Using Heterogeneity in Commercial-Court Decisions to Measure the Stigma of Bankruptcy Filing in France

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ABSTRACT

This paper measures the stigma attached to public court-supervised bankruptcy filing in France. The procedure (known as a Redressement Judiciaire, or *RJ*) is widely-used in France, but only around 25% of firms succeed in renegotiating their debt. In 2006, a new bankruptcy procedure called Sauvegarde was introduced in French Commercial Law, for which only firms that are not (yet) insolvent can file: over 60% of these firms succeed in renegotiating their debt under this procedure. A Court can decide to convert a Sauvegarde case into an *RJ* if it considers that the firm is already insolvent or on the verge of insolvency. Courts differ in their view of the financial situations that trigger conversion. Using Court-conversion rates as an instrument, we measure the impact of this conversion on debt restructuring. We estimate that 36% of firms filing for Sauvegarde are at the margin of *RJ* conversion. For these marginal firms, conversion significantly reduces the chance of debt restructuring. As the two procedures differ only little except in name, one possible interpretation of our results is that the track-record of the *RJ* is so bad that it puts stigma on firms that significantly reduces their chances of survival.

JEL Classification: G33, K22.

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Firms filing for bankruptcy have to convince their clients, trade creditors, employees and suppliers to carry on doing business with them. Failing to do so increases their financial weakness, and further reduces their chances of renegotiating their debts. In other words, a firm that files for bankruptcy immediately suffers from stigma. This stigma is all the more serious that the track-record of the bankruptcy procedure is poor. This is part of the indirect costs of financial distress.[1] These indirect costs are difficult to measure, but are commonly-viewed in the literature as being substantial[2] and significantly larger than the direct costs of bankruptcy.

This paper contributes to this literature by measuring the stigma attached to bankruptcy filing via the coexistence, in France, of two Court-supervised bankruptcy procedures (in addition to liquidation). These two bankruptcy procedures yield radically-different results in term of the success of debt restructuring. We use the fact that some cases are switched from one bankruptcy procedure to the other by Commercial Courts to identify the cost to firms of the procedure with the lower success rate. We apply this identification strategy to an (almost) exhaustive sample of French bankruptcy filings over the 2010-2016 period.

The main bankruptcy procedure available to French firms is known as a Redressement Judiciaire (*RJ*), which dates in its current form from 1985. In many dimensions an *RJ* is similar to the US Chapter 11 procedure, differing only in that only firms that are already in a dire financial situation can (and must) file 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 for up to 18 months the firm either reaches an agreement with its

[1] These indirect costs arise because of inter- or intra-group conflicts of interest, asymmetric information, hold-out problems, lost sales and competitive positions, higher operating costs, and the ineffective use of management time (Altman, 1984; Opler and Titman, 1994; Bris, Welch, and Zhu, 2006; Almeida and Philippon, 2007).

[2] Observing and measuring indirect bankruptcy costs is difficult, and economists have to come up with methods to infer them. Given the variety of methods and natural experiments used in this context, the comparison of the estimated size of indirect bankruptcy costs is not straightforward. These have been estimated at between 10% and 23% of firm value given default (e.g. Andrade and Kaplan, Bris, Welch and Zhu, 2006, Hotchkiss et al. 2008, Davydenko, Strebulaev, and Zhao, 2012); these contributions infer bankruptcy costs from market prices. Hortacsu et al. (2013) show that the financial distress of car manufacturers reduces the price of their product in the second-hand market. They interpret this as reflecting substantial indirect financial-distress costs for car manufacturers.

creditors to restructure its debt or is liquidated.[3] This bankruptcy procedure has a low firm-survival rate. In the dataset we construct of all initial *RJ* filings[4] in France over the 2008 – 2016 period and their outcomes up to June 2018 only around 25% of firms entering an *RJ* manage to restructure their debt. Due to this low *RJ* survival rate, a firm filing for an *RJ* will immediately be viewed as fragile by its suppliers, creditors, and clients. This is part of the indirect costs associated with bankruptcy that may help reduce the survival chances of firms in bankruptcy. We call this potential reduction in survival chances the stigma attached to RJ, and we here attempt to measure it.

In 2006, a new bankruptcy procedure, called *Sauvegarde* (Safeguarding) was introduced in French Commercial Law. This new procedure aims to give firms time to restructure their debt and business before they reach the weak financial situation that triggers *RJ* filing. Even if most of the provisions of the Law regarding this bankruptcy procedure are similar to those for an *RJ*, *Sauvegarde* has better results: in our dataset, over 60% of firms filing 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 reflect pro-active managers who are willing to act early to solve firm financial difficulties rather than waiting for insolvency to enter an *RJ*. Interestingly, even though the *Sauvegarde* procedure resembles the *RJ*, the precise wording of the Law tries to distinguish between the two as much as possible, and the Banque de France in its monthly bulletin on new defaulting firms does not count *Sauvegarde* filings.[5]

We construct a dataset of all public bankruptcy filings over the 2010 to 2016 period and their outcomes up to June 2018. We do so using the public daily records of Commercial Courts' main decisions. Our initial data set contains 7,547 *Sauvegarde* filings, of which 909 (12.0%) were converted into an *RJ*. We then merge this data set with firm annual fiscal records (income statements and balance sheets) from INSEE.

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

[4] That is, all firms which entered directly into *RJ* status rather than initially entering into Sauvegarde and then being converted into *RJ*.

[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.

Given attrition, our final sample contains 6,283 *Sauvegarde* cases, of which 797 of (12.7%) were converted into an *RJ*.

To measure the stigma attached to *RJ*, we use the fact that a significant share of *Sauvegarde* cases are subsequently converted into *RJ*s by the court. A firm's assignment to a Commercial Court is based on the location of the firm's headquarter, preventing firms from "forum-shopping". The Court can convert a *Sauvegarde* case into an *RJ* if the assessment of the financial situation of the firms reveals that the firm is already insolvent or on the verge of insolvency. But the exact situation triggering conversion may be interpreted differently across Commercial Courts. We find considerable heterogeneity in the yearly conversion rates of the 134 Commercial Courts, ranging from 0% to 100%, which remains substantial even when controlling for local firm characteristics and economic conditions. We use this heterogeneity (over space and time) in conversion rates to construct an instrument to identify the impact of conversion on the probability of successful debt restructuration.

Our method builds on a number of empirical papers using heterogeneity in judicial decisions as an instrument to measure the impact of decisions. For example Bernstein et al. (2016 and 2017) use the Judge conversion rate of Chapter 11 to Chapter 7 cases to explain the reallocation of assets and bankruptcy spillovers. In a different field, Maestas et al. (2013) use examiner heterogeneity in granting disability benefits to measure the impact of receiving these benefits on labor supply. Their identification strategies are based on the random allocation of Judges or examiners to cases. Unlike these contributions we do not have data on Judges but only on Commercial Courts, and the allocation of bankruptcy cases to Courts is not random as it depends on the firm's headquarter's location. To ensure that our instrument is valid, we carry out a number of empirical tests to show that we are actually close to random assignment once we control for firm-specific characteristics and local economic conditions. We notably show that, at the Court - year level, past conversion rates are not associated with the current relative number of *Sauvegarde* filings, and the current court conversion rate is not correlated with the past relative number of *Sauvegarde* filings.

