BEYOND THE DEBTORS' EDGE: AN ANALYSIS OF BANKRUPTCY SPILLOVER EFFECTS ON CORPORATE CREDITORS

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Abstract: This essay investigates bankruptcy spillover effects on corporate creditors. We employ a difference-in-differences matching estimator strategy to compare the performance of bankrupt firms' creditors (treated group) and similar firms without any business relationship with a bankrupt firm (control group). We implement a propensity score sample matching to obtain our control group from the population of nontreated firms. Our implicit hypothesis concerns that the treated group might underperform the control group after the bankruptcy event. We create a novel dataset on hand-collected bankruptcy proceedings judicial data from the State Court of São Paulo (TJSP) matched to Brazilian employer-employee administrative data (RAIS). We adopt the number of employees and the total remuneration of employees as proxies for performance to examine the effects of a bankruptcy event on corporate creditors. Our main results indicate that the contagion effects of bankruptcy reach both the treated group (corporate creditors) and control group (similar firms with no direct link to a bankruptcy reorganization event). There is little evidence that the impact is different between the two groups. Moreover, we assume that the adverse spillover effects on both groups are mainly from bankruptcy reorganization cases converted to liquidation. Together the findings suggest that a more profound corporate crisis leading to a liquidation may spill substantially more over other firms linked to the bankrupt firm but also in the local economy or related industries. The findings may extend the current bankruptcy literature to better understand the boundaries of a corporate crisis and contribute to the formulation of legal reforms.

Keywords: Corporate Bankruptcy. Spillover Effects. Contagion. Corporate Creditors.

1. Introduction

Legal and institutional environments are widely believed to underpin economic development. Bankruptcy regimes play an important role in the economy and in society. They establish coordinated proceedings to resolve problems of firms that are unable to serve their debts. Bankruptcy norms and their interpretation provide institutional solutions to corporate crises through liquidation or reorganization. They also guide how economic agents act in the business market during and before the onset of corporate crisis.

The role of bankruptcy regimes may be strengthened in emerging markets. These markets are characterized by weaker institutions and higher volatility. Since mid-2014, the Brazilian economic and political crises have led to an increase in the number of insolvent firms[1–2]. The *Covid-19* outbreak had also negatively impacted the local economy and the Brazilian firms'

[1] Serasa Experian's (2022) data indicates that the number of delinquent firms in December 2019 was around 6,1 million. At the beginning of the time series (March 2016), the number of delinquent firms was 4,2 million.

[2] Bankruptcy liquidation is just one way for firms to exit the market. Because of tax liabilities, bankruptcy norms, and procedural costs, anecdotal evidence suggests that most insolvent firms in Brazil exit the market without a regular administrative or legal process. The firms' formal registration in a governmental or regulatory authority subsists, sometimes with related due debts, but without any economic activity. Regarding reorganization bankruptcy, out-of-court proceedings are also an alternative from initiating a legal proceeding.

financial health. Consequently, scholars and legal practitioners have intensified the debate about the adequacy of the Brazilian bankruptcy law (Law 11.101/2005) to provide efficient solutions to a sustainable corporate business market[3]. **Figure 1** shows the number of requested bankruptcy liquidations and court-supervised reorganizations in Brazil from 2010 to 2019 (before the impact of the global pandemic).

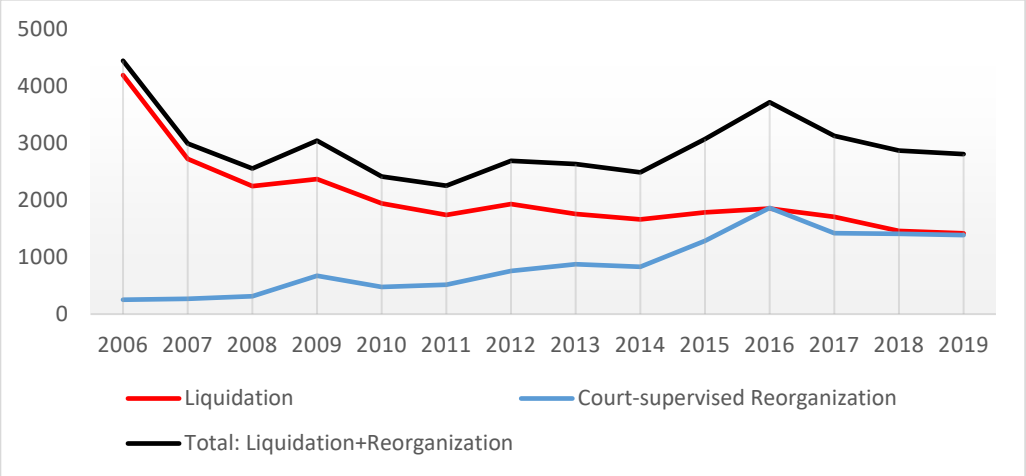

Figure 1 – Bankruptcy liquidation and court-supervised reorganization requests in Brazil (2010-2019).
The figure shows the yearly number of bankruptcy liquidation and reorganization requests in Brazil during the period 2010-2019. The data is extracted from the *Serasa Experian* Bankruptcy Index[4].

Most of the empirical bankruptcy literature focuses on the effects of bankruptcy proceedings' attributes on its outcomes (Warren et al., 2009; LoPucki & Doherty, 2014) or its costs (Weiss, 1990; Bris et al., 2006; Jupetipe et al., 2017), on the ex-post effects on the bankrupt firms itself (Bernstein, Colonnelli, & Iverson, 2019), or the ex-ante effects on the business market, including the influence on capital production factors, such as labor and credit markets (Cornelli & Felli, 1997; Ponticelli & Alencar, 2016; Agrawal et al., 2019).

However, bankruptcy liquidations and reorganizations often generate negative externalities to other firms (Altman et al., 2019). The effects of corporate bankruptcy play a central role in the financial situation of bankrupt firms' claimholders. The postponement or ceasing of scheduled payments may worsen the creditors' financial health. Thus, the financial difficulties of an insolvent firm may spill over into its creditors[5]. In an efficient bankruptcy regime, spillover effects should be unimportant. Despite its relevance, to the best of our knowledge, there is scarce empirical literature on bankruptcy spillover effects, especially in emerging markets context. We intend to partly fill this research void by empirically evaluating the impact of bankruptcy reorganization and liquidation on bankrupt firms' creditors in Brazil.

To examine the existence of bankruptcy spillover effects on corporate creditors, we employ a difference-in-differences matching estimator strategy to compare the performance of bankrupt firms' creditors (treated group) and similar firms that have not claimed a debt repayment

[3] The Brazilian National Congress recurrently discussed a new bankruptcy law reform. In December 2020, the Brazilian bankruptcy law was overhauled with substantial changes. Nevertheless, the lawmaking discussion lacked proper empirical appraisal of the bankruptcy law in force.

[4] SERASA EXPERIAN. *Bankruptcy Index*. 2022. Available at: <https://www.serasaexperian.com.br/amplie-seus-conhecimentos/indicadores-economicos>.

[5] Bankruptcy liquidation and reorganization effects may also span to other economic agents, such as employees, shareholders, government, local firms, and even competitors.

under a bankruptcy proceeding (control group). We implement a propensity score sample matching that incorporates observable firm characteristics (industry, age, location, legal form, profit tax regime, and employees' education level and gender) to obtain our control group from the population of nontreated firms. Our implicit hypothesis concerns that the treated group might underperform the control group after the bankruptcy event. Thus, we assume that treated and control firms' expected differential performance is solely caused by bankruptcy spillover effects on corporate claimholders. Both treated and control firms would perform similarly in the absence of these spillover effects.

To conduct our research, we create a novel dataset on hand-collected bankruptcy lawsuit data from the State Court of São Paulo (TJSP) matched to Brazilian employer-employee administrative data (RAIS)[6]. Because of limited access to financial data of private-held firms, we use the percentage change in the number of employees (firm's net hiring) and the percentage change in the total remuneration of employees (firm's net labor costs) to proxy for the firm's performance. We claim that an increase (decrease) in performance should be correlated to higher (lower) levels of net hiring and labor costs. We follow both treated and control firms' data over time, beginning three years before up to three years after the bankruptcy filing, depending on data availability.

To investigate additional evidence on bankruptcy spillover effects, we also examine if the type of bankruptcy proceeding, reorganization or liquidation, imposes different impacts on corporate creditors' performance. In an efficient bankruptcy regime and frictionless market, both bankruptcy proceedings should lead to similar outcomes (Bernstein, Colonnelli, & Iverson, 2019), since both reorganization and liquidation approaches should provide the best level of outstanding debts repayment. On the other hand, frictions and inefficiencies may not lead to the best use of bankruptcy proceedings.

Reorganization proceedings may provide bankrupt firms with mechanisms to overcome the financial difficulties for later repayment of debts. The maintenance of economically viable businesses may also contribute to creditor suppliers' future revenue. However, the continuation of inefficient bankrupt firms may decrease its assets' value over time, potentially affecting creditors' recovery rate in later liquidations. Carrying on the supply of debtors' activity during reorganizations may also negatively affect creditors' performance in unsuccessful restructurings, even with post-repayment priority. Economic inefficient firms also prevent the efficient reallocation of assets that should generate positive externalities (Bernstein, Colonnelli, & Iverson, 2019). Still, liquidation proceedings may reflect the severity of the bankrupt firms' financial health and the potential negative impact on corporate claimholders. The disruption of the production of bankrupt firms and the lower local economic activity levels may also impose adverse shocks on creditors' future revenue (Bernstein, Colonnelli, & Iverson, 2019). Nevertheless, if the liquidation is of an economically inefficient firm, asset reallocation could increase corporate creditors' credit recovery. The efficiency of asset auctions is pivotal to mitigating potential spillover effects. Thus, we expect positive and negative effects of reorganization and liquidation, and we address the empirical issue of identifying the sum direction of these effects. Consequently, we evaluate the Brazilian corporate bankruptcy regime efficiency.

[6] *Relação Anual de Informações Sociais* (RAIS) is a Brazilian administrative dataset on employer-employee information. It is a mandatory annual survey filled out by all firms in Brazil.

