

Practice Makes Perfect: Judge Experience and Bankruptcy Outcomes *

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Abstract

Exploiting the within-district random assignment of bankruptcy cases to judges, we provide new evidence on the effects of judges' on-the-bench experience on large public corporate Chapter 11 outcomes. We find that cases assigned to more experienced judges spend less time in bankruptcy, are more likely to be reorganized rather than liquidated, but are not more likely to refile for bankruptcy after emergence. Our heterogeneity test suggests that the effect of judge experience is stronger when the court is busy. In addition, we show that prior judgeship professional experience and personal attributes do not consistently explain case outcomes. Our evidence collectively highlights the importance of on-the-bench experience for judges and the impact of judge characteristics on large corporate bankruptcies.

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1 Introduction

The bankruptcy judge, charged with overseeing the restructuring process, is the most important decision maker in corporate bankruptcies in the United States. Bankruptcy judges possess large amounts of discretion in interpreting bankruptcy law and ruling on all major actions undertaken by firms in bankruptcy, including compensating managers and professionals (Goyal and Wang 2017), granting post-petition financing (Dahiya, John, Puri, and Ramirez 2003; Li and Wang 2016), approving asset sales and liquidation (Bernstein, Colonnelli, and Iverson 2016; Bernstein, Colonnelli, Giroud, and Iverson 2017), resolving creditor valuation disputes (Sharfman 2005; Ayotte and Morrison 2017), and confirming reorganization plans. In spite of the importance of judicial discretion in bankruptcy rulings (Gennaioli and Rossi 2010; Chang and Schoar 2013), little is known about the extent to which judges' experience and personal attributes affect bankruptcy outcomes.

This paper, to the best of our knowledge, provides the first empirical study on the effect of bankruptcy judges' judicial experience on the restructuring outcomes of large US corporate Chapter 11 filings, and the extent to which judicial experience matters relative to judges' educational background, work experience, and personal attributes.

We construct a comprehensive bankruptcy sample of all major Chapter 11 filings by US public firms with more than \$50 million in assets overseen by 309 unique bankruptcy judges in 75 bankruptcy courts between 1980 and 2012. These large cases are not only economically important but also complex, often involving controversial issues and competing demands from various stakeholders—cases where judicial discretion and experience likely matter the most. For each judge in our sample, we obtain comprehensive information on judicial experience, previous professional experience, educational background, and personal characteristics, compiled from an array of sources including the U.S. Courts system, LinkedIn, LexisNexis personal reports, press releases, and voting records.

One important aspect of the bankruptcy process that facilitates our empirical identification is that bankruptcy judges are randomly assigned to cases filed in their bankruptcy court,

which allows causal interpretation of the effects we examine. While prior work documents random assignment of judges to bankruptcy filings, these studies employ datasets that are dominated by small business filings.¹ There are several reasons to question, however, whether large Chapter 11 cases are randomly assigned. For instance, judges have incentives to compete for large cases to gain national recognition; courts have incentives to selectively assign large cases to optimize judicial efficiency, case load, and exploit judges' industry expertise; and large firms have incentives to influence the judge assignment.

We perform two sets of empirical tests to address these concerns. First, we examine the set of judges eligible to be assigned a large Chapter 11 case filed in their home court and show that the likelihood a judge is assigned the case is unrelated to the judge's past experience or existing caseload. Second, we examine the correlation between the assigned judge's level of experience and firm characteristics such as book assets, financial leverage, operating performance, industry, and the number of business entities filing for Chapter 11. We find little relationship between these characteristics and judicial experience. Further, we find that these firm characteristics have almost no explanatory power for the overall variation in judge experience. Taken together, this evidence is consistent with the random assignment of large Chapter 11 filings.

Exploiting this random assignment, we examine the causal effect of judges' on-the-bench experience on three major bankruptcy outcomes: the duration of the bankruptcy, the probability of emergence (vs. liquidation in Chapter 11 or conversion to Chapter 7), and recidivism (refiling for bankruptcy within three years after emergence). These measures capture potential costs of the restructuring process and efficiency of bankruptcy judges. Our analysis provides evidence on how judicial experience acquired in the court affects these outcomes. We include firm-level controls and both court and industry fixed effects in all our empirical specification.

¹ Several prior studies have used judge random assignment as an identification mechanism, including Chang and Schoar (2013), Dobbie and Song (2015), Bernstein, Colonnelli, and Iverson (2016), Bernstein, Colonnelli, Giroud, and Iverson (2017).

We find that cases assigned to more-experienced judges spend less time in court and at the same time are significantly more likely to emerge from bankruptcy instead of being liquidated. In terms of economic magnitudes, cases assigned to a judge with twice as much time on the bench realize a 5.5% decrease in time spent in bankruptcy, a decline of nearly one month relative to the average duration of bankruptcies in our sample (16.5 months). This reduced time in bankruptcy represents a savings of more than \$2 million in legal fees alone for the average case in our sample, based on an assumption that legal fees represent approximately 2% of assets (LoPucki and Doherty 2004). Similarly, a one-standard-deviation increase in the judge's time on the bench leads to a 3% increase in the probability that the firm emerges from bankruptcy, which corresponds to 5.15% of the sample average (57%). Our estimated learning curve appears to be particularly steep at the beginning of a judge's term: during judges' first two years, cases take 18% longer (an increase of 3 months relative to the sample average) and are 6.5% less likely to emerge (11.4% of the sample average).

Our results further show that cases assigned to more experienced judges do not have higher bankruptcy refiling rates. The combined evidence suggests that cases assigned to experienced judges are able to move through the bankruptcy process more quickly and more firms are preserved as going-concerns, without higher recidivism. Interestingly, we find that a judge's experience gained from overseeing large Chapter 11 cases is not associated with bankruptcy outcomes for subsequent large Chapter 11 cases. Put differently, this evidence suggests that overall time on the bench matters more than judicial experience gained from overseeing large cases.

There are two potential explanations for our findings. On the one hand, judges gain experience on the bench (from managing both small and large bankruptcy cases) and this specific experience improves the efficiency in their rulings on large Chapter 11 cases. On the other hand, firms and their lawyers learn how to work with the judge more efficiently over time by observing judges' previous case rulings. We exploit variation in bankruptcy court caseloads to provide suggestive evidence on the mechanism driving our empirical findings.

Because the number of judges in a court is fixed, when more firms and individuals file for bankruptcy—for example during economic recessions—judges have higher workloads (Iverson 2017). Presumably, lawyers representing debtor firms have strong incentives to learn about judges’ rulings in prior cases regardless of whether the court is busy or not. Thus we would not expect the effects of judicial experience on case outcomes to vary with court caseloads if our results are driven by lawyers learning judges’ styles. In contrast, judge experience should matter more during periods of elevated caseloads if our results are driven by judges gaining valuable on-the-bench experience, as these are times when cases are possibly harder to manage, creditor conflicts are more severe, and thus experience is plausibly more valuable. Consistent with the latter possibility, we find that when caseloads are high, experienced judges significantly reduce case duration while simultaneously allowing more firms to emerge, whereas differences between experienced and inexperienced judges are less pronounced when caseloads are low. The evidence supports the notion that judges perfect their judicial skills while serving on the bench.

Finally, we compare the effects of judge on-the-bench experience to general experience (measured by educational background, years of prior work experience, and type of prior work) as well as judge personal characteristics (measured by gender, political affiliation, and military service). In contrast to on-the-bench experience, we find little association between a judge’s general professional experience prior to becoming a bankruptcy judge and bankruptcy outcomes. Meanwhile, we do find some associations between judge gender and military experience and case outcomes, suggesting that these characteristics affect judge decision-making. Importantly, our main findings on the effects of on-the-bench experience remain robust after including these judge characteristics as additional control variables.

Our paper contributes to two strands of research. First, our study provides evidence to further our understanding on the effects of judges on corporate bankruptcies. Most prior work focuses on judicial discretion and pro-debtor biases in bankruptcy rulings (Gennaioli and Rossi 2010; Chang and Schoar 2013). Dobbie and Song (2015), Bernstein, Colonnelli,

and Iverson (2016) and Bernstein, Colonnelli, Giroud, and Iverson (2017) examine fixed characteristics of judges and their effects on firm outcomes. Iverson (2017) finds that judges become more debtor-friendly when caseloads increase, allowing more firms to reorganize. Different from these studies, we document that some judicial characteristics change over time, and in particular that a judges' judicial experience plays an important role in determining the cost and efficiency of Chapter 11 outcomes. Our results suggest that specialization developed on the bench is incremental to previous work experience, political affiliations, and personal attributes in explaining judges' bankruptcy rulings (Ashenfelter, Eisenberg, and Schwab 1995; Rachlinski, Guthrie, and Wistrich 2006; Posner 2008).

Second, our study contributes to research on general versus specialized skills in human capital. A growing literature studies the differential effect of managers' general skills versus specific expertise on both corporate policies and their own compensation (Murphy and Zabojnik 2006; Guner, Malmendier, and Tate 2008; Lazear 2009; Custodio, Ferreira, and Matos 2013; Custodio and Metzger 2014).² Presently, the evidence is inconclusive as to which skill type matters most for human capital value and the managerial labor market. Our empirical findings that judges'—the most important “manager” of the restructuring process—on-the-bench experience affects bankruptcy outcomes suggest that specific expertise developed through time on the job matters more than general skills developed through prior work experience in resolving large and complex Chapter 11 cases.

The rest of the paper is organized as follows: Section 2 provides institutional background on judge assignment and forum shopping; Section 3 describes the data sample and defines the variables; Section 4 presents the main results and discussions; Section 5 concludes.

² A number of studies document the effect of managerial traits and attributes on corporate policies and CEO compensation (see Malmendier, Tate, and Yan (2011), Graham, Li, and Qiu (2012), Benmelech and Frydman (2015), for example).

2 Institutional Background

2.1 Judge Appointment

Each bankruptcy district has a fixed number of judgeships set by Congress.³ When a judgeship becomes available, announcement of the vacancy is made in newspapers and bankruptcy practitioner publications. Applicants are required to be members of the bar in good standing and to have at least five years of experience practicing law, unless the circuit’s judicial council determines that other relevant legal experience can be substituted. The vast majority of bankruptcy judges thus worked as lawyers (often, as bankruptcy lawyers) before being appointed to the bench (Mabey 2005).⁴ On average, there are 28 applicants for each judicial vacancy (Reddick and Knowlton 2013).

