Busy Bankruptcy Courts and the Cost of Credit

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This paper estimates the effect of bankruptcy court caseload on access to credit by exploiting firms' plausibly exogenous exposure to the largest recorded drop in court backlog in the United States following the 2005 consumer bankruptcy reform. I show that a drop in court congestion reduces the time firms spend in bankruptcy and increases recovery values, which is priced into credit spreads and loan maturities. Consistent with a shock to credit supply, less congested courts increase firm leverage but leave default risk unchanged. A back-of-the-envelope calculation suggests that backlog in bankruptcy courts costs corporate borrowers at least $740 million per year in interest payments.

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1 Introduction

"Our judicial resources are strained. And the cost to society of an overburdened bankruptcy system ... is enormous."

– U.S. District Judge Barbara Lynn[1]

Legal institutions play an important part in shaping the cost of financing for firms and households.[2] Higher creditor and property rights, for example, are associated with the development of larger and more sophisticated financial sectors.[3] Functioning bankruptcy courts in particular are often seen as playing a key role in the economy. However, there is little credible microeconomic evidence on how courts shape ex-ante outcomes outside of bankruptcy, because the functioning of courts is endogenous to the state of the economy.

In this paper, I study how bankruptcy court backlog affects ex-ante contracting by exploiting the largest recorded drop in the caseload of U.S. bankruptcy judges in the wake of the Bankruptcy Abuse Prevention and Consumer Protection Act of 2005 (BAPCPA). The BAPCPA reform fundamentally changed the bankruptcy code for individual debtors, making it considerably harder and more expensive for them to default. In the two years following its implementation, bankruptcy filings essentially halved—-the largest decrease outside of war times since the beginning of records in 1899 (Federal Judicial Center, 2019). This drop in filings permanently reduced the number of cases per bankruptcy court, and thus the workload of judges.

Despite its effect on individual debtors, BAPCPA left *corporate* bankruptcies essentially unaffected. As a result, the drop in judicial workloads after the reform was larger in bankruptcy districts with a higher share of non-business filings before. This generates plausibly exogenous variation in court backlog for firms located in the bankruptcy districts that were particularly exposed to BAPCPA. Building on Iverson (2018), I exploit this variation in a difference-in-differences framework to study the impact of court backlog on publicly listed firms.

To start, I show that a one standard deviation increase in the non-business share in a court's caseload is associated with a 64 hour drop in the annual workload of judges (around two work

[1] As reported in the National Law Journal online, see https://www.law.com/almID/1202431566285/?slreturn=20190418090514.

[2] See, for example, the seminal work of La Porta, de Silanes, Shleifer & Vishny (1997), La Porta, de Silanes, Shleifer & Vishny (1998), Levine (1999), and Levine, Loayza & Beck (2000).

[3] See, among others, Djankov, McLiesh & Shleifer (2007), Qian & Strahan (2007), Bae & Goyal (2009), Davydenko & Franks (2008), Haselmann, Pistor & Vig (2010), and Acharya, Sundaram & John (2011).

weeks). This drop in court backlog causes a 5.5% reduction in the length of bankruptcy cases and a 6-12% increase in recovery values for creditors. Using contract-level data on syndicated loans from DealScan, I then estimate that this improvement in the functioning of bankruptcy courts leads to a 20 basis points drop in interest rate spreads (a 10% drop) and 9% increase in loan maturities. These results suggest that lower court congestion is priced into firms' ex-ante financing terms by increasing expected recovery values.

Consistent with the predictions of a simple theoretical framework in the spirit of Hart & Moore (1998), the drop in caseload also reduces the gap in loan terms between risky and safe borrowers. Exploiting heterogeneity in pre-reform borrower characteristics in a triple-difference approach, I find that judicial backlog matters almost exclusively for firms with a higher probability of default and a higher expected loss given default. This also allows me to absorb bankruptcy district $\times$ time fixed effects, which rules out many potentially confounding factors.

A few additional tests support a causal interpretation of these results. First, loan terms trended similarly for more and less exposed bankruptcy districts before the drop in court caseload, when uncertainty about the impact of BAPCPA lifted. This suggests that creditors interpret observable court backlog as a noisy signal about future recovery values. Initially, interest rate spreads only drop for short-term loans, for which the reduction in backlog in early 2006 is the most informative signal about expected recovery values. By late 2007, however, the effect on shorter and longer loans converges, suggesting that creditors learn over time whether changes in judicial caseload are permanent or transitory.

Second, the share of non-business filings—which I use to measure exposure to BAPCPA—is uncorrelated with firm and loan characteristics before the reform. This also makes it less likely that I am capturing unobserved differences between districts.

Third, some borrowers were additionally exposed to changes in credit supply because they had to roll over loans that were issued in the years before BAPCPA passed (also see Almeida, Campello, Laranjeira & Weisbenner, 2012). The estimates from using this pre-determined variation suggest that my findings are entirely driven by credit supply, not unobserved shocks to firms.

Next, I estimate the effect of court backlog on firm leverage and default risk, measured by credit ratings and ex-post bankruptcies. Consistent with an expansion of credit supply, book leverage increased by around 2% relative to the mean for a one standard deviation larger drop in court caseload. However, I find a precisely estimated zero effect on credit ratings, the probability of borrowers being rated, as well as the number of firm bankruptcies. This is likely because—although leverage increased—lower debt service payments decrease default risk. I also find a zero effect on the frequency of bankruptcy filings during the financial crisis. Taken together, this suggests that the drop in court backlog did not change firms' ex-ante or realized probability of default. Rather, fewer court cases decrease the expected loss given default, which is immediately priced in by creditors.

I conduct a simple back-of-the-envelope calculation to get a sense of the costs judicial backlog imposes on borrowing firms. While it requires a set of strong assumptions, only some of which I can test empirically, changes to interest rate spreads translate naturally into a macroeconomic quantity: the approximately $400 billion per year U.S. non-financial corporations pay to service their debt. My estimates suggest that the costs to firms arising from overburdened bankruptcy judges are around $740 million per year. While this estimate does not necessarily tell us about the welfare effects of busy bankruptcy courts, these magnitudes are large compared to the costs of creating additional judgeships that would lower court congestion. This extrapolation exercise suggests that debt enforcement plays a key role in the ex-ante design of financial contracts. From a policy perspective, it provides some evidence that bankruptcy judges may be relatively "cheap" compared to their value added, at least in terms of the savings to borrowers' debt service payments.

I also test and reject potential alternative explanations of my findings. A host of robustness checks suggests that the housing boom in the run-up to the 2007-08 financial crisis is unlikely to play a role. Exposure to BAPCPA is not correlated with key housing variables, and excluding securitized (CLO) loans or the construction and nontradable industries—or adding lender $\times$ year fixed effects—makes little difference to my point estimates. Event study plots further suggest a discontinuous and permanent effect of court caseload, rather than a boom-bust pattern.

My results are also not driven by minor changes the reform made to corporate bankruptcies. I find almost equivalent estimates for firms who were clearly not affected by changes in the applicable bankruptcy framework. "Forum shopping", i.e. the leeway the largest U.S. borrowers have over where to file for bankruptcy, is also unlikely to play a role. Theory suggests that this would lead me to understate the effect of court congestion: if anything, less busy courts should increase bankruptcy filings and make judges more debtor-friendly (Gennaioli & Rossi, 2010), and thus—all else equal– lead to worse contract terms for borrowers. In contrast, I find that less congested courts improve contract terms and do not change the number of firm bankruptcies. My baseline results also remain unchanged when I exclude the firms most likely to engage in forum shopping.

My paper builds on a large literature on legal frameworks and economic outcomes. First, my work builds on research that focuses on the enforcement of existing law. Djankov, Hart, McLiesh &

Shleifer (2008) use a representative bankruptcy case for 88 countries and show that better debt enforcement is associated with higher credit market development. Jappelli, Pagano & Bianco (2005) show a negative correlation of judicial backlog and interest rate spreads for Italian provinces.[4] However, these correlations do not constitute causal effects, because court backlog is likely correlated with other factors that also matter for credit provision. For example, areas that lack public funding to hire judges likely have both more congested courts, firms at a higher risk of defaulting, and a higher cost of credit even in the absence of any causal link. My contribution is to provide, to my knowledge, a first causal estimate of how court backlog affects ex-ante contract outcomes.

I build on the insight of Iverson (2018) that BAPCPA constituted a shock to the U.S. bankruptcy courts with the highest pre-reform share of non-business bankruptcies. Iverson shows that the drop in court congestion around the reform reduced repeated bankruptcy filings, decreased the time larger firms spent in court and lowered banks' charge-offs for business lending. My contribution is to study whether and how such expected ex-post effects alter the ex-ante contracting environment. I show how banks adjust contracts in a forward-looking manner—usually more associated with equity markets—when news about recovery values arrive. In addition, I provide a back-of-the-envelope estimate of the aggregate costs court backlog imposes on firm borrowers. In other related work, Boehm & Oberfield (2018) study the effect of court congestion on the input choices of Indian manufacturing firms. Brown, Cookson & Heimer (2016) study the effect of a 1953 law that imposed external state courts on Native American reservations.[5]

Second, I add to the literature studying how the effect of major legal reforms depends on pre-existing levels of judicial backlog. Ponticelli & Alencar (2016) and Rodano, Serrano-Velarde & Tarantino (2016) show for Brazil and Italy, respectively, that financial reforms interact with legal institutions governing courts. Their work, however, estimates the value of major changes in bankruptcy regimes, not whether judicial backlog matters *per se*. In other words, these studies do not tell us whether courts have an effect without a contemporaneous legal reform that fundamentally alters bankruptcy law. My contribution is to estimate the effect of the caseload burden of judges on firms' financing terms while holding the applicable bankruptcy law constant. My results suggest that court congestion also matters for the United States, which has one of the most

[4]Figure A1 in the online appendix shows a similar correlation between loan terms and the workload of judges across U.S. bankruptcy districts.

[5]Also related are Schiantarelli, Stacchini & Strahan (Schiantarelli et al.), who show that poor enforcement increases the incentive of borrowers to default against banks with high credit losses. Favara, Morellec, Schroth & Valta (2017) show that riskier firms invest and grow less in countries with better debt enforcement.

sophisticated bankruptcy law and court systems in the world.

Third, this paper adds to the literature on legal determinants of financial contracts more broadly. Qian & Strahan (2007), Bae & Goyal (2009), and Laeven & Majnoni (2005) show that better property rights, creditor rights, and legal institutions are correlated with lower loan spreads across countries. Vig (2013) studies a legal reform in India which increased creditor rights and decreased the delay between default and liquidation. I also add to a large literature on the heterogeneous effects of bankruptcy frameworks.[6]

The remainder of the paper is structured as follows. In section 2, I discuss the institutional background governing bankruptcy in the United States generally and BAPCPA in particular. Section 3 introduces the data and variable construction. Section 4 introduces the empirical strategy based on a simple conceptual framework. Section 5 discusses the main results. Section 6 concludes.

2 Background: Bankruptcy Courts in the United States

2.1 Courts and Judges

Bankruptcy courts are units of the district courts that operate across 90 judicial districts in the mainland United States.[7] 27 states only have a single bankruptcy district. The decision-making power over bankruptcy cases lies with the bankruptcy judge. As of September 2012, there were 350 bankruptcy judgeships in the United States, out of which 34 were temporary. Judges have full authority over their cases, e.g. whether debtors are eligible to file for bankruptcy in a district or should receive debt relief.

In the case of Chapter 11 filings, bankruptcy judges are responsible for confirming or disapproving plans of reorganization and thus whether firms should be reorganized or liquidated. In particular, motions to dismiss cases or convert them to a Chapter 7 liquidation are a crucial decision for the fate of debtor firms. Bankruptcy judges in the United States are appointed for 14 years by the respective court of appeals in their district, as governed by 28 U.S.C. §152. s

[6] See, among others, Gropp, Scholz & White (1997), Bris, Welch & Zhu (2006), Acharya & Subramanian (2009), Haselmann et al. (2010), von Lilienfeld-Toal, Mookherjee & Visaria (2012), Favara, Schroth & Valta (Favara et al.), Hackbarth, Haselmann & Schoenherr (2015), Cerqueiro & Penas (2016), Cerqueiro, Hegde, Penas & Seamans (2017), and Schoenherr (2018).

[7] In Arkansas, the Western and Eastern districts share bankruptcy judges. For the purpose of this study, I treat them as a single district.

2.2 Measuring Court Backlog

The total number of bankruptcy filings between 1980 and 2010 increased by 381%, while the number of bankruptcy judgeships in the United States increased by 53%. As a result, the number of cases the average bankruptcy judge had to handle in 2010 is 3.1 times the number in 1980. One of the reasons for this trend is that the creation of additional judgeships requires the passage of a bill by the House of Representatives and the Senate. In October 2017, President Trump signed the Bankruptcy Judgeship Act of 2017, which effectively added four permanent judgeships and extended a number of temporary ones. Before that, the last change to the number of *permanent* bankruptcy judgeships was implemented by the Bankruptcy Judgeship Act of 1992. In 2005, as part of BAPCPA, 28 additional temporary judgeships were created, one of which was discontinued in 2010.

However, these numbers do not take into account that bankruptcy cases differ markedly in the time they take to process. Consumer bankruptcies, especially Chapter 7 cases, are usually settled without the debtor appearing in court. This leads to a considerably lower time effort on part of the bankruptcy judge. To accommodate these differences, the Judicial Conference of the United States makes use of a weighting system to determine the caseload of a bankruptcy district. These generic case weights are based on a 1989 study by Bermant, Lombard & Wiggins (1991) and are still in use today (see e.g. USGAO, 2009, 2013).

Table A1 in the online appendix shows the approximate number of hours judges spend on different types of consumer and business filings, which are the weights used in the calculation of court caseload. Chapter 11 filings are the most time-intensive, with the average case taking around 8 hours. On the other extreme, consumer Chapter 7 cases only take an average of six minutes. While there is considerable heterogeneity underlying these averages, they reflect the best practice of the Judicial Conference. Multiplied with the number of cases per judge, the resulting measure of weighted caseload per judge is the single most important criterion used to allocate new judgeships across bankruptcy districts in the United States (USGAO, 1997, 2013). In the remainder of the paper, I thus use it as a proxy for judicial backlog.[8]

An intuitive interpretation of the weighted caseload variable is the number of hours per year a judge spends on bankruptcy cases alone. This estimate does not include other tasks, such as

[8]Throughout the paper, I use the term judicial backlog to refer to the caseload burden of bankruptcy courts. Conceptually, it can also be interpreted as a proxy for the functioning of the bankruptcy court system, which may encompass other aspects.

adversary proceedings, court administration, and travel. According to Bermant et al. (1991), work on cases and proceedings make up only around 57% of a judge's time; 29% are spent on court administration, work-related travel, and other judicial activities; and the remainder on personal time.

Figure 1 plots the weighted caseload per judge from 1990 to 2017. For reference, consider that judges had an average workload of around 500 hours per year in 1980. This increased to over 1,000 hours in the early 1990s. Until mid-2005, the caseload hovered between 1,000 and 1,250 hours per year. With 250 work days a year, these are 4 to 5 hours per day. The workload saw an unprecedented drop in the first quarter of 2006 after the implementation of BAPCPA. The number of filings spiked in the aftermath of the 2007-2008 financial crisis, leading to a sharp increase in judge workload. By 2015, workloads were back to early 2006 levels, suggesting that BAPCPA had a permanent effect.

The high workload is a well-known fact in the judiciary and yields yearly recommendations by the Judicial Conference to create additional judgeships.When asked by the Huffington Post, one district judge summarized the caseload situation as follows:

"For the most part, we've just resigned ourselves that this is our fate and there's nothing we can do about it. We've complained. We've begged. We've cajoled. We've done everything you can humanly do to try to get additional judgeships." (Huffington Post, 2015)

2.3 2005 Bankruptcy Reform Details

President George W. Bush signed the Bankruptcy Abuse Prevention and Consumer Protection Act of 2005 into law on April 20, 2005. The law made it considerably more difficult for consumers to file for bankruptcy under Chapter 7. The bill had a long legislative history: originally drafted in 1997, the law was de-facto vetoed by President Clinton in 2000 and then introduced in each Congress, only to be shelved over disagreements about proposed amendments between Republicans and Democrats. Only after the Republican party increased their majority in the Senate and House in the 2004 elections was the bill re-introduced in the Senate on February 1, 2005. BAPCPA passed Senate on March 10 and the House of Representatives on April 14. Bush signed the bill on April 20. Importantly, most provisions only applied to cases filed on or after October 17, 2005.

Support for the act came mainly from banks and credit card companies, who expected to benefit from "a stronger hand in recovering unpaid consumer debt" (American Banker, 2005a). BAPCPA introduced a number of amendments to steer debtors with incomes above the state median from Chapter 7 towards Chapter 13, which required them to pledge future income and allowed for less debt forgiveness. If a filer's income over the previous six months is above the state median, they are now subject to a "means test". A major amendment is that BAPCPA allows filings to be dismissed based on a "presumption of abuse", depending on the outcome of the means test or through a finding of bad faith.

BAPCPA also changed various other aspects of consumer bankruptcy, e.g. by increasing the minimum time between bankruptcy filings and limiting homestead exemptions, which allow debtors to exempt the value of their homes from creditors. In addition, BAPCPA made bankruptcy considerably more expensive: a report by the United States Government Accountability Office estimates that attorney fees for Chapter 7 cases increased by approximately 50%, as did filing fees (USGAO, 2008).

These changes had a dramatic effect on the caseload of U.S. bankruptcy courts. Because the bill was signed in April but largely applied only from October, many individuals attempted to make use of the "old" law in a rush to file for bankruptcy. An American Banker article at the time reported that "[p]eople were lined up around the block Friday at the U.S. Bankruptcy Court for the Southern District of New York, hoping to file the last business day before the new rules took effect" (American Banker, 2005a). This spike can clearly be seen in the data on consumer case filings (see figure 2). The increase was anticipated and expected to be short-lived, as evidenced by a Wall Street Journal article from June 1 aptly titled "A Surge in Bankruptcy Filings Is Expected Ahead of New Law" (The Wall Street Journal, 2005).

More importantly, the number of consumer bankruptcies collapsed going into the first quarter of 2006, once BAPCPA took effect and the courts had digested the filing frenzy. As a result, the caseload per judge more than *halved*, dropping to levels in the ballpark of what had last been seen around 1980. During 2006 and 2007, there was only a slight upward correction before going into the Great Recession. Gross, Kluender, Liu, Notowidigdo & Wang (2019) use excess mass methods from the tax notch literature and show that, even taking into account the "rush to file", BAPCPA led to a large *net* reduction in bankruptcies. The average caseload per judge in 2006 and 2007 was around 566 hours, the largest ever percentage change reduction of court workload in peace times from an average of 1,059 hours in 2004 and 2005.

While bankers and analysts at the time anticipated that filings would drop eventually after the short-lived initial increase, there was considerably uncertainty about *when exactly* that would happen. The American Banker, for example, reported that Bank of America faced $500 million in additional charge-offs in the last quarter 2005 due to the unexpected number of filings and added that "[s]ome doubt that the effects will be over this quarter" (American Banker, 2005b). By 2006, the sharp drop in consumer cases was well-known in the industry (see American Banker, 2006).

Crucially, while BAPCPA fundamentally reformed personal bankruptcy, it left firm bankruptcies largely unaffected. The most obvious indication of this is that the number of Chapter 11 filings around the reform show no particular trend (see figure 2).[9] Nevertheless, it is important to discuss a few provisions that may be relevant for the large businesses in my sample.

First, BAPCPA introduced a "drop dead" date for the exclusive right of a debtor-in-possession to file a plan of reorganization. Before BAPCPA, the initial 120-day period could be extended indefinitely; after BAPCPA, it was capped at 18 months. Since this provision only applied to cases filed on or after October 17, 2005, it would not "bite" until mid-April 2007 (see e.g. Jones Day, 2007). Second, BAPCPA introduced limits to the applicability of automatic stay in bankruptcy for repeat filers to prevent abuse. The new §362(c)(3) provided that for debtors filing within one year after a dismissed earlier chapter 7, 11, or 13 bankruptcy case, the automatic stay terminates 30 days after the filing.[10] To put this into perspective, Altman (2013) finds that 15% of debtors once in Chapter 11 ultimately file for bankruptcy again. Third, BAPCPA amends provisions regarding the treatment of unexpired leases of non-residential real property, which has to be returned to the lessor within 120 days (with a single possible court extension of 90 days). While this seemingly eased the previous 60-day requirement, it permitted courts to grant only a single extension where no limit was in place prior to BAPCPA. This particularly affected borrowers who make regular use of leasing, such as retail and wholesale traders.

In my empirical analysis, I take great care to show that my findings are not driven by these provisions. To begin, the non-business share in a bankruptcy district's caseload is orthogonal to observable firm and loan characteristics. As such, there is no reason why firms in districts with a higher exposure to the drop in caseload would be differentially affected by the minor legal changes, except through BAPCPA's effect on judicial backlog. I also show that my findings are unchanged if I exclude the firms that were likely most affected by these provisions.

[9] Note that the small increase between the passing and application of BAPCPA is entirely driven by the smallest firms who will not be in my dataset (Iverson, 2018).

[10] Filers or other parties can make a motion to provide evidence the bankruptcy is filed in good faith with respect to the creditor(s). §362(i) also states that subsequent cases should not be considered to be in bad faith if a prior dismissal was due to the creation of a debt repayment plan.

3 Data

3.1 Loan Contract and Balance Sheet Information

I use loan-level data from Thomson Reuters' DealScan. The main variables I construct from DealScan are the the interest rate spread (usually over LIBOR), the natural logarithm of the loan maturity (in months), and size of the loan (in logs). I further include a dummy for whether a loan is backed by collateral.