Our results evidence that the contagion effects of bankruptcy reach both the treated group (corporate creditors) and control group (similar firms with no direct link to a bankruptcy). There is little evidence that the impact is different between the two groups. Since we match the creditors to similar firms considering the microregion and industry, the findings suggest that the dynamics of bankruptcy may propagate the corporate crises to the local economy or related industries. The results indicate with minor statistical significance (at 10% level) that treated firms performed relatively slightly better than control firms, losing fewer employees in the sample period. The average size of treated firms, which are larger, may partially guide attenuating the negative externalities of bankruptcy when compared to the smaller control firms. Finally, the adverse spillover effects on both groups appear to be mainly from bankruptcy reorganization cases converted to liquidation. This finding indicates that a more profound corporate crisis leading to a liquidation may spill substantially more over other firms (linked or not to the bankrupt firm).

This research contributes to the empirical literature on corporate bankruptcy. First, we provide evidence of spillover effects on corporate creditors in an emerging market context. Other papers have investigated how bankruptcy effects spill over to the local economy (Bernstein et al., 2019; Moraes, 2019), instead of just focusing on immediate related agents like corporate creditors. Moreover, most of the previous studies explored developed markets. Second, we apply a difference-in-differences matching estimator strategy (DID-ME) to mitigate endogeneity concerns in our estimations of treated and control firms. Although the methodology does not apply for causality since it lacks an exogenous variation, the estimated models extend the current empirical literature on the topic and indicate potential future research agenda. Third, we test the distinct effects of reorganization and liquidation proceedings on corporate creditors' performance. Compared to previous literature findings that suggest higher negative externalities of liquidations relative to reorganizations (Bernstein et al., 2019), our results provide similar evidence of more significant negative spillover effects from bankruptcy reorganization cases converted to liquidation. Last, we extend the corporate bankruptcy literature to better understand the boundaries of a corporate crisis and contribute to the formulation of legal reforms and norms' interpretation.

The remainder of this essay proceeds as follows. Section 2 discusses the related literature. Section 3 describes the institutional features of the Brazilian bankruptcy law. Section 4 describes the data. Section 5 presents our empirical strategy (research design). Section 6 shows and discusses our results. Section 7 concludes.

2. Related Literature

The literature on the **optimal design of bankruptcy law** discusses the mechanisms to minimize the social costs of bankruptcy (Aghion et al., 1994; Berkovitch & Israel, 1999). The bankruptcy proceedings must strike the right balance of incentives and protection of rights between shareholders, managers, creditors, and other related parties[7]. The efficiency of a bankruptcy regime relies on its ability to screen out inviable businesses and to maintain economically viable businesses (Altman et al., 2019; Araujo & Funchal, 2006). An extensive theoretical literature has modeled optimal design of bankruptcy regimes (Aghion et al., 1994; Berkovitch & Israel, 1999; Araujo & Funchal, 2006). This essay intends to bring new evidence to the empirical literature on the topic (Djankov et al., 2008; Araujo et al., 2012;

[7] Principal-agent relationships and conflicts of interest are substantial during and before the onset of corporate crises (Jensen & Meckling, 1976; Berkovitch & Israel, 1999).

Ponticelli & Alencar, 2016; Bernstein, Colonnelli, & Iverson, 2019) by examining one dimension of efficiency of the Brazilian bankruptcy regimes related to mitigating negative externalities in corporate creditors.

Closely related is the literature on **bankruptcy effects** that examines the *ex-ante* and *ex-post* consequences of bankruptcy (Cornelli & Felli, 1997; Bebchuk, 2002; Araujo & Funchal, 2006). The *ex-ante* effects consist of the influence of bankruptcy regimes on macroeconomic indicators and on how economic agents act in the business market before the onset of a corporate crisis. The empirical literature evaluates the impacts on the credit market (Araujo et al., 2012; Barbosa et al., 2017; Ponticelli & Alencar, 2016), labor market (Fonseca & Doornik, 2019; Graham et al., 2019), firms' financial management (Agrawal et al., 2019), and investments level (Ponticelli & Alencar, 2016). The *ex-post* effects are related to the frictions and the costs and benefits of bankruptcy proceedings on businesses' or firms' post-performance and real outcomes (Bernstein, Colonnelli, Giroud, et al., 2019). Previous empirical papers have examined how bankruptcy proceedings' attributes affect its outcomes, such as plan confirmation rates (Warren et al., 2009), bankrupt firms' survival rates (LoPucki & Doherty, 2014), and procedural costs and recovery rates (Weiss, 1990; Bris et al., 2006; Jupetipe et al., 2017). Most of the literature on bankruptcy effects focuses exclusively on the bankrupt firm (debtor) and overlook the impacts on other related parties, such as shareholders, managers, creditors, workers, and governments. This essay will complement the empirical literature by investigating bankruptcy *ex-post* effects on corporate creditors outcomes.

The growing literature on **bankruptcy spillover effects** relies on the idea that bankruptcy regimes may also produce substantial externalities (Warren et al., 2009; Skeel, 2014; Altman et al., 2019; Bernstein, Colonnelli, Giroud, et al., 2019). These externalities may be positive, such as protecting employment and advancing community stability (Warren et al., 2009), or negative, such as reducing local plant occupancy and employment (Bernstein, Colonnelli, & Iverson, 2019). The industry and the size of the bankrupt firm may be important determinants of the intensity of the contagion effects (Skeel, 2014). The recent empirical literature focuses on bankruptcy spillovers effects on several individuals, such as geographically proximate firms (Bernstein, Colonnelli, Giroud, et al., 2019; Moraes, 2019) and consumers (Shoag & Veuger, 2018), as well as on the propagation across economic networks (Acemoglu et al., 2012; Carvalho, 2014) and intra-industry (Jorion & Zhang, 2007). We aim to contribute to the literature by examining the contagion effects on corporate creditors in an emerging market context. We differ from prior studies since we will apply a difference-in-differences matching estimation procedure to support our identification strategy. It exploits differentials in the performance of bankrupt firms' corporate creditors and similar firms that have not claimed a debt repayment under a bankruptcy proceeding. Even if bankruptcy reorganization and liquidation affect the entire local market, we deem the effects would be more pronounced in creditors.

Finally, recent empirical research considers the **background of the Brazilian Bankruptcy Law (Law 11.101/2005)**. Most of the articles exploit the 2005 law reform as an exogenous source of variation that enhanced secured creditors' protection. Following a quasi-experimental approach, these papers examine the effects of the law reform on firms' debt financing, and cost of debt (Araujo et al., 2012), firms' investments level, access to finance, and size (Ponticelli & Alencar, 2016), and employment and earnings of high- and low-skilled workers (Fonseca & Doornik, 2019). Ponticelli and Alencar (2016) also exploit the variation in the congestion of civil courts across Brazilian municipalities to estimate the effects of court enforcement on firms' outcomes. Their identification strategy contributed to further research investigating the impacts of Brazilian court enforcement on banks' decision to file for a

debtor to go into bankruptcy, resolutions of bankruptcy proceedings, and employment in firms geographically close to a bankrupt firm (Moraes, 2019). In contrast to these previous studies, this essay does not exploit the law reform background but focuses on an identification strategy that applies a difference-in-differences approach through a matching estimation procedure (DID-ME). This essay also differs from most previous papers since it centers the attention of the bankruptcy effects on corporate creditors. Thus, we aim to disentangle the spillover effects on creditors and non-creditors proximate firms.

3. The Brazilian Bankruptcy Law

The going Brazilian bankruptcy law (BBL) entered into force in 2005 during a wave of bankruptcy law reforms influenced by the recommendations from the World Bank and the United Nations Commission on International Trade Law (UNCITRAL)[8]. The law reform sought to preserve the debtors' going-concern value, allocate assets to their best use, improve credit recovery, and strike a balance between bankruptcy liquidation and reorganization proceedings (Campana Filho, 2009; Uncitral, 2005). The Brazilian bankruptcy regime provides two main alternative legal proceedings: bankruptcy liquidation and bankruptcy reorganization (court-supervised reorganization)[9].

The bankruptcy liquidation is the legal proceeding to coordinate a debtor firm to an end and distribute the proceeds from the sale of its assets to creditors. Both the debtor (voluntary petition) and creditors (involuntary petition) have legal rights to initiate a bankruptcy liquidation proceeding[10]. In involuntary proceedings, the debtor firm may challenge the creditors' claim, pay the claimed debt, come to an out-of-court agreement, or file for a court-supervised reorganization proceeding. Once the legal requirements to file for bankruptcy are satisfied, the court appoints a trustee to manage the bankruptcy estate[11]. The trustee collects the debtors' assets, appraise their value, and provide the assets sales in court auctions to pay off the creditors. The distribution of the auctions' proceeds must follow the absolute priority rule order: (i) labor claims, (ii) secured claims, (iii) tax claims, (iv) unsecured claims; and (v) equity claims[12]. The debtor is discharged only after paying at least half of the unsecured

[8] In December 2020, the Act 14.112/2020 overhauled the Brazilian bankruptcy law (Law 11.101/2005) with substantial changes. Nevertheless, our research sample encompasses data from the period 2010-2017 and is not affected by the potential influence of the norms' alterations. We emphasize that the lawmaking reform discussion lacked proper empirical appraisal of the bankruptcy law.

[9] The Brazilian bankruptcy law also provides an out-of-court reorganization procedure, an analogous proceeding to prepackaged restructurings of other jurisdictions. The debtor firm privately negotiates creditors' acceptance of a proposed reorganization plan to further file for court ratification. It requires the approval of 3/5 of the secured and unsecured creditors (labor claimers are excluded). All creditors, even dissenting ones, are subjected to the plan if confirmed by the court.

[10] Debtors rarely file for bankruptcy liquidation in Brazil. In the case of involuntary petitions, legal requirements must be met. A debtor must be unable to repay outstanding debts at a value equivalent to 40 months of minimum wages, remain inert in an enforcement proceeding (no repayment or pledge of collateral), or act fraudulently within the prebankruptcy period.

[11] The BBL assures that buyers of assets sold in court auctions will not hold any debtor liabilities. The law also prioritizes the sale of the whole business, or as separate productive units, instead of individual assets, to mitigate business' value decrease and protect the going-concern value.