Overall our empirical results suggest that the indirect costs associated with *RJ* are high. Around 36% of firms filing for Sauvegarde are on the margin of being converted to an *RJ*. For these marginal firms, conversion significantly reduces the chance of restructuring their debt (the point estimate in the linear estimation is 0.664, so that the probability of debt restructuring falls by 66%). We carry out various robustness checks, none of which affects the results. Perhaps the most interesting of these robustness checks uses the new provisions that were introduced into the Law in mid-2014 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 that is only granted to the firm's management, 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 and 27% after). However, conversion to *RJ* (instrumented as above) reduces the probability of a debt-restructuring agreement to the same degree before and after the change in the Law.

The remainder of the paper is organized as follows. Section 1 discusses the two French bankruptcy procedures and Section 2 presents our original dataset and the main characteristics of the bankruptcy-procedure outcomes. Section 3 then discusses the identification strategy, notably as firms are not randomly allocated to Commercial Courts we carry out a number of tests to check instrument validity. Section 4 contains our main econometric results, their discussion and the robustness checks. Last, Section 5 concludes.

I. Bankruptcy Procedures and Commercial Courts in France

A. Bankruptcy procedures

French Commercial Law is such that a firm that cannot meet its payment obligations is considered as insolvent if these are not covered by its liquid assets. Insolvent firms should file for an *RJ* within 45 days. In practice, some insolvent firms do not respect this time limit, and here the firm's creditors, as well as the

Court itself, can bring the case before Court to trigger an *RJ*. Firms that face even more severe financial distress can file directly for liquidation (a procedure akin to US Chapter 7).

Once the enterprise files for an *RJ*, there is a six-month "observation period" to assess the financial situation of the firm. During this period a Court-appointed Receiver is in charge of establishing the list of the firm's liabilities. The Court also nominates an Insolvency Administrator who monitors the firm's day-to-day operations, notably all of its financial transactions as well as some important restructuration decisions (firing employees, selling assets etc.). The Administrator can also prevent the firm manager from taking actions that would reduce the firm's asset value. The Administrator and the Receiver negotiate with creditors to establish a debt-restructuration plan. If the situation deteriorates further, the firm can be liquidated even before the end of this six-month period. Most of the time, the observation period is renewed for six months. The observation period can be renewed twice, and so lasts for a maximum of 18 months. At any time during the observation period, the Court can accept (or reject) the debt-restructuration plan negotiated with creditors. The typical plan is a mixture of debt-rescheduling (up to a 10-year horizon) and a haircut (if, given the option, some creditors prefer an immediate partial payment to settle the final balance to rescheduling). If the situation deteriorates further during the observation period and there is no hope of reaching an agreement with creditors to keep the firm as a going concern, the Court can decide to liquidate the firm. Liquidation can take two forms. In the most abrupt form, the firm's assets are sold on the market, with the proceeds going to the firm's stakeholders according to priority rules (with employees having the highest priority for unpaid wages). A smoother form of liquidation consists in the opening of a bidding process for all or part of the firm's assets and some or all of its employees, with a view to keeping at least part of the firm going. The Court receives bids from potential buyers and chooses amongst them. If there are no buyers, or the Court rejects all of the offers, the firm ceases operations, its assets are sold, and stakeholders are reimbursed according to the same priority rule as above.

This procedure dates in its current form from 1985. We collected data on all procedure openings from 2008 to 2016, and outcomes up to June 2018 (see the section "Data and Summary Statistics" for details). Over the 2008 – 2016 period, we recorded 133,065 *RJ* openings (of which 69,465 were voluntary filings, and the others triggered by either creditors or the Court itself). Only 27.5% of firms that started an *RJ* managed to restructure their debt, and the survival rate of firms that did manage to restructure is low: only 43.3% were still operating five years later. These figures are overall somewhat better for firms that voluntarily filed for an *RJ*, with 23.3% restructuring their debt and a 69.1% five-year survival rate.

The bad track-record of *RJ* in terms of debt restructuration and firm survival makes it difficult for firms to persuade their clients, trade creditors, employees and suppliers to continue doing business with them once the filing becomes public. This reduces further the chances of debt renegotiation. In 2006 a new bankruptcy procedure was introduced in French Commercial Law,[6] known as "Sauvegarde". Firms can file for this "Sauvegarde" procedure if they are not (yet) insolvent[7] but face financial difficulties that they consider impossible to overcome without debt restructuration. The "Sauvegarde" procedure is otherwise quite similar to the *RJ*: it is public,[8] the six-month observation period is twice renewable, and the Court appoints a Receiver. The Court can also appoint an Administrator, and has to do so for the largest firms (in the *RJ*, the Court appoints an Administrator regardless of firm size). The role of the Administrator is slightly less important in Sauvegarde than in *RJ*: he/she only assists the manager and cannot make decisions without the consent of the manager, whereas this is the case in *RJ*.

The raw numbers show that, over the 2010 – 2016 period, 64.2% of the firms filing for Sauvegarde restructured their debt, a figure three times that for the firms entering an *RJ*. These firms are also more likely to survive after debt renegotiation: five years after restructuration 61.9% were still operating (as compared to 43.3% for *RJ* firms). These numbers do not however necessarily prove the superiority of

[6] The bankruptcy protection act of July 26th 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 of the main Court decisions during the procedures are available in a daily publication online since 2008.

Sauvegarde over *RJ*, as firms entering these two bankruptcy procedures do not start with the same degree of financial distress. By design, firms filing for Sauvegarde have less-severe financial problems than those filing for *RJ*. They may also have unobservable characteristics distinguishing them from those filing for *RJ* that affect the outcome of the bankruptcy procedure: being more proactive or having a better-informed management. Our identification of the effect of RJ will thus rely on examining the fate of firms that initially filed for *Sauvegarde* and were subsequently converted into *RJ* cases by the Court.

As stated by the Law, the Court can convert a *Sauvegarde* case into an *RJ* at any moment in time during the observation period if the assessment of the financial situation reveals that the firm was already insolvent at the time it filed for *Sauvegarde*, or is currently on the verge of insolvency. Discussions with various bankruptcy procedure stakeholders lead us to think that, in practice, there are (at least) three different situations that can potentially trigger a conversion decision. In the first, a careful examination of the financial accounts of the firms, often with the help of Chartered Accountants, raises doubts about the firm still being solvent by the time it filed for *Sauvegarde*. In this case, the conversion does nothing to help the firm reach a debt-restructuring agreement. On the contrary, the firm may now carry the stigma associated with an *RJ* (if any) that may reduce its survival chances. The motivation for the Court to convert the case probably reflects its preference for the strict enforcement of the Law. Court preferences can vary over space and time. In the second situation, the firm runs out of cash during the observation period and defaults on the financial obligations it contracted after filing for *Sauvegarde*. Here the conversion to *RJ* increases the amount of debt covered by the protection of the Court. It is unclear whether, even abstracting from the stigma attached to *RJ*, this increases the chances of reaching a deal with creditors. The preferences of the Court for the strict enforcement of the Law probably play a smaller role in these types of conversions, as they are triggered by observable default. In addition to these two situations, which fit the wording of the Law fairly well, a third situation was once mentioned during our discussions with stakeholders: the Court may consider that measures being taken by the debtor are endangering the firm. The Court then converts the case to an *RJ* so as to appoint an Insolvency

Administrator of its choice to run the company. The Court will consider conversion at the request of the Administrator, the Receiver, the Public Prosecutor's Office or (since September 2014) the firm itself. Up to July 2014, the Judge him or herself could carry out this conversion.