Balance sheet data come from the annual Compustat Fundamentals, which I extend with rating data from Mergent FISD. Following standard procedure in the literature, I exclude firms in the finance and real estate business, as well as regulated public utilities. All balance sheet variables are winsorized at the 1st and 99th percentile. I construct the firm-level control variables book leverage, total assets (millions of dollars, in natural logarithm), ROA, sales growth, a dummy for the existence of a credit rating, and the share of fixed assets (tangibility). Following Nini, Smith & Sufi (2009) and Sufi (2007), I further use the debt to cash flow ratio to proxy for credit risk, which is captured using two dummy variables indicating whether a firm is in the top quartile of the ratio or has negative cash flow. The exact definitions of all variables can be found in the appendix.

The Compustat data are matched to DealScan using the 2018 vintage of the link table provided by Roberts & Sufi (2009). As I will discuss below, I restrict the sample to the period 2004 to 2007 for the main analysis. This leaves me with up to 3,744 loans and 1,116 borrowing firms in the estimation sample.

I focus on the market for large, mostly syndicated corporate loans covered by DealScan for a number of reasons. First, in the specific setting of BAPCPA, small businesses were directly affected by the reform because their fortunes are immediately tied to their owners' personal finances, including their ability and expected costs to file for bankruptcy. This means an analysis of local outcomes, for example on small business loans, would not be instructive about the role of backlog in bankruptcy courts. To my knowledge, there is further no loan-level dataset for regular small business lending. Second, syndicated loans are complex contracts fraught with incentive and moral hazard problems (see e.g. Sufi, 2007). In contrast to small business loans, lawyers play a key role in this market by negotiating contract provisions, preparing transaction-specific credit agreements, and conducting a thorough legal review of location-specific differences in bankruptcy proceedings.[11] One indication of this is that law firms make up most of the members of the Loan

[11] A lender's legal counsel ("lead counsel") is also involved in the enforcement of contract terms, e.g. in the case a

Syndication and Trading Association (LSTA).[12] The "league tables" that document which players are most important in the syndicated loan market also have a dedicated ranking for law firms.[13][14] Credit rating agencies covering the syndicated loan market also pay attention to the backlog of bankruptcy courts: Standard & Poor's, for example, explicitly track case histories and the time to resolution in bankruptcy courts when evaluating a borrower's expected recovery values (Standard & Poor's, 2011).

Bankruptcy and default are not unusual for the types of firms in my sample. In 2001 and 2009, 8% and 11% of leveraged loans were in default, respectively.[15] 6% of the firms in the merged DealScan-Compustat sample file for bankruptcy or are liquidated at some point.[16] Data from Moody's suggest that the median recovery rate for syndicated loans is around 75 cents on the dollar for B-rated firms and 58 cents for Caa-rated firms (Moody's, 2004). This suggests that, even for large borrowers, creditors are exposed to substantial losses when they go bankrupt.

3.2 Bankruptcy Court Information

Data on the caseload and number of judges per bankruptcy district come from the Administrative Office of the U.S. Courts (U.S. Courts, 2019a). The time series for the number of judges in each bankruptcy district was shared with me by the Administrative Office via email. These data cover the period 1980 to 2017.

borrower violates a loan covenant restriction. See, for example, https://www.investopedia.com/terms/l/loansyndication.asp.

[12]See https://www.lsta.org/members/ for a full list of LSTA members.

[13]See, for example, the 2015-H1 ranking at https://www.loanpricing.com/2015/06/1h15-us-lender-law-firm-bookrunner-by-volume/.

[14]These features stand in stark contrast to the market for small business loans. Data from the FDIC's Small Business Lending Survey 2018 suggests that almost 80% of small banks and more than 90% of large banks use individuals' credit scores when evaluating loan applications. During the period around BAPCPA, the 20 largest U.S. banks had a market share of around 60% in small business lending. In the early 2000s, large banks further started relying more on automated systems such as credit scoring (Mills & McCarthy, 2014). Taken together, this suggests that at least in the U.S., the conditions of local bankruptcy courts may be less important for small businesses compared to syndicated loans.

[15]See https://www.lcdcomps.com/d/public/defaults1111.html. In comparison, Giesecke, Longstaff, Schaefer & Strebulaev (2011) report a default rate of around 3% for bonds around the dot-com crisis.

[16]48 loans in the estimation sample were issued by 16 borrowers who filed for bankruptcy in the year of loan origination. As I show below, excluding these loans makes no difference to the results.

3.3 Data on Bankruptcy Cases and Recovery Values

To look at the effect of court congestion on recovery values, I use two sources of data. First, I make use of Moody's Default and Recovery Database, which includes data on recovery values for firms in bankruptcy, measured as the trading price of defaulted debt (in cents on the dollar). Second, I look at the liquidation proceeds and court expenses of non-business Chapter 7 cases overseen by the U.S. Trustee Program (USTP). The USTP is in charge of overseeing private Chapter 7 trustees who are responsible for the collection and liquidation of assets and their distribution to creditors. As part of these cases, trustees are required to file a report with the USTP that breaks down the distribution of liquidation values among creditors. These reports include information on court and administrative expenses that these trustees pay to the court, e.g. for filing motions or appeals.[17]

To estimate the elasticity of the length of business bankruptcy cases to a reduction in court caseload, I use case-level data from the Federal Judicial Center. I obtain data on the filing and closing dates of 19,902 bankruptcy cases where the filer was a corporation and the nature of the debt primarily business-related. I define the length of a case as the number of days between the original filing and closing date.

3.4 Summary Statistics

Table 1 presents summary statistics for the estimation sample. The median borrower is large, with more than $1 billion in assets, has positive sales growth of around 9%, and a credit rating. Around 34% of firms had a loan maturing in 2006 or 2007 that had been issued before BAPCPA was passed, suggesting a refinancing need. The median loan has a size of around $180 million and comes with a 175 bps spread and a maturity of around 5 years. Most loans are backed by collateral, a well-known feature of the syndicated loan market. There is considerable variation in borrowers' exposure to BAPCPA, i.e. the share of nonbusiness cases in the total weighted court caseload. The standard deviation is around 12%. The median district has a nonbusiness share of around 81%.

[17] Between 2004 and 2007, a total of 253,540 cases were reported to the USTP. Unfortunately, the USTP offices do not correspond to judicial districts: some districts have multiple USTP offices, and some offices cover multiple districts. I can thus only use the 27 states with a single bankruptcy district.

3.5 Filing Location and the Issue of "Forum Shopping"

A key question in my setting is which firms are exposed to the caseload of a given bankruptcy district. In the empirical analysis, I assign borrowing firms to bankruptcy districts based on their headquarter location using the address data from Compustat.

According to 28 U.S.C. §1408, however, firms in the U.S. can file for bankruptcy where they (1) are incorporated; (2) have their principal place of business; or (3) have an affiliate that filed for bankruptcy. In practice, this means that the largest nationwide firms have considerable leeway over where to file for bankruptcy, a phenomenon often referred to as "forum shopping". Delaware and Southern New York are arguably the most frequent recipient districts of such forum shopping filings. In high-profile bankruptcy cases such as Enron or WorldCom, the companies filed in districts where they did not have the majority of their business operations. As a result, there is an ongoing debate among legal practitioners and academics about the pervasiveness of forum shopping.

In my setting, assigning firms to bankruptcy districts based on their headquarter location is likely prudent for a number of reasons. First, the empirical strategy plausibly rules out most concerns about forum shopping by construction. All regressions include borrower and time fixed effects, which means any change in the propensity to "forum shop" would have to change differentially and permanently within the same firm precisely in early 2006. As I will show below, exposure to the caseload drop caused by BAPCPA—-as measured by a bankruptcy district's pre-reform caseload share of nonbusiness filings—-is also largely orthogonal to firm characteristics. This suggests that geographical exposure to the BAPCPA caseload drop is likely uncorrelated with the propensity to shop for a preferred venue across firms. To affect my findings in triple difference regressions, there would have to be a sudden change in lenders' expectation of forum shopping for the same borrower, depending on the pre-determined nonbusiness share of the bankruptcy court *and* different firm characteristics. This seems unlikely.

Second, forum shopping is overall relatively rare in a quantitative sense. Iverson (2018) shows that out of *all* Chapter 11 cases filed at U.S. bankruptcy courts between 2004 and 2007, 91.3% of filings occurred within a debtor's headquarter state. The Commercial Law League of America (CLLA), an advocate for reforming bankruptcy venue regulation, has identified a forum shopping motive for 745 cases filed in the districts of Delaware and Southern New York between 2004 and 2014 (CLLA, 2016). Even in these prominent districts, however, this accounts for only 2.36% of business cases.[18] Further, bankruptcy judges are keenly aware of forum shopping and regularly overrule requests to file in "dubious" locations (see e.g. Gayda, 2015). As a result, these filing numbers likely overstate the frequency of *successful* forum shopping, because judges shoot down requests to file in a district that is not the main location of a borrower's business.

Third, forum shopping is mostly limited to "mega bankruptcy cases", i.e. to the largest companies in the U.S. As I show in the empirical analysis, the elasticity of loan contract outcomes to court caseload considerably *decreases* with firm size. This implies that smaller firms, who are more likely to file in their headquarter district, are more affected. What is more, even though the borrowers in my sample are large, they are still considerably smaller than the median publicly listed forum shopper, which has $3.2 billion in assets.[19] This is considerably above the 75th percentile of borrowers in my sample. The results also hold when excluding the largest firms; states or industries who are the most frequent victims of forum shopping; or dropping firms headquartered in Delaware and Southern New York.

Fourth, headquarter location is likely a sound proxy for the location of the majority of firms' assets, a standard assumption in the literature on geographical factors in empirical finance research (e.g. Coval & Moskowitz, 2001; Loughran & Schultz, 2004; Malloy, 2005; Ivković & Weisbenner, 2005). Given that borrowers are assigned based to bankruptcy districts on their headquarter location likely introduces classical measurement error that, if anything, should lead to an underestimate of the true effect of congested courts. This is because if creditors additionally take into account firms' other potential bankruptcy venues, I will only capture a partial treatment effect. Ultimately, if creditors did not care about borrowers' headquarter locations, local judicial backlog should have no effect on loan terms; as I will show below, this is strongly rejected by the data.

4 Empirical Strategy

4.1 First Stage: Court Backlog, Case Length, and Recovery Values

BAPCPA approximately halved the congestion of U.S. bankruptcy courts before the onset of the Great Recession. Because BAPCPA targeted consumer bankruptcies, there is an almost linear re-

[18]These percentages are calculated by dividing the 745 cases identified by CLLA by the number of overall business bankruptcy cases filed in Delaware and Southern New York between 2004 and 2014 (653,235 and 78,947, respectively). The total number of cases are from the U.S. Courts statistics, see section 3.2.

[19]See the UCLA-LoPucki Bankruptcy Research Database (http://lopucki.law.ucla.edu/design_a_study.asp?OutputVariable=Shop).

lationship between the share of nonbusiness cases in total caseload prior to the reform ($Exposure$) and the subsequent drop in caseload per judge. Figure 3 visualizes this point, where the caseload drop is calculated as the difference between the averages for 2006-2007 (*Post BAPCPA*) and 2004-2005 (*Pre BAPCPA*). I exploit this geographical variation, jointly with pre-determined firm characteristics, to identify the effect of BAPCPA on loan terms.

Table 2 confirms the relationship in a regression framework. In column 1, I start by regressing the drop in caseload per judge on the weighted non-business caseload share ($Exposure$). The coefficient on the exposure variable is highly statistically significant and implies that a standard deviation increase in the non-business share (about 12 percentage points) reduced the caseload per judge by 64 hours, or about 12% of the average caseload drop (530 hours). In column 2, I introduce the number of judges created by BAPCPA as a control variable. The coefficient on $Exposure$ now *increases*, suggesting that the caseload drop is not driven by additional judges. Column 3 controls for the change in caseload between 2004-2005 and 2002-2003, which also increases the point estimate.

In figure 3, the districts of Delaware (DE) and Southern New York (NY,S) are clear outliers. For robustness, I drop these in column 4. As would be expected from a visual inspection of the figure, the slope is now considerable steeper, doubling the point estimate in size. At last, I rerun the regression for the districts in the loan-level estimation sample in column 5. The point estimate is almost equivalent to that in column 1.

Did this large decrease in judicial caseload indeed affect businesses that were not subject to any legal change? Four pieces of evidence suggest this interpretation. First, Iverson (2018) investigates Chapter 11 bankruptcies from 2004 through 2007. He finds that BAPCPA decreased the likelihood of smaller firms to be liquidated in Chapter 7 (compared to Chapter 11 reorganization). The reform also decreased the time in bankruptcy (as a function of firm size), as well as the likelihood of repeat filings by previously bankrupt companies. Iverson also finds that BAPCPA reduced bank charge-offs on *corporate* lending.

Second, I provide additional evidence on the length of firm bankruptcy cases using data from the Federal Judicial Center in the online appendix. Figure A4 and Table A3 report the result of case-level regressions suggesting that lower court caseload reduces the time firms spend in bankruptcy. Figure A5 shows the cross-sectional correlation between exposure to BAPCPA and changes in the median length of bankruptcy cases across districts around the reform. Column 1 of Table A3 suggests that a one standard deviation higher non-business caseload share (Std.

Dev. of $Exposure = 0.17$) is associated with a 4.6% drop in the length of firm bankruptcy cases $((EXP(-0.28 \times 0.17) - 1) \approx -0.046.)$. The magnitude increases to 5.5% when including case controls in column 2. The elasticity is similar when excluding the bankruptcy districts of Delaware and Southern New York in column 3.

Third, individual-level data on the recovery values of Chapter 7 cases suggest that less congested courts increase recovery values to creditors. Table A4 in the online appendix reports the results from running case-level regressions. They show that, starting in 2006, bankruptcy fees and court expenses decreased in relative terms in areas with a higher share of non-business cases. This may suggest a more efficient handling of bankruptcy cases requiring fewer motions or appeals. More importantly, lower court backlog increased recovery values (as measured by total net receipts as a fraction of the total fees). Quantitatively, a one standard deviation increase in $Exposure$ in this sample (0.057) is associated with a 9.9% reduction in court costs and a 6% increase in recovery values.[20]

Fourth, data from Moody's Default and Recovery Database suggest a sudden jump in recovery values in 2006. For firms that filed for bankruptcy, the median recovery rate increased from 28 cents on the dollar (in both 2004 and 2005) to 70 cents on the dollar in 2006. This is likely influenced by the housing boom, which increased firms' asset values. However, defaults outside of bankruptcy saw no such disproportionate increase in recovery rates, which increased smoothly from 62 and 69 cents on the dollar in 2004 and 2005, respectively, to 78 cents in 2006.

As an additional exercise, I test whether lower congestion increased ex-post recovery rates in firm bankruptcies during the Great Recession in 2008 and 2009, drawing on data Moody's Default and Recovery Database. The results in table A5 suggest that a one standard deviation higher exposure to BAPCPA increased the recovery values of firms who went bankrupt by 12 cents on the dollar, a sizeable effect. A sudden drop in court congestion should thus increase the expected returns to creditors.

4.2 Econometric Framework

How should a drop in court backlog affect firms' ex-ante financing terms? To gain intuition, section A in the online appendix describes a simple conceptual framework, in which a borrowing firm's

[20]Note that this does not mean that debtors' costs of filing for bankruptcy dropped after BAPCPA, which is counterfactual in the time series (Albanesi & Nosal, 2018). It does, however, suggest that these costs increased less in courts with the highest share of non-business cases.

interest rate depends on the congestion of bankruptcy courts. The key prediction from this model, based on on Hart & Moore (1994) and Hart & Moore (1998), is that a decrease in court congestion leads to lower interest rates, particularly for firms with higher default risk.

This theoretical framework suggests an empirical test that relates the interest rate a firm pays on its newly issued debt to a measure of court backlog. However, one could easily imagine that firms' probability of default, for example, is systematically higher in areas with high court backlog. Estimating a naïve regression would then conflate court backlog with differences in credit risk.

To overcome this challenge, I exploit geographical exposure to the historic caseload drop around BAPCPA across bankruptcy districts as an exogenous shock that is plausibly uncorrelated with firms' ex-ante default probabilities. This quasi-experimental design allows me to estimate the elasticity of financing terms to court congestion using difference-in-difference regressions of the following form:

$$Y_{jidt} = \beta \; Post \; BAPCPA_t \times Exposure_d + \gamma X_{ijdt} + \alpha_t + \alpha_i + \alpha_b + \varepsilon_{jidt}. \tag{1}$$

The dependent variable Y_{jidt} is a loan contract term (the interest rate spread or the loan maturity) of credit facility j. i denotes borrowers, d bankruptcy districts, b creditors, and t years. $Post \; BAPCPA_t$ is set to 0 for 2004 and 2005 and 1 for 2006 and 2007. This timing is motivated by the actual drop in caseload per judge, which only occurred after the reform had been digested following the dramatic "file to rush" in the run-up to the implementation date in mid-october (see figure 1). In section 5, I show event study plots which clearly suggest this timing captures the contemporaneous drop in caseload rather than the passing or implementation of BAPCPA itself. I consider alternative time windows for robustness. $Exposure_d$ is the pre-reform share of non-business bankruptcies in a district in 2003. The coefficient of interest is β, which estimates the effect of an exogenous drop in court caseload. Note that $Post \; BAPCPA_t$ and $Exposure_d$ are absorbed by the fixed effects α_t and α_i. Standard errors are clustered by bankruptcy district; I consider other clustering choices for robustness.

In the baseline set-up, I include borrower, lender, and year fixed effects. This means I track changes in the financing terms of borrowers issuing multiple loans between 2004 and 2007. Borrower fixed effects control for unobserved time-invariant differences across firms. As I show below, the results also hold with more stringent fixed effects, e.g. borrower-lender or lender-time dummies. I control for lagged borrower characteristics (asset tangibility, total assets, ROA, sales growth, book leverage, a credit rating dummy, dummies for firms that have negative or top-quartile debt-to-cash flow ratios) and loan characteristics (size, collateralization), similar to Schwert (2018).[21]

The identifying assumption underlying this research design is that the financing terms of corporations in districts with a high and low caseload drop would have trended similarly in the absence of BAPCPA. Four facts support this parallel trends assumption. First, prior to the reform, borrower fundamentals and loan characteristics are remarkably similar in districts with high and low $Exposure_d$. I discuss this in more detail in the next section. Second, controlling for county characteristics (interacted with the $Post\ BAPCPA_t$ dummy) makes little difference to the estimates. Third, the correlation of exposure to BAPCPA with changes in financing terms is driven by smaller borrowers with a higher probability of default, as suggested by equation 5 of the conceptual framework in the online appendix. Spreads also drop first for shorter term loans, for which the level of court congestion is relatively predictable. This feature of the data allows me to absorb a full set of $borrower \times lender \times year$ dummies, ruling out many alternative explanations. Fourth, and most importantly, loan spreads and maturities trended similarly for borrowers in districts with high and low exposure prior to the drop in caseload in early 2006. This is also true for firms with ex-ante higher or lower bankruptcy risk.

4.3 Are Consumer-Centric Districts Different?

To investigate observable differences across bankruptcy districts that correlate with the share of non-business bankruptcies before BAPCPA, I plot the results of univariate regressions of a host of covariates on $Exposure_d$ in table 3. Borrower and loan characteristics are averaged on the district level. All variables are standardized to have mean 0 and a standard deviation of 1 to make the coefficients comparable. Reassuringly, column 1 in panels A and B suggests that borrower and loan characteristics in the pre-reform period were essentially uncorrelated with exposure to BAPCPA. This suggests that, from a borrower's perspective, exposure to the drop in court caseload following BAPCPA was as good as random.

Column 3 in panel C shows that $Exposure_d$ is significantly correlated with a few county characteristics, namely the share of manufacturing in total employment (positive), the share of finance employees (negative), income per capita (negative), and the Republican vote share in 2000 (posi-

[21] I find no effect of exposure to BAPCPA on these loan controls (see section 5.6). I also present robustness exercises in section 5.4 where I exclude all covariates or use their pre-reform values interacted with the $Post\ BAPCPA$ dummy. Collapsing the data into a single firm-level cross section based on the "pre"- and "post"-period yields similar results.

tive). There is a also negative correlation with the share of Asians and Hispanics as fraction of the total population.

Exposure to the intensity of the housing boom during the period does not appear to be positively correlated with the nonbusiness caseload share. Changes in employment in the construction or real estate sectors are far from being statistically significant. The exposure variable is *negatively* correlated with house price growth during the boom across states (but not within). If anything, this suggests that there was less of a demand boom in areas with a higher share of individual bankruptcies.

I control for the interaction of $Post\ BAPCPA_t$ with these county characteristics in my regressions. While this makes little difference to the point estimates, one may still be concerned that unobserved district-level factors confound my results. I attempt to circumvent this issue twofold. First, I exploit exposure to the caseload drop given by differences in the share of non-business caseload *within* states. In that case, I restrict the sample to states with multiple bankruptcy districts and include $state \times year$ fixed effects. Columns 2 and 4 of table 3 show that the exposure variable shows lower correlations with observable county characteristics *within* states, and the signs for many borrower and loan controls switch. Second, I will exploit that borrowers differ markedly in their exposure to the backlog of bankruptcy courts even *within* the same district, using a triple-difference specification.