Applicants are evaluated by a merit selection panel, which is appointed by the judicial council in each circuit. The composition of merit review panels vary across circuit courts, but typically contain 5-8 members and consist of a mix of sitting judges, law practitioners, and academics. Merit review panels examine the applications of all applicants and, after additional interviews, recommend three to five “best qualified” candidates in ranked order. Although there is no universal set of qualifications that merit review panels examine, evidence in Reddick and Knowlton (2013) suggests that among the most important qualities are impartiality and fairness, strong background in bankruptcy law, organizational skill, decisiveness, and a commitment to the work. The recommendations of the merit review panel are passed on to active judges in the court of appeals who make the appointment and rarely deviate from the recommendations of the merit panel.⁵

³ The Judicial Conference of the United States conducts a study of judgeship needs every other year, and makes recommendations to Congress. However, because creating new judgeships requires passage of a bill by Congress, it is rare that new judgeships are created.

⁴ 92% of our sample judges have private law practice experience before judgeship.

⁵ Reddick and Knowlton (2013) provide extensive information on the appointment process for bankruptcy judges.

2.2 Forum Shopping and Judge Assignment

It is important to point out a few caveats regarding large chapter 11 filings and judge assignment which relate to our study. First, firms (especially large firms such as those in our sample) have some choice in where they choose to file for bankruptcy. The US Code Title 28 Chapter 87 §1408 states that a debtor can file under Chapter 11 in one of the following four locations: (1) the debtor’s place of domicile or residence, commonly referred to as the place of incorporation; (2) the debtor’s principal place of business; (3) the location of the debtor’s principal assets; (4) any district where a bankruptcy case is pending against the debtor’s affiliate. For small firms, these four locations are all the same, and thus they cannot select their bankruptcy venue.

However, the legal literature documents that an increasing number of large firms file in a court that is not in geographic proximity to their principal place of business or operations, a practice commonly known as “forum shopping.” The US bankruptcy courts for the District of Delaware and the Southern District of New York have emerged as the most popular venues among the 94 bankruptcy courts for forum shoppers since 1990. The choice is not innocuous. Eisenberg and LoPucki (1999), LoPucki and Doherty (2004), and LoPucki (2005) point out that firms engage in forum shopping to choose debtor-friendly venues. Furthermore, firms appear to choose courts with relevant expertise and which are more efficient compared to other jurisdictions (Skeel 1998; Ayotte and Skeel 2004). Venue choice is still a subject of intense debate and controversy. Given the potentially unobservable firm heterogeneity that is correlated with court choice, we include court fixed effects in all our regressions to study the relation between judge experience and Chapter 11 case outcomes. This regression framework allows us to exploit within-court cross-judge variation in judicial experiences at different points in time, while controlling for potentially omitted time-invariant heterogeneity across courts.

A second concern relates to whether *large* Chapter 11 cases are truly randomly assigned. When a corporation files for bankruptcy in a given bankruptcy district, the majority of

bankruptcy courts' stated policy is to randomly assign the case to one of the district's active bankruptcy judges.^{6,7} This random assignment has been used to generate empirical identification in a growing number of studies, including Chang and Schoar (2013), Dobbie and Song (2015), Bernstein, Colonnelli, and Iverson (2016) and Bernstein, Colonnelli, Giroud, and Iverson (2017). These studies uniformly find evidence that bankruptcy case characteristics are orthogonal to judge characteristics, but the samples are dominated by small business filings.

Due to the significant differences between small and large firms, there are reasons to question random assignment of large cases. First, judges may compete for large cases, as overseeing these cases will potentially lead to national recognition and prestigious status for the judge (LoPucki 2005). Second, since large cases require extensive efforts and a significant time commitment, courts could potentially assign larger and more complex cases to judges with more judicial experience who are also plausibly more efficient. Third, courts may manage judge case load optimally and be less likely to assign large, demanding cases to judges who are already busy with heavy case loads. Fourth, bankruptcy cases in certain industries may possess unique characteristics that would benefit from having a judge with specific industry knowledge and/or past experience handling these type of cases, so courts may potentially assign cases from particular industries nonrandomly. Finally, large firms (or their lawyers) may have enough knowledge of the court system to time their filings and

⁶ Technically, judge random assignment occurs at the divisional office level, as cases are filed in a particular office of a bankruptcy district. Across the U.S., there are 276 divisions, each pertaining to one of 94 bankruptcy districts. Because our sample is focused on very large bankruptcies, nearly all of the cases are filed in the main divisional office of each district. For example, among cases filed in the Southern District of New York in our sample, 93.3% are in Manhattan, 5.4% are in White Plains, and 1.2% are in Poughkeepsie. Because of this concentration of cases in large cities, bankruptcy district fixed effects are nearly equivalent to division fixed effects. Indeed, all of our results are qualitatively the same with similar magnitudes when controlling for divisional fixed effects. However, doing so more than doubles the number of fixed effects in the regressions, and, given our small sample sizes, as a result a few of the results are no longer statistically significant.

⁷ Anecdotal evidence suggests that there are a small number of courts that handle the assignment of judges to large, complex Chapter 11 cases differently from the assignment for small-business Chapter 11 filings. For example, in 2016 the Southern District of Texas assigned 50 percent of complex Chapter 11 cases to Judge Isgur and 50 percent to Judge Jones, while the remaining two judges (Bohm and Brown) received smaller Chapter 11 cases but none of the complex cases.

increase the likelihood of being assigned a particular judge or, alternatively, able to sway the court clerk to bypass the random assignment system. Due to these concerns, we adopt two sets of empirical tests in Section 4.1 to formally test judge random assignment to large Chapter 11 cases.

3 Data and Variable Construction

3.1 Chapter 11 Sample

Our initial bankruptcy sample contains the universe of Chapter 11 filings by public US firms with a filing date between 1980 and 2012 and that have assets of at least \$50 million at the time of filing.⁸ We initially identify 1,424 such Chapter 11 filings, and collect detailed information on firm characteristics at the time of filing, plan confirmation and effective dates, restructuring outcomes (emergence, acquisition, liquidation in Chapter 11 or converted to Chapter 7), and the judge assigned to the case. We drop five cases that were not confirmed as of 2015, 14 cases for which we cannot identify the judge at filing, 56 cases overseen by a district judge, and 39 cases that were transferred to other courts. Our final sample comprises 1,310 Chapter 11 filings⁹ assigned to 309 unique judges located in 75 bankruptcy courts, and is one of largest samples among studies of large corporate bankruptcies. For firms that successfully reorganize and emerge from bankruptcy, we search both BRD and bankruptcydata.com to determine whether they refile for Chapter 11 (i.e., “Chapter 22” filings).

We focus on three primary outcome variables: *Duration*, the natural logarithm of the

⁸ Specifically, we require these firms have filed financial statements with the SEC in any of the three years before bankruptcy. We end our sample in 2012 to avoid potential survival bias in measuring both the resolution of the case and any subsequent refiling. We use both UCLA LoPucki Bankruptcy Research Database (BRD) and New Generation Research’s bankruptcydata.com for data retrieval. Upon observing inconsistency between the two databases we resort to Public Access to Court Electronic Records (PACER) for verification.

⁹ The exact number of observations in each analysis varies due to the availability of judge experience measures and control variables.

number of months from the date of Chapter 11 filing to the date of plan confirmation; *Emergence*, an indicator variable set equal to one if a firm emerges from Chapter 11; and *Refile 3Y*, an indicator if a firm that emerged from bankruptcy filed again for bankruptcy within three years after emergence.

3.2 Judge Experience and Personal Attributes

We compile bankruptcy judges' career history by first requesting judges' resumes directly from bankruptcy courts. We supplement the resume data with information posted on bankruptcy court websites, LinkedIn, LexisNexis personal reports database, press releases, and other online and library resources. Importantly, we use official announcements of judge appointment and retirement dates to measure on-the-bench experience. This comprehensive search process enables us to identify each judge's on-the-bench court experience, professional experience before becoming a bankruptcy judge, and other personal experiences and attributes such as educational background, gender, age when appointed, and military services. In addition, we requested state voting records to infer judges' political affiliations and supplement this information with data from L2 Politics (a political campaign database).

We define four case-specific measures that capture the assigned judge's on-the-bench experience as of the case filing date. The first two measures, *Log(Months as Judge)* and *First 2 Years*, capture the amount of time the judge has been on the bench at the time of the bankruptcy filing.¹⁰ *Log(Months as Judge)* is defined as the natural logarithm of number of months since a judge has been appointed to the bankruptcy court. To capture any potentially nonlinear effects and because of a potential "learning curve," we also use the indicator *First 2 Years* to capture cases seen by a judge who has been on the bench for two years or less. The second two measures, *Log(Large)* and *First 2 Large*, capture judges' experience with large Chapter 11 filings.¹¹ While on the bench, a judge may handle many

¹⁰ For 10 judges we are unable to identify their appointment date and thus set these measures as missing for those cases.

¹¹ Because our sample of large cases begins in 1980, we set both case count variables as missing for any judge

cases in personal bankruptcy (under Chapters 7 and 13) and small business bankruptcy cases (under Chapters 7 and 11). However, handling *large* public bankruptcies can be a different task given the complex and influential nature of these cases, especially the first time a judge is assigned a large case. Therefore, we use the log number of Chapter 11 cases with more than \$50 million in assets that the judge has previously overseen ($\text{Log}(\text{Large})$) to measure judges' experience with large bankruptcy cases.¹² Because the learning curve for bankruptcy judges may not be linear, we also use the indicator variable *First 2 Cases*, which takes the value of one if the judge has previously seen two or fewer large cases. Together, these four measures allow us to capture different dimensions of a judge's on-the-bench experience.¹³

To measure judges' other professional experience, we consider $\text{Log}(\text{Years before Bench})$, the number of years of professional work experience since law school graduation; *Top5 Law School*, a dummy variable indicating that the judge attended a top 5 law school according to the 2009 US News law school rankings;¹⁴ and *Public Sector*, a dummy variable indicating if a judge has public sector experience, such as a law clerk or other government agency positions. We consider three measures for judges' personal characteristics and attributes, including *Male*, an indicator for male judges; *Military*, a dummy variable indicating if a judge ever served in the US military; and *Democrat*, which identifies whether a judge belongs to the Democratic party according to his/her voting record.

appointed before 1980 or with a missing appointment date.

¹² We include *all* filings of more than \$50 million in this case count variable, including both public and private firms, to capture experience with large Chapter 11 filings. However, in our analysis of restructuring outcomes we focus on only the sample of firms that filed with the SEC within three years prior to bankruptcy and for which we have the necessary control variables.

¹³ In unreported results, we use information from Lexis Nexis on all Chapter 11 filings overseen by a judge to measure a judge's overall experience with Chapter 11 filings, large or small. We find that this experience measure is highly correlated with $\text{Log}(\text{Months as Judge})$, and find our results are qualitatively similar when using this alternative experience measure.

¹⁴ US News only started to publish law school ranking on regular basis after 1990, which is before the period that the majority of our bankruptcy judges went to law school.

