Get in Line: Chapter 11 Restructuring in Crowded Bankruptcy Courts

APPENDIX

Benjamin Iverson

November 2012

Appendix A – Dismissal from court

In the text, I argue that dismissal from court is equivalent to liquidation for most firms. To verify this, I randomly selected 100 dismissed firms that filed for Chapter 11 and examined the reasons for their dismissal using court documents on the U.S. Court's Public Access to Court Electronic Records (PACER) system. In general, the reasons for dismissal can be sorted into four categories: (1) the debtor failed to follow court procedure, such as failure to file specific documents, failure to hire counsel, or failure to show up in court; (2) the debtor is deemed to have abused the system by filing in bad faith, or filing repeatedly without making efforts to repay its debts; (3) there is no possibility that the debtor can successfully reorganize; (4) the debtor has reached a settlement with its creditors and therefore no longer needs bankruptcy protection. Unsurprisingly, the reason for dismissal varies considerably depending on which party files the motion. When the trustee or court files the motion for dismissal, it is typically because the debtor did not obey a court order of some sort, but in a significant minority of cases it is also because there is no hope of reorganization. When a creditor files a successful motion the reason for dismissal is often because the debtor has abused the bankruptcy system in some way. Debtor-filed motions, however, are nearly equally split between debtors who have no hope of reorganizing, and who wish to leave bankruptcy and simply liquidate without incurring further legal fees, and debtors who have either found a buyer or have reached a settlement with their creditors. It should be noted that in many cases when a debtor sees no hope of reorganization and files for dismissal of the case the court has previously granted motions in favor of the creditors, such as lifting the automatic stay or denying the use of cash collateral. Thus, although these cases appear to be voluntary shutdowns, the debtor really had no other choice available due to previous actions of the court (Morrison, 2005).

Overall, dismissal is a close equivalent to conversion in many cases; the firm is dismissed from court but will still be liquidated.[1] 53 of the dismissed firms I examined were liquidated shortly after dismissal, while an additional 33 were dismissed from court without resolving their financial distress and were likely liquidated as well, although the PACER documents did not make that explicit. Only 10 firms were dismissed because they had reached a settlement with their creditors. Of the remaining 4 firms, 3 were sold as going concerns and one was dismissed because it wasn't actually in financial distress. By and large, dismissal from Chapter 11 is akin to conversion to Chapter 7.

Appendix B – Data construction

A. *LexisNexis' data coverage*

As stated in the text, LexisNexis has essentially complete coverage of bankruptcy filings in their dataset. This can be verified by examining the aggregate filing statistics available from the U.S. Courts system.[2] Specifically, LexisNexis contains a total of 21,833 business Chapter 11 bankruptcy filings in the 50 states and the District of Columbia between 2004 and 2007. During the same period, U.S. Courts report that a total of 25,095 business Chapter 11 filings. The discrepancy between the two datasets can be fully accounted for by differences in how a "business" bankruptcy filing is defined. The U.S. Courts count a filing as a business filing if the majority of the debt associated with the filing is business-related, and thus some of the "business" Chapter 11 filings will include individuals who file for Chapter 11 with business debt. Meanwhile, LexisNexis only counts a filing as a business filing if the debtor declared himself a corporation or partnership on the voluntary petition for bankruptcy. To ensure that LexisNexis' data contains complete coverage, I randomly selected two dates and compared the total number (both business and non-business) of Chapter 11 filings in LexisNexis to the U.S. Courts statistics for a random subset of 14 bankruptcy districts. For these groups, LexisNexis had information on 693 Chapter 11

[1] Indeed, many motions for dismissal are joint motions for either dismissal or conversion to Chapter 7.
[2] Available at http://www.uscourts.gov/Statistics/BankruptcyStatistics.aspx.

filings as compared to 700 recorded by the U.S. Courts system. Hence, LexisNexis has about a 99% coverage rate, indicating that the discrepancy in business Chapter 11 filings is due entirely to how a "business" is defined by the two sources.