[12] Nevertheless, the law provides "superpriority" rules (claims that must be paid before the APR) to trustee fees, procedure administrative expenses, post-petition trade credits, and debtor-in-possession ("DIP") financing. Moreover, the law caps the priority repayment of labor or occupational accident claims to 150 minimum wages per creditor and secured claims to the collateral asset's value. The remainder of both claims is classified as unsecured. The priority position of tax claim exempts tax penalties, which are positioned after unsecured claims.

claims, or after five years from the end of the liquidation proceeding. **Figure 2** summarizes the Brazilian bankruptcy liquidation proceeding.

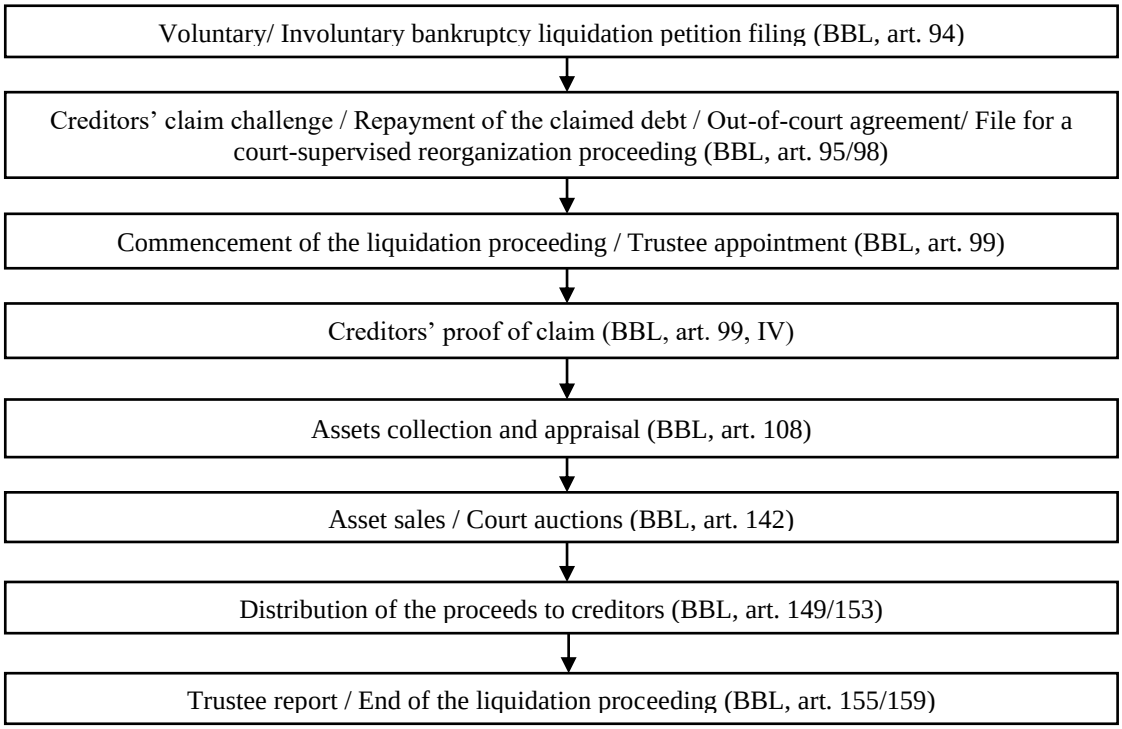

Figure 2 - Brazilian bankruptcy liquidation flowchart (before the 2020 BBL amendment).
Source: Adapted from Bezerra Filho (2018).

The Brazilian bankruptcy reorganization (court-supervised reorganization proceeding) is designed to preserve employment and viable firms' economic activity (going-concern value). The BBL determines that only debtors are allowed to initiate the reorganization proceeding (voluntary petition filing only)[13]. Once legal requirements are confirmed, the court grants the reorganization proceeding, appointing a trustee that oversees the debtor's activity (debtor in possession) and assists the court during the entire proceeding. An automatic stay period of 180 days on enforcement of actions by creditors applies. The reorganization plan must be submitted for creditors' approval within 60 days after the court accepts to initiate the bankruptcy proceeding[14]. If a single creditor poses objections to the plan, the court must schedule a general meeting of creditors to approve, modify, or reject the debtor restructuring plan. In the case of creditors` acceptance of the plan and ratification by the court, the plan binds all creditors, even dissenting ones[15]. According to the BBL, the reorganization case ends

[13] In the BBL provisions before the recent 2020 law reform, although creditors were not entitled to file for reorganization bankruptcy or pose an alternative restructuring plan, they might propose a debtor's plan overhaul in the general meeting of creditors. The debtor's approval of the plan amendments was mandatory in these cases. The 2020 law overhaul allows creditors to submit an alternative restructuring plan if the creditors reject the debtor's plan or if the debtor does not file the reorganization plan in due course.

[14] Creditors are divided into four classes: labor claimers, secured claimers, unsecured claimers, and small-sized unsecured claimers. Tax liabilities, leasing loans, fiduciary ownership of real property, and exchange currency loans for exportations are not enrolled in reorganization proceedings.

[15] In the BBL, before the recent 2020 law reform, the court converted the reorganization proceeding into a liquidation proceeding in the case of the plan's rejection by the creditors. The amended law now allows creditors to submit an alternative restructuring plan before the conversion from reorganization to bankruptcy liquidation.

after two years of the plan confirmation by the court. **Figure 3** exhibits a simplified Brazilian court-supervised reorganization flowchart.

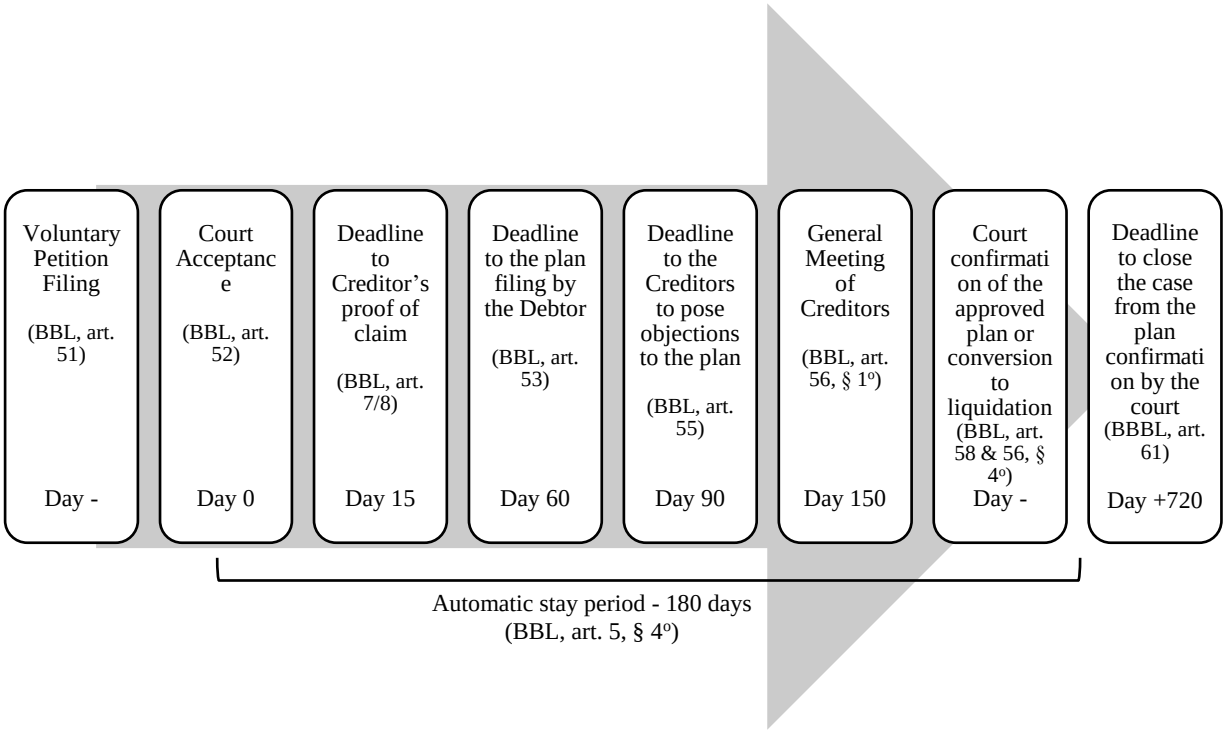

Figure 3 - Brazilian bankruptcy court-supervised reorganization flowchart (before the 2020 BBL amendment). Source: Adapted from Anapolsky and Woods (2013).

4. Data

Brazilian bankruptcy lawsuit data

Although Brazil is a federalist nation, most laws and legal codes encompass the entire country. It is the case of the Brazilian bankruptcy code (Law 11.101/2005). The civil judicial system is divided into federal and state courts. Legal demands regarding corporate and bankruptcy laws follow legal channels on one of the 27 state courts. The BBL provides that the debtor (or creditor) must file for bankruptcy liquidation or reorganization in the judicial district of the debtor's main establishment[16]. In most judicial districts, general civil courts handle bankruptcy proceedings. However, larger commercial cities (like São Paulo and Rio de Janeiro) created corporate or bankruptcy specialized courts.

To conduct our research, we create a novel dataset on hand-collected bankruptcy lawsuit data from the State Court of São Paulo (TJSP). We accessed data on a list of 630 bankruptcy reorganizations filings in the state of São Paulo between January 2010 and July 2017. Based on the bankruptcy case registration number, we extracted case information details available at the State Court of São Paulo website. It includes the debtor name, creditors name, other related agents name (trustees, third parties, tax collector), filing date, judicial district, judge, bankruptcy proceeding type, total claims value, and procedural steps up to April 2020.

[16] Bankruptcy forum shopping is not allowed. The Brazilian bankruptcy reorganization proceeding is voluntary petition filing only (exclusively for the debtor).

A caveat of our extracted lawsuit data concerns the lack of firm's tax identification number (*Cadastro Nacional de Pessoa Jurídica - CNPJ*). Therefore, we match our bankruptcy lawsuit data to our administrative employer-employee data by firms' name (*razão social*).