B. Commercial Courts

There are currently 134 Commercial Courts spread over the French territory.[9] There is at least one Commercial Court in each of the 95 "départements", with some "départements" having two or even three. These Courts deal with bankruptcy proceedings for companies and commercial disputes, with the former representing 20% of the cases they hear. In bankruptcy hearings, the firm is assigned to the relevant Court according to the firm's headquarters' location, and there is no possibility of "forum shopping" by either the firm or its creditors.

There are around 3 000 commercial Judges, who are unpaid volunteers. They are chosen from among entrepreneurial leaders and elected by them. Each Judge is initially elected for a two-year mandate, and can then be reelected three times for a four-year mandate (for a maximum service of 14 years). There is one election per year, and mandates start in early January. There is thus regular turnover amongst Judges. Judges receive legal training after being chosen and during their term of office. A typical hearing involves more than one Judge, as Courts are organized by Chambers. Each case is allocated to a specific Judge, but decisions are taken by the Judges of the Chamber in a collegiate manner. Judges sit only one or two half-days per week and go on with their normal business activities the rest of the time. For obvious reasons, they cannot work on cases related to their own business. Judges elect amongst them the President of the Court as well as two vice-Presidents for a 4-year mandate that is not renewable.

II. Data and Summary Statistics

[9] While French Commercial Law does not differ from one Court to another, we exclude some parts of France from our research: the Courts in three départements (Moselle, Haut-Rhin and Bas-Rhin) as, for historical reasons, they operate somewhat differently from Courts in the rest of the country, and overseas départements and Territories for the same reason.

A. Data Sources

The data used here comes 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 available in electronic format since January 2008,[10] with one electronic file per business day. We constructed a dataset of all bankruptcy filings in France over the 2010-2016 period and followed their outcomes up to June 2018. Notably, we identify Sauvegarde cases that are converted to *RJ*. Our dataset contains 315,345 initial filings[11]: 7,700 Sauvegarde filings, 94,467 *RJ* filings and 213,178 direct liquidations. We are able to follow-up 7,547 Sauvegarde cases (98%) and 93,467 *RJ* cases (99%). By definition, there is no follow-up for liquidations. We call the sample for which we have follow-up information our "initial sample" (see the top panel of Table 1). In this sample we have information on the firm's address, the Commercial Court in charge of the case, and the dates of filing and the subsequent main judgements by the Court (renewal of observation period, conversion to *RJ*, agreement of debt restructuration with creditors, liquidation etc.). There is, however, no information on other relevant variables such as the number of employees, the industry and total sales. We also do not know about previous out-of-court restructuring or, for *RJ*, whether the filing was voluntary or triggered by either the Court or creditors.

We complement these data with additional bankruptcy information provided by the CNAJMJ.[12] Notably, the CNAJMJ dataset contains information on previous out-of-court debt restructuring, and whether the *RJ* filing was voluntary. However, this information is only partial: while the presence of a previous out-of-court restructuration does indeed mean there was one, the lack of any such mention does not mean there

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

[11] These numbers are below those reported by Altares a privately-owned company that produces statistics on bankruptcy filings in France. Our sample is restricted to Commercial Courts. As such, it excludes bankruptcy filings from farmer, self-employed and liberal practices. It also excludes Courts in three French metropolitan départments as well as oversea territories (see footnote 9).

[12] The "*Conseil National des Mandataires Judiciaires et Administrateurs Judiciaires*" is an association of Court-appointed Receivers and Insolvency Administrators.

was not one; the same point applies to the nature of the *RJ*. As there are doubts about the completeness of this data set, information from it will be used with caution and only in the robustness checks.

Last, data on firms' economic and financial performance over the 2009-2015 period are extracted from the balance sheets and income statements made available by INSEE, the French Statistical Office. This provides us with the last available information on the firm before it filed for bankruptcy, including data on the number of employees, total assets, total debt and its structure (bank, suppliers, other), interest payments, total sales, operational income, industry, age, legal status and so on. Due to attrition, our final sample (which we call the "working sample") contains 6,283 Sauvegarde cases, 797 (12.7%) of which were converted to *RJ*, and 66,142 *RJ* filings (see the bottom panel of Table 1).

B. Summary Statistics

Table 1 shows the composition of both the initial and working samples. As far as Sauvegarde is concerned, the two samples are very similar, as we would expect as the working sample contains over 83% of the cases 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, and 64.2% of Sauvegarde filings led to debt restructuring in the initial sample, as opposed to 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 in the initial sample. Even so, the restructuration rate is quite similar between the two samples (28% in the former and 32% in the latter).

On average, 64.6% of the 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 that for voluntary *RJ* filings (25.4%). For firms that manage to restructure their debt, the two-year survival rate for 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 analogous gap in the five-year survival rate is slightly larger at 60.1% and 52.4% respectively, but the difference is not statistically significant in the working sample.

Regarding the financial and economic situation of our sample firms, the average firm filing for Sauvegarde has 28 employees, is 12 years old, has a debt-to-asset ratio of 81%, with supplier debt representing over 25% of total debt. Last, 58.4% of firms filing for Sauvegarde are labelled as "Zombies" as their financial obligations were greater than their operational income the year before the filing.[13]

Firms whose Sauvegarde filings are not converted into an *RJ* are on average younger compared to firms whose Sauvegarde filings are (11.3 versus 13.5 years-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 the equality of means reveals that these differences are statistically different from zero (see column 5 of Table 1). These differences suggest selection into the two groups that we will need to treat.

There is great heterogeneity amongst Commercial Courts. These register on average 709 initial bankruptcy filings per year (*Liquidation, RJ* and *Sauvegarde*), with a median figure of 455 and a standard deviation of 737. The first percentile figure is 77 filings per year, and that in the ninety-ninth percentile 3643, attesting to the great variety in Court 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 increased steadily until 2013, stabilized in 2014 and then fell significantly in 2016. The share of cases converted ranges between 9.4% and 16.7%, and is lower in the second half of the period examined.

III. Identification Strategy

A. Empirical Design

To measure the indirect costs of *RJ*, we focus on firms that filed for Sauvegarde, and exploit that a significant fraction of these (12.7%) are subsequently converted into *RJ* by Courts. This allows us to eliminate the unobservable characteristics of firms that file for Sauvegarde compared to *RJ*.

