Using Heterogeneity in Commercial Court Decisions
to Measure Indirect Costs of Bankruptcy Filing in France

Anne Epaulard and Chloé Zapha[*]

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ABSTRACT

This paper measures the indirect cost associated with public court-supervised bankruptcy procedure in France. The procedure (known as Redressement Judiciaire or *RJ*) is widely used in France but only around 25% of firms succeed at renegotiating their debt. In 2006, a new bankruptcy procedure called Sauvegarde was introduced into the French commercial law. Only firms that are not (yet) insolvent can fill for this new procedure. More than 60% of these firms succeed at renegotiating their debt under this procedure. A court can decide to convert a Sauvegarde case into an *RJ* if it considers the firm is already insolvent or on the verge of insolvency. Courts can differ in judging which financial situation triggers conversion. Using court conversion rates as an instrument, we measure the impact of conversion on debt restructuring. We estimate that 36% of firms filling for Sauvegarde are at the margin of being converted (or not) to *RJ*. For these marginal firms, conversion reduces significantly the chance of restructuring their debt. As the two procedures differ only slightly but in name, a possible interpretation of our results is that the track-record of the *RJ* is so bad, that it amounts to a stigma on the firm that significantly reduces its survival chances.

JEL Classification: G33, K22

Firms filing for bankruptcy have to convince their clients, trade creditors, employees and suppliers to keep doing business with them. Failing to do so increases financial weaknesses of these firms, and reduces further their chances to renegotiate their debt. The financial literature refers to this as indirect costs of financial distress[2]. These indirect costs are difficult to measure but the general view is that they are quite substantial[3] and significantly larger than bankruptcy direct costs.

This paper contributes to the literature that seeks at measuring the indirect costs associated with bankruptcy by taking advantage of the coexistence, in France, of two court-supervised bankruptcy procedures (in addition to liquidation). The two bankruptcy procedures that coexist in France bring radically different results in term of success of debt restructuring. Using the fact that some cases are converted from one bankruptcy procedure to the other by commercial courts we aim at measuring the cost associated with being converted to the procedure with the lower rate of success. We apply our identification strategy on an (almost) exhaustive sample of bankruptcy filings in France over the period 2010 – 2016.

The main bankruptcy procedure available to French firms is known as Redressement Judiciaire (RJ). Its current form dates from 1985. In many dimensions RJ is similar to the US Chapter 11 procedure. It differs from the US Chapter 11 by the fact that only firms that are already in a dire financial situation can (and must) fill for it. Depending on the economic situation, between 30 000 and 50 000 firms enter an RJ each year. After an observation period that can last up to 18 months the firm either reaches an agreement with its creditors to restructure its debt or is liquidated[4]. This bankruptcy procedure results in low chance of survival for the firm. The data set we built by collecting all initial RJ filings in France over the period 2008 – 2016 and their outcomes up to June 2018, shows that only around 25% of firms entering RJ manage to restructure their debt. Because of this low rate of survival in RJ, a firm filing for RJ will

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[2] Indirect costs arise because of inter- or intra-group conflicts of interest, asymmetric information, holdout problems, lost sales and competitive positions, higher operating costs, and ineffective use of management's time Altman, 1984; Opler and Titman, 1994; Bris, Welch, and Zhu, 2006; Almeida and Philippon, 2007.

[3] Observing and measuring indirect bankruptcy cost is difficult, and economists need to come up with methods to infer these costs. Given the variety of methods and natural experiments used to measure indirect costs of bankruptcy the comparison of the estimated magnitude of indirect cost is not straightforward. Indirect costs have been estimated to vary between 10% and 23% of firm value given default (e.g. Adrade and Kaplan, Bris, Welch and Zhu, 2006, Hotchkiss et al. 2008, Davydenko, Strebulaev, and Zhao, 2012 and Reindl, Stoughton, and Zechner (2017) infer bankruptcy costs from market prices. Hortaçsu et al., 2013, show that the financial distress of car manufacturer reduces the price of their product observed in the second market. They interpret these results as showing that there are substantial indirect costs of financial distress for car manufacturers.

[4] In liquidation, the firm's assets are either sold to a single buyer with the purpose of keeping the firm as a going concern (potential buyers compete by sending bids to the court that chooses amongst them), or put on the market as the firm is winded down.

immediately be viewed as very fragile by its suppliers, creditors, and clients. This is part of the indirect cost associated with bankruptcy.

In 2006, a new bankruptcy procedure, called *Sauvegarde* (Safeguarding) was introduced into the French commercial law. This new procedure aims at giving firms time to restructure their debt and business before they reach the very weak financial situation that triggers *RJ* filing. Even if most of the provisions of the law regarding this bankruptcy procedure are similar to that of RJ, the *Sauvegarde* brings more positive results: according to our dataset, more than 60% of firms filling for *Sauvegarde* manage to restructure their debt. This higher restructuration rate is directly linked to the better financial situation of *Sauvegarde* filers. It might also come from pro-active managers willing to act early to solve financial difficulties of their firms, rather than waiting to reach insolvency to start an *RJ* procedure. Interesting enough, even though the *Sauvegarde* procedure resembles the *RJ*, the precise wording of the law tries to distinguish as much as possible between the two bankruptcy procedures, and the Banque de France in its monthly bulletin about new defaulting firms does not count *Sauvegarde* filings[5].

We build a dataset of all public bankruptcy filings over the period 2010 to 2016 and their outcomes till June 2018. This is done by using public daily records of main decisions by commercial courts. Our initial data set contains 7,547 *Sauvegarde* filings, of which 909 (12.0%) were converted into *RJ*. We then merge this data set with annual fiscal records (income statement and balance sheet) of these firms provided by INSEE. Given attrition, our final sample contains 6,283 *Sauvegarde* cases, of which 797 of (12.7%) were converted into *RJ*.

To measure the stigma attached to *RJ* and associated indirect costs, we use the fact that a significant share of *Sauvegarde* cases are subsequently converted into RJ by the court. Firm assignment to commercial court is based on the firm headquarter location, which prevents "forum-shopping" by firms. The court can convert a *Sauvegarde* case into an *RJ* if the assessment of the financial situation of the firms shows that the firm is already insolvent or on the verge of insolvency. But the exact situation that triggers conversion may receive different interpretations by different commercial courts. Indeed, there is a large heterogeneity of yearly conversion rates amongst the 134 commercial courts. They range from 0% to 100%. Heterogeneity in conversion rates remains high even after controlling for characteristics of local firms and local economic conditions. We use this heterogeneity (in space and time) in conversion rate to build an instrument to identify the impact of conversion on the probability of a successful debt restructuration.

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[5] "The counts presented in this "Stat Info" cover *RJ* and liquidations, as of the date of judgment, insofar as these collective proceedings give rise to the filing of a declaration of cessation of payment, which is not the case for the opening of Sauvegarde proceedings." Translated by the authors from https://www.banque-france.fr/sites/default/files/media/2018/11/13/methode_stat_info_defaillances_9-juin-2016.pdf

Our methodology builds on several empirical papers that use judicial decisions heterogeneity as an instrument to measure the impact of a decision. For example Chang and Schoar (2013) use judges debtor friendliness to explain firm survival after Chapter 11, and Bernstein et al. (2016) and (2017) use judge conversion rate to Chapter 7 of Chapter 11 cases to explain reallocation of assets and bankruptcy spillovers. In a different field, Maestas et al. (2013) use examiner heterogeneity in granting disability benefits to measure the impact of receiving these benefits on labor supply. Their identification strategies are based on a random allocation of judges or examiners to cases. Unlike theses papers we don't have data on judges but only on Commercial courts, and allocation of bankruptcy cases to court is not random as it depends on the firm headquarter location. To ensure that our instrument is valid, we perform a number of empirical tests to show that we are in condition close to a random assignment once we control for firm specific characteristics and local economic conditions. Notably, we show that, at the court - year level, past conversion rates are not associated with the current relative number of *Sauvegarde* filings, and current court conversion rate is not associated with past relative number of *Sauvegarde* filings.

