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Chloé Zapha

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THÈSE DE DOCTORAT
DE L'UNIVERSITÉ PSL

Préparée à Université Paris-Dauphine

**An Economic Analysis of
French Corporate Bankruptcy Laws**

THREE ESSAYS IN MICROECONOMETRICS

Soutenue par

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Introduction Générale

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La défaillance d'entreprise fait peur.

Dans l'imaginaire collectif, la défaillance, c'est l'échec d'une entreprise. C'est la fin de son activité économique, le licenciement de ses salariés, l'épanchement de ses dettes impayées, les sanctions des banques, la fin de la confiance des fournisseurs et le jugement du tribunal.

Pour le dirigeant d'entreprise, c'est une procédure administrative et juridique longue et fastidieuse. Cela suppose de justifier ses choix et sa gestion, chercher à sauver ce qui peut l'être en préservant sa réputation. Pour les employés, cela résulte en une forte période d'incertitude, jusqu'à plusieurs mois sans salaire, la peur d'être licencié. Pour les fournisseurs, ce sont des mois de commandes impayées, parfois même une activité mise en danger par le client défaillant. Pour les autres créanciers – banques, clients, États – cela représente des sommes avancées qui risquent de ne jamais être remboursées.

Cet imaginaire sombre de la défaillance recoupe une réalité humaine, sociale et économique certaine. Mais la défaillance, telle que pensée en France, est un processus complexe qui va au-delà de ce tableau noir. Elle se distingue de la faillite et de la banqueroute, dont les termes ont disparu en 1985 du langage juridique de l'entreprise et se cantonnent désormais aux mesures pénales à l'encontre d'un dirigeant fautif. À l'inverse, dans le droit français et la plupart des pays européens, la défaillance d'entreprise est pensée comme une protection.

Elle est déclenchée par l'état de cessation des paiements, c'est-à-dire l'incapacité pour l'entreprise de faire face à son passif exigible (ses dettes dues) avec son actif disponible (sa trésorerie et autres ressources rapidement mobilisables). Dès lors, l'entreprise dispose de quarante-cinq jours pour se rendre à son tribunal de commerce d'affectation afin d'entamer une procédure de défaillance.

Ainsi, la procédure de défaillance garantit la protection du dirigeant à travers la suspension des poursuites des créanciers, pour laisser le temps au management de faire l'état des lieux sans être inquiété par les créances impayées. Elle garantit aussi la protection des employés, à travers le paiement des arriérés de salaires par le Fonds de garantie des salaires, qui prend le relais de l'entreprise défaillante pour prendre en charge les employés. Elle coordonne des créanciers – les banques, les clients et les fournisseurs, mais aussi les actionnaires et l'État – à travers l'ordre de priorité des créances.

Le droit des faillites français organise la défaillance et le traitement des difficultés des entreprises dans un objectif d'efficacité économique. Ex-ante, un droit des faillites efficace (i) favorise l'innovation, en incitant les entrepreneurs à prendre des risques en leur assurant une résolution non punitive en cas de difficultés. Parallèlement, il (ii) soutient la confiance des investisseurs et encourage le financement des entreprises en garantissant aux créanciers un traitement équitable dans leurs droits à recouvrer leurs créances en cas d'échec. Ex-post, un système de faillite efficace organise (iii) la restructuration ordonnée des dettes afin de permettre aux entreprises viables de se remettre sur pied, préservant ainsi emplois et activités économiques. À l'inverse, en liquidant les entreprises non-viables, il assure (iv) la bonne réallocation des actifs (capital et travail) à des entreprises plus productives.

Ces critères font du droit des faillites un pilier indispensable à la croissance économique et à la stabilité financière des pays.

1 Le droit français : un arsenal de procédures

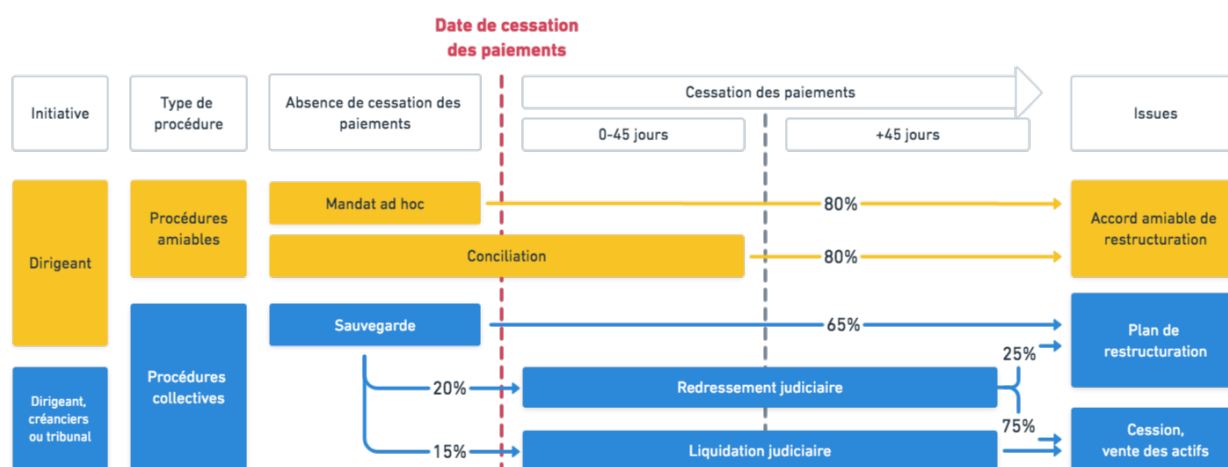
Dans sa recherche d'efficacité, le droit français propose un arsenal de procédures en constante évolution.

La plus ancienne et plus sollicitée des procédures est la liquidation judiciaire, définie par la loi du 13 juillet 1967¹. Son objectif est d'apurer les actifs de l'entreprise en la cédant tout ou en partie à un ou des repreneurs, afin de rembourser au mieux les créanciers. La liquidation représente le dernier pallier quand l'entreprise apparaît impossible à redresser ; elle aboutit à la disparition de l'entreprise. Lorsqu'une entreprise franchit les portes du tribunal, dans près de deux cas sur trois, la décision de liquidation est déjà quasiment prise, que ce soit par le dirigeant lui-même ou par le tribunal. C'est ce que l'on observe Figure 2, parmi les plus de 50 000 entreprises ayant entamé une procédure en 2018, environ 33 000 se dirigent ou sont dirigées vers une liquidation judiciaire directe. L'écrasante majorité des cas de liquidations judiciaires directes concernent des micro-entreprises². Plus l'entreprise est grande, et moins la liquidation

¹Loi n° 67-563 du 13 juillet 1967 sur le règlement judiciaire, la liquidation des biens, la faillite personnelle et les banqueroutes.

²Une micro-entreprise compte moins de 10 salariés et moins de 2 millions d'euros de chiffre d'affaires.

Figure 1: Synthèse des principales procédures françaises et leur taux de restructuration



Note: La Figure 1 résume les principales procédures de traitement des difficultés des entreprises en France. La ligne rouge dénote le timing de leur déclenchement par rapport à l'état de cessation des paiements (incapacité pour l'entreprise de rembourser son passif exigible avec son actif disponible). Les pourcentages représentent les taux moyens de restructuration des procédures, ou de conversion en redressement ou en liquidation judiciaire.

Sources: Calculs de l'auteurice, approximation sur la base du BODACC et des chiffres du CNAJMJ. (2009-2019).

judiciaire directe est probable (cf. Figure 3).

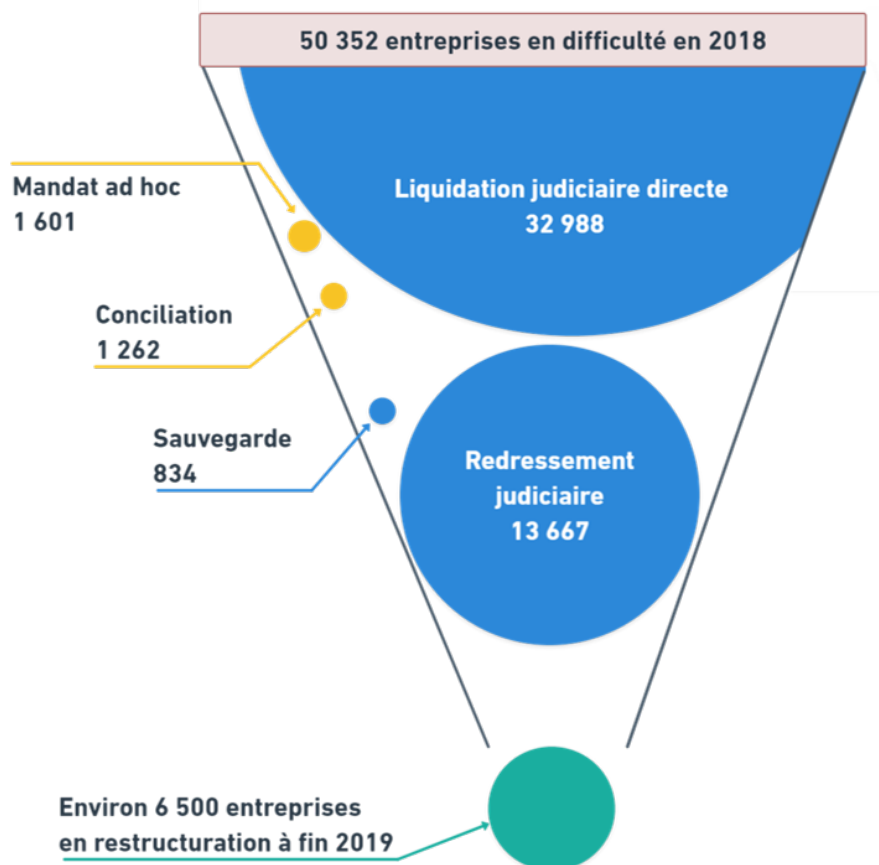
En alternative à la liquidation judiciaire, et si la restructuration est envisageable, l'entreprise peut entamer une procédure de redressement judiciaire, définie dans sa version moderne par la loi de 1985³. Pour 75% des entreprises, le redressement échoue et débouche sur une liquidation (cf. Figure 1). Ces deux procédures sont tardives et déclenchées par l'état de cessation des paiements, c'est-à-dire lorsque la situation économique de l'entreprise est déjà très détériorée.

Aux côtés de ces deux procédures traditionnelles cohabitent des procédures préventives : la procédure de sauvegarde depuis 2005⁴, qui est publique, et les procédures de mandat *ad hoc* et de conciliation, qui sont confidentielles. Ces procédures, comme le redressement judiciaire, visent à mettre en place la restructuration de l'entreprise. Accessibles en amont de la cessation des paiements, elles permettent la saisie des difficultés de l'entreprise avant qu'elles ne soient trop sévères. Les plans

³Loi n° 85-98 du 25 janvier 1985 relative au redressement et à la liquidation judiciaires des entreprises.

⁴Loi n° 2005-845 du 26 juillet 2005 de sauvegarde des entreprises.

Figure 2: Ventilation et issues des procédures entamées en 2018



Note: Chiffres des entrées en procédure en 2018 dont les jugements (de restructuration ou de liquidation) ont été rendus avant fin 2019

Source: [Ministère de la Justice](#). Exploitation statistique du répertoire général civil, année 2018.

de restructuration mélangent remise partielle de dette et rééchelonnement de la dette jusqu'à un horizon de dix ans. Le but est de permettre à l'entreprise de poursuivre son activité tout en repayant ses créanciers à un rythme soutenable.

Si l'on juge l'efficacité d'une procédure par son taux de restructuration, alors l'efficacité des procédures préventives ne fait aucun doute. Figure 1, on constate que les procédures amiables mènent à un plan de restructuration dans 65% à 80% des cas, contre 25% en redressement judiciaire. Au regard de ces bons résultats, le droit français cherche à favoriser la prévention de la défaillance, présentée comme une solution aux dirigeants. Dans l'Union européenne, l'adoption des procédures préventives s'est accélérée depuis l'entrée en vigueur de la directive sur les cadres de restructuration préventive⁵.

Pourtant, le recours aux procédures préventives est rare (elles représentent 7% de toutes les entrées en procédure en 2018, cf. Figure 2), d'autant plus rare que l'entreprise est petite (cf. Figure 3). Les dirigeants d'entreprise ne semblent pas passer la porte d'un tribunal s'ils n'y sont pas obligés, c'est-à-dire lorsque la situation est déjà compromise, et l'entreprise en cessation des paiements. Outre la peur du tribunal de commerce et son association à l'échec, ce phénomène pourrait être attribué à un manque d'information, de nombreux dirigeants de petites structures ignorant peut-être l'existence même des procédures préventives. Une mauvaise conception de ces procédures, parfois jugées trop complexes pour des entreprises de petite taille, pourrait également être en cause. Or, les microentreprises, très petites, petites et moyennes entreprises, regroupées sous le terme générique de PME, sont primordiales dans le tissu économique français. Elles comptent pour 99,9% de l'ensemble des entreprises françaises, 44% de la valeur ajoutée des entreprises et près de la moitié de l'emploi⁶. Il est indispensable que le droit des faillites s'adapte aussi à ces petites structures.

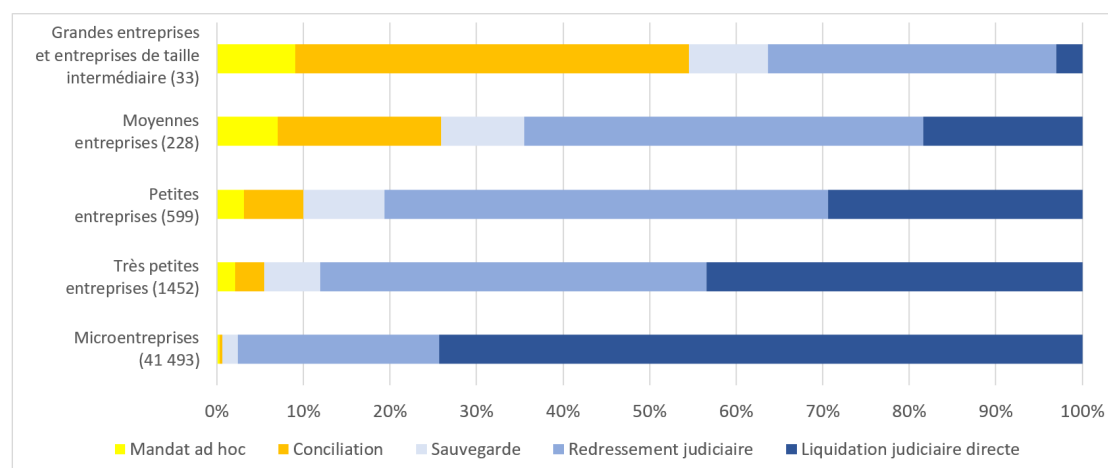
2 Comment juger de l'efficacité économique du droit ?

Seul, le taux de restructuration des entreprises en difficulté ne permet pas de mesurer l'efficacité du droit des faillites. D'une part, un fort taux de restructuration peut

⁵Directive (UE) 2019/1023 du 20 juin 2019 relative aux cadres de restructuration préventive, à la remise de dettes et aux déchéances, et aux mesures à prendre pour augmenter l'efficacité des procédures en matière de restructuration, d'insolvabilité et de remise de dettes

⁶Source : <https://www.insee.fr>

Figure 3: Plus l'entreprise est grande, plus le recours à des procédures préventives et amiables est fréquent



Note: Statistiques établies sur 43 795 entreprises entrées en procédure en 2018 dont les données financières sont disponibles.

Sources: Calculs de l'auteur, approximation sur la base du [BODACC](#), de FARE et du [CNAJMJ](#) (2009-2019).

témoigner de trop nombreuses restructurations d'entreprises non viables, non-efficaces car emprisonnant du capital et du travail au sein de structures peu productives. D'autre part, beaucoup d'autres facteurs sont déterminants pour juger de l'efficacité du processus de défaillance, parmi lesquels la durée des procédures, le taux de recouvrement des créanciers à l'issue de la procédure, la bonne réallocation du capital libéré par la liquidation, la performance des entreprises restructurées, le rebond des dirigeants d'entreprises, etc. Cette section vise à lister quelques caractéristiques principales de la littérature sur le sujet.

La plupart des analyses empiriques de l'efficacité du droit des faillites sont menées sur données américaines car l'affectation aléatoire des juges aux dossiers offre un cadre propice aux identifications causales.⁷ Outre-Atlantique, les procédures ressemblent à ce qui existe dans le droit français : liquidation régie par le Chapter 7 du *Bankruptcy Code* américain, et restructuration régie par le Chapter 11.⁸ La logique du Chapter 11 rejoint la France dans la volonté de privilégier le maintien de l'entreprise. D'autres

⁷Par exemple, [Bernstein et al. \(2019\)](#) utilise cette caractéristique du droit américain pour questionner la bonne réallocation des actifs libérés par la liquidation.

⁸Le droit américain diffère du droit français en cela que les particuliers sont jugés sous un régime similaire aux entreprises, par les mêmes juges, en Chapter 7 et 13 pour surendettement.

droits des faillites, tels que les droits anglais, allemand ou encore suédois, recherchent davantage la protection des créanciers et le recouvrement de leurs dettes. Le droit français est souvent critiqué car il défavoriserait les créanciers ([Plantin et al. \(2013\)](#)) et que ses taux de recouvrement moyen seraient faibles ([Blazy et al. \(2018a\)](#)). Le fait que le droit français des faillites favorise le débiteur et la protection des droits sociaux des salariés au détriment des investisseurs répond d'une orientation politique assumée. En outre, lorsque l'on juge la prévention et la protection du débiteur comme des critères d'efficacité en soi, le droit des faillites français se distingue comme étant particulièrement bon ([Adalet McGowan and Andrews \(2018\)](#)).

Il est assez largement admis qu'un droit des faillites efficace est un droit qui limite les coûts des procédures. Les coûts associés à la faillite ne se limitent pas aux coûts directs, estimés entre 1,5% à 3,5% de la valeur de l'entreprise aux États-Unis ([Hotchkiss et al. \(2008\)](#)). Ces coûts directs, immédiatement mesurables, englobent les frais légaux et administratifs qui comprennent les honoraires des avocats, des experts financiers, des administrateurs et mandataires judiciaires et autres parties impliquées dans le processus.

Les coûts économiques les plus importants, au-delà des coûts sociaux et psychologiques que cette thèse ne cherche pas à traiter, se situent avant tout dans les effets indirects de perte de réputation et d'opportunités. On estime que les coûts indirects sont cinq à dix fois plus élevés que les coûts directs ([Andrade and Kaplan \(1998\)](#), [Bris et al. \(2006\)](#), [Davydenko and Franks \(2008\)](#)). Ils se manifestent par la perte de confiance des fournisseurs, des créanciers et des clients envers l'entreprise défaillante. Cette méfiance peut se propager dans le tissu économique plus large, affectant la réputation et la valeur des entreprises avec lesquelles elle interagit. De plus, la faillite peut engendrer des perturbations dans les chaînes d'approvisionnement et réduire l'accès aux financements, freinant ainsi l'activité économique globale.

Les coûts indirects sont d'autant plus lourds que la procédure est longue. Ainsi, une mesure empirique fréquente de l'efficacité du droit des faillites se retrouve dans la congestion dans les tribunaux de commerce ([Iverson \(2018\)](#), [Müller \(2022\)](#)) et des délais de résolution, parfois très longs ([Pezone \(2023\)](#)). La durée moyenne des procédures judiciaires est une mesure importante de l'application de la loi car la lenteur des tribunaux désincite le financement ex-ante ([Visaria \(2009\)](#)). Une trop longue durée

peut aussi contribuer à détériorer la situation de l'entreprise à travers le découragement des dirigeants à rechercher des solutions précoces (Zapha (2020)).

En Italie, lorsque l'on mesure l'efficacité des procédures, l'accent est mis sur les durées. Ça n'est pas le cas en France pour au moins deux raisons. D'une part, les durées sont strictement encadrées par la loi et n'atteignent jamais des niveaux extrêmes comme ceux constatés en Italie par exemple⁹. D'autre part, l'application de ces durées est relativement homogène, rendant difficile l'identification de gains ou de perte d'efficacité associés à un raccourcissement ou à allongement des délais.

3 L'objet de la thèse

L'efficacité économique du droit des faillites français a été peu étudié dans la littérature empirique. Il est, la plupart du temps, l'objet d'études comparatives (Davydenko and Franks (2008), Adalet McGowan et al. (2017), Adalet McGowan and Andrews (2018), Blazy et al. (2018a), Blazy et al. (2018b)), ou étudié à l'aune de corrélations effectuées sur des échantillons restreints qui questionnent les déterminants de la défaillance (Fimayer (2011), Ayadi-Ben Lakhak (2011)). Le rôle des acteurs de la défaillance a également été questionné (Blazy and Esquerré (2021)) mais il existe peu d'études de plus grande ampleur (Esquerré (2019)).

Par essence, les procédures confidentielles ne disposent pas de données publiques exploitables. À l'inverse, les jugements des procédures publiques (sauvegarde, redressement judiciaire et liquidation judiciaire) sont publiés depuis 2008 sur le site du BODACC (Bulletin Officiel d'Annonces Civiles et Commerciales) et nous renseignent sur les différentes étapes du processus de défaillance entrepris par les entreprises en difficulté. Les détails des plans de restructuration et des recouvrements ne sont pas renseignés, mais l'accès à l'exhaustivité des jugements de défaillance permet déjà un élargissement considérable du champ de recherche.

Cette thèse se distingue par l'appariement de ces données publiques avec des données confidentielles sur les entreprises (données de bilans, crédit bancaire) et des données de

⁹Même si en médiane, l'Italie présente des durées de procédures identiques à ce que l'on observe en France (de l'ordre de 300 jours), Pezone (2023) constate des délais de résolution pouvant atteindre jusqu'à 1,674 jours.

défaillances historiques (antérieures à 2008). Cette richesse permet l'étude du droit des faillites français à une échelle inédite. Dans une approche *Law and Economics*, cette thèse entend ainsi combler ce manque en questionnant plusieurs aspects du droit des faillites français. À travers l'application du droit par les tribunaux de commerce et les dispositions juridiques pour prévenir les difficultés des entreprises et accompagner leur rebond, le droit des faillites français permet-il un traitement économiquement efficace des difficultés des entreprises ?

Les trois chapitres de cette thèse proposent une identification causale d'un aspect différent du droit des faillites français : l'efficacité des procédures préventives (chapitre 1), l'efficacité des tribunaux de commerce (chapitre 2) et l'impact de l'information sur la défaillance passée (chapitre 3).

Ces questionnements permettent à la fois de mettre en lumière les spécificités du droit français et la qualité de son fonctionnement, mais aussi de tirer des enseignements pour la manière dont est pensé l'accompagnement des entreprises en difficulté dans les économies développées.

Chapitre 1

Dans un premier chapitre, co-écrit avec Anne Epaulard, nous questionnons l'efficacité de la procédure préventive de sauvegarde par rapport au redressement judiciaire. Si on juge l'efficacité d'une procédure à l'aune de son taux de restructuration (par rapport à la liquidation), alors les chiffres de la sauvegarde pointent à sa plus grande efficacité : 65% des entreprises en sauvegarde se restructurent, contre 25% en redressement judiciaire. L'explication la plus intuitive de ces résultats serait de dire que la procédure préventive est meilleure car les entreprises sont en moins grande difficulté, donc leurs chances que la restructuration réussisse sont plus grandes. En effet, la principale différence entre la sauvegarde et le redressement judiciaire tient au fait qu'en sauvegarde, l'entreprise n'est pas en état de cessation des paiements. Nous avons souhaité tester cette hypothèse avec une mesure causale de l'impact de la sauvegarde sur la restructuration des entreprises par rapport au redressement judiciaire. Nous avons montré que, pour une même entreprise, l'entrée en procédure de sauvegarde augmente de 50 points de pourcentage les chances d'obtenir une restructuration par rapport à une conversion en redressement judiciaire.

Ce résultat ne découle pas de la meilleure santé financière de l'entreprise ; il découle de la réputation des procédures. Le redressement judiciaire fait peur, désincite les clients et parties prenantes à s'engager avec l'entreprise par peur de la liquidation, et par un effet auto-réalisateur, diminue grandement les chances de survie de l'entreprise. On estime que ce coût indirect et réputationnel peut monter jusqu'à 20% de la valeur de l'entreprise. Ce premier papier s'inscrit dans le sens des politiques publiques récentes : il faut mettre l'accent sur la prévention des difficultés pour maximiser les chances de survie des entreprises.

La méthode de ce premier chapitre repose sur une observation empirique : les tribunaux de commerce sont hétérogènes. Nous avons exploité ce constat comme un outil, tirant parti du fait que d'un tribunal à l'autre, l'évaluation de l'état de cessation des paiements diffère et une procédure en sauvegarde peut être ou ne pas être convertie en redressement judiciaire durant la période d'observation, selon l'appréciation des juges du tribunal. Ainsi, pour toute une partie des entreprises entrées en sauvegarde et dont la cessation des paiements est sujette à interprétation, la conversion de sa procédure en redressement judiciaire est exogène à sa situation économique réelle et dépend uniquement du tribunal auquel elle est assignée. La propension à convertir du tribunal représente notre instrument dans notre méthode par variable instrumentale.

Le constat empirique de l'hétérogénéité des tribunaux de commerce m'a menée vers l'objet de mon deuxième chapitre : cette hétérogénéité a-t-elle un impact sur l'efficacité du droit des faillites ?

Chapitre 2

Dans ce deuxième chapitre, co-écrit avec Anne Epaulard, nous sommes parties du constat que les tribunaux de commerce sont hétérogènes entre eux, et de la croyance populaire que les petits tribunaux sont moins efficaces que les grands. En effet, la proximité entre les juges et les dirigeants d'entreprises, les éventuels conflits d'intérêts découlant du manque d'anonymat, ou encore la pression sociale à sauvegarder l'emploi local orienteraient les jugements des petits tribunaux en faveur d'un biais de continuation, c'est-à-dire la réorganisation d'entreprises non-viables. Or, un processus de défaillance est efficace s'il évite deux types d'erreurs : restructurer les entreprises non-viables

(l'erreur de Type 1) et liquider les entreprises viables (l'erreur de Type 2).

Nous tirons parti de la réforme de la carte judiciaire de 2009 pour prédire et tester empiriquement les gains d'efficacité provoqués par l'absorption des plus petits tribunaux. Nous proposons un cadre conceptuel qui définit les erreurs de Type 1 et de Type 2 selon les différentes étapes du processus de défaillance, à savoir l'entrée en redressement judiciaire ou en liquidation judiciaire directe qui découle du choix du tribunal, l'obtention d'un plan de restructuration qui implique en plus les créanciers, et la survie de l'entreprise qui émerge de la défaillance. Par une méthode en différence-de-différence, nous observons si les tribunaux impactés par la réforme ont des taux de redressement, de restructuration et de survie différents après la réforme, en comparaison des tribunaux non affectés par la réforme. Nous trouvons que l'issue des procédures est modifiée pour les entreprises qui dépendaient du ressort d'un tribunal absorbé, tandis que les entreprises dépendant du ressort d'un tribunal absorbant ne sont pas impactées. En l'occurrence, l'absorption des petits tribunaux résulte en la diminution de l'erreur de Type 1, et n'impacte pas l'erreur de Type 2. Nous interprétons ces résultats par le fait que les petits tribunaux absorbés par la réforme présentaient effectivement un biais de continuation, mais uniquement pour les microentreprises. Pour les entreprises de taille supérieure, la réforme n'a pas eu d'impact notable.

La modification de l'issue de la faillite s'explique par une transmission du comportement du tribunal absorbant aux entreprises issues de la juridiction du tribunal absorbé. L'influence entre les deux tribunaux est réciproque, mais le poids du tribunal absorbant est le plus important. Nous concluons sur l'importance de la qualité du tribunal absorbant pour garantir des gains d'efficacité en cas de réorganisation judiciaire.

Chapitre 3

Une fois l'entreprise parvenue à un accord de restructuration, est-elle tirée d'affaire ? Les deux précédents chapitres se sont fondés sur l'obtention d'une restructuration, et éventuellement de la survie après la restructuration, comme facteur d'efficacité de la défaillance. Pour survivre, l'entreprise doit encore respecter les échéances de remboursement de sa dette restructurée et maintenir son activité. Parmi les facteurs décisifs de sa réussite figure son accès au crédit bancaire. Sachant que les plans de

restructuration durent en moyenne dix ans, si l'entreprise ne peut plus emprunter ni investir, sa survie en sera fortement compromise.

C'est pourquoi le troisième chapitre de cette thèse analyse l'accès au crédit bancaire des entreprises restructurées. Je questionne l'existence de contraintes de crédit bancaire associées à une stigmatisation liée à la défaillance passée.

Pour tester l'existence d'une telle stigmatisation, je tire parti du fait que l'information sur la défaillance passée de l'entreprise est prise en compte dans sa cote de crédit attribuée par la Banque de France. La cotation de la Banque de France a pour objectif d'informer les banques de la santé financière d'une entreprise et de sa capacité à honorer ses engagements. Lorsque l'entreprise émerge de défaillance avec un plan de restructuration, sa cote de crédit prend automatiquement une valeur « risquée ». Au bout de trois à cinq ans après la défaillance, l'information sur la défaillance passée est effacée et la cote de crédit évolue librement. Par une méthode en différence-de-différence, je montre que la suppression de l'information sur la défaillance passée permet aux entreprises d'accroître leur accès au crédit bancaire. Les entreprises dont la cote de crédit évolue très favorablement obtiennent du crédit de la part de leurs banques historiques. Pour les entreprises dont la cote de crédit devient « neutre » et qui sont donc davantage contraintes que les entreprises dont la cote est bonne, j'observe également une augmentation de crédit bancaire, moins importante, et qui provient en partie de la formation de nouvelles relations bancaires.

Je discute le fait que les banques réagissent à ce changement de cotation malgré le fait que l'information sur la défaillance passée reste disponible par ailleurs gratuitement. Une partie de l'effet provient vraisemblablement de la cote de crédit de la Banque de France qui oriente le comportement des investisseurs. In fine, cette augmentation de l'accès au crédit bancaire résulte en une baisse de l'utilisation du crédit inter-entreprises et une augmentation du taux d'investissement des entreprises en restructuration. Mes résultats vont dans le sens des politiques récentes qui visent à réduire le temps de mise à disposition de l'information sur les défaillances passées. En se concentrant sur l'après-faillite, ce chapitre met l'accent sur l'importance de l'accompagnement des entreprises dans leur rebond.

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Chapter 1

Bankruptcy Costs and the Design of Preventive Restructuring Procedures

This chapter is co-authored with Anne Epaulard and published in
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Preventive Restructuring Procedures. Journal of Economic Behavior &
Organization, 196:229–250.*

Abstract

A European directive requires Member States to give firms access to preventive restructuring procedures. This paper assesses the benefits of having a procedure that is distinct from that for insolvent firms. It is based on the French experience, where a preventive procedure has coexisted alongside the more common restructuring procedure¹ since 2006. The spatial and temporal heterogeneity of the commercial courts' decisions allows the identification of the causal impact of the conversion from the preventive procedure to the common one on the firm's survival chances. Using an (almost) exhaustive sample of preventive bankruptcy filings over 2010-2016, we show that conversion reduces the probability of firm survival by 50 percentage points, which corresponds to indirect bankruptcy costs of around 20% of the firm's assets. Our interpretation is that the low restructuring rate under the common bankruptcy procedure may alarm some of the firm's stakeholders, especially its customers. This in turn aggravates the firm's difficulties and reduces its chances of restructuring under the common procedure. We provide some empirical evidence to support this interpretation. A distinct preventive procedure helps prevent this spiral.

¹In this article, we will refer to the French bankruptcy procedure "Redressement Judiciaire" as "RJ".

Acknowledgements specific to Chapter 1

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“Pioneer in sales, Pixmania files for safeguard”

Le Monde – October 27, 2015

“Pixmania soon in receivership”

Le Monde – January 6, 2016

“Pixmania in receivership: Don’t order anymore!”

Le Monde – January 26, 2016

1 Introduction

In 2019, an EU directive invited Member States to introduce into their commercial laws a court-supervised preventive procedure for businesses.¹ The rationale for a preventive procedure is that the sooner a firm’s difficulties are addressed, the greater its chances of survival. This EU initiative reflects a recent general interest in preventive procedures, as exemplified by the recent OECD study in which the existence of a preventive procedure is considered a good idea.²

There are two ways of introducing a preventive procedure into commercial law. One can either extend existing bankruptcy procedures to allow firms on the verge of insolvency to file for it – as is the case in US Chapter 11 – or create a specific procedure exclusively for these firms. In this paper, based on an empirical analysis of the French experience of the preventive procedure introduced in 2006, we argue that if the existing bankruptcy procedure entails low survival rates for firms, creating a specific procedure might be more efficient. The reason is that the specific procedure would serve as a signal for stakeholders that firms allowed to file for it are in a relatively better financial situation than firms entering the regular bankruptcy procedure. That would help reduce indirect bankruptcy costs and thus preserve the survival chances of these firms.

It is well known that firms usually prefer out-of-court restructuring as it better preserves their reputation vis-a-vis not only their creditors and suppliers, but also their clients and employees. Yet, out-of-court restructuring is not always possible, as it generally requires consensus amongst the creditors involved in the restructuring process, and restructuring agreements cannot be extended to other creditors. Also, out-of-court restructuring can suffer from coordination problems amongst creditors (see [Morris and Shin \(2004\)](#)). For already insolvent firms, the usual path is to file for bankruptcy. Once the firm files for bankruptcy, its financial difficulties are exposed, and its reputation suffers. The firm then has to convince its clients, trade creditors, employees, and suppliers to carry on doing business with it. [Altman \(1984\)](#) highlighted that a bankruptcy filing could trigger changes in clients’ behavior who become reluctant

¹cf. <https://eur-lex.europa.eu/legal-content/EN/TXT/PDF/?uri=CELEX:32019L1023>

²[Adalet McGowan and Andrews \(2018\)](#).

to deal with the firm. This results in lost sales and profits. There is indeed some empirical evidence that customers' behavior changes after a bankruptcy announcement. In the automotive industry, [Hortaçsu et al. \(2013\)](#) shows that after a car manufacturer files for bankruptcy, the price of its cars falls in the second-hand market. The empirical literature has also shown that firms in bankruptcy respond to the threat of a drop in demand for their products by reducing their price ([Borenstein and Rose \(1995\)](#), [Ciliberto and Schenone \(2012\)](#), [Phillips and Sertsios \(2013\)](#)). This supports the idea that what is at stake after a bankruptcy filing is not only the goodwill of creditors but also the demand for the firm's products. Indeed, the communication of firms filing for bankruptcy is often aimed at reassuring its customers³ more than its creditors (who already have all the information, and whose coordination is ensured by the court). Costs associated with reputational losses are part of the indirect costs of bankruptcy.⁴ A bankruptcy procedure specific to fragile but still solvent firms might reduce reputational losses and better preserve survival chances. This is all more the case given that the usual bankruptcy procedure has low survival rates.

To test the hypothesis that a specific procedure better preserves survival chances, we take advantage of the coexistence in France of two court-supervised debt restructuring bankruptcy procedures: the receivership (*Redressement Judiciaire*, or RJ) for insolvent firms, and the safeguard (*Sauvegarde*) procedure available to firms that are not yet insolvent. These two bankruptcy procedures yield radically different results in terms of the success of the debt restructuring. Commercial courts can convert a case from the "good" bankruptcy procedure (safeguard) to the one that is "not as good" (receivership). In turn, the conversion may trigger changes in the behavior of some or all of the firm's stakeholders. This can be illustrated by the way newspapers report on bankruptcy procedures.

An extreme example is Pixmania, a French e-commerce company founded in 2000,

³For example, [Altman \(1984\)](#) points to the efforts made by Chrysler's management to maintain public confidence in the firm's ability to continue after it filed for bankruptcy.

⁴In addition to lost sales and profit, these indirect costs arise due to inter- or intra-group conflicts of interest, asymmetric information, hold-out problems, higher operating costs, and the ineffective use of management time ([Altman \(1984\)](#), [Opler and Titman \(1994\)](#), [Bris et al. \(2006\)](#), [Almeida and Philippon \(2007\)](#)). These indirect costs are difficult to measure but are commonly viewed in the literature as being substantial, at up to 20% of firm asset value (e.g., [Andrade and Kaplan \(1998\)](#), [Bris et al. \(2006\)](#), [Hotchkiss et al. \(2008\)](#), [Davydenko et al. \(2012\)](#)).

that filed for safeguard in 2015. On October 27, 2015, the French newspaper *Le Monde* ran a short article with a somewhat flattering title, “Pioneer in online sales, Pixmania is placed in safeguard procedure.” This short article highlighted that the firm was working on a new operating model to restore profitability. On January 6, 2016, the same newspaper ran a short article about the likely conversion of the case to a receivership. Then on January 26, after some consumers reported not receiving their orders, it ran a new article with the alarming title, “Pixmania in receivership: Don’t order anymore!”. Delays in shipments happen all the time, but in the case of Pixmania, the fact that the company was in receivership triggered an overreaction by the newspaper... and maybe by the firm’s clients. Ultimately, Pixmania did not restructure its debt and was liquidated.

To measure the impact of receivership on restructuring, we rely on the fact that a significant share of safeguard cases are subsequently converted into receiverships by courts, as was the case for Pixmania. This allows us to use data only on bankruptcy cases that started as a safeguard, thus avoiding the selection bias associated with safeguard filing relative to receivership filing. The court can convert a safeguard case into a receivership if the assessment of the firm’s financial situation reveals that it is already insolvent or on the verge of insolvency. However, the exact trigger for conversion may be interpreted differently across commercial courts and over time. In our (almost) exhaustive sample of safeguard bankruptcy filings in France over 2010-2016, we find considerable heterogeneity in yearly conversion rates amongst the 134 French commercial courts, ranging from 0% to 100%. We use this heterogeneity in court \times year conversion rates to construct an instrument to identify the impact of conversion on the probability of successful debt restructuring.

Overall, our empirical results suggest that conversion to receivership reduces the chance of debt restructuring by 50 percentage points. It also reduces by 40 percentage points the survival rate of firms at the five-year horizon. Additional results suggest that firms in sectors more likely to suffer from reputational losses once converted are indeed more impacted by conversion. In addition, we carry out various robustness checks, none of which affect the results. A back-of-the-envelope calculation based on our results suggests indirect costs associated with receivership represent about 20% of the total book value of the firm.

2 Bankruptcy Procedures in France and Their Outcomes

2.1 Bankruptcy procedures

There are two different procedures for a firm seeking court-supervised debt restructuring in France: safeguard and receivership. The main difference between the two is the extent of the financial difficulties that the firm faces. An insolvent firm has access to receivership, while a firm that is not (yet) insolvent but can prove that it is facing severe financial problems has access to safeguard. Table 1 summarizes the main features of the two procedures, both of which resemble the US Chapter 11 bankruptcy procedure.⁵ A standard liquidation procedure exists in addition to these two procedures (akin to US Chapter 7).

The receivership procedure

French commercial law is such that a firm that cannot meet its payment obligations is considered insolvent if its liquid assets do not cover these. Unless it starts an out-of-court restructuring, an insolvent firm has to file for receivership within 45 days to start a debt restructuring process with its creditors. In practice, some insolvent firms do not respect this 45-day limit, and the firm's creditors, as well as the court itself, can bring the case before the court to trigger a receivership. Insolvent firms can also file directly for liquidation.

Once the firm files for receivership, it is protected from creditors, and interest and principal payments are suspended. The firm also has access to a loan to pay up to three months of wage arrears to its employees.⁶ There is a six-month observation period to assess the firm's financial situation. During this period, a court-appointed receiver establishes the list of the firm's liabilities. The court also appoints an insolvency administrator to monitor the firm's day-to-day operations, notably all of its financial transactions as well as some major restructuring decisions (e.g., firing employees, selling

⁵For additional comparisons between American and French bankruptcy procedures, see [Plantin et al. \(2013\)](#).

⁶This scheme is funded by a mandatory contribution based on each private firm's wage bill. At the macro level, the contribution rate is adjusted to keep the fund balanced. Since July 2017, this rate has been 0.15%.

2. BANKRUPTCY PROCEDURES IN FRANCE AND THEIR OUTCOMES

Table 1: Comparison of the safeguard and receivership procedures

	Safeguard	Receivership
Conditions for starting the procedure	The firm is not insolvent and can prove it is facing severe financial difficulties	The firm is insolvent (short-term financial obligations not covered by liquid assets)
Who can start the procedure?	Only the debtor	Once insolvent, the debtor <u>must</u> start the procedure within 45 days. Its creditors or the court can trigger the procedure if the debtor does not
Is court approval needed to start the procedure?	YES The court allows the opening of the procedure after checking that the firm: (i) is not insolvent (ii) is facing severe financial difficulties	NO
Role of the court at the onset of the procedure	Appoints a receiver Appoints an administrator for large firms (the debtor can suggest an administrator)	Appoints a receiver Appoints an administrator
Observation period	6 months twice-renewable	
Protection of the debtor from its creditors during the observation period	YES	
Grace period on interest and principal payments	YES	
Role of the receiver	Establishes a comprehensive list of the firm's debts	
Role of the administrator	Assists the firm management in most decisions	Makes most decisions
Access to loan for wage arrears	NO	YES
Debt restructuring plan	Negotiated with creditors by the debtor and the administrator	
Creditor committees	YES for large firms	
Possible outcomes	Restructuring Conversion to receivership Liquidation	Restructuring Liquidation

assets). The administrator can also prevent the firm's manager from undertaking actions that would reduce the firm's asset value. The administrator and the receiver negotiate with creditors to establish a debt restructuring plan. The observation period can be renewed twice and lasts for a maximum of 18 months. At any time during the observation period, the court can accept (or reject) a debt restructuring plan negotiated with creditors. The typical plan is a mixture of debt rescheduling (up to 10 years) and a haircut (if, given the option, some creditors prefer an immediate partial payment to settle the final balance over rescheduling). If the situation deteriorates further during the observation period or there is no hope of reaching an agreement with creditors, the court can decide to liquidate the firm.

