

# Judge Specific Differences in Chapter 11 and Firm Outcomes

Tom Chang  
and  
Antoinette Schoar<sup>1</sup>

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<sup>1</sup>USC Marshall School of Business; MIT Sloan School of Management, NBER and CEPR. Michael Sugar, Michael Altemin, Dwayne Kennemore, Rachel Shaw, Stewart Park, Suhitha Reddy, Iraklis Sbarounis and Wendy Wen provided outstanding research assistance. We thank Bernard Black, Glenn Ellison, Stuart Gilson, Edith Hotchkiss, Lynn LoPucki, Judge James Massey, Sendhil Mullainathan, Nancy Rose, Karen Thorburn, Ivo Welch and participants at the AFA and ALEA annual meetings, NBER Summer Institute, Financial Contracting: Theory and Evidence conference and seminar participants at CREST, Dartmouth, ERIM, Harvard, Harvard Business School, HEC School of Management, Kellogg School of Management, MIT, Michigan State University, Oxford, Ross School of Business, UCLA Anderson School of Business, USC Marshall School of Business, and Vanderbilt Law School.

## **Abstract**

This paper uses case information on Chapter 11 filings for 7,824 private companies across 13 courts in the US between 1989 and 2006. We first establish that within district courts cases are assigned randomly between pro-debtor and pro-creditor judges, which allows us to estimate judge specific fixed effects in their Chapter 11 rulings. We find strong and economically significant differences across judges in the propensity to grant or deny specific motions. Specifically some judges rule consistently more favorably towards creditors (or debtors). Based on the judge fixed effects we created an aggregate score to measure the pro-debtor friendliness of judges. We show that pro-debtor judges have worse firm outcomes after Chapter 11: they see lower rates of firm survival and lower sales and employment growth. This result seems to be driven by an increased misalignment of incentives between managers and shareholders in distress and the ability of managers to extract private benefits from the firm.

# 1 Introduction

An efficient bankruptcy process has to strike a delicate balance between the bargaining power that it assigns to creditors and debtors. On the one hand it must uphold the claims of creditors in order to preserve the bonding role of debt and prevent the inefficient continuation of non-viable companies. On the other, the process should allow debtors to prevent inefficient liquidation and asset stripping by creditors when the firm is viable as a going concern. Finding the right balance between these opposing forces in the distress resolution also plays an important role *ex ante* to maintain the incentives of entrepreneurs to start firms and for investors to provide them with capital. In fact, Chapter 11 of the US bankruptcy code has often been praised as one of the cornerstones of effective company restructuring.<sup>1</sup> It aims to provide court protection to distressed firms that are economically viable and puts a stay on their assets in order to avoid inefficient liquidation and maintain firms as going concerns.

A large literature has described this inherent tension in the design of an efficient bankruptcy regime. For example, Gertner and Scharfstein (1991) and Hart (1999) provide a theoretical framework of the incentives of debt and equity claims in Chapter 11 resolutions. Since debt holders do not participate in the upside of the firm, the interest of secured debt holders is to preserve the collateral value of the firms assets rather than allowing the firm to undertake new and risky investments. Moreover, given that debt holders are senior in liquidation, they have a claim on the payoff from any additional investment that is made once the firm is in distress. This can lead to underinvestment and inefficient liquidation, since effectively the old claimholders constitute a tax on any new investor. This is the famous debt overhang argument, as pointed out in the seminal paper of Myers (1977). In contrast, equity holders favor overinvestment and inefficient continuation; due to limited liability they benefit from risk shifting *à la* Jensen and Meckling (1976). This view of firm restructuring would predict

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<sup>1</sup>As cited in Smith and Strömberg (2005), the United Kingdom, Japan, Germany, France and Sweden have all recently instituted ‘more debtor-friendly, U.S. styled reorganization codes into their bankruptcy laws. Indeed, the World Bank, International Monetary Fund (IMF), and the European Union (EU) now encourage member countries to adopt bankruptcy laws that have a reorganization code as one of their cornerstones.’

that a change towards a more debtor friendly regime (which gives more bargaining power to debt holders) should lead to more continuation and investment while a more creditor friendly regime should lead to more liquidation.<sup>2</sup>

In this paper we test whether these basic hypotheses about the incentives of claim holders reflect the reality of how changes in the bargaining power of either creditors or debtors affect the outcome of distress resolutions in Chapter 11. It is usually very difficult to find situations where the bargaining power of either side changes exogenously, since bankruptcy laws apply nationwide and change very infrequently.<sup>3</sup> To circumvent these problems, we exploit the large heterogeneity among US bankruptcy judges in their interpretation of the bankruptcy laws as an instrument for the debtor (creditor) friendliness of the bankruptcy environment that a company faces in Chapter 11. We can estimate these fixed effects in a meaningful way since both the official court procedures and our analysis of the data confirm that cases are randomly assigned to judges within a court. Our estimation strategy follows the work by Kling (2006) and Anderson, Kling and Stith (2001) who first introduced this identification strategy to look at the impact of the length of sentencing decisions on inmate recidivism in criminal cases.

While bankruptcy law is set at the federal level, we show that the interpretation or application of the law varies widely across judges. Among the judges in our sample there are significant differences in the likelihood of granting or denying motions that favor either creditors or debtors. In fact, using principal component analysis we find a strong *systematic* pattern across different judge decisions: judges either tend to rule in favor of, or against, creditors across *all* types of motions. These findings suggest that the particular judge who a firm draws in Chapter 11 is a significant determinant for how the bankruptcy code is applied. Moreover given the systematic nature of the rulings, we can use the specific judge as a proxy for whether the Chapter 11 process is marginally more (less) tilted towards the

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<sup>2</sup>When a firm nears distress, the claims of unsecured debt holders align closely with those of equity holders rather than secured creditors since both depend on the continuation value of the firm to improve in order to get paid.

<sup>3</sup>Any cross sectional tests of the correlation between observed bankruptcy rulings and firm outcomes could be driven by omitted variables; for example more difficult cases might require tougher rulings and at the same time have worse outcomes.

debtor or the creditor. The existence of robust judge fixed effects is consistent with Bris, Welch and Zhu (2006) and their discussion of the existence of ‘behavioral difference among judges.’ Using more detailed case information on a smaller number of cases for the state of Arizona, they find statistically significant differences across judges in terms of the payments awarded to creditors, adherence to APR, and the length of the proceedings.

We use these estimated judge fixed effects to build an index that summarizes the debtor or creditor friendliness of a given judge across a set of important rulings in Chapter 11. Specifically we classify any motions as pro-debtor that aim to prevent asset stripping and encourage follow-on investment to maintain the firm as a going concern. Our classification of pro-debtor and pro-creditor motions is confirmed by the fact that they coincide exactly with the party that files for these motions. For example, creditors are the only ones who ever ask to lift the automatic stay on assets while debtors are the only ones to ask for the use of cash collateral. The index then allows us to characterize situations where either debtors or creditors have more bargaining power and analyze how distress resolutions change under these circumstances.

Interestingly, our findings are exactly opposite to the simple tradeoff laid out by the theories described above. We find that increasing the debtor friendliness of the distress resolution environment leads to an *increase* in firm shut downs and lower sales and employment growth coming out of Chapter 11. We find the opposite results for an increase in the creditor friendliness. We also confirm that these results are not mechanically driven by differential survival rate in Chapter 11. One could worry that pro-creditor judges have a higher hurdle rate for the firms they allow to pass through Chapter 11. As a result the few surviving firms would be of better quality. But to the contrary, we find no difference between the fraction of firms which are shut down during the Chapter 11 process under a pro-creditor judge.

At face value, these findings are very surprising: Bankruptcy workouts, where the creditors have more power relative to those where the debtors have more power, increase the likelihood of continuation of firms and show better performance ex post. On average credi-

tors rather than debtors seem to be pushing for restructuring solutions in Chapter 11 that allow for more successful continuation of the firm.

We show that these apparently counter-intuitive results seem to be driven by agency problems that arise in Chapter 11. Agency problems can occur either between management and shareholders (or unsecured creditors), if management focuses on private rent extraction and self-dealing once a firm enters financial distress rather than maximizing its long term value.<sup>4</sup> Alternatively, there can be an agency conflict between owner-managers and the rest of the stakeholders in the firm. If equity has the de facto control rights in the firm, these equity holders would have an incentive to extract financial resources from the distressed firm instead of keeping assets tied up and thus having to share future income with the other claim holders of the firm. A judge who is more creditor-friendly might thus allow owner-managers to circumvent creditors and pay themselves in bankruptcy. If this extraction technology is not too costly, maintaining the firm as a going concern might not be the most attractive option for managers. In contrast, (unsecured) creditors cannot engage in this type of behavior since they do not have de facto control rights in the firm. As such they might be more interested in preserving the firm as a going concern.

To investigate the specific nature of the agency problem driving our results, we break out the sample by type of ownership structures. If misalignment of incentives between management and dispersed shareholders is the primary determinant we should see a disproportionately strong effect among public firms. Indeed we do find that public firms have differentially worse outcomes if they are matched with a pro-debtor judge in Chapter 11 (lower sales and employment compared to private firms). This points to the fact that pro-debtor judges might make it easier for managers in public firms to engage in self-dealing, by shifting power towards the debtor.

But when looking at private firms, we find a more complex relationship between ownership and the magnitude of the judge effects. Specifically while the effect of pro-debtor

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<sup>4</sup>For example, Weiss and Wruck (1993) argue in the context of the Eastern Airlines bankruptcy that a debtor friendly bankruptcy judge allowed for excessive asset stripping and delayed the shutdown of the firm. Similarly Betker (1995) provides evidence that management uses its ability to draw out the bankruptcy process as leverage in negotiations with creditors, at times to the detriment of equity.

judges on incorporated firms is very similar to that in the full sample, pro-debtor judges have the largest negative effect on partnerships and sole proprietorship show no significant difference in outcomes. One can imagine that for these small, private firms the agency issues between managers and equity holders are less severe, since the owners usually hold very concentrated stakes in the firm. So even if these parties gain more power in Chapter 11 (when being assigned to a pro-debtor judge), they do not distort the continuation value of the firm. However, in a partnership structure several people de facto hold the residual control rights in the firm. As a result a pro-debtor judge enables the partners to tunnel assets or cash out of the partnership for private benefits rather than work to maintain the firm as a going concern. Overall, these results support the idea that the negative impact of pro-debtor judges stem from providing the party which has de facto control rights with an opportunity to extract private rents from the organization at the expense of its continuation value.