While there were a total of 21,833 business Chapter 11 filings during the sample period, many of these filings are made by related entities. Often, when a company files a Chapter 11 petition, each of its subsidiaries will file separate petitions in the same court on the same day, or soon thereafter. Because these cases are typically consolidated and managed jointly, for my purposes they should be treated as a single case. I identify related filings by comparing the company name, address, filing date, and exit date for each filing in my sample, and keep only one observation per group. This reduces the total number of filings in my sample period to 14,825 separate bankrupt entities. As described in the text, I have full financial information for 3,327 of these filings.

Tables A.1 and A.2 show the distribution of the 3,327 bankruptcies in my final sample across industries and bankruptcy districts.

B. Recidivism

On average, 5.7% of sample firms that are either reorganized or dismissed re-file for bankruptcy within 3 years of their original filing. This recidivism rate is substantially lower than the rate reported by Hotchkiss (1995), who finds that 17.7% of the firms in her sample file a second bankruptcy, but slightly higher than the 2.9% rate reported by Chang & Schoar (2007). Differences between the reported refilling rates can likely be attributed to the fact that Hotchkiss (1995) considers a longer period of time post-bankruptcy (generally 5 years) while Chang & Schoar (2007) consider only firms that re-file in the *same* district within 3 years. In addition, Morrison (2005) finds that a significant number of small businesses fail in the first year after bankruptcy without re-filing for bankruptcy. This will depress the observed recidivism rate in my sample and Chang & Schoar (2007), as our samples contain much smaller firms than Hotchkiss (1995). Across all business Chapter 11 filings in LexisNexis from 1990-2011, I find that about 10% of all firms re-file for either Chapter 7 or Chapter 11 bankruptcy at any point after the bankruptcy.

C. Bank loan loss accounting

The results on default costs borne by commercial banks use data obtained from the Consolidated Report of Condition and Income (commonly known as the Call Reports), available from the Federal Financial Institutions Examination Council at https://cdr.ffiec.gov/public/. I measure default costs as the net charge-off rate, defined as

$$NetChargeOffRate_t = \frac{GrossChargeOff_t - Recoveries_t}{\left(\frac{\sum_{k=0}^{3} TotalLoans_{t-k}}{4}\right)},$$

where k indexes quarters. Because charge-offs and recoveries are reported on a year-to-date basis, I only use the financial reports from December of each year (i.e. t pertains only to the 4^{th} quarter of each year, and the denominator is simply the average reported outstanding loans over each quarterly report in a given year). This gives a total of four observations per bank, one for each year from 2004 through 2007. The denominator uses the average loan balances during the year to account for possible differences in the timing of reported charge-offs and recoveries. Specifically, the accounting standards in FAS 114 state that bad debt should be written off when "it is probable that a creditor will be unable to collect all amounts due according to the contractual terms of the loan agreement." Obviously, there is some discretion in this exact timing, and certainly some of the charge-offs reported at time t correspond to loans that were on the books in previous quarters but, since they have been written off, are no longer recorded at time t. Because the amount of total loans is relatively stable across time for most banks, this choice to average across four quarters makes little difference in my estimates. I find essentially identical results if the denominator is total loans at time t or if the average is taken over the previous 6 quarters.

Because banks have some discretion in reporting charge-offs and recoveries, one might be concerned that this affects my measure of default costs. While it likely makes the measure noisier, the difference-in-differences identification should account for biases in a particular direction. Further, it is important to recognize that charge-offs and recoveries have no direct effect on either the income statement or the balance sheet of the bank, which minimizes the incentive for banks to manage these accounts. This is because banks create a loan loss reserve which acts as a contra asset on the balance sheet, and absorbs any net movement in loan losses.

A simple example will illustrate how this works. Suppose that in period 1 a bank disburses $1000 worth of new loans. The bank will expect that some of these loans will default, and will thus provision for loan losses by adding, say, $30 to the loan loss reserve, a contra-asset that reduces the total amount of loans on the balance sheet. This $30 reserve must come out of income in this period; it cannot be deferred until later. Thus, in period 1, the impact of the new lending on the bank's balance sheet and income statement is:

Assets	
Loans	$1,000
Loan loss reserve	($30)
Total	$970

Income	
Loan loss provision	($30)

In period 2, suppose that $25 worth of lending goes into default, but the bank chooses to wait to see if the default can be cured before it writes off the loans as losses. Then, in this period nothing changes on the balance sheet, but $25 of loans will be reported as non-performing in a separate schedule in the Call Reports.