Liquidation can take two forms. In the most abrupt form, the firm's assets are sold on the market, with the proceeds going to the firm's stakeholders according to priority rules⁷ (with employees having the highest priority for unpaid wages). A smoother form of liquidation consists of opening a bidding process for all or part of the firm's assets and some or all of its employees, with a view to keeping at least part of the firm as a going concern. The court receives bids from potential buyers and chooses amongst them. If there are no buyers or the court rejects all of the offers, the firm ceases operations, its assets are sold, and stakeholders are reimbursed according to the same priority rule as above.

The safeguard procedure

In 2006, to allow earlier debt restructuring, a new bankruptcy procedure was introduced in French commercial law⁸: the safeguard procedure. Firms can file for safeguard if they are not (yet) insolvent but face financial difficulties that they consider impossible to overcome without a debt restructuring. After a rapid background check of the firm's financial situation, the court accepts (or rejects) the safeguard filing. The safeguard filing can be rejected if the firm is already insolvent (in which case a receivership procedure is

⁷Data on the recovery rates for creditors are challenging to find. [Blazy et al. \(2018\)](#) calculate that, in a small sample of 264 cases opened in the Paris commercial court, the recovery rates in liquidation are as low as 34.9% for secured creditors and 5.0% for unsecured creditors, as compared to 75.5% and 73.1% respectively in continuation.

⁸The Bankruptcy Protection Act of July 26, 2005, came into force on January 1, 2006. At the same time, informal preventive procedures were also enhanced in France. [Kastrinou \(2016\)](#) compares pre-insolvency informal procedures in France and the UK.

opened) or, on the contrary, if the court considers that the financial situation does not require debt restructuring.

Once the court accepts the safeguard filing, the steps are pretty similar to those of the receivership procedure: the proceedings are public,⁹ the six-month observation period is twice-renewable, and the court appoints a receiver. The court can also appoint an administrator and must do so for the largest firms (in receivership, the court appoints an administrator regardless of firm size). The role of the administrator is less important in a safeguard than in receivership: she only assists the manager. She cannot make decisions without the manager's consent, whereas she can in receivership. An important difference between the safeguard and receivership procedures is that in a safeguard, the firm does not have access to a loan from the insurance scheme to pay for wage arrears. Table 1 summarizes the main features of the two procedures.

Conversion from safeguard to receivership

As stated by the law, the court can convert a safeguard case into a receivership at any time during the observation period if the assessment of the financial situation reveals that the firm was already insolvent at the time it filed for safeguard, has become insolvent, or is currently on the verge of insolvency. The court will consider conversion at the request of the administrator, the receiver, the Public Prosecutor's Office, or (since September 2014) the firm itself. Up to July 2014, the judge herself could carry out this conversion.

Discussions with various bankruptcy-procedure stakeholders have led us to hypothesize that, in practice, there are (at least) three situations that can potentially trigger a conversion decision. First, a careful examination of the firm's financial accounts, often with the help of chartered accountants appointed by the court, raises doubts that the firm was still solvent when it filed for safeguard. In the second situation, the firm runs out of cash during the observation period and defaults on the financial obligations it contracted after filing for safeguard. In addition to these two situations, which fit the wording of the law fairly well, a third situation was once mentioned during our discussions with stakeholders: the court may consider that the measures being taken by

⁹All openings of bankruptcy procedures (safeguard, receivership and liquidation), as well as all of the main court decisions during the procedures, are available in a daily publication that has appeared online since 2008.

the debtor are endangering the firm. The court then converts the case to a receivership to appoint an insolvency administrator of its choice to run the company. It is worth noting that the conversion of safeguard to receivership does not grant access to a loan to pay for wage arrears (whereas direct receivership entry does).

Role of creditors and shareholders in the procedure

During the observation period, creditors cannot decide on the appointment of the administrator or the receiver nor the duration of the procedure. They cannot ask to convert a safeguard case into a receivership. The court-appointed receiver consults with the creditors to draw up a proposal for a debt restructuring plan. The creditors then express an opinion on the plan proposal. The law provides several ways of expressing these opinions; however, in practice, the process generally takes the form of a written consultation by the receiver. The law stipulates that creditor committees must be organized for larger firms (creditors are assigned to committees according to their seniority), which allows specific proposals to be reviewed at meetings of creditors. In parallel to these committees, and for smaller firms, a consultation is organized with creditors who are not part of the committees. At the end of the consultation, the court rules on the proposed plan according to the creditors' answers. If the court accepts the plan, it will take note of the terms of repayment accepted by the creditors and impose terms of repayment on those creditors who have refused the proposal. [Kastrinou \(2016\)](#) provides a good description of the main provisions of the safeguard procedure, as well as detailed information on informal pre-insolvency procedures in France.

Regarding equity holders, capital transactions are supervised by the court once the firm files for receivership. This is not the case with a safeguard. In contrast to receivership, the adoption of the plan in a safeguard cannot be made conditional on the forced transfer of shares. In both receivership and safeguard, equity holders cannot block a proposal that the creditors have approved. In addition, if part of the debt is converted into capital, the court can force the shareholders to accept capital dilution.

2.2 Data and bankruptcy-procedure outcomes

The data used here come from bankruptcy filings contained in the *Bulletin Officiel d'Annonces Civiles et Commerciales* (BODACC), provided by the registry offices of the commercial courts. This public information has been available electronically since January 2008 as one electronic file per business day.¹⁰ We constructed a dataset of all bankruptcy filings in France over the 2010-2016 period and followed their outcomes up to December 2018 and their survival up to May 2020. Our dataset contains 324,748 filings:¹¹ 8,070 safeguard filings, 96,105 receivership filings, and 220,573 direct liquidations. We follow up on 7,927 safeguard cases (98%) and 95,017 receivership cases (99%), constituting our initial sample. By definition, there is no follow-up for liquidations. From the BODACC, we gather information on each firm's address, the commercial court in charge of the case, the dates of filing, and the subsequent main judgments (renewal of the observation period, agreement on debt restructuring with creditors, liquidation, etc.). We notably identify the safeguard cases that are converted to receivership.

We restrict the sample of safeguard-filing firms to exclude those liquidated within six months of the filing. This allows us to eliminate obvious errors in the choice of procedure for the firm (restructuring vs. liquidation) and to concentrate our work on firms with a reasonable chance of restructuring.¹²

We complement these data with firm-level economic and financial information from the balance sheets and income statements made available by INSEE, the French statistics office, over the 2009-2015 period. This provides us with the last available information on each firm before it filed for bankruptcy, including data on the number of employees, total assets and their structure (financial, tangible, and intangible), total debt and its structure (banks, suppliers, and other), interest payments, total sales, operating income, industry, age, legal status, and so on. After matching, our final sample contains

¹⁰See <https://www.data.gouv.fr/fr/datasets/bodacc/>.

¹¹These numbers are below those reported by Altares, a privately-owned company that produces statistics on bankruptcy filings in France. Our sample is restricted to commercial courts. As such, it excludes bankruptcy filings from farmers, the self-employed, and professional practices. It also excludes courts in three French metropolitan *départements* as well as in the overseas territories (see footnote 16).

¹²This operation removed 656 safeguard-filing firms from our sample. We thank Claire Lelarge for this suggestion.

5,678 safeguard cases, 805 (14.2%) of which are converted to receivership, and 66,849 receivership filings, of which 39,565 are voluntary (with the others triggered by either creditors or by the court itself¹³).

Table 2: Breakdown per year of filing

Table 2 shows the number of safeguard entries over the 2010-2016 period, the ratio of safeguard cases over all restructuring cases (safeguard and direct receivership), and the share of safeguard cases converted by year of filing. For instance, our analysis sample contains 737 firms that filed for safeguard in 2010, 15.6% of which were subsequently converted into receivership.

Year of filing	Number of Safeguard filings	Share of safeguard cases converted to receivership	Share of Safeguard filings
2010	691	0.156	0.116
2011	737	0.183	0.114
2012	844	0.150	0.113
2013	935	0.139	0.128
2014	900	0.124	0.133
2015	868	0.105	0.123
2016	703	0.145	0.107
Total	5,678	0.142	0.120

Table 2 presents a breakdown of the number of safeguard filings per year in our final sample over the period 2010-2016. The number of filings increase steadily to slightly over 900 cases per year until 2013, stabilize in 2014, and fell below 800 in 2016. Over 2010-2016, safeguard filings represent between 10.7% and 13.3% of court-supervised reorganizations. The vast majority of court-supervised reorganizations are receivership procedures.

The outcomes of the safeguard procedure are summarized in Table 3. In the full sample over the 2010-2016 period, 69.7% of safeguard-filing firms managed to restructure their debt with their creditors (column (1)), compared with only 27.3% for receivership-filing firms (column (5)). From this point of view, the safeguard procedure has a higher percentage of restructuring deals than US Chapter 11.¹⁴

¹³The information regarding the nature of the receivership filing was provided by the Conseil National des Mandataires Judiciaires et Administrateurs Judiciaires, an association of court-appointed receivers and insolvency administrators.

¹⁴Bernstein et al. (2019b) note that in their large sample, around 40% of Chapter 11 cases were converted to liquidation.

Table 3: Summary Statistics

Table 3 shows the summary statistics for firms filing for safeguard and receivership between 2010 and 2016. Column (4) reports the test of the equality of the means between the samples of safeguard cases that were not converted to receivership (column (2)) and those converted to receivership (column (3)). The full sample contains all cases from the BODACC as described in Section 2.2. We restrict the sample of safeguard-filing firms by excluding firms which were liquidated within six months of their filing. After matching with the financial and economic information, we obtain the final sample. The other entries are self-explanatory.

Initial filing in:	Safeguard			Receivership		
	All Safeguard (1)	Cases not converted to receivership (2)	Cases converted to receivership (3)	Diff (2)-(3) (4)	All Receivership (5)	Voluntary Receivership filing (6)
Full sample	7,927	6,682	1,245		95,017	50,295
Share of debt restructuring	0.615	0.697	0.178	0.520***	0.273	0.234
Final sample	5,678	4,873	805		66,849	39,565
Share of debt restructuring	0.719	0.799	0.232	0.567***	0.325	0.255
Survival rate after restructuring						
After 2 years	0.809	0.808	0.844	-0.036	0.711	0.837
After 4 years	0.679	0.682	0.606	0.077*	0.506	0.725
Survival rate after filing						
After 3 years	0.606	0.674	0.199	0.475***	0.239	0.218
After 5 years	0.475	0.532	0.144	0.389***	0.169	0.171
Number of employees	29.6	29.1	32.5	-3.435	15.2	18.0
Age	11.9	11.7	13.6	-1.899***	8.9	9.6
Share of zombie firms	0.579	0.565	0.661	-0.096***	0.605	0.634
Fixed assets/Assets	0.545	0.554	0.493	0.060***	0.494	0.507
Financial assets/Fixed assets	0.161	0.163	0.151	0.012	0.098	0.101
Total debt/Assets	0.804	0.805	0.798	0.06	1.024	0.997
Supplier debt/Total debt	0.262	0.257	0.294	-0.038***	0.275	0.281

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

In our final sample over 2010-2016, 71.9% of the firms filing for safeguard restructured their debt, which is higher than the share for firms under a receivership procedure. 80.9% of the firms that reached a deal with their creditors in safeguard were still operating two years after the deal, while for receivership firms the proportion was 71.1%. When we look at the survival rate after filing for bankruptcy, irrespective of whether the firms reached a debt restructuring agreement, 60.6% of safeguard-filing firms were still alive three years into the procedure, compared with 23.9% for receivership-filing firms. These numbers do not, however, necessarily prove the superiority of safeguard over receivership, as firms entering these two bankruptcy procedures do not start with the same level of financial distress. By design, firms filing for safeguard have less-severe financial problems than those filing for receivership. They may also have unobservable characteristics that distinguish them from those filing for receivership and affect the outcome of the bankruptcy procedure: they may be more proactive, for example, or have a better-informed management. Our identification of the effect of receivership will thus examine the fate of firms that initially filed for safeguard and were subsequently converted to receivership by the court.

Amongst the safeguard filings, 14.2% were subsequently converted into a receivership. For firms whose safeguard filings were converted into receivership, the probability of reaching a debt restructuring agreement with their creditors was 23.2%, which is lower than the rate for voluntary receivership filings (41.2%), and much lower than the rate for firms whose safeguard filings were not converted into a receivership (79.9%). For firms that managed to restructure their debt, the two-year survival rate for those converted to receivership was 84.4%, which is slightly higher than for cases that remained in the safeguard procedure up to the end (80.8%), but the difference is not statistically different from zero. The analogous four-year survival rates are 60.6% and 68.2%, respectively. The unconditional survival rates, however, three and five years after filing for safeguard, are respectively 67.4% and 53.2% for firms whose safeguard filings were not converted into a receivership, and 19.9% and 14.4% exclusively for firms whose safeguard filing were converted into a receivership. These differences are statistically significant at the 1% level.

Regarding the financial and economic situation of our sample firms, those whose

safeguard filings were converted into receivership were, on average, older than those whose safeguard filings were not converted (13.6 years old versus 11.7 years), had more supplier debt (29.4% of their total debt vs. 25.7%) and lower fixed assets (49.3% of their total assets vs. 55.4%) and were more likely to be labeled as “zombies”¹⁵ (66.1% vs. 56.5%). A simple test of the equality of means reveals that these differences are statistically different from zero (see column (4) of Table 3).

3 Heterogeneity in Commercial Court Decisions

3.1 Commercial courts and lay judges

There are currently 134 commercial courts and 28 courts of appeal spread out over France.¹⁶ There is at least one commercial court in each of the 95 *départements*, with some *départements* having two or even three. These courts deal with bankruptcy proceedings for companies and commercial disputes, with the former accounting for 20% of the cases that they hear. In bankruptcy hearings, the firm is assigned to a court according to the location of its headquarters, and there is no possibility of forum shopping by either the firm or its creditors. Note that, due to administrative boundaries, firms are not necessarily assigned to the commercial court that is nearest to them, a fact that we will exploit later for identification purposes.

There are around 3,000 commercial lay judges. These are chosen from and elected by business leaders and executives. Judges are unpaid volunteers; they sit for only one or two half-days per week and carry on with their everyday business activities the rest of the time. They cannot work on cases related to their own businesses for obvious reasons. Each judge is initially elected for a two-year term and can then be re-elected three times for a four-year term (for a maximum of 14 years). There is one election per

¹⁵We label a firm as a “zombie” if its financial obligations were greater than its operational income the year before the filing. Our definition is slightly different from that used by the OECD (see [Adalet McGowan et al. \(2017\)](#)), for which “zombie” firms are over ten years old with financial obligations greater than operational income for over three consecutive years.

¹⁶While French commercial law does not differ from one court to another, we exclude some parts of France from our research: the Courts in three *départements* (Moselle, Haut-Rhin and Bas-Rhin) as, for historical reasons, they operate somewhat differently from courts in the rest of the country. We also exclude the overseas *départements* and territories for the same reason. [Esquerré \(2019\)](#) shows that courts in Alsace-Moselle deal differently with bankrupt firms.

year at the court level, and the terms start in early January. There is thus a regular turnover among judges. The judges elect from amongst themselves the President of the Court and two vice-presidents appointed for a four-year, non-renewable term. Judges receive legal training both after being chosen and during their term of office. A typical hearing involves more than one judge, as courts are organized into chambers. Each case is allocated to a specific judge¹⁷, but decisions are taken by consensus by all the judges in the chamber.

3.2 Heterogeneity

Our identification strategy, which we present in Section 4, is based on the observation that while French bankruptcy law is national, its interpretation can differ from one court to another and over time. This is especially true for the decision to convert a safeguard into a receivership, which is based on the notion of insolvency or “closeness to insolvency.” Here insolvency means that a firm’s short-term debt exceeds its liquid assets. There is room for interpretation of the firm’s financial situation, as the valuation of the firm’s liquid assets and the meaning of “close to insolvency” can differ from one court to another (depending, for example, on their chartered accountants). In addition, the interpretation of the law may vary from one court to another and over time, notably because of the regular arrival of inexperienced judges. [Iverson et al. \(Forthcoming\)](#) show that, in the United States, the outcome of bankruptcy procedures varies with the judges’ experience. Our instrument relies on these heterogeneities, which we discuss below.

Court heterogeneity

There is significant heterogeneity amongst commercial courts, both in terms of their size and the outcomes of their procedures. Regarding heterogeneity in court size, Table 4 shows that, over the 2010-2016 period, on average, around 2,414 new bankruptcy cases (direct liquidations, receivership, and safeguard procedures) were opened, with the smallest court registering only 273 cases and the largest one almost 20,000. The smallest court has only nine judges, and the largest 180. The share of safeguard filings amongst restructuring filings (thus excluding direct liquidations) is, on average, 8.1%

¹⁷Note that the name of the judge to whom the case is assigned is not available in our database.

3. HETEROGENEITY IN COMMERCIAL COURT DECISIONS

Table 4: Court Heterogeneity

Table 4 shows the summary statistics for the 134 French commercial courts over the 2010-2018 period. The share of direct liquidations is the number of direct liquidations over the number of direct liquidations and direct receiverships. The share of safeguard filings is the number of safeguard filings over the number of safeguard and direct receivership filings. The share of cases converted is the number of safeguard filings converted by the court over the total number of safeguard filings.

	Number of courts	Mean	Median	Standard deviation	Min.	Max.
	(1)	(2)	(3)	(4)	(5)	(6)
Number of judges	134	24.5	19.5	19.2	9	180
Number of filings	134	2414.4	1,681.0	2,473.7	273	19,954
Safeguard	134	59.2	41.0	53.5	4	308
Receivership	134	709.2	546.5	542.5	74	3,079
Liquidation	134	1,646.1	1,065.5	2,013.4	191	16,982
Share of direct liquidations	134	0.668	0.665	0.069	0.461	0.897
Share of safeguard filings	134	0.081	0.066	0.052	0.013	0.326
Share of cases converted	134	0.170	0.165	0.086	0.000	0.500
Share of debt restructuring						
in safeguard	134	0.579	0.589	0.117	0.200	0.929
in receivership	134	0.335	0.330	0.069	0.179	0.505

but with considerable variations across courts, with one, in particular, registering 32.6% safeguard procedures. Regarding outcomes, heterogeneity between courts is larger for safeguard procedures than receivership. The average court reaches a restructuring deal in 57.9% of its safeguard cases, with the actual percentage ranging from 20% to 93%. This range is much narrower for receivership, at 15% to 48% with a mean of 31%. The share of safeguard cases converted to receivership is also heterogeneous across courts, ranging from 0% to 50%. This is depicted in Figure 1, which maps the average conversion rate of courts over the 2010-2018 period. High conversion rates (over 21.7%) are found everywhere, north and south, east and west, and do not necessarily correspond to large cities or rural areas. For example, Toulouse and Lille have amongst the highest conversion rates (29% and 28% of cases converted, respectively) while other large cities such as Bordeaux and Paris are in the second quartile (with shares of 19% and 13% respectively). Looking at rural areas,¹⁸ it is striking that two adjacent jurisdictions that deal with the same type of firms, Cahors and Aurillac, have very different conversion

¹⁸Farmers' bankruptcies are not included in our sample as they are dealt with in specific courts.

rates (5% and 36%, respectively). The same applies to the courts in Montauban and Albi in the southwest of France (3% and 29%, respectively), which are mid-sized cities in the greater periphery of Toulouse.

Time variation within courts

There is slight variation over time in court size, measured as either the number of judges or bankruptcy-case openings. There is much more variation over time within courts in other indicators, notably in the decisions to convert safeguard cases into receivership. The variation in conversion rates within courts actually explains 80% of the total variance in the court \times year conversion rates. This heterogeneity within courts can be the result of the overall or local economic situation, the regular turnover in lay judges, or other potential unobservable factors.

4 Identification Strategy

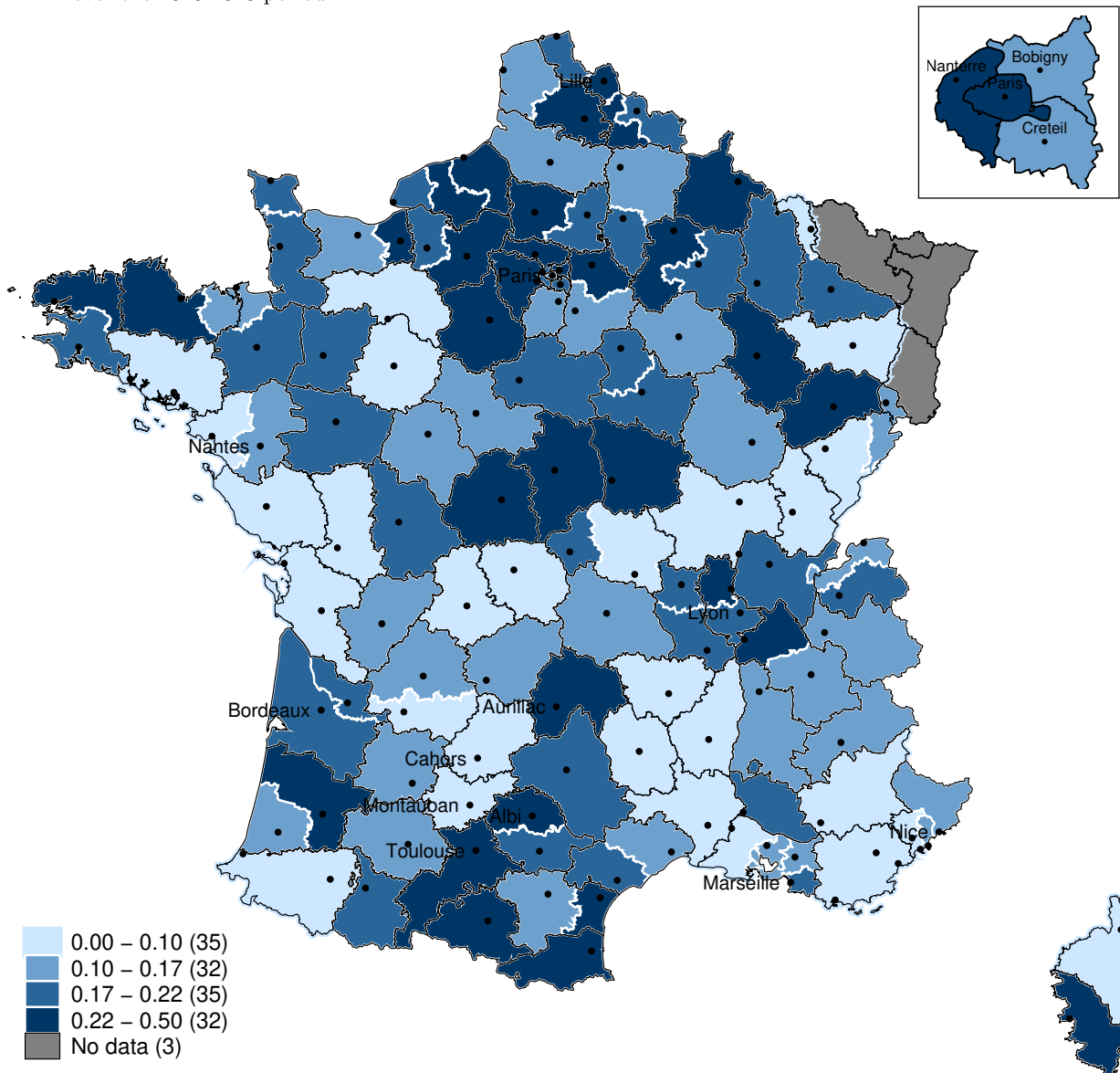
Our identification strategy builds on several empirical studies using heterogeneity in judicial decisions as an instrument to measure the impact of decisions. For example, [Bernstein et al. \(2019a\)](#) and [Bernstein et al. \(2019b\)](#) use the judicial conversion rate of Chapter 11 cases to Chapter 7 to explain the reallocation of assets and bankruptcy spillovers. In a different field, [Maestas et al. \(2013\)](#) use examiner heterogeneity in granting disability benefits to measure the impact of receiving these benefits on labor supply. Their identification strategies are based on the random allocation of judges or examiners to cases. Unlike these contributions, we do not have data on judges but only on commercial courts, and the allocation of bankruptcy cases to courts is not random, as it depends on the location of the firm's headquarters. To ensure that our strategy is valid, we conduct several empirical tests to show that the experiment approaches random assignment once we control for firm-specific characteristics.

4.1 Empirical design

To measure the impact of receivership compared to safeguard, we focus on firms that initially filed for safeguard and exploit that courts subsequently convert a significant

Figure 1: Commercial courts' average conversion rates over the 2010-2018 period

The jurisdictions of French commercial courts are based on *département* boundaries. Some *départements* have two or more commercial courts. Figure 1 maps these jurisdictions: black lines show the *département* boundaries, and the white lines those of jurisdictions within a *département*. The black dots show the location of commercial courts. The three *départements* of Moselle, Haut-Rhin, and Bas-Rhin are excluded (see footnote 16). The upper-right part of the figure zooms in on the region of Paris. Figure 1 shows the average conversion rates of safeguard to receivership in commercial courts by jurisdiction over the 2010-2018 period.



fraction of safeguard cases (14.2%) into receivership. This allows us to eliminate the unobservable characteristics of firms that file for safeguard as compared to receivership.

Following [Bernstein et al. \(2019b\)](#), our model specification is:

$$Y_{i,j,t'} = \alpha + \beta \cdot \text{Conversion}_{i,j,t'} + \gamma_1 X_{i,t} + \mu_{s,t'} + \mu_j + \epsilon_{i,j,t'} \quad (1)$$

Here $Y_{i,j,t'}$ is the dependent variable, the probability that firm i assigned to court j restructures its debt, with t' the year at the end of the observation period, which we will refer to as the judgment year. $\text{Conversion}_{i,j,t'}$ is a dummy for the safeguard case i having been converted to a receivership at t' in court j . We wish to estimate β , the effect of receivership conversion on $Y_{i,j,t'}$. $X_{i,t}$ is a set of firm-level characteristics measured the year before the filing. We include dummies $\mu_{s,t'}$ for industry \times year-of-judgment fixed effects and dummies μ_j for each court. Under the null hypothesis that receivership conversion does not affect $Y_{i,j,t'}$, β should not be statistically different from zero, while a negative value of β means that receivership conversion reduces the chances of debt restructuring. We use a standard IV-2SLS model, as the literature recommends ([Angrist and Pischke \(2008\)](#)). The standard errors are clustered at the court \times year-of-judgment level.

We will later also consider the dependent variable $Y_{i,j,t'}$ as firm survival two and five years after the agreement with the firm's creditors has been reached.

The vector $X_{i,t}$ contains firm financial variables such as the ratio of fixed to total assets, the structure of the assets (financial assets over fixed assets), the debt ratio (total debt over total assets), and the structure of the firm's debt (supplier debt over total debt). It also includes the size of the firm as measured by the number of employees (in logs), dummies for legal status, firm age (1 if the firm is over five years old), and the "zombie" status of the firm. All of these firm-level controls come from the last balance sheet and income statement available before the filing.

Despite all these controls, endogeneity remains a potential problem. The conversion of a filing suggests worsening the firm's financial health that mechanically reduces its chances of survival. We, therefore, identify the causal effect of receivership conversion on firm survival by drawing on the heterogeneity in commercial courts' propensity to

convert safeguard cases into receivership as an instrumental variable.

The instrument

Our instrument is constructed as follows:

$$\phi_{i,j,t'} = \frac{\#conversion_{j,t'} - 1(\text{converted}_{i,t'} = 1)}{\#cases_{j,t'} - 1} \quad (2)$$

where $\phi_{i,j,t'}$ is the share of safeguard cases converted by court j in year t' , excluding the present case i . This measure excludes the mechanical relationship between the instrument for a given case and its conversion decision (Maestas et al. (2013)). To consider that a significant share of the judges in each court are replaced each calendar year, $\phi_{i,j,t'}$ is estimated by year of judgment t' . As 2016 filing judgments can take place up to 2018, the instrument covers the period 2010-2018. The validity of this instrument relies on several assumptions that are discussed below.

The territorially-competent court of a firm filing for bankruptcy is located in the same geographical area as the firm’s headquarters. Thus, the firm has no choice over its court (preventing forum shopping). Even so, firms may choose their headquarters’ location based on the behavior of the commercial court; we have no evidence that this is the case. Empirical analyses of firm locations in France, such as Houdebine and Schneider (1997) and Costes (2008), do not mention this possibility and consider only standard location determinants such as local taxes and subsidies, local policies, and agglomeration economies. In our sample, we can identify only one firm whose change in location (in the three years before filing for safeguard) resulted in a change in its assigned court.

Next, we must ensure that there is no “time shopping” either. This is unlikely as once a firm faces financial difficulties severe enough for a safeguard filing, the time window to file for safeguard is relatively narrow. A firm that waits too long to file may no longer be eligible for safeguard and may thus have to file for receivership. To rule out the possibility of “time shopping,” we can test whether firms’ tendency to file for safeguard is negatively correlated with their assigned court’s current or past conversion rates. The results appear in Table A.1 (see Appendix), where the dependent variable is the ratio of safeguard cases to all restructuring cases at the court \times year level. There is no significant

correlation between current safeguard filings and current or past conversion rates.

We then turn to the firm-level and ask whether a firm's decision to file for safeguard is affected by the court's propensity to convert safeguard cases. For example, firms may be discouraged from filing for safeguard if they know their court has a high conversion rate. We test this assumption by looking at the correlation between the probability that a firm will file for safeguard instead of receivership and the conversion rate of its court in the current and previous years. The results in Table A.2 again reveal no significant relationship.

We can thus consider court \times year assignment as close to random and, after controlling for time and the observable characteristics of the firm, use annual court conversion rates as a source of exogenous variation in the probability that a given case will be converted. Figure 2 shows the distribution of court conversion rates minus the annual mean across all courts, adjusted and unadjusted for the controls and fixed effects in the specification (1). The set of controls does not reduce the standard deviation of the instrument distribution (which is 0.14, compared to 0.15 unadjusted) that we will exploit in our model.

The first stage of our instrumental-variable estimation is as follows:

$$Conversion_{i,j,t'} = \rho + \pi \cdot \phi_{i,j,t'} + \gamma_1 X_{i,t} + \mu_{s,t'} + \mu_j + \epsilon_{i,j,t'} \quad (3)$$

where $Conversion_{i,j,t'}$ is a dummy for firm i being converted to receivership at time t' in court j . The coefficient π shows the impact of the commercial court's propensity $\phi_{i,j,t'}$ to convert a safeguard into a receivership on the probability that a case actually will be converted.

The model

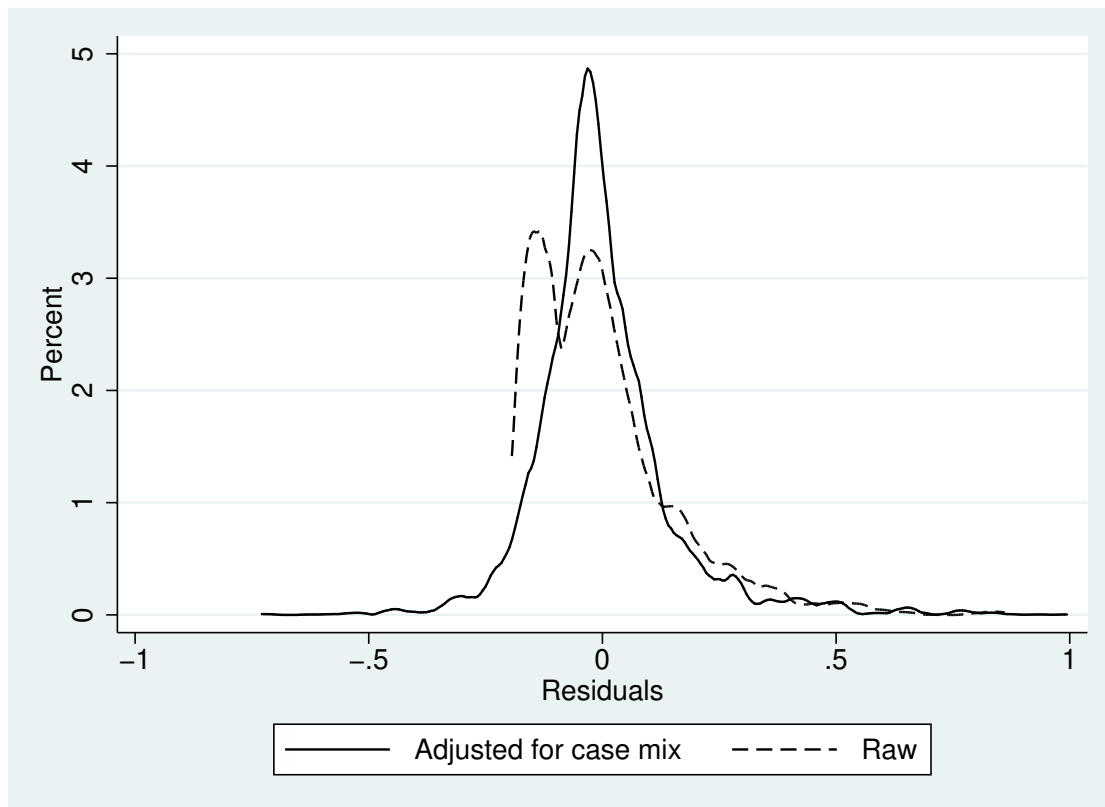
The second step is estimated as follows:

$$Y_{i,j,t'} = \alpha + \beta \cdot \widehat{Conversion}_{i,j,t'} + \gamma_1 X_{i,t} + \mu_{s,t'} + \mu_j + \epsilon_{i,j,t'} \quad (4)$$

where $\widehat{Conversion}_{i,j,t'}$ are the values predicted from the first-stage regression. This

Figure 2: The distribution of court deviations from the mean initial conversion rate

The raw distribution reflects the difference between the court's conversion rate and the unadjusted annual average conversion rate; the adjusted distribution is the difference between the court's conversion rate and the annual average conversion rate adjusted for all of the controls and fixed effects, as in column (2) of Table 5.



equation is similar to equation (1), except that the variation in $\widehat{Conversion}_{i,j,t'}$ comes from the exogenous variation introduced by the court's tendency to convert. If the instrument is valid, then β captures the causal effect of receivership conversion on the firm's probability of debt restructuring. This effect is a local average treatment effect (Angrist et al. (1996)).

Court dummies μ_j appear in equations (3) and (4) to control for unobservable characteristics of the courts and the firms under their jurisdiction. As discussed above, heterogeneity in conversion rates is a combination of heterogeneity between courts and heterogeneity over time within each court. The use of court dummies, with the instrument calculated at the court \times year level, means that we do not draw on heterogeneity in conversion practices between courts: our estimate of the impact of conversion is based only on changes in conversion rates over time within each court.

Assigned court vs. closest court

In order to rule out any possibility that the instrument reflects the local economic conditions rather than the behavior of the court, we design an alternative first stage. We exploit the fact that while firms are assigned to a court depending on their location, due to administrative boundaries, a significant share of firms (19%) are geographically closer to a court that is different from the one to which they are assigned.¹⁹

Let us consider a dummy CC that equals 1 for firms for which the assigned court is also the closest one and 0 otherwise. The first-stage equation is then :

$$\begin{aligned} Conversion_{i,j,t'} = & \rho + \pi_1 \cdot \phi_{i,j,t'} \times CC + \pi_2 \cdot \phi_{i,j,t'} \times (1 - CC) + \pi_3 \cdot \phi'_{i,j',t'} \times (1 - CC) \\ & + \gamma_1 X_{i,t} + \mu_{s,t'} + \mu_a + \epsilon_{i,j,t'} \end{aligned} \tag{5}$$

where $\phi_{i,j,t'}$ is the share of other cases converted in year t' by court j to which firm i is assigned, and $\phi'_{i,j',t'}$ is the analogous figure for the closest court j' . We drop the court-level dummies and introduce court of appeal-level dummies μ_a . We are interested in the coefficients π_1 , π_2 , and π_3 . Because there are no court fixed effect,

¹⁹For example, consider a firm in the northeast of the Cahors jurisdiction. This firm will be closer to the Aurillac commercial court than the one in Cahors to which it is assigned (see Figure 1).

π_1 is probably biased as the instrument includes both court preferences and other local characteristics. π_2 is less likely to be biased, as it is estimated based on firms whose conversion depends on the yearly court preference for conversion but much less on other local characteristics. We thus expect π_1 to be larger than π_2 . In addition, if the unobservable local characteristics reflected in the instrument are important, we would expect π_3 to be positive and significant. However, if unobservable local characteristics are not reflected in the instrument, we would expect π_3 to be insignificant, i.e., the chance of being converted would not depend on the conversion rate of the closest court.

The second step is then as follows:

$$Y_{i,j,t'} = \alpha + \beta \cdot \widehat{Conversion}_{i,j,t'} + \gamma_1 X_{i,t} + \mu_{s,t'} + \mu_a + \epsilon_{i,j,t'} \quad (6)$$

4.2 First-stage results

The court conversion rate, as shown in Table 5, is strongly correlated with the probability of receivership conversion. The F-statistic measuring instrument strength is about 26.6, above the threshold of 10 suggested by [Staiger and Stock \(1997\)](#). The coefficient associated with the instrument is statistically significant at the 1% level and robust to the introduction of multiple controls: the point estimate varies from 0.293 without any control variables other than the legal status, court, and industry \times year fixed effects (column (1) of Table 5), to 0.265 when all the controls and fixed effects are included (column (3) of Table 5). The latter is our preferred first-stage estimation. The point estimate of 0.265 implies that a one standard deviation rise (34.9%) in the conversion rate increases the probability of being converted by 9.2%, much higher than the unconditional propensity of 14.2%.

The results of the alternative first stage that differentiates between the closest and assigned courts appear in Table 6. The first-stage estimate of the impact of the court's conversion rate is more or less the same as that in Table 5. Notably, in the firms for which the coefficient is not biased, the coefficient π_2 column (2) is 0.265, the same as the figure reported in Table 5 column (2). Interestingly, as expected, the estimated value of π_1 is slightly higher than π_2 , and π_3 is not significant. We can be confident that the

Table 5: First Stage

Table 5 shows the results from the first-stage regressions. The dependent variable is a dummy for the firm being converted to receivership, and the instrument is the share of cases converted by the commercial court every year, excluding the current case. The model is robust to the introduction of firm control variables (column (2)). All specifications contain 134 commercial court fixed effects, industry \times year-of-judgement fixed effects, and legal status dummies. The standard errors are clustered at the court \times year-of-judgement level.

Dependent variable:	Conversion to receivership	
	(1)	(2)
Share of other cases converted	0.293*** (5.59)	0.265*** (5.16)
Ln(employees)		0.0195*** (4.57)
Age(> 5 years old)		-0.0196* (-1.96)
Zombies		0.0179* (1.85)
Fixed asset/asset		-0.0127 (-0.67)
Financial asset/fixed asset		0.0102 (0.61)
Total debt/asset		0.00317 (0.40)
Supplier debt/debt		0.0185 (0.63)
Legal-status dummies	Yes	Yes
Court fixed effects	Yes	Yes
Industry \times year-of-judgement fixed effects	Yes	Yes
Observations	5,678	5,678
Adjusted R-squared	0.063	0.044
F-statistic for instrument	31.234	26.620

t statistics in parentheses

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

sensitivity of Conversion to the instrument reflects the behavior of the court rather than local economic conditions.

Table 6: First Stage - Assigned Court vs. Closest Court

We exploit the fact that, due to administrative boundaries, a significant share of firms are not assigned to their closest court. CC is a dummy for the firm being assigned to its closest court. We introduce in column (2) of Table 6 firm-level control variables, as in Table 5 column (2). All specifications contain court of appeal fixed effects and industry \times year-of-judgement fixed effects. Standard errors are clustered at the court \times year-of-judgement level.

Dependent variable:	Conversion to receivership	
	(1)	(2)
Share of other cases converted of assigned court \times CC	0.367*** (7.42)	0.362*** (7.46)
Share of other cases converted of assigned court \times (1- CC)	0.263*** (3.48)	0.265*** (3.52)
Share of cases converted of closest court \times (1- CC)	-0.0700 (-0.51)	-0.0719 (-0.53)
All control variables	No	Yes
Court fixed effects*	Yes	Yes
Industry \times year-of-judgement fixed effects	Yes	Yes
Observations	5,678	5,678
Adjusted R-squared	0.072	0.076
F-statistic for instrument	19.45	19.70

t statistics in parentheses.

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

* This specification includes court of appeal fixed effects.

As pointed out by Angrist et al. (1996), the causal effect of the instrument on the probability of being converted to receivership cannot be generalized to the whole population of safeguard filings. The measured conversion impact (β in equation (4)) is only valid for the firms that are sensitive to the court's conversion propensity: the compliers. Following the analysis in Maestas et al. (2013), as our treatment is binary, the proportion of compliers equals the first-stage coefficient times the range of the instrument (in this case, 1). In our data, 26.5% of the firms filing for safeguard may be converted to receivership depending only on their court's propensity to convert. With the average conversion rate being 14.2%, 3.8% of firms filing for safeguard are converted due to this

propensity, and 22.7% are not converted for the same reason. This also implies that, regardless of the court year to which they are assigned, 10.4% of the firms filing for safeguard would always be converted (the always-takers), and 63.1% would never be converted (the never-takers).

The above analysis holds only under the monotonicity, or no-defiers, assumption: all the sensitive firms must be affected identically by a given commercial court (the likelihood of being converted either rises or falls for all firms assigned to the same court). In our setting, sub-samples should have a non-negative first-stage estimate: we carry out this test in Table 7. We split our sample by industry, age, firm size, and financial ratios. The instrument coefficient is positive and significant in all sub-samples and does not vary significantly, supporting the monotonicity assumption.