Importantly, while our paper presents interesting new evidence on the importance of the interaction between the debtor (or creditor) friendliness of the bankruptcy system and governance issues, it should be noted that our results cannot speak to the *ex ante* incentive effects of a more debtor friendly system.

The rest of the paper is structured as follows: Section 2 provides a literature review. Section 3 lays out the identification strategy. Section 4 explains the data sources and collection process. Section 5 and 6 verify the random assignment of judges to cases and the heterogeneity in judge decisions. Section 7 describes the construction of the pro-debtor (creditor) index and its impact on firm outcomes. And finally section 8 concludes.

## **2 Literature Review**

Our paper contributes to the debate about distress resolution and Chapter 11 in a number of ways. First we provide a novel instrument to estimate the causal impact of greater debtor (creditor) friendliness on the outcomes of the workout process. While there is still a lot of debate about the benefits of more (less) debtor friendly systems, the evidence is

currently based largely on cross sectional comparisons of firms that go through distress. Our approach allows us to build on the earlier literature but go beyond descriptive analysis of the bankruptcy process.

One of the first papers is Hotchkiss (1995) that looks at the performance of 197 public companies post Chapter 11 and finds that a large fraction of firms which go through Chapter 11 either go bankrupt or land back in Chapter 11. Additionally they find that firm performance in the three years following bankruptcy was worse if management was not replaced. For a similar conclusion see Gilson (1993). On the other hand Baird and Rasmussen (2002) and Skeel (2003) argue that contractual developments have allowed creditors to “neutralize” inefficiencies due to the pro-debtor nature of Chapter 11, and that creditors have managed to undo any pro-debtor biases through private contracting.<sup>5</sup>

The role of agency issues between equity holders and managers in Chapter 11 has been widely discussed in the law literature. For example, Bradley and Rosenzweig (1992) suggest that bankruptcy law fails to provide managers with appropriate incentives to allocate corporate resources to their highest-valued uses, but instead rewards managers for taking inefficiently high risks. Betker (1995) argues that management has the real control in Chapter 11 and their interests are not always aligned with their shareholders. He provides some empirical evidence to support his idea by looking at cases where CEO pay and shareholder wealth were negatively related and show that management incentives correlate with violation of absolute priority. Eckbo and Thorburn (2003) show that reputation effects temper CEO taste for excessive risk taking in Swedish automatic bankruptcy auctions. Franks and Torous (1994) compare creditor recovery rates during distressed exchanges and Chapter 11 reorganizations. They find creditors have lower recovery rates in Chapter 11, and argue

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<sup>5</sup>Skeel (2003) specifically identifies the use of debtor-in-possession financing agreements and the pay-to-stay arrangements, which give key managers bonuses for meeting specified performance goals, as the two key contractual developments since the 1980s. Kahl (2002) argues that the poor performance of firms post debt-restructuring is not due to coordination problems among creditors, or an inefficient bankruptcy process, but instead are simply the result of the dynamic learning strategies of creditors. Internationally, Thorburn (2000) finds that Sweden’s more creditor friendly auction based bankruptcy system leads fewer deviations from absolute priority, lower cost, and are resolved faster than the U.S. Chapter 11 cases. In contrast Ravid and Sundgren (1998) examine the relative efficiency of the creditor-oriented Finnish bankruptcy code to the US system and finds that U.S. reorganizations are more efficient.

that there is a high cost (to creditors) to formal reorganization.

On the theory side a large literature has focused on the efficiency effects based on the composition of a firms creditors. For example, Berglof and von Thadden (1994), Dewatripoint and Tirole (1994) and Bolton and Scharfstein (1996) show that in a world of incomplete contracts, the bonding effect of having multiple creditors (or investors) can lead to increased ex ante efficiency at the cost of ex post efficiency. Ayotte (2007) goes a step beyond and explicitly models incentives for different claim holders of the firm. His work suggests that when the value of the firm as a going concern is dependent on the effort of an owner-manager, ex-post efficiency is best served by a fresh start for the indebted entrepreneur.

Most closely related to our paper is a small but growing literature on the role of individual bankruptcy judges and their influence over bankruptcy cases. Bris, Welch and Zhu (2005) find significant behavioral differences across judges in terms of the fraction paid out to creditors, adherence to absolute priority, and case length. LoPucki and Whitford (1993) state that with the level of discretion afforded bankruptcy judges, “a skilled, aggressive bankruptcy judge who wished to do so could wield virtually complete power over the governance of a reorganizing company.” Based on interviews and independent analysis the authors conclude that for more than a quarter of their cases the judge chose to play a major role in the case. Similarly, Aghion, Hart and Moore (1992) realize the importance of judges in the distress resolution process and point to the potential agency problems that might arise from it.

### **3 The Chapter 11 Process**

Chapter 11 bankruptcy protection is an attempt by the bankruptcy code to allow firms which are in financial distress, but otherwise are viable as going concerns, to restructure their financials and their physical assets in order to keep operating. The aim of Chapter 11 is to prevent creditors from stripping the assets of the firm in cases where it is socially optimal to allow the firm to continue (i.e. if the value of the firm as a going concern is

higher than its liquidation value). Many practitioners therefore describe Chapter 11 as a fresh start for the firm. To avoid a run on the assets, Chapter 11 places an automatic stay on the assets of the firm, but the bankruptcy judge had the power to lift the stay. Moreover if the judge assigned to a case does not feel the firm meets this requirement, then the case is summarily converted to a Chapter 7 liquidation bankruptcy or dismissed altogether. Conversions and dismissals of this type are quite common, and many bankruptcy cases never reach the hearing stage. So the bankruptcy judge invariably has a lot of power within the process.

Chapter 11 formally begins with the filing of a petition in district bankruptcy court. Although the debtor almost always files such petitions, creditors meeting certain requirements may force a firm to file for involuntary bankruptcy. In addition an increasing number of cases are filed ‘pre-packaged’. Essentially prepackaged cases are ones where the debtor files a petition with a reorganization plan that has already been negotiated by the firm, its creditors, and if relevant, voted on by stockholders. For the purpose of this paper we will not include pre-packaged bankruptcy cases, since those cases do not allow for any action on the part of the judge. In addition the only state where pre-packed cases are of importance is Delaware. We confirm that our results hold even if we exclude Delaware from our sample entirely.

After filing, Chapter 11 consists of three main parts. The first step is the presentation of a plan of reorganization. Under Chapter 11 U.S. Code Section 1121, prior to 2005 the debtor in possession had a 120 filing day exclusivity period during which they have the sole right to file a plan of reorganization. If the debtor has failed to file a plan during the first 120 filing days, the debtor can request an extension of the exclusive period. If the request for an extension is denied, other parties may file their own plan for firm reorganization. However, in many cases judges would grant the request for extension, which could often delay the resolution. To prevent these delays the Chapter 11 process was amended in 2005 so that debtors get a 180 exclusivity period but no possibility to ask for an extension. We stop our data collection prior to 2005 in order not to be affected by these institutional changes.

After a plan has been filed, creditors and equity holders vote to confirm the plan. A plan is considered confirmed when a majority consensus is reached as measured both in the number of creditors and fraction of the total debt owed. Once a plan has been confirmed, the process of reorganization begins with the implementation of the now approved plan of reorganization. After the plan has been implemented to the court's satisfaction, the case is officially closed and the bankruptcy process is considered complete. Although on average this process takes less than two years, it can take much longer depending on the complexity of each particular case. Successful reorganization should allow a firm to operate as a financially viable entity so both firm dissolution and subsequent re-filing for bankruptcy are generally seen as a failure of the bankruptcy process.

## 4 Data Description

The data for this paper stem from three main sources: (1) The Public Access to Court Electronic Records (PACER) electronic public access service, (2) Dun & Bradstreet and (3) the National Establishment Time-Series (NETS) Database. First, we obtain full case histories including the case docket from PACER. PACER is an internet based data service which provides registered users with access to case and docket information for bankruptcy cases.<sup>6</sup> Although the PACER service is run federally by the Administrative Office of the United States Courts, each district is responsible for maintaining its own separate website and database of case information. As such the content and quality of information provided varies by district and also over time. Most districts start filing systematically only in the second half of the 1990s.

Our PACER data universe consists of all Chapter 11 bankruptcies available between 1989 and 2003 from the PACER system for the districts of Arizona, California (Central, Eastern, Northern, Southern), Delaware, Georgia (Northern) and New Jersey. We chose these five states since they were the earliest ones to adopt the PACER system and have the cleanest data within the system. The dataset includes approximately 11,000 cases filed

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<sup>6</sup>We thank the participating district courts for their generosity in waiving the fees for accessing their PACER entries.

and completed for private and public companies. After dropping pre-packaged cases, Dun & Bradstreet were able to match 7,824 cases in their data universe.<sup>7</sup>

The PACER system allows authorized users to download the complete docket information for cases filed in Bankruptcy court. The case docket is a line-by-line account of all the motions that were filed by claimants in a case and the judges responses to these. To deal with the enormous size of the sample, we develop a computer algorithm to parse through the docket information and code individual motions and judicial decisions. We concentrate on docket items that from our reading of the legal literature on the Chapter 11 process are considered most important. We confirmed the algorithm with a number of legal scholars. We are in especially grateful to Lynn LoPucki and Douglas Baird for their insights.<sup>8</sup>

We construct a large number of indicator variables, which have a value of 1 whenever a judge approves a specific type of ruling or an objection is filed in a given case. The most important decisions taken by bankruptcy judges are orders to (1) dismiss a case or convert it from Chapter 11 to Chapter 7<sup>9</sup>, (2) grant an extension of the exclusivity period, (3) grant a lift of the automatic stay, (4) allow the debtor to use cash collateral and (5) allow the sale of assets.

