Learning by Doing: Judge Experience and Bankruptcy Outcomes *

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Abstract

Exploiting the within-district random assignment of large corporate Chapter 11 filings, we estimate the costs of inexperience for bankruptcy judges. Inexperienced judges rule slower from the bench, and their cases spend more time in bankruptcy. Firms with inexperienced judges are less likely to reorganize and have lower debt recovery rates. The learning curve is approximately four years, but exposure to more corporate cases and a greater diversity of businesses accelerates judges' learning. The costs of inexperience are higher when courts are busy. Judges' general skill and personal attributes do not consistently explain case outcomes.

Keywords: Bankruptcy judges, human capital, learning by doing, job-specific skills;
JEL: G33, G34, J24

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1 Introduction

Many jobs require skilled workers to accumulate human capital through both formal education and on-the-job training. While workers can learn some skills in classrooms and simulated scenarios, in many cases the only way for an employee to "move up the learning curve" is to be assigned tasks for which they may not be fully prepared. For example, at some point every surgeon must perform their first surgery, every engineer must draft their first blueprint, and every entrepreneur must start their first company. Economists have long recognized the importance of learning by doing (Arrow (1962); Becker (1962)). However, it is empirically difficult to estimate the importance of on-the-job training and its costs because, in most settings, one cannot observe task-specific outcomes and more complex tasks are typically assigned to more skilled workers.

In this paper, we study the costs of inexperience and importance of on-the-job training in the specific context of bankruptcy judges. This setting overcomes the endogenous matching of skills and tasks because bankruptcy judges are randomly assigned to cases. In addition, each bankruptcy case is a separate task for which we can observe key outcomes that are impacted by judges' discretion, including speed of ruling, case duration, likelihood of emergence, refiling rates, and recovery rates for creditors.[1] In addition, bankruptcy judges typically have significant prior legal experience and education, often including years of experience as attorneys and clerks in bankruptcy courts. Thus, our setting provides an opportunity to quantify the importance of *job-specific* judicial skills (e.g., ruling on motions, resolving disputes, managing large caseloads, etc.) for individuals who already have significant *general* human capital.

Our sample consists of all Chapter 11 filings by U.S. public firms with more than $50 million in assets (hereafter "large cases") filed between 1980 and 2012. These large cases were overseen by 309 unique bankruptcy judges in 75 bankruptcy courts, and are not only economically important but also complex, often involving controversial issues and competing demands from various stakeholders—cases where judicial discretion and skill likely matter the most. For each judge in

[1] Previous work has shown that 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 (Dahiya et al. (2003); Sharfman (2005); Bris et al. (2006); Gennaioli and Rossi (2010); Chang and Schoar (2013); Li and Wang (2016); Ayotte and Morrison (2017); Bernstein et al. (2017, 2018); Goyal and Wang (2017)).

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. Because judges' accumulation of job-specific skills is unobservable, we use a judge's tenure as of the filing date of a large case as our primary measure of on-the-bench experience.

Our primary outcome measures are duration of restructuring and the average number of days between motion filing and judge order. We also jointly examine the probability of emergence (versus liquidation) and recidivism, as well as post-bankruptcy debt recovery rates and debt value changes during restructuring. Although each of these individual measures captures a specific outcome of bankruptcy, combined they provide insights into how judges' efficiency impacts the bankruptcy process and value of the estate. Furthermore, we study how quickly new judges learn, factors that accelerate their learning curve, and the relative importance of prior professional experience and personal attributes to provide a comprehensive picture of judges' learning on the bench.

An important characteristic of bankruptcy courts for our identification purpose is that bankruptcy judges are randomly assigned to cases. While prior work documents the random assignment of judges, these studies employ data sets that are dominated by small business filings (Chang and Schoar (2013); Bernstein et al. (2017, 2018)). It is possible, however, that large Chapter 11 cases are not randomly assigned for a variety of reasons (see discussion in Section 2.2). Thus the first set of empirical tests that we perform is to shed light on judge random assignment for large cases. We find that tenure, current caseload of large cases, and industry expertise do not predict the assignment of large cases. Furthermore, there is little correlation between the assigned judge's level of experience and firm characteristics and firm characteristics have almost no explanatory power for the overall variation in judge experience. Taken together, this evidence is consistent with the random assignment of large Chapter 11 filings.

We next exploit this random assignment with respect to judge tenure to examine the effect of judges' on-the-bench experience on large case outcomes. We include firm-level controls and both court and industry fixed effects in all empirical specifications. Our identifying assumption is that confounding factors that affect case outcomes are orthogonal to judge experience. We find that large cases assigned to judges who have been on the bench longer spend less time in court. Large 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 in our sample (16.5 months). The effect of *inexperience* is particularly striking, with large cases assigned to judges who have two or fewer years on the bench realizing 18% longer durations on average (approximately three months). Based on estimates from prior studies, this increased time in bankruptcy represents an additional $7.5 million in legal fees alone for the average case in our sample.

While it is intuitive that more experienced judges can move cases through the process more quickly, it is less clear the mechanism by which this occurs. Using electronic dockets of 535 cases filed after 2002, we identify 74,890 individual motions filed and measure the time until the assigned judge issues an order ruling on each motion. On average, 33 days pass between the filing of a motion and the corresponding order. We find that judges in their first two years on the bench spend an additional 3.4 days on each motion, a 10% increase from the sample mean. Thus, a significant portion of the overall 18% increase in bankruptcy duration is due to inexperienced judges taking longer to issue rulings on specific motions. Importantly, we do not find a significant relationship between judicial experience and the number of motions filed, suggesting that the increased duration for inexperienced judges is not due to a higher number of motions being filed in these cases.

Our results on the restructuring outcome of the bankruptcy case show that large cases assigned to judges with longer tenure are more likely to emerge from bankruptcy. A one-standard-deviation increase in the assigned judge's time on the bench leads to a 3% increase in the probability that a large case emerges from bankruptcy, which corresponds to 5.15% of the sample average (57%). At the same time, large cases assigned to judges with longer tenure are not more likely to refile for bankruptcy within three years of emergence. In addition, large cases assigned to more experienced judges have higher debt recovery rates at case resolution and the defaulted debt experiences larger increases in value from Chapter 11 filing to case resolution. The evidence is inconsistent with longer-tenured judges becoming more lenient at the cost of lower quality restructuring (i.e., "kicking the can down the road"). Instead, the combined evidence suggests that judges become more efficient at managing large cases the longer they have served on the bench, helping achieve successful reorganizations for companies and improving creditors' welfare. To provide a sense of the aggregate costs of inexperience, we consider a counterfactual scenario where judges with the least experience are assigned the smallest cases in our sample. Under this alternative to full randomization, we estimate that creditors could recover \$17.32 billion more across all cases in our sample. This back-of-the-envelope estimate suggests that the costs of inexperience may be substantial.

Several institutional features of bankruptcy courts allow us to rule out alternative explanations typically present in studies of experience and human capital. First, judges are appointed to renewable 14-year terms. These appointments provide strong incentives for judges to acquire job-specific skills due to low labor turnover (Becker (1962); Jovanovic (1979)), and also rule out the possibility that our results are driven by competition eliminating inefficient or incompetent judges. Second, the majority of bankruptcy judges end their career as judges and there are, once appointed, no promotions to "senior" bankruptcy judge. In addition, judges' compensation structure is flat. Thus, there are limited incentives for judges to signal their type by working harder or learning faster and there are fewer agency issues (e.g., revolving door, conservative or risk-taking behavior) which might influence judge performance (Carmichael (1983); Prendergast (1993)). Third, although judges have significant legal experience, new judges have no experience ruling on bankruptcy cases, allowing us to cleanly identify and quantify the importance of learning by doing relative to prior relevant experience. Finally, because we observe bankruptcy outcomes, we can assess the value of judges' job-specific experience and quantify their learning curve.

To shed light on the slope of the learning curve, we examine average large case outcomes at various levels of judicial experience and find that it takes up to four years until a new judge has similar large case durations and time to rule on motions as more experienced judges. Furthermore, drawing on insights from the human capital and learning-by-doing literatures, we use two cross-sectional tests to better understand how judges move up this learning curve. First, we predict that new judges accumulate job-specific human capital (i.e., efficiently manage large cases) faster by seeing more relevant cases. Because bankruptcy judges handle all types of personal and corporate bankruptcy filings in their districts, the rate at which judges learn to efficiently manage large Chapter 11 business filings should increase in the flow of more relevant business filings to their districts as opposed to less relevant personal filings. Second, since there are diminishing returns associated with learning from repetition of essentially similar problems (Arrow (1962)), we predict that the rate at which judges learn is increasing in the diversity of businesses filing in their districts.

We test these predictions by analyzing all large cases assigned to judges with four or fewer years of on-the-bench experience so that all judges have similar tenure but, due to the unique composition of each court, different types of on-the-bench experience during their initial four years. Consistent with our prediction, there is no association between the total (i.e., business and personal) bankruptcy filings previously assigned to a judge and case duration; however, judges who have seen a higher ratio of business filings to personal filings exhibit greater efficiency, with their large cases spending less time in court. This suggests that it is the specific experience with business filings that improves judges' ability to manage large cases. Meanwhile, judges who have seen more diverse business filings, as measured by the diversity of industries and firm sizes located in their district, also process large cases more quickly. Taken together, these results suggest that both relevance of experience and diversity of tasks allow judges to accelerate their learning curve.

We next compare the effects of judges' on-the-bench experience to more general human capital (measured by years of prior work experience) as well as judge personal characteristics (measured by educational background, gender, political affiliation, and military service). In contrast to on-the-bench experience, we find little association between judges' prior professional experience or personal characteristics and bankruptcy outcomes. One exception to this general finding is that judges with more years of prior work experience move up the learning curve more quickly. We also find an association between judge gender and large case duration, suggesting that personal 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 skills acquired through learning-by-doing relative to pre-existing general skills and personal attributes.

