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Get in Line: Chapter 11 Restructuring in Crowded Bankruptcy Courts

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Abstract. Bankruptcy costs depend not only on the laws that govern financial distress but also on the ability of the court to rehabilitate distressed firms. This paper tests whether Chapter 11 restructuring outcomes are affected by time constraints in busy bankruptcy courts. Using the passage of the Bankruptcy Abuse Prevention and Consumer Protection Act as an exogenous shock to caseloads, I find that commercial banks report lower charge-offs on business lending when court caseloads decline, suggesting that the costs of financial distress are lower in less-congested courts. Further, court caseload affects how restructuring takes place. Less-busy bankruptcy judges liquidate fewer small firms, but more large firms. When caseload declines, large firms spend less time in court and firms that are dismissed from court are less likely to refile for bankruptcy. In addition, firms are less likely to sell assets or obtain debtor-in-possession financing in less-busy courts.

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1. Introduction

The purpose of Chapter 11 bankruptcy is to protect the assets of financially distressed firms from seizure by creditors while the restructuring options available to the firm can be considered. The laws that govern this process play an important role in determining the costs of financial distress and the eventual allocation of capital in an economy, as shown by prior research.[1] However, the efficiency of the institution that governs this process—the bankruptcy court—could also affect bankruptcy outcomes. Indeed, organizational efficiency and time constraints have been shown in other contexts to play a key role in decisions made by private firms.[2] The main contribution of this paper is to show that the efficiency of the court itself, and not just the laws that govern the court, has an important impact on the costs of financial distress and on the ultimate outcome of the bankruptcy.

In particular, I focus on the total caseload that bankruptcy judges must deal with. Judge workload fluctuates widely as economic conditions change. By definition, judges become busiest when financial distress is most prevalent—when a large number of firms and individuals are facing financial distress. For example, total bankruptcy filings nationwide rise on average by 32% during economic recessions. Large differences in workload are also common cross-sectionally, as local economic deteriorations lead to increasing caseloads for judges in those areas.[3] Court congestion, then, is potentially an important source of inefficiency because it occurs when the bankruptcy system is most needed: during economic downturns when failing firms have the fewest outside options for restructuring and when the value of correctly allocating capital is the highest (Eisfeldt and Rampini 2006).

It is natural to expect that time constraints might limit the ability of the court to manage cases effectively, thereby leading to an increase in the costs of financial distress. But it is less clear through which channel court congestion might do this. On the one hand, a busy judge may look to free up time by liquidating more firms or dismissing them from court altogether, since once this action is taken the judge has less to do in managing the case. This action could reduce overall recovery rates by forcing liquidations of viable firms, potentially at fire-sale prices. Alternatively, a time-pressured judge may fear making errors in judgment because of an inability to gather and consider information about each case. Given this, a busy judge may be reluctant to liquidate a distressed firm, preferring instead to allow the firm to reorganize and preserving the option to liquidate the firm at a future date. In addition, reorganization may be the path of least resistance for the judge in many cases, since the debtor's management, who typically seeks to have the firm reorganized, retains control of the firm by default (Franks et al. 1996). Under

this hypothesis, high caseloads could harm creditors by leading to either lengthy stays in bankruptcy or the inefficient continuation of firms.[4]

To empirically test the impact of busy courts on financially distressed firms, I use a natural experiment that exogenously impacted the caseload of bankruptcy courts. In 2005, Congress passed the Bankruptcy Abuse Protection and Consumer Protection Act (BAPCPA), which made it substantially more difficult for households to file for bankruptcy protection. After BAPCPA's passage, nonbusiness bankruptcy filings dropped dramatically and stayed at extremely low levels until the onset of the financial crisis (Figure 1, panel A). Since bankruptcy judges rule on *both* business and nonbusiness cases (i.e., there is no specialization among bankruptcy judges), BAPCPA created a large shock to the workload of bankruptcy judges across the nation, cutting average caseloads in half. BAPCPA did not impact all districts equally, however. In particular, courts that handled a relatively higher share of personal bankruptcy cases saw caseloads drop by larger amounts after BAPCPA took effect. For example, caseload in the District of Oregon decreased by 62% after BAPCPA, while just to the south caseload only fell by 39% in the relatively more business-centric Northern District of California. Using difference-in-differences specifications, I exploit this exogenous variation to estimate the causal effect of total judge caseload on a variety of firm outcomes.

I first test how court caseload is related to default costs. Because the equity value of the bankrupt firm is negative or close to zero, additional costs of financial distress are principally borne by the unsecured creditors of the firm. Using regulatory data reported by commercial banks, I indirectly measure the default costs passed on to creditors by examining the net charge-off rate of commercial and industrial (C&I) loans held by banks that were particularly exposed to the BAPCPA caseload shock. Because local banks are the predominant source of funding for small businesses (Petersen and Rajan 1994), they should bear the brunt of higher bankruptcy costs when these firms default. Consistent with the intuition that time constraints cause higher default costs, I find that banks that are located in bankruptcy districts with exogenously lower caseloads report significantly lower C&I loan charge-off rates. Specifically, I estimate that a 64-hour reduction of bankruptcy caseload per year (equivalent to one standard deviation within the context of the BAPCPA natural experiment, but only a 6% decline from the mean caseload), reduces loss given default on C&I loans by 3.9 percentage points, an 11% decline from the mean.

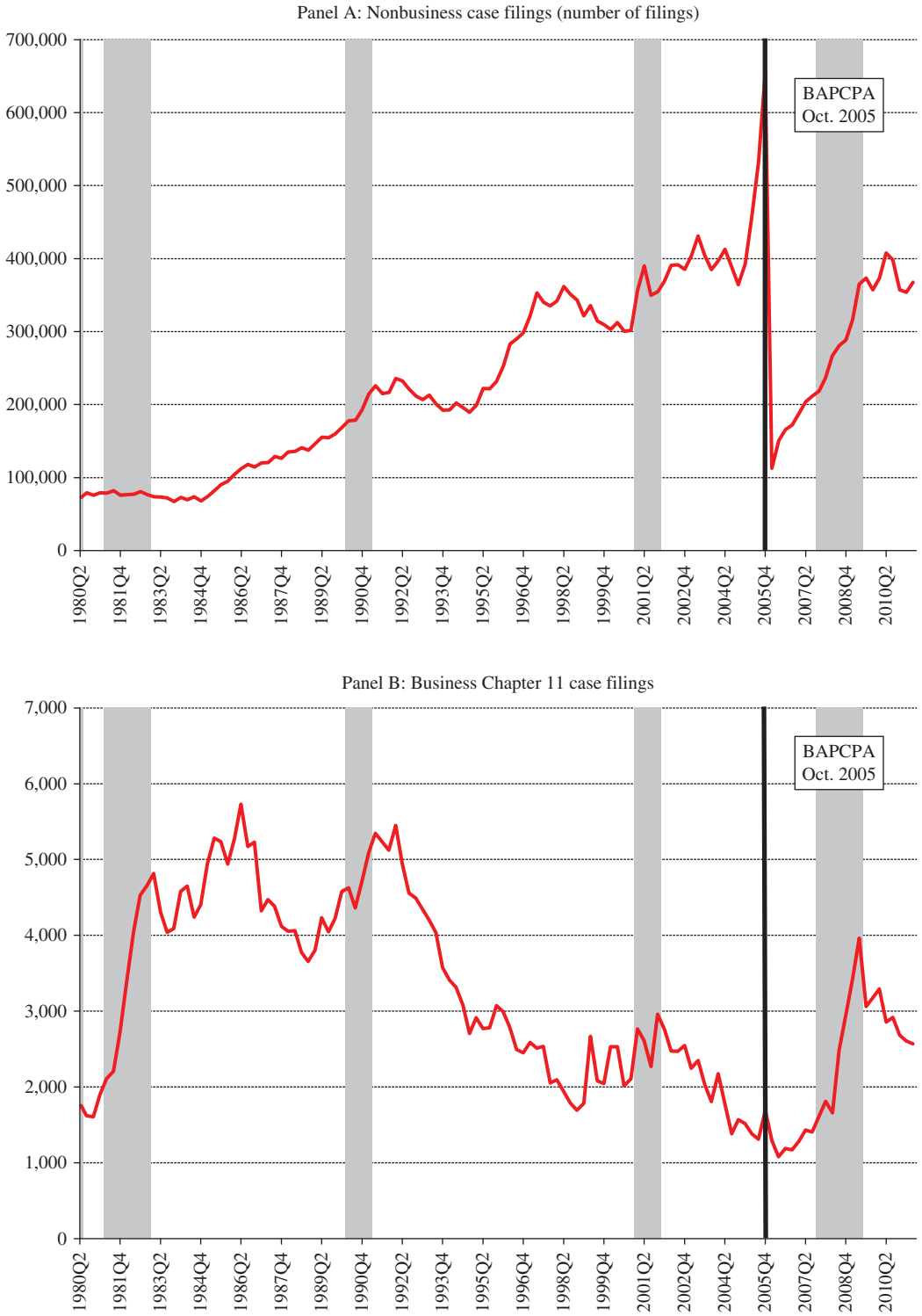
I then use information on 3,236 Chapter 11 bankruptcies filed between 2004 and 2007 to test how court caseload affects bankruptcy outcomes. I find that an exogenous decline in court caseload leads to an increase in case dismissals, thereby denying some firms bankruptcy protection. In addition, I also show that larger firms, which are most likely to tax busy judges, are more likely to be liquidated in less-busy courts, and correspondingly less likely to reorganize and emerge from bankruptcy. Put differently, a decline in caseload changes the outcomes of marginal bankruptcy cases by pushing firms away from reorganization and toward dismissal (for smaller firms) or liquidation (for larger firms).

While these results show that caseload affects case outcomes, it is not clear how this might translate into reduced bankruptcy costs. One possibility is that less-busy courts have lower recidivism rates—the probability that a firm reenters bankruptcy within three years of its original filing.[5] I find that firms that successfully reorganize in busy bankruptcy courts are no more likely to refile for bankruptcy than firms that reorganize in less-busy courts. However, firms that are dismissed from less-busy courts have substantially lower recidivism rates, and this lower recidivism likely lowers the costs of financial distress for these firms as it eliminates direct and indirect bankruptcy costs incurred the second time in court.[6]

Court caseload also impacts other aspects of restructuring that relate to bankruptcy costs. I find that large firms spend less time in bankruptcy when caseload decreases, particularly when the firm is eventually liquidated. This is consistent with busy judges being hesitant to push large firms into liquidation, instead allowing them to remain in bankruptcy longer. Meanwhile, smaller firms are liquidated more quickly in busy courts. In addition, I find that firms that file in less-busy courts are less likely to sell assets, which could reduce bankruptcy costs by eliminating fire sales (Shleifer and Vishny 1992). Further, debtors are less likely to obtain debtor-in-possession (DIP) financing when caseload declines. This is consistent with the idea that firms do not need to obtain as much outside cash (e.g., via asset sales or DIP loans) when courts are less crowded.

Taken together, my results show that overall costs of financial distress are lower when court caseload declines, and that court caseload has a significant impact on how capital is reallocated in bankruptcy. These findings relate to a large literature on the costs of financial distress (Warner 1977, Andrade and Kaplan 1998, Elkamhi et al. 2012) as well as investigations into the design of bankruptcy systems and their impact on debt contracts (Gertner and Scharfstein 1991, Aghion et al. 1992, Bolton and Scharfstein 1996, Stromberg 2000). Much of the research in this area has focused on the design of bankruptcy institutions, but a growing literature points to the judge as having a large effect on bankruptcy outcomes (Bris et al. 2006, Gennaioli and Rossi 2010, Chang and Schoar 2013, Bernstein et al. 2017).

Figure 1. (Color online) Bankruptcy Cases Filed per Quarter—Nonbusiness and Chapter 11



Notes. Panel A shows the total number of nonbusiness bankruptcy filings per quarter in the U.S. Courts system from 1980Q2 to 2011Q2, whereas panel B shows the total number of business Chapter 11 cases filed. In both charts, the vertical line identifies the passage of BAPCPA in October 2005, while light-gray shading indicates NBER recessions.

However, to date, this line of research has treated judge characteristics as fixed. This paper contributes to this line of research by showing that distress costs and firm outcomes are significantly affected by time constraints faced by the judge, and not just the Bankruptcy Code or a judge's own fixed biases.[7]

In addition, this paper broadly relates to the literature on complexity costs and bounded rationality (Hong and Stein 1999, Hirshleifer and Teoh 2003, Cohen and Lou 2012). In this vein, research that examines job performance and decision making under time constraints is particularly relevant to my research, and in recent years a growing literature has pointed to time constraints as playing an important role in decision making.[8] Agarwal et al. (2017) show that banks with fewer employees per loan, less training for staff, and longer wait times for phone calls were significantly less likely to modify mortgages to avoid costly foreclosures. Fich and Shivdasani (2006) show that busy boards are associated with weak corporate governance. Coviello et al. (2014) show that judges who juggle too many cases at once have decreased productivity, and Ponticelli and Alencar (2016) show that congested courts were less effective at incorporating reforms in the Brazilian bankruptcy system.