In period 3 the bank learns that it will only recover $10 of the $25 total of defaulted loans, resulting in a net charge-off of $15. It is this net charge-off that I use in my analysis, rather than the loan loss provision recorded in period 1. Net charge-offs in period 3 do not affect either the balance sheet or the income statement of the bank, since these losses were already accounted for in period 1. Specifically, the $15 loss will reduce the amount of loans but also reduce the contra-asset, so that the new balance sheet will be:

Assets	
Loans	$975
Loan loss reserve	($15)
Cash (from recovery)	$10
Total	$970

Charge-offs, recoveries, and Non-performing loans	
Gross charge-off	$25
Recovery	$10
Net charge-off	$15
Non-performing loans	$0

Total assets still stand at $970, so recognizing the loss does not affect assets. It also does not affect income in period 3 because the loan losses were provisioned in period 1. Because actual losses are isolated from earnings and assets in this way, bank managers that are seeking to meet earnings expectations will typically do so by managing the provision for loan losses recorded in period 1 rather than actual loan losses. Liu & Ryan (2006) give further detail on the management of loan loss provisions by banks.

Appendix C – Robustness checks

In this appendix, I provide results and more detail on the robustness checks described in Section V.G of the text. To be concise, I report regression results only for the three main results of the paper: the probability of reorganization, the recidivism rate, and charge-off rates at commercial banks. All other results are available by request.

A. Outliers

As described in the text, two possible concerns related to outliers are the effects of extremely large firms in the sample and of the business-centric courts of Delaware and the Southern District of New York. Table A.3 reports regression results when these outliers are winsorized. In the case of firm *size*, I winsorize at the 99th percentile, which reduces the mean *size* from $156.7 million to $28.5 million in the sample, but has only a small effect on average ln(*size*). To account for Delaware and the Southern District of New York, I set their non-business caseload share at 54%, equal to the next lowest share in Alaska. Reducing the impact of outliers in this way has no effect on the estimated impact of caseload on bankruptcy outcomes.

B. Exclusion restriction

I test for the possibility that three other channels could be biasing my estimates of the impact of caseload on bankruptcy outcomes by allowing time fixed effects to vary by firm size, industry, or geographic region. If firms of a particular size or industry are concentrated in bankruptcy districts with high non-business caseload, my estimates could be biased if these firms changed after BAPCPA for some reason other than differences in judge caseload. Similarly, general regional trends could bias the estimates if bankruptcy districts with high or low non-business caseload are clustered together geographically. The downside of allowing for separate time effects for each of these groups is it drastically reduces the statistical power available due to the inclusion of many more covariates. For example, my main specifications include 30 industry fixed effects and 16 quarter fixed effects (in addition to the 89 bankruptcy district fixed effects), while taking every pairwise combination of these two groups in my data results in a total of 400 industry-quarter fixed effects. Thus, one would expect that statistical power will be somewhat reduced in these specifications.

Table A.4 shows the results with different time effects for each group. In the first set of results, I allow the estimated impact of ln(*size*) to vary of each quarter by including ln(*size*)-by-quarter fixed effects. This does not affect the estimates or statistical significance in any way. Adding industry-by-quarter fixed effects reduces the statistical significance of the effect of caseload on the probability of reorganization (*p*-value=0.13) and the effect of caseload on the probability of re-filing for bankruptcy (*p*-value=0.09). In both cases the coefficient estimates are nearly identical to my main specifications, indicating that the loss of significance is due only to the reduced statistical power in these robustness checks. Aside from these two estimates, all other coefficients retain statistical significance and are essentially unchanged with the inclusion of industry-quarter fixed effects. Finally, I use the region of the country that each bankruptcy district lies in to create region-by-time fixed effects. Regions are defined by the U.S. Census into four groups: Northeast, South, Midwest, and West.[3] These fixed effects account for any clustering of consumer-centric districts by using only variation within each region to identify the impact of BAPCPA on caseload. Including separate time fixed effects for each region does not affect my estimates in any significant way.

Table A.5 runs similar robustness checks on the commercial bank regressions. Here I again allow for varying time effects by the size of the bank and by the region that the bank is located in. For banks with branches in multiple regions, I use the state in which the largest portion of the bank's deposits are

[3] The map at https://www.census.gov/geo/www/us_regdiv.pdf shows exactly which states lie in each region.

located to identify the census region it belongs to. The inclusion of these additional controls does not affect my results.