Table 6 shows that cash collateral requests are granted in only around 1% of cases and denied in 10% of them; conversion to chapter 7 or case dismissal occurs in 3% of cases and denied in 19% of them; lifting of the automatic say is granted in 3.5% of case and denied in 25% of them; and finally both the granting and denying of a sale of assets occur in less than 1% of cases.

For each case in our sample, we then re-examined the entire docket coding by hand to verify that the extract filings correspond to the actual rulings. We were especially careful to check that the algorithm did not suffer from type II errors and excluded valid motions. We include Chapter 11 filings of private as well as public companies, with the majority being

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<sup>7</sup>Dun & Bradstreet were able to match over 8,000 cases to firms, but we chose to drop those matches Dun & Bradstreet did not designate as high confidence matches.

<sup>8</sup>See for example Baird (2002).

<sup>9</sup>The text of the dockets did not generally let us distinguish between dismissals and conversions so we have combined them into one category.

private firms. The frequency of the filing events by year and district are presented in Table 3.

The data from Dun & Bradstreet (D&B) provides some characteristics for the firms in our bankruptcy sample. The D&B data contains information on the sales, number of employees and some financial information for nearly 100 million firms. The benefit of D&B is that it also includes information on *private firms* if these firms ever had a credit record. This will almost by definition be the case for the majority of firms that land in Chapter 11. By using a combination of firm name, address and Tax ID numbers, the cases were matched to firm financial records by Dun & Bradstreet. Fourth quarter credit ratings were available for 1317 of these firms for the years 1997-2003.

Firms in the D&B sample have yearly sales between 0 dollars to over \$500 million, with mean and median values of \$1.1 million \$0.16 million respectively. The mean and median number of employee's are 96.1 and 2 respectively, and range from 0 to 39,400 employees. The vast majority of case filings (97%) are voluntary. We also obtain information on the re-filing rate (and date) of cases that had previously gone through Chapter 11 if they happen within our sample period. About 2% of the cases in our sample land in bankruptcy court again, and the firms that re-file take on average approximately 1.1 years before re-entering bankruptcy.

We use a second data set called National Establishment Time Series (NETS) that is a panel data of the largest firms within PACER it covers approximately one third of the D&B and focuses on the larger firms. In addition because NETS collects data at the establishment and not firm level, we have limited this data to firms that have only one location (i.e. establishment). NETS was able to match 2027 firms of our original PACER universe. As we can see from Panel B of Table 2 the average firm in the NETS sub-sample tends to be larger than those in the entire sample as expected. NETS matched firms have mean sales of \$2.8 million, ranging from \$16,700 to \$23 million. In terms of employees, NETS firms have both lower means and variance, but higher median values. In comparing the two data sets we find that the main observable differences between the D&B universe

and the NETS data is that NETS data have on average significantly higher median sales and employment, but less variance and extreme values, all of which seem to be driven by the fact that the NETS data have fewer firms in the tails of the distribution (i.e. the NETS sample does not include many of the smaller firms in the D&B dataset or the largest, multi-location firms).

## 5 Identification Strategy

In this section we first lay out the logic of the identification strategy and then present the details of the estimation. Our identification strategy relies crucially on the assumption that cases are randomly assigned to judges in Chapter 11 within a district court. If we can confirm random assignment, we can then use the estimated judge specific fixed effects to identify more creditor (or debtor) friendly judges. These are thus valid estimates of a judges bias and are not driven by selection of specific cases to judges. It is important to note that we are not imposing random assignments *across* different bankruptcy courts, but just between judges *within* a given district court. This assumption is in line with the provisions of the law. While a large literature has shown that there appears to be significant forum shopping across jurisdictions, judge shopping is widely condemned and actively discouraged.<sup>10</sup> In addition, several of the clerks of the bankruptcy courts within our sample confirmed that they use a random assignment rule to allocate cases to judges.<sup>11</sup>

We also present empirical evidence to corroborate this assumption of random assignment. To that end we look for differences in the sales, number of employees, probability of being a public firm, probability of being incorporated, and credit rating of firms the year prior to filing for Chapter 11 and find no difference in either the average characteristics

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<sup>10</sup>See for example Eisenberg and LoPucki (1999).

<sup>11</sup>An exception to this method occurs when two or more cases are related. Although the assignment of related cases is not random, as long as the initial case is randomly assigned, effective randomization should still occur. New Jersey explicitly states their rule for case assignment in D.N.J. LBR 1073-1(d): ‘If the petition commencing a case states in writing that the case is related to another case which has been or is being filed in the same vicinage, the clerk shall assign the case to the judge to whom the lowest numbered related case has been assigned. All other case assignments shall be made by the random draw method used by the Court.’

of firms assigned to a judge, nor across pro-debtor and pro-creditor judges within a given district court.

Once we verify that cases are randomly assigned to judges, we repeat a similar set of regressions for the different rulings of judges in Chapter 11 (e.g. granting or denying the lift of an automatic stay, the use of cash collateral, the dismissal of the case). One by one we regress the judges decisions on a set of judge fixed effects and district times year effects. In sharp contrast to firm characteristics, we find significant differences in judicial rulings. In particular we find strong differences on some of the more prominent decisions such as granting or denying the lifting of an automatic stay or allowing the use of cash collateral. The results do not change if we include firm level controls for size, number of employees or industry. These findings are quite remarkable since they suggest that judges indeed differ systematically in their likelihood to rule in favor of creditors or debtors in Chapter 11. We can then use the estimated fixed effects to form an index of whether a judge is more strongly ruling in favor of debtors or creditors as will be discussed below.

## 5.1 Confirming Random Assignment

We first confirm that judges are randomly assigned to cases. As discussed above our identification strategy relies centrally on the assumption that in Chapter 11 cases are randomly matched to judges. If this assumption was violated, judge specific effects could not be meaningfully estimated. Instead the observed ruling of a judge might reflect the demands of the case and not the judge's judicial philosophy or biases. For example, imagine a judge who is specialized in difficult cases and another judge in simpler bankruptcy cases. Then the judge with the more difficult cases might appear to grant more extensions of automatic stay which could be misinterpreted as having a pro-debtor bias. But in fact, the ruling of the judge would be driven by the characteristics of the case, e.g. difficult cases more often require the extension of the automatic stay, since the parties in the case cannot agree on a compromise.

By law a firm is only allowed to file for bankruptcy in districts in which it operate. Since most small firms have operations in only one district, they do not have a choice which

district to file in. This is different for large firms that have operations in several states and district and thus might be able to engage in so called forum shopping. Therefore throughout the paper we make sure all our results hold when we exclude the top decile (or quartile) of firms by sales.<sup>12</sup>

One might be worried, however, that even if the courts use random assignment to decide which judge presides over a given case, the timing of filing of cases is not. For example, knowledgeable bankruptcy lawyers might know when a given judge has a light case load and thus file at a strategic time to obtain a higher chance to be assigned to this judge. For that purpose we test the assumption of random assignment more directly. We run a regression of different characteristics of the firms that end up in Chapter 11 on the set of judge fixed effects. The observable characteristics that are available to us are the annual sales of the firm, the number of employees and the credit rating for the year prior to which the firm files for bankruptcy. We also need to control for an interaction of district and year fixed effects. This control is important if the case load and composition of cases changes over time within a district. Therefore a control for the changes in the composition of cases that come to the court in a given period is required.

Specifically for each type of filing, we estimate the following regression:

$$y_i = \alpha_{dt} + \gamma_i + \beta X_{it} + \lambda_{judge} + \epsilon_i, \quad (1)$$

where  $y_i$  is a dummy for whether a particular order was issued,  $\alpha_{dt}$  are court varying year fixed effects,  $\gamma_i$  is a dummy for whether the case was filed voluntarily,  $X_{it}$  represents a vector of district varying firm level controls and  $\epsilon_i$  is an error term. The remaining term  $\lambda_{judge}$  is the fixed effect for the individual judges. Because each judge serves in only one district,<sup>13</sup> district effects are not included as they would be perfectly collinear with judge fixed effects.

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<sup>12</sup>Excluding the top decile (or quartile) of firms by sales has no significant effect on any of our findings. This should not be seen as evidence either for or against the existence of forum shopping. Rather it merely suggests that forum shopping by the multi-State firms simply does not lead to a significant change in the overall composition of bankruptcies filed in any given district.

<sup>13</sup>One judge did have a small fraction of her cases in a second district. Those cases were dropped from our sample.

Therefore, whenever we need to directly compare individual fixed effects across our different districts, the fixed effect coefficients are demeaned at the district level.

Table 4 show the results from using F-tests on the set of judge fixed effects with pre-bankruptcy firm sales and employment levels, dummies for whether the firm is public or incorporated, and credit rating using the specification described above. Columns 1 thru 4 incorporate an increasing number of controls, with column 4 corresponding to the controls used later in the regressions on firm outcome. Column 1, which contain no controls, finds jointly significant judge fixed effects for pre-bankruptcy firm characteristics, confirming a compositional effect across different district courts. In column 2, where we control for district trends, we see that the F-statistic is small and rejects the hypothesis that the judge fixed effects are jointly significant. In the next two columns we also include industry and voluntary filing dummies and we see these specification also fail the F-test. As seen in Table 4, there appears to be no evidence that judge fixed effects jointly explain average sales. We then repeat these tests for the number of employees in the second row of the table. Again we find that the F-test on the joint significance of the judge fixed effects is not significant. These results also hold true if we estimate judge fixed effects for each individual district court separately. In regressions not shown, we also re-estimate the judge fixed effects for different subsets of the data, windsorizing either at the 5%, 10% or 20% hurdle and get quantitatively similar results.