Perhaps most closely related to this paper is Huang (2011), who uses an empirical methodology similar to mine to show that busy appellate court judges exhibit lightened scrutiny over district court decisions. This paper builds on his work by examining the effect of time constraints in bankruptcy courts, where the effect of time constraints is theoretically ambiguous since dismissing or liquidating a case could be less work for the judge, but doing so is not the default option for the judge. Further, bankruptcy court caseload naturally fluctuates over the business cycle and thus these effects arise systematically, making it important from a policy perspective to understand the effects of caseload on bankruptcy courts in particular.

The rest of the paper proceeds as follows. Section 2 gives more background about the role of the judge in Chapter 11 bankruptcy and measures of judge caseload. Section 3 describes the impact of BAPCPA on court caseload and develops my empirical strategy. Section 4 describes the data in my sample. Section 5 analyzes the impact of caseload shocks on restructuring firms. Section 6 concludes.

2. Bankruptcy Process

2.1. The Role of the Bankruptcy Judge

When a corporation files for Chapter 11 bankruptcy protection, it is randomly assigned to one of the bankruptcy judges in the district in which it files.[9] From the first-day motions until the end of the bankruptcy case, the judge's main role is to review motions that are brought before the court and to determine whether to grant those motions. The bankruptcy judge therefore plays an integral role in Chapter 11 restructuring, with the judge being responsible for setting corporate operating policies and, ultimately, determining whether a debtor firm should be liquidated or reorganized.

Among the most important motions brought before the judge are petitions to dismiss a bankruptcy case or convert it to Chapter 7 liquidation. While conversion to Chapter 7 almost certainly means the death of the firm, motions for dismissal are less clear. Dismissal from court essentially means that the firm remains as if no bankruptcy had ever been filed, and thus creditors have power to seize assets or seek legal action against the debtor. Dismissed firms can refile for bankruptcy, but they must show that they are in need of bankruptcy protection and have potential to be successfully rehabilitated; otherwise, the case will either be dismissed again or converted to Chapter 7, potentially with legal consequences for a bad-faith filing. Dismissal typically leads to either liquidation or a subsequent bankruptcy filing because the firm has not been restructured in any way (Morrison 2007).[10] This is particularly true for smaller firms, which have less ability to fight lawsuits in court or negotiate with creditors outside of court.

Another key role of the bankruptcy judge is to rule on the feasibility of a Chapter 11 plan of reorganization. The plan of reorganization outlines how the operations and capital structure of the firm will be restructured and how the creditors of the firm will be repaid. The plan must also estimate the enterprise value of the firm under Chapter 11 continuation, and show that this value is greater than the expected value if the firm were to be liquidated under Chapter 7. While creditors must vote to accept a plan, even after it has been accepted by the creditors the judge has the responsibility to determine if "confirmation of the plan is not likely to be followed by liquidation . . . or the need for further financial reorganization" (United States Courts 2011). In short, the judge must agree that the plan does enough to ensure that the firm will be viable going forward. While this objective is specifically laid out for the judge in the Bankruptcy Code, there are no direct monetary consequences for a judge who allows an unviable firm to reorganize, since in practice it is nearly impossible to determine when this occurs. However, there are reputational concerns for bankruptcy judges, who are well-known within legal communities (LoPucki 2005) and whose decisions are part of the public record.

Aside from direct decisions that determine whether a firm is allowed to reorganize, judges also rule on motions that alter other important aspects of the bankruptcy process. One of the most important of these is the motion to sell assets in so-called "Section 363" sales (named after the section of the Bankruptcy Code that governs the sales). Asset sales can bring much-needed cash into the firm, allowing it to continue operations during bankruptcy or to pay off creditors who are holding up negotiations. However, Pulvino (1999) shows

that assets sold in Chapter 11 restructuring are typically sold at deeply discounted prices, indicating that these sales could hurt recovery rates for creditors. It is up to the judge to determine whether these sales should be allowed to take place and to ensure that the auction process is fair.

Other motions that judges consider include petitions to lift the automatic stay and allow creditors to seize certain assets, to extend the exclusivity period, or to allow the use of cash collateral. A growing body of literature shows that judges have a significant amount of discretion in their rulings and are important players in the restructuring process. For example, Bris et al. (2006) show that judge fixed effects account for 19% of the variation in bankruptcy durations and 10% of the variation in unsecured creditor recovery rates.

Finally, what is important to keep in mind is that the perceived likelihood that a judge will rule a given way will affect how debtors and creditors behave during the bankruptcy. In equilibrium, a debtor is likely to be more aggressive if they perceive that the judge is sympathetic to their cause, and vice versa for procreditor judges. Thus, if busy judges rule differently, it will also affect the set of motions filed in court and other actions taken by debtors and creditors. The outcomes I observe in this paper are the net result of all of these actions after a shock to court caseload, and are thus not solely attributable to actions taken by the judge alone.

2.2. Measuring Bankruptcy Court Caseload

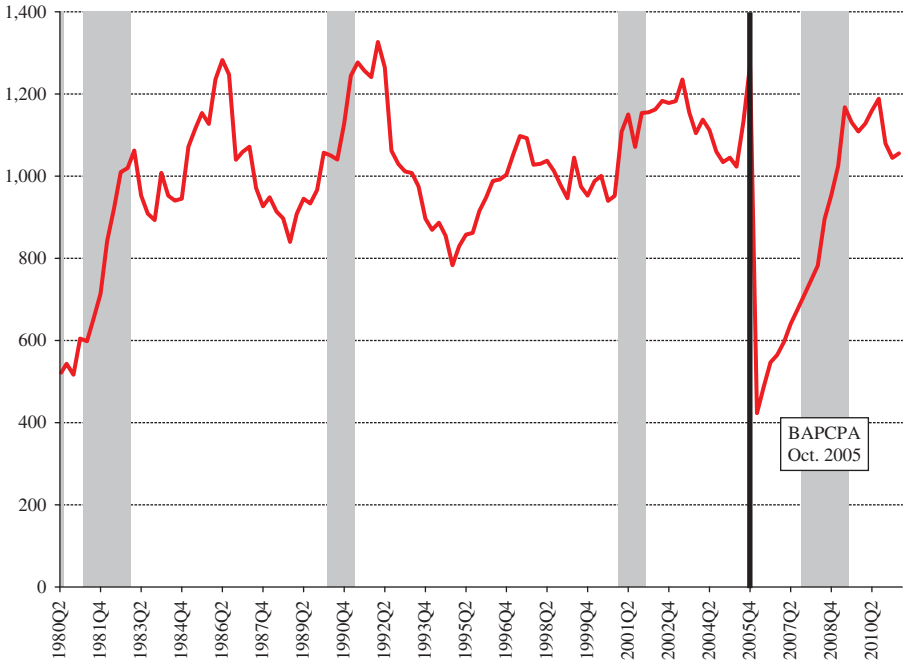
The number of bankruptcy judgeships in the United States is determined by Congress, and the creation of new judgeships requires the passage of a bill by both the House of Representatives and the Senate. Every other year, the Judicial Conference of the United States conducts a study of the caseload of bankruptcy judges and recommends to Congress the number of new judgeships that are needed for each bankruptcy district. Despite consistent pleas for more judges from the Judicial Conference, the last time Congress approved new permanent judgeships was in 1992.[11] As a result, judge workloads have increased dramatically. From 1980 to 2010, total bankruptcy filings rose by 381% while the total number of bankruptcy judges only increased by 53%. Put differently, the average bankruptcy judge in 2010 handled 3.1 times more cases than the average judge in 1980.

But each bankruptcy case does not demand an equal amount of the judge's time. Personal Chapter 7 cases rarely go before a judge, while a complex Chapter 11 filing will take many hours of court time. Because of these differences, the Judicial Conference uses a weighting system to calculate the caseload for each bankruptcy district. The weights come from a judge time study conducted in 1989 (Bermant et al. 1991), and indicate the number of hours a judge spends on each of six types of bankruptcy cases (see Table A.3 in the online appendix for weights): nonbusiness Chapter 7, business Chapter 7, Chapter 11, Chapter 12, Chapter 13, and other. While nonbusiness Chapter 7 cases on average take only six minutes of a judge's time, the average Chapter 11 case uses up nearly eight hours.[12] Following the Judicial Conference, I measure *caseload* as the weighted number of cases filed per judge in each bankruptcy district. Because the weights are expressed in the number of hours the judge is expected to spend on the case, weighted caseload can be interpreted as the number of hours (per year) the judge would spend administering the particular mix of six bankruptcy case types filed in his bankruptcy district. Importantly, weighted caseload measures only the time spent by a judge administering bankruptcy cases, and is therefore *not* a measure of the total workload of a bankruptcy judge. Specifically, it omits time spent on adversary proceedings, court administration, and travel. Bermant et al. (1991) show that case-related work and adversary proceedings together consume about 57% of a judge's total time, but do not break out the percentages for casework alone. Roughly speaking, if casework alone consumes 50% of a judge's time (and adversary proceedings the remaining 7%), then a judge with a weighted caseload of 1,000 hours per year (the average in my sample) has a total workload of about 2,000 hours, equivalent to a 40-hour work week with no vacation time. Because I cannot measure total workload, in this paper I focus only on weighted caseload as a proxy for the total amount of work a judge must do.

On a weighted basis, judges in 1980 had, on average, a total caseload of 503 hours per year. By 2010, that workload had more than doubled to 1,141 hours per year. However, much of that increase came in the first few years of the 1980s, when business bankruptcy filings rose quickly in the aftermath of two closely spaced economic recessions. Since 1983 total weighted caseload has fluctuated around 1,000 hours per year (Figure 2). In general, total bankruptcy caseload rises during or shortly after economic recessions, and often these increases can be substantial. The average peak-to-trough change in caseload since 1983 is 264 hours, or 25% of the mean caseload per year.

Moreover, there is wide variation in caseload across the 89 bankruptcy districts in the United States.[13] Taking the average weighted caseload for each district from 1983 to 2011, I find that the standard deviation across districts is 311 hours, or 7.8 40-hour work weeks. At the extremes, the bankruptcy judge in Vermont had an average total workload of 305 hours per year, while the judges of the Western District of Tennessee averaged 1,664 hours per year. More recently, areas that have experienced particularly difficult economic recessions have seen dramatic increases in the caseload required of each judge. For example, since

Figure 2. (Color online) Caseload per Judge



Notes. This figure displays the total weighted caseload per judge across the U.S. courts system from 1980Q2 to 2011Q2. The y axis can be interpreted as the total expected hours a judge will spend on casework annually. The vertical line identifies the passage of BAPCPA in October 2005, while light-gray shading indicates NBER recessions.

2009, bankruptcy districts in Nevada (2,161 hours), Middle District of Florida (2,041 hours), Eastern Michigan (1,865 hours), Northern Mississippi (1,833 hours) and Northern Georgia (1,771 hours) have been particularly stressed.

3. Identification Strategy

Bankruptcy filings typically rise when economic conditions deteriorate, leaving judges with the heaviest workloads during economic recessions. Because of this, a simple comparison of the bankruptcy outcomes of firms that file in busy courts versus those that file in nonbusy courts would be confounded by multiple other factors. In particular, during recessions firms have worse outside options for dealing with financial distress. Raising new capital is difficult because credit is tight, asset sales would likely yield lower proceeds due to fire sale pricing, and negotiations with creditors might be more difficult as creditors are potentially facing their own financial issues during recessions. Further, there are selection effects, as high-beta firms are more likely to go bankrupt in recessions. For these reasons, I cannot simply compare firms that file during busy times to those that file when judges have more time available.

To identify the causal effect of caseloads on restructuring, I use difference-in-difference specifications that exploit an exogenous shock to caseloads that affected some bankruptcy districts more than others. On April 20, 2005, the Bankruptcy Abuse Prevention and Consumer Protection Act was signed into law, although most of the provisions of the Act only applied to bankruptcy cases that were filed on or after October 17 of that same year. BAPCPA was focused mainly on nonbusiness bankruptcies, and, as its name suggests, its primary aim was to prevent abuse of the bankruptcy system by individual filers.

Prior to BAPCPA, individual filers could choose the chapter of bankruptcy under which they filed. BAPCPA prevents high-income filers from filing for Chapter 7 bankruptcy and instead forces them to file for Chapter 13 bankruptcy, where less debt is discharged and future income must be pledged toward paying back creditors. In addition, BAPCPA increased the costs of filing for bankruptcy by between 50-70% because of increases in filing fees, lawyer fees, and required debt counseling (U.S. Government Accountability Office 2008). Finally, BAPCPA also capped the amount of homestead exemptions at $125,000, which impacted filers in states that traditionally allowed home owners to protect large amounts of home equity.

