic judicial procedures go with longer proceedings and less consistent decisions (Djankov et al., 2003), and the efficiency of debt enforcement varies widely and predicts the development of debt markets (Djankov et al., 2008). Within countries, the identity of the judge changes the outcome of bankruptcy cases, a fact widely used to build instruments (Antill, 2022; Araujo et al., 2023; Bernstein et al., 2019; Chang & Schoar, 2013; Dobbie & Song, 2015; Iverson et al., 2023). The closest evidence on persistent differences between courts concerns US personal bankruptcy, where the share of Chapter 13 filings varies widely and durably across districts, a pattern described as "local legal culture" (Lawless & Littwin, 2018; Sullivan et al., 1994); these are, however, procedural choices strongly shaped by attorneys (Braucher et al., 2012).

Three questions remain open. First, how large are differences between courts once sampling noise is removed? Most courts handle few cases, so raw differences overstate true ones, and the local legal culture literature relies on raw proportions. Second, what carries these differences? A court's practice may reflect its judges, its registry and the

professionals around it, or the firms and economy of the territory it serves; local legal culture conflates the two. Third, which lever moves them? Reforms of the rules apply to all courts at once and are rarely evaluated in terms of dispersion, and changes in court organisation are rarer still.

France provides an unusually informative setting. Corporate insolvency cases are decided by commercial courts staffed by lay judges, business people elected by their peers, who apply a standard that the Commercial Code does not define: a reorganisation plan is adopted when there is "a serious possibility for the firm to be saved". Parliamentary inquiries have long pointed to divergent practices across courts (Montebourg & Colcombet, 1998; Untermaier & Bonnot, 2013), the profile of judges affects the chances of reorganisation (Blazy & Esquerré, 2021), and differences across courts have been used to identify the costs of bankruptcy (Epaulard & Zapha, 2022). Above all, France pulled two distinct levers, the rules and the courts, separately and within a short period. It reformed its insolvency rules in 2014, made training compulsory for commercial judges from 2016 and transposed the 2019 Directive in 2021, without changing its courts. And on 1 January 2009 it abolished 55 commercial courts and transferred their territories to neighbouring courts, without changing the rules. Earlier work on these mergers shows that the practices of absorbing courts prevailed (Arcuri et al., 2023; Epaulard & Zapha, 2025; Esquerré, 2019); we ask whether the practice belongs to the court or to the territory. Since 2025, twelve courts have been experimenting with a new organisation as economic activity courts (*tribunaux des activités économiques*), so the question of which lever works is also a live domestic one.

We reconstruct every collective-proceeding judgment published in the *Bulletin officiel des annonces civiles et commerciales* (Bodacc), the official gazette of commercial announcements, between 2008 and 2025, including judgments published under generic labels, which we reclassify from their text. We follow the 160,000 reorganisation proceedings (*redressements judiciaires*) opened between 2008 and 2023 to their first outcome within 24 months—a reorganisation plan, conversion into liquidation or the sale of the business—and locate each debtor, from its published address, in the territory of the court that had jurisdiction over it in 2008. To separate true differences from noise, we borrow the methods developed to measure teacher value added (Chetty et al., 2014; Kane & Staiger, 2002): the cases of each court are split into two independent half-samples, whose covariance measures the variance of true differences between courts, while covariances across periods, computed on distinct cases, measure their persistence. The same split provides instruments that correct for measurement error when past court practice is used as a regressor.

The answer is that practices belong to jurisdictions. First, differences between courts are real and sizeable: the noise-corrected standard deviation of plan-adoption rates is 5.0 to 6.9 percentage points, for a mean rate of 21 to 28 percent, so that the outcome of about 350 firms a year depends on the court that hears them. Observable case composition and local economic conditions explain 16 percent of the variance, and the differences persist between courts of the same *département* and for cases handled by the same court-appointed insolvency practitioner. Second, differences persist but drift: their correlation from one three-year period to the next is 0.87 to 0.99 until 2021, but only 0.36 between 2017–2019 and 2022–2023. Third, rule reforms did not

compress them: their dispersion in 2017–2023 is no lower than before 2014, and the data rule out a reduction of more than a sixth of the standard deviation. A standard difference-in-differences that uses past court rates as treatment intensity suggests strong convergence; we show that it reflects noise and drift, not harmonisation. Fourth, changing the jurisdiction moved them at once: after the 2009 mergers, the former territories of abolished courts immediately adopted their new court's practice in screening cases at opening, whereas a maintained court's own practice persists at about 80 percent. This test can only be run on screening, the one decision abolished courts took in sufficient numbers before disappearing, and it partly rests on a single territory; it is consistent with the persistence of differences between courts of the same *département*. Fifth, discretion moves across stages and has no clear cost: since 2017, courts that open more reorganisations adopt fewer plans, and the additional plans adopted by more lenient courts do not fail significantly more often, although an excess failure rate of a few points cannot be ruled out.

The paper contributes to three strands of the law and economics literature. The first is the implementation of legal reforms and the harmonisation of insolvency law. We provide direct evidence that, within a single country and under a single code, harmonising rules did not harmonise their application, whereas changing the jurisdiction did: the organisation of courts—their territorial map, their composition and the professionals around them—is a lever in its own right, which bears on the design and evaluation of the EU insolvency directives and on the debate on specialised courts (Garoupa et al., 2008). The second is the measurement of judicial heterogeneity and local legal culture. We provide, to our knowledge, the first noise-corrected estimates of the size, persistence and evolution of differences between courts in corporate insolvency, show that raw indicators overstate their dispersion while understating their persistence, and separate the court from the territory it serves. The third is methodological. A negative slope of a future rate on a past rate does not imply a reduction in dispersion—"Galton's fallacy" in the convergence literature (Friedman, 1992; Quah, 1993). A continuous-treatment difference-in-differences (Callaway et al., 2024) whose intensity is a past value of the outcome combines noise shared with the baseline (Chay et al., 2005), the drift of differences and their possible compression; an exact identity separates the last two, and a simulation shows that only a comparison of dispersions distinguishes harmonisation from its absence. Designs that use past court leniency as treatment intensity are common in empirical law and economics, and evaluations of harmonisation, including that of the French economic activity courts, should rely on dispersions instead. The reconstructed panel, built from public sources and validated registry by registry, provides a baseline for such evaluations.

Section 2 describes the institutional setting and Section 3 states the two levers and their predictions. Section 4 presents the data and Section 5 the empirical strategy. Section 6 reports the results and Section 7 discusses their implications for the implementation and harmonisation of insolvency law.

2 Institutional setting

2.1 Procedures

French insolvency law provides three collective proceedings. Safeguard (*sauvegarde*), created in 2005, is opened at the request of a debtor that is not yet insolvent but faces difficulties it cannot overcome. Reorganisation (*redressement judiciaire*) concerns a debtor in cessation of payments (insolvency on a cash-flow basis) whose rescue is not manifestly impossible; it may be requested by the debtor, which must in principle file within 45 days of the cessation of payments, by a creditor or by the public prosecutor. The court fixes the date of cessation of payments and may backdate it by up to 18 months (Commercial Code, art. L631-8). Judicial liquidation is opened directly when rescue is manifestly impossible; this choice, made by the court at opening, determines which cases enter reorganisation.

During the observation period that follows the opening of a reorganisation, the court may convert the proceeding into liquidation at any time (art. L631-15 II). At its end, it adopts a reorganisation plan or orders the sale of the business. The plan is adopted when there is "a serious possibility for the firm to be saved" (art. L626-1, applicable through art. L631-19), a notion the Code does not define and which is left to the judges' assessment. This decision is our main object of study; the opening decision is examined in Section 6.3.

2.2 Lay judges and the jurisdiction

Commercial courts are staffed by lay judges, unpaid business people elected for renewable terms by representatives of the business community; there is no professional judge on the bench. The reform of the judicial map (Decree No. 2008-146 of 15 February 2008) abolished 55 commercial courts and created six as of 1 January 2009, the territory of each abolished court being transferred to a neighbouring court; metropolitan France then had about 134 commercial courts. In Alsace-Moselle, commercial cases are heard by commercial chambers of the general civil courts, composed of one professional judge and two lay assessors. A court is more than its judges: the insolvency practitioners it appoints (*mandataires* and *administrateurs judiciaires*), the public prosecutor, counsel and the court registry also shape the outcome of proceedings. We call the "jurisdiction effect" the deviation of a court's plan-adoption rate from the mean that is not explained by the observable composition of its cases; it covers the whole chain of decisions, from the screening of cases at opening to their outcome, and Section 6.3 examines its components.