3.3 Summary Statistics

We summarize case characteristics in Panel A of Table 1.¹⁵ For our sample of 1,310 cases, the average case spent 16.54 months in Chapter 11, and 57% of these cases emerged from Chapter 11. Conditional on emergence, 8% of cases refiled for Chapter 11 within 3 years after emergence. In terms of experience measures, the average judge has been on the bench for 114.49 months (standard deviation 97.18), and 14% of cases (180) are assigned to judges who are in their first two years. The average (median) case is assigned to a judge who has previously seen 20 (5) large cases, and 30% of cases are assigned to a judge who has previously seen two or fewer large cases.¹⁶

Examining the characteristics of firms filing for bankruptcy, the average firm has assets of \$2,001.8 million in 2016 US dollars (median \$488.6 million). Firms that filed for Chapter 11 unsurprisingly have fairly high debt-to-assets leverage on average (1.01) and negative return on assets (-0.24%). Twenty-six percent of cases are filed as either pre-packaged or pre-negotiated cases, where negotiations between creditors and debtors have predominantly occurred prior to filing for bankruptcy. Twenty-nine percent of cases are filed in Delaware, and 18% of case are filed in the Southern District of New York.

As shown in Panel B of Table 1, 79% of the sample bankruptcy judges are male, 12% graduated from a top 5 law school, 23% served in the military, and 61% worked in the public sector before becoming a judge.

Panel C of Table 1 presents the correlation matrix for our four experience measures. $\text{Log}(\text{Months as Judge})$ and $\text{Log}(\text{Large})$ are modestly positively correlated with each other (0.245) and negatively correlated with the indicator variables First 2 Years and First 2 Large . Similarly, First 2 Years and First 2 Large are modestly positively correlated with each other (0.229). Together, these correlations suggest that the time-based and case-based

¹⁵ Please refer to Appendix for detailed variable definitions.

¹⁶ The average time the judge has been on the bench for the 353 cases assigned to a judge who has previously seen two or fewer cases is 78 months, suggesting First 2 Years and First 2 Large capture different aspects of judge experience.

experience measures capture different dimensions of judges' on-the-bench experience.

4 Analysis

4.1 Randomization Tests

The key identifying assumption in our empirical strategy is that judges are randomly assigned to bankruptcy cases. If judges are endogenously matched to particular cases, it is impossible to tell if the association between judge experience and case outcomes is causal in nature. Importantly, several prior empirical studies confirm that bankruptcy filings appear to be randomly assigned in practice.¹⁷ One difference between our study and previous work is our focus on large corporate bankruptcies, where judge experience is likely to matter most due to the complexity of the cases. It is thus possible that most Chapter 11 cases are randomly assigned while the largest cases are not.¹⁸ In this section, we examine whether *large* Chapter 11 cases appear randomly assigned to judges.

We begin with our sample of all large (i.e., more than \$50 million in assets) public Chapter 11 filings in the United States between 1990 and 2012.¹⁹ For each case, our objective is to determine the set of “eligible” judges which could have been assigned the case, and then determine whether these large, complex cases are more or less likely to be assigned to an experienced judge. Finding a lack of any such relationship is consistent with random assignment.

To determine the set of eligible judges, we first combine our sample of large chapter 11 filings with Lexis Nexis data of all bankruptcy filings to identify judges serving in the

¹⁷ See Dobbie and Song (2015), Chang and Schoar (2013), Bernstein, Colonnelli, and Iverson (2016), Bernstein, Colonnelli, Giroud, and Iverson (2017).

¹⁸ For example, it is possible that large firms are able to circumvent the random assignment process through timing their filings or exerting pressure on courts to receive a specific judge.

¹⁹ We restrict this sample to cases filed after 1990 because we require data from Lexis Nexis to determine the set of eligible judges and also to verify judges' end-of-term dates. Lexis Nexis data is only available (consistently) after 1990.

bankruptcy court where a case was filed.²⁰ We then use judges' appointment and retirement dates to determine the set of judges serving at the time the case was filed.²¹ If assignment is truly random, each of these judges should have an equal probability of being assigned a given case, regardless of that judge's level of experience.

Identifying the set of eligible judges is complicated by two features of bankruptcy courts. First, at least eight bankruptcy courts in our sample rely on "visiting" judges, where a judge from another district "visits" the court for a period of time.²² Typically, these judges continue to receive cases in their home court, and are at the visiting court for short periods of time (e.g., one week each month). Second, due to a shortage of bankruptcy judgeships, Delaware used both visiting and Delaware *district* judges to oversee bankruptcy cases in the early 2000's. The presence of both visiting and district judges complicates tests of random assignment, as it is not clear that visiting and district judges have equal probabilities of being assigned large Chapter 11 cases as the court's own bankruptcy judges. Indeed, empirically we find that visiting judges are assigned only a small number of large Chapter 11 cases.²³ Thus, including visiting and district judges in the set of eligible judges likely overstates the number of potential judges that could be assigned to a large case. We address these issues by dropping all cases assigned to a visiting or district judge, and excluding these judges from the set of eligible judges for that court (we however still include these visiting judges

²⁰ Importantly, we thus include 77 bankruptcy judges which, although serving in a court where a large case was filed, were never assigned a large chapter 11 filing.

²¹ Where retirement dates are unavailable, we use 30 days after the last date the judge saw *any* Chapter 11 case, based on information from the universe of Chapter 11 filings from LexisNexis.

²² Visiting judges are sometimes used by bankruptcy courts that have abnormally large case load relative to their capacity. For example, Delaware saw the number of bankruptcy filings rise sharply in the late 1990s' and as a result the court recruited visiting judges from other districts and recalled retired judges to oversee cases when the court case load went beyond its capacity. The eight bankruptcy courts we identified with visiting judges during our sample period are the Northern District of California, Delaware, the Southern District of Georgia, the Eastern District of Michigan, the Eastern District of Missouri, Nevada, the Southern District of New York, and the Southern District of Ohio.

²³ For seven of the eight courts with visiting judges, visiting judges saw a minority of large Chapter 11 filings (e.g., three cases in the northern district of California were assigned to visiting judges, compared to 51 cases assigned to judges appointed to that court). The only court with visiting judges where the majority of cases was *not* seen by its own judges is the southern district of Georgia, where only four large cases have been filed, with two cases assigned to visiting judges and two cases assigned to its own appointed judges.

in the set of eligible judges for their home court). Finally, we drop 14 cases filed in a court for which we have only one case for that court, as it is empirically impossible to evaluate random assignment for those courts. Our final sample used to evaluate random assignment consists of 1,174 large (i.e., more than \$50 million in assets at time of filing) cases.

To test for random assignment, we create a data set in which each of the 1,174 large bankruptcy observations is linked to each eligible judge. We create an indicator variable *Assigned* which equals one for the judge that was actually assigned to the case, and zero otherwise. The resulting sample contains 6,799 case-judge observations, representing 60 bankruptcy courts and 350 bankruptcy judges. For each case, the average number of eligible bankruptcy judges is 8.4 (min 1 and max 20). Using this sample we estimate linear probability models of the following form:

$$\text{Assigned}_{i,t} = \alpha + \beta_1 \text{JudgeExp}_{i,t} + \theta \text{Case FE} + \epsilon_{i,t} \quad (1)$$

where *JudgeExp* is one of the four judge experience measures (i.e., *Log(Months as Judge)*, *First 2 Years*, *Log(Large)*, or *First 2 Large*) or the log number of large cases currently assigned to the judge and not yet resolved (*Log(Large Caseload)*), all measured at the time a large case is filed.²⁴ We include case fixed effects, and cluster standard errors by court. If more experienced judges are more often assigned these complex cases, we would expect the coefficient to be positive for *Log(Months as Judge)* and *Log(Large)*, and negative for *First 2 Years* and *First 2 Large*. Similarly, if courts tend to not assign cases to judges with a heavy caseload, then the coefficient β_1 will be negative and statistically significant for the measure *Log(Large Caseload)*.

Table 2 presents the results of estimating equation 1. The unconditional probability of

²⁴ For eight judges we are unable to identify an appointment date, and 27 judges were appointed before 1980 (the earliest we have bankruptcy filings). To ensure the lack of an experience measure for one eligible judge does not bias the estimation, we drop all cases for which we do not have the relevant experience measure for all eligible judges for that particular specification. As a result, we have a different number of observations for each specification.

being assigned a case (mean of the dependent variable) is 0.17.²⁵ Across all five judge experience measures, we find that experience as judge and current caseload are unrelated to the assignment of cases, consistent with random assignment (or at least assignment that is independent of judge experience). For robustness, we also examine whether the insignificant relationship between experience and case assignment persists in seven subsamples (see Table A1 in the online appendix). Across all five judicial experience measures, we find an insignificant association between judicial experience and case assignment for samples that exclude prepacks, cases with more than \$500 million in assets, cases with less than \$500 million in assets, cases filed in either the Southern District of New York or Delaware, cases filed in only Delaware or cases filed in only the Southern District of New York, and all cases filed outside these two courts. Furthermore, due to Delaware’s reliance on both district and visiting judges, case assignment may have differed in Delaware before 2006, as it is difficult to assess random assignment during this period for that court. In table A2 we redo the main analysis from table 2 after dropping all cases filed in Delaware before 2006 and find similar results. We infer from our results that courts appear to randomly assign large cases to judges.

In addition, we also evaluate whether there is any correlation between judge experience and the assigned bankrupt firm’s characteristics within our sample. If large cases are assigned randomly, then firm characteristics such as size and leverage should be uncorrelated with the assigned judge’s level of experience. To empirically examine this aspect of random assignment, we return to our primary sample of all public Chapter 11 filings between 1980 and 2012 and estimate regressions of the following form:

$$\text{Experience} = \alpha + \beta_1 \text{Firm Characteristics} + \delta \text{Industry FE} + \theta \text{Court FE} + \epsilon_{i,t} \quad (2)$$

where *Experience* is one of the four measures of the judge’s experience at the time of filing,

²⁵ The case-specific average is 0.27.

and *Firm Characteristics* include $\text{Log}(\text{Assets})$ (the log number of assets in 2016 dollars upon filing for Chapter 11) and NumFilings (the total number of subsidiaries filing) to control for case complexity; and Leveragefiling and ROAfiling to control for firm performance upon filing for Chapter 11. Court and industry (Fama French 12) fixed effects are also included in each regression. In addition, the dummy variable Prepack/Preneg indicates that the case was prepackaged/prenegotiated. Standard errors are clustered by bankruptcy court.

