Get in Line: Chapter 11 Restructuring in Crowded Bankruptcy Courts

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APPENDIX

Appendix A – Dismissal from court and recidivism

A. Dismissal

In the text, I argue that most firms that are dismissed from court are likely to continue experiencing financial distress and that many will either liquidate outside of court or re-file for bankruptcy. To verify this, I randomly select 100 dismissed firms that filed for Chapter 11 and examine 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 2007).

Overall, dismissal is a close equivalent to conversion in many cases; the firm is dismissed from court but will still be liquidated.[1] Based on information in PACER, 53 of the dismissed firms I examined expected to liquidate shortly after dismissal. Very few dismissed firms were rehabilitated: 10 firms were dismissed because they had reached a settlement with their creditors, three firms were sold as going concerns and one was dismissed because it wasn't actually in financial distress.

The remaining 33 firms were dismissed from court without resolving their financial distress and would either need to resolve their financial distress out of court, liquidate out of court, or re-file for bankruptcy. 10 of these 33 firms re-filed for bankruptcy within 3 years of their original filing, while I cannot observe what happens to the remaining 23 firms. Evidence in Morrison (2007) suggests that many of them likely liquidated out of court. The regressions in the text of the paper show that firms that are dismissed from busy bankruptcy courts are substantially more likely to re-file for bankruptcy. This is likely driven by this category of firms which are dismissed without either explicit plans to liquidate or any clear solution to their financial distress. In addition, I find that the increased recidivism in busy courts is driven by dismissed firms that re-file for Chapter 11 (as opposed to Chapter 7) bankruptcy. Of the 16 total re-filings in the sample of 100, ten re-filed for Chapter 11 bankruptcy within 3 years of their initial filing, while 6 re-filed for Chapter 7. Nearly all (8 out of 10) of the "Chapter 22" filings were by firms that were dismissed from court without resolving their financial distress but for whom court documents do not explicitly show that the debtor planned to liquidate out of court. Meanwhile, 4 out of 6 of the firms that re-entered court in Chapter 7 were firms for whom liquidation was expected after dismissal. Assuming that this random sample of 100 firms is representative of the broader set of firms, this would suggest that most "Chapter 22" re-filers are firms that, after dismissal, attempt to continue operations and possibly renegotiate outside of court, but failing to do so are forced to re-enter bankruptcy.

[1] Indeed, many motions for dismissal are joint motions for either dismissal or conversion to Chapter 7.

B. Comparison of recidivism rate with previous literature

Among dismissed firms in my sample, 7.4% re-file for bankruptcy more than 3 months but less than 3 years after their original filing. An additional 2.0% of dismissed firms re-file within 3 months of their original filing. Meanwhile, 1.6% of reorganized firms re-file within 3 months of their initial filing, and 2.5% re-file between 3 months and 3 years. These recidivism rates are 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 (on average across both dismissed and reorganized firms) than the 2.9% rate reported by Chang & Schoar (2013). 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 (2013) consider only firms that re-file in the *same* district within 3 years. In addition, Morrison (2007) 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 (2013), 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.

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,

[2] Available at http://www.uscourts.gov/Statistics/BankruptcyStatistics.aspx.

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 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.

The main analysis focuses on the caseload of each bankruptcy court, as described in the text. Table A.3 provides the weights used to calculate weighted caseload in each bankruptcy district. As described in the main text, this weighted caseload is used to calculate the non-business share of total caseload. The non-business share of caseload in 2003 is the main cross-sectional variation used to identify districts that experience large drops in caseload after BAPCPA. Figure A.2 displays a map of the non-business share of caseload across the 89 bankruptcy districts in my sample. While the figure shows that

there is some geographic clustering of the non-business caseload share, robustness checks shown in Table A.9 show that my results hold within geographic regions.

Finally, Table A.4 gives information on the definition and source of control variables used in the main analysis.

B. Time in bankruptcy and workload required of court

There are two possible dates from which to mark the conclusion of a bankruptcy case: the date a decision was made about the fate of the firm (whether to be reorganized, liquidated, or dismissed), which I'll call the "resolution date," and the date on which the case was officially closed, the "closing date." For dismissed cases, these two dates are essentially the same, but for liquidated and reorganized cases legal issues typically continue to be worked out for several months after the a case is converted to Chapter 7 (the resolution date for most liquidations) or a plan of reorganization is confirmed (the resolution date for reorganizations). While the data from LexisNexis contains the date that cases are converted to Chapter 7, it does not contain the date on which a plan of reorganization is confirmed. Hence, I cannot measure the time to resolution for reorganized firms; I only observe the total time until a case is closed. Because of this, the regressions in Table VII in the text measure the time in bankruptcy as the number of months between the filing date and the resolution date for dismissed and liquidated cases, but use the closing date as the end of the case for reorganized firms. This has a minimal effect on the main coefficients of interest – in fact, regressions including fixed effects for the bankruptcy outcome show essentially identical results – but it artificially makes reorganized cases appear longer, since the period between resolution and closure is included for these cases.[3] Indeed, using this definition of time in bankruptcy, dismissed firms are in court for 13 fewer months than reorganized firms, and liquidated firms are in court for 10 fewer months. One should be careful not to interpret these estimates as suggesting that dismissals and liquidations necessarily require less work or less total time in court, as it is simply a result of a data limitation. Indeed, Bris, Welch, & Zhu (2006) show that converted cases in their sample spend as long in bankruptcy as regular Chapter 11 cases.

[3] Table A.7, described below, shows results conditional on the bankruptcy outcome.

To investigate this issue more fully, I hand-collected data directly from court dockets in PACER for 150 randomly-selected firms. Of these 150 firms, 40 were reorganized, 54 were dismissed, and 56 were liquidated. From these dockets I obtain the date of plan confirmation for reorganized firms, the date the case was dismissed for dismissed firms, and the date the case was converted to Chapter 7 for liquidated firms. I can then more directly compare the time to resolution for each of these cases.

Focusing first on the 40 reorganized cases, I find that the total time in court is a good proxy for time to resolution. The raw correlation between the two measures is 0.33 (p-value=0.03). The relationship is also statistically significant in a regression setting after controlling for size, industry and leverage of the firm. This is important as it means that I would likely find similar results to those in Table VII in the text even if I could measure time to resolution for all of the reorganized cases in my sample.

Panels A and B of Table A.5 compare the amount of time in bankruptcy across the three bankruptcy outcomes. There are no significant differences in the amount of time it takes to reach resolution for each of the three outcomes. On average, a plan of reorganization is confirmed after 12.5 months in court, cases are converted after an average of 11.5 months, and cases are dismissed after an average of 13.7 months. None of these differences is statistically different. This is in line with Bris et al. (2006) and shows that the duration until the outcome of each case is known is roughly the same for each type of outcome. Meanwhile, the amount of time until a case is fully closed is much shorter for dismissals (averaging 15 months) than for reorganizations (30 months) and liquidations (44 months).

In addition to recording the time to resolution, I also code the number of docket items for each of the 150 bankruptcy cases in the subsample. Each docket item typically represents a single event in court, and hence is a rough proxy for the amount of work the court must do to handle a case.[4] Panel C of Table A.5 tests whether the number of docket items per month differs by bankruptcy outcome, after controlling for other factors that may affect the complexity of the case, such as size and industry. I find that dismissed and liquidated cases have on average 5.3 and 9.1 fewer docket items per month than

[4] Examples of docket items include motions for specific actions, objections to those motions, notices of hearings, orders of the court, and affidavits of particular services.

reorganizations, respectively. However, it should be noted that these are non-causal estimates, and indeed the causality may go in the opposite direction: some cases may be dismissed or liquidated precisely because the firm is not filing reports or moving the case forward in court. Because of this, it is difficult to determine empirically whether pushing a marginal case to liquidate rather than reorganize is more or less work for the judge overall. While the *average* dismissal or liquidation may have fewer docket items, liquidating or dismissing a *marginal* case could in fact be quite costly for a busy judge, as he would likely have to do over the objections of the debtor's management.