C. Alternative measures of bank loan losses

In the text of the paper, I scale net charge-offs by the average total outstanding amount of lending during the year as a measure of bank loan losses. Scaling net charge-offs by total loans means that this charge-off rate is roughly equivalent to the probability of default times the loss given default for a particular loan:

$$NetChargeOffRate_t \approx PD_t \times LGD_t$$

One would expect that busy bankruptcy courts principally impact LGD_t rather than PD_t, and therefore it would be ideal to measure LGD_t alone for each bank by scaling net charge-offs by the amount of *defaulted* loans rather than scaling by *total* loans. In practice, however, matching charge-offs directly to loans that are in default is impossible using Call Report data. In each quarter, banks report their year-to-date charge-offs and recoveries as well as the current balance of "non-performing" loans – loans that are over 90-days past-due or non-accruing – which I use as a measure of total defaulted loans. However, the reported net charge-offs in quarter t could be related to loans that were non-performing in some previous quarter. Thus, scaling net charge-offs by non-performing loans from period t gives an incorrect estimate of LGD_t, but it is unclear how to combine the non-performing loan data from previous quarters to get a better measure. For example, the following is data from an actual bank in my sample:

Date	*Year-to-date Net C&I Loan Charge-offs*	*Non-performing C&I Loans*	*Total Outstanding C&I Loans*	*Net Charge-off Rate*	*LGD1*	*LGD2*
2005q1	-22	244	35460			
2005q2	103	121	34840			
2005q3	117	501	31225			
2005q4	211	353	33249	0.63%	69.24%	42.12%
2006q1	101	286	31102			
2006q2	170	232	31640			
2006q3	145	320	31234			
2006q4	263	81	29666	0.85%	114.47%	82.19%

In aggregate, this bank lost $211,000 in bad debt in 2005. The net charge-off rate used in the text of the paper (displayed in the fifth column) is calculated by scaling this amount by the average of total outstanding C&I loans for the year, in this case $33.7 million, giving a total charge-off rate of 0.63% in 2005. Because total loans are fairly stable over time, this is likely a close estimate of the true $PD_t \times LGD_t$ for that year. Estimating LGD_t by itself is not straightforward because the level of non-performing loans fluctuates widely over time and charge-offs are not matched directly to non-performing loans. The table above gives two possible alternatives. $LGD1$ is calculated by dividing end-of-year net charge offs by the average of non-performing loans over the year, e.g. 211 / 304.75 in 2005. This is similar to how $NetChargeOffRate$ is calculated, but it has the drawback of being very volatile. For example, $LGD1$ in 2006 is greater than 100%, which logically doesn't make sense and is likely because the bank wrote off a large portion of loans in late 2006, leaving a low non-performing loan balance at the end of the year but high net charge-offs. $LGD2$ alleviates this problem to some extent by scaling net charge-offs instead by the maximum of non-performing loans during the year, e.g. 211 / 501 in 2005 and 263 / 320 in 2006. This measure has the advantage of ignoring low values of non-performing loans, which in most cases gives a more accurate estimate of the true loss given default since non-performing loans decrease after charge-offs are recognized.

Importantly, neither $LGD1$ or $LGD2$ is likely to be a biased measure of loss given default, only noisy. Accordingly, Table A.6 presents regressions similar to those in Table VIII in the text of the paper except it uses these two alternative definitions of loss given default as the dependent variable. As in the text of the paper, all bank variables are winsorized at the 1^{st} and 99^{th} percentile to account for outliers, an adjustment that is particularly important for the noisy measures of loss given default. The regressions show that these two alternative measures of credit losses produce nearly identical results to those in Table VIII, although the statistical significance of $LGD1$ and $LGD2$ is slightly lower because they are less precisely measured. Specifically, a 306-hour increase in caseload is estimated to increase $LGD1$ by 37 percentage points, a 46% increase relative to its mean value of 80 percent. As measured by $LGD2$, the same shock to caseload increases losses by 19 percentage points, a 52% rise relative to its mean of 36 percent. Thus, the impact of a 306-hour rise in caseload by any of the three measures of credit losses is close to a 50% increase relative to the mean loss amount.