5 Empirical Results: Busy Courts and Loan Contracts

5.1 Baseline Estimates

How do forward-looking creditors price in changes to the congestion in bankruptcy courts? Column 1 of table 4 starts to investigate this question by running equation 1 with interest rate spreads as the dependent variable. The estimated coefficient is around -107 and highly statistically significant. It implies that a one standard deviation increase in the exposure to BAPCPA (around 0.12) is associated with 13 basis points (bps) lower spreads ($-106.53 \times 0.121 \approx -13$). In column 2 I allow for the inclusion of district-level characteristics interacted with the $Post\ BAPCPA$ dummy. The purpose is to control for local differences in employment structure and a few other significant covariates identified in section 4.3. Matching districts on these control variables slightly increases the estimate to -122.

In column 3, I only exploit variation across bankruptcy districts within the same state. This means limiting the sample to states with multiple districts and adding state $\times$ year fixed effects. Recall from section 4.3 that, perhaps unsurprisingly, observable differences in county characteristics are more muted within the same state. The estimate of around -153 in column 3 is again highly statistically significant. A one standard deviation increase in $Exposure$ in this sample is associated with a drop in spreads of around 20 basis points, almost 10% of the sample mean.

In columns 4 through 6, I repeat the same exercise using the natural logarithm of loan maturities as the dependent variable. Again, all specifications are highly precisely estimated. In the baseline regression, the coefficient of 0.62 implies that a one standard deviation increase in exposure extends maturities by approximately $0.62 \times 0.121 \approx 7.5\%$. Including interacted district controls in column 5 leaves the estimate essentially unchanged. This magnitude slightly increases to around 9% in column 6 when only using within-state variation.

What do the magnitudes I find here imply? One clue comes from the effect of the BAPCPA-induced drop in judicial backlog on creditors' charge-off rates on business loans (Iverson, 2018). Iverson finds that a one standard deviation increase in the share of nonbusiness cases is associated with an around 10% decrease in banks' charge offs on business loans. In section 4.1, I find similar magnitudes for firm recovery rates in bankruptcy and liquidation values in Chapter 7 cases. These numbers suggest close to a one-to-one pass-through of higher expected recovery values into spreads and loan maturities: credit spreads decrease by at least 13 basis points (a 6% drop) if bankruptcy cases are processed 5.5% more quickly or recovery values increase by 6-12%. Put differently, decreasing the time firms spend in bankruptcy by a quarter could shave 60 basis points off their borrowing costs ($(25/5.5) \times 13 \approx 60$).

Figure 4 allows for an event study specification, where I interact year-quarter dummies with the $Exposure$ variable. The last quarter of 2005, before the caseload drop, serves as excluded period. There is no evidence of pre-existing trends before the caseload drop: the non-business caseload share was uncorrelated with loan terms before 2006. The passing and implementation of BAPCPA itself in March and October 2005 also had a zero effect on spreads and maturities. Instead, the sizeable changes in loan terms coincided with the drop in caseload in early 2006 shown in figure 1. This suggests the judicial backlog, not changes to the legal framework, are behind my results. It also suggests that other factors—-e.g. the propensity of firms to "forum shop"—-are unlikely drivers. Any other factor would have to explain the sudden improvement in loan terms that precisely coincides with the drop in court caseload.

This pattern also suggests that creditors distinguish between permanent and transitory changes to recovery values. Despite the massive spike in bankruptcy filings in the run-up to the BAPCPA implementation in October 2005, I do not observe a contemporaneous change in corporate loan terms. The reason is likely that this increase in the workload of judges was expected and known to be transitory, rather than permanent (see e.g. The Wall Street Journal, 2005). Given the relatively quick turnaround of consumer cases in bankruptcy courts, figure 4 is thus most consistent with lenders pricing in permanent changes to expected recovery values as soon as uncertainty subsides.

5.2 Exploring Borrower-Level Exposure

If the channel I am capturing is indeed a change to the backlog of bankruptcy courts, we would expect some borrowers to be more affected than others. Exploiting borrower characteristics also allows me to address the challenge that unobserved district factors may bias my estimates by including $district \times year$ fixed effects.

Motivated by the conceptual framework outlined above, one would expect bankruptcy court congestion to matter particularly for firms that have a higher probability of default. In the data, I measure the probability of default using market leverage, numerical credit ratings, and a firm's total assets. Since these firm variables could themselves have changed as a result of the caseload drop in early 2006, I define them as their average value in 2004 and 2005. I add the interaction of these pre-reform characteristics with the $Post\ BAPCPA \times Exposure$ variable in regressions of the type in equation 1, which creates variation on the borrower-time level. This allows me to absorb unobserved variation across bankruptcy courts using a full set of $district \times year$ dummies. Note that the fixed effects in this specification absorb the main "treatment" effect as well as the pre-reform values.

Table 5 shows the results of including these additional interactions. To aid comparisons, I reproduce the baseline result in column 1. Column 2 considers whether firms with higher market leverage were more affected by the BAPCPA-induced caseload drop. The coefficient of -579.99 implies that a standard deviation increase in both market leverage (0.23) and the non-business share (0.12) is associated with a differential effect of about 16 bps in spreads and 11 log-points in maturities. Figure 5 provides graphical evidence, where I again replace the $Post\ BAPCPA_t$ dummy with dummies for the individual quarters in the sample. Financing terms essentially only improved particularly for riskier borrowers, contemporaneously with the drop in bankruptcy court caseload, but not before.

I find similar effects in column 3 using credit ratings to measure default probability. The estimate of the triple interaction is -35.23 for spreads (highly statistically significant). A higher rating number signifies higher risk. Since AAA ratings are assigned a 1 in the rating variable and BB+ an 11, this captures the difference in spreads between prime- and junk-rated borrowers. The coefficient thus implies that the drop in caseload had a differential effect of $35.23 \times 0.121 \times 10 \approx 43$ bps on junk-rated borrowers for a one standard deviation increase in $Exposure$. For maturities, the estimate implies a 18 log points larger increase for junk compared to prime borrowers.

In column 4, I consider whether the effect of the BAPCPA-induced caseload drop varies by firm size. This also serves as a sanity check for the importance of forum shopping: if my findings were spuriously driven by the largest firms with potential leeway over their filing location, the effect should *increase* in firm size. The data suggest the opposite. For interest rates, the estimate of 29.94 for the triple interaction (highly statistically significant) implies that a standard deviation higher pre-reform size (1.601) paired with a one standard deviation higher $Exposure$ is associated with an almost 6 bps lower effect. For maturities, the corresponding value is $-0.31 \times 1.601 \times 0.121 \approx 6$ log-points.

Next, I exploit that around 30% of the borrowers in my sample had taken out loans prior to BAPCPA that matured after the drop in court congestion. It is unlikely that firms factored in a potential drop in the caseload of the bankruptcy court in their district when they took out loans prior to 2005, often years before it was clear whether BAPCPA would pass Congress. As such, firms that issued loans with a fixed maturity prior to the reform had an exogenously determined demand for credit in the post-reform period (also see Almeida et al., 2012). This exposed them to shifts in credit supply unrelated to borrower fundamentals.

I exploit this exogenous refinancing need by constructing a dummy for borrowers who issued a term loan prior to 2005 with a scheduled maturity date in 2006 or 2007. I exclude credit lines because I do not observe drawdowns. Column 5 of table 5 plots the results when including the triple interaction. The coefficients of -124.51 for spreads and 0.86 for maturities are statistically significant. The implied magnitudes are close to but slightly larger than the baseline coefficients in column 1. Taken at face value, this suggests that the effect of the caseload drop is entirely due to changes in credit supply, not demand. To illustrate, a one standard deviation increase in $Exposure$ decreased spreads of borrowers with a loan maturing in 2006 or 2007 by around 15 bps. For loan maturities, the implied magnitude is an increase of around 10%.

As a last exercise, I test whether the impact of court congestion on interest rate spreads differs by loan maturity. If creditors are pricing in higher expected recovery values in real-time, this should first occur for short-term loans. This is because, for shorter term loans, creditors are immediately exposed to the observable caseload in case a borrower defaults. For longer term loans, there is considerably uncertainty about whether changes in caseload are permanent or transitory. As such, they should only price in the caseload drop once they are convinced it is permanent.

Clearly, loan maturity is endogenous, so this exercise can only be regarded as illustrative. Further, banks and firms agreeing on short-term or long-term loans may be inherently different. However, 65% of borrowers in my data receive multiple loans from the *same bank* in the *same year*, and 35% of these loans differ in their maturity. This allows me to absorb a full set of $lender \times borrower \times year$ dummies. In other words, I can compare how lenders price two similar loans with different maturities that a borrower takes out in a given year.

Column 6 shows the results. The estimate on the triple interaction is positive (135.66) and highly statistically significant. It suggests that spreads drop 9 bps less for an around 20 months longer loan maturity (one standard deviation) and a one standard deviation increase in $Exposure$.

Overall, the results in this section are most consistent with the interpretation that court backlog matters particularly for borrowers that are most likely to file for bankruptcy. By exploiting arguably exogenous refinancing needs of firms, and differences in loan maturities between the same borrower and lender in the same year, I conclude that the effect of lower caseload is likely driven by an increase in credit supply.

5.3 Alternative Explanations

A few potential alternative explanations of my fundings are worth discussing. First, there may be a concern that borrower-level exposure of firms subject to the same court backlog shock within the same district picks up positive demand shocks from the 2000s housing boom. To investigate such concerns, I follow Mian & Sufi (2014) and construct measures of the nontradable and construction sectors. I then drop these sectors from the estimation since they are most directly affected by swings in house prices and local demand. Column 2 of table 6 shows that this has no bearing on my main results. The point estimates are almost unchanged compared to baseline coefficients.[22]

Second, BAPCPA included a few provisions for corporate bankruptcies, although it is unclear why they should differently affect firms based on court's ex-ante share of non-business caseload.[23]

[22] Also recall from section 4.3 that BAPCPA exposure is if anything *negatively* correlated with the change in house prices, a proxy for household demand, from 2001 through 2006.

[23] Levin & Ranney-Marinelli (2005) conduct a detailed legal analysis of BAPCPA and conclude that, if anything, the

However, Sautner & Vladimirov (2017) find that riskier U.S. firms used more trade credit (and had higher sales) in 2006 compared to 2003-2005, which they attribute to the changes in corporate bankruptcy law that were part of BAPCPA.[24] Because these amendments to corporate bankruptcies were largely targeted at retailers in particular, I can run a test where I exclude them from the sample.[25] Column 3 of table 6 shows that excluding borrowers with SIC codes starting with 50 makes no difference to the estimates. It is also unlikely that my results have to do with changes to the treatment of derivatives in bankruptcy (Edwards & Morrison, 2005), because my sample only includes non-financial corporations.[26]

BAPCPA also likely decreased interest rates on car loans by eliminating so-called cramdown provisions (Chakrabarti & Pattison, 2019).[27] Chakrabarti and Pattison measure the exposure to BAPCPA using the share of non-business bankruptcies that are filed under Chapter 13, which has a relatively low correlation of 0.19 with the share of non-business cases in total caseload. Including their interaction in column 4 makes little difference to the point estimates.

A third concern might be that the relatively large borrowers in my sample should not be sensitive to local judicial backlog due to "forum shopping". While I already outlined in section 2 why forum shopping, if anything, should bias the coefficients towards zero, I run three simple validity checks. In columns 5 through 7 of table 6, I exclude firms that are most likely to be forum shoppers: very large firms (those in the top 10% of total assets); firms headquartered in the five states most subject to forum shopping (CA, NJ, PA, IL, FL); and firms in industries where more than 75% of public bankruptcy cases are marked as "forum shopped".[28] The results suggest that these exclusions do not meaningfully change the point estimates.

reform changes to business cases might *reduce* recovery values for firms, which works against finding an improvement in financing terms.

[24] Sautner & Vladimirov (2017) mainly focus on a cross-country panel of differences in debt enforcement.

[25] Before BAPCPA, debtors-in-possession had to assume or reject unexpired leases of nonresidential real property within 60 days after the initial filing, a period that could be extended by the court. After BAPCPA, bankruptcy courts were only allowed to grant a single extension. Since retailers often have large numbers of leased properties, they were particularly affected by this provision. BAPCPA also hit businesses with large inventories by giving these "administrative expense priority status", which again impacted retailers. See Levin & Ranney-Marinelli (2005) for further discussion.

[26] See, e.g., a post on the "Synthetic Assets" blog for a thorough discussion of the effects of BAPCPA on the classification of different financial instruments.

[27] Before BAPCPA, borrowers with underwater car loans could have their debts reduced to the market value of the car through a "cramdown" in Chapter 13 bankruptcy. This was quite common because cars depreciate quickly. BAPCPA eliminated these for the first 910 days of car loans.

[28] The latter two measures are constructed using the UCLA-LoPucki Bankruptcy Research Database at http://lopucki.law.ucla.edu/.

Fourth, the 2004-2007 period saw an increase in the use of securitization (see e.g. Keys, Mukherjee, Seru & Vig, 2010; Benmelech, Dlugosz & Ivashina, 2012).[29] If lenders' propensity to securitize loans is correlated with the $Exposure$ measure, this could also explain an improvement in financing terms. To assess the impact of securitization, I exclude loan facilities that are possibly owned by CLOs.[30] Column 8 of table 6 shows that, despite the substantial drop in sample size, the coefficients are similar and significant at the 1% level.

Fifth, one could still be worried that other omitted geographical drivers affecting firm risk are behind the results. In an alternative story, firms in these areas could have experienced a sudden change in fundamentals that made firms less likely to default. This is counterfactual. In section 5.5, I show that borrower ratings did not change with the drop in caseload. Most likely, this is because firm leverage slightly increased, offsetting the increase in cash flows due to lower debt service payments.

I also test whether the share of nonbusiness bankruptcy cases ($Exposure$) picks up local co-movement or "beta" with the business cycle. I address this by constructing a placebo exercise in which I allow both $Exposure$ and the time dummy $Post\ BAPCPA$ to vary over the time.[31] Figure A8 in the online appendix plots the distribution of t-statistics for these placebo coefficients against the baseline estimates. The placebo t-statistics are roughly normally distributed around a zero mean and far from the estimates in table 4.

5.4 Further Robustness Checks

I report a battery of robustness checks in the online appendix. In table A6, I begin by establishing that, consistent with the graphical evidence in figures 4 and 5, the choice of time window does not drive my findings. Columns 2 through 5 allow the $Post\ BAPCPA_t$ dummy to either exclude 2005 or 2007 from the "treatment" or "control" status or extending the pre and post period. The results still hold, suggesting that the effects of lower court backlog are permanent. In column 6, I

[29] Data reported by the Securities Industry and Financial Markets Association (SIFMA) suggest that the volume of outstanding Collateralized Loan Obligations (CLOs), CDOs backed by corporate loans, increased from \$123.4 to \$189.7 between 2005 and 2006, the highest growth rate since the mid-1990s.

[30] I build on Benmelech et al. (2012) and drop all term loan B and C facilities, which are specifically structured for institutional investors. I also drop loans of borrowers with no credit rating at issuance. Since CLOs are primarily active in the leveraged loan segment, this likely excludes the vast majority of securitized loans.

[31] Specifically, I construct the share of nonbusiness cases for each year between 1994 and 2009 and define the placebo $Post\ BAPCPA$ dummy equivalent to the regression framework in equation 1. For example, if $Exposure$ is defined as of 1994, the $Post\ BAPCPA$ placebo dummy is 0 in 1995-1996 and 1 in 1997-1998. The intuition is that, if $Exposure$ merely measures cyclicality, it should also attract similar coefficients in other time periods.

further introduce control variables for the different legislative steps of BAPCPA as another way to hold constant any direct effect they may have had across bankruptcy districts. Again, this makes little difference to the point estimates.

Next, I conduct specification checks in table A7. In columns 2 and 3, I control for the interaction of a dummy for the 14 districts where BAPCPA introduced new judgeships, interacted with the $Post\ BAPCPA$ variable, or drop these districts entirely. Both approaches do not make a meaningful difference to my estimates. Next, I address that the districts of Delaware and Southern New York represent outliers in the first stage relationship between the non-business caseload share and the subsequent drop in the caseload per judge (see section 4.1). As a robustness exercise, I thus "winsorize" these to take the caseload share of Alaska, as in Iverson (2018). Another way to think about this is that I am adjusting for the structurally lower share of consumer cases in these districts. The coefficients in column 4 are again similar. Column 5 drops these districts entirely, which yields almost equivalent coefficients to the baseline results. In columns 6 and 7, I define the share of nonbusiness cases as of 2002 or 2004 (instead of 2003). This does not make a meaningful difference either.

As an alternative to the loan-level analysis, I collapse the difference in loan terms between the pre- and post-period into a single firm-level cross section. I construct averages of spreads and maturities before and after BAPCPA for each firm and take the difference between these two values. Column 9 presents the result of regressing the borrower-level change in contract terms on the $Exposure$ variable. While this approach has other disadvantages, it is reassuring that the result is again closely aligned with the baseline coefficient in column 1.

In table A8, I consider alternative specifications that add additional fixed effects or time trends. The inclusion of lender $\times$ year, SIC $\times$ year dummies, and linear or quadratic district time trends makes no difference for my point estimates. In column 7, I also find that the results do not change if I define the borrower controls as of 2003 and interact them with the $Post\ BAPCPA$ dummy. Column 8 shows that dropping a small amount of loans issued by firms that go bankrupt throughout the sample leaves the estimates unaffected.

As last exercises, I investigate whether weighting the estimates by population or other variables affects the coefficient magnitudes in table A9. I find that (unweighted) OLS yields the most conservative estimates. I also test alternative standard errors in table A10. The results are robust to a wide range of alternatives.

5.5 Effects on Firm Risk and Leverage

The results in the previous sections suggest a causal effect of court congestion on loan terms. One question that arises is what happens to borrower risk. The array of cross-sectional results above strongly suggest that the improvement in financing conditions was not driven by a sudden drop in unobserved default risk unrelated to court congestion. All else equal, however, lower debt servicing costs should decrease firms' default risk. On the other hand, lower court backlog should also increase firm leverage, which in turn increases firm risk.

In this section, I provide some additional evidence by regressing credit ratings and firm leverage on exposure to BAPCPA in a firm-year panel specification that is otherwise similar to equation 1:

$$Y_{it} = \alpha_i + \alpha_{jt} + \beta\ Post\ BAPCPA_t \times Exposure_d + \gamma X_{it} + \varepsilon_{it}. \quad (2)$$

The control variables are the same borrower controls as above; in columns 1 and 3, I only control for firm size.

Table 7 plots the outcomes of this exercise. I present regressions both for *levels* and *changes* in ratings and leverage. Riskier firms have higher numerical ratings. In Panel A, we can immediately see that the drop in court congestion around BAPCPA was not associated with a significant improvement in ratings: the estimated coefficients switch signs between specifications and are close to zero. To see this, consider that the coefficient of 0.24 in column 1 means that a one standard deviation increase in $Exposure$ (around 0.12) is associated with a change in ratings of 0.03, compared to a mean of 11.80. Similarly, panel B shows that lower caseload did not increase the likelihood of having any rating. Here, the estimate of 0.07 in column 1 suggests a one standard deviation increase in $Exposure$ is associated with a 0.008 percentage point increase in the probability of having a rating, compared to a baseline of 0.56.

Panel C plots the results for book leverage. Consistent with an increase in debt capacity, lower court backlog increases leverage. Quantitatively, a one standard deviation higher $Exposure$ (0.12) is associated with 2% higher leverage relative to the mean ($(0.12 \times 0.05)/0.33 \approx 0.02$). Taken together with the evidence on interest rate spreads, this is consistent with an increase in credit supply.

Figure A9 in the online appendix plots these patterns graphically, where I replace the $Post\ BAPCPA_t$ dummy with dummies for 2004, 2006, and 2007 (2005 is the excluded group). Again, the pattern is that the drop in caseload increased leverage but left borrower ratings unaffected. This suggests an offsetting effect of higher leverage and lower debt service payments on default risk.

As an alternative test for assessing *realized* default risk, I regress the total number of business or Chapter 11 bankruptcies (in natural logarithm) in a district on the interaction $Post\ BAPCPA_t \times Exposure_d$. I also test whether firm bankruptcies were more or less likely in more exposed bankruptcy districts in 2008-2009 (the years classified as NBER recessions) compared to 2004-2007. The results in table A11 in the online appendix suggest that exposure to BAPCPA neither mattered for changes in the number of business bankruptcies in 2006-2007 nor during the Great Recession. The coefficient estimates are far from statistical significant and economically small. For Chapter 11 cases, for example, the coefficient in column 3 suggests that a one standard deviation increase in exposure (0.11) is associated with a 0.9% increase in bankruptcies. This further supports the idea that the drop in court congestion did not coincide with an unobserved sudden drop in local default risk.

5.6 Effects on Other Loan Terms

In table A12, I look at the effect of court congestion on other contract terms. In column 1, I consider the effect of busy courts on the *total* cost of borrowing, i.e. interest rate spreads plus fees, using data from Berg, Saunders & Steffen (2016). The negative coefficient of -103.50 is highly statistically significant and suggests that the BAPCPA-induced drop in caseload lowered total borrowing costs, not just spreads. In column 2, I find a very small statistically insignificant negative coefficient for the size of a loan facility decreased with BAPCPA. The negative sign is consistent with the findings in Ponticelli & Alencar (2016) for a major bankruptcy reform in Brazil. Similar to the results in section 5.2, this likely reflects an increase in access to credit for smaller firms. Columns 3 to 6 look at whether the type of loan issued after BAPCPA varied with exposure to the caseload drop. I find no evidence that loans were more or less likely to be backed by collateral or part of CLOs. There is also no evidence for a change from credit lines to term loans (or vice versa).