[13] Our definition of a "Zombie" firm is less restrictive than that used by the OECD, for which "Zombie" firms are over ten years old with financial obligations greater than operational income for over three consecutive years (see Adalet McGowan et al., 2017).

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

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

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

where the dependent variable $Y_{i,t,t'}$ is the probability that firm i entering the procedure in year t restructure its debt, t' is the year at the end of the observation period, which we will refer to as the *judgment year*, and $Conversion_{i,t'}$ a dummy for the Sauvegarde case having been converted to an *RJ* at t'. We wish to estimate β, the effect of *RJ* conversion on $Y_{i,t}$. The firm-level control variables $X_{i,t}$ such as the pre-bankruptcy filing financial ratios, employment and age, come from the last available date before the filing year t. The Court-level control variables $\Omega_{j,t'}$ such as the size of the Court and its share of direct liquidations, are recorded at the judgement year t'. We include dummies μ_k, μ_l, μ_r and μ_t for, respectively, industry, legal status, region and year of filing. Standard errors are clustered at the Court-judgment year level. Under the null hypothesis that an *RJ* conversion has no effect on $Y_{i,t,t'}$, β should not be statistically different from zero, while a negative β means that *RJ* conversion reduces the chances of debt restructuration.

Despite the many controls, there remains potential endogeneity. The conversion of a filing suggests a worsening of the firm's financial health that mechanically reduces its survival chances. We therefore identify the causal effect of *RJ* conversion on firm survival by appealing to the heterogeneity in Commercial Courts' propensity to convert Sauvegarde into *RJ* as an instrumental variable.

Our identification strategy is based on the observation that, while French Bankruptcy Law is national, its interpretation can differ from one Court to another. This is especially true for the conversion decision from Sauvegarde to *RJ*. This takes place if the firm becomes insolvent or close to insolvency during the observation period, where insolvency means that short-term debt is greater than the firm's liquid assets.

Here not only the valuation of the firm's liquid assets but also "close to insolvency" can differ from one Court to another (depending on their chartered accountants). In addition, as previously discussed, Court preferences for the strict enforcement of the Law may vary over space and time. Our instrument relies on these heterogeneities 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 $\phi_{ij,t'}$ is the number of cases i in Court j converted related to the total number of Sauvegarde cases that the Court judged in year t', excluding the present case. This measure follows that in Maestas et al. (2013). 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 number of Judges in each Court are renewed each calendar year, $\phi_{ij,t'}$ is estimated by year of judgment (t'). As 2016 filing judgments can take place up to 2018, the instrument covers the period 2010-2018.

Table 3 reports the mean and standard deviation of the instrument (the annual Court conversion rate) per year. Every year, some Courts never convert, while others always do. The share of converted cases falls from 2014, from 16.2% of cases converted in 2012 to 11.3% in 2015; the median figure falls as well. However, heterogeneity remains, as seen in the standard deviation that is above 16% over the whole period (with the exception of the first half of 2018): this heterogeneity is behind the exogenous variation in the model. However, instrument validity relies on a number of assumptions that are discussed below.

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

In a second step, we will focus on how conversion to *RJ* affects the survival of firms that obtained a restructuration deal. The specification remains the same as in (1), where the dependent variable $Y_{i,t,t'}$ becomes the probability that firm i entering the procedure in year t be alive after restructuration, whether

it has been converted in year t' or not. We will study the survival at the horizons of two and five years after the agreement with the firm's creditors being reached.

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 over its Court (preventing forum shopping). However, as it depends on firm location, Commercial-Court assignment will not necessarily be random. Randomness here requires us to test two additional assumptions and control for firm characteristics and local economic conditions.

First, the firm's decision to file for *Sauvegarde* must not be affected by the Court's propensity to convert *Sauvegarde* cases. If firms are discouraged from filing for *Sauvegarde* as they know that the Court has a high conversion rate of *Sauvegarde* cases to *RJ*, the instrument would be endogenous. We test this assumption by looking at the correlation between the Court share of *Sauvegarde* cases relative to its total number of direct *Sauvegarde* and direct *RJ* filings, and its conversion rate in preceding years. We show (Table 4) that there is no correlation between these rates, so that the past Court conversion rate does not affect the firms' decision to file for *Sauvegarde*. Put differently, Courts do not have a track-record of *RJ* conversion that affects firm behavior regarding *Sauvegarde*. Second, we need to make sure that the court's conversion rate is not a biased reflection of the local population of firms. Our instrument will not be exogenous if the share of converted cases rises 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- and local-level control variables. These include 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 firm population (greater direct liquidations suggesting

a less financially-healthy local firm population). We also include the *département's* unemployment rate as a proxy for the local economy (the département is that of the firm's headquarters, and also that of the Commercial Court as firms are assigned to Courts by département). Finally, we include the total number of filings (direct liquidations, *RJ* and Sauvegarde filings) per Court to capture Court size. All of these control variables are calculated annually.

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

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

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

where $Conversion_{i,t'}$ is a dummy for firm i being converted to *RJ* at t'. The coefficient π shows the impact of the Commercial Court's propensity $\phi_{ij,t'}$ to convert *Sauvegarde* to *RJ* on the probability that a case actually be 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 from the first-stage regression. This equation is similar to equation (1), except that the variation in $\widehat{Conversion}_{i,t'}$ comes from the exogenous variation introduced by the Court's tendency to convert. If the instrument is valid, then β captures the causal effect of *RJ*

conversion on the firm's probability of debt restructuring. This effect is a LATE, as described in Angrist et al. (1996).

C. RJ Conversion and Marginal Firms in the Bankruptcy System

For the Court conversion rate to be a valid instrument, it must be strongly correlated with the probability of *RJ* conversion. We check this assumption in the first-stage results in Table 6. The F-statistic measuring instrument strength ranges between 64.63 and 83.05, and is above the threshold of 10 suggested by Staiger and Stock (1997). Our instrument is positively correlated with the endogenous variable. The coefficient on the instrument is statistically significant at the 1% level in all specifications, and robust to the introduction of multiple controls: the point estimate varies from 0.364 without any controls variables other than fixed effects (column 1 of Table 6) to 0.354 when all the controls and fixed effects are included (column 3 of Table 6). The latter is our preferred first-stage estimation. The point estimate of 0.354 implies that at a one standard-deviation rise (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. Some firms never convert no matter which Court they are assigned to (the never-takers) and others convert no matter which court they are assigned to (the always-takers). The measured conversion impact (β in equation 3) is only valid for the firms that are sensitive to the Court conversion propensity: the compliers. This is true only if the monotonicity assumption, or no-defiers assumption, holds. This assumption implies that all the sensitive firms must be affected identically by a given Commercial Court (the likelihood of being converted either rises or falls for all firms that are assigned to the same Court). In our example, all subsamples should have a non-negative first-stage estimate. This analysis is carried out in Table 7, where we test the first stage on a number of subsamples. The instrument coefficient is positive and significant in each subsample, tending to confirm the monotonicity assumption.