Overall our empirical results suggest that indirect costs associated with *RJ* are high. Around 36% of firms filing for Sauvegarde are on the margin of being (or not) converted to *RJ*. For these marginal firms, conversion reduces significantly the chance of restructuring their debt (the point estimates is 0.664, meaning that the probability to restructure the debt is reduced by 0.664). We perform various robustness checks and none of them alters these results. Perhaps the most interesting of these robustness checks uses the fact that in mid-2014 new provisions were introduced into the law regarding the list of stakeholders allowed to bring a conversion case before the court. As of September 2014, the judge him or herself can no longer bring the conversion case before the court, a prerogative granted only to the management of the firm, the court-appointed administrator (if any), the court-appointed receiver and the public prosecutor's office. Restricting our sample to 2013-2015 filings, the change in the law is associated with a significant reduction in the share of *Sauvegarde* cases on the margin of being converted (33% before September 2014, 27% after). However, conversion to *RJ* (instrumented as before) reduces the probability of a debt restructuring agreement by the same level before and after the change in the law.

The remainder of the paper is as follows. Section 1 discusses the two bankruptcy procedures in France. Section 2 presents our original dataset and main characteristics of bankruptcy procedure outcome. Section 3 discusses the identification strategy, notably because firms are not allocated randomly to a Commercial Court but according to their headquarter location we need to perform a number of tests to check the validity of our instrument. Section 4 contains our main econometric results, their discussion and robustness checks. Section 5 concludes.

I.   Bankruptcy Procedures and Commercial Courts in France

A.   *Bankruptcy procedures*

The French commercial law is such that a firm that cannot face its payment obligations is considered as insolvent if its payment obligations are not covered by its liquid assets. Insolvent firms should fill for *RJ* within 45 days. In practice, some insolvent firms do not fill for RJ within these 45 days. In these cases, firm's creditors, as well as the court itself, can bring the case before the court to trigger *RJ*. For firms facing even more severe financial distress, there is a possibility to fill directly for liquidation (a procedure akin to US Chapter 7).

Once the enterprise fills for RJ, a 6-month "observation period" starts during which the financial situation of the firm is assessed. During this period a court appointed receiver is in charge of establishing the list of liabilities of the firm. The court also nominates an insolvency administrator that monitors the day to day operations of the firm, notably all its financial transactions as well as some important restructuration decisions (firing of employees, selling of assets). The administrator can also forbid the firm manager to take some actions that would reduce the value of the assets of the firm. The administrator and the receiver negotiate with creditors to establish a debt restructuring plan. If the situation deteriorates further, the firm can be liquidated even before the end of this 6-month period. Most of the time, the observation period is renewed for 6 months. The observation period can be renewed twice, lasting up to 18 months in total. Anytime during the observation period, the court can validate (or reject) a debt restructuring plan negotiated with creditors. The typical plan is a mixed of debt rescheduling (up to a 10-year horizon) and hair-cut (if given the option, some creditors prefer an immediate partial payment as the final balance of all accounts rather than a rescheduling). Anytime during the observation period, if the situation deteriorates further and there is no hope to reach an agreement with creditors to keep the firm as a going concern, the court can decide that the firm should be liquidated. Liquidation can take two forms. In the most abrupt form, the assets of the firm are sold on the market, and proceeds go to firms' stakeholders according to priority rules (with employees having the highest priority for unpaid wages). A smoother form of liquidation sees the opening of a bidding process for the all or part of the assets of the firm and some or all of its employees, with a view to keep at least part of the firm going. The court receives bids by potential buyers and chooses amongst them. If there is no potential buyer or the court rejects all the offers, the firm ceases its operations, its assets are sold, and stakeholders reimburse according to the same priority rule as before.

In its current form, this procedure dates from 1985. We collected data on all openings of procedure from 2008 to 2016, and observed the outcome up to June 2018 (see the section "Data and Summary statics" for details). Over the period 2008 – 2016, we recorded 133,065 RJ openings (of which 69,465 were voluntary

fillings, and the other triggered either by creditors or by the court itself). Only 27.5 % of firms that started an RJ managed to restructure their debt and the survival rate of firms that manage to restructure their debt is low: only 43.3% are surviving after 5 years. These numbers are slightly better for firms that voluntary fill for an RJ, with 23.3% restructuring their debt and a 69.1% 5-year survival rate.

The bad track-record of RJ in terms of debt restructuration and survival of firms increases the challenge for firm to persuade its clients, trade creditors, employees and suppliers to continue doing business with it once the filing is made public. This reduces further the chances to renegotiate debt. In 2006 a new bankruptcy procedure was introduced into the French commercial law[6]. It is known as "Sauvegarde". Firms can fill for this "Sauvegarde" procedure if they are not (yet) insolvent[7] but face financial difficulties that they consider impossible to overcome without a debt restructuration. The "Sauvegarde" procedure is otherwise quite similar to the RJ: it is public[8], the 6-month observation period is renewable twice, and the Court appoints a receiver. The Court can also appoint an administrator and must do so for the largest firms (in the RJ, the Court appoints an administrator regardless of the size of the enterprise). The role of the administrator is slightly less important in Sauvegarde than in RJ: it only assists the manager and cannot make decisions without the consent of the manager as it is the case in RJ. Anytime during the observation period – at the request of the administrator, the receiver, the Public Prosecutor's Office or (since September 2014) the firm itself – the case can be converted into an RJ case by the Court. Up to July 2014, the Court itself could request the conversion of the case.

Raw numbers show that, over the period 2010 – 2016, 64.2% of firms that filed for Sauvegarde manage to restructure their debt, three times more than firms entering RJ. These firms are also more likely to survive after debt renegotiation: 5 years after restructuration 61.9% are still operating (compare with 43.3% for RJ firms). These numbers cannot be taken at face value to measure the benefit of Sauvegarde over RJ since firms entering these two bankruptcy procedures do not start the bankruptcy procedure with the same financial difficulties. By design, firms filing for Sauvegarde face less financial difficulties than those filing for RJ. Also, firms filing for Sauvegarde may have non observable characteristics that set them apart from those filing for RJ and have an impact on the outcome of the bankruptcy procedure: more proactive or better informed business leader.

B. Commercial Courts

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[6] The bankruptcy protection act of July 26, 2005, started to apply on January 1st 2006.
[7] The value of their liquid assets is higher than their short term debt
[8] All openings of Bankruptcy procedures (Sauvegarde, RJ and Liquidation), as well as all the main Court decisions during the procedure are published in a daily publication that is available online since 2008.

There are currently 134 commercial courts on the French territory[9]. There is at least one commercial court in each of the 95 "départements" with some "départements" having 2 or even 3. These courts are in charge of bankruptcy proceedings for companies as well as commercial disputes. Bankruptcy proceedings represent 20% of the cases heard by the commercial courts. In bankrutcy, the firm is assigned to the relevant court on the base of the firm headquarters' location and there is no possibility of "forum shopping" either for the firm or its creditors.

There is around 3 000 commercial judges. They are volunteers and unpaid. They are chosen from among entrepreneurial leaders and elected by them. They receive legal training after being chosen and during their term of office. Each judge is initially elected for an initial two-year mandate, and can then be reelected 3 times for a 4-year mandate (so the maximum time a judge can serve is 14 years). This ensures a regular turnover amongst judges. Each judge sits only one or two half-days a week and goes on with its normal business activities the rest of the time. For obvious reasons, judges cannot work on cases related to their own business.

II. Data and Summary Statistics

A. Data Sources

The data used in the study is drawn from bankruptcy filings contained in the "*Bulletin officiel d'annonces civiles et commerciales*" (BODACC) provided by the registry offices of the commercial courts. This information is public and in electronic format since January 2008[10], with one electronic file per business day. We build a dataset of all bankruptcy filings in France over the period 2010-2016 and follow-up on their outcome till June 2018. Notably, we identify Sauvegarde cases converted to RJ. Our dataset contains 315,365 initial filings, of which 7,700 Sauvegarde filings, 94,467 RJ filings, 213,178 direct liquidations). We are able to follow-up on 7,547 Sauvegarde cases (98%) and 93,467 RJ cases (99%). By definition, there is no follow-up for liquidations. We call this sample of cases for which we have follow-ups our "initial sample" (see upper part of Table 1). For these cases, the set of information includes the address of the firm, the commercial court in charge of the case, the date of filing as well as the date of subsequent main judgements by the Court (e.g. renewal of observation period, conversion to RJ, agreement of a debt restructuration deal with creditors, liquidation...). There is however no indication regarding other meaningful information as for example the number of employees, the industry, total sales…. There is no

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[9] While the French commercial law does not differ from one Court to the other, some parts of the French territory were not included into our research. We excluded the courts relevant in 3 départements (Moselle, Haut-Rhin and Bas-Rhin) because, for historical reasons, their functioning slightly differs from that of courts on the rest of the territory. Oversea departments and territories were excluded for the same reason. In addition,

[10] https://www.data.gouv.fr/fr/datasets/bodacc/

indication either regarding previous out-of-court restructuring, and for RJ whether the filing is voluntary or triggered by either the court or creditors.