One interesting point that comes from using LGD_t rather than overall credit losses is that it appears that small banks experience the largest credit losses when bankruptcy courts become busy. In the main regressions in Table VIII, I do not find any differential effects for large and small banks. If it is indeed the case that small banks are most affected by crowded courts, this would mirror the fact that larger debtors are able to sway the courts in their favor when judges are busy. Similarly, these findings suggest that large banks may be able to lobby the busy judge or otherwise mitigate the effects of crowded courts. However, since this effect is not consistently found for all three measures of charge-offs, it should be viewed with some skepticism.

References

Chang, T., & Schoar, A. (2007). Judge Specific Differences in Chapter 11 and Firm Outcomes. *SSRN Electronic Journal*.

Hotchkiss, E. S. (1995). Postbankruptcy Performance and Management Turnover. *The Journal of Finance*, *50*(1), 3–21.

Liu, C., & Ryan, S. G. (2006). Income Smoothing over the Business Cycle : Changes in Banks ' Coordinated Management of Provisions for Loan Pre-1990 Bust to the 1990s Boom. *The Accounting Review*, *81*(2), 421–441.

Morrison, E. R. (2005). Bankruptcy Decisionmaking : An Empirical Study of Continuation Bias in Small Business Bankruptcies. *American Law & Economics Association Annual Meetings Report*.

FIGURE A.1
PARTIES INVOLVED IN BANKRUPTCY

This schematic depicts the various parties involved in bankruptcy courts and how they interact with the bankruptcy judge. When BAPCPA passed, it dramatically reduced the number of household bankruptcy filings, on the right. This feeds through to the judge, who is left with a far lighter docket, while the corporations, creditors, and corporate law firms remain relatively unaffected by the law.

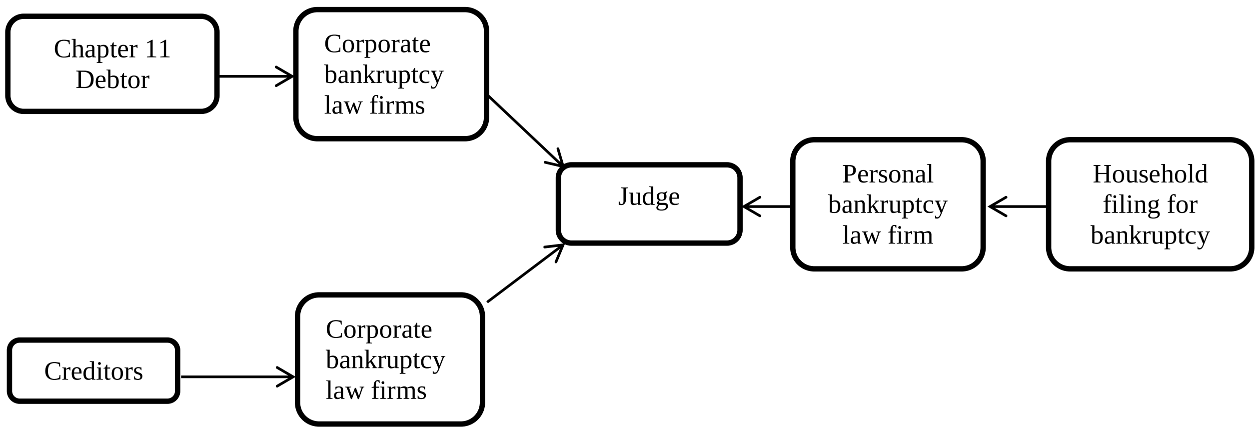

TABLE A.I
INDUSTRY DISTRIBUTION

This table presents the 30 Fama-French industries and the number of sample firms in each industry. Definitions of the industries are pulled from Kenneth French's website at http://mba.tuck.dartmouth.edu/pages/faculty/ken.french/data_library.html. Where possible, I use the SIC code reported by Capital IQ to classify the firms. In cases where the SIC code is not provided, I use the description of the industry from The Deal Pipeline to classify the firm.