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 by observing judges' previous case rulings. We exploit variation in bankruptcy court caseloads to provide suggestive evidence on whether this particular economic mechanism likely drives our empirical findings. Because the number of judges in a court is fixed, when more firms and individuals file for bankruptcy judges have higher workloads (Iverson (2017)). Judge's on-the-bench experience should be more valuable when caseloads are high, as these are times when large cases are possibly harder to manage and creditor conflicts are 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 the duration of large cases and their time to rule on motions, 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 contributes to research on learning by doing and job-specific human capital. Prior studies provide a theoretical foundation for understanding employees' and managers' investment and accumulation of job- or task-specific human capital in an organization (Arrow (1962); Becker (1962); Carmichael (1983); Prendergast (1993); Gibbons and Waldman (1999, 2004); Lazear (2009); Levitt et al. (2013)). Direct empirical evidence to date is limited, however, in part due to confounding factors such as age and the endogenous matching between workers and tasks, as well as measuring both worker productivity and the likelihood of labor turnover (Thompson (2010)).[2] Our setting circumvents many of these institutional limitations and challenges to provide clear evidence that economically important decision makers with substantial formal education and years of accumulated relevant experience gain job-specific human capital over time. Our study provides estimates of the costs of inexperience for bankruptcy judges, and highlights judges' accumulation of job-specific human capital through learning-by-doing and how it accelerates with exposure to task variety and complexity.

Second, this paper contributes to our understanding on the differential effect of general versus specific skills on corporate outcomes, using the setting of bankruptcy judges. A growing literature studies the effect of managers' skill on corporate policies, managerial compensation, and mutual fund management, but the evidence is inconclusive as to what type of skill matters most for human capital value and the managerial labor market.[3] Our results suggest that judges—the most important "manager" of the corporate restructuring process—accumulate specific expertise through time on

[2]See also Pastor and Veronesi (2009) for a review of learning in financial markets.

[3]See Guner et al. (2008); Custodio et al. (2013); Custodio and Metzger (2014); Chernenko et al. (2017); Kempf et al. (2017); Bradley et al. (2017). A number of studies also document the effect of managerial traits and attributes (e.g., gender, early life experiences, military experience) on corporate policies and CEO compensation (see for example Malmendier et al. (2011); Graham et al. (2012); Ahern and Dittmar (2012); Benmelech and Frydman (2015)).

the job, and that this specific expertise is incremental to judges' general skills and personal attributes (Ashenfelter et al. (1995); Rachlinski et al. (2006); Posner (2008)).

Finally, our study provides evidence on the impact of judge characteristics on corporate bankruptcies. Most prior work focuses on judicial discretion and biases in bankruptcy rulings (Sharfman (2005); Gennaioli and Rossi (2010)). Chang and Schoar (2013), Dobbie and Song (2015), and Bernstein et al. (2017) examine fixed characteristics of judges and their effects on case outcomes. Different from these studies, we document that time-varying judicial characteristics play an important role in determining large Chapter 11 outcomes, and that it takes time for judges to develop the specific skills needed to efficiently manage large cases. Our analyses highlight a potential benefit of allowing large cases to be filed in courts with more experienced judges and providing timely judicial training for rookie judges.

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.[4] When a judgeship becomes available, announcement of the vacancy is made in newspapers and bankruptcy practitioner publications. Applicants are required to be members of the bar in good standing and to have at least five years of experience practicing law, unless the circuit's judicial council determines that other relevant legal experience can be substituted. The vast majority of bankruptcy judges (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)).

[4]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.

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.[5] Bankruptcy judges serve renewable 14-year terms.

2.2 Forum Shopping and Judge Assignment

A few caveats regarding large cases and judge assignment 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.

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, attracting the filings of almost 50% of our sample firms. 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

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

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)). 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 large case outcomes within each bankruptcy court. This regression framework allows us to exploit within-court cross-judge variation in judicial experiences at different points in time, while controlling for potentially omitted time-invariant heterogeneity across courts.

A second concern relates to whether *large* Chapter 11 cases are truly randomly assigned. When a corporation files for bankruptcy in a given bankruptcy district, the majority of bankruptcy courts' stated policy is to randomly assign the case to one of the district's active bankruptcy judges.[6] This random assignment has been used to generate empirical identification in an increasing number of studies (Chang and Schoar (2013); Dobbie and Song (2015); Bernstein et al. (2017, 2018)). 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 cases to judges with more judicial experience. 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 large cases

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

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 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. We use both UCLA LoPucki Bankruptcy Research Database (BRD) and New Generation Research's bankruptcydata.com for data retrieval.[7] 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 trace a firm and collect whether they refile for Chapter 11 (i.e., "Chapter 22" filings).

We use three sets of measures to capture the efficiency of bankruptcy judges. Our primary measure is *Duration*, the natural logarithm of the number of months from Chapter 11 filing date to plan confirmation date, which proxies for the overall costs of restructuring.[8] In order to understand

[7] 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. Upon observing inconsistency between the two databases we resort to Public Access to Court Electronic Records (PACER) for verification.

[8] Bankruptcy costs include not only legal and administration fees but also other opportunity costs that are related to the loss of customers, suppliers, and employees, etc. (Opler and Titman (1994); Andrade and Kaplan (1998)).

the mechanism by which judges affect *Duration*, we gather docket information on all cases in our sample that have electronic dockets on PACER, which are typically available beginning in 2002. Bankruptcy dockets list all motions filed and orders granted during the case, and allow us to link motions filed with the judicial order which rules on each motion. Using this information, we calculate *Ave Days(Ruling)* as the average number of days between the motion and the related order across all motions in a case. In doing so, we drop all "first-day" motions, which are typically routine and require little consideration by the judge. Our final sample contains 74,890 individual motions filed in 535 cases.

Our second set of variables includes *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. Combined, these two variables give an indication of the efficiency of the restructuring, although we caution that we cannot measure full economic efficiency due to an inability to observe outcome measures for firms that liquidate.

Third, we use total recovery rate (*Total Recovery*) defined as the average recovery rate across all debt instruments listed in the reorganization or liquidation plan that is confirmed by the judge,[9] and changes in the market value of debt from default to plan confirmation ($\Delta\, Debt\, MV$) using Moody's Default & Recovery Database (DRD) to measure creditors' payoffs.[10] These two variables provide evidence on how the bankruptcy process impacts creditor welfare.

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.

These costs are significantly higher in prolonged cases (see Altman et al. (2018) for a detailed discussion and survey of the literature).

[9]Debt recovery information for cases filed from 1996-2007 in the LoPucki sample is taken from Jiang et al. (2012) and we manually collect recovery rate for large cases filed from 2008-2012 using reorganization or liquidation plans confirmed by the court. Refer to their study for detailed discussion on the construction of debt recovery rate.

[10]While Moody's DRD provides information on each debt instrument's contract terms, principal default amount, and market-based measurements of recovery rates, one downside is that recovery data is only available for debt instruments that Moody's rates. This leads to a significant decline in sample size for these tests.

This comprehensive search process enables us to identify each judge's on-the-bench experience, professional experience before becoming a bankruptcy judge, and other personal attributes such as educational background, gender, 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)* 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 2Y* 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.[11] Following prior literature, we consider four measures for judges' personal characteristics and attributes, including *Top5 Law School*, a dummy variable indicating that the judge attended a top 5 law school according to the 2009 US News law school ranking; *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.[12]

3.3 Court and Judge Caseload

In several heterogeneity tests we use the mix of all past cases overseen by judges as well as measures of court caseload. 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

[11] The number of years of professional work experience is highly correlated with a judge's age when appointed to the bench. In our sample, the average judge graduated from law school at the age of 27 and over 80% of our judges graduate from law school between the age of 25 and 30.

[12] US News started regularly publishing law school rankings after 1990, which is before the majority of our bankruptcy judges went to law school. Our results are robust to using a top 10 or top 25 law school indicator. Ashenfelter et al. (1995) and Rachlinski et al. (2006) examine the effect of judges' political affiliation. Malmendier et al. (2011) and Benmelech and Frydman (2015) study CEOs with military backgrounds; Ahern and Dittmar (2012), Matsa and Miller (2013), and Faccio et al. (2016) examine CEO gender effects.

(business or personal).[13] Using this information, we 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. Given random case assignment, this is likely a close proxy to actual cases overseen by each judge. Using this method, for each large Chapter 11 filing in our sample we estimate the total number of filings across both business and personal bankruptcies, and the share of of business filings (*Bus Filings/Total Filings*) previously seen by the assigned judge.

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 et al. (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) that 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 the judge is facing.

3.4 Summary Statistics

We summarize large case characteristics in Panel A of Table 1, with detailed variable definitions presented in the Appendix. For our sample of 1,310 large 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. For 535 cases with electronic dockets, the average case has 150 motions (some filed simultaneously) and each motion takes on average 33.29 days from filing to the issue of a corresponding order. For cases with Moody's Recovery information, the average total recovery rate across debt instruments is 52.9% and the average change of debt market value from filing to plan confirmation is 17.91%. In terms of experience measures, the average judge has

[13] Both businesses and individuals can file for each chapter of bankruptcy. However, essentially all Chapter 11 filings are business filings, whereas nearly all Chapter 13 filings are personal filings. Chapter 7 filings are a mixture of both business and personal. There are a very small number of filings under Chapters 9, 12, and 15, which we group as additional non-business filings.

been on the bench for 114.49 months (standard deviation 97.18), and 14% of the large cases (180) are assigned to judges who are in their first two years.

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 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 63% are affiliated with the Democratic party. Panel C of Table 1 presents the correlation matrix for judges' general experience and personal attributes. The evidence suggests that judges who went to a top law school tend to have more prior work experience. Male judges are more likely to have served in the military, are less likely to be democrats, and tend to have more prior work experience.