We can therefore characterize the population of compliers in each subsample. Following the analysis in Maestas et al. (2013), as 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 data, 35.4% of the firms filing for Sauvegarde may be converted to *RJ* depending only on their Court's propensity to convert. With the conversion average being 12.7%, 4.6% of firms filing for Sauvegarde are converted due to this propensity, and 31.2% are not converted for the same reason. This also implies that, regardless of the Court to which they are assigned, 8.2% of the firms filing for Sauvegarde would be converted (the always-takers) and 56.0% would never be converted (the never-takers).

Table 7 shows the first-stage estimates for the samples split by industry, age, financial ratios, year of filing and year of judgment. In particular, we list the fraction of always-takers, which is the proportion of firms that would be converted even if they were assigned to a Court that is never seen to convert ($\phi_{ij} = 0$). We estimate the relative likelihood described by Maestas et al. (2013) as the probability that a marginal firm have a certain observable characteristic, as compared to the average firm filing for Sauvegarde. This is given by the ratio of the first-stage coefficient conditional on this characteristic to the overall first-stage coefficient.

Table 7 shows that the size of the first-stage coefficient varies between groups. For example, the probability of being sensitive to the Court's conversion propensity rises with firm size: 29% of firms with fewer than 10 employees are at the margin of being converted, a share that rises to 47% for firms with over 50 employees. Smaller firms are less-often converted, with 9% conversion for smaller firms compared to 21% conversion for larger firms. As such, under 7% of firms with fewer than 10 employees would always be converted, versus 11% for larger firms. The relative likelihood reveals that marginal firms are 31% more likely to have over 50 employees than the average firm and 20% less likely to have under 10 employees.

We also find noticeable differences in the first-stage results when we split by industry. Firms in the Transport sector are more sensitive to the Court conversion propensity (75% compliers) than Manufacturing firms (29% compliers), the latter having, interestingly, the largest share of always-takers (13%). The marginal firm is almost twice as likely to be in Transport than the average firm, and 19% less likely to be in Manufacturing. When we split our sample between "Zombie" and non-Zombie firms, the latter are more sensitive (44% compliers, versus 32%) but are on average less converted (10% versus 15%) with a lower always-converted figure (6% versus 10%). The marginal firm is 22% more likely to be a non-Zombie firm than the average firm. On the contrary, we see little difference in the samples split by debt ratio, age, date of entry or date of judgment.

D. The Exclusion-Restriction Condition

As the Court has a role not only in conversions but also debt renegotiation, we check that the exclusion-restriction condition is met. This latter requires that while the Court conversion propensity does indeed affect the firm conversion probability, it has no direct effect on the probability of reaching an agreement with the firm's creditors. In theory, this condition cannot be checked in the data. It is however possible to test it in another population of firms similar to that considered here: firms that filed directly for *RJ*. As the Sauvegarde and *RJ* procedures 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, carried out on the sample of all direct *RJ* filings and the subsample of voluntary *RJ* filers. Voluntary *RJ* files are more similar to Sauvegarde filers, as they are likely to be more pro-active when faced with financial difficulties as compared to *RJ*s, which are triggered by the firm creditors. We find no statistically-significant coefficients, so that our instrument is uncorrelated with the probability of debt restructuring in direct *RJ* filings. This suggests that the process of reaching an agreement to restructure firm debt is not related to the Court's conversion propensity.

IV. Empirical Results

A. The impact of RJ conversion on debt restructuration

A.1 Main Results

We focus on how conversion affects debt restructuration, where the latter is a dummy variable for the firm reaching a debt-restructuration agreement with its creditors.

The second-stage estimates appear in Table 9, which compares the OLS and 2SLS estimates. The former suggest that conversion is associated with a fall in debt restructuring of approximately 48 percentage points. On the contrary, our IV estimates imply that conversion results in a 69 percentage-point fall in debt restructuring. As such, accounting for selection produces a higher estimate of the effect of *RJ* conversion on the probability of debt restructuring.

It is also notable that the firm-level control variables are more significant than those at the Court level. Older and larger firms are more likely to reach an agreement with their creditors. As we might expect, higher debt-to-asset and supplier debt to total debt ratios are associated with lower probability of reaching an agreement. So is the initial situation where operating income is lower than annual interest payments at the onset of bankruptcy ("Zombie" firms). Neither Court size nor local economic conditions (as measured by the local employment rate) are associated with restructuration. The only Court-level control variable that is significant is the share of direct liquidations in the Court that year.

A.2 Robustness tests

Various robustness checks are carried out to ensure the validity of our results. Table 10 presents three specifications that differ slightly from the main specification (column 3 of Table 6). The first uses the IV-2SLS approach with the instrument *share of other cases converted* by the Court calculated over the whole 2010-2018 period and no longer annually. With the non-annual conversion rate as the instrument, the F-statistic remains high (24.7). The share of marginal firms is somewhat higher (46.8%) than with the annual conversion rates (35.4%). Even so, the second-stage estimate for the impact of conversion is similar to that with annual conversion rates (-0.652 versus -0.664). The second and third models add external control

variables to the main specification. In the second model, *administrator-choice* is a dummy for the firm being assisted by a Court-appointed Administrator even though, given the size of the firm, this is not mandatory.[14] In the third model, *out-of-court restructuration* is a dummy for the firm having tried to reach a confidential agreement with its creditors prior to filing for Sauvegarde.[15] Unfortunately, while we are sometimes able to identify the presence of an Administrator or an out-of-court Restructuration, this information is not exhaustive. As mentioned above, while the presence of a previous out-of-court restructuration does indeed mean there was one, its absence does not mean there was not. This also applies to the presence of an Administrator. In addition, this information may also be endogenous: a firm choosing an Administrator reveals managerial preferences and/or its perception of the challenge the firm faces. The same holds for the prior attempts (successful or not) at confidential restructuring. Given these limitations, the results in Table 10 should be interpreted with caution, and we are mainly interested in checking the stability of the estimates of our coefficients of interest and instrument validity. The coefficient on the instrument (*share of other cases converted)* remains unchanged (0.351 and 0.354 with the introduction of *administrator-choice* and *out-of-court restructuration* respectively, versus 0.354 previously), and the F-statistic is well above the critical threshold (61.3 and 64.1). Firms that are supervised by an Administrator even though this is not mandatory are more likely to be converted. Prior confidential restructuring attempts do not predict firm outcomes in the Court-supervised procedure. Last, the impact of conversion on debt restructuration (-0.664 and -0.663) is the same as that estimated previously (-0.664).

We take advantage of a change in the Law regarding the conversion of Sauvegarde cases to *RJ* to carry out an additional robustness test. In mid-2014, new provisions were introduced regarding the stakeholders who can ask the Court to convert the case. As of July 2014, the Judge can no longer ask for conversion, a

[14] When filing for Sauvegarde, the court has to appoint an Administrator only if the firm has over 250 employees or €3 million in total sales. Under these thresholds, it is the firm's choice whether to call on an Administrator. Our *administrator-choice* dummy is zero for large firms and small firms that chose not to be assisted, and one for small firms that chose to be assisted. This information comes from BODACC, which includes the name of the Administrator in charge.