4.3 Testing the exclusion-restriction condition

As the court has a role not only in conversion, but also in debt renegotiation, we need to check that the exclusion-restriction condition is met. This latter requires that while the court conversion propensity does indeed affect the firm conversion probability, it does not directly affect the probability of reaching an agreement with the firm's creditors. It also requires that no omitted variable impacts both the conversion rate and the probability of a deal with creditors.

In our model, there is a potential risk that the exclusion-restriction condition will not be met: local economic factors may affect both the conversion rate and the probability of reaching a deal with creditors. This will be the case if, due to the local economy being in a bad state, firms in restructuring are more likely to be liquidated, and safeguard firms are more likely to become insolvent and thus be converted to receivership (with the reverse argument in the case of a good local economy).

It is well known that the exclusion-restriction condition cannot be directly checked in the identification data. However, the intuition that the instrument (conversion rate) affects the outcome (restructuring) only through the endogenous regressor (conversion) can be reinforced by looking at the outcome for a sample of firms that are different yet quite similar to those in our main sample.

The different sample we consider is that of firms that filed directly for receivership

Table 7: First-Stage Heterogeneity

Table 7 shows the first-stage regressions presented in column (3) of Table 5 re-run on sub-samples, splitting firms by their financial characteristics. By sub-sample, column (1) shows the number of firms, column (2) the unconditional share of cases converted to receivership, column (3) the coefficient of the instrument *share of other cases converted*, interpreted as being the share of marginal firms, columns (4) and (5) the t-statistic and F-statistic respectively, and column (6) the fraction of firms that would be converted regardless of the court. The “Other” category includes the Manufacturing and Transport industries.

	Number of observations	Percent converted	Coefficient on share converted	T-stat	F-stat	Fraction of always takers
	(1)	(2)	(3)	(4)	(5)	(6)
Full Sample	5,678	0.127	0.222***	5.0	26.6	0.099
Employees						
0-9	2,966	0.098	0.205***	3.6	13.2	0.078
1-50	2,134	0.185	0.303***	4.5	20.4	0.129
>50	578	0.206	0.356**	2.3	20.4	0.133
Industry						
Construction	780	0.171	0.385***	3.4	11.7	0.105
Wholesale and Retail trade	1,504	0.126	0.360***	4.3	18.7	0.080
Services	2,440	0.117	0.242***	3.2	10.5	0.089
Other	954	0.208	0.308***	2.6	6.8	0.144
Age						
5 years old or less	2,223	0.135	0.303***	4.1	16.6	0.094
Over 5 years old	3,455	0.146	0.228***	4.0	16.1	0.113
Zombie firm						
No	2,393	0.114	0.237***	3.7	13.7	0.087
Yes	3,285	0.162	0.245***	3.9	15.5	0.122
Fixed assets/assets						
Below median	2,839	0.163	0.289***	4.8	23.3	0.116
Above median	2,839	0.121	0.179**	2.9	8.6	0.099
Financial assets/ Fixed assets						
Below median	2,839	0.129	0.199**	2.9	8.4	0.103
Above median	2,839	0.155	0.311***	5.0	25.3	0.107
Total Debt/Assets						
Below median	2,839	0.141	0.205***	3.5	12.5	0.112
Above median	2,839	0.143	0.318***	4.7	22.1	0.098
Supplier Debt/debt						
Below median	2,839	0.122	0.259***	3.9	14.8	0.090
Above median	2,839	0.162	0.242***	4.1	16.8	0.123

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

(i.e., without going through the safeguard procedure). This sample consists of 66,849 direct receivership cases. By definition, receivership cases cannot be converted. The instrument would only affect the debt restructuring of these receivership cases if the instrument encompasses confounding factors at the court-year level. To test for that, we regress the outcome in receivership on the conversion rate in safeguard and a set of control variables. If the conversion rate were to explain the outcome in receivership, the exclusion restriction would be violated.

Table 8 shows the results for the sample of all direct receivership filings in column (1) and the sub-sample of voluntary receivership filers in column (2). Voluntary receivership filers are more similar to safeguard filers, as they are likely to be more proactive when faced with financial difficulties than receivership that the firm's creditors trigger. We find no statistically-significant coefficients, such that our instrument is uncorrelated with the probability of debt restructuring in direct receivership filings. In other words, the court conversion rate for safeguard does not explain the restructuring of direct receivership filing cases. We can therefore consider that conversion in safeguard and restructuring in receivership are not explained by the same local factors that the conversion rate would encompass.

As the safeguard and receivership procedures are very similar, we can assume that the process leading to restructuring a firm's debt is similar and that local economic conditions play the same role regardless of the procedure.

5 Main Results and Robustness

5.1 The impact of receivership conversion on debt restructuring

We now focus on how conversion affects the firm's probability of reaching a debt restructuring agreement with its creditors.

The second-stage estimates (equation (4)) appear in column (2) of Table 9 and the OLS estimates in column (1). The OLS estimates suggest that conversion is associated with a fall in the probability of debt restructuring of approximately 56 percentage points; in our IV estimates, conversion results in a 49.5 percentage-point fall in the probability of debt restructuring. As such, accounting for selection produces a smaller estimate of

Table 8: Exclusion Restriction

Table 8 presents a test for the exclusion restriction. We consider the sample of direct receivership-filing firms (66,849 cases). If the restructuring and conversion decision were explained by the same local factors, one would expect that the conversion rate for safeguard would help explain the restructuring of direct receivership cases. The results presented in Table 8 show that this is not the case: the court conversion rate does not explain the restructuring of direct receivership-filing cases. Under the mild assumption that local factors impact restructuring decisions in the same way for safeguard and receivership, this shows that the conversion and restructuring decision for safeguard are not explained by the same local factors. Column (1) shows the regression results for the sample of all receivership, and column (2) those for the sub-sample of voluntary receivership filers. Voluntary receivership filers are the most similar to safeguard filers, as they are likely to be more pro-active in the face of financial difficulties. Standard errors are clustered at the court \times year-of-judgement level.

Dependent variable:	Debt restructuring in receivership	
	All receivership (1)	Voluntary receivership (2)
Share of cases converted	0.00871 (0.87)	0.00497 (0.41)
Ln(#employees)	0.00925*** (4.95)	0.00389* (1.80)
Age(> 5 years old)	0.120*** (30.27)	0.110*** (21.86)
Zombies	-0.0235*** (-5.70)	-0.0359*** (-6.98)
Fixed asset/asset	0.00482 (0.82)	0.00152 (0.63)
Financial asset/fixed asset	-0.0169 (-1.43)	-0.0519*** (-4.78)
Total debt/asset	-0.0127*** (-5.04)	-0.0123*** (-3.74)
Supplier debt/debt	-0.103*** (-9.68)	-0.0932*** (-7.56)
Legal-status dummies	Yes	Yes
Court fixed effects	Yes	Yes
Industry \times year-of-filing fixed effects	Yes	Yes
Observations	66,849	39,565
Adjusted R-squared	0.087	0.119

t statistics in parentheses.

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

the effect of receivership conversion on the probability of debt restructuring. It should be remembered that the IV estimate measures a LATE (i.e., effect on compliers) while OLS estimates an average effect on the total population.

Older and larger firms are more likely to reach an agreement with their creditors. Regarding the balance sheet, a higher fixed asset ratio is associated with a better probability of reaching an agreement, and a higher level of debt is associated with lower chances of restructuring. “Zombie” status produces a lower probability of debt restructuring.

The LATE can also be obtained by dividing the reduced-form estimate by the first-stage coefficient. Column (4) of Table 9 presents the results of the reduced form: a 10 percentage point increase in the conversion rate decreases debt restructuring by 13 percentage points for safeguard-filing firms. If we assume that the only impact of the conversion rate is through conversion, then dividing this coefficient by the first-stage coefficient (0.265) implies that conversion reduces restructuring by 50 percentage points.

Based on these results, for firms that are on the margin of being converted, conversion significantly reduces their chances of reaching a debt restructuring agreement.

5.2 Economic mechanisms

How can we explain that conversion from safeguard to receivership significantly reduces the chances of restructuring even after controlling for selection bias (i.e., the fact that converted firms are amongst the weakest ones)? Our suggested explanation is that conversion triggers a change in behavior in at least some of the firm’s stakeholders, notably its clients. This can push the firm into liquidation.

This hypothesis is at the heart of the theoretical model in [Hortaçsu et al. \(2011\)](#). In their approach, the firm’s bankruptcy reduces consumers’ utility and, thus, the demand for its products. This drop in demand harms the firm’s profitability and exacerbates its financial distress, further reducing demand. This vicious circle can continue until the firm falls into liquidation. There is empirical evidence that bankruptcy filing reduces demand. This is shown implicitly by [Hortaçsu et al. \(2013\)](#): the price of cars manufactured by a bankrupt car company falls in the second-hand market. [Huse and Koptuyug \(2017\)](#) show that Swedish consumers reduced their demand for Saab cars by

Table 9: Main Results

The dependent variable is a dummy for the firm having reached a debt restructuring agreement with its creditors. *Conversion* is a dummy for the firm having been converted from safeguard to receivership. The regression in column (1) is estimated by OLS. The regressions in columns (2) and (3) are estimated by IV-2SLS, with the first stages being respectively in Table 5 column (2), and Table 6 column (2). Column (4) presents the reduced-form estimates. Standard errors are clustered at the court \times year-of-judgement level.

Dependent variable:	Debt restructuring (YES/NO)			
	OLS (1)	IV-2SLS (2)	IV-2SLS ACvsCC (3)	Reduced form (4)
Conversion to receivership	-0.562*** (-29.74)	-0.497*** (-3.22)	-0.518*** (-4.16)	
Share of other cases converted				-0.132*** (-2.97)
Ln(employees)	0.0333*** (6.98)	0.0320*** (5.71)	0.0346*** (6.59)	0.0223*** (4.13)
Age(> 5 years old)	0.0874*** (6.49)	0.0885*** (6.61)	0.0846*** (6.27)	0.0983*** (6.87)
Zombies	-0.0437*** (-3.56)	-0.0449*** (-3.62)	-0.0489*** (-3.94)	-0.0537*** (-3.96)
Fixed asset/asset	0.0588*** (2.61)	0.0597*** (2.71)	0.0536*** (2.42)	-0.0661*** (-2.71)
Financial asset/fixed asset	-0.0634*** (-3.02)	-0.0641*** (-3.08)	-0.0621*** (-2.93)	-0.0692*** (-3.02)
Total debt/asset	-0.0342*** (-2.48)	-0.0344*** (-2.53)	-0.0347*** (-2.58)	-0.0360*** (-2.49)
Supplier debt/debt	-0.0179 (-0.57)	-0.0189 (-0.60)	-0.0234 (-0.73)	-0.0281 (-0.79)
Legal-status dummies	Yes	Yes	Yes	Yes
Court fixed effects*	Yes	Yes	Yes	Yes
Industry \times year-of-judgement fixed effects	Yes	Yes	Yes	Yes
Observations	5,678	5,678	5,678	5,678
Adjusted R-squared	0.240	0.200	0.218	0.0679

t statistics in parentheses.

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

* Column (3) includes court of appeal fixed effects.

50% after it filed for bankruptcy. More recently, [Antill and Hunter \(2021\)](#) show that consumers reduce their willingness to pay for a car rental company after being told it has filed for bankruptcy. This paper also shows that consumers react to the survival probability of firms in Chapter 11, as does [Noh et al. \(2021\)](#).

In the case of safeguard, it is unlikely that conversion adds much information to informed stakeholders²⁰. Conversion, however, can deliver additional information to less informed stakeholders, such as clients. Given the bad track record of receivership procedures, less-informed stakeholders may decide to stop dealing with the firm altogether, regardless of its actual financial situation. Just as in [Hortaçsu et al. \(2011\)](#), this may be a self-fulfilling prophecy that further worsens the firm's situation and chances of reaching an agreement with its creditors. This, in turn, could reduce the enthusiasm of informed stakeholders for reaching a debt restructuring agreement with the firm.

For this mechanism to play a significant role, it has to be the case that consumers are aware of the dire consequences of receivership compared to safeguard. Receivership is well-known in France for its low probability of leading to a restructuring agreement. Consumers do not know for sure the exact survival probabilities of the firm, but they know by experience that they are pretty low. It contrasts with safeguard, which is not known at all by ordinary consumers. Indeed, courts complain that it is not even known by entrepreneurs themselves (see [Zapha and Fouet \(2021\)](#)). It is also common for newspapers reporting on the opening of a new safeguard procedure to run short articles explaining why the safeguard procedure is different and much better than a receivership.²¹

To support the idea that the impact of conversion on the probability of restructuring comes from the behavior (or the change in behavior) of “less informed stakeholders,” we design additional empirical tests. These tests are based on splitting our sample into sub-samples of firms according to some rough measures regarding the type of clients (informed/less informed) they are dealing with. If one considers that final customers

²⁰Creditors are informed stakeholders: they are aware of the firm's financial situation, and the court cannot provide them with much new information. In addition, note that the safeguard procedure suspends debt repayments, and from this point of view, conversion to receivership does not change much for creditors. Thus, conversion is unlikely to reduce creditors' trust in the firm.

²¹See for example, “La sauvegarde, une procédure collective pour prévenir des difficultés”, *Les Echos*, May 24th 2019. This was also the case in the reporting of Pixmania by *Le Monde* in October 2015.

are less aware than other businesses of the meaning of safeguard, firms whose products are mainly aimed at customers (as opposed to other businesses) will suffer more from conversion than B-to-B firms. In the same vein, one can expect that conversion from safeguard to receivership will have a smaller impact on firms with foreign clients, as foreign clients are less likely to be aware of the difference between the two bankruptcy procedures.

The results of these tests are presented in Table A.3 in the Appendix and are consistent with the hypothesis that the impact of the conversion on restructuring chances is driven by the behavior of some of the firm's stakeholders.

5.3 Robustness tests

We carry out various robustness checks to ensure the validity of our results, including a placebo model (see Figure A.1). Here, we address the concern that our results are driven by small courts that deal with only a small number of safeguard cases each year. Time variation in the conversion rates for small courts is higher than that for larger ones. To check that this does not affect our results, we re-run our estimations using only the cases assigned to the largest 75% of courts (regarding safeguard openings over the 2010-2016 period). It leaves us with 4,750 safeguard cases, of which 16.1% are converted into a receivership. The results are shown in columns (5) and (6) of Table 10: the first-stage results are very similar to those above, and the impact of the conversion on the chances of reaching a debt restructuring deal is around -40%.

In columns (3) and (4) of Table 10, we remove 3,977 safeguard cases that were assigned to the 50% of courts with the highest number of judges, such that the conversion ratio is judge-specific rather than due to unobservables at the court-year level. It leaves us with 72 courts with 19 judges or fewer. The results are robust to this change even though the number of observations is reduced from 5,678 to 1,701.

Additionally, and as pointed out by Iverson (2018), the caseload of judges could be an important factor in explaining the outcome of bankruptcy procedures. For instance, safeguard may require more resources, and a case could be more likely to be converted when the judges are busy. We run additional robustness tests with proxy for judges' caseloads and rule out any impact of the busyness of judges on the procedure outcomes.

Table 10: Robustness Test

We present in Table 10 a variant of the main specification in equations (3) and (4). In columns (1) and (2), we remove from our sample the 928 safeguard cases that were assigned to the 25% smallest commercial courts to ensure that our results are not driven by small courts. In columns (3) and (4), we remove 3,977 safeguard cases that were assigned to the 50% of courts with the highest number of judges, such that the conversion ratio could be considered slightly more judge-specific. The first-stage estimates in column (1) and (3) use the instrument *share of other cases converted* as in Table 5 column (2). The second-stage estimates appear in column (2) and (4). The regressions contain the full set of controls and fixed effects used in column (2) of Table 5. Standard errors are clustered at the court \times year-of-judgement level.

Specification:	IV-2SLS excluding the 25% smallest courts		IV-2SLS on the 50% courts with fewer judges	
	1 st stage conversion (1)	2 nd stage debt restructuring (2)	1 st stage conversion (3)	2 nd stage debt restructuring (4)
Share of other cases converted	0.328*** (4.58)		0.157** (2.36)	
Conversion to receivership		-0.395** (-2.19)		-0.791** (-2.19)
All control variables	Yes	Yes	Yes	Yes
All fixed effects	Yes	Yes	Yes	Yes
Observations	4,750	4,750	1,701	1,701
Adjusted R-squared	0.025	0.194	0.024	0.077
F-statistic for instrument	20.96		5.57	

t statistics in parentheses.

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

The results are shown in the Appendix B together with those of the other robustness checks.

5.4 The impact of receivership conversion on survival rates

Survival rates at 2 and 4 years conditional on a debt restructuring agreement

Reaching a debt restructuring agreement with creditors is not the end of the story for the firm and its creditors. As discussed above, the typical restructuring agreement is a mix of debt rescheduling for up to 10 years and debt forgiveness. Unlike in the United States, creditors (who in France are mainly banks and suppliers) rarely exchange debt for control of the firm, and the issuance of new debt does not cover their claims. Existing

creditors thus greatly have a vested interest in the firm's survival after a debt agreement is reached.

We want to see whether receivership conversion affects survival after an agreement with creditors. As mentioned above, conversion is endogenous, which prevents us from estimating a hazard model; we thus rely on 2SLS linear models. We use the same instrument as before.

Table 11 shows the impact of receivership conversion on firm survival after debt restructuring. As we consider survival up to May 2020, we look at the two-year survival rate of the 3,833 firms that obtained a restructuring deal before May 2018 and the four-year survival rate of the 3,053 firms with a restructuring deal before May 2016. We report OLS and IV-2SLS second-stage estimates, which both convey the same message: once the firm reaches an agreement, its survival is not affected by its filing having been converted. This result holds for both the two- and four-year survival rates. It could mean that any negative impact associated with receivership disappears once the firm has been granted a second chance.

Unconditional survival rate at 3 and 5 years after bankruptcy filing

There are, however, two possible interpretations of the absence of an impact of conversion on the survival of firms that have restructured their debt, as described in the previous section. A first interpretation is that once the firm has restructured its debt, the loss of reputation associated with the conversion to receivership disappears, and the converted firms do just as well as the firms that remained in safeguard. The second interpretation is that some of the firms that remained in safeguard and obtained a debt restructuring agreement were on the margin of being converted. Even though they managed to restructure their debt, these firms did not survive for long afterward. This amounts to saying that a part of the non-converted firms do as badly as the converted ones. One way to test this is to look at whether the survival rate of firms entering safeguard – whether or not they restructure their debt – depends on conversion. Suppose the unconditional survival rate does not depend on conversion. In that case, the conclusion will be that non-converted firms at the margin of being converted will do just as badly as those that have been converted. Conversion to receivership would ultimately only

Table 11: Impact of Conversion on Survival

The dependent variables are survival at the two- and four-year horizons after debt-restructuring, and at the three- and five-year horizons after bankruptcy filing. We display the coefficient of *Conversion*. The regressions in column (2) are estimated by OLS; the regressions in column (3) are the second-stage results of the IV-2SLS that uses the court's annual *share of other cases converted* as the instrument (see the first stage in Table 5, column (3)). All of the regressions contain the full set of controls and fixed effects used in column (2) of Table 5. Standard errors are clustered at the court \times year-of-judgement level.

	Number of observations (1)	OLS (2)	IV-2SLS (3)
Survival rate after restructuring			
2 years after restructuring	3,833	0.041 (1.15)	-0.184 (0.49)
4 years after restructuring	3,053	-0.087* (-1.80)	-0.136 (-0.29)
Unconditional survival rate			
3 years after filing	5,678	-0.471*** (-26.27)	-0.478*** (-3.00)
5 years after filing	4,452	-0.396*** (-20.85)	-0.434** (-2.49)

t statistics in parentheses.

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

be a short-term event without a long-term impact on the firm. If, on the contrary, the unconditional survival rate depends on conversion, the effect of conversion is potentially significant, not only for restructuring but also for the longer-term survival of the firm.

We thus test whether conversion to receivership affects the survival of firms, regardless of whether they reach an agreement with their creditors. As previously mentioned, conversion is endogenous, and we use the same instrument as before. The results in Table 11 show that the unconditional survival at three and five years after a firm files for bankruptcy significantly depends on whether it has been converted. These results support our hypothesis that the behavior of stakeholders is a determining factor in the survival of the firms over a short and long-term horizon.

5.5 Economic importance and external validity

We can convert the reduction of 50 percentage points in the probability of reaching a debt agreement after conversion (see Table 10 column (4)) into an indirect cost of receivership. According to Blazy et al. (2018), recovery rates in France in continuation are around 73% for unsecured creditors and 75% for secured creditors, with the analogous figures in liquidation being 5% and 35%. In our sample, the average firm entering safeguard has a debt-to-asset ratio of 80% (see Table 3), 75% of which is secured.²² These numbers lead to indirect costs of 20% of the firm's book assets if shareholders lose everything, in both continuation and liquidation. This estimate is towards the top end of those in the literature, which suggests average indirect bankruptcy costs of between 10% and 20% of firm value.²³

There are around 1,000 safeguard filings per year, some 14% of which are converted to receivership. According to the first-stage results, 26% of safeguard filings are at the margin of being converted. It means that, on average, 36.4 firms per year ($1,000 \times 26\% \times 14\%$) are converted to receivership only because of the court's preferences that year. For these 36 firms, reaching a debt restructuring deal with their creditors will be much more challenging than if all else being equal, they had not been converted to receivership by their court. The aggregate economic impact of these findings could be

²²We hypothesize that suppliers are unsecured creditors.

²³See for example Hotchkiss et al. (2008) and Bris et al. (2006).

small if the effect we measure concerns only the compliers, i.e., the marginal safeguard filers. However, if these indirect bankruptcy costs apply to the 15,000 firms that file for receivership each year, the overall economic impact could be considerable.

6 Conclusion

Using a novel and almost exhaustive data set of court-supervised bankruptcy procedures in France, this paper analyzes the effect of being converted from a preventive procedure (safeguard) into the regular bankruptcy procedure (receivership). Both bankruptcy procedures are public and court-supervised. We use heterogeneity in commercial court conversion rates as an instrument for conversion and show that conversion reduces the probability of reaching a deal with creditors by around 50 percentage points for firms at the margin of being converted. It corresponds to indirect bankruptcy costs of up to 20% of book assets. As there is little difference in the legal provisions for the two procedures, we attribute this to indirect costs associated with the bad track record of receivership. The track record of receivership is so bad that the stakeholders of firms converted to receivership change their behavior toward the firm. This behavior, based on a prophecy of failure, is self-fulfilling.

Our identification strategy is based on heterogeneity in commercial court conversion rates. We have yet to consider whether this heterogeneity is good or bad for the efficiency of bankruptcy procedures. Due to the likely role of lay judges in producing this heterogeneity, the French government has taken several steps to make commercial court decisions more predictable and homogeneous.²⁴ We can, however, speculate that the mere possibility of the safeguard procedure being (wrongly or not) converted to a receivership may deter firms from filing for safeguard in the first place, and delay restructuring. On the contrary, the risk of conversion may provide an incentive for firms to seek out-of-court debt restructuring. These private restructurings may produce even better results, as self-fulfilling expectations are less likely to play a role.

²⁴(i) A law passed in 2016 specifies the number of days of training that newly-elected judges have to undertake at the National School for the Judiciary, as well as the number of continuous training days for judges in their following mandates; (ii) As of July 2014, the judge him- or herself cannot carry out a conversion of a safeguard case into receivership; and (iii) Since early 2016, large bankruptcy cases have been assigned to one of the eighteen “Specialized commercial courts”. We checked that this 2016 provision in the law does not affect the commercial courts assigned in our sample.

Overall, a bankruptcy system's efficiency depends on its capacity to quickly identify firms facing financial difficulties and identify appropriate solutions. Non-viable firms should be liquidated to allow an efficient reallocation of resources toward more productive firms. In contrast, viable firms should be able to initiate a restructuring process without suffering a loss of value. To this end, the safeguard procedure was introduced into French commercial law to offer a distinct court-supervised debt restructuring procedure to fragile, not-yet insolvent firms. Our results support this policy choice rather than the simple extension of receivership to these firms. The safeguard procedure allows stakeholders to differentiate the most viable firms from the average firm filing for bankruptcy, thus increasing the chances of keeping the firm as a going concern. This benefits both secured and unsecured creditors. Given the dire consequences of safeguard conversion to receivership, one may wonder why a court would take such a decision. It should be remembered that the court has to enforce the law: make sure that already insolvent companies do not benefit from safeguard, where debtors retain the decision-making power in the company. If the court does not play its role of gatekeeper, there are two risks: (i) firms in a relatively good financial situation will file for bankruptcy in the expectation of getting a better deal from their creditors, and (ii) firms in a very bad financial situation will try to benefit from the less damaging safeguard procedure compared to receivership.

The 2019 EU directive proposed the introduction of a preventive restructuring procedure in all Member States that only non-insolvent firms could access. Adding a new preventive procedure will avoid them being contaminated by the regular debt restructuring procedure, notably in countries where its track record is poor. For example, [García-Posada Gómez and Vegas Sánchez \(2018\)](#) report that only around 7% of firms manage to restructure their debt in Spain, and [Franks and Loranth \(2013\)](#) mention that it is about 20% in Hungary. Other countries, therefore, should consider drawing on the French experience.

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A Appendix A

A.1 Validity of the instrument

The assignment to courts is not random but depends on the firm's location. We test in Table A.1 whether the share of safeguard filing depends on the court's track record of conversion rates. The dependent variable *Share of safeguard filings* is the ratio of safeguard filings to all safeguard and receivership filings at time t . The regressions are estimated at the court level. Column (1) includes the years $t = 2010$ to 2018, and column (2) $t = 2011$ to 2018.

Table A.1: The share of safeguard filings does not depend on the court's past and present conversion rates

Dependent variable:	Share of safeguard filings in year t	
	(1)	(2)
Share of cases converted in year t	0.0103 (1.55)	
in year $t - 1$		-0.0117 (-1.60)
<u>Annual local-level control variables</u>		
Share of direct liquidations	0.0846*** (2.85)	0.0921*** (2.74)
Unemployment rate	0.0308 (0.13)	-0.0830 (-0.32)
Ln(#bankruptcy filings)	-0.0268* (-1.95)	-0.00968 (-0.64)
Court fixed effects	Yes	Yes
Year fixed effects	Yes	Yes
Observations (court \times year)	1,042	895
Adjusted R-squared	0.086	0.077

t statistics in parentheses.

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

We test in Table A.2 whether the probability that a firm will enter safeguard depends on its court's track record of conversion rates. The dependent variable

Filing for safeguard is 1 if the firm files for safeguard at t and 0 if it files for receivership. The regressions are estimated at the firm level. Standard errors are clustered at the court \times year-of-judgment level. Column (1) includes the years $t = 2010$ to 2018, and column (2) $t = 2011$ to 2018.

Table A.2: The probability of entering safeguard does not depend on the court's past and present conversion rates

Dependent variable:	Filing for safeguard in year t	
	(1)	(2)
Share of cases converted		
in year t	-0.00223 (-0.44)	
in year $t - 1$		-0.00508 (-0.94)
Ln(#employees)	0.0144*** (11.97)	0.0147*** (11.59)
Age(> 5 years old)	0.0266*** (12.44)	0.0273*** (12.26)
Zombies	-0.0182*** (-8.15)	-0.0199*** (-8.54)
Fixed asset/asset	0.00142 (0.92)	0.00140 (0.92)
Financial asset/fixed asset	0.0207 (1.26)	0.0206 (1.24)
Total debt/asset	-0.0206*** (-15.60)	-0.0214*** (-15.35)
Supplier debt/debt	-0.0480*** (-7.93)	-0.0494** (-7.70)
Legal-status dummies	Yes	Yes
Court fixed effects	Yes	Yes
Industry \times year-of-judgement fixed effects	Yes	Yes
Observations (Firms)	73,604	69,160
Adjusted R-squared	0.064	0.063

t statistics in parentheses.

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

A.2 Economic mechanisms

We use the 2-digit SIV codes to separate B-to-B firms from B-to-C ones. Using national accounts, we calculate the household expenditure ratio (investment and final consumption) to that of businesses (intermediate consumption, investment) for each product. According to this methodology, 46% of the firms in our sample are in sectors that produce mainly for households, and 54% are in sectors producing mainly for other businesses.

We use the same method to identify firms with foreign clients, relying on national accounts. With this methodology, 33% of the firms in our sample are in sectors with a high level of exports, and 77% in sectors producing mainly for domestic clients. The sample size of firms in exporting sectors is small, which might reduce our ability to estimate the impact of conversion on debt restructuring. We thus implement a second approach that splits the final sample into two sub-samples, according to the ratio of foreign sales to domestic sales in the firm's income statement, compared to its median value over the sample. By construction, the sizes of the two sub-samples are the same.

The results are shown in Table [A.3](#). We would expect firms in B-to-B sectors to be less impacted by conversion, and the corresponding IV estimate should be smaller for these firms than those in B-to-C sectors. This is indeed the case, with an IV estimate at -0.350 for B-to-B and -0.661 for B-to-C. Also, the B-to-B coefficient is significant at the 10% level only. We also expect firms in exporting sectors to be less impacted by conversion, as foreign clients are probably less aware of the subtleties of French bankruptcy laws. The IV estimate is smaller and not significantly different from 0 for these firms, while it is significant for non-exporting sectors. However, one cannot rule out that the fact that the IV estimates are not significant stems from the small sample size of firms in the non-exporting sectors. When the sample of firms is split according to the median of firms' ratio of domestic sales to foreign sales, the IV coefficients are significant in the two sub-samples, but they are stronger and more significant for less export-reliant firms.

Table A.3: Impact of Conversion on Debt Restructuring on Different Subsamples

Table A.3 presents the OLS and IV estimates of *Conversion* on debt restructuring for different sub-samples. The regressions contain the full set of controls and fixed effects used in column (2) of Table 5. Standard errors are clustered at the court \times year-of-judgement level.

Sub-sample	Number of observations (1)	OLS (2)	IV-2SLS (3)
Sub-samples based on SIV-2 digit			
BtoB	3,067	-0.608*** (-27.67)	-0.350* (-1.87)
BtoC	2,611	-0.492*** (-15.02)	-0.661** (-2.38)
Foreign			
	1,923	-0.568*** (-17.29)	-0.233 (-0.80)
Domestic			
	3,755	-0.561*** (-24.98)	-0.656*** (-3.90)
Sub-samples based on firm data			
Foreign			
	2,801	-0.579*** (-23.83)	-0.490* (-2.51)
Domestic			
	2,877	-0.541*** (-19.37)	-0.548*** (-2.62)

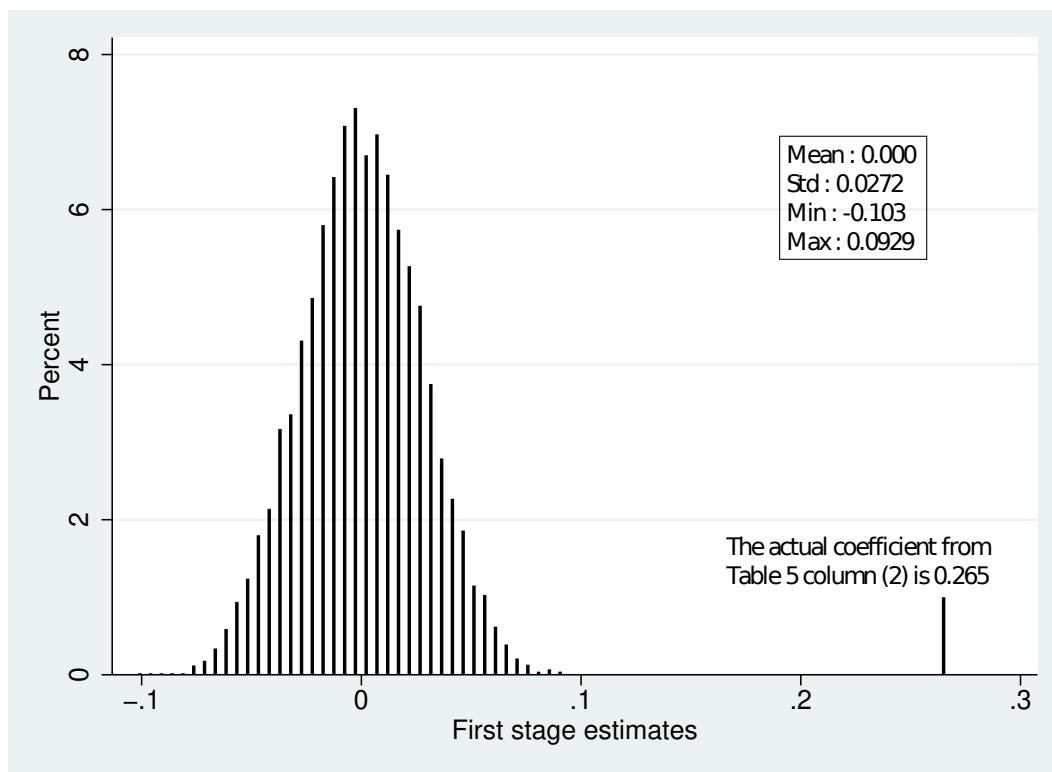
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

A.3 Placebo model

We next carry out a placebo test to rule out the possibility that our results here only come about by chance. We substitute our instrument with a placebo instrument that randomly assigns the values of *share of other cases converted* within the sample. We replicate our preferred specification (column (2) of Table 5) with this randomly assigned instrument 10,000 times. Similar results in these regressions would call into question the validity of our identification strategy. Figure A.1 plots the distribution of the 10,000 placebo regression coefficients π , and the main statistics of the resulting distribution. On average, the coefficient on the randomly-assigned instrument is zero. The actual coefficient of *share of other cases converted* (0.265) is about ten standard deviations (0.0272) above the mean (0.000), and far above the distribution's maximum estimate (0.0929). While these results do not prove that our instrument is valid, they do alleviate concerns that our main results only reflect chance.

Figure A.1: Placebo Test

This figure shows the histogram of the coefficient on the share of other cases converted from 10,000 placebo regressions where the instrument *share of other cases converted* was randomly assigned within the sample. The regressions contain the full set of controls and fixed effects from column 2 of Table 5.



B Appendix B

In the following appendix, we provide additional tests to show that results presented in the main paper are solely driven by the instrument (yearly court conversion rate) and not by other economic variables. We collected a number of local variables that might influence the conversion rate or the restructuring. These local factors are:

- The local annual unemployment rate,
- The number of direct liquidations at the court-year level,
- The share of direct liquidations over total bankruptcy filings at the court-year level,
- The average number of employees per establishment at the court-year level,
- The share of manufacturing employment at the court-year level,
- The regional conversion rate (excluding the assigned court),
- The caseload per judge at the court-year level.

Summary statistics for these variables are presented in Table [B.1](#). We use these variables in three different ways:

1. As control variables in the first and second stages (with the instrument being the conversion rate): see Table [B.2](#),
2. As instruments (instead of the conversion rate): see Table [B.3](#),
3. As potential explanatory variables for the conversion rate at the court-year level: see Table [B.4](#).

Tables [B.2](#), [B.3](#) and [B.4](#) show that in these estimations, these local factors have no significant role. This can be surprising. It should be remembered however that there are court (and year) fixed effects in all of our regressions. This means that all permanent local factors are already absorbed by the court fixed effects.

Table B.2 presents the main specification as described in the paper with local variables as additional control variables. This is done at the firm-level (5,678 observations).

Top panel of Table B.2 shows the results of the first-stage equation:

$$Conversion_{i,j,t'} = \rho + \pi \cdot \phi_{i,j,t'} + \gamma_1 X_{i,t} + \gamma_2 LocalVar + \mu_{s,t'} + \mu_j + \epsilon_{i,j,t'}$$

with $Conversion_{i,j,t'}$ equals to 1 if the safeguard case i has been converted to receivership at t' in court j . $\phi_{i,j,t'}$ is the instrument, the share of cases converted, excluding the current case. $X_{i,t}$ is a set of firm-level characteristics measured the year before the filing, and $LocalVar$ are the local variables listed above. The specification contains industry \times year fixed effects $\mu_{s,t'}$ and court fixed effects μ_j . Standard errors are clustered at the court \times year-of-judgment level.

Bottom panel of Table B.2 presents second-stage estimates, following the equation:

$$Y_{i,j,t'} = \alpha + \beta \cdot \widehat{Conversion}_{i,j,t'} + \gamma_1 X_{i,t} + \gamma_2 LocalVar + \mu_{s,t'} + \mu_j + \epsilon_{i,j,t'}$$

with $Y_{i,j,t'}$ the probability of debt restructuring, and $\widehat{Conversion}_{i,j,t'}$ the values predicted from the first-stage regression. Standard errors are clustered at the court \times year-of-judgment level.

We are interested in γ_2 , the impact of the local variables on our dependent variables. The results in Table B.2 show that none of these variables help explain the conversion nor the debt restructuring. Moreover, the coefficient of the instrument is not affected. While these additional specifications are important for robustness, one has to be aware that adding these local factors could very well bring back some endogeneity in our specification, notably if the local variables are correlated with the firms' performance (which is arguably likely to be the case).

Table B.3 presents the results of our main specification with local variables as new instruments (and without the conversion rate). This is done at the firm-level (5,678 observations).

Top panel of Table B.3 presents the results of the following first-stage equation:

$$Conversion_{i,j,t'} = \rho + \pi \cdot LocalVar + \gamma_1 X_{i,t} + \mu_{s,t'} + \mu_j + \epsilon_{i,j,t'}$$

And the bottom panel of Table B.3 presents the estimates of the second-stage equation:

$$Y_{i,j,t'} = \alpha + \beta \cdot \widehat{Conversion}_{i,j,t'} + \gamma_1 X_{i,t} + \mu_{s,t'} + \mu_j + \epsilon_{i,j,t'}$$

Standard errors are clustered at the court \times year-of-judgment level. We are interested in π and the validity of the local variables as potential instruments to explain the conversion of safeguard cases into receivership. Table B.3 shows that none of the estimates are significantly different from 0, and the F-stat of the first-stage is well below the threshold of 10. It suggests that the local variables could not be plausible instruments. The second-stage estimates are not any more significant.

Tables B.4 presents the results of the following equation :

$$\phi_{j,t'} = \rho + \beta \cdot LocalVar + \mu_{t'} + \mu_j + \epsilon_{j,t'}$$

where $\phi_{i,j,t'}$ is our instrument, the share of cases converted. Estimations are made at the court-year level (1,132 observations), and standard errors are clustered at the court \times year-of-judgment level. We are interested in β , the correlation between our instrument and the local variables to explore if there is court-year level exogenous variations that could affect the conversion rate. The results Table B.4 show that this is not the case: none of the local variables help explain the court conversion rate.

Tables B.2 to B.4 use information retrieved from URSSAF and its open data website Acoess²⁵. Acoess publishes on-line the annual number of employees and establishments in the private sector (level 732 aggregation) in each city of France between 2010 and 2018. Thanks to this data, we created at the court-year level the average number of employees per establishment, and the share of employment in the manufacturing sector.

We also use information on unemployment retrieves from INSEE²⁶. The French unemployment rates are obtained at the *département* level between 2010 and 2018.

We obtain information about the number of judges and chambers per commercial

²⁵[https://urldefense.com/v3/__https://www.open.urssaf.fr__;!!PKypRkOJOBIS2E0s1VA7CdIcKyQ5Nhnf1cILJkPul1R_EP59tymI5HAQIvHPvFkZLRFud-Fmc0-zqnnwv5nTfc1vsWdGpbCSkqw1gmXU8VZguo\\$](https://urldefense.com/v3/__https://www.open.urssaf.fr__;!!PKypRkOJOBIS2E0s1VA7CdIcKyQ5Nhnf1cILJkPul1R_EP59tymI5HAQIvHPvFkZLRFud-Fmc0-zqnnwv5nTfc1vsWdGpbCSkqw1gmXU8VZguo$)

²⁶[https://urldefense.com/v3/__https://www.insee.fr__;!!PKypRkOJOBIS2E0s1VA7CdIcKyQ5Nhnf1cILJkPul1R_EP59tymI5HAQIvHPvFkZLRFud-Fmc0-zqnnwv5nTfc1vsWdGpbCSkqw1gmXcted3kK\\$](https://urldefense.com/v3/__https://www.insee.fr__;!!PKypRkOJOBIS2E0s1VA7CdIcKyQ5Nhnf1cILJkPul1R_EP59tymI5HAQIvHPvFkZLRFud-Fmc0-zqnnwv5nTfc1vsWdGpbCSkqw1gmXcted3kK$)

court in the decrees fixing the number of judges²⁷. The decision to modify the number of judges or the number of chambers can be taken at any time of the year, each year, but come into force the January, 1st of the following year. Election of judges takes place in November (recall that a judge is elected for 4 years and can serve up to 14 years).

Table B.1: Summary statistics of local variables

	Number of observations (1)	Mean (2)	Median (3)	Standard deviation (4)	Min. (5)	Max. (6)
Unemployment rate	1,206	0.0956	0.0938	0.0181	0.0483	0.155
# of direct liquidations	1,206	226.63	143	287.67	2	3,213
Share of direct liquidations	1,206	0.662	0.665	0.0917	0.0244	0.940
Average # of employees per establishment	1,206	9.046	9.016	2.054	4.913	18.83
Share of manufacturing employment	1,206	0.184	0.184	0.0736	0.0301	0.404
Regional share of cases converted (excluding assigned court)	1,206	0.162	0.147	0.108	0	0.767
Average # of safeguard cases per judges	1,206	0.343	0.267	0.311	0	2.678
Average # of receivership cases per judges	1,206	4.013	3.822	1.538	0.889	12.552
Average # of liquidation cases per judges	1,206	8.157	7.544	3.459	0.133	29.151

Note: Table B.1 presents the summary statistics of local variables for the 134 French commercial courts between 2010 and 2018. The share of direct liquidations is the number of direct liquidations over the number of direct liquidations and direct receiverships. The average number of employee per establishment is total number of employees over the total number of establishment. The share of manufacturing employment is total number of employees in the manufacturing sector over the total number of employees. The average safeguard (receivership, liquidation) cases per judge is the number of safeguard (receivership, liquidation) over the total number of judges.

