WORKING paper

BANQUE DE FRANCE
EUROSYSTÈME

Access to Bank Credit after Emerging from Corporate Bankruptcy

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November 2023, WP #928

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.

Keywords: Corporate bankruptcy, Debt Restructuring, Credit Rating, Bank Lending Relationship, SMEs

JEL classification: G21, G24, G33, G34

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I am grateful to Antoine Baena, Nicoletta Berardi, Régis Blazy, Benjamin Bureau, Eve Caroli, Anne Epaulard, Claire Lelarge, Gianluca Orefice, Loriane Py, Federica Salvadè and Frédéric Vinas for insightful comments and discussions. I also thank seminar participants at the French Economic Association meeting 2022, the Scottish Economic Society Annual Conference 2022, the International Conference of the French Finance Association 2022, and the Econometric Society European Meeting.

NON-TECHNICAL SUMMARY

When a firm faces difficulties repaying its debts, whether insolvent or on the verge of insolvency, it can undertake a debt restructuring procedure. Upon emerging from the procedure and after the court's decision to agree on a debt-restructuring plan, the "restructured" firm must meet its new repayment deadlines to remain in business. One of the decisive factors for success is its access to bank credit. Since restructuring plans last an average of ten years, the incapacity for the firm to borrow or invest could seriously compromise its survival.

In this paper, I analyze the access to bank credit of restructured firms. I question the existence of bank credit constraints associated with the information on firms' past bankruptcy. 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.

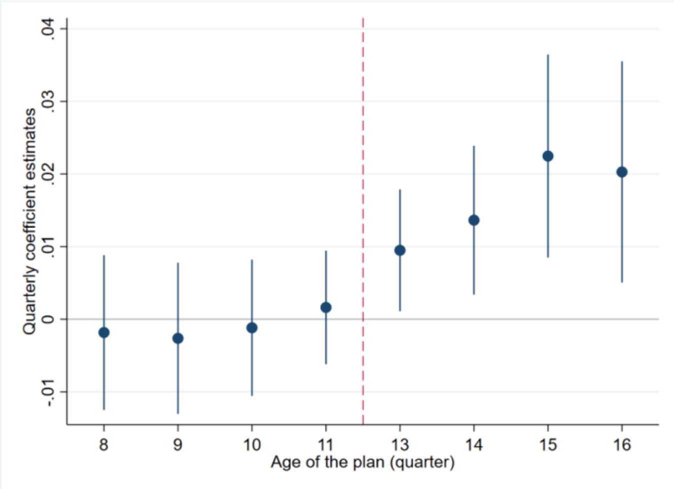
To assess the extent to which policymakers can influence restructured firms' access to bank credit, I measure the causal impact of removing information about past corporate bankruptcies on SMEs' access to bank credit.

My method takes advantage of the fact that the Banque de France credit rating takes into account information on the firm's past bankruptcy, as long as that information can effectively be used. The purpose of the Banque de France rating is to inform banks about the financial health of a firm and its ability to honor its payment obligations. When a firm emerges from bankruptcy with a restructuring plan, its rating takes into account the information about the past bankruptcy and automatically takes on a 'risky' value. However, three years (for safeguard) to five years (for receivership) after the procedure, the information on the past bankruptcy is, according to regulation, no longer used for the credit rating, which is no longer automatically set as 'risky'. Using a difference-in-difference method, I show that flag removal allows firms to increase their access to bank credit. Firms with a favorable credit rating obtain credit from their historical banks. For firms whose credit rating becomes 'neutral' and which are therefore more constrained than firms with a good rating, I also observe an increase in bank credit, although smaller, which is partly due to the formation of new banking relationships.

I question why banks react to this change in rating even though information on past bankruptcy remains freely available elsewhere. I argue that the Banque de France's credit rating guides investors' behavior.

Ultimately, I measure the real economic impact of bankruptcy flag removal. I show that this increase in access to bank credit results in a fall in the use of trade credit and an increase in the investment rate of firms undergoing restructuring. My results are in line with recent policies that aim at reducing the time during which information on past insolvencies is available. This paper emphasizes the importance of supporting firms in their recovery.

Figure 1. Quarterly effect of bankruptcy flag removal on treated firm's credit variation



Note: This figure illustrates the evolution of bank credit for firms undergoing debt-restructuring between the second and fourth year of their restructuring plan. It shows variation in credit of treated firms compared with control firms. Treated firms are those which information about their past bankruptcy is removed from their credit rating at the third year of their plan, while control firms retain information about their past bankruptcy in their credit rating until the fifth year of their plan. We observe that the flag removal of treated firms significantly increase their access to bank credit compared with control firms.