Brazilian employer-employee data (RAIS)

The Annual Social Information Report (*Relação Anual de Informações Sociais - RAIS*) is an administrative dataset on employer-employee information. The data covers all those individuals formally employed from private and public sectors. It is a mandatory annual survey filed by all organizations (including firms) in Brazil, even those with no hiring or firing in the relevant year. Since there are severe penalties for incomplete or late information, there is a high degree of compliance, which leads to an almost complete coverage of the formal sector (Fonseca & Doornik, 2019).

The data includes information on employers (firms), such as opening date, industry, municipality, profit tax regime[17], and number of employees. It also includes information on demographic, occupational, and income characteristics of employees. For instance, RAIS reports workers' age, gender, race, educational level, occupation, monthly earnings, and number of hours worked. Moreover, it covers the labor force movement (hiring and firing balance), month by month.

We match our bankruptcy lawsuit data to RAIS data by firms' names (*razão social*) since the lawsuit data lacks firm's tax identification number (*Cadastro Nacional de Pessoa Jurídica - CNPJ*).

5. Empirical Strategy

To examine the existence of bankruptcy spillover effects on corporate creditors, we employ a difference-in-differences matching estimator strategy (DID-ME). The potential difference across the performance of bankrupt firms' creditors and the performance of firms with no bankruptcy claims after the event of a debtor filing for bankruptcy reorganization gives the desired scenario to estimate the sensitivity of a creditor to a legal court-supervised bankruptcy reorganization or liquidation proceeding[18].

Our identification strategy relies on the performance comparison of bankrupt firms' creditors (treated group) and similar firms that do not hold bankruptcy claims (control group). Our assumption holds up on the argument that both creditors and control firms would have similar performance in the absence of the debtor's bankruptcy event. The control group provides a counterfactual scenario.

[17] In Brazil, there are three different corporate profit tax regimes: real profit regime, presumed profit regime, and a simplified tax regime for small businesses (*Simples Nacional*). Conceição et al. (2018) report that more than 70% of micro and small enterprises opt for the *Simples Nacional* since it reduces and simplifies the tax burden.

[18] One research caveat relies on the lack of information about the amount owed by each creditor within a bankruptcy case (we did not have access to the debtor's or trustee's creditor list). Data on revenue or profits of private-held firms is neither available. Thus, we could not calculate the ratio of a creditor's amount owed in a court-supervised bankruptcy proceeding to the creditors' revenue or profit. If available, it would be possible to evaluate the effective impact of the relevant debt on creditors financial health. Our empirical strategy accounts for creditors regardless of the potential financial impact of complete or partial credit loss.

We expect the treated group to underperform the control group only after the bankruptcy event. Thus, we assume (and test) that treated firms and control firms may behave very similar before the event of bankruptcy, following *parallel trends*. Post-bankruptcy's expected differential performance should be solely caused by bankruptcy spillover effects on corporate claimholders[19–20].

Because of limited access to financial data of private-held firms, we use data from RAIS to estimate the log of the change in the number of employees (firm's net hiring) and the log of the change in the total remuneration of employees (firm's net labor costs) to proxy for firms' performance. We claim that an increase (decrease) in performance should be correlated to higher (lower) levels of net hiring and labor costs[21]. Upon data availability, we evaluate the potential spillover effects for one and up to three years after the bankruptcy filing year to also estimate the 'duration' of the spillover effects on corporate creditors.

We consider in our final sample only firms from the State of São Paulo, since our data encompass only bankruptcy lawsuit from the State Court of São Paulo (TJSP). Moreover, since the number of employees is our proxy for performance, we exclude all firms registered with zero employees in the entire sample period in the employer-employee dataset (RAIS).

Control group selection- propensity score matching

To estimate our difference-in-difference specification, we implement a propensity score sample matching to obtain our control group from the population of nontreated firms. Thus, we draw control firms from the entire population of non-excluded firms which are not bankrupted nor holds claims in a liquidation or reorganization bankruptcy proceeding[22]. This strategy involves selecting control firms that best match treated firms in multiple observable characteristics, restricting our sample of counterfactuals.

[19] A caveat of this assumption is that a debtor bankruptcy proceeding may also indirectly affects a control firm through its possible effects on competitors, other firms of the same industry, local market firms, and other stakeholders. For instance, it may occur when a control firm is a creditor of a bankrupt firm's competitor, and this competitor (industry) is indirectly affected by the bankruptcy. The impact could be through the rise of the industry's interest rates because of the industry's increased risk or rating downgrade. Nevertheless, if that is the case, our estimations would be the lower bound effect. These situations reinforce the potential bankruptcy contagion on other economic agents.

[20] Since we acknowledge arguments that our empirical strategy may lack an exogenous variation (debtor filing for bankruptcy may not apply as exogenous to creditors), we may not address causality in this study.

[21] One concern may be the variation in the number of employees or the total remuneration of employees because of changes in the firms' labor productivity. If that is the case, deviation in labor productivity and bankruptcy spillovers could result in confounding effects. For instance, a decrease in the number of employees would rather represent productivity gains than performance downtrend or financial difficulties. However, we argue that breakthrough technologies may affect treated and control firms' net hiring and labor costs in longer terms than sudden debtor bankruptcy crises.

[22] One identification strategy caveat is that our control firms could be creditors of a bankrupt firm in another restructuring or liquidation venue rather than the State of São Paulo. Since we have not yet been able to gather information about bankruptcy proceedings in other Brazilian states, this situation may noise our estimations. It would be the case foremost for medium- and big-sized firms that may have business relationships with firms from other Brazilian states or firms located close to a state border. Nevertheless, if that is the case, our estimations of the spillover effects would be biased downwards.

Our criteria to match bankrupt firm's creditors to firms of the non-treated population consider observable firm characteristics available in RAIS dataset, namely, industry[23], age, location (microregion), profit tax regime[24], and employees' demographic, occupational, and income characteristics. We aim to mitigate selection bias and ensure that our treated and control firms have similar distributions along all the covariates. The matching estimator corroborates the strategy to compare treated and control firms within the same industry with very close characteristics, underpinning the argument that these firms would behave similarly in the absence of bankruptcy spillover effects of bankrupt firms.

Almeida et al. (2015) emphasize some potential advantages of the matching estimator methodology over a standard OLS approach. The matching estimator reduces the problem of poor distributional overlap of covariates across treated and control firms, which may affect OLS regression effectiveness, by selecting the closest covariate values when defining the firms in the control group. Moreover, it mitigates potential outliers' problems that affect OLS estimates, once the outliers are not selected to form the control group.

We implement a Mahalanobis propensity score matching to form our control group from non-treated firms. The underlying assumption is that conditional on the covariates, the fact of being a creditor of a bankrupt firm is orthogonal (independent) of the outcomes of interest. Once a control group is selected from all non-treated firms, we then estimate the average effect of the treatment on the treated (ATT), following the specification on **Equation 1**. The central aspect of this identification strategy is that we compare the deviation in employment level (or total remuneration) across the treated and control groups after the treatment (the event of a debtor bankruptcy reorganization), instead of just comparing the employment level of treated and control firms itself. This strategy mitigates the potential bias concerning uncontrolled firm-specific differences before the bankruptcy event (Almeida et al., 2012).

Empirical Model Specification

Our primary difference-in-difference regression model specification is represented by **Equation 1**.

$$\boldsymbol{log(Y_{jt}) = \beta_0 + \beta_1 * After_{ijt} + \beta_2 * Treated_j + \beta_3 * After_{ijt} * Treated_j + \delta + \varepsilon_{ijt}}$$

Equation 1 - DID-ME regression model specification.

where Y is the log of the outcome of interest (number of employees or total remuneration of employees) that varies across creditor firms and time. The subscript *i* identifies bankrupt firms, *j* identifies creditor firms, and *t* identifies time. The dummy $After_{it}$ captures the timing (*t*) of the filing for bankruptcy reorganization by the bankrupt firm *i* (it equals 0 before the bankruptcy event and 1 after the bankruptcy filing). The dummy $Treated_j$ captures if the firm *j* is a creditor of a bankrupt firm *i* (it equals 1 if it is a creditor firm - "treated group" and 0 if it is a similar firm that does not hold any bankruptcy claims - "control group"). We also control for fixed effects of firm, microregion and year of the bankruptcy event (δ) in our various specifications.

[23] We use the National Classification of Economic Activities (CNAE) at the 2-digit code level.

[24] The dummy variable equals 1 if the firm opted for the simplified tax regime for small businesses (*Simples Nacional*), and 0 if the firm did not choose the simplified tax regime.

The main coefficient of interest β_3 captures the performance differences (log of the change of the number of employees or total remuneration of employees) between treated and control firms after the onset of a reorganization or liquidation bankruptcy proceeding. The differential performance estimates the bankruptcy spillover effects.

Sample and Summary Statistics

The empirical design discussed previously requires merged lawsuit data from the State Court of São Paulo (TJSP) and administrative data from the Annual Social Information Report (RAIS). **Table 1** provides the sampling procedure and the summary statistics for bankruptcy reorganization cases in the state of São Paulo. Although we have accessed TJSP data on bankruptcy filings between January 2010 and July 2017, our employer-employee data (RAIS) covers only the period of 2011-2017. Since we need corporate data in the year prior to the bankruptcy event (Year -1) and at least one year after the bankruptcy (year+1) for our estimations, our TJSP lawsuit data sample consists of cases filed from 2012-2016.

Table 1 – TJSP lawsuit data: summary statistics.
The table reports the sampling procedure and summary statistics. The final sample contains 2126 corporate creditors listed in no more than one bankruptcy reorganization filing in the state of São Paulo (single-case creditors) from 2012-2016 (374 cases). Panel A summarizes the sample selection process. Panels B reports the sample case distribution by year. Panel C indicates the bankruptcy resolution as of April 2020. TJSP is the State Court of São Paulo.