²⁷See the governmental website that publish French legislative and regulatory texts:
[https://urldefense.com/v3/__https://www.legifrance.gouv.fr__;!!PKypRk0JOBIS2E0s1VA7CdIcKyQ5NhnflcILJkPul1R_EP59tymI5HAQIvHPvFkZLRFud-Fmc0-zqnnwv5nTfc1vsWdGpbCSkqw1gmxfUw-bnJ\\$](https://urldefense.com/v3/__https://www.legifrance.gouv.fr__;!!PKypRk0JOBIS2E0s1VA7CdIcKyQ5NhnflcILJkPul1R_EP59tymI5HAQIvHPvFkZLRFud-Fmc0-zqnnwv5nTfc1vsWdGpbCSkqw1gmxfUw-bnJ$)

Table B.2: Controlling for local factors – as control variables

Specification:	IV-2SLS 1 st stage									
Dependent variable:	Conversion									
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Share of other cases converted	0.264*** (5.28)	0.263*** (5.07)	0.264*** (5.11)	0.265*** (5.16)	0.264*** (5.16)	0.265*** (5.16)	0.265*** (5.14)	0.265*** (5.15)	0.264*** (5.09)	0.263*** (5.05)
Unemployment rate	-2.055 (-0.69)									
Log(# direct liquidations)		0.0447 (1.55)								
Share of direct liquidations			0.135 (1.30)							
Average # of employees per establishment				-0.0289 (-0.80)						
Share of manufacturing employment					-1.160 (-1.12)					
Regional share of cases converted (excluding assigned court)						0.0638 (1.28)				
Average # of safeguard cases per judge							-0.0124 (-0.63)			-0.0125 (-0.62)
Average # of receivership cases per judge								-0.00201 (-0.32)		-0.00370 (-0.58)
Average # of liquidation cases per judge									0.00368 (1.08)	0.00415 (1.23)
All control variables	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
All fixed effects	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Observations	5,678	5,678	5,678	5,678	5,678	5,678	5,678	5,678	5,678	5,678
Adjusted R-squared	0.044	0.044	0.043	0.044	0.044	0.044	0.043	0.044	0.044	0.044
F-statistic for instrument	27.83	25.74	26.08	26.59	26.60	26.63	26.57	26.43	25.87	25.53
Specification :	IV-2SLS 2 nd stage									
Dependent variable:	Debt Restructuring									
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Conversion to receivership	-0.503*** (-3.34)	-0.500*** (-3.20)	-0.497*** (-3.19)	-0.493*** (-3.19)	-0.497*** (-3.22)	-0.496*** (-3.19)	-0.497*** (-3.22)	-0.497*** (-3.22)	-0.492*** (-3.17)	-0.491*** (-3.16)
Unemployment rate	-2.245 (-0.06)									
Log(# direct liquidations)		0.0131 (0.37)								
Share of direct liquidations			-0.0166 (-0.12)							
Average # of employees per establishment				0.465 (1.19)						
Share of manufacturing employment					0.0956 (0.07)					
Regional share of cases converted (excluding assigned court)						-0.104 (-1.51)				
Average # of safeguard cases per judge							0.00210 (0.07)			0.00218 (0.07)
Average # of receivership cases per judge								0.00135 (0.17)		0.00280 (0.34)
Average # of liquidation cases per judge									-0.00277 (-0.81)	-0.00309 (-0.88)
All control variables	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
All fixed effects	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Observations	5,678	5,678	5,678	5,678	5,678	5,678	5,678	5,678	5,678	5,678
Adjusted R-squared	0.200	0.200	0.200	0.200	0.200	0.200	0.200	0.200	0.199	0.199

t statistics in parentheses.

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

Table B.3: Controlling for local factors – as new instruments

Specification:	IV-2SLS 1 st stage									
Dependent variable:	Conversion									
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Unemployment rate	-2.964									
	(-0.67)									
Log(# direct liquidations)		0.0601*								
		(1.73)								
Share of direct liquidations			0.180							
			(1.43)							
Average # of employees per establishment				-0.0389						
				(-0.89)						
Share of manufacturing employment					-1.607					
					(-1.24)					
Regional share of cases converted (excluding assigned court)						0.0671				
						(1.08)				
Average # of safeguard cases per judge							-0.0136			-0.0135
							(-0.57)			(-0.56)
Average # of receivership cases per judge								-0.00281		-0.00516
								(-0.37)		(-0.66)
Average # of liquidation cases per judge									0.00494	0.00557
									(1.20)	(1.36)
All control variables	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
All fixed effects	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Observations	5,678	5,678	5,678	5,678	5,678	5,678	5,678	5,678	5,678	5,678
Adjusted R-squared	0.031	0.031	0.031	0.031	0.031	0.031	0.031	0.031	0.031	0.031
F-statistic for instrument	0.45	3.01	2.05	0.78	1.54	1.18	0.33	0.14	1.44	0.75
Specification :	IV-2SLS 2 nd stage									
Dependent variable:	Debt Restructuring									
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Conversion to receivership	0.255	-0.284	-0.589	-1.687	-0.557	-2.051	-0.652	-0.977	-1.052	-1.003
	(0.24)	(-0.48)	(-0.82)	(-0.95)	(-0.64)	(-1.23)	(-0.31)	(-0.33)	(-1.48)	(-1.59)
All control variables	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
All fixed effects	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Observations	5,678	5,678	5,678	5,678	5,678	5,678	5,678	5,678	5,678	5,678
Adjusted R-squared	-0.183	0.158	0.202	-0.530	0.202	-1.08	0.198	0.102	0.063	0.089

t statistics in parentheses.

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

Table B.4: Controlling for local factors – as explanatory variables of the conversion rate at the court-year level

Specification:	OLS									
Dependent variable:	Share of cases converted									
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Unemployment rate	-3.591									
	(-1.54)									
Log(# direct liquidations)		0.0305								
		(0.87)								
Share of direct liquidations			0.0596							
			0.43							
Average # of employees per establishment				-0.0210						
				(-0.44)						
Share of manufacturing employment					-1.678					
					(-1.36)					
Regional share of cases converted (excluding assigned court)						-0.0155				
						(-0.22)				
Average # of safeguard cases per judge							0.0272			0.0252
							(0.76)			(0.71)
Average # of receivership cases per judge								0.00249		-0.000630
								(0.27)		(-0.06)
Average # of liquidation cases per judge									0.00573	0.00563
									(1.14)	(1.07)
Court fixed effect	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Year fixed effects	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Observations (Court × Year)	1,132	1,132	1,132	1,132	1,132	1,132	1,132	1,132	1,132	1,132
Adjusted R-squared	0.041	0.040	0.043	0.039	0.041	0.039	0.040	0.039	0.040	0.039

t statistics in parentheses.

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

Chapter 2

Corporate Bankruptcy and the (In-)Efficiency of Small Commercial Courts

This chapter is co-authored with Anne Epaulard

Abstract

This paper assesses the efficiency of small commercial courts. We take advantage of a reform that regroups small commercial courts and estimate its impact on firms' bankruptcy outcomes on the quality of rulings in bankruptcy cases. A conceptual framework allows us to derive the impact of the reform on Type 1 error (restructuring a non-viable firm) and Type 2 error (liquidating a viable firm) from estimated coefficients of a set of difference-in-difference equations. We apply this strategy to an (almost) exhaustive sample of 600 000 bankruptcy cases in France that started between 2000 and 2019. We show that the reform unambiguously reduced Type 1 errors for small firms in absorbed court and left unchanged Type 2 errors. We show that the behavior of the absorbing commercial court now applies to firms that belong to the catching areas of absorbed commercial courts.

Acknowledgements specific to Chapter 2

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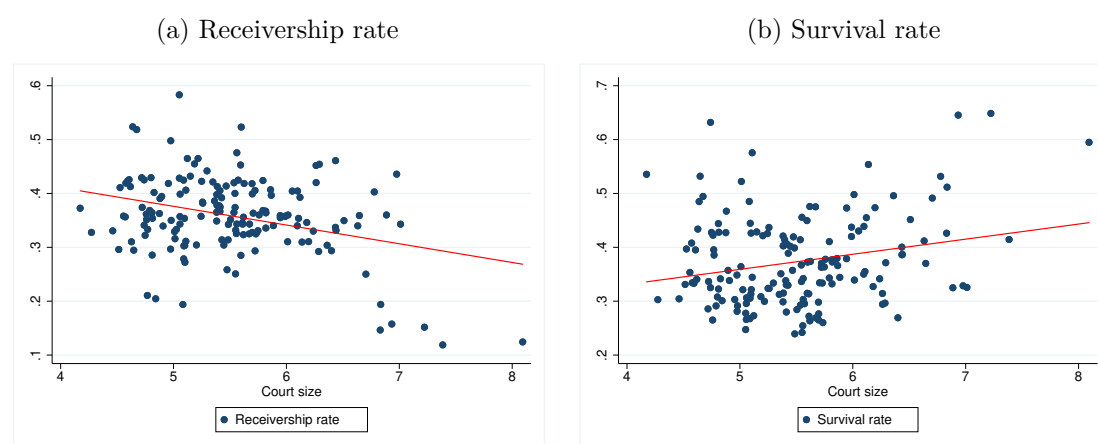
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1 Introduction

“We must close small commercial courts” stated Jean Messinesi, former president of the Commercial Court of Paris, in 2021¹. The arguments against small courts are many, and they are not new.

Raw statistics show that small commercial courts are more likely to give a chance to firms filing for bankruptcy, which are then less likely to survive: over 2008–2019, the share of firms that are allowed to enter a discussion with their creditors decreases with the share of the commercial court (Figure 1, panel (a)) and there is positive correlation between the size of the court and the 7-year survival rate of these firms (Figure 1, panel (b)). These correlations are significant and of the same sign when controlling for firm characteristics (size, industry, and time) and annual local unemployment rate (see Table 1). These correlations are consistent with the view that small courts have a continuation bias or are not as good as large ones in liquidating non-viable firms early in the process.

Figure 1: Receivership and survival rates in French commercial courts (2008-2019)



Note: Panel (a) plots the share of firms allowed to enter a discussion with their creditors after filing for bankruptcy against the size of the commercial court. Panel (b) plots the 7-year survival rate of these firms against the size of the commercial court. These are average annual values at the commercial court level from 2008 to 2019.

How can this alleged continuation bias of small commercial courts be explained? Several explanations are usually put forward. Firstly, critics of small courts consider

¹Jean Messinesi “Les intervenants des procédures collectives de faillite ne méritent pas tous les critiques dont ils sont l’objet”, *Le Monde*, May 30, 2020.

Table 1: Controlling for firms' characteristics and economic cycle

	Receivership	Survival after 7 years
	(1)	(2)
Court size	-0.539*** (-102.95)	0.0619*** (6.66)
Year \times Industry FE	✓	✓
Unemployment rate	✓	✓
Firm size	✓	✓
Observations	469,939	102,605
Adj. R ²	0.068	0.048

t statistics in parentheses

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

Note: We consider 470,000 bankruptcy filing cases in Metropolitan France from 2008 to 2019. The first column reports the estimated coefficient in an equation where we explain the probability of being sent to receivership by the size of the court, the size of the firm, a year \times industry fixed effect, and the local unemployment rate. The second column reports the estimated coefficient in an equation where the left-hand variable is the 7-survival of the firm after receivership, with the same independent variables as in column (1).

their judges too close to firm managers (see [AN \(1998\)](#)). Commercial court judges are not professional magistrates but lay judges elected by entrepreneurs and business executives. This may lead to consanguinity, the judges of the courts being too close (and often in the same social network) to the business leaders who elect them. This is reinforced in small courts where elections often appear to be quite close to co-optation². Proximity leads to a lack of anonymity for the managers of distressed firms. This can result in reputational risk, causing delays in bankruptcy filing, negatively impacting the firm's chances of survival ([Epaulard and Zapha \(2022\)](#)). In addition, this proximity increases the likelihood that social pressure will be exerted on the court (as shown by [Bassanini et al. \(2017\)](#) for businesses), encouraging it to favor receivership over liquidation even for non-viable firms. Thus, all in all, it is likely that continuation bias for firms is more considerable in smaller courts because of proximity. This being said proximity between the commercial courts and the firms can also be a valuable source of information, allowing a better knowledge of cases and their economic environment.

²In many cases, there are as many candidates as available positions.

Secondly, it is often considered that small commercial courts lack competence due to too few cases handled annually (AN (1998) and AN (1987)). As a former French Minister of Justice³ put it: “A commercial court can only be as good as the number of cases it has to deal with [...] just as one is a great surgeon when one operates often, one can rarely claim to be a competent magistrate when one rules only very rarely”. According to this view, lay judges in small commercial courts do not see enough cases to develop sufficient skills.⁴ In addition, as shown by Blazy and Esquerré (2021) in the case of the Paris commercial court, the professional experience of the lay judge, as well as his or her academic background and gender, have an impact on the decisions made throughout the bankruptcy proceedings and on the chances of survival of the firm⁵. In the worst-case scenario, this lack of skills in small courts may result in the liquidation of more viable firms.

The inefficiency of small commercial courts has not yet been demonstrated in the literature⁶, in this paper, we seek to shed some light on the subject. We examine the impact of a reform consisting of regrouping small commercial courts on the efficiency of commercial court bankruptcy decisions. We depart from most of the empirical literature on judicial efficiency that proxy judicial efficiency by procedure length.⁷ Our measure of efficiency is based on the quality of commercial court decisions. More precisely, we are interested in the reform’s impact on the chance of a non-viable firm not to be liquidated early in the judicial process (Type 1 error) and of a viable firm to be liquidated (Type 2 error).

We develop a conceptual framework that shows that the qualitative reform’s impact on Type 1 and Type 2 errors can be derived from the estimated coefficients of a set of difference-in-difference equations. The overall impact of merging commercial court on Type 2 error depends on the sign of the reform’s impact on the survival rate of a firm

³AN (1998) page 35

⁴The same problem can be found among the administrators and judicial representatives, but this problem is less prevalent because the court can hire legal professionals outside its jurisdiction.

⁵In the same vein, Chang and Schoar (2013) find that judges with a continuation bias are associated with lower firm survival rates.

⁶In Italy, Peyrache and Zago (2016) question the optimal structure of the justice sector and find that courts that are too big account for up to a quarter of total inefficiency.

⁷In Appendix A, we show that merging small commercial courts with bigger ones has no impact on procedure length.

that starts a bankruptcy procedure. The Type 1 error can be retrieved from the sign of the reform's impact on the survival rate of firms starting a bankruptcy procedure, and on their restructuring rate. The reform's impact on the quality of the sorting of firms at the first step of the procedure can be partially deducted from the sign of the reform's impact on the probability of being sent to receivership.

Our empirical strategy is based on this conceptual framework. We use an (almost) exhaustive sample of the 600,000 bankruptcy cases in France started over 2000 - 2019 and estimate a set of six standard difference-in-difference equations. We show that the reform unambiguously reduced Type 1 errors and did not impact Type 2 errors. These results are consistent with [Esquerré \(2019\)](#), which measures the impact of the same reform on court bankruptcy decisions and bankruptcy outcomes. We find that the reform's impact shows entirely on bankruptcy outcomes of firms whose court was absorbed and have no impact on bankruptcy outcomes for firms in districts with absorbing courts. These results are robust but apply only to small firms. At the time of the reform, there was a fear that absorbing courts would not cope with their growth. We show that this risk did not materialize. In addition, we show that the behavior of absorbing courts influences more the bankruptcy outcomes of firms in the catching area of the court they absorbed than the opposite. Finally, one might worry that the effects we measure are affected by the 2009 financial crisis. It might be the case if the characteristics of firms filing for bankruptcy are affected by the crisis differently across jurisdictions. We show in [Appendix A.3](#) that this is not the case.

Our paper contributes to the literature on the impact of continuation and liquidation bias in bankruptcy on firm survival. In the US, [Morrison \(2007\)](#) show that, contrary to popular belief, Chapter 11's continuation bias (Type 1 error) for small firms is either absent or empirically insignificant, a result confirmed by [Dou et al. \(2021\)](#) for very large firms who also show that excess liquidation (Type 2 error) is also rare for very large firms. The continuation bias is even more likely in France, where the bankruptcy laws are more pro-debtor than in the US. [Blazy et al. \(2011\)](#) show that while commercial courts play a role in employment preservation, how the court handles the procedure has little impact on creditors' recovery rates. In Spain, [Sánchez-Vidal et al. \(2023\)](#) find that insolvency administrators and the courts influence the outcome of the insolvency

process.

Another stream of the literature studies the role of individual judges (rather than courts) on firms. Most of that literature looks at labor courts rather than commercial courts. In France, while [Desrieux and Espinosa \(2019\)](#) finds that the political orientation of judges does not affect the decision rendered by the labor court, [Cahuc et al. \(2021\)](#) shows that labor judges' bias may have a significant effect on the survival of low-performing small firms. [Marinescu \(2011\)](#) in the UK, [Ichino et al. \(2003\)](#) in Italy, and [Berger and Neugart \(2012\)](#) in Germany find that judges are sensitive to economic conditions (such as bankruptcy and unemployment rates). [Malo et al. \(2018\)](#) in Spain, [Freyens and Gong \(2020\)](#) in Australia, and [Abrams et al. \(2022\)](#) in the US emphasize the importance of the peer effect. Notably, [Abrams et al. \(2022\)](#) shows that judges joining a new court tend to adapt to the local norms. While our paper relates to this literature by looking at transmission channels, our approach studies transmission at the court level and not at the individual judge level.

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

2 French Commercial Courts and the 2009 Reform

2.1 The French bankruptcy process

In France, commercial courts deal with corporate bankruptcies and commercial disputes. Each corporate bankruptcy case is assigned to a specific judge⁸ (that we will refer to as “bankruptcy judge”), but decisions are taken by consensus amongst the judges in the chamber.

When a firm is insolvent (i.e., its available liquid assets do not cover its short-

⁸In our database, we do not have judges' identities.

term debts) and without an informal agreement with one or more creditors, it must file for bankruptcy.⁹ A deliberation hearing brings together the bankruptcy chamber judges (at least three) and the firm's management. They arbitrate between liquidation and receivership; the decision is usually made within a week. Judicial liquidation is pronounced when it is deemed that the debtor has no apparent chance of pursuing a viable activity. Conversely, if the firm is considered potentially viable, receivership is chosen in an attempt to reorganize the business and the debt. In this case, an observation period is triggered for six months, renewable twice, up to a maximum of 18 months. The firm is protected from creditors, an administrator is appointed to advise or replace the manager, a judicial receiver defends creditors' rights, and the bankruptcy judge supervises the case.

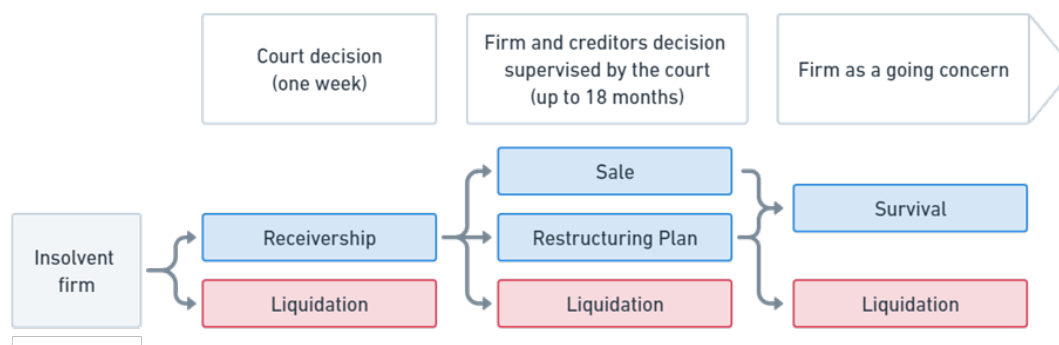
At the end of the observation period, the judicial chamber deliberates on whether to approve or reject a restructuring plan. In case of rejection, the court pronounces the firm's liquidation. This decision is based on reports by the court-appointed administrator and bankruptcy judge and after consulting with the creditors. Creditors are brought together in a creditors' committee for firms exceeding a certain threshold (over 150 employees, with sales over 20 million euros). The final decision rests with the court. In practice, even if a debt-restructuring plan is approved, difficulties may worsen, and the plan may fail. The firm is then liquidated. Figure 2 summarizes the main stages of the bankruptcy process.

One specificity of French commercial courts¹⁰ is that judges are not professionals but lay judges. There are currently over 3,000 lay commercial judges. Their election is a two-stage process: firm managers (legal entities or registered individuals who carry out commercial transactions) elect their representatives, who then elect the judges. Candidates must be registered in the Trade and Companies Register or have run a firm for at least five years. Each judge is initially elected for a two-year term and may then be re-elected three times for a four-year term (for a maximum duration of 14 years). They are unpaid volunteers, sitting only one or two half-days a week and pursuing their usual professional activities the rest of the time. They receive legal training after their

⁹Creditors can also bring the case to the commercial court to trigger the bankruptcy filing.

¹⁰That excludes the *département* of Moselle, Haut-Rhin, and Bas-Rhin that operate differently for historical reasons.

Figure 2: French bankruptcy process



Note: Figure 2 summarises the main stages of the bankruptcy process for insolvent firms in France. In our analysis, we consider the outcome “sale” as if the firm obtains a restructuring plan. It concerns only 3% of firms after receivership and their exclusion does not modify the results and their interpretation.

election and during their term of office. For obvious reasons, they cannot work on cases relating to their firm. The judges elect from among themselves the President of the court and two vice presidents appointed for a non-renewable four-year term.

2.2 The 2009 reform beyond commercial justice

While the French judicial map set up in 1958 had been criticized for its obsolescence, it had not undergone any substantial change before 2009. The 2009 reform is ambitious and concerned the entire French judicial map and not only the organization of the commercial courts. Actually, reforming the commercial court map was not the authorities’ main objective but rather a by-product of the overall reform. Official reports by the French National Assembly ([AN \(2008\)](#)), Senate ([Sénat \(2012\)](#)), and Court of Auditors ([CdC \(2015\)](#)) regarding the effect of the reform of the judicial map barely mention the impact it had on commercial courts and rather insisted on the impact on civil and high courts. The French Court of Auditors only regrets that the reform has insufficiently reorganized the commercial courts.¹¹

The reform aimed to rationalize and adapt the judicial organization to the population dynamic.¹² One of the first objectives of the reform was thus to better distribute justice

¹¹“The Court reiterates the recommendation it made in a referendum of May 13, 2013, to deepen the reform of the commercial court map” – [CdC \(2015\)](#)

¹²For instance, the number of high courts per inhabitant varied from 1 to 19 for territories of

resources over the territory. Most of the reform consisted in choosing the civil and high courts that would be closed.¹³ In most cases, the decision regarding these civil and high courts also applied to the local commercial courts¹⁴. The criteria in choosing which court to close (e.g., distance to a prison, distance to a psychiatric hospital, avoiding the isolation of magistrates, see Cahu (2015)) had nothing to do with commercial or bankruptcy laws. The low weight given to commercial courts in the selection process is linked to their low weight in the French judicial system: commercial courts represent only 15% of all French jurisdictions¹⁵ and about 3% of civil justice's budget¹⁶.

In cases where commercial courts were closed regardless of the decision made regarding the local high or civil court, the reform aimed for at least one commercial court per *département*. By default, the biggest court was set to be the absorbing one. This resulted in closing the smallest commercial court(s) in each *département*, regardless of their performance.¹⁷

In total, the reform closed 55 commercial courts¹⁸ and created 5 others.¹⁹ Discussed since May 2007, the details of the reform were announced in late 2007, and the reform law passed in February 2008. It became effective on January 1, 2009. As soon as the law was adopted, the actual reorganization of courts started. Figures A.1 and A.2 illustrate the suppression and creation of commercial courts. Figure 3 presents a close-up of comparable size. (AN (2008)).

¹³The choices were widely criticised (Cahu (2015)), leading to protests and resignations (CdC (2015)) in some instances. Some high courts closed in 2009 and reopened in 2013. By contrast, the reform of the commercial courts has been rather well-received (CdC (2015))

¹⁴Out of the 23 high courts that were closed, 21 commercial courts in the same town were also closed.

¹⁵There were roughly 1,200 jurisdictions before the reform. As a result of the reform, 29% of the commercial courts (55 out of 186) have closed, compared with 37% of the civil courts (*Tribunaux d'instance*, 178 out of 476), 11% of the high courts (*Tribunaux de Grande Instance*, 23 out of 182) and 23% of the labor courts (*Prudh'ommes*, 62 out of 271).

¹⁶See <https://www.budget.gouv.fr/>.

¹⁷Only five absorbing commercial courts are slightly smaller than those they absorb. Three of them were in the same city as a closing high court or civil court. An individual study of these cases does not reveal clear political stakes in choosing which court to keep.

¹⁸We exclude the particular case of Bernay/Pont-Audemer. Bernay was created, and Pont-Audemer closed without any change in the catching area. We thus retain 54 absorbed courts and 4 creations in our analysis.

¹⁹In some of the *départements*, bankruptcy and commercial disputes were handled by high courts with commercial authority (e.g., in Mende, Lozère). After 2009, these courts were replaced by new commercial courts. Because of the different functioning of high court with commercial authority, we exclude from our analysis commercial courts that have absorbed or replaced high courts. Similarly, in the Grand-Est region and for historical reasons, commercial bankruptcy and disputes are dealt with by high courts. In overseas regions, commercial justice is handled by mixed commercial courts. The 2009 reform has not modified this organization. We also exclude these regions from our analysis.

the *départements* of Maine-et-Loire and Indre-et-Loire to describe the three types of commercial courts: absorbed, absorbing, and control courts. The Section 4 discusses a large set of descriptive statistics of the reform's impact on courts' activity.

Figure 3: Close-up on the 2009 reform of the judicial map



Note: Figure 3, left image represents the absorbing (Angers), absorbed (Saumur) and control (Tour) courts before the reform. Right image represents the new (Angers) and control (Tours) courts after the reform. All firms in the absorbed court's catching area before reform are assigned to the absorbing court after reform.

3 Measuring Commercial Court Efficiency

Most empirical papers aiming at explaining the efficiency of commercial courts and its impact on firms (their size, their access to bank loans) measure efficiency by the speed at which the court discharges cases or some similar indicators such as the clearance rate or backlog rate²⁰. There is indeed some indication that in Italy, faster judicial procedures are associated with better access to finance (Jappelli et al. (2005)) and larger firms (Giacomelli and Menon (2017)). Pezone (2023) exploits the mergers of Italian courts to measure that a reduction in average trial length has a large, positive effect on firm employment. Similarly, Müller (2022) shows that a drop in court congestion increases firms' leverage. However, these efficiency measures might not be helpful in the cases of French bankruptcy procedures as the law strictly sets the time frame, and there is little heterogeneity between commercial courts in this regard (see Section 4). Moreover,

²⁰Backlog rate is defined as the number of pending cases in a court at the beginning of the year over the number of judges working in that court over the year.

even if speed is essential for a well-functioning judicial system, the quality of judicial decisions also matters. Measuring the quality of judicial decisions is difficult. In the case of bankruptcy courts, the quality of their decisions is probably easier to assess than that of other judicial courts. Indeed, few recent papers try to measure the impact of some reforms affecting commercial courts on the quality of their ruling. This is the case of [Iverson \(2018\)](#), which tests the impact of an exogenous drop in caseload in bankruptcy courts on recidivism. [Giné and Love \(2010\)](#) analyzes the impact of bankruptcy reform in Colombia on efficiency by studying its effect not only in reorganization duration, but also on firm survival after reorganization. [Antill \(2022\)](#) tries to measure whether the right firm survives bankruptcy. Our approach follows the same vein, but we propose a more formal framework to measure gains in court quality. This conceptual framework provides a way to interpret our empirical results regarding the impact of judicial reform on court efficiency.

Our starting point is that an efficient commercial court is the one that separates firms that are economically viable from those that are not early in the bankruptcy process, and ensures that the firms that reorganize are those with a good chance of surviving through well-designed restructuring plans.²¹

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

3.1 Conceptual framework

Notations and simple arithmetic

Let us consider that there are two types of firms filing for bankruptcy: viable firms (i.e., the ones with survival chances if restructured) noted with a “*h*,” and non-viable firms

²¹Another approach consists in measuring efficiency by creditor recovery rates, as in [Antill \(2022\)](#). However, we do not have access to creditor recovery rates.

(i.e., the ones with no survival chances even if restructured) noted with a “*l*.” We note x_j the share of viable firms at the onset of the procedure in the catching area of court j (the statistician does not observe x_j).

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

$$R_j = x_j R_j^h + (1 - x_j) R_j^l \quad (1)$$

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

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

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

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

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

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

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

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

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

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

3.2 Empirical framework

In the previous section, we did not need to introduce heterogeneity between firms within a given group when considering two types of firms (viable firms if restructured and non-viable firms even if restructured). When bringing the framework to the data, we need

to consider the fact that viable firms (resp. non-viable) are not identical and control for firm characteristics.

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

1. The firm is sent to receivership (as opposed to being liquidated).
2. The firm restructures and continues as a going concern.
3. The firm survives a given number of years²² after filing for bankruptcy.

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

$$Y_{ijt} = \alpha + \sum_{k=g,d} \beta_k (Reform_k \times Post) + \gamma_1 X_i + \gamma_2 u_j + \theta_j + \theta_{s \times t} + \epsilon_{ijt} \quad (4)$$

where Y_{ijt} is a dummy variable (taking the value of 1 or 0) for each of the three above-mentioned outcomes. The index j refers to the jurisdiction based on the pre-reform division. X_i is a vector of firm observable characteristics, θ_j a pre-reform jurisdiction fixed effect that controls for the non-observable characteristics of the firms within the jurisdiction, and $\theta_{s \times t}$ an industry \times year fixed effect. t refers to the year the firm started the bankruptcy procedure. $Post$ equals 1 after 2008, and $Reform_k$ takes the following values: *Absorbed* if the firm i is located in a jurisdiction whose court is absorbed (indexed by $k = d$), and *Absorbing* if the firm i is in a jurisdiction j whose court absorbed another court (indexed by $k = g$). The control sample is made of firms located in a jurisdiction j whose court is unaffected by the reform. In addition, we include the local annual unemployment rate (u_j) to control for economic conditions at the (pre-reform) jurisdiction level.²³

Having firm characteristics as well as pre-reform court fixed effects, industry \times year fixed effects and the unemployment rate at the pre-reform jurisdiction level on the right-hand side of the equation allows us to take into account the fact that firms are not

²²We consider the survival 7 years after filing for bankruptcy.

²³As pointed out by Iverson (2018), this control is all the more critical if the different jurisdictions face economic conditions that evolve differently over time (Appendix A.3).

identical (within each group of firms) and that bankruptcy outcomes can vary according to industry and year.

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

1. β_R the coefficients of the reform's impact when the left-hand variable is the dummy for the outcome at the first stage of the procedure (1 if the firm is sent to receivership and 0 if it is liquidated right away). It is estimated on the whole sample of firms filing for bankruptcy.
2. β_P the coefficients of the reform's impact when the left-hand variable is a dummy for restructuring (1 if the firm manages to restructure and 0 otherwise). It is estimated on the whole sample of firms filing for bankruptcy.
3. β_P^R the same coefficients as in 2. estimated on the restricted sample of firms sent to receivership (restructuring after receivership).
4. β_S the coefficients of the reform's impact when the left-hand variable is a dummy for the firm's survival (1 if the firm survives a given number of years and 0 otherwise). It is estimated on the whole sample of firms filing for bankruptcy.
5. β_S^R the same coefficients as in 4. estimated on the restricted sample of firms sent to receivership (survival after receivership).
6. β_S^P the same coefficients as in 4. estimated on the restricted sample of firms that restructure (survival after restructuring).

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

We estimate the reform's impact by OLS in a regular difference-in-difference fashion with clustered standard errors at the pre-reform court level.²⁴

²⁴The outcomes of interest are dummy variables with outcomes that are not rare events; we chose to

3.3 From empirical equations to Type 1 and Type 2 errors

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

We make the two following identification assumptions:

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

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

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

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

The reform's impact on Type 1 errors is less straightforward to recover. The estimate for β_P together with that of β_S allows for recovery change in Type 1 error in some specific

use regular OLS and not logit model that is more suitable in case of the model with rare events.

cases. We already show that a positive β_S means that more viable firms restructure after the reform (a lower T_2), then if β_P is negative (fewer firms restructure after the reform), one can conclude that less non-viable firms restructure, meaning a reduction in Type 1 error as well. The same can be conclude if $\beta_S > 0$ and $\beta_P = 0$. A reform that reduces the restructuring of viable firms ($\beta_S < 0$) while increasing the overall restructuring rate ($\beta_P > 0$) is increasing the restructuring of non-viable firms. In this case, both Type 1 and Type 2 errors increase.

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

Table 2: Type 1 and Type 2 errors matrix

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

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

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

1. If the reform reduces the overall probability of a firm to be sent to receivership

($\beta_R < 0$), it does not increase the probability of a non-viable firm to be sent to receivership ($\Delta R_j^l \leq 0$).

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

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

It is immediate to see that under the first hypothesis:

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

And that under the second hypothesis:

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

These two measures are summarized in Table 3.

Table 3: Type 1 and Type 2 errors at the onset of the bankruptcy procedure

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

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

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

For example, the equation of the survival rate after restructuring (β_S^P) helps check the robustness of the result. The reasoning is the following: a reform that reduces Type 1 errors ($\beta_P > 0$) and reduces or leaves unchanged Type 2 errors ($\beta_S \geq 0$) also increases the share of viable firms within the firms that restructure. One would then expect the survival rate after restructuring to increase (i.e., $\beta_S^P > 0$). The same reasoning applies if the reform increases Type 1 errors while increasing or leaving unchanged Type 2 errors. In this case, one would expect $\beta_S^P < 0$. This is shown in the bottom part of Table 4. The same reasoning applies to finding conditions for β_P^R , β_S^R . A formal proof is given in Appendix B. If coefficients derived from conditional equations do not meet the conditions set in Table 4, one could argue that the results derived from outcomes on the whole sample of firms filing for bankruptcy lack robustness.

Table 4: 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	$\beta_P^R < 0$	$\beta_P^R < 0$?			

4 Data and Descriptive Statistics

4.1 Data sources and firms characteristics

Our data include all corporate bankruptcies and their outcome in France from 2000²⁵ to 2019. We use data from FIBEN (*Fichier Bancaire des Entreprises*, the information system of Banque de France). Bankruptcy data come from the commercial court registries. These data provide the date and nature of the judgments at each stage of the bankruptcy procedures. For each case, it is possible to retrieve the date of the bankruptcy filing, the date of restructuring, the date of sale, the duration of the procedure, the firm's survival if it emerges from bankruptcy, and the date of liquidation at any stage of the process. From FIBEN, we also have information on the firm's location (postal code).

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

For almost every firm in our sample, FIBEN provides information on its size and sector of activity.²⁶ The final sample includes 580,227 unique firms whose bankruptcy case was handled by commercial courts between 2000 and 2019. We observe the survival of these firms up to 2022. Our analysis uses firm data as cross-sectional data, with each firm being observed only once when filing for bankruptcy.

Table 5 describes the composition of firms entering bankruptcy. 92,4% of firms are very small, "micro"-enterprises (less than 10 employees and less than €2 million in annual turnover). 36% of the sample firms are in the service industry, 25% in the construction

²⁵We start our sample in 2000 not to be impacted by the 1999 reform that started modifications to the judicial map, although not to the same extent as the 2009 reform.

²⁶Complete balance sheet data are only available for a small proportion of the sample. Financial information on firms that have filed for bankruptcy is scarce, not only because the vast majority of these firms are very small firms, for which data are less accessible than for large firms, but also because these firms, being in difficulty, generally provide less information on their accounts than healthy firms.

and 24% in trade, the remaining in the manufacturing and transport industries.²⁷

Table 5: Firms summary statistics

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

Note: Table 5 presents summary statistics on our cross-sectional firm data. Size and industry are the one the year before the firm filed for bankruptcy. First row and first column report the number of firms for each category. The rest of the Table displays percent calculated on the first row. Micro-enterprises are those that have less than 10 employees and less than €2 million annual turnover. Other industry includes manufacturing and transport industries.

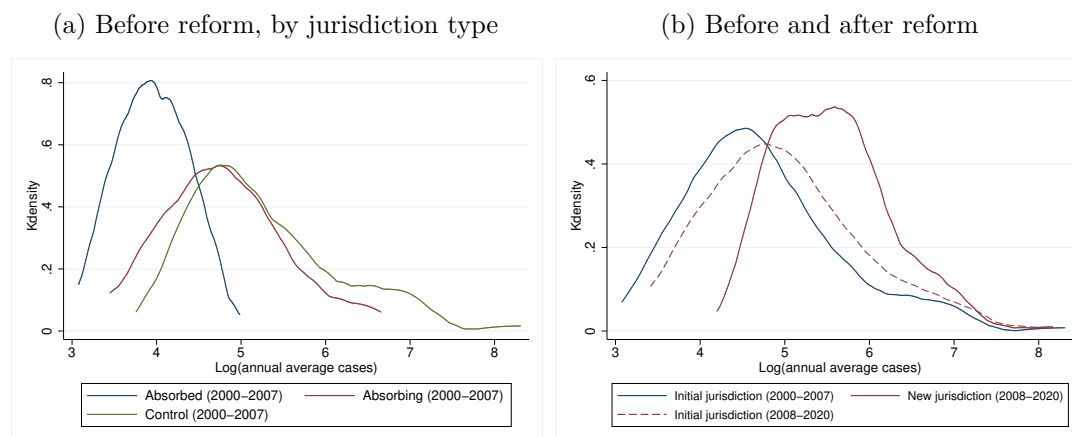
4.2 A snapshot of commercial courts' activity before and after the reform

Absorbed courts are different from other courts. First, they are much smaller, as shown by Figure 4, panel (a). Absorbed courts handled 49 cases over the pre-reform period against nearly 128 in the absorbing courts and 154 in the control courts (Table 6, first panel). The new courts resulting from the reform handle as many cases as the control courts. We illustrate the court size distribution shift before and after reform Figure 4, panel (b). After the reform, courts are, on average, bigger and more concentrated (the red line compared to the blue line). This effect does not come from a cyclical effect, as evidenced by the red dotted line that illustrates the size the former jurisdictions would have had after 2009. Given these figures, the objective of the reform to harmonize the

²⁷We removed agricultural firms from the sample because their bankruptcy process differs from other sectors.

size of the courts across the territory is reached. However, there is still a considerable heterogeneity in court size after the reform.

Figure 4: Court size distribution

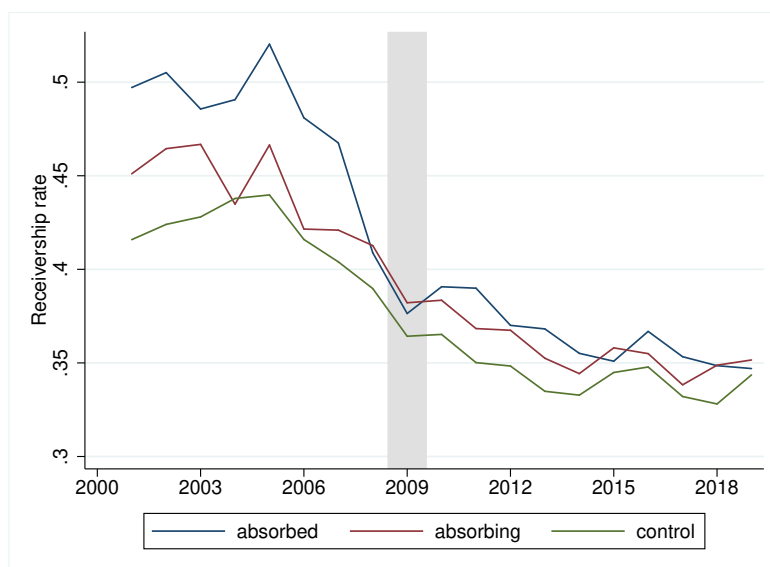


Note: Figure 4 panel (a) reports the distribution of each type of jurisdiction before reform. Figure 4 panel (b) shows the court size distribution before and after reform (resp. blue and red line). Court size is measured as the log of the average number of cases per year. To ensure that the change in distribution is not due to a cyclical effect, the dashed red line represents what the distribution of court size would have been in the absence of a reform (i.e., for the former jurisdictions).

Commercial courts are also heterogeneous regarding bankruptcy outcomes (reported in Table 6). The average absorbed court granted receiverships to 49.5% firms. This compares to 44.9% for absorbing courts and 42.3% for control courts. After the reform, the receivership rate of the average new jurisdiction dropped to 36.4%, on par with that of the average controlling court (34.8%). The higher rate of receivership in the absorbed jurisdictions is also illustrated in Figure 5. This graph reveals that, before the reform, trends over time are parallel across types of jurisdictions; this point is essential for our identification strategy that will rely on the parallel trends hypothesis. There is much less divergence when looking at the other outcomes.

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

Figure 5: Receivership rate per jurisdiction



Note: Figure 5 represents the annual rates of receivership by jurisdiction type (absorbed, absorbing and control), on average.

with the sum of the judges in the former courts.²⁸ Let us take the example of the Vosges department: the former court of Épinal had 15 judges, Mirecourt 8 and Saint-Dies-Dès-Vosges 9, for a total of 32 judges in office before reform. After the reform, the new Épinal court had only 20 judges for the whole department. Only 10% of the new courts retained at least as many judges as the sum of merging courts.