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 by scaling net charge-offs either by total outstanding loans or non-performing loans. Appendix C Section C discusses in detail the choice of these two measures and also shows that using an alternative measure gives identical results to those presented in the paper. In this section, I discuss how banks are required to account for loan losses and whether this induces a bias in my estimates.

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. This discretion in reporting charge-offs and recoveries might make one 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. Specifically, there is no clear reason why BAPCPA might alter the incentives for banks in reporting loan charge-offs, nor is there any reason to believe that this would affect banks differentially in consumer-centric bankruptcy districts. 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 – Additional analysis and robustness checks

A. Additional analysis

Table A.6 reports the effect of an exogenous drop in caseload on commercial real estate (CRE) loan charge-offs. In the main text, I focus on commercial and industrial (C&I) loan charge-offs because they are typically unsecured and are thus most sensitive to changes in default costs. Meanwhile, CRE loans are, by definition, secured and thus should be insulated from shocks to court caseload. Accordingly, Table A.6 shows that banks located in courts with exogenously lower caseload did not experience lower CRE loan charge-off rates. This can be viewed as a placebo test of sorts for the C&I loan charge-off results in the main test. If business conditions had improved systematically in low-caseload courts after BAPCPA, one would expect CRE loan charge-off rates to fall as well. Instead, I find that the improvements in charge-offs were concentrated in C&I loans.

Table A.7 repeats that analysis of Table VII in the main text, but on separate subsamples of reorganized, liquidated, and dismissed cases. Among firms that reorganize, the point estimate of the key coefficient is negative for both large and small firms, suggesting that a reduction in caseload marginally reduces time in bankruptcy for all firms that reorganize. Meanwhile, in liquidated and dismissed cases, time in bankruptcy is estimated to rise for small firms and decrease for large firms. Due to smaller sample sizes in these regressions, coefficient estimates are not precise and only in the case of liquidated firms is the difference between large and small firms statistically significant (and that only at the 10% level). But these results are at least qualitatively consistent with the hypothesis that busy judges tend to be hesitant to

liquidate large cases while simultaneously liquidating or dismissing smaller cases more quickly. Thus, when caseload falls, judges can take relatively more time to liquidate small cases. This mirrors the results discussed in Section V.B of the main text, which show that when caseload declines large firms are less likely to be reorganized and more likely to liquidate.

B. 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.8 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.

C. Parallel Trends

In order for the difference-in-differences regressions to detect a causal effect of caseload, the parallel trends assumption must hold. I test this assumption in Figure A.3 by running regressions of the form:

$$Y_i = \sum_{t=2004Q1}^{2007Q4} \beta_t(\mu_t \times NonBusCaseload_d) + \gamma X_i + \tau_d + \mu_t + \varepsilon_i,$$

where μ_t are year-quarter dummies for all quarter in my sample. Figure A.3 then plots the coefficients β_t (as well as 95% confidence intervals) over time. The parallel trends assumption is satisfied if the coefficients are stable in the period prior to BAPCPA, indicating that Y_i was not changing differentially in more affected courts prior to the caseload shock. Similarly, in the right-hand panels I also display coefficients on the interaction $\mu_t \times NonBusCaseload_d \times \ln(size)$, to verify that there are no pre-trends

differentially by size. In all cases I fail to find evidence that the key outcome variables were trending prior to BAPCPA, consistent with the parallel trends assumption.

D. 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 48 month 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 1,440 industry-month fixed effects. Thus, one would expect that statistical power will be somewhat reduced in these specifications.

Table A.9 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, with the exception of the effect of caseload on asset sales, which is slightly smaller. Adding industry-by-month fixed effects reduces the statistical significance of the effect of caseload on the probability of reorganization. However, the coefficient estimates are very similar to my main specifications, indicating that the loss of significance is due only to the reduced statistical power in these robustness checks. Aside from this specification, all other coefficients retain statistical significance and are essentially unchanged or even larger with the inclusion of industry-month 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 nine groups: New England, Middle Atlantic, East North Central, West North Central, South Atlantic,

East South Central, West South Central, Mountain, and Pacific.[5] 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 any of the broad conclusions. The largest difference from the baseline estimates is that the effect of caseload on asset sales is slightly smaller and insignificant. Finally, I show in Table A.9 results when also including firm controls interacted with the post-BAPCPA dummy, to allow the impact of firm characteristics to vary over time. Results are unchanged in these specifications.

Table A.10 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 located to identify the census region it belongs to. The inclusion of region-by-time fixed effects reduces the statistical significance of a few of the point estimates, but the economic magnitude of all estimates is close to the baseline.

The exclusion restriction could also be violated if there is selection bias after BAPCPA. Specifically, different types of firms may file for bankruptcy when caseload falls if debtors and creditors change their tactics in response to court congestion. The tests in the bottom of Table A.9 suggest that these effects are not driving my estimates by showing that my results hold even when allowing for the impact of firm characteristics to change after BAPCPA. I further test this in Table A.11 by regressing the main cross-sectional variable *low caseload court* on each firm characteristic. If a decline in caseload leads particular types of firms to opt in or out of bankruptcy, one would expect to find a significant relationship between *low caseload court* and firm characteristics. However, I fail to find such a relationship.

Finally, another possible violation of the exclusion restriction relates to the way that courts incorporated the BAPCPA law changes. Although BAPCPA was a national law, if consumer-centric courts enforced it differently (e.g. more strictly), then it is possible that the effects I find are due to the

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

law change and not court caseload. I test for this by examining one of the main changes imposed by BAPCPA on corporate bankruptcies: case deadlines. Specifically, BAPCPA limits the exclusivity period (the period during which only the debtor firm may propose a plan of reorganization) to 18 months, whereas prior to BAPCPA it could be extended indefinitely. Further, following BAPCPA, firms with debt under $2 million are required to file a plan of reorganization within 300 days. If consumer-centric courts applied these deadlines more strictly after BAPCPA, one would expect that following BAPCPA there would be a high number of cases that exit soon after these deadlines in these courts. I test for this by displaying Q-Q plots, which compare 100 quantiles of the months-in-bankruptcy distribution across each type of court, in Figure A.4. Panel A shows the Q-Q plot during the pre-BAPCPA period, and Panel B shows it for the post-BAPCPA period. If the distribution of time in bankruptcy is similar in consumer- and business-centric courts, the points should line up along the 45-degree line. On the other hand, if there is bunching in a particular kind of court, the points would deviate from this line. As can be seen, there is no bunching apparent in either the pre- or post-BAPCPA time periods.