Fama-French industry code (30 industries)	No. of firms	%
Food Products	92	2.77%
Beer & Liquor	4	0.12%
Tobacco Products	3	0.09%
Recreation	144	4.33%
Printing and Publishing	49	1.47%
Consumer Goods	60	1.80%
Apparel	26	0.78%
Healthcare, Medical Equipment, Pharmaceutical Products	204	6.13%
Chemicals	13	0.39%
Textiles	18	0.54%
Construction and Construction Materials	383	11.51%
Steel Works Etc	20	0.60%
Fabricated Products and Machinery	148	4.45%
Electrical Equipment	12	0.36%
Automobiles and Trucks	54	1.62%
Aircraft, ships, and railroad equipment	17	0.51%
Precious Metals, Non-Metallic, and Industrial Metal Mining	5	0.15%
Coal	9	0.27%
Petroleum and Natural Gas	25	0.75%
Utilities	20	0.60%
Communication	66	1.98%
Personal and Business Services	347	10.43%
Business Equipment	69	2.07%
Business Supplies and Shipping Containers	29	0.87%
Transportation	128	3.85%
Wholesale	172	5.17%
Retail	308	9.26%
Restaraunts, Hotels, Motels	263	7.91%
Banking, Insurance, Real Estate, Trading	516	15.51%
Everything Else	123	3.70%
Total	3,327	100.00%

TABLE A.2
BANKRUPTCY DISTRICT DISTRIBUTION

This table gives the full list of 89 bankruptcy districts used in the sample, and the number of sample firms in each district. The two districts in Arkansas share bankruptcy judges, and so are treated as one district in this study. The bankruptcy districts in the Northern Marianas Islands, the Virgin Islands, Guam, and Puerto Rico have been omitted.

Bankruptcy court	No. of firms	%	Bankruptcy court	No. of firms	%
Alaska	5	0.15%	Louisiana - East	21	0.63%
Alabama - Middle	13	0.39%	Louisiana - Middle	4	0.12%
Alabama - North	22	0.66%	Louisiana - West	22	0.66%
Alabama - South	13	0.39%	Massachusetts	63	1.89%
Arkansas	23	0.69%	Maryland	59	1.77%
Arizona	78	2.34%	Maine	16	0.48%
California - Central	181	5.44%	Michigan - East	64	1.92%
California - East	38	1.14%	Michigan - West	24	0.72%
California - North	71	2.13%	Minnesota	43	1.29%
California - South	30	0.90%	Missouri - East	13	0.39%
Colorado	59	1.77%	Missouri - West	24	0.72%
Connecticut	28	0.84%	Mississippi - North	11	0.33%
Washington, D.C.	12	0.36%	Mississippi - South	18	0.54%
Delaware	115	3.46%	Montana	6	0.18%
Florida - Middle	139	4.18%	North Carolina - East	32	0.96%
Florida - North	10	0.30%	North Carolina - Middle	14	0.42%
Florida - South	74	2.22%	North Carolina - West	23	0.69%
Georgia - Middle	9	0.27%	North Dakota	2	0.06%
Georgia - North	107	3.22%	Nebraska	20	0.60%
Georgia - South	12	0.36%	New Hampshire	14	0.42%
Hawaii	9	0.27%	New Jersey	127	3.82%
Iowa – North	6	0.18%	New Mexico	11	0.33%
Iowa – South	4	0.12%	Nevada	66	1.98%
Idaho	9	0.27%	New York - East	71	2.13%
Illinois - Central	12	0.36%	New York - North	35	1.05%
Illinois - North	98	2.95%	New York - South	206	6.19%
Illinois - South	10	0.30%	New York - West	22	0.66%
Indiana - North	31	0.93%	Ohio - North	54	1.62%
Indiana - South	53	1.59%	Ohio - South	27	0.81%
Kansas	22	0.66%	Oklahoma - East	4	0.12%
Kentucky - East	17	0.51%	Oklahoma - North	12	0.36%
Kentucky - West	28	0.84%	Oklahoma - West	14	0.42%