Table 3 presents coefficient estimates of equation 2. Across the four experience measures, the only firm characteristic to consistently have a significant coefficient is Prepack/Preneg . The results suggest that these cases are significantly more likely to be assigned to judges with more experience, based on both time as judge and the amount of cases previously seen. While this possible violation could cast doubt of complete random assignment in our sample, we note that prepackaged bankruptcies are precisely the cases in which judges have the least influence, since negotiations have largely been completed before the bankruptcy is filed. Further, and more importantly, in Appendix Table A3 we show that all of our results hold when we drop all prepackaged or prenegotiated cases from our sample.

Examining the coefficient estimates on the remaining firm characteristics, we find no consistent pattern of significant coefficients across the different experience measures. Although three of the coefficients are significant at the 5% level, the sign of each significant coefficient changes from one experience measure to the next, which casts doubt that these firm characteristics are meaningfully associated with judge experience, although it is possible there is some marginal relationship.²⁶ We also examine the extent to which including firm characteristics increases the adjusted R^2 , relative to a baseline regression that only includes court fixed effects. We find that the adjusted R^2 increases from 0.05 to 0.07 for the $\text{Log}(\text{Months as Judge})$ specification, from 0.00 to 0.02 for the First 2 Years specification, is unchanged for the $\text{Log}(\text{Large})$ specification, and increases from 0.32 to 0.33 for the First 2 Large spec-

²⁶ As evidence of the mixed patterns, Num filings is significantly *negatively* associated with $\text{Log}(\text{Months as Judge})$ but significantly positively associated with $\text{Log}(\text{Large})$, despite these two experience measures being positively correlated.

ification (adjusted R^2 for both regression specifications tabulated in Table 3). Thus, case characteristics explain less than 2% of the variation in judge experience. Combined with the evidence in Table 2, our analysis suggests that even large cases appear on average randomly assigned to judges.

4.2 Judge On-the-Bench Experience and Chapter 11 Outcomes: Main Results

To test the impact of judges' on-the-bench experience on Chapter 11 case outcomes, we estimate the following regression:²⁷

$$\text{Outcome} = \alpha + \beta_1 \text{JudgeExp}_{i,t} + \beta_2 \text{Controls} + \delta \text{FEs} + \epsilon_{i,t} \quad (3)$$

where the dependent variable is either *Duration* (the log number of months a case is under Chapter 11), *Emergence* (a dummy variable indicating a firm emerges from Chapter 11), or *Refile3Y* (a dummy variable indicating a firm refiles for Chapter 11 within 3 years after emergence). We control for a time trend as well as a Post-BAPCPA dummy to account for the fact that average bankruptcy outcomes are changing over time (Bharath, Panchapagesan, and Werner 2010) and that the Bankruptcy Abuse Prevention and Consumer Protection Act (BAPCPA) altered some laws with regards to Chapter 11. We use the four judge experience measures and case controls previously defined. We continue to include court and industry (Fama French 12) fixed effects in each regression and cluster standard errors by court.

Table 4 presents coefficient estimates for the analysis of *Duration*. We find that the time-on-the-bench experience measures significantly impact how long the firm is in bankruptcy: more experienced judges process cases significantly faster. Focusing first on $\text{Log}(\text{Months as Judge})$, we estimate that the elasticity of *Duration* with respect to experience is -0.055 (based on Column (5)). Thus, being randomly assigned to a judge with twice as much time on the

²⁷ For outcomes that are indicator variables we use linear probability models.

bench would result in a 5.5% decrease in bankruptcy duration, a decline of nearly 1 month relative to the mean *Duration* of 16.5 months. Meanwhile, the coefficients on *First 2 Years* show that the impact of experience on duration is significantly higher at the beginning of a judge’s term: cases assigned to judges in their first two years have 18.05% longer durations, which corresponds to an increase of 2.99 months relative to the sample mean.²⁸ In contrast, *Log(Large)* and *First 2 Large* are insignificantly associated with *Duration*.

Table 5 presents the analysis of *Emergence*. We find that judges with more time on the bench realize significantly higher emergence rates. In terms of economic magnitudes, a one-standard-deviation increase in *Log(Months as Judge)* leads to a 2.96% increase in the probability that the firm emerges from Chapter 11 (rather than being liquidated), which corresponds to 5.15% of the sample mean (57%). For cases assigned to judges in the first two years of their terms, the emergence probability is 6.5% lower, which corresponds to 11.4% of the sample mean. *Log(Large)* and *First 2 Large* do not exhibit any significant association with *Emergence*.

In both Tables 4 and 5, experience measures based on time on the bench exhibit significant relations with case outcomes, whereas experience measures based on judicial experience with large cases are insignificantly associated with case outcomes. As noted in section 3.3, the majority of judges have significant on-the-bench experience by the time they have seen two large cases: the average judge has been on the bench 86 months after seeing his/her second large case (median 73 months). Our evidence suggests that time on the bench matters more than judicial experience with large cases. One possible explanation is that most judges seeing their first large case have already seen many smaller corporate bankruptcies and personal bankruptcies. From these other cases they have likely established past rulings which possibly allow them to manage large corporate bankruptcies more efficiently, despite their lack of experience with the complexities specific to large bankruptcies.

²⁸ Since this is a log-linear model with the independent variable of interest, *First 2 Years*, being a dummy variable, the estimated impact of moving from a judge with less than 2 years experience to more than 2 years is $100[\exp(\beta_1) - 1]$.

One concern with decreased time in bankruptcy is that more experienced judges might rush Chapter 11 cases through the reorganization process too quickly without enough due diligence. If the reorganization process is not carefully done and the firm is allowed to emerge, then the judge may be “kicking the can down the road.” To evaluate whether experienced judges are imposing costs on the reorganization process, we estimate the effect of judge experience on the likelihood that the emerged firm subsequently refiles for bankruptcy within three years. The results are presented in Table 6. We find no evidence that more experienced judges have higher refile rates. For each of the four experience measures, refile rates are statistically similar for experienced and inexperienced judges.

To examine the robustness of these findings with respect to prepackaged and prenegotiated cases, we present regression estimates in Table A3 that exclude prepackaged/prenegotiated cases. Table A3 delivers the same message as our main tests: more experienced judges process Chapter 11 cases significantly faster, their cases are more likely to emerge from Chapter 11, and the refile rates for their emerged cases are similar. Overall, the evidence suggests that as a judge accumulates more experience on the bench he/she becomes more efficient, with firms realizing shorter time in bankruptcy, higher likelihoods of emerging from bankruptcy, and similar probabilities of subsequently refile for bankruptcy.

We also examine the effect of different levels of on-the-bench experience to better understand how long it takes a judge to become “experienced.” We create a set of dummy variables indicating cases assigned to judges during their first two, four, six, and eight years. The results are presented in Table 7. In columns 1 through 4, the impact of experience on *Duration* displays a smooth declining trend, with the magnitude shrinking from 0.17 to 0.08 as judges’ experience increases from their first two years to their first eight years. All coefficients remain statistically significant, suggesting that even cases assigned to judges halfway through their 14-year term exhibit significantly longer time in bankruptcy relative to their more experienced colleagues. In columns 5 through 8 the effect of having a new judge on *Emergence* decreases from a 0.07 reduced emergence probability for cases seen during judges’

first two and four years, to a 0.03 reduced emergence probability for cases seen during judges' first six and eight years, with the estimates statistically insignificant after four years. In columns 9 through 12 the effect of experience on *Refile3Y* is statistically insignificant at all levels of experience. The evidence suggests that it takes at least four years for judges to manage bankruptcy cases in a fairly efficient manner.

4.3 Court Caseload

In this section we further examine the mechanisms driving our empirical findings by examining how court caseload affects the impact of judge experience on case outcomes. Because the number of judges in a court is fixed, when more firms and individuals file for bankruptcy—for example during economic recessions—workloads are higher for judges (Iverson 2017). During periods of elevated caseloads, judge experience may matter more if experienced judges are able to process cases more efficiently. Further, because court caseloads are typically high during periods of general economic distress, it is possible that these cases are harder to manage due to increased creditor conflicts and fewer outside options for asset sales. If this is the case, judge skill should matter more during these time periods. In contrast, if the effect of judge experience on case outcomes is driven by lawyer's learning about judges' styles, then the effect of judge's experience on case outcomes should not differ by caseload, since lawyers have incentives to adapt to the judge regardless of the court's caseload. Alternatively, if lawyers' efforts to learn the judge's style are constrained when there are a large number of bankruptcy cases for them to represent at a given time, we expect to see weaker effects of experience when the judge's caseload is high.²⁹

We measure caseload by the weighted number of bankruptcy filings in the court calendar quarter when a firm filed for Chapter 11. The weights come from Bermant, Lombard, and Wiggins (1991), who suggest specific hours that judges approximately spend on six distinct

²⁹ Even if the supply of top lawyers is limited, attorneys have incentives to maximize the wealth of their clients by working efficiently, regardless of the judge's caseload.

types of bankruptcy cases.³⁰ This weighted caseload measure can be interpreted as the number of hours (per year) the judge would spend administering the particular mix of six bankruptcy case types actually filed in his/her bankruptcy district. Table 8 presents results that split our full sample by median court caseload in the sample. *High group* includes cases with a court caseload above or equal to the median number, and *Low group* includes cases with caseload below the median value. We continue to include case controls and court and industry fixed effects. Standard errors are clustered by court.

Table 8 shows that the impact of judges' on-the-bench experience is more important when judges face above-median caseloads. In Panel A, judge experience, measured by *Log(Months as Judge)*, significantly reduces *Duration* and increases *Emergence* in the high caseload group, whereas the coefficients are not statistically significant for the low caseload group in columns (2) and (4). In Panel B, the coefficient on *First 2 Years* suggests significantly longer duration in column 1 for the high caseload group, but no difference in column 2 for the low caseload group. The evidence is consistent with experienced judges significantly reducing case duration while simultaneously allowing more firms to emerge, especially when the court is busy. Meanwhile, when caseloads are low we find insignificant differences between experienced and inexperienced judges. Our results are intuitive as judges are able to devote more efforts to individual cases when they have more time at their disposal.

4.4 Judges' Other Experiences and Personal Attributes

There is a growing literature that examines the importance of managers' general skills versus specific expertise for both corporate policies and managers' own compensation (see Murphy and Zbojnik (2006), Guner, Malmendier, and Tate (2008), Lazear (2009), Custodio, Ferreira, and Matos (2013), Custodio and Metzger (2014), for example). The evidence is inconclusive as to which skill matters more for human capital value. There is also an ongoing

³⁰ The six types are non-business Chapter 7, business Chapter 7, Chapter 11, Chapter 12, Chapter 13, and other (i.e., Chapter 9 and Chapter 15).

debate in the law and economics literature as to whether judges' personal attributes and political affiliations are related to their judicial decisions. For example, Ashenfelter, Eisenberg, and Schwab (1995) study case rulings by non-bankruptcy federal judges and find no evidence that judges' political affiliations or the political party of the president who appointed the judge affects their judicial decisions. In contrast, Rachlinski, Guthrie, and Wistrich (2006) find that bankruptcy judges exhibit behavioral (e.g. anchoring, framing) biases in their rulings and find correlations between judges' political affiliations and Chapter 11 outcomes. Moreover, the CEO literature suggests that certainly managerial traits and characteristics such as early life experience (Great Depression, military experience, for example) matter to corporate policies (Malmendier, Tate, and Yan 2011; Graham, Li, and Qiu 2012; Benmelech and Frydman 2015).