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

²⁸This analysis excludes courts that have absorbed a high court. Because High courts operate differently, counting the number of judges dedicated to corporate bankruptcy is impossible.

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

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

Table 6: Summary statistics per jurisdiction

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

Table 6: Summary statistics per jurisdiction – continued

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

Note: Table 6 presents summary statistics at the court level. All statistics are annual averages per type of jurisdiction. The right-hand side of the table presents the statistics for the pre-reform period, and the left-hand side for post-reform period for which new = absorbed + absorbing.

5 The reform's impact

5.1 Main Results

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

Whatever the outcome, we can see that the bankruptcy of firms in absorbing districts is not impacted by the reform (second line of Table 7, coefficients are not significantly different from zero). On the contrary, as shown by coefficients reported on the first line of Table 7, the bankruptcy outcomes of firms in absorbed districts are affected by the

reform: they are less likely to be sent to receivership (-6.43 percentage points). Their probability of restructuring after filing is also lower. The probability of survival seven years after filing for bankruptcy is unaffected, but the probability of surviving seven after receivership and five years after restructuring are significantly higher. According to the model developed Section 3, results in columns (2) and (4) mean that the overall Type 2 error for firms in absorbed jurisdictions is unchanged (the probability of a viable firm to be liquidated). In contrast, the overall Type 1 error (the probability for a non-viable firm to be restructured) is reduced (see Table 2). These results are comforted by the positive survival rate after restructuring (column (6)). This is indeed the sign that the composition of restructured firms has changed with relatively fewer non-viable firms (this is also what was expected; see upper right part of Table 4).

Table 7: The reform's impact

Bankruptcy outcome	R_{ijt}		P_{ijt}		S_{ijt}	
Coefficient of interest	β_R	β_P	β_P^R	β_S	β_S^R	β_S^P
	(1)	(2)	(3)	(4)	(5)	(6)
Absorbed \times Post	-0.0643*** (-4.51)	-0.0346*** (-5.66)	-0.0156 (-1.22)	-0.00265 (-0.67)	0.0147** (2.08)	0.0322** (1.98)
Absorbing \times Post	-0.0234 (-1.56)	-0.00746 (-1.21)	0.00221 (0.23)	-0.00240 (-0.77)	-0.000710 (-0.11)	0.0223* (1.66)
Unemployment rate	-0.0254*** (-3.99)	-0.0100*** (-4.12)	-0.00366 (-0.85)	-0.00440*** (-2.92)	-0.00229 (-0.56)	-0.000397 (-0.08)
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	0.072	0.035

t statistics in parentheses

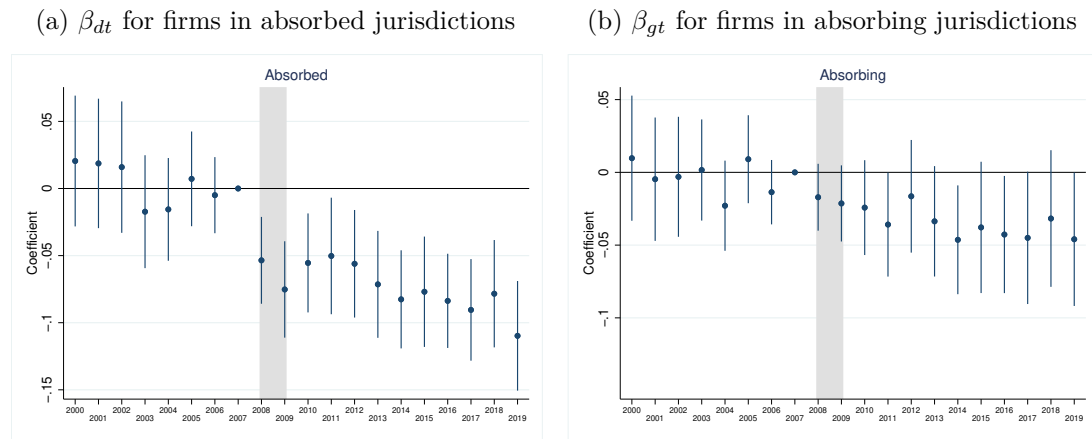
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

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

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

is also what is shown in Figure 6: the effect of the reform is direct and significant at the 1% level. As soon as the reform was passed in 2008 and became effective in 2009, firms' probability of entering receivership decrease by 6 percentage points on average in the absorbed jurisdictions. The effect varies from -5 p.p. in 2009 to more than -10 p.p. in 2019, but remains globally stable over time. This graph also provides evidence towards to validity of our method with respect to the parallel trends hypothesis. Prior to 2008, there is no significant difference in treatment between absorbed and control jurisdictions, nor between absorbing and control jurisdictions. As discussed in the conceptual framework section, we interpret that as the fact that the Type 1 error at the onset of the procedure (T_{1R}) has also been reduced (Table 3). Our experiment does not allow us to conclude regarding the Type 2 error at the onset of the procedure (T_{2R}).

Figure 6: Reform's impact on the probability of receivership



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

$$Y_{ijt} = \alpha + \sum_{k=g, d} \sum_{t \neq 2007} \beta_{kt} \cdot (\mathbb{1}_t \times Reform_k) + \gamma_1 X_i + \gamma_2 u_j + \theta_j + \theta_s \times t + \epsilon_{ijt} \quad (7)$$

where $\mathbb{1}_t$ are year dummies. Figure 6 plots the coefficients β_{kt} on the probability of entering receivership (compared to direct liquidation). Left hand side shows the β_t for firms in absorbed jurisdictions ($k = d$), and right hand side shows the β_t for firms in absorbing jurisdictions ($k = g$). Standards errors are clustered at the pre-reform court level.

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

One concern with the empirical experiment we just conducted is that our control

group includes firms in jurisdictions of various sizes, including very large ones. The absorbed courts are among the smallest, as discussed in Section 2.2. Also, even if, following Iverson (2018), we check in the Appendix A.3 that the 2009 financial crisis did not impact the composition of firms in different jurisdictions, there could be a suspicion that firms in smaller jurisdictions were more impacted by it²⁹. To ensure that our results do not come from a size effect of control courts compared to absorbed ones, we reduce our control group to courts with a size comparable to that of absorbed courts³⁰ and run the same set of equations. Results shown in Table 8 are qualitatively identical to previously reported results. The impact is slightly stronger for survival after receivership and restructuring (columns (5) and (6)) and slightly smaller (but still significant) for receivership and the probability of restructuring after filing (columns (1) and (2)). The check for internal consistency of estimates across equation are also the same.

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

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

In France, most bankruptcy procedures concern small firms. In our sample, over the 2000-2019 period, 92,4% of firms have less than 10 employees and an annual turnover of less than €2 million (“micro-enterprises”). One would expect that the continuation bias is bigger for these firms than for larger ones for which financial stakes are of greater

²⁹Boekwa et al. (forthcoming) show that the most affected commuting zones in terms of private employment by the 2009 financial crises were usually the smallest.

³⁰The largest absorbed courts had up to 15 judges (Annonay, Argentan, Charleville-Mézières, Dole) whereas the smallest of the control courts had only 8 judges before the reform (Foix, Gap). These courts, albeit small, were not concerned by the reform because they were the only commercial court in the *département*. We keep in the control group firms in jurisdictions with only one commercial chamber and 15 judges or less. It corresponds to the maximum number of judges in absorbed courts and the median number of judges in control courts (Table A.1).

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

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

t statistics in parentheses

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

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

consequences, and thus courts (and creditors) decisions more likely to be rational³¹ Consequently, one would expect the reform to have little or no impact on larger firms. In order to check that, we split our sample according to firm size. The first sample comprises micro-enterprises. It has 546,147 bankruptcy cases. The second sample include all other firms, bigger than micro-enterprises. It has 34,067 bankruptcy cases. We then run the same set of estimations.

Results are presented in Table 9, panel A for small enterprises and Table 9, panel B for other firms. The results are clear-cut. The reform does not impact larger firms' bankruptcy outcomes. The impact on small firms is the same as the whole sample. The continuation bias is thus reduced for small firms. It is not reduced for larger ones, maybe because there is no continuation bias to begin with for these firms. In terms of macroeconomic impact, reducing the continuation bias for small firms is a good thing. Even if each firm is small because there are so many of them, reducing the continuation

³¹Bernstein et al. (2019) also make this point, stating that “presumably the stakes are large enough in these cases that judicial preferences are of less consequence.”

bias can have a significant economic impact. Indeed, the 546,147 small firms' bankruptcy cases concentrate almost as many employees (737,000) as larger firms (953,876).

Table 9: The reform's impact – by firms' size

Panel A						
Micro-enterprises						
Bankruptcy outcome	R_{ijt}	P_{ijt}		S_{ijt}		
Coefficient of interest	β_R	β_P	β_P^R	β_S	β_S^R	β_S^P
	(1)	(2)	(3)	(4)	(5)	(6)
Absorbed \times Post	-0.0658*** (-4.57)	-0.0357*** (-5.65)	-0.0183 (-1.36)	-0.00265 (-0.70)	0.0189** (2.58)	0.0376** (2.28)
Absorbing \times Post	-0.0241 (-1.58)	-0.00802 (-1.30)	0.00186 (0.19)	-0.000719 (-0.25)	0.00510 (0.77)	0.0264* (1.90)
Unemployment rate	-0.0252*** (-4.13)	-0.00991*** (-4.20)	-0.00491 (-1.04)	-0.00407*** (-3.17)	-0.00176 (-0.48)	-0.00358 (-0.72)
Year \times Industry FE	✓	✓	✓	✓	✓	✓
Court FE	✓	✓	✓	✓	✓	✓
Observations	546,147	546,147	176,909	423,628	140,469	44,408
Adj. R ²	0.128	0.083	0.074	0.044	0.054	0.036

Panel B						
Larger than micro-enterprises						
Bankruptcy outcome	R_{ijt}	P_{ijt}		S_{ijt}		
Coefficient of interest	β_R	β_P	β_P^R	β_S	β_S^R	β_S^P
	(1)	(2)	(3)	(4)	(5)	(6)
Absorbed \times Post	-0.0183 (-0.79)	0.00191 (0.08)	0.0158 (0.57)	-0.00185 (-0.09)	-0.0164 (-0.69)	-0.0212 (-0.45)
Absorbing \times Post	0.000449 (0.02)	0.00296 (0.21)	0.00403 (0.20)	-0.0260 (-1.59)	-0.0456** (-2.41)	-0.0122 (-0.38)
Unemployment rate	-0.0178** (-2.26)	-0.0000477 (-0.01)	0.0107 (1.59)	0.00228 (0.40)	0.00615 (0.75)	0.0251* (1.84)
Year \times Industry FE	✓	✓	✓	✓	✓	✓
Court FE	✓	✓	✓	✓	✓	✓
Observations	34,067	34,067	22,031	29,816	19,215	6,099
Adj. R ²	0.186	0.111	0.080	0.108	0.081	0.042

t statistics in parentheses

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

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

6 Transmission of Courts' Behavior

What are the channels that lead to the reduction in the overall Type 1 error (T_1 and T_{1R}) for firms in absorbed courts? To answer this question, we develop a simple empirical method to measure how absorbing courts transmit their behavior to the firms in the catching area of the court they absorb. We also leave the possibility of an absorbing court influencing the behavior of its absorbing court. Indeed, because some judges were transferred from the absorbed court to the absorbing one, it is reasonable that they also influence absorbing court behavior. In a sense, this experiment is close to the work by [Abrams et al. \(2022\)](#) that shows there is a convergence in judges' decisions within a commercial court that reflects a process of acquiring local practices by new judges.³²

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

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

³²However, whereas their study is based on judges' rotation, our experiment is set at the court level.

$$\begin{aligned}
 Y_{ijt} = & \alpha + \sum_{k=d,g} \beta_k (Reform_k \times Post) \\
 & + \sum_{k=d,g} \delta_k (\overline{\Delta Share Y_{jk}} \times Post) + \gamma_1 X_i + \gamma_2 u_j + \theta_j + \theta_{s \times t} + \epsilon_{ijt}
 \end{aligned} \tag{8}$$

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

Table 10: Transmission of courts' behavior

	R_{ijt}		P_{ijt}		S_{ijt}	
	(1)	(2)	(3)	(4)	(5)	(6)
	β_R	β_P	β_P^R	β_S	β_S^R	β_S^P
Absorbed \times Post	-0.0450*** (-3.37)	-0.0253*** (-3.78)	0.00205 (0.19)	0.00296 (0.77)	0.0174** (2.46)	0.0357** (2.52)
Absorbing \times Post	-0.0359*** (-3.14)	-0.0184** (-2.47)	-0.00502 (-0.49)	-0.00455 (-1.16)	-0.00624 (-0.91)	0.00408 (0.31)
Post \times $\Delta Share_{g \rightarrow d}$	0.424*** (5.84)	0.424*** (3.53)	0.659*** (7.14)	0.598*** (4.47)	0.573*** (5.11)	0.617*** (6.09)
Post \times $\Delta Share_{d \rightarrow g}$	0.282*** (3.19)	0.340*** (3.10)	0.120*** (3.69)	0.157 (1.39)	0.176*** (3.45)	0.179* (1.83)
Unemployment rate	-0.0261*** (-4.23)	-0.00984*** (-3.94)	-0.00298 (-0.67)	-0.00436*** (-2.86)	-0.00146 (-0.35)	0.000308 (0.06)
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	0.072	0.071	0.069	0.035

t statistics in parentheses

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

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

A potential problem with this experiment is that the differences in bankruptcy outcomes between courts might reflect differences in the composition of firms in the two jurisdictions rather than that of their behavior. We thus use another measure of the

outcomes by computing the part of the bankruptcy outcomes not explained by firms' characteristics: the court average residual of equation (4) without court fixed effect before the reform.³³ We then construct the difference in behavior between pairs of merging courts and estimate the equation (8). Results are shown in Table 11. They are qualitatively the same as the one reported in Table 10.

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

	R_{ijt}		P_{ijt}		S_{ijt}	
	(1)	(2)	(3)	(4)	(5)	(6)
	β_R	β_P	β_P^R	β_S	β_S^R	β_S^P
Absorbed \times Post	-0.0389*** (-3.15)	-0.0209*** (-3.51)	-0.00525 (-0.48)	0.000758 (0.22)	0.0118* (1.73)	0.0305** (2.23)
Absorbing \times Post	-0.0359*** (-3.27)	-0.0182** (-2.38)	-0.00493 (-0.50)	-0.00383 (-1.08)	-0.00441 (-0.67)	0.00355 (0.28)
Post \times $\Delta Residuals_{g \rightarrow d}$	0.596*** (7.44)	0.521*** (5.86)	0.604*** (6.58)	0.677*** (5.96)	0.705*** (6.98)	0.673*** (6.05)
Post \times $\Delta Residuals_{d \rightarrow g}$	0.359*** (5.31)	0.304*** (3.26)	0.139*** (4.37)	0.147 (1.61)	0.188*** (3.42)	0.197** (2.54)
Unemployment rate	-0.0265*** (-4.38)	-0.00987*** (-3.97)	-0.00294 (-0.66)	-0.00436*** (-2.86)	-0.00136 (-0.33)	0.000599 (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	0.072	0.071	0.069	0.036

t statistics in parentheses

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

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

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

³³See Appendix C for details.

$$\begin{aligned}
 Y_{ijt} = & \alpha + \sum_{k=g,d} \sum_{t \neq 2007} \beta_{kt} (\mathbf{1}_t \times Reform_k) + \gamma_1 X_i + \gamma_2 u_j + \theta_j + \theta_{s \times t} + \epsilon_{ijt} \\
 & + \sum_{k=g,d} \left(\sum_{t=2000}^{2007} \tau_{kt} (\mathbf{1}_t \times \Delta Share Y_{jkt}) + \sum_{t=2008}^{2019} \delta_{kt} (\mathbf{1}_t \times \overline{\Delta Share Y_{jk}}) \right) \quad (9)
 \end{aligned}$$

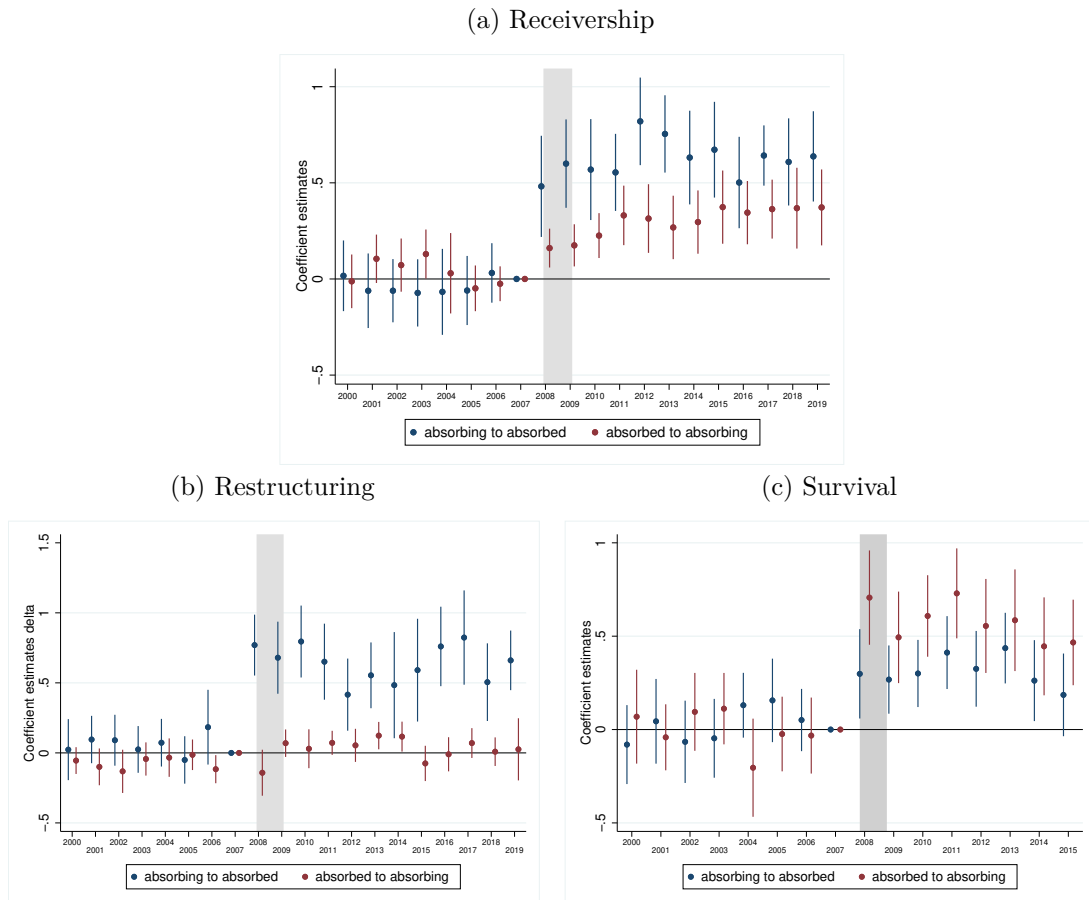
Before reform, we interact the annual measure $\Delta Share Y_{jt}$ with year dummy $\mathbf{1}_t$. The coefficient τ_{gt} (resp. and τ_{dt}) captures the proportion of the behavior of the absorbing (resp. absorbed) courts that has an impact on the firms in the absorbed (resp. absorbing) jurisdiction. After reform, we consider $\overline{\Delta Share Y_j}$, the pre-reform average behavior. The coefficient δ_{gt} (resp. and δ_{dt}) captures the proportion of the pre-reform behavior of the absorbing (resp. absorbed) court that has an impact on firms from the absorbed (resp. absorbing) jurisdiction.

In all panels of Figure 7, we see that before the reform, τ_{jdt} and τ_{jgt} are not significantly different from zero: for firms in absorbed and absorbing jurisdictions, the difference in behavior between the two courts had no impact on the bankruptcy outcomes. This result supports the parallel trends hypothesis.

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

To summarize, in this section, we show that the behavior of absorbing courts influences

Figure 7: Transmission of courts' behavior



Note: Figure 7 shows the coefficients of equation (9) τ_{jg} and δ_{jg} in blue, and τ_{jd} and δ_{jd} in red. Panel (a), the dependent variable is the probability of entering receivership compared to direct liquidation, and $\Delta Share_{jt}$ is the difference in receivership rates between absorbing and absorbed jurisdiction. Panel (b), the dependent variable is the probability of restructuring after receivership and $\Delta Share_{jt}$ the difference in restructuring rate between absorbing and absorbed jurisdiction. Panel (c), the dependent variable is the 7-year survival after filing and $\Delta Share_{jt}$ is the difference in the 7-year survival rates between absorbing and absorbed jurisdiction.

Reading: Panel (a) after the 2009 reform, the blue points represent the δ_{jg} coefficients. They are significantly positive and vary between 0.5 and 0.75. This means that 50% to 75% of the entry into receivership of firms from absorbed jurisdictions is explained by the pre-reform receivership rate of the court that absorbed them.

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

7 Conclusion

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

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

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

catching area of the court they absorbed than the opposite.

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

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

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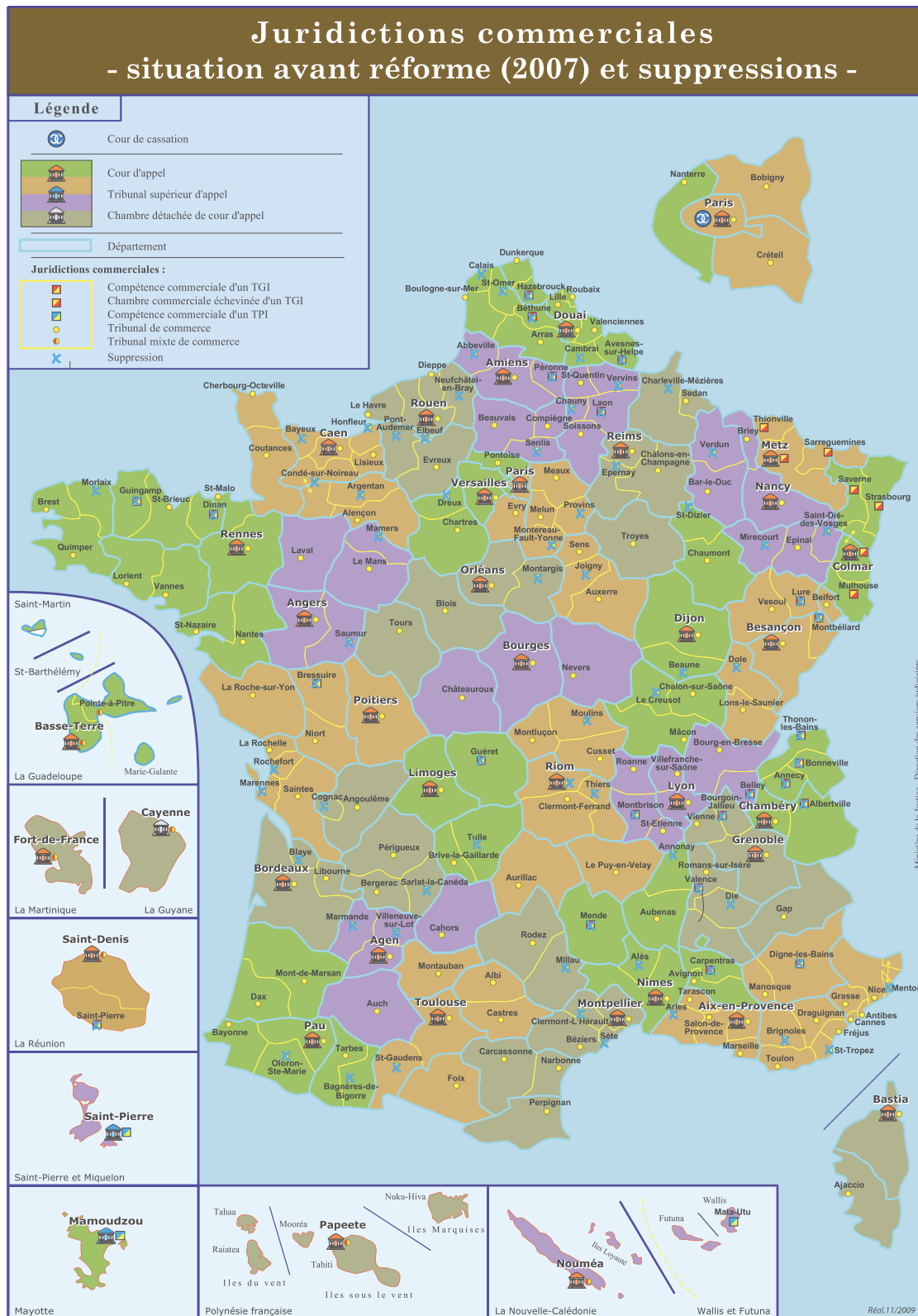
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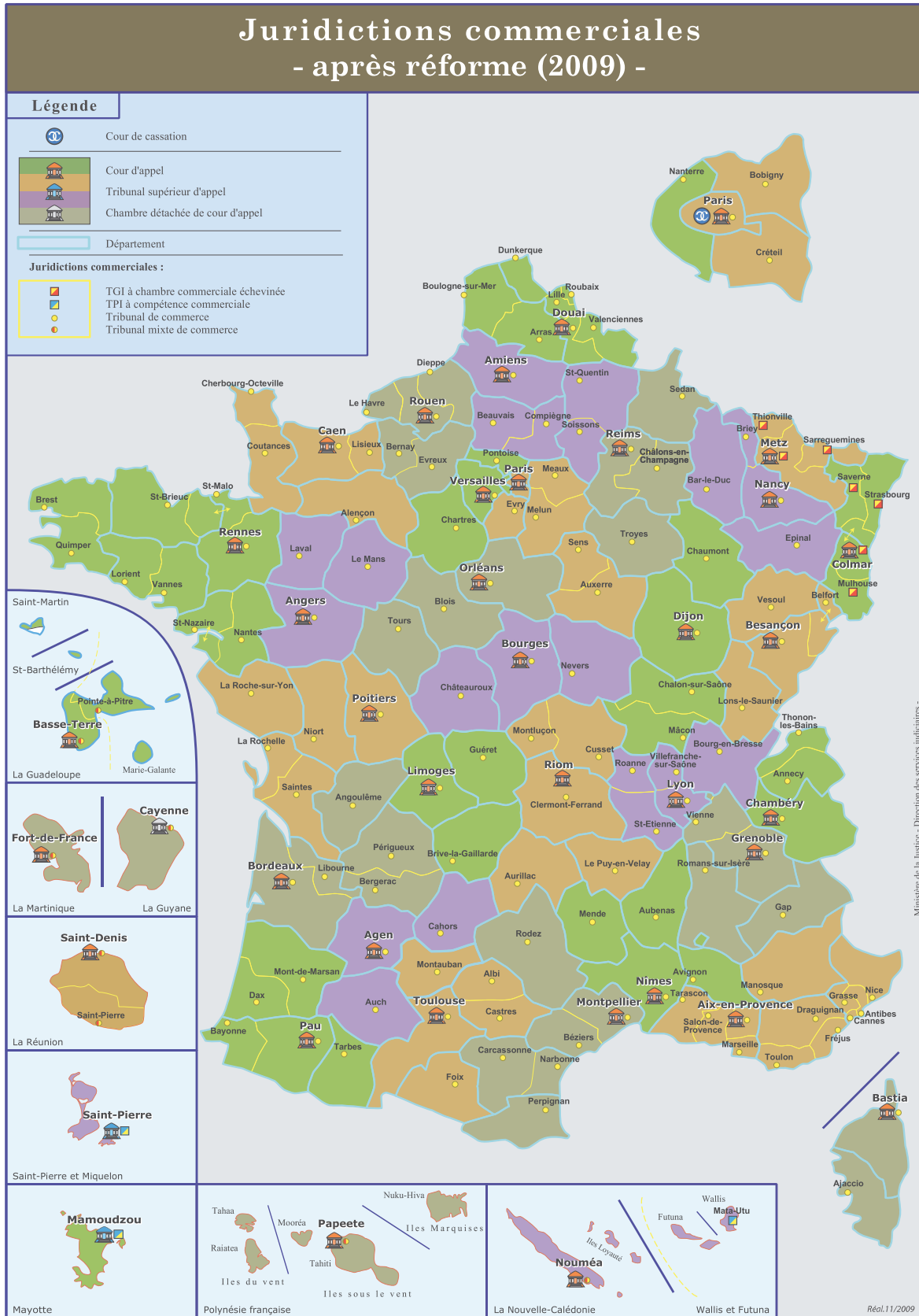
A Additional Materials and Robustness

Figure A.1: French judicial map before reform



Source : French Ministry of Justice

Figure A.2: French judicial map after reform



Source : French Ministry of Justice

Table A.1: Additional summary statistics: average per jurisdiction

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

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

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

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

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

A.2 Judges' caseload

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

The literature expects an impact of judges' workload on the outcome of judgments, even if the effect is difficult to predict (Iverson (2018), Müller (2022)). One possibility is

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

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

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

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

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

Table A.3: The impact of judges' caseload on bankruptcy outcomes and procedure length

Bankruptcy outcome	R_{ijt}		P_{ijt}		S_{ijt}	
Coefficient of interest	β_R	β_P	β_P^R	β_S	β_S^R	β_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.3 reproduce the baseline specification (equation (4)) on firms' bankruptcy outcomes and procedure length with as an additional control variable the judges' caseloads. The judges' caseload variable is measured by the average number of cases per judge, per court, and per year. Because we have court fixed effects, we measure the impact of a change in judge caseload within courts. Standards errors are clustered at the pre-reform court level.

specification.

We find that the variables correlated with judge caseloads are the probability of

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

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

A.3 Robustness to the 2009 financial crisis

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

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

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

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

log of its total assets, leverage and debt composition³⁴) and the reform.

We propose the same test on the sectoral composition of each jurisdiction. We create dummies equal to 1 if the firm is in the service sector for example, 0 otherwise. We consider four sectors: construction, services, trade, and other (agriculture, manufacturing, and transport). The results are also reported in Figure A.3.³⁵ The β coefficients are overall not significantly different from zero, although some trends suggest that the sectoral composition of jurisdictions may have varied over time (in the services sector, for example, where β is relatively positive in absorbed jurisdictions compared to control jurisdictions). To account for any differences along these dimensions, we include in all the specifications of our paper firms characteristics and Industry \times Year fixed effects.

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

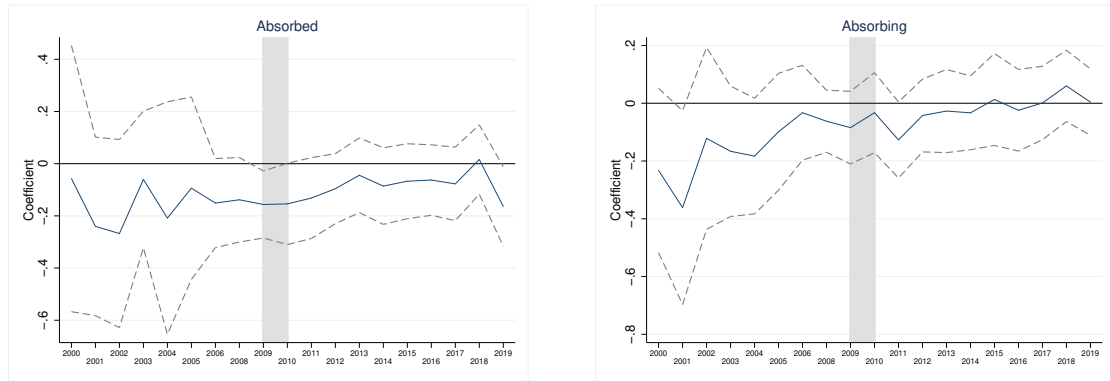
³⁴Such financial information is only available for about 8% of our full sample via FIBEN. It concerns mainly bigger firms and might therefore not be fully representative of the full sample.

³⁵Regressions on industry do not include industry \times year fixed effects, but only year fixed effects.

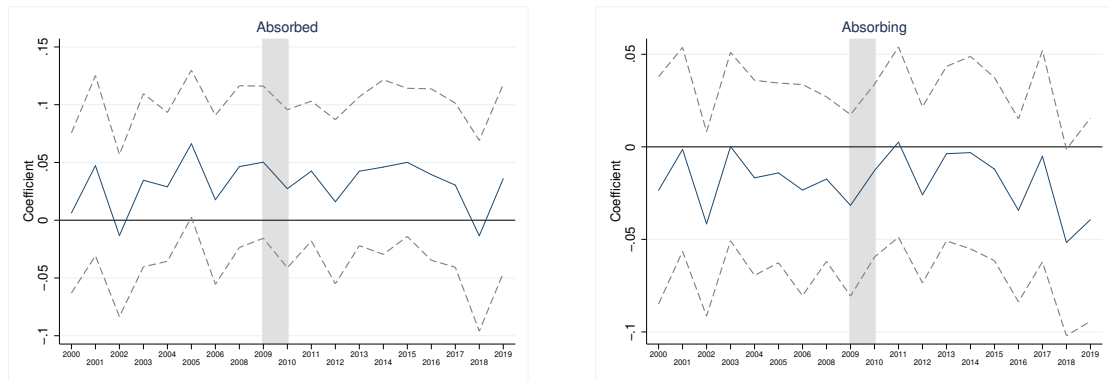
Figure A.3: Dependent variables pre-trends

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

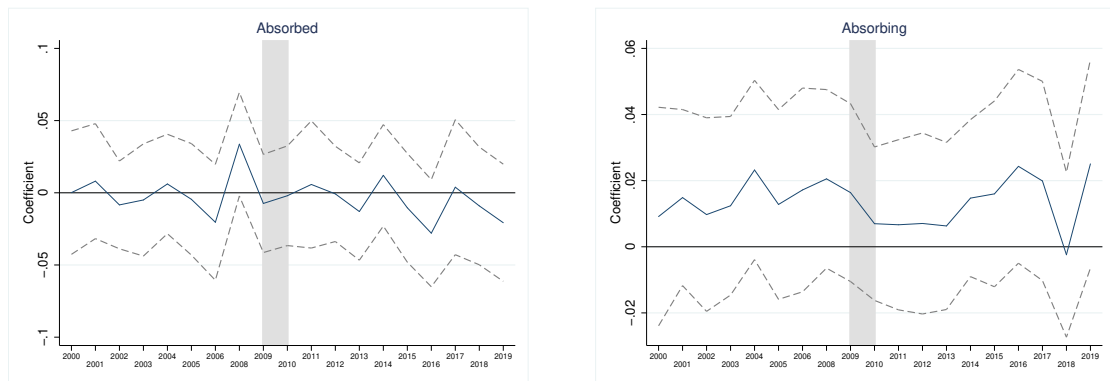
Ln(Total Assets)



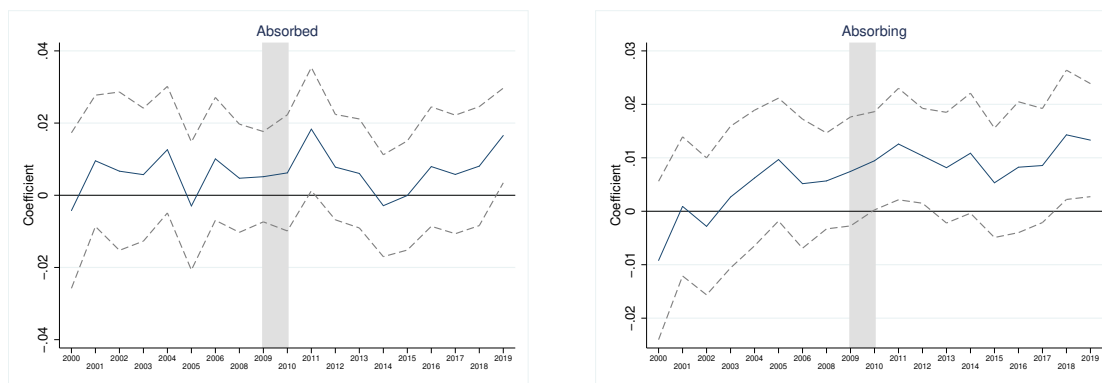
Total Debt / Total Asset



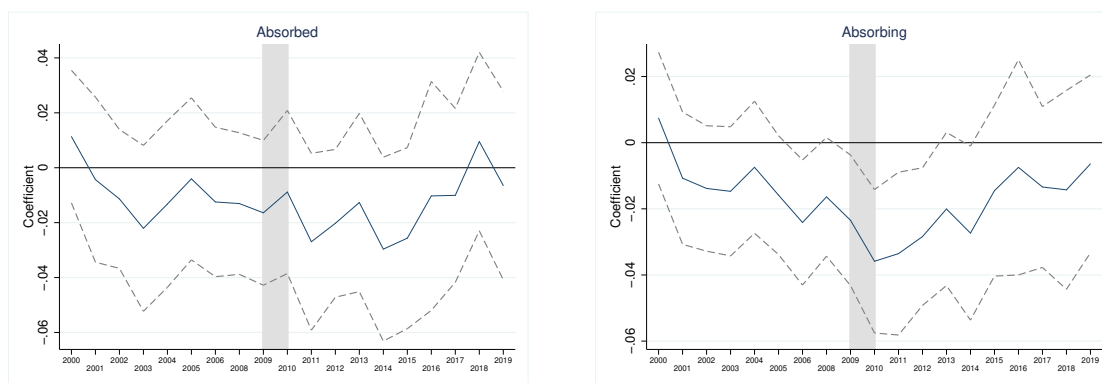
Supplier Debt / Total Debt



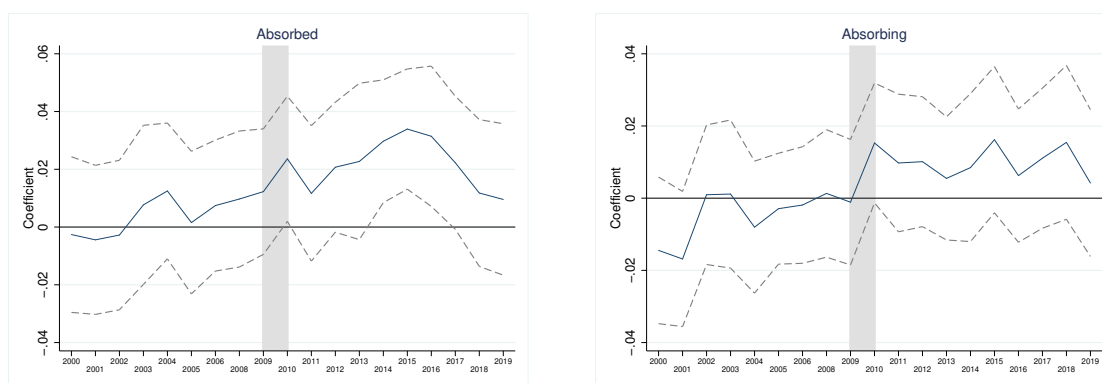
Manufacturing sector



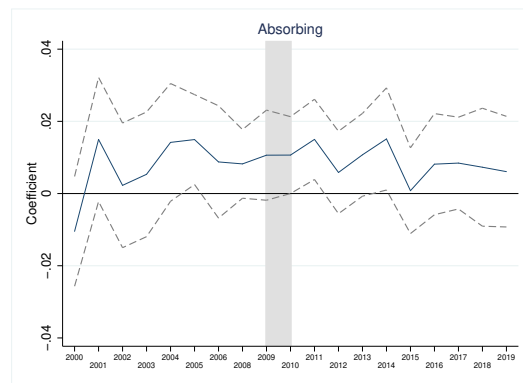
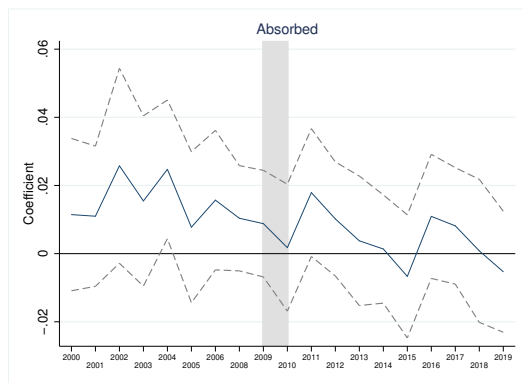
Service sector



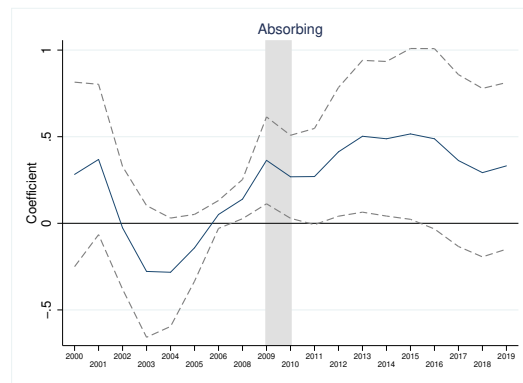
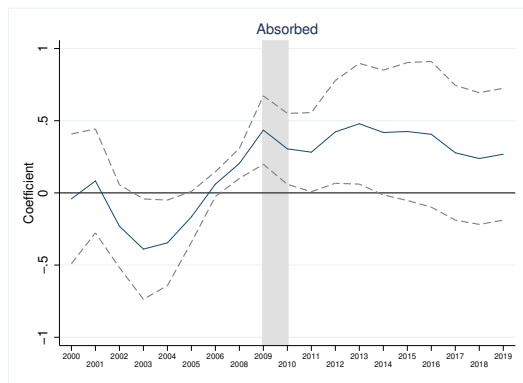
Trade sector



Other sectors



Unemployment rate



B Conceptual Framework: Derivation of results

Starting from the six initial equations:

$$\left\{ \begin{array}{l} R_j = x_j R_j^h + (1 - x_j) R_j^l \\ P_j = x_j R_j^h P_j^h + (1 - x_j) R_j^l P_j^l \\ P_j^R = \frac{x_j R_j^h P_j^h + (1 - x_j) R_j^l P_j^l}{R_j} = \frac{P_j}{R_j} \\ S_j = s_j x_j R_j^h P_j^h \\ S_j^R = \frac{S_j}{R_j} \\ S_j^P = \frac{S_j}{P_j} \end{array} \right. \quad (11)$$

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

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

$$\left\{ \begin{array}{l} \Delta R_j = x_j \Delta R_j^h + (1 - x_j) \Delta R_j^l \\ \Delta P_j = x_j (\Delta R_j^h P_j^h + \Delta P_j^h R_j^h) + (1 - x_j) (\Delta R_j^l P_j^l + \Delta P_j^l R_j^l) \\ \Delta P_j^R = \frac{\Delta P_j R_j - \Delta R_j P_j}{(R_j)^2} \\ \Delta S_j = s_j x_j (\Delta R_j^h P_j^h + \Delta P_j^h R_j^h) \\ \Delta S_j^R = \frac{\Delta S_j R_j - \Delta R_j S_j}{(R_j)^2} \\ \Delta S_j^P = \frac{\Delta S_j P_j - \Delta P_j S_j}{(P_j)^2} \end{array} \right. \quad (12)$$

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

$$\left\{ \begin{array}{l} \Delta R_j \equiv \beta_R \\ \Delta P_j \equiv \beta_P \\ \Delta P_j^R \equiv \beta_P^R \\ \Delta S_j \equiv \beta_S \\ \Delta S_j^R \equiv \beta_S^R \\ \Delta S_j^P \equiv \beta_S^P. \end{array} \right. \quad (13)$$

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

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

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

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

$$T_1 = R_j^l P_j^l \Rightarrow \Delta T_1 = \Delta R_j^l P_j^l + \Delta P_j^l R_j^l \quad (15)$$

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

These first results are reported in the matrix below (Table 2 in the paper).