Because the law was passed in April but not effective until October, there was a window within which individuals could still file under the old law, and this explains the large spike in filings in mid-2005 as individuals rushed to file before the October 17th effective date (Figure 1, panel A). More importantly, once the law took effect, personal bankruptcy filings

Table 1. Caseload Summary Statistics

	Obs.	Mean	Std. dev.	5th percentile	Median	95th percentile
Nonbusiness share of caseload (2003)	89	79.4%	11.5%	63.2%	81.6%	92.3%
Avg. caseload 2004–2005 (pre-BAPCPA)	89	1,095.05	429.90	425.07	1,107.66	1,842.90
Avg. caseload 2006–2007 (post-BAPCPA)	89	565.54	267.99	165.76	543.23	1,063.19
BAPCPA caseload drop	89	529.51	215.27	207.77	528.87	908.77
Below-median nonbusiness *caseload*	45	456.54	196.30	170.56	484.20	792.15
Above-median nonbusiness *caseload*	44	604.14	210.08	321.32	566.18	1,000.40

Notes. This table reports the distribution of caseload for the eight quarters before and after BAPCPA, as well as the distribution of the nonbusiness share of caseload in 2003 for the 89 bankruptcy districts in my sample. *Caseload* is measured as the weighted number of filings in each district per quarter per bankruptcy judge, using the weights in Table A.3 in the online appendix. I multiply caseload by four in order to annualize the figures. The weights in Table A.3 represent the number of hours a judge is expected to spend on a bankruptcy case, and therefore the caseload statistics presented in this table can be interpreted as the total number of hours a judge will spend administering cases per year. *BAPCPA caseload drop* is defined as the difference in the average caseload from 2004Q1 to 2005Q4 and the average caseload from 2006Q1 to 2007Q4 for each district. In the last two lines of the table, the sample is split into those firms that had below- and above-median share of nonbusiness caseload in 2003, to show that the drop was significantly larger in those districts that had fewer business bankruptcy filings.

dropped to the lowest levels on record, and remained depressed for some time, leaving judges with substantially fewer cases on their dockets. Bankruptcy judges do not specialize in a particular type of bankruptcy. Because of this, *all* judges were affected by the rush to file and subsequent dearth of consumer bankruptcy filings. In effect, BAPCPA created a natural shock to bankruptcy caseloads faced by courts across the nation.

The drop in personal bankruptcy filings was both large and long lasting. In 2004–2005, before BAPCPA took effect, the average caseload for bankruptcy judges was 1,059 hours, while in the two years after BAPCPA average caseload was only 566 hours (Table 1). In essence, BAPCPA halved the caseloads faced by bankruptcy courts, and filings stayed low well into 2008 (Figure 1, panel A).

Although BAPCPA was focused on personal bankruptcy, it included three main provisions that affected Chapter 11 restructuring as well. First, the law capped extensions of the exclusivity period—the amount of time that the debtor has the exclusive right to file a plan of reorganization—at 18 months total, while previously extensions were unlimited. It also limited the window within which the debtor has to decide whether it will assume or reject leases on commercial property. Second, BAPCPA imposed penalties on repeat filers. Firms that refile for bankruptcy within one year after reorganizing have the automatic stay lifted after 30 days unless the court grants an extension. Third, BAPCPA made "prepackaged" bankruptcy filings more attractive by allowing the solicitation of votes on the prearranged plan to continue while the firm formally files for bankruptcy.

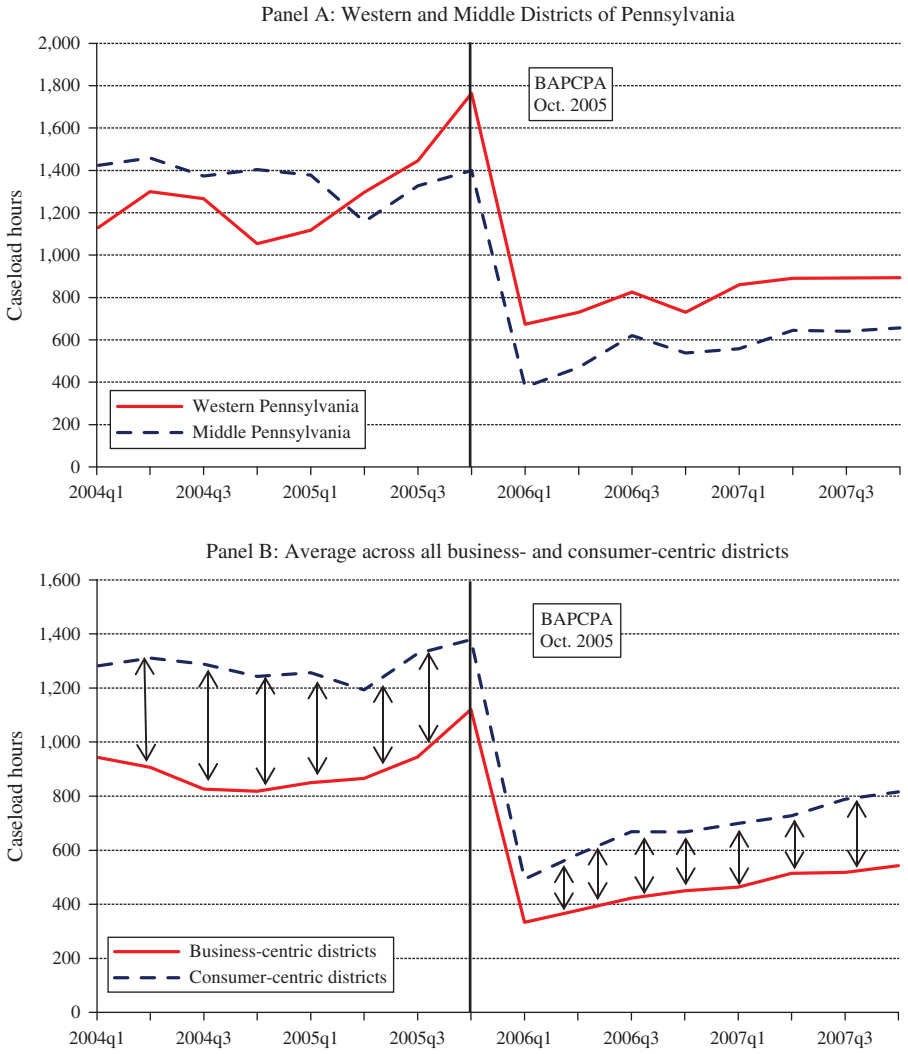
These alterations to the law do not appear to have altered the Chapter 11 filing rate in an economically significant way (Figure 1, panel B). By the first quarter of 2006 the number of filings was nearly identical to the number in the third quarter of 2005. While a few firms rushed to file just before the October deadline,[14] they tended to be very small firms that were also affected by the changes to personal bankruptcy laws. These firms are excluded from my sample (see Section 4). In my sample, there is no observable change in the Chapter 11 bankruptcy filing rate around the passage of BAPCPA.

Because BAPCPA affected some aspects of the Chapter 11 process, and to avoid possible impacts of time effects,[15] I do not simply compare cases that were filed before and after the law to test the impact of caseload on bankruptcy outcomes. Instead, I employ a difference-in-differences framework that focuses on bankruptcy districts that were disproportionately affected by the law. Because of BAPCPA's focus on consumer bankruptcies, its passage caused a disproportionately larger drop in caseload in those districts that spend more of their time on nonbusiness bankruptcy filings. I use the share of caseload that stems from nonbusiness filings in 2003 as a measure of how consumer-oriented each court is.[16] A bankruptcy district that spends the majority of its time on personal bankruptcies saw its workload drop by more because of BAPCPA.

For example, panel A of Figure 3 shows the differential impact of BAPCPA in two bordering bankruptcy districts, the Western and Middle Districts of Pennsylvania. Because Western Pennsylvania takes in Pittsburgh, its bankruptcy court is more business oriented. In 2003, nonbusiness bankruptcies accounted for 67% of total caseload in Western Pennsylvania, while the nonbusiness share of caseload in the Middle District was 83%. Because of this, when BAPCPA passed and the nonbusiness filing rate dropped, caseload dropped by more in the consumer-centric Middle District than in the Western District. Specifically, caseload in the Middle District dropped by about 800 hours after BAPCPA, as compared to a drop of only 485 hours in the Western District.

This pattern holds across for the full sample. Panel B of Figure 3 plots the average caseload of consumer-centric bankruptcy districts—defined as those districts that

Figure 3. (Color online) BAPCPA's Effect on Consumer- and Business-Centric Bankruptcy Districts



Notes. This figure shows how court caseload evolved in consumer- and business-centric districts from 2004 to 2007. Panel A uses an example of two neighboring bankruptcy districts: the Western and Middle Districts of Pennsylvania. The Middle District of Pennsylvania spends about 83% of its time on consumer bankruptcy cases, as compared to 67% in the Western District. BAPCPA decreased caseload by substantially more in the consumer-centric Middle District. Panel B shows a similar pattern for all 89 bankruptcy districts. In this chart, districts with an above-median nonbusiness share of caseload are classified as "consumer centric," while the remaining districts are "business centric." The average caseload for each group is then plotted in the solid and dotted lines over time. Because BAPCPA disproportionately impacted the consumer-centric groups, the difference between the two lines (indicated by the arrows) shrinks by nearly half after its passage.

had an above-median nonbusiness share of caseload in 2003—versus the caseload of the more business-centric courts. Importantly, the two sets have parallel trends before and after BAPCPA, but the consumer-centric courts experienced a larger drop in caseload when BAPCPA took effect. This can be seen even more clearly in Figure 4, which shows a scatter plot comparing the drop in caseload from before BAPCPA (2004–2005) to after BAPCPA (2006–2007) against the nonbusiness share of 2003 bankruptcy caseload in each district. The positive relationship between nonbusiness caseload and the impact of BAPCPA is quite robust.[17] This is formally tested in a regression setting in Table 2. Without accounting for any other variables, a one standard deviation increase in the nonbusiness share of caseload (increase of 11.5 percentage points) results in an additional caseload decrease of 64 hours following BAPCPA, a drop of 12%. This effect persists after controlling for other factors that impacted caseloads. Aside from affecting filing rates for personal bankruptcy, BAPCPA also created 28 new judgeships, which resulted in decreased caseloads per judge in 20 affected districts. Including a control for the number of new judges appointed following BAPCPA strengthens the relationship between nonbusiness caseload share and the decrease in workload.[18] In this specification, a bankruptcy district with a one standard deviation higher share of nonbusiness caseload experienced

Table 2. Decrease in Caseload Due to BAPCPA in Consumer-Centric Districts

Dependent variable	*BAPCPA caseload drop*		
Nonbusiness caseload (2003)	555.076**	772.055***	714.222***
	(257.969)	(218.711)	(229.337)
No. of new judges	—	108.906**	146.058***
		(44.580)	(38.557)
Change in unemployment rate	—	—	4.907
			(51.278)
Change in house price appreciation	—	—	−1,037.417***
			(252.373)
Growth in per capita income	—	—	200.056
			(1,000.379)
Population growth	—	—	1,043.399
			(979.314)
Observations	89	89	89
R^2	0.089	0.204	0.325

Notes. This table shows that bankruptcy districts that had a higher share of nonbusiness cases in 2003 experienced larger declines in caseload following BAPCPA. In each regression, the dependent variable is the drop in caseload following BAPCPA, defined as the difference in the average caseload from 2004Q1 to 2005Q4 and the average caseload from 2006Q1 to 2007Q4 for each bankruptcy district. *Nonbusiness caseload* (2003) is the share of weighted caseload in 2003 that was attributable to nonbusiness bankruptcy filings. In the second column, I control for the number of new judgeships that were created by BAPCPA (28 judgeships in 20 districts). In the final column, controls are added for changes in economic conditions and total population from the pre-BAPCPA period (2004–2005) to the post-BAPCPA period (2006–2007). All regressions are estimated by regular OLS, and robust standard errors are reported in parentheses.

***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

an additional caseload drop of 90 hours following BAPCPA, or more than two full work weeks. Controlling for changes in unemployment, house prices, per capita income, and population in each bankruptcy district does not affect the relationship between nonbusiness caseload and the BAPCPA shock. This is important, as it shows that, although caseload is affected by changes in economic conditions (e.g., changes in house prices), the variable that I use to identify the effect of BAPCPA is orthogonal to these factors.

The identification of particular bankruptcy districts that were disproportionately affected by BAPCPA allows me to estimate difference-in-differences regressions of the form

$$Y_i = \beta(PostBAPCPA_t \times NonBusCaseload_d) + \gamma X_i + \tau_d + \mu_t + \varepsilon_i,$$

where Y_i is the outcome of interest for bankruptcy filing i, in bankruptcy district d, in month t, and X_i is a vector of firm characteristics, τ_d is a bankruptcy district fixed effect, and μ_t is a time fixed effect. The coefficient of interest is β, which captures the impact of filing in the post-BAPCPA period when bankruptcy caseloads were low, in districts that experienced the largest declines in bankruptcy caseload. For ease of explanation, I will refer to the interaction term $PostBAPCPA_t \times NonBusCaseload_d$ as "*Low caseload court*" in the remainder of the paper. $PostBAPCPA_t$ and $NonBusCaseload_d$ are not included separately in the regression because their effects are absorbed completely by the time and bankruptcy district fixed effects.