We complement these data with additional bankruptcy information provided by the CNAJMJ[11]. Notably, the CNAJMJ dataset contains information regarding the existence of a previous out-of-court debt restructuring, and whether an RJ filing is voluntary or not. However, this information is partial: if an indication of a previous out-of-court restructuration means there was one, the absence of any mention of a previous out-of-court restructuration doesn't mean there was none. The same can be said regarding the nature of the RJ. Because we have doubts about the completeness of this data set, we will use information from this source with caution and only for robustness checks.

Finally, data on the economic and financial situation of the firms over the period 2009-2015 are extracted from the balance sheets and income statements made available by INSEE, the French statistical office. This gives us the last available information of the firm before it fills for bankruptcy. And we have data regarding the number of employees, total assets, total debt and its structure (bank, suppliers, other), interest payments, total sales, operational income, industry, age, legal form…). Because of attrition, our final sample (to which we will refer to "working sample") contains 6,283 Sauvegarde cases, 797 (12.7%) of them converted to RJ, and 66,142 RJ filings (see bottom part of Table 1).

B. Summary Statistics

Table 1 reports the composition of the both the initial and working samples. As far as Sauvegarde is concerned, the two samples are very similar, as one would expect since the working sample contains more than 83% of cases recorded in the initial sample. The share of cases converted into RJ is 12.0% in the initial sample and 12.7% in the working sample. 64.2% of Sauvegarde filings lead to a debt restructuring in the initial sample, and 64.6% in the working sample. The attrition rate between the two samples in RJ is higher, and the working sample contains only 68% of the cases recorded in the initial sample. Yet, the restructuration rate is quite similar between the two samples (28% in the initial sample and 32% in the working sample).

On average, 64.6% of firms filing for Sauvegarde in the working sample reach a restructuration agreement with their creditors. This number drops to 23.5% for Sauvegarde filings converted into RJ, a rate similar to the one observed for voluntary RJ filings (25.4%). For firms that manage to restructure their debt, the two years survival rate of all Sauvegarde is 81.2% and about the same for the two subgroups: 81.1% for Sauvegarde cases that were not converted and 83.1% for Sauvegarde cases that were converted to RJ. The

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[11] "*Conseil National des Mandataires Judiciaires et Administrateurs Judiciaires*" is an association of court-appointed receivers and insolvency administrators.

difference in the 5-year survival rate of the two subgroups is slightly higher: 60.1% for Sauvegarde cases that were not converted and 52.4% for Sauvegarde cases that were converted to RJ, but the difference is not significantly different from zero in the working sample.

Regarding the financial and economic situation of firms in our sample, the average firm filing for Sauvegarde has 28 employees, is 12 year old, its debt to asset ratio is 81%, and supplier debt represents more than 25% of the total debt. Finally, 58.4% of firms filling for Sauvegarde are labelled as "zombie". A firm is considered as a "zombie" if its financial obligations were higher than its operational income the year preceding the filing[12].

Firms which Sauvegarde filing is not converted to RJ are on average younger compared to firm which Sauvegarde filing is converted to RJ (11.3 year-old versus 13.5 year-old), have less supplier debt (26.0% of their total debt versus 29.4%) and are less likely to be zombies (57.2% versus 66.2%). A simple test of equality of means reveals that these differences are statistically different from zero (see column 5 of Table 1). These differences point to a selection into the two groups that need to be treated.

There is a huge heterogeneity amongst commercial courts. A commercial court registers on average 709 initial bankruptcy filings per year (*Liquidation, RJ* and *Sauvegarde*), with a median of 455 cases and a standard deviation of 737. The first percentile equals 77 filings per year, while the ninety-ninth reaches 3643 filings per year, attesting for the large variety of courts in size. Table 2 presents a breakdown of the number of Sauvegarde filings per year over 2010 – 2016 for both the initial and working samples, and the percentage of cases that were subsequently converted. The number of filings increases steadily until 2013, stabilizes in 2014 and declines significantly in 2016. The share of cases converted ranges between 9.4% and 16.7%, and is lower on the second half of the period.

II. Identification Strategy

A. Empirical Design

To measure the indirect cost of *RJ*, we focus on firms that filled for Sauvegarde and exploit the fact that a significant fraction of them (12.7%) are subsequently converted to *RJ* by the court. This allows us to eliminate non observable characteristics of firms filing for Sauvegarde compare to RJ filers.

A.1 The impact of conversion to RJ on the probability of reaching debt restructuration deal with creditors

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[12] Our definition of "zombie" firm is more severe than the one used by OECD for which a firm is "zombie" if it is more than ten years old and with financial obligations higher than its operational income for more than three consecutive years (see Adalet McGowan and al. (2017)).

Following Bernstein et al. (2016), the specification of our model is:

$$Y_{i,t,t'} = \alpha + \beta \cdot Conversion_{i,t'} + \gamma_1 X_{i,t} + \gamma_2 \Omega_{j,t'} + \mu_k + \mu_l + \mu_r + \mu_t + \epsilon_{i,t,t'} \qquad (1)$$

where the dependent variable $Y_{i,t,t'}$ is the probability for the firm i entering the procedure in year t to restructure its debt. t' indexes the year at the end of the observation period, that we will refer to as the *year of judgment*. $Conversion_{i,t'}$ is a dummy equal to one if the Sauvegarde case has been converted to *RJ* at the year of judgment t'. We want to estimate β, the effect of conversion to *RJ* on $Y_{i,t}$. The firm-level control variables $X_{i,t}$ such as pre-bankruptcy filing financial ratios, employment and age, are the last available information before the year of filing t. The court-level control variables $\Omega_{j,t'}$ such as the size of the court or its share of direct liquidations, are indexed at the year of judgement t'. We include the fixed-effects μ_k, μ_l, μ_r and μ_t for, respectively, the industry, legal form, region and year of filing. Standard errors are clustered at the court-by-year of judgment level. Under the null hypothesis that conversion to *RJ* has no effect on $Y_{i,t,t'}$, β should not be statistically different from zero. A negative β would mean that a conversion to *RJ* reduces the chance of reaching a debt restructuration agreement.

Despite the many controls, there remain sources of endogeneity. The conversion of a filing suggests a firm's deteriorated financial health that mechanically reduces the firm's chances of survival. Therefore, to identify the causal effect of conversion to *RJ* on firm continuation, we rely on commercial court heterogeneity in their propensity to convert Sauvegarde to *RJ* as an instrumental variable.

Our identification strategy is based on the fact that while the French bankruptcy law is uniform at national level, its interpretation can differ from one court to the other. This is especially true in the case of the decision to convert a Sauvegarde case into an *RJ* case. A Sauvegarde case can be converted to an *RJ* if the firm becomes insolvent or close to insolvency during the observation period, where insolvency defines a situation where short term debt is higher than the value of the liquid assets of the firms. Not only the valuation of liquid assets of the firm is subject to interpretation, but also what constitutes "closeness to insolvency" can differ from one court to another. Our instrument relies on this variation of interpretation to introduce exogenous variation.

This instrument is constructed as follows:

$$\phi_{ij,t'} = \frac{n\_conversion_{j,t'} - 1(converted_{i,t'} = 1)}{n\_cases_{j,t'} - 1}$$

where $\phi_{ij,t'}$ captures for the court j the number of cases i converted related to its total number of Sauvegarde cases the court judges in year t', excluding the present case. This measure follows Maestas et

al. (2012). By construction it excludes the mechanical relationship that would exist between the instrument for a given case and its conversion decision. To take into account the fact that a large proportion of judges within each court is renewed each calendar year, $\phi_{ij,t'}$ is estimated by year of judgment (t'). As judgments of filing in 2016 can take place up to 2018, the instrument covers the period 2010-2018.