TJSP lawsuit data		
Panel A - Sample Selection (Creditors)	Obs.	
Creditors from 2012 to 2016	22985	
Identified in RAIS	5691	
From the State of São Paulo (SP)	3830	
From SP with 1 or more employees	2943	
From SP with 1 or more employees and single case	2357	
Propensity score sample (final sample)	2126	
Panel B - Bankruptcy reorganization cases		
	Reorganization cases from 2012 to 2016	Reorganization cases in our final sample
Number of cases		
Total	521	374
2012	47	31
2013	114	82
2014	94	72
2015	131	97
2016	135	92
Creditors by case		
Average	53,84	71,61
S.D.	109,05	124,11
Median	23	35,5
Total Claims (in BRL)		
Average	12.300.267,21	16.008.220,87
S.D.	80.767.949,02	94.491.223,42
Median	100.000,00	100.000,00
Panel C - Bankruptcy resolution		

Reorganization	398	281
Liquidation	123	93

Table 1, Panel A, outlines the sample selection procedure. The sampling starts with 22985 creditors-case observations from 521 bankruptcy reorganization filings in the State Court of São Paulo from 2012-2016. This first sample comprises individuals (employees), corporate creditors, public administration, and municipalities. We then merged the data to RAIS and identified 5.691 corporate creditors[25]. We dropped firms registered outside the state of São Paulo and considered only creditors connected to single bankruptcy cases to avoid confounding effects of various events[26]. Our final sample consists of the 2126 creditor firms peered to firms not linked to any bankruptcy event through a propensity score matching method. Panel B displays the distribution for the initial and final samples. We observe a slight concentration of bankruptcy filings in our final sample in the years of 2015-2016. Panel C indicates the bankruptcy resolution as of April 2020. The data reveals that 25% of the bankruptcy reorganization cases were converted to a bankruptcy liquidation proceeding.

Table 2 presents summary statistics for the variables extracted from RAIS for the two groups of firms of our empirical design: control firms and treated firms (bankrupt firm's creditors)[27]. It also shows mean-comparison tests (t-tests). The control group comprises 2114 peered firms with no link to a bankruptcy event. The propensity score matching uses the variables Negative RAIS, Simples, Branch, Firm age, Industry (CNAE at 2-digit code level), employee education level, employees' gender, microregion (location), and legal form to peer the firms from the non-treated population.

The t-tests of the summary statistics reveal that although we employed the propensity score matching to identify similar firms of our treated group, there are still statistical differences in three critical variables. The average and median values of the total number of employees for the treated group (control group) are 325 (179) and 31 (16), respectively. The average of the total yearly remuneration of employees is BRL 834,480.00 for the treated group and BRL 446,730.00 for the control group. The t-tests (p-value 0.00) on these variables provide evidence of a statistically significant difference in the size of the firms from the treated and control groups. The economic variance in the number of employees and total remuneration between the groups also seems relevant. The data suggest that treated firms employ more workers and have higher yearly total remuneration than control firms[28]. These findings may mitigate the potential causal inference from our multivariate estimations.

The t-test also shows the statistical difference in the mean values of firm age (p-value 0.03). However, the economic impact does not appear to be relevant since the mean (median) of firm age for the treated group is 20.4 (17.3) and for the control group is 19.5 (16.4). There is

[25] The substantial decrease in the number of identified creditors when we match the TJSP data to the RAIS data is due to the significant number of labor-related claimholders (individuals whose names are missing in RAIS, since the dataset is on firm-level).

[26] We deem that creditor firms that have been subject to sequential bankruptcies will likely face confounding effects of different lawsuits, hindering the implementation of our empirical strategy through the comparison to a control firm (counterfactual).

[27] The comparison between non-treated and treated firms' characteristics showed statistically significant mean differences in several variables. It reinforces that our matching estimator approach may best fit our research proposal, mitigating endogeneity.

[28] Part of the higher yearly total remuneration of treated firms is mechanically because of the higher numbers of workers.

no difference in the mean tests for the other variables (which are all dichotomous). Treated and control firms present similar characteristics regarding the tax regime (*Simples*), industry, and employees' education level, among others.

Table 2 – Propensity score matching (on RAIS data): summary statistics.
This table provides summary statistics of the data from RAIS based on the two groups of firms (treated and control). The statistics are measured at the end of year -1. Year 0 denotes the year of bankruptcy reorganization filing. All variables are defined in **Table 5** (**Appendix 1**). The final sample (treated group) contains 2126 corporate creditors listed in no more than one bankruptcy reorganization filing in the state of São Paulo (single-case creditors) from 2012-2016 (374 cases). The control group comprises 2114 peered firms with no link to a bankruptcy event. The propensity score matching uses the variables Negative RAIS, Simples, Branch, Firm age, Industry (CNAE at 2-digit code level), employee education level, employees' gender, microregion (location), and legal form to peer the firms. RAIS is the Annual Social Information Report.

	Control				Treated				Mean difference (ttest)	
	(1)				(2)				(1)-(2)	
Variable	mean	sd	p50	count	mean	sd	p50	count	Diff	p-value
Total number of employees	179,61	909,19	16,00	2114	325,69	1347,56	31,00	2126	-146,08	0,00
Total remuneration	446729,40	2266919,00	28701,11	2114	834480,80	3889457,00	71499,29	2126	-387751,40	0,00
Average monthly wage	2444,08	3145,49	1849,33	2037	2592,99	1821,71	2107,32	2081	-148,92	0,06
Negative RAIS	0,02	0,14	0	2114	0,02	0,14	0	2126	0,00	0,98
Simples	0,19	0,39	0	2114	0,18	0,39	0	2126	0,01	0,47
Branch	0,36	0,48	0	2114	0,38	0,49	0	2126	-0,01	0,33
Firm age	19,47	13,45	16,39	2114	20,40	14,03	17,33	2126	-0,93	0,03
Industry										
Manufacturing	0,33	0,47	0	2114	0,33	0,47	0	2126	-0,01	0,67
Construction	0,04	0,20	0	2114	0,04	0,20	0	2126	0,00	0,97
Service	0,29	0,45	0	2114	0,28	0,45	0	2126	0,00	0,77
Commerce	0,32	0,47	0	2114	0,32	0,47	0	2126	0,00	0,90
Agriculture	0,01	0,12	0	2114	0,01	0,12	0	2126	0,00	0,88
Public administration	0,02	0,12	0	2114	0,02	0,13	0	2126	0,00	0,92
Employee education										
Incomplete primary education	0,01	0,05	0	2114	0,02	0,06	0	2126	0,00	0,01
Complete primary education	0,06	0,12	0	2114	0,07	0,12	0,018237	2126	-0,01	0,03
Lower secondary education	0,11	0,18	0,05	2114	0,12	0,18	0,06	2126	-0,01	0,35
Incomplete upper secondary education	0,07	0,11	0,03	2114	0,07	0,12	0,04	2126	-0,01	0,15
Complete upper secondary education	0,53	0,28	0,53	2114	0,49	0,27	0,50	2126	0,04	0,00
Post-secondary education	0,22	0,27	0,11	2114	0,23	0,27	0,13	2126	-0,02	0,06
Male	0,67	0,27	0,73	2114	0,67	0,27	0,73	2126	0,00	0,88

6. Results & Discussion

This section presents the results from our empirical specifications. We analyze and discuss the main diff-in-diff estimations and the findings of our empirical strategy.

Baseline specification regressions

Table 3 reports the estimations for four specifications of our baseline regression, progressively saturated with fixed effects (year, microregion, and firm). We applied a propensity score matching method to partly account for endogeneity and functional form misspecification. We reached a final sample of 2126 treated firms and 2114 control firms. All regression models are estimated using difference-in-differences regressions with robust standard errors clustered by firm. In Panel A, we account for the number of employees as the dependent variable. In Panel B, we consider the log of the number of employees. We estimated the effects of a bankruptcy reorganization event on corporate creditors for one year (Year + 1) and an average of one, two, or three years, upon data availability (Up to Year +3).

In the regressions shown in **Table 3**, Panel A, Column 1, which does not include fixed effects, establishes the basic pattern of our estimations. Panel A holds a level-level regression coefficient estimates interpretation. For both sample lengths (Year + 1 and Year + 3), our main coefficient of interest (*Treated*After*) captures that treated firms lose, on average, approximately 10 employees more than control firms after an event of bankruptcy reorganization filing by a debtor[29–30]. However, the coefficient is not statistically significant. We add year and microregion fixed effects in Columns 2 and 3, and the estimations remain the same. We note that the significant initial differences in the average number of employees between treated and control firms (see **Table 2**) provide arguments for a higher absolute loss of employment in large firms. We can also observe this initial difference in the coefficient estimations of β_1 (Treated) in Columns 1-3, which are positive and statistically significant. Column 4, which includes firm fixed effects, accounting for unobservable firms' characteristics, corroborates with the previous models showing a more significant absolute loss of workers for treated firms. Nevertheless, also statistically not significant. In contrast to the other models, in Column 4, the coefficient of After is negative, suggesting a decrease in the number of employees for both control and treated firms after bankruptcy events.

Table 3, Panel B provides estimations for the log of the number of employees (which applies for a log-level regression coefficient estimate interpretation). The estimations for both sample lengths (Year + 1 and Year + 3) are essentially similar. Columns 1 to 4 show a reduction in the log of employees for all groups after the bankruptcy reorganization event. The variable *After* is constantly negative in our specifications. These findings indicate a negative externality of the bankrupt firm not only to linked firms but also to control firms not directly connected to the bankruptcy. The statistical significance is slightly greater in the first year after the bankruptcy event (Year + 1), suggesting immediate effects. Finally, Column 4, our more robust model specification, which considers firm fixed effects, surprisingly reveals that treated firms perform relatively better than control firms after an event of bankruptcy

[29] Tread group: the average number of employees is 325. Control group: the average number of employees is 179.

[30] The bankruptcy reorganization costs to corporate creditors include not only the defaulted debt but also other direct costs, such as fees paid to claimants' lawyers and consultants. Moreover, creditors also bear indirect bankruptcy costs, like management's diversion from running the business (e.g., time spent on bankruptcy negotiation and travels to attend general meetings of creditors) (Bris et al., 2006; Wang, 2022).

reorganization. Analyzing the primary coefficient of interest (*Treated*After*) and the negative variable *After*, we argue that treated firms slightly lose fewer employees than control firms. Nonetheless, the estimation is statistically significant only at the 10% level and for the length Year + 1.