I include in my sample firms that filed for Chapter 11 between January 1, 2004 and December 31, 2007, a time period centered on the passage of BAPCPA that ends before the main increase in caseload due to the onset of the financial crisis in 2008. Following Bertrand et al. (2004), in all specifications I cluster standard errors by bankruptcy district in order to account for serial correlation within bankruptcy courts.[19]

While the above specification captures the overall effect of BAPCPA, one would expect that the impact of a drop in caseload varies depending on the complexity and relative bargaining power of the bankrupt firm. Large firms in particular are more complex and have a stronger presence in court because they are better able to hire top-notch lawyers, demand more of their creditors and suppliers, and are also more likely to get press coverage should they fail. To empirically test whether caseload fluctuations differ by the size of the firm, I add another interaction term to the regression equation:

$$Y_i = \beta_0(PostBAPCPA_t \times NonBusCaseload_d) + \beta_1(PostBAPCPA_t \times NonBusCaseload_d \times \ln(Size_i)) + \gamma X_i + \tau_d + \mu_t + \varepsilon_i,$$

where $Size_i$ is the assets or liabilities of the firm (whichever is largest) at filing.[20] Here, the coefficient β_1 captures the differential effect that the law had on large firms, while β_0 captures the estimated impact of BAPCPA on a firm of $Size_i$ = \$1 M. To isolate the true impact of the triple interaction term, in these regressions I include $PostBAPCPA_t \times \ln(Size_i)$ and $NonBusCaseload_d \times \ln(Size_i)$ as additional controls in X_i

The difference-in-differences estimator shows the causal impact of caseload on bankruptcy outcomes only if two conditions hold: (i) there are parallel trends across bankruptcy districts prior to BAPCPA and (ii) the exclusion restriction is not violated. I test for parallel trends in Figure A.3 in the online appendix by regressing each Y_i on interactions between $NonBusCaseload_d$ and year-quarter dummies for each quarter before and after BAPCPA. I then plot the coefficients from these interactions for each quarter in my sample period in the figure. Similarly, I display the coefficients on interactions between $NonBusCaseload_d \times \ln(Size_i)$ and year-quarter dummies to test for pretrends that vary by firm size. In all cases there do not appear to be significant pretrends.

In terms of the exclusion restriction, the confound that one worries about is whether firms that file in consumer-centric bankruptcy districts all changed in

Figure 4. (Color online) Business Caseload and the BAPCPA Caseload Drop

Notes. This figure plots the decrease in caseloads due to BAPCPA against the nonbusiness share of caseload in 2003 for each of the 89 bankruptcy districts in my sample. The drop in caseload is calculated as the average caseload in the district during 2004–2005 less the average caseload in 2006–2007. The nonbusiness share of caseload is the share of weighted caseload in 2003 that is due to nonbusiness bankruptcy filings. Districts shown in red also received new judgeships with the passage of BAPCPA, and consequently had larger drops in caseload than would otherwise be expected.

a particular way after BAPCPA, and that this change was unrelated to judge caseload. I directly examine how firm characteristics evolved around BAPCPA in both business-centric and consumer-centric districts in Table 3, panel D.[21] The table confirms that, consistent with intuition, larger and more complex firms tend to file in business-centric districts. Further, it shows that average size declines after BAPCPA. Importantly, this trend is present in both business-centric and consumer-centric districts. Similarly, public firms and firms with multiple filings make up a smaller portion of the sample after BAPCPA in all districts. The only characteristic that does not show a similar trend across the two district types is firm leverage. In consumer-centric districts the share of firms that has liabilities greater than assets when filing for bankruptcy dropped from 68.2% to 60.0% after BAPCPA, while in business-centric districts it remained constant at 60.0% in both time periods. In all regressions I control for all firm characteristics, including leverage, in order to account for any differences along these dimensions.

Further, I test for correlation between firm characteristics and $PostBAPCPA_t \times NonBusCaseload_d$ by using each firm characteristic in X_i as the dependent variable in regressions similar to those described above. These tests determine whether the composition of Chapter 11 filers changed after BAPCPA in a systematic way that is directly related to my main cross-sectional variable. As shown in Online Appendix C, all firm characteristics are unrelated to $PostBAPCPA_t \times NonBusCaseload_d$. Moreover, including the interaction of all variables in X_i and $PostBAPCPA_t$ as additional controls, which allows the effect of any firm characteristic to vary after BAPCPA, does not impact any of my estimates.

In Online Appendix C I discuss further tests for violations of the exclusion restriction such as including size-by-time fixed effects and industry-by-time fixed effects. These additional controls allow for there to be varying trends over time for different firm sizes or industries, thus ruling out alternative stories that relate to the composition of firms filing in particular bankruptcy districts. In all cases, including these controls does not affect my estimates. I also present evidence that consumer-centric and business-centric courts imposed new deadlines set by BAPCPA similarly, thereby ruling out the possibility that my effects are driven by different interpretations of the bankruptcy reform across bankruptcy courts.

The exclusion restriction would also be violated if economic conditions in consumer-centric bankruptcy districts varied differently from business-centric districts after BAPCPA in a way that altered the outcomes of bankruptcies in those districts, either because the firms themselves are different or because judges treat them differently because economic conditions have changed (Ichino et al. 2003). In particular, one might

Table 3. Summary Statistics

Panel A: Continuous variables

	Obs.	Mean	Std. dev.	5th percentile	Median	95th percentile	Max
Dependent variables							
Months in bankruptcy	3,282	18.30	16.60	1.55	13.03	52.83	93.91
Sale price/Assets	417	0.588	2.627	0	0.225	1.127	42.00
Control variables							
Size at filing	3,282	$156.64	$5,339.28	$0.70	$4.33	$111.00	$301,816
Winsorized size *at filing*	3,282	$27.97	$93.08	$0.71	$4.33	$111.00	$735.82
Other descriptive stats:							
Assets at filing	3,282	$142.96	$5,320.65	$0.05	$2.04	$71.50	$301,816
Liabilities at filing	3,282	$60.71	$836.03	$0.50	$3.43	$100.00	$28,270
Employees (when available)	886	1,092.21	6,708.49	8	118.5	3,212	146,600
Liabilities/Assets	3,247	13.61	95.45	0.33	1.30	34.00	3,558.99
No. of entities filing together	3,282	1.694	3.990	1	1	4	133

Panel B: Binary variables

	Obs.	% obs.
Dependent variables		
Outcome		
Reorganized	3,282	29.74
Liquidated	3,282	35.95
converted to Chapter 7	3,282	28.09
in Chapter 11	3,282	4.36
section 363 sale of all assets	3,282	6.85
Dismissed	3,282	34.31
Has asset sale	3,282	12.80
Refiles for bankruptcy within three years	2,102	5.66
Reorganized	976	3.07
Dismissed	1,126	7.90
Prepackaged bankruptcy	3,282	1.40
Obtained DIP loan	3,282	15.75
Control variables		
Liabilities > Assets	3,282	61.00
Public firm	3,282	4.87
Has related filings	3,282	14.17

be concerned that BAPCPA itself might have altered economic conditions in areas with more household bankruptcies by limiting the ability of consumers to enter bankruptcy. If this were the case, one would expect there to be a significant correlation between my difference-in-difference estimator and measures of local economic conditions. However, as mentioned above, including controls for unemployment, house price changes, and per capita income in Table 2 does not affect the relationship between $NonBusCaseload_d$ and the drop in caseload after BAPCPA. Further, I explicitly test for significant correlation between these economic indicators and $PostBAPCPA_t \times NonBusCaseload_d$ by using the economic variables as dependent variables in regressions similar to those outlined above. The coefficient on $PostBAPCPA_t \times NonBusCaseload_d$ is insignificant in all cases. Finally, including them as controls in the regressions presented in Section 5 does not affect my estimates.[22]

A final concern relates to forum shopping. Some firms do have discretion in choosing the bankruptcy district where they file, and therefore they could move to a different venue if low or high caseloads in a particular court will adversely affect their outcome. This selection effect could potentially bias my estimates. As described in Section 4, my sample consists mostly of midsize firms that do not have a choice in venue. Regardless, I can use the address of the firm to identify debtors that file in nonlocal bankruptcy districts, and take that as an indicator of firms that picked an alternate venue. I find that 8.7% of the filings in my sample occurred in states other than the home state of the debtor. Omitting these firms from the sample does not alter my results.

4. Data

I gather information on Chapter 11 bankruptcy filings from LexisNexis Law, which obtains bankruptcy filing data from the U.S. Courts system. I focus on a four-year period surrounding the passage of BAPCPA, from 2004 to 2007. I end the sample in 2007 to avoid the sharp uptick in caseload that resulted from the

Table 3. (Continued)

Panel C: Commercial banks

	Obs.	Mean	Std. dev.	5th percentile	Median	95th percentile	Max
Dependent variables							
Net C&I loan charge-offs							
% of total C&I loans	29,012	0.51	1.43	−0.33	0.02	2.92	9.33
% of max. C&I nonperforming loans	22,008	36.41	108.86	−36.79	8.47	182.61	704.17
Control variables							
Annual asset growth	29,012	7.67	12.00	−6.32	5.42	30.12	63.27
Net charge-off rate on all other lending							
% of total loans	29,012	0.16	0.34	−0.43	0.06	0.70	2.44
% of max. nonperforming loans	22,008	20.51	43.23	−5.28	8.87	84.29	350.00

Panel D: Firm characteristics across districts

	Business-centric districts		Consumer-centric districts	
	Pre-BAPCPA	Post-BAPCPA	Pre-BAPCPA	Post-BAPCPA
Observations	498	1,132	443	1,209
Dependent variables				
Months in Bankruptcy	23.81	16.17	23.71	16.04
Outcome				
Reorganized (%)	36.55	27.39	35.89	26.88
Liquidated (%)	36.75	38.25	35.67	33.58
Dismissed (%)	26.71	34.36	28.44	39.54
Has asset sale (%)	18.07	12.10	15.58	10.26
Refiles for bankruptcy within three years (%)	6.67	5.72	3.51	5.98
Reorganized (%)	3.30	4.19	1.89	2.46
Dismissed (%)	11.28	6.94	5.56	8.37
Prepackaged bankruptcy (%)	4.42	1.50	0.68	0.33
Obtained DIP loan (%)	23.29	14.84	21.67	11.33
Control variables				
ln(*Size*)	2.49	1.48	2.11	1.30
Liabilities > Assets (%)	59.84	59.89	68.17	59.88
Public firm (%)	10.84	3.55	8.14	2.45
Has related filings (%)	22.09	12.99	19.41	10.09

Notes. This table provides summary statistics of the characteristics of the bankruptcy cases in the sample. Panels A and B pertain to data used on bankruptcy filings. In panel A, *Size* is defined as the maximum of assets or liabilities reported at filing. Panel C provides information on the commercial bank panel data used in Section 5.1. All variables in panel C have been winsorized at the 1st and 99th percentiles. Panel D shows summary statistics split by business-centric districts (those with a below-median share of nonbusiness caseload) and consumer centric, both before and after BAPCPA.

financial crisis in 2008 and 2009, and also to have a three-year period (2008–2010) in which I can examine recidivism into bankruptcy for firms that file near the end of my sample. During this period, LexisNexis has legal information on 14,825 separate business Chapter 11 bankruptcy filings in the 50 states and the District of Columbia. Because LexisNexis' data come directly from the U.S. Courts, there is essentially 100% coverage of Chapter 11 cases in my data. The benefit of using data from LexisNexis is that it is more easily obtained for the entire, nationwide set of bankruptcies. While several previous bankruptcy studies have used court records to compile data on bankruptcies, due to the difficulty of obtaining these data directly from the U.S. Courts, these studies have been limited in scope, typically focusing only on a subset of bankruptcy districts or only on public firms, which have more information readily available. To my knowledge, this is the first study to make use of LexisNexis' universal coverage.

The LexisNexis data contain legal information from the U.S. Courts system, including the date the case was filed, the court in which it was filed, the judge assigned to the case, a flag indicating whether the debtor has distributable assets, and information on the outcome of the case: whether it was dismissed from court, converted to Chapter 7, transferred to another court, or reorganized.

I augment this legal information with financial data obtained from Capital IQ and The Deal Pipeline. From these two sources, I obtain the full list of firms that filed for Chapter 11 bankruptcy in their databases, and match them to LexisNexis using bankruptcy case number, filing date, company name, and address. Using this information, I am able to match over 99% of Chapter 11 cases in Capital IQ and The Deal Pipeline during my sample period. From Capital IQ and The Deal Pipeline, I obtain the assets and liabilities reported by the firm at the time of the bankruptcy filing, the industry of the

firm, and a flag indicating whether the firm obtained DIP financing. I also use the text in the description of the bankruptcy to determine whether the firm filed with a prearranged or "prepackaged" bankruptcy plan.

Between Capital IQ and The Deal Pipeline, I match a total of 7,223 firms to LexisNexis, which makes up 49% of the 14,825 total bankruptcy filings between 2004 and 2007. To get the final sample, I remove firms that are transferred to other courts, for which there is no exit information in LexisNexis, or that were involuntary bankruptcy filings (706 firms).[23] Finally, about half of the filings recorded in Capital IQ or The Deal Pipeline are missing information on industry, assets, or liabilities, reducing my final sample to 3,282 firms, or 22% of all firms that filed for bankruptcy during the sample period.

Because I rely on financial information in Capital IQ and The Deal Pipeline, which do not have information on the smallest firms, the sample used in this study is composed of larger, more complex firms than the overall sample of Chapter 11 filers. For example, 14.1% of the firms in my sample filed jointly with related entities, while only 6.9% of the out-of-sample firms did so. The larger firms in my sample are precisely the cases in which judges are needed to mediate complex negotiations, determine just outcomes, and discern when liquidation is the optimal path for a firm. In addition, these larger firms are those that are least likely to be directly affected by changes to personal bankruptcy laws enacted by BAPCPA (Fan and White 2003). As a result, there is no "rush to file" before BAPCPA among firms in my sample; the filing rate is completely flat through 2005 and 2006.