Table 3 reports the mean and standard deviation of the instrument (annual court conversion rate) per year. Every year, some court never converts, while other always does. The share of case converted is declining from 2014, going from 16.2% of cases converted in 2012 to 11.3% in 2015. The median is decreasing as well. But the heterogeneity remains, as pointed out by the standard deviation which stays above 16% over the period (with the exception of the first half of 2018). This heterogeneity is used to introduce exogenous variation in the model, but this instrument is only valid under several requirements and assumptions that are discussed below.

A.2 The impact of conversion to RJ on the survival rates after restructuration deal

TO BE COMPLETED

B. *Assignment to Commercial Courts*

Our initial sample covers the 134 commercial courts across France. The territorially competent court of a firm filing for bankruptcy is that located in the same geographical territory as its headquarters. Thus, the firm does not have a choice of its court (preventing forum shopping). However, because it depends on the location of the firm, the assignment of commercial court cannot be considered as random. To consider the assignment of firms to commercial courts as if random, we need to test two additional assumptions and to control for a set of firm characteristics and local economic conditions.

Firstly, the firm's decision to filing for *Sauvegarde* must not be influenced to the court's propensity to convert *Sauvegarde* cases. Indeed, if a firm is discouraged to fill for *Sauvegarde* because it is aware that its court has a high conversion rate of *Sauvegarde* cases to *RJ*, the instrument would be biased. We test this assumption by studying the correlation between a court's share of *Sauvegarde* cases relative to its total number of direct *Sauvegarde* and direct RJ filings, and its conversion rate of the preceding years. We show (Table 4) that there is no relation between these rates, supporting as evidence that past court conversion rate has not impact the firms' decision of filing for *Sauvegarde*. Put differently, courts do not have a track-record regarding conversion to *RJ* that would impact the firm behavior towards entering *Sauvegarde*. Secondly, we need to make sure that the court's conversion rate is not a biased reflection of the local population of firms. Our instrument would not be exogenous if the share of cases converted was

increasing with the number of *Sauvegarde* entries. We show (Table 5) that the number of *Sauvegarde* filings does not influence the court's share of cases converted, as the coefficients of interest are not statistically different from zero.

Finally, to take care of any remaining endogeneity, we include court level and local control variables. We introduce the court's share of direct liquidations (relative to the total of direct liquidations and direct RJ filings) as an estimator of the health of the local population of firms (a high rate of direct liquidations suggesting a less financially healthy firm population). We also include the *département's* unemployment rate as a proxy for the local economy (the department is that of the firm headquarter, and also that of commercial court since firms are assigned to a court in their department). Finally, we include the total number of filings (direct liquidations, RJ and Sauvegarde filings) per court to capture the size of the court. All these control variables are calculated on an annual basis.

We can then consider the court assignment as if random conditionally on these controls, and use annual court conversion rates as source of exogenous variation in the probability that a given case is converted. Figure 1 shows the distribution of the courts conversion rates, minus the annual mean over all court, adjusted and unadjusted for the controls and fixed effects described in specification (1). The set of controls does not reduce the standard deviation the instrument distribution (0.16 compared with 0.15 unadjusted) that we will exploit in our model.

The first stage of our instrumental variable approach is as follows:

$$Conversion_{i,t'} = \rho + \pi \cdot \phi_{ij,t'} + \lambda_1 X_{i,t} + \lambda_2 \Omega_{j,t'} + + \mu_k + \mu_l + \mu_r + \mu_t + \epsilon_{i,t'} \quad (2)$$

where $Conversion_{i,t}$ equals one if the firm i has been converted to *RJ*, and zero otherwise. The coefficient π represents the impact of the commercial court propensity $\phi_{ij,t'}$ to convert a *Sauvegarde* to *RJ*, on the probability that a case is actually converted.

The second step is estimated as follows:

$$Y_{i,t,t'} = \alpha + \beta \cdot \widehat{Conversion}_{i,t'} + \gamma_1 X_{i,t} + \gamma_2 \Omega_{j,t'} + + \mu_k + \mu_l + \mu_r + \mu_t + \epsilon_{i,t,t'} \quad (3)$$

where $\widehat{Conversion}_{i,t'}$ are the values predicted by the first-stage regression. This equation is similar to equation (1), except that the variation of $\widehat{Conversion}_{i,t'}$ is induced by the exogenous variation introduced by courts' tendency to convert. If the instrument is valid, then β captures the causal effect of conversion to *RJ* on the firm probability of restructuring its debt. This effect would be a LATE as described by Angrist et al. (1996).

C. Conversion to RJ and Marginal Firms in the Bankruptcy System

For the court's conversion rate to be a valid instrument, it must be strongly correlated with the probability of conversion into *RJ*. We validate this assumption by the results of the first stage, presented in Table 6. The F-stat, that measures the strength of the instrument ranges between 64.63 and 83.05, above the threshold of 10 suggested by Staiger and Stock (1997). Our instrument is positively correlated to the endogenous variable. In all specifications, the coefficient associated with the instrument is statistically significant at 1% and robust to the introduction of multiple controls: the point estimate varies from 0.404 without any control (column 1 Table 6) to 0.354 when all controls and fixed effects are included (column 4 Table 6). The latter is our preferred first-stage estimation. The point estimate of 0.354 implies that at an increase of a one-standard deviation (16.7%) in the conversion rate increases the probability of being converted by 6.0%. This is almost half of the unconditional propensity of 12.7%.

As pointed out by Angrist et al. (1996), the causal effect of the instrument on the probability of being converted to *RJ* cannot be generalized to the whole population of Sauvegarde filings. There are some firms that would never be converted no matter which court they would be assigned to (the never-takers), and firms which would be converted no matter which court they would be assigned to (the always-takers). The measured impact of conversion (the coefficient β in equation 3) is only valid for the firms sensitive to their court propensity to convert a filing: the compliers. This is true only if the monotonicity assumption, or no defiers assumption, is met. This assumption implies that all the sensitive firms must be impacted in the same way by a given commercial court (their likelihood of being converted either increase or decrease for all of them when assigned to the same court). In our example, all subsamples should have a non-negative first stage estimate. This analysis is performed Table 7, where we test the first stage on several subsamples. For each subsample, the instrument coefficient is positive and significant, which tends to confirm the monotonicity assumption.

We can therefore characterize the population of compliers in each subsample. Following Maestas et al. (2012) analysis, since our treatment is binary, the proportion of marginal firms equals the first stage coefficient times the range of the instrument (in this case, 1). In our example, it means that 35.4% of firms filing for Sauvegarde may or may not be converted to *RJ* depending on their court propensity to convert alone. Considering the conversion average of 12.7%, it implies that 4.6% of firms filing for Sauvegarde are converted because of their court propensity to convert, and 31.2% are not converted for the same reason. It also implies that 8.2% of firms filing for Sauvegarde would be converted regardless of the court they would be assigned to (the always takers), and 56.0% of them would never be converted regardless of the court they would be assigned to (the never takers).

We report in Table7 the result of the first stage estimates for the samples split by industry, age, financial ratios, year of filing and year of judgment. In particular, we report the fraction of always takers which is the proportion of firms who would have been converted even if they were assigned to the court that never converts ($\phi_{ij} = 0$). We estimate the relative likelihood described by Maestas et al. (2013) as the probability of a marginal firm to present a certain observable characteristic, compared to the average firm filing for Sauvegarde. It is given by the ratio of the first stage coefficient conditionally on this characteristic, to the overall first stage coefficient.

Table 7 shows that the magnitude of the first-stage coefficient varies amongst groups. For example, we find that the probability of being sensitive to the court propensity to convert increases with the size of the firm: 29% of firms with less than 10 employees are at the margin of being converted, a share that goes up to 47% for firms with more than 50 employees. Smaller firms are less often converted, with 9% of conversion amongst smaller firms compared to 21% of conversion amongst bigger firms. Hence, less than 7% of firms with 10 or less employees would always been converted, versus 11% for bigger firms. The relative likelihood indicates that a marginal firm is 31% more likely to have more than 50 employees than the average firm and 20% less likely to have less than 10 employees than the average firm.

We also find noticeable differences in the first stage when we split the sample by industry. Firms in the sector of Transport are more sensitive to their court propensity to convert (75% of compliers) than firm in Manufacturing (29% of compliers), the latter having, interestingly, the bigger share of always takers (13%). The marginal firm is almost twice more likely to be in the Transport industry than the average firm and 19% less likely to be in the Manufacturing industry. When we split our sample between "zombie" firms and the one which aren't, we notice that non-"zombie" firms are more sensitive (44% of compliers versus 32%), but are on average less converted (10% converted versus 15%), and have a smaller share of always converted firms (6% versus 10%). The marginal firm is 22% more likely to be a non-"zombie" firm than the average firm. We see little difference for samples split by debt ratios, age, date of entry and date of judgment.