As a final test in Table 5 we verify that cases are randomly assigned between pro-debtor and pro-creditor judges, since this is the major dimension by which we will differentiate cases. Each cell in the table represents a regression of the dependent variable reported on the left hand side on our measure of pro-debtor tendencies (as explained in section 5.3), controlling for district trends, industry fixed effects and whether the filing was voluntary. The standard errors in these regressions are clustered at the judge level. We use the same set of dependent variables as before from D&B and NETS. As is apparent from the table, none of the coefficients are significant.

Overall these results suggest that random assignment of Chapter 11 cases to judges

*within* a district court holds in our sample, and thus case allocation is independent of the firms' observable characteristics, such as firm size, measured as sales and employees at the time of filing. Given the stated allocation rules of bankruptcy courts, these findings reassure us that firms are not matched to judges in some systematically biased way. This is a very powerful result since it will allow us to estimate judge fixed effects on the decisions they take and interpret them as a reflection of the judges specific leanings or biases and not an outcome of the type of cases that are allocated to the judge.

## 5.2 Judge Specific Differences

After verifying that cases are randomly assigned to judges, we next estimate whether there are judge specific fixed effects in the way judges rule in Chapter 11. To test whether judges vary systematically in their approach to Chapter 11 rulings, we repeat an estimation strategy parallel to the one used above, but use as our dependent variables the different motions a judge approves or dismisses during the Chapter 11 process, as well as the filing of objections by the debtor or creditors and sua sponte actions by the judge. For example, we regress an indicator for whether a judge grants a creditor's motion for relief from the automatic stay on the set of judge dummies and the interaction of year and district fixed effects. We then conduct an F-test for whether the judge fixed effects are jointly significant. We repeat this estimation procedure separately for all docket actions.

While we have shown that cases seem to be randomly assigned to judges in Chapter 11, the different parties in bankruptcy might know the reputation of the judge to which they were assigned. Given their expectation of succeeding with a specific motion, the different parties in bankruptcy might endogenously choose whether or not to file a motion in the first place. Take the case of a very strict judge who is known to only very rarely allow an extension of exclusivity or to lift an automatic stay. In that case the lawyers of the different parties in bankruptcy might not even try to file a certain motion, since they expect rejection and vice versa in the case of a judge who is considered more permissive. Therefore, a permissive judge might not only have a higher number of approvals, but even a higher number of denials, since he will see more marginal requests than a judge with a

less permissive reputation. However, because of this endogeneity in the number of filings we cannot use the *fraction* of approved motions or the *number of denied* motions as an indicator for judge bias. In addition, because of the nature of the docket filings themselves, it is not always clear whether a particular docket entry refers to a distinct ruling so the approval or denial of a single motion can correspond to multiple docket entries. Therefore, only the *number* of approved motions is a well-defined indicator of judicial tendencies, and we will use this in all specifications.

The results from this exercise are markedly different from the results in the previous section. In fact, almost all the F-test on the different judge decisions are jointly significant. Table 6 shows that F-tests on most of the variables are large and significant. Specifically we find large and very significant F-tests for the granting and denying of motions requesting the use of cash collateral, granting and denying motions to dismiss or convert the case to Chapter 7, and granting and denying motions for relief from the automatic stay. According to the bankruptcy literature these are very important decisions in Chapter 11, see for example Gertner and Scharfstein (1991). In contrast, we find that the F-tests tend not to be significant for those rulings that are rarely utilized, such as granting a sale of assets and the granting of an extension to the exclusivity period. Both of these rulings occur in less than 0.5% of the bankruptcy cases, which suggests that the incidence rate of these motions is too low to find significant results. The results hold if we include firm level controls in the regression such as logarithm of sale and the number of employees. This is not surprising since we had previously shown that judges are randomly assigned to cases.

Overall, these results suggest that there is large and significant heterogeneity in the propensity of judges to grant a given motion. This finding demonstrates that depending on which judge is assigned to a given case, the rulings on the case vary dramatically. In regressions not shown, we estimated fixed effects for the individual judges and found that in the cases where the F-test is highly significant, even the t-tests on the individual fixed effects are generally significant for most of the judges. So a judge who has a particularly low estimated fixed effect has a lower than average propensity to grant a particular motion. And

a judge who is at the higher end of the fixed effect distribution has also a higher propensity to grant the motion. Put simply, judges appear to play a central role in how bankruptcy law is applied to an individual case.

### 5.3 Pro-debtor and pro-creditor index

We now want to understand whether there are consistent patterns in the rulings of judges *across* all the different petitions. Judges can use the rulings analyzed above in different combinations or rely on only a subset of rulings in order to provide more leeway to either debtors or creditors. So in order to classify a judge as pro-debtor or pro-creditor we need to understand if there are consistent patterns of how judges apply these motions or if each judge has a personal style of which motions they use to bring about say a pro-debtor outcome. For example, does a judge who has a strong positive fixed effect on granting extensions of the exclusivity period also displays pro-debtor tendencies on other motions, e.g. allows the use of cash collateral or never lifts the automatic stay. Or does each judge pick his or her favorite motion.

To test the correlation structure between different judge fixed effects we conduct a principal component analysis. As discussed above, we only include fixed effects on the petitions that are granted by a judge but not those that were denied. We include the most important rulings in the Chapter 11 process that can be clearly characterized as pro-debtor or pro-creditor. Under the rather reasonable assumption that no party would request something harmful to itself, we classify motions that are requested by creditors as pro-creditor and those submitted by debtors as pro-debtor. These are seven types of motions that are filed exclusively by either the debtor or creditors. The debtor filed motions are request for (D1) the use of cash collateral, (D2) extensions to the exclusivity period, (D3) the sale of assets, and (D4) objections by the debtor to the reorganization plan. The creditor filed motions are requests for (C1) case conversion to Chapter 7 or dismissal, (C2) lifting the automatic stay, and (C3) objections by creditors to the reorganization plan.

In Table 7 we use the estimated fixed effects for each judge across these dimensions and find a very consistent structure in our principal component analysis. The first principal

component is by far the most important one and explains about 40% of the variation across judge fixed effects, while the next principal component explains less than half of the variation that the first PC does. This first component loads positively on the motions that are pro-debtor (D1 to D4) and negatively on the motions that are pro-creditor (C1 to C3). These findings suggest that judges are very consistent in how they rule on motions, i.e. across the different motions a given judge shows either a systematic bias towards debtors or the creditors.

The findings also suggest that we can combine the estimated judge fixed effects across different motions to create one proxy for each judge that captures the judges overall tendency to rule in favor of either the debtor or creditor. For that purpose we use the first principal component to classify pro-debtor and pro-creditor judges. We construct a proxy for the pro-debtor bias of a judge by using the factors weights that come out of our principal component analysis to form a weight the estimated fixed effects for each judge across all their decisions. This pro-debtor measure is then normalized to have a mean of zero with a standard deviation of one, so the coefficient for this variable can be interpreted as the effect of one standard deviation increase in the pro-debtor tendency of a judge.

We also replicate all the results using a pro-debtor index that equal weights all the different judge fixed effects - we create a dummy variable equal one if a judge fixed effects is above the mean on a specific dimension and zero otherwise and then sum up these dummies across all the different motions a judge can undertake. The results using this equal weighted measure (not reported) are qualitatively similar to the results obtained from using the weights of the first principal component.

## **6 The Effect of Pro-Debtor or Pro-Creditor Judges**

In Table 8 we report the results of regressions of different outcome variables for the firms that go through the Chapter 11 process on the pro-debtor/creditor index controlling for district and year effects. We first want to understand whether the firms that are assigned to pro-debtor judges have a different experience during the Chapter 11 process compared

to those that meet a pro-creditor judge.

In the first row we show the results from a regression where the dependent variable is a dummy for whether the firm still in operation two years after filing for bankruptcy on the pro-debtor dummy controlling for industry fixed effects, district trends, whether the filing was voluntary and pre-bankruptcy sales. Standard errors are clustered at the level of the judge, and the sample limited to firms for which we have data for two years after filing for Chapter 11. Since on average firms bankruptcies take between 1 and 2 years, this is a good indicator for whether a given judge is more likely to shut down a firm in bankruptcy. Understanding this baseline is important, since we want to ensure that any differences in outcomes between pro-debtor and pro-creditor judges which we might find, is not driven by a differential propensity to shut down firms in Chapter 11. So if pro-debtor judges were less likely to shut down firms, since they are lenient, it would not be surprising if these judges also had higher failure rates going forward. However, the first row of column 1 shows that there is no significant difference in the likelihood of being shut down between pro-debtor and pro-creditor judges.

In the second row of this table we then replicate the regression using the duration in log days of the case as the dependent variable. One might worry that pro-debtor judges put more or less effort or time into finding a lasting solution, which could then lead to a shorter process and a higher likelihood of re-filing. However, again we do not find a significant effect for judges with an estimated coefficient close to zero.

The dependent variable in the third row is the firm's likelihood of re-filing for Chapter 11 within the first three years after the initial bankruptcy filing. As before we see that coefficient on the pro-debtor judge dummy is close to zero and not significant (about 0.2%). This result suggests that there is no significant difference in re-filing rates, but since the baseline incidence level of re-filing is very small to start with (about 2%), the result could simply be a reflection of the fact that we do not have sufficient power to identify any potential effect.

We now turn to more comprehensive measures of firm outcomes post Chapter 11 filing.

In Table 9 we look at firm shut down for up to 5 years post Chapter 11 filing. Specifically we code a dummy equal 1 if a firm is shut down in the year 2,3,4 or 5 after filing for Chapter 11. Firm shut down is much more common than re-filings, since many firms do not have enough assets to warrant a court procedure and just close their doors. The first two panel of the table shows the estimated coefficient of a probit and OLS regressions where the left hand variable is one if the firm survives to the given year on the pro-creditor indicator variable, while the third panel shows the base survival rate. As before all regressions control for district trends, industry F.E., voluntary filing, pre-bankruptcy sales and employment with standard errors clustered on the judge level. Here we see that there is a significantly lower likelihood that a firm survives between year 4 and 5 after coming out of Chapter 11, if it was assigned to a pro-debtor judge. The effect is economically very strong, and indicates that a one standard deviation increase in the pro-debtorship of a judge increases the shutdown rate by almost 10%. For years 2 and 3 the estimated coefficient on the pro-debtor dummy is negative as well, but smaller and not significant at conventional levels.