In this section, we compare the effects of judges' specific expertise (developed from time on the bench) and general skills (developed from prior work experience and education) as well as personal attributes on Chapter 11 outcomes to draw inferences on the relative importance of the skill types and personal traits. Specifically, we test whether judges' on-the-bench experience (i.e., specific expertise) matters after considering other individual factors, we rerun our main regression after including additional judge characteristics. We consider three proxies for judges' general experience (*Log(Years before Bench)*, *Top 5 Lawschool*, *Public Sector*) and three proxies for personal characteristics (*Male*, *Military*, *Democrat*). We include *Log(Months as Judge)* as a measure of specific expertise developed from time on the bench, and continue to include case controls and court and industry fixed effects. The results are presented in Table 9. Panels A, B, and C examine *Duration*, *Emergence*, and *Refile*, respectively, with column (1) in each panel depicting the baseline results from Tables 4, 5, and 6. In column (2) we add the three general experience measures, and in column (3) we add the three personal characteristics measures. In column (4) we include all judge variables.

In Table 9 we find that including these additional characteristics as controls does not

reduce the significance of our time-based experience measures. Across the three panels, both the economic magnitude and statistical significance of $\text{Log}(\text{Months as Judge})$ remains stable across the different specifications including these additional judge characteristics. More experienced judges move cases through Chapter 11 faster, are more likely to reorganize these firms rather than liquidating them, yet still have similar refile probabilities within three years after emergence, regardless of their personal characteristics.

This is not to say that judge personal characteristics do not matter. In particular, in Panels A and C we find that time in bankruptcy court is shorter and refile rates are lower on average when cases are assigned to male judges. The economic magnitude is fairly significant for this gender dummy, as it suggests that male judges process cases 15% faster, which corresponds to 2.47 months relative to the mean duration for large Chapter 11 filings, and reduce refile rates by 6.6%, which corresponds to nearly two thirds of the average refile rate in our sample. Military experience is associated with a 12.4% increased emergence probability. Aside from these characteristics, however, we do not find that personal characteristics or previous work experience have a large effect on case outcomes. These findings suggest that certain characteristics (such as gender) result in persistent judge biases, as documented in previous research (Dobbie and Song 2015; Chang and Schoar 2013; Bernstein, Colonnelli, and Iverson 2016; Bernstein, Colonnelli, Giroud, and Iverson 2017).

4.5 Debt Recovery

In this subsection we provide suggestive evidence regarding the welfare implications of judge experience for creditors by examining the effects of judge experience on recovery rates. We obtain debt recovery information from Moody's Default & Recovery Database (DRD). The data contain information from Moody's own proprietary database of issuers, defaults, and recoveries. One key advantage of the DRD database lies in its granularity, with information on each debt instrument's contract terms, principal default amount, and final recovery amount, allowing us to measure the final settlement value of each debt instrument. Moody's

uses this information to calculate a family recovery rate, defined as the enterprise value across all instruments of a corporation relative to that firm's total liabilities. In addition, the DRD contains unsecured recovery rates, which is the combined recovery rate on all unsecured debt. We link the DRD to our bankruptcy sample by matching company name and filing time. While Moody's DRD provides clean, market-based measurements of recovery rates, one downside is that recovery data is only available for debt instruments which are rated by Moody's. This leads to a significantly smaller sample size for our tests and, hence, significantly less statistical power in our regression analysis, given the large amount of fixed effects included relative to the sample size.

Table 10 presents both family recovery and unsecured recovery outcomes associated our time-based experience measures (*Log(Months as Judge)* and *First 2Years*). Across both experience measures, we find that higher experience leads to higher creditor recovery rates. However, none of the coefficient estimates are statistically significant, largely due to the reduced sample size in these regressions. In terms of economic significance, when we include all controls we estimate that creditors receive on average 4.88 percentage point lower recovery rates when they are assigned to judges in their first two years, a 9% decline from the mean recovery of 54.5%. However, magnitudes are smaller using *Log(Months as Judge)* as the experience measure, with a one-standard-deviation increase in experience corresponding to an increase in recovery rates of 0.83 percentage points. Point estimates are similar when using unsecured recovery as the dependent variable. However, average recoveries for unsecured creditors are only 28.7%, so the estimated effects are nearly twice as large relative to the mean.

We reiterate that none of these point estimates are statistically significant, so we cannot draw any strong conclusions from these estimates. Directionally, the coefficients are consistent with more experienced judges having a small positive effect on creditor recoveries. Importantly, however, we also point out that these estimates are inconsistent with the interpretation that more experienced judges move cases through bankruptcy more quickly but at

the expense of worse outcomes for creditors. Instead, similar to our findings of similar refile rates for experienced judges in Table 6, we find that experienced judges decrease duration and increase the likelihood of emergence without significantly higher or lower recovery rates.

5 Conclusion

This paper provides the first empirical study on the effect of judges' judicial experience on the restructuring outcomes of large US corporate bankruptcies. We first perform a series of tests to provide evidence consistent with large bankruptcy cases being randomly assigned to judges. Using random assignment for identification, we document the causal effect of judges' judicial experience on three major restructuring outcomes: time spent in bankruptcy, the probability of emergence, and recidivism. We find that firms assigned to more experienced judges spend less time in bankruptcy and are more likely to be kept as a going-concern, but are not more likely to refile for bankruptcy after emergence. Our heterogeneity tests show that judicial experience matters more when judges have large caseloads. We further document that judges' educational background, non-judicial experience, and personal attributes are not consistently related to bankruptcy outcomes and do not explain our findings. Our evidence collectively suggests that judges' specific skills developed on the bench matter more than general skills.

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Table 1 Summary Statistics

Panel A presents the summary statistics for sample major U.S. public Chapter 11 cases, including judge experience measures at case assignment, case characteristics, and final outcomes. Panel B summarizes judge characteristics, and Panel C shows the correlation matrix for our four judge experience measures.

Panel A: Case Characteristics

	N	Mean	Median	SD	P10	P90
Log(Months in Ch11)	1,310	2.40	2.52	0.97	1.01	3.53
Months in Ch11	1,310	16.54	12.48	15.38	2.75	34.03
Emergence	1,310	0.57	1.00	0.49	0.00	1.00
Refile 3Y	721	0.08	0.00	0.28	0.00	0.00
Log(Months as Judge)	1,288	4.32	4.58	1.14	2.81	5.47
Months as Judge	1,288	114.49	97.18	85.21	16.60	237.03
First 2 Years	1,288	0.14	0.00	0.34	0.00	1.00
Log (Large)	1,179	1.91	1.61	1.47	0.00	4.22
Large	1,179	20.40	5.00	34.36	1.00	68.00
First 2 Large	1,179	0.30	0.00	0.46	0.00	1.00
Assets (Mils)	1,310	2,001.83	488.61	5,011.32	119.72	3,995.94
Num filings	1,259	10.67	3.00	20.78	1.00	25.00
ROA filing	1,240	-0.24	-0.11	0.40	-0.61	0.02
Leverage filing	1,280	1.01	0.92	0.51	0.55	1.50
Prepack/Preneg	1,310	0.25	0.00	0.43	0.00	1.00
Delaware	1,310	0.29	0.00	0.46	0.00	1.00
NY SD	1,310	0.18	0.00	0.39	0.00	1.00

Panel B: Judge Characteristics

	N	Mean	Median	SD	P10	P90
Log(Years before Bench)	296	2.82	2.89	0.46	2.20	3.40
Years before Bench	297	18.49	18.00	7.79	8.00	30.00
Top 5 Law School	309	0.12	0.00	0.33	0.00	1.00
Male	309	0.79	1.00	0.41	0.00	1.00
Military	305	0.23	0.00	0.42	0.00	1.00
Public Sector	170	0.61	1.00	0.49	0.00	1.00
Democrat	206	0.63	1.00	0.48	0.00	1.00

Panel C: Correlation Matrix

	Log(Months as Judge)	First 2 Years	Log (Large)	First 2 Large
Log(Months as Judge)	1			
First 2 Years	-0.772	1		
Log (Large)	0.245	-0.235	1	
First 2 Large	-0.283	0.229	-0.717	1

Table 2 Randomization

This table presents linear regression estimates of judge assignment. The initial sample consists of all large (> \$50 million in assets at time of filing) public Chapter 11 bankruptcy cases filed in the United States between 1990 and 2012. We drop all cases assigned to a visiting judge, district judge, and cases filed in a court with only one large case. The resulting sample contains 1,174 cases from 60 courts and 350 unique judges. For each case, we identify the set of judges currently serving in the bankruptcy court where the case was filed using judges' appointment and retirement dates. The average number of available judges is 8.4 (min 1 max 20). The dependent variable, $Assigned_{i,t}$, is an indicator equal to one if judge i was assigned to a case filed in their court at time t , zero otherwise and has a mean of 0.17. We regress this assignment indicator on five separate measures of judge experience/activity: the log number of months the judge has been on the bench ($Log(Months\ as\ Judge)$), an indicator for the first two year's of a judge's tenure ($First\ 2\ Years$), the log of one plus the number of cases with more than \$50 million in assets at the time of filing previously assigned to the judge ($Log(Large)$), an indicator if the judge has seen two or fewer large cases ($First\ 2\ Large$), and the log of one plus the number of large cases currently assigned to the judge but not yet confirmed ($Log(Caseload)$). We set these experience measures as missing for all cases for which we do not have a judge experience measure for each eligible judge. Case fixed effects are included in each regression, standard errors are clustered by court, and *, **, *** indicate 10%, 5%, and 1% two-tailed statistical significance, respectively.

$$Assigned_{i,t} = \alpha + \beta_1 JudgeExp_{i,t} + \theta Case\ FE + \epsilon_{i,t}$$

	(1)	(2)	(3)	(4)	(5)
	Assigned	Assigned	Assigned	Assigned	Assigned
Log(Months as Judge)	0.003 (0.46)				
First 2 Years		-0.011 (-0.67)			
Log(Large)			0.008 (1.46)		
First 2 Large				-0.011 (-0.86)	
Log(Large Caseload)					0.013 (1.06)
Observations	6,523	6,523	4,876	4,876	6,799
Adj R-Squared	-0.08	-0.08	-0.11	-0.11	-0.09
Case FE	Yes	Yes	Yes	Yes	Yes