4 Analysis

4.1 Randomization Tests

The key identifying assumption in our empirical strategy is that, because judges are randomly assigned to bankruptcy cases, confounding factors do not affect case outcomes in the same time-varying manner as judges' tenure. 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. We focus on large corporate bankruptcies where judge experience is likely to matter most due to the complexity of the cases. Although bankruptcy courts assert that all cases are randomly assigned to judges, it is plausible that large firms are able to affect this allocation mechanism, as discussed in Section 2.2, which would raise endogeneity concerns. In this section, we empirically test whether *large* Chapter 11 cases are randomly assigned to judges.

Because our research question examines the effects of job-specific experience, the primary concern is whether cases are randomly assigned with respect to judge tenure. If case assignment is independent of tenure, 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 job-specific experience. We test for such random assignment by estimating linear probability models of the following form:

$$\text{Assigned}_{i,j} = \alpha + \beta_1 \text{JudgeExp}_{i,j} + \theta \text{Case FE} + \epsilon_{i,j} \tag{1}$$

where $Assigned_{i,j}$ is an indicator variable which equals one if judge i was assigned case j, and zero otherwise. $JudgeExp_{i,j}$ is one of four measures that capture judge i's court-level experience at the time case j was filed. The first two measures capture judge i's tenure (*Log(Months)* and *First 2Y*). We also examine whether cases are assigned randomly with respect to judge caseload using the number of large cases already assigned to judge i and not yet confirmed at the time case j was filed (*Caseload large*). Finally, we examine random assignment with respect to industry-level experience using the number of large confirmed cases from the same two-digit SIC industry previously assigned to judge i (*Industry Experience*). We include case fixed effects and cluster standard errors by court.

If more experienced judges are more often assigned large cases, then we would expect the coefficient β_1 to be positive for *Log(Months)* and negative for *First 2Y*. If courts tend to not assign cases to judges with a heavy caseload, then the coefficient β_1 will be negative for the measure *Caseload large*, and if courts assign cases to judges with experience ruling on cases from the same industry, then the coefficient β_1 will be positive for *Industry Experience*. A lack of any significant relationship for these measures is consistent with random assignment with respect to tenure, caseload, and industry-level experience.

To identify the set of eligible judges when a case was filed, we combine our sample with Lexis Nexis data of all corporate bankruptcy filings to identify judges contemporaneously serving in that court. Because the Lexis Nexis data is incomplete prior to 1993, we exclude all large cases filed prior to 1993 from these randomization tests. Identifying eligible judges is complicated, however, 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.[14] 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 the same probability of being assigned a large Chapter 11 case as the court's own bankruptcy judges. Empirically we find that visiting judges are assigned only a small number of large Chapter 11 cases.[15] Including visiting and district judges in the set of eligible judges thus likely overstates the number of potential judges that could be assigned 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 visiting judges in the set of eligible judges for their home court). Finally, for judges appointed prior to 1980, we have incomplete information on the number and type of large cases they saw prior to 1980. We thus drop all cases for which we have incomplete information on any of the eligible judges, and also drop 15 courts that have only one large case. Due to the use of case fixed effects, cases with only one eligible judge are also automatically dropped. Our final randomization sample consists of 6,136 case-judge links representing 51 bankruptcy courts and 1,028 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 four judge experience measures, we find that judge tenure, current caseload, and industry experience are unrelated to large case assignment, consistent with assignment being independent of these judge experience measures. In Appendix Table A1 we examine whether the insignificant relationship between experience and case assignment persists in seven subsamples: 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

[14] Visiting judges are sometimes used by bankruptcy courts that have abnormally large caseloads relative to their capacity. For example, Delaware saw the number of bankruptcy filings rise sharply in the late 1990's and as a result recruited visiting judges from other districts and recalled retired judges. 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.

[15] For seven of the eight courts with visiting judges, visiting judges saw a minority of large Chapter 11 filings (e.g., three large cases in the northern district of California were assigned to visiting judges, compared to 51 large cases assigned to the court's own judges).

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. The results are generally consistent with case assignment that is independent of judge experience.[16] 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 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 with respect to experience, 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 large cases and estimate regressions of the following form:

$$\text{Experience}_{i,j} = \alpha + \beta_1 \text{Firm Characteristics} + \delta \text{Industry FE} + \theta \text{Court FE} + \epsilon_{i,j} \tag{2}$$

where $Experience_{i,j}$ is one of the two measures of judge i's tenure at the time case j was filed, and *Firm Characteristics* include $Log(Assets)$ (the log number of assets in 2016 dollars upon filing for Chapter 11) and $Log(NumFiling)$ (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. 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 and solicitations of vote have largely been completed before the bankruptcy is filed. Columns (3) and (4) present the coefficient estimates using the

[16] We note three possible deviations from random assignment: large cases in Delaware are marginally less likely to be assigned to a judge with a large caseload, and judges with relevant industry experience are more likely to be assigned a smaller case (i.e., between \$50 million and \$500 million in assets at time of filing) or assigned a case if they are not in Delaware or the Southern District of New York.

non-prepack sample and none of the firm characteristics are statistically significant. We include a prepack indicator in all our main analysis. 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)* in column (1) and remains at 0.07 in column (3). For the *First 2Y* 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 the footnotes of Panel B. Thus, case characteristics, including *Prepack/Preneg*, explain less than 2% of the variation in judge experience. Combined with the evidence in Panel A, our analysis suggests that large cases are most likely randomly assigned to judges with respect to tenure, caseload, and industry experience.

4.2 Main Results

We focus on *Duration* and *Ave Days(Ruling)* as the principal outcome variables and use *Emergence*, $Refile3Y$, *Total Recovery*, and Δ *Debt MV* as supplementary measures. *Duration* affects overall bankruptcy costs and can be feasibly impacted by a judge's experience and *Ave Days(Ruling)* specifically captures the length of time that it takes for a judge to rule on motions. The combined analysis of *Emergence* and $Refile3Y$ measures "efficient" reorganizations, and the combined analysis of *Total Recovery* and Δ *Debt MV* measures creditors' welfare.

To test the impact of judges' on-the-bench experience on large Chapter 11 case outcomes, we estimate regressions of the following form:[17]

$$\text{Outcome}_{i,j} = \alpha + \beta_1 \text{JudgeExp}_{i,j} + \beta_2 \text{Controls} + \delta \text{FEs} + \epsilon_{i,j} \tag{3}$$

[17]For outcomes that are indicator variables we use linear probability models.

using the $Outcome_{i,j}$ and $JudgeExp_{i,j}$ measures mentioned previously.[18] We include a time trend to control for trends in bankruptcy outcomes (Bharath et al. (2010)), as well as a Post-BAPCPA dummy, as the Bankruptcy Abuse Prevention and Consumer Protection Act (BAPCPA) altered some laws with regards to Chapter 11. We continue to include the case controls previously defined as well as court and industry (Fama French 12) fixed effects. We cluster standard errors by court.

Panel A of 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, with more experienced judges processing large cases significantly faster. We estimate that the elasticity of *Duration* with respect to $Log(Months)$ 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 one month relative to the mean *Duration* of 16.5 months. Meanwhile, the coefficients on $First\,2Y$ show that the impact of experience on duration is significantly higher at the beginning of a judge's term: large 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.[19] This increased time in bankruptcy represents an additional $7.5 million in legal fees alone for the average case in our sample.[20]

In Panel B of Table 3 we closely examine actions taken by judges to reduce *Duration*. In these regressions, the dependent variable is the average number of days from the filing of a motion until the judge issues a corresponding order. Consistent with Panel A, we find that less experienced judges take longer to rule on motions. We estimate that a judge with twice as much experience will issue an order on average 1.5 days sooner across all motions, and a judge with less than 2 years of experience will take 3.4 days longer to issue an order. Given that the average number of days

[18] In untabulated results we also examined two experience measures based on the number of large Chapter 11 filings previously assigned to the judge (*Log(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 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. We explore this explanation further in Section 4.4 below.

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

[20] Prior research suggests that legal fees represent approximately 2% of assets (LoPucki and Doherty (2004); Bris et al. (2006)). Based on average assets and durations in our sample, per month legal fees are approximately $2.5 million.

between motion and order is 33.3, these economic magnitudes represent a decrease of 4.5% and an increase of 10.2%, respectively, and are comparable to the overall effects of experience on *Duration*. Thus, a significant portion of the overall decrease in bankruptcy durations due to experience is driven by experienced judges' ability to rule more quickly on each motion brought before the court. Furthermore, we find no relation between the total number of motions filed and judge experience (see Appendix Table A3). The combined evidence suggests that reduced bankruptcy durations are driven by improvements in judges' efficiency at handling specific tasks as they gain experience.

Table 4 presents the analysis of $Emergence$ and $Refile3Y$. We find that large cases assigned to judges with more time on the bench are significantly more likely to emerge. In terms of economic magnitudes, a one-standard-deviation increase in *Log(Months)* 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 large 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. A higher rate of emergence could be consistent with more experienced judges being more lenient, allowing less viable firms to emerge from bankruptcy rather than being liquidated. However, we find no evidence that more experienced judges are associated with higher refiling rates. Taken together, the evidence suggests that experienced judges help improve the likelihood that firms emerge from bankruptcy, but not at the cost of higher refiling rates.

To demonstrate the robustness of our main findings with respect to prepackaged and prenegotiated cases, we present regression estimates in Appendix Table A4 that exclude prepackaged and prenegotiated cases. Appendix Table A4 delivers the same message as our main tests: more experienced judges process large cases significantly faster, their large cases are more likely to emerge from Chapter 11, and the refile rates for their emerged large cases are similar. Moreover, we perform the same analysis by removing the largest 20% cases in asset size and present the results in Appendix Table A5. The results in these two tables are similar to the ones using the full sample, alleviating concerns that a few extremely large cases drive our main results.

Another 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. In 1996, Congress amended the Bankruptcy Amendments and Federal Judgeship Act of 1984 (BAFJA) to incorporate a presumption of reappointment, under which the court of appeals considers whether to reappoint an incumbent judge seeking reappointment before considering other possible candidates. This concern reflects a potential selection issue where better judges get reappointed and are therefore associated with more efficient outcomes. We present robustness tests in Appendix Table A6 that only include large cases assigned to judges during their first term. The effect of judge experience on large case outcomes is robust to excluding highly experienced judges.