2.3 A succession of reforms

Ordinance No. 2014-326 of 12 March 2014, in force on 1 July 2014, strengthened out-of-court procedures, created an accelerated safeguard procedure and a simplified procedure for individual debtors without assets (*rétablissement professionnel*), and reinforced the role of creditors and of the public prosecutor. The Law of 18 November 2016 made training compulsory for lay commercial judges; it applies to judges elected after its entry into force, and thus at the pace of each court's renewal. The 2020 texts

adapted the procedures during the COVID-19 crisis, while state-guaranteed loans and payment deferrals reduced the number of openings. Ordinance No. 2021-1193 of 15 September 2021 transposed Directive (EU) 2019/1023. None of these texts explicitly aims at reducing differences between courts and, except for compulsory training, all apply to all courts at the same date. The reform of the judicial map is of a different nature: it changed no rule, but changed the court with jurisdiction over part of the territory. The specialised commercial courts created in 2015 handle a very small share of the reorganisations studied here. Since 1 January 2025, twelve commercial courts have been experimenting, until the end of 2028, with a new organisation as economic activity courts (Law No. 2023-1059 of 20 November 2023, art. 26), with an evaluation committee; it is too early to assess them.

These French reforms are embedded in a European movement. Directive (EU) 2019/1023 set minimum standards for preventive restructuring frameworks, the discharge of debt and the efficiency of procedures; France transposed it in 2021. Its Article 25 requires Member States to ensure that members of the judicial and administrative authorities dealing with these procedures receive suitable training and have the necessary expertise, without prejudice to judicial independence and to differences in the organisation of the judiciary, and its recitals specify that judges need not deal exclusively with these matters. Its Article 33 provides for a periodic Commission review of its application and impact. Directive (EU) 2026/799, published on 1 April 2026 and to be transposed by early 2029, harmonises avoidance actions, pre-packaged sales, the duty to request the opening of proceedings and creditors' committees. Both directives harmonise rules; neither harmonises the organisation of the courts that apply them.

3 Two levers: rules and jurisdictions

A legislator who wants the law to be applied more uniformly can act on two levers. The first is the rules: a more precise standard, new procedures, new powers for creditors or the public prosecutor, or compulsory training of judges. The second is the jurisdiction: which court hears which cases, how it is composed, and which professionals work around it. The two levers make different predictions, which the French experience allows us to confront with the data.

If differences in application stem from the rules—an undefined standard that each court interprets in its own way—a reform that clarifies or reshapes them should compress the distribution of court practices. The dispersion of true court effects should fall after the reform, and the slope of post-reform effects on pre-reform effects should decline. A mere reshuffling of relative positions at constant dispersion would not be harmonisation.

If differences stem from the territory—the viability of local firms, the local economy, regional networks of professionals—they should survive a change of court. When the territory of an abolished court is transferred to a neighbouring court, its former practice should persist for its former territory, as a maintained court's practice persists for its own.

If differences stem from the jurisdiction—its judges, its registry, the practitioners it appoints—they should persist between courts that serve similar territories, for instance

within the same *département*, and disappear for a territory when its court changes: the former territory should adopt the practice of its new court.

The differences must also be real, persistent and legible for any of these mechanisms to matter for economic agents. Froute (2007), a court's reputation for favouring continuation plays an informational role: when a court is known to be favourable, more distressed firms declare their difficulties rather than conceal them, which improves the average quality of borrowers financed outside the proceedings. The idea of a court screening viable from non-viable firms goes back to White (1994), and Gennaioli and Rossi (2010) analyse the role of judicial discretion in creditor protection. Such a reputation requires that differences can be learned from a court's past decisions, and it makes a court's pool of cases depend on its past practice, so that the differences we measure combine the court's practice and the debtors' response. Finally, a more lenient practice may have a cost if the additional plans fail more often.

We therefore measure the size, persistence and legibility of differences between courts (Sections 6.1 to 6.3), test the rules lever on the 2014 reform (Section 6.4) and the jurisdiction lever on the 2009 mergers (Section 6.5), and examine the use of safeguard proceedings and the quality of plans (Sections 6.6 and 6.7).

4 Data

4.1 The Bodacc and the reconstruction of proceedings

Publication in the Bodacc makes insolvency judgments enforceable against third parties. The French Directorate of Legal and Administrative Information (DILA) releases these announcements as open data, as annual archives for 2008–2016 and 2022–2025 and as files by issue for 2017–2021: 3.48 million announcements. The numbering of the 2017–2021 issues is continuous, and the number of judgments shows no break at the format changes of 2017 and 2022 (Online Resource 1, Section S1).

Reconstructing proceedings raises five difficulties. The encoding changes from ISO-8859-1 to UTF-8 in 2017; we classify each judgment from a skeleton of its label stripped of non-ASCII characters. Several registries publish most of their decisions under generic labels such as "other judgment"; at the Paris commercial court, for instance, 80 percent of liquidation judgments are published in this way. We reclassify these announcements from the text of the judgment. Applied to the 212,488 correctly labelled announcements, our rules recover the correct category in 99.3 to 99.8 percent of cases; on a sample of 90 generic announcements drawn from the nine metropolitan registries that publish most of them and coded by hand, 89 are correctly classified (116 out of 120 when three overseas registries are added), and the rules were corrected for the errors found. A liquidation pronounced while a reorganisation or safeguard opened within the previous 24 months is pending is treated as a conversion. We keep a single record per court, judgment type, firm and date to remove republications, and 62 manually verified matches unify different spellings of the same court's name. Each outcome is linked to the opening of the same debtor, identified by its SIREN (the national firm identifier), whichever court publishes it, which covers the 2009 mergers and the renaming of courts in 2025; only the first outcome is kept. A reorganisation with no published plan, conversion or sale whose plan is later rescinded is counted as

having ended in a plan (1,137 cases, mostly in 2008–2013). At the *département* level, the openings thus reconstructed have a correlation of 0.98 with the business failures published by the national statistical institute (Insee), and their annual changes a correlation of 0.91 (Online Resource 1, Section S1). Data processing and estimation code was written with the assistance of a large language model (Claude, Anthropic) under the author's direction; all steps are documented in the replication package.

We keep legal persons in metropolitan France. Commercial courts record 160,514 reorganisation openings between 2008 and 2023, as well as 400,008 direct liquidations and 14,649 safeguards. We follow the 159,356 reorganisations whose debtor is identified by its SIREN. The opening judgment names the insolvency practitioner (92 percent of cases) and the date of cessation of payments fixed by the court (78 percent).

4.2 Variables

For court j and opening year t, the plan-adoption rate is

$$r_{jt} = \frac{\text{Plans}_{jt}}{\text{Plans}_{jt} + \text{Conversions}_{jt} + \text{Sales}_{jt}},$$

where the three terms count the reorganisations opened in year t whose first outcome within 24 months is, respectively, a reorganisation plan, conversion into liquidation or a sale plan. The 24-month window covers the maximum length of the observation period and captures 99.8 percent of the outcomes observed within three years. Conversions occur at a median of 2.5 months after opening and plans at 13 months, so a rate computed on the decisions of a calendar year would mix openings of different years. The 5.0 percent of reorganisations with no observed outcome within the window are excluded from the denominator; their share, 11 percent in 2008–2010 and 7 percent in 2011–2013, is about 2 percent afterwards, and Section 6.4 bounds the results by counting them as plans and then as conversions. Sales are included in the denominator because they end the continuation of the business by the debtor; the results are similar when sales are excluded or counted as successes.

This rate is conditional on the opening of a reorganisation: a court that directs doubtful cases to direct liquidation hears more favourable reorganisations. We therefore also measure the share of reorganisations among reorganisation and direct-liquidation openings ("screening at opening"). The safeguard share relates safeguards to all proceedings opened.

Each SIREN is matched to the Sirene business register (October 2026 extract), which gives the main activity (88 divisions of the French classification of activities, NAF rev. 2) and the date of creation; the match covers 99.98 percent of the reorganisations followed. Local economic conditions are measured by the local unemployment rate and the number of business failures by *département* and year (Insee). The register's headcount band, recorded after the outcome, is missing for 99.7 percent of debtors whose reorganisation was converted, against 73 percent of those that obtained a plan: it depends on the outcome and is not used as a control.

Each announcement also gives the debtor's address. Its postal code places the debtor in the territory of the court that had jurisdiction over its municipality in 2008, defined as the court that pronounced most of the 2008 judgments concerning that

postal code; we keep the 4,210 postal codes for which at least 80 percent of these judgments come from the same court. 81 percent of the reorganisations followed are located in this way (Online Resource 1, Section S8).

4.3 First facts

The plan-adoption rate rises from 24 percent for reorganisations opened in 2008 to 25–28 percent for those opened in 2009–2015, then falls to 22 percent for openings of 2018–2019 (Table 1 and Fig. 1). It reaches a low of 17 percent for openings of 2021 before recovering to 21 percent in 2023. This trough may reflect the selection of cases under the emergency measures or the follow-up of 2021 openings, which coincides with the end of payment deferrals and the repayment of guaranteed loans; the share of reorganisations among openings, the lowest of the period in 2021 (22 percent), does not settle the question. Each year, the gap between the 10th and 90th percentiles of courts is about 20 points; much of this apparent dispersion is sampling noise.