Accès au Crédit Bancaire des Entreprises en Restructuration

RÉSUMÉ

Cet article identifie les restrictions de crédit bancaire auxquelles sont confrontées les entreprises en restructuration. Il repose sur une stratégie en différence-de-différence qui exploite sur le 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 pendant une période limitée. Il étudie la suppression de cette information de la cote de crédit, qualifiée de « *flag removal* », sur l'accès au crédit des entreprises. L'échantillon se concentre sur les TPE et PME en restructuration entre 2012 et 2019 ; l'analyse montre que le *flag removal* conduit à une augmentation du crédit bancaire de 1,7 % et à une augmentation des chances de nouer de nouvelles relations bancaires de 2 points de pourcentage. Les entreprises dont la cote de crédit devient éligible augmentent leur accès au crédit auprès de leurs banques historiques ; les autres, davantage contraintes, augmentent leur accès au crédit auprès de nouvelles banques. Suite au *flag removal*, les entreprises substituent le crédit bancaire au crédit inter-entreprises et augmentent leur taux d'investissement. Ainsi, cet article soutient les politiques publiques visant à réduire le temps de mise à disposition de l'information sur les défaillances passées.

Mots-clés : défaillance d'entreprises, restructuration de la dette, cote de crédit, relations bancaires, PMEs

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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 moderate[1] 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

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

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

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 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 SMEs[3] is critical, not only because SMEs represents 99.95% of bankruptcy filings[4] 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.

[3]Small 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.

[4]Source: https://www.banque-france.fr.

[5]Source: https://www.insee.fr.

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 increase in credit supply comes from less well-informed lenders and new banks. Less well-informed historical[6] 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

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

[7] Empirical 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)).

[8] In 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)).

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 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

[9] The 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.

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-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 databse. 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

[10] In 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.

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

firms' financial statements and when significant developments occur.

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.

The rating scale[12] 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 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

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

[13] While 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.).

in charge of the case, banks can access firms' current rating (in our example, 5[14]) 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.

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).

2.2.1 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

[14] The 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).

“*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:

- 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]

2.2.2 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.

[15] Up 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.

[16] Under 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)

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;
- 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.

[17]See footnote 16.

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 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.

[18]"*Bulletin Officiel d'Annonces Civiles et Commerciales*", see https://www.data.gouv.fr/en/datasets/bodacc/en.

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. 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

[19] See footnote 15.

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 12^{th} quarter of their plan; this number drops to below 30% in the 13^{th} quarter. Of firms in receivership 75% were rated 6 before the 20^{th} quarter of the plan; only 10% of them are in the 21^{st} quarter.

Table 2: Summary Statistics

	Panel A: Firm-quarter characteristics					
	N	Mean	Median	St.Dev.	5th Pct.	95th Pct.
Treated group: firms in safeguard						
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
Control group: firms in receivership						
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.
Treated group: firms in safeguard						
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
Control group: firms in receivership						
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

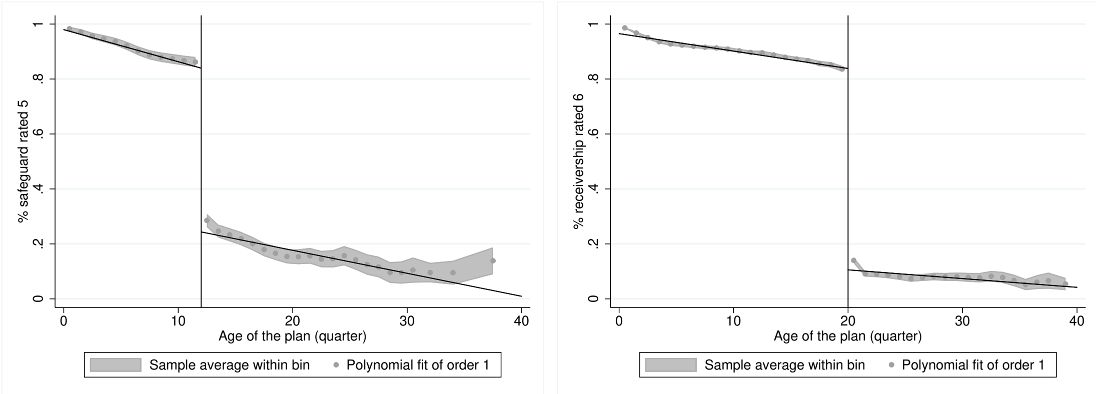
	Panel C: Firm characteristics					
	N	Mean	Median	St.Dev.	5th Pct.	95th Pct.
Treated group: firms in safeguard						
Length of the plan (years)	825	9.566	10.000	1.212	7.000	10.000
Control group: firms in receivership						
Length of the plan (years)	3,475	9.590	10.000	0.995	8.000	10.000

	Panel D: Firm-bank characteristics					
	N	Mean	Median	St.Dev.	5th Pct.	95th Pct.
Treated group: firms in safeguard						
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
Control group: firms in receivership						
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