Table 3 – Diff-in-diff baseline regression models.
This table shows coefficient estimates from difference-in-differences regressions for examining bankruptcy spillover effects on corporate creditors. Year 0 denotes the year of bankruptcy reorganization filing. The dependent variable is the number of employees (Panel A) and the logarithm of the number of employees (Panel B). The dependent variables are calculated by the differences between the [log of] total number of employees before (in Year -1) and after (Year +1 or the average of Year +1, +2, or +3, upon data availability) the bankruptcy reorganization filing year. The final sample (treated group) contains 2126 corporate creditors listed in no more than one bankruptcy reorganization filing in the state of São Paulo (single-case creditors) from 2012-2016 (374 cases). The control group comprises 2114 peered firms with no link to a bankruptcy event. We progressively add fixed effects (year, microregion, and firm) in Specifications 2, 3, and 4. Standard errors (in brackets) are robust and clustered at the firm level. ∗∗∗, ∗∗, and ∗ denote significance at the 1%, 5%, and 10% level, respectively.

	(1)	(2)	(3)	(4)
Panel A: Number of employees				
Year + 1				
Treated	146.1***	146.1***	146.2***	
	(35.29)	(35.29)	(35.20)	
After	4.476	19.14	31.20	-2.821
	(4.873)	(30.41)	(32.69)	(4.648)
Treated*After	-9.697	-9.648	-8.706	-6.890
	(7.864)	(7.895)	(7.998)	(7.571)
Observations	8,347	8,347	8,347	8,214
R-squared	0.004	0.004	0.015	0.989
Up to Year + 3				
Treated	146.1***	146.1***	146.4***	
	(35.29)	(35.28)	(35.16)	
After	9.608	25.59	37.12	-5.796
	(10.68)	(38.47)	(40.31)	(6.836)
Treated*After	-9.506	-9.480	-9.366	-5.814
	(17.72)	(17.77)	(17.81)	(10.04)
Observations	13,543	13,543	13,543	13,429
R-squared	0.003	0.003	0.015	0.979
Panel B: Log Number of employees				
Year + 1				
Treated	0.598***	0.598***	0.598***	
	(0.0580)	(0.0580)	(0.0566)	
After	-0.151***	-0.133**	-0.0861	-0.214***
	(0.0190)	(0.0525)	(0.0534)	(0.0166)
Treated*After	0.00612	0.00596	0.00751	0.0415*
	(0.0256)	(0.0257)	(0.0254)	(0.0227)
Observations	8,347	8,347	8,347	8,214
R-squared	0.025	0.027	0.073	0.966

Up to Year + 3				
Treated	0.598***	0.598***	0.598***	
	(0.0580)	(0.0580)	(0.0566)	
After	-0.189***	-0.102	-0.0551	-0.278***
	(0.0242)	(0.0633)	(0.0635)	(0.0175)
Treated*After	-0.0224	-0.0224	-0.0224	0.0374
	(0.0337)	(0.0338)	(0.0335)	(0.0241)
Observations	13,543	13,543	13,543	13,429
R-squared	0.023	0.024	0.072	0.951
Year FE	no	yes	yes	no
Microregion FE	no	no	yes	no
Firm FE	no	no	no	yes

Figure 4 depicts the logarithm of the number of employees from the time to the bankruptcy event (before -3 and after +3). It provides additional visual evidence of the negative effects of bankruptcy reorganization filings for both treated and control firms. The lines depicting the evolution of the log of the number of employees regarding the time to the bankruptcy reorganization event may suggest parallel trends of treated and control groups.

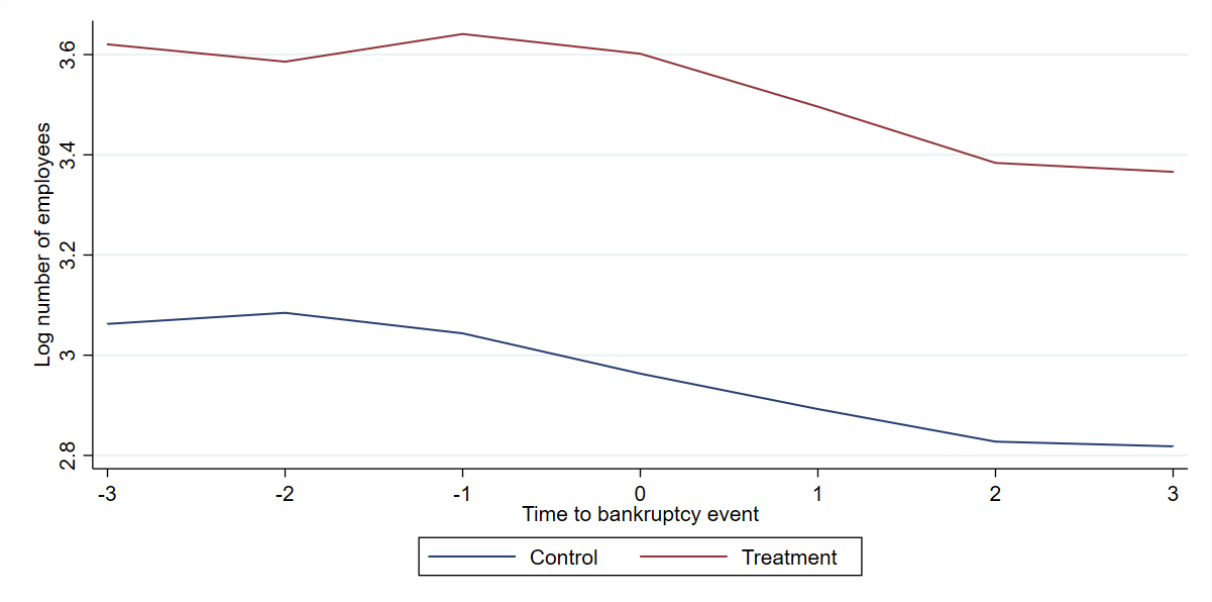

Figure 4 – Logarithm of corporate creditors' number of employees regarding time to the bankruptcy event.

Bankruptcy resolution specification regressions

To provide additional evidence on bankruptcy spillover effects, we examine if the type of bankruptcy proceeding outcome, reorganization or liquidation, imposes different impacts on corporate creditors' performance. We divided our sample into two sub-samples based on the bankruptcy reorganization outcome as of April 2020. This strategy is like adding a dummy variable that indicates the type of bankruptcy resolution.

Table 1 shows in the summary statistics that 93 out of 374 bankruptcy reorganization cases were converted into a liquidation proceeding (25% of our final sample). This division allows for testing the effects of the different bankruptcy resolutions on the (log) number of employees of treated *versus* control firms. **Table 4** reports the estimations in similar

specification models as of our baseline regressions displayed in **Table 3**. We present the estimations only for the extended sample length considering the average of one, two, or three years, upon data availability (up to Year +3). The results only one year after the bankruptcy event (Year + 1) are equivalent.

Table 4, Panel A, provides three interesting pieces of information on the specifications considering the number of employees (level-level). First, the spillover effects on bankruptcy (based on the variable After) appear to be solely from bankruptcy reorganization cases converted to liquidation. Shedding light on our more robust specification (Column 4), which includes firm fixed effects, we note an economically large and statistically significant at a 5% level of the variable After. It suggests that a bankruptcy liquidation event reduces, on average, 20 employees, considering both treated and control firms (it represents 6% of the mean of treated only). Second, the significant initial differences in the average number of employees between treated and control firms are more pronounced in the cases that still as a bankruptcy reorganization. The coefficient estimations of β_1 (Treated) for Columns 5-7 or reorganization as the resolution is statistically significant. Third, the main variable of interest (*Treated*After*) is not statistically significant in any specification split by bankruptcy resolution. The estimations reinforce the previous results in **Table 3**, despite the different directions of the economic effects between the bankruptcy resolutions.

Table 4, Panel B, examines the specifications of the logarithm of the number of employees (log-level). In this context, both firms ending in liquidation or reorganizing show statistical differences in the size of the firms. Moreover, analyzing Columns 4 and 8, the estimations corroborate with a decline in employment for treated and control firms after a bankruptcy event, which can be associated with an impact on the local economy (Bernstein et al., 2019). The absence of statistical significance for treated firms after the event may suggest low counterparty effects of bankruptcy reorganization (Helwege & Zhang, 2016).

Table 4 – Diff-in-diff Regression models by bankruptcy resolution (liquidation or reorganization).
This table shows coefficient estimates from difference-in-differences regressions for examining bankruptcy spillover effects on corporate creditors by bankruptcy resolution (liquidation or reorganization). Year 0 denotes the year of bankruptcy reorganization filing. The dependent variable is the number of employees (Panel A) and the logarithm of the number of employees (Panel B). The dependent variables are calculated by the differences between the [log of] total number of employees before (in Year -1) and after (average of Year +1, +2, or +3, upon data availability) the bankruptcy reorganization filing year. The treated group contains 2126 corporate creditors listed in no more than one bankruptcy reorganization filing in the state of São Paulo from 2012-2016. The control group comprises 2114 peered firms with no link to a bankruptcy event. We add year, microregion, and firm fixed effects in Specifications 2, 3, and 4. Standard errors (in brackets) are robust and clustered at the firm level. ***, **, and * denote significance at the 1%, 5%, and 10% level, respectively.