Although my sample is limited only to those firms that are in Capital IQ or The Deal Pipeline, it still contains a significant number of smaller firms. Table 3 provides summary statistics on the bankrupt firms in my sample. The median firm reports $2.06 million in assets and $3.5 million in liabilities at filing, while roughly 10% of my sample has either assets or liabilities of less than $1 million. On the other extreme the firm at the 90th percentile has assets or liabilities of about $50 million.

Firms may try to underreport the true value of their assets in order to appear more in need of bankruptcy protection than they really are. Because of this, for many debtor firms total liabilities is likely a better measure of the size of the firm than total assets. To overcome this issue, I define a new variable *Size*, equal to the maximum of either assets or liabilities at filing, to capture the true scale of the firm. The median firm has a *Size* of $4.4 million, but the distribution contains a few outliers (e.g., Delta Airlines) that skew the average *Size* to a much larger $156.7 million. In all regressions I use the natural log of *Size* to decrease the influence of these outliers, and in Online Appendix C, I describe robustness checks that verify that these outliers are not driving my results.[24]

Based on the description of the bankruptcy in Capital IQ or The Deal Pipeline, I only find that 47 (1.4%) of the firms in my sample filed prepackaged plans. When a firm has a prepackaged plan, the judge has very little to do in the case, and so in most of my empirical results I omit these firms from the sample. Unconditionally, debtors are a bit more likely to be liquidated (36.1%) or dismissed (34.1%) than reorganized (29.8%). Liquidation can come in three different forms, however: conversion to Chapter 7 (28.1%), liquidation directly from Chapter 11 via a "liquidating plan"[25] (4.4%), or the sale of substantially all assets of the firm via a section 363 sale (6.9%). These are not necessarily mutually exclusive; a firm that sells all of its assets in a section 363 sale is often subsequently liquidated via Chapter 7 or a Chapter 11 liquidating plan.

I measure recidivism rates as the propensity to file for either Chapter 7 or Chapter 11 bankruptcy within three years of the original filing date of the bankruptcy. To identify repeat filers, I use information on all business bankruptcy filings (either Chapter 7 or Chapter 11) from LexisNexis from 2004 to 2010, and match the original Chapter 11 filings to future Chapter 7 or 11 filings using tax ids, firm names and aliases, and addresses of the bankrupt firms. Limiting to a three-year window avoids time effects; firms that file for bankruptcy in 2004 have a much longer time period in which to refile than those that file in 2007, and will thus naturally have a higher recidivism rate if the whole time period is examined. Also, I do not count firms that refile within three months of their original filing as having refiled, since these can hardly be considered "separate" bankruptcies; these firms likely exited court due to unusual circumstances (e.g., they were dismissed for failing to file the proper paperwork) and quickly refiled once the issue was resolved. The three-month cutoff is somewhat arbitrary; my results are identical if I use a two-month or four-month cutoff instead. On average, 2.5% of reorganized firms and 7.4% of dismissed firms refile for bankruptcy within three years of their original filing in my sample.[26] It is important to note that I can only measure recidivism *into bankruptcy*; firms that exit bankruptcy and then liquidate are not observed and therefore not included in the rate, although this could also be viewed as a failure of the bankruptcy process. I cannot test whether firms that file in busy bankruptcy courts are more likely to liquidate outside of court following their restructuring, only whether their recidivism rate is higher. Online Appendix A compares my recidivism rate to that found in other studies.

Both Capital IQ and The Deal Pipeline maintain databases of bankruptcy sales transactions. I use these

databases to identify firms that sell assets during the course of bankruptcy, and find that 13% of the firms in my sample have an asset sale recorded. In many cases it is difficult to determine exactly which assets were sold in the auction; the transaction might list a particular piece of property or a division of the company, or it might just list the name of the company. In 53% of the sales (228 cases), however, the phrase "substantially all assets" is used in the description of the asset, signifying that in these cases the entire firm was sold. I mark these firms as having been liquidated completely.

Loan-level and firm-level recovery rates are not available for private companies, which comprise the majority of my sample. Instead, I turn to regulatory data reported by U.S. commercial banks in the Consolidated Report of Condition and Income (commonly known as the Call Reports). From the end-of-year Call Reports from 2004 to 2007, I obtain information on the net charge-offs reported by each bank on its C&I lending. *Net charge-offs* are calculated as the total amount written off during the year less any recoveries received and hence represent the aggregate loss sustained by the bank. In addition to this main dependent variable, I also collect information on asset growth and the net charge-off rate on all other lending at the bank. To avoid undue influence of outliers, I winsorize each of these variables at the 1st and 99th percentiles. Summary statistics on the banks used in my sample are presented in Table 3, panel C.

The exposure of each bank to the BAPCPA shock depends on its location; banks in consumer-centric districts saw caseloads drop by more after BAPCPA than those located in business-centric districts. Using the FDIC's Summary of Deposits data from 2003, I first determine the share of a bank's deposits that were located in each bankruptcy district in that year. I then calculate the weighted average nonbusiness share of caseload across all bankruptcy districts in which a bank has deposits, using the share of deposits in each district as a weight. This weighted average of nonbusiness caseload then acts as a proxy for the size of the caseload shock experienced by the bank following BAPCPA.[27]

Finally, as outlined above, panel D of Table 3 shows summary statistics separately for firms in business-centric and consumer-centric districts both before and after BAPCPA. These are provided to show how firm characteristics change in relation to the identification strategy used in Section 5.

5. The Effect of Caseload on Chapter 11 Restructuring

In this section, I test how court caseload affects bankruptcy outcomes, focusing on its effect on default costs in Section 5.1, and then on the channel through which those costs are reduced in the remaining sections.

5.1. Bank Charge-offs

If less busy courts reduce the costs of restructuring, these cost savings will be largely passed on to creditors, since bankrupt firms have little or no equity. Because banks are the main creditors for many small and mid-sized businesses (Petersen and Rajan 1994), I examine net charge-offs on C&I loans reported by commercial banks as measures of the default costs borne by creditors. Importantly, since the majority of C&I loans are unsecured, one would expect losses to be concentrated in this lending.[28]

As described in Section 4, I use a bank's exposure to bankruptcy districts with higher nonbusiness caseload as a proxy for banks whose clients experienced exogenously lower caseloads post-BAPCPA. Banks whose branches are located in more consumer-centric bankruptcy districts are likely to lend to businesses that are also located in those districts, and thus these banks would have seen caseloads drop by the largest amount after BAPCPA.[29]

In Table 4, I report panel regressions that contain annual data for 7,741 commercial banks from 2004 to 2007. These regressions use two separate measures of C&I loan default costs as dependent variables. The first is *Net charge-off rate*, defined as total net charge-offs on C&I loans reported by the bank in a particular year, scaled by the average total outstanding C&I lending reported across the four quarterly reports during the year. I use the average of C&I lending over the year to give a better measure of the total amount of C&I lending typically done by the bank, and to help account for the fact that credit losses can be reported with a lag. However, my results are unchanged if I scale by C&I lending reported at the end of the year, or averages over longer periods of time.

Scaling charge-offs by total C&I lending makes *Net charge-off rate* an estimate of the probability of default multiplied by the loss given default, i.e., total expected losses. Because busy bankruptcy courts principally affect the loss given default (not the probability of default), Table 4 also contains regressions where the dependent variable is *Net loss given default*, a proxy for the loss given default calculated by scaling net charge-offs on C&I loans by the maximum of nonperforming C&I loans during the year. Loss given default cannot be measured exactly using Call Report data because charge-offs are not directly tied to nonperforming loans. I proxy for loss given default by scaling net charge-offs by the maximum of nonperforming loans in a given year. Dividing by the maximum of nonperforming loans during a year gives a better proxy than dividing by the end-of-year amount because once a charge-off is recognized the stock of nonperforming loans decreases (since the charged-off loan is no longer nonperforming). Online Appendix B gives more detail

Table 4. The Effect of Caseload on C&I Loan Charge-offs

Commercial and industrial (C&I) loans				
	Net charge-offs on C&I loans			
Dependent variable:	*% of total C&I loans*		*% of maximum nonperforming C&I loans*	
Low caseload court	−0.437**	−1.099	−34.219*	−139.705***
	(0.194)	(0.730)	(20.065)	(50.420)
Low caseload court × ln(*Bank assets*)	—	0.110	—	17.145**
		(0.121)		(7.583)
PostBAPCPA × ln(*Bank assets*)	—	−0.088	—	−11.576*
		(0.095)		(5.967)
Nonbusiness caseload share × ln(*Bank assets*)	—	0.162*	—	17.735**
		(0.098)		(8.150)
Asset growth	−0.382***	−0.438***	−6.518	−12.365
	(0.097)	(0.099)	(8.498)	(8.919)
Net charge-off rate on all other loans	0.664***	0.660***	0.034	0.034
	(0.068)	(0.068)	(0.030)	(0.030)
ln(*Per capita income*)	−0.822	−0.945*	23.156	1.043
	(0.549)	(0.555)	(50.522)	(51.329)
ln(*Population*)	−2.336***	−2.625***	−65.803	−99.808
	(0.756)	(0.773)	(79.089)	(80.310)
Unemployment rate	0.067**	0.068**	2.037	2.249
	(0.030)	(0.030)	(2.796)	(2.809)
House price appreciation	−0.194	−0.160	−84.947***	−72.291***
	(0.226)	(0.228)	(26.195)	(26.662)
Fixed effects				
Bank	Yes	Yes	Yes	Yes
Year	Yes	Yes	Yes	Yes
Observations	29,012	29,012	22,008	22,008
Adjusted R^2	0.187	0.187	0.099	0.100

Notes. This table shows how changes in caseload affected the performance of C&I and CRE loans held by commercial banks. These panel regressions use regulatory data reported by commercial banks at year-end from 2004 to 2007. The dependent variable is defined as the total charge-offs on C&I loans reported by the bank during the calendar year less any recoveries received on C&I loans, as a percentage of the average total outstanding balance of C&I loans held by the bank over the year or the maximum reported nonperforming C&I loans during the year. *Low caseload court* is defined as the interaction of a *post-BAPCPA* dummy, equal to one for all 2006 and 2007 observations, and *Nonbusiness caseload*. Because some banks have branches in multiple bankruptcy districts, *Nonbusiness caseload* in this table is defined as the weighted average nonbusiness share of court caseload across all districts in which the bank had deposits in 2003. The share of deposits held in each bankruptcy district serves as the weight in this average. *Asset growth* is defined as the log difference in assets from the previous year. *Net charge-off rate on all other loans* is defined similarly to the dependent variable. All regressions include fixed effects for the 7,741 banks included in the sample as well as year fixed effects. All models are estimated by OLS. Standard errors are clustered by bank to account for serial correlation across years, and are reported in parentheses. In this and future tables, shaded rows highlight the key coefficients that identify the impact of caseload on bank and firm outcomes.

***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

on this issue, and Online Appendix C shows that scaling by average nonperforming loans over the year gives nearly identical results.

In all specifications I include both bank and year fixed effects, and cluster the standard errors by bank in order to account for serial correlation within each bank. I also control for the asset growth at each bank and the net charge-off rate on all other loans, as well as general economic conditions in the area that might affect loan performance.[30] I winsorize all bank-level variables at the 1st and 99th percentiles to prevent undue influence from outliers.

Consistent with the idea that busy bankruptcy courts impose higher restructuring costs, in Table 4 I find that banks that were located in bankruptcy courts with exogenously lower caseloads experience lower C&I loan charge-offs relative to banks in busier courts. The interaction term *Low caseload court* × ln(*Bank assets*) indicates that the effect is strongest for smaller banks, especially when default costs are measured using *Net loss given default*, consistent with the idea that smaller banks are most likely to lend to local firms.

The analysis in Table 4 focuses on C&I loan charge-offs. In Table A.6 in the online appendix, I show that

caseload does not affect commercial real estate (CRE) loan charge-off rates. Because CRE loans are secured, they should be more insulated from default costs, and thus these tests serve as a placebo test to verify that it is not the case that commercial loan performance in general is declining in consumer-centric courts after BAPCPA. Instead, lender losses appear to be concentrated among junior and unsecured loans, such as C&I lending.

There are several ways to quantify the economic magnitude of the effect of court caseload on C&I loan charge-off rates. As described in Section 3, a one standard deviation increase in the nonbusiness share of caseload (11.5%) is associated with an additional 64.1 hour drop in caseload following BAPCPA. Thus, I estimate that on average, a 64-hour decrease in caseload reduces *Net charge-off rate* by an average of 5 basis points ($11.5\% * -0.437 = 5.03$), a 10% decline from the unconditional mean of 51 basis points. The same reduction in caseload causes the *Net loss given default* to fall by 3.9 percentage points, an 11% decline relative to its mean of 36%. The fact that the estimated gains are nearly identical across the two measures also shows that nearly all of the effect is due to a decrease in loss given default, rather than a decline in the probability of default, as expected.