(a) Safeguard (b) Receivership



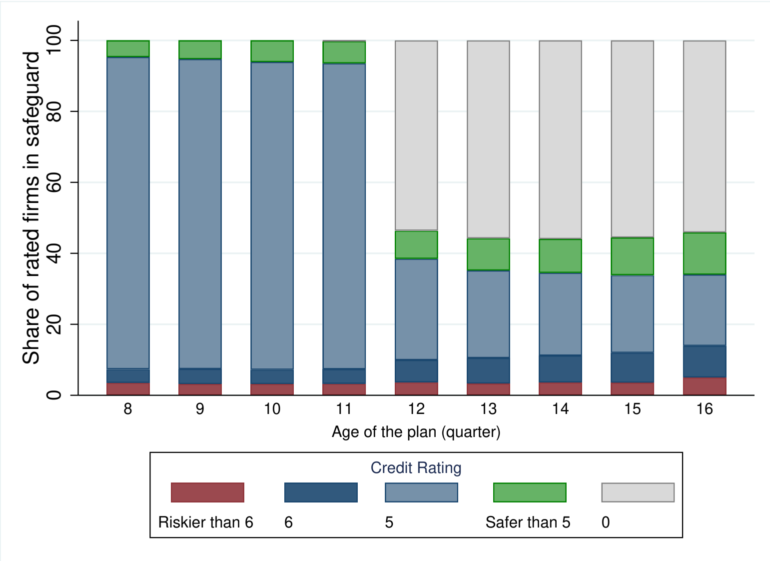
Note: Panel (a) represents the sharp drop in the proportion of firms in safeguard rated 5 around flag removal at the 12^{th} quarter of the plan. Panel (b) represents the sharp drop in the proportion of firms in receivership rated 6 around flag removal at the 20^{th} quarter of the plan. We can see that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

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 11^{th} quarter of their plan. This proportion drops to 35% after flag removal in the 12^{th} quarter.

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

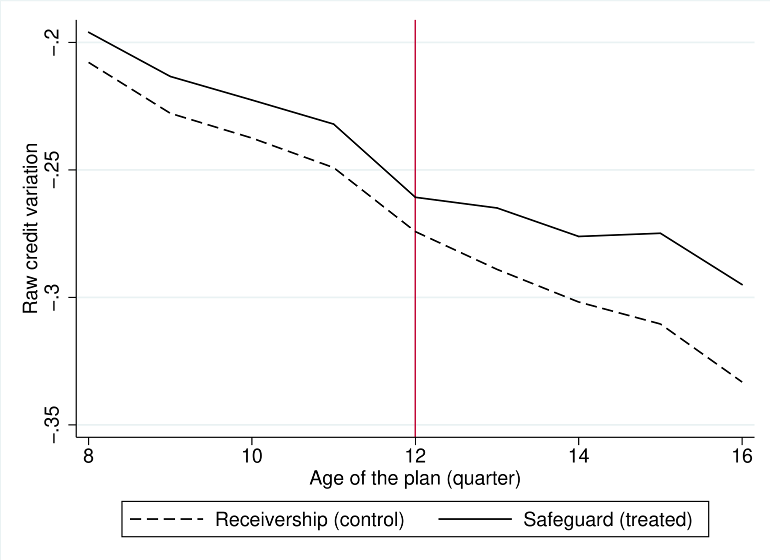
4.1 Intensive Margins: Credit variation

I am interested in variation in credit. Following Amiti and Weinstein (2018), I measure the percentage change[20] 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:

$$\%\Delta Credit_{i,q} = \frac{Credit_{i,q} - \overline{Credit}_{i,q=pre\text{-}bnkcy}}{\overline{Credit}_{i,q=pre\text{-}bnkcy}} \tag{1}$$

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

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.

where $Credit_{i,q}$ is the total amount of credit (short-term plus long-term)[21] at quarter q, for firm i with all of its banks. $\overline{Credit}_{i,q=pre\text{-}bnkcy}$ 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

[21]This 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, 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.

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.

4.1.1 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-indifference equation:

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

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:

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

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.

Table 3: Effect of flag removal on firms' credit

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

p-values in parentheses

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

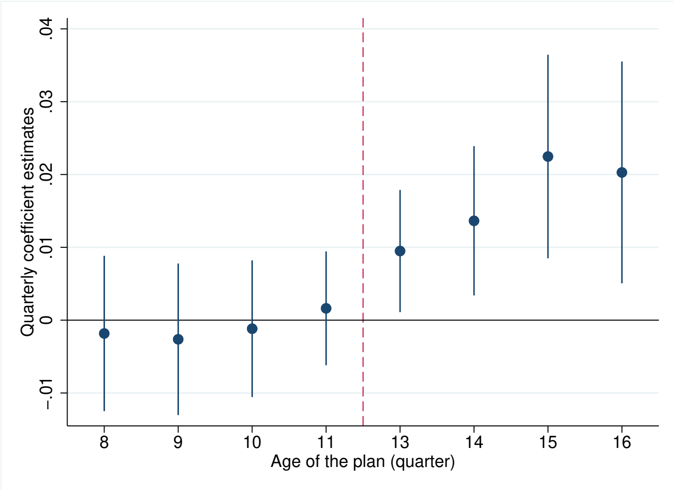
Note: Table 3 reports the difference-in-difference estimate of equation (2) on the safeguard flag removal effect on firms' credit variation. The dependent variable is the quarterly variation in total credit compared with the firm's average level of credit the year before it filed for bankruptcy. *Treated* 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.