D. Exclusion Restriction Condition

Because the court has a role not only in conversion, but also in the debt renegotiation process, we must ensure that the exclusion restriction condition is met. The exclusion restriction requires that if the court propensity to convert does indeed impact the probability of being converted, it has no direct effect on the probability of reaching an agreement with the firm creditors. In theory, this condition cannot be verified from the data. However, it is possible to test whether this condition is verified on another population of firms similar to the one studied: the firm that filed directly for RJ. As the Sauvegarde procedure and RJ

are very similar, we can assume that the decision process leading to the restructuration of a firm debt is similar in both procedures.

Table 8 reports the results of this test, performed on the sample of all direct RJ filings, and on the subsample of voluntary *RJ* filers. Voluntary RJ files are closer to Sauvegarde filers as they are likely to be more pro-active in face of financial difficulties compared to RJ that are triggered by the firm creditors. Results report non-statistically significant coefficients, meaning that our instrument is uncorrelated with the probability of restructuring debt in direct *RJ* filings. These results suggest that the process of reaching an agreement to restructure a firm's debt may be mostly unrelated to the court's propensity to convert.

IV. Empirical Results

A. *The impact of conversion to RJ on debt restructuration*

A.1 Main Results

We focus on how conversion affects debt restructuration. The debt restructuration variable equals one if the firm reaches a debt restructuration agreement with its creditors, and zero otherwise.

The results of the second stage estimates are presented in Table 9, which confronts the OLS estimates to 2SLS estimates. The OLS estimate suggests that conversion is associated with an approximately 48 percentage point drop in debt restructuring. In contrast, our IV estimate implies that conversion causes a 69 percentage point decrease in debt restructuring. Thus, accounting for selection leads to a higher estimate of the effect of conversion to RJ on the probability of debt restructuring.

It is worth noting that firm-level control variables are more significant than court-level control variables. Older and larger firms are more likely to reach an agreement with their creditors. As one would expect, a high debt-to-asset ratio, or a high proportion of supplier debt in total debt are associated with a lower probability of reaching an agreement. So does an initial situation where operating income is lower than annual interest payment at the onset of bankruptcy ("zombie" = 1). Neither the size of court nor the local economic conditions (as measured by the local employment rate) are associated with higher restructuration chances. The only court-level control variable that is significant is the share of direct liquidation registered by the court that year.

A.2 Robustness tests

Various robustness checks are performed to ensure the validity of our results. Table 10 presents three specifications that slightly differ from the main one (column 4 of Table 6). The first model uses the IV-2SLS approach with the instrument *share of other cases converted* by court calculated over the period

2010-2018 and no longer on a yearly basis. With the non-annual conversion rates as instrument, the F-stat is still high (24.7). The share of marginal firms appears slightly higher (46.8%) than with annual conversion rates (35.4%). Yet, the second stage estimate for the impact of conversion is close to the one obtained with annual conversion rates (-0.652 versus -0.664 previously). The second and third models reproduce the main specification and introduce external additional control variables. In the second model, *administrator* is a dummy equal to one if the firm is assisted by a court-appointed administrator even though, given the size of the firm, a court-appointed administrator is not mandatory[13], it is equal to zero otherwise. In the third model, *out-of-court restructuration* is a dummy equal to one if in the recent history of the firm prior filling for Sauvegarde, the firm had tried to reach a confidential agreement with its creditors[14]. Unfortunately, while we are sometimes able to identify the presence of an administrator or the existence of an out-of-court restructuration, the information is not exhaustive. As mentioned earlier, while an indication of a previous out-of-court restructuration means there was one, the absence of any indication does not mean there was none. It goes the same way for the presence of an administrator. Additionally, this information also may be endogenous: a firm choosing to have an administrator reveals the preferences of the manager and/or its perception of the challenge the firm is facing. The same can be said for prior attempts (successful or not) at confidential restructuring. Given these limitations, results presented in Table 10 should be interpreted with caution and we are mainly interested in checking the stability of the estimates for the coefficients of interest and the validity of the instrument. The coefficient for the instrument (*share of other cases converted)* remains stable (0.351 and 0.354 with the introduction of *administrator* and *out-of-court restructuration* respectively, versus 0.354 previously), and the F-stat well above the threshold (61.3 and 64.1). We find that a firm that is supervised by an administrator whilst not being mandatory is more likely to be converted. Prior confidential attempts at restructuring is not revealing of the firm's outcomes in the court-supervised procedure. Eventually, the impact of conversion on debt restructuring (-0.664 and -0.663) is the same as previous (-0.664).

We take advantage of a change in the law regarding the conversion of Sauvegarde cases to RJ to conduct an additional robustness test. In mid-2014, new provisions were introduced into the law regarding the list of stakeholders allowed to ask the court to convert the case. As of July 2014, the judge him or herself can no longer ask for conversion, a prerogative granted only to the management of the firm, the court-appointed administrator (if any), the court-appointed receiver and the public prosecutor's office. To better

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[13] When filling for Sauvegarde, the court must appoint an administrator only if the firm has more than 250 employees or €3 million in total sales. Below these thresholds, it is the firm's choice to call upon an administrator. Our dummy *administrator* takes the value of zero for big firms and for small firms that chose not to be assisted, and one when small firms chose to be assisted. We find this information in the BODACC, when the name of the administrator in charge is disclosed.

[14] Two different confidential procedures exist (« *Mandat ad hoc* » and « *Conciliation* »). An agreement can be reach only if it is unanimously agreed by all creditors taking part in the confidential procedure (usually the largest ones).

capture the effects of this change in the law, we restrict our sample to 2013-2015 filings, and we use for instrument the *share of other cases converted* calculated on a semi-annual basis. We compare the results of our main specification (column 4 of Table 6 on the 2010 – 2018 period) with this new instrument run over the 2013 – 2015 period on a semi-annual basis (columns 1 and 2 of Table 11). Overall, the results are similar. The point estimate in the first stage is slightly lower than previously (0.275 versus 0.354) and the effect of conversion on debt restructuration slightly stronger (-0.749 versus -0.664). Columns 3 to 5 introduce the dummy "Before July 1st 2014" that equals one if the observation period of the Sauvegarde case ends before the change in the law. In the first-stages (columns 3 and 4), we instrument *Conversion* and *Conversion×Before* with the semi-annual instruments *Share of other cases converted* and *Share of other cases converted×Before*. We find that point estimates are significant and similar is the two first-stage equations (0.246 and 0.272), meaning that the share of Sauvegarde cases on the margin of being converted is similar before and after the change in law. Since *Conversion×Before* is not significant in the second stage (column 5), we conclude that *Conversion* reduces the probability of a debt restructuring agreement by the same level before and after the change in law (by -0.775, identical to the latter results on the same period).

Finally, we perform a placebo test to rule out the possibility that our results are merely based on chance, by substituting our instrument with a randomly generated variable that matches the original values of *share of other cases converted*. We replicate our preferred specification (column 4 of Table 6) with this randomly generated instrument 10 000 times. If we were to find similar results with these regressions, it would question the validity of our identification strategy. Figure 2 plots the distribution of the 10 000 placebo regressions and the mains statistics of the resulting estimates. On average, the coefficient associated with the randomly generate instrument is null (0.000). The actual coefficient of *share of other cases converted* (0.354) is more than ten times the standard deviation (0.027) above the mean, and much greater than the distribution's maximum estimate (0.108). While these results do not prove that our instrument is valid, they alleviate concerns about being only driven by chance.