Table 3 Firm Characteristics

This table presents regression estimates of our four judge experience measures on firm characteristics upon filing for Chapter 11. Court and industry fixed effects are included in each regression. We also tabulate in the notes the adjusted R^2 from a specification that includes only court fixed effects. Standard errors are clustered at the court level, and *, **, *** indicate 10%, 5%, and 1% two-tailed statistical significance, respectively.

$$\text{Experience Measure} = \alpha + \beta_1 \text{Firm Characteristics} + \delta \text{Industry FE} + \theta \text{Court FE} + \epsilon_{i,t}$$

	Log(Months as Judge)	First 2 Years	Log(Large)	First 2 Large
ln(Assets)	0.027 (1.21)	0.001 (0.09)	-0.002 (-0.08)	-0.019** (-2.46)
Num filings	-0.001* (-1.81)	0.000 (0.50)	0.002* (1.82)	-0.000 (-0.55)
Leverage filing	0.075 (0.72)	-0.006 (-0.20)	0.034 (0.42)	0.007 (0.26)
ROA filing	0.154 (1.18)	-0.040 (-1.04)	0.284** (2.37)	-0.042 (-0.82)
Prepack/Preneg	0.249*** (4.07)	-0.042** (-2.66)	0.200** (2.23)	-0.047 (-1.48)
Observations	1,153	1,153	1,051	1,051
Adj R-Squared	0.07	0.02	0.59	0.33
Court FE	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes
Adj R2 w/o Controls	0.05	0.00	0.59	0.32

Table 4 Bankruptcy Duration

This table presents regression estimates of case duration, measured by the log number of months a case spends under Chapter 11, on our four judge experience measures. Court and industry fixed effects are included in each regression. Standard errors are clustered at the court level, and *, **, *** indicate 10%, 5%, and 1% two-tailed statistical significance, respectively.

$$\text{Duration} = \alpha + \beta_1 \text{JudgeExp}_{i,t} + \beta_2 \text{Controls} + \delta \text{Industry FE} + \theta \text{Court FE} + \epsilon_{i,t}$$

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Log(Months as Judge)	First 2 Years	Log(Large)	First 2 Large	Log(Months as Judge)	First 2 Years	Log(Large)	First 2 Large
exp_measure	-0.099*** (-6.44)	0.215*** (3.17)	-0.038 (-0.73)	-0.006 (-0.08)	-0.055*** (-4.29)	0.166** (2.41)	-0.001 (-0.02)	-0.065 (-0.86)
Time Trend	-0.025** (-2.60)	-0.028*** (-2.74)	-0.034*** (-4.87)	-0.037*** (-4.27)	-0.023*** (-3.34)	-0.024*** (-3.43)	-0.030*** (-4.50)	-0.032*** (-4.43)
Post-BAPCPA	0.047 (0.75)	0.070 (1.08)	0.077 (1.10)	0.098 (1.47)	0.116** (2.17)	0.127** (2.25)	0.155** (2.60)	0.170*** (2.83)
ln(Assets)					0.085*** (4.35)	0.083*** (4.24)	0.093*** (5.50)	0.092*** (5.55)
Num filings					0.044*** (3.49)	0.044*** (3.38)	0.033** (2.64)	0.032** (2.59)
Leverage filing					-0.146** (-2.63)	-0.150** (-2.66)	-0.140*** (-2.68)	-0.140*** (-2.74)
ROA filing					-0.111** (-2.64)	-0.114*** (-2.78)	-0.126** (-2.44)	-0.129** (-2.20)
Prepack/Preneg					-1.191*** (-17.98)	-1.197*** (-17.63)	-1.133*** (-17.36)	-1.135*** (-18.28)
Adjusted R^2	0.08	0.07	0.07	0.07	0.41	0.41	0.38	0.38
Observations	1,274	1,274	1,163	1,163	1,153	1,153	1,051	1,051
Industry FE	No	No	No	No	Yes	Yes	Yes	Yes
Court FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Table 5 Emergence

This table presents linear probability regression estimates of a dummy variable indicating a firm emerges from Chapter 11 on our four judge experience measures. Court and industry fixed effects are included in each regression. Standard errors are clustered at the court level, and *, **, *** indicate 10%, 5%, and 1% two-tailed statistical significance, respectively.

$$\text{Emergence Dummy} = \alpha + \beta_1 \text{JudgeExp}_{i,t} + \beta_2 \text{Controls} + \delta \text{Industry FE} + \delta \text{Court FE} + \epsilon_{i,t}$$

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Log(Months as Judge)	First 2 Years	Log(Large)	First 2 Large	Log(Months as Judge)	First 2 Years	Log(Large)	First 2 Large
exp_measure	0.041*** (3.66)	-0.062** (-2.25)	0.025** (2.10)	-0.037 (-1.17)	0.026** (2.05)	-0.065* (-1.68)	0.004 (0.43)	0.020 (0.46)
Time Trend	-0.018*** (-4.11)	-0.017*** (-3.69)	-0.013*** (-4.09)	-0.012*** (-3.72)	-0.020*** (-5.74)	-0.019*** (-5.59)	-0.015*** (-4.71)	-0.014*** (-4.42)
Post-BAPCPA	0.122** (2.40)	0.112** (2.18)	0.090** (2.03)	0.085** (2.04)	0.077 (1.46)	0.072 (1.34)	0.049 (1.10)	0.042 (1.04)
ln(Assets)					0.060*** (4.89)	0.061*** (4.95)	0.064*** (4.51)	0.065*** (4.51)
Num filings					0.023** (2.48)	0.022** (2.62)	0.021* (1.96)	0.021* (1.93)
Leverage filing					0.154*** (6.16)	0.156*** (6.59)	0.157*** (6.69)	0.158*** (6.83)
ROA filing					0.046 (0.86)	0.048 (0.92)	0.060 (1.12)	0.062 (1.20)
Prepack/Preneg					0.296*** (14.25)	0.299*** (14.52)	0.305*** (16.03)	0.306*** (16.30)
Adjusted R^2	0.06	0.05	0.04	0.04	0.20	0.20	0.18	0.19
Observations	1,274	1,274	1,163	1,163	1,153	1,153	1,051	1,051
Industry FE	No	No	No	No	Yes	Yes	Yes	Yes
Court FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Table 6 Refile Within 3 Years

This table presents linear probability regression estimates of a dummy variable indicating a firm refiles for Chapter 11 within 3 years after emergence on our four judge experience measures. Court and industry fixed effects are included in each regression. Standard errors are clustered at the court level, and *, **, *** indicate 10%, 5%, and 1% two-tailed statistical significance, respectively.

$$\text{Refile within 3Y} = \alpha + \beta_1 \text{JudgeExp}_{i,t} + \beta_2 \text{Controls} + \delta \text{Industry FE} + \theta \text{Court FE} + \epsilon_{i,t}$$

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Log(Months as Judge)	First 2 Years	Log(Large)	First 2 Large	Log(Months as Judge)	First 2 Years	Log(Large)	First 2 Large
exp_measure	0.010 (1.32)	0.015 (0.47)	0.014*** (2.85)	-0.044 (-1.67)	0.010 (0.97)	0.011 (0.29)	0.007 (1.08)	-0.020 (-0.64)
Time Trend	-0.004 (-1.22)	-0.003 (-1.04)	-0.003 (-1.31)	-0.003 (-1.32)	-0.005 (-1.17)	-0.004 (-1.07)	-0.003 (-1.26)	-0.003 (-1.25)
Post-BAPCPA	0.023 (0.66)	0.020 (0.58)	0.021 (0.64)	0.022 (0.66)	0.034 (0.86)	0.032 (0.81)	0.029 (0.89)	0.028 (0.89)
ln(Assets)					-0.008 (-1.41)	-0.008 (-1.37)	-0.008 (-1.31)	-0.008 (-1.36)
Num filings					0.012 (1.43)	0.012 (1.48)	0.016* (1.82)	0.016* (1.85)
Leverage filing					0.036 (1.31)	0.038 (1.36)	0.048* (1.74)	0.049* (1.79)
ROA filing					0.014 (0.51)	0.016 (0.59)	0.035 (1.02)	0.036 (1.09)
Prepack/Preneg					0.046* (1.72)	0.048* (1.79)	0.045* (1.88)	0.046* (1.94)
Adjusted R^2	-0.01	-0.01	-0.01	-0.01	0.02	0.01	0.03	0.04
Observations	697	697	608	608	624	624	542	542
Industry FE	No	No	No	No	Yes	Yes	Yes	Yes
Court FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Table 7 Learning Curve

This table presents regression estimates of judge experience measures indicating first two years, first four years, first six years, and first eight years on case outcomes. Three outcome variables include the log number of months a case is under Chapter 11 (*Duration*) in columns (1)-(4); a dummy variable indicating a firm emerges from Chapter 11 (*Emergence*) in columns (5)-(8); and a dummy variable indicating a firm refiles for Chapter 11 within 3 years after emergence in columns (9)-(12). Court and industry fixed effects are included in each regression. Standard errors are clustered at the court level, and *, **, *** indicate 10%, 5%, and 1% two-tailed statistical significance, respectively.

$$\text{Outcomes} = \alpha + \beta_1 \text{JudgeExp}_{i,t} + \beta_2 \text{Controls} + \delta \text{Industry FE} + \theta \text{Court FE} + \epsilon_{i,t}$$

	Duration				Emergence				Refile3Y			
	(1) First2Y	(2) First4Y	(3) First6Y	(4) First8Y	(5) First2Y	(6) First4Y	(7) First6Y	(8) First8Y	(9) First2Y	(10) First4Y	(11) First6Y	(12) First8Y
exp_measure	0.17** (2.41)	0.17*** (4.82)	0.12*** (3.91)	0.08** (2.21)	-0.07* (-1.68)	-0.06* (-1.82)	-0.03 (-1.32)	-0.03 (-0.79)	0.01 (0.29)	-0.01 (-0.42)	-0.02 (-0.85)	-0.03 (-1.32)
ln(Assets)	0.08*** (4.24)	0.09*** (4.38)	0.09*** (4.40)	0.08*** (4.28)	0.06*** (4.95)	0.06*** (4.84)	0.06*** (4.85)	0.06*** (4.88)	-0.01 (-1.37)	-0.01 (-1.43)	-0.01 (-1.42)	-0.01 (-1.36)
Num filings	0.04*** (3.38)	0.04*** (3.41)	0.04*** (3.56)	0.04*** (3.64)	0.02** (2.62)	0.02** (2.49)	0.02** (2.45)	0.02** (2.43)	0.01 (1.48)	0.01 (1.47)	0.01 (1.44)	0.01 (1.43)
Leverage filing	-0.15** (-2.66)	-0.15** (-2.67)	-0.15*** (-2.68)	-0.15** (-2.63)	0.16*** (6.59)	0.16*** (6.43)	0.16*** (6.48)	0.16*** (6.50)	0.04 (1.36)	0.04 (1.35)	0.04 (1.34)	0.04 (1.31)
ROA filing	-0.11*** (-2.78)	-0.11** (-2.55)	-0.12** (-2.54)	-0.12*** (-2.69)	0.05 (0.92)	0.05 (0.88)	0.05 (0.90)	0.05 (0.91)	0.02 (0.59)	0.02 (0.57)	0.01 (0.53)	0.01 (0.49)
Time Trend	-0.02*** (-3.43)	-0.02*** (-3.27)	-0.02*** (-3.33)	-0.02*** (-3.55)	-0.02*** (-5.59)	-0.02*** (-5.39)	-0.02*** (-5.55)	-0.02*** (-5.36)	-0.00 (-1.07)	-0.00 (-1.09)	-0.00 (-1.15)	-0.00 (-1.21)
Post-BAPCPA	0.13** (2.25)	0.11** (2.04)	0.11** (2.08)	0.12** (2.18)	0.07 (1.34)	0.08 (1.42)	0.08 (1.43)	0.08 (1.41)	0.03 (0.81)	0.03 (0.80)	0.03 (0.86)	0.03 (0.91)
Prepack/Preneg	-1.20*** (-17.63)	-1.19*** (-18.14)	-1.20*** (-18.16)	-1.20*** (-18.17)	0.30*** (14.52)	0.30*** (14.51)	0.30*** (14.60)	0.30*** (14.48)	0.05* (1.79)	0.05* (1.76)	0.05* (1.74)	0.05* (1.76)
Observations	1,153	1,153	1,153	1,153	1,153	1,153	1,153	1,153	624	624	624	624
Adj R-Squared	0.41	0.41	0.41	0.40	0.20	0.20	0.20	0.20	0.01	0.01	0.02	0.02
Industry FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Court FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Table 8 Sample Splitting Case Load

This table presents regression estimates of experience measures on three outcome variables by splitting the sample according to bankruptcy court case load at filing. High (H) group include cases with total case load above the median, and Low (L) include cases with case load below the median. Three outcome variables include: the log number of months a case spends under Chapter 11 (*Duration*), a dummy variable indicating a firm emerges from Chapter 11 (*Emergence*), and a dummy variable indicating a firm refiles for Chapter 11 within 3 years after emergence (*Refile3Y*). We use our two time-based experience measures (*Log(Months as Judge)*) and *First 2 Years*). Standard errors are clustered at the court level, and *, **, *** indicate 10%, 5%, and 1% two-tailed statistical significance, respectively.

$$\text{Outcome} = \alpha + \beta_1 \text{experience} + \beta_2 \text{Controls} + \delta \text{Industry FE} + \theta \text{Court FE} + \epsilon_{i,t}$$

Panel A: Ln(Months as Judge)

	Duration		Emergence		Refile3Y	
	(1)	(2)	(3)	(4)	(5)	(6)
	High	Low	High	Low	High	Low
Log(Months as Judge)	-0.062*** (-2.87)	-0.030 (-0.97)	0.028** (2.71)	0.020 (1.19)	0.015 (0.80)	0.000 (0.02)
Observations	572	565	572	565	308	302
Adjusted R^2	0.36	0.46	0.21	0.23	0.04	0.00
Controls	Yes	Yes	Yes	Yes	Yes	Yes
FE	Yes	Yes	Yes	Yes	Yes	Yes

Panel B: First Two Year

	Duration		Emergence		Refile3Y	
	(1)	(2)	(3)	(4)	(5)	(6)
	High	Low	High	Low	High	Low
First 2 Years	0.213*** (3.47)	0.091 (0.56)	-0.046 (-0.99)	-0.061 (-1.49)	0.026 (0.48)	-0.007 (-0.09)
Observations	572	565	572	565	308	302
Adjusted R^2	0.36	0.46	0.20	0.23	0.04	0.00
Controls	Yes	Yes	Yes	Yes	Yes	Yes
FE	Yes	Yes	Yes	Yes	Yes	Yes

Table 9 Other Characteristics vs Ln(Months as Judge)

This table presents horse race regression estimates of judge experience, measured by the log number of months the judge has been on the bench ($Ln(Months\ as\ Judge)$), versus other judge characteristics on bankruptcy outcomes. Three outcome variables include: the log number of months a case is under Chapter 11 (*Duration*), a dummy variable indicating a firm emerges from Chapter 11 (*Emergence*), and a dummy variable indicating a firm refiles for Chapter 11 within 3 years after emergence (*Refile3Y*). Other judge Characteristics include: a dummy variable indicating male (*Male*), a dummy variable indicating top 5 law school (*Top5Lawschool*), a dummy variable indicating military service experience (*Military*), a dummy variable indicating public sector work experience (*PublicSector*), a dummy variable indicating Democrat affiliation (*Democrat*), and age appointed as a bankruptcy judge (*AgeAppointed*). Court and industry fixed effects are included in each regression. Standard errors are clustered at the court level, and *, **, *** indicate 10%, 5%, and 1% two-tailed statistical significance, respectively.

$$\text{Outcome} = \alpha + \beta \text{Experience} + \beta_1 \text{Characteristics} + \beta_2 \text{Controls} + \delta \text{FEs} + \epsilon_{i,t}$$

Panel A: Duration				
	(1)	(2)	(3)	(4)
Log(Months as Judge)	-0.053*** (-4.17)	-0.060*** (-3.90)	-0.051*** (-4.52)	-0.048*** (-3.46)
Log(Years before Bench)		0.012 (0.23)		0.096 (1.36)
Top5 Lawschool		0.030 (0.29)		0.020 (0.19)
Public Sector		-0.020 (-0.18)		-0.023 (-0.28)
Male			-0.151** (-2.34)	-0.195*** (-3.36)
Military			0.042 (0.68)	-0.052 (-0.50)
Democrats			0.006 (0.09)	-0.038 (-0.55)
Observations	1147	925	1146	925
Adjusted R^2	0.41	0.39	0.41	0.39
Controls	Yes	Yes	Yes	Yes
FE	Yes	Yes	Yes	Yes

Panel B: Emergence				
	(1)	(2)	(3)	(4)
Log(Months as Judge)	0.024* (1.88)	0.030* (1.90)	0.020 (1.61)	0.026* (1.74)
Log(Years before Bench)		0.022 (0.67)		0.013 (0.55)
Top5 Lawschool		0.036 (0.92)		0.021 (0.52)
Public Sector		-0.014 (-0.27)		-0.011 (-0.22)
Male			-0.017 (-0.49)	-0.005 (-0.17)
Military			0.124*** (3.05)	0.123*** (3.04)
Democrats			0.029 (0.64)	-0.003 (-0.06)
Observations	1147	925	1146	925
Adjusted R^2	0.20	0.23	0.20	0.23
Controls	Yes	Yes	Yes	Yes
FE	Yes	Yes	Yes	Yes

Panel C: Refile within 3Y

	(1)	(2)	(3)	(4)
Log(Months as Judge)	0.011 (1.13)	0.005 (0.63)	0.010 (1.30)	0.004 (0.65)
Log(Years before Bench)		-0.080*** (-2.84)		-0.057** (-2.64)
Top5 Lawschool		0.021 (0.88)		0.007 (0.24)
Public Sector		0.021 (0.47)		0.015 (0.36)
Male			-0.066** (-2.69)	-0.060 (-1.69)
Military			-0.003 (-0.09)	0.055 (1.11)
Democrats			-0.024 (-0.77)	0.005 (0.17)
Observations	620	500	620	500
Adjusted R^2	0.02	0.04	0.02	0.04
Controls	Yes	Yes	Yes	Yes
FE	Yes	Yes	Yes	Yes

Table 10 Recovery Outcome

This table presents OLS regression estimates of recovery rate on two judge experience measures: the log number of months the judge has been on the bench ($Ln(MonthsasJudge)$) and an indicator for the first two year's of a judge's tenure ($First2Y$). Columns(1)-(4) report results with family recovery as the outcomes and Columns(5)-(8) report unsecured recovery as the key outcome variable. Court and industry fixed effects are included in each regression. Standard errors are clustered at the court level, and *, **, *** indicate 10%, 5%, and 1% two-tailed statistical significance, respectively.

$$\text{Recovery} = \alpha + \beta_1 \text{JudgeExp}_{i,t} + \beta_2 \text{Controls} + \delta \text{Industry FE} + \theta \text{Court FE} + \epsilon_{i,t}$$

	Family Recovery				Unsecured Recovery			
	(1) Log(Months as Judge)	(2) First 2Y	(3) Log(Months as Judge)	(4) First 2Y	(5) Log(Months as Judge)	(6) First 2Y	(7) Log(Months as Judge)	(8) First 2Y
exp_measure	1.876 (1.10)	-6.762* (-1.97)	0.729 (0.45)	-4.879 (-1.46)	1.691 (0.87)	-7.757* (-1.85)	0.289 (0.15)	-5.680 (-1.09)
Time Trend	0.649* (1.93)	0.686* (1.90)	0.688 (1.68)	0.687 (1.64)	0.058 (0.18)	0.097 (0.29)	-0.305 (-1.05)	-0.314 (-1.06)
Post-BAPCPA	-0.918 (-0.26)	-1.615 (-0.43)	-1.356 (-0.22)	-1.697 (-0.28)	-2.544 (-0.60)	-3.428 (-0.85)	-0.513 (-0.08)	-0.879 (-0.14)
ln(Assets)			-1.520 (-1.51)	-1.551 (-1.63)			1.114 (0.78)	1.076 (0.79)
Num filings			-1.616 (-0.91)	-1.574 (-0.90)			-1.344 (-0.70)	-1.378 (-0.74)
Leverage filing			-1.900 (-0.36)	-1.768 (-0.35)			-1.541 (-0.20)	-1.605 (-0.22)
ROA filing			-0.022 (-0.00)	-0.374 (-0.04)			-4.388 (-0.38)	-4.874 (-0.41)
Prepack/Preneg			5.581*** (3.33)	5.580*** (3.11)			13.370*** (7.53)	13.219*** (7.77)
Observations	454	454	414	414	368	368	334	334
Adj R-Squared	-0.00	0.00	0.04	0.04	0.02	0.02	0.10	0.11
Industry FE	No	No	Yes	Yes	No	No	Yes	Yes
Court FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Appendix