4.1.2 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%).

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

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.

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 those who receive a significant, good rating from the Banque de France to those who do not, I provide additional evidence[22] 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.

[22] See Table B.3.

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:

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

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

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***		0.0501		-0.00273
	(0.004)		(0.426)		(0.200)
Treated × Post	0.605***	0.150***	0.343***	0.0395***	0.0186**
	(0.000)	(0.000)	(0.003)	(0.000)	(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 × 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.

total population of firms, flag removal significantly increases the probability of forming new banking relationships, by 4.0 percentage points (column (4)). Theses 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]

4.3 Robustness Tests

4.3.1 TWFE robustness to heterogeneous treatment effect

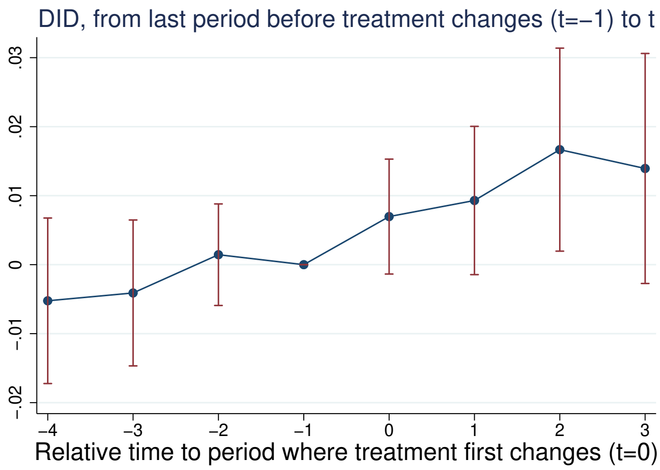
As pointed out by Roth et al. (2023) and De Chaisemartin and D'Haultfoeuille (2022) in their review of the latest difference-in-difference methodology literature, Two-Way Fixed Effects estimations are valid in specific conditions: when the parallel trends assumption is met, everyone is treated at the 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_multiplegt`, 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

[23] Cahn et al. (2023) report a quarterly probability of starting a new banking relationship of 6% on average for healthy firms.

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.

Figure 7: Time- and group-robust DID estimator



Note: Figure 7 shows that the coefficients are 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 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.

4.3.2 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.

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

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

	%Δ Credit	Pr(New Bank)
	(1)	(2)
Post	-0.0147***	-0.0196***
	(0.004)	(0.008)
Treated × Post	0.0263***	0.0331**
	(0.003)	(0.022)
Firm FE	✓	✓
Quarter × 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.

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?

5.1.1 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:

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

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).

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.

[24] I 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.

[25] My 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.

Table 6: Credit variation by lender type

	% Δ New	% Δ Hist.
	(1)	(2)
Post	0.00144***	-0.0102***
	(0.000)	(0.000)
Treated × Post	0.00255**	0.0135***
	(0.011)	(0.008)
Firm FE	✓	✓
Quarter × 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.

5.1.2 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 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 $Credit^H_{i,q}$, and credit from lenders with a low level of information, denoted $Credit^L_{i,q}$.

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

and

$$\%\Delta Credit^L_{i,q} = \%\Delta Hist.BankCredit_{i,q} - \%\Delta Credit^H_{i,q}$$

with $Hist.BankCredit_{i,q} = Credit^H_{i,q} + Credit^L_{i,q}$, and $\overline{Credit^H}_{i,q=\textit{pre-bnkcy}}$ 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 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

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

Dependent variable	Credit variation split amongst hist. banks			
Measure	Geo. proximity		Share in firms' credit	
Level of information	High	Low	High	Low
	(1)	(2)	(3)	(4)
Post	-0.00693***	-0.00627**	-0.00395***	-0.00845**
	(0.000)	(0.038)	(0.009)	(0.044)
Treated × Post	0.00331	0.0101**	0.00708	0.00719
	(0.420)	(0.023)	(0.185)	(0.288)
Firm FE	✓	✓	✓	✓
Quarter × 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.

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 also used to determine the eligibility of firms' loans as collateral with the ECB. Under

[26]See 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.