Table 1 Openings of collective proceedings and outcomes of reorganisation proceedings in commercial courts, by year of opening

Year	Reorganisations opened	Direct liquidations	Safeguards	Plans	Conversions	Sales	No outcome	Plan-adoption rate (%)
2008	8,583	19,480	430	1,756	5,127	394	1,219	24.1
2010	10,995	24,487	905	2,695	6,851	527	883	26.8
2012	10,915	25,571	1,056	2,619	6,795	489	948	26.4
2014	12,045	29,837	1,285	3,136	8,066	564	195	26.7
2016	11,594	28,172	996	2,862	7,926	475	257	25.4
2018	10,967	26,746	787	2,333	7,812	441	265	22.0
2020	5,464	17,282	646	1,184	3,763	286	186	22.6
2021	4,396	15,899	541	705	3,276	235	159	16.7
2022	7,382	24,095	910	1,314	5,516	350	174	18.3
2023	11,464	31,646	1,265	2,315	8,297	493	301	20.8
2008–2023	160,514	400,008	14,649	36,734	107,533	7,127	7,962	24.3

Notes: selected years; all years are in the replication files. Plans, conversions, sales and "no outcome": first outcome, within 24 months and in any court, of the reorganisations opened in the year (debtors identified by their SIREN); plans include reorganisations whose plan is later rescinded although its adoption was not published. Plan-adoption rate = plans / (plans + conversions + sales). There are 133 to 135 commercial courts each year from 2009 (181 in 2008, before the reform of the judicial map). Legal persons, commercial courts, metropolitan France. Source: Bodacc 2008–2025, author's calculations

5 Empirical strategy

5.1 Separating signal from noise

Let r_{jp} be the plan-adoption rate of court j over opening period p: three-year blocks (2008–2010, 2011–2013, 2014–2016, 2017–2019), then two-year blocks (2020–2021 and 2022–2023). This rate decomposes as $r_{jp} = \mu_{jp} + e_{jp}$, where μ_{jp} is the court's own propensity and e_{jp} a sampling error. The cases of each court are split into two half-samples A and B according to the parity of the last digit of the debtor's SIREN, a

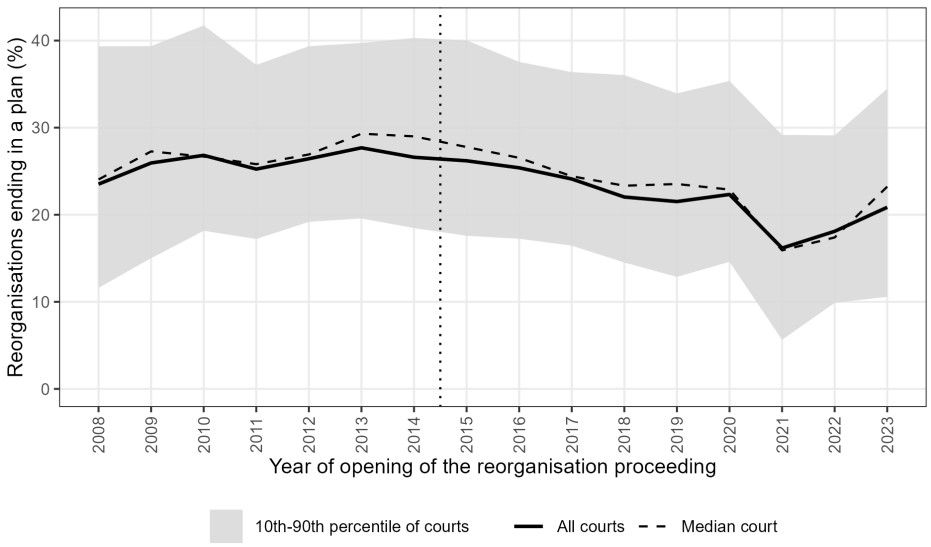

Fig. 1 Plan-adoption rate by year of opening of the reorganisation proceeding. Solid line: all courts; dashed line: median court; shaded area: 10th to 90th percentile of courts with at least 20 reorganisations reaching an outcome; dotted vertical line: entry into force of the 2014 ordinance

check digit unrelated to the judicial outcome. As the two halves contain different cases decided by the same court in the same period,

$$\mathrm{Cov}_j\left(r_{jp}^A, r_{jp}^B\right) = \mathrm{Var}_j\left(\mu_{jp}\right).$$

This covariance measures the variance of the signal. Shocks common to both halves—the simultaneous failure of companies of the same group, publication practices of the registry—are also included in the signal; Section 6.1 checks that the results do not depend on them. The correlation of court effects between periods p and q is obtained by dividing $\mathrm{Cov}_j(r_{jp}, r_{jq})$, computed on distinct cases, by the square root of the product of the two signal variances. We keep court-period pairs with at least 15 reorganisations reaching an outcome in each half-sample; unless stated otherwise, each court receives the same weight and confidence intervals are obtained by bootstrapping courts.

Suppose that annual effects follow an autoregressive process, $\mu_{jt} = \rho\,\mu_{j,t-1} + \eta_{jt}$. As the rate of a block is an average over its years, the covariance between two blocks P and Q is proportional to $K_\rho(P,Q)$, the mean of $\rho^{|t-s|}$ over the pairs of years of the two blocks (with equal weights for each year). We estimate ρ by minimum distance on the fifteen correlations between periods and compare it with a model that includes a permanent component (Online Resource 1, Section S3).

Without any assumption on the evolution of dispersion, the slope of a later rate r_{jt} on a baseline rate r_j^0 measured on independent cases equals $\mathrm{Cov}(\mu_t, \mu_0)/\,\mathrm{Var}(r^0) = \lambda\,\kappa_t\,\sigma_t/\sigma_0$, where $\lambda = \sigma_0^2/\,\mathrm{Var}(r^0)$ is the reliability of the baseline, σ_t the standard deviation of the signal and κ_t the correlation of court effects with those of the baseline

period. A difference-in-differences that uses this rate as treatment intensity therefore estimates, between a pre-period a and a post-period b,

$$\beta = \frac{\lambda}{\sigma_0}\left[\kappa_b\,\sigma_b - \kappa_a\,\sigma_a\right] = \underbrace{\frac{\lambda\,\sigma_a}{\sigma_0}\left(\kappa_b - \kappa_a\right)}_{\text{loss of correlation}} + \underbrace{\frac{\lambda\,\kappa_b}{\sigma_0}\left(\sigma_b - \sigma_a\right)}_{\text{change in dispersion}} .$$

The first term is negative as soon as court effects drift, even without any reform; only the second measures a compression of the distribution. When the baseline shares cases with pre-reform years, the slope for those years is further inflated by common sampling error (Chay et al., 2005).

5.2 Case composition, local economies and insolvency practitioners

A court may display a high rate because it hears more viable firms. We therefore replace the plan indicator with the residual obtained after accounting for the debtor's characteristics, with coefficients estimated in a common regression that includes court-period fixed effects so as not to absorb differences specific to courts (Chetty et al., 2014); cross-fitting, in which coefficients estimated on one half are applied to the other, prevents a common estimation error from inflating the covariance. We successively add legal form (M1), sector (M2), age (M3), and fluctuations of local economic conditions around their court-specific mean (M4). M4 is our reference specification. Specification M5 estimates all coefficients, including those on the levels of local conditions, without court fixed effects; as these levels are measured by *département*, it attributes to local conditions part of the differences between courts of different *départements* and provides an upper bound on the share explained.

Unmeasured local factors are addressed by comparing courts within the same *département*. For a court in a *département* with $n_d \geq 2$ courts observed over the period, we compute the deviation of each half from the unweighted mean of the same half in the other $n_d - 1$ courts; the expectation of the product of these deviations is then exactly $\sigma_w^2(1 + 1/(n_d - 1))$, where σ_w^2 is the true within-*département* variance of court effects. Each product is corrected by its own factor, then products are averaged and divided by the signal variance on the same court-period pairs.

Insolvency practitioners (*mandataires judiciaires*), appointed by the court, often work in several courts. We write the rate of a court $\times$ practitioner cell as $c_{jm} = \theta_j + \mu_m + \eta_{jm}$. Computed on different half-samples, the covariance between two cells of the same court with different practitioners estimates $\mathrm{Var}(\theta)$, and that between two cells of the same practitioner in different courts estimates $\mathrm{Var}(\mu)$, without the interaction η. Screening at opening is measured by the share of reorganisations among reorganisation and direct-liquidation openings; its true covariance with the plan-adoption rate is obtained by crossing half-samples.

5.3 Did reforms reduce heterogeneity?

Had a reform harmonised practices, the standard deviation of the signal should have fallen, and the slope of post-reform effects on pre-reform effects, $\kappa\,\sigma_{post}/\sigma_{pre}$, should

be lower than before the reform. A correlation is not enough: a proportional compression of differences leaves it unchanged, whereas a reshuffling at constant dispersion reduces it. These comparisons are not affected by regression to the mean due to noise. For comparison with standard evaluations, we also estimate a continuous-treatment difference-in-differences:

$$r_{jt} = \alpha_j + \alpha_t + \sum_{k \neq 2013} \beta_k \, \mathbf{1}[t = k] \, \tilde{r}_j^{\,0} + \varepsilon_{jt},$$

where t is the opening year and $\tilde{r}_j^{\,0}$ the court's centred baseline rate. It is measured either on 2011–2013, a period overlapping with the pre-reform years, the estimation then covering 2008–2023, or on 2008–2010, the estimation covering 2011–2023 with no case in common with the baseline. Regressions are weighted by the number of reorganisations reaching an outcome, with standard errors clustered by court.