In Table 10, we repeat the previous analysis, but with post-bankruptcy sales and employment growth up through the 5th year after filing for Chapter 11 as the dependent variables. Panel A, Row 1 reports the coefficient on the pro-debtor measure where the outcomes is sales growth and firms that are no longer in operation have their sales set to zero. Each cell in the table again is based on a separate regression for sales in years 0 to 5. Based on this specification pro-debtor judges have a negative impact on firm performance post-bankruptcy. This effect becomes stronger and more significant for later years. We then repeat the same regression using sales growth conditional on firm survival (so firms that are no longer in operation are dropped from the sample). Here we find that the estimated coefficient on the pro-debtor dummy is now positive and become more significant and larger in magnitude starting 3 years after filing for Chapter 11. In Panel B of 10 we repeat these regressions using employment growth as the dependent variable. The pattern is parallel to the results on sales: In Row 1 we code firms that shut down as having zero employees. We see that these firms have reduced employee growth and the coefficients are significant at the

10% and 5% level 4 and 5 years after filing for bankruptcy. And again when we run these regressions conditional on firm survival, we see a positive effect for the pro-debtor judges in year 4 and 5.

Finally in regressions not shown, the above analysis was run on changes in credit scores post-bankruptcy. For credit scores, while the point estimates followed the same pattern as sales and employees, they were not generally significant at conventional levels.

The combination of these results suggests that the negative impact of the pro-debtor judges is especially on the extensive margin: it is not the case that all firms across the distribution have worse sales growth, but there is a much larger fraction that does not survive. One might conjecture that pro-debtor judges simply do not prepare the cases they preside over very well, and thus have worse survival rates of the firms after coming out of Chapter 11 compared to pro-creditor judges. Alternatively these pro-debtor judges might set up the firm during the Chapter 11 process in such a way to allow the debt holders to increase the risk taking of the firm relative to the outcome with a pro-creditor judge.

These outcomes are very surprising. Based on the simple tradeoff between debt overhang and risk shifting, one might have expected that pro-debtor judges would have a tendency to maintain firms as going concerns. In contrast judges who we classify as pro-creditor allow many more motions that are aimed at protecting the assets of the creditors such as lifting the automatic stay, and thus should lead to less continuation (even efficient continuations). However, we actually find that pro-debtor judges, those that supposedly protect the assets of the firm, produce higher shut down rates (but not necessarily higher refilling rates) than pro-creditor judges. On average the firms in the pro-debtor sample also have lower sales growth and employment growth. These latter two results are driven by the extensive margin. It appears that pro-creditor judges are more beneficial for the medium term continuation value of the firm *conditional on survival* than pro-debtor judges. Importantly this outcome is not driven by the fact that pro-creditor judges have a higher hurdle rate in whom they let through Chapter 11.

## 6.1 Differential Effects Based on the Ownership Structure of the Firm

How can we explain these counter intuitive results? One possible interpretation is that debtors are less aligned with the long run continuation value of the firm once it reaches distress. This misalignment of incentives could result from two different sources. On one hand managers who have the de facto control rights in the firm might engage in personal value maximizing behavior at the expense of shareholders, and therefore jeopardize the continuation of the firm. For example they might extract personal benefits or pay themselves high wages if they know that there is a chance that the firm will not survive for long. In contrast debt holders (especially unsecured debt holders) do not have these de facto control rights, and thus may have incentives more aligned with the long run survival of the firm. On the other hand, rent extraction out of distressed firms could be driven by the owners themselves. Owner-managers might be able to extract assets or profits from the firm in bankruptcy and thus avoid sharing any future cash flows with the creditors. This is similar to the idea of tunneling in pyramidal firms that has received a lot of recent attention in the development literature.<sup>14</sup>

To analyze which of these channels are more predominant we focus on whether ex ante differences in the governance structure of firms translates into a differential impact on the restructuring process in Chapter 11. If misalignments of incentives are more important in explaining the pro-debtor effect, we would expect that the results are stronger for firms that have more separation between managers and owners. In contrast if the tunneling story was more prominent, we would expect to find a stronger effect when there exists less separation between managers and owners. While we would ideally like to obtain detailed information about the exact ownership structure of the firms, in our data we are limited to somewhat crude proxies for the governance structure of the firms. Specifically we know whether a firm is a sole proprietorship, a partnership, a non-profit or incorporated. The bulk of the firms in our sample are incorporated, about 79%. About 12.5% of the firms are sole proprietorships, 7% are partnerships, and less than 2% are non-profits. In addition we have data as to

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<sup>14</sup>See for example Djankov et al (2008).

whether the firm is a public or private. Since only 3.25% firms in our sample are public, this variation is based on a relatively small set of firms.

If our results are mainly driven by the misalignment of the incentives of managers and owners in Chapter 11, we would expect that pro-debtor judges have a smaller in magnitude effect on sole proprietorships compared to partnerships or incorporated firms. And we would expect the largest effects for public firms compared to privately held firms, since these groups of firms are likely to have the largest separation between management and control. In contrast if the results are due to the fact that pro-debtor judges might make it easier for equity to divert resources from the bankruptcy estate, then the effect should be larger for owner-operated firms, especially sole proprietorships.

### **6.1.1 Organizational Structure**

In Table 11 we look at firm continuation after Chapter 11 using the regressions structure from Table 9, but we include separate interaction terms between the pro-debtor index and dummies for firm structure. Specifically we include interaction terms for whether the firm is a sole proprietorship, partnership or a non-profit. The omitted category is incorporated firms.

Table 11 shows that the negative effect of a pro-debtor judge on the firms likelihood of continuation is particularly prevalent in partnerships: There is a large negative coefficient on the interaction term between the pro-debtor and the partnership dummies. This effect declines in years 3-5, and the coefficients are significant at the 1% level in all years except for the final year. This suggests that pro-debtor judges have a large initial impact on firm closures for partnerships, with the effect declining over time. We also see a positive coefficient on the interaction between pro-debtor and sole proprietorships. But this coefficient is not statistically significant. While the magnitude of this coefficient is similar to the coefficient on the direct effect (but of the opposite sign), the net effect of pro-debtor judges on sole proprietorships is indistinguishable from zero. These results suggests that pro-debtor judges have a larger (negative) effect on firms that are partnerships, and a smaller effect on sole proprietorships compared to incorporated firms.

In Table 12, we repeat the analysis presented in Table 10 with the addition of dummies for whether the firm is a sole proprietorship, partnership or a non-profit, and interaction terms between those dummies and our pro-debtor variable. Incorporated firms are again the omitted category. As before, in Panel A we code firm sales as zero if the firm is no longer in operation, while in Panel B we drop closed firms from the sample. The results here parallel those from ownership and continuation: the negative effect of a pro-debtor judge on continuation seemed most pronounced for partnerships, and more muted for sole proprietorships compared to incorporated firms.

Table 13 repeats this analysis but with log employees as the dependent variable. Again we see the same pattern as before. Specifically the impact of judges on incorporated firms is quite similar to that in the overall sample (i.e. Table 10). Given that incorporated firms make up the majority of the sample, this result is not too surprising. In addition we find that the effects of pro-debtor bias in judges are generally larger for partnerships than for incorporated firms, but largely attenuated for sole proprietorships.

## 6.2 Public vs. Private firms

In this table, we repeat the analysis of the previous section using a dummy for whether the firm is public. As previously discussed, the idea here is that the public firms have clear separation of ownership and control, so one would expect that conflicts of interest between management and owners are most prevalent here.

In Table 14, we look for differential effects of pro-debtor bias by type of listing status on closure. We find that the interaction term on Pro-debtor \* Public is always negative and large, but significant only for year 4. In Table 15 we repeat the analysis with log sales as the dependent variable where closed firms are either coded as having zero sales (Panel A) or dropped from the sample (Panel B). For Panel A, we find that similar to the closure results, the interaction term on Pro-debtor \* Public is large and negative for all but the first year and significant at the 5% level for most year. Interestingly, the pattern continues even when closed firms are dropped from the sample (Panel B). Specifically after the first

year, the interaction coefficient grows monotonically more negative.<sup>15</sup> This result suggest that for public firms, being assigned to a pro-debtor judge not only increase the probability of closure, but even conditional on survival the firm is likely to see worse outcomes over time.

In Table 16 we repeat the analysis with log employees as the dependent variable where closed firms are either coded as having zero employees (Panel A) or dropped from the sample (Panel B). For Panel A, we find a similar pattern to that for sales. Specifically, the coefficient for the interaction term Pro-debtor \* Public is negative and significant for both the case where we code closed firms as having zero employees and when we drop them from the sample. As with sales, this result suggest that for public firms, a pro-debtor judge leads worse outcomes even conditional on survival.

Overall these results suggest that pro-debtor judges seem to be particularly problematic for the public firms that go through Chapter 11 but also for partnership firms. One can conjecture that at the top end of the size distribution (public firms versus private ones), the governance issues in Chapter 11 are magnified with a pro-debtor judge. If these judges transfer more bargaining power to management, which in a public firm is only loosely overseen by boards and dispersed shareholder, the top managers might therefore have more leeway to act on their own behalf rather than shareholder. The results however also suggest that this effect does not uniformly translate to all incorporated firms, say compared to sole proprietorships. As there is no significant difference between proprietorships and incorporated firms. But there is only a more negative effect on the partnerships. This effect might suggest that private and incorporated firms do not have the same governance issues in Chapter 11 as public firms, possibly since they usually have very concentrated shareholders. However, the partnerships might suffer from the fact that they have a diffuse set of residual claimants who have de facto partial control rights. Thus a pro-debtor judge might provide a lax enforcement regime that allows different partners to tunnel resources out of the bankruptcy estate and hurt the survival of the partnership in the long run.

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<sup>15</sup>The regression results for column 5 should be interpreted with caution since the number of surviving firms is quite small (N=25).

