Learning by Doing: Evidence from Bankruptcy Judges *

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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, and judges' general skills 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)). Quantifying the costs of on-the-job training, however, is empirically challenging because task-specific outcomes are rarely observed 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' skill, including speed of ruling, case duration, likelihood of emergence, refiling rates, and creditors' recovery rates. In addition, bankruptcy judges have significant formal education and prior legal experience, often including 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 an important decision-maker who already has significant *general* human capital.

Our sample consists of 1,304 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 306 unique bankruptcy judges in 74 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 most. For each judge in our sample, we compile information on judicial experience, previous professional experience, educational background, and personal characteristics from an array of sources including the U.S. Courts system, Linkedin, LexisNexis personal reports, press releases, and voting records. 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 empirical analysis is the random assignment of cases to bankruptcy judges. Although prior work documents random assignment for bankruptcy judges, these studies employ data sets that are dominated by small business filings (Chang and Schoar (2013); Bernstein et al. (2019)), and thus a concern is whether *large* Chapter 11 cases are randomly assigned. We review each bankruptcy court's stated policies regarding judge assignment, survey the court clerks in our sample, and conduct our own empirical analysis of case assignment. The combined evidence supports the notion that judge experience does not affect the assignment of large Chapter 11 filings.

We 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 document an elasticity of -0.054, suggesting that large cases assigned to a judge with twice as much time on the bench (e.g., from 3 to 6 years) realize a 5.4% decrease in time spent in bankruptcy, a decline of nearly one month relative to the average duration in our sample (16.6 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 (approximately three months). Importantly, we find similar effects in a larger sample of smaller Chapter 11 filings that facilitates the inclusion of judge fixed effects, suggesting that omitted and time-invariant judge characteristics do not explain these results.

To better understand *how* experienced judges move cases through bankruptcy more quickly, we examine the dockets of 533 cases filed after 2002. We identify 80,502 motions and find that, on average, 33 days pass between the filing of a motion and the assigned judge's corresponding ruling. We further document that judges in their first two years spend an additional 3.4 days on each motion, a 10% increase from the sample mean. Thus, a significant portion of the 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 is not due to more motions for inexperienced judges.

We next examine restructuring outcomes and find that large cases assigned to more experienced judges are more likely to emerge from bankruptcy. A one-standard-deviation increase in the assigned judge's time on the bench (approximately 7 years) leads to a 3.1% increase in the probability that a large case emerges from bankruptcy (5.4% of the sample average of 57%). Large cases assigned to more experienced judges are also not more likely to refile for bankruptcy within three years of emergence, have higher debt recovery rates, and realize larger increases in the value of the defaulted debt. The evidence is inconsistent with lower quality restructuring by more experienced judges (i.e., "kicking the can down the road"). Instead, the combined evidence suggests that judges become more efficient the longer they have served on the bench, resulting in more successful reorganizations and improved creditors' welfare.

We estimate the slope of judges' learning curve and find that it takes up to four years until a new judge has similar durations and average days ruling as more experienced judges. Drawing on insights from the human capital and learning-by-doing literature, we use two cross-sectional tests to better understand how judges move up this learning curve. We predict that new judges accumulate job-specific human capital faster (as manifested by shorter durations) by seeing more relevant business filings as opposed to less relevant personal filings. Due to the diminishing returns associated with learning from the repetition of essentially similar problems (Arrow (1962)), we also predict that the rate at which judges learn is increasing in the diversity of business filings to which they are exposed.

We test these predictions by analyzing all large cases assigned to judges with four or fewer years of on-the-bench experience. Due to the unique composition of each court, each of these judges have different types of on-the-bench experience but similar overall tenure. Consistent with our prediction, 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. We also find that 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. 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 faster. 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 well as estimates of judges' time-invariant characteristics. Our results highlight the importance of specific skills acquired through learning-by-doing relative to pre-existing general skills and personal attributes.

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, judges' compensation structure is flat, and there are no "promotions." Thus, there are limited incentives for judges to signal their type by working harder or learning faster and there are also 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 prior legal experience (92% of our sample judges previously worked as lawyers), new judges have no experience ruling on bankruptcy cases, allowing us to quantify the importance of learning by doing relative to prior relevant experience. Finally, because we observe outcomes over which the judge has both discretion and influence (ruling on motions, duration, restructuring, recidivism, creditor welfare, etc.), we can assess the value of judges' job-specific experience and quantify their learning curve.