Table B.4: Type 1 and Type 2 errors matrix

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

Using equations for ΔP_j^R , ΔS_j^R and ΔS_j^P , and noting that $R_j > 0$, $P_j > 0$ and $S_j > 0$ one can derive the robustness matrix below (Table 4 in the paper) that gives the expected signs for $\beta_S^R > 0$, $\beta_S^P > 0$ and $\beta_P^R > 0$ in function of the signs of β_S , β_P and β_R .

Table B.5: Robustness matrix

		β_R			β_P		
		> 0	$= 0$	< 0	> 0	$= 0$	< 0
β_S	> 0	?	$\beta_S^R > 0$	$\beta_S^R > 0$?	$\beta_S^P > 0$	$\beta_S^P > 0$
	$= 0$	$\beta_S^R < 0$	$\beta_S^R = 0$	$\beta_S^R > 0$	$\beta_S^P < 0$	$\beta_S^P = 0$	$\beta_S^P > 0$
	< 0	$\beta_S^R < 0$	$\beta_S^R < 0$?	$\beta_S^P < 0$	$\beta_S^P < 0$?
β_P	> 0	?	$\beta_P^R > 0$	$\beta_P^R > 0$			
	$= 0$	$\beta_P^R < 0$	$\beta_P^R = 0$	$\beta_P^R > 0$			
	< 0	$\beta_P^R < 0$	$\beta_P^R < 0$?			

C Measuring Courts' Behavior

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

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

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

with $\#cases_{jt}$ being the number of filings (receiverships + direct liquidations), of receiverships, or of restructurings, depending on the outcome.

For absorbed jurisdictions (indexed by d), we take the difference between the absorbing court and its absorbed court :

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

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

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

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

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

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

$$Y_{ijt} = \alpha + \beta(Reform_j \times Post) + \gamma_1 X_i + \gamma_2 u_j + \theta_{s \times t} + \epsilon_{ijt} \quad (16)$$

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

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

In the same way for absorbing jurisdictions :

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

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

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

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

Table C.1: Summary of $\Delta ShareY$ and $\Delta ResidualsY$

		Receivership									
		$\Delta Share_{jt}$ (2000-2007)					$\Delta Residuals_{jt}$ (2000-2007)				
	N	Mean	Median	St. Dev.	Min	Max	Mean	Median	St. Dev.	Min	Max
$g \rightarrow d$	50	-0.047	-0.035	0.101	-0.342	0.153	-0.044	-0.040	0.087	-0.280	0.156
$d \rightarrow g$	42	0.043	0.040	0.101	-0.155	0.340	0.043	0.042	0.086	-0.156	0.280
		Restructuring after filing									
		$\Delta Share_{jt}$ (2000-2007)					$\Delta Residuals_{jt}$ (2000-2007)				
	N	Mean	Median	St. Dev.	Min	Max	Mean	Median	St. Dev.	Min	Max
$g \rightarrow d$	50	-0.027	-0.020	0.041	-0.129	0.042	-0.030	-0.026	0.049	-0.173	0.078
$d \rightarrow g$	42	0.022	0.018	0.038	-0.045	0.110	0.027	0.026	0.042	-0.076	0.128
		Restructuring after receivership									
		$\Delta Share_{jt}$ (2000-2007)					$\Delta Residuals_{jt}$ (2000-2007)				
	N	Mean	Median	St. Dev.	Min	Max	Mean	Median	St. Dev.	Min	Max
$g \rightarrow d$	50	-0.034	-0.023	0.086	-0.316	0.167	-0.023	-0.019	0.096	-0.328	0.184
$d \rightarrow g$	42	0.028	0.019	0.086	-0.167	0.321	0.016	0.019	0.089	-0.184	0.296
		Survival 7 years after filing									
		$\Delta Share_{jt}$ (2000-2007)					$\Delta Residuals_{jt}$ (2000-2007)				
	N	Mean	Median	St. Dev.	Min	Max	Mean	Median	St. Dev.	Min	Max
$g \rightarrow d$	50	-0.012	-0.009	0.023	-0.076	0.037	-0.008	-0.008	0.028	-0.070	0.049
$d \rightarrow g$	42	0.010	0.009	0.021	-0.032	0.071	0.007	0.003	0.026	-0.048	0.064
		Survival 7 years after receivership									
		$\Delta Share_{jt}$ (2000-2007)					$\Delta Residuals_{jt}$ (2000-2007)				
	N	Mean	Median	St. Dev.	Min	Max	Mean	Median	St. Dev.	Min	Max
$g \rightarrow d$	50	-0.011	-0.004	0.058	-0.182	0.121	0.000	0.000	0.058	-0.156	0.146
$d \rightarrow g$	42	0.006	0.005	0.056	-0.110	0.178	-0.002	-0.007	0.058	-0.146	0.156
		Survival 5 years after restructuring									
		$\Delta Share_{jt}$ (2000-2007)					$\Delta Residuals_{jt}$ (2000-2007)				
	N	Mean	Median	St. Dev.	Min	Max	Mean	Median	St. Dev.	Min	Max
$g \rightarrow d$	50	-0.013	-0.025	0.113	-0.311	0.295	-0.002	-0.010	0.112	-0.212	0.289
$d \rightarrow g$	42	0.003	0.001	0.104	-0.288	0.174	-0.000	0.007	0.108	-0.289	0.174

Chapter 3

Access to Bank Credit after Emerging from
Corporate Bankruptcy

Abstract

This paper identifies the bank credit restrictions that small firms face after bankruptcy. Using the French credit register, I implement a difference-in-difference strategy that exploits staggered removal of bankruptcy flags in the form of an exogenous change in credit ratings. I focus on small and medium-sized businesses between 2012 and 2019 and show that flag removal leads to an increase in bank credit of 1.7% and a 2 percentage point higher chance of forming new banking relationships. Less well-informed banks increase their credit supply after flag removal, particularly to firms whose credit rating reveals good financial performance. New banks start lending to the most constrained firms. As a result, firms substitute trade credit for bank credit and increase their investment rate. This paper supports the policy choice of shortening the bankruptcy flag.

Acknowledgements specific to Chapter 3

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1 Introduction

Recovering from bankruptcy is a challenge. Loss of investor confidence makes economic recovery difficult. In the case of spectacular bankruptcies, such as those of countries, large banks, or multinationals, past bankruptcy is public knowledge that remains available for a long time, and the stigma persists. The challenge lies in forgiving rather than forgetting (Marchesi et al. (2023)). In the case of individuals or firms with less media coverage, information about past bankruptcies is a public policy issue. It is up to policymakers to decide whether to share or remove information about past bankruptcies from public registries to shape investors' behavior.

In this paper, I measure the causal impact of removing information about past corporate bankruptcies on SMEs' access to bank credit. I estimate whether information on firms' past bankruptcy triggers bank credit restrictions, and I measure the real economic impact of bankruptcy flag removal.

My method exploits the fact that firms' bankruptcy flag removal differs by bankruptcy type. There are two public debt-restructuring bankruptcy procedures in France: safeguard (*sauvegarde*) and the receivership (*redressement judiciaire*). The information about safeguard is removed from the credit register three years after the debt-restructuring agreement. In the case of receivership, the information is removed five years after the debt-restructuring agreement. I use this exogenous event in a difference-in-difference strategy that compares access to bank credit of safeguard firms (the treatment group) with receivership firms (the control group) from four quarters before to four quarters after safeguard flag removal. My sample consists of around 1,000 treated firms and 6,000 control firms observed quarterly between 2012 and 2019. I measure a causal effect under the parallel trend hypothesis, for which I provide supporting empirical evidence and extensive discussion.

Information on firms' past bankruptcy is made available to banks via firms' credit rating. The Banque de France assigns its credit rating to inform lenders about the viability of borrowers. "Bankruptcy flag removal" consists of removing past bankruptcy information from the credit rating. Under the bankruptcy flag, SMEs are assigned a high-risk credit rating. With flag removal, the rating varies according to the regular

analysis of the firm's situation. Most SMEs in my sample are under the Banque de France rating threshold (annual turnover of €750K). This means that aside from the bankruptcy flag, they are usually assigned a "non-significant" (or "neutral") credit rating. About 7% of treated firms obtain a significant, good rating after flag removal.

First, I show that flag removal has a statistically significant effect on firms' access to bank credit. The effect appears immediately and grows linearly over time. Debt restructuring is designed to enable debt repayment over a sustainable time frame. This means that compared with the pre-bankruptcy period, restructuring firms are repaying their debt, and so their total amount of credit decreases with time. Their repayment schedule follows a regular rhythm of roughly -2.4% per quarter compared with the pre-bankruptcy period. With bankruptcy flag removal, treated firms increase their credit by 1.7% . This means they continue to reduce their credit by about -0.7% per quarter compared with the pre-bankruptcy average ($=-2.4\%+1.7\%$). I show that this effect is only partially explained by firms for which flag removal triggers a good rating, and I argue that the increase in credit mainly results from a supply effect. I also find that firms' probability of starting new banking relationships increases by 2 percentage points. As robustness tests, I implement a propensity score matching procedure to control for observed heterogeneity. I then confront these results with the most recent and extensive literature that addresses the drawbacks of the difference-in-difference approach. I confirm that my results are robust to time, group, and cohort heterogeneity.

Regarding firms' economic performance, flag removal has a quite moderate¹ but positive impact: the investment rate is boosted from 3.1% to 3.6% ² but employment, sales, and profit margins do not change significantly. We see interesting changes in firms' debt composition: firms rely less on trade credit following flag removal, which aligns with the prediction that suppliers lend to constrained client firms. Because the relaxation of bank credit restrictions allows for the substitution of funding sources and an increase in the investment rate, this paper supports public policy's desire to shorten the time that information about past bankruptcy is available.

This paper contributes to the literature in several ways. First, the impact of

¹I only study the performance of firms in the year following flag removal, which may explain the absence of a more pronounced effect.

²This result is valid on the sample of firms that survive at least four years after bankruptcy.

past bankruptcy information on access to bank credit has been documented as far as individuals are concerned. Notably, [Bos and Nakamura \(2014\)](#), [Han and Li \(2011\)](#), [Cohen-Cole et al. \(2009\)](#), [Musto \(2004\)](#), [Dobbie et al. \(2020\)](#), and [Saengchote and Tirapat \(2017\)](#) have studied the impact of bankruptcy flag removal on consumers' access to bank credit. My results are in line with the empirical literature that usually finds that removing information about a past personal bankruptcy leads to better access to credit. [Cahn et al. \(2021\)](#) find the same result by empirically studying the removal of corporate bankruptcy flags on entrepreneurs' access to bank credit. In their theoretical model, [Elul and Gottardi \(2015\)](#) also show that removing information about entrepreneurs' past bankruptcy is welfare improving and must therefore be the outcome of a regulatory intervention. Despite this comprehensive literature, few papers study the impact of bankruptcy stigma on firms' rather than individuals' access to credit. Yet, a focus on SMEs³ is critical, not only because SMEs represents 99.95% of bankruptcy filings⁴ and 47% of employment in France,⁵ but also because small firms have limited access to financial markets and thus rely heavily on bank credit ([Lé and Vinas \(2022\)](#)). I therefore contribute directly to the literature on the impact of past bankruptcy information on firms' access to bank credit. Moreover, I also explore the impact of past bankruptcy on small firms' access to bank credit. Among the few papers exploring this issue, [Berkowitz and White \(2004\)](#) show that after a bankruptcy, small firms in the U.S. have restricted access to credit if they are located in a state where the owner may have homestead exemptions that allow them to file for personal bankruptcy with the firms' liabilities. [Bonfim et al. \(2012\)](#) find that, in Portugal and after resolving default, firms have difficulties in regaining access to credit if they are small, bank-dependent, or if their default was severe.

Next, I refer to the literature on banks' behavior toward distressed borrowers. In line with [Huang et al. \(2015\)](#), [Li et al. \(2019\)](#) and [Salvadè et al. \(2022\)](#), I find that the best-informed banks are not the one that increase their credit supply. Instead, the

³Small and medium-sized enterprises (SMEs) are those which, on the one hand, employ fewer than 250 people and, on the other, have an annual turnover not exceeding €50 million or a balance sheet total not exceeding €43 million. In addition, they include the category of micro-enterprises which employ fewer than 10 people and have an annual turnover or balance sheet total not exceeding €2 million—definition from <https://www.insee.fr>.

⁴Source: <https://www.banque-france.fr>.

⁵Source: <https://www.insee.fr>.

increase in credit supply comes from less well-informed lenders and new banks. Less well-informed historical⁶ lenders provide new credit to firms whose rating after flag removal reflects good financial performance. As predicted by Ioannidou and Ongena (2010), I find that the most constrained firms must turn to new banks. I discuss the fact that the increase in credit supply is not mutually exclusive with the fact that information on past bankruptcies remains public and freely available as long as credit is supplied to potentially viable borrowers (Sharpe (1990), Rajan (1992), Von Thadden (2004)).

Finally, this paper contributes to the empirical literature that explores the impact of external credit ratings on firms' access to bank credit and real outcomes. The literature on small firms focuses mainly on the impact of the existence of a credit rating,⁷ while my paper focuses on an exogenous improvement in SMEs' credit rating. The literature that focuses on the effect of an improvement or deterioration in credit rating on firm financing and real outcomes focuses primarily on large firms.⁸ I show that credit ratings also impact small firms' funding sources and investment rates.

The remainder of the paper is organized as follows. Section 2 discusses the institutional background. Section 3 introduces the data. Section 4 describes the empirical strategy to identify the credit constraints. Section 5 discusses the behavior of banks to understand the mechanisms behind the results. Section 6 sets out elements relating to overall economic efficiency, and Section 7 concludes.

2 Institutional Background

2.1 Two Bankruptcy Procedures

There are two public debt-restructuring procedures in France: the safeguard procedure (*sauvegarde*) and receivership (*redressement judiciaire*). The main difference between

⁶I define historical banks as banks already lending to the firm before bankruptcy flag removal.

⁷Empirical research finds that the existence of a small business credit rating is associated with an increase in lending, notably thanks to the reduction in information costs (Berger et al. (2010), Berger et al. (2005), Frame et al. (2001)), which is more marked in small firms than in large ones (Berger and Udell (1995)).

⁸In the case of large firms, the literature has shown that credit ratings can trigger change in firms' leverage and capital structure (Kisgen (2006), Tang (2009), Sufi (2009), Faulkender and Petersen (2005)), real outcomes and investment decisions (Lemmon and Roberts (2010), Chernenko and Sunderam (2012), Harford and Uysal (2014)).

the two is the extent of the financial difficulties that the firm faces. An insolvent firm has access to receivership, while a firm that is not insolvent but can prove that it is facing severe financial problems has access to safeguard. Apart from this difference and a few other specificities listed below, both procedures operate mainly in the same way (see [Kastrinou \(2009\)](#) and [Epaulard and Zapha \(2022\)](#) for in-depth comparisons).

Once the firm files for safeguard or receivership, a six-month observation period (renewable twice)⁹ starts, to assess the firms' financial situation. During the observation period in receivership, the judicial administrator may take over the firm's management. Also, firms in receivership access loans from the Wage Guarantee Scheme (*Régime de garantie des salaires, AGS*) to pay up to three months of wage arrears, which is not possible in safeguard. In both safeguard and receivership, the judicial receiver consults the creditors and drafts a repayment plan proposal. The formation of creditors' committees is mandatory above the threshold of 150 employees or €20 million in turnover. Smaller firms may request them as well. Two committees, one for banks and the other for suppliers, bring together creditors to express their views and impose majority decisions on recalcitrant creditors. Creditors may choose from a variety of options. They can, for example, select between a proposal for full repayment over several years or a shorter repayment plan with partial debt forgiveness. The court is then provided with the proposed plan and decides on its adoption and how long it should last. The plan may be ten years at most (15 years in the agricultural sector). At any time during the observation period, and if no solution is possible, the court can order the firm's liquidation. In receivership, the court may also open up bidding to potential buyers.

The safeguard and receivership restructuring plans are mostly identical:¹⁰ they are both plans organizing the repayment of creditors over ten years on average, according to (i) how much the firm owes and (ii) how much the firm can reasonably repay annually. Once the plan is approved, a commissioner is appointed to ensure compliance with the deadlines and commitments. In both procedures, the firm starts with a one-year non-

⁹The Pacte Act, implemented on January 1, 2020, shortened the maximum length of the observation period in safeguard to 12 months. My sample, which focuses on 2012-2019, is unaffected by this policy change.

¹⁰In practice, the safeguard plan is governed by Articles L626-1 et seq. of the Commercial Code, the text of which essentially refers to the receivership plan.

payment period. Afterwards, annual payments increase gradually, with a minimum of 5% required by law from the third year onwards.

Empirically, [Despierre et al. \(2018\)](#) examine the repayment plans of a small sample of firms that filed in the Commercial Court of Paris between 2006 and 2015:¹¹ according to [Figure B.5](#), safeguard and receivership repayment plans appear to be strictly identical. This preliminary analysis gives grounds for the parallel trend assumption that my identification strategy requires.

2.2 Banque de France Credit Rating

The Banque de France credit rating is a tool for banks accessible by subscription and available to all investors via FIBEN (*Fichier Bancaire des Entreprises*), the Banque de France’s companies database. It serves as a standard reference to monitor the credit risk of potential borrowers; it is an assessment of a firm’s ability to meet its financial commitments over a three-year horizon. It is based on the analysis of the firm’s accounting and financial data, the soundness of its economic environment and partners, and the occurrence of events such as default or bankruptcy. The rating also includes information from analysts in the Banque de France network who conduct interviews, extra-accounting analyses, and rigorous qualitative research. The European Central Bank (ECB) uses it to qualify eligible collateral. It is revised annually on receipt of firms’ financial statements and when significant developments occur.

The rating scale¹² contains 12 significant notches going from P (bankruptcy) to 3++ (safest) and a thirteenth non-significant notch: 0. [Table 1](#) summarizes their description. Firms with a turnover of more than €750K are registered in FIBEN and given a significant rating (i.e., other than 0). Below the €750K threshold and in the absence of adverse information, firms are rated 0.¹³ As soon as a significant development occurs, such as a default or bankruptcy, firms below the threshold receive a significant rating. The Banque de France notifies firms whenever they receive a significant rating (i.e., other

¹¹This small sample of firms is not the working sample of the paper.

¹²This study is based on the old rating scale in effect over the period studied (2012-2019), which was revised on January 1, 2020.

¹³While most of the time 0 means that the firm’s turnover is below the €750K threshold, it can also mean that the firm does not have adequate recent accounting documentation or has one that analysts cannot use because of its activity (e.g., holdings, real estate, legal support firms, etc.).

Table 1: Banque de France credit rating

Rating	Firm's repayment ability is:
3++	Excellent
3+	Very Strong
3	Strong
4+	Quite Strong
4	Good
5+	Quite Weak
5	Weak
6	Very Weak
7	Needs specific attention
8	Threatened
9	Compromised
P	Bankruptcy procedure
0	Non-significant (no negative information)

Note: Table 1 reports a brief overview of the Banque de France credit rating scale for firms. Ratings that qualify firms' loans to be eligible as collateral for refinancing at the ECB are 3++ to 4.

than 0). Firms are not informed when their rating is changed to 0.

A screenshot Figure 1 shows an extract of the information that banks access via FIBEN. In addition to the firm's name, postal address, and Banque de France's branch in charge of the case, banks can access firms' current rating (in our example, 5¹⁴) and their significant ratings over the last 23 months. Banks can also access the firm's latest court rulings (in our example, the adoption of a safeguard plan) and can trace court rulings back up to three years after approval of a safeguard plan or five years after approval of a receivership plan. They can therefore link the rating to the court rulings. Once these three- and five-year periods have elapsed, all the rulings disappear, and the significant ratings linked to the rulings are deleted. Other financial items to which banks have access are not shown here.

The following subsections and Figure 2 summarize the ratings associated with the different stages of bankruptcy.

¹⁴The complete rating is X5. The first letter, X, refers to the firm's level of turnover and ranges from A to N, with X meaning "not significant" (information missing or too old).

Figure 1: FIBEN's module for banks



Note: This figure is a screenshot of information banks access via FIBEN. The SIREN number of the company, its name, and its postal address have been hidden in the interests of confidentiality. The firm filed for safeguard on 08/04/2022 and was rated X5 on 14/04/2022 (FIBEN takes several days to process judicial information). The firm then started a restructuring plan on 20/07/2023; FIBEN updated its rating on 07/08/2023. The bank observes the latest ruling, “adoption of a safeguard plan,” and is informed that the X5 rating is linked to this ruling (when clicking on the “more information” link, see Figure A.2).

Credit Rating in the Case of Receivership

Whenever a firm files for receivership, the court registries automatically enter it into the FIBEN database. The firm is automatically assigned a credit rating of P (for bankruptcy “*procédure*”) that lasts for the whole duration of the observation period, up to 18 months. The observation period ends with either a debt-restructuring agreement between the firm and its creditors or its sale or liquidation. In the event of a restructuring plan, the credit rating changes to 6.¹⁵

The credit ratings resulting from court decisions can be described as “semi-automatic” and can be revised based on relevant evidence. In principle, the rating 6 is maintained during the execution of the receivership plan. However, once the plan is adopted and following the analysis of accounting documents, the rating 6 may be replaced by a more (or less) favorable credit rating before the completion of the plan. Specifically, a firm executing a receivership plan may receive the following:

¹⁵Up to 2011, the credit rating of firms undergoing a receivership plan was 5. It was changed to 6 on January 1, 2012, to better convey the credit risk receivership firms entail. My sample, which focuses on the 2012-2019 period, is unaffected by this policy change. I exploit this policy change in Appendix D.

- a rating of 5 or 5+ if the criteria for the assignment of one of these ratings are met;
- a rating of 7, 8, or 9 in the presence of payment defaults;
- a rating of P if the plan fails and the firm files for liquidation.

After five years, information about the receivership plan is removed from FIBEN and no longer influences the rating. From then on, the rating varies according to the regular analysis of the firm's financial accounts.¹⁶

Credit Rating in the Case of Safeguard

If a firm files for safeguard, the court registries automatically enter it into the FIBEN database. The firm is automatically assigned a credit rating of 5. At the end of the observation period and in the event of a debt-restructuring agreement, the rating of 5 remains.

As with receivership, firms' credit rating in safeguard plans is "semi-automatic" and can be revised based on relevant evidence. During the execution period of the safeguard plan, in principle the credit rating of 5 is maintained. However, once the plan is adopted and following the analysis of accounting documents, the rating 5 may be replaced by a more (or less) favorable credit rating before the completion of the plan. Specifically, a firm executing a safeguard plan may receive the following:

- a rating of 5+, 4, or 4+ if all the conditions for the assignment of one of these ratings are met;
- a rating of 4+ if the criteria for the assignment of a rating 3++, 3+, or 3 are met;
- a rating of 6 if the situation deteriorates further to a point where it jeopardizes the implementation of the plan;
- a rating of 7, 8, or 9 in the presence of payment defaults;

¹⁶Under Decree 2011-1836 of December 7, 2011: "The opening of a safeguard plan or a receivership plan [...] are subject to mentions in the Trade and Companies Register. This decree provides for the automatic removal of these mentions after three years in the event of a safeguard plan or five years in the case of a receivership plan, to assist firms that have proved their ability to restructure." (translated by the author)

- a rating of P if the plan fails and the firm files for receivership or liquidation.

After three years, information about the safeguard plan is removed from FIBEN and no longer influences the rating.¹⁷ From then on, the rating varies according to the regular analysis of the firm's financial accounts. This exogenous rating removal provides my identification strategy.

Figure 2: Summary of credit ratings associated with court rulings



Note: Figure 2 summarizes the timing of the credit ratings associated with bankruptcy.

3 Data and Summary Statistics

3.1 Data Sources and Sample Selection

My analysis exploits bankruptcy data from two sources: the first is supplied directly by the court registries directly in FIBEN and is used as input to set the credit rating. It includes the dates of filing, approval of a debt-restructuring plan, and liquidation of the firm, where relevant. The second source is the BODACC,¹⁸ provided by the court registries online, in electronic form since January 2008. Information reported by BODACC is public and provides complementary information on the rulings. Notably, it informs us about the duration of the plan.

My research then uses data from the SCR (*Service Central des Risques*), the French credit register. The SCR records the loans granted by credit institutions, primarily

¹⁷See footnote 16.

¹⁸“*Bulletin Officiel d’Annonces Civiles et Commerciales*”, see <https://www.data.gouv.fr/en/datasets/bodacc/en>.

banks, to each client firm every month. Since 2006, loans have been recorded when the total cumulative loan between a firm and a bank, regardless of the type of declaration, reaches €25K. Because of this threshold, some time periods are missing for some units in the sample, and the database is unbalanced. In the analysis, I restrict the sample to firms with no gap in the data.

I collate credit data with bankruptcy data. Given the nature of the firms I observe and their financial difficulties, a proportion of them do not successfully restructure and file for liquidation during the plan. In my baseline analysis, I restrict the sample to firms whose plan was carried out for at least four years. I control for attrition and I test that my sample selection does not bias the results in Appendix B.4. To observe firm survival over a four-year horizon (unaffected by the 2020 health crisis), the sample focuses on firms that started a plan between 2008 and 2016. I consider data from after 2012 because of the policy change concerning the credit rating in receivership:¹⁹ we observe the sample firms between 2012 and 2019. I remove holdings, agricultural firms, and subsidiary firms. In the end, I follow 983 firms in safeguard and 5,082 in receivership.

Lastly, I complement my database with financial information from FIBEN. Because of the collection threshold of €750K in turnover, I do not have complete financial information for all the firms in my sample. Using refined financial ratios such as leverage, investment, or profit margin leads to a significant loss of observations. I use these financial ratios in Section 6 and in robustness tests.

3.2 Descriptive Statistics

I provide summary statistics for the sample in Table 2. Panel A shows summary statistics of firm credit at the firm-quarter level. The average credit exposure of firms in safeguard is about €387K, greater than that of firms in receivership (€206K). Firms in safeguard have, on average, slightly more banks than firms in receivership (1.8 vs. 1.4, respectively). Regarding debt composition, on average, firms in safeguard have more long-term debt (over one-year maturity) than those in receivership (46% vs 43%).

Panel B provides statistics of the firms' financial variables available on an annual basis. The median turnover for safeguard firms is €676K, and €394K for firms in receivership.

¹⁹See footnote 15.

This means that most firms are below the €750K Banque de France rating threshold. The differences in other financial variables between safeguard and receivership are small (safeguard firms are, on average, less leveraged, have more cash, more employees, a lower cost of debt and lower gross operating profit margins) and may not be significant. The investment rate and share of supplier debt in total debt is identical in both groups. As a robustness test, I develop a propensity score matching procedure to control for observable differences between the two groups.

Panel C shows summary statistics about the restructuring plans in safeguard and receivership. On median for the two procedures, plans last for 10 years (with a mean length of 9.6 years), which is the maximum length. Lastly, panel D provides statistics on firm-bank relationships. On average, firms filing for safeguard rely less on main banking relationships than firms filing for receivership (47% against 61% respectively). 44% to 46% of the relationships occur in the same French department.

3.3 Flag Removal

Before the econometric identification, let us analyze the characteristics of flag removal in safeguard and receivership. Figure 3 shows the sharp drop in the credit rating of firms in safeguard and receivership around their flag removal. Almost 80% of firms in safeguard were rated 5 before the 12th quarter of their plan; this number drops to below 30% in the 13th quarter. Of firms in receivership 75% were rated 6 before the 20th quarter of the plan; only 10% of them are in the 21st quarter.

Table 2: Summary Statistics

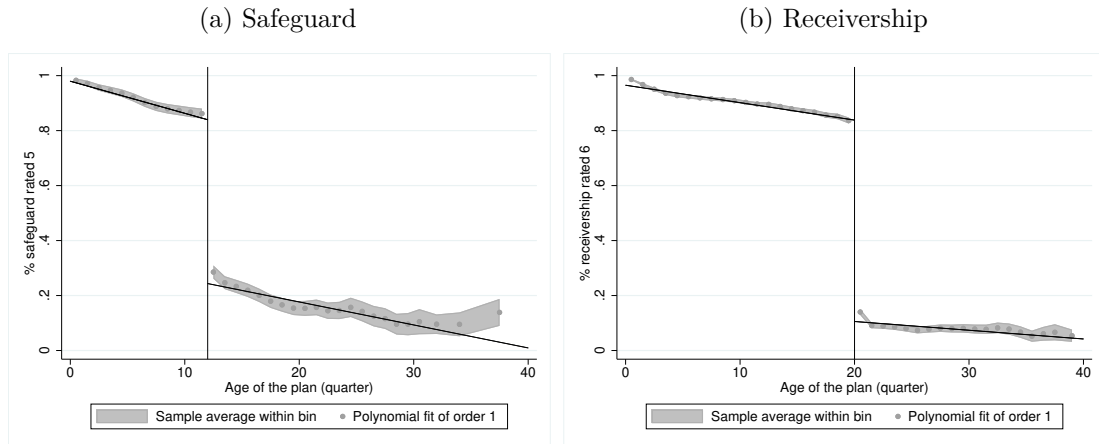
	Panel A: Firm-quarter characteristics					
	N	Mean	Median	St.Dev.	5th Pct.	95th Pct.
<i>Treated group: firms in safeguard</i>						
Bank credit (K€)	8,322	387	163	660	33	1560
Long-term/Total credit	8,281	0.460	0.435	0.420	0.000	1.000
Number of banks	8,322	1.782	1.000	1.273	1.000	4.000
<i>Control group: firms in receivership</i>						
Bank credit (K€)	40,196	206	90	742	28	670
Long-term/Total credit	39,891	0.428	0.310	0.426	0.000	1.000
Number of banks	40,196	1.405	1.000	0.892	1.000	3.000
	Panel B: Firm-year characteristics					
	N	Mean	Median	St.Dev.	5th Pct.	95th Pct.
<i>Treated group: firms in safeguard</i>						
Turnover (K€)	5,168	1247	646	1736	67	5156
Asset (K€)	5,168	1024	524	1290	79	4079
Leverage (Total debt/Total assets)	5,147	1.173	0.929	1.014	0.434	2.491
Supplier debt/Total debt	5,134	0.185	0.137	0.156	0.018	0.504
Cash/Total assets	5,147	0.112	0.070	0.122	0.001	0.375
Apparent Cost of debt	4,469	0.274	0.017	1.319	0.000	0.953
Investment rate	4,532	0.031	0.007	0.058	0.000	0.156
#Employees	5,163	7.358	4.000	10.430	0.000	28.500
Profit margins	4,513	0.066	0.059	0.144	-0.166	0.311
<i>Control group: firms in receivership</i>						
Turnover (K€)	24,365	762	367	1207	48	2838
Asset (K€)	24,365	548	284	850	22	1961
Leverage (Total debt/Total assets)	23,767	1.448	1.097	1.230	0.477	3.568
Supplier debt/Total debt	23,625	0.185	0.143	0.152	0.022	0.500
Cash/Total assets	23,767	0.097	0.056	0.115	0.000	0.344
Apparent Cost of debt	18,696	0.348	0.013	1.526	0.000	1.388
Investment rate	18,684	0.032	0.008	0.059	0.000	0.160
#Employees	24,260	5.351	3.000	8.067	0.000	21.000
Profit margins	19,101	0.089	0.072	0.152	-0.133	0.379

Table 2: Summary Statistics – continued

	Panel C: Firm characteristics					
	N	Mean	Median	St.Dev.	5th Pct.	95th Pct.
<i>Treated group: firms in safeguard</i>						
Length of the plan (years)	825	9.566	10.000	1.212	7.000	10.000
<i>Control group: firms in receivership</i>						
Length of the plan (years)	3,475	9.590	10.000	0.995	8.000	10.000
	Panel D: Firm-bank characteristics					
	N	Mean	Median	St.Dev.	5th Pct.	95th Pct.
<i>Treated group: firms in safeguard</i>						
Main relationship	3,131	0.470	0.000	0.498	0.000	1.000
Same French department	3,092	0.459	0.339	0.467	0.000	1.000
<i>Control group: firms in receivership</i>						
Main relationship	12,962	0.609	1.000	0.486	0.000	1.000
Same French department	12,635	0.439	0.198	0.465	0.000	1.000

Note: Table 2 reports sample summary statistics for key variables. The sample period is from 2012 to 2019. To control for sample attrition, we restrict our sample to firms whose plan was adopted between 2008 and 2016 and carried out for at least four years. We remove holdings, agricultural firms, and subsidiary firms. We follow firms from four quarters before the flag removal to four quarters after it. Each panel compares the treatment group, safeguard firms, with the control group, receivership firms. Panel A reports the firm’s quarterly credit information obtained from the French credit register. Panel B reports firms’ annual financial variables obtained via FIBEN. Panel C reports information on firms’ restructuring plans obtained from the BODACC. Panel D reports the characteristics of the firm-bank relationships obtained from the French credit register. Apparent Cost of Debt = Interest Expenses / Debt. Investment rate = (Tangible + Intangible Investments) / Lagged Total Assets. Margins are the Gross Operating Profit Margins = Value-Added - Staff Cost / Revenue. The “main relationship” dummy equals 1 if the firm-bank relationship represents the firm’s largest share of bank credit. The “same French department” dummy equals 1 if the firm’s headquarters and the bank’s local branch are in the same French department.

Figure 3: Credit rating of firms in continuation plan



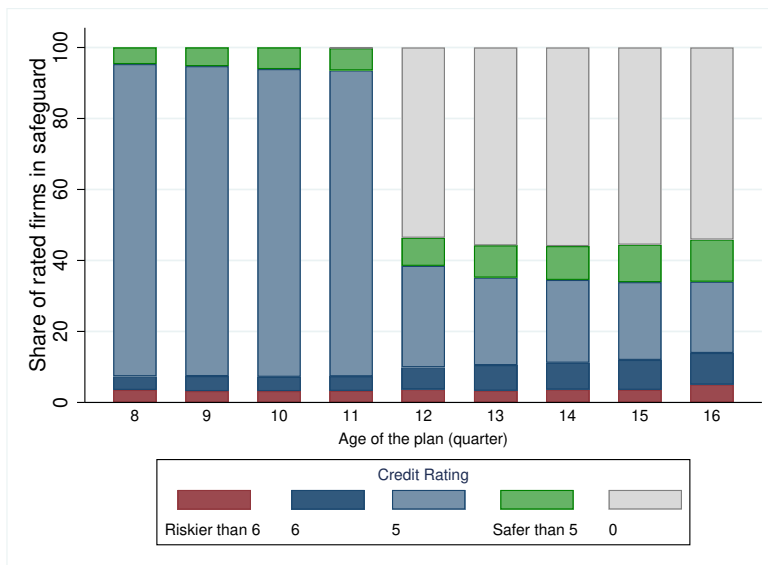
Note: Panel (a) represents the sharp drop in the proportion of firms in safeguard rated 5 around flag removal at the 12th quarter of the plan. Panel (b) represents the sharp drop in the proportion of firms in receivership rated 6 around flag removal at the 20th quarter of the plan. We can see that 75% of firms in receivership were rated 6 before the 20th quarter of the plan; only 10% of them are in the 21st quarter.

When I examine the detailed variation in firms' credit rating in safeguard (Figure 4), I observe that most firms are rated 0 after flag removal. As explained in Section 2.2, the rating 0 means that the firm has a non-significant credit rating: either because its annual turnover is below the minimum threshold of €750K or because it did not provide enough accounting information. More importantly, a rating of 0 means no salient negative information on the firm. 54% of firms in safeguard are rated 0 following flag removal. Only a few firms obtain a better credit rating after the cut-off point: just 8% are rated 5+ or safer.

4 Empirical Analysis: Identifying Credit Constraints

To measure the impact of flag removal on firms' access to bank credit, I implement a difference-in-difference strategy based on a comparison of restructured firms in safeguard (the treatment group) with restructured firms in receivership (the control group). Firms in safeguard and receivership are in a similar situation. They have started a public restructuring procedure and agreed with their creditors to renegotiate their debt over a maximum of ten years. They have very similar repayment schedules. Firms in safeguard

Figure 4: Transition matrices of firm ratings around safeguard flag removal



Note: Figure 4 shows the proportion of rated firms in safeguard at each quarter of the plan around the flag removal. We can see that 81% of firms in safeguard are rated 5 in the 11th quarter of their plan. This proportion drops to 35% after flag removal in the 12th quarter.

have their semi-automatic credit rating changed after three years (12th quarter of the plan), while firms in receivership have their rating changed after five years (20th quarter of the plan). This distinction is at the core of my strategy.

4.1 Intensive Margins: Credit variation

I am interested in variation in credit. Following [Amiti and Weinstein \(2018\)](#), I measure the percentage change²⁰ in credit relative to a base period. I study the change in credit at each quarter relative to the firm's average level of total credit the year before it filed for bankruptcy:

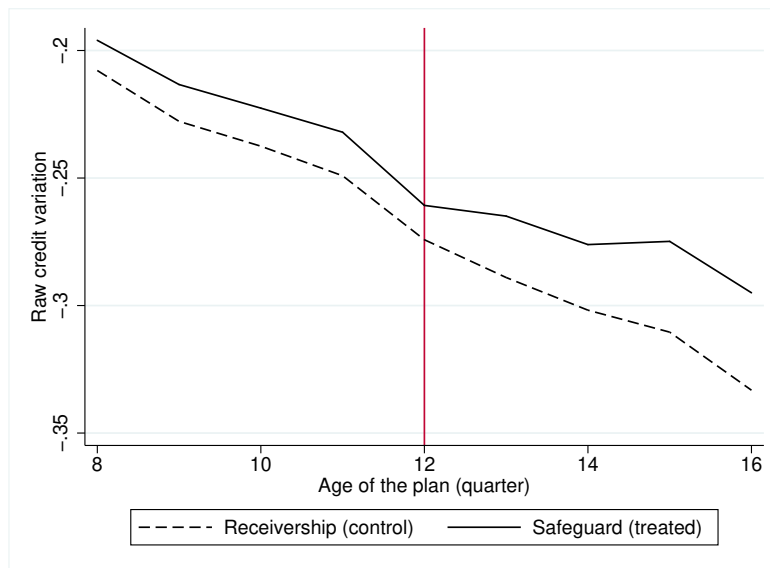
$$\% \Delta Credit_{i,q} = \frac{Credit_{i,q} - \overline{Credit}_{i,q=pre-bnkcy}}{\overline{Credit}_{i,q=pre-bnkcy}} \quad (1)$$

where $Credit_{i,q}$ is the total amount of credit (short-term plus long-term)²¹ at quarter

²⁰Which is preferable to a log change given the formation and termination of lending relationships.

²¹This study will only focus on the outstanding amount of credit, with no analysis by credit maturity. The reason for this is that when a firm files for receivership, banks must register its loans as doubtful or compromised. Credit will be reported as short-term regardless of the initial maturity. In the event of a safeguard procedure, the bank must assess whether there are grounds for declaring loans doubtful or compromised. If the bank deems the loan sufficiently viable, it will keep its initial maturity; otherwise,

Figure 5: Average credit growth for safeguard and receivership firms around safeguard flag removal



Note: Figure 5 shows the raw quarterly average credit variation for firms around flag removal for treated firms (in safeguard, solid line) and controls firms (in receivership, dashed line). The variation in credit is $\% \Delta Credit_{i,q}$, the quarterly variation in total credit compared with the firm’s average level of credit the year before it filed for bankruptcy.

q , for firm i with all of its banks. $\overline{Credit}_{i,q=pre-bnkcy}$ is the quarterly average over all banks of the total credit of firm i the year before the procedure.

Figure 5 shows the raw variation in credit for treated and control firms. Credit decreases as firms execute their restructuring plan (i.e., repay their debt). The repayment schedule follows a regular rhythm for both treated and control firms. On average, firms have repaid 26.4% of their debt after three years, or a repayment schedule of about 9.5% per year, or 2.4% per quarter.