E. Alternative measures of bank loan losses

In the text of the paper, I scale net charge-offs two separate ways to measure default costs borne by banks due to busy courts. I calculate the overall charge-off rate by scaling net C&I 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 reported in quarter 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 precise measure. For example, the following is data from an actual bank in my sample:

				Net charge-offs scaled by:		
Date	*Year-to-date net C&I loan charge-offs*	*Non-performing C&I loans*	*Total outstanding C&I loans*	*Avg. total C&I loans*	*Max. non-performing C&I loans*	*Avg. non-performing C&I loans*
2005q1	-22	244	35460			
2005q2	103	121	34840			
2005q3	117	501	31225			
2005q4	211	353	33249	0.63%	42.12%	69.24%
2006q1	101	286	31102			
2006q2	170	232	31640			
2006q3	145	320	31234			
2006q4	263	81	29666	0.85%	82.19%	114.47%

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. Indeed, scaling by end-of-year outstanding C&I loans or C&I loans averaged over the previous 6 quarters does not change my estimates appreciably.[6] 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. In the text, I use net charge-offs scaled by the maximum level of non-performing C&I loans during the year to proxy for LGD_t. This measure is provided for the bank in the table above in the sixth column, and would be calculated as 211 / 501 in 2005 and 263 / 320 in 2006. This measure has the advantage of ignoring low values of non-performing loans (for example in 2006q4), 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. The table above also shows another

[6] Results not reported, but available upon request.

alternative proxy for LGD_t in the final column, measured by scaling 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 the *net charge-off rate* is calculated, but it has the drawback of being very volatile. For example, for this bank the value 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. For this reason, my preferred measure uses the maximum of non-performing loans as the denominator.

Importantly, neither proxy for LGD_t is likely to be a biased measure of loss given default, only noisy. Accordingly, Table A.12 presents regressions similar to those in Table IV in the text of the paper except it uses the alternative definition of loss given default as the dependent variable. As in the text of the paper, all bank variables are winsorized at the 1st and 99th percentile to account for outliers, an adjustment that is particularly important for this noisy measure of loss given default. I find nearly identical results using this alternative measure of credit losses, although the statistical significance is slightly lower because it is less precisely measured (and both are noisier measures than the *net charge-off rate*). Specifically, a 64-hour decrease in caseload is estimated to reduce this alternative measure of LGD_t by 7.7 percentage points, a 9.6% decrease relative to its mean value of 80 percent. This is very close to the estimated impacts of 10% and 11% reported in the text of the paper. Further, I continue to find that small banks are most affected by busy courts, which mirrors 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.

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TABLE A.1
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
BANKRUPTCY CASE WEIGHTS

This table displays the weights assigned to each of six different bankruptcy types by Bermant et al. (1991) in their bankruptcy court time study. The weights are equal to the expected number of hours a judge will spend on an individual case of that type.

Bankruptcy Type	Expected hours per case
Ch. 11	7.559
Ch. 12	4.04
Business Ch. 7	0.397
Ch. 13	0.381
Other	0.194
Non-business Ch. 7	0.101

TABLE A.4
VARIABLE DESCRIPTIONS

This table defines the main variables used in the analysis of the paper and gives their source.

Variable name	Description	Source
Non-business caseload share	% of total caseload derived from non-business bankruptcy filings in 2003	US Courts filing statistics
Post BAPCPA	Dummy = 1 if after October 2005	
Low caseload court	Non-business caseload share * Post BAPCPA	
Ln(size)	Natural log of max(assets, liabilities) at filing	CapitalIQ if avail., then Deal Pipeline
Liabilities > assets at filing	Dummy = 1 if liabilities greater than assets	CapitalIQ if avail., then Deal Pipeline
Group filing	Dummy = 1 if multiple related entities filed jointly	LexisNexis
Distributable assets	Dummy = 1 if firm had non-exempt assets available for distribution	LexisNexis
Public firm	Dummy = 1 if firm was public at time of bankruptcy	CapitalIQ and the Deal Pipeline
Got DIP loan	Dummy = 1 if firm obtained debtor-in-possession financing	CapitalIQ and the Deal Pipeline
Industry fixed effects	30 Fama-French industry dummies	CapitalIQ and the Deal Pipeline
Month fixed effects	48 year-month dummies	
Bankruptcy District fixed effects	89 bankruptcy district dummies	LexisNexis

TABLE A.5
TIME IN BANKRUPTCY SUMMARY STATS FOR SUBSAMPLE

This table tests whether time in bankruptcy and court workload differs depending on the ultimate outcome of the bankruptcy case for a random subsample of 150 cases for which I hand-collected information on directly form PACER. Panel A shows summary statistics on the number of months spent until an outcome of the case is decided. The "resolution date" is the date on which a plan is confirmed, the case is converted to Chapter 7, or the case is dismissed. Panel B shows summary statistics on the total number of months until a case is fully closed in court. For reorganized and liquidated firms, court proceedings continue for many months after the outcome has been decided as the plan is implemented or assets are sold. Panel C presents regressions which show that dismissed and liquidated firms have fewer docketed items per month than reorganized firms. In the first column, the dependent variable is the total number of docket items divided by the number of months until the case is fully closed. The dependent variable in the second column is the number of docket items recorded until the outcome is decided, divided by the number of months until this decision is reached. Control variables are identical to those used in the text of the paper, except fixed effects for 5 Fama-French industries are used, instead of 30, due to the small sample size. ***, ** and * indicate statistical significance at 1%, 5%, and 10% level, respectively.

Panel A: Months to Bankruptcy Resolution

	Obs.	Mean	Std. Dev.	Min	Median	Max
Reorganized	40	12.5	7.2	3.9	10.0	39.1
Liquidated	56	11.5	8.6	0.3	9.6	38.2
Dismissed	54	13.7	14.1	0.8	10.1	84.7

Panel B: Months to Bankruptcy Case Closure

	Obs.	Mean	Std. Dev.	Min	Median	Max
Reorganized	40	30.2	17.9	7.9	23.0	76.6
Liquidated	56	44.2	24.0	2.0	44.6	95.5
Dismissed	54	14.8	15.2	1.1	10.2	84.7

TABLE A.5 - *continued*

Panel C: Docket items per month

Dependent variable:	*Number of docket items per month*	
Sample:	Full case	Until case resolution
Dismissed	-5.352**	-14.404***
	(2.416)	(3.768)
Liquidated	-9.058***	-10.688**
	(2.804)	(4.809)
Ln(size)	5.719***	13.348***
	(1.358)	(3.526)
Liabilities > assets at filing	3.565*	6.429
	(1.946)	(4.660)
Group filing	0.692	10.078
	(4.131)	(7.434)
Distributable assets	-1.773	-4.883
	(2.720)	(5.549)
Industry fixed effects	Yes	Yes
Observations	150	150
R-squared	0.582	0.487

TABLE A.6
THE EFFECT OF CASELOAD ON CRE LOAN CHARGE-OFFS

This table shows how changes in caseload affected the performance of commercial real estate (CRE) loans held by commercial banks. These regressions are similar to Table IV in the main text, except they use charge-off rates on CRE loans as the dependent variable instead of C&I loans. The purpose of these regressions is to show that recovery rates on secured loans, such as CRE, were not affected by changes in caseload, as opposed to recovery rates on C&I lending. All models are estimated by OLS. Standard errors are clustered by bank to account for serial correlation across years, and are reported in parentheses. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% level, respectively.