A 10% decline in charge-offs is a relatively large effect for a 64-hour decline in caseload. However, a few factors are important to keep in mind. First, the regressions in Table 4 give equal weight to all banks, but also show that the largest banks are less affected by court caseload. In unreported regressions, I find that the effect size is about 15% smaller if the regressions are value weighted. Further, this is the effect on C&I loan charge-offs only. Indeed, as I argue above, C&I loans should be among the most sensitive to caseload changes because they are typically unsecured and thus in the middle of the capital structure. Meanwhile, recovery rates for other creditors, such as CRE or equity, are likely unaffected by court caseload, and thus the effect of court congestion on overall recovery rates is likely substantially smaller.

On the other hand, a 64-hour shock is relatively small compared to many of the caseload changes that occur in bankruptcy courts. Nationwide, weighted caseload per judge has risen on average by 305.6 hours in the two years following the midpoint of an economic recession (as defined by the National Bureau of Economic Research). Meanwhile, variation across bankruptcy districts tends to be more substantial. If one ranks the 89 districts by their average caseload since 1980, moving from the district at the 25th percentile (Hawaii) to the 75th percentile (Utah) results in an increased caseload of 457 hours. The standard deviation across all 89 districts is 361 hours.

While caseload changes on the order of 300 hours are commonly observed, a shock of this size is outside the context of the difference-in-differences regressions using BAPCPA. If I assume that the impact of caseload scales linearly, then a rough estimate is that a 300-hour decline in caseload, which is 4.7 times larger than the 64-hour decline mentioned above, would reduce loan losses by about 50% relative to their mean levels. However, it is difficult to say whether a nonlinearity in the relationship between caseload and bankruptcy outcomes might affect the estimates when extrapolating in this way.[31] Thus, to be conservative I will focus on a caseload change of 64 hours to discuss the economic magnitude of my estimates in the remainder of the paper. But I will also occasionally refer to a change of 300 hours to put the results into context of the typical variation in caseload.

5.2. Bankruptcy Outcomes: Reorganization, Liquidation, or Dismissal

I now focus on how caseload is related to the restructuring process, using 3,326 bankruptcy filings in the LexisNexis data set. In this section, I first focus on estimating the effect that decreased caseloads following BAPCPA had on the outcome for firms in Chapter 11. In general, a firm that files for Chapter 11 bankruptcy can have one of three outcomes:

1. *Reorganization*: A restructuring plan is formed and accepted, previous debtors are paid according to the plan, a new capital structure is put in place, and the debtor emerges from bankruptcy.

2. *Liquidation*: The debtor's case is converted to Chapter 7, the debtor is liquidated directly from Chapter 11, or the debtor's assets are sold as part of a Chapter 11 bankruptcy auction.

3. *Dismissal*: The case is dismissed and the debtor remains as if no bankruptcy filing had occurred.

I exclude from these regressions firms that filed with prepackaged bankruptcy plans, since the outcome of these cases is usually predetermined, leaving the court with little to do. In these models, I also control for firm size, leverage,[32] whether multiple subsidiaries filed jointly, public/private status, whether the firm received a DIP loan, and month, industry, and bankruptcy district fixed effects. These control variables are described in Table A.4 in the online appendix.

In the results presented in Table 5, there are two main coefficients of interest. First, the variable *Low caseload court*, defined as the interaction of a post-BAPCPA dummy and the nonbusiness share of caseload in the bankruptcy district, captures the effect of filing in districts that experienced the largest decreases in caseload following BAPCPA. Because my estimates include both industry and month fixed effects, the coefficient on *Low caseload court* effectively compares two firms from the same industry that filed for bankruptcy in the same

Table 5. The Effect of Caseload on Bankruptcy Outcome

Dependent variable	*Reorganized*		*Liquidated*		*Dismissed*	
Low caseload court	−0.099	0.104	−0.052	−0.177*	0.151***	0.073
	(0.091)	(0.134)	(0.068)	(0.100)	(0.050)	(0.089)
Low caseload court × ln(*Size*)	—	−0.100**	—	0.075**	—	0.025
		(0.042)		(0.032)		(0.032)
PostBAPCPA × ln(*Size*)	—	0.052**	—	−0.041***	—	−0.011
		(0.024)		(0.015)		(0.019)
Nonbusiness caseload share × ln(*Size*)	—	0.050	—	0.006	—	−0.056*
		(0.033)		(0.025)		(0.032)
ln(*Size*)	0.012**	−0.009	0.017**	0.003	−0.029***	0.006
	(0.006)	(0.017)	(0.008)	(0.013)	(0.006)	(0.021)
Liabilities > Assets at filing	−0.021	−0.020	0.082***	0.082***	−0.061***	−0.062***
	(0.019)	(0.019)	(0.018)	(0.018)	(0.020)	(0.020)
Group filing	0.038	0.034	0.028	0.033	−0.066**	−0.067**
	(0.031)	(0.031)	(0.024)	(0.024)	(0.028)	(0.028)
Public firm	−0.036	−0.042	0.102*	0.111*	−0.066**	−0.070**
	(0.047)	(0.046)	(0.060)	(0.059)	(0.030)	(0.030)
Got DIP loan	0.071**	0.068**	0.053**	0.053**	−0.123***	−0.121***
	(0.029)	(0.029)	(0.025)	(0.025)	(0.020)	(0.020)
Month, industry, and district fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Observations	3,236	3,236	3,236	3,236	3,236	3,236
Adjusted R^2	0.055	0.056	0.064	0.065	0.125	0.125

Notes. This table explores the relation between the change in caseload due to BAPCPA and whether the bankrupt firm was reorganized, liquidated, or dismissed from court. *Low caseload court* is defined as the interaction of a *post-BAPCPA* dummy, equal to one if the firm filed on or after October 17, 2005, and *Nonbusiness caseload*, the share of caseload in 2003 that was derived from nonbusiness filings. *Size* is the maximum of either assets or liabilities reported at the time of filing. The other control variables indicate whether the firm reported liabilities greater than assets at filing, if the firm had other related entities that filed jointly, if the firm had assets available for distribution to creditors, if the firm was public, and if the firm obtained DIP financing. Forty-seven firms that filed with prepackaged plans are omitted from the sample. All regressions include 89 district fixed effects, 48 month fixed effects, and 30 industry fixed effects. All models are estimated using linear least squares. Standard errors are clustered by bankruptcy district and reported in parenthesis.

***, **, and * indicate statistical significance at 1%, 5%, and 10% levels, respectively.

month but in districts that had exogenously different caseload due to BAPCPA. The estimates show that Chapter 11 debtors that file in districts with the lightest caseloads are significantly more likely to be dismissed from court. As explained in Section 2.1 and in Online Appendix A, dismissal favors creditors by allowing them to seize assets and in most cases is akin to liquidation, especially for small firms. My results show that less-busy judges are more likely to force firms out of court, denying them bankruptcy protection. To the extent that dismissal is equivalent to liquidation for small firms, it is likely less costly than conversion to Chapter 7 because it avoids further direct court and lawyer fees.

The second coefficient of interest in Table 5 is that of the interaction term *Low caseload court* × ln(*Size*), which tests whether these effects differ by the size of the firm. I find that a decline in caseload significantly decreases the likelihood of reorganization for larger firms, while the smallest firms are perhaps more likely to reorganize in less busy courts (although the coefficient on *Low caseload court* is insignificant). Meanwhile, larger firms are relatively more likely to liquidate when caseload declines and smaller firms are less likely to do so. The differential impact on larger firms is consistent with the idea that it is costly for a busy judge to determine the viability of the largest, most complex bankrupt firms, making them relatively more likely to allow a large firm to reorganize rather than liquidate. In addition, larger firms are likely better able to lobby a busy judge to allow reorganization: they can better afford high-quality lawyers, and the (likely negative) publicity for the judge will be much larger if a large firm is liquidated.[33] While these tests do not specifically identify the channel for this effect, it is clear that as caseload declines judges are more likely to push larger firms to liquidation, while smaller firms are somewhat less likely to liquidate.

It may seem surprising that busier judges are more likely to reorganize large firms, when these reorganizations on average require more negotiation, spend longer in court, and hence may entail more overall work for the busy judge. It is important to keep in mind that, while reorganizations are likely more work for a bankruptcy judge *on average*, this is not necessarily true *on the margin*. That is, should a busy judge

attempt to liquidate or dismiss a marginal firm that in the judge's view just misses the threshold for reorganization, he is likely to meet with significant resistance from the debtor's management, particularly the management of a large, sophisticated firm. Thus, it is possible that pushing marginal firms into dismissal or liquidation increases the judge's workload, and hence one would expect judges to dismiss more firms as caseload declines.[34]

I estimate that on average, a 64-hour decrease in caseload increases the probability of dismissal by 11.5% ∗ 15.1% = 1.7%, a modest increase from the unconditional mean of 34.3%. Meanwhile, a back-of-the-envelope estimate of the effect of a 300-hour decline in caseload is that it would increase the probability of dismissal by 1.7% ∗ 4.77 = 8.2%.[35] Further, the effect differs substantially by the size of firm. The firm at the 10th percentile in my sample has *Size* equal to $1 million, the median firm has *Size* of $4.42 million, and the firm at the 90th percentile has *Size* of $48.9 million. Using this as a guideline, I use firms of *Size* $1 million, $5 million, and $50 million to give an idea of how the change in caseloads affects firms of varying sizes. Based on the coefficients in Table 5, a 64-hour decrease in bankruptcy caseloads would have the following impact on the probability of each bankruptcy outcome:

Change in probability of:	Firm *Size*		
	$1 million	$5 million	$50 million
Reorganization	1.20	−0.65	−3.30***
Liquidation	−2.04*	−0.65	1.34
Dismissal	0.84	1.30**	1.96**

In this and future tables that display the estimated impact of a rise in caseload, ***, **, and * are used to indicate whether the estimate is statistically different from zero at the 1%, 5%, and 10% levels, respectively. These tests are performed using a Wald test of the linear combination $\beta_0 + \beta_1 \times \ln(Size)$, where β_0 is the coefficient on *Low caseload court* and β_1 is the coefficient on *Low caseload court* × ln(*Size*). Note that it is possible that the effect on large firms is statistically different from the effect on small firms even while neither is statistically different from zero.

5.3. Recidivism

Ideally, an efficient bankruptcy court would separate those firms that are economically viable from those that are not, and then ensure that the firms that leave court have a good chance of not falling back into bankruptcy. In this section I look more closely at the postbankruptcy performance of firms that pass through busy bankruptcy courts.

I test whether the drop in caseload following BAPCPA affected the probability that a firm refiles for bankruptcy (either Chapter 11 or Chapter 7) within three years of its original filing date.[36] In this analysis I only consider firms that are either reorganized or dismissed from court, as liquidated firms cease to exist and therefore cannot refile for bankruptcy. Results are presented in Table 6. I find that low caseload courts see significantly lower recidivism. However, this effect is concentrated among firms that are dismissed from court, while debtors that are reorganized in low caseload courts do not appear to have lower recidivism rates. In addition, the effects appear to be the strongest for smaller firms. A decrease in bankruptcy caseloads of 64 hours has the following impact on the recidivism rates for firms of various sizes:

Change in probability of refiling within three years	Firm *Size*		
	$1 million	$2 million	$50 million
All cases	−1.58***	−1.04**	−0.27
Reorganizations	−0.22	0.06	0.46
Dismissals	−3.54***	−2.01	0.19

These estimated impacts are quite large; the unconditional probability of a dismissed firm refiling for bankruptcy within three years is only 7.4%, suggesting that the 64-hour shock to caseload decreases the recidivism rate by nearly 27% for the median firm.

The fact that caseload does not appear to affect the recidivism rate for firms that reorganize is somewhat surprising given that Section 5.2 shows that caseload affects bankruptcy outcomes. In particular, larger firms are less likely to be reorganized when caseload is low. If fewer large firms are reorganized in less-busy courts, one might expect that those firms that do reorganize are on average more financially secure and would therefore end up back in bankruptcy less often. One possibility is that the three-year horizon is too short to find an effect for reorganized firms, which likely take longer to reenter financial distress than dismissed firms.[37] It is also possible that a high percentage of reorganized firms continue to experience financial distress outside of court, even if they do not file for bankruptcy again, as documented in previous research (Hotchkiss 1995, Gilson 1997, Chang and Schoar 2013, Morrison 2007). Both of these possibilities are particularly likely for larger firms. On the other hand, it may be that the quality of restructurings in busy courts is equally as high as that of less-busy courts, albeit at elevated costs because bankruptcy proceedings take longer to complete. Regardless, within a three-year window, it does not appear that the exogenous shock to caseload resulted in higher recidivism for firms that reorganize in busier courts.