A possible alternative explanation for our results is that rather than judges learning, firms and their lawyers learn over time how to work more efficiently with judges. We exploit variation in judges' caseloads to provide suggestive evidence on whether this particular economic mechanism drives our empirical findings. 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 (Iverson (2018)). In contrast, lawyers' incentives to learn should not vary with a judge's current caseload. We find that when caseloads are high, experienced judges significantly reduce case duration and average days ruling, 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.

The economic implications of learning by doing are significant. Based on estimates from prior studies, the increased time in bankruptcy represents an additional $7.5 million in legal fees alone for the average case in our sample.[1] To provide a sense of the aggregate costs of inexperience, we consider several counterfactual scenarios where cases are endogenously assigned based on a judge's experience.[2] As an alternative to full randomization, these estimates suggests that not assigning large cases to inexperienced judges could reduce direct legal fees by $812 million to $3.5 billion and increase creditor recoveries by $12.1 billion. These "back-of-the-envelope" estimates suggest that both the direct and indirect costs of inexperience may be substantial.

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

[1] 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 for the large cases in our sample are approximately $2.5 million.

[2] There are also benefits to random assignment which we cannot quantify. Such benefits should be considered along with estimated costs for policy-making purposes.

human capital (Arrow (1962); Becker (1962); Carmichael (1983); Prendergast (1993); Gibbons and Waldman (1999, 2004); Lazear (2009)). Related empirical studies estimate the dynamics and spillover effects of the learning process in a variety of settings (Jarmin (1994); Jovanovic and Nyarko (1995); Darr et al. (1995); Benkard (2000); Sinclair et al. (2000); Thornton and Thompson (2001); Levitt et al. (2013)). Well-identified empirical estimates of the costs of inexperience, however, are limited due to the endogenous matching between workers and tasks, age effects, and difficulty measuring both worker productivity and the likelihood of labor turnover (Thompson (2010)).[3] Our setting circumvents many of these institutional limitations to quantify increases in job-specific human capital. Prior research also focuses on manufacturing settings with available data on relatively simple tasks, and finds that the learning curve is only a couple months in these settings (Jovanovic and Nyarko (1995); Levitt et al. (2013)). In contrast, our study provides estimates of the costs of inexperience for economically important decision makers with substantial formal education and years of accumulated relevant experience. We find that learning takes substantially longer for bankruptcy judges who face non-standard and complex decisions, and document that task variety and complexity accelerate the learning process.

Second, our study provides evidence on the impact of judge characteristics on judicial decision-making. Most prior work focuses on judicial discretion, behavioral mistakes, and personal biases in rulings (Sharfman (2005); Gennaioli and Rossi (2010); Chen et al. (2016); Cohen and Yang (2018); Posner (2008)). Chang and Schoar (2013), Dobbie and Song (2015), and Bernstein et al. (2019) examine fixed characteristics of bankruptcy judges and their effects on case outcomes, whereas Ashenfelter et al. (1995) examine district judges. Different from these studies, we document that time-varying judicial characteristics play an important role in judicial efficiency. Two closely related studies are Rachlinski et al. (2006) and LoPucki and Doherty (2015). Rachlinski et al. (2006) use survey evidence to study the role of judicial specialization in overcoming known behavioral biases, but do not directly examine learning by doing. LoPucki and Doherty (2015) document several factors, including the number of large cases previously assigned to the judge, that affect the probability of survival for large bankrupt firms.[4]

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

[4]In untabulated results we also examine experience measures based on the number of large Chapter 11 filings previously assigned to the judge and find insignificant results, suggesting that total on-the-bench experience matters

Finally, this paper contributes to research on the differential effects of general and specific skills on corporate outcomes. A growing literature studies the effect of managers' skill (as well as individual traits and attributes) on corporate policies, managerial compensation, and mutual fund management, but the evidence is presently inconclusive as to the relative importance of each skill type for human capital value and the managerial labor market.[5] Using the setting of bankruptcy judges, our results suggest that judges—the most important "manager" of the corporate restructuring process—accumulate specific expertise through time on the job, and that this specific expertise is incremental to judges' general skills and personal attributes.

The rest of the paper is organized as follows: Section 2 describes the data sample and defines the variables; Section 3 provides institutional background on judge random assignment and related empirical verification; Section 4 presents the main results and discussions; Section 5 presents results on judge other experiences and personal attributes as well as estimations using a larger sample that includes smaller Chapter 11 filings from LexisNexis; Section 6 concludes.