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 × 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

	All	Credit variation split by lender type					
Dependent variables	% 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***	0.00144***	-0.0103***	-0.00695***	-0.00631**	-0.00852**	-0.00398***
	(0.001)	(0.000)	(0.000)	(0.000)	(0.037)	(0.043)	(0.008)
Treated × Post	0.0145**	0.00236**	0.0118**	0.00304	0.00821*	0.00674	0.00611
	(0.014)	(0.017)	(0.021)	(0.466)	(0.065)	(0.326)	(0.255)
Eligible × Post	0.125**	0.00540	0.106**	0.0198	0.0901**	0.0492	0.0492**
	(0.017)	(0.460)	(0.024)	(0.111)	(0.035)	(0.153)	(0.019)
Firm FE	✓	✓	✓	✓	✓	✓	✓
Quarter × 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

[28] Eligible 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 2.2.2). 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).

[29] One 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 Post $\times$ 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 × industry fixed effects as follows :

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

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

[30] The 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), Kliger and Sarig (2002)).

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

	Economic outcomes			
	(1) Margins	(2) Investment	(3) Δ Turnover	(4) Δ Employment
Post	-0.00418	-0.00191	0.00762	0.0199
	(0.352)	(0.446)	(0.434)	(0.191)
Treated × Post	0.000570	0.00535**	0.00717	-0.000470
	(0.913)	(0.027)	(0.503)	(0.977)
Firm FE	✓	✓	✓	✓
Year × 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. %Δ Turnover and %Δ 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.

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 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

[32] In 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).

[33] See 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.

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

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 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

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

[36]We 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.

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 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" privilege[38] 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.

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

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

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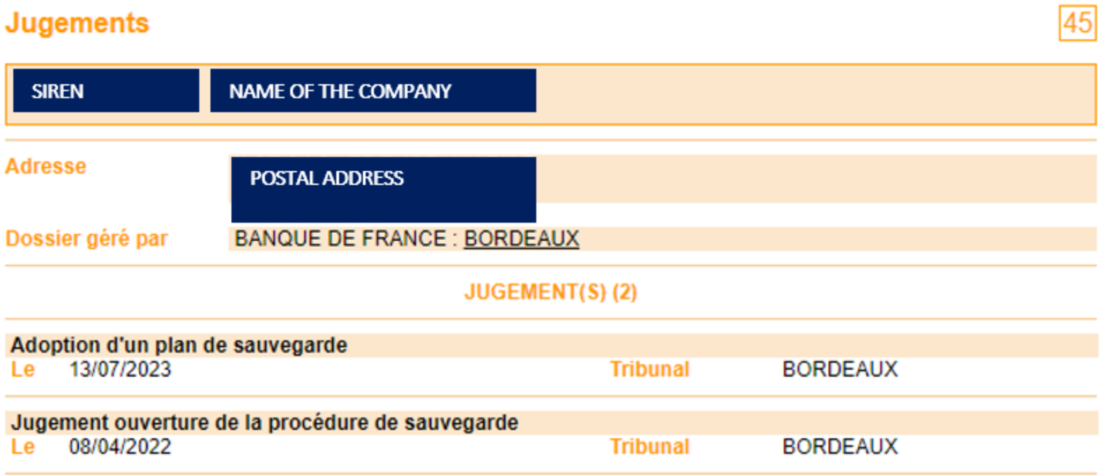
Appendix A FIBEN

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

Cotation et son explication

37

SIREN	NAME OF THE COMPANY	Cotation : X5 depuis le 14/04/2022 (actualisée le 07/08/2023)

Adresse	POSTAL ADDRESS
Dossier géré par	BANQUE DE FRANCE : BORDEAUX
Activité BDF	INDUSTRY

COTE D'ACTIVITÉ

X : CA inconnu ou trop ancien — Attribuée le 21/12/2011

COTE DE CRÉDIT

5 : **La capacité de l'entreprise à honorer ses engagements financiers est jugée fragile**

Attribuée le 14/04/2022

Positionnement de l'entreprise sur l'échelle de cotation

0 | 1+ 1 1- 2+ 2 2- 3+ 3 3- 4+ 4 4- 5+ 5 5- 6+ 6 6- 7 8 P

EXPLICATION DE LA COTE DE CRÉDIT

Éléments d'analyse	Observations	Élément(s) Déterminant(s)
Incidents de paiement effets et créances douteuses	Pas d'information défavorable	NON
Situation des représentants légaux	Pas d'information défavorable	NON
Appartenance à un groupe consolidant	Pas d'information défavorable	NON
Perte de plus de la moitié du capital social	Pas d'information défavorable	NON
Décision judiciaire (Tribunaux de Commerce)	**Jugement(s) rendu(s) par une juridiction commerciale ou assimilée** **Pour plus de renseignement : Module 45 (Jugements)**	OUI
Date de mise à jour : 14/04/2022		

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").