   B. *The impact of conversion to RJ on the survival rates after restructuration deal*

TO BE COMPLETED

V. Conclusion

TO BE COMPLETED

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Figure 1
Distribution of Court Deviation from Mean Initial Conversion Rate

The *raw* distribution represents the deviation of the courts conversion rates from their annual average unadjusted, and the *adjusted* distribution the same deviation with all controls and fixed effects as in column 4 of Table 6.
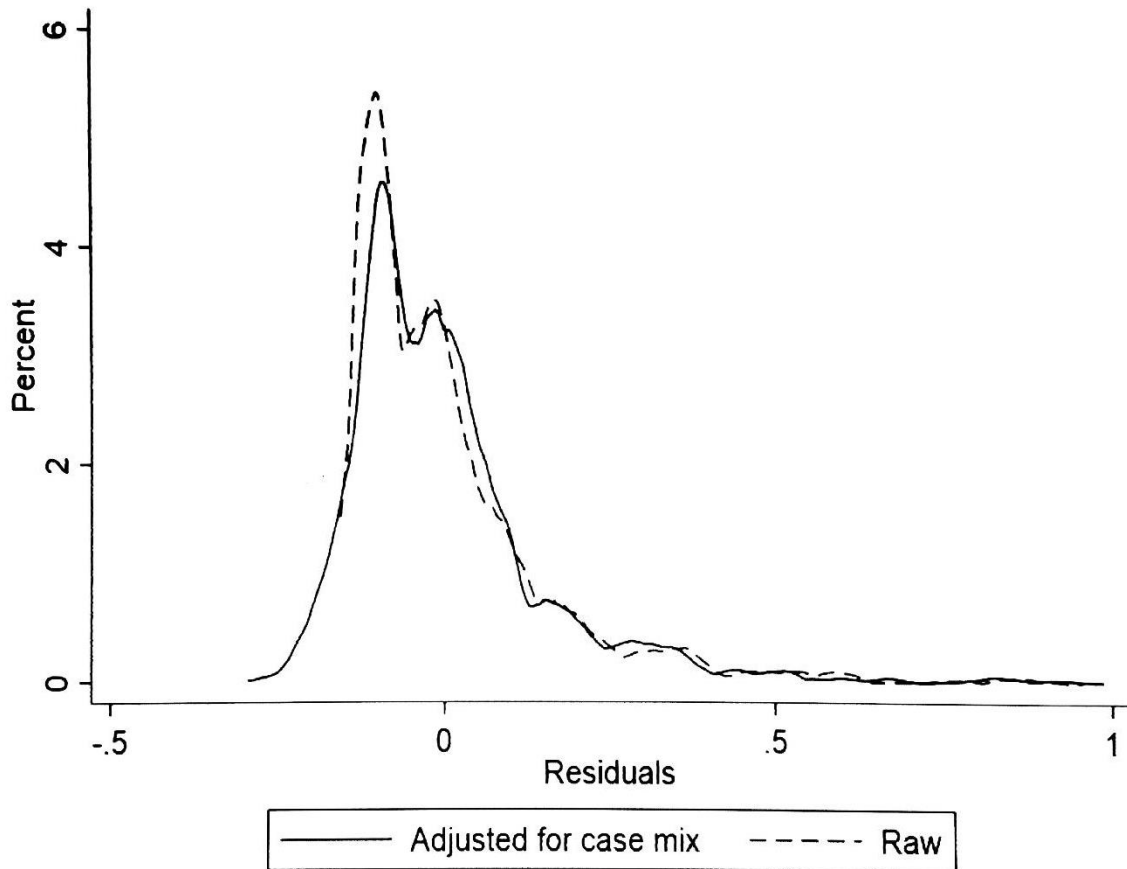
Figure 2
Placebo Test

This figure shows a histogram of the coefficient on *share of other cases converted* from 10,000 placebo regressions where the instrument *share of other cases converted* is randomly assigned within the sample. It contains the full set of controls and fixed effects of column 4 Table 6.
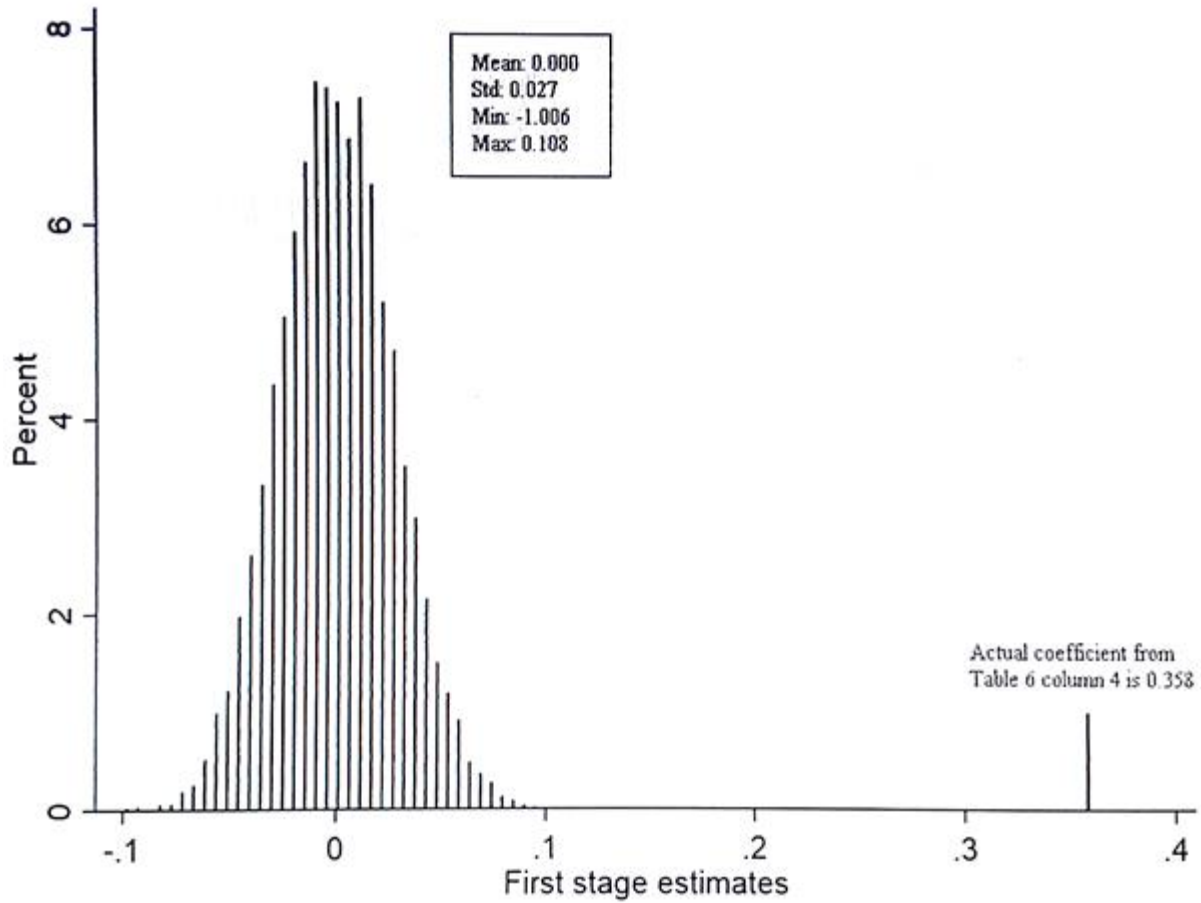
Table 1
Summary statistics for firms entering bankruptcy procedures between 2010 and 2016

This table reports summary statistics of the initial and working samples for firms initially filing for Sauvegarde and firms initially filing for RJ between 2010 and 2016. We compute our instrument *share of cases converted* by court j on year t' on the initial sample. The working sample contains the observations for which we were able to gather all financial information. Despite the reduction in size, the working sample is representative of the initial sample as the statistics of debt restructuration and survival rate in the two groups are similar. Column 5 reports the t-stat of the test of equality of mean between the samples of Sauvegarde cases that were not converted to RJ (column 3) and Sauvegarde cases converted to RJ (column 4). All other entries are self-explanatory.