5.7 Extrapolation: The Aggregate Costs of Court Backlog

I provide a simple back-of-the-envelope calculation on the aggregate costs of court backlog on borrowers' debt service. This exercise adopts the "naive" assumption that, when courts become less busy, firms' likelihood of filing for bankruptcy does not change in a way that undoes the observed improvements in financing terms. The assumption is strong but consistent with the reduced-form evidence above that the BAPCPA-induced drop in caseload did not alter firms' default risk and corporate bankruptcy filings. Still, such an extrapolation should only be interpreted as an illustrative exercise. It also does not allow me to make statements about the welfare effects of lower court congestion.[32]

The starting point of my estimation is that a reduction in interest rate spreads (a price term) can be used to estimate the partial equilibrium savings in corporate debt servicing costs (a quantity). Lower spreads reduce the annual interest burden of non-financial corporations: data from the Bank for International Settlements (BIS) suggest U.S. corporates pay around $400 billion per year to service their debts (Drehmann, Illes, Juselius & Santos, 2015).[33] I outline the results of this calculation here; section A.4 in the online appendix provides more details.

I calculate the macroeconomic savings in corporate debt service burden based on simplified installment loan formulas used by the Federal Reserve (Dynan, Johnson & Pence, 2003) and the BIS (Drehmann et al., 2015) to estimate economy-wide debt service:

$$\underbrace{\Delta d}_{\substack{\text{Change in}\\ \text{debt}\\ \text{service}}} = \underbrace{\sum_d}_{\substack{\text{Sum}\\ \text{over}\\ \text{districts}}} \underbrace{\Delta C_d}_{\substack{\text{Caseload}\\ \text{change}}} \times \underbrace{\frac{\hat{\beta}^S}{\hat{\beta}^C}}_{\substack{\text{Spreads}\\ \text{elasticity}}} \times \underbrace{L_d}_{\substack{\text{Outstanding}\\ \text{debt}}} \quad . \tag{3}$$

Δd is the change in aggregate corporate debt service burden. L_d is total outstanding corporate debt in bankruptcy district d. $\hat{\beta}^S/\hat{\beta}^C$ is the elasticity of interest rate spreads to an exogenous one hour drop in caseload per judge I estimate in this paper. $\hat{\beta}^S$ is the elasticity of spreads to $Exposure$ from estimating equation 1. $\hat{\beta}^C$ is the estimated elasticity of the caseload per judge to $Exposure$. ΔC_d is a hypothetical change in the caseload per judge.

L_d is easily observable in the data. I use three different measures. First, I only consider the debt of firms in the estimation sample. Second, I use the debt of all non-financial firms in Compustat for which I can identify a bankruptcy district. Third, I scale up the debt of Compustat firms using data on total corporate debt from the Financial Accounts of the United States, published by the

[32] Higher spreads due to judicial backlog could be a deadweight loss if they benefit neither creditors (who have to compensate for lower expected returns) nor borrowers (who have to devote more of their income to interest payments). However, higher recovery values could also be a sign of more creditor-friendly outcomes, which would have to be weighed against the benefits of lower debt servicing costs for borrowers.

[33] BIS statistics suggest that the debt service to income ratio of the U.S. non-financial corporate sector was around 40% for the years 2016 through 2018. The Bureau of Economic Analysis reports that U.S. non-financial corporations made around $1 trillion in after-tax profits per year during that period.

Federal Reserve.[34]

I use the smallest estimate I find for $\hat{\beta}^S$ (≈ -50.64) from column 7 in table A7. For $\hat{\beta}^C$, I use the estimate from column 1 in table 2.[35]

Finally, I consider three hypothetical changes in the caseload per judge, ΔC_d. First, I look at the observed caseload drop around BAPCPA. Second, I use the Bankruptcy Judgeship Act of 2017, which passed the Senate on September 5, 2017 and—following the recommendations of the Judicial Conference—added four new permanent judgeships for the districts Delaware (2), Florida Middle (1), and Michigan East (1). Third, I estimate the effect of hiring one additional judge in the districts with the most congested courts (those in the top 10% of the caseload per judge).

Plugging these inputs into equation 3 suggests that—keeping the strong assumption above in mind—congested courts may substantially increase borrowers' debt service burden. The estimates imply that the historic drop in caseload around BAPCPA saved U.S. corporations perhaps around $10 billion in annual interest payments. However, BAPCPA was a watershed event in the history of bankruptcy in the United States and as such perhaps a poor benchmark. The estimates for the Bankruptcy Judgeship Act of 2017 imply that it may have reduced the annual corporate debt service burden by between $350 and $800 million. I find slightly larger estimates for the hypothetical scenario of creating one additional judgeship in the most congested courts, ranging from $440 million to around $1.5 billion. Averaging the estimates suggests that busy bankruptcy courts cost borrowers an additional $740 million per year in debt servicing payments.[36]

While these estimates are relatively small compared to GDP, they are substantial relative to the costs of hiring additional judges. For the Bankruptcy Judgeship Act of 2017, the Congressional Budget Office (CBO) estimated that bankruptcy judges earn about $232,000 in salaries and benefits.[37] The CBO also provides an estimate for judicial administrative costs for personnel, security,

[34] Depending on the scenario, described below, the debt of the firms in the estimation sample makes up between 27% and 31% of total corporate debt reported in the Financial Accounts.

[35] These estimates imply that a one standard deviation increase in $Exposure$ (0.132) is associated with a drop of 64 hours in annual caseload per judge ($0.115 \times 555.08 \approx 64$) and a ≈ 7 bps drop in spreads.

[36] Despite the clear limitations, there are reasons to believe that this estimate is likely a lower bound. First, it ignores the interest burden of households. Second, I do not consider the costs from an inefficient resolution of bankruptcy cases due to congested courts (see e.g. Iverson, 2018) and knock-on effects such as skewed input choices for firms (Boehm & Oberfield, 2018). Third, I do not consider the effect of caseload on loan maturities. Drehmann et al. (2015) show that even small changes to maturity of outstanding debt can drastically shift the interest burden.

[37] Bankruptcy judges are entitled to compensation equal to 92% of that of a district judge, which puts their listed annual salary at approximately $191,000. See 28 U.S. Code § 153 for the background covering bankruptcy judge compensation and U.S. Courts (2019b) for the time series of judicial pay. District judges in the United States were entitled to $208,100 in compensation in 2018.

and court operations of about $700,000 per judge per year. Creating an additional judgeship thus costs approximately $932,000. My estimates based for the Bankruptcy Judgeship Act and the 10% most congested courts are based on hiring an additional four or eight judges, respectively. The cost of hiring new judges is thus small compared to what a back-of-the-envelope calculation suggests borrowers lose in debt servicing costs due to congested bankruptcy courts.

6 Conclusion

Exploiting exposure to the Bankruptcy Abuse Prevention and Consumer Protection Act of 2005 as an exogenous shock to court caseload, I show that judicial backlog has first-order effects on the ex-ante contract environment in the corporate loan market. To the best of my knowledge, my paper provides among the first estimates of the causal effects of debt enforcement on ex-ante outcomes. I estimate that a 64 hour drop in annual caseload per judge reduces the time bankruptcy cases spend in court by 5.5% and increases recovery values for creditors by 6-12%. As a result, credit spreads drop by up to 13-20 basis points (or 6-10%) and loan maturities increase by 7.5-9%.

Under the assumption that borrowers' probability of filing for bankruptcy does not change with the drop in court caseload—for which I provide some evidence—one can get a sense of the costs of judicial backlog on aggregate debt servicing costs using a back-of-the-envelope exercise. Because the outstanding dollar value of debt is large, my estimates imply that court backlog may cost U.S. corporations more than $700 million per year in additional debt service.

A caveat to this work is that the particular institutional setting here does not allow me to reliably estimate the effect of court backlog on regional outcomes. Such estimates may be important for assessing the macroeconomic costs of a congested court system, and future research should provide more evidence on this front.

An important question is how my findings apply to other settings. There are some reasons to believe the estimates here constitute a lower bound for the importance of courts. First, I find larger effects on smaller, riskier firms. While the institutional setting I study requires a sample of mainly large corporations, this suggests smaller businesses may benefit even more from lowering the caseload burden of courts. Second, for a variety of reasons, the firms studied here have relatively high expected recovery values. It is likely that courts matter much for access to credit for smaller, more opaque businesses, especially in countries where the bankruptcy code favors outright liquidation. Third, I have not considered that congested courts may lower firms' productivity and thus creditworthiness by distorting input choices (Boehm & Oberfield, 2018). Such general equilibrium effects are another reason the estimates here may be a lower bound.

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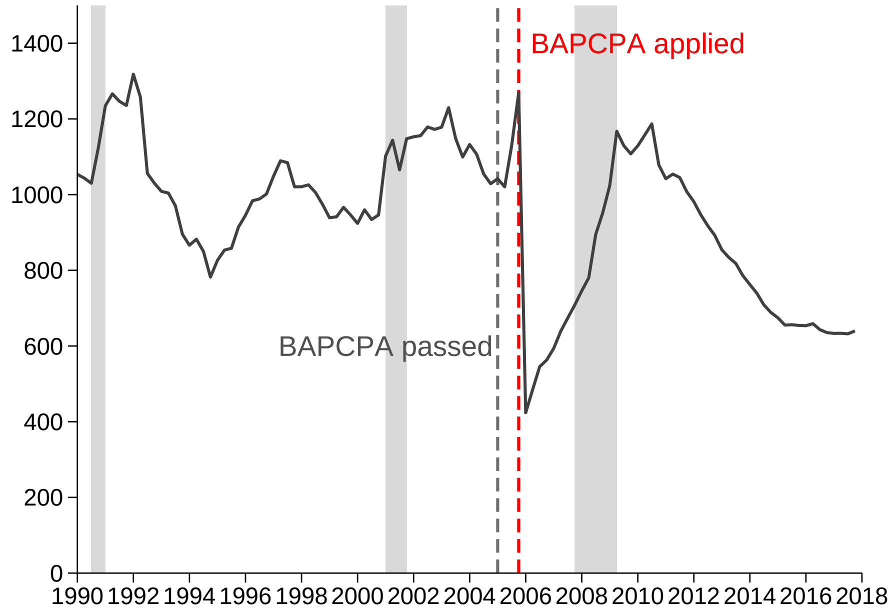
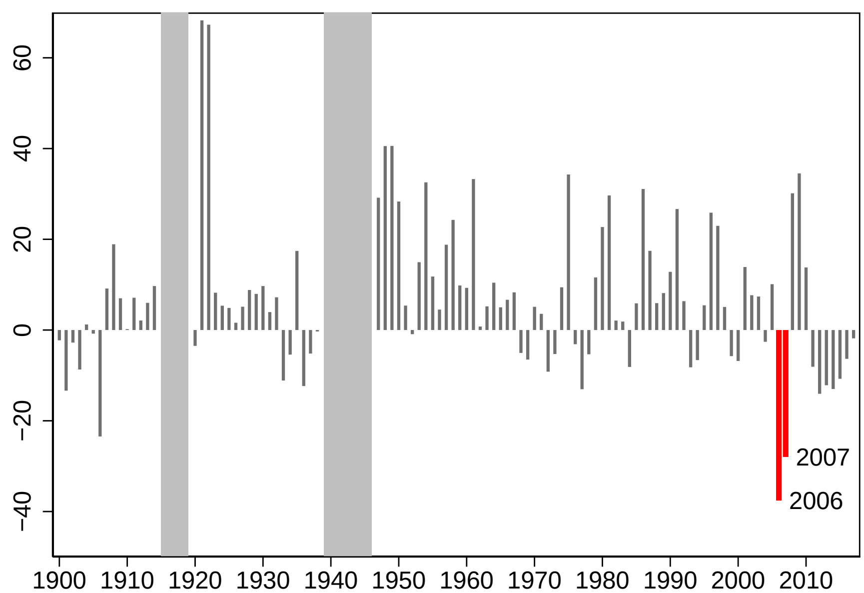
Figure 1: BAPCPA and the Caseload per Judge

(a) Weighted Annual Caseload per Judge, 1990-2017



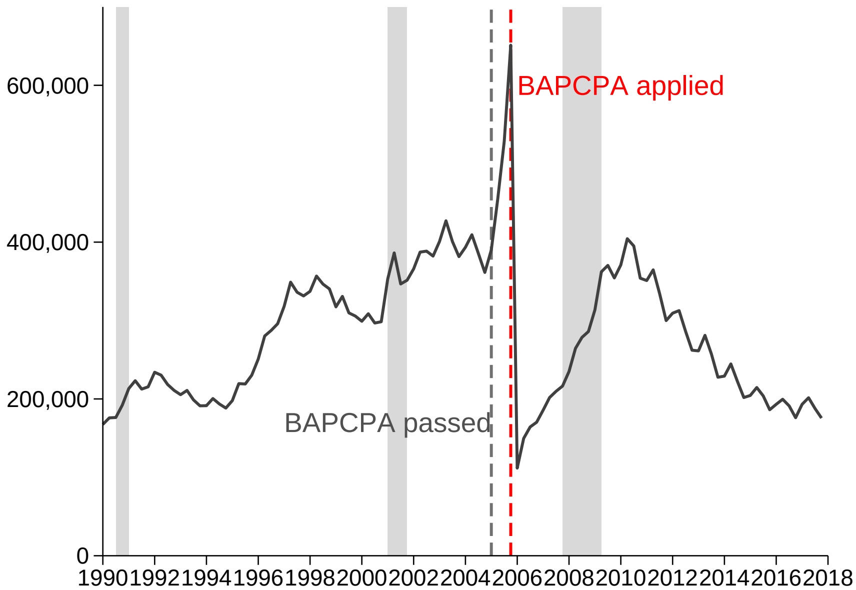
(b) Year-on-Year Change in Total Bankruptcy Filings (in %), 1900-2017



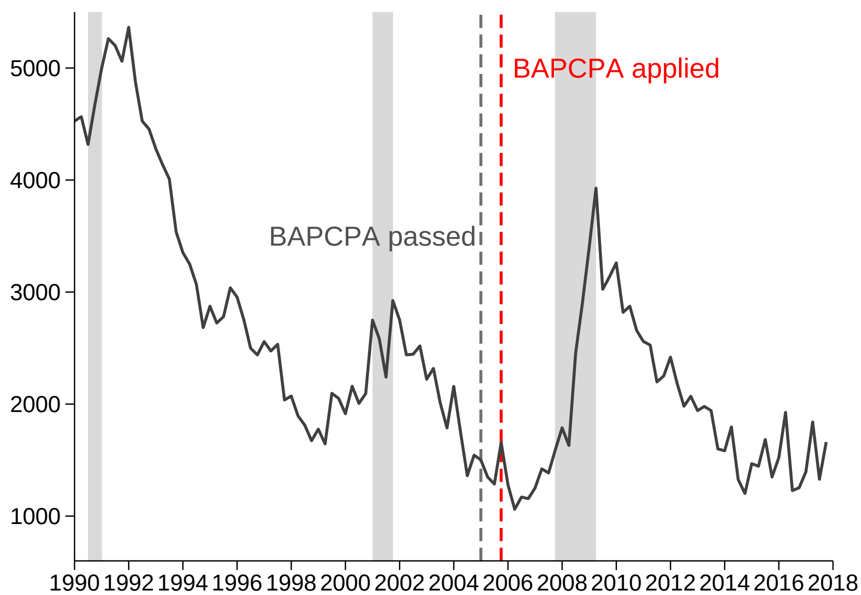
Notes: Panel A plots the weighted caseload per judge between 1990 and 2017. Intuitively, this measures the hours per year an average bankruptcy judge is expected to work on his cases (excluding other work). NBER recessions are shaded in gray. Panel B plots year-on-year percentage changes in the total number of bankruptcy filings in the United States from 1900 to 2017, excluding observations recorded during the two World Wars (in gray). The data are from the United States Court System and Federal Judicial Center (2019).

Figure 2: Non-Business and Chapter 11 Cases, 1990-2017

(a) Non-Business Cases Filed per Quarter

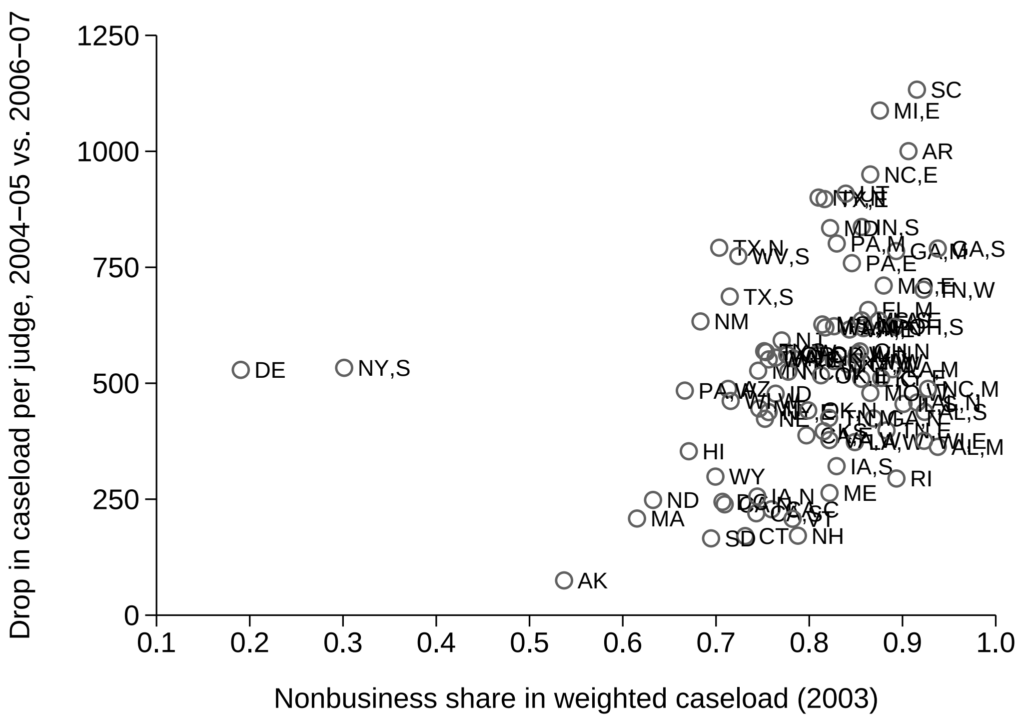


(b) Chapter 11 Business Cases Filed per Quarter



Notes: NBER recessions are shaded in gray. The data are from the United States Court System.

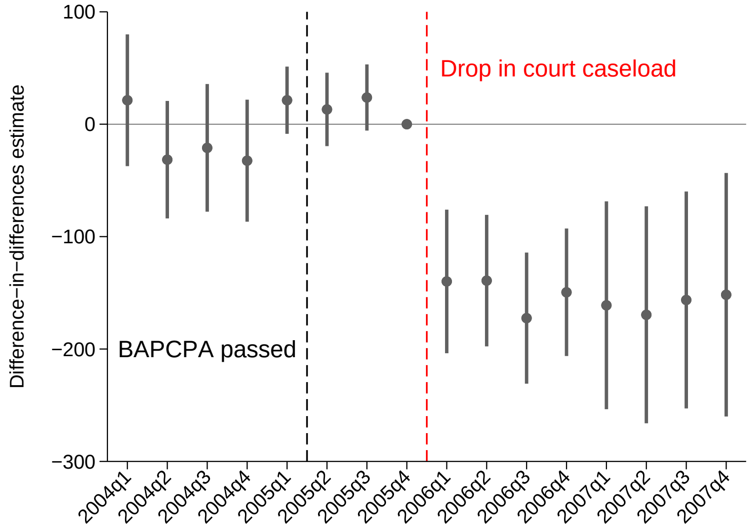
Figure 3: District Exposure and the Post BAPCPA Caseload Drop



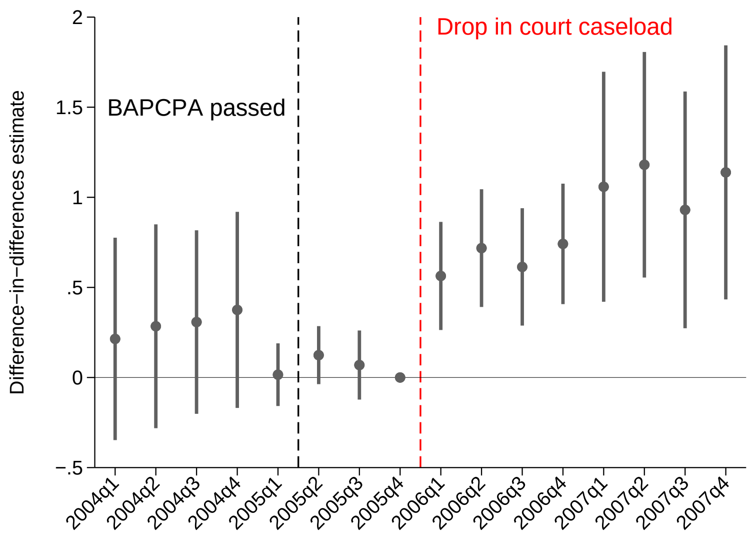
Notes: This figure plots the drop in the caseload per judge against the share of non-business cases in the weighted caseload of a bankruptcy district in 2003. The caseload drop is calculated as the difference of the average caseload in a district between 2006-2007 and 2004-2005. The non-business share is the weighted caseload for non-business bankruptcy cases as percentage of the total weighted caseload in 2003. The source of these data is the United States Court System.

Figure 4: Court Congestion and Financing Terms

(a) Interest rate spread (in bps)



(b) Log(Loan Maturity)



Notes: These figures show the effect of an exogenous drop in bankruptcy court caseload on loan terms. They plot the estimated coefficients β_t of the following regression:

$$Y_{jidt} = \sum_{t=2004q1}^{2007q4} \beta_t D_t \times Exposure_d + \gamma X_{jidt} + \alpha_t + \alpha_i + \varepsilon_{jidt},$$

where Y refers to the interest rate spread in panel (a) and the natural logarithm of loan maturities in panel (b). The last quarter of 2005 is the omitted category. "BAPCPA passed" refers to the passing of BAPCPA in the Senate on March 10, 2005. 95% confidence intervals are based on standard errors clustered by district.