Although the evidence in Table 4 shows that more experienced judges allow more firms to emerge without increased refiling rates, the overall welfare gain from judge experience is not clear from these regressions because we cannot observe what happens to the assets of liquidated firms. To provide suggestive evidence on creditors' welfare, regardless of case outcomes, we examine the effects of judge experience on debt recovery rates at case resolution and the change in debt market value from bankruptcy filing to plan confirmation. Table 5 presents coefficient estimates from regressions of both $Total\ Recovery$ and $\Delta\ Debt\ MV$ on our two time-based experience measures (*Log(Months)* and *First 2Y*). For both outcome measures, the coefficient for *First 2Y* is statistically significant at either the 5% or 10% level with firm-level and industry controls, while *Log(Months)* is statistically significant in only column (1) for debt recovery. The reduced significance for *Log(Months)* is potentially due to the reduced sample sizes in these regressions, as described in Section 3.1, and a non-linear effect that concentrates in judges' first two years. In terms of economic magnitude, coefficient estimates in column (4) of both panels suggest that creditors recover 4.9% less at plan confirmation and that their bonds experience 19% lower returns throughout the restructuring process if the judge is inexperienced. Our evidence is consistent with less experienced judges having a negative effect on creditors' welfare.

Using these estimates, we conduct a back-of-the-envelope calculation to estimate total costs due to judge inexperience. For each year, we sort our sample large cases by total assets and then "assign" large cases based on judge experience, with the least experienced judge receiving the smallest case. We calculate counterfactual bankruptcy costs as 4.9% of the asset value for the cases assigned to inexperienced judges by non-random assignment, and compare this to the estimated realized loss of 4.9% of asset value for cases that were actually assigned an inexperienced judge. Summing across all sample years, we estimate the additional bankruptcy costs associated with judge inexperience amount to \$17.32 billion in our sample.

Overall, the evidence suggests that as judges accumulate experience on the bench they become more efficient, with large cases realizing shorter time in bankruptcy as judges rule faster on motions, higher likelihoods of emerging from bankruptcy with similar refiling probabilities, and better recovery rates for creditors.

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 2Y*), third or fourth year (*Year3-4*), or fifth or sixth year (*Year5-6*) on the bench. We include all three dummy variables as measures of judge experience in equation 3, where the omitted category, and thus benchmark, is the average outcome of cases assigned to judges with more than six years experience. We continue to include both the control variables as well as court and industry fixed effects and cluster 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 of new judges become indistinguishable from the case outcomes of more experienced judges.

The results are presented in Table 6. In column (1), the impact of judges' time on the bench on *Duration* displays a declining trend, with the magnitude 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 coefficient estimates translate into 21.8% longer durations (3.6 months) in the first two years and 16.5% longer durations (2.7 months) in years 3–4, respectively. The statistically insignificant coefficient on dummy *Year5-6* suggests that the duration outcome does not differ between large 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 2Y* 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 a similar monotonic pattern with regard to *Ave Days(Ruling)*. Judges take almost 4 days longer to rule on each motion during their first two years, only slightly reducing to 3.2 days in years 3–4 (although this coefficient estimate is only marginally significant). By years 5–6, the effect is much smaller at 1.5 days.[21]

The combined evidence suggests that judges' learning concentrates in their first two years, but that it can take up to four years for a judge to manage large cases in a manner similar to more experienced judges. This is a significant length of time, as judges are only appointed to 14 year terms. The long learning curve is also surprising, given the average new bankruptcy judge has 18 years of relevant work experience. Finally, the curvature of the learning curve supports our identification assumption, as potential confounding factors such as judges' biases and cognitive abilities are unlikely to affect case outcomes in the same time-varying manner as judges' tenure.[22]

4.4 Learning Accelerators

The results presented to this point demonstrate that judges with more time on the bench are able to resolve large bankruptcy cases more quickly and that it takes up to four years for judges to accumulate enough job-specific experience to be efficient. In this section, we examine *how* judges can accelerate their learning in the early stage of their judicial career by focusing on two hypotheses based on insights from the learning by doing and human capital literatures (Arrow (1962); Becker (1962); Lazear (2009)).

[21] In Appendix TableA7 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 this subsample.

[22] Dobbie and Song (2015) and Bernstein et al. (2017) show that judges' biases with respect to case emergence is not time-varying. 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 a judge's tenure, as these factors should affect judges' ruling throughout the entire 14-year term.

First, we posit that judges who quickly accrue the most relevant experience move up the learning curve for efficiently managing large cases more quickly. Judges 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 gain more relevant experience compared to judges who spend the majority of their time on non-business bankruptcies. We thus predict that, conditional on the length of tenure, 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 tasks is useful, there are likely diminishing returns to seeing a large number of similar business cases. Arrow (1962) emphasizes that "to have steadily increasing performance ... the stimulus situations must themselves be steadily evolving" (p. 156). In order for judges to continue learning and "move up the learning curve," we thus also predict that judges with exposure to a greater diversity of business cases learn faster.[23] We empirically proxy for case diversity along two dimensions: the size and industry of bankrupt firms. Judges who oversee a greater diversity 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 alternative explanations, as it is unlikely that the effects of alternative factors 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 large cases assigned to judges in either their first four years (309 cases) or first six years (444 cases). These subsamples are sufficiently large for empirical analysis, yet also contain judges with relatively little time on the bench who simultaneously exhibit significant variations in their types of experience.

[23]Management studies (e.g. Campion et al. (1994)) suggest that the exposure of employees and managers to a variety of tasks and experiences in the context of job rotation stimulates faster development of their professional skills.

We focus on case duration, our primary measure of judge efficiency, as the outcome in this analysis. As discussed in section 3.3, we develop judge-specific measures of past experience using aggregate filing statistics for each bankruptcy court because data on the specific cases assigned to each judge is unavailable. In Table 7 Panel A column 1, 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. In Panel B 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 either specification, a one-standard-deviation increase in the share of business cases leads to approximately 0.89 fewer months (5.4% of the sample average) in bankruptcy.[24]

The remaining columns of both panels of Table 7 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*), defined as one minus the Herfindahl concentration index. Results in column (2) 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.

Next, 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, 250-499, 500-999, 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

[24] 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.

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. Our evidence shows that judges' exposure to relevant tasks and a variety of tasks during their early years as a judge help accelerate their learning.

4.5 Judges' Other Experiences and Personal Attributes

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 duration to draw inferences on the relative importance of the skill types and personal traits. Specifically, we rerun our main regression after including additional judge characteristics. We proxy for judges' prior professional experience using *Log(Years before Bench)* and consider four personal characteristics (*Top 5 Lawschool*, *Male*, *Miltary*, *Democrat*). We include our measures of specific expertise developed from time on the bench, and case controls as well as court and industry fixed effects. The results are presented in Table 8, with columns (1)-(2) depicting the baseline results from Tables 3. In column (3)-(4) we add the judges' prior professional experience, and in column (5)-(6) we add the other personal characteristics measures.

We find that including these additional characteristics as controls does not reduce the significance of our time-based job-specific experience measures. Across the three panels, both the economic magnitude and statistical significance of *Log(Months)* remains stable across the different specifications that include these additional judge characteristics. Further, we do not find that previous work experience has a large effect on *Duration*, in sharp contrast to the effects of job-specific experience. However, prior general experience clearly should matter, as a new judge hired with limited general experience would face a very steep learning curve. To test this conjecture empirically, we interact the three dummies variables (*First 2Y*), (*Year3-4*), and (*Year5-6*) with *Log(Years before Bench)* and run regression similar to those in Table 6 to quantify whether the slope of learning curve differs for judges that have longer prior work experience. Appendix Table A8 shows that the coefficients for all interacted variables are largely negative and statistically significant at the 5% or 10% level for the *Duration* outcome. The combined evidence suggests that although prior work experience does not appear to have a direct effect on bankruptcy outcomes, it helps accelerate judges' accumulation of job-specific skills.

Examining the effect of personal characteristics, we find that time in bankruptcy is shorter when cases are assigned to male judges, consistent with judge time-invariant preferences (Chang and Schoar (2013); Dobbie and Song (2015); Bernstein et al. (2017, 2018)). The economic magnitude is fairly significant, with male judges processing cases 15% faster, which corresponds to 2.47 months relative to the sample mean. We do not find any significant relationship between *Duration* and *Top 5 Lawschool*, *Military*, or *Democrat*.

4.6 Court Caseload

The combined evidence thus far is consistent with judges becoming more efficient at handling large cases as they gain job-specific experience. An alternative mechanism potentially driving our empirical findings is lawyers learning over time about judges' decision-making. In this section we provide suggestive evidence to rule out this alternative mechanism by examining the relative importance of judges' job specific experience for periods of 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)). A rise in caseload also 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 judges' close attention and often daily rulings. 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. In contrast, if the effect of judge experience on case outcomes is driven by lawyers learning about judges' decision making, the effect of judge 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 9 presents results that split our full sample by median court caseload in the sample. The *High* group includes cases with a court caseload above or equal to the median value, and the *Low* group includes cases with caseload below the median value. We continue to include case controls and court and industry fixed effects. We find that the impact of judges' on-the-bench experience is more important in periods with above-median caseloads. Panel A shows that judge experience, measured by *Log(Months)*, significantly reduces *Duration* and *Ave Days(Ruling)* 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, we find similar evidence when measuring experience by the *First 2Y* dummy. On average, the effect of experience is 3 to 5 times larger in the high caseload group. The evidence suggests that experience matters most when courts are busiest, which is more consistent with the learning-by-doing hypothesis rather than lawyers learning judges' preferences and style.