Initial filing in:	Sauvegarde					RJ	
	All Sauvegarde (1)	Percent converted (2)	Cases not converted to RJ (3)	Cases converted to RJ (4)	Diff (4) – (3) (5)	All RJ (6)	Voluntary RJ filing (7)
Initial sample	**7,547**	**12.0%**	**6,638**	**909**		**93,467**	**50,260**
Share of debt restructuration	64.2%		69.7%	24.2%	-0.46***	27.5%	23.3%
Survival rate after debt restructuring							
After 2 years	82.3%		82.3%	83.3%	0.01	72.2%	85.8%
After 5 years	61.9%		62.4%	53.1%	-0.09**	43.3%	69.1%
Working sample	**6,283**	**12.7%**	**5,486**	**797**		**63,573**	**38,247**
Share of debt restructuration	64.6%		70.5%	23.5%	-0.47***	32.1%	25.4%
Survival rate after debt restructuring							
After 2 years	81.2%		81.1%	83.1%	0.02	71.9%	85.6%
After 5 years	59.7%		60.1%	52.4%	-0.08	42.7%	69.0%
Number of employees	27.7		27.0	32.6	5.58	15.3	18.0
Age	11.6		11.3	13.5	2.27***	8.9	9.6
Percent "zombie"	58.4%		57.2%	66.2%	0.09***	60.3%	63.2%
Total Debt / Asset	81.0%		81.2%	79.9%	-0.01	1.02%	99.6%
Supplier Debt / Total Debt	26.4%		26.0%	29,4%	0.03***	27.5%	28.1%

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

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Table 2

Breakdown of working sample per year of filing

This table reports the number of *Sauvegarde* entries over the 2010-2016 period and the share of cases converted by year of filing. For instance, our working sample contains 775 firms that filed for Sauvegarde in 2010, 13.8% of which were subsequently converted into RJ.

Year of filing	Number of firms (initial sample)	Percent converted	Number of firms (working sample)	Percent converted
2010	962	13.3%	775	13.8%
2011	998	16.3%	814	16.7%
2012	1,116	12.9%	943	13.5%
2013	1,223	11.4%	1,045	12.3%
2014	1,191	10.3%	1,006	11.0%
2015	1,153	8.8%	951	9.4%
2016	904	12.3%	749	13.2%
2010 – 2016	**7,547**	**12.0%**	**6,283**	**12.7%**

Table 3

Commercial courts' share of cases converted by year of judgement

This table reports the summary statistics of the share of Sauvegarde cases converted to RJ per court, for courts with at least 2 Sauvegarde cases. Unlike the instrument used in regressions, the share of cases converted presented here includes the current case (and takes a single value per court for each year). This table aims to show the heterogeneity amongst courts each year of judgment, as well as overall the period January 2010 - June 2018.

Year of judgement	Number of courts	Mean	Median	Std	Min	Max
2010	41	0.385	0.333	0.300	0	1
2011	95	0.145	0.071	0.189	0	0.750
2012	108	0.162	0.125	0.186	0	1
2013	117	0.126	0.056	0.177	0	1
2014	113	0.120	0.042	0.172	0	1
2015	113	0.113	0.048	0.162	0	1
2016	115	0.121	0.042	0.179	0	1
2017	110	0.080	0	0.175	0	1
2018	29	0.006	0	0.031	0	0.167
2010 - 2018	**134**	**0.136**	**0.128**	**0.067**	**0**	**0.316**

Table 4
The share of Sauvegarde filing does not depend on court's past conversion rate

The assignment of courts is not random, but depends on the firm's location. For the instrument to be exogenous, we test Table 4 that the decision of filing for Sauvegarde does not depend on the court conversion rate track-record. The share of cases converted used in this table does not exclude the present case. The regression is run on court-level, from year t 2012 to 2016.

Dependent variable	Share of Sauvegarde filings on year t
Share of cases converted	
On year $t-1$	-0.0126
	(0.0121)
On year $t-2$	-0.00841
	(0.00117)
Unemployment rate	0.00740***
Ln(size of the court)	-0.0168***
Share of direct liquidations	0.133***
Region fixed effects	Yes
Year of filing fixed effects	Yes
Observations	585
Adjusted R-squared	0.209

Standard errors in parentheses are clustered at court-by-year level
*** p<0.01, ** p<0.05, * p<0.1

Table 5
The court's conversion rate does not depend on its share of Sauvegarde filings in previous years

The assignment of courts is not random, but depends on the firm's location. For the instrument to be exogenous, we test Table 5 that the court conversion rate is not influenced by its share of Sauvegarde filings of the previous years. The share of cases converted used in this table doesn't exclude the present case. The regression is run on court-level, from year t 2012 to 2016.

Dependent variable	Share of cases converted on year t
Share of Sauvegarde filings	
On year $t-1$	-0.263
	(0.183)
On year $t-2$	-0.0224
	(0.178)
Unemployment rate	-0.00447
Ln(size of the court)	-0.00396
Share of direct liquidations	0.148
Region fixed effects	Yes
Year of filing fixed effects	Yes
Observations	663
Adjusted R-squared	0.020

Standard errors in parentheses are clustered at court-by-year level
*** p<0.01, ** p<0.05, * p<0.1

Table 6
First Stage

This table reports the results of the first stage regressions. The dependent variable is a dummy equal to one if the firm is converted to RJ, and the instrument the share of cases converted by a commercial court every year, excluding the current case. The model is robust to the introduction of fixed effects, firm and court-level control variables (columns 2, 3 and 4 respectively). All specifications contain 18 region fixed-effects, 5 industry fixed effects, 3 legal form fixed effect, and 6 year of filing fixed effects. Standard errors in parentheses are clustered at court-by-year of judgment level.

Dependent variable	Conversion to RJ			
	(1)	(2)	(3)	(4)
Share of other cases converted	0.404***	0.364***	0.360***	0.354***
	(0.0443)	(0.0453)	(0. 0443)	(0. 0441)
Firm-level control variables				
Ln(employees)			0.0228***	0.0227***
Age (> 5 years old)			-0.0148*	-0.0147*
Zombies			0.0152*	0.0131
Total debt / asset			0.00335	0.00102
Supplier debt / debt			0.0172	0.0168
Court-level control variables				
Unemployment rate				-0.00368
Share of direct liquidations				-0.119***
Ln(size of the court)				0.0287***
Region fixed effects	No	Yes	Yes	Yes
Industry fixed effects	No	Yes	Yes	Yes
Legal Form fixed effects	No	Yes	Yes	Yes
Year of filing fixed effects	No	Yes	Yes	Yes
Observations	6,283	6,283	6,283	6,283
Adjusted R-squared	0.033	0.040	0.047	0.057
F-stat for instrument	83.05	64.64	66.06	64.28

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

Table 7
First Stage Heterogeneity

This table reports the first stage regressions of column 4 Table 6 run on subsamples splitting firms by financial characteristics. By subsample, column 1 shows the number of firms, column 2 the unconditional share of cases converted to RJ, column 3 the coefficient of the instrument *share of other cases converted*, interpreted as being the share of marginal firms, columns 4 and 5 the t-stat and F-stat respectively, column 6 the fraction of firms which would be converted regardless of the court, and column 7 the relative likelihood as described in the text. Because of collinearities, some observations were omitted in the subgroups split by industry, number of employees and debt ratios, which total does not add up to 6,283.

	Observation (1)	Percent Converted (2)	Coefficient on *share converted* (3)	t-stat (4)	F-stat (5)	Fraction of always takers (6)	Relative likelihood (7)
Full Sample	6,283	12.7%	0.354***	8.0	64.3	8.2%	
Employees in firm							
0-9	3,118	9.1%	0.288***	6.2	38.0	6.5%	0.80
1-50	2,319	16.8%	0.407***	5.7	32.4	9.9%	1.14
> 50	537	20.7%	0.468***	3.4	11.4	11.0%	1.31
Industry							
Manufacturing	841	18.3%	0.289***	2.8	7.7	13.0%	0.81
Construction	904	14.5%	0.335***	3.5	12.3	9.6%	0.94
WholeSale and Retail Trade	1,680	11.2%	0.343***	5.1	26.0	7.4%	0.96
Transport	177	22.6%	0.750***	3.4	11.7	5.6%	2.09
Services	2,680	10.6%	0.364***	6.3	39.3	6.7%	1.02
Age							
5 years old or less	3,713	11.8%	0.338***	6.3	39.7	7.8%	0.94
More than 5 years old	2,570	13.6%	0.380***	6.3	39.4	8.4%	1.06
Zombie							
No	2,617	10.3%	0.436***	6.0	44.8	5.8%	1.22
Yes	3,666	14.5%	0.310***	5.9	35.3	10.0%	0.88
Total Debt / Asset							
Below median	3,143	12.8%	0.310***	6.2	36.3	8.8%	0.88
Above median	3,139	12.6%	0.403***	6.1	46.4	7.5%	1.14
Supplier Debt / Debt							
Below median	3,153	11.0%	0.396***	7.6	58.1	6.6%	1.12
Above median	3,129	14.5%	0.315***	5.5	30.1	9.9%	0.89
Date of judgment							
Before July 1st 2014	3,089	16.0%	0.349***	5.8	34.1	10.4%	0.97
After July 1st 2014	3,194	9.6%	0.265***	3.9	15.4	7.0%	0.74
Year of filing							
2010-2013	3,577	13.9%	0.349***	6.2	38.7	9.1%	0.97
2014-2016	2,706	11.1%	0.301***	4.0	15.7	7.7%	0.84

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

Table 8
Exclusion Restriction

This table presents a test for the exclusion restriction. We run the regression of column 4 Table 6 with debt restructuring in RJ as the dependent variable. We assume that if the propensity to convert has no impact on obtaining a restructuration debt in RJ, it does not have any impact either in Sauvegarde which is very similar. Column 1 reports the regression on the sample of all RJ and column 2 the regression on the subsample of voluntary RJ filers. Voluntary RJ filers are most resembling Sauvegarde filers as they are likely to be more pro-active in face of financial difficulties. Standard errors in parentheses are clustered at court-by-year of judgement level.