5.4 The 2009 court mergers: jurisdiction or territory?

The successor of an abolished court a is the court that hears most of its former postal codes in 2010–2011. For the 51 abolished courts for which a successor can be identified, it hears a median of 99.3 percent of them, and it coincides in 98 percent of cases with the court that publishes later judgments on the debtors of the abolished court. After the merger, successor c hears both its original territory and the former territory T_a. For a case i opened from 2009 onwards in a maintained court, we estimate

$$y_i = \pi \, r_{c,2008} + \beta_T \, \mathbf{1}[i \in T_a] \, (r_{a,2008} - r_{c,2008}) + \delta \, \mathbf{1}[i \in T_a] + X_i'\gamma + \alpha_t + \varepsilon_i,$$

where $r_{c,2008}$ and $r_{a,2008}$ are the 2008 practices of the court that hears the case and of the former court of its territory, X_i the debtor's legal form, sector and age, and α_t year effects. The coefficient π measures the persistence of a court's practice and β_T the weight that the former practice retains for the former territory. If practice belongs to the jurisdiction, $\beta_T = 0$. If it belongs to the territory—its firms, its economy, its professionals—the former territory keeps following the former practice as a maintained court follows its own, and $\beta_T = \pi$. Because each territory is compared with itself, the test neutralises everything specific to it and stable over time, which the within-*département* comparison does only for *département*-level factors. The 2008 rates are measured on half-sample A and instrumented by those of half-sample B, which corrects for attenuation due to noise; Anderson–Rubin confidence sets (Anderson & Rubin, 1949) remain valid when instruments are weak. Standard errors are clustered by court.

As the open Bodacc data begin in 2008, the pre-merger practice of abolished courts is observed over a single year, and only for decisions they took in sufficient numbers. This is the case for screening at opening, y_i then indicating the opening of a reorganisation rather than a direct liquidation: in 2008, an abolished court pronounced a median of 36 openings but only 14 outcome decisions on reorganisations. For the plan-adoption rate, we therefore only compare, after the merger, the true gap between the former territory and the original territory of the same successor with the true gap between two distinct courts of the same *département*, both computed from half-samples.

6 Results

6.1 The size of heterogeneity

The noise-corrected standard deviation of plan-adoption rates is between 5.0 and 5.9 points after 2011, except in 2020–2021 (6.5 points), and reaches 6.9 points in 2008–2010 (Table 2). Relative to a mean rate of 21 to 28 percent, a court one standard deviation above the mean adopts a plan in about a quarter more cases; under normality, the true gap between the first and last deciles of courts is 13 to 18 points depending on the period, although the distribution is skewed (Fig. 2). The signal accounts for 63 to 75 percent of the raw variance of rates computed over three years. Weighted by the number of cases, the standard deviation is 5.5 to 6.9 points. Summing the estimated deviations of each court from the mean, the outcome of about 350 reorganisations a year, out of some 9,000 reaching an outcome, depends on the court that hears them.

These estimates are robust to the main sources of bias (Online Resource 1, Section S5). Grouping linked cases reduces the standard deviation by 3 to 15 percent; full-sample estimators give 5.2 to 7.0 points; cases randomly reassigned across courts give less than one point. Estimated court effects are unrelated to the registry's share of generic labels (coefficient 0.17, standard error 0.18) and only weakly related to its share of reorganisations without outcome (−0.50; 0.29), and excluding the eight courts with more than 10 percent of generic announcements leaves the standard deviation unchanged.

Table 2 Raw and true dispersion of plan-adoption rates across courts

	2008–2010	2011–2013	2014–2016	2017–2019	2020–2021	2022–2023
Mean plan-adoption rate	27.3	27.6	28.4	24.8	21.4	20.6
Raw standard deviation across courts	8.0	6.4	6.6	6.5	8.2	7.0
True standard deviation (signal)	6.9	5.0	5.4	5.4	6.5	5.9
95% confidence interval	[5.7; 8.0]	[3.9; 5.9]	[4.2; 6.2]	[4.4; 6.4]	[4.4; 8.2]	[4.8; 6.8]
True s.d., linked cases grouped	6.7	4.9	5.2	5.0	6.1	5.1
True s.d., hierarchical model	7.0	5.7	6.1	5.6	6.4	5.4
True s.d., weighted by cases	6.8	5.9	6.4	6.1	6.9	5.5
Number of courts	129	130	131	131	85	124

Notes: percentage points; unweighted mean of courts; s.d.: standard deviation. True standard deviation: square root of the covariance between the rates of the two half-samples; bootstrap intervals over courts (499 draws); courts with at least 15 reorganisations reaching an outcome in each half-sample. Linked cases grouped: median over 200 random partitions in which reorganisations opened on the same day in the same court and reaching their outcome on the same day are assigned to the same half. Hierarchical model: logit-normal model with a court random effect, estimated on all courts and converted to points at the mean rate. Source: Bodacc 2008–2025, author's calculations

Are these differences legible for a given court? Empirical-Bayes estimates (Fig. 2) give a median reliability of a court's rate of 0.74 over three years of openings and

0.67 over two years, but 0.49 to 0.51 over one year. The 95 percent interval excludes the mean for 29 percent of courts in 2017–2019 and 21 percent in 2022–2023: about a quarter of courts stand out clearly, but several years of decisions are needed to identify them. The rate of the following period increases by 0.81 points (standard error 0.04) per point of prediction, against 0.84 (0.06) for the slope of true effects, $\kappa\,\sigma_{p+1}/\sigma_p$, estimated on the same pairs of periods: predictions are unbiased beyond the drift of effects. A raw indicator of "local legal culture" overstates the standard deviation by 16 to 26 percent over three years while understating persistence (raw correlation of 0.60 between 2008–2010 and 2011–2013, against 0.88 after correction).

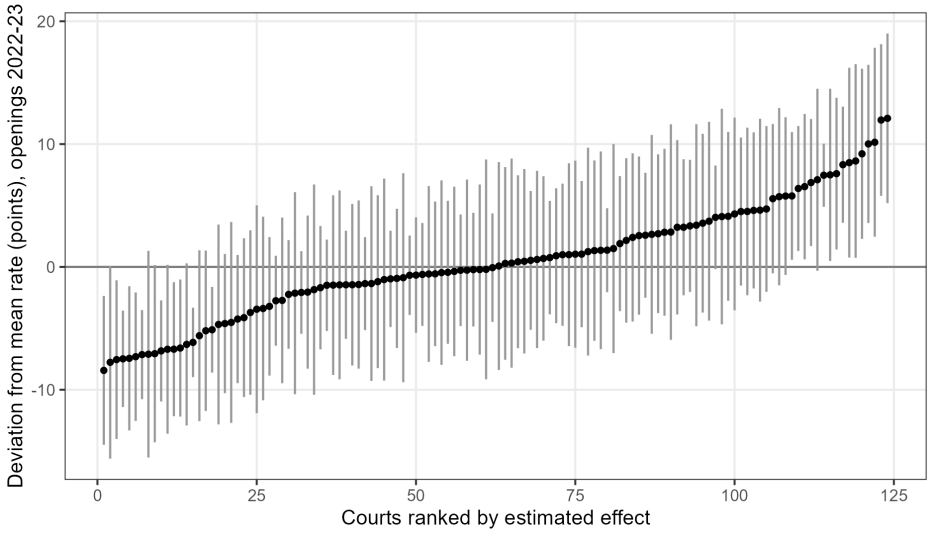

Fig. 2 Estimated deviations of courts from the mean plan-adoption rate, reorganisations opened in 2022–2023. Empirical-Bayes estimates $\hat{\theta}_j = \lambda_j(\bar{r}_j - \bar{r})$ with $\lambda_j = \hat{\sigma}^2/(\hat{\sigma}^2 + \bar{r}(1-\bar{r})/n_j)$, and 95 percent intervals; 124 courts with at least 15 reorganisations reaching an outcome in each half-sample

6.2 Persistence

The correlation of court effects between adjacent periods is high until 2021: 0.88 between 2008–2010 and 2011–2013, 0.99 and 0.96 for the following pairs, and 0.87 between 2017–2019 and 2020–2021 (Fig. 3). It is lower with reorganisations opened in 2022–2023: 0.66 with 2020–2021, 0.36 [0.14; 0.57] with 2017–2019 and 0.41 [0.13; 0.68] with 2011–2013. An autoregressive process fitted to all correlations gives an annual persistence of 0.91 [0.88; 0.94], but it fits neither phase well: it underestimates short-distance correlations before 2020 (0.83 predicted between adjacent blocks) and overestimates those involving 2022–2023 (0.71 predicted against 0.36 observed with 2017–2019). Fitted to the pairs before 2020 only, it gives a persistence of 0.95 and reproduces these pairs almost exactly (root mean squared error of 0.05, against 0.14 on all pairs). A permanent component does not improve the fit (estimated share zero, upper bound 0.40). Relative positions were thus reshuffled during the wave of failures

of 2022–2023 far more than their earlier drift predicted. This is not an artefact of the renaming of courts in 2025: outcomes are linked by SIREN, the share of reorganisations without outcome in 2022–2023 is low in the renamed courts (1.4 percent), and correlations computed without them are 0.31 and 0.35.