Appendix B Robustness

B.1 TWFE Robustness

Following on Section 4.3.1, this appendix provides additional tests that challenge the baseline results for time, group, and cohort heterogeneity, as well as the parallel trend hypothesis.

B.1.1 Heterogeneous treatment effect

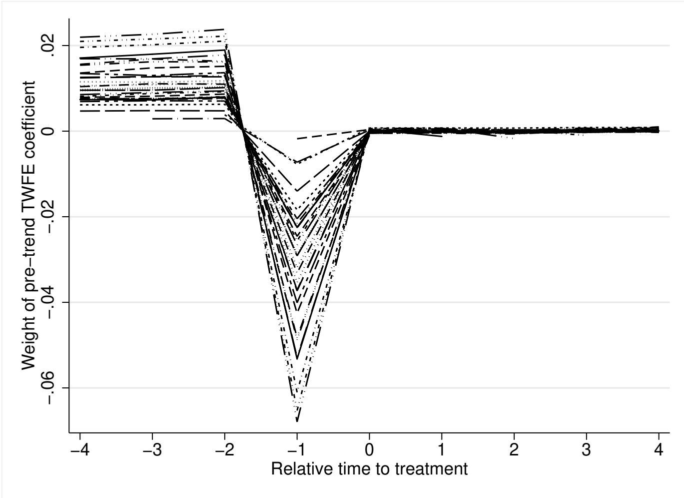
De Chaisemartin and D'Haultfoeuille (2020), Borusyak et al. (2022), and Goodman-Bacon (2021) highlight that TWFE regressions may not always estimate a convex combination of treatment effects in the presence of negative weights. In the worst-case scenario, it could mean that the DID estimates a positive 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 `twowayfeweight` 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_multiplegt` 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



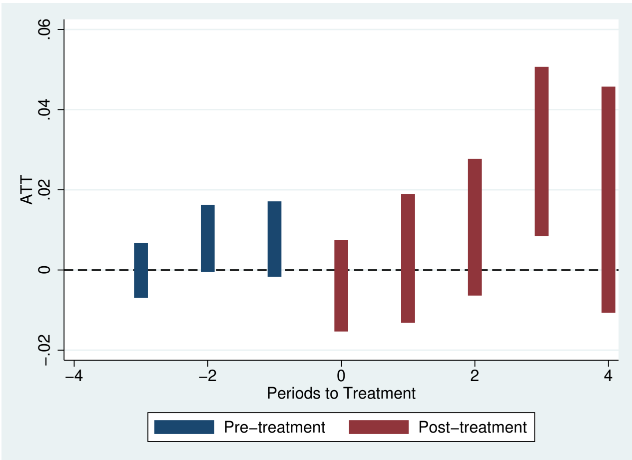
Note: Figure B.3 shows the weights underlying the pre-trend coefficient two quarters from the flag removal. Each line is a different cohort from 2012 Q1 to 2019 Q4.

B.1.2 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 SantAnna (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.