Meanwhile, dismissed firms refile at significantly lower rates when caseload falls. There are at least two factors that may be driving this result. First, if less-busy judges tend to scrutinize cases more closely, then a firm

Table 6. The Effect of Caseload on Recidivism

Dependent variable:	*Refiled for bankruptcy within three years*			
Sample:	Reorganized		Dismissed	
Low caseload court	0.002	−0.019	−0.227**	−0.308**
	(0.050)	(0.055)	(0.102)	(0.120)
Low caseload court × ln(*Size*)	—	0.015	—	0.083
		(0.022)		(0.060)
PostBAPCPA × ln(*Size*)	—	0.007	—	−0.087*
		(0.008)		(0.044)
Nonbusiness caseload share × ln(*Size*)	—	−0.011	—	0.004
		(0.010)		(0.050)
ln(*Size*)	0.006	0.003	0.004	0.020
	(0.008)	(0.006)	(0.005)	(0.031)
Liabilities > Assets at filing	0.005	0.006	−0.026*	−0.024
	(0.011)	(0.012)	(0.014)	(0.015)
Group filing	0.014	0.015	−0.022	−0.021
	(0.017)	(0.016)	(0.028)	(0.028)
Public firm	−0.045**	−0.038**	−0.029	−0.027
	(0.019)	(0.016)	(0.042)	(0.041)
Got DIP loan	0.017	0.020	0.031	0.029
	(0.025)	(0.026)	(0.054)	(0.052)
Month, industry, and district fixed effects	Yes	Yes	Yes	Yes
Observations	938	938	1,125	1,125
Adjusted R^2	−0.018	−0.015	0.040	0.043

Notes. This table explores the relation between the change in caseload due to BAPCPA and the likelihood a firm refiles for bankruptcy. The dependent variable is equal to one if the firm filed for either Chapter 11 or Chapter 7 bankruptcy within three years of its original bankruptcy filing, but more than three months after that date. All independent variables are defined as in Table 5. For clarity, the key variables that identify the impact of caseload are shaded. All regressions include 89 district fixed effects, 48 month fixed effects, and 30 industry fixed effects. All models are estimated using linear least squares. Standard errors are clustered by bankruptcy district and reported in parenthesis.

***, **, and * indicate statistical significance at 1%, 5%, and 10% levels, respectively.

that is correctly dismissed might be less willing to try refiling in the low caseload court since it will likely be dismissed again. Meanwhile, when caseloads are high judges may be unwilling or unable to spend the time necessary to find proper cause for dismissal, thereby making it easier for debtors to appeal the decision or fix the issue that led to the initial dismissal and refile for bankruptcy protection. Second, it is possible that a higher portion of the firms that busy judges dismiss from court are actually viable entities in need of Chapter 11 protection in order to restructure.

Two further tests can shed some light on the mechanism that drives higher recidivism in busy courts. First, defining recidivism to only include refilings that occur more than 12 months (but still less than three years) after the initial filing does not alter the economic magnitude or statistical significance of the results. Thus, the high recidivism rate among firms dismissed from busy courts is not due to quick refilings that occur less than a year after the initial filing, which casts doubt on the idea that the recidivism effect is driven by easily fixed technicalities or firms that quickly refile in busy courts in hopes of getting a more lenient judge the second time through. Second, the recidivism effect is driven completely by refilings for Chapter 11 (so-called "Chapter 22" bankruptcies, which account for a little more than half of the refilings in my sample), rather than firms who file for Chapter 7 in their second filing. This shows that the effect is not being driven by unviable firms that choose to liquidate in court after being initially dismissed from busy courts.[38] Rather, elevated recidivism in busy courts comes from dismissed firms that survive for at least a few months and then attempt to restructure in Chapter 11 once again.

Regardless of whether high recidivism among dismissed firms in busy courts reflects debtors seeking to take advantage of prodebtor courts or firms that were initially dismissed that could truly benefit from bankruptcy protection, increased recidivism likely drives up both direct and indirect costs of financial distress for these firms, since it drags out the legal process as well as the period of financial distress. However, I note that while it is ex post efficient to reduce recidivism, it is not necessarily ex ante efficient. The optimal recidivism rate is not zero, and therefore it is not necessarily welfare enhancing that the recidivism rate is lower when

court caseloads decrease. For example, if courts become too strict when caseloads are low, thereby discouraging some firms from optimally refiling, the decline in recidivism would be welfare reducing.

5.4. Time in Bankruptcy

Several previous studies have used time in bankruptcy as an indirect measure of total bankruptcy costs, including Bris et al. (2006), Franks and Torous (1989) and Thorburn (2000). To the extent that increases in caseload force courts to stretch out the proceedings for each bankruptcy case, filings in busier courts could be substantially more costly than those in less-busy courts. On the other hand, when courts are busy, judges, debtors, and creditors may take actions to speed up the bankruptcy process to compensate for the congested court, thereby leaving time in bankruptcy relatively unaffected. Importantly, Bris et al. (2006) find that judges are particularly important determinants of the length of the bankruptcy case, suggesting that judges have large amounts of leeway in determining the speed at which cases resolve. In Table 7, I test whether a drop in caseload allows Chapter 11 debtors to spend a shorter amount of time in bankruptcy. For these regressions, I define time in bankruptcy as the number of months between the filing date of the case and the date that a resolution was reached. For reorganizations and dismissals, the resolution date corresponds to the date on which the case was discharged from court. For liquidations, the resolution date is the date on which the case is converted to Chapter 7. Converted cases will remain in court for several months after this date while the trustee oversees the liquidation of the assets, but at this point the judge has little left to do on the case, as the decision to liquidate has already been made.[39]

Table 7. The Effect of Caseload on Time in Bankruptcy

Dependent variable:	*Months in bankruptcy*	
Low caseload court	−0.051	7.448
	(4.258)	(5.038)
Low caseload court × ln(*Size*)	—	−3.868***
		(1.119)
PostBAPCPA × ln(*Size*)	—	1.105**
		(0.529)
Nonbusiness caseload share × ln(*Size*)	—	2.848***
		(1.077)
ln(*Size*)	1.785***	0.977
	(0.278)	(0.650)
Liabilities > Assets at filing	−0.391	−0.344
	(0.616)	(0.617)
Group filing	0.467	0.207
	(0.968)	(0.947)
Public firm	0.482	0.109
	(1.682)	(1.611)
Got DIP loan	5.106***	4.840***
	(1.070)	(1.075)
Month, industry, and district fixed effects	Yes	Yes
Observations	3,236	3,236
Adjusted R^2	0.151	0.155

Notes. This table explores the relation between the change in caseload due to BAPCPA and the duration of the firm's time in bankruptcy. The dependent variable is the number of months between the bankruptcy filing and the resolution date of the bankruptcy. All independent variables are defined as in Table 5. For clarity, the key variables that identify the effect of caseload on time in bankruptcy are shaded. All regressions include 89 district fixed effects, 48 month fixed effects, and 30 industry fixed effects. All models are estimated using linear least squares. Standard errors are clustered by bankruptcy district and reported in parenthesis.

***, **, and * indicate statistical significance at 1%, 5%, and 10% levels, respectively.

In Table 7, I find that there is no average effect of caseload on time in bankruptcy. This is remarkable, as it suggests that bankruptcy cases in exogenously busier courts are resolved as quickly as cases in less-busy courts, either through judges working longer hours, working more efficiently, or simply spending less time per case. Importantly, if judges are spending less time per case, this could be because judges are simply making decisions faster or because other participants in the bankruptcy, knowing there could be a delay in ruling due to court congestion, decide to bring fewer issues before the busy judge.

While there is no average effect on time in bankruptcy, there is a significant difference between large and small firms, as shown in the second column of Table 7. In particular, in low caseload districts smaller firms remain in court longer while larger firms are in court a shorter amount of time. This is consistent with busy judges adjusting their workflow by resolving smaller cases more quickly, while taking more time on complex cases. In terms of economic magnitude, I estimate that a 64-hour decrease in caseload lengthens the time in bankruptcy by 0.76 months for a $1 million firm, but decreases time in bankruptcy by 0.5 months for a $50 million firm. The 1.26 month difference between these two extremes is equivalent to a 6.9% change from the mean of 18.3 months.[40]

5.5. Bankruptcy Sales and DIP Financing

Asset sales are an important feature of the bankruptcy process. Through Section 363 of the Bankruptcy Code, the debtor firm is able to sell some or all of its assets to a third party without the need of creating a plan of reorganization and going through the voting process, although these sales must still be approved by the court. In general, the judge must verify that there is a "good business reason" for the sale (Wolf et al. 2010).

One of the main benefits of 363 sales is that they bring cash to the firm much more quickly than a traditional reorganization plan. Because of this, 363 sales occur more often under emergency circumstances when firms need cash quickly and cannot bring in outside capital through DIP lending.[41] Thus, when the

Table 8. The Effect of Caseload on Asset Sales and DIP Lending

Dependent variable:	*Has asset sale*		*Sale price/Assets*		*Obtained DIP loan*	
Low caseload court	−0.094**	−0.111	−0.206	3.301	−0.112**	−0.003
	(0.047)	(0.068)	(0.895)	(2.882)	(0.052)	(0.079)
Low caseload court × ln(*Size*)	—	0.018	—	−1.373	—	−0.049**
		(0.027)		(1.008)		(0.021)
PostBAPCPA × ln(*Size*)	—	0.004	—	0.135	—	0.008
		(0.017)		(0.264)		(0.015)
Nonbusiness caseload share × ln(*Size*)	—	−0.003	—	0.245	—	0.069***
		(0.035)		(0.283)		(0.019)
ln(*Size*)	0.050***	0.040	−0.491*	−0.255*	0.085***	0.055***
	(0.005)	(0.027)	(0.284)	(0.136)	(0.005)	(0.011)
Liabilities > Assets at filing	−0.004	−0.004	−0.596	−0.638	0.021*	0.022**
	(0.012)	(0.012)	(0.580)	(0.578)	(0.011)	(0.011)
Group filing	0.075***	0.078***	0.240	0.154	0.090***	0.087***
	(0.019)	(0.020)	(0.337)	(0.321)	(0.017)	(0.017)
Distributable assets	0.139***	0.144***	0.081	−0.065	0.239***	0.236***
	(0.044)	(0.044)	(0.234)	(0.234)	(0.043)	(0.043)
Got DIP loan	0.248***	0.250***	0.208	0.360	—	—
	(0.029)	(0.028)	(0.371)	(0.424)		
Substantially all assets sold	—	—	0.463	0.597	—	—
			(0.387)	(0.418)		
Prepackaged bankruptcy	—	—	—	—	0.180***	0.191***
					(0.061)	(0.061)
Month, industry, and district fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Observations	3,236	3,236	409	409	3,282	3,282
Adjusted R^2	0.296	0.297	−0.065	−0.028	0.334	0.337

Notes. This table explores the relation between the change in caseload due to BAPCPA and the need to raise capital during bankruptcy. In the first two columns, the dependent variable is equal to one if the firm sold any assets in bankruptcy. In the middle two columns the dependent variable is the sale price scaled by the assets of the firm, for the firms that had at least one asset sale. In the final two columns the dependent variable is equal to one if the firm obtained debtor-in-possession financing. All control variables are defined as in Table 5, with the addition of a control for whether the asset sale was for substantially all of the assets of the firm. For clarity, the key variables that identify the impact of caseload are shaded. All regressions include 89 district fixed effects, 48 month fixed effects, and 30 industry fixed effects. All models are estimated using linear least squares. Standard errors are clustered by bankruptcy district and reported in parenthesis.

***, **, and * indicate statistical significance at 1%, 5%, and 10% levels, respectively.

bankruptcy process is expected to be drawn out, such as when bankruptcy caseloads are high, more asset sales should be expected. Further, busy judges are also more likely to approve asset sales, since they typically speed up the bankruptcy process by removing the need for complex debtor-creditor negotiations and detailed reorganization plans. Indeed, if high caseloads are associated with increased asset sales, this could explain why there is not a large effect of court congestion on bankruptcy durations, as shown in the previous section.

Table 8 shows that this is the case. After BAPCPA, the courts that experienced the largest drop in caseload also had the largest decrease in the share of cases that had 363 sales. My estimates suggest that a 64-hour decline in caseload reduces asset sales by 1.1 percentage points, an 8.3% decrease from the unconditional mean of 13%. The impact does not vary much depending on the size of the firm, although it appears to be slightly larger for small firms. This makes sense, since small firms have a harder time accessing outside capital and therefore would more likely have to resort to asset sales in cases when the bankruptcy filing drags on for a long period of time.

While data from Capital IQ and the Deal Pipeline do contain prices on the bankruptcy transactions, it is difficult to measure whether these prices are discounted from full value because I cannot observe exactly which assets are sold. As a proxy, I scale the selling price by the total assets of the firm, and test whether 363 sales that take place in low caseload courts have higher price-to-asset ratios than those that are in busier courts. This is a very rough proxy, as it is driven not only by "fire sale" prices but also by the amount of assets the firm is selling. For example, a firm that is selling substantially all of its assets will have a higher price-to-asset ratio than a firm that is selling only a small piece of the business, regardless of whether either firm is selling at discounted prices. I control for this to the extent possible by including a dummy variable indicating whether the transaction noted that the firm is selling substantially all assets.[42] In Table 8 I test whether the sale price-to-asset ratio is affected

by bankruptcy caseload for the 422 sales in my sample. I fail to find a strong relationship between sale prices and court caseload, likely due to a lack of statistical power and imprecise measurement. However, the estimates are in the anticipated direction: small firms, whose likelihood of selling assets declines the most with a caseload drop, also sell at higher prices when courts are less busy. Large firms do not reap the same benefits.

Although my data do not allow me to estimate a tight relationship between caseload and sale prices, previous work has shown that forced asset sales typically occur at sharply reduced prices (Pulvino 1998, Campbell et al. 2011). Further, Pulvino (1999) shows that bankruptcy does not prevent fire sales. Thus, to the extent that high caseloads lead to more asset sales, it is likely that these asset sales reduce recovery rates to creditors.