	Net charge-offs on CRE loans			
Dependent Variable:	*% of total CRE loans*		*% of maximum non-performing CRE loans*	
Low caseload court	-0.028	-0.118	0.091	-4.365
	(0.039)	(0.147)	(3.458)	(11.508)
Low caseload court * ln(bank assets)	--	0.013	--	0.573
		(0.022)		(1.837)
Post BAPCPA * ln(bank assets)	--	-0.007	--	-0.479
		(0.017)		(1.433)
Non-business caseload share * ln(bank assets)	--	0.060***	--	4.198**
		(0.021)		(1.961)
Asset growth	-0.102***	-0.122***	-5.867***	-7.454***
	(0.021)	(0.022)	(2.004)	(2.033)
Net charge-off rate on all other loans	0.090***	0.089***	0.009	0.009
	(0.011)	(0.011)	(0.006)	(0.006)
Ln(per capita income)	-0.099	-0.160	-7.611	-10.334
	(0.123)	(0.124)	(10.868)	(10.920)
Ln(population)	-0.090	-0.201	5.910	1.831
	(0.169)	(0.169)	(14.667)	(14.659)
Unemployment rate	0.004	0.005	0.948	0.966*
	(0.007)	(0.007)	(0.583)	(0.583)
House price appreciation	0.090**	0.121***	8.402**	9.351**
	(0.041)	(0.041)	(4.001)	(4.115)
Fixed effects:				
Bank	Yes	Yes	Yes	Yes
Year	Yes	Yes	Yes	Yes
Observations	28,941	28,941	17,154	17,154
Adjusted R-squared	0.128	0.129	0.054	0.054

TABLE A.7

THE EFFECT OF CASELOAD ON TIME IN BANKRUPTCY – SEPARATED BY CASE OUTCOME

This table explores the relation between the change in caseload due to BAPCPA and the duration of the firm's time in bankruptcy. These regressions are similar to those in Table VII in the paper, except here the sample is split by the eventual bankruptcy outcome. 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% level, respectively.

Dependent variable:	*Months in bankruptcy*					
Sample:	Reorganized		Liquidated		Dismissed	
Low caseload court	-6.052	-1.331	5.362	10.655	4.255	5.151
	(7.036)	(7.742)	(4.338)	(8.437)	(3.924)	(5.243)
Low caseload court * ln(size)	--	-2.110	--	-4.607*	--	-0.652
		(1.722)		(2.321)		(3.402)
Post BAPCPA * ln(size)	--	1.249*	--	-0.874	--	0.607
		(0.728)		(1.509)		(2.484)
Non-business caseload share * ln(size)	--	1.556	--	2.696	--	0.359
		(1.859)		(1.989)		(2.650)
Ln(size)	1.497***	0.587	2.517***	3.629***	0.799**	0.445
	(0.543)	(1.347)	(0.477)	(1.241)	(0.394)	(1.776)
Liabilities > assets at filing	-1.380	-1.356	-1.858**	-1.714*	1.682**	1.669**
	(1.147)	(1.150)	(0.898)	(0.896)	(0.827)	(0.836)
Group filing	-0.884	-0.955	-0.344	-1.110	1.716	1.705
	(1.767)	(1.737)	(1.587)	(1.568)	(1.900)	(1.905)
Public firm	-2.178	-2.051	0.726	0.439	0.361	0.374
	(2.666)	(2.405)	(2.487)	(2.441)	(3.568)	(3.533)
Got DIP loan	-1.895	-2.012	6.954***	6.895***	8.047***	8.059***
	(2.046)	(2.044)	(1.734)	(1.727)	(2.334)	(2.353)
Month, industry, and district fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Observations	938	938	1,173	1,173	1,125	1,125
Adjusted R-squared	0.177	0.175	0.211	0.235	0.088	0.085

TABLE A.8
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 for dismissed firms. 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 (Dismissed firms)*		*Months in bankruptcy*		*Has asset sale*		*Obtained DIP loan*	
Baseline	Low caseload court	-0.099	0.104	-0.227**	-0.308**	1.383	6.610	-0.094**	-0.111	-0.112**	-0.003
		(0.091)	(0.134)	(0.102)	(0.120)	(3.730)	(5.197)	(0.047)	(0.068)	(0.052)	(0.079)
	Low caseload court * ln(size)	--	-0.100**	--	0.083	--	-2.794**	--	0.018	--	-0.049**
			(0.042)		(0.060)		(1.123)		(0.027)		(0.021)
Size winsorized at 99th percentile	Low caseload court	-0.099	0.117	-0.227**	-0.312**	1.407	5.299	-0.092*	-0.105	-0.110**	-0.024
		(0.092)	(0.130)	(0.102)	(0.121)	(3.829)	(5.290)	(0.048)	(0.073)	(0.051)	(0.080)
	Low caseload court * ln(size)	--	-0.105**	--	0.088	--	-2.335**	--	0.017	--	-0.040**
			(0.042)		(0.061)		(1.149)		(0.030)		(0.019)
DE & SDNY winsorized	Low caseload court	-0.053	0.184	-0.359**	-0.459***	2.620	8.272	-0.089	-0.117	-0.218**	-0.035
		(0.158)	(0.196)	(0.159)	(0.157)	(5.183)	(6.001)	(0.088)	(0.107)	(0.090)	(0.113)
	Low caseload court * ln(size)	--	-0.173**	--	0.168*	--	-4.100**	--	0.027	--	-0.104***
			(0.080)		(0.094)		(1.943)		(0.052)		(0.035)

TABLE A.9
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. The top four rows of the table repeat the baseline regression results reported in the text of the paper. Subsequently, results are shown when including as controls (i) ln(*size*)-by-time fixed effects, (ii) industry-by-time fixed effects, (iii) census division-by-time fixed effects, and (iv) all firm controls interacted with the post BAPCPA dummy. 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 (Dismissed firms)*		*Months in bankruptcy*		*Has asset sale*		*Obtained DIP loan*	
Baseline	Low caseload court	-0.099	0.104	-0.227**	-0.308**	1.383	6.610	-0.094**	-0.111	-0.112**	-0.003
		(0.091)	(0.134)	(0.102)	(0.120)	(3.730)	(5.197)	(0.047)	(0.068)	(0.052)	(0.079)
(167 total fixed effects)	Low caseload court * ln(size)	--	-0.100**	--	0.083	--	-2.794**	--	0.018	--	-0.049**
			(0.042)		(0.060)		(1.123)		(0.027)		(0.021)
Size X time fixed effects	Low caseload court	-0.134	0.153	-0.255**	-0.050	0.701	9.456*	-0.060	-0.103*	-0.145**	-0.011
		(0.094)	(0.138)	(0.102)	(0.138)	(3.997)	(5.444)	(0.057)	(0.053)	-0.066	(0.070)
(48 additional fixed effects)	Low caseload court * ln(size)	--	-0.128***	--	-0.024	--	-2.800*	--	0.018	--	-0.043**
			(0.042)		(0.096)		(1.418)		(0.023)		(0.019)
Industry X time fixed effects	Low caseload court	0.002	0.159	-0.321***	-0.508**	-6.657	3.522	-0.203**	-0.123*	-0.164**	-0.022
		(0.106)	(0.215)	(0.117)	(0.201)	(5.045)	(6.293)	(0.101)	(0.069)	(0.066)	(0.107)
(1,440 additional fixed effects)	Low caseload court * ln(size)	--	-0.086	--	0.156	--	-5.255***	--	-0.035	--	-0.057*
			(0.067)		(0.114)		(1.890)		(0.030)		(0.031)
Region X time fixed effects	Low caseload court	-0.187*	0.024	-0.329	-0.528**	-0.616	6.642	-0.066	-0.011	-0.006	0.115
		(0.102)	(0.129)	(0.217)	(0.222)	(3.135)	(4.442)	(0.055)	(0.068)	(0.062)	(0.108)
(432 additional fixed effects)	Low caseload court * ln(size)	--	-0.093**	--	0.206**	--	-3.483***	--	-0.010	--	-0.057**
			(0.036)		(0.079)		(1.260)		(0.024)		(0.028)
Controls X Post BAPCPA dummy	Low caseload court	-0.102	0.125	-0.232**	-0.318**	-0.456	6.068	-0.073	-0.131*	-0.152**	-0.002
		(0.095)	(0.138)	(0.106)	(0.127)	(4.733)	(5.514)	(0.047)	(0.069)	(0.058)	(0.080)
	Low caseload court * ln(size)	--	-0.104**	--	0.090	--	-2.809**	--	0.028	--	-0.056**
			(0.040)		(0.058)		(-2.809)		(0.028)		(-0.021)