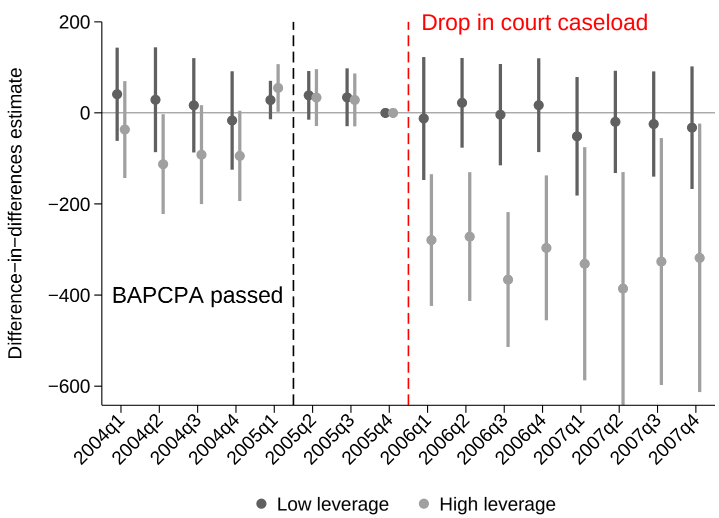
Figure 5: Court Caseload and Loan Terms - Split by Pre-Reform Market Leverage

Notes: Figure 5 shows the effect of an exogenous drop in bankruptcy court caseload on interest rate spreads and loan maturities. They plot the estimated β_t coefficients of the following regression:

$$Y_{jidt} = \sum_{t=2004q1}^{2007q4} \beta_t D_t \times Exposure_d + \gamma X_{jidt} + \alpha_t + \alpha_i + \varepsilon_{jidt}$$

The excluded group is the last quarter of 2005. "High leverage" refers to firm-year observations in the top quartile of market leverage in 2004-2005; "Low leverage" to those in the bottom quartile. 95% confidence intervals are based on standard errors clustered by bankruptcy district.

(a) Interest rate spread (in bps)



(b) Log(Maturity)

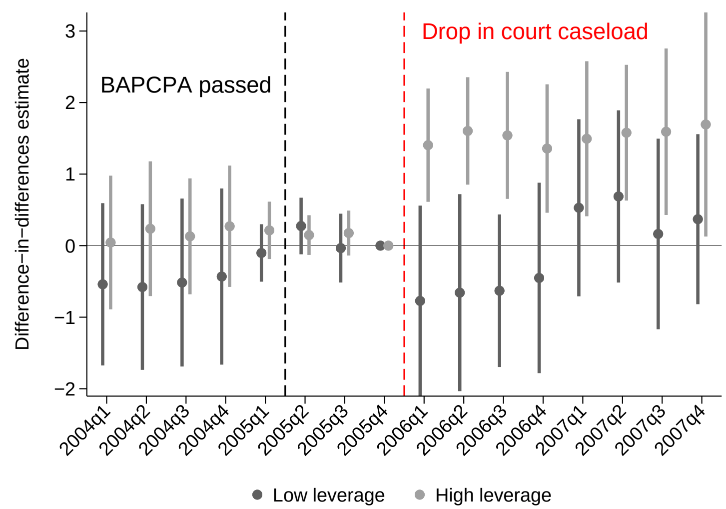


Table 1: Summary Statistics

	N	Mean	S.D.	p10	p50	p90
Borrower characteristics						
Book leverage	3805	0.38	0.25	0.08	0.34	0.74
Log(Total assets)	3805	7.04	1.60	4.94	7.05	9.15
ROA	3805	0.13	0.10	0.05	0.12	0.23
Negative debt-to-cash flow	3805	0.02	0.14	0	0	0
High debt-to-cash flow	3805	0.51	0.50	0	1	1
Sales growth	3805	0.56	22.65	-0.06	0.09	0.43
Rating dummy	3805	0.59	0.49	0	1	1
Tangibility	3805	0.31	0.24	0.05	0.25	0.68
Borrower-level reform exposure						
Refi need post-BAPCPA	3805	0.34	0.48	0	0	1
Credit rating (pre-reform)	2329	12.31	3.28	8.00	13.00	15.50
Log(Total assets) (pre-reform)	3716	7.07	1.60	5.01	7.08	9.18
Market leverage (pre-reform)	3320	0.32	0.23	0.06	0.26	0.65
Loan characteristics						
Interest rate spread	3805	212.36	150.80	50.00	175.00	375.00
Log(Maturity)	3750	3.89	0.56	3.18	4.09	4.36
Log(Loan size)	3805	5.00	1.52	2.96	5.19	6.82
Collateral dummy	3805	0.78	0.41	0	1	1
District characteristics						
Exposure	3805	0.78	0.12	0.70	0.81	0.88
Manufacturing emp. share	3801	0.10	0.05	0.05	0.09	0.16
Finance emp. share	3801	0.04	0.01	0.03	0.03	0.05
Income per capita	3801	30227	6163	23978	27894	41171
Log(Population density)	3801	5.89	1.34	4.41	5.71	7.70
Republican vote (2000)	3801	52.36	9.84	36.96	52.73	63.26
High school graduates/Pop.	3801	78.32	5.39	70.38	80.11	84.03
Asians/Pop.	3801	1.94	2.38	0.44	1.14	4.42
Hispanics/Pop.	3801	0.12	0.13	0.02	0.07	0.34
African-Americans/Pop.	3801	8.06	7.58	1.63	5.01	15.63
ΔReal estate emp. ('01-'06)	3801	12.46	9.20	2.78	9.60	28.65
ΔConstruction emp. ('01-'06)	3801	5.38	4.65	0.91	4.27	13.71
ΔHouse prices ('02-'06)	3801	142.07	29.78	114.52	130.99	187.88

Table 2: The Share of Non-Business Cases and the BAPCPA Caseload Drop

Notes: This table shows the effect of exposure to BAPCPA, as measured by the share of non-business cases in a bankruptcy district court's total weighted caseload, on the drop in caseload per judge. The dependent variable is the difference between the average caseload per judge in 2006-2007 and 2004-2005. *BAPCPA judge* is a dummy variable for the 20 districts where BAPCPA added at least one additional temporary judgeship. *Lagged caseload drop* is the difference between the average caseload per judge in 2004-2005 and 2002-2003. Robust standard errors are reported in parentheses. ***, ** and * indicate statistical significance at 1%, 5%, and 10% level, respectively.

Dependent variable:	BAPCPA Caseload Drop				
	Full sample			Drop DE/NY,S	Estimation sample
	(1)	(2)	(3)	(4)	(5)
Exposure	555.08**	621.59***	811.00***	1,182.10***	555.39**
	(260.84)	(179.32)	(223.49)	(313.40)	(276.98)
BAPCPA judge		222.67***			
		(54.05)			
Lagged caseload drop			0.47***		
			(0.10)		
Observations	89	89	89	87	80
R^2	0.09	0.28	0.25	0.19	0.09
Mean of dependent variable	529.51	529.51	529.51	529.47	535.23
S.D. of $Exposure$	0.115	0.115	0.115	0.080	0.116

Table 3: Comparing Bankruptcy Districts Before the Reform

Notes: This table presents reports coefficients from regressing each firm, loan, and county characteristic on *Exposure*, a bankruptcy district's share of non-business bankruptcy cases in 2003. I standardize all dependent variables and *Exposure* to have a mean of 0 and standard deviation of 1 to make coefficients comparable. Firm and loan characteristics are defined as of 2003 and are averaged on the district-level. County characteristics are as of 2003, except for the education and ethnic composition variables, which refer to 2000. The regressions in columns 2 and 4 include state fixed effects and are restricted to states with multiple bankruptcy districts. Standard errors reported in parentheses are clustered by district.

	Exposure *Across* States (1)	Exposure *Within* States (2)
Panel A: Borrower characteristics		
Book leverage	-0.09	0.16
	(0.12)	(0.18)
Log(Total assets)	-0.01	-0.16
	(0.08)	(0.15)
ROA	0.08	0.00
	(0.08)	(0.14)
Negative debt-to-cash flow	-0.07	0.04
	(0.08)	(0.13)
High debt-to-cash flow	-0.09	0.03
	(0.12)	(0.14)
Sales growth	0.13	-0.06
	(0.09)	(0.10)
Rating dummy	-0.07	-0.03
	(0.11)	(0.12)
Credit rating	-0.01	0.02
	(0.09)	(0.09)
Tangibility	0.00	0.17**
	(0.10)	(0.08)
Panel B: Loan characteristics		
Log(Loan size)	0.10	0.11
	(0.07)	(0.10)
Log(Maturity)	-0.10	0.06
	(0.12)	(0.15)
Interest rate spread	0.01	0.02
	(0.10)	(0.12)
Collateral dummy	-0.12	-0.04
	(0.11)	(0.17)

	Exposure *Across* States (3)	Exposure *Within* States (4)
Panel C: County characteristics		
Manufacturing employment share	0.26***	0.13**
	(0.07)	(0.05)
Finance employment share	-0.23***	-0.14**
	(0.07)	(0.06)
Income/capita ($\times$ 1000)	-0.39***	-0.39***
	(0.07)	(0.08)
Log(Population density)	-0.19***	-0.14
	(0.06)	(0.10)
Republican vote (2000)	0.36***	0.19***
	(0.09)	(0.07)
High school graduates/Pop.	-0.10	-0.01
	(0.07)	(0.05)
Asians/Pop.	-0.25**	-0.09
	(0.11)	(0.07)
Hispanics/Pop.	-0.27***	-0.14**
	(0.09)	(0.06)
African-Americans/Pop.	0.15*	0.02
	(0.09)	(0.06)
ΔReal estate emp. ('01-'06)	-0.01	-0.06
	(0.04)	(0.05)
ΔConstruction emp. ('01-'06)	-0.05	0.04
	(0.07)	(0.05)
ΔHouse prices ('02-'06)	-0.19**	-0.07
	(0.09)	(0.07)

Table 4: Court Congestion and Ex-Ante Loan Contract Terms

Notes: This table presents estimated coefficients from equation 1. The dependent variables are the interest rate spread (in basis points) or the natural logarithm of loan maturity. Post BAPCPA is 0 for the years 2004 and 2005; and 1 for the years 2006 and 2007. *Exposure* is a bankruptcy district's share of non-business bankruptcy cases in 2003. In columns 3 and 6, I restrict the sample to states that have multiple bankruptcy districts, which allows for the inclusion of $state \times year$ dummies. See text for description of control variables. Standard errors reported in parentheses are clustered by bankruptcy district. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% level, respectively.

	Interest rate spread (in bps)			Log(Loan Maturity)		
	(1)	(2)	(3)	(4)	(5)	(6)
Post BAPCPA × Exposure	-106.53***	-122.11***	-152.69***	0.62***	0.64***	0.67***
	(20.24)	(35.77)	(47.36)	(0.14)	(0.18)	(0.18)
Borrower and loan controls	Yes	Yes	Yes	Yes	Yes	Yes
Post BAPCPA × District controls		Yes			Yes	
Borrower, lender, year FE	Yes	Yes	Yes	Yes	Yes	Yes
State × Year FE			Yes			Yes
Observations	3,805	3,801	2,879	3,967	3,963	3,005
R^2	0.76	0.76	0.78	0.54	0.55	0.58
Mean of dependent variable	212.4	212.5	212.9	3.885	3.886	3.873
S.D. of $Exposure$	0.122	0.122	0.131	0.121	0.121	0.130

Table 5: Borrower Characteristics, Court Congestion, and Ex-Ante Loan Terms

Notes: This table presents estimated coefficients from regression 1, allowing for the interaction of the main treatment effect with cross-sectional firm and loan characteristics. The dependent variables are the interest rate spread (in basis points) or the natural logarithm of loan maturity. Post BAPCPA is 0 for the years 2004 and 2005; and 1 for the years 2006 and 2007. *Exposure* is a bankruptcy district's share of non-business bankruptcy cases in 2003. *Refi need* tags borrowers that issued a term loan prior to 2005 with a maturity date in 2006 or 2007. In column 6, *Interaction* is equal to *Log(Loan Maturity)*, which is why there are no estimates in panel B. Note that $Exposure \times Log(LoanMaturity)$ and $Log(LoanMaturity)$ by itself in column 6 are included but unreported. See text for description of control variables. Standard errors reported in parentheses are clustered by bankruptcy district. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% level, respectively.

		Interaction with pre-reform...				
	Baseline (1)	Market Leverage (2)	Credit Rating (3)	Log(Total Assets) (4)	Refi Need (5)	Log(Loan Maturity) (6)
Panel A: Interest rate spread (in bps)						
Post × Exposure	-106.53***	–	–	–	–	
	(20.24)					
Post × Exposure × Interaction		-579.99***	-35.23***	29.94**	-124.51*	135.66***
		(130.28)	(7.71)	(12.65)	(67.15)	(44.08)
Post × Interaction		335.64***	17.38***	-20.43**	58.64	-16.65
		(108.72)	(5.93)	(9.13)	(48.65)	(24.17)
Observations	3,805	3,233	2,249	3,627	3,716	2,421
R^2	0.76	0.80	0.80	0.79	0.79	0.80
Panel B: Log(Loan Maturity)						
Post × Exposure	0.62***	–	–	–	–	–
	(0.14)					
Post × Exposure × Interaction		3.84***	0.15***	-0.31***	0.86**	–
		(1.03)	(0.04)	(0.07)	(0.38)	
Post × Interaction		-3.05***	-0.13***	0.28***	-0.65**	–
		(0.81)	(0.03)	(0.05)	(0.27)	
Observations	3,967	3,365	2,341	3,784	3,703	–
R^2	0.54	0.60	0.60	0.60	0.59	–
Borrower FE	Yes	Yes	Yes	Yes	Yes	–
Lender × Year FE	Yes	Yes	Yes	Yes	Yes	–
District × Year FE		Yes	Yes	Yes	Yes	–
Borrower × Lender × Year FE						Yes
Borrower and loan controls	Yes	Yes	Yes	Yes	Yes	Yes
S.D. of $Interaction$ term		0.236	3.289	1.601	0.475	0.547

Table 6: Alternative Explanations

Notes: This table presents estimated coefficients from regression 1. The dependent variables are the interest rate spread (in basis points) or the natural logarithm of loan maturity. Post BAPCPA is 0 for the years 2004 and 2005; and 1 for the years 2006 and 2007. *Exposure* is a bankruptcy district's share of non-business bankruptcy cases in 2003. Column 2 drops borrowers in the construction and nontradable industries, as in Mian & Sufi (2014). Column 3 drops retailers (SIC codes starting with 5). Column 4 controls for the share of non-business bankruptcies filed as Chapter 13 in 2003 (as in Chakrabarti & Pattison (2019)). Column 5 rops the largest 10% of firms (in pre-reform assets). Column 6 drops the states CA, NJ, PA, IL, and FL. Column 7 drops borrowers in industries where forum shopping is most common (see 5.3). Column 8 drops CLO loans, defined similar to Benmelech et al. (2012). Standard errors reported in parentheses are clustered by district and year. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% level, respectively.

	Baseline (1)	Exclude construction and nontradables (2)	Exclude retailers (3)	Cramdown control (4)	Exclude largest 10% (5)	Exclude forum shopping victim states (6)	Exclude forum shopping victim industries (7)	Exclude potential CLO loans (8)
Panel A: Interest rate spread (in bps)								
Post BAPCPA × Exposure	-106.53***	-98.76***	-89.87***	-97.78***	-110.11***	-113.65***	-119.79***	-98.14**
	(20.24)	(21.83)	(20.46)	(20.43)	(21.81)	(20.16)	(30.26)	(42.48)
Observations	3,805	3,372	3,196	3,805	3,366	2,819	2,639	1,573
R^2	0.76	0.76	0.76	0.76	0.75	0.76	0.78	0.81
Panel B: Log(Loan Maturity)								
Post BAPCPA × Exposure	0.62***	0.66***	0.57***	0.49***	0.64***	0.64***	0.85***	0.63***
	(0.14)	(0.14)	(0.18)	(0.14)	(0.18)	(0.14)	(0.21)	(0.23)
Observations	3,967	3,501	3,324	3,967	3,509	2,941	2,784	1,671
R^2	0.54	0.55	0.54	0.55	0.56	0.54	0.54	0.50
Borrower, lender, year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Borrower and loan controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Table 7: Court Backlog, Risk, and Leverage

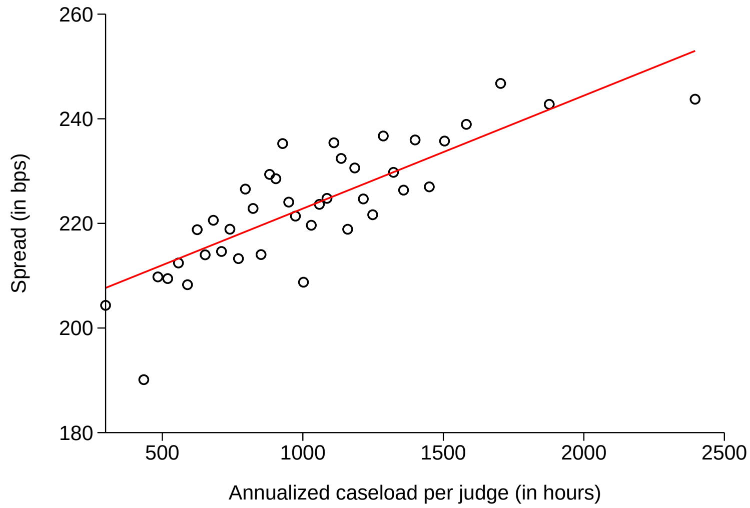
Notes: This table presents estimated coefficients of regressing measures of credit ratings and leverage on exposure to a drop in bankruptcy court caseload. The dependent variable are listed in the panel header. See text for description of other variables. Columns 1 and 3 only control for the natural logarithm of total assets; columns 2 and 4 add the other control variables described above. In columns 1 and 2 the dependent variables are in levels; in columns 3 and 4 they are in first differences. Standard errors reported in parentheses are clustered by bankruptcy district. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% level, respectively.

	(1)	(2)	(3)	(4)
Panel A: Numerical credit ratings				
	Level		First difference	
Post BAPCPA × Exposure	0.24	-0.00	-0.09	-0.19
	(0.44)	(0.35)	(0.25)	(0.24)
Observations	2,186	2,171	1,943	1,936
R^2	0.94	0.95	0.36	0.37
Mean of dependent variable	11.80	11.78	0.09	0.09
Panel B: $\mathbb{1}$(Firm has rating)				
	Level		First difference	
Post BAPCPA × Exposure	0.07	0.02	-0.00	0.04
	(0.05)	(0.03)	(0.05)	(0.03)
Observations	3,943	3,848	3,763	3,676
R^2	0.91	0.93	0.27	0.59
Mean of dependent variable	0.56	0.57	0.01	0.01
Panel C: Book leverage				
	Level		First difference	
Post BAPCPA × Exposure	0.05**	0.03*	0.04**	0.04**
	(0.02)	(0.02)	(0.02)	(0.02)
Observations	3,928	3,830	3,745	3,660
R^2	0.87	0.89	0.26	0.57
Mean of dependent variable	0.33	0.33	-0.00	-0.00
S.D. of $Exposure$	0.12	0.12	0.12	0.12
Borrower FE	Yes	Yes	Yes	Yes
Size control	Yes	Yes	Yes	Yes
Other borrower controls		Yes		Yes

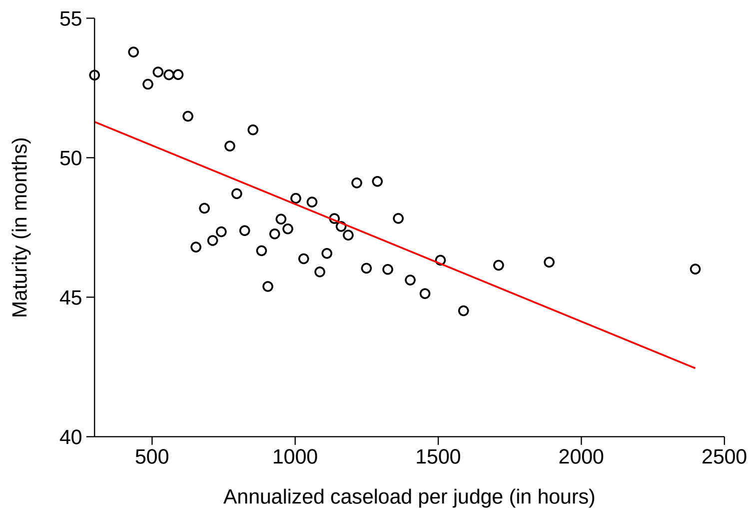
Internet Appendix

Figure A1: Caseload per Judge and Loan Terms – Binned Scatter Plots

s **(a) Interest rate spread (in bps)**

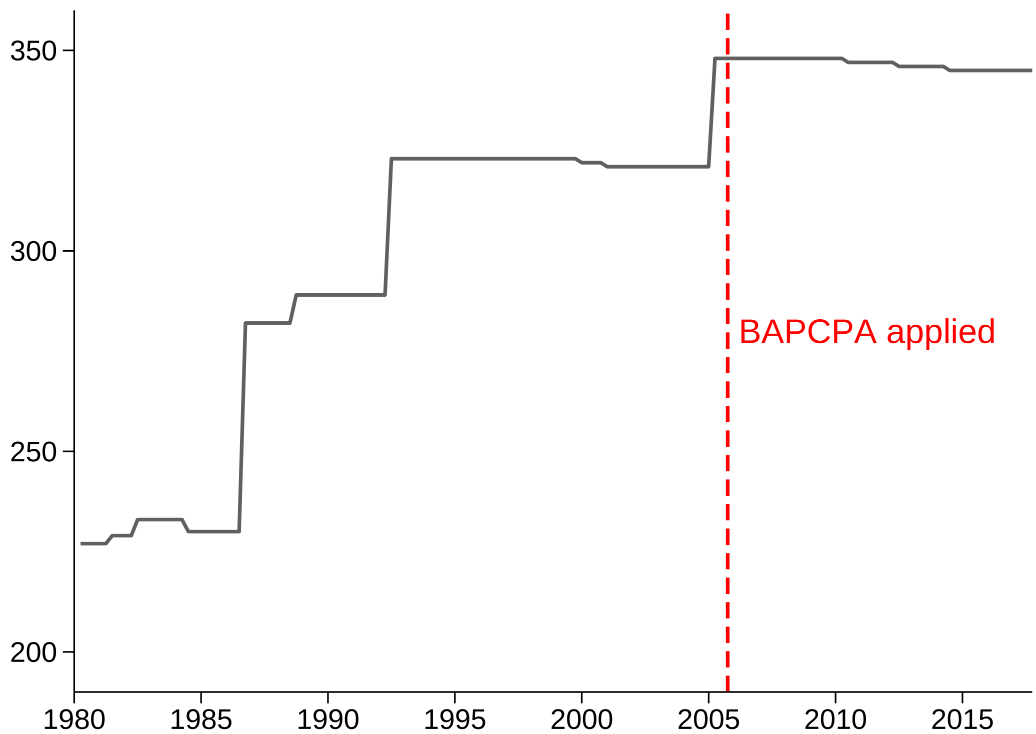


(b) Log(Loan Maturity)