5 Conclusion

We provide evidence of learning by doing and estimate costs of inexperience by exploiting the random assignment of large Chapter 11 filings to bankruptcy judges. We provide evidence that the assignment of large bankruptcy cases is independent of judges' tenure, and find that cases assigned to more experienced judges spend less time in bankruptcy, due principally to the judge's ability to rule faster on individual motions. These cases are also more likely to be kept as a going-concern but are not more likely to refile for bankruptcy after emergence, and realize higher creditor recovery rates, consistent with increased judicial efficiency for these large cases.

Our estimates of judges' learning curve suggest that it takes on average up 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' non-judicial experience and personal attributes are not consistently related to bankruptcy outcomes and do not explain our findings. Our evidence collectively suggests that judges' job-specific skills developed while serving as a bankruptcy judge matter more than prior general skills, and that judges perfect these specific skills while serving on the bench. Our findings have potential implications for the bankruptcy filing process (e.g., proposed Bankruptcy Venue Reform Act of 2018), assignment of cases to judges, and recruitment and training of new bankruptcy judges.

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Table 1: Summary Statistics

Panel A presents the summary statistics for the 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
Ave Days(Ruling)	535	33.29	29.69	24.06	16.00	53.16
Log(Num of Motion)	535	4.49	4.53	1.12	3.14	5.87
Num of Motion	535	149.79	93.00	162.61	23.00	354.00
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
Total Recovery	451	52.90	50.00	35.26	0.60	100.00
Δ Debt MV (%)	335	17.91	1.08	86.42	-80.90	149.10
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
Log(Num filing)	1,259	1.38	1.10	1.31	0.00	3.22
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	2.83	2.89	0.45	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
Democratic	206	0.63	1.00	0.48	0.00	1.00

Panel C: Correlation Matrix

	Years before Bench	Top 5 Law School	Male	Military	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	
Democratic	0.06	0.03	-0.16*	-0.12	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, $Assigned_{i,j}$, is an indicator equal to one if judge i was assigned to case j, zero otherwise. We regress this assignment indicator on four separate measures of judge experience/activity: the log number of months the judge has been on the bench ($Log(Months)$), an indicator for the first two years of a judge's tenure (*First 2Y*), the number of large cases currently assigned to the judge but not yet confirmed ($Caseload\,Large$), and the number of large confirmed cases previously assigned to the judge from the same two-digit SIC industry (*Industry Experience*). 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. We include t-stats in the parentheses and *, **, *** indicate 10%, 5%, and 1% statistical significance, respectively.

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

Panel A: Randomization

	(1) Assigned	(2) Assigned	(3) Assigned	(4) Assigned
Log(Months)	0.003			
	(0.46)			
First 2Y		-0.008		
		(-0.56)		
Caseload large			-0.003	
			(-0.44)	
Industry experience				0.006
				(1.15)
Observations	5,929	5,929	6,136	5,862
R-Squared	0.10	0.10	0.10	0.09
Case FE	Yes	Yes	Yes	Yes

Panel B: Firm Characteristics

	Full		No Prepacks	
	Log(Months)	First 2Y	Log(Months)	First 2Y
Log(Assets)	0.022	0.002	0.039	-0.007
	(1.00)	(0.26)	(1.45)	(-0.79)
Log(Num filing)	0.000	-0.002	-0.011	0.009
	(0.02)	(-0.24)	(-0.47)	(1.19)
Leverage filing	0.079	-0.007	0.092	-0.015
	(0.75)	(-0.22)	(0.69)	(-0.47)
ROA filing	0.158	-0.040	0.228	-0.043
	(1.19)	(-1.05)	(1.44)	(-1.05)
Prepack/Preneg	0.252***	-0.043***		
	(4.06)	(-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 two outcome variables: the log number of months a case spends under Chapter 11 (*Duration*) in Panel A, and the average days from motion filing (excluding filing date motions) to the passing of a corresponding order (*AveDays*(*Ruling*)) in Panel B. The two judge experience measures include: the log number of months the judge has been on the bench (*Log*(*Months*)) and an indicator for the first two year's of a judge's tenure (*First 2Y*). Court and industry fixed effects are included in each regression. Standard errors are clustered at the court level. We include t-stats in the parentheses and *, **, *** indicate 10%, 5%, and 1% statistical significance, respectively.

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

Panel A: Duration

	(1) Log(Months)	(2) First 2Y	(3) Log(Months)	(4) First 2Y
Experience Measure	-0.099***	0.215***	-0.055***	0.166**
	(-6.45)	(3.18)	(-4.28)	(2.41)
Post-BAPCPA	0.047	0.070	0.116**	0.127**
	(0.75)	(1.08)	(2.18)	(2.26)
Time Trend	-0.025**	-0.028***	-0.023***	-0.024***
	(-2.61)	(-2.74)	(-3.34)	(-3.43)
Log(Assets)			0.084***	0.082***
			(4.32)	(4.21)
Log(Num filing)			0.044***	0.044***
			(3.51)	(3.39)
Leverage filing			-0.146**	-0.150**
			(-2.62)	(-2.66)
ROA filing			-0.110**	-0.113***
			(-2.64)	(-2.79)
Prepack/Preneg			-1.190***	-1.196***
			(-17.92)	(-17.57)
Observations	1274	1274	1153	1153
Adjusted R^2	0.08	0.07	0.41	0.41
Industry FE	No	No	Yes	Yes
Court FE	Yes	Yes	Yes	Yes

Panel B: Ave Days(Ruling)

	(1) Log(Months)	(2) First 2Y	(3) Log(Months)	(4) First 2Y
Experience Measure	-1.024*	2.911	-1.458**	3.401**
	(-2.02)	(1.63)	(-2.24)	(2.17)
Post-BAPCPA	4.916	4.838	11.137	11.149
	(0.80)	(0.78)	(1.57)	(1.51)
Time Trend	-1.483*	-1.455	-2.369**	-2.352**
	(-1.73)	(-1.64)	(-2.57)	(-2.38)
Log(Assets)			0.579	0.568*
			(1.69)	(1.77)
Log(Num filing)			1.466*	1.427*
			(1.86)	(1.81)
Leverage filing			-3.452*	-3.532*
			(-1.76)	(-1.79)
ROA filing			-1.945	-1.885
			(-0.36)	(-0.34)
Prepack/Preneg			-5.801***	-5.980***
			(-6.62)	(-7.00)
Observations	510	510	477	477
Adjusted R^2	0.21	0.21	0.25	0.25
Industry FE	No	No	Yes	Yes
Court FE	Yes	Yes	Yes	Yes

Table 4: Emergence and Refile

This table presents linear probability regression estimates of dummy variables indicating a firm emerges from Chapter 11 (*Emergence*) in Panel A and a firm refile for bankruptcy within 3 years after emergency ($Refile3Y$) in Panel B. The two judge experience measures include: the log number of months the judge has been on the bench ($Log(Months)$) and an indicator for the first two year's of a judge's tenure ($First\ 2Y$). Court and industry fixed effects are included in each regression. Standard errors are clustered at the court level. We include t-stats in the parentheses and *, **, *** indicate 10%, 5%, and 1% statistical significance, respectively.

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

Panel A: Emergence

	(1)	(2)	(3)	(4)
	Log(Months)	First 2Y	Log(Months)	First 2Y
Experience Measure	0.041***	-0.062**	0.026**	-0.065*
	(3.66)	(-2.25)	(2.05)	(-1.68)
Post-BAPCPA	0.122**	0.112**	0.077	0.072
	(2.40)	(2.18)	(1.47)	(1.34)
Time Trend	-0.018***	-0.017***	-0.020***	-0.019***
	(-4.11)	(-3.69)	(-5.77)	(-5.60)
Log(Assets)			0.059***	0.060***
			(4.75)	(4.80)
Log(Num filing)			0.023**	0.023***
			(2.54)	(2.68)
Leverage filing			0.154***	0.156***
			(6.17)	(6.60)
ROA filing			0.047	0.048
			(0.87)	(0.93)
Prepack/Preneg			0.296***	0.299***
			(14.25)	(14.53)
Observations	1274	1274	1153	1153
Adjusted R^2	0.06	0.05	0.20	0.20
Industry FE	No	No	Yes	Yes
Court FE	Yes	Yes	Yes	Yes

Panel B: Refile 3Y

	(1)	(2)	(3)	(4)
	Log(Months)	First 2Y	Log(Months)	First 2Y
Experience Measure	0.010	0.015	0.010	0.011
	(1.33)	(0.47)	(0.97)	(0.29)
Post-BAPCPA	0.023	0.020	0.034	0.032
	(0.66)	(0.58)	(0.86)	(0.82)
Time Trend	-0.004	-0.003	-0.005	-0.004
	(-1.22)	(-1.04)	(-1.17)	(-1.07)
Log(Assets)			-0.008	-0.008
			(-1.42)	(-1.37)
Log(Num filing)			0.012	0.012
			(1.43)	(1.48)
Leverage filing			0.036	0.038
			(1.31)	(1.36)
ROA filing			0.013	0.016
			(0.51)	(0.59)
Prepack/Preneg			0.046*	0.048*
			(1.72)	(1.79)
Observations	697	697	624	624
Adjusted R^2	-0.01	-0.01	0.02	0.01
Industry FE	No	No	Yes	Yes
Court FE	Yes	Yes	Yes	Yes

Table 5: Debt Recovery

This table presents OLS regression estimates of recovery rate: the average recovery rate across all debt instrumented listed at plan confirmation ($TotalRecovery$) in Panel A and changes in the debt market value from default to plan confirmation ($\Delta DebtMv$) in Panel B. The two judge experience measures include: the log number of months the judge has been on the bench ($Log(Months)$) and an indicator for the first two year's of a judge's tenure ($First\ 2Y$). Court and industry fixed effects are included in each regression. Standard errors are clustered at the court level. We include t-stats in the parentheses and *, **, *** indicate 10%, 5%, and 1% statistical significance, respectively.

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

Panel A: Total Recovery (%)

	(1) Log(Months)	(2) First 2Y	(3) Log(Months)	(4) First 2Y
Experience Measure	2.006*	-6.423*	1.180	-4.948*
	(1.96)	(-2.02)	(1.19)	(-1.72)
Time Trend	0.115	0.116	0.306	0.287
	(0.18)	(0.18)	(0.33)	(0.30)
Post-BAPCPA	3.123	2.856	2.149	2.099
	(0.41)	(0.38)	(0.19)	(0.18)
Log(Assets)			1.620	1.661
			(1.04)	(1.06)
Log(Num filing)			-3.937***	-3.918***
			(-5.23)	(-5.20)
Leverage filing			-0.508	-0.551
			(-0.12)	(-0.13)
ROA filing			9.007*	8.863
			(1.73)	(1.60)
Prepack/Preneg			12.996**	13.034**
			(2.41)	(2.44)
Observations	435	435	405	405
Adjusted R^2	0.02	0.02	0.07	0.07
Industry FE	No	No	Yes	Yes
Court FE	Yes	Yes	Yes	Yes

Panel B: Δ Debt MV (%)

	(1) Log(Months)	(2) First 2Y	(3) Log(Months)	(4) First 2Y
Experience Measure	0.591	-12.190**	-0.232	-18.950***
	(0.25)	(-2.09)	(-0.06)	(-3.87)
Time Trend	0.578	0.623	-0.335	-0.323
	(0.46)	(0.47)	(-0.27)	(-0.26)
Post-BAPCPA	-4.423	-5.652	12.858	11.635
	(-0.27)	(-0.33)	(1.11)	(0.98)
Log(Assets)			15.789***	15.900***
			(3.71)	(3.78)
Log(Num filing)			-11.265**	-11.253**
			(-2.38)	(-2.39)
Leverage filing			5.420	5.548
			(0.34)	(0.37)
ROA filing			4.647	5.566
			(0.51)	(0.63)
Prepack/Preneg			13.724	12.052
			(1.64)	(1.42)
Observations	314	314	289	289
Adjusted R^2	-0.01	-0.00	0.09	0.09
Industry FE	No	No	Yes	Yes
Court FE	Yes	Yes	Yes	Yes

Table 6: Learning Curve

This table presents regression estimates of judge experience measures indicating first two years, years 3-4, and years 5-6 on two outcome variables: the log number of months a case spends under Chapter 11 (*Duration*) and the average days from motion filing (excluding filing date motions) to the passing of a corresponding order (*AveDays*(*Ruling*)). Court and industry fixed effects are included in each regression. Standard errors are clustered at the court level. We include t-stats in the parentheses and *, **, *** indicate 10%, 5%, and 1% statistical significance, respectively.

$$\text{Outcomes}_{i,j} = \alpha + \beta_1 \text{Year1-2}_{i,j} + \beta_2 \text{Year3-4}_{i,j} + \beta_3 \text{Year5-6}_{i,j} + \beta_4 \text{Controls} + \delta \text{Industry FE} + \theta \text{Court FE} + \epsilon_{i,j}$$

	(1) Duration	(2) Ave Days(Ruling)
First 2Y	0.197***	3.914**
	(3.34)	(2.07)
Year3-4	0.153**	3.233
	(2.48)	(1.62)
Year5-6	0.009	1.502
	(0.16)	(0.82)
Log(Assets)	0.085***	0.489
	(4.37)	(1.35)
Log(Num filing)	0.044***	1.432*
	(3.38)	(1.77)
Leverage filing	-0.148**	-3.527*
	(-2.66)	(-1.70)
ROA filing	-0.113**	-1.913
	(-2.63)	(-0.37)
Prepack/Preneg	-1.193***	-6.374***
	(-17.91)	(-7.22)
Time Trend	-0.023***	-2.655**
	(-3.28)	(-2.57)
Post-BAPCPA	0.113**	13.038*
	(2.01)	(1.72)
Observations	1,153	482
Adj R-Squared	0.41	0.25
Industry FE	Yes	Yes
Court FE	Yes	Yes
P(Y12=Y34)	0.657	0.716
P(Y12=Y56)	0.045	0.360
P(Y34=Y56)	0.024	0.400
P(Y12=Y34=Y56)	0.027	0.629

Table 7: Learning Accelerators

This table presents regression estimates of the effects of judge experience on *duration*, measured as the log number of months a case spends under Chapter 11. We estimate the types of cases previously seen by each judge using historical court-level filings and diversity of local businesses using census data at each court level. Panel A includes all cases assigned to judges during their first four years on the bench, and Panel B includes all cases assigned to judges during their first six years on the bench. All explanatory variables are standardized. Filing year fixed effects are included in each regression. Case controls include $Log(Assets)$, $Log(Num\ Filing)$, $Leveragefiling$, $ROAfiling$, and $Prepack/preneg$. Standard errors are clustered at the court level. We include t-stats in the parentheses and *, **, *** indicate 10%, 5%, and 1% statistical significance, respectively.

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

Panel A: First Four Years

	(1)	(2)	(3)
Past Total Filings	0.00	0.04	0.06
	(0.07)	(0.82)	(1.34)
Bus Filings/Total Filings	-0.12**	-0.10**	-0.11***
	(-2.63)	(-2.28)	(-2.82)
Diversity-Industry		-0.10**	
		(-2.08)	
Diversity-Size			-0.14***
			(-3.88)
Observations	309	309	309
Adj R-Squared	0.41	0.42	0.43
Filing Year FE	Yes	Yes	Yes
Case Controls	Yes	Yes	Yes

Panel B: First Six Years

	(1)	(2)	(3)
Past Total Filings	-0.04	-0.01	-0.00
	(-1.08)	(-0.28)	(-0.11)
Bus Filings/Total Filings	-0.11***	-0.09**	-0.10***
	(-2.83)	(-2.32)	(-2.83)
Diversity-Industry		-0.10***	
		(-2.81)	
Diversity-Size			-0.11***
			(-3.54)
Observations	444	444	444
Adj R-Squared	0.46	0.47	0.47
Filing Year FE	Yes	Yes	Yes
Case Controls	Yes	Yes	Yes

Table 8: Other Experience and Personal Attributes

This table presents regression estimates of judge on-the-bench experience versus other judge characteristics on *Duration*, measured by the log number of months a case spends under Chapter 11. Other judge Characteristics include: the log number of years from law school graduation to judge appointment ($Log(YearsbeforeBench)$), a dummy variable indicating male ($Male$), a dummy variable indicating top 5 law school ($Top5Lawschool$), a dummy variable indicating military service experience ($Miltary$), and a dummy variable indicating Democrat affiliation ($Democrat$). Controls include $Log(Assets)$, $Log(Num\ Filing)$, $Leverage\ filing$, $ROA\ filing$, $Prepack/preneg$, $Time\ trend$ and Post-BAPCPA. 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. We include t-stats in the parentheses and *, **, *** indicate 10%, 5%, and 1% statistical significance, respectively.

$$\text{Duration}_{i,j} \quad = \quad \alpha + \beta\text{Experience}_{i,j} + \beta_1\text{Characteristics} + \beta_2\text{Controls} + \delta\text{FEs} + \epsilon_{i,j}$$

	(1) Log(Months)	(2) First 2Y	(3) Log(Months)	(4) First 2Y	(5) Log(Months)	(6) First 2Y
Experience Measure	-0.055***	0.166**	-0.051***	0.160**	-0.045***	0.135*
	(-4.28)	(2.41)	(-4.09)	(2.22)	(-3.39)	(1.69)
Log(Years before Bench)			0.018	0.038	0.096	0.115*
			(0.37)	(0.71)	(1.56)	(1.76)
Top5 Lawschool					-0.050	-0.043
					(-0.71)	(-0.61)
Male					-0.186***	-0.189***
					(-3.84)	(-4.00)
Military					0.034	0.024
					(0.54)	(0.37)
Democrats					-0.005	-0.011
					(-0.08)	(-0.16)
Observations	1153	1153	1147	1147	1146	1146
Adjusted R^2	0.41	0.41	0.41	0.41	0.41	0.41
Industry FE	Yes	Yes	Yes	Yes	Yes	Yes
Court FE	Yes	Yes	Yes	Yes	Yes	Yes
Case Controls	Yes	Yes	Yes	Yes	Yes	Yes

Table 9: Sample Splitting Case Load

This table presents regression estimates of experience measures on two outcome variables by splitting the sample according to bankruptcy court caseload at filing. High (H) group includes cases with total caseload above the median, and Low (L) includes cases with caseload below the median. Two outcome variables include: the log number of months a case spends under Chapter 11 (*Duration*) and the average days from motion filing (excluding filing date motions) to the passing of a corresponding order (*AveDays*(*Ruling*)). 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. We include t-stats in the parentheses and *, **, *** indicate 10%, 5%, and 1% statistical significance, respectively.