The slope between true effects of 2011–2013 and 2022–2023 is 0.37 (0.18): about a third of a court's deviation from the mean survives after a decade (Online Resource 1, Section S2). For an observer who only sees raw rates, the information is more fragile still. The best linear prediction of a court's current effect from a rate observed over three years has a standard deviation of $\sigma\,\kappa\sqrt{\lambda}$: about 4.5 points from the previous period when the correlation is that observed before 2020 (0.88), but 2 points from a ten-year-old rate and 1.7 points from an annual rate of the same age, for a mean rate of 21 percent.

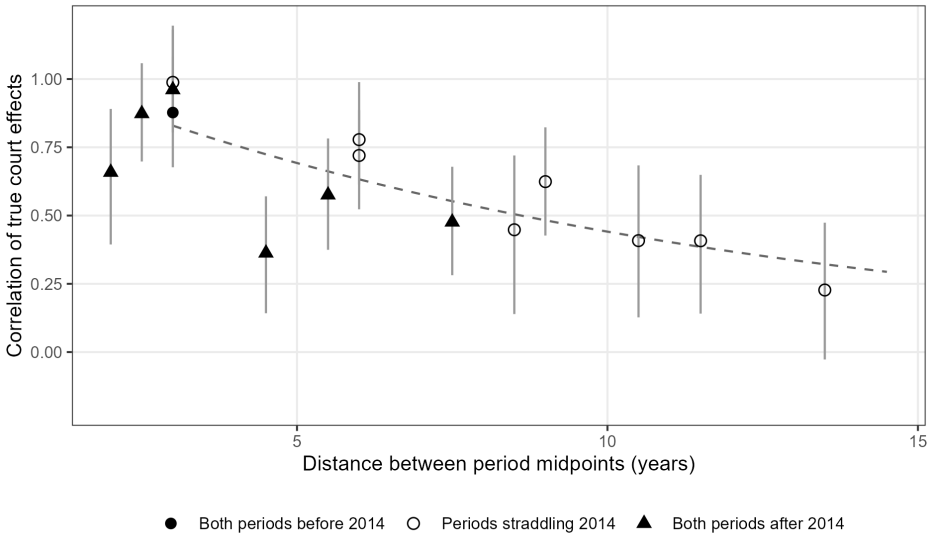

Fig. 3 Persistence of true court effects by distance between periods. Correlation of true court effects between two periods, with its 95 percent confidence interval (bootstrap over courts, 499 draws). Dashed line: correlation between two three-year blocks when annual effects follow a first-order autoregressive process with persistence 0.91, estimated on all pairs (root mean squared error 0.14); fitted values for each pair are given in Online Resource 1, Section S3

6.3 Case composition, local environment and screening at opening

Legal form hardly changes the true dispersion until 2021; in 2022–2023 it explains about 8 percent of it (Table 3, panel A). Sector and age then reduce it by about a tenth: with M4, composition and fluctuations in local conditions explain 16 percent [10; 22] of the signal variance, and the true standard deviation remains between 4.7 and 6.3 points. M5, which attributes to local conditions part of the differences between

départements, raises this share to 25 percent [15; 33]; even this upper bound leaves a standard deviation of 4.3 to 6.1 points. Cross-fitting leaves these estimates unchanged. Oster (2019), eliminating heterogeneity would require selection on unobservables 9 times stronger than on observables if the maximum explainable share of variance is 1.3 times that observed, but 2.8 times if it is twice as large and 1.4 times if it is three times as large; with a case-level R^2 of 0.08, the last case remains plausible, and the size or financial situation of debtors could explain a notable share of the differences (Online Resource 1, Section S7).

Panel B addresses unmeasured local factors. In the 33 *départements* with at least two commercial courts, 0.64 [0.36; 0.94] of the true variance lies between courts of the same *département* without controls, 0.61 [0.33; 0.91] with M4 and 0.65 [0.30; 0.99] with M5; cases randomly reassigned across the courts of a *département* give a share of zero. The local economy measured at the *département* level thus does not explain most of the heterogeneity. Persistence owes little to composition: with M4, the correlation between 2011–2013 and 2022–2023 is 0.37, against 0.41 without controls (Online Resource 1, Section S2).

Table 3 Case composition, local economies and court heterogeneity

A. True standard deviation of plan-adoption rates	2008–2010	2011–2013	2014–2016	2017–2019	2020–2021	2022–2023
M0: no control	6.9	5.0	5.4	5.4	6.5	5.9
M1: + legal form	6.9	5.0	5.4	5.3	6.5	5.7
M2: + sector (NAF division)	6.6	4.8	5.0	5.0	6.3	5.5
M3: + age	6.3	4.7	4.8	4.8	6.1	5.3
M4: + local fluctuations (reference)	6.3	4.7	4.9	4.8	6.2	5.4
95% confidence interval (M4)	[5.3; 7.4]	[3.4; 5.5]	[3.9; 5.7]	[3.8; 5.7]	[4.4; 7.9]	[4.3; 6.4]
M5: levels and fluctuations, no court effects (upper bound)	6.1	4.6	4.3	4.7	5.7	5.2

B. Within-*département* decomposition	True within-*département* s.d.	True s.d., same courts	Within-*département* share [95% CI]
M0	4.3	5.4	0.64 [0.36; 0.94]
M4	3.9	5.0	0.61 [0.33; 0.91]
M5	3.8	4.7	0.65 [0.30; 0.99]

Notes: standard deviations in percentage points. Reorganisations matched to the Sirene register (99.98 percent). M1 to M3: coefficients estimated within court-period pairs. M4: M3 plus deviations of unemployment and of log *département* business failures from their court mean. M5: all controls, including levels of local conditions, coefficients estimated without court effects. Panel B: 33 *départements* with at least two commercial courts (389 court-period pairs); deviation from the simple mean of the other courts of the *département*, each product corrected by its own factor $1 + 1/(n_d - 1)$; shares computed over all periods, net of period means; bootstrap intervals over *départements* (499 draws). Source: Bodacc 2008–2025, Sirene register, Insee local unemployment and business failures; author's calculations

Part of the jurisdiction effect lies in the court's professional environment. The insolvency practitioner is identified for 92 percent of the reorganisations followed; 39 percent of practitioners work in at least two courts within the same period, and the court × practitioner cells retained cover 98 percent of these cases. Covariances between cells give a true standard deviation of 5.7 points [4.5; 6.7] for court effects (131 courts) and 4.1 points [2.7; 5.3] for practitioners (136 practitioners), against 6.9 points at the cell level: courts account for about two thirds of the variance of cells, practitioners for about a third, and the interaction is negligible (Online Resource 1, Section S6). As the court appoints the practitioner, these differences may, however, reflect the allocation of cases. The commercial chambers of Alsace-Moselle, presided over by a professional judge, have a mean rate (23 percent) equal to that of commercial courts and a true dispersion of 4.5 points [1.4; 5.7], against 5.1 points for lay commercial courts of similar size. Based on seven chambers, this comparison is uninformative: its interval is compatible with a dispersion three times smaller as well as with an equal one.

Screening at opening also matters. The share of reorganisations among reorganisation and direct-liquidation openings varies strongly across courts (true standard deviation of 6.4 to 8.1 points, for a mean falling from 37 to 28 percent). Before 2017 it is uncorrelated with the plan-adoption rate (true correlations of −0.09 to 0.05); since then, courts that direct more cases to reorganisation adopt plans less often (−0.36 to −0.45; −0.19 [−0.34; −0.01] over all periods). Part of the recent differences thus reflects the screening of cases at opening rather than leniency at the outcome stage: discretion can move from one stage of the proceeding to another.

6.4 Did the 2014 reform reduce heterogeneity?

The true standard deviation was 6.9 points for reorganisations opened in 2008–2010 and 5.9 points for those opened in 2022–2023. Among the 122 courts observed in the four reference periods, the ratio of the standard deviation after the 2014 reform (2017–2019 and 2022–2023) to that before (2008–2010 and 2011–2013) is 1.02 [0.84; 1.24]; it is 1.00 [0.83; 1.26] with M4 and 0.99 [0.81; 1.27] with M5 (Table 4, panel B). No decline is detectable: the data rule out a reduction of the standard deviation of more than 16 percent, that is of more than 29 percent of the variance, but not a smaller one. A simulation reproducing court sizes shows that the test detects a 40 percent decline of the standard deviation in 98 percent of simulations, but a decline of a quarter in only 68 percent and a decline of 15 percent in 28 percent (Online Resource 1, Section S4).