Dependent variable	Debt restructuring in RJ	
	All RJ (1)	Voluntary RJ (2)
Share of other cases converted	-0.0163	0.00619
	(0.0113)	(0.0145)
Firm-level control variables		
Ln(employees)	0.00956***	0.00308
Age (> 5 years old)	0.134***	0.125***
Zombies	-0.0196***	-0.0357***
Total debt / asset	-0.0112***	-0.0132***
Supplier debt / debt	-0.122***	-0.0105***
Court-level control variables		
Unemployment rate	-0.0132***	-0.0287***
Share of direct liquidations	-0.136***	0.0456
Ln(size of the court)	-0.0143***	0.00411
Region fixed effects	Yes	Yes
Industry fixed effects	Yes	Yes
Legal Form fixed effects	Yes	Yes
Year of filing fixed effects	Yes	Yes
Observations	63,573	38,247
R-squared	0.042	0.061

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

Table 9
Main Results

The dependent variable is the court decision of restructuring the corporate debt with the firm's creditors. Conversion is a dummy variable that indicates whether the firm has been converted from Sauvegarde to RJ. The regression in column 1 is estimated by OLS; the regression in column 2 is the reduced form estimated by 2SLS using as instrument the court's *share of other cases converted* every year. All regressions contain the full set of controls and fixed effects used in column 4 of Table 6. Standard errors in parentheses are clustered at court-by-year of judgement level.

Dependent variable	Debt restructuring (YES/NO)	
Model	OLS (1)	IV-2SLS (2)
Conversion	-0.466***	-0.664***
	(0.0200)	(0.138)
Firm-level control variables		
Ln(employees)	0.0507***	0.0553***
Age (> 5 years old)	0.119***	0.117***
Zombies	-0.0602***	-0.0571***
Total debt / asset	-0.0373***	-0.0373***
Supplier debt / debt	-0.111***	-0.107***
Court-level control variables		
Unemployment rate	-0.000126	-0.000917
Share of direct liquidations	0.190***	0.168**
Ln(size of the court)	-0.0292***	-0.0230***
Region fixed effects	Yes	Yes
Industry fixed effects	Yes	Yes
Legal Form fixed effects	Yes	Yes
Year of filing fixed effects	Yes	Yes
Observations	6,283	6,283
Adjusted R-squared	0.169	0.138

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

Table 10
Robustness Tests

This table reports versions of the first and second stages regression containing the full set of controls used in column 4 Table 6. The first specification columns 1 and 2 uses non-annual *share of other cases converted* for instrument. The regressions columns 3 to 6 introduce additional control variables: *administrator* is a dummy equal to one if the firm has an administrator whilst not being mandatory, and *out-of-court restructuration* is a dummy equal to one if prior to the entry in bankruptcy, the firm has tried to reach a confidential agreement with its creditors. All regressions contain the full set of controls and fixed effects used in column 4 of Table 6. Standard errors in parentheses are clustered at court-by-year of judgement level.

Specification	IV-2SLS with non-annual conversion rate for instrument		IV-2SLS using the presence of an administrator as additional information		IV-2SLS using the existence of an out-of-court restructuration as additional information	
Dependent variable	1st stage Conversion (1)	2nd stage Debt Restructuring (2)	1st stage Conversion (3)	2nd stage Debt Restructuring (4)	1st stage Conversion (5)	2nd stage Debt Restructuring (6)
Share of other cases converted	0.468*** (0.0943)		0.351*** (0.0448)		0.354*** (0.0441)	
Conversion		-0.652*** (0.233)		-0.664*** (0.138)		-0.663*** (0.137)
Administrator			0.0401*** (0.0106)	0.000437 (0.0152)		
Out-of-court restructuration					0.0128 (0.0200)	0.0331 (0.0192)
Firm-level controls	Yes	Yes	Yes	Yes	Yes	Yes
Court-level controls	Yes	Yes	Yes	Yes	Yes	Yes
All fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Observations	6,283	6,283	6,283	6,283	6,283	6,283
Adj. R-squared	0.141	0.029	0.060	0.138	0.057	0.139
F-stat for instrument	24.68		61.32		64.09	

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

Table 11
Using the change in law that prevents courts from deciding the conversion of a case

The regressions columns 3 to 6 consider the change in law of July 1st, 2014 that prevents the court from deciding the conversion of a case. We introduce the dummy "Before July 1st 2014" that equals one if the case is judged before the change in law. We restrain the period studied to 2013-2015 to focus on the most impacted period. *Share of other cases converted* is calculated for each semester. For comparison, column 1 and 2 report the first and second stages of the standard specification (without the change in law) with this new instrument. Column 6 reports the OLS results with the change in law. All regressions contain the full set of controls and fixed effects used in column 4 of Table 6. Standard errors in parentheses are clustered at court-by-year of judgement level.

Period (year of judgment)	2013-2015					
Specification	IV-2SLS with semi-annual conversion rate for instrument		IV-2SLS with semi-annual conversion rate and the introduction of the change in the law			OLS
Dependent variable	1st stage Conversion (1)	2nd stage Debt Restructuring (2)	1st stage Conversion (3)	Conversion×Before (4)	2nd stage Debt Restructuring (5)	Debt Restructuring (6)
Share of other cases converted	0.275*** (0.0477)		0.246*** (0.0830)	-0.0180 (0.0130)		
Share of other cases converted×Before			-0.0173 (0.0987)	0.272*** (0.0580)		
Conversion		-0.749*** (0.191)			-0.775*** (0.288)	-0.431*** (0.0427)
Conversion×Before					0.334 (0.354)	-0.0510 (0.0571)
Dummy "Before July 1st 2014"			0.161***	0.195***	-0.233***	-0.227***
Firm-level controls	Yes	Yes	Yes	Yes	Yes	Yes
Court-level controls	Yes	Yes	Yes	Yes	Yes	Yes
All fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Observations	2,830	2,830	2,830	2,830	2,830	2,830
Adj. R-squared	0.132	0.054	0.080	0.135	0.173	0.132
F-stat for instrument	33.18		13.23	10.99		

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

Table 12
Survival after debt restructuration

Dependent variable					Survival rate at different horizon					
Horizon		1 year		2 years		3 years		4 years		5 years	
	OLS (1)	IV 2nd stage (2)	OLS (3)	IV 2nd stage (4)	OLS (5)	IV 2nd stage (6)	OLS (7)	IV 2nd stage (8)	OLS (9)	IV 2nd stage (10)
Conversion	-0.0360	-0.0089	-0.0168	-0.2900	0.0033	0.0798	0.0100	-0.2590	0.0008	-0.1920
Firm-level control variables										
Ln(employees)										
Age (> 5 years old)										
Zombies										
Total debt / asset										
Supplier debt / debt										
Court-level control variables										
Unemployment rate										
Share of direct liquidations										
Ln(size of the court)										
All fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	3,603	3,603	3,037	3,037	2,329	2,329	1,726	1,726	1,126	1,126
Adjusted R-squared	0.010	0.003	0.030	0.004	0.030	0.018	0.039	0.006	0.074	0.028
F-stat for instrument		9.47		8.73		7.02		7.24		8.17

Standard errors in parentheses are clustered at court-by-year of judgement level
*** $p<0.01$, ** $p<0.05$, * $p<0.1$

TO BE COMPLETED