TABLE A.10
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. 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 9 census division: New England, Middle Atlantic, East North Central, West North Central, South Atlantic, East South Central, West South Central, Mountain, and Pacific. All specifications are otherwise identical to those presented in the tables in the paper. 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			
Model:		*% of total C&I loans*		*% of maximum non-performing C&I loans*	
Baseline	Low caseload court	-0.437**	-1.099	-34.219*	-139.705***
		(0.194)	(0.730)	(20.065)	(50.420)
	Low caseload court * ln(*size*)	--	0.110	--	17.145**
			(0.121)		(7.583)
Ln(assets) X time fixed effects	Low caseload court	-0.432**	-1.084	-34.649*	-139.154***
(4 additional fixed effects)		(0.194)	(0.730)	(20.121)	(50.373)
	Low caseload court * ln(*size*)	--	0.107	--	17.074**
			(0.120)		(7.578)
Region X time fixed effects	Low caseload court	-0.403*	-0.870	-23.021	-115.344**
(36 additional fixed effects)		(0.218)	(0.747)	(23.382)	(53.588)
	Low caseload court * ln(*size*)	--	0.076	--	15.026**
			(0.122)		(7.720)

TABLE A.11
TESTS FOR SELECTION EFFECTS

This table reports regressions which test for a relationship between *low caseload court* and firm characteristics. The dependent variable is a firm characteristic, and for each characteristic I test for this correlation both including and excluding all other firm characteristics as controls. 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.

Dependent variable:	*Ln(size)*		*Liabilities>assets at filing*		*Group filing*		*Public Firm*	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Low caseload court	-0.191	0.160	-0.047	-0.028	-0.114	-0.088	0.004	0.031
	(0.609)	(0.550)	(0.141)	(0.144)	(0.072)	(0.060)	(0.027)	(0.024)
Ln(size)	--	--	--	-0.022***	--	0.061***	--	0.025***
				(0.006)		(0.006)		(0.004)
Liabilities>assets at filing	--	-0.179***	--	--	--	0.032**	--	-0.010
		(0.047)				(0.013)		(0.010)
Group filing	--	1.135***	--	0.071**	--	--	--	0.059***
		(0.107)		(0.027)				(0.016)
Public firm	--	1.236***	--	-0.060	--	0.159***	--	--
		(0.206)		(0.056)		(0.046)		
Fixed effects:								
Industry	No	Yes	No	Yes	No	Yes	No	Yes
Year-month	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
District	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	3,236	3,236	3,236	3,236	3,236	3,236	3,236	3,236
Adj. R-squared	0.172	0.303	0.0264	0.0854	0.0635	0.164	0.0537	0.130

TABLE A.12
ALTERNATIVE MEASURE OF BANK CREDIT LOSSES

This table repeats the regressions of Table VIII in the text of the paper using an alternative measure of credit losses. In these regressions, 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. Control variables are defined as in Table IV 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 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.

Dependent variable:	*Net C&I loan charge-offs (% of average non-performing C&I loans)*	
Low caseload court	-66.978	-368.898***
	(51.958)	(130.790)
Low caseload court * ln(Assets)	--	49.761**
		(19.633)
Post BAPCPA * ln(Assets)	--	-35.344**
		(15.488)
Non-business caseload share * ln(Assets)	--	36.460*
		(20.617)
Asset growth	2.415	-9.567
	(22.002)	(22.919)
Net charge-off rate on all other loans	0.049	0.049
	(0.041)	(0.041)
Ln(per capita income)	81.824	37.225
	(126.359)	(128.126)
Ln(population)	-258.819	-330.064*
	(180.676)	(183.117)
Unemployment rate	6.462	6.701
	(7.168)	(7.214)
House price appreciation	-190.139***	-167.250**
	(65.335)	(66.797)
Fixed effects:		
Bank	Yes	Yes
Year	Yes	Yes
Observations	22,008	22,008
R-squared	0.111	0.112

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.

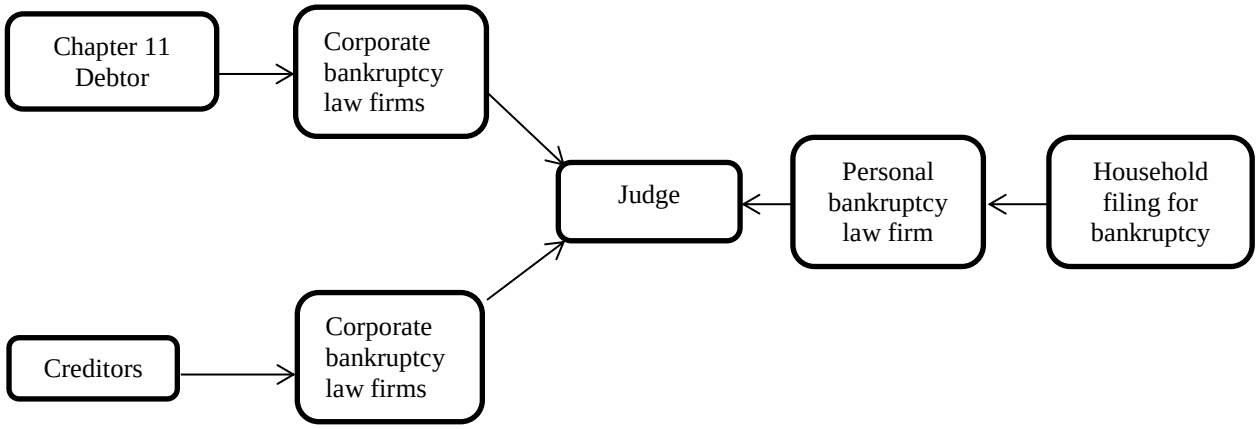


FIGURE A.2
U.S. BANKRUPTCY DISTRICT MAP

This map displays the 89 bankruptcy districts across the United States. Colors correspond to the share of 2003 caseload that was related to non-business bankruptcy filings. Districts in yellow have the highest non-business share of caseload and hence experienced the largest drop in workload following BAPCPA. Red districts are the most business-centric, while orange districts lie in the middle of the distribution.