[15] There are two different confidential procedures, the "*Mandat ad hoc*" and "*Conciliation*". Agreement requires the unanimously acceptance of all creditors taking part in the confidential procedure (these are typically the largest).

prerogative that is granted only to the firm's management, the Court-appointed Administrator (if any), the Court-appointed Receiver and the Public Prosecutor's office. To better capture the effects of this change, we restrict our sample to 2013-2015 filings, and use as an instrument the *share of other cases converted* calculated on a semi-annual basis. We compare the results in our main specification (column 3 of Table 6 on the 2010 – 2018 period) to those 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 smaller than before (0.275 versus 0.354) and the effect of conversion on debt restructuration slightly larger (-0.749 versus -0.664). Columns 3 to 5 introduce the dummy "Before July 1st 2014" for the Sauvegarde observation period ending before the change in the Law. In the first-stages (columns 3 and 4), we instrument *Conversion* and *Conversion×Before* by 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), so that the share of Sauvegarde cases on the margin of being converted is similar before and after the change in the Law. As *Conversion×Before* is not significant in the second stage (column 5), we conclude that *Conversion* reduces the probability of a debt-restructuring similarly before and after the change in the Law (by -0.775, identical to the baseline results for the same period).

Last, we carry out a placebo test to rule out the possibility that our results come about only by chance, substituting our instrument by a randomly-generated variable that matches the original values of *share of other cases converted.* We replicate our preferred specification (column 3 of Table 6) with this randomly-generated instrument 10 000 times. Were we to find similar results in these regressions, we would question the validity of our identification strategy. Figure 2 plots the distribution of the 10 000 placebo regressions and the main statistics from the resulting estimates. On average, the coefficient on the randomly-generated instrument is zero. The actual coefficient on *share of other cases converted* (0.354) is over ten standard deviations (0.027) above the mean, and much above the distribution's maximum

estimate (0.108). While these results do not prove that our instrument is valid, they do alleviate concerns that it only reflects chance.

B. The impact of RJ conversion on survival rates after restructuration

Table 12 shows the impact of conversion to *RJ* on firm survival after debt restructuration. We focus on survival at two and five years after agreement has been reached with the creditors. Since we cover filing outcomes up to June 2018, we consider the two-year survival of the 3,037 firms that obtained a restructuration deal before June 2016, and the five-year survival of the 1,126 firms that obtained a restructuration deal before June 2013. We report OLS and IV-2SLS second-stage estimates, which both convey the same message: once the firm reaches an agreement, its survival is not influenced by whether its filing was converted. This holds both in the first years after restructuration and much later. The stigma associated with *RJ* disappears as soon as the firm has been granted a second chance.

We do note however that the firm's characteristics at the beginning of the bankruptcy procedure remain decisive with respect to its survival after restructuration. Older and larger firms are more likely to survive, while ("zombie") firms with the weakest financial situation are less resilient. One important finding is that survival is not correlated with the initial amount of total debt, but is correlated with the share of supplier debt in total debt at bankruptcy entry. The Court-level control variables no longer explain firm survival.

IV. Conclusion

Using a novel – and almost exhaustive – data set of Court-supervised bankruptcy procedures in France, this paper analyzes the effect of being converted from a bankruptcy procedure that is restricted to firms that are not yet insolvent (*Sauvegarde*) into the regular bankruptcy procedure (*RJ*), that which applies to insolvent firms. Both bankruptcy procedures are public and Court-supervised. Using heterogeneity in Commercial-Court conversion rates as an instrument for conversion, we show that for firms at the margin of being converted, conversion reduces their chance of reaching a deal with their creditors by 64

percentage points. As the legal provisions differ little between the two procedures, we attribute this to a pure signal effect. The track-record of *RJ* is so bad that stakeholders of firms converted into *RJ* anticipate little renegotiation success. This anticipation is self-fulfilling. Interestingly, this stigma attached to conversion seems to disappear once the firm reaches an agreement with its creditors: the survival rates two and five years after successful debt-restructuring do not depend on whether the bankruptcy procedure was converted (instrumented as before by the Commercial-Court's propensity to convert cases). These results are robust across specifications, even taking into account a change to the Law in mid-2014 that reduced the Judge's prerogatives in conversion.

The rationale for introducing the *Sauvegarde* procedure into French Commercial Law was to propose a Court-supervised debt-restructuring procedure to fragile firms that were not yet insolvent. Our results support this policy choice, rather than the simple extension of *RJ* to these firms. *Sauvegarde* allows financial stakeholders to differentiate firms from the average firm filing for bankruptcy, thus better preserving the chance of retaining the firm as a going concern.

Given the small numbers of firms filing for *Sauvegarde* (around 1 000 per year), compared to over 10 000 per year filing for *RJ*, the next question would be whether more firms should be encouraged to fil for *Sauvegarde*. Informal discussions with Judges, Court-appointed Receivers and Insolvency Administrators lead us to think that some firms filing for RJ would indeed qualify for *Sauvegarde*. First, more – and better – information about the *Sauvegarde* procedure could be provided to firms, notably when they are about to file for *RJ*. Second, firms filing for *RJ* immediately qualify for a loan from the AGS (a not-for-profit business association) to pay worker wage arrears. Firms filing for *Sauvegarde* do not have access to this funding, even if their case is subsequently converted to *RJ*. That may deter firms from filing for *Sauvegarde*, which could be remedied by allowing firms converted to *RJ* access to the wage-arrears loan.

Last, we can speculate whether the mere possibility of the *Sauvegarde* procedure being (wrongly) converted to *RJ* may deter firms from filing for *Sauvegarde* in the first place. Our results show that Commercial Courts with a history of high conversion rates do not receive relatively fewer *Sauvegarde*

filings: we indeed take this as evidence that our instrument is valid. The theoretical literature is ambiguous on the impact of random judicial mistakes. For example, Bernhardt and Nosal (2004) argue that it may be optimal for Commercial Courts to make random errors. In our case, the risk that the case be converted could provide an incentive for firms to seek private debt-restructuration. Removing this risk and making the *Sauvegarde* procedure more predictable would then have the opposite effect.

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Figure 1
The Distribution of Court Deviations from the Mean Initial Conversion Rate

The *raw* distribution represents the difference between the Court's conversion rates and the unadjusted annual average, and the *adjusted* distribution of the same difference with all controls and fixed effects as in column 3 of Table 6.