Difference-in-difference strategies require that the credit trends of the treatment and control groups would have been identical in the absence of flag removal. From a visual analysis, we can see that the pre-treatment trends are strictly similar for both groups. After flag removal, the debt of treated firms decreases at a slower pace compared with control firms and the previous trend. The following econometric analysis shall confirm the existence of a significant effect of flag removal.

it will be reported as short-term. Once the restructuring plan starts in receivership and safeguard, the loan may not return to its initial maturity. The hazards generated by the reporting rules prevent any analysis by maturity.

Identification strategy

For my baseline specification, I compare the credit variation of treated and control firms four quarters before and four quarters after flag removal. I estimate the difference-in-difference equation:

$$\% \Delta Credit_{i,q} = \alpha Post_q + \beta (Post_q \times Treated_i) + \gamma_i + \gamma_{s \times t} + \epsilon_{i,q} \quad (2)$$

where i denotes the firm, t the calendar quarter, q the quarter of the plan, and s the industry. $Post_q$ is a dummy that equals 1 when the firm's plan is older than three years. $Treated_i$ equals 1 for firms in safeguard and 0 for firms in receivership. β is the variable of interest that measures the divergence in the evolution of the dependent variable between the treated and control firms. I control for firm γ_i and industry \times quarter fixed effects $\gamma_{s \times t}$. To avoid serial correlation, I cluster standard errors at the firm level.

Alternatively, I conduct the dynamic analysis at the quarter level:

$$\% \Delta Credit_{i,q} = \sum_{q \neq 12} \alpha_q \mathbb{1}_q + \sum_{q \neq 12} \beta_q (\mathbb{1}_q \times Treated_i) + \gamma_i + \gamma_{s \times t} + \epsilon_{i,q} \quad (3)$$

where $\mathbb{1}_q$ is a dummy for each quarter of the plan. I omit the flag removal period $\beta_q = 12$ so that the other β_q can be interpreted relative to this baseline. It is expected that β_q will not be significantly different from 0 for $q < 12$ to support the parallel trend hypothesis so that β_q for $q > 12$ captures the causal effect of the exogenous flag removal in q .

Results

The result of equation (2) is presented in Table 3. The coefficient β is positive and significant at the 1% level, meaning that on average treated firms experience a 1.69% increase in credit in the year following flag removal. Because firms repay their debt at roughly -2.4% per quarter, the 1.7% increase in credit means that they are not increasing their total level of credit compared with the pre-bankruptcy period. Instead, they continue to reduce it by an average of -0.7% per quarter ($-2.4\% + 1.7\%$).

Table 3: Effect of flag removal on firms' credit

	% Δ Credit
	(1)
Post	-0.00591*** (0.001)
Treated \times Post	0.0169*** (0.005)
Firm FE	✓
Quarter \times Industry FE	✓
Observations	48,518
Adj. R ²	0.926

p-values in parentheses

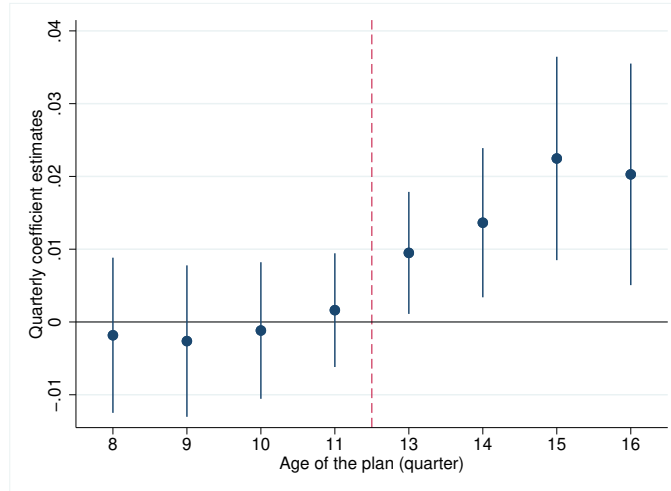
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Note: Table 3 reports the difference-in-difference estimate of equation (2) on the safeguard flag removal effect on firms' credit variation. The dependent variable is the quarterly variation in total credit compared with the firm's average level of credit the year before it filed for bankruptcy. *Treated* takes the value of 1 for safeguard firms, and *Post* takes the value of 1 when the debt-restructuring plan is older than 12 quarters. Firms are tracked from four quarters before flag removal to four quarters after it. Standard errors are clustered at the firm level.

Coefficient estimates β_q of equation (3) are depicted in Figure 6: the total credit of safeguard firms increases substantially compared with firms in receivership for $q \geq 13$. The flag removal effect grows stronger over time, from 0.9% to 2%. For $q < 12$, the point estimates are non-significantly different from 0, meaning there is no systemic relationship between the age of the plan and the variation in credit in the pre-removal quarters. These results provide support for the parallel trends assumption. I provide further discussion of the validity of the parallel trends hypothesis and robustness tests to confirm that the results are robust to introducing time, group, and cohort heterogeneity in Section 4.3.

In summary, I measure a positive causal effect of flag removal in the variation of credit of treated firms. One can argue that this effect is driven by a supply effect more than a demand effect. Indeed, in the case of a change in rating to a non-significant rating (i.e., 0), the Banque de France does not notify firms' managers. It only notifies them when the rating changes to a significant value. In my case study, 82% of treated firms do not receive any notification (29% remain rated 5, and 54% switch to 0) and are therefore unlikely to react directly to bankruptcy flag removal. By dividing treated firms between

Figure 6: Quarterly effect of safeguard flag removal on firms' credit



Note: Figure 6 reports difference-in-difference estimates β_q of the effect of flag removal in safeguard on the variation in firms' credit (see equation (3)). The dependent variable is $\% \Delta Credit_{i,q}$, the quarterly variation in total credit compared to the firm's average level of credit the year before it filed for bankruptcy. Firms are tracked from four quarters before flag removal to four quarters after it. The vertical red line identifies the safeguard flag removal at $q = 12$. Standard errors are clustered at the firm level.

those who receive a significant, good rating from the Banque de France to those who do not, I provide additional evidence²² that the effect is not driven by firms for which the flag removal triggers a notification. These elements are essential to argue that, although the rise in bank credit relies on the demand side from firms, the flag removal effect is mainly driven by a relaxation of bank credit supply constraints.

4.2 Extensive Margins: Probability of forming new banking relationships

Access to bank credit is also reflected in firms' ability to establish new banking relationships. To estimate whether firms' propensity to start borrowing from a new bank is impacted by flag removal, and following Gopalan et al. (2011), I estimate panel logit regressions that are variants of the form:

$$Pr(NewBank_{i,q} = 1) = \alpha Post_q + \beta(Post_q \times Treated_i) + \gamma_i + \gamma_t + \epsilon_{i,q} \quad (4)$$

²²See Table B.3.

where $NewBank_{i,q}$ is a dummy variable that equals 1 before $q = 12$ if the firm starts borrowing from a new bank before $q = 12$, 0 otherwise; and equals 1 after $q = 12$ if the firm starts borrowing from a new bank after $q = 12$, 0 otherwise.

In a first specification reported in Table 4 column (1), I estimate a conditional logit regression that includes firm and quarter fixed effects γ_i and γ_t . The corresponding marginal effect is reported in column (2). The panel logit regression is estimated only on observations for which the dependent variable varies within the period under review. This means that the model focuses only on firms that start a new banking relationship from four quarters before to four quarters after flag removal. However, forming a new banking relationship is a rare event, and limiting the sample to those firms is very restrictive. For this reason, I also estimate in Table 4 column (3) a panel logit regression with no firm fixed effect but with additional controls at the firm level: length of the plan, the firm's size measured with the lagged log of its total assets, the ratios of long-term credit to total credit, and leverage (total debt/ total assets). The financial information is available only for part of the total sample, leading to a loss of observations mainly of the smallest firms. The associated marginal effect is reported in column (4). OLS estimates with fixed effects are reported in column (5).

When restricting the sample to the population of firms that form new banking relationships within the period, I find that the probability of forming a new relationship is 15 percentage points greater after flag removal than before (column (2)). In the total population of firms, flag removal significantly increases the probability of forming new banking relationships, by 4.0 percentage points (column (4)). These results are consistent with the OLS estimates column (5), although the linear estimate is two times smaller (1.9 percentage points). This difference in magnitude may come from the non-linearity of the specification or the sample size. This effect of 2 to 4 percentage points is substantial given that the unconditional propensity of starting a new banking relationship at a given quarter for firms in safeguard is 2.6%. This result is larger but in line with the literature, where [Cahn et al. \(2023\)](#) find that a rating surprise for healthy and well-rated firms leads to a greater probability of starting a new banking relationship of 0.8 percentage points.²³

²³[Cahn et al. \(2023\)](#) report a quarterly probability of starting a new banking relationship of 6% on

Table 4: Effect of flag removal on firms' probability of starting a new banking relationship

	Pr(New Bank)				
	(1)	(2)	(3)	(4)	(5)
	Logit	Marginal effect	Logit	Marginal effect	OLS
Post	-0.179*** (0.004)		0.0501 (0.426)		-0.00273 (0.200)
Treated \times Post	0.605*** (0.000)	0.150*** (0.000)	0.343*** (0.003)	0.0395*** (0.000)	0.0186** (0.038)
Log($assets_{t-1}$)			0.415*** (0.000)		
Length of the plan (years)			-0.0295 (0.244)		
Long term/Total credit			-0.612*** (0.000)		
Leverage			-0.0799** (0.034)		
Firm FE	✓	✓	✓	✓	✓
Quarter FE	✓	✓			
Quarter \times Industry FE			✓	✓	✓
Observations	6,299	6,299	27,272	27,272	48,518
Adj. R ²					0.425

p-values in parentheses

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

Note: Table 4 reports difference-in-difference estimates of the effect of flag removal on the probability of forming new bank lending relationships (see equation (4)). The dependent variable $Pr(NewBank_{i,q})$ equals 1 before $q = 12$ if the firm starts borrowing from a new bank before $q = 12$, 0 otherwise; and equals 1 after $q = 12$ if the firm starts borrowing from a new bank after $q = 12$, 0 otherwise. Columns (1) and (3) report the results of the logit model, (2) and (4) their respective marginal effects, and column (5) a linear probability model. *Treated* takes the value of 1 for firms in safeguard, and *Post* takes the value of 1 when the debt-restructuring plan is older than 12 quarters. Firms are tracked from four quarters before flag removal to four quarters after it. Standard errors are clustered at the firm level.

4.3 Robustness Tests

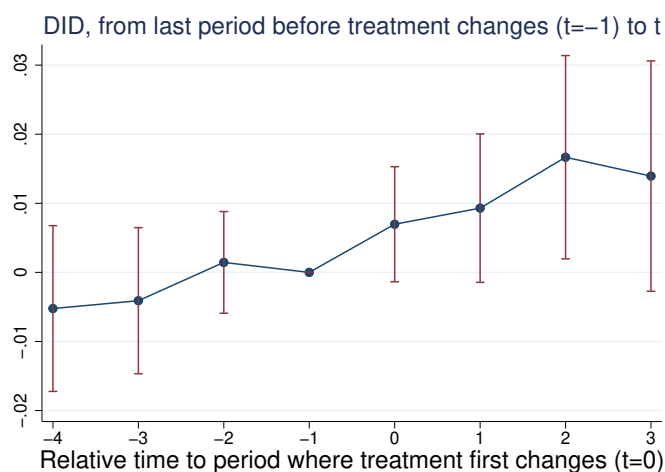
TWFE robustness to heterogeneous treatment effect

As pointed out by [Roth et al. \(2023\)](#) and [De Chaisemartin and D’Haultfoeuille \(2022\)](#) in their review of the latest difference-in-difference methodology literature, Two-Way Fixed Effects estimations are valid in specific conditions: when the parallel trends assumption is met, everyone is treated at the same time, and the effect is homogeneous between groups and over time. In the case of treatment effect heterogeneity or invalid parallel trends, the TWFE method can suffer from serious pitfalls that may invalidate the results. In this section and Appendix [B.1](#), I address most of them using statistical tests suggested by the recent literature.

Heterogeneous treatment effect. I test my estimates’ validity by implementing robust methods for possible time or group heterogeneity. Following [De Chaisemartin et al. \(2019\)](#), I implement the DID design `did_multipligt`, which is robust to dynamic treatment effects under the parallel trends assumption. This estimator is represented in [Figure 7](#) and is a weighted average that is unbiased under heterogeneous and dynamic effects. The figure shows that the coefficients pre-treatment are not statistically different from 0, as per parallel trends hypothesis. Following the treatment, the coefficients are positive and statistically different from zero at the 5% level at $t + 3$, and at the 10% level at $t + 1$, $t + 2$, and $t + 4$. The results are thus robust to introducing time and group heterogeneity.

Challenging the parallel trends hypothesis. My results so far rely on the validity of the parallel trends assumption. Why would the control and treated groups have similar trends in my setup? One could argue that firms that file for safeguard differ from those that file for receivership and that unobserved differences may prevent the parallel trend assumption from holding. For instance, and as described in [Epaulard and Zapha \(2022\)](#), there may be a selection bias at the onset of the procedure. Managers that file for safeguard do so voluntarily as a preventive means to avoid insolvency, while the receivership procedure is mandatory for already insolvent firms. Managers that choose safeguard over receivership may possess managerial skills or other unobservable average for healthy firms.

Figure 7: Time- and group-robust DID estimator



Note: Figure 7 shows that the coefficients are robust to introducing time and group heterogeneity.

characteristics that I need to account for, as they may lead to differences in outcome.

Although it is impossible to test the parallel trends *per se*, mitigating the concern that a selection bias may invalidate them is possible. First, the use of firm fixed effects in the baseline specification controls for unobservable characteristics that do not change over time. The selection bias needs to have time-varying implications to potentially disrupt my results. Another example would be a change in management that could undermine the parallel trends hypothesis. However, by looking at the repayment behavior of firms, I note in Figure B.5 that the safeguard and receivership repayment plans of a small sample of firms appear to be strictly identical on average. This descriptive evidence illustrates that the two groups of firms follow the same trajectory.

Also, [Epaulard and Zapha \(2022\)](#) have proven several interesting points. First, we have shown in this previous work that the selection bias at the onset of the procedure is not the main driver of the bankruptcy procedure outcome. Second, we have shown that once a debt-restructuring plan has been adopted, the two- and four-year survival rates after restructuring are not impacted by whether the firm was in safeguard or receivership (after exogenous conversion of the safeguard procedure). This previous research provides insight that the procedure does not impact the firm's outcomes once the firm has been restructured, at least in the short run. This result is meaningful in the framework of this

paper, where I study firms after approval of the restructuring plan. Additional tests on the parallel trends hypothesis are described in Appendix B.1, and the following section details the propensity score matching procedure that controls for observable differences in firms' characteristics.

Propensity Score Matching

The parallel trends hypothesis does not require the outcome to be identical across the treated and control groups, as the estimation differences out any time-invariant disparities. Nevertheless, in the following analysis, I correct for treated and control firms' heterogeneity in observable characteristics with a matching procedure.

I perform a nearest-neighbor matching method that minimizes the Mahalanobis distance between firms' characteristics. To do so, I select two matched control firms for each treated firm, with the possibility for control firms to serve as matches more than once to reduce the estimation bias (although it increases the variance). Following the literature that uses matching methods in finance ([Almeida et al. \(2017\)](#), [Chernenko and Sunderam \(2012\)](#), [Lemmon and Roberts \(2010\)](#)), I match treated and control firms based on their financial characteristics the year before they filed for bankruptcy. The categorical variables include the year of adoption of the plan, the firm's industry, the region of its headquarters, and whether the firm had a significant rating prior to bankruptcy (i.e., other than 0). The non-categorical variables include the length of the plan, the firm's size, investment, cash, leverage, and the share of long-term debt. All financial variables are winsorized at the 1% percentiles at both tails. The matching quality is good and discussed in Appendix B.2.

Table 5 presents the results of the linear difference-in-difference estimations on the matched sample. The dependent variable column (1) is the variation in credit, and column (2) is the probability of forming new banking relationships. The results are statistically significant at the 1% and 5% levels and in line with the previous section. The treated firms increase credit by 2.6% and have a 3.3 percentage point higher chance of forming new banking relationships. The matching estimators are larger than previously (0.0263 compared to 0.0169 in Table 3, and 0.0331 compared with 0.0186 in Table 4, column (5)), suggesting that differences between treatment and control

groups may lead to an underestimation of the effect. Still, the PSM estimate should be considered cautiously as it significantly reduces the sample size for firms for which financial information is available, and according to [King and Nielsen \(2019\)](#), greatly increases the risk of sample imbalance.

Table 5: Effect of flag removal on firms' access to credit on matched sample

	% Δ Credit	Pr(New Bank)
	(1)	(2)
Post	-0.0147*** (0.004)	-0.0196*** (0.008)
Treated \times Post	0.0263*** (0.003)	0.0331** (0.022)
Firm FE	✓	✓
Quarter \times Industry FE	✓	✓
Observations	12,193	12,193
Adj. R ²	0.909	0.446

p-values in parentheses

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

Note: Table 5 reports difference-in-difference results of flag removal on the matched sample. Treated firms are safeguard firms; control firms are matched receivership firms. The dependent variable column (1) is the variation in credit, and column (2) is the probability of forming new banking relationships. Both regressions are OLS. Standard errors are clustered at the firm level.

5 Mechanisms: Discussing Banks' Behavior

Following bankruptcy flag removal, firms increase their access to bank credit. However, in France, information on past bankruptcies remains public. A simple query on the BODACC website provides access to the entire history of firms' bankruptcy rulings since 2008. I argue that the effect I measure is driven by a supply effect, but even if it was a demand effect, in both cases banks provide more credit despite the fact that the bankruptcy information remains freely available. How can we explain this behavior?

An initial explanation would be that banks are naive and do not seek information beyond the Banque de France credit rating. Since firms are small and the credit amounts relatively low, bank monitoring may be less thorough. [Cahn et al. \(2021\)](#) explores this possibility by questioning a similar behavior: banks lend more to bankrupt entrepreneurs

after their flag is removed, even though information on their past bankruptcy remains available at low cost. The representativeness heuristic may explain this irrational behavior: banks rely more heavily on credit ratings than on external information (Gennaioli and Shleifer (2010), Kahneman and Tversky (1972)). This behavior is all the more likely for banks that are less well informed about the firm.

The first part of the following section tests this assumption by looking at the reaction of less well-informed lenders to flag removal. The second part further questions the rationale behind lending to restructuring firms.

5.1 Who lends?

Supply from new lenders

Where does the new credit come from? First, I break down credit between new and historical lenders with the following dependent variables:

$$\% \Delta NewBankCredit_{i,q} = \frac{NewBankCredit_{i,q}}{Credit_{i,q=pre-bnkcy}}$$

and

$$\% \Delta Hist.BankCredit_{i,q} = \% \Delta Credit_{i,q} - \% \Delta NewBankCredit_{i,q}$$

where $NewBankCredit_{i,q}$ is the credit borrowed from new lenders: a lender is considered new after flag removal if the firm starts borrowing from it at quarter $q > 12$ and has never borrowed from it before.²⁴ $HistBankCredit_{i,q}$ is the credit supplied by banks that were lending to the firm before flag removal. I estimate equation (2) on these subsets of credit; the results are depicted in Table 6.

Firm credit increases by 1.35% due to an increase in historical banks' supply, and by 0.25% thanks to the formation of new banking relationships. This means new banking relationships formed after flag removal account for only 15% of the total increase (=0.00255/0.0169, see Table 3).

²⁴I examine all of the firms' previous loans since 2004. I consider the firm-bank relationship at the bank level, not the local branch level. Because of the collection threshold of €25K, the occurrence of a new banking relationship in the data may stem from the fact that the firm-bank relationship previously existed but the total credit exposure was below the threshold.

When looking at the profile of the new lenders, I find that 96% of them were already lending to restructuring firms in my sample.²⁵ This means that 39% of banks already lending to restructuring firms before flag removal chose to lend to new firms undergoing restructuring after flag removal.

Table 6: Credit variation by lender type

	% Δ New	% Δ Hist.
	(1)	(2)
Post	0.00144*** (0.000)	-0.0102*** (0.000)
Treated \times Post	0.00255** (0.011)	0.0135*** (0.008)
Firm FE	✓	✓
Quarter \times Industry FE	✓	✓
Observations	48,518	48,518
Adj. R ²	0.373	0.919

p-values in parentheses

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

Note: Table 6 reports a difference-in-difference estimate of the effect of flag removal in safeguard on the variation in firms' credit divided between the new bank credit column (1) and the historical bank credit column (2). Standard errors are clustered at the firm level.

Historical lender heterogeneity

Next, I want to challenge historical lenders' behavior based on their level of information. We cannot observe the level of information between a firm and its bank directly; the empirical literature on relationship banking has relied on indirect, data-driven measures to infer proxies (see e.g., Degryse and Ongena (2008), Harhoff and Körting (1998)). Following this literature, I consider two measures of information:

- **GEOGRAPHICAL PROXIMITY:** the greater the physical distance between a firm and its banks, the higher the monitoring costs (Bolton et al. (2016), Agarwal and Hauswald (2010), Degryse and Ongena (2005), DeYoung et al. (2008)). In the following test, a lender has a low (resp. high) level of information when its

²⁵My sample includes 271 banks. 108 banks lend after flag removal, of which only four did not lend to any firm in my sample prior to flag removal.

local branch and the firm's headquarters are not (resp. are) in the same French department.

- **SHARE IN FIRM'S TOTAL CREDIT:** the main lender possesses more information about the firm than other lenders (Rajan (1992), Petersen and Rajan (1994)). I define the bank with the largest share of the firm's total credit as the main lender, with a high level of information, whereas secondary lenders (all other banks) have a low level of information.

Under the hypothesis that less well-informed banks rely more on the Banque de France credit rating, they should react more to bankruptcy flag removal. Amongst the historical lenders, I break down the credit between credit from lenders with a high level of information, denoted $Credit_{i,q}^H$, and credit from lenders with a low level of information, denoted $Credit_{i,q}^L$.

$$\% \Delta Credit_{i,q}^H = \frac{Credit_{i,q}^H - \overline{Credit}_{i,q=pre-bnkcy}^H}{\overline{Credit}_{i,q=pre-bnkcy}^H}$$

and

$$\% \Delta Credit_{i,q}^L = \% \Delta Hist.BankCredit_{i,q} - \% \Delta Credit_{i,q}^H$$

with $Hist.BankCredit_{i,q} = Credit_{i,q}^H + Credit_{i,q}^L$, and $\overline{Credit}_{i,q=pre-bnkcy}^H$ is the quarterly average of credit from highly informed lenders the year before bankruptcy. I estimate equation (2) on these subsets of credit and report the results in Table 7. I find that coefficient estimates β in columns (1) and (3) are not significantly different from zero: well-informed lenders do not increase their credit supply after flag removal. There is a significant increase in credit from less well-informed lenders, defined as banks whose local branches are not in the same French department as the firm's headquarters (column (2)). Well-informed and less well-informed lenders defined by their share in firms' credit seem to behave in the same way (columns (3) and (4)).

In summary, and in line with the idea that less well-informed lenders rely more on the Banque de France credit rating, the increase in credit supply comes from new banks and less well-informed historical banks.

Are these banks naive? It is difficult to simply conclude that banks are behaving

Table 7: Credit variation by historical lenders' level of information

Dependent variable	Credit variation split amongst hist. banks			
	Geo. proximity		Share in firms' credit	
	High (1)	Low (2)	High (3)	Low (4)
Post	-0.00693*** (0.000)	-0.00627** (0.038)	-0.00395*** (0.009)	-0.00845** (0.044)
Treated \times Post	0.00331 (0.420)	0.0101** (0.023)	0.00708 (0.185)	0.00719 (0.288)
Firm FE	✓	✓	✓	✓
Quarter \times Industry FE	✓	✓	✓	✓
Observations	48,518	48,518	48,518	48,518
Adj. R ²	0.973	0.964	0.871	0.944

p-values in parentheses

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

Note: Table 7 reports difference-in-difference estimates of flag removal depending on historical banks' level of information. Credit variation from less well-informed lenders is reported in columns (2) and (4). In column (2), the bank's local branch and the firm's headquarters are not located in the same French department. In column (4), the firm-bank relationship is not the firm's main relationship. Standard errors are clustered at the firm level.

irrationally. According to [Padilla and Pagano \(2000\)](#), information on creditors' past defaults, let alone past bankruptcies, is among the most easily shared and observable by lenders. To challenge the idea that banks are naive, I explore other reasons why banks would increase their credit supply to restructuring borrowers in the following section.

5.2 Why lend?

It is not mutually exclusive to think that banks have information about past bankruptcy but still rationally decide to lend. The literature makes a number of points about why banks would lend to distressed borrowers.

On the one hand, the literature on bank lending finds that banks have an interest in lending to new firms. Theory from [Sharpe \(1990\)](#), [Rajan \(1992\)](#) and [Von Thadden \(2004\)](#), empirically validated by [Ioannidou and Ongena \(2010\)](#), shows that banks derive rent from their informational advantage on client firms that allow them to offer lower-rate credit to new borrowers. New borrowers may be less experienced and of poorer quality. Nevertheless, the strategy is viable as long as the new credit supply enables the acquisition of sound new borrowers.

On the other hand, many theoretical and empirical papers predict that historical banks support their clients in financial distress through adjusted interest rates or collateral requirements.²⁶ However, some empirical papers reveal that this financial support is not systematic in the case of strong bank-firm relationships.²⁷ This may be true if, thanks to the collection of soft information, the best-informed lenders judge firms to be non-viable.

The following test aims to shed some light on why banks would increase their lending supply according to borrowers' viability. In my setup, I can easily account for borrowers' quality by using the credit rating after flag removal.

In addition to assessing the viability of firms, the Banque de France's credit rating is

²⁶See [Rajan \(1992\)](#), [Schäfer \(2019\)](#), [Bolton et al. \(2016\)](#), [Peek and Rosengreen \(2005\)](#), [Rosenfeld \(2014\)](#) to cite a few. Similarly, [Micucci and Rossi \(2017\)](#) found that debt-restructuring of SMEs is more likely with relationship banks.

²⁷[Huang et al. \(2015\)](#) find that banks with informational advantages decrease the probability of debt restructuring. [Li et al. \(2019\)](#) shows that the share of bank loans granted by relationship banks decreases in the case of distress. [Salvadè et al. \(2022\)](#) find that banks with private information terminate their relationships with firms when – and long before – they approach default.

also used to determine the eligibility of firms' loans as collateral with the ECB. Under the bankruptcy flag, restructured loans are not eligible.²⁸ With flag removal, the loans of 5.8% of safeguard firms become newly eligible. Beyond the regulatory implications, firms with an eligible credit rating are simply those of the highest quality. Assuming that banks lend to firms after flag removal with a view to acquiring or retaining viable borrowers, we should observe a greater effect of flag removal for firms whose loans become eligible for ECB collateral.

In my baseline specification, I identify firms with eligible ratings by a quarterly dummy $Eligible_{i,q}$ equal to 1 if the rating is eligible at quarter q , 0 otherwise. I distinguish the effect of receiving a newly-eligible rating by the interaction term $Eligible \times Post$. To better understand banks' motives, I reproduce the estimation on the subsets of credit by lender type as defined earlier. The results are reported in Table 8.

Table 8: Flag removal's impact on newly-eligible firms by lender type

Dependent variables	All		Credit variation split by lender type				
	% Δ Credit	% Δ New	% Δ Hist.	Amongst Hist. Banks			
				Geo. Proximity		Share in firm's credit	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Level of information			High	Low	High	Low	
Post	-0.00598*** (0.001)	0.00144*** (0.000)	-0.0103*** (0.000)	-0.00695*** (0.000)	-0.00631** (0.037)	-0.00852** (0.043)	-0.00398*** (0.008)
Treated \times Post	0.0145** (0.014)	0.00236** (0.017)	0.0118** (0.021)	0.00304 (0.466)	0.00821* (0.065)	0.00674 (0.326)	0.00611 (0.255)
Eligible \times Post	0.125** (0.017)	0.00540 (0.460)	0.106** (0.024)	0.0198 (0.111)	0.0901** (0.035)	0.0492 (0.153)	0.0492** (0.019)
Firm FE	✓	✓	✓	✓	✓	✓	✓
Quarter \times Industry FE	✓	✓	✓	✓	✓	✓	✓
Observations	48,518	48,518	48,518	48,518	48,518	48,518	48,518
Adj. R ²	0.926	0.373	0.919	0.973	0.964	0.944	0.871

p-values in parentheses

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

Note: Table 8 reports difference-in-difference estimates of flag removal (equation (2)) on different subsets of credit as defined earlier. Standard errors are clustered at the firm level.

First, I observe in column (1) that the effect of flag removal on the total variation in credit is robust for firms whose rating is not newly eligible: their credit increases by

²⁸Eligible ratings are from 3++ to 4, see Table 1. While restructured firms in receivership cannot receive an eligible rating, in safeguard, eligibility is possible under strict conditions before flag removal (see Section 2.2). This concerns only 0.5% of firms.

1.45%. I find that firms with newly-eligible ratings obtain up to ten times more credit than firms with non-eligible ratings: their credit increases by 12.5%.²⁹ By comparing the coefficient associated with firms whose rating is non-eligible (0.0145) with the baseline coefficient (0.0169, see Table 3), I can conclude that the 6% of firms with newly-eligible ratings account for 14% of the total effect ($= (0.0169 - 0.0145) / 0.0169$). Eligibility explains part of the main effect but not the whole story.

Then, in column (2), I observe that credit from new banks is only supplied to firms whose rating is not eligible. In column (3), I see that credit from historical banks is supplied both to firms with newly-eligible and non-eligible ratings. When I look at the details of credit supplied by historical lenders, I see that less well-informed banks, due to geographical distance, lend to both newly eligible and non-eligible rated firms (column (6)). Less well-informed banks, due to their smaller share in the firm's total credit, increase their lending to newly-eligible firms only (column (7)). Highly-informed lenders never increase their lending supply (columns (4) and (6)).

One possible way of understanding these results is that thanks to their newly-eligible rating, viable firms can increase credit with their historical banks; they do not need to form new banking relationships. In line with the idea that the best-informed banks may not always support their distressed borrowers, only less well-informed lenders provide restructuring firms with more credit. It could be that, despite the eligible rating, the best-informed lenders judge the firm as non-viable based on their soft information. It could also result from a reputational effect: banks may refuse new loans to their defaulting borrowers to enhance their reputation (Sharpe (1990)).

Finally, firms with non-eligible ratings find stronger credit restrictions from their historical banks and therefore rely more on new banking relationships: new bank credit accounts for up to 30% of their increased credit supply (compared with 15% previously).

²⁹One should keep in mind that in the case of a change in rating to a significant rating (i.e., other than 0), the Banque de France notifies managers of the change. Therefore, the $\text{Post} \times \text{Eligible}$ estimate may be driven by a more substantial demand effect than for other firms.

6 Firm Performance and Economic Outcomes

This section assesses the impact of flag removal and increased access to bank credit on firms' economic performance. An extensive literature finds that by mitigating firms' constraints, credit ratings have a significant impact on firm financing and real outcomes.³⁰ These analyses, however, focus mainly on large firms, whereas I focus on bank-dependent SMEs.

Studying firms' performance is essential as it provides insights into the real economic impact of flag removal, but this analysis is challenging for two reasons. The first is the availability of data, which is annual and only available for about two-thirds of the firms in my sample.³¹ The second is the time frame of the analysis: to avoid too much attrition, I focus on the year following flag removal. However, the real effect may take some time to appear. This analysis highlights some initial results, but we should keep in mind that the real effects may take some time to fully appear.

I estimate a two-period difference-in-difference in firms' financial and economic outcomes. I consider the year before and after flag removal and control for year \times industry fixed effects as follows :

$$Y_{i,a} = \alpha Post_a + \beta(Post_a \times Treated_i) + \gamma_i + \gamma_{s \times t} + \epsilon_{i,a} \quad (5)$$

where $Y_{i,a}$ is the economic outcome of firm i , a denotes the restructuring plan's age in years and t calendar years. As before, standard errors are clustered at the firm level.

Table 9 presents the results. First, I test for variables that can shed light on the cost of credit. Ideally, I would like to observe the interest rate at which firms take out their new credit, but I do not have that information. Instead, I look at the impact of flag removal on the apparent cost of financial debt. Results column (1) is not different from zero. This is not so surprising: the shock of flag removal is not so great as to impact

³⁰The reduction in financial and capital constraints is possible via the reduction in credit market information asymmetry. Credit ratings can then trigger changes in firms' leverage and capital structure (Kisgen (2006), Tang (2009), Sufi (2009), Faulkender and Petersen (2005)), real outcomes and investment decisions (Lemmon and Roberts (2010), Chernenko and Sunderam (2012), Harford and Uysal (2014)). Conversely, bad ratings can cause a rise in the cost of debt (Almeida et al. (2017), Klinger and Sarig (2002)).

³¹The smallest firms are marginally more impacted: micro-enterprises account for 80% of the initial sample and 86% of attrition.

Table 9: Effect of flag removal on firms' outcomes

	Financial outcomes			
	(1)	(2)	(3)	
	Apparent Cost of Debt	Leverage	Supplier Debt/ Total Debt	
Post	-0.0535*	-0.00143	0.00208	
	(0.055)	(0.925)	(0.431)	
Treated \times Post	0.0133	0.00774	-0.00770***	
	(0.571)	(0.650)	(0.006)	
Firm FE	✓	✓	✓	
Year \times Industry FE	✓	✓	✓	
Observations	7,681	8,097	8,821	
Adj. R ²	0.741	0.899	0.887	

	Economic outcomes			
	(1)	(2)	(3)	(4)
	Margins	Investment	Δ Turnover	Δ Employment
Post	-0.00418	-0.00191	0.00762	0.0199
	(0.352)	(0.446)	(0.434)	(0.191)
Treated \times Post	0.000570	0.00535**	0.00717	-0.000470
	(0.913)	(0.027)	(0.503)	(0.977)
Firm FE	✓	✓	✓	✓
Year \times Industry FE	✓	✓	✓	✓
Observations	8,202	9,632	7,859	8,303
Adj. R ²	0.677	0.420	0.886	0.768

p-values in parentheses

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

Note: Table 9 reports difference-in-difference results of flag removal on firms' annual outcomes (equation (5)). The top panel presents the results on financial variables; Apparent Cost of Debt = Interest Expenses / Financial Debt. Leverage = Total Debt / Total Assets. The bottom panel presents the results on economic variables; Profit Margins are the Gross Operating Profit Margins = (Value-Added - Staff Cost) / Revenue; Investment = (Tangible + Intangible) / Lagged Total Assets. $\% \Delta$ Turnover and $\% \Delta$ Employment are changes relative to the pre-bankruptcy period. All variables are winsorized at the 2% percentiles at both tails. Standard errors are clustered at the firm level.

all the firm's debts. I am thus not able to ascertain the impact of flag removal on the interest rates faced by firms.

I then test columns (3) and (4) for firms' total leverage and debt composition, particularly supplier debt. The literature has widely discussed the importance of trade credit as a source of external funding for firms. Supplier financing accounts for a large share of small firms' funding,³² and even more so for constrained SMEs.³³ Also, the information held by suppliers is different from that of banks and is acquired through business transactions.³⁴ In our case, suppliers do not have access to the Banque de France rating. They are therefore not expected to react directly to the change in credit rating. Rather, their reaction could be the indirect consequence of the change in the firm's access to bank credit.

The results in column (4) show that flag removal decreases the share of supplier debt in total debt significantly, by 0.77 percentage points, or 4% ($=1-(0.185-0.0077)/0.185$, see Table 2). Also, in column (3), total leverage is not impacted; the overall debt level of treated firms does not increase despite the rise in bank credit, nor decrease despite the decrease in supplier debt. These results seem to suggest that, since there is no change in leverage, the mix of debt has been impacted: treated firms substitute part of their supplier debt for bank debt.

I now turn to economic outcomes, starting with investment. We expect that the relaxation of the financial constraints will result in higher investment. The bottom panel of Table 9, column (2), shows that this is the case: the investment rate of treated firms significantly increases, by 0.53 percentage points, from 3.1% to 3.6%.³⁵ I do not find any effect of flag removal on profit margins (column (1)). Again, the timescale may be too short to observe any substantial change. I do not see a significant impact

³²In my sample, I find that supplier debt accounts for 18.5% of total debt (see Table 2), lower than what [Murro and Peruzzi \(2022\)](#) find in the U.S. and [Uchida et al. \(2013\)](#) in Japan (32% and 30% respectively).

³³See [Carbó-Valverde et al. \(2016\)](#) and [Forier et al. \(2021\)](#) for empirical evidence. [Petersen and Rajan \(1997\)](#) discuss the fact that suppliers would have an interest in lending to potentially low-quality firms because, on the one hand, they can extract rent from otherwise constrained firms. On the other, they may have an interest in keeping them as a going concern. This is especially true if the supplier has no substitute for the customer.

³⁴See [Smith \(1987\)](#), [Brennan et al. \(1988\)](#) or [Biais and Gollier \(1997\)](#) for early work on the subject.

³⁵This estimate should be considered with caution as the robustness test in Appendix B.4 suggests it may result from sample selection.

on turnover or employment either (columns (3) and (4)); the coefficient estimates are relatively small and not significantly different from zero.

In summary, flag removal has a moderate but quite positive impact on firms' economic and financial outcomes the year after. The investment rate is boosted when we focus on firms surviving at least four years under restructuring.³⁶ Employment, sales, and profit margins do not change significantly. We see interesting changes in firms' debt composition: firms rely less on supplier credit following flag removal, which is in line with the literature that finds an advantage in suppliers lending to constrained client firms.

7 Conclusion

The Banque de France compiles its credit rating to reduce information asymmetry between banks about firms' repayment capabilities. Indirectly, it also impacts other forms of lending, notably trade credit, which adjusts in response to firms' access to bank credit.

The choice of disclosing or removing past bankruptcy information from credit ratings significantly impacts firms' access to credit and real outcomes. Irrespective of the fact that information on past bankruptcies remains freely available, the credit rating guides bank behavior. This is particularly true of credit ratings that determine firms' loan eligibility for ECB collateral. Policymakers have a tangible way of shaping restructuring firms' access to bank financing by deciding on the timing of bankruptcy flag removal.

In the wake of the 2020 health crisis, many firms – that are otherwise viable – have suffered from fluctuating business conditions and have needed government support, notably in the form of publicly-guaranteed loans, to keep their business afloat. According to [Demmou et al. \(2021\)](#), about 8% of all firms in OECD countries will be threatened by debt overhang and require debt restructuring in the future. In this context, alleviating

³⁶We should keep in mind that I restricted my sample to firms that survive four years after adopting the restructuring plan. As a result, I exclude all firms liquidated within these four years, possibly because of credit constraints or simply because they were not viable. I show in Appendix B.4 that my main results on access to credit and the decrease in supplier debt are robust to sample selection. However, investment and total leverage are not: in Table B.5 I find no effect of flag removal on investment and a positive impact on leverage. The conclusion of this section applies to firms that have survived the first four years of safeguard restructuring.

financial constraints faced by firms after financial distress has become a priority. In France, the timing for flag removal (three years after the agreement of a safeguard plan and five years after the agreement of a receivership plan) was reduced to two years for both procedures in 2020.³⁷ By demonstrating that the economic situation of firms is not worsened by flag removal and the relaxation of credit constraints, I show that this policy is a good idea. In addition, to further ease access to bank credit for these firms, a “post-money” privilege³⁸ has been introduced to encourage banks to lend to restructuring firms by guaranteeing lenders a preferential ranking in the event of failure. These measures are aimed specifically at encouraging and facilitating the credit access of restructuring firms to support their recovery.

³⁷Decree N-2020-106 of February 10, 2020.

³⁸Order No. 2020-596 of May 20, 2020, extended by Order No. 2021-1193 of September 15, 2021.