TABLE A.2 – *continued*

Bankruptcy court	No. of firms	%	Bankruptcy court	No. of firms	%
Oregon	12	0.36%	Texas - West	62	1.86%
Pennsylvania - East	43	1.29%	Utah	12	0.36%
Pennsylvania - Middle	25	0.75%	Virginia - East	56	1.68%
Pennsylvania - West	58	1.74%	Virginia - West	13	0.39%
Rhode Island	4	0.12%	Vermont	1	0.03%
South Carolina	31	0.93%	Washington - East	14	0.42%
South Dakota	5	0.15%	Washington - West	49	1.47%
Tennessee - East	22	0.66%	Wisconsin - East	13	0.39%
Tennessee - Middle	25	0.75%	Wisconsin - West	9	0.27%
Tennessee - West	18	0.54%	West Virginia - North	6	0.18%
Texas – East	32	0.96%	West Virginia - South	13	0.39%
Texas – North	158	4.75%	Wyoming	9	0.27%
Texas – South	157	4.72%			
			Total	3,327	100.00%

TABLE A.3
ROBUSTNESS CHECKS: OUTLIERS

This table presents robustness checks of my main results after accounting for outliers in either *size* or in the non-business share of caseload. To be succinct, the coefficients on control variables have been omitted from the table, and I only report the results for the probability of being reorganized and of re-filing for bankruptcy within 3 years. The top four rows of the table repeat the baseline regression results reported in the text of the paper. The next four lines report the coefficients on the main interaction variables when *size* has been winsorized at the 99th percentile. The bottom four rows again re-run the regression models except in these specifications the non-business share of caseload for Delaware and the Southern District of New York has been "winsorized" to 54%, equal to that of Alaska. All specifications are otherwise identical to those presented in the tables in the paper. Standard errors are clustered by bankruptcy district and reported in parenthesis. ***, ** and * indicate statistical significance at 1%, 5%, and 10% level, respectively.

Model:		*Reorganized*		*Re-filed for bankruptcy within 3 years*	
Baseline	Busy court	0.149**	0.106	0.115**	0.127**
		(0.061)	(0.064)	(0.050)	(0.050)
	Busy court * ln(*size*)	--	0.029**	--	-0.008
			(0.015)		(0.007)
Size winsorized at 99th %ile	Busy court	0.149**	0.106	0.115**	0.127**
		(0.061)	(0.065)	(0.050)	(0.050)
	Busy court * ln(*size*)	--	0.029*		-0.008
			(0.016)		(0.008)
DE & SDNY winsorized	Busy court	0.169**	0.131	0.146**	0.162**
		(0.081)	(0.091)	(0.070)	(0.070)
	Busy court * ln(*size*)		0.024		-0.009
			(0.015)		(0.007)

TABLE A.4
ROBUSTNESS CHECKS: EXCLUSION RESTRICTION

This table presents robustness checks of my main results with the inclusion of time fixed effects that differ for groups of bankruptcy filings. To be succinct, the coefficients on control variables have been omitted from the table, and I only report the results for the probability of being reorganized and of re-filing for bankruptcy within 3 years. The top four rows of the table repeat the baseline regression results reported in the text of the paper. The next four lines report the coefficients on the main interaction variables when ln(*size*)-by-time fixed effects have been included in the set of controls. The next four rows present results when separate time fixed effects are included for each of the 30 industries. The bottom four rows contain results when separate time effects have been included for each the 4 census regions: Northeast, South, Midwest, and West. All specifications are otherwise identical to those presented in the tables in the paper. Standard errors are clustered by bankruptcy district and reported in parenthesis. ***, ** and * indicate statistical significance at 1%, 5%, and 10% level, respectively.

		Reorganized		*Re-filed for bankruptcy within 3 years*	
Baseline (135 total fixed effects)	Busy court	0.149**	0.106	0.115**	0.127**
		(0.061)	(0.064)	(0.050)	(0.050)
	Busy court * ln(*size*)	--	0.029**	--	-0.008
			(0.015)		(0.007)
Size X time fixed effects (16 additional fixed effects)	Busy court	0.159**	0.044	0.119**	0.135**
		(0.069)	(0.081)	(0.053)	(0.061)
	Busy court * ln(*size*)	--	0.053**	--	-0.008
			(0.023)		(0.013)
Industry X time fixed effects (354 additional fixed effects)	Busy court	0.104	0.035	0.098*	0.097*
		(0.069)	(0.071)	(0.056)	(0.058)
	Busy court * ln(*size*)	--	0.045***	--	0.001
			(0.016)		(0.010)
Region X time fixed effects (48 additional fixed effects)	Busy court	0.176**	0.135*	0.111**	0.126**
		(0.071)	(0.077)	(0.054)	(0.054)
	Busy court * ln(*size*)	--	0.027*	--	-0.01
			(0.016)		(0.007)