This ratio must be judged against its variability without reform (Table 5). Between 2008–2010 and 2011–2013, the standard deviation falls from 6.9 to 5.0 points (ratio 0.75 [0.52; 0.93]), a larger fall than any measured around 2014; between post-2014 periods, the ratio ranges from 0.91 to 1.16. Counting cases without outcome as plans or as conversions gives a ratio of 0.95 or 1.06; with 2011–2013 as the only pre-period, it is 1.13 to 1.19. The size that can be ruled out depends on the scale and window: on the logit scale, which neutralises the fall in the mean rate, the ratio is 1.18 [0.97; 1.43]; with 2017–2019 as the only post-period, it is 0.92 [0.70; 1.14] in points, and a 30 percent decline can no longer be ruled out. On the 57 courts observed in the four periods in *départements* not affected by the 2009 mergers, it is 1.19 [0.96; 1.60] (Online

Resource 1, Section S4). These comparisons describe the evolution of dispersion without establishing what it would have been without the reform; compulsory training of judges, which took effect as new judges were elected, cannot be isolated in this way.

Nor did the slope between true effects decline after 2014: it is 0.75 (0.13) between 2008–2010 and 2011–2013, 0.94 (0.15) between the periods around the reform and 0.97 (0.15) between 2014–2016 and 2017–2019, whereas a compression of differences would have reduced it. The reshuffling of relative positions appears only with reorganisations opened in 2022–2023, eight years after the reform.

The standard difference-in-differences nevertheless leads to an apparently opposite conclusion (Table 4, panel A, and Fig. 4). With a baseline measured on 2011–2013, the long-run coefficient is large and highly significant (-0.63), and the negative coefficients of 2008–2010 signal regression to the mean from a noisy baseline (pre-trend $p = 0.07$). With the independent 2008–2010 baseline, the short-run effect is close to zero and the pre-trend is not significant ($p = 0.15$), but the long-run coefficient remains negative (-0.40), as does a placebo comparing 2012–2013 with 2011 (-0.14; standard error 0.07), as drift alone would predict. The identity of Section 5.1, computed with the regression weights, gives -0.41, of which -0.39 for the loss of correlation and -0.02 for the change in dispersion: the apparent convergence comes entirely from the reshuffling of differences. The movement implied by drift alone, computed on the same courts, is -0.31 with the persistence estimated on all periods and -0.24 with that of the only pre-reform pair, against -0.35 observed; the gaps, -0.05 [-0.19; 0.10] and -0.11 [-0.46; 0.14], are not significant, but the second leaves room for a period-specific convergence of a third of the coefficient. In Fig. 4, the annual coefficients remain within the band expected without reform.

A simulation reproducing the observed caseloads shows what this estimator can and cannot detect (Online Resource 1, Section S4). Without reform, with stationary effects of persistence 0.94, the difference-in-differences with the 2008–2010 baseline averages -0.32 [-0.51; -0.12]; if differences are compressed by a third from 2015, it averages -0.43 [-0.59; -0.27]; if their persistence falls to 0.80 from 2015, -0.57. The observed coefficient is compatible with the first two scenarios: the difference-in-differences cannot distinguish harmonisation from its absence, unlike the ratio of dispersions. This does not prove that the reform had no effect: a reshuffling of practices is itself a change that a reform may cause.

6.5 Changing the court: the 2009 mergers

In 2008, screening at opening in the abolished courts differed markedly from that of their successors: the share of reorganisations among openings was 36 percent, against 39 percent in their successors, and the true standard deviation of the gap between an abolished court and its successor reached 12 points (15 pairs with at least 20 openings per half-sample). These gaps did not survive the merger (Table 6). The practice of a maintained court is highly persistent ($\pi = 0.80$, standard error 0.05, for openings of 2009–2013), but the former territory retains no trace of its own: $\beta_T = -0.06$ (0.09), and the Anderson–Rubin set, $[-0.32; 0.10]$, rules out that more than one eighth of the former practice followed the territory. The result holds from 2009–2010 onwards (-0.01; standard error 0.12) and for 2014–2019 (0.03; 0.10), and screening in the

Table 4 The 2014 reform and the dispersion of court practices

A. Continuous-treatment difference-in-differences	Short run (2015–2016)	Long run (2022–2023)	Pre-trend (p)
Baseline 2011–2013 (same cases; estimation 2008–2023)	−0.184 (0.069)	−0.626 (0.088)	0.07
Baseline 2008–2010 (independent cases; estimation 2011–2023)	0.031 (0.072)	−0.397 (0.082)	0.15
Baseline 2008–2010, composition-adjusted rates (M4)		−0.404 (0.086)	0.28
Placebo (baseline 2008–2010): 2012–2013 vs 2011	−0.142 (0.066)		
Decomposition: loss of correlation / change in dispersion		−0.391 / −0.018	
Movement predicted by drift (persistence, all pairs: 0.91)		−0.308	
Movement predicted by drift (persistence, pre-reform pair: 0.94)		−0.243	
Observed movement, same courts and weights		−0.353	

B. Ratio of true standard deviations after / before the reform	Ratio	95% CI
No control (M0)	1.02	[0.84; 1.24]
Composition and local fluctuations (M4)	1.00	[0.83; 1.26]
Levels and fluctuations, no court effects (M5)	0.99	[0.81; 1.27]

Notes: panel A, weighted least squares (weights: reorganisations reaching an outcome), court and opening-year fixed effects, standard errors clustered by court in parentheses; 129 courts with at least 30 reorganisations reaching an outcome in the baseline period; court-year pairs with at least 5 reorganisations. Pre-trend: joint nullity of coefficients before 2013. Decomposition: identity of Section 5.1, case-weighted moments. Predicted movement: $\hat{b}_{2011\text{–}13}\,[K_\rho(2008\text{–}10, 2022\text{–}23)/K_\rho(2008\text{–}10, 2011\text{–}13) - 1]$, where $\hat{b}_{2011\text{–}13}$ is the observed slope of 2011–2013 rates on 2008–2010 rates and K_ρ the block covariance of the autoregressive process; observed movement: change in this slope between 2011–2013 and 2022–2023, 122 courts, equal weights. Panel B: 122 courts observed in the four periods; bootstrap intervals over courts (999 draws). Reading: with a 2011–2013 baseline, a court whose baseline rate exceeds the mean by 10 points sees its rate fall by 6.3 points more than the mean for reorganisations opened in 2022–2023. Source: Bodacc 2008–2025, author's calculations

former territory does not differ in level from that in the original territory (δ close to zero). A territory that changes court thus immediately adopts the screening practice of its new court, as expected if the gap lies in the jurisdiction rather than in the firms of the territory or in its economy.

This test partly rests on one case: in 2008, the share of reorganisations among openings was 42 points lower in the Saint-Tropez commercial court than in the Fréjus court that absorbed it. Without it, instruments become weak and estimates imprecise. The hypothesis that practice follows the territory is still rejected ($p = 0.004$ for 2009–2013, 0.024 for 2014–2019), but the Anderson–Rubin set now only bounds the share that follows the territory at about one half. Extended to the 38 abolished courts with at least ten openings per half-sample, the test leads to the same conclusion (upper bound 0.12; $p = 0.02$), which becomes marginal without Saint-Tropez ($p = 0.05$).

For the plan-adoption rate, the post-merger comparison points in the same direction without being conclusive. Adjusted for composition, the true standard deviation

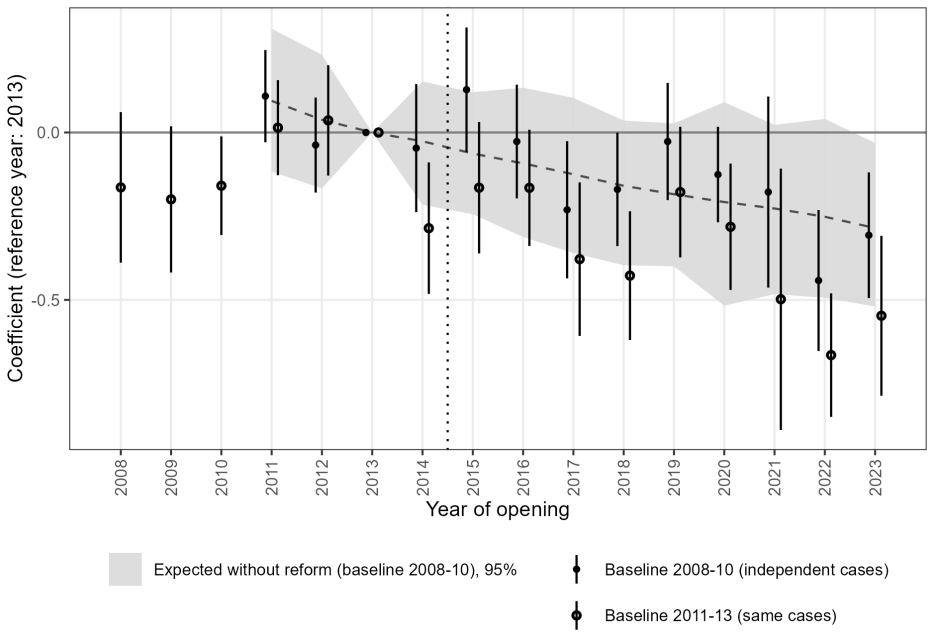

Fig. 4 Annual difference-in-differences coefficients by baseline period. Coefficients β_k and 95 percent confidence intervals; reference year 2013. Shaded area and dashed line: mean and 95 percent interval of the same estimator, with the 2008–2010 baseline, over 200 simulations of stationary autoregressive effects with annual persistence 0.94 (pre-reform pair), with the observed caseloads. Dotted vertical line: entry into force of the 2014 ordinance