	Bankruptcy resolution: liquidation				Bankruptcy resolution: reorganization			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: Number of employees								
Up to Year + 3								
Treated	134.8	135.0	137.9		149.3***	149.3***	148.9***	
	(95.09)	(95.05)	(95.17)		(36.40)	(36.39)	(36.18)	
After	-12.13	-167.1**	-185.0**	-21.68**	14.84	73.66	95.89*	-0.997
	(11.60)	(79.25)	(92.16)	(9.672)	(13.67)	(48.21)	(51.62)	(8.403)
Treated*After	7.396	6.683	3.637	18.27	-14.60	-14.58	-14.08	-13.07
	(37.52)	(37.71)	(38.03)	(23.80)	(19.39)	(19.47)	(19.40)	(10.94)
Observations	3,260	3,260	3,260	3,236	10,283	10,283	10,283	10,193
R-squared	0.002	0.004	0.018	0.984	0.004	0.005	0.021	0.976
Panel B: Log Number of employees								
Up to Year + 3								
Treated	0.685***	0.686***	0.690***		0.573***	0.573***	0.572***	
	(0.123)	(0.122)	(0.119)		(0.0658)	(0.0657)	(0.0639)	
After	-0.215***	-0.500***	-0.436***	-0.267***	-0.178***	0.0229	0.0955	-0.281***
	(0.0472)	(0.134)	(0.133)	(0.0366)	(0.0282)	(0.0746)	(0.0745)	(0.0200)
Treated*After	0.0249	0.0240	0.0272	0.0665	-0.0425	-0.0427	-0.0401	0.0287
	(0.0638)	(0.0642)	(0.0646)	(0.0495)	(0.0395)	(0.0397)	(0.0391)	(0.0276)
Observations	3,260	3,260	3,260	3,236	10,283	10,283	10,283	10,193
R-squared	0.034	0.039	0.111	0.948	0.020	0.024	0.083	0.951
Year FE	no	yes	yes	no	no	yes	yes	no
Microregion FE	no	no	yes	no	no	no	yes	no
Firm FE	no	no	no	yes	no	no	no	yes

Alternative Specifications & Robustness Check

To check for the robustness of our results, we performed several alternative estimations. First, we conducted identical specifications for the (log) number of employees for Year +1 e up to Year + 3 using the total yearly remuneration of firms' employees instead of the number of employees to proxy for firms' performance. All results are substantially the same as the ones presented in **Table 3**. In general, the main estimations including firm fixed effects report lower or no statistical significance.

Second, we sort our final sample into two groups by firm age equal to or less than two years and more than two years to investigate a potential age effect and survival bias directing our results. Since the number of firm-observations equal to or less than two years is small, the results are mainly guided by firms with more than two years and are like the ones reported on our baseline specifications.

Third, we divided our sample by firms adopting the tax regime for small businesses (*Simples Nacional*) to proxy for firm size or not. In brief, we proxy for small-firms if the company opts for Simples[31] and medium-sized and large if the firm adopts another tax regime. We aim to allow for investigating an effect size on our results. The results from our baseline specifications are primarily directed by the results of firms not adopting the *Simples Nacional* (medium-sized and large) and corroborate the estimations reported in **Table 3**. Since treated firms are larger regarding the number of employees and, thus, more likely to not opt for the *Simples Nacional,* it provides evidence of consistency in our findings.

Lastly, we sort our sample into two groups by the date of the bankruptcy event. The first sub-sample comprises corporate creditors linked to bankruptcies events in 2012-2013. The second group consists of the creditors of bankruptcies that occurred in 2015-2016. We aim to disentangle our results from the Brazilian political and economic crisis. Since both periods (pre-crisis and during the crisis) show similar results, especially concerning the variable *After*, we deem that our results are not guided by macroeconomic context. Interestingly, the relatively better performance of treated firms (at 10% level), regarding losing fewer employees, seems to be directed only by the period of crisis. Since our treated group is, on average larger, it corroborates with the arguments that smaller firms (in our sample, control group) are more affected by macroeconomic shocks.

We provide the tables containing the coefficient estimates for the alternative specifications discussed in **Appendix 2**.

7. Conclusion

This research investigated bankruptcy spillover effects on corporate creditors in the Brazilian context using a novel dataset on hand-collected bankruptcy lawsuit data and employer-employee administrative data. Our empirical strategy employs difference-in-differences estimations comparing the labor performance of the bankrupt firms' creditors (treated group) to similar firms that are not connected to a bankruptcy reorganization proceeding (control group). We peered treated firms to control firms through a propensity score matching that incorporates observable firm characteristics. Our analysis focuses only on single-case creditors, mitigating the risk of confounding effects from different bankruptcy events.

[31] Conceição et al. (2018) report that more than 70% of micro and small enterprises opt for the *Simples Nacional* since it reduces and simplifies the tax burden

Analyzing our main findings, the spillover effects of bankruptcy reached both the treated group (corporate creditors) and control group (similar firms with no direct link to a bankruptcy reorganization event). There is little evidence that the impact is different between the two groups. The results indicate with minor statistical significance (at 10% level) that treated firms performed relatively slightly better than control firms, losing fewer employees in the sample period. Even after applying our matching procedure, the average size of treated firms, which are larger, may partially guide the attenuated negative externalities of bankruptcy compared to the smaller control firms.

Since we match the creditors to similar firms considering the microregion and industry, the findings suggest that the dynamics of bankruptcy may propagate the corporate crises to the local economy or related industries. The trade credit bankruptcy propagation mechanism can be driven by both credit losses and demand shrinkage (Jacobson & von Schedvin, 2015) and may affect other firms through production link networks (Fujiwara, 2008; Acemoglu, Akcigit, & Kerr, 2016). Moreover, the adverse spillover effects on both groups appear to be mainly from bankruptcy reorganization cases converted to liquidation. This finding indicates that a more profound corporate crisis leading to a liquidation may spill substantially more over other firms (linked or not to the bankrupt firm), corroborating with findings of previous studies (Bernstein et al., 2019; Moraes, 2019).

Our research contributes to extending the current literature on corporate bankruptcy to better understand the boundaries of a corporate crisis. We provide evidence of spillover effects on corporate creditors and in an emerging market context, partly filling a gap in the empirical research on the topic. Our focus on the corporate creditor instead of the bankrupt firm provides empirical evidence on an economic agent that is less examined by the empirical literature. The findings may contribute to the discussion in the academic field and formulation of local legal reforms.

One caveat of our research concerns the lack of data on other bankruptcy reorganization and liquidation proceedings in the State of São Paulo and, especially, in other Brazilian States. Thus, our assumption to separate treated firms (linked to a bankruptcy event) and control firms (similar firms not listed in a bankruptcy procedure) may be weakened. Furthermore, because of limited data availability, we could not perform placebo tests to provide an additional check on the robustness of our results. The likely confounding effect of the Brazilian economic and political crisis (2015-2016) is also a caveat of our research. However, we deemed that the potential confounding effect was mitigated by using year and firm fixed effects in our regressions and performing a specification of the pre-Brazilian crisis (2012-2013) and post-Brazilian crisis (2015-2016).

Finally, there are several avenues for future research on bankruptcy spillover effects. First, extending our analysis through new studies addressing causal inference of formal bankruptcy externalities on claimants is still critical. Second, examining bankruptcy in emerging markets is an important opportunity for research. Expanding our sample to additional periods and mainly to more State Courts can supplement our research for the Brazilian context. Moreover, adding data from bankruptcy liquidation filings can also increase the examination of effects. Third, we focused on investigating spillover effects only on corporate creditors. Addressing the effects on other linked parties, such as banks, bondholders, and employees, is crucial to better understanding the full effects. Last, there are vast opportunities to employ new

approaches to gather and analyze data, such as machine learning for textual analysis of the legal process documentation.

Appendix 1

Table 5 – Variables Definition
This table provides the definitions of the variables used in this research for investigating bankruptcy spillovers on corporate creditors.

Variable	Definition	Source
Creditor Firm characteristics		
Total number of employees	Total number of employees registered at the end of the relevant year.	RAIS
Firm age	Firm age in years.	RAIS
Industry code	National Classification of Economic Activities (CNAE) at the 2-digit code level.	RAIS
Microregion	Dummy indicating the location (microregion) of the firm.	RAIS
Simples	Dummy indicating that the firm adopts the simplified tax regime for small businesses (*Simples Nacional*).	RAIS
Branch	Dummy indicating that the firm owns branch(es).	RAIS
Negative RAIS	Dummy indicating that the firm did not employ any worker in the relevant year.	
Employees characteristics		
Employees total remuneration	Total remuneration of firm's employees in the relevant year (in BRL).	RAIS
Employee average remuneration	Average of firm employees' monthly wage in the relevant year (in BRL).	RAIS
Employee gender	Dummy indicating the employee gender (male= 1; female= 0).	RAIS
Employee educational level	Dummy indication the employee educational level (6-level scale[32]).	RAIS
Bankruptcy Case data		
Court	Dummy indicating the judicial district of the case.	TJSP
Total claims	Total value of listed claims (in BRL).	TJSP
Number of corporate creditors	Number of corporate creditors listed (#).	TJSP
Liquidation	Dummy indicating if the bankruptcy reorganization has been converted to liquidation (as of April 2020).	TJSP

[32] Educational levels: incomplete primary education; complete primary education; lower secondary education; incomplete upper secondary education; complete upper secondary education; post-secondary education.

Appendix 2

Table 6 – Diff-in-diff regression models: dependent variable total remuneration and log of total remuneration.
This table shows coefficient estimates from difference-in-differences regressions for examining bankruptcy spillover effects on corporate creditors. Year 0 denotes the year of bankruptcy reorganization filing. The dependent variable is the total yearly remuneration of employees (Panel A) and the logarithm of the total yearly remuneration of employees (Panel B). Standard errors (in brackets) are robust and clustered at the firm level. ∗∗∗, ∗∗, and ∗ denote significance at the 1%, 5%, and 10% level, respectively.