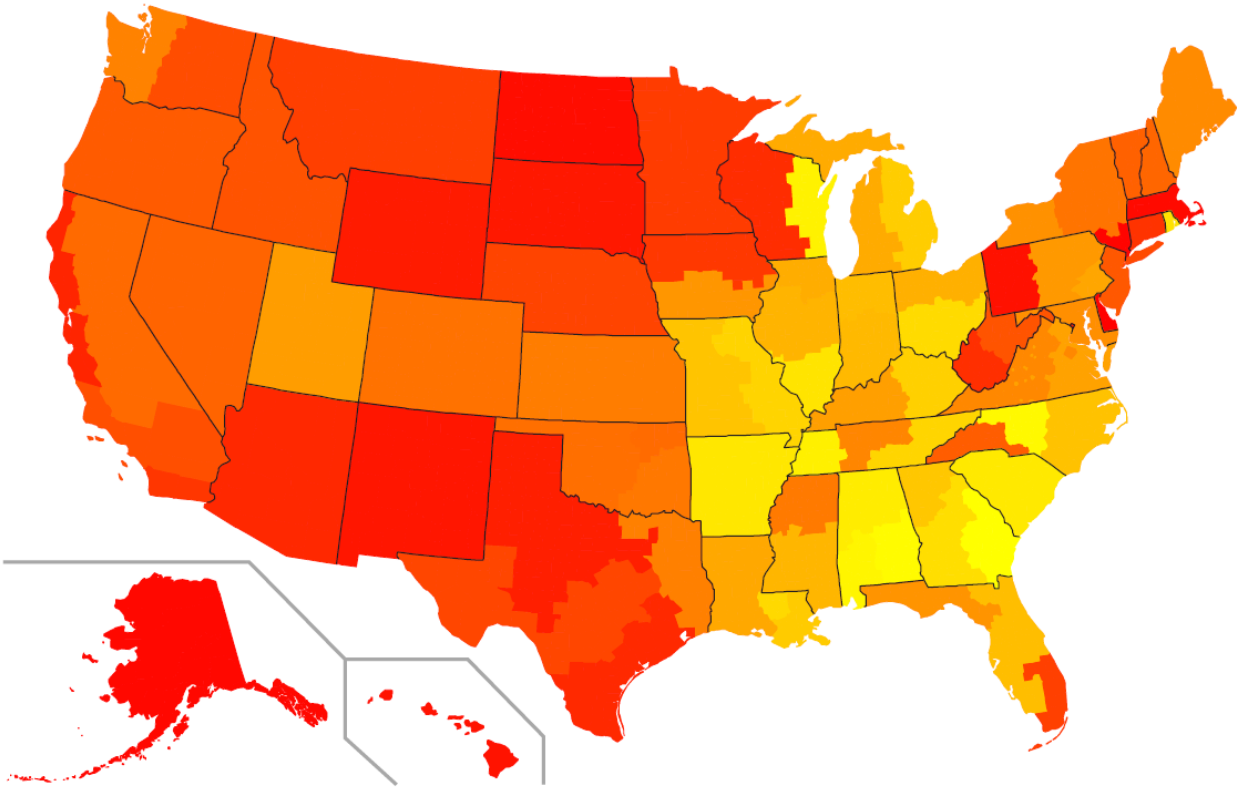
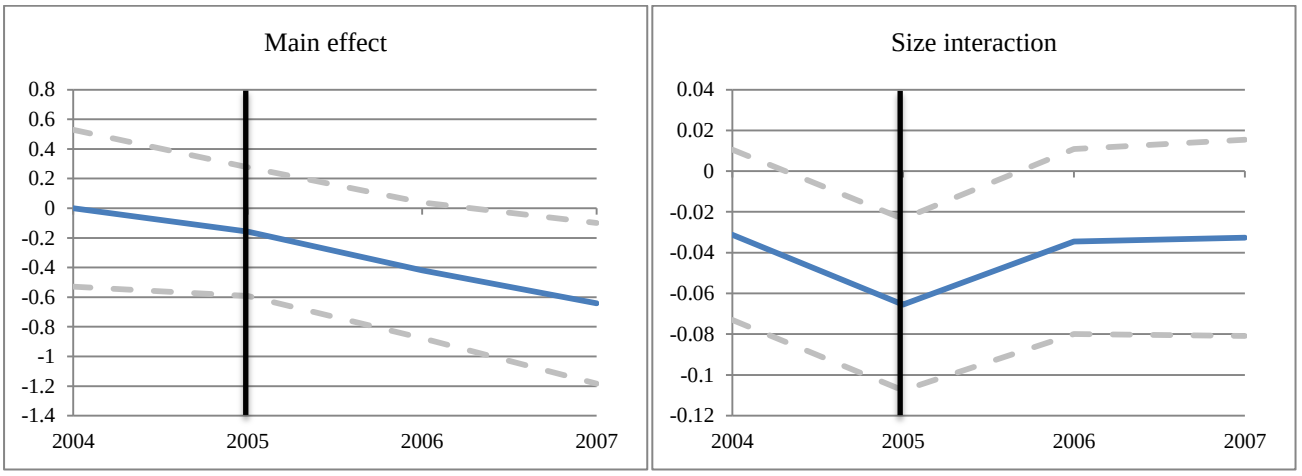


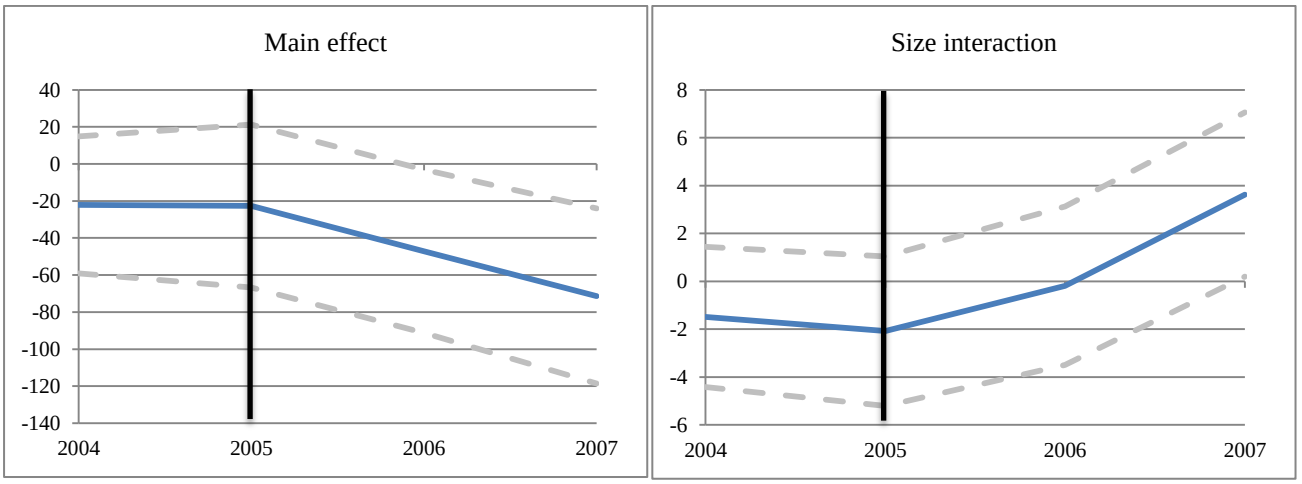
FIGURE A.3
DEPENDENT VARIABLE PRE-TRENDS

This figure tests for pre-trends in the key dependent variables in the paper. In each panel, the left figure ("main effect") displays the coefficient and 95% confidence interval on the interaction of *non-business caseload* and year-quarter dummies from a regression on the dependent variable indicated. Meanwhile, the right figure ("size interaction") displays the coefficient on the interaction of *non-business caseload*ln(size)* and year-quarter dummies, to test for size-specific pre-trends. Only annual data is available for bank charge-off rates, so annual dummies are used for these regressions. Vertical lines denote the passage of BAPCPA, so if pre-trends exist it should be prior to this time period.

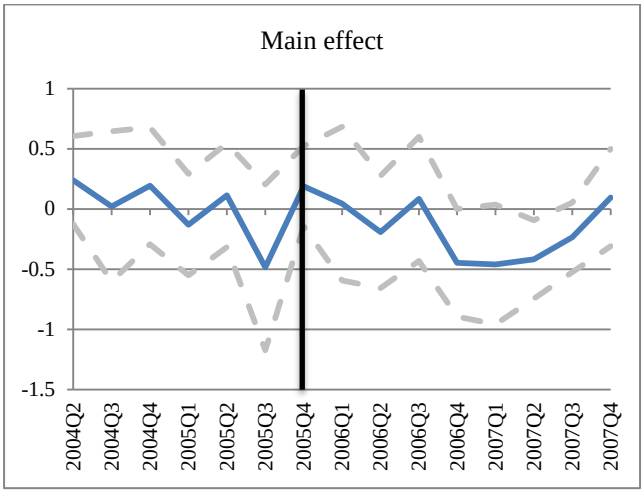
Panel A: Net charge-offs on C&I loans (% of total C&I loans)

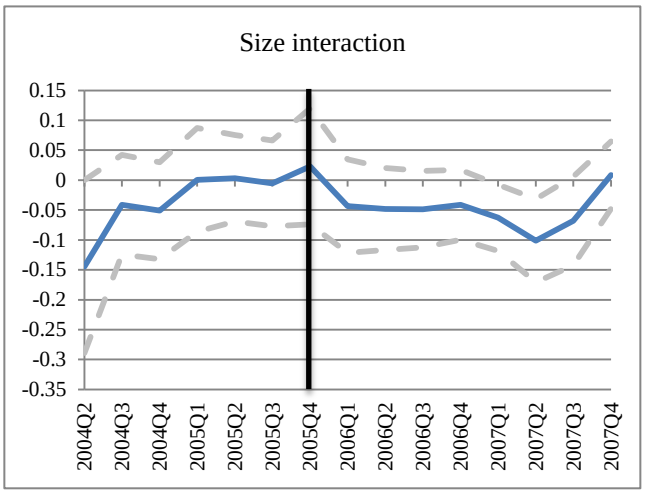


Panel B: Net charge-offs on C&I loans (% of maximum non-performing C&I loans)

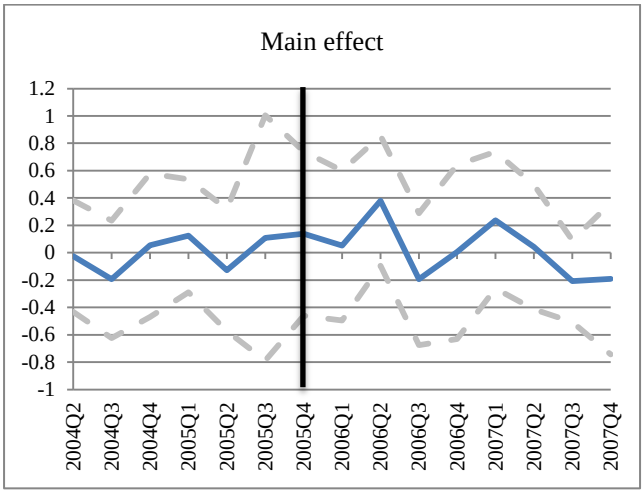


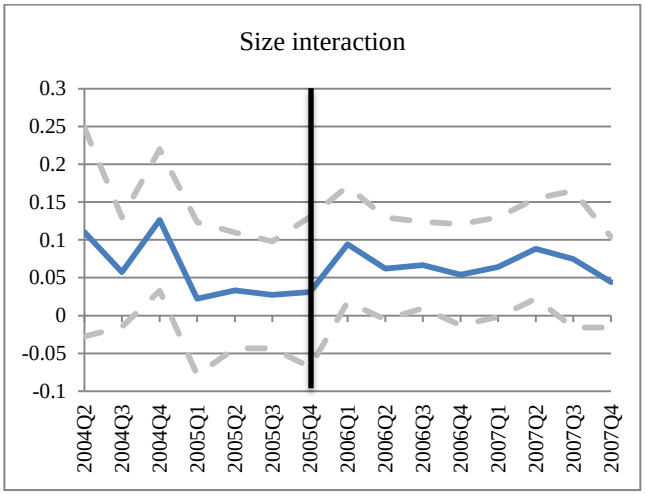
Panel C: Reorganized



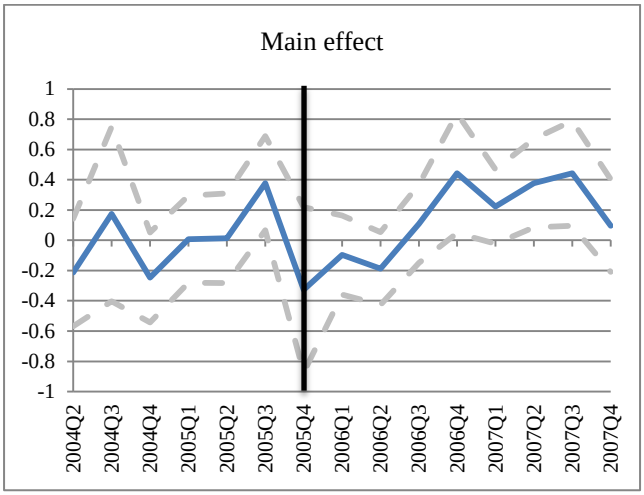


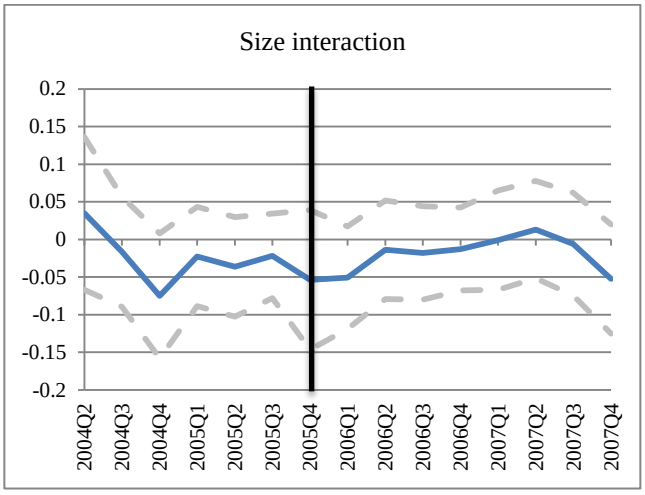
Panel D: Liquidated



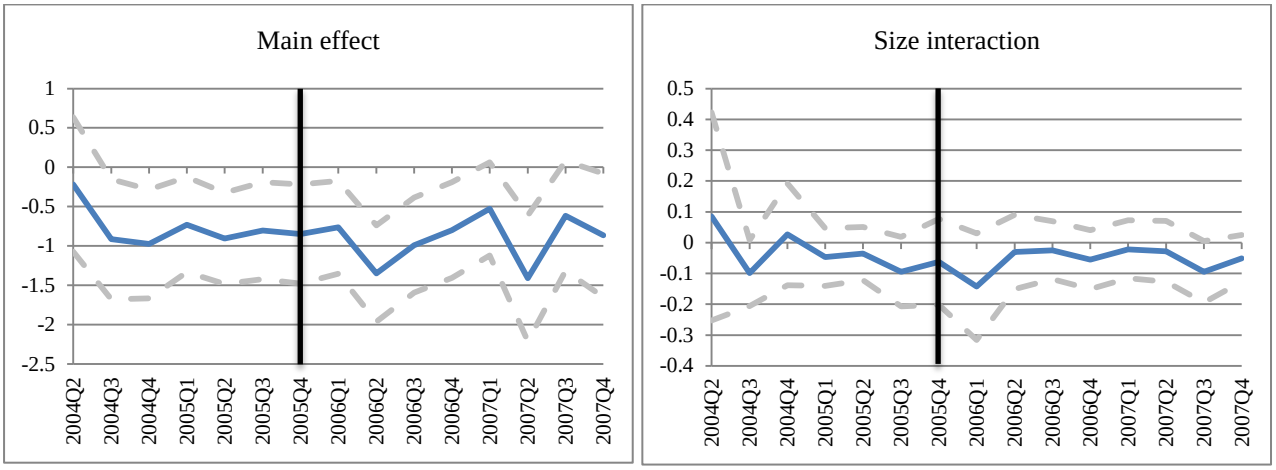


Panel E: Dismissed

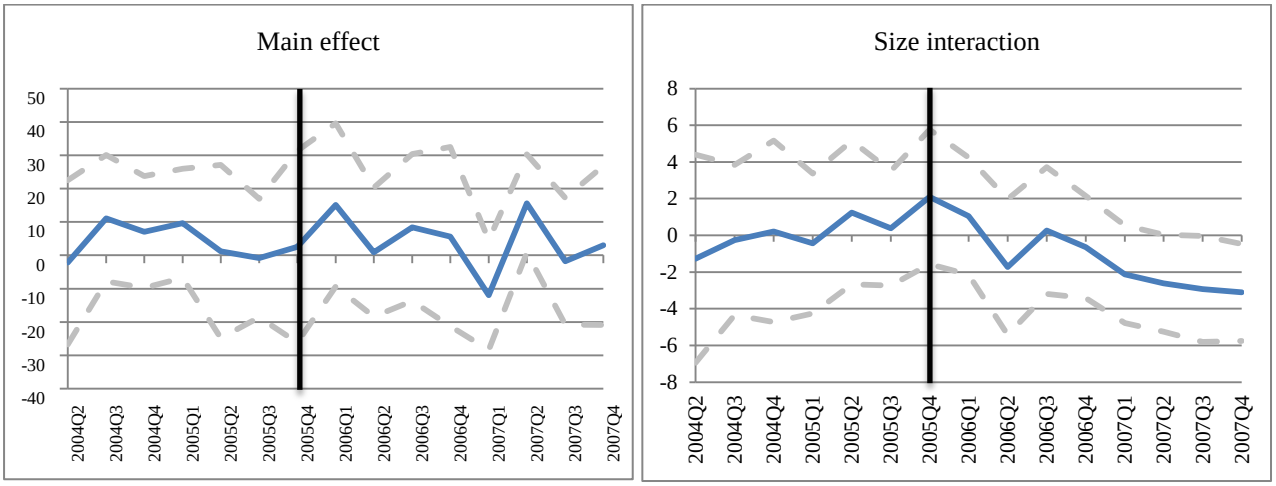




Panel F: Recidivism – Dismissed firms

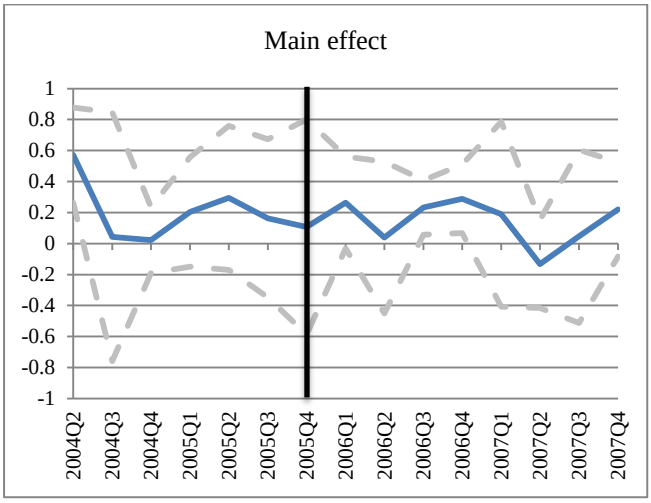


Panel G: Months in bankruptcy



Panel H: Has Asset Sale

Panel I: Obtained DIP Loan



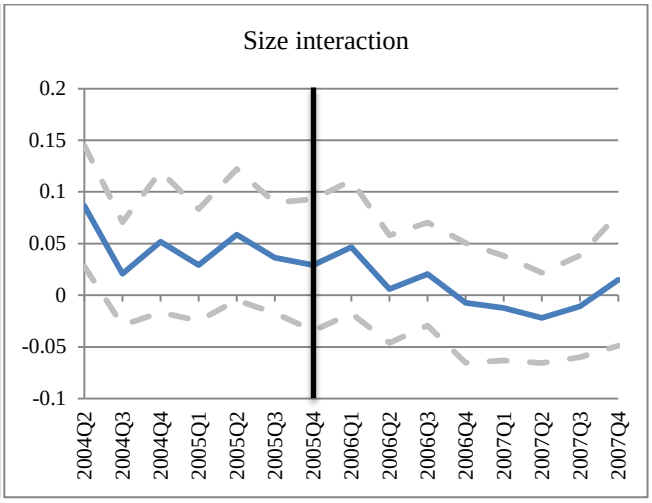
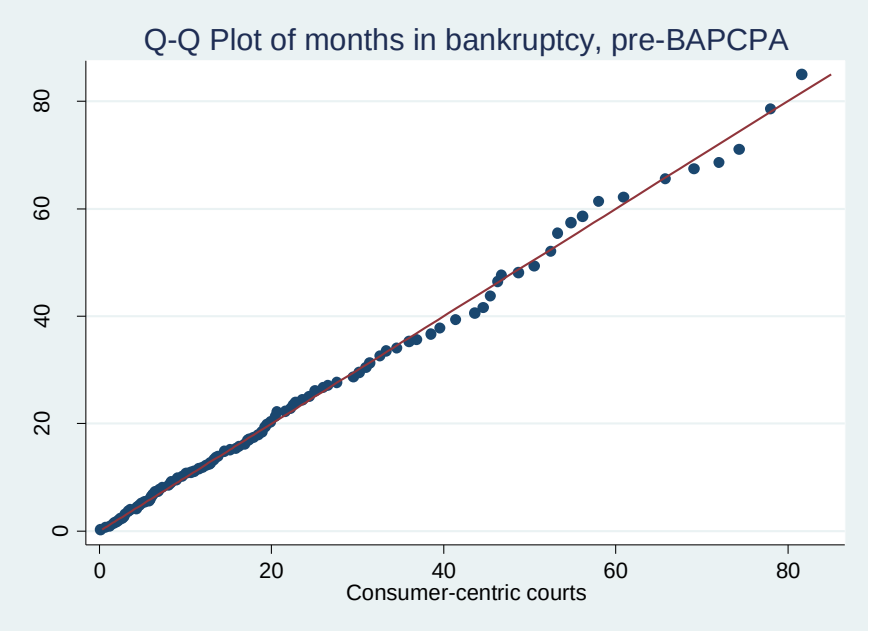


FIGURE A.4
BAPCPA CASE DEADLINES

This figure examines whether new deadlines imposed by BAPCPA were more strictly enforced in business- or consumer-centric bankruptcy courts. Each figure shows a Q-Q plot which compares 100 quantiles of the distribution of months in bankruptcy in consumer-centric to those of business-centric courts. If the distributions are similar, the points should line up on the displayed 45-degree line. Panel A shows the Q-Q plot prior to BAPCPA, while Panel B shows the plot using post-BAPCPA cases.

Panel A: Pre-BAPCPA



Panel B: Post-BAPCPA