Table 5 Ratio of true standard deviations after / before the reform: variants

Variant	Ratio	95% CI
Percentage points (reference)	1.02	[0.84; 1.24]
Cases without outcome counted as plans / as conversions	0.95 / 1.06	
Logit scale	1.18	[0.97; 1.43]
Logit scale, post-period limited to 2017–2019	0.97	[0.76; 1.22]
Points, post-period limited to 2017–2019	0.92	[0.70; 1.14]
Relative scale, composition-adjusted rates (M4)	1.08	[0.88; 1.34]
Relative scale, M4, post-period limited to 2017–2019	0.93	[0.71; 1.20]
Excluding sales	0.99	[0.86; 1.18]
Plan or sale counted as success	1.02	[0.88; 1.20]
Placebo without reform: 2011–2013 vs 2008–2010	0.75	[0.52; 0.93]

Notes: "after" = 2017–2019 and 2022–2023, "before" = 2008–2010 and 2011–2013 unless stated otherwise; courts observed in the four periods (122, or 126 when the post-period is limited to 2017–2019); bootstrap intervals over courts (999 draws). Logit scale: half-sample rates transformed by $\log((k+0.5)/(n-k+0.5))$. Relative scale: standard deviation divided by $\sqrt{\bar{p}(1-\bar{p})}$. Placebo: ratio of standard deviations of two pre-reform periods. Source: Bodacc 2008–2025, author's calculations

of the gap between the former territory and the original territory of the same successor is 5.2 points [0; 8.5] for reorganisations opened in 2009–2013 and 1.5 points [0; 6.4] for those opened in 2014–2016, against 8.0 and 6.5 points between two distinct courts of the same *département*; the mean gap between the two territories is 1 to 3 points (Online Resource 1, Section S8). The former court boundary matters less than the boundary between two courts, but these estimates are imprecise and do not tell whether successors themselves took over part of the practices of the courts they absorbed.

Table 6 The 2009 mergers: does screening at opening follow the jurisdiction or the territory?

Sample	Openings	π	β_T	AR set (β_T)	p ($\beta_T = \pi$)	Abolished courts	Cases from former territories
Reference	2009–2013	0.80	−0.06	[−0.32;	< 0.001	14	4,776
		(0.05)	(0.09)	0.10]			
Reference	2014–2019	0.82	0.03	[−0.30;	< 0.001	14	5,883
		(0.07)	(0.10)	0.20]			
Without Saint-Tropez	2009–2013	0.80	−0.03	(−∞;	0.004	13	4,455
		(0.05)	(0.38)	0.38]			
Without Saint-Tropez	2014–2019	0.82	−0.25	(−∞;	0.024	13	5,496
		(0.07)	(0.51)	0.46]			
Extended sample	2009–2013	0.80	−0.12	(−∞;	0.019	38	7,778
		(0.05)	(0.17)	0.12]			
Extended sample, without Saint-Tropez	2009–2013	0.80	−0.49	unbounded	0.052	37	7,457
		(0.05)	(1.11)				

Notes: dependent variable, opening of a reorganisation rather than a direct liquidation; openings in maintained courts whose debtor is located in their original territory or in that of an abolished court they took over. Two-stage least squares: 2008 rates of half-sample A instrumented by those of half-sample B; year, legal form, NAF division and age-band fixed effects; standard errors clustered by court in parentheses. Reference: courts with at least 20 openings in 2008 in each half-sample; extended sample: at least 10. AR set: Anderson–Rubin 95 percent confidence set for β_T; p: Anderson–Rubin test of $\beta_T = \hat{\pi}$. ($-\infty$: set unbounded below.) Reading: for openings of 2009–2013, a 10-point screening gap in 2008 between a maintained court and the mean persists at 8 points; for the former territory of an abolished court, the 2008 gap with its successor leaves no trace (−0.6 points). Source: Bodacc 2008–2023 (debtors' addresses), Sirene register; author's calculations

6.6 Propensity to adopt plans, use of safeguard and filing delay

Across courts, a ten-point higher plan-adoption rate is associated with a 0.4 to 0.5 point higher safeguard share, whose mean is 2.5 percent (Table 7, columns 1 and 2); the association is of the same order when the rate is instrumented by that of the other half-sample or adjusted for composition (columns 3 to 5). It is no longer detectable between courts of the same *département* (0.026, standard error 0.034) or within a court over time (0.005, standard error 0.024), but these less precise estimates do not rule out a coefficient equal to the cross-court one. When the safeguard share is related to the rate of the previous period, measured on other cases, the coefficient is 0.052 (0.036) across courts and −0.009 (0.017) within a court: only the latter is both close to zero and precise enough to rule out a value of 0.05. The delay between the date of cessation

of payments set by the court and the opening also varies across courts (true standard deviation of about 3 months, for a mean of 3 to 5 months), with no relation to the court's past propensity to adopt plans (Online Resource 1, Section S6). As the court sets this date and may backdate it, this delay measures its dating practices as much as the debtors' timeliness. These associations, compatible with regional ecosystems of professionals as much as with a reputation effect, are therefore not given a causal interpretation.

Table 7 Propensity to adopt plans and the safeguard share of openings

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Plan-adoption rate	0.041	0.052	0.065	0.039	0.052	0.026	0.005
	(0.028)	(0.012)	(0.016)	(0.014)	(0.020)	(0.034)	(0.024)
Estimation	OLS	OLS	IV	OLS	IV	IV	IV
Composition-adjusted rates (M4)	no	no	no	yes	yes	yes	yes
Court size and Île-de-France controls	no	yes	yes	yes	yes	size	size
Département fixed effects	no	no	no	no	no	yes	no
Court fixed effects	no	no	no	no	no	no	yes
First-stage F			751		586	133	100

Notes: dependent variable, safeguard share of a court's openings in a period (mean 0.025). 730 court-period pairs (135 courts), the same in all columns (727 in column 7). Regressions weighted by the number of openings; period fixed effects; standard errors clustered by court in parentheses. OLS: ordinary least squares; IV: instrumental variables, the adoption rate of half-sample A being instrumented by that of half-sample B. Court size: log number of openings. Columns (4) to (7): safeguard share and adoption rate adjusted for composition (M4). Source: Bodacc 2008–2025, author's calculations

6.7 Propensity to adopt plans and plan quality

A court that adopts plans more often may do so at the cost of weaker plans (Epaulard & Zapha, 2025; Leyman et al., 2011; Morrison, 2007). We follow each first reorganisation plan and observe whether the firm subsequently enters liquidation, through rescission of the plan or a new proceeding, within two, three or five years. Of the plans adopted in 2008–2013, 44 percent are followed by liquidation within three years, against 39 percent of those adopted in 2014–2017 (Fig. 5); at five years, the share falls from 56–60 percent to 48–49 percent for plans of 2016–2017. Plans of 2018–2020 fail even less often, but their follow-up overlaps the period of support measures; the improvement observed after 2014 coincides both with the reform and with the recovery of 2015–2019.

Plans adopted by the courts that adopt the most plans are not significantly more often followed by liquidation (Table 8). The court's rate is computed on the other half-sample, which does not include the firm concerned. A ten-point higher adoption rate is associated with a 0.3 point higher probability of liquidation within three years at the plan level, and with 1.0 point (standard error 1.1) at the court level with an instrumented rate, but with 2.1 points (1.1) with composition-adjusted rates or within *départements*. If the failure rate F of a court rises with its adoption rate r with slope b,