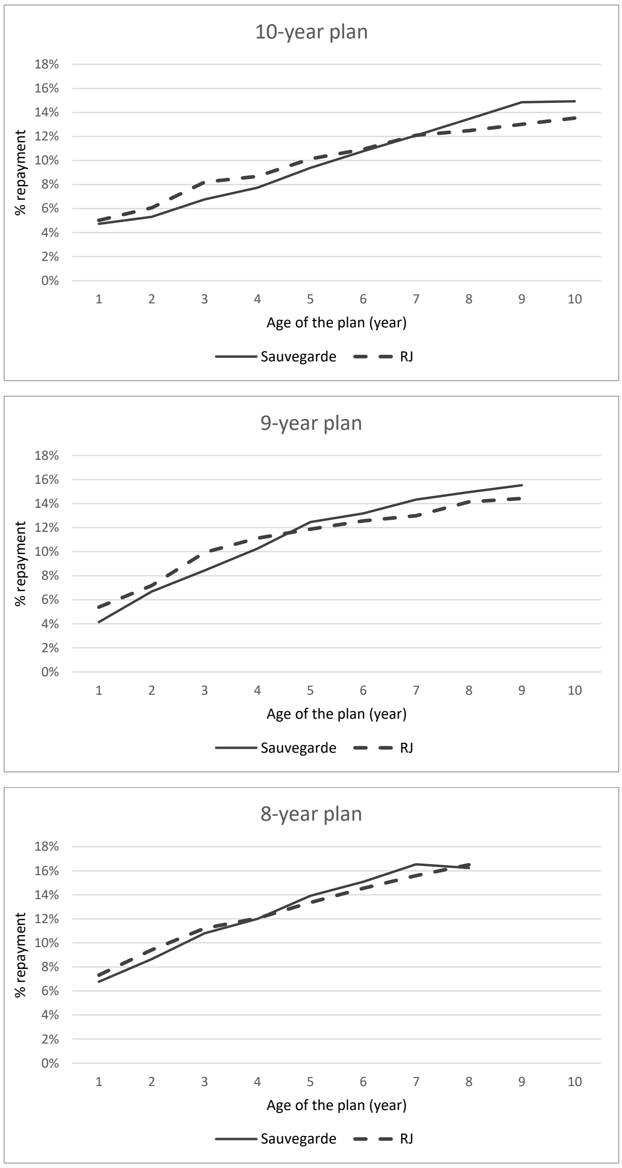
Without covariates, the Cramér-von Mises test for the parallel trend rejects the parallel trend assumption at the 10% level (p-value = 0.0743). When I introduce calendar time × industry fixed effects as covariates, the results illustrated in Figure B.4 confirm the parallel trend hypothesis that the pre-trends 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 × industry fixed effects, the Cramér-von Mises test indicates a p-value of 0.0929 for the parallel trends plausibility test. Thus, my setup seems better off with calendar time × 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 × 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-R^2 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 4.3.2 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		
	Summary Statistics					
	Control	Treatment	Diff	Control	Treatment	Diff
Lenght of the plan (years)	9.601	9.526	0.075*	9.564	9.534	0.031
	(0.889)	(0.994)	[1.857]	(0.949)	(0.990)	[0.619]
Log(assets)	5.842	6.414	-0.572***	6.137	6.404	-0.267
	(1.023)	(1.117)	[-12.318]	(0.980)	(1.112)	[-4.975]
Investment	0.051	0.046	0.005	0.042	0.046	-0.003
	(0.131)	(0.119)	[0.870]	(0.117)	(0.119)	[-0.498]
Leverage	1.276	1.067	0.208***	1.133	1.069	0.063
	(0.644)	(0.484)	[7.647]	(0.484)	(0.484)	[2.535]
Long term/Total credit	0.588	0.633	-0.044***	0.617	0.633	-0.017
	(0.364)	(0.364)	[-2.747]	(0.362)	(0.364)	[-0.888]
Cash	0.065	0.068	-0.003	0.063	0.068	-0.005
	(0.089)	(0.082)	[-0.684]	(0.081)	(0.082)	[-1.088]
Rating (Y/N)	0.501	0.586	-0.085***	0.543	0.585	-0.042
	(0.500)	(0.493)	[-3.820]	(0.498)	(0.493)	[-1.626]
Log(Number of banks)	2.117	2.408	-0.291***	2.233	2.410	-0.177
	(1.766)	(2.182)	[-3.506]	(1.803)	(2.188)	[-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	Post-Match
	(1)	(2)
Lenght of the plan (years)	-0.0413	0.0537
	(0.427)	(0.376)
Log(assets)	0.477***	-0.0527
	(0.000)	(0.765)
Investment	0.0296	0.508
	(0.941)	(0.283)
Cash	1.415**	0.769
	(0.011)	(0.347)
Leverage	-0.464***	0.0584
	(0.000)	(0.725)
Long term/Total credit	0.505***	-0.126
	(0.003)	(0.627)
Rating (Y/N)	0.182	0.00756
	(0.106)	(0.959)
Log(Number of banks)	-0.0116	0.0448
	(0.911)	(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

	%Δ Credit	Pr(New banks)
	(1)	(2)
Post	-0.00594***	-0.00199
	(0.001)	(0.356)
Treated × Post	0.0118**	0.0171*
	(0.039)	(0.065)
Notification × Post	0.0619**	0.0160
	(0.017)	(0.683)
Firm FE	✓	✓
Quarter × 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 firms[39]) 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)).

[39] 2,840 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 × 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 × 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*	0.0377	-0.155	0.0388***	-0.151	-0.00513	-0.409*	0.000220
	(0.088)	(0.328)	(0.439)	(0.006)	(0.476)	(0.679)	(0.081)	(0.999)
Post	-0.00599***	-0.00205	-0.0872***	0.00298	-0.0133	-0.000423	0.0302*	0.00351
	(0.010)	(0.312)	(0.001)	(0.300)	(0.519)	(0.860)	(0.059)	(0.870)
Treated × Post	0.0156***	0.0168*	0.0746**	-0.0120***	0.00186	0.000346	-0.0874	-0.00495
	(0.009)	(0.068)	(0.048)	(0.000)	(0.944)	(0.880)	(0.184)	(0.783)
Firm FE	✓	✓	✓	✓	✓	✓	✓	✓
Year × 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

Appendix 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's[40]. 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 Exercise[41] 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:

$$\begin{aligned}\%\Delta Credit_{i,q} = \beta_0 Post_q + \beta_1 (Post_q \times Treated_i) \\ + \beta_2 Low_{i,t-1} + \beta_3 (Low_{i,t-1} \times Treated_i) + \beta_4 (Low_{i,t-1} \times Post_q) \\ + \beta_5 (Post_q \times Treated_i \times Low_{i,t-1}) + \gamma_i + \gamma_{s\times t} + \epsilon_{i,q}\end{aligned} \tag{6}$$