## 7 Conclusion

Our study provides a number of new insights into the distress resolution process. First, we document large heterogeneity in the ruling of judges in Chapter 11. Independent of the characteristics of the case, judges differ in their propensity to rule in favor of creditors or debtors. Interestingly we find some evidence that judges seem to use the different motions in Chapter 11 in a consistent way to either allow for more debtor or creditor friendly outcomes in Chapter 11. When we use these estimated judge fixed effects to proxy for the pro-debtor (pro-creditor) friendliness of a court, we find that firms which are assigned to judges who score higher on our pro-debtor index, tend to have a lower likelihood to survive in the five years after Chapter 11, they also have larger deterioration of sales and employment after Chapter 11. These results are counter-intuitive based on the simple idea that debtors who have limited liability in distress should be more concerned about continuation.

We propose that a failure of governance within firms that go into distress could be at the core of these findings. Our results suggest that the negative effect of pro-debtor judges is particularly strong for public firms and partnerships. If the interest of equity is not adequately represented in the bankruptcy process, more specifically, if management is able to extract private financial benefits during the restructuring process, it might come at the expense of the long-term continuation value for equity holders. We conjecture that this channel explains why we find a more negative effect of pro-debtor judges on public firms. An additional channel would be that a restructuring process that is tilted towards debtors allows equity itself to extract financial resources from the firm instead of keeping assets tied up in firm. For example, owner-managers who are in control of day-to-day management of the firm might be able to take the most important assets out of the firm to set up a similar firm in parallel. If this extraction technology for equity is not too inefficient, continuation might be less attractive than a prolonged bankruptcy process and potential shut down, since in continuation the owners would have to share future income with other claim holders. We believe that this effect is prevalent for the partnerships, where individual partners have the de facto control rights to tunnel resources out of firm, unless prevented from doing so by a

(pro-creditor) judge. These results point to the importance of understanding who has de facto control rights in the distress resolution and whether control holders have opportunities to deploy the firms assets more profitably outside the current firm.

Ignoring the importance of firm governance in the restructuring of distressed firms might result in policy suggestions that are not in the interest of the parties they are supposed to protect. For example, many developing countries adopt bankruptcy policies that are aimed to stimulate firm creation and protect firms as a going concern by providing more power to debtors in financial distress. However, in light of the findings in this paper one might worry that this could have the opposite effect if debtors are able to divert assets more easily out of the firm. In turn, this would not only have a negative effect on protecting firms as a going concern but also dissuade investors from providing financing to businesses in the first place. Going forward much more research is needed to understand how distress resolution interacts with the governance structure of firms that enter distress.

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**Table I**  
**Variable Descriptions**

<i>Name</i>	<i>Source</i>	<i>Description</i>
Case Duration	<i>PACER</i>	The number of days between initial filing and case closure date.
Corporation	<i>Dun &amp; Bradstreet</i>	A dummy for whether a firm is a formal corporation.
Out of Business	<i>NETS</i>	A dummy for whether a firm disappears from the NETS universe.
PAYDEX	<i>NETS</i>	PAYDEX is a dollar-weighted 1-100 numerical score that indicates a company's payment performance as reported to D&B. Higher scores are better.
Refile	<i>PACER</i>	Whether a firm refiles for bankruptcy in the <i>same</i> district within three years.
Sales	<i>Dun &amp; Bradstreet</i>	Total sales for the year <i>before</i> bankruptcy filing.
	<i>NETS</i>	Total sales by year for years 1991-2003.
Voluntary	<i>PACER</i>	A dummy for whether a firm voluntarily filed for Chapter 11.

**Table II**  
**Descriptive Statistics<sup>a</sup>**

<i>Panel A</i>		<i>Firm level data: Dun &amp; Bradstreet</i>				
	<i>Mean</i>	<i>Median</i>	<i>Std. Dev.</i>	<i>Min Val.</i>	<i>Max Val.</i>	<i>Count</i>
Sales	1.1 million	0.16 million	13 million	0	558 million	7827
Employees	96.10	2	1111	0	39,400	7827
Refile Rate	0.02	0	0.15	0	1	7827
Voluntary	0.97	1	0.16	0	1	7827

  

<i>Panel B</i>		<i>Firm level data: NETS</i>				
	<i>Mean</i>	<i>Median</i>	<i>Std. Dev.</i>	<i>Min Val.</i>	<i>Max Val.</i>	<i>Count</i>
Sales	2.8 million	0.55 million	2.3 million	16,700	23 million	2024
Employees	24.27	7	94.19	1	3,000	2027
Refile Rate	0.03	0	0.17	0	1	2027
Voluntary	0.99	1	0.12	0	1	2027
Public	0.03	0	0.18	0	1	2027
Corporation	0.79	1	0.41	0	1	1716

<sup>a</sup>Notes:

1. Descriptive statistics are all based on the year prior to the firm filing for bankruptcy. The top panel reports data from Dun & Bradstreet, the bottom panel uses data from NETS, which is a subset of the D&B sample. Sales and Employment are based on information from D&B. Refile is a dummy variable equal one if the firm refilled for either Chapter 11 or 7 within three years after the last filing. Voluntary is an index variable equal one if the bankruptcy filing was voluntarily initiated by management. Corporate is a variable that signifies if the firm is incorporated rather than a sole proprietorship, partnership or non-profit.

**Table III**  
**Descriptive Statistics<sup>a</sup>**

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<i>Case Count by Court</i>		
	<i>Dun &amp; Bradstreet</i>	<i>NETS</i>
AZ (Phoenix)	2,404	698
AZ (Tucson)	595	167
AZ (Yuma)	53	22
CA (LA)	657	275
CA (ND)	95	37
CA (RS)	214	102
CA (SA)	459	165
CA (SV)	249	116
DEL	2,359	783
GA (North)	362	182
NJ (Camden)	66	30
NJ (Newark)	172	78
NJ (Trenton)	142	73

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<sup>a</sup>Notes: The table reports sample sizes broken down by district court.

**Table IV**  
**Random Assignment Tests<sup>a</sup>**

	<i>Joint F-test for Judge F.E.</i>			
<i>Sales</i>				
Number of Cases	7827	7827	7827	7827
Number of Judges	54	54	54	54
Prob > F	0.05	0.99	1.00	1.00
<i>Number of Employees</i>				
Number of Cases	7827	7827	7827	7827
Number of Judges	54	54	54	54
Prob > F	0.03	1.00	1.00	1.00
<i>Public</i>				
Number of Cases	2027	2027	2027	2027
Number of Judges	48	48	48	48
Prob > F	0.00	0.32	0.57	0.42
<i>Corporation</i>				
Number of Cases	1713	1713	1713	1713
Number of Judges	48	48	48	48
Prob > F	0.00	0.55	0.83	0.83
<i>Credit Rating</i>				
Number of Cases	1426	1426	1426	1426
Number of Judges	48	48	48	48
Prob > F	0.00	0.53	0.65	0.57
<hr/>				
District Trends	No	Yes	Yes	Yes
Industry F.E.	No	No	Yes	Yes
Voluntary Filing dummy	No	No	No	Yes

<sup>a</sup>Notes:

1. Each cell in the table reports the number of observations, number of judges and the results of a joint F test for all judge fixed effect coefficients using the dependent variable reported on the left side of each panel. The regressions sequentially include district trends, industry fixed effects and a control for voluntary filing dummies. The definitions of the dependent variables are provided in Table 1.

**Table V**  
**ProDebtor Random Assignment Test<sup>a</sup>**

	<i>Pro-Debtor Coefficient</i>	
ln(Sales)	-0.389 (0.428)	-0.014 (0.072)
N	7827	7827
ln(Employees)	-0.047 (0.059)	0.018 (0.021)
N	7827	7827
Public (Probit)	0.074 (0.096)	-0.001 (0.120)
N	2027	1123
Corporation (Probit)	0.048 (0.053)	-0.018 (0.023)
N	2261	1962
Credit Rating	0.284 (0.561)	-0.254 (0.769)
Count	893	893
Controls	N	Y

<sup>a</sup>Notes:

- Each cell in the table reports the coefficient and standard error from a regression of the dependent variable reported on the left side on a dummy for whether the judge is pro-debtor, as well as the observation count. All regressions include court fixed effects and period fixed effects. The results in the second column control for district trends, industry F.E. and voluntary filing dummy. Standard errors are clustered on judges. + Significant at 10%; \* Significant at 5%; \*\* Significant at 1%

**Table VI**  
**Frequency of Filings<sup>a</sup>**

	<i>Filing Count</i>		<i>Judge Fixed Effect</i>	
	<i>Mean</i>	<i>Std. Dev.</i>	<i>F(49,7526)</i>	<i>Prob&gt;F</i>
CC Agreed	0.002	0.049	0.67	0.96
CC Grant	0.008	0.091	2.42	0.00
CC Deny	0.096	0.295	2.65	0.00
Convert/Dismiss	0.033	0.180	4.69	0.00
Convert/Dismiss Deny	0.431	0.192	4.51	0.00
Exclusive Grant	0.002	0.041	1.06	0.36
Exclusive Deny	0.014	0.153	1.53	0.01
JudgeSub Grant	0.000	0.000	2.06	0.00
JudgeSub Deny	0.044	0.206	3.34	0.00
LiftStay Grant	0.035	0.184	2.82	0.00
LiftStay Deny	0.254	0.436	1.83	0.00
Objection Debtor	0.002	0.048	1.64	0.00
Objection Creditor	0.004	0.064	2.13	0.00
Sale Grant	0.003	0.054	1.08	0.33
Sale Deny	0.007	0.081	3.00	0.00
SuaSponte	0.002	0.042	1.81	0.00

<sup>a</sup>Notes:

1. Each row in the table reports the following statistics for the dependent variable reported on the left hand side: the average frequency that a motion is requested across all the cases in the sample; the standard deviation; the F-statistics from a joint F-test of all the judge fixed effects. Regressions used to generate the F test include controls for district times year F.E., industry F.E., and a control for voluntary filing.