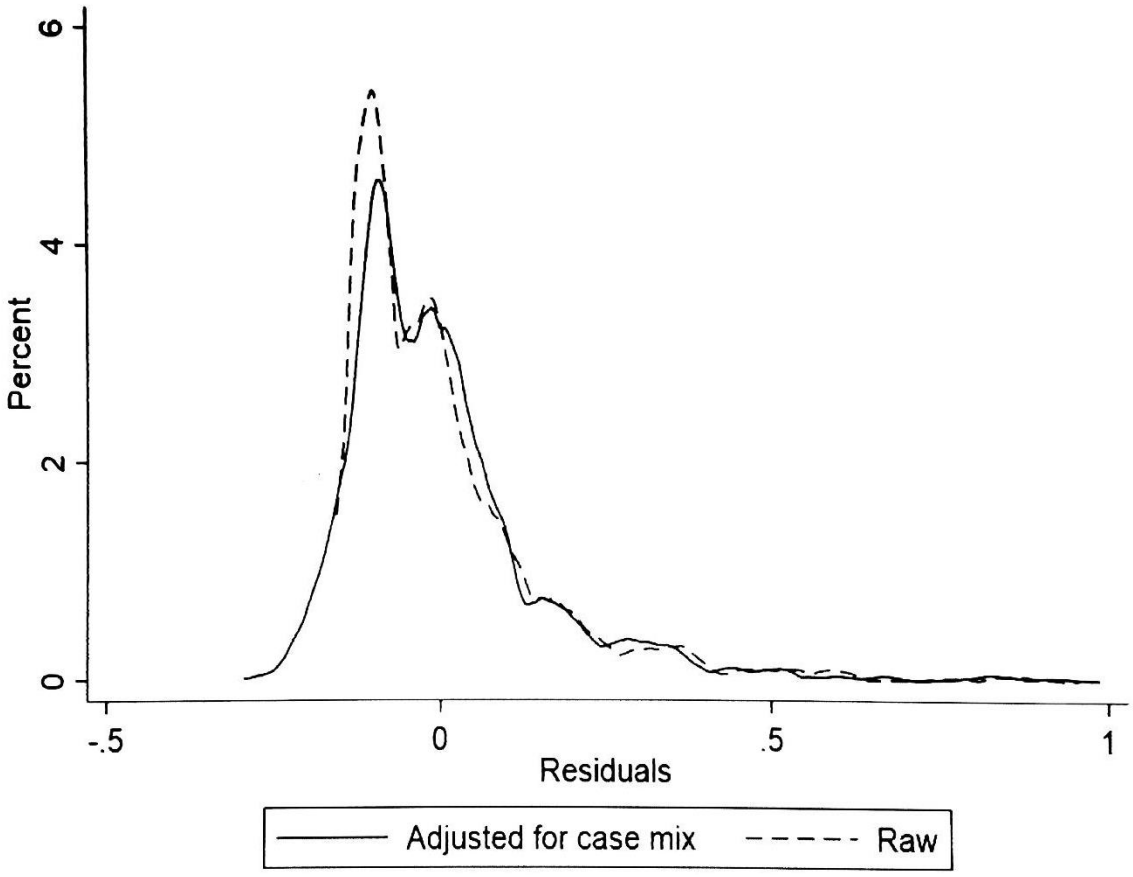

Figure 2
Placebo Test

This figure shows the histogram of the coefficient on the *share of other cases converted* from 10,000 placebo regressions where the instrument *share of other cases converted* is randomly assigned within the sample. It contains the full set of controls and fixed effects from column 3 Table 6.

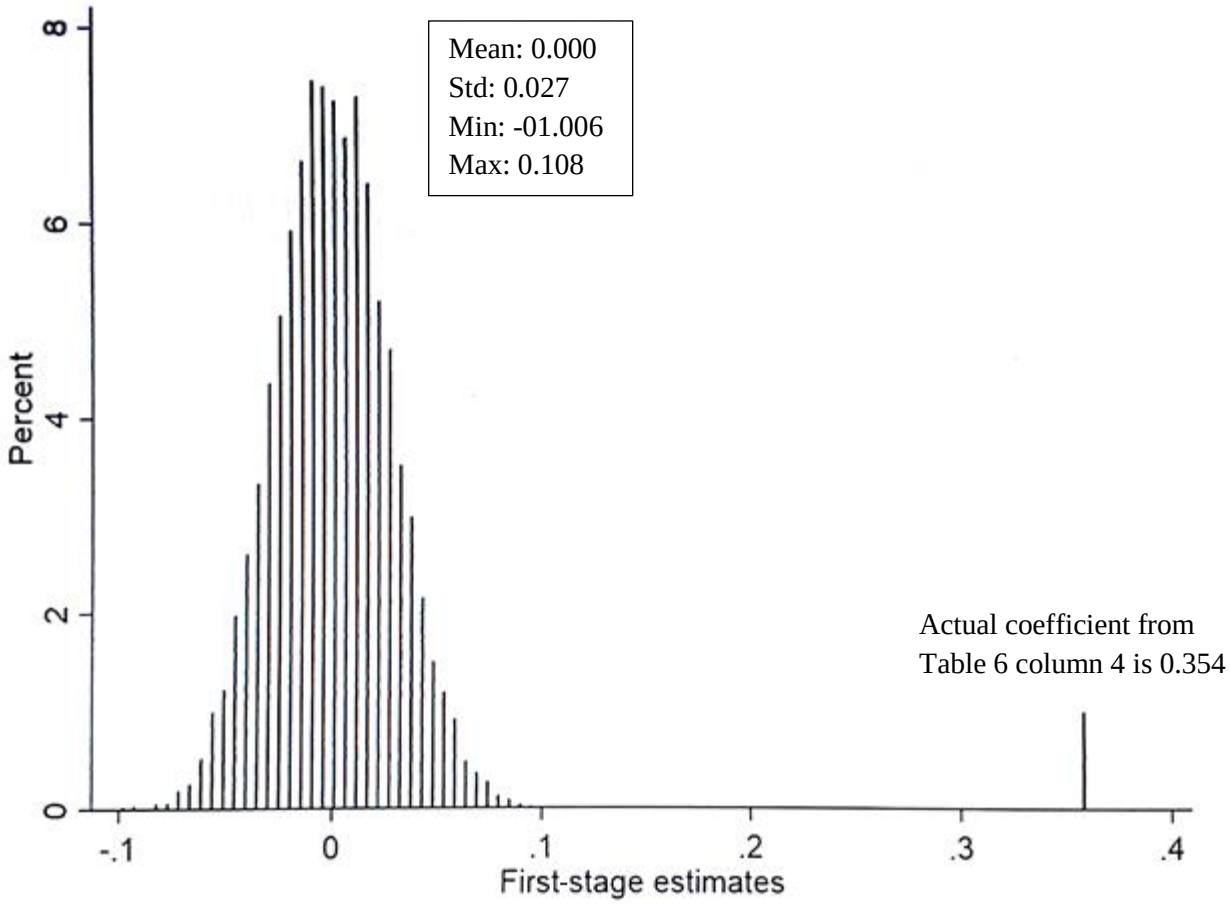

Table 1
Summary statistics for firms entering bankruptcy procedures between 2010 and 2016

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

Table 2
Breakdown per year of filing

This table shows 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	Initial sample		Working sample	
	Number of firms	Percent converted	Number of firms	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 lists 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 the regressions, the share of cases converted presented here includes the current case (and has one single value per Court per year). This table aims to show the heterogeneity amongst Courts by judgment year, as well as over the entire January 2010 - June 2018 period.