	(1)	(2)	(3)	(4)
Panel A: Total Remuneration				
Year + 1				
Treated	387,751***	387,768***	387,542***	
	(97,713)	(97,677)	(97,358)	
After	95,896***	-24,402	-6,524	40,379**
	(21,172)	(80,862)	(85,228)	(17,843)
Treated*After	-21,565	-20,866	-15,585	-15,420
	(34,368)	(34,891)	(34,551)	(32,783)
Observations	8,097	8,097	8,097	7,714
R-squared	0.004	0.005	0.017	0.976
Up to Year + 3				
Treated	387,751***	387,770***	387,953***	
	(97,706)	(97,640)	(97,233)	
After	144,795***	2,866	16,876	66,725**
	(45,765)	(114,519)	(118,088)	(27,801)
Treated*After	-50,667	-50,449	-45,158	-12,180
	(66,197)	(66,384)	(67,027)	(39,821)
Observations	12,849	12,849	12,849	12,525
R-squared	0.003	0.004	0.016	0.966
Panel B: Log Total Remuneration				
Year + 1				
Treated	0.908***	0.908***	0.909***	
	(0.0888)	(0.0887)	(0.0867)	
After	0.180***	-0.0149	0.0458	-0.308***

	(0.0646)	(0.0971)	(0.0975)	(0.0487)
Treated*After	-0.151*	-0.152*	-0.142*	0.0478
	(0.0842)	(0.0844)	(0.0837)	(0.0644)
Observations	8,097	8,097	8,097	7,714
R-squared	0.020	0.023	0.071	0.868
Up to Year + 3				
Treated	0.908***	0.908***	0.909***	
	(0.0888)	(0.0887)	(0.0867)	
After	0.210***	0.0505	0.103	-0.387***
	(0.0598)	(0.0996)	(0.0992)	(0.0475)
Treated*After	-0.167**	-0.168**	-0.164**	0.0703
	(0.0775)	(0.0776)	(0.0770)	(0.0614)
Observations	12,849	12,849	12,849	12,525
R-squared	0.018	0.020	0.067	0.814
Year FE	no	yes	yes	no
Microregion FE	no	no	yes	no
Firm FE	no	no	no	yes

Table 7 – Diff-in-diff regression models by firm age.
This table shows coefficient estimates from difference-in-differences regressions for examining bankruptcy spillover effects on corporate creditors by firm age. We sort our sample into two groups: (1) firm age equal to or less than two years and (2) firm age more than two years. Year 0 denotes the year of bankruptcy reorganization filing. The dependent variable is the number of employees (Panel A) and the logarithm of the number of employees (Panel B). The dependent variables are calculated by the differences between the [log of] total number of employees before (in Year -1) and after (average of Year +1, +2, or +3, upon data availability) the bankruptcy reorganization filing year. Standard errors (in brackets) are robust and clustered at the firm level. ∗∗∗, ∗∗, and ∗ denote significance at the 1%, 5%, and 10% level, respectively. The estimations for Year + 1 are similar to those reported up to Year + 3.

	Firms age: equal to or less than 24 months				Firms age: more than 24 months			
	(1)	(2)	(3)	(4)	(1)	(2)	(3)	(4)
Panel A: Number of employees								
Up to Year + 3								
Treated	177.4	177.1	193.7		145.2***	145.2***	145.1***	
	(140.1)	(140.0)	(150.9)		(36.11)	(36.10)	(35.97)	
After	14.13	-60.37	-86.29	49.13	9.536	30.00	43.69	-7.436
	(19.82)	(99.02)	(138.8)	(51.53)	(10.99)	(39.59)	(41.42)	(6.870)
Treated*After	-26.06	-23.49	-22.67	-81.64	-8.968	-8.903	-8.600	-3.520
	(105.1)	(104.9)	(107.7)	(83.01)	(17.98)	(18.04)	(18.07)	(10.03)
Observations	405	405	405	401	13,138	13,138	13,138	13,028
R-squared	0.008	0.011	0.044	0.895	0.003	0.003	0.015	0.981
Panel B: Log Number of employees								
Up to Year + 3								
Treated	0.293	0.287	0.276		0.607***	0.607***	0.607***	
	(0.298)	(0.294)	(0.287)		(0.0586)	(0.0586)	(0.0574)	
After	-0.255*	-0.654*	-0.764*	-0.226**	-0.187***	-0.0613	-0.0148	-0.279***
	(0.131)	(0.346)	(0.400)	(0.113)	(0.0246)	(0.0640)	(0.0643)	(0.0177)
Treated*After	0.0499	0.0604	0.0863	-0.0293	-0.0240	-0.0238	-0.0218	0.0394
	(0.228)	(0.230)	(0.233)	(0.197)	(0.0339)	(0.0340)	(0.0338)	(0.0241)
Observations	405	405	405	401	13,138	13,138	13,138	13,028
R-squared	0.013	0.028	0.178	0.868	0.024	0.026	0.070	0.952
Year FE	no	yes	yes	no	no	yes	yes	no
Microregion FE	no	no	yes	no	no	no	yes	no
Firm FE	no	no	no	yes	no	no	no	yes

Table 8 – Diff-in-diff regression models by the tax regime adopted (*Simples Nacional* or another).
This table shows coefficient estimates from difference-in-differences regressions for examining bankruptcy spillover effects on corporate creditors by the tax regime adopted (*Simples Nacional* or another). We sort our sample into two groups: (1) firms adopting the tax regime for small businesses (*Simples Nacional*), and (2) firms adopting another tax regime. This sort proxies for firm size. Year 0 denotes the year of bankruptcy reorganization filing. The dependent variable is the number of employees (Panel A) and the logarithm of the number of employees (Panel B). The dependent variables are calculated by the differences between the [log of] total number of employees before (in Year -1) and after (average of Year +1, +2, or +3, upon data availability) the bankruptcy reorganization filing year. Standard errors (in brackets) are robust and clustered at the firm level. ∗∗∗, ∗∗, and ∗ denote significance at the 1%, 5%, and 10% level, respectively. The estimations for Year + 1 are similar to those reported up to Year + 3.

	Simples 0				Simples 1			
	(1)	(2)	(3)	(4)	(1)	(2)	(3)	(4)
Panel A: Number of employees								
Up to Year + 3								
Treated	178.7***	178.7***	178.6***		-1.235	-1.229	-1.274	
	(42.91)	(42.90)	(42.80)		(4.890)	(4.878)	(4.730)	
After	11.01	26.58	38.85	-6.951	1.078	12.11	14.68	-0.524
	(12.98)	(46.33)	(48.06)	(8.332)	(2.476)	(8.731)	(9.223)	(0.870)
Treated*After	-10.55	-10.49	-10.65	-6.878	-1.776	-1.763	-1.501	-1.012
	(21.58)	(21.66)	(21.69)	(12.25)	(2.940)	(2.962)	(2.932)	(1.278)
Observations	11,081	11,081	11,081	10,999	2,462	2,462	2,462	2,430
R-squared	0.004	0.004	0.015	0.979	0.000	0.005	0.041	0.985
Panel B: Log Number of employees								
Up to Year + 3								
Treated	0.676***	0.676***	0.673***		0.242***	0.242***	0.244***	
	(0.0634)	(0.0634)	(0.0626)		(0.0778)	(0.0776)	(0.0743)	
After	-0.201***	-0.143**	-0.104	-0.286***	-0.154***	0.0444	0.0364	-0.240***
	(0.0273)	(0.0698)	(0.0695)	(0.0200)	(0.0410)	(0.0947)	(0.100)	(0.0339)
Treated*After	-0.00600	-0.00599	-0.00426	0.0524*	-0.0748	-0.0744	-0.0687	-0.0308
	(0.0373)	(0.0375)	(0.0373)	(0.0272)	(0.0597)	(0.0600)	(0.0603)	(0.0516)
Observations	11,081	11,081	11,081	10,999	2,462	2,462	2,462	2,430
R-squared	0.030	0.032	0.060	0.948	0.013	0.022	0.123	0.882
Year FE	no	yes	yes	no	no	yes	yes	no
Microregion FE	no	no	yes	no	no	no	yes	no
Firm FE	no	no	no	yes	no	no	no	yes

Table 9 - Diff-in-diff regression models by bankruptcy filing period.
This table shows coefficient estimates from difference-in-differences regressions for examining bankruptcy spillover effects on corporate creditors by the bankruptcy filing period. We sort our sample into two groups: (1) creditors linked to bankruptcies events in 2012-2013; and (2) creditors linked to bankruptcies events in 2015-2016. This sort proxies for periods of pre-crisis and during the crisis. Year 0 denotes the year of bankruptcy reorganization filing. The dependent variable is the number of employees (Panel A) and the logarithm of the number of employees (Panel B). The dependent variables are calculated by the differences between the [log of] total number of employees before (in Year -1) and after (average of Year +1, +2, or +3, upon data availability) the bankruptcy reorganization filing year. Standard errors (in brackets) are robust and clustered at the firm level. ∗∗∗, ∗∗, and ∗ denote significance at the 1%, 5%, and 10% level, respectively. The estimations for Year + 1 are similar to those reported up to Year + 3.

	2012 - 2013				2015 - 2016			
	(1)	(2)	(3)	(4)	(1)	(2)	(3)	(4)
Panel A: Number of employees								
Up to 3 periods								
Treated	112.7*	112.9*	111.7*		152.1***	152.1***	150.2***	
	(66.53)	(66.52)	(65.28)		(45.30)	(45.30)	(44.96)	
After	16.58			6.438	-5.505			-17.33***
	(18.45)			(17.55)	(8.309)			(6.278)
Treated*After	-11.71	-11.52	-9.777	-8.752	-14.26	-14.25	-14.18	-10.90
	(21.65)	(21.68)	(21.84)	(20.70)	(15.14)	(15.14)	(15.23)	(12.05)
Observations	4,773	4,773	4,773	4,749	5,292	5,292	5,292	5,228
R-squared	0.002	0.003	0.037	0.970	0.005	0.005	0.032	0.981
Panel B: Log Number of employees								
Up to 3 periods								
Treated	0.580***	0.581***	0.578***		0.617***	0.617***	0.614***	
	(0.105)	(0.105)	(0.100)		(0.0830)	(0.0829)	(0.0803)	
After	-0.136***			-0.227***	-0.273***			-0.326***
	(0.0356)			(0.0317)	(0.0337)			(0.0253)
Treated*After	-0.0409	-0.0398	-0.0355	0.0134	0.0145	0.0143	0.0222	0.0679*
	(0.0477)	(0.0480)	(0.0479)	(0.0436)	(0.0466)	(0.0467)	(0.0457)	(0.0352)
Observations	4,773	4,773	4,773	4,749	5,292	5,292	5,292	5,228
R-squared	0.020	0.026	0.110	0.946	0.028	0.029	0.098	0.952
Year FE	no	yes	yes	no	no	yes	yes	no

Microregion FE	no	no	yes	no	no	no	yes	no
Firm FE	no	no	no	yes	no	no	no	yes

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