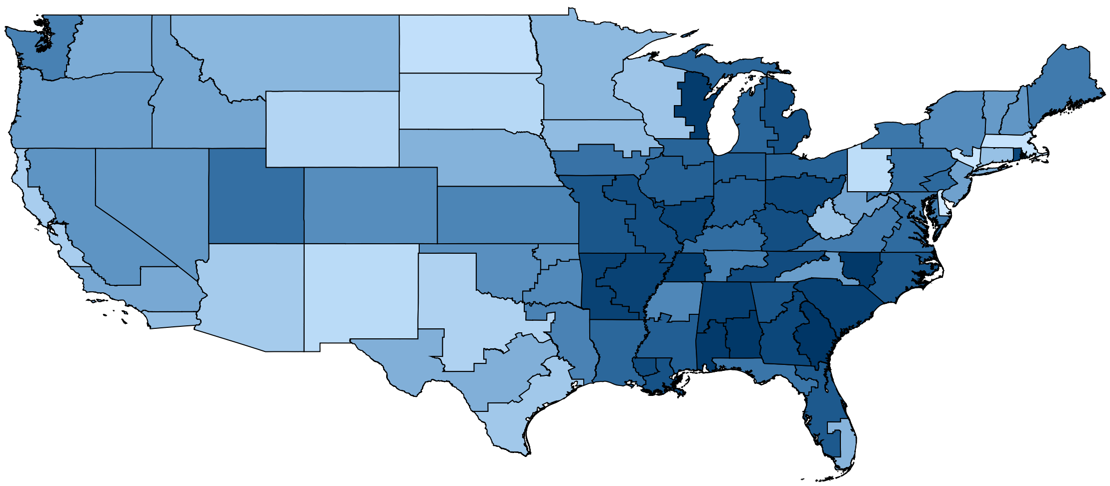
Notes: These figures show binned scatter plots where the interest rate spread and loan maturity are grouped into 20 quantiles of the caseload per judge. Data on the caseload per judge are from the United States Court System, matched to borrowers using headquarter location data from Compustat.

Figure A2: The Number of Bankruptcy Judges, 1990-2017



Source: United States Court System.

Figure A3: The Share of Nonbusiness Cases in Total Caseload Across Districts



Notes: Figure A3 plots the share of nonbusiness cases in total caseload ($Exposure$) across districts. Darker shades of blue reflect higher values of $Exposure$.

Figure A4: Court Caseload and Firm Bankruptcy Case Length

Notes: Figure A4 shows the estimated effect of an exogenous drop in bankruptcy court caseload on the length of business bankrupcty cases using data from the Federal Judicial Center. The sample includes 15,830 firm bankruptcy cases where the debtor is a corporation and the debt predominantly business-related. I plot the estimated β_t coefficients of the following regression:

$$Log(Bankruptcy\ case\ length)_i = \sum_{t=2004}^{2007} \beta_t D_t \times Exposure_d + \gamma X_i + \alpha_t + \alpha_d + \varepsilon_i$$

The excluded group is 2005. i, d, and t refer to bankruptcy cases, districts, and years, respectively. 95% confidence intervals are based on standard errors clustered by bankruptcy district.

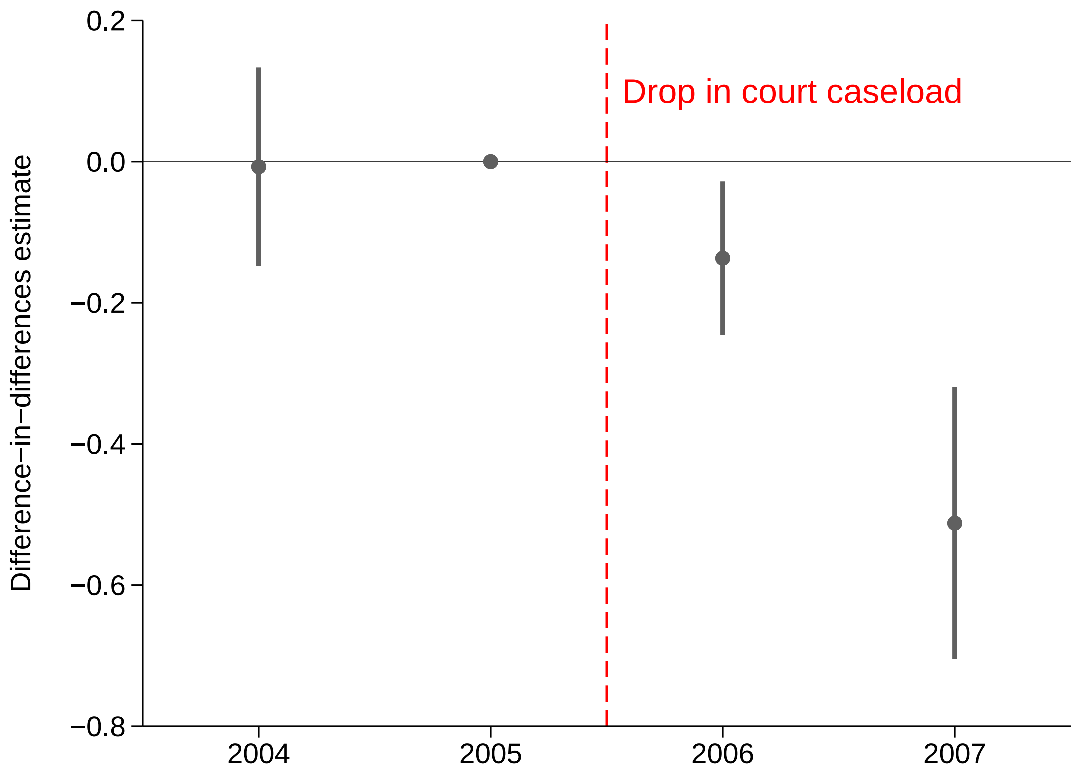
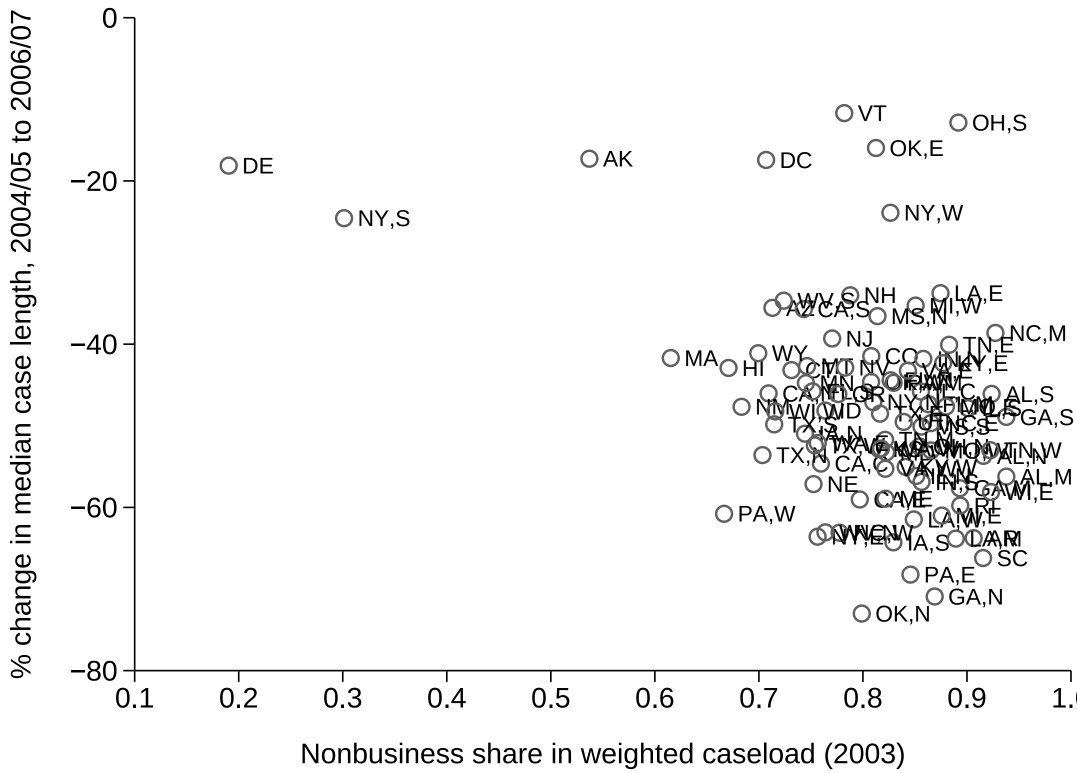


Figure A5: District Exposure and Change Median Length of Firm Bankruptcies



Notes: This figure plots the percentage change in the median number of days a firm bankruptcy case spends in court between 2004-2005 and 2006-2007 against the share of non-business cases in the weighted caseload of a bankruptcy district in 2003. Data on bankruptcy case length are based on a sample of 19,902 firm cases with primarily business debts from the Federal Judicial Center.

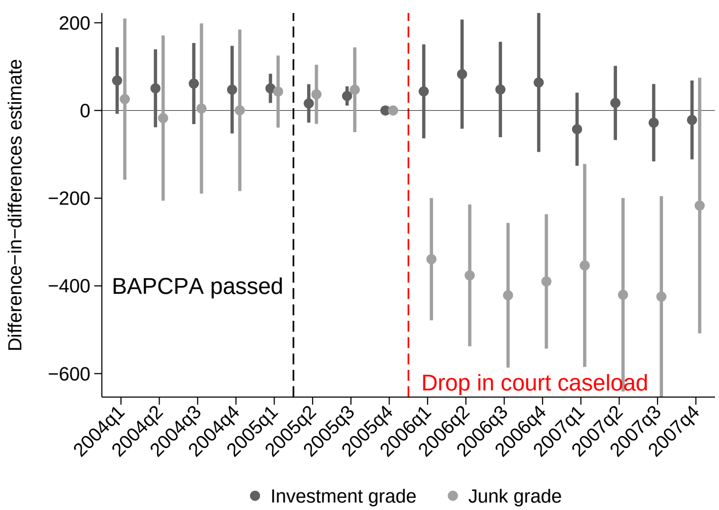
Figure A6: Court Caseload and Loan Terms - Split by Pre-Reform Credit Rating

Notes: Figure A6 shows the effect of an exogenous drop in bankruptcy court caseload on interest rate spreads and loan maturities. Panels (a) and (b) plot the estimated β_t coefficients of the following regression:

$$Y_{jidt} = \sum_{t=2004q1}^{2007q4} \beta_t D_t \times Exposure_d + \gamma X_{jidt} + \alpha_t + \alpha_i + \varepsilon_{jidt}$$

The excluded group is the last quarter of 2005. "Junk grade" refers to firm-year observations with a credit rating of B+ or worse in 2004-2005; "Investment grade" to those with a rating of BBB- or better. 95% confidence intervals are based on standard errors clustered by bankruptcy district.

(a) Interest rate spread (in bps)



(b) Log(Maturity)

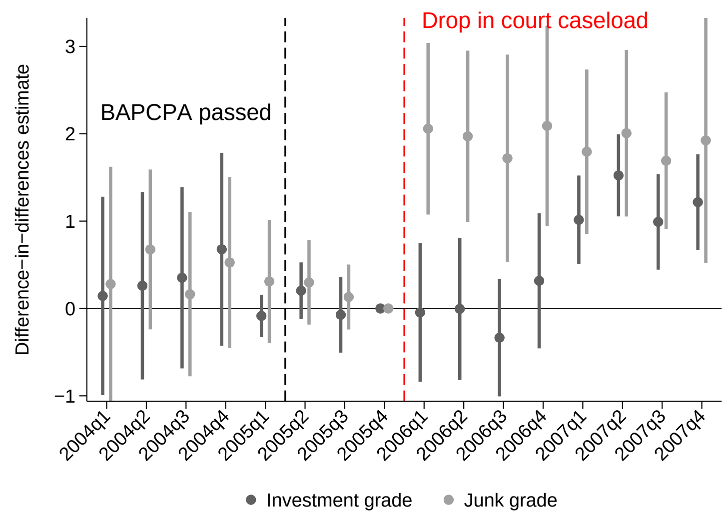


Figure A7: Court Caseload and Interest Rate Spreads - Split by Maturity

Notes: Figure A7 shows the effect of an exogenous drop in bankruptcy court caseload on interest rate spreads, split by loan maturity. It plots the estimated β_t coefficients of the following regression:

$$Y_{jidt} = \sum_{t=2004q1}^{2007q4} \beta_t D_t \times Exposure_d + \gamma X_{jidt} + \alpha_t + \alpha_b + \alpha_i + \varepsilon_{jidt}$$

The excluded group is the last quarter of 2005. "Short maturity" refers to loans with a maturity of less than 36 months (the 33rd percentile in the estimation sample); "Long maturity" refers to all other loans. 95% confidence intervals are based on standard errors clustered by bankruptcy district.

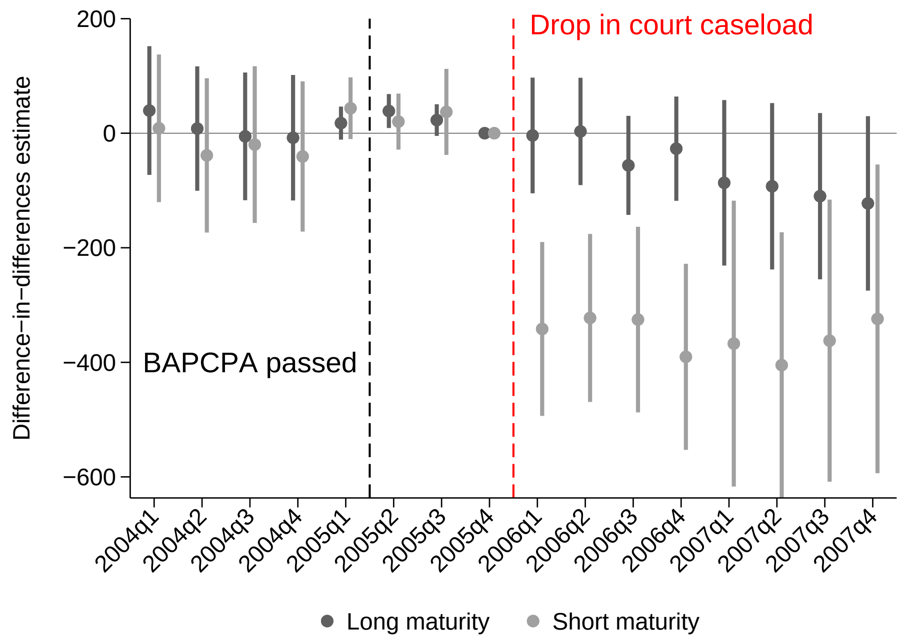
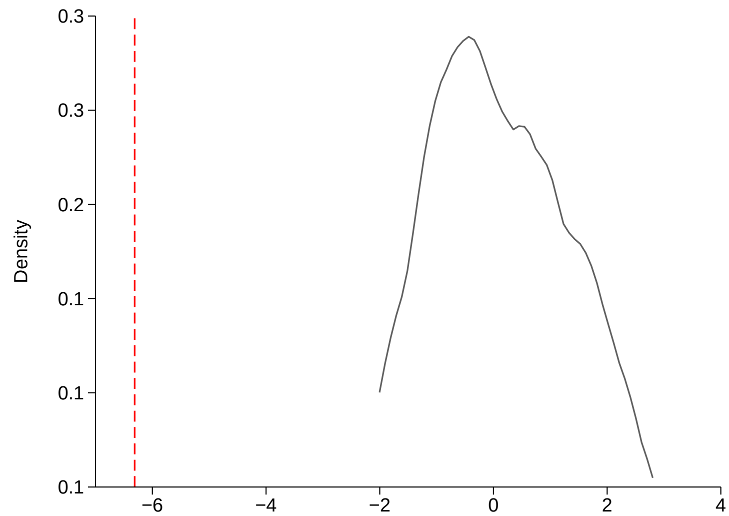


Figure A8: Placebo Estimates - Exposure in Other Periods and Firm Financing Terms

Notes: Figure A8 plots kernel density estimates of t-statistics based on placebo regressions. I construct placebo $Exposure$ variables as the share of non-business cases in a bankruptcy district's weighted caseload for each year between 1994 and 2009. I also construct placebo $Post\ BAPCPA$ dummies akin to those in equation 1. For example, if $Exposure$ is measured in 1994, the $Post\ BAPCPA$ dummy takes on a value of 0 for 1995-1996 and 1 for 1997-1998. I then run the regression in equation 1 using these placebo values and plot the resulting t-statistics. The red dotted line is the point estimate of the baseline specification where $Exposure$ is measured in 2003 and $Post\ BAPCPA$ tags the years 2004-2005 compared to 2006-2007.

(a) Interest rate spread



(b) Log(Maturity)

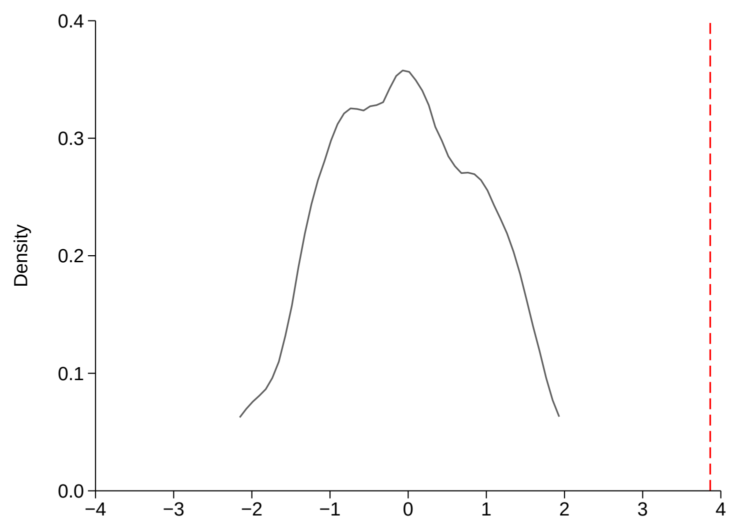


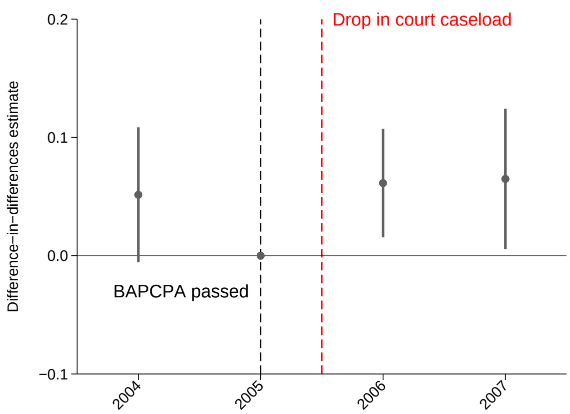
Figure A9: Court Caseload, Borrower Risk, and Leverage

Notes: Figure A9 shows the effect of an exogenous drop in bankruptcy court caseload on firm leverage and credit risk. They plot the estimated β_t coefficients of the annual firm-level regression:

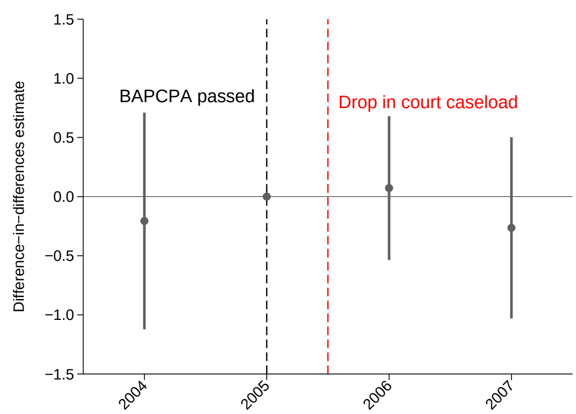
$$Y_{it} = \sum_{t=2004}^{2007} \beta_t D_t \times Exposure_d + \gamma X_{it} + \alpha_{jt} + \alpha_i + \varepsilon_{it}$$

α_{jt} are industry-year dummies. The excluded group is 2005. In figure (a), X_{it} only includes lagged total assets and a district-specific linear trend; in figures (b) and (c), I include the full vector of borrower controls (see main text). 95% confidence intervals are based on standard errors clustered by bankruptcy district.