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 filings does not depend on the Court's past conversion rate

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

Dependent variable	Share of Sauvegarde filings in year t
Share of cases converted	
In year $t-1$	-0.0126
	(0.0109)
In year $t-2$	-0.00841
	(0.00108)
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 the Court-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 to Courts is not random, but depends on the firm's location. For the instrument to be exogenous, we test in Table 5 that the Court conversion rate is not influenced by its share of Sauvegarde filings in previous years. The share of cases converted in this table does not exclude the present case. The regression is run at the Court-level, from year t=2012 to 2016.

Dependent variable	Share of cases converted in year t
Share of Sauvegarde filings	
In year $t-1$	-0.263
	(0.167)
In year $t-2$	-0.0224
	(0.154)
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 the Court-year level
*** p<0.01, ** p<0.05, * p<0.1

Table 6
First Stage

This table reports the results from the first-stage regressions. The dependent variable is a dummy for the firm being converted to *RJ*, and the instrument the share of cases converted by the Commercial Court every year, excluding the current case. The model is robust to the introduction of fixed effects and 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 effects and 6 year-of-filing fixed effects. The standard errors in parentheses are clustered at the Court-year of judgment level.

Dependent variable	Conversion to *RJ*		
	(1)	(2)	(3)
Share of other cases converted	0.364***	0.360***	0.354***
	(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	Yes	Yes	Yes
Industry fixed effects	Yes	Yes	Yes
Legal-Form fixed effects	Yes	Yes	Yes
Year-of-filing fixed effects	Yes	Yes	Yes
Observations	6,283	6,283	6,283
Adjusted R-squared	0.040	0.047	0.057
F-statistic for instrument	64.64	66.06	64.28

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

Table 7
First-Stage Heterogeneity

This table shows the first-stage regressions in column 3 of 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-statistic and F-statistic respectively, column 6 the fraction of firms that would be converted regardless of the Court, and column 7 the relative likelihood as described in the text. Due to collinearity, some observations were omitted in the subgroups split by industry, number of employees and debt ratios, the totals of which do 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 3 of Table 6 with debt restructuring in *RJ* as the dependent variable. We assume that if the propensity to convert has no impact on debt restructuration in *RJ*, it will have no impact either in Sauvegarde, which is very similar. Column 1 reports the regression on the sample of all *RJ*, and column 2 that on the subsample of voluntary *RJ* filers. Voluntary *RJ* filers are most similar to Sauvegarde filers as they are likely to be more pro-active in the face of financial difficulties. Standard errors in parentheses are clustered at the Court-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 to restructure 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 estimated by 2SLS using as an 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 3 of Table 6. Standard errors in parentheses are clustered at the Court-year of judgement level.

Dependent variable	Debt restructuring (YES/NO)	
Model	OLS (1)	IV-2SLS (2)
Conversion	-0.466*** (0.0200)	-0.664*** (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 shows versions of the first- and second-stage regressions containing the full set of controls used in column 3 of Table 6. The first specification in columns 1 and 2 uses the non-annual *share of other cases converted* as an instrument. The regressions in columns 3 to 6 introduce additional control variables: *administrator-choice* is a dummy for the firm having an Administrator when this is not mandatory, and *out-of-court restructuration* is a dummy for the firm having tried to reach a confidential agreement with its creditors prior to bankruptcy entry. All regressions contain the full set of controls and fixed effects used in column 3 of Table 6. Standard errors in parentheses are clustered at the Court-year of judgement level.

Specification	IV-2SLS with non-annual conversion rate as an instrument		IV-2SLS using the presence of an Administrator as additional information		IV-2SLS using the existence of 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***		0.351***		0.354***	
	(0.0943)		(0.0448)		(0.0441)	
Conversion		-0.652***		-0.664***		-0.663***
		(0.233)		(0.138)		(0.137)
Administrator-choice			0.0401***	0.000437		
			(0.0106)	(0.0152)		
Out-of-court restructuration					0.0128	0.0331
					(0.0200)	(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 the Law that prevents Courts from deciding the conversion of a case

The regressions in columns 3 to 6 consider the change in the Law of July 1st 2014 preventing the Court from deciding the conversion of a case. We introduce the dummy "Before July 1st 2014" for the case being judged before the change in the Law. We restrict the period studied to 2013-2015 to focus on the most-affected period. *Share of other cases converted* is calculated by semester. For comparison, columns 1 and 2 report the first and second stages of the standard specification (without the change in the Law) with this new instrument. Column 6 reports the OLS results with the change in the Law. All regressions contain the full set of controls and fixed effects used in column 3 of Table 6. Standard errors in parentheses are clustered at the Court-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
	1st stage	2nd stage	1st stage		2nd stage	
Dependent variable	Conversion	Debt Restructuring	Conversion	Conversion×Before	Debt Restructuring	Debt Restructuring
	(1)	(2)	(3)	(4)	(5)	(6)
Share of other cases converted	0.275***		0.246***	-0.0180		
	(0.0477)		(0.0830)	(0.0130)		
Share of other cases converted×Before			-0.0173	0.272***		
			(0.0987)	(0.0580)		
Conversion		-0.749***			-0.775***	-0.431***
		(0.191)			(0.288)	(0.0427)
Conversion×Before					0.334	-0.0510
					(0.354)	(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-statistic for instrument	33.18		13.23	10.99		

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

Table 12
Survival after debt restructuration

The dependent variables are the survival at the two- and five-year horizons after debt restructuration. Conversion is a dummy variable that indicates whether the firm was converted from Sauvegarde to *RJ*. The regression columns 1 and 3 are estimated by OLS; the regressions columns 2 and 4 are the reduced forms estimated by 2SLS using as the 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 3 of Table 6. Standard errors in parentheses are clustered at the Court-by-year of judgement level.

Dependent variable	Survival rate at different horizons			
Horizon	Two years		Five years	
	OLS	IV 2nd stage	OLS	IV 2nd stage
	(1)	(2)	(3)	(4)
Conversion	-0.0136	-0.168	0.0913	-0.133
	(0.0399)	(0.222)	(0.0662)	(0.269)
Firm-level control variables				
Ln(employees)	-0.0151**	-0.0139**	-0.0299**	-0.0289**
Age (> 5 years old)	-0.0825***	-0.0856***	-0.148	-0.152***
Zombies	0.0367**	0.0368**	0.0749**	0.0749**
Total debt / asset	0.0178	0.0174	0.0486	0.0479
Supplier debt / debt	0.148***	0.147***	0.213***	0.207***
Court-level control variables				
Unemployment rate	0.00395	0.00334	0.0185	0.0161
Share of direct liquidations	-0.0655	-0.0536	-0.188	-0.197
Ln(size of the Court)	0.000262	-0.00170	-0.0338	-0.0360
All fixed effects	Yes	Yes	Yes	Yes
Observations	3,037	3,037	1,126	1,126
Adjusted R-squared	0.023	0.008	0.098	0.049

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