2 Data and Variable Construction

2.1 Chapter 11 Sample

Our initial bankruptcy sample contains all Chapter 11 filings by public US firms with a filing date between 1980 and 2012 and that have assets of at least $50 million. We use both UCLA LoPucki Bankruptcy Research Database (BRD) and New Generation Research's bankruptcydata.com for data retrieval.[6] 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 as-

more than specific experience with large cases. Most judges seeing their first large case have already seen many smaller corporate bankruptcies, plausibly allowing them to manage large corporate cases more efficiently. We explore this explanation further in Section 4.3.

[5]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); Malmendier et al. (2011); Graham et al. (2012); Ahern and Dittmar (2012); Benmelech and Frydman (2015); Matsa and Miller (2013); Faccio et al. (2016).

[6]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.

signed to the case. We drop five cases that were not confirmed as of the beginning of 2016, 14 cases for which we cannot identify the judge at filing, 56 cases overseen by a district judge, 39 cases that were transferred to other courts, and 6 cases filed in Wisconsin (where the court's policy is not to randomize judge assignment). Our final sample comprises 1,304 Chapter 11 filings assigned to 306 unique judges located in 74 bankruptcy courts, and is one of largest samples among studies of large corporate bankruptcies. For firms that successfully reorganize and emerge from bankruptcy, we identify those that refile for Chapter 11 within three years (i.e., "Chapter 22" filings).

We use three sets of case outcome measures to capture the efficiency of bankruptcy judges. Our primary measure is *Duration*, the natural logarithm of the number of months from the Chapter 11 filing date to plan confirmation date, which proxies for the overall costs of restructuring.[7] To study how judges affect *Duration*, we gather docket information from PACER for 533 cases with electronic dockets (typically available for cases filed after 2002). Bankruptcy dockets allow us to link all motions filed (e.g., compensation issues, post-petition financing, asset sales and liquidation, creditor valuation disputes, reorganization plans, etc.) with the judicial order ruling on each motion and measures the average efficiency of judges in resolving complex issues that arise in bankruptcy (Dahiya et al. (2003); Goyal and Wang (2017)). We identify 80,502 motions and calculate *Ave Days(Ruling)* as the average number of days between the motion and the related order across all motions in a case. We drop all "first-day" motions, which are typically routine and require little consideration by the judge.

Our second set of outcome 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 the 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, and changes in the market value of debt from default to plan confirmation ($\Delta\,Debt\,MV$)

[7] Bankruptcy costs include both legal and administration fees as well as opportunity costs (e.g., loss of customers, suppliers, and employees). Both costs are significantly higher in prolonged cases (see Altman et al. (2019)).

using Moody's Default & Recovery Database (DRD) to measure creditors' payoffs.[8] These two variables provide evidence on how the bankruptcy process impacts creditor welfare.

2.2 Judge Experience and Personal Attributes

We compile bankruptcy judges' career history by 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. 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 service. In addition, we use state voting records and data from L2 Politics to infer judges' political affiliation.

We define two case-specific measures of judicial experience. *Log(Months)* is defined as the natural logarithm of the number of months since a judge has been appointed to the bankruptcy court. To capture any nonlinear effects and because of a potential "learning curve," we also use the indicator *First 2Y* to capture cases seen by judges with two or fewer years on the bench. To measure judges' other professional experience, we use *Log(Years before Bench)*, the number of years of professional work experience since law school graduation. We also use four indicator variables for judges' personal characteristics and attributes: *Top5 Law School*,[9] *Male*, *Military*, and *Democrat*. See the Appendix for detailed variable definitions.

2.3 Summary Statistics

We summarize large case characteristics in Panel A of Table 1. For our sample of 1,304 large cases, the average case spent 16.57 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 533 cases with electronic dockets, the average case has 149 motions (some filed simultaneously) and

[8]See Jiang et al. (2012) for details on the construction of debt recovery rates. Moody's DRD provides detailed information for only debt instruments rated by Moody's, resulting in a significant decline in sample size for these tests.

[9]We use the 2009 US News law school rankings, as rankings are sticky and generally unavailable for the years our sample judges went to law school. Results are robust to using a top 10 or top 25 law school indicator.

each motion takes on average 33.33 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 17.86%. In terms of experience measures, the average judge has been on the bench for 114.49 months (standard deviation 85.22), and 13% