

Practice Makes Perfect: Judge Experience and Bankruptcy Outcomes *

Benjamin Iverson[†] Joshua Madsen[‡] Wei Wang[§] Qiping Xu[¶]

June 20, 2018

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. Heterogeneity tests suggest that exposure to a higher percentage of business filings and a greater diversity of business filings accelerates judges' learning curve, and 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 specific skills relative to general skills for major corporate bankruptcies and contributes to research on investment in human capital and learning by doing.

Keywords: Bankruptcy judges, human capital, learning by doing

JEL: G33, G34, J24

*We thank Judge Shelley Chapman (United States Bankruptcy Court, Southern District of New York) and Chief Judge Brendan Shannon (United States Bankruptcy Court, District of Delaware) for institutional knowledge on judge assignments and rulings. We are grateful for comments from Jan Zabochnik. We thank Carmen Chen, Mitch Schinbein, Vania Shi, Erin Shin, and Shimei Zhou for excellent research assistance. All errors are our own.

[†]Brigham Young University, biverson@byu.edu

[‡]University of Minnesota, jmmadsen@umn.edu

[§]Queen's University, wwang@queensu.ca

[¶]University of Notre Dame, qxu1@nd.edu

1 Introduction

Bankruptcy judges possess large amounts of discretion in interpreting the bankruptcy law and ruling on all major actions undertaken by firms in bankruptcy, including compensating managers and professionals, granting post-petition financing, approving asset sales and liquidation, resolving creditor valuation disputes, and confirming reorganization plans.¹ Because of this discretion, judges have a large effect on bankruptcy outcomes (Bris, Welch, and Zhu 2006; Gennaioli and Rossi 2010; Chang and Schoar 2013). However, little is known about what influences judges' decision-making.

Bankruptcy judges typically have many years of prior work experience, often including relevant work experience as attorneys and/or clerks in bankruptcy courts. This familiarity with the court system suggests that new bankruptcy judges already have significant *general* experience. New judges, with 14-year terms, also have incentives to invest in *job-specific* skills so they can efficiently process cases. We study how the accumulation of job-specific human capital by bankruptcy judges influences their efficiency in handling large corporate bankruptcy filings, and the relative importance of judges' general vs. specific human capital.

Our comprehensive sample consists 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 compile information on judicial experience, previous professional experience, educational background, and personal characteristics from an array of sources including the U.S. courts system, LinkedIn, LexisNexis personal reports, press releases, and voting records. Our primary measure of job-specific experience is the time a judge has served on the bench as of the filing date of a large Chapter 11 case.

¹ Dahiya, John, Puri, and Ramirez (2003), Sharfman (2005), Li and Wang (2016), Ayotte and Morrison (2017), Bernstein, Colonnelli, and Iverson (2017), Bernstein, Colonnelli, Giroud, and Iverson (2017), Goyal and Wang (2017).

An important characteristic of bankruptcy courts is that bankruptcy judges are randomly assigned to cases, which allows us to identify the effect of judicial experience on case outcomes. We first provide empirical evidence consistent with random assignment of large Chapter 11 cases,² and next exploit this random assignment to examine the causal effect of judges' on-the-bench experience on three aspects of the case outcome: duration of the bankruptcy, 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 the efficiency of bankruptcy judges. We include firm-level controls and both court and industry fixed effects in all empirical specifications.

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; Bris, Welch, and Zhu 2006). Similarly, a one-standard-deviation increase in the judge's time on the bench leads to a 3% increase in the probability that a firm emerges from bankruptcy, which corresponds to 5.15% of the sample average (57%). We estimate that it takes up to four years until a new judge has similar case durations and emergence probabilities as more experienced judges.

One possible explanation for these findings is that longer-tenured judges process cases faster and become more lenient at the cost of lower quality restructuring. However, we find that cases assigned to more experienced judges do not have higher bankruptcy refiling rates

² Specifically, we show that the likelihood a judge is assigned to a case is unrelated to the judge's past experience or existing caseload, that there is little correlation between the assigned judge's level of experience and firm characteristics, and that firm characteristics have almost no explanatory power for judge experience.

or lower debt recovery rates, which is inconsistent with this interpretation. Instead, the combined evidence is consistent with judges becoming more efficient and perfecting their skill through serving on the bench.

Several institutional features of bankruptcy courts allow us to rule out alternative explanations typically present in studies of experience and human capital. First, as federal employees, judges are appointed to 14-year terms. These appointments provide strong incentives for investments in job-specific human capital due to low labor turnover (Becker 1962), and also rule out the possibility that our results are driven by competition eliminating inefficient or incompetent judges. Second, the majority of judges end their career as judges and there are no “junior” bankruptcy judges. In addition, judges’ compensation structure is flat. Thus, there are limited incentives for judges to signal their type by working harder and also fewer agency issues (e.g., revolving door, conservative or risk-taking behavior) that might influence judge performance. Finally, bankruptcy judges do not specialize, but are expected to handle all types of personal and corporate bankruptcy filings (e.g., Chapter 7, Chapter 11, Chapter 13), providing opportunities for judges to learn from evolving, relevant stimuli (Arrow 1962).

Drawing on insights from the investment in human capital and learning-by-doing literatures, we use two cross-sectional tests to better understand how judges move up the learning curve. First, judges should accumulate specific human capital faster by seeing more relevant cases. Therefore, we predict that the rate at which judges learn during their early years should increase in the flow of more relevant business filings to their districts as opposed to less relevant personal filings. However, there are also diminishing returns associated with learning from repetition of essentially similar problems (Arrow 1962). We thus also predict that the rate at which judges learn increases in the diversity of businesses filings for bankruptcy in their districts, where more diversity allows judges to see and learn from a greater variety of corporate settings.

We test these predictions by analyzing all cases assigned to judges with four or less years of on-the-bench experience, so that all judges in the sample have similar tenure but, due to the unique composition of each court, different types of on-the-bench experience. Consistent with our prediction, judges who have seen a higher ratio of business filings to personal filings exhibit greater efficiency, with their large Chapter 11 filings spending less time in court. However, we find no association between the total (i.e., business and personal) bankruptcy filings assigned to a judge and case duration, suggesting that it is business bankruptcy experience in particular that increases judges' human capital. Meanwhile, judges who see more diverse business filings, as measured by the diversity of industries and firm sizes located in their district, also process large Chapter 11 filings more quickly. Taken together, these results suggest that both relevant experience gained through overseeing business bankruptcy filings, as well as experience gained through overseeing a greater diversity of business filings, allows judges to accelerate the learning curve.

We next compare the effects of judges' on-the-bench experience to past 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 results highlight the importance of specific experience acquired through learning-by-doing relative to pre-existing general skills.

One possible alternative explanation for our main results is that, rather than judges perfecting their skills through on-the-bench experience, firms and their lawyers learn how to work with judges more efficiently over time by observing judges' previous case rulings.

We exploit variation in bankruptcy court caseloads to provide suggestive evidence on which economic mechanism is likely 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). Judge on-the-bench experience should be more valuable when caseloads are high, as these are times when cases are possibly harder to manage and creditor conflicts more severe. In contrast, lawyers’ incentives to learn about judges’ rulings in prior cases should not vary with judges’ current caseload. 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 is more consistent with the notion that judges perfect their judicial skills while serving on the bench.

Our paper contributes to three strands of research. First, our study provides evidence on the impact of judge characteristics on corporate bankruptcies. Most prior work focuses on judicial discretion and pro-debtor biases in bankruptcy rulings (Sharfman 2005; Gennaioli and Rossi 2010; Chang and Schoar 2013). Dobbie and Song (2015), Bernstein, Colonnelli, and Iverson (2017) and Bernstein, Colonnelli, Giroud, and Iverson (2017) examine fixed characteristics of judges and their effects on firm outcomes. Different from these studies, we document that time-varying judicial characteristics play an important role in determining the cost and efficiency of Chapter 11 outcomes. Our analyses highlight a potential benefit of allowing firms to file in courts with more experienced judges.

Second, our study contributes to research on learning by doing and investments in specialized human capital (Arrow 1962; Becker 1962). Empirical evidence to date is limited, in part due to challenges in separating “learning by doing” from pure time and age effects, as well as measuring both worker productivity and the likelihood of labor turnover (Thompson 2010).³ We provide evidence that senior, economically important decision makers with sub-

³ See Pastor and Veronesi (2009) for a review of learning in financial markets.

stantial formal education and years of accumulated experience realize significant returns to investments in specialized human capital and “learn by doing.”

Finally, this paper contributes to our understanding on the differential effect of specific skills and general experience on corporate outcomes, using the setting of bankruptcy judges. A growing literature studies the effect of managers’ general versus specific skills on corporate policies, managerial compensation, and mutual fund management, but is inconclusive as to which skill type matters most for human capital value and the managerial labor market.⁴ Our results suggest that judges’—the most important “manager” of the restructuring process—develop specific expertise through time on the job which affects their bankruptcy rulings, and that this on-the-bench experience is incremental to judges’ general skills (Ashenfelter, Eisenberg, and Schwab 1995; Rachlinski, Guthrie, and Wistrich 2006; Posner 2008).

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 Institutional Background

2.1 Judge Appointment

Each bankruptcy district has a fixed number of judgeships set by Congress.⁵ 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

⁴ See Guner, Malmendier, and Tate (2008), Lazear (2009), Custodio, Ferreira, and Matos (2013), Custodio and Metzger (2014), Chernenko, Hanson, and Sunderam (2017), Kempf, Manconi, and Spalt (2017). A number of studies also document the effect of managerial traits and attributes on corporate policies and CEO compensation (see for example Malmendier, Tate, and Yan (2011), Graham, Li, and Qiu (2012), Benmelech and Frydman (2015)).

⁵ 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.

council determines that other relevant legal experience can be substituted. The vast majority of bankruptcy judges (92% in our sample) thus worked as lawyers (often, as bankruptcy lawyers) before being appointed to the bench (Mabey 2005). 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 all applications 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.⁶

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 likely all the same, and thus they have very limited flexibility in selecting their bankruptcy venues.

⁶ Reddick and Knowlton (2013) provide extensive information on the appointment process for bankruptcy judges.

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 (2002), and LoPucki (2005) point out that firms engage in forum shopping to choose debtor-friendly venues. Others suggest that firms choose courts with relevant expertise and which are more efficient compared to other jurisdictions (Skeel 1998; Ayotte and Skeel 2004). In particular, Ayotte and Skeel (2004) find that court-level experience is correlated with quicker reorganizations, especially in Delaware, and that more experienced courts tend to reorganize marginal firms more often. 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.⁷ This random assignment has been used to generate empirical identification in an increasing number of studies, including Chang and Schoar (2013), Dobbie and

⁷ 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.

Song (2015), Bernstein, Colonnelli, and Iverson (2017) and Bernstein, Colonnelli, Giroud, and Iverson (2017). These studies uniformly find evidence that bankruptcy case characteristics are orthogonal to judge characteristics, with the caveat that their 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 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 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 filings 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 that capture the efficiency of the bankruptcy process: *Duration*, the natural logarithm of the 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

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.

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 two case-specific measures that capture the amount of time the judge has been on the bench at the time of the bankruptcy filing. *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.

To measure judges' other professional experience, we consider *Log(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;⁹ 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.

3.3 Court Caseload and Bankruptcy Experience

In several heterogeneity tests we use the mix of all past cases overseen by judges as well as measures of court caseload in our analysis. The starting point for these measures is quarterly court-level filing statistics obtained from the U.S. Courts Administrative Office. This data is available beginning in 1980, and contains information on total filings across filing types (Chapters 7, 11, 13) and nature of debt (business or personal).¹⁰ Using this information, we

⁹ 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.

¹⁰ Both businesses and individuals can file for each chapter of bankruptcy. However, essentially all Chapter 11 filings are business filings, while nearly all Chapter 13 filings are personal filings. Chapter 7 filings

estimate the number of cases overseen by a judge in a given quarter as the total number of cases filed in his/her court divided by the number of judges in the court that quarter. We then sum this number from the beginning of a judge's tenure until the filing date of a given case to obtain a time-varying judge-case specific measure of bankruptcy experience.

We measure the current caseload of each judge as the weighted number of bankruptcy filings in the court-quarter per judge when a firm files for Chapter 11. The weights come from Bermant, Lombard, and Wiggins (1991), who suggest specific hours that judges approximately spend on six distinct types of bankruptcy cases. 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, and thus proxies for the overall time constraints facing the judge.

3.4 Summary Statistics

We summarize case characteristics in Panel A of Table 1, with detailed variable definitions presented in the Appendix. 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.

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 a fairly high debt-to-assets ratio on average (1.01) and negative return on assets (-0.24%). Twenty-six percent of cases are filed as either part of a pre-packaged or

are a mixture of both business and personal. In addition, there are a very small number of filings under Chapters 9, 12, and 15, which we group as additional non-business filings.

pre-negotiated plan, 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.

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. As we discussed in Section 2.2, our paper focuses on large corporate bankruptcies where judge experience is likely to matter most due to the complexity of the cases, which makes it more susceptible to endogeneity concerns. In this section, we empirically test whether *large* Chapter 11 cases are randomly assigned to judges.

If assignment is truly random, then when a large case is filed in a given court, each of the court’s judges should have an equal probability of being assigned the case, regardless of that judge’s level of experience. We thus test for random assignment by estimating 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 *Assigned* is an indicator variable which equals one for the judge that was actually assigned to the case, and zero otherwise. *JudgeExp* is one of the two judge experience measures (i.e., *Log(Months as Judge)* or *First 2 Years*) or the log number of large cases

currently assigned to the judge and not yet resolved ($\text{Log}(\text{Large Caseload})$), all measured at the time a large case is filed. 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 β_1 to be positive for $\text{Log}(\text{Months as Judge})$ and negative for First 2 Years . Similarly, if courts tend to not assign cases to judges with a heavy existing caseload, then the coefficient β_1 will be negative and statistically significant for the measure $\text{Log}(\text{Large Caseload})$. In other words, a lack of any such relationship is consistent with random assignment.

To identify the set of eligible judges when a case was filed, we combine our sample with Lexis Nexis data of all bankruptcy filings (available from 1990) and identify judges contemporaneously serving in that court using judges' tenure information. However, this process 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.¹¹ 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. It is not clear if visiting and district judges have equal probabilities of being assigned large Chapter 11 cases as the court's own bankruptcy judges. Empirically we find that visiting judges are assigned only a small number of large Chapter 11 cases,¹² thus, including visiting and district judges

¹¹ 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.

¹² 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 had been filed, with two cases assigned to visiting judges and two cases assigned to its own appointed 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 in the set of eligible judges for their home court). Finally, we drop courts with only one case or cases with incomplete eligible judge information. Our final randomization sample consists of 6,799 case-judge links representing 60 bankruptcy courts and 1,174 large cases.

Table 2 Panel A presents the results of estimating equation 1. The unconditional probability of being assigned a case (mean of the dependent variable) is 0.17. Across all three judge experience measures, we find that experience as judge and current caseload are unrelated to the assignment probability, consistent with random assignment (or at least assignment that is independent of judge experience). For robustness, we examine whether the insignificant relationship between experience and case assignment persists in seven subsamples in Appendix Table A1: 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 Appendix Table A2 we redo the main analysis from Table 2 after dropping all cases filed in Delaware before 2006 and find similar results.

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. We return to our primary sample of all public Chapter

11 filings 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 two measures of the assigned judge’s experience at the time of filing, and *Firm Characteristics* include $\text{Log}(\text{Assets})$ (the log number of assets in 2016 dollars upon filing for Chapter 11) and $\text{Log}(\text{NumFiling})$ (the log 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 2 Panel B presents coefficient estimates of equation 2. Across the two experience measures in columns (1) and (2), the only firm characteristic to consistently have a significant coefficient is *Prepack/Preneg*. 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. Columns (3) and (4) present the coefficient estimates using the non-prepack sample and none of the firm characteristics show statistical significance. We include Prepack indicator in all our main analysis. Further, and more importantly, in robustness tests we show that all of our main results hold when we drop all prepackaged or prenegotiated cases from our sample.

We also examine to what extent 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 *Log(Months as Judge)* in column (1) and remains at 0.07 in column (3). For the *First 2 Years* specification the adjusted R^2 increases from 0.01 to 0.02 in column (2) and from 0.03 to 0.04 in column (4), as tabulated in footnotes of Panel B. Thus, case characteristics explain less than 2% of the variation in

judge experience. Combined with the evidence in Panel A, our analysis shows that even large cases are randomly assigned to judges.

4.2 Main Results

To test the impact of judges' on-the-bench experience on Chapter 11 case outcomes, we estimate the following regression:¹³

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

where the dependent variable is *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 measure to account for the mechanical growth in experience as time goes by and the fact that average bankruptcy outcomes are changing over time (Bharath, Panchapagesan, and Werner 2010). We also include a Post-BAPCPA dummy, as the Bankruptcy Abuse Prevention and Consumer Protection Act (BAPCPA) altered some laws with regards to Chapter 11. We use the two judge experience measures and case controls are previously defined. We continue to include court and industry (Fama French 12) fixed effects in each regression and cluster standard errors by court.

Table 3 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. We estimate that the elasticity of *Duration* with respect to $\text{Log}(\text{Months as Judge})$ is -0.055 (based on the coefficient estimate in Column (3)). Thus, being randomly assigned to a judge with twice as much time on the 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*

¹³ For outcomes that are indicator variables we use linear probability models.

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 3 months relative to the sample mean.¹⁴

Table 4 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 $\text{Log}(\text{Months as Judge})$ 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.¹⁵

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, perhaps due to reduced career concerns (Holmström 1999) or a higher cost of effort. 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 5. 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.

¹⁴ 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 $100[\exp(\beta_1) - 1]$.

¹⁵ In untabulated results we also examined two experience measures based on the number of large Chapter 11 filings previously assigned to the judge ($\text{Log}(\text{Large})$ and *First 2 Large*). Judges handle both personal and business bankruptcy filings, but handling *large* public bankruptcies can be a different task given the complex and influential nature of these cases. These alternative experience measures produce insignificant coefficient estimates, suggesting that total on-the-bench experience matters more than specific experience with large Chapter 11 cases. One possible explanation is that most judges seeing their first large case have already seen many smaller corporate bankruptcies which allow them to manage large corporate cases more efficiently. For example, judges seeing their second or third large case in our data have been on the bench an average of 78 months. We explore this explanation further in Section 4.4 below.

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.

A concern is that judges who serve more than one term, i.e., judges who are on the long-end of the experience measure, are driving the results. This concern reflects a potential selection issue if better judges get reappointed are therefore associated with more efficient outcomes. We present robustness tests in Appendix Table A4 that only include cases assigned to judges during their first term. The effect of judge experience on case outcomes is robust to excluding these highly experienced judges.

To provide further suggestive evidence regarding creditors' welfare, we examine the effects of judge experience on debt recovery rates at case resolution. We obtain debt recovery from Moody's Default & Recovery Database (DRD) and link it to our bankruptcy sample by matching company name and filing time. This dataset contains information on each debt instrument's contract terms, principal default amount, and final recovery amount. We examine family recovery rate, defined as the enterprise value across all instruments of a corporation relative to that firm's total liabilities, and unsecured recovery rates, which is the combined recovery rate on all unsecured debt. While Moody's DRD provides clean, market-based measurements of recovery rates, one downside is that recovery data is only available for debt instruments that Moody's rate. This leads to a significant decline in sample size for our tests and, hence, significantly less statistical power in our regression analysis.

Table 6 presents both family recovery and unsecured recovery outcomes associated our two time-based experience measures (*Log(Months as Judge)* and *First 2Years*). Across both experience measures, we find that longer experience leads to higher creditor recovery rates. However, the coefficient for *First 2 Years* is statistically significant at 10% level without firm-level and industry controls and loses statistical significance after controls are included.

These insignificant results are potentially due to the reduced sample sizes in these regressions. Nonetheless, directionally, the coefficients are consistent with more experienced judges having a small positive effect on creditor recoveries.

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 re-filing for bankruptcy.

4.3 Learning Curve

Our main analysis examines both the elasticity of case outcomes with respect to judge experience as well as average outcomes associated with inexperienced judges (i.e., judges with two or fewer years on the bench). In this section we expand this analysis to examine average case outcomes at various levels of judicial experience, allowing us to map out judges' learning curve and better understand how long it takes a judge to become "experienced."

Specifically, we create a set of dummy variables indicating whether a case was assigned to a judge during her first two years (*First 2 Years*), third and fourth year (*Year3-4*), or fifth and sixth year (*Year5-6*) on the bench. We include all three dummy variables as measures of judge experience in equation 3, where the omitted variable, and thus benchmark, is the average outcome of cases assigned to judges with more than six years experience. We continue to include both sets of control variable as well as court and industry fixed effects, clustering standard errors by court. By testing for differences across the coefficient estimates on these judge experience indicators, we are able to estimate when case outcomes assigned to new judges become indistinguishable from the case outcomes of more experienced judges.

The results are presented in Table 7. In column (1), the impact of judges' time on the bench on *Duration* displays a declining trend, with the magnitude of estimates decreasing from a statistically significant 0.197 to a statistically insignificant 0.009 as judges' experience increases from their first two years to their fifth and sixth years. The coefficients translate into

21.8% longer durations (3.6 months) in year 1-2 and 16.5% longer durations (2.7 months) in year 3-4, respectively. The statistically insignificant coefficient on dummy *Year5-6* suggests that the duration outcome does not differ between cases that are assigned to a judge who is in year 5-6 versus judges with more than six years of experience. We test for differences across the coefficient estimates, and find no significant differences between *First 2 Years* and *Year3-4* (tabulated in table footnotes). However, both variables differ significantly from *Year5-6* as well as judges with more than 6 years of experience.

Column (2) shows that the effect of having a new judge in her first two years of tenure on *Emergence* is a statistically significant 0.07 reduced emergence probability. The economic magnitude is 12.4% of the sample average. Similar to the declining effect of experience on *Duration*, the magnitude of the estimate in column 2 declines monotonically as judges gain more experience, although the coefficients for *Year3-4* and *Year5-6* are not statistically different from each other or from zero. Column (3) shows that the effect of experience on the probability that the firm refiles for bankruptcy is statistically insignificant at all levels of experience.¹⁶

The combined evidence suggests that judges' learning concentrates in the first two years of tenure, but that it can take up to four years for a judge to manage large Chapter 11 filings in a manner similar to more experienced judges. This curvature of the learning curve supports our "learning by doing" hypothesis against the confounding effects associated with aging such as cognitive ability or "older and wiser." Given that the average judge in our sample is appointed at the age of 47, the deterioration in cognitive ability associated with aging is likely to bias against our findings. Moreover, neither cognitive ability nor "older and wiser" can explain the flattening of the learning curve over the first four years of judges' tenure, as these confounding effects should affect judges' ruling throughout the whole tenure.

¹⁶ In Appendix Table A5 we conduct a robustness check by including only cases assigned to judges during their first term to rule out the possibility that judges serving more than one terms are driving our results. The learning curve pattern remains robust in the subsample.

4.4 Decomposition of Learning

The results presented to this point demonstrate that judges with more time on the bench are able to resolve bankruptcy cases more quickly and efficiently allow more firms to emerge from Chapter 11. In this section, we examine *how* judges learn by focusing on two hypotheses based on insights from the learning by doing and investment in human capital literatures.

First, we posit that judges who quickly accrue the most relevant experience move up the learning curve more quickly. Judges do not specialize in case types, but handle a mix of business and personal filings. In some bankruptcy districts, such as large urban areas, judges see a relatively high volume of business bankruptcy filings, and thus should move up the large Chapter 11 learning curve more quickly as they gain more relevant experience. Meanwhile, judges who spend the majority of their time on non-business bankruptcies will take longer to gain relevant experience to efficiently manage large complex Chapter 11 cases. We thus predict that, conditional on overall time as a judge, judges who have seen a larger number of more relevant business filings are able to more efficiently manage large Chapter 11 filings.

While exposure to relevant experiences is useful, Arrow (1962, p.155) points out that “learning associated with repetition of essentially the same problem is subject to sharply diminishing returns.” Thus, there are likely diminishing returns to seeing a large number of similar business cases. In order for judges to continue learning and “move up the learning curve,” we also predict that judges with exposure to a greater diversity of business cases learn faster. We empirically proxy for case diversity along two dimensions: the size and industry of bankrupt firms. Judges who oversee a more even mix of industries will likely encounter a broader set of issues than judges who mostly oversee a single industry. Similarly, a judge who is assigned a mix of large and small businesses will experience a broader set of issues, which will potentially accelerate learning relative to a more focused judge.

These cross-sectional predictions further separate our “learning by doing” hypothesis from the effects of aging, as it is unlikely that the effects of aging should vary across case

and industry compositions in the precise direction that “learning by doing” predicts. We test both predictions by using a modified version of equation 3 to examine cross-court variations in the levels and diversity of business filings seen by judges. To examine how variation in the type of experience affects case outcomes holding constant judge tenure, we restrict this analysis to all cases assigned to judges in either their first four years (309 cases) or first six years (444 cases). We focus on these subsamples as they are sufficiently large for empirical analysis, yet also contain judges with relatively little time on the bench who simultaneously exhibit significant variation in their type of experience during the first four or six years.

We focus on case duration as the outcome in this analysis. We develop judge-specific measures of past experience using aggregate filing statistics for each bankruptcy court. Specifically, we first calculate the number of bankruptcy cases per judge filed in each calendar quarter. Next, for each judge-case pair, we aggregate the number of past cases assigned to a judge by summing the cases per judge in that court from the time of the judge’s appointment until the filing date of a large Chapter 11 case assigned to the judge.¹⁷ Using this method, for each large Chapter 11 filing in our sample we estimate the total number of filings (across all types, including consumer bankruptcy cases), and the share of of business filings (*Bus Filings/Total Filings*) previously seen by the assigned judge.

In Column (1) of Table 8, we find that cases assigned to judges who have overseen a higher share of past business filings have a shorter duration, while the total number of cases overseen by a judge is not associated with case duration. Thus, it is the relevant experience of overseeing a high share of business cases that increases judge efficiency on large Chapter 11 cases, rather than simply overseeing a high total volume across all case types. We find essentially identical results when we increase the sample to include all cases assigned to judges with less than six years of experience, in Column (4). In either specification, a one-

¹⁷ Data on the specific cases assigned to each judge is unavailable. For this reason we use total filings in the court divided by the number of judges as an estimate of the number of cases received by each judge. Because case assignment is random across all case types, this is likely a close proxy to actual cases overseen by each judge.

standard-deviation increase in the share of business cases leads to about 0.89 fewer months (5.4% of the sample average) in bankruptcy.¹⁸

The remaining columns of Table 8 test whether judges who are exposed to a greater diversity of business bankruptcies learn faster using two cross-sectional measures: industry diversity and size diversity. We create both diversity measures using the Census County Business Patterns dataset covering the years 1986 to 2015. For industry diversity, we first calculate the share of business establishments in a bankruptcy court in each two-digit SIC industry and convert this to a diversity measure (*Diversity-Industry*), calculated as one minus the Herfindahl concentration index. Results in columns (2) and (5) show that judges in courts with more diversified local industry composition resolve large Chapter 11 cases more quickly, relative to judges with similar tenure but less diversified industry composition. A one-standard-deviation increase in *Diversity-Industry* leads to 0.69 months (4.2% of the sample average) shorter duration. Second, we perform a similar analysis using cross-sectional variation in firm sizes. To create *Diversity-Size*, we calculate the share of business establishments in a bankruptcy district across size buckets of 1-4, 5-9, 10-19, 20-49, 50-99, 100-249, 500-599, and 1000+ employees, with the assumption that businesses that file for bankruptcy in a district have a similar size distribution to the overall set of businesses in the area. Then, as before, we calculate *Diversity-Size* as 1 minus the Herfindahl concentration index of these size buckets. We find that judges that oversee a broader mix of firm sizes are able to resolve large Chapter 11 cases more quickly. This result is statistically significant at the 1% level in both the 4-year and 6-year samples, with an economic magnitude of 0.96 months (5.8% of the sample average) shorter duration for a one-standard-deviation increase in *Diversity-Size*. Importantly, we note that the effect of *Bus Filings/Total Filings* remains unchanged with the inclusion of these diversity measures, suggesting that both channels lead to quicker learning by judges.

¹⁸ Interestingly, we do not find that being assigned a high proportion of Chapter 11 cases specifically accelerates judge learning. It appears that both Chapter 7 and Chapter 11 business filings provide relevant experience for the judge.

4.5 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.¹⁹ The evidence is inconclusive as to which skill type matters more for human capital value. The CEO literature suggests that certain managerial traits and characteristics (e.g., gender, early life experiences, military experience) affect corporate policies.²⁰ In addition, there is also an ongoing 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.

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, to 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

¹⁹ See Guner, Malmendier, and Tate (2008), Lazear (2009), Custodio, Ferreira, and Matos (2013), Custodio and Metzger (2014), for example.

²⁰ Malmendier, Tate, and Yan (2011), Graham, Li, and Qiu (2012), Ahern and Dittmar (2012), Benmelech and Frydman (2015), among other.

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 3, 4, and 5. 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.

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 that include 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, consistent with evidence in the previous research (Dobbie and Song 2015; Chang and Schoar 2013; Bernstein, Colonnelli, and Iverson 2017; Bernstein, Colonnelli, Giroud, and Iverson 2017).

4.6 Court Caseload

In this section we provide suggestive evidence on the mechanism driving our empirical findings by examining the relative importance of experience for differing caseloads. Because the number of judges in a court is fixed, when more firms and individuals file for bankruptcy—for example during economic recessions—judges’ workloads are higher (Iverson 2017). During periods of elevated caseloads, judge experience is expected to matter more to restructuring outcomes if experienced judges are able to process cases more efficiently. Specifically, a rise in caseload often coincides with an increase in the number of filings by firms with large asset bases and complex operations, cases which typically have multiple classes and severe creditor conflicts. These cases require judge’s close attention and often daily rulings. In summary, judge’s skill should matter more during periods of elevated caseloads.

In contrast, if the effect of judge experience on case outcomes is driven by lawyer’s learning about judges’ decision making, the effect of judge’s experience on case outcomes should not differ by caseload, since lawyers have incentives to learn about judge’s past rulings regardless of the court 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.

Table 10 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 value, 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. We find that the impact of judges’ on-the-bench experience is more important in periods of above-median caseloads. Panel A shows that judge experience, measured by $\text{Log}(\text{Months as Judge})$, 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 positive coefficient for *First 2 Years* in column (1) suggests significantly longer duration for the high caseload group while column

(2) shows no significant difference 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 there are no pronounced differences between experienced and inexperienced judges. We conclude that the evidence is more consistent with judges learning-by-doing rather than lawyers learning judges' preferences and style.

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 estimates on judges' learning curve suggest that it takes on average two to four years for a judge to efficiently manage large Chapter 11 filings. Exposure to business filings and a greater diversity of case types has a greater impact on judges' ability to handle large complex filings than exposure to non-business or concentrated filing types. 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. At last, our heterogeneity tests show that judicial experience matters more when judges have large caseloads. Our evidence collectively suggests that judges' specific skills developed while serving as a bankruptcy judge matter more than general skills they master prior to the judgeship, and that they perfect such skills while serving on the bench.

Our findings also have implications for the bankruptcy filing process. Businesses presently have some flexibility in where they file for bankruptcy (forum shopping). Our results suggest that greater restriction of this flexibility (e.g., proposed Bankruptcy Venue Reform Act of 2018) could impose a cost on firms and their creditors by forcing them to file in courts with potentially less experienced and less efficient judges.

References

- Ahern, K. R. and A. K. Dittmar (2012). The changing of the boards: The impact on firm valuation of mandated female board representation. *The Quarterly Journal of Economics* 127(1), 137–197.
- Arrow, K. J. (1962). The economic implications of learning by doing. *The Review of Economic Studies* 29(3), 155–173.
- Ashenfelter, O., T. Eisenberg, and S. Schwab (1995). Politics and the judiciary: The influence of judicial background on case outcomes. *Journal of Legal Studies* 24, 257–281.
- Ayotte, K. and E. Morrison (2017). Valuation disputes in corporate bankruptcy. Working paper.
- Ayotte, K. and D. Skeel (2004). Why do distressed companies chose delaware? an empirical analysis of venue choice in bankruptcy. Working paper, University of Pennsylvania Law School.
- Becker, G. S. (1962). Investment in human capital: A theoretical analysis. *Journal of Political Economy* 70(5, Part 2), 9–49.
- Benmelech, E. and C. Frydman (2015). Military CEOs. *Journal of Financial Economics* 117, 43–59.
- Bermant, G., P. A. Lombard, and E. C. Wiggins (1991). A day in the life: The federal judicial center’s 1988-1989 bankruptcy court time study. *Am. Bankr. LJ* 65, 491.
- Bernstein, S., E. Colonnelli, X. Giroud, and B. Iverson (2017). Bankruptcy spillovers. Working paper.
- Bernstein, S., E. Colonnelli, and B. Iverson (2017). Asset allocation in bankruptcy. *Journal of Finance*, forthcoming.
- Bharath, S., V. Panchapagesan, and I. Werner (2010). The changing nature of Chapter 11. Working paper, Arizona State University, Ohio State University.
- Bris, A., I. Welch, and N. Zhu (2006). The costs of bankruptcy: Chapter 7 liquidation versus Chapter 11 reorganization. *Journal of Finance* 61, 1253–1303.
- Chang, T. and A. Schoar (2013). Judge specific differences in chapter 11 and firm outcomes.
- Chernenko, S., S. Hanson, and A. Sunderam (2017). Who neglects risk? investor experience and the credit boom. *Journal of Financial Economics* forthcoming.

- Custodio, C., M. Ferreira, and P. Matos (2013). Generalists versus specialists: Lifetime work experience and CEO pay. *Journal of Financial Economics* 108, 471–492.
- Custodio, C. and D. Metzger (2014). Financial expert CEOs: CEO’s work experience and firm’s financial policies. *Journal of Financial Economics* 114, 125–154.
- Dahiya, S., K. John, M. Puri, and G. Ramirez (2003). Debtor-in-possession financing and bankruptcy resolution. *Journal of Financial Economics* 69, 259–280.
- Dobbie, W. and J. Song (2015). Debt relief and debtor outcomes: Measuring the effects of consumer bankruptcy protection. *American Economic Review* 105, 1272–1311.
- Eisenberg, T. and L. LoPucki (1999). Shopping for judges: An empirical analysis of venue choice in large Chapter 11 reorganizations. *Cornell Law Review* 84, 967–1003.
- Gennaioli, N. and S. Rossi (2010). Judicial discretion in corporate bankruptcy. *Review of Financial Studies*, 4078–4114.
- Goyal, V. K. and W. Wang (2017). Provision of management incentives in bankrupt firms. *Journal of Law, Finance, and Accounting* 2(1), 87–123.
- Graham, J., S. Li, and J. Qiu (2012). Managerial attributse and executive compensation. *Review of Financial Studies* 25, 144–186.
- Guner, A., U. Malmendier, and G. Tate (2008). Financial expertise of directors. *Journal of Financial Economics* 88, 323–354.
- Holmström, B. (1999). Managerial incentive problems: A dynamic perspective. *The Review of Economic Studies* 66(1), 169–182. 10.1111/1467-937X.00083.
- Iverson, B. C. (2017). Get in line: Chapter 11 restructuring in crowded bankruptcy courts. *Management Science, Forthcoming*.
- Kempf, E., A. Manconi, and O. Spalt (2017). Learning by doing: the value of experience and the origins of skill for mutual fund managers.
- Lazear, E. (2009). Firm-specific human capital: A skill weights approach. *Journal of Political Economy* 117, 914–940.
- Li, K. and W. Wang (2016). Debtor-in-possession financing, loan-to-loan, and loan-to-own. *Journal of Corporate Finance* 39, 121–138.
- LoPucki, L. (2005). *Courting failure: How competition for big cases is corrupting the bankruptcy courts*. The University of Michigan Press.
- LoPucki, L. and J. Doherty (2002). Why are delaware and new york bankruptcy reorganizations failing? *Vanderbilt Law Review* 55, 1935–1985.

- LoPucki, L. and J. Doherty (2004). The determinants of professional fees in large bankruptcy reorganization cases. *Journal of Empirical Legal Studies* 1, 111–141.
- Mabey, R. (2005). The evolving bankruptcy bench: How are the “units” fairing? *Boston College Law Review* 47, 105–124.
- Malmendier, U., G. Tate, and J. Yan (2011). Overconfidence and early-life experiences: the effect of managerial traits on corporate financial policies. *Journal of Finance* 66, 1687–2733.
- Pastor, L. and P. Veronesi (2009). Learning in financial markets. *Annu. Rev. Financ. Econ.* 1(1), 361–381.
- Posner, E. (2008). Does political bias in the judiciary matter?: Implications of judicial bias studies for legal and constitutional reform. *The University of Chicago Law Review* 75, 853–883.
- Rachlinski, J., C. Guthrie, and A. Wistrich (2006). Inside the bankruptcy judge’s mind. *Boston University Law Review* 86, 1227–1265.
- Reddick, M. and N. Knowlton (2013). A credit to the courts: The selection, appointment, and reappointment process for bankruptcy judges. Technical report.
- Sharfman, K. (2005). Judicial valuation behavior: Some evidence from bankruptcy. *Florida State University Law Review* 32, 387–400.
- Skeel, D. A. (1998). Bankruptcy judges and bankruptcy venue: Some thoughts on delaware. *Faculty Scholarship*, 719.
- Thompson, P. (2010). Learning by doing. In *Handbook of the Economics of Innovation*, Volume 1, pp. 429–476. Elsevier.

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 fixed judge characteristics.

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	4.31	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
Assets (Mils)	1,310	2,105.29	488.61	5,717.04	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

	Years Before	Top 5 Law	Male	Military	Public Sector	Democratic
Years before Bench	1.00					
Top 5 Law School	0.13*	1.00				
Male	0.18**	0.05	1.00			
Military	0.08	0.02	0.25***	1.00		
Public Sector	-0.12	-0.06	-0.03	-0.04	1.00	
Democratic	0.06	0.03	-0.16*	-0.12	-0.13	1.00

Table 2 Randomization

Panel A presents linear regression estimates of judge assignment, using the set of judges eligible when a case was filed in a given court. The dependent variable, $Assigned_{i,t}$, is an indicator equal to one if judge i was assigned to case, zero otherwise. We regress this assignment indicator on three 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 ($First\ 2\ Years$), and the log of one plus the number of large cases currently assigned to the judge but not yet confirmed ($Log(Caseload)$). Panel B presents regression estimates of our two judge experience measures on firm characteristics upon filing for Chapter 11. We also tabulate in the notes the adjusted R^2 from a specification that includes only court fixed effects. Standard errors are clustered by court, and *, **, *** indicate 10%, 5%, and 1% two-tailed statistical significance, respectively.

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

Panel A: Randomization

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

Panel B: Firm Characteristics

	Full		No Prepacks	
	Log(Months as Judge)	First 2 Years	Log(Months as Judge)	First 2 Years
Log(Assets)	0.022 (1.00)	0.002 (0.26)	0.039 (1.45)	-0.007 (-0.79)
Log(Num filing)	0.000 (0.02)	-0.002 (-0.24)	-0.011 (-0.47)	0.009 (1.19)
Leverage filing	0.079 (0.75)	-0.007 (-0.22)	0.092 (0.69)	-0.015 (-0.47)
ROA filing	0.158 (1.19)	-0.040 (-1.05)	0.228 (1.44)	-0.043 (-1.05)
Prepack/Preneg	0.252*** (4.06)	-0.043*** (-2.69)		
Observations	1,153	1,153	853	853
Adj R-Squared	0.07	0.02	0.07	0.04
Court FE	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes
Adj R2 w/o Controls	0.05	0.01	0.07	0.03

Table 3 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 two judge experience measures: the log number of months the judge has been on the bench (*Log(Months as Judge)*) and an indicator for the first two year's of a judge's tenure (*First 2 Years*). 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)
	Log(Months as Judge)	First 2 Years	Log(Months as Judge)	First 2 Years
Experience Measure	-0.099*** (-6.45)	0.215*** (3.18)	-0.055*** (-4.28)	0.166** (2.41)
Time Trend	-0.025** (-2.61)	-0.028*** (-2.74)	-0.023*** (-3.34)	-0.024*** (-3.43)
Post-BAPCPA	0.047 (0.75)	0.070 (1.08)	0.116** (2.18)	0.127** (2.26)
Log(Assets)			0.084*** (4.32)	0.082*** (4.21)
Log(Num filing)			0.044*** (3.51)	0.044*** (3.39)
Leverage filing			-0.146** (-2.62)	-0.150** (-2.66)
ROA filing			-0.110** (-2.64)	-0.113*** (-2.79)
Prepack/Preneg			-1.190*** (-17.92)	-1.196*** (-17.57)
Adjusted R^2	0.08	0.07	0.41	0.41
Observations	1,274	1,274	1,153	1,153
Industry FE	No	No	Yes	Yes
Court FE	Yes	Yes	Yes	Yes

Table 4 Emergence

This table presents linear probability regression estimates of a dummy variable indicating a firm emerges from Chapter 11 on our two judge experience measures: the log number of months the judge has been on the bench (*Log(Months as Judge)*) and an indicator for the first two year's of a judge's tenure (*First 2 Years*). 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)
	Log(Months as Judge)	First 2 Years	Log(Months as Judge)	First 2 Years
Experience Measure	0.041*** (3.66)	-0.062** (-2.25)	0.026** (2.05)	-0.065* (-1.68)
Time Trend	-0.018*** (-4.11)	-0.017*** (-3.69)	-0.020*** (-5.77)	-0.019*** (-5.60)
Post-BAPCPA	0.122** (2.40)	0.112** (2.18)	0.077 (1.47)	0.072 (1.34)
Log(Assets)			0.059*** (4.75)	0.060*** (4.80)
Log(Num filing)			0.023** (2.54)	0.023*** (2.68)
Leverage filing			0.154*** (6.17)	0.156*** (6.60)
ROA filing			0.047 (0.87)	0.048 (0.93)
Prepack/Preneg			0.296*** (14.25)	0.299*** (14.53)
Adjusted R^2	0.06	0.05	0.20	0.20
Observations	1,274	1,274	1,153	1,153
Industry FE	No	No	Yes	Yes
Court FE	Yes	Yes	Yes	Yes

Table 5 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 two judge experience measures: the log number of months the judge has been on the bench (*Log(Months as Judge)*) and an indicator for the first two year's of a judge's tenure (*First 2 Years*). 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)
	Log(Months as Judge)	First 2 Years	Log(Months as Judge)	First 2 Years
Experience Measure	0.010 (1.33)	0.015 (0.47)	0.010 (0.97)	0.011 (0.29)
Time Trend	-0.004 (-1.22)	-0.003 (-1.04)	-0.005 (-1.17)	-0.004 (-1.07)
Post-BAPCPA	0.023 (0.66)	0.020 (0.58)	0.034 (0.86)	0.032 (0.82)
Log(Assets)			-0.008 (-1.42)	-0.008 (-1.37)
Log(Num filing)			0.012 (1.43)	0.012 (1.48)
Leverage filing			0.036 (1.31)	0.038 (1.36)
ROA filing			0.013 (0.51)	0.016 (0.59)
Prepack/Preneg			0.046* (1.72)	0.048* (1.79)
Adjusted R^2	-0.01	-0.01	0.02	0.01
Observations	697	697	624	624
Industry FE	No	No	Yes	Yes
Court FE	Yes	Yes	Yes	Yes

Table 6 Debt Recovery

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 (*Log(Months as Judge)*) and an indicator for the first two year's of a judge's tenure (*First 2 Years*). 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)	(2) First 2Y	(3) Log(Months)	(4) First 2Y	(5) Log(Months)	(6) First 2Y	(7) Log(Months)	(8) First 2Y
Experience Measure	1.879 (1.10)	-6.762* (-1.97)	0.732 (0.45)	-4.880 (-1.46)	1.693 (0.87)	-7.757* (-1.85)	0.291 (0.15)	-5.676 (-1.09)
Time Trend	0.649* (1.93)	0.686* (1.90)	0.689 (1.68)	0.688 (1.64)	0.058 (0.18)	0.097 (0.29)	-0.306 (-1.06)	-0.316 (-1.07)
Post-BAPCPA	-0.918 (-0.26)	-1.615 (-0.43)	-1.361 (-0.22)	-1.702 (-0.28)	-2.545 (-0.60)	-3.428 (-0.85)	-0.483 (-0.08)	-0.849 (-0.14)
Log(Assets)			-1.466 (-1.47)	-1.498 (-1.59)			1.175 (0.84)	1.137 (0.84)
Log(Num filing)			-1.628 (-0.92)	-1.586 (-0.91)			-1.358 (-0.71)	-1.392 (-0.75)
Leverage filing			-1.896 (-0.36)	-1.763 (-0.35)			-1.528 (-0.19)	-1.590 (-0.21)
ROA filing			-0.085 (-0.01)	-0.434 (-0.04)			-4.455 (-0.39)	-4.935 (-0.42)
Prepack/Preneg			5.586*** (3.33)	5.585*** (3.11)			13.389*** (7.54)	13.238*** (7.80)
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

Table 7 Learning Curve

This table presents regression estimates of judge experience measures indicating first two years, first 3-4 years, and first 5-6 years on case outcomes. 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.

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

	(1)	(2)	(3)
	Duration	Emergence	Refile
First 2 Years	0.197*** (3.34)	-0.071** (-2.09)	0.001 (0.02)
Year3-4	0.153** (2.48)	-0.042 (-0.66)	-0.032 (-1.45)
Year5-6	0.009 (0.16)	0.012 (0.37)	-0.033 (-1.32)
Log(Assets)	0.085*** (4.37)	0.060*** (4.87)	-0.008 (-1.40)
Log(Num filing)	0.044*** (3.38)	0.023** (2.61)	0.012 (1.42)
Leverage filing	-0.148** (-2.66)	0.155*** (6.45)	0.037 (1.34)
ROA filing	-0.113** (-2.63)	0.048 (0.91)	0.014 (0.51)
Time Trend	-0.023*** (-3.28)	-0.019*** (-5.47)	-0.004 (-1.15)
Post-BAPCPA	0.113** (2.01)	0.076 (1.32)	0.035 (0.91)
Prepack/Preneg	-1.193*** (-17.91)	0.299*** (14.43)	0.047* (1.77)
Observations	1,153	1,153	624
Adj R-Squared	0.41	0.20	0.01
Industry FE	Yes	Yes	Yes
Court FE	Yes	Yes	Yes
P(Y12=Y34)	0.657	0.721	0.404
P(Y12=Y56)	0.045	0.035	0.246
P(Y34=Y56)	0.024	0.460	0.966
P(Y12=Y34=Y56)	0.027	0.088	0.503

Table 8 Decomposition of Experience

This table presents regression estimates of the effects of judge experience on bankruptcy duration. We estimate measures of the types of cases previously seen by each judges using historical court-level filings, and diversity measures of local business using census data at each court level. Columns (1)-(3) include all cases assigned to judges during their first four years, and columns (4)-(6) include all cases assigned to judges during their first six years. All explanatory variables are standardized. Filing year fixed effects are included in each regression. Case controls include $\text{Log}(\text{Assets})$, $\text{Log}(\text{Num Filing})$, Leverage filing , ROA filing , and Prepack/preneg . Standard errors are clustered at the court level, and *, **, *** indicate 10%, 5%, and 1% two-tailed statistical significance, respectively.

$$\text{Duration} = \alpha + \beta_1 \text{Past Experience}_{i,t} + \beta_2 \text{Controls} + \theta \text{Filing Year FE} + \epsilon_{i,t}$$

	First Four Years			First Six Years		
	(1)	(2)	(3)	(4)	(5)	(6)
Past Total Filings	0.00 (0.07)	0.04 (0.82)	0.06 (1.34)	-0.04 (-1.08)	-0.01 (-0.28)	-0.00 (-0.11)
Bus Filings/Total Filings	-0.12** (-2.63)	-0.10** (-2.28)	-0.11*** (-2.82)	-0.11*** (-2.83)	-0.09** (-2.32)	-0.10*** (-2.83)
Diversity-Industry		-0.10** (-2.08)			-0.10*** (-2.81)	
Diversity-Size			-0.14*** (-3.88)			-0.11*** (-3.54)
Observations	309	309	309	444	444	444
Adj R-Squared	0.41	0.42	0.43	0.46	0.47	0.47
Filing Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Case Controls	Yes	Yes	Yes	Yes	Yes	Yes

Table 9 General Experience and Personal Attributes

This table presents 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*). Case controls include *Log(Assets)*, *Log(Num Filing)*, *Leverage filing*, *ROA filing*, and *Prepack/preneg*. We also induce a dummy variable indicating missing voting records. 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*** (-4.16)	-0.059*** (-3.88)	-0.051*** (-4.51)	-0.047*** (-3.43)
ln(Years before Bench)		0.017 (0.32)		0.104 (1.51)
Top5 Lawschool		0.029 (0.28)		0.018 (0.18)
Public Sector		-0.019 (-0.18)		-0.023 (-0.29)
Male			-0.150** (-2.34)	-0.198*** (-3.44)
Military			0.041 (0.66)	-0.053 (-0.52)
Democrats			0.006 (0.10)	-0.039 (-0.57)
Observations	1147	925	1146	925
Adjusted R^2	0.41	0.39	0.41	0.39
Case Controls	Yes	Yes	Yes	Yes
FE	Yes	Yes	Yes	Yes

Panel B: Emergence				
	(1)	(2)	(3)	(4)
Log(Months as Judge)	0.024* (1.89)	0.030* (1.92)	0.021 (1.62)	0.026* (1.76)
ln(Years before Bench)		0.023 (0.69)		0.014 (0.58)
Top5 Lawschool		0.036 (0.92)		0.021 (0.53)
Public Sector		-0.014 (-0.27)		-0.011 (-0.22)
Male			-0.017 (-0.49)	-0.006 (-0.18)
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
Case 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 (0.62)	0.010 (1.30)	0.004 (0.66)
ln(Years before Bench)		-0.080*** (-2.80)		-0.056** (-2.53)
Top5 Lawschool		0.020 (0.85)		0.006 (0.21)
Public Sector		0.021 (0.49)		0.016 (0.38)
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 (0.19)
Observations	620	500	620	500
Adjusted R^2	0.02	0.04	0.02	0.04
Case Controls	Yes	Yes	Yes	Yes
FE	Yes	Yes	Yes	Yes

Table 10 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*). Controls include *Log(Assets)*, *Log(Num Filing)*, *Leverage filing*, *ROA filing*, *Prepack/preneg*, *Time trend* and Post-BAPCPA. 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.

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

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)	-0.052** (-2.42)	-0.032 (-1.17)	0.030** (2.51)	0.016 (1.02)	0.016 (0.93)	-0.002 (-0.22)
Observations	571	564	571	564	309	300
Adjusted R^2	0.36	0.46	0.21	0.22	0.04	0.01
Case 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	0.187*** (3.01)	0.092 (0.58)	-0.042 (-0.89)	-0.057 (-1.45)	0.011 (0.25)	0.011 (0.15)
Observations	571	564	571	564	309	300
Adjusted R^2	0.36	0.46	0.20	0.22	0.03	0.01
Case Controls	Yes	Yes	Yes	Yes	Yes	Yes
FE	Yes	Yes	Yes	Yes	Yes	Yes

Appendix

Variable Definitions

Experience Measures	
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
Judge Characteristics	
Log(Years before Bench)	Log(number of years after law school and before 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
Democrats	A dummy variable=1 for affiliation with the Democratic party
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
Log(Num filing)	Log(Number of subsidiaries 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
Leverage 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 Panel A. 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*), and the log number of large cases currently assigned to the judge but not yet confirmed in Panel C (*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)	(2)	(3)	(4)	(5)	(6)	(7)
	No Prepack	Large	Small	NYSD/DE	DE	NYSD	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 Panel A 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) Assigned	(2) Assigned	(3) Assigned
Log(Months as Judge)	0.003 (0.50)		
First 2 Years		-0.012 (-0.66)	
Log(Large Caseload)			0.016 (1.47)
Observations	6,117	6,117	6,393
Adj R-Squared	-0.11	-0.11	-0.11
Case FE	Yes	Yes	Yes

Table A3 Robustness Check: No Prepackaged/Prenegotiated Cases

This table presents regression estimates of judge experience measures on case outcomes, excluding all pre-package and pre-negotiate cases from the sample. The three outcome variables include: the log number of months a case is under Chapter 11 (*Duration*) in columns (1)-(2), a dummy variable indicating a firm emerges from Chapter 11 (*Emergence*) in columns (3)-(4), and a dummy variable indicating a firm refiles for Chapter 11 within three years after emergence (*Refile3Y*) in columns (5)-(6). The two judge experience measures include: the log number of months the judge has been on the bench (*Log(MonthsasJudge)*); and an indicator for the first two year's of a judge's tenure (*First2Years*). 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
Experience Measure	-0.055*** (-3.73)	0.145*** (3.18)	0.036** (2.02)	-0.088** (-2.33)	0.009 (0.70)	-0.013 (-0.28)
Log(Assets)	0.118*** (6.23)	0.117*** (6.21)	0.061*** (4.19)	0.063*** (4.22)	0.003 (0.33)	0.003 (0.34)
Log(Num filing)	0.015 (0.88)	0.015 (0.88)	0.023* (1.74)	0.023* (1.85)	-0.003 (-0.32)	-0.003 (-0.29)
Leverage filing	-0.116** (-2.38)	-0.121** (-2.53)	0.140*** (2.81)	0.143*** (3.04)	0.002 (0.08)	0.003 (0.12)
ROA filing	-0.045 (-0.69)	-0.052 (-0.80)	-0.002 (-0.03)	0.003 (0.03)	-0.052** (-2.13)	-0.050** (-2.03)
Time Trend	-0.033*** (-6.97)	-0.034*** (-7.16)	-0.021*** (-5.11)	-0.020*** (-4.88)	-0.004 (-1.34)	-0.004 (-1.23)
Post-BAPCPA	0.163** (2.42)	0.173** (2.61)	0.074 (1.31)	0.068 (1.18)	0.001 (0.03)	-0.001 (-0.01)
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

Table A4 Robustness Check: First-term judges

This table presents regression estimates of judge experience measures on case outcomes, including only cases assigned to judges during their first term. The three outcome variables include: the log number of months a case is under Chapter 11 (*Duration*) in columns (1)-(2), a dummy variable indicating a firm emerges from Chapter 11 (*Emergence*) in columns (3)-(4), and a dummy variable indicating a firm refiles for Chapter 11 within three years after emergence (*Refile3Y*) in columns (5)-(6). The two judge experience measures include: the log number of months the judge has been on the bench (*Log(MonthsasJudge)*); and an indicator for the first two year's of a judge's tenure (*First2Years*). 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
Experience Measure	-0.069*** (-3.99)	0.164** (2.04)	0.035*** (3.16)	-0.056 (-1.38)	-0.003 (-0.25)	0.026 (0.66)
Log(Assets)	0.082*** (3.97)	0.079*** (3.88)	0.061*** (3.73)	0.062*** (3.89)	-0.008 (-1.25)	-0.008 (-1.22)
Log(Num filing)	0.078*** (5.85)	0.079*** (5.91)	0.010 (1.09)	0.009 (1.06)	0.014* (2.02)	0.015* (2.00)
Leverage filing	-0.330*** (-3.50)	-0.334*** (-3.57)	0.205*** (8.13)	0.207*** (8.54)	0.058** (2.55)	0.058** (2.55)
ROA filing	-0.201*** (-2.86)	-0.207*** (-2.95)	0.051 (0.92)	0.055 (1.00)	0.048 (1.07)	0.047 (1.05)
Time Trend	-0.034*** (-3.41)	-0.035*** (-3.57)	-0.014*** (-4.33)	-0.014*** (-4.11)	-0.002 (-0.54)	-0.002 (-0.54)
Post-BAPCPA	0.066 (0.80)	0.071 (0.88)	0.063 (1.53)	0.061 (1.45)	0.010 (0.23)	0.012 (0.28)
Observations	837	837	837	837	446	446
Adj R-Squared	0.13	0.13	0.13	0.13	0.01	0.01
Industry FE	Yes	Yes	Yes	Yes	Yes	Yes
Court FE	Yes	Yes	Yes	Yes	Yes	Yes

Table A5 Learning Curve: First-term Judges

This table presents regression estimates of judges' learning curve, including only cases assigned to judges during their first term. See Table 7 for additional details. 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.

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

	(1)	(2)	(3)
	Duration	Emergence	Refile
Year1-2	0.163** (2.23)	-0.052 (-1.39)	0.024 (0.53)
Year3-4	0.145** (2.28)	-0.038 (-0.69)	-0.003 (-0.13)
Year5-6	-0.003 (-0.05)	0.022 (0.66)	-0.010 (-0.39)
Log(Assets)	0.091*** (4.71)	0.059*** (4.09)	-0.008 (-1.26)
Log(Num filing)	0.039*** (3.03)	0.020** (2.09)	0.016* (1.81)
Leverage filing	-0.164** (-2.28)	0.162*** (6.47)	0.054** (2.47)
ROA filing	-0.150** (-2.10)	0.040 (0.70)	0.044 (1.04)
Time Trend	-0.025*** (-3.50)	-0.016*** (-5.07)	-0.002 (-0.60)
Post-BAPCPA	0.079 (1.27)	0.059 (1.30)	0.010 (0.23)
Prepack/Preneg	-1.189*** (-18.41)	0.314*** (13.88)	0.024 (0.84)
Observations	837	837	446
Adj R-Squared	0.40	0.20	0.01
Industry FE	Yes	Yes	Yes
Court FE	Yes	Yes	Yes
P(Y12=Y34)	0.860	0.861	0.527
P(Y12=Y56)	0.066	0.054	0.249
P(Y34=Y56)	0.020	0.400	0.761
P(Y12=Y34=Y56)	0.020	0.126	0.414