(a) Leverage



(b) Credit Ratings



(c) Firm has rating

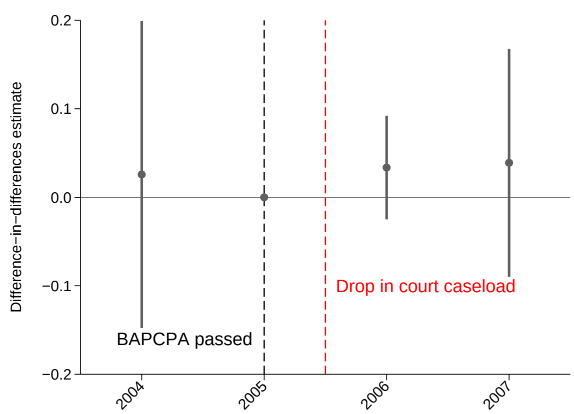


Table A1: Bankruptcy Case Weights

This table shows the average number of hours spent on bankruptcy cases of a particular type, building on the findings of Bermant et al. (1991).

Type of case	Case weight in hours
Chapter 7 Business	0.397
Chapter 7 Consumer	0.101
Chapter 11	7.559
Chapter 12	4.040
Chapter 13	0.381
Other cases	0.194

Table A2: Variable Definitions

Variable	Description
Borrower characteristics (Compustat)	
Book leverage	[Long-term debt (dltt) + debt in current liabilities (dlc)] / Total assets (at).
Log(Total assets)	Natural logarithm of total assets (at).
ROA	Operating income before depreciation (oibdp) / Total assets (at).
Negative debt-to-cash flow	[Long-term debt (dltt) + debt in current liabilities (dlc)] / [Operating income before depreciation (oibdp) + Depreciation and amortization (dp). Equal to 1 for negative values.
High debt-to-cash flow	[Long-term debt (dltt) + debt in current liabilities (dlc)] / [Operating income before depreciation (oibdp) + Depreciation and amortization (dp). Equal to 1 for the top quartile.
Sales growth	Growth in sales/turnover (net) [(sale - sale (t - 1)) / sale (t - 1)].
Rating dummy	Equal to 1 if a firm has any rating from Standard & Poors, Fitch, Moody's, or Duffs & Phelps.
Tangibility	[Property, plant and equipment (ppent) / Total assets (at).
Pre-reform borrower characteristics (Compustat/DealScan, average for 2004 and 2005)	
Refinancing need	Dummy variable equal to 1 for borrowers with a term loan with an issuance date prior to 2005 and a maturity date in 2006 or 2007.
Firm rating	Numerical credit rating, ranging from AAA to D.
Log(Total assets)	Natural logarithm of total assets (at).
Market leverage	[Long-term debt (dltt) + debt in current liabilities (dlc)] / [Market value of capital (csho $\times$ prcc_c + dlc + dltt)].
Loan characteristics (DealScan)	
Interest rate spread	Interest rate spread, usually over LIBOR, in basis points.
Log(Maturity)	Natural logarithm of loan maturity in months.
Log(Loan size)	Natural logarithm of facility amount in million USD.
Collateral dummy	Equal to 1 if loan is backed by collateral.
Bankruptcy district characteristics (U.S. Courts/BEA/U.S. Census)	
Exposure	Share of non-business cases in total weighted caseload (2003).
Manufacturing emp. share	Share of employees in manufacturing (2003, BEA).
Finance emp. share	Share of employees in finance (2003, BEA).
Income per capita	Income per capita in 2003 (BEA).
Population density	Population scaled over county area (Census).
Republican vote (2000)	Republican share in total votes (Census).
High school graduates/Pop.	Share of population with a high school degree or higher (Census).
Asians/Pop.	Asian population share (Census).
Hispanics/Pop.	Hispanic population share (Census).
African-Americans/Pop.	African-American population share (Census).
ΔReal estate emp. ('01-'06)	Percentage growth in real estate employees (BEA).
ΔConstruction emp. ('01-'06)	Percentage growth in construction employees (BEA).
ΔHouse prices ('02-'06)	Percentage growth in Federal Housing Finance Agency's all-transactions house price index from 2002 to 2006. I add house prices in the order 5-digit zip code, 3-digit zip code, county, state.

Table A3: Court Caseload and Bankruptcy Case Length

This table shows results on the effect of court caseload on business bankruptcy outcomes based on data from the Federal Judicial Center. The regression specification is

$$Log(Bankruptcy\ case\ length)_i = \alpha_d + \alpha_t + \beta Post\ BAPCPA_t \times Exposure_d + \gamma X_i + \varepsilon_i,$$

where i, d, and t index bankruptcy cases, districts, and years, respectively. Bankruptcy case length is defined as the number of days between the original bankruptcy filing and the closing date. $Post_t$ is a dummy equal to 1 for the years 2006 and 2007, and 0 for 2004 and 2005. $Exposure_d$ is the share of the weighted non-business bankruptcy caseload in the total district caseload in 2003. The case-level control variables X_i refer to total assets, total debt, and monthly income at the time of the bankruptcy filing. The sample period is 2004 to 2007. Standard errors reported in parentheses are clustered by bankruptcy district.

	Baseline (1)	Add controls (2)	Drop DE/NY,S (3)
Post BAPCPA $\times$ Exposure	-0.28***	-0.33***	-0.72**
	(0.09)	(0.08)	(0.30)
District FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
Case controls		Yes	Yes
Observations	19,902	15,830	14,268
R^2	0.28	0.35	0.35
Mean of dependent variable	6.863	6.790	6.751

Table A4: Court Caseload and Chapter 7 Case Outcomes

This table shows results on the effect of court caseload on Chapter 7 case outcomes reported to the U.S. Trustee Program. The regression specification is

$$Y_i = \alpha_s + \alpha_t + \beta Post\ BAPCPA_t \times Exposure_s + \gamma X_i + \varepsilon_i,$$

where i, s, and t index bankruptcy cases, states, and year-months, respectively . $Post_t$ is a dummy equal to 1 for the years 2006 and 2007, and 0 for 2004 and 2005. $Exposure_s$ is the share of the weighted non-business bankruptcy caseload in the total district caseload in 2003. Because the UST office regions and bankruptcy districts cannot be clearly assigned to each other, the sample is restricted to the 27 states with a single bankruptcy district. The case-level control variables X_i are the log number of days a case took and the logarithm of the total amount of gross receipts. The sample period is 2004 to 2007. Standard errors reported in parentheses are clustered by state.

	Log(Total Fees) (1)	Log(Court Costs) (2)	Court Costs/ Total Fees (3)	Net Receipts/ Fees (4)
Post BAPCPA × Exposure	-0.27*	-1.73*	-0.07	1.06***
	(0.15)	(0.90)	(0.05)	(0.26)
Log(Days)	0.31***	0.80***	0.02***	-0.76***
	(0.02)	(0.08)	(0.00)	(0.06)
Log(Gross Receipts)	0.88***	0.10***	-0.01***	0.40***
	(0.01)	(0.02)	(0.00)	(0.04)
State FE	Yes	Yes	Yes	Yes
Year-Month FE	Yes	Yes	Yes	Yes
Observations	88,521	88,521	88,441	88,441
R^2	0.88	0.09	0.08	0.21

Table A5: Court Caseload and Firms' Recovery Values in Bankruptcy

This table shows the effect of court congestion on ex-post firms recovery values during the Great Recession, based on data from Moody's Default and Recovery database. The regression specification is

$$Y_i = \alpha_d + \alpha_t + \beta Crisis_t \times Exposure_d + \gamma X_i + \varepsilon_i,$$

where i, d, and t index bankruptcy cases, districts, and years, respectively . $Crisis_t$ is a dummy equal to 1 for the years 2008 and 2009 (NBER recessions), and 0 for the years 2004 through 2007. $Exposure_d$ is the share of the weighted non-business bankruptcy caseload in the total district caseload in 2003. X_i can include the size of the default claims (in natural logarithm) or 1-digit SIC dummies interacted with year dummies. The sample period is 2004 to 2020. Standard errors reported in parentheses are clustered by bankruptcy district.

	(1)	(2)	(3)
Crisis × Exposure	54.16**	51.20**	81.30**
	(20.95)	(20.65)	(35.91)
Log(Default amount)		1.26*	0.81
		(0.71)	(0.69)
District FE	Yes	Yes	Yes
Year FE	Yes	Yes	—
Industry × Year FE			Yes
Observations	1,045	1,045	964
R^2	0.46	0.46	0.63
Mean of dependent variable	32.86	32.86	31.64

Table A6: Robustness - Reform and Treatment Timing

Notes: This table presents estimated coefficients from regression 1. The dependent variables are the interest rate spread (in basis points) or the natural logarithm of loan maturity. In columns 1 and 6, $Post\ BAPCPA$ is 0 for the years 2004 and 2005; and 1 for the years 2006 and 2007. Columns 2 through 5 tweak it as described in the top rows. *Exposure* is a bankruptcy district's share of non-business bankruptcy cases in 2003. In column 6, I control for the interaction of $Exposure$ with key dates of BAPCPA implementation: *Senate Intro.* is 1 for the period after February 1, 2005, and 0 otherwise; *Senate Pass* is 1 for the period after March 10, 2005, and 0 otherwise; *Applied* is 1 for the period after October 17, 2005, and 0 otherwise. See text for description of other variables. Standard errors reported in parentheses are clustered by bankruptcy district. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% level, respectively.

	Baseline (1)	Exclude 2007 (2)	Exclude 2005 (3)	Post until 2017 (4)	Pre from 2002 (5)	Legislative steps control (6)
Panel A: Interest rate spread (in bps)						
Post BAPCPA × Exposure	-106.53***	-75.21***	-75.59***	-71.65***	-65.95***	-78.64***
	(20.24)	(25.49)	(24.75)	(22.84)	(22.43)	(25.35)
Observations	3,805	2,899	2,598	5,884	6,110	3,805
R^2	0.76	0.77	0.76	0.74	0.75	0.76
Panel B: Log(Loan Maturity)						
Post BAPCPA × Exposure	0.62***	0.42***	0.57**	0.31***	0.28**	0.74***
	(0.14)	(0.12)	(0.23)	(0.11)	(0.12)	(0.17)
Observations	3,967	3,018	2,709	6,200	6,418	3,967
R^2	0.54	0.60	0.57	0.52	0.56	0.55
Borrower, lender, year FE	Yes	Yes	Yes	Yes	Yes	Yes
Borrower and loan controls	Yes	Yes	Yes	Yes	Yes	Yes

Table A7: Robustness - Different Exposure Measures and Collapsed Data

Notes: This table presents estimated coefficients from regression 1. The dependent variables are the interest rate spread (in basis points) or the natural logarithm of loan maturity. $Post\ BAPCPA$ is 0 for the years 2004 and 2005; and 1 for the years 2006 and 2007. *Exposure* is a bankruptcy district's share of non-business bankruptcy cases in 2003 (except in columns 6 and 7, where it is defined as of 2002 and 2004, respectively). In column 2, I control for the interaction of the number of judges BAPCPA added to 14 districts with the $Post\ BAPCPA$ dummy; I drop these districts entirely in column 3. Column 4 "winsorizes" the $Exposure$ of the Southern District of New York and Delaware to the value of Alaska, as in Iverson (2018). I drop these districts in column 5, where I also add interacted county controls. Columns 6 and 7 define $Exposure$ as of 2002 and 2004, respectively. Column 8 drops all controls but a dummy for secured loans. In column 9, I collapse the time series by taking the difference between a firm's average interest rate or maturity in the before and after period, and then regressing it on the *Exposure* variable as well as the pre-post difference in all control variables. See text for description of other variables. Standard errors reported in parentheses are clustered by bankruptcy district. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% level, respectively.

	Baseline (1)	BAPCPA judges control (2)	BAPCPA judges dropped (3)	Winsorize DE and NY,S (4)	Drop DE and NY,S (5)	Exposure in 2002 (6)	Exposure in 2004 (7)	No controls (8)	Collapsed first difference (9)
Panel A: Interest rate spread (in bps)									
Post BAPCPA × Exposure	-106.53***	-107.63***	-113.72**	-138.16***	-104.47**	-88.48***	-100.12***	-50.64**	-116.21***
	(20.24)	(19.04)	(48.67)	(38.13)	(46.53)	(17.77)	(34.78)	(25.77)	(31.50)
Observations	3,805	3,805	2,990	3,805	3,651	3,805	3,805	4,134	302
R^2	0.76	0.76	0.76	0.76	0.76	0.76	0.76	0.73	0.16
Panel B: Log(Loan Maturity)									
Post BAPCPA × Exposure	0.62***	0.63***	0.46**	0.73**	0.62**	0.49***	0.63**	0.47***	0.27**
	(0.14)	(0.14)	(0.22)	(0.29)	(0.29)	(0.15)	(0.25)	(0.10)	(0.10)
Observations	3,967	3,967	3,136	3,967	3,529	3,967	3,967	4,312	302
R^2	0.54	0.54	0.57	0.54	0.55	0.54	0.54	0.53	0.10
Borrower and loan controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Borrower FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Implicit
Lender FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	–
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	–

Table A8: Robustness - Fixed Effects, Time Trends, and Additional Tests

Notes: This table presents estimated coefficients from regression 1. The dependent variables are the interest rate spread (in basis points) or the natural logarithm of loan maturity. $Post\ BAPCPA$ is 0 for the years 2004 and 2005; and 1 for the years 2006 and 2007. *Exposure* is a bankruptcy district's share of non-business bankruptcy cases in 2003. I include additional fixed effects or time trends as indicated in the top rows. In column 7, I replace the lagged borrower controls with their values in 2003, interacted with the $Post\ BAPCPA$ dummy. In column 8, I drop loans issued by firms that went bankrupt during the sample period. Standard errors reported in parentheses are clustered by bankruptcy district. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% level, respectively.

	Baseline (1)	Lender × year FE (2)	Lender × borrower FE (3)	SIC × year FE (4)	Linear district trends (5)	Quadratic district trends (6)	Interacted pre-period controls (7)	Drop bankrupt firms (8)
Panel A: Interest rate spread (in bps)								
Post BAPCPA × Exposure	-106.53***	-99.69***	-71.76***	-95.16***	-123.04***	-117.03***	-77.23***	-104.21***
	(20.24)	(22.14)	(23.41)	(28.32)	(19.65)	(20.46)	(20.72)	(21.05)
	(20.24)	(22.14)	(23.41)	(28.32)	(20.57)	(21.11)	(20.72)	(21.05)
Observations	3,805	3,758	1,986	3,789	3,805	3,805	3,807	3,717
R^2	0.76	0.78	0.77	0.78	0.76	0.77	0.74	0.76
Panel B: Log(Loan Maturity)								
Post BAPCPA × Exposure	0.62***	0.61***	0.79***	0.56***	0.62***	0.59***	0.47***	0.61***
	(0.14)	(0.16)	(0.17)	(0.15)	(0.16)	(0.17)	(0.10)	(0.14)
Observations	3,967	3,910	2,046	3,950	3,967	3,967	3,980	3,871
R^2	0.54	0.57	0.55	0.58	0.56	0.57	0.53	0.55
Borrower and loan controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Borrower FE	Yes	Yes	–	Yes	Yes	Yes	Yes	Yes
Lender FE	Yes	–	–	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	–	Yes	–	Yes	Yes	Yes	Yes

Table A9: Robustness - Weighted Least Squares

Notes: This table presents estimated coefficients from regression 1. The dependent variables are the interest rate spread (in basis points) or the natural logarithm of loan maturity. $Post\ BAPCPA$ is 0 for the years 2004 and 2005; and 1 for the years 2006 and 2007. *Exposure* is a bankruptcy district's share of non-business bankruptcy cases in 2003. I apply regression weights as described in the top rows. In column 3, number of firms refers to the number of in-sample firms in each district. In column 4, I weight by the total number of Chapter 11 bankruptcy cases in a district. See text for description of other variables. Standard errors reported in parentheses are clustered by bankruptcy district. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% level, respectively.

	Baseline (1)	Population weights (2)	Number of firms (3)	Number busi. cases (4)
Panel A: Interest rate spread (in bps)				
Post BAPCPA × Exposure	-106.53***	-112.99***	-120.67***	-129.42***
	(20.24)	(24.20)	(22.72)	(36.70)
Observations	3,805	3,801	3,772	3,805
R^2	0.76	0.75	0.76	0.76
Panel B: Log(Loan Maturity)				
Post BAPCPA × Exposure	0.62***	0.65***	0.72***	0.72***
	(0.14)	(0.16)	(0.12)	(0.15)
Observations	3,967	3,963	3,928	3,967
R^2	0.54	0.55	0.56	0.59
Borrower and loan controls	Yes	Yes	Yes	Yes
Borrower, lender, year FE	Yes	Yes	Yes	Yes

Table A10: Robustness - Alternative Standard Errors

Notes: This table presents estimated coefficients from regression 1. The dependent variables are the interest rate spread (in basis points) or the natural logarithm of loan maturity. $Post\ BAPCPA$ is 0 for the years 2004 and 2005; and 1 for the years 2006 and 2007. *Exposure* is a bankruptcy district's share of non-business bankruptcy cases in 2003. See text for description of other variables. Standard errors reported in parentheses are constructed as indicated, with clustering by bankruptcy district being the baseline.

	Interest rate spread (1)	Log(Loan maturity) (2)
Post BAPCPA $\times$ Exposure	-106.53	0.62
Standard errors		
Heteroskedasticity-robust	(35.86)	(0.22)
Cluster: District	(20.24)	(0.14)
Cluster: District + Year-Quarter	(24.15)	(0.10)
Cluster: State	(18.95)	(0.13)
Cluster: State + Year-Quarter	(23.36)	(0.09)
Cluster: Borrower	(33.26)	(0.27)
Cluster: Borrower + Year-Quarter	(34.43)	(0.20)
Cluster: Lender	(43.65)	(0.26)
Cluster: Lender + Year-Quarter	(41.56)	(0.18)
Borrower and loan controls	Yes	Yes
Borrower, lender, year FE	Yes	Yes

Table A11: Exposure to BAPCPA Does Not Capture Differences in Realized Risk

Notes: This table presents the estimated coefficients of panel regressions on the district-year level. The dependent variable is the natural logarithm of the number of bankruptcy cases in a district (with 1 added inside), where bankruptcies refer to all firm cases in column 1 and 2 and Chapter 11 cases in column 3 and 4. $Post\ BAPCPA$ is 0 in 2004 and 2005; and 1 in 2006 and 2007. $Crisis$ is 0 in 2004-2007; and 1 in 2008-2009 (NBER recessions). All regressions include district and year fixed effects. Standard errors reported in parentheses are clustered by district.

	Log(Firm bankruptcies)		Log(Chapter 11 bankruptcies)	
	(1)	(2)	(3)	(4)
Post BAPCPA × Exposure	0.48		0.08	
	(0.50)		(0.59)	
Crisis × Exposure		-0.02		-0.54
		(0.35)		(0.49)
District FE	Yes	Yes	Yes	Yes
Year-Quarter FE	Yes	Yes	Yes	Yes
Observations	1,424	1,424	2,136	2,136
R^2	0.89	0.82	0.89	0.84
Mean of dependent variable	4.012	2.294	4.185	2.443

Table A12: Court Caseload and Other Loan Terms

Notes: This table presents estimated coefficients from regression 1. The dependent variable is listed in the top row. $Post\ BAPCPA$ is 0 for the years 2004 and 2005; and 1 for the years 2006 and 2007. *Exposure* is a bankruptcy district's share of non-business bankruptcy cases in 2003. Standard errors reported in parentheses are clustered by bankruptcy district. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% level, respectively.

	Total cost of borrowing (1)	Log(Loan size) (2)	Collateral dummy (3)	CLO dummy (4)	Term loan dummy (5)	Credit line dummy (6)
Post BAPCPA × Exposure	-103.50***	-0.04	0.12	-0.04	0.06	0.02
	(32.37)	(0.19)	(0.08)	(0.06)	(0.06)	(0.06)
Observations	2,521	4,047	4,047	4,047	4,047	4,047
R^2	0.70	0.79	0.85	0.39	0.36	0.36
Borrower and loan controls	Yes	Yes	Yes	Yes	Yes	Yes
Borrower, lender, year FE	Yes	Yes	Yes	Yes	Yes	Yes

A A Simple Conceptual Framework

How should we think about the role of bankruptcy courts in firms' ex-ante financing terms? To gain intuition, I describe a simple conceptual framework in which a borrowing firm's interest rate depends on the congestion of bankruptcy courts based on Hart & Moore (1994) and Hart & Moore (1998). The analysis is kept purposefully stylized to focus on the key prediction: a decrease in court congestion leads to lower interest rates, particularly for firms with higher default risk.

Consider a setting with two periods $t = 1, 2$. There is an entrepreneur with no initial endowment who needs funding to invest in a project normalized to have unit size $K = 1$. In $t = 1$, the entrepreneur can borrow from a creditor at an interest rate $R = 1 + r$. In $t = 2$, the project is undertaken and yields cash flows c that can be either high (h) or low (l). High cash flows are denoted by $\bar{c} = c(h)$, low cash flows by $\underline{c} = c(l)$. Cash flows are high with probability p and low with probability $(1 - p)$, with $1 > p > 0$. I assume that both the entrepreneur and the creditor are risk neutral and have symmetric information.