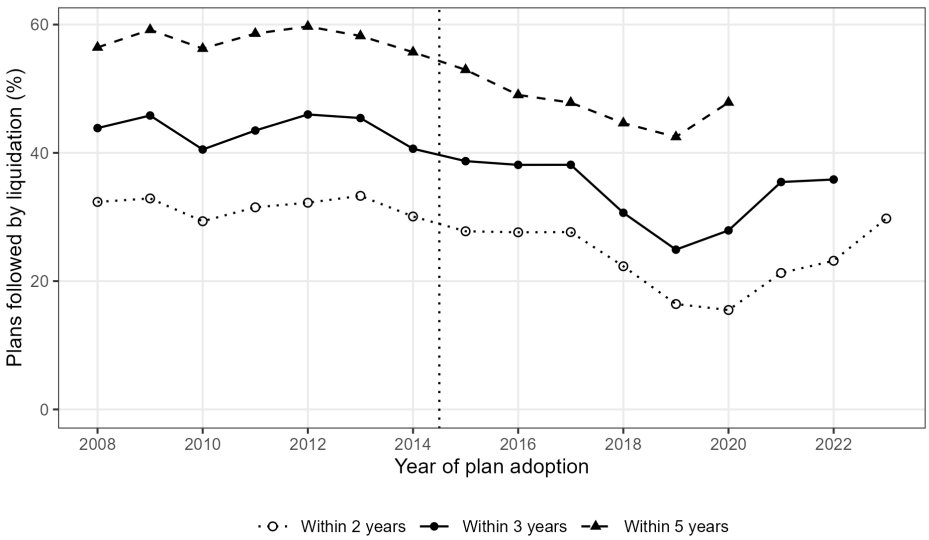

Fig. 5 Share of reorganisation plans followed by liquidation, by year of adoption. First plan observed for each firm and court. A plan is considered to have failed if it is rescinded or if the firm enters judicial liquidation in any court between 30 days and two, three or five years after the judgment adopting the plan. A cohort is shown only if the whole follow-up period is observed

the additional plans fail at rate $F_m = F + b\,r$. This rate is 39 to 44 percent at three years depending on the specification, against 38.7 percent on average; in the composition-adjusted or within-*département* specifications it is 44 percent [38; 49]. Restricted to plans adopted up to 2017, whose follow-up precedes the support measures, the same specification gives 47 percent [40; 53] against 41.7 percent. Marginal plans therefore do not fail significantly more often than others, but an excess failure rate of up to about ten points cannot be ruled out. This calculation also assumes that courts handle pools of similar quality: if a better pool produces both more plans and fewer failures, b is biased downwards. Counting closures recorded in Sirene as failures changes nothing.

7 Discussion

7.1 Rules, jurisdictions and the harmonisation of insolvency law

The French experience allows the two levers to be compared. Reforms of the rules—the 2014 ordinance, compulsory training of judges from 2016, the 2021 transposition of Directive (EU) 2019/1023—left the dispersion of court practices unchanged within the precision of our tests: a decline of a sixth of the standard deviation cannot be ruled out, but a large one can. The reform of the judicial map changed no rule, but changed the court with jurisdiction over the territory of the abolished courts, and these territories immediately adopted the screening practice of their new court, consistent with earlier evidence that the practices of absorbing courts prevailed after the mergers (Epaulard & Zapha, 2025). Together with the persistence of differences between courts

Table 8 Propensity to adopt plans and failure of reorganisation plans

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Dependent variable	Failure within 3 years	Failure within 3 years	Failure within 3 years	Failure within 5 years	3-year failure rate	3-year failure rate	3-year failure rate	5-year failure rate
Court plan-adoption rate	0.033 (0.075)	0.009 (0.052)	0.058 (0.083)	0.025 (0.083)	0.099 (0.114)	0.207 (0.110)	0.205 (0.124)	0.091 (0.124)
Adoption rate × after 2014			−0.047 (0.041)					
Estimation	OLS	OLS	OLS	OLS	IV	IV	IV	IV
Composition-adjusted rates (M4)	no	no	no	no	no	yes	no	no
Département fixed effects	no	yes	no	no	no	no	yes	no
Unit of observation	plan	plan	plan	plan	court-period	court-period	court-period	court-period
N	36,439	36,439	36,439	33,880	622	622	621	578
Failure rate of marginal plans, F_m (%)	39 [36; 43]	39 [36; 41]	40 [36; 44]	54 [50; 57]	41 [36; 46]	44 [38; 49]	44 [38; 50]	55 [49; 61]

Notes: columns (1) to (4), one observation per plan; the adoption rate is computed on the half-sample that does not contain the firm, which attenuates the coefficient. Columns (5) to (8), one observation per court and period (at least 10 plans followed), columns (5) to (7) on the same sample; IV: rate of half-sample A instrumented by that of half-sample B (first-stage F of 121 to 538); weighted by the number of plans followed. Period fixed effects in all columns; standard errors clustered by court in parentheses. Mean failure rate: 0.387 at three years, 0.530 at five years. $F_m = F + b\,\bar{r}$ with $\bar{r} = 0.24$; 95 percent intervals derived from the standard error of b. Plans adopted between 2008 and 2022 whose follow-up window is fully observed. Source: Bodacc 2008–2025, author's calculations

of the same *département*, this indicates that practices belong to jurisdictions rather than to the territories they serve. Changing the rules did not bring practices closer; changing the court moved them.

Three features of the results matter for the design of reforms. First, practices are attached to institutions: they are not explained by observable case composition, survive within *départements*, and disappear for a territory when its court changes. Second, discretion moves across stages: since 2017, courts that open more reorganisations adopt fewer plans, so that a reform targeting one decision may be offset at another. Third, practices drift and are hard to read: several years of decisions are needed to identify a court that stands out, and relative positions reshuffled in 2022–2023, shortly after the transposition of the 2019 Directive and during the post-pandemic wave of failures. The informational role of a court's reputation (Froute, 2007) is limited by the time it takes to learn it.

These results bear directly on European insolvency policy. The two directives harmonise rules, and the 2019 Directive approaches courts only through the training and expertise of judges (Article 25), leaving their organisation to Member States. Within a single Member State and under a single code, we find that the probability that a firm obtains a reorganisation plan varies across courts with a standard deviation of 5 to 7 percentage points, and that rule reforms did not compress this dispersion. Harmonising rules across Member States is therefore unlikely to harmonise outcomes within them, and the effect of the transposition of Directive (EU) 2026/799 will depend on

the courts that apply it, as the effect of the Brazilian reform depended on court congestion (Ponticelli & Alencar, 2016). The organisation of jurisdictions—their territorial map, their size, their composition and the professionals around them—appears to be a more effective lever than the text. Whether specialisation is the right form of organisation is a separate question, on which the evidence is mixed (Garoupa et al., 2008); our results only show that the jurisdiction, not the territory, carries the practice. The periodic review of the 2019 Directive and the evaluation of the French economic activity courts are natural occasions to measure the dispersion of court practices, not only average outcomes.

The contrast has limits. The merger test concerns screening at opening, partly rests on a single territory, and says nothing about the outcome of reorganisations, which the data do not allow us to observe before the merger. Nor does it tell whether successors took over part of the practices of the courts they absorbed, or what—judges, registry or practitioners—carries the practice of a jurisdiction. The comparison with the Alsace-Moselle chambers is too imprecise to say whether heterogeneity stems from the lay character of the judges. The reshuffling of 2022–2023 may reflect the profile of judges (Blazy & Esquerré, 2021), the role of a small group of them (Blum, 2026), their experience (Iverson et al., 2023), the electoral renewal of benches or the professional environment. Dating changes in the composition of benches, from the court registries' assignment orders, would make it possible to distinguish these explanations and to exploit the only staggered reform of the period, compulsory training for judges elected after 2016. The economic activity courts introduced in 2025 offer another opportunity, for which the panel built here provides the baseline.

7.2 Evaluating harmonisation

For empirical studies, defining treatment intensity from a past value of the outcome exposes the analysis to two sources of spurious convergence: noise shared with the baseline period and the drift of differences between units, even with exhaustive administrative data. Measuring intensity on independent observations removes the first; decomposing the coefficient into loss of correlation and change in dispersion isolates the second, and only the comparison of dispersions distinguishes harmonisation from its absence. Measurement itself matters: counting the decisions of the year rather than following proceedings, ignoring generic labels or linking outcomes only to the court of opening substantially changes the diagnosis.

7.3 Limitations

We observe neither the size nor the financial situation of debtors at opening, and the Oster sensitivity analysis shows that they could explain a notable share of the differences; the within-*département* comparison neutralises unobserved local factors only if they are common to the courts of a *département*. A court-by-court comparison with Ministry of Justice statistics remains to be done. As the open Bodacc data begin in 2008, the practice of the courts abolished in 2009 is observed over a single year, which confines the merger test to screening at opening. Finally, the reputation mechanism of Section 3 predicts that a court's propensity to adopt plans affects the cost of credit for

firms in its jurisdiction; testing this prediction requires firm-level data on new loans to small and medium-sized enterprises.

8 Conclusion

In French corporate reorganisation, the outcome of a case depends substantially on the court that hears it. These differences are real once noise is removed, persist for years while drifting, are not explained by observable case composition or the local economy, and were not reduced by the successive reforms of insolvency law. When the territorial organisation of courts changed, however, practices moved with the court. For legal reforms whose effects depend on decentralised and discretionary application, including the harmonisation of insolvency law in the European Union, the jurisdiction is not a neutral channel through which rules pass: it is where much of the law in action is made.

Statements and Declarations

Funding. No funding was received for conducting this study.

Competing interests. The author has no relevant financial or non-financial interests to disclose.

Data and code availability. All data are public: Bodacc announcements (open data released by the French Directorate of Legal and Administrative Information), the Sirene business register and Insee local statistics. The code that reconstructs the proceedings from the raw announcements and reproduces all tables and figures is deposited on Zenodo: https://doi.org/10.5281/zenodo.24284771

Use of AI tools. The author used Claude (Anthropic), a large language model, to assist in writing and running the data-processing and estimation code and in drafting and translating the manuscript. The author designed the study, checked the code and results, and takes full responsibility for the content.

Author contributions. Philippe Froute is the sole author: conceptualisation, data collection, analysis and writing.

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