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

Panel A: Log(Months as Judge)

	Duration		Ave Days(Ruling)	
	(1)	(2)	(3)	(4)
	High	Low	High	Low
Log(Months)	-0.058***	-0.012	-2.532***	-0.742
	(-2.80)	(-0.33)	(-6.34)	(-0.43)
Observations	571	558	239	236
Adjusted R^2	0.36	0.46	0.11	0.32
Case Controls	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes
Court FE	Yes	Yes	Yes	Yes

Panel B: First Two Years

	Duration		Ave Days(Ruling)	
	(1)	(2)	(3)	(4)
	High	Low	High	Low
First 2Y	0.193***	0.062	7.889***	1.966
	(3.32)	(0.36)	(4.44)	(0.87)
Observations	571	558	239	236
Adjusted R^2	0.36	0.46	0.10	0.32
Case Controls	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes
Court FE	Yes	Yes	Yes	Yes

Appendix

Variable Definitions

Experience Measures	
Log(Months)	Log(number of months from a judge's appointment date to the filing date of a case)
First 2Y	A dummy=1 for the first two years a judge's term
Judge Characteristics	
Democrats	A dummy variable=1 for affiliation with the Democratic party
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
Case Characteristics	
Ave Days(Ruling)	The average days from motion filing (excluding filing date motions) to the passing of a corresponding order
Duration	Log(number of months a case spent in Chapter 11)
Δ Debt MV	Change in the market value of debt from default to plan confirmation
Emergence	A dummy variable =1 for firms emerged from Chapter 11
Leverage Filing	$\frac{liabilities}{Assets}$ at filing
Log(Assets)	Log of assets dollar value at filing (in 2016 dollars)
Log(Num filing)	Log(Number of subsidiaries associated with a case at filing)
Log(Num motion)	Log(Number of motions filed with a case)
Post BAPCPA	A dummy variable=1 for cases filed after the Bankruptcy Abuse
Prepack/Preneg	A dummy variable=1 for a prepackaged or prenegotiated case
	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)
Total Recovery	The average recovery rate across all debt instruments listed in the reorganization or liquidation plan that is confirmed by the judge
Court Characteristics	
Caseload	The weighted number of bankruptcy filings in the court-quarter per judge upon filing
Bus Filings/Total Filings	The share of business filings to the total number of cases per judge
Diversity-Industry	1 minus the Herfindahl index of establishments across two-digit SIC industries
Diversity-Size	1 minus the Herfindahl index of establishments across buckets of 1-4, 5-9, 10-19, 20-49, 50-99, 100-249, 250-499, 500-999, and 1000+ employees
Past total filings	The number of cases per judge from the judge's appointment until the filing date of a case assigned to the judge

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$, column 7). The judge experience measure include: the log number of months the judge has been on the bench in Panel A ($Log(Months)$), an indicator for cases assigned during the first two years of a judge's tenure in Panel B ($First\,2Y$), the number of large cases currently assigned to the judge but not yet confirmed in Panel C ($Caseload\,large$)), and the number of confirmed cases previously assigned to the judge from the same two-digit SIC industry in Panel D ($Industry\,experience$). 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% statistical significance, respectively. The average dependent variable (likelihood a judge is assigned a case) is tabulated in the table footnotes.

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

Panel A

	(1) No Prepack	(2) Large	(3) Small	(4) NYSD/DE	(5) DE	(6) NYSD	(7) Other
Log(Months)	0.001	-0.005	0.009	-0.004	0.005	-0.008	0.011
	(0.11)	(-0.50)	(1.63)	(-0.52)	(0.31)	(-0.96)	(1.24)
Observations	5,067	2,837	3,092	2,962	1,193	1,769	2,967
R-Squared	0.10	0.10	0.10	0.12	0.12	0.00	0.07
Case FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Avg Dep Var	0.17	0.17	0.17	0.18	0.28	0.11	0.16

Panel B

	(1) No Prepack	(2) Large	(3) Small	(4) NYSD/DE	(5) DE	(6) NYSD	(7) Other
First 2Y	-0.006	0.011	-0.027	0.007	-0.004	0.012	-0.033
	(-0.35)	(0.44)	(-1.34)	(0.32)	(-0.07)	(0.52)	(-1.05)
Observations	5,067	2,837	3,092	2,962	1,193	1,769	2,967
R-Squared	0.10	0.10	0.10	0.12	0.12	0.00	0.07
Case FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Avg Dep Var	0.17	0.17	0.17	0.18	0.28	0.11	0.16

Randomization Robustness Tests (cont)

Panel C

	(1) No Prepack	(2) Large	(3) Small	(4) NYSD/DE	(5) DE	(6) NYSD	(7) Other
Caseload large	-0.003	0.001	-0.007	-0.007	-0.013*	0.002	0.026
	(-0.49)	(0.17)	(-1.00)	(-1.42)	(-1.71)	(0.41)	(1.66)
Observations	5,256	2,913	3,223	2,962	1,193	1,769	3,174
R-Squared	0.10	0.10	0.10	0.12	0.13	0.00	0.08
Case FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Avg Dep Var	0.17	0.17	0.17	0.18	0.28	0.11	0.16

Panel D

	(1) No Prepack	(2) Large	(3) Small	(4) NYSD/DE	(5) DE	(6) NYSD	(7) Other
Industry experience	0.010	-0.015	0.018***	-0.000	0.002	-0.007	0.088**
	(1.49)	(-1.23)	(4.96)	(-0.04)	(0.16)	(-0.58)	(2.40)
Observations	5,033	2,814	3,048	2,868	1,099	1,769	2,994
R-Squared	0.10	0.09	0.10	0.10	0.12	0.00	0.09
Case FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Avg Dep Var	0.17	0.17	0.17	0.18	0.28	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. We include t-stats in the parentheses and *, **, *** indicate 10%, 5%, and 1% statistical significance, respectively.

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

	(1) Assigned	(2) Assigned	(3) Assigned	(4) Assigned
Log(Months)	0.003			
	(0.51)			
First 2Y		-0.009		
		(-0.55)		
Caseload large			0.001	
			(0.10)	
Industry experience				0.006
				(1.01)
Observations	5,523	5,523	5,730	5,550
R-Squared	0.05	0.05	0.05	0.05
Case FE	Yes	Yes	Yes	Yes

Table A3: Number of Motion

This table presents regression estimates of the log number of motions for a case on our two judge experience measures: the log number of months the judge has been on the bench ($Log(Months)$) and an indicator for cases assigned during the first two years of a judge's tenure ($First\ 2Y$). Court and industry fixed effects are included in each regression. Standard errors are clustered at the court level. We include t-stats in the parentheses and *, **, *** indicate 10%, 5%, and 1% statistical significance, respectively.

$$\text{Log(Num of Motion)}_{i,j} = \alpha + \beta_1 \text{JudgeExp}_{i,j} + \beta_2 \text{Controls} + \delta \text{Industry FE} + \theta \text{Court FE} + \epsilon_{i,j}$$

	(1)	(2)	(3)	(4)
	Log(Months)	First 2Y	Log(Months)	First 2Y
Experience Measure	0.013	-0.045	0.053	-0.116
	(0.23)	(-0.36)	(0.88)	(-0.68)
Post-BAPCPA	-0.070	-0.067	0.250	0.251
	(-0.28)	(-0.26)	(1.50)	(1.56)
Time Trend	0.010	0.010	-0.052**	-0.052**
	(0.34)	(0.31)	(-2.73)	(-2.64)
Log(Assets)			0.291***	0.292***
			(8.37)	(8.61)
Log(Num filing)			0.201***	0.203***
			(3.70)	(3.61)
Leverage filing			-0.027	-0.025
			(-0.34)	(-0.30)
ROA filing			-0.163*	-0.163*
			(-1.78)	(-1.79)
Prepack/Preneg			-0.842***	-0.836***
			(-11.53)	(-11.98)
Observations	516	516	483	483
Adjusted R^2	0.10	0.10	0.50	0.50
Industry FE	No	No	Yes	Yes
Court FE	Yes	Yes	Yes	Yes

Table A4: 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 outcome variables include: the log number of months a case spends under Chapter 11 ($Duration$) in columns (1)–(2), the average days from motion filing (excluding first-day motions) to the passing of a corresponding order ($AveDays(Ruling)$) in columns (3)–(4), a dummy variable indicating a firm emerges from Chapter 11 ($Emergence$) in columns (5)–(6), and a dummy variable indicating a firm refiles for Chapter 11 within three years after emergence ($Refile3Y$) in columns (7)–(8). The two judge experience measures include: the log number of months the judge has been on the bench ($Log(Months)$) and an indicator for cases assigned during the first two years of a judge's tenure ($First\,2Y$). Court and industry fixed effects are included in each regression. Standard errors are clustered at the court level. We include t-stats in the parentheses and *, **, *** indicate 10%, 5%, and 1% statistical significance, respectively.

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

	Duration		Ave Days(Ruling)		Emergence		Refile 3Y	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Log(Months)	First 2Y	Log(Months)	First 2Y	Log(Months)	First 2Y	Log(Months)	First 2Y
Experience Measure	-0.053***	0.142***	-2.242**	3.266	0.036*	-0.083**	0.005	-0.011
	(-3.46)	(3.04)	(-2.44)	(1.24)	(1.97)	(-2.21)	(0.38)	(-0.24)
Log(Assets)	0.121***	0.120***	0.892	0.952	0.061***	0.062***	0.004	0.004
	(6.61)	(6.49)	(1.39)	(1.46)	(4.06)	(4.11)	(0.43)	(0.42)
Log(Num filing)	0.012	0.012	2.074	1.953	0.023*	0.023*	-0.002	-0.002
	(0.74)	(0.70)	(1.70)	(1.60)	(1.78)	(1.87)	(-0.20)	(-0.18)
Leverage filing	-0.119**	-0.124**	-3.011	-3.413	0.139***	0.143***	0.002	0.003
	(-2.47)	(-2.64)	(-1.26)	(-1.43)	(2.74)	(3.04)	(0.10)	(0.12)
ROA filing	-0.047	-0.056	0.300	0.013	-0.005	0.002	-0.049**	-0.048**
	(-0.73)	(-0.87)	(0.04)	(0.00)	(-0.05)	(0.03)	(-2.16)	(-2.11)
Time Trend	-0.034***	-0.035***	-2.308**	-2.437**	-0.022***	-0.021***	-0.003	-0.003
	(-6.32)	(-6.71)	(-2.14)	(-2.08)	(-4.97)	(-4.85)	(-1.04)	(-0.99)
Post-BAPCPA	0.166**	0.176**	10.564	11.432	0.078	0.071	0.003	0.002
	(2.35)	(2.52)	(1.13)	(1.14)	(1.33)	(1.22)	(0.09)	(0.07)
Log(Years before Bench)	0.019	0.042	-3.425	-2.348	0.024	0.008	-0.067**	-0.068***
	(0.37)	(0.78)	(-1.32)	(-0.90)	(0.64)	(0.22)	(-2.55)	(-2.87)
Observations	848	848	305	305	848	848	392	392
Adj R-Squared	0.18	0.18	0.26	0.25	0.14	0.14	0.03	0.03
Industry FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Court FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Table A5: Robustness Check: Removing the Largest 20% Cases

This table presents regression estimates of judge experience measures on case outcomes, removing cases that belong to the largest 20% in asset values. The outcome variables include: the log number of months a case spends under Chapter 11 ($Duration$) in columns (1)–(2), the average days from motion filing (excluding first-day motions) to the passing of a corresponding order ($AveDays(Ruling)$) in columns (3)–(4), a dummy variable indicating a firm emerges from Chapter 11 ($Emergence$) in columns (5)–(6), and a dummy variable indicating a firm refiles for Chapter 11 within three years after emergence ($Refile3Y$) in columns (7)–(8). The two judge experience measures include: the log number of months the judge has been on the bench ($Log(Months)$) and an indicator for cases assigned during the first two years of a judge's tenure ($First\,2Y$). Court and industry fixed effects are included in each regression. Standard errors are clustered at the court level. We include t-stats in the parentheses and *, **, *** indicate 10%, 5%, and 1% statistical significance, respectively.