If the project yields high cash flows, creditors are paid back R. If the project yields low cash flows, the firm enters a bankruptcy proceeding where only a fraction of cash flows δ is recovered due to congested courts, with $0 < \delta < 1$. This yields a distressed value of cash flows $\delta\underline{c} < \underline{c}$.

Previous empirical studies on court backlog, such as Ponticelli & Alencar (2016) and Rodano et al. (2016), have focused on the value of changes in bankruptcy regimes. One could think of this as a setting where the losses in bankruptcy arise from court congestion δ and, in addition, the bankruptcy code. For example, δ could be higher if productive and fundamentally sound firms are allowed to reorganize, as in Chapter 11, instead of being forcefully liquidated. In contrast, I am interested in estimating the elasticity of financing terms with respect to judicial backlog, holding the legal regime constant.

For a project of unit size, the creditor's expected profit function is given by:

$$\Pi \; = \; -1 \; + \; pR \; + \; (1 - p)\delta\underline{c} \tag{4}$$

To solve the model, I assume that creditors make zero profits in expectation, i.e. $\Pi = 0$.[38] Rear-

[38] It is easy to see that one could allow for profitable creditors without changing the analysis. I focus on the competitive case to highlight the key mechanism I investigate in this paper.

ranging for R then yields:

$$R = \frac{1}{p} - \underbrace{\frac{(1-p)}{p}}_{\text{PD}} \underbrace{\delta \underline{c}}_{\text{1 / LGD}} \tag{5}$$

I solve the model by further applying the following parametric restriction:

Assumption 1 $\bar{c} > \frac{(1-p)}{p}\delta\underline{c}$

Assumption 1 ensures that the entrepreneur's expected payoff $p(\bar{c} - R)$ is positive, so that the project is undertaken. Note that in the low cash flow state, the entrepreneur gets zero: his cash flows $\delta\underline{c}$ go to the creditor, and the remainder $(1-\delta)\underline{c}$ is a deadweight loss due to congested courts.

The central prediction coming out of equation 5 I will investigate empirically is that an exogenous decrease in court backlog δ will lead to lower R. The terms $\frac{1}{p}$ and $\frac{(1-p)}{p}$ describe the probability of default (PD). R decreases with p; in other words, interest rates increase with the probability of default. Interest rates also decrease with higher $\delta\underline{c}$, the value of cash flows in the bad state, which one can think of as recovery values or the inverse of the loss given default ($1/LGD$).

If we consider heterogeneous firms, the setup in equation 5 also yields predictions about which firms should be most affected by a change in bankruptcy court backlog. In particular, if we allow for firm-specific probability of default p_i, it is straightforward to see that an exogenous increase in δ should decrease interest rates for firms with higher p_i. I test this prediction empirically by exploiting differences in pre-determined firm characteristics for borrowers exposed to the same change in court congestion δ.

B Back of the Envelope Calculation – Additional Details

How large are the macroeconomic losses due to congested courts? In this section, I use the estimated effects of quasi-random exposure to the BAPCPA-induced drop in caseload to conduct a back of the envelope calculation. Far from being a precise exercise, this is meant to illustrate that the costs of an overburdened bankruptcy system may indeed be "enormous". These numbers do not, however, allow me to make statements about welfare.

At the heart of my estimation lies the idea that, using several simplifying assumptions, a reduction in interest rate spreads (a price term) can be used to estimate the partial equilibrium savings in corporate debt servicing costs (a quantity). Lower spreads reduce the annual interest burden of non-financial corporations: data from the Bank for International Settlements (BIS) suggest U.S. corporates pay around $400 billion per year to service their debts (Drehmann et al., 2015).[39]

A.1 Estimating Changes to the Interest Burden of U.S. Corporations

I calculate hypothetical changes to US-wide interest payments of non-financial corporations Δd for a given drop in judicial caseload. The starting point is the following formula:

$$\Delta d = \Delta r L \ , \tag{6}$$

where L is total outstanding corporate debt and Δr is the estimated change in interest rates for a given drop in court caseload. In section A.5, I show that this simple approximation can be derived from the installment loan formulas used by the Federal Reserve (Dynan et al., 2003) and the BIS (Drehmann et al., 2015) to calculate macroeconomic debt service ratios, or for the special case of coupon bonds.

Bankruptcy districts vary widely in their caseload per judge. I thus adjust equation 6 to allow for heterogeneity across districts, which yields:

$$\Delta d = \sum_d \Delta r_d \ L_d \ , \tag{7}$$

where Δd is the sum of the district-specific changes in debt service payments $\Delta r_d L_d$. L_d is easily

[39] BIS statistics suggest that the debt service to income ratio of the U.S. non-financial corporate sector was around 40% for the years 2016 through 2018. The Bureau of Economic Analysis reports that U.S. non-financial corporations made around $1 trillion in after-tax profits per year during that period.

observable in the data. I use three different approaches to measure it. First, I only consider the total debt of firms in the estimation sample. Second, I use the total debt of all non-financial firms in Compustat for which I can identify a bankruptcy district. Third, I will scale up these estimates using data on total corporate debt from the Financial Accounts of the United States, published by the Federal Reserve. Depending on the scenario, the debt of the firms in the estimation sample makes up between 27% and 31% of total corporate debt reported in the Financial Accounts.

My empirical estimates allow me to approximate how much spreads Δr_d in equation 7 would change for a given observable drop in caseload ΔC_d:

$$\widehat{\Delta r_d} = \Delta C_d \, \frac{\hat{\beta}^S}{\hat{\beta}^C} \,, \tag{8}$$

where $\hat{\beta}^S$ is the sensitivity of spreads to $Exposure$ from equation 1; and $\hat{\beta}^C$ is the sensitivity of the caseload per judge to $Exposure$ reported in table 2. The interpretation of $\frac{\hat{\beta}^S}{\hat{\beta}^C}$ is how much spreads would change for an exogenous one hour drop in caseload per judge. Plugging equation 8 into equation 7 then yields the final equation to estimate the costs in firms' debt service burden caused by congested bankruptcy courts:

$$\widehat{\Delta d} = \sum_d \Delta C_d \, \frac{\hat{\beta}^S}{\hat{\beta}^C} \, L_d \,. \tag{9}$$

Equation 9 suggests that the estimated change in corporate debt service burden can be approximated using data on changes in caseload burdens, outstanding corporate debt, and the elasticity of credit spreads to a drop in caseload per judge.

A.2 Estimating the Elasticity of Loan Terms to Judicial Caseload

To estimate macroeconomic costs using equation 9, I need point estimates for how much interest rate spreads would change for a given exogenous drop in caseload, i.e. $\frac{\hat{\beta}^S}{\hat{\beta}^C}$. For $\hat{\beta}^S$, I use the lowest estimate I find (≈ -50.64) from column 8 in table A7. This estimate implies that a one standard deviation increase in $Exposure$ (0.132) in the estimation sample decreases spreads by ≈ 7 bps. For $\hat{\beta}^C$, I use the estimate from column 1 in table 2, which implies that a one standard deviation increase in exposure to BAPCPA is associated with a drop of around 64 hours in annual caseload per judge ($0.115 \times 555.08 \approx 64$).

Assuming a linear relationship and permanent effects, we can interpret these estimates as an elasticity of around 7 basis points for spreads to a drop in caseload per judge of around 64 hours. The key implied elasticity of interest rate spreads is 0.09 basis points per hour of judicial workload.

Next, I need to determine ΔC_d, the "excessive" component of judicial backlog. I consider three scenarios. First, I take the caseload drop around BAPCPA as indicative of resolving excessive judicial burdens. Second, I use the Bankruptcy Judgeship Act of 2017 which passed the Senate on September 5, 2017 and, following the recommendations of the Judicial Conference, added four new permanent judgeships for the districts Delaware (2), Florida Middle (1), and Michigan East (1). Third, I estimate the effect of hiring one additional judge in the districts with the most congested courts (those in the top 10% of the caseload per judge).

A.3 The Estimated Aggregate Costs of Congested Courts

Plugging the inputs described above into equation 9 suggests that congested courts may have substantial aggregate costs to the U.S. corporate sector. The estimates suggest that the historic drop in caseload around BAPCPA saved U.S. corporations at least \$10.13 billion in interest payments. However, BAPCPA was a watershed event in the history of bankruptcy in the United States and as such perhaps a poor benchmark for resolving excessive caseload.

The estimates for the Bankruptcy Judgeship Act of 2017 also suggest that congested courts are costly. Depending on the value of outstanding corporate debt, the Act reduced the annual corporate debt service burden by between \$350 and \$800 million. I find slightly larger estimates for the hypothetical scenario of creating one additional judgeship in the most congested courts, ranging from \$440 million to \$1.48 billion. The average estimated macroeconomic costs of overburdened judges are \$740 million per year.

While these estimates are small compared to U.S. gross domestic product, they are substantial relative to what it would likely cost to reduce caseload burdens. For the Bankruptcy Judgeship Act of 2017, the Congressional Budget Office (CBO) estimated that bankruptcy judges earn about \$232,000 in salaries and benefits.[40] The CBO also provides an estimate for judicial administrative costs for personnel, security, and court operations of about \$700,000 per judge per year. Creating an additional judgeship thus costs approximately \$932,000. The estimates based on the Bankruptcy Judgeship Act and the 10% most congested courts are based on hiring an additional four or eight

[40] Bankruptcy judges are entitled to compensation equal to 92% of that of a district judge, which puts their listed annual salary at approximately \$191,000. See 28 U.S. Code § 153 for the background covering bankruptcy judge compensation and U.S. Courts (2019b) for the time series of judicial pay. District judges in the United States were entitled to \$208,100 in compensation in 2018.

judges, respectively. Compared to the estimated savings in corporate debt service burden, the costs of hiring new judges is thus likely small.

These findings rely on simplifying assumptions. Most obviously, I have to assume that firms' propensity to file for bankruptcy does not change in reaction to lower caseload. The results in table A11 indeed suggest no differential change in corporate bankruptcy filings depending on a district's exposure to BAPCPA. Despite the caveats, my estimates are likely a lower bound for a number of reasons. First, they ignore the interest burden of households, and thus that court backlog may also matter for personal bankruptcies. Second, I do not consider the costs from an inefficient resolution of bankruptcy cases due to congested courts (see e.g. Iverson, 2018) and knock-on effects such as skewed input choices for firms (Boehm & Oberfield, 2018). Third, I do not consider the effect of caseload on loan maturities, which is conservative: Drehmann et al. (2015) show that even small changes to the average maturity of outstanding debt can drastically shift the interest burden because repayment is spread out over a longer time period. All of these factors likely bias my estimates downward.

A.4 Details on Data and Empirical Estimates

Table A13 plots the required inputs and estimates of the macroeconomic costs of congested courts using equation 9. The first two panels plot the required input variables: the estimated elasticity of spreads to a one hour drop in caseload ($\hat{\beta}^S/\hat{\beta}^C$), and the outstanding corporate debt L. I plot the total debt in the estimation sample (column 1), Compustat (column 2), and the financial accounts (column 3).

Scenario 1 approximates the macroeconomic savings from the drop in caseload per judge around BAPCPA. For ΔC_d, I simply plug in the observed difference in caseload per judge for 2006-2007 compared to 2004-2005 used as the dependent variable in table 2 (and in figure 3). For L_d, I use values as of 2004. The counterfactual is how much the annual debt service payments of U.S. corporates dropped due to less congested courts, holding total debt constant.

I start with the in-sample calculation for the publicly listed firms in the estimation sample in column 1. These firms had a total of \$1,545 billion in outstanding debt prior to BAPCPA in 2004. This means they account for a substantial 31% of the total outstanding debt of non-financial corporates of around \$4,997 in that year. Equation 9 implies that the drop in caseload per judge following BAPCPA saved the sample firms \$10.13 billion in interest payments *per year*, a substantial magnitude.

Next, I turn to the debt service burden of all non-financial corporations in Compustat in column 2. This increases the estimate to \$15.8 billion in annual savings. In column 3, I extrapolate these figures using total outstanding corporate debt from the Financial Accounts. Because the borrowers in Compustat make up a significant part of total borrowing, I use their locations to approximate the geographical distribution of total corporate debt. With that assumption in mind, I scale up the Compustat estimate of \$15.8 billion by the factor of total to Compustat debt ($\frac{\$4,997}{\$2,632}$ billion). This implies that the caseload drop following BAPCPA reduced annual debt service payments by \$30 billion.

How do these savings in debt service burden compare to the costs? The United States Government Accountability Office estimated that implementing BAPCPA cost around \$72 million. These costs, however, were almost exclusively related to the legal changes for consumer cases. They also do not include the 28 new temporary judgeships BAPCPA created (USGAO, 2008). As I will explain in more detail below, bankruptcy judgeships cost approximately \$1 million a year, including all administrative costs. Yet even after adding \$28 million in annual expenses, these costs pale in comparison with the estimated savings from the drop in caseload for my sample firms alone. From a fiscal policy perspective, these estimates imply high "multipliers" for resolving congested courts.

Of course, BAPCPA was a watershed event in the history of bankruptcy in the United States. This makes it attractive to isolate the causal effect of judicial backlog but perhaps less attractive for determining what constitutes "excessive" caseload. After all, the drop in bankruptcy cases following BAPCPA reflected a substantial legal change.

For scenario 2, I thus turn to the Bankruptcy Judgeship Act of 2017 as a template for a minimum desirable reduction in caseload. The Act passed the Senate on September 5, 2017 and added four new permanent judgeships for the districts Delaware (2), Florida Middle (1), and Michigan East (1), following the recommendations of the Judicial Conference.[41] I use data from the Federal Judicial Caseload Statistics for Q1/2017 to calculate how the new judgeships affected annual weighted caseload per judge (ΔC_d), holding the number of filings constant. For Delaware, the projected judge workload dropped by 157 hours (from 626 to 469); for Florida Middle by 101 hours (from 906 to 805); and for Michigan East by 158 hours (from 948 to 790). The average drop in these districts is 138 hours per judge per year, or around three work weeks.

[41] See Congress (2017) for the full text of the bill and U.S. Courts (2017) for the recommendations of the Judicial Conference. The Act also made permanent many previously temporary judgeships that were extended on a regular basis. Since these judges were already in place, this does not have an immediate effect on the court workload.

Table A13: The Macroeconomic Costs of Congested Bankruptcy Courts

Notes: This table presents the inputs I use to calculate the social costs of excessive bankruptcy court caseload and three scenarios. $\hat{\beta}^S$ is the estimated effect of $Exposure$ on interest rate spreads. $\hat{\beta}^C$ is the estimated effect of $Exposure$ on the drop in caseload per judge. $\hat{\beta}^S$ / $\hat{\beta}^C$ is the implied elasticity of interest rate spreads to a given drop in caseload. Note that I ignore that the standard deviations of $Exposure$ in the samples are slightly different; incorporating this difference would yield a slightly larger elasticity of 0.10. The *Judge multiplier* is the ratio of savings in interest burden to required judgeships. For outstanding debt from the financial accounts data, I define corporate debt as the sum of total debt securities (time series FL104122005) and total loans (time series FL104123005).

		Firms in sample	Compustat firms	Financial accounts
		(1)	(2)	(3)
Point estimates				
Elasticity of interest rate spreads to $Exposure$	$\hat{\beta}^S$		50.64	
Elasticity of caseload per judge to $Exposure$	$\hat{\beta}^C$		555.08	
Implied elasticity of spreads to caseload per judge	$\hat{\beta}^S/\hat{\beta}^C$		-0.091	
Outstanding corporate debt				
Total debt (2004, in \$ billion)	$\sum_d L_d$	1,545	2,632	4,997
Total debt (2016, in \$ billion)	$\sum_d L_d$	2,253	5,073	8,478
Scenario 1: Estimated effect of BAPCPA				
Drop in caseload per judge (average)	ΔC_d	529	529	529
Savings in interest burden (in \$ billion)	$\widehat{\Delta d}$	10.13	15.80	30.00
Implementation costs (USGAO)		0.072	0.072	0.072
New judgeships (in \$ billion)		0.028	0.028	0.028
Total multiplier		101	158	300
Scenario 2: Bankruptcy Judgeship Act of 2017				
Drop in caseload per judge (average)	ΔC_d	138	138	138
Savings in interest burden (in \$ billion)	$\widehat{\Delta d}$	0.35	0.48	0.80
Required judgeships (in \$ billion)		0.004	0.004	0.004
Judge multiplier		88	120	200
Scenario 3: Hiring new judges in highly congested courts				
Drop in caseload per judge (average)	ΔC_d	310	310	310
Savings in interest burden (in \$ billion)	$\widehat{\Delta d}$	0.44	0.88	1.48
Required judgeships (in \$ billion)		0.008	0.008	0.008
Judge multiplier		55	110	185

Scenario 3 estimates the effect of hiring one additional judge in the most congested courts. I define these as districts above the 90th percentile in Q1/2017.[42] This would reduce the annual workload per judge in these courts by an average of 310 hours per year, ranging from a minimum drop of 102 caseload hours in the northern district of Illinois to a maximum of 731 hours in the northern district of Mississippi. I will use these values for ΔC_d. Note that these courts do not overlap with the districts receiving new judgeships as part of the 2017 bill: the average weighted caseload per judge of Delaware, Florida Middle, and Michigan East in Q1/2017 was 827 hours per year, which lies between the 75th and 90th percentile of the caseload distribution.

The bottom two panels of table A13 plot the results of plugging in the respective values into equation 9. I use total debt as of 2016 for these calculations; the sample firms account for around 27% of all outstanding corporate debt. For the Bankruptcy Judgeship Act of 2017, this procedure yields estimated savings in macroeconomic debt service burden of approximately \$800 million for the entire stock of non-financial corporate debt (\$480 million for Compustat firms, \$350 million in-sample). If one were to hire an additional judge in each of districts with a caseload above the 90th percentile, the savings would amount to \$1.48 billion (\$880 million for Compustat firms, \$440 million in-sample). The simple average of these six estimates is \$740 million per year—indicating that the costs of overburdened courts are indeed "enormous".

How much would it cost to resolve excessive court congestion as defined in the latter two scenarios? For the Bankruptcy Judgeship Act of 2017, the Congressional Budget Office estimates annual salaries and benefits for bankruptcy judges of about \$232,000.[43] The Congressional Budget Office also provides an estimate for judicial administrative costs for personnel, security, and court operations of about \$700,000 per judge per year. Creating an additional judgeship thus costs approximately \$932,000; I will round this estimate up to \$1 million for simplicity. Four judges in the Bankruptcy Judgeship Act scenario thus would cost \$4 million and the eight new judgeships required by the 90th percentile scenario \$8 million. Clearly, these costs are small compared to the estimated benefits: from a fiscal policy perspective, they imply "judge multipliers" of between 55 and around 200. The average multiplier for the six estimates in scenarios 2 and 3 is 126.

Taken together, I conclude that the macroeconomic costs of court backlog are likely large.

[42] In Q1/2017, these were the districts AL,M; GA,N; IL,N; LA,W; MO,E; MS,N; TN,W; and TX,E.

[43] Bankruptcy judges are entitled to compensation equal to 92% of that of a district judge, which puts their listed annual salary at approximately \$191,000. See 28 U.S. Code § 153 for the background covering bankruptcy judge compensation and U.S. Courts (2019b) for the time series of judicial pay. District judges in the United States were entitled to \$208,100 in compensation in 2018.

Although the estimates here can only be regarded as illustrative, they suggest that new bankruptcy judgeships have potentially high social returns.

A.5 Deriving Approximate Changes in the Debt Service Burden

In the paper, I use the simple approximation that changes in debt service payments Δd are equal to changes in the interest rate Δr times total outstanding debt L (see equation 6). In this section, I show that this approximation holds true for two cases that the data suggest are relevant in the U.S. setting: (1) installment loans with either short-term or very long maturities and (2) coupon bond debt.

As a starting point, consider the approach used by the Federal Reserve (Dynan et al., 2003), also used by the Bank for International Settlements (Drehmann et al., 2015), who calculate the macroeconomic debt service burden as follows:

$$d = \frac{rL}{(1-(1+r)^{-m})}\ , \tag{10}$$

where L refers to the total stock of non-financial corporate debt; r to the average interest rate on the existing stock of debt; and m to its average remaining maturity. This assumes that a representative firm issues installment loans and pays back an equal fraction of debt each year. In principle, r is composed of an underlying reference rate and an interest rate spread. Because I am only interested in changes to the spread component, I implicitly hold reference rates constant.

For the case of $m = \infty$, this yields

$$d = \frac{rD}{(1-\frac{1}{(1+r)^{\infty}})} \approx rD. \tag{11}$$

Differentiating d with respect to rL (keeping L constant) yields $\Delta d \approx \Delta rL$. It is equally easy to see that setting $m = 1$,

$$d = \frac{rL}{(1-\frac{1}{1+r})} = (1+r)L. \tag{12}$$

Again differentiating d with respect to rL yields $\Delta d = \Delta rL$.

As a special case, consider the formula for the annual payments of a coupon bond:

$$d = rL, \tag{13}$$

which does not depend on the bond's time to maturity. In other words, for non-zero coupon bonds, $\Delta d = \Delta r L$ (holding L constant).

In the data, around 69% of outstanding U.S. corporate debt in 2016 was accounted for by bonds, the overwhelming majority of which carry a coupon payment. For loans in the DealScan database, around 40-50% of issued syndicated loans are term loans, for which the installment formula in equation 10 is applicable. Most other contracts are credit lines, which have a payment structure that perhaps more closely mirrors the case of $m = \infty$ (absent default). Around 10-15% of issued syndicated loans have a maturity of one year or less, i.e. $m = 1$. Taken together, these data points suggest that for the U.S. corporate debt market, the approximation s $\Delta d \approx \Delta r L$ is likely a valid starting point.