TABLE A.5
ROBUSTNESS CHECKS: EXCLUSION RESTRICTION ON BANK DATA

This table presents robustness checks of my main results that examine the effect of caseload on bank charge-offs. In these regressions, I allow for time fixed effects that vary by the size or geographic region of the bank. To be succinct, the coefficients on control variables have been omitted from the table, and I only report the results for the probability of being reorganized and of re-filing for bankruptcy within 3 years. The top four rows of the table repeat the baseline regression results reported in the text of the paper. The next four lines report the coefficients on the main interaction variables when ln(*size*)-by-time fixed effects have been included in the set of controls. The bottom four rows contain results when separate time effects have been included for each the 4 census regions: Northeast, South, Midwest, and West. All specifications are otherwise identical to those presented in the tables in the paper. Standard errors are clustered by bankruptcy district and reported in parenthesis. ***, ** and * indicate statistical significance at 1%, 5%, and 10% level, respectively.

Model:		*Net charge-offs on C&I loans (% of total C&I loans)*	
Baseline	Busy court	0.437**	0.484**
		(0.194)	(0.213)
	Busy court * ln(assets)	--	-0.008
			(0.015)
Ln(assets) X time fixed effects (4 additional fixed effects)	Busy court	0.432**	1.147
		(0.194)	(0.729)
	Busy court * ln(assets)	--	-0.128
			(0.120)
Region X time fixed effects (12 additional fixed effects)	Busy court	0.495**	0.594**
		(0.210)	(0.232)
	Busy court * ln(assets)		-0.016
			(0.016)

TABLE A.6
ALTERNATIVE MEASURES OF BANK CREDIT LOSSES

This table repeats the regressions of Table VIII in the text of the paper using two alternative measures of credit losses. In the first three columns the dependent variable is net C&I loan charge-offs scaled by the average balance of non-performing C&I loans reported by the bank during that year. The last three columns scale net charge-offs by the maximum reported non-performing C&I loan balance during the year. Control variables are defined as in Table VIII in the paper, except *net charge-off rate on all other loans* is defined similarly to the dependent variable—e.g. it is scaled by average or maximum non-performing loans. All regressions include fixed effects for the 6,896 banks included in the sample as well as year fixed effects. All models are estimated by OLS. Standard errors are clustered by commercial bank and reported in parenthesis. ***, ** and * indicate statistical significance at 1%, 5%, and 10% level, respectively.

	Net charge-offs on C&I loans					
Dependent Variable:	*% of average non-performing C&I loans*			*% of maximum non-performing C&I loans*		
Busy court	90.657*	66.978	110.053**	43.887**	34.219*	55.067***
	(49.823)	(51.958)	(54.550)	(19.264)	(20.065)	(21.251)
Busy court * ln(Assets)	--	--	-7.229**	--	--	-3.498***
			(2.844)			(1.176)
Asset growth	-6.987	2.415	4.067	-10.731	-6.518	-5.710
	(21.894)	(22.002)	(21.994)	(8.479)	(8.498)	(8.496)
Net charge-off rate on all other loans	0.048	0.049	0.049	0.034	0.034	0.035
	(0.041)	(0.041)	(0.041)	(0.030)	(0.030)	(0.030)
Ln(per captia income)	--	81.824	53.746	--	23.156	9.562
		(126.359)	(127.374)		(50.522)	(51.036)
Ln(population)	--	-258.819	-281.983	--	-65.803	-76.980
		(180.676)	(180.636)		(79.089)	(79.201)
Unemployment rate	--	6.462	7.358	--	2.037	2.470
		(7.168)	(7.193)		(2.796)	(2.802)
House price appreciation	--	-190.139***	-159.768**	--	-84.947***	-70.250***
		(65.335)	(66.398)		(26.195)	(26.527)
Fixed effects:						
Bank	Yes	Yes	Yes	Yes	Yes	Yes
Year	Yes	Yes	Yes	Yes	Yes	Yes
Observations	22,008	22,008	22,008	22,008	22,008	22,008
R-squared	0.001	0.002	0.002	0.001	0.002	0.003