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

	Duration		Ave Days(Ruling)		Emergence		Refile 3Y	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Log(Months)	First 2Y	Log(Months)	First 2Y	Log(Months)	First 2Y	Log(Months)	First 2Y
Experience Measure	-0.101***	0.261***	-1.213	1.274	0.043***	-0.079*	0.009	0.015
	(-6.76)	(3.14)	(-1.50)	(0.66)	(3.34)	(-1.82)	(0.90)	(0.38)
Log(Assets)	-0.065**	-0.066**	-1.409	-1.384	0.108***	0.108***	0.030***	0.030***
	(-2.26)	(-2.37)	(-1.49)	(-1.45)	(6.76)	(6.75)	(3.25)	(3.23)
Log(Num filing)	0.067***	0.069***	1.370	1.363	0.011	0.010	0.016*	0.017*
	(4.07)	(3.90)	(0.92)	(0.92)	(1.12)	(1.09)	(1.79)	(1.82)
Leverage filing	-0.270***	-0.282***	-4.015	-4.189*	0.186***	0.191***	0.047*	0.048*
	(-4.70)	(-4.74)	(-1.69)	(-1.80)	(8.28)	(9.10)	(1.80)	(1.82)
ROA filing	-0.090	-0.103	-2.333	-2.336	0.025	0.032	0.004	0.006
	(-1.36)	(-1.48)	(-0.42)	(-0.41)	(0.66)	(0.83)	(0.16)	(0.23)
Time Trend	-0.027**	-0.031***	-2.793*	-2.842*	-0.019***	-0.017***	-0.005	-0.004
	(-2.62)	(-2.87)	(-1.89)	(-1.84)	(-3.72)	(-3.38)	(-1.15)	(-1.00)
Post-BAPCPA	0.032	0.056	13.301	13.708	0.081	0.070	0.079**	0.076*
	(0.35)	(0.61)	(1.45)	(1.45)	(1.41)	(1.20)	(2.08)	(2.01)
Log(Years before Bench)	-0.015	0.030	-4.344*	-3.803*	0.027	0.006	-0.066**	-0.073***
	(-0.29)	(0.62)	(-2.03)	(-1.71)	(0.61)	(0.12)	(-2.40)	(-2.89)
Observations	904	904	361	361	904	904	471	471
Adj R-Squared	0.12	0.12	0.25	0.25	0.13	0.13	0.02	0.02
Industry FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Court FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Table A6: 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 outcome variables include: the log number of months a case spends under Chapter 11 ($Duration$) in columns (1)–(2), the average days from motion filing (excluding first-day motions) to the passing of a corresponding order ($AveDays(Ruling)$) in columns (3)–(4), a dummy variable indicating a firm emerges from Chapter 11 ($Emergence$) in columns (5)–(6), and a dummy variable indicating a firm refiles for Chapter 11 within three years after emergence ($Refile3Y$) in columns (7)–(8). The two judge experience measures include: the log number of months the judge has been on the bench ($Log(Months)$) and an indicator for cases assigned during the first two years of a judge's tenure ($First\,2Y$). Court and industry fixed effects are included in each regression. Standard errors are clustered at the court level. We include t-stats in the parentheses and *, **, *** indicate 10%, 5%, and 1% statistical significance, respectively.

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

	Duration		Ave Days(Ruling)		Emergence		Refile 3Y	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Log(Months)	First 2Y	Log(Months)	First 2Y	Log(Months)	First 2Y	Log(Months)	First 2Y
Experience Measure	-0.070***	0.166*	-1.289	3.290**	0.034***	-0.053	-0.004	0.030
	(-3.88)	(1.93)	(-1.68)	(2.25)	(3.15)	(-1.27)	(-0.32)	(0.70)
Log(Assets)	0.082***	0.079***	0.590	0.537	0.060***	0.062***	-0.006	-0.006
	(3.94)	(3.84)	(1.34)	(1.31)	(3.63)	(3.79)	(-0.98)	(-0.94)
Log(Num filing)	0.079***	0.080***	0.950	0.941	0.009	0.008	0.016**	0.016**
	(5.92)	(5.94)	(1.36)	(1.32)	(1.01)	(0.97)	(2.14)	(2.14)
Leverage filing	-0.323***	-0.328***	-6.802**	-6.746**	0.202***	0.204***	0.061**	0.061**
	(-3.62)	(-3.71)	(-2.70)	(-2.70)	(8.34)	(8.75)	(2.71)	(2.68)
ROA filing	-0.192***	-0.198***	-3.926	-3.835	0.048	0.053	0.045	0.045
	(-2.72)	(-2.80)	(-0.88)	(-0.84)	(0.86)	(0.94)	(0.98)	(0.94)
Time Trend	-0.031***	-0.033***	-1.800**	-1.750*	-0.016***	-0.015***	-0.001	-0.000
	(-3.04)	(-3.17)	(-2.11)	(-2.03)	(-4.49)	(-4.26)	(-0.14)	(-0.12)
Post-BAPCPA	0.049	0.054	4.692	4.349	0.074*	0.072*	0.004	0.006
	(0.62)	(0.68)	(0.89)	(0.83)	(1.80)	(1.71)	(0.13)	(0.18)
Log(Years before Bench)	-0.113	-0.113	-0.681	-0.924	0.052	0.052	-0.078***	-0.079***
	(-1.41)	(-1.38)	(-0.27)	(-0.34)	(1.33)	(1.37)	(-3.20)	(-3.19)
Observations	835	835	335	335	835	835	446	446
Adj R-Squared	0.13	0.13	0.08	0.08	0.13	0.13	0.02	0.02
Industry FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Court FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Table A7: Learning Curve: First-term Judges

This table presents regression estimates of judges' learning curve, including only cases assigned to judges during their first term. The two outcome variables include: the log number of months a case spends under Chapter 11 (*Duration*) and the average days from motion filing (excluding first-day motions) to the passing of a corresponding order (*AveDays*(*Ruling*)). Court and industry fixed effects are included in each regression. Standard errors are clustered at the court level. We include t-stats in the parentheses and *, **, *** indicate 10%, 5%, and 1% statistical significance, respectively.

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

	(1) Duration	(2) Ave Days(Ruling)
First 2Y	0.163**	3.945*
	(2.23)	(2.02)
Year3-4	0.145**	2.839
	(2.28)	(1.38)
Year5-6	-0.003	0.229
	(-0.05)	(0.19)
Log(Assets)	0.091***	0.504
	(4.71)	(1.38)
Log(Num filing)	0.039***	0.908
	(3.03)	(1.27)
Leverage filing	-0.164**	-5.802**
	(-2.28)	(-2.30)
ROA filing	-0.150**	-3.085
	(-2.10)	(-0.75)
Prepack/Preneg	-1.189***	-5.276***
	(-18.41)	(-7.61)
Time Trend	-0.025***	-1.703**
	(-3.50)	(-2.21)
Post-BAPCPA	0.079	4.482
	(1.27)	(0.95)
Observations	837	338
Adj R-Squared	0.40	0.09
Industry FE	Yes	Yes
Court FE	Yes	Yes
P(Y12=Y34)	0.860	0.499
P(Y12=Y56)	0.066	0.071
P(Y34=Y56)	0.020	0.185
P(Y12=Y34=Y56)	0.020	0.185

Table A8: Learning Curve: Prior Experience

This table presents regression estimates of judges' learning curve. The dependent variable is the log number of months a case spends under Chapter 11 (*Duration*). Court and industry fixed effects are included in each regression. Standard errors are clustered at the court level. We include t-stats in the parentheses and *, **, *** indicate 10%, 5%, and 1% statistical significance, respectively.

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

	(1) Duration	(2) Duration
First 2Y	1.055*	0.541***
	(1.70)	(3.24)
Year3-4	0.578**	0.416***
	(2.17)	(3.59)
Year5-6	0.837*	0.326
	(1.71)	(1.22)
First 2 Years*Log(Years before Bench)	-0.291	
	(-1.34)	
Year3-4*Log(Years before Bench)	-0.145*	
	(-1.71)	
Year5-6*Log(Years before Bench)	-0.285*	
	(-1.73)	
Log(Years before Bench)	0.094	
	(1.58)	
First 2 Years*Long experience before Bench		-0.235*
		(-1.73)
Year3-4*Long experience before Bench		-0.175**
		(-2.39)
Year5-6*Long experience before Bench		-0.212
		(-1.21)
Long experience before Bench		0.064
		(1.26)
Log(Assets)	0.087***	0.085***
	(4.49)	(4.43)
Log(Num filing)	0.042***	0.043***
	(3.20)	(3.38)
Leverage filing	-0.151***	-0.148***
	(-2.80)	(-2.81)
ROA filing	-0.109***	-0.113***
	(-2.80)	(-2.73)
Prepack/Preneg	-1.180***	-1.179***
	(-17.98)	(-18.40)
Time Trend	-0.022***	-0.022***
	(-3.45)	(-3.38)
Post-BAPCPA	0.090	0.107*
	(1.48)	(1.82)
Observations	1147	1147
Adjusted R^2	0.41	0.41
Industry FE	Yes	Yes
Court FE	Yes	Yes