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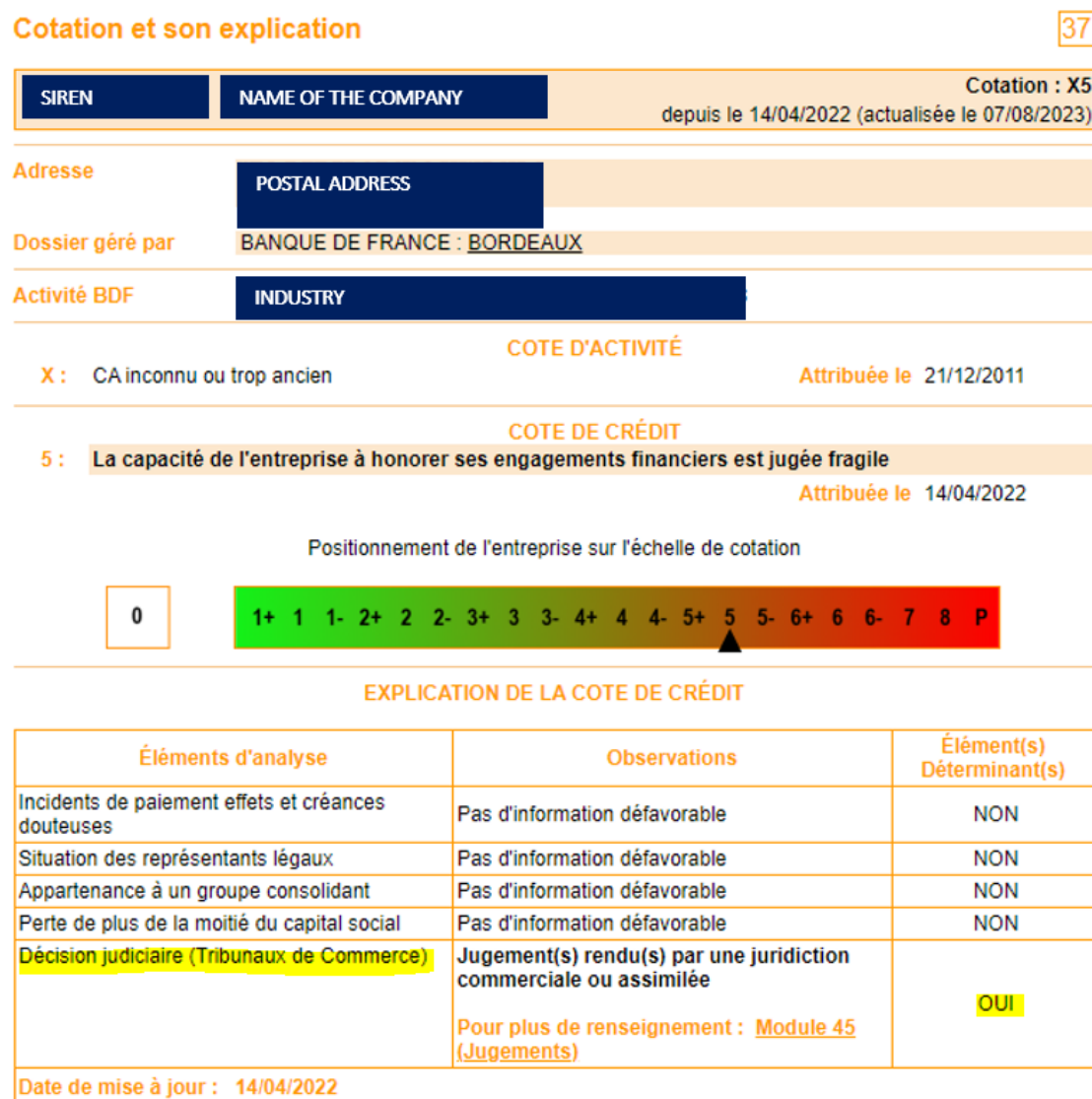
A FIBEN

Figure A.1: FIBEN's module for banks – Details of the judgments

Jugements		45
SIREN	NAME OF THE COMPANY	
Adresse	POSTAL ADDRESS	
Dossier géré par	BANQUE DE FRANCE : BORDEAUX	
JUGEMENT(S) (2)		
Adoption d'un plan de sauvegarde		
Le	13/07/2023	Tribunal BORDEAUX
Jugement ouverture de la procédure de sauvegarde		
Le	08/04/2022	Tribunal BORDEAUX

Note: Figure A.1 details what banks can access by clicking on the “more information” link in Figure 1. This page presents the details of the firm's judgments. This screenshot was taken on 21/08/2023, after the adoption of the new law removing bankruptcy flag two years after the start of a restructuring plan (effective as of 10/02/2020), but remains accurate for this paper. In this example, information about the safeguard decisions will be deleted on 13/07/2025.

Figure A.2: FIBEN's module for banks – Explanation of the credit rating



Note: Figure A.2 details what banks can access by clicking on the “more information” link in Figure 1. This page explains the firm’s rating. In the case of a firm undergoing restructuring, it is said that the decisive factor is the court decision. This screenshot was taken on 21/08/2023 and displays the new rating scale effective as of 01/01/2020 but remains accurate for this paper. If the firm meets its repayments schedule and does not have to file for receivership or liquidation, the information relating to the judgment will be deleted on 07/13/2025, and the credit rating will evolve freely (probably to 0 given that this firm’s turnover is “missing or too old”).

B Robustness

B.1 TWFE Robustness

Following on Section 4.3, this appendix provides additional tests that challenge the baseline results for time, group, and cohort heterogeneity, as well as the parallel trend hypothesis.

Heterogeneous treatment effect

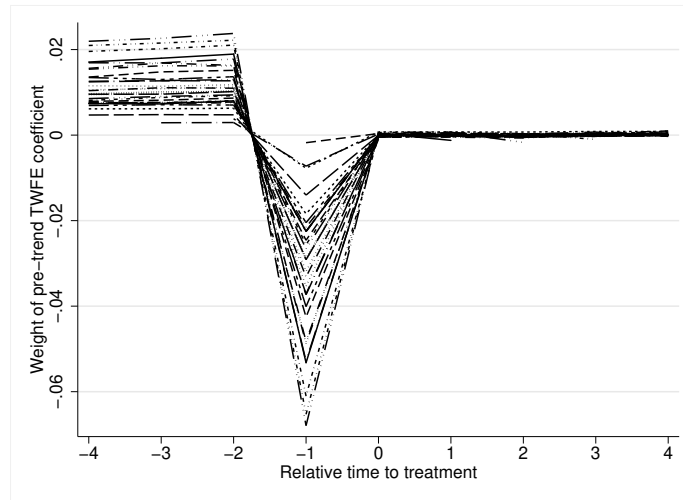
De Chaisemartin and D’Haultfoeuille (2020), Borusyak et al. (2022), and Goodman-Bacon (2021) highlight that TWFE regressions may not always estimate a convex combination of treatment effects in the presence of negative weights. In the worst-case scenario, it could mean that the DID estimates a positive effect even though the treatment effect is strictly negative for every observation. The following section questions whether this could be the case in my framework.

On the one hand and following De Chaisemartin and D’Haultfoeuille (2020) analysis, the probability of having negative weights in my setup is small because (i) the treatment is binary, (ii) there are no always-treated, and no time periods where most groups are treated, and (iii) the `twowayfweight` package by De Chaisemartin et al. (2019) reveals the absence of negative weight in the sample.

On the other hand, I have so far assumed that the treatment effect is homogeneous across units (as an average of unit-level treatment effect). However, my framework has more than two time periods, and units have different treatment timings. Even if my weights are non-negative, they could be poorly estimated in case of time or group heterogeneity. I further test my estimates’ validity by implementing robust methods for possible time or group heterogeneity. Following De Chaisemartin et al. (2019), I first implement the DID design `did_multipligt` that is robust to dynamic treatment effects under the parallel trends assumption. This estimator is represented in Figure 7 in the paper and shows robust results to time and group heterogeneity.

In the same vein and as pointed out by Sun and Abraham (2021), the DID coefficient estimate can be biased, and the parallel trends assumption violated if the treatment effect is heterogeneous across cohorts. To rule out this potential pitfall, I follow Sun and Abraham (2021) and estimate the weight associated with the pre-trend coefficient using the package `eventstudyweights` (Sun (2020)). Figure B.3 shows that the weights have a zero magnitude for lags of treatment. This would imply that the effects are homogeneous across cohorts. This result follows the properties described in Sun and Abraham (2021) and is important as it alleviates the concern that the pre-trend coefficients may be contaminated by treatment effects cohort-heterogeneity and, therefore, invalid.

Figure B.3: Estimated weights underlying pre-trend coefficient



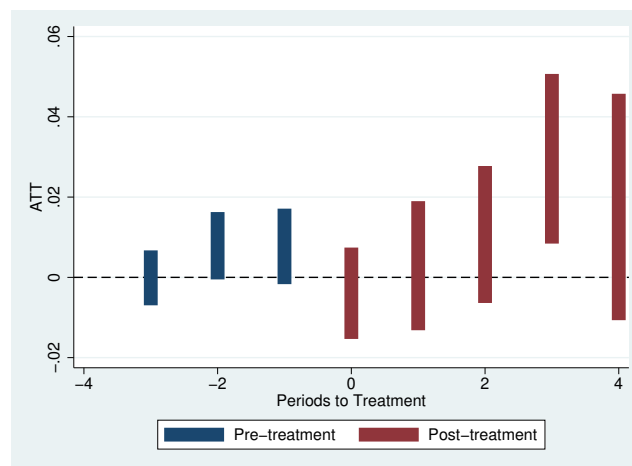
Note: Figure B.3 shows the weights underlying the pre-trend coefficient two quarters from the flag removal. Each line is a different cohort from 2012 Q1 to 2019 Q4.

Parallel trends hypothesis

To corroborate the parallel trend hypothesis, I next turn to statistical tests described in the literature. In my model, I introduce firm fixed effects that control for any time-unvarying factors at the individual level. The last remaining concern is that there subsist time-varying characteristics that may have differentially affected the outcome of the treated group. I follow [Callaway and Sant’Anna \(2021\)](#) and their package `csdid` to test for the plausibility of the parallel trend assumption. One important takeaway of this paper is that the parallel trend may hold conditioned on observed covariates. I perform their suggested test to ponder whether my parallel trend assumption is valid or better off conditioned on observed covariates.

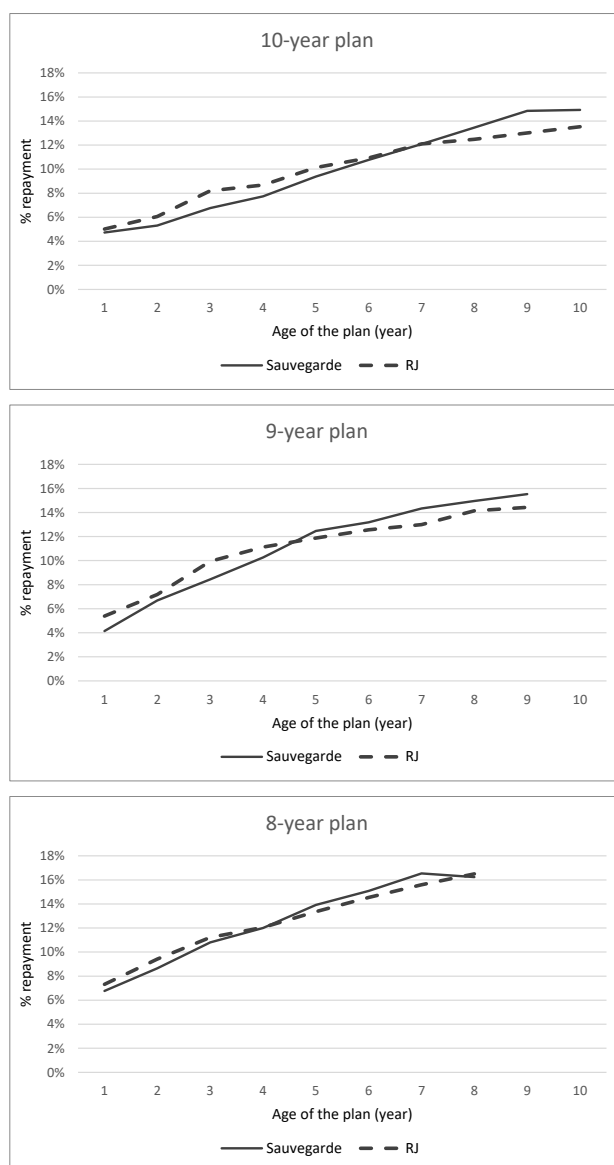
Without covariates, the Cramér-von Mises test for the parallel trend rejects the parallel trend assumption at the 10% level ($p\text{-value} = 0.0743$). When I introduce calendar time \times industry fixed effects as covariates, the results illustrated in [Figure B.4](#) confirm the parallel trend hypothesis that the pre-trends coefficients are not statistically different from zero at the 10% level. [Figure B.4](#) further confirms the robustness of my result to dynamic treatment effects: the coefficients post-treatment are positive and significant at the 10% level at $t + 3$, as already suggested by the previous heterogeneity-robust tests. Conditioned on calendar time \times industry fixed effects, the Cramér-von Mises test indicates a $p\text{-value}$ of 0.0929 for the parallel trends plausibility test. Thus, my setup seems better off with calendar time \times industry fixed effects as covariates, which are included in all specifications.

Figure B.4: Heterogeneity-robust DID estimates under conditional parallel trends



Note: Figure B.4 confirms the robustness of the results to time and group heterogeneity under the parallel trend assumption conditioned on calendar time \times industry fixed effects.

Figure B.5: Plan repayment schedules at different horizons



Note: Figure B.5, from [Despierre et al. \(2018\)](#) analysis, shows repayment schedules of a small sample of firms that filed for safeguard and receivership in the Commercial Court of Paris between 2006 and 2015. The sample contains 59 safeguard filings and 78 receivership filings that obtained a debt-restructuring agreement with the detail of their yearly repayment prevision: 13 safeguard firms and 27 receivership firms that obtained a debt-restructuring plan at the 10-year horizon, 11 safeguard firms and 31 receivership firms at the 9-year horizon, and 13 safeguard firms and 12 receivership firms at the 8-year horizon. At the 10-year horizon, we observe that firms start repaying an average of 5% per year from the first year until 15% in the tenth year. The same increasing pattern is followed for firms with a shorter plan. Most importantly, the safeguard and receivership's repayment schedules are strictly identical in this sample.

B.2 Propensity Score Matching

This section details the matching procedure that aims at controlling for treated and control firms' observable heterogeneity.

As mentioned in the paper and following the literature that uses matching methods in finance (Almeida et al. (2017), Chernenko and Sunderam (2012), Lemmon and Roberts (2010)), I match treated and control firms based on their financial characteristics the year before they filed for bankruptcy. The categorical variables include the year of adoption of the plan, the firm's industry, the region of its headquarters, and whether the firm had a significant rating prior to bankruptcy (i.e., other than 0). The non-categorical variables include the length of the plan, the firm's size, investment, cash, leverage, and the share of long-term debt. All financial variables are winsorized at the 1% percentiles at both tails.

The first three columns of Table B.1 present means, standard errors (in parentheses), differences of means, and t-statistics (in brackets) of the above-listed variables across the treatment and control groups before the matching procedure. On average and before matching, safeguard firms are larger. They have more long-term credit at the onset of the bankruptcy procedure, more cash, and less leverage. I implement the matching procedure with a logit regression at the firm level of the binary variable *Treated* on the firm characteristics. The regression is estimated on a cross-section of 641 safeguard (treated) firms and 2,326 receivership (control) firms, for which I have enough financial information. The estimation results are presented in Table B.2, column (1), and reveal differences that are in line with those found in the comparison of Table B.1, first three columns.

The last three columns of Table B.1 reveal the accuracy of the matching process with no statistically significant differences of means across any of the firm characteristics between the two groups. As a result of the matching process, I have 900 matched receivership firms to 641 safeguard firms. Similarly, column (2) of Table B.2 reveals that none of the determinants are statistically significant in a logit regression restricted to the matched sample and accordingly weighted. We also note that the magnitudes of the coefficients estimates and the Pseudo-R² decline significantly from the Pre-Match to the Post-Match estimation, ensuring that the matching process has removed any meaningful differences along observable characteristics from the two groups of firms.

The matched sample is used as a robustness test in Section 4.3 of the paper.

Table B.1: Propensity Score Matching – Summary Statistics

Table B.1 presents means, standard errors (in parentheses), differences of means and t-statistics (in brackets) of observable characteristics across treated and control firms before and after the matching procedure. Treated firms are safeguard firms, control firms are receivership firms.

	Pre-Match			Post-Match		
	Control	Treatment	Diff	Control	Treatment	Diff
			Summary Statistics			
Lenght of the plan (years)	9.601 (0.889)	9.526 (0.994)	0.075* [1.857]	9.564 (0.949)	9.534 (0.990)	0.031 [0.619]
Log(assets)	5.842 (1.023)	6.414 (1.117)	-0.572*** [-12.318]	6.137 (0.980)	6.404 (1.112)	-0.267 [-4.975]
Investment	0.051 (0.131)	0.046 (0.119)	0.005 [0.870]	0.042 (0.117)	0.046 (0.119)	-0.003 [-0.498]
Leverage	1.276 (0.644)	1.067 (0.484)	0.208*** [7.647]	1.133 (0.484)	1.069 (0.484)	0.063 [2.535]
Long term/Total credit	0.588 (0.364)	0.633 (0.364)	-0.044*** [-2.747]	0.617 (0.362)	0.633 (0.364)	-0.017 [-0.888]
Cash	0.065 (0.089)	0.068 (0.082)	-0.003 [-0.684]	0.063 (0.081)	0.068 (0.082)	-0.005 [-1.088]
Rating (Y/N)	0.501 (0.500)	0.586 (0.493)	-0.085*** [-3.820]	0.543 (0.498)	0.585 (0.493)	-0.042 [-1.626]
Log(Number of banks)	2.117 (1.766)	2.408 (2.182)	-0.291*** [-3.506]	2.233 (1.803)	2.410 (2.188)	-0.177 [-1.736]
Observations	2,326	641	-	900	641	-

Standards errors in parentheses, *t*-statistics in brackets

Table B.2: Propensity Score Matching – Logit Regression Results

Dependent variable:	Treated	
	Pre-Match (1)	Post-Match (2)
Lenght of the plan (years)	-0.0413 (0.427)	0.0537 (0.376)
Log(assets)	0.477*** (0.000)	-0.0527 (0.765)
Investment	0.0296 (0.941)	0.508 (0.283)
Cash	1.415** (0.011)	0.769 (0.347)
Leverage	-0.464*** (0.000)	0.0584 (0.725)
Long term/Total credit	0.505*** (0.003)	-0.126 (0.627)
Rating (Y/N)	0.182 (0.106)	0.00756 (0.959)
Log(Number of banks)	-0.0116 (0.911)	0.0448 (0.711)
Year of the plan	✓	✓
Industry	✓	✓
Region	✓	✓
Observations	2,971	1,540
Pseudo R ²	0.115	0.078

p-values in parentheses

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

Note: Table B.2 presents the logit regression results of the matching procedure. The dependent variable is *Treated*, equal to 1 for safeguard firms and 0 for receivership firms. Covariates include financial characteristics the year before firm filed for bankruptcy: the length of the plan, the firm's size measured as the log of its total assets, investment rate (tangible + intangible investments over total assets), leverage (debt on total asset), the ratio of short term credit over total credit, cash over assets, whether the firm had a rating prior to bankruptcy, and its number of banks. Categorical variables include the year of adoption of the plan, the firm's industry and the region of its headquarters. Column (1) presents the result of the matching procedure, and Column (2) the same logit regression estimated on the subsample of matched treatment and control observations.

B.3 Managers notified of flag removal do not drive the results

To measure whether the main effect is driven by managers who receive a notification of flag removal by the Banque de France, I divide treated firms between those whose rating changed to a good, significant rating and the others. In the following test, *Notification* equals 1 for treated firms whose rating changes to 3++, 3+, 3, 4+, 4, or 5+ after flag removal. It concerns 7,7% of treated firms.

Results Table B.3 show that the main effects are robust amongst firms that do not receive a notification: they increase their credit by 1.18% and have 1,7 p.p. more chance of forming new banking relationships. The 7,7% of firms that receive a notification explains 30% of the baseline effect ($= (0.0169 - 0.0118) / 0.0169$, see Table 3) on credit variation. Their probability of forming new banking relationships is not impacted.

Table B.3: Effect of flag removal depending on whether treated firms received a notification

	$\% \Delta$ Credit	Pr(New banks)
	(1)	(2)
Post	-0.00594*** (0.001)	-0.00199 (0.356)
Treated \times Post	0.0118** (0.039)	0.0171* (0.065)
Notification \times Post	0.0619** (0.017)	0.0160 (0.683)
Firm FE	✓	✓
Quarter \times Industry FE	✓	✓
Observations	48,518	48,518
Adj. R ²	0.926	0.436

p-values in parentheses

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

Note: Table B.3 reports OLS difference-in-difference estimates of the flag removal impact on firms' access to bank credit. The dependent variable column (1) is the credit variation, and column (2) is the probability of starting a new banking relationship. Treated firms are distinguished according to whether they received a notification from the Banque de France because their rating changed to a significant rating (i.e., 5+ or safer). Standard errors are clustered at the firm level.

B.4 Robustness to Sample Selection

As mentioned in Section 3, the sample is subject to attrition for two main reasons. The first reason is that the credit database SCR (*Service Central des Risques*) does not report the credit exposure between a bank and a firm below the €25K threshold. The second and most problematic reason for attrition is the liquidation of firms whose restructuring plan has failed. This attrition is not random but concerns less viable firms. The baseline specification focuses on firms having survived at least four years under restructuring. In this section, I relax this assumption to assess whether sample selection may have distorted the results.

I first need to estimate the extent of attrition. The initial sample consists of 20,895 firms³⁹) for a total of 72,234 observations. Of these, 14,369 firms (representing 23,716 observations, or 33% of the final sample) are excluded from the analysis due to attrition :

1. For 10,509 firms, observations are truncated because of the failure of the restructuring plan. I observe firms' liquidation before the fourth year of restructuring.
2. For the remaining 3,860 firms, I do not observe any liquidation. Attrition may be random or the result of the threshold effect.

A simple test suggested by Nijman and Verbeek (1992) confirms the non-randomness of attrition: I include in the baseline specification a lagged selection indicator $s_{i,t-1}$, under the null hypothesis that the selection is not related to the idiosyncratic error, $s_{i,t-1}$ should be non-significant. In my case (unreported results), $s_{i,t-1}$ is negative and significant at the 1% level with a p-value of 0.000.

I reproduce the baseline analysis on the unselected sample. To control for the MNAR (missing not at random), I weight my baseline specification by the IPW (inverse probability weight) as recommended by Wooldridge (2010). I first predict the probability of being selected by logistic regression with the following covariates: credit variation, flag removal, firms characteristics (total assets, credit rating, share of long-term credit, plan characteristics, region of the headquarters), and quarter \times industry dummies. I do not include variables that are not correlated with selection, nor variables that would restrict my sample too much (because I do not have full financial information for all the population of interest, e.g., investment). Table B.4 reports the results.

I then recover the inverse of the predicted probability of being selected that I use as a weight in the outcome analysis. Firms with a higher probability of being liquidated are given a higher weight in the analysis to compensate for similar firms missing (Wooldridge (2007)).

³⁹2,840 firms in safeguard and 18,055 firms in receivership.

Table B.4: Probability of selection – Logit regression result

Dependent variable:	Pr(Selected) (1)
% Δ Credit	0.248*** (0.001)
Treated	-0.0964 (0.427)
Post	0.305*** (0.000)
Treated \times Post	0.173* (0.098)
Log(assets _{t-1})	0.244*** (0.000)
Long Term/Total credit	0.151** (0.022)
Length of the plan (years)	0.133*** (0.000)
Quarter of the plan	0.263*** (0.000)
Credit rating	✓
Region	✓
Quarter \times Industry FE	✓
Observations	49,378
Pseudo R ²	0.116

p-values in parentheses

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

Note: Table B.4 presents the result of the logistic regression on the probability of being selected. A firm is not selected if it was liquidated during the first four years of the restructuring plan or excluded from the sample due to the threshold effect or random attrition.

I compare results in Table B.5 with my baseline analysis. Most of the results are robust to this new sample: column (1), flag removal causes a 1.56% increase in credit (compared with 1.69% in the baseline analysis Table 3), and column (2), treated firms have a 1.68 percentage point greater chance of forming a new banking relationship (compared with 1.96 percentage point, column (5) of Table 4). The analysis of firms' financial performance and real economic outcomes reported in columns (4)-(5) and (7)-(8) are in line with the previous results: flag removal has no impact on the margin rate, turnover, or employment. It has no impact on the apparent cost of debt (not shown for brevity). The share of supplier debt in total debt has fallen by 1.2 percentage point (compared with 0.77 percentage point, Table 9).

Two results differ from the baseline analysis: on the one hand, in column (3) I see an increase in leverage of 7.5 percent points, whereas it was previously not significantly different from zero. On the other hand, in column (6), the investment rate is not significantly affected by flag removal.

The increase in total leverage does not invalidate that firms rely less on supplier credit. The conclusion that firms substitute supplier credit for bank debt still holds, as the share of supplier debt on total debt still decreases. As far as investment rate is concerned, the absence of any apparent increase calls into question the diagnosis that the situation of firms improves. With the increase in leverage, the situation is more mitigated. One interpretation may be that the firms that survive are less leveraged and invest more, which would explain the difference in results on the selected sample. This test suggests that the sample selection does not fully represent the total population of firms undergoing restructuring, and the main results should be interpreted accordingly.

Table B.5: Attrition controlled sample

Note: Table B.5 reports the results of the main specifications reproduced on a new sample, not selected for attrition, and weighted by IPW. Column (1) reproduces column (1) of Table 3, column (2) reproduces column (5) of Table 4, and columns (3) to (8) reproduce Table 9. All regressions are OLS, with standard errors clustered at the firm level.

	Credit outcomes		Financial outcomes		Economic real outcomes			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Δ Credit	Pr(New banks)	Leverage	Supplier Debt/ Total Debt	Margin	Investment	Δ Turnover	Δ Employment
Treated	-0.0928* (0.088)	0.0377 (0.328)	-0.155 (0.439)	0.0388** (0.006)	-0.151 (0.476)	-0.00513 (0.679)	-0.409* (0.081)	0.000220 (0.999)
Post	-0.00599*** (0.010)	-0.00205 (0.312)	-0.0872*** (0.001)	0.00298 (0.300)	-0.0133 (0.519)	-0.000423 (0.860)	0.0302* (0.059)	0.00351 (0.870)
Treated \times Post	0.0156*** (0.009)	0.0168* (0.068)	0.0746** (0.048)	-0.0120*** (0.000)	0.00186 (0.944)	0.000346 (0.880)	-0.0874 (0.184)	-0.00495 (0.783)
Firm FE	✓	✓	✓	✓	✓	✓	✓	✓
Year \times Industry FE	✓	✓	✓	✓	✓	✓	✓	✓
Observations	63,255	63,255	10,410	10,384	10,400	10,394	7,880	8,497
Adj. R ²	0.928	0.511	0.838	0.874	0.415	0.407	0.811	0.832

C Banks' regulatory behavior

I want to test if flag removal impacts banks' behavior through the capital requirements channel. The idea behind this channel covers two possibilities:

The first possibility comes from the fact that banks can declare the credit risk of their borrowers based on their internal monitoring system or on external credit rating as the Banque de France's⁴⁰. In this case, the flag removal can be a positive shock that alleviates banks' capital requirements as their borrowers suddenly become risk-free, according to the Banque de France credit rating.

The second possibility concerns guidelines that may shape banks' lending behavior. Independently from the actual financial situation of the borrower, banks – and in particular local branches, far from the banks' headquarters that decide on the guidelines – may be constrained to follow rules that prohibit them from lending to firms poorly rated by external rating agencies. In this case, the flag removal could make it possible to lend to the firm again.

In both scenarios, I expect flag removal to positively impact banks that are the most financially constrained. Note that the change in rating would not bring any new information to lenders but instead relieve them from financial or regulatory constraints. The following aims to test this hypothesis.

To test the impact of flag removal on bank credit supply via the solvency channel, I gather data from the European Banking Authority (EBA) Transparency Exercise⁴¹ on the Tier 1 ratio of the leading French banking groups. The Tier 1 solvency ratios divide the bank's Tier 1 capital by its risk-weighted assets. The EBA information is available for 62% of the banks in my sample, which represents 75% of the firms in my sample. I measure the Tier 1 ratio quarterly deviation from the EU average for each bank. I then calculate the lagged weighted average of the banks' ratio at the firm level. I finally create the dummy $Low_{i,t-1} = 1$ if the lagged weighted average of the firm's banks' Tier 1 ratios is in the lower quartile of the distribution. $Low_{i,t-1} = 1$ means that the firm's banks are more constrained than the average. I add this dummy in a triple interaction term in the baseline equation:

$$\begin{aligned} \% \Delta Credit_{i,q} = & \beta_0 Post_q + \beta_1 (Post_q \times Treated_i) \\ & + \beta_2 Low_{i,t-1} + \beta_3 (Low_{i,t-1} \times Treated_i) + \beta_4 (Low_{i,t-1} \times Post_q) \quad (6) \\ & + \beta_5 (Post_q \times Treated_i \times Low_{i,t-1}) + \gamma_i + \gamma_{s \times t} + \epsilon_{i,q} \end{aligned}$$

In this specification, β_5 captures the differential effect of the flag removal on

⁴⁰See Methods of calculating of prudential ratios under the CRDIV, <https://acpr.banque-france.fr/>

⁴¹See <https://www.eba.europa.eu/>.

constrained banks from a solvency perspective. If banks adjust their regulatory behavior according to the Banque de France’s credit rating, or if banks rely on Banque de France’s external credit rating to declare the risk carried by their borrowers, one would expect the flag removal to relieve banks with low solvency more than the average, and β_5 to be positive.

Table C.1 presents the estimation results. Columns (1) and (2) present the result of equation (6) at the firm level, with and without the triple interaction term. Columns (3) and (4) present the same specification but at the firm-bank level, with additional bank fixed effects. We first note that β_1 , the difference-in-difference coefficient estimate, is robust to these alternate specifications. The coefficient β_2 associated with the dummy $Low_{i,t-1}$ is rather negative, meaning that firms with constrained banks have less credit on average, but the coefficient is not always significantly different from 0. Most importantly, in columns (2) and (4), we read that β_5 is not different from 0. Solvency constraints do not explain the credit supply after the flag removal.

An alternative test would be to compare firms in safeguard with firms with the same credit rating but, not because of a bankruptcy proceeding, simply because of their financial and economic situation. In this way, measuring the impact of flag removal would inform us about the banks’ credit risk management linked to the Banque de France credit rating. However, given the timing of flag removal (3 years after the start of the plan), it would be necessary to match firms in safeguard with firms with the same rating for at least 3 years – or, alternatively, with a comparable life cycle. This analysis is challenging and has not been included in this paper. It could be the subject of further research.

Although it is difficult to conclude the absence of effect, this first analysis leads us to believe that banks do not use Banque de France credit rating to manage the risk of their borrowers. The first reason may be that banks usually base their declaration on their internal rating, and restructured firms are always classified as “doubtful” or even “compromised” until the plan successfully ends. Banks do not draw regulatory information from Banque de France credit rating on their restructuring client firms. Additional evidence in Appendix D reveals that banks are not impacted by an exogenous downgrade of restructured firms’ external credit rating either.

Table C.1: Banks' solvency channel

	% Δ Credit			
	(1)	(2)	(3)	(4)
Post	-0.00635*** (0.008)	-0.00529* (0.078)	-0.00480** (0.028)	-0.00419* (0.059)
Treated \times Post	0.0184*** (0.004)	0.0160** (0.027)	0.0127*** (0.001)	0.0150*** (0.000)
Low Tier1	-0.00754* (0.094)	-0.00229 (0.636)	-0.00437 (0.580)	-0.00308 (0.794)
Treated \times Low Tier1		-0.0207 (0.146)		0.0224 (0.431)
Post \times Low Tier1		-0.00326 (0.495)		-0.0106 (0.145)
Treated \times Post \times Low Tier1		0.00798 (0.470)		-0.0298 (0.141)
Firm FE	✓	✓	✓	✓
Quarter \times Industry FE	✓	✓	✓	✓
Bank FE			✓	✓
Observations	36,177	36,177	46,871	46,871
Adj. R ²	0.920	0.920	0.869	0.869

p-values in parentheses

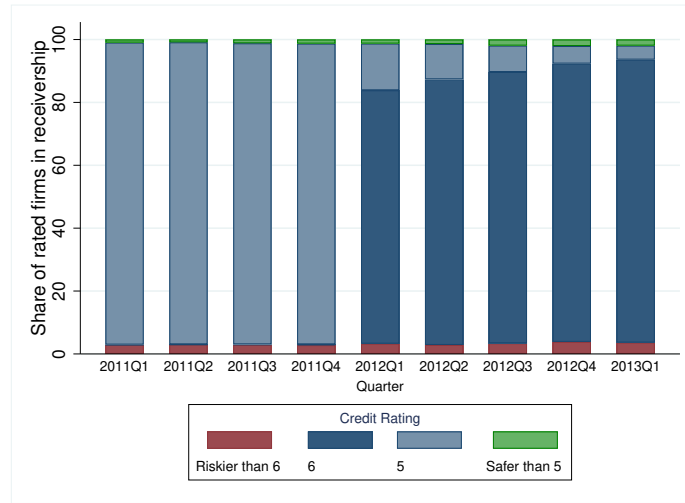
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Note: Table C.1 reports difference-in-difference estimates of flag removal depending on the level of solvency constraints the banks face, equation (6). Estimates columns (1) and (2) are at the firm level where $Low_{i,t-1}$ equals 1 if the lagged weighted average of the firm's banks' Tier 1 ratios is in the lower quartile of the distribution. Estimates columns (3) and (4) are at the firm-bank level and include bank fixed effects, and $Low_{i,b,t-1}$ equals 1 if the banks' Tier 1 ratios are in the lower quartile of the distribution. Standard errors are clustered at the firm level.

D The 2012 policy change

In this appendix, I exploit that up to 2011, the credit rating of firms executing a receivership plan was 5. It was downgraded to 6 on January 1, 2012, to better convey the credit risk carried by these firms. Figure D.1 presents the effect of this policy change on the rating of firms between 2011 and 2013. At the end of 2011, 96% of firms were rated 5. At the beginning of 2012, 83% were rated 6.

Figure D.1: Transition matrices of the rating of firms in receivership around the policy change



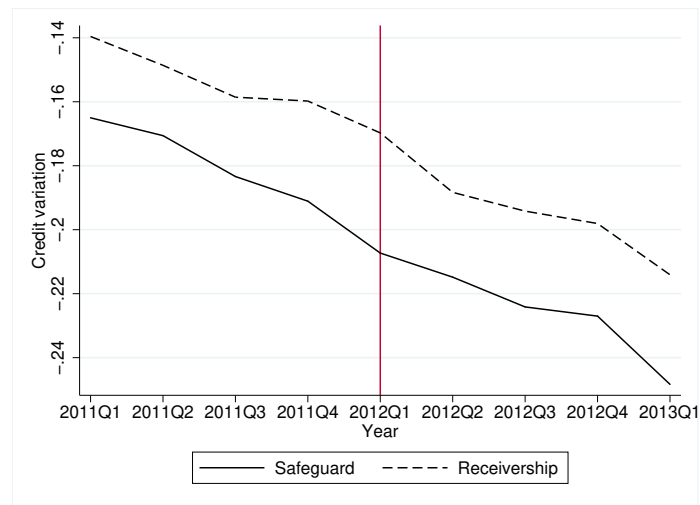
Note: Figure D.1 shows the proportion of rated firms in receivership each quarter around the 2012 policy change.

As previously, using a two-way fixed effects difference-in-difference design, I estimate the impact of the policy change on firm credit variation. I compare receivership firms (treated group) with safeguard firms (control group). As discussed in the paper, safeguard firms form an ideal control group as they have a similar repayment schedule as receivership firms, and their rating remained constant, set to 5, between 2011 and 2013. To avoid being affected by safeguard flag removal, I focus on firms in the first three years of restructuring. I restrict my sample to firms that survived at least three years. Figure D.2 shows the average credit variation of the two groups between 2011 and 2013. The parallel trends assumption requires that the credit growth follows the same trend before 2012, which is clearly the case.

To measure the causal effect of the policy change, I estimate the following:

$$\% \Delta Credit_{i,q} = \beta (Post2012 \times Treated) + \gamma_q + \gamma_i + \gamma_{s \times t} + \epsilon_{i,q} \quad (7)$$

Figure D.2: Average credit growth for safeguard and receivership firms around the 2012 policy change



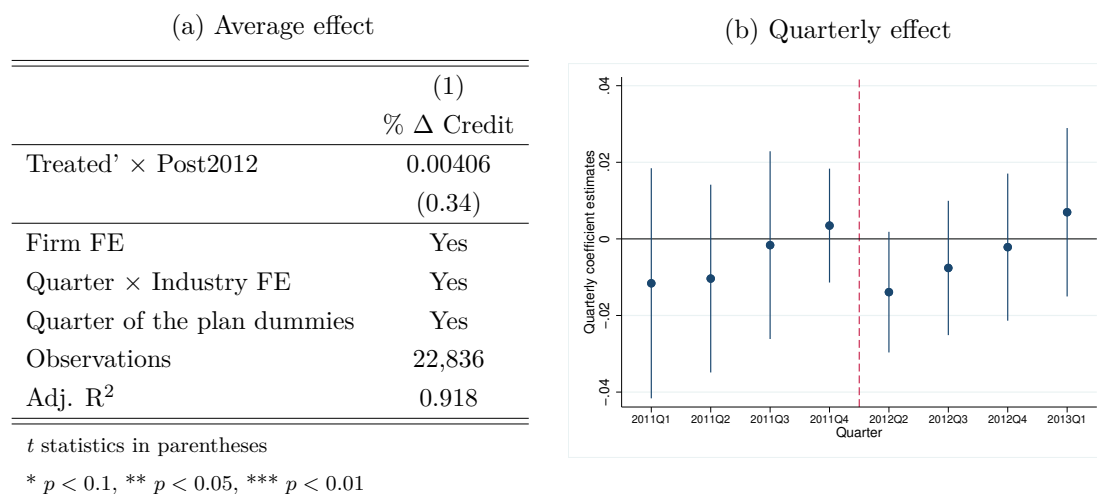
Note: Figure D.2 shows the raw quarterly average credit variation for firms around the policy change for safeguard firms (solid line) and receivership firms (dashed line). The variation of credit is $\% \Delta Credit_{i,q}$, the quarterly variation of total credit compared to the firm's average level of credit the year before it filed for bankruptcy.

where $Post2012$ is a dummy that equals 1 after 2012 and 0 before, and $Treated'$ equals 1 for receivership firms and 0 for safeguard firms. I follow firms from four quarters before to four quarters after the policy change. Unlike the baseline specification, the exogenous shock occurs at a specific date, regardless of the plan's age. Therefore, I introduce γ_q for quarter-of-the-plan fixed effects. All the other variables are the same as described before, and the standard errors are clustered at the firm level.

Results are presented Table D.3a. The coefficient β is not significantly different from 0, suggesting that the downgrade of credit rating did not cause a change in credit variation. The dynamic analysis reported in Figure D.3b confirms this result: although the first quarters after the policy change are rather negative, they are not statistically different from zero.

I see two possible explanations for this (absence of) result. First, and contrary to the baseline study of the paper, here, the bankruptcy flag remains salient. Banks have access via FIBEN to the bankruptcy judgments behind the semi-automatic rating, which remains unchanged. More than the rating, the bankruptcy judgment may shape banks' lending supply. Also, the change in rating, although exogenous, remains small. Both ratings 5 and 6 are significant, risky ratings. These reasons may explain the absence of any pronounced effect.

Figure D.3: Effect of policy change on firm credit variation



Note: Table D.3a reports difference-in-difference estimates of the effect of the policy change on the variation of firm’s credit (equation (7)), and Figure D.3b the dynamics difference-in-difference estimates β_q (see equation (3)). The vertical red line identifies the policy change in 2012. The dependent variable is $\% \Delta Credit_{i,q}$, the quarterly variation of total credit compared to the firm’s average level of credit the year before it filed for bankruptcy. *Treated'* takes the value of 1 for receivership firms, and *Post2012* takes the value of 1 after 2012. Firms are tracked from four quarters before the policy change to four quarters after it. Standard errors are clustered at the firm level.

RÉSUMÉ

Un droit des faillites efficace facilite le financement des entreprises, leur croissance et l'innovation : il renforce la confiance des investisseurs *ex-ante*, et la résolution ordonnée des difficultés des entreprises *ex-post*. Le droit des faillites français est en constante évolution pour répondre au mieux à ces enjeux, mais peu d'études questionnent son efficacité économique. Cette thèse développe une approche empirique pour contribuer, sous trois angles distincts, à ce pan manquant de la littérature en Économie du Droit. Le premier chapitre questionne l'efficacité des procédures préventives de traitement des difficultés des entreprises par rapport aux procédures classiques de restructuration. Il montre que la mauvaise réputation des procédures classiques effraie les parties prenantes des entreprises défaillantes qui, par un effet auto-réalisateur, diminuent leur chance de survie. Ainsi, les procédures préventives permettent de préserver la réputation des entreprises et augmentent leurs chances de se restructurer. Le deuxième chapitre analyse l'application de la loi par les tribunaux de commerce. Étant garants d'une loi uniforme sur tout le territoire, les tribunaux ont pourtant un fonctionnement hétérogène. Ce chapitre montre que les petits tribunaux de commerce ont un biais de continuation qui favorise la poursuite d'activité des petites entreprises peu viables. Il met en évidence des effets de transmission de comportement d'un tribunal à l'autre. Le troisième chapitre s'intéresse aux contraintes de crédit bancaire auxquelles les entreprises font face suite à une défaillance. Il montre que la suppression de l'information sur la défaillance passée permet aux entreprises d'accroître leur accès au crédit bancaire, notamment par le biais de nouvelles relations bancaires pour les entreprises les plus contraintes. Les entreprises réduisent alors l'usage de crédit inter-entreprises et augmentent leur investissement. Ce chapitre conclut sur l'importance de l'accompagnement des entreprises dans leur rebond suite à une défaillance.

MOTS CLÉS

Droit et Économie, Faillites d'entreprises, Microéconométrie, Restructuration de dette, Procédures préventives

ABSTRACT

Efficient bankruptcy law facilitates firm financing, growth, and innovation. It boosts investors' trust and the orderly resolution of business difficulties. French bankruptcy law constantly evolves to meet these challenges, but only a few studies have questioned its economic efficiency. This thesis develops an empirical approach to contribute, from three distinct angles, to this missing facet of the literature in Law and Economics. The first chapter examines the effectiveness of preventive procedures for dealing with corporate difficulties, compared with traditional restructuring procedures. It shows that the bad reputation of conventional procedures frightens the stakeholders of failing companies, which, through a self-fulfilling effect, reduce their chances of survival. In this way, preventive procedures preserve the companies' reputation and increase their chances of restructuring. The second chapter analyzes the application of the law by the commercial courts. As guarantors of a uniform law throughout the country, the courts operate heterogeneously. This chapter shows that small commercial courts have a continuity bias that favors the continued operation of unviable small businesses. It also highlights the transmission effects of behavior from one court to another. The third chapter looks at the bank credit constraints faced by firms following insolvency. It shows that removing information on past bankruptcy enables firms to increase their access to bank credit, notably through new banking relationships for the most constrained firms. Firms then reduce their use of inter-firm credit and increase their investment. This chapter concludes with the importance of supporting companies in their recovery from default.

KEYWORDS

Law and Economics, Corporate Bankruptcy, Econometrics, Debt restructuring, Preventive Proceedings