In this specification, β_5 captures the differential effect of the flag removal on

[40]See Methods of calculating of prudential ratios under the CRDIV, https://acpr.banque-france.fr/

[41]See 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***	-0.00529*	-0.00480**	-0.00419*
	(0.008)	(0.078)	(0.028)	(0.059)
Treated × Post	0.0184***	0.0160**	0.0127***	0.0150***
	(0.004)	(0.027)	(0.001)	(0.000)
Low Tier1	-0.00754*	-0.00229	-0.00437	-0.00308
	(0.094)	(0.636)	(0.580)	(0.794)
Treated × Low Tier1		-0.0207		0.0224
		(0.146)		(0.431)
Post × Low Tier1		-0.00326		-0.0106
		(0.495)		(0.145)
Treated × Post × Low Tier1		0.00798		-0.0298
		(0.470)		(0.141)
Firm FE	✓	✓	✓	✓
Quarter × 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

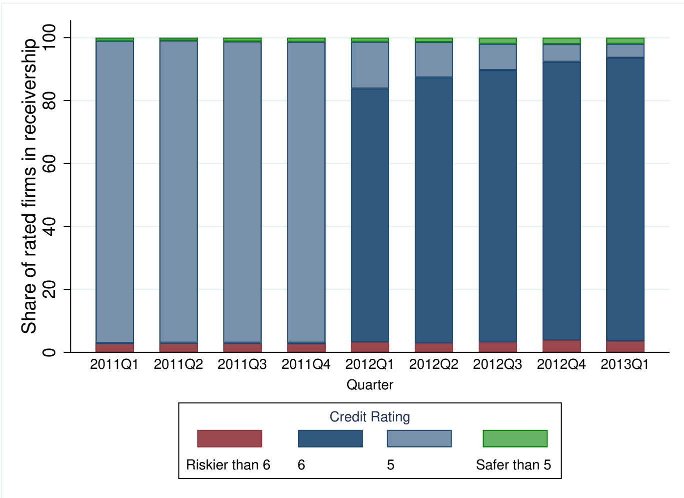
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

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.

Appendix 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



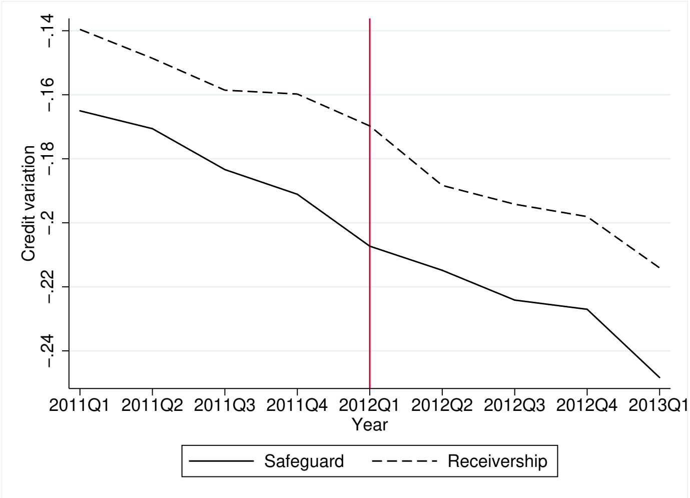
Note: Figure D.1 shows the proportion of rated firms in receivership each quarter around the 2012 policy change.

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:

$$\%\Delta Credit_{i,q} = \beta \left(Post2012 \times Treated'\right) + \gamma_q + \gamma_i + \gamma_{s \times t} + \epsilon_{i,q} \tag{7}$$

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

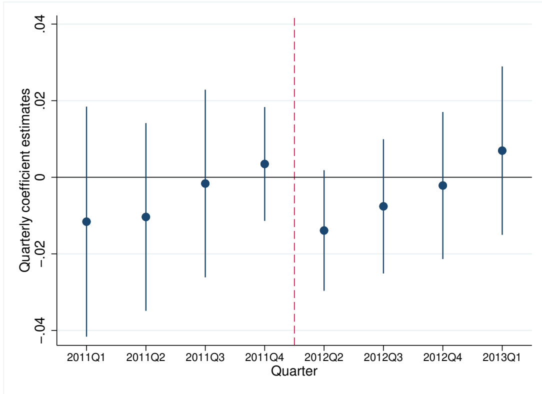
(a) Average effect

	(1)
	% Δ Credit
Treated' × Post2012	0.00406
	(0.34)
Firm FE	Yes
Quarter × Industry FE	Yes
Quarter of the plan dummies	Yes
Observations	22,836
Adj. R^2	0.918

t statistics in parentheses

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

(b) Quarterly effect



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.