Selling assets is one way that cash-strapped debtors can raise capital. An alternative method of gaining cash is through the issuance of a DIP loan, which can provide the working capital necessary to maintain operations through the bankruptcy process. The final two columns of Table 8 test whether firms in busy bankruptcy courts are more likely to obtain DIP financing. Here we again see the same relationship: as caseload declines, firms are less likely to use DIP loans. Interestingly, the effect is strongest for larger firms. Thus, while smaller firms reduce asset sales as caseload declines, larger firms—those that are more likely to have access to the DIP loan market—tend to reduce DIP loan borrowing. In both cases, the need for raising external cash is lower when courts are less busy, and these results highlight the different ways in which large and small firms tend to raise that cash.

A summary of the impact of a 64-hour decrease in bankruptcy caseloads on the propensity to sell assets, on the sale price/asset ratio and on the propensity of obtain DIP financing is as follows:

	Firm *Size*		
	\$1 million	\$5 million	\$50 million
Increase in probability of having asset sale	−1.28	−0.94*	−0.47
Change in price/ Asset ratio	0.38	0.13	−0.24
Increase in probability of DIP financing	−0.03	−0.94	−2.24***

It is important to keep in mind that my sample period is 2004–2007, a time when it was relatively easy to obtain credit and when merger and acquisition activity was quite robust. Using BAPCPA for identification is useful because it allows me to focus directly on an exogenous shock to judge time constraints. However, typically courts become crowded during economic recessions, when credit is tight and M&A activity is depressed. In my sample, I find that firms are able to compensate for busier courts by selling assets or obtaining DIP financing, but in recessionary periods they may not be able to find a willing buyer for their assets or a DIP lender. If this is the case, then the impact of heavy caseloads in recessions is likely larger than I have estimated. Rather than obtaining DIP financing, firms could well be forced to sell assets in 363 sales (as Chrysler and General Motors did in 2009) or simply liquidate completely. Further, asset sales in recessions are also affected by fire sale prices and a lack of buyers who can best use the assets (Shleifer and Vishny 1992, Bernstein et al. 2017). In this way, difficult credit and M&A environments during recessions likely exacerbate the costs of busy bankruptcy courts.

6. Discussion and Conclusions

This paper has shown that time constraints on bankruptcy judges alter the outcomes of firms that restructure in busy courts. When caseload falls, larger firms are less likely to be reorganized and more likely to either be liquidated or dismissed from court. Smaller firms, meanwhile, are actually slightly more likely to be reorganized and less likely to be liquidated when courts are less busy. These results are consistent with the idea that busy judges optimize their time by spending relatively less time on smaller firms while stretching out the proceedings for larger firms. In support of this idea, I find that larger firms spend less time in court as caseloads decline, relative to smaller firms.

In addition, my findings have important implications for the costs of financial distress. As direct evidence of this, I find that banks experience fewer charge-offs as local bankruptcy courts become less busy. Although my data do not allow me to pin down the precise channel through which charge-offs are reduced, there are several possible avenues, including reduced recidivism of dismissed firms, lower likelihood of asset sales, and shorter spells in bankruptcy.

While my evidence shows that the costs of financial distress are lower in less-busy bankruptcy courts, the overall welfare implications are less clear. In particular, the fact that more firms are dismissed or liquidated when caseloads decline does not necessarily mean that this is socially optimal, despite the fact that bank charge-offs are lower. In theory, somewhere between the extremes of liquidating all firms or none of them, there is some optimal level of "pro-debtorness" for a bankruptcy judge that balances ex post efficiency and ex ante incentives (Aghion et al. 1992, Hart 2000, Bernhardt and Nosal 2004, Gennaioli and Rossi 2010). While several previous papers have argued that the Chapter 11 process is biased toward inefficient

continuation of firms (Baird 1986, Weiss and Wruck 1998, LoPucki and Kalin 2001), Morrison (2007) shows that many smaller firms are liquidated and argues that there is no continuation bias. The location of Chapter 11 on this liquidation-continuation spectrum remains an open question due to difficulties in finding clean empirical identification. However, both Chang and Schoar (2013) and Becker and Stromberg (2012) use natural experiments to show that procreditor shifts in the interpretation of the Bankruptcy Code tend to enhance firm value. This evidence suggests that busy bankruptcy courts, which tend to allow more continuation, are likely to reduce overall firm value.[43]

This paper uses the passage of BAPCPA as an exogenous shock to bankruptcy caseloads. While this identification allows me to make causal estimates of the impact of changes in court caseload, it is important to keep in mind the external validity of the experiment. BAPCPA occurred during relatively good economic times, but typically bankruptcy filings spike during economic recessions, and it is possible that the effects I have identified are either tempered or exacerbated by these poor market conditions. In particular, during recessions distressed firms have less ability to roll over previous debts, obtain DIP financing, or sell assets at reasonable (not fire sale) prices—actions that would help these firms avoid bankruptcy or handle longer bankruptcy proceedings. If this is the case, this would likely exacerbate the costs of financial distress due to busy courts.

This is true both nationwide and on a more local level. Court caseloads vary more cross-sectionally than they do over time. While I have couched most of my results in terms of nationwide economic recessions, it is also true that local economic conditions can be quite different across the United States. Localized economic malaise will have the same impact on caseloads in affected bankruptcy districts as nationwide recessions will. In terms of bankruptcy caseload, *where* a case is filed can matter as much as *when* it is filed.

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Endnotes

[1] See Hotchkiss et al. (2008) for a review of this literature.

[2] For example, Agarwal et al. (2017) show that banks with more loans per employee and longer phone waiting times were significantly less likely to modify mortgage contracts in the wake of the financial crisis.

[3] For example, following the collapse in house prices in 2007 and 2008, judge caseload doubled in Arizona relative to Texas, where the house price drop was not as severe.

[4] In addition, as judges become busier it can change the incentives of both creditors and debtors, which will then also affect bankruptcy outcomes. For example, if a busy judge is more likely to approve an asset sale, then petitioners have a larger incentive to ask for the asset sale in the first place.

[5] Gilson (1997) and Hotchkiss (1995) use recidivism as a measure of inefficient restructuring. However, because the optimal recidivism rate is likely not zero, an increase in recidivism is not necessarily inefficient.

[6] Section 5.3 discusses more fully why dismissed firms, but not reorganized firms, have higher recidivism rates in busy courts.

[7] Legal researchers have long been concerned about the effect of heavy caseloads on federal judges' decision making. See, for example, Friendly (1973) and Ginsburg (1983).

[8] See Jex (1998) for an overview of the psychological research in this area.

[9] Corporations can file for bankruptcy either (1) where they are incorporated, (2) where they are headquartered, or (3) where they do the bulk of their business. This gives the largest, nationwide firms substantial leeway in the choice of bankruptcy venue, but for most corporations these three locations are one and the same and therefore they are not able to "shop" for a more suitable bankruptcy venue. In my sample, 295 firms (8.9%) filed in bankruptcy districts different from the address they reported on their petitions. Excluding these firms from the sample does not change any of my conclusions.

[10] In Online Appendix A I provide more detail about why firms are dismissed from court and what happens to them after they leave court.

[11] In 2005, 28 new temporary judgeships were created in conjunction with BAPCPA, although the Judicial Conference had requested 47 permanent positions. Section 3 discusses BAPCPA in more detail.

[12] This is an average across all Chapter 11 cases filed, and is thus not a reflection of "mega" Chapter 11 cases that cost judges significantly more time.

[13] There are a total of 94 bankruptcy districts in the U.S. Courts system, but I exclude the Northern Marianas Islands, the Virgin Islands, Guam, and Puerto Rico from my study. In addition, the Western and Eastern Districts of Arkansas share bankruptcy judges, and so I treat them as a single district for this study.

[14] This is apparent in the one-quarter increase in the Chapter 11 filing rate in 2005Q4 in Figure 1, panel B.

[15] Baird and Rasmussen (2002) and Bharath et al. (2010) explore how Chapter 11 is changing over time. My empirical strategy nets out any time effects by comparing firms that filed in the same month to each other.

[16] The nonbusiness share of caseload is quite static over time. For example, the cross-sectional correlation between this measure in 1995 and 2003 is 0.76 and significant at the 1% level. Figure A.2 in the online appendix displays a map of the nonbusiness share of caseload in 2003.

[17] Delaware and the Southern District of New York show up as clear outliers in Figure 4. In Online Appendix C, I provide results where these two districts are "winsorized" to match the nonbusiness caseload of Alaska. Doing so does not affect my results. Further, while there is some geographic clustering of consumer-centric courts

(see Figure A.2 in the online appendix), Online Appendix C contains robustness checks that verify that this clustering has no effect on the results.

[18] One might be concerned that some of the results in Section 5 are influenced by the introduction of these new judges. Removing all cases assigned to the new judges does not affect the conclusions in any significant way.

[19] Clustering at the state level, or double clustering by bankruptcy district and time do not affect the significance of my results.

[20] In the bank charge-off analysis of Table 4, I interact with ln(*Bank assets*) rather than firm size to test if the impact is larger for small banks.

[21] Table 3 contains summary statistics and is fully described in Section 4.

[22] A variant of this concern is that BAPCPA may have impacted businesses by limiting the ability of their owners to file for personal bankruptcy, as there are cases where entrepreneurs use personal bankruptcy to discharge business debts (Fan and White 2003). This is unlikely to affect my estimates because my data do not include the smallest businesses where these personal loan guarantees are likely to occur. Further, even if this is the case, it should apply in both consumer-centric and business-centric courts, and would thus be differenced out in my estimates.

[23] A "voluntary" filing is one in which the debtor filed the petition, while "involuntary" filings are instigated by a creditor or creditors. Involuntary filings compose less than 2% of the sample.

[24] Including ln(*Assets*) and ln(*Liabilities*) as controls instead of ln(*Size*) does not affect my results.

[25] Liquidating plans in Chapter 11 function just like reorganizing plans: they are proposed, voted on, and approved in the same manner. The only difference is that there is no expectation that the debtor will continue operations after exiting.

[26] I do not consider firms that are liquidated in my analyses of recidivism, since these firms cease to exist after their original bankruptcy and cannot refile.

[27] Online Appendix B contains more detail on LexisNexis' coverage of bankruptcy filings and the variables derived from the data, and the dispersion of cases by industry and bankruptcy district.

[28] According to the Survey of Terms of Business Lending, produced by the Federal Reserve Board of Governors, about 60% of C&I lending is unsecured.

[29] The underlying assumption that banks lend predominantly to local firms is supported by the fact that over 70% of firms borrow from banks located less than 20 miles away, according to the 2003 Survey of Small Business Finances produced by the Federal Reserve Board of Governors.

[30] Economic indicators are all first calculated for each bankruptcy district using county-level data weighted by the population of each county. Then for each bank, I take the weighted average across all bankruptcy districts in which the bank had deposits, using the amount of deposits in each district as the weight.

[31] One should also keep in mind that typical increases in caseload occur when economic conditions deteriorate, when outside factors other than caseload will also affect the outcome variables. The concluding section discusses this further.

[32] I control for leverage using a dummy equal to one if the firm had liabilities greater than assets at the time of filing. Results are unchanged if I instead control using a continuous leverage ratio.

[33] Judge career concerns have received a fair amount of attention in the academic literature as a possible reason why judges are reluctant to liquidate large firms. Recent examples include LoPucki (2005) and Gennaioli and Rossi (2010).

[34] Online Appendix B contains more information about time in court and the workload required of judges across bankruptcy outcomes for a random subsample of 150 firms.

[35] This magnitude is comparable in size to Bernstein et al. (2017), who examine a judge's fixed bias toward converting to Chapter 7. They estimate that a one standard deviation increase in a judge's conversion bias increases the likelihood that a given case will be converted by 7.5%.

[36] As mentioned previously, I do not count a firm as having refiled if it files again within three months of its original filing date, as such filings are likely due to cases in which the firm was dismissed on a technicality and then subsequently refiled once the problem was rectified.

[37] I cannot test longer horizons, as my data end in December 2010.

[38] For brevity, the results from these two tests are unreported, but are available from the author by request.

[39] For reorganizations, the data from LexisNexis do not contain the date the plan of reorganization is confirmed, which could be viewed as the "resolution" of the case, although the case remains in court for several months after a plan is confirmed. Because this date is not available, I use the date the case was closed as the terminal date for these cases. Online Appendix B, which more fully discusses this issue, gives evidence that this data limitation does not affect the estimates in Table 7.

[40] Table A.7 in the online appendix shows results separately for reorganized, liquidated, or dismissed cases. These results show that in high caseload courts, large firms are particularly slow to be liquidated, while small firms are liquidated more quickly. Meanwhile, both large and small firms take slightly longer to reorganize in congested courts.

[41] Lehman Brothers, Chrysler, and General Motors are examples of this motive. In each case the judge approved a quick asset sale because the firms' value as a going concern was diminishing like a "melting ice cube," and they had little access to outside funding at the time.

[42] If I restrict the regression to the subsample of sales in which substantially all assets are sold I find similar coefficient estimates.

[43] On the other hand, Bernstein et al. (2017) show that liquidation is often costly due to difficulties in reallocating assets of bankrupt firms. Importantly, their study focuses mainly on small firms, which are least likely to have a continuation bias.

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