Variable Definitions

Experience Measure	
Log(Months as Judge)	Log(number of months from a judge's appointment date to the filing date of a case)
First 2 Years	A dummy=1 for the first two years a judge's term
Log(Large)	Log (number of large cases (\geq \$50 Mil at filing) a judge has seen)
First 2 Cases	A dummy=1 for the first two cases in the sample for a judge
<hr/>	
Judge Characteristics	
Age Appointed	Age when appointed as a bankruptcy judge
Male	A dummy variable =1 for male judge
Military	A dummy variable=1 for judges with military service before bankruptcy judgeship
Public Sector	A dummy variable=1 for judges with public sector experience before bankruptcy judgeship
Top 5 Law School	A dummy variable =1 if a law school is ranked in the top 5 according to 2009 U.S. News
<hr/>	
Case Characteristics	
Log(Assets)	Log of assets dollar value at filing (in 2016 dollars)
Duration	Log(number of months a case spent in Chapter 11)
Emergence	A dummy variable =1 for firms emerged from Chapter 11
Num filings	Number of filings associated with a case at filing
Family Recovery	The enterprise value across all claims relative to that firm's total liabilities.
Unsecured Recovery	Combined recovery rate for unsecured claims
Lev Filing	$\frac{liabilities}{Assets}$ at filing
Prepack/Preneg	A dummy variable=1 for a prepackaged or prenegotiated case
Post BAPCPA	A dummy variable=1 for cases filed after the Bankruptcy Abuse Prevention and Consumer Protection Act of 2005 (BAPCPA)
Refile 3Y	A dummy variable=1 if a firm refiles for Chapter 11 within 3 years after emergence
ROA Filing	$\frac{NetIncome}{Assets}$ at filing
Time Trend	Year of filing -1980 (beginning year of Lopucki data)

Table A1 Randomization Robustness Tests

This table presents robustness tests of judge random assignment (see Table 2 for details of sample construction). We estimate the linear probability model below on subsamples of the entire population of courts and cases analyzed in Table 2. Specifically, we examine whether judge experience affects the likelihood of being assigned a case for the following seven subsamples: cases that were not filed as a prepack (*No Prepack*, column 1); cases with total assets (current dollars) larger than or equal to \$500 million at time of filing (*Large*, column 2); cases with total assets less than \$500 million at time of filing (*Small*, column 3); cases filed in either the southern district of New York or Delaware (*NYSD/DE*, column 4); cases filed in just Delaware (*DE*, column 5); cases filed in just the NYSD (*NYSD*, column 6); and cases filed in a court other than Delaware or NYSD (*Other*, column 7). The judge experience measure is the log number of months the judge has been on the bench in Panel A (*Log(Months as Judge)*), an indicator for the first two year’s of a judge’s tenure in Panel B (*First 2 Years*), the log number of large cases previously assigned to the judge (*Log(Large)*) an indicator if the judge has previously seen 2 or fewer cases in Panel C (*First 2 Large*), and the log number of large cases currently assigned to the judge but not yet confirmed (*Log(LargeCaseload)*). Case fixed effects are included in each regression, standard errors are clustered by court (or use robust standard errors when analyzing a subset of courts), and *, **, *** indicate 10%, 5%, and 1% two-tailed statistical significance, respectively. The average dependent variable (likelihood a judge is assigned a case) is tabulated in the table footnotes.

$$\text{Assigned}_{i,t} = \alpha + \beta_1 \text{JudgeExp}_{i,t} + \theta \text{Case FE} + \epsilon_{i,t}$$

Panel A

	(1) No Prepack	(2) Large	(3) Small	(4) NYSD/DE	(5) DE	(6) NYSD	(7) Other
Log(Months as Judge)	0.001 (0.14)	-0.001 (-0.07)	0.006 (1.14)	-0.006 (-0.75)	0.006 (0.33)	-0.010 (-1.25)	0.013 (1.56)
Observations	5,624	3,142	3,381	3,098	1,197	1,901	3,425
Adj R-Squared	-0.08	-0.08	-0.08	-0.07	-0.22	-0.12	-0.09
Case FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Avg Dep Var	0.17	0.17	0.17	0.18	0.29	0.11	0.16

Panel B

	(1) No Prepack	(2) Large	(3) Small	(4) NYSD/DE	(5) DE	(6) NYSD	(7) Other
First 2 Years	-0.010 (-0.53)	0.004 (0.16)	-0.025 (-1.45)	0.009 (0.40)	-0.004 (-0.07)	0.014 (0.62)	-0.038 (-1.24)
Observations	5,624	3,142	3,381	3,098	1,197	1,901	3,425
Adj R-Squared	-0.08	-0.08	-0.08	-0.07	-0.22	-0.12	-0.09
Case FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Avg Dep Var	0.17	0.17	0.17	0.18	0.29	0.11	0.16

Randomization Robustness Tests (cont)

Panel C

	(1) No Prepack	(2) Large	(3) Small	(4) NYSD/DE	(5) DE	(6) NYSD	(7) Other
Log(Large)	0.006 (0.79)	0.009 (1.10)	0.008 (1.12)	0.004 (0.52)	-0.006 (-0.46)	0.010 (1.25)	0.025 (1.60)
Observations	4,157	2,537	2,339	2,884	1,103	1,781	1,992
Adj R-Squared	-0.11	-0.10	-0.13	-0.07	-0.20	-0.11	-0.16
Case FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Avg Dep Var	0.17	0.17	0.17	0.18	0.29	0.11	0.16

Panel D

	(1) No Prepack	(2) Large	(3) Small	(4) NYSD/DE	(5) DE	(6) NYSD	(7) Other
First 2 Large	-0.008 (-0.53)	-0.024 (-1.06)	0.002 (0.10)	-0.020 (-0.98)	0.006 (0.07)	-0.024 (-1.17)	-0.004 (-0.21)
Observations	4,157	2,537	2,339	2,884	1,103	1,781	1,992
Adj R-Squared	-0.11	-0.10	-0.13	-0.07	-0.20	-0.11	-0.17
Case FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Avg Dep Var	0.17	0.17	0.17	0.18	0.29	0.11	0.16

Panel E

	(1) No Prepack	(2) Large	(3) Small	(4) NYSD/DE	(5) DE	(6) NYSD	(7) Other
Log(Large Caseload)	0.010 (0.73)	0.012 (0.88)	0.013 (0.93)	-0.002 (-0.13)	-0.061 (-1.36)	0.012 (0.94)	0.033 (1.63)
Observations	5,878	3,258	3,541	3,098	1,197	1,901	3,701
Adj R-Squared	-0.09	-0.08	-0.09	-0.07	-0.22	-0.12	-0.10
Case FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Avg Dep Var	0.17	0.17	0.17	0.18	0.29	0.11	0.16

Table A2 Randomization Dropping Delaware Pre-2006

This table presents linear regression estimates of judge assignment. We restrict the sample from Table 2 to exclude all cases filed in Delaware before 2006 when there were only two official bankruptcy judgeships. Case fixed effects are included in each regression, standard errors are clustered by court, and *, **, *** indicate 10%, 5%, and 1% two-tailed statistical significance, respectively.

$$\text{Assigned}_{i,t} = \alpha + \beta_1 \text{JudgeExp}_{i,t} + \theta \text{Court FE} + \epsilon_{i,t}$$

	(1)	(2)	(3)	(4)	(5)
	Assigned	Assigned	Assigned	Assigned	Assigned
Log(Months as Judge)	0.003 (0.50)				
First 2 Years		-0.012 (-0.66)			
Log(Large)			0.009* (1.69)		
First 2 Large				-0.011 (-0.93)	
Log(Large Caseload)					0.016 (1.47)
Observations	6,117	6,117	4,564	4,564	6,393
Adj R-Squared	-0.11	-0.11	-0.13	-0.13	-0.11
Case FE	Yes	Yes	Yes	Yes	Yes

Table A3 Robustness Check: No Prepackaged/Prenegotiated Cases

This table presents regression estimates of judge experience measures on case outcomes by excluding pre-package and pre-negotiate cases from the sample. Three outcome variables include: the log number of months a case is under Chapter 11 (*Duration*) in columns (1)-(3), a dummy variable indicating a firm emerges from Chapter 11 (*Emergence*) in columns (4)-(6), and a dummy variable indicating a firm refiles for Chapter 11 within three years after emergence (*Refile3Y*) in columns (7)-(9). Four judge experience measures include: the log number of months the judge has been on the bench (*Log(MonthsasJudge)*), an indicator for the first two year's of a judge's tenure (*First2Years*), the log number of large chapter 11 cases the judge has previously seen (*Log(Large)*), and an indicator for the first two large cases a judge has seen (*First2Large*). Court and industry fixed effects are included in each regression. Standard errors are clustered at the court level, and *, **, *** indicate 10%, 5%, and 1% two-tailed statistical significance, respectively.

$$\text{Outcome} = \alpha + \beta_1 \text{JudgeExp}_{i,t} + \beta_2 \text{Controls} + \delta \text{Industry FE} + \theta \text{Court FE} + \epsilon_{i,t}$$

	Duration		Emergence		Refile3Y	
	(1) Log(Months as Judge)	(2) First 2 Years	(3) Log(Months as Judge)	(4) First 2 Years	(5) Log(Months as Judge)	(6) First 2 Years
exp_measure	-0.056*** (-3.73)	0.145*** (3.18)	0.036** (2.02)	-0.088** (-2.33)	0.009 (0.70)	-0.013 (-0.28)
ln(Assets)	0.120*** (6.34)	0.118*** (6.32)	0.062*** (4.37)	0.064*** (4.40)	0.003 (0.34)	0.003 (0.35)
Num filings	0.015 (0.87)	0.014 (0.87)	0.022* (1.69)	0.023* (1.80)	-0.003 (-0.32)	-0.003 (-0.29)
Leverage filing	-0.116** (-2.38)	-0.120** (-2.52)	0.140*** (2.80)	0.143*** (3.03)	0.002 (0.09)	0.003 (0.13)
ROA filing	-0.045 (-0.69)	-0.053 (-0.80)	-0.004 (-0.04)	0.002 (0.02)	-0.051** (-2.12)	-0.050* (-2.02)
Time Trend	-0.033*** (-6.96)	-0.034*** (-7.15)	-0.021*** (-5.08)	-0.020*** (-4.86)	-0.004 (-1.34)	-0.004 (-1.23)
Post-BAPCPA	0.163** (2.43)	0.173** (2.61)	0.074 (1.31)	0.068 (1.17)	0.001 (0.03)	-0.001 (-0.02)
Observations	853	853	853	853	395	395
Adj R-Squared	0.18	0.18	0.14	0.14	0.01	0.01
Industry FE	Yes	Yes	Yes	Yes	Yes	Yes
Court FE	Yes	Yes	Yes	Yes	Yes	Yes