**Table VII**  
**Principal Component Analysis**

	Comp1	Comp2	Comp3	Comp4	Comp5	Comp6	Comp7
Proportion	0.3926	0.1995	0.1469	0.0910	0.0801	0.0524	0.0376
Pro-Debtor							
Cash Collateral	0.4710	0.0552	-0.0898	0.4685	0.4439	0.5840	0.0970
Extend Exclusivity	0.3906	-0.1778	0.4940	-0.2826	-0.5375	0.4335	-0.1229
Sale of Assets	0.5129	0.2036	-0.1050	-0.0639	-0.2089	-0.3574	0.7135
Obj. by Creditor	0.4928	0.0200	0.3150	0.1614	0.2206	-0.5624	-0.5163
Pro-Creditor							
Convert or Dismiss	-0.0725	0.6487	0.3625	-0.4993	0.4165	0.1266	0.0610
Lift Stay	-0.1857	0.6283	0.1599	0.5711	-0.4578	0.0206	-0.0949
Obj. by Debtor	-0.2828	-0.3285	0.6933	0.3083	0.1965	-0.1028	0.4327

**Table VIII**  
**ProDebtor Outcomes** <sup>a</sup>

<i>Pro-Debtor Coefficient</i>	
	<i>D&amp;B</i>
<i>Open</i>	-0.007 (0.016)
N	1982
<i>Log(Case Duration)</i>	-0.007 0.064
N	6151
<i>Refile</i>	0.002 (0.002)
N	7827

<sup>a</sup>Notes:

1. Each cell reports the coefficient on the pro-debtor dummy from a regression of the dependent variable reported on the left hand side on a pro-debtor dummy. All regressions include controls for district trends, industry F.E., voluntary filing and pre-bankruptcy sales and employment. Standard errors are clustered by judge. Re-file is defined as the same firm re-filing for bankruptcy within three years after a previous filing. Dropping cases that occur less than three years before the last date of our PACER case data has no material effect on the results. <sup>+</sup> Significant at 10%; \* Significant at 5%; \*\* Significant at 1%

**Table IX**  
**Firm Continuation <sup>a</sup>**

	<i>Years after Initial Filing</i>			
	$\leq 2$	<i>3</i>	<i>4</i>	<i>5</i>
<i>Panel A: Probit</i>				
Pro-debtor	-0.053 (0.063)	-0.092 (0.060)	-0.135 (0.066)*	-0.155 (0.047)**
N	1829	1828	1706	1504
<i>Panel B: OLS</i>				
	-0.009 (0.017)	-0.028 (0.021)	-0.044 (0.024)+	-0.052 (0.017)**
N	2024	1926	1772	1571
<i>Panel C: Fraction Open</i>				
Mean	0.778	0.621	0.502	0.424
N	2024	1926	1772	1571

<sup>a</sup>Notes:

1. The dependent variable Firm Continuation is a dummy with a value equal to one if NETS records a non-missing sales number for the firm N years after a bankruptcy filing, conditional on the bankruptcy occurring at least N years before the last year in the data. Each cell reports the coefficient from a regression of Firm Continuation on the pro-debtor dummy, if the firm is more than 2, 3, 4, or 5 years out of Chapter 11. All regressions include controls for district trends, industry F.E., voluntary filing and pre-bankruptcy sales and employment. Standard errors are clustered on judges. + Significant at 10%; \* Significant at 5%; \*\* Significant at 1%

**Table X**  
**Sales and Employment<sup>a</sup>**

	<i>Years after Initial Filing</i>					
	<i>0</i>	<i>1</i>	<i>2</i>	<i>3</i>	<i>4</i>	<i>5</i>
<i>Panel A</i>						
	<i>Log Sales</i>					
	<i>Setting Closed Firms' Sales = 0</i>					
Pro-debtor	0.006 (0.123)	-0.163 (0.191)	-0.125 (0.585)	-0.406 (0.276)	-0.601 (0.304)+	-0.684 (0.220)**
r2	0.432	0.208	0.170	0.162	0.159	0.145
N	2024	2024	2024	1926	1772	1571
<i>Dropping Closed Firms</i>						
Pro-debtor	0.009 (0.010)	0.022 (0.021)	0.012 (0.034)	0.012 (0.037)	0.093 (0.037)*	0.111 (0.048)*
r2	0.954	0.904	0.880	0.842	0.796	0.723
N	1977	1825	1570	1197	890	666
<hr/>						
<i>Panel B</i>						
	<i>Log Employees</i>					
	<i>Setting Closed Firms' Employees = 0</i>					
Pro-debtor	0.001 (0.012)	-0.011 (0.031)	-0.018 (0.043)	-0.054 (0.047)	-0.071 (0.042)+	-0.100 (0.039)*
r2	0.432	0.208	0.170	0.162	0.159	0.145
N	2024	2024	2024	1926	1772	1571
<i>Dropping Closed Firms</i>						
Pro-debtor	0.007 (0.010)	0.023 (0.019)	0.008 (0.028)	0.022 (0.028)	0.081 (0.028)**	0.067 (0.039)+
r2	0.955	0.900	0.870	0.833	0.787	0.763
N	1982	1828	1572	1197	890	668

<sup>a</sup>Notes:

- The dependent variable in Panel A is log(sales) and in Panel B is log(employment) both reported from NETS, where employees are measured as the number of employees reported by the firm. Each column reports the coefficient from a regression of the dependent variable on the pro-debtor dummy, if the firm is more than 1, 2, 3, 4, or 5 years out of Chapter 11. The first half of each Panel reports the results when we set the number of sales (or employees) to zero, once a firm drops out from the data. This means we assign the full loss of sales or employees to the firm. In the second half of each Panel we drop observations one a firm is closed and leaves the sample, which means we de facto condition on survival. All regressions include controls for district trends, industry F.E., voluntary filing and pre-bankruptcy sales and employment. Standard errors are clustered on judges. In this specification we drop firms for which our data could not have a fully populated panel of sales or employment. + Significant at 10%; \* Significant at 5%; \*\* Significant at 1%

**Table XI**  
**Firm Continuation & Ownership<sup>a</sup>**

	<i>Years after Initial Filing</i>			
	$\leq 2$	<i>3</i>	<i>4</i>	<i>5</i>
	<i>Ownership Type</i>			
Pro-debtor	-0.052 (0.067)	-0.096 (0.063)	-0.105 (0.074)	-0.132 (0.052)*
Pro-debtor*Proprietorship	0.266 (0.120)	0.183 (0.115)	0.104 (0.114)	0.058 (0.133)
Pro-debtor*Partnership	-0.589 (0.184)**	-0.439 (0.189)**	-0.385 (0.009)**	-0.138 (0.253)
Pro-debtor*Non-Profit	0.614 (0.292)*	-0.240 (0.335)	0.143 (0.340)	0.508 (0.500)
Sole	0.087 (0.141)	0.060 (0.121)	0.205 (0.111)+	0.180 (0.109)+
Partnership	0.150 (0.206)	0.239 (0.166)	0.515 (0.134)**	0.190 (0.158)
Non-Profit	0.782 (0.501)	-0.096 (0.370)	-0.139 (0.364)	-0.187 (0.411)
N	1481	1517	1411	1231

<sup>a</sup>Notes:

1. Each column represent the results of a separate Probit regression. The dependent variable Firm Continuation is a dummy with a value equal to one if NETS records a non-missing sales number for the firm N years after a bankruptcy filing, conditional on the bankruptcy occurring at least N years before the last year in the data. Sole-Proprietor, Partnership and Non-Profit refer to the legal status of the firm. The omitted category is Incorporated firms. All regressions include controls for district trends, industry F.E., voluntary filing and pre-bankruptcy sales and employment. Standard errors are clustered by judge.
2. + Significant at 10%; \* Significant at 5%; \*\* Significant at 1%

**Table XII**  
**Ownership and Sales<sup>a</sup>**

	<i>Years after Initial Filing</i>					
	<i>0</i>	<i>1</i>	<i>2</i>	<i>3</i>	<i>4</i>	<i>5</i>
<i>Log Sales</i>						
<i>Setting Closed Firms' Sales = 0</i>						
Pro-debtor	0.004 (0.079)	-0.218 (0.146)	-0.126 (0.255)	-0.364 (0.311)	-0.428 (0.339)	-0.565 (0.247)*
Pro-debtor*Proprietorship	-0.036 (0.190)	0.355 (0.284)	0.599 (0.442)	0.630 (0.510)	0.394 (0.590)	0.213 (0.678)
Pro-debtor*Partnership	-0.208 (0.161)	-0.587 (0.387)	-1.266 (0.617)*	-1.223 (0.778)	-1.260 (0.565)*	-0.522 (0.576)
Pro-debtor*Non-Profit	0.030 (0.323)	0.640 (0.662)	1.443 (1.019)	-0.633 (1.263)	0.982 (1.593)	2.498 (2.501)
r2	0.496	0.210	0.185	0.186	0.180	0.157
N	1710	1710	1710	1628	1483	1311
<i>Dropping Closed Firms</i>						
Pro-debtor	0.020 (0.014)	0.037 (0.023)	0.019 (0.038)	0.045 (0.038)	0.103 (0.041)*	0.125 (0.047)**
Pro-debtor*Proprietorship	-0.048 (0.033)	-0.060 (0.051)	-0.074 (0.065)	-0.129 (0.083)+	-0.127 (0.070)+	-0.170 (0.081)*
Pro-debtor*Partnership	0.018 (0.034)	0.008 (0.076)	0.029 (0.090)	0.100 (0.080)	0.152 (0.147)	0.177 (0.186)
Pro-debtor*Non-Profit	-0.095 (0.068)	0.038 (0.127)	0.209 (0.123)+	-0.074 (0.092)	-0.020 (0.174)	-0.131 (0.144)
r2	0.953	0.898	0.876	0.846	0.836	0.795
N	1683	1571	1347	1019	763	564

<sup>a</sup>Notes:

- The dependent variable is log(sales) reported from NETS. Each column reports the coefficient from a regression of the dependent variable on the pro-debtor dummy, dummies for firm legal status (not shown), and their interactions if the firm is more than 1, 2, 3, 4, or 5 years out of Chapter 11. Sole-Proprietor, Partnership and Non-Profit refer to the legal status of the firm. The omitted category is Incorporated firms. The first half of each Panel reports the results when we set the number of sales to zero, once a firm drops out from the data. This means we assign the full loss of sales to the firm. In the second half of each Panel we drop observations once a firm is closed and leaves the sample, which means we de facto condition on survival. All regressions include controls for district trends, industry F.E., voluntary filing and pre-bankruptcy sales and employment. Standard errors are clustered on judges. In this specification we drop firms for which our data could not have a fully populated panel of sales or employment. + Significant at 10%; \* Significant at 5%; \*\* Significant at 1%

**Table XIII**  
**Ownership and Employment<sup>a</sup>**

	<i>Years after Initial Filing</i>					
	<i>0</i>	<i>1</i>	<i>2</i>	<i>3</i>	<i>4</i>	<i>5</i>
	<i>Log Employees</i>					
	<i>Setting Closed Firms' Employees = 0</i>					
Pro-debtor	0.012 (0.010)	0.033 (0.021)	0.019 (0.030)	0.037 (0.032)	0.093 (0.032)**	0.095 (0.039)*
Pro-debtor*Proprietorship	-0.047 (0.028)+	-0.057 (0.038)	-0.076 (0.053)	-0.126 (0.067)+	-0.153 (0.073)*	-0.184 (0.068)**
Pro-debtor*Partnership	0.020 (0.019)	-0.003 (0.029)	-0.004 (0.055)	0.027 (0.070)	0.096 (0.143)	0.084 (0.163)
Pro-debtor*Non-Profit	-0.143 (0.053)**	-0.176 (0.062)**	-0.090 (0.058)	-0.149 (0.087)+	-0.024 (0.113)	-0.165 (0.146)
r2	0.956	0.901	0.871	0.835	0.791	0.768
N	1982	1828	1572	1197	890	668
	<i>Dropping Closed Firms</i>					
Pro-debtor	0.014 (0.011)	0.027 (0.021)	0.015 (0.034)	0.020 (0.040)	0.098 (0.047)*	0.125 (0.055)*
Pro-debtor*Proprietorship	-0.052 (0.032)	-0.059 (0.049)*	-0.084 (0.061)	-0.143 (0.077)+	-0.172 (0.071)*	-0.193 (0.074)*
Pro-debtor*Partnership	0.015 (0.034)	0.011 (0.071)	0.035 (0.084)	0.114 (0.082)	0.166 (0.145)	0.204 (0.180)
Pro-debtor*Non-Profit	-0.126 (0.072)*	0.017 (0.124)	0.143 (0.126)	-0.140 (0.106)	-0.089 (0.159)	-0.204 (0.142)
r2	0.954	0.904	0.881	0.844	0.815	0.776
N	1977	1825	1570	1197	890	666

<sup>a</sup>Notes:

- The dependent variable is log(employees) reported from NETS, where employees are measured as the number of employees reported by the firm. Each column reports the coefficient from a regression of the dependent variable on the pro-debtor dummy, dummies for firm legal status (not shown), and their interactions if the firm is more than 1, 2, 3, 4, or 5 years out of Chapter 11. Sole-Proprietor, Partnership and Non-Profit refer to the legal status of the firm. The omitted category is Incorporated firms. The first half of each Panel reports the results when we set the number of employees to zero, once a firm drops out from the data. This means we assign the full loss of employment to the firm. In the second half of each Panel we drop observations once a firm is closed and leaves the sample, which means we de facto condition on survival. All regressions include controls for district trends, industry F.E., voluntary filing and pre-bankruptcy sales and employment. Standard errors are clustered on judges. In this specification we drop firms for which our data could not have a fully populated panel of sales or employment. + Significant at 10%; \* Significant at 5%; \*\* Significant at 1%

**Table XIV**  
**Public vs. Private: Firm Continuation <sup>a</sup>**

	<i>Years after Initial Filing</i>			
	$\leq 2$	<i>3</i>	<i>4</i>	<i>5</i>
	Public v Private			
Pro-debtor	-0.043 (0.064)	-0.084 (0.061)	-0.128 (0.068)+	-0.152 (0.047)**
Pro-debtor*Public	-0.407 (0.277)	-0.414 (0.258)	-0.720 (0.341)*	-0.636 (0.423)
Public	-0.732 (0.064)**	-0.507 (0.191)**	-1.508 (0.366)**	-1.739 (0.509)**
N	1829	1828	1706	1504

<sup>a</sup>Notes:

1. Each column represents the results of a separate Probit regression. The dependent variable Firm Continuation is a dummy with a value equal to one if NETS records a non-missing sales number for the firm N years after a bankruptcy filing, conditional on the bankruptcy occurring at least N years before the last year in the data. Public refers to the listing status of the firm. The omitted category is Private firms. All regressions include controls for district trends, industry F.E., voluntary filing and pre-bankruptcy sales and employment. Standard errors are clustered by judge.
2. + Significant at 10%; \* Significant at 5%; \*\* Significant at 1%

**Table XV**  
**Public Firms and Sales<sup>a</sup>**

	<i>Years after Initial Filing</i>					
	<i>0</i>	<i>1</i>	<i>2</i>	<i>3</i>	<i>4</i>	<i>5</i>
	<i>Log Sales</i> <i>Setting Closed Firms' Sales = 0</i>					
Pro-debtor	0.016 (0.078)	-0.120 (0.120)	-0.098 (0.232)	-0.369 (0.280)	-0.559 (0.310)+	-0.645 (0.230)**
Pro-debtor*Public	-0.603 (0.284)*	-2.603 (1.096)*	-1.972 (1.350)	-2.256 (1.331)+	-2.645 (1.135)*	-1.722 (0.962)+
Public	-0.770 (0.245)**	-2.743 (0.952)**	-3.395 (1.081)**	-2.844 (1.005)**	-6.061 (1.316)**	-6.139 (1.461)**
r2	0.432	0.208	0.170	0.162	0.159	0.145
N	2024	2024	2024	1926	1772	1571
	<i>Dropping Closed Firms</i>					
Pro-debtor	0.008 (0.011)	0.019 (0.022)	0.014 (0.033)	0.015 (0.037)	0.098 (0.037)*	0.110 (0.049)*
Pro-debtor*Public	0.031 (0.102)	0.192 (0.197)	-0.154 (0.141)	-0.291 (0.119)*	-0.680 (0.182)**	-17.584 (2.804)**
Public	-0.073 (0.068)	-0.109 (0.094)	0.046 (0.105)	-0.136 (0.112)	0.113 (0.322)	6.052 (1.070)**
r2	0.954	0.904	0.880	0.843	0.811	0.771
N	1977	1825	1570	1197	890	666

<sup>a</sup>Notes:

- The dependent variable is log(sales) reported from NETS. Each column reports the coefficient from a regression of the dependent variable on the pro-debtor dummy, if the firm is more than 1, 2, 3, 4, or 5 years out of Chapter 11. Public refers to the listing status of the firm. The omitted category is Private firms. The first half of each Panel reports the results when we set the number of sales to zero, once a firm drops out from the data. This means we assign the full loss of sales to the firm. In the second half of each Panel we drop observations once a firm is closed and leaves the sample, which means we de facto condition on survival. All regressions include controls for district trends, industry F.E., voluntary filing and pre-bankruptcy sales and employment. Standard errors are clustered on judges. In this specification we drop firms for which our data could not have a fully populated panel of sales or employment. + Significant at 10%; \* Significant at 5%; \*\* Significant at 1%

**Table XVI**  
**Public Firms and Employment<sup>a</sup>**

	<i>Years after Initial Filing</i>					
	<i>0</i>	<i>1</i>	<i>2</i>	<i>3</i>	<i>4</i>	<i>5</i>
	<i>Log Employees</i>					
	<i>Setting Closed Firms' Employees = 0</i>					
Pro-debtor	0.009 (0.010)	0.023 (0.019)	0.011 (0.027)	0.024 (0.028)	0.085 (0.028)**	0.067 (0.039)+
Pro-debtor*Public	-0.086 (0.034)*	0.031 (0.175)	-0.326 (0.101)**	-0.224 (0.062)**	-0.529 (0.146)**	3.378 (1.940)+
Public	0.008 (0.025)	-0.113 (0.095)	-0.129 (0.052)*	-0.182 (0.052)**	-0.112 (0.172)	-0.972 (0.759)
r2	0.956	0.901	0.871	0.834	0.787	0.763
N	1982	1828	1572	1197	890	668
	<i>Dropping Closed Firms</i>					
Pro-debtor	0.009 (0.010)	0.023 (0.019)	0.011 (0.027)	0.024 (0.028)	0.085 (0.028)**	0.067 (0.039)+
Pro-debtor*Public	-0.086 (0.034)	0.031 (0.175)	-0.326 (0.101)**	-0.224 (0.062)**	-0.529 (0.146)**	3.378 (1.940)+
Public	0.008 (0.025)	-0.113 (0.095)	-0.129 (0.052)*	-0.182 (0.052)**	-0.112 (0.172)	-0.972 (0.759)
r2	0.956	0.901	0.880	0.843	0.811	0.771
N	1982	1828	1572	1197	890	668

<sup>a</sup>Notes:

- The dependent variable is log(employees) reported from NETS. Each column reports the coefficient from a regression of the dependent variable on the pro-debtor dummy, if the firm is more than 1, 2, 3, 4, or 5 years out of Chapter 11. Public refers to the listing status of the firm. The omitted category is Private firms. The first half of each Panel reports the results when we set the number of employees to zero, once a firm drops out from the data. This means we assign the full loss of employment to the firm. In the second half of each Panel we drop observations once a firm is closed and leaves the sample, which means we de facto condition on survival. All regressions include controls for district trends, industry F.E., voluntary filing and pre-bankruptcy sales and employment. Standard errors are clustered on judges. In this specification we drop firms for which our data could not have a fully populated panel of sales or employment. + Significant at 10%; \* Significant at 5%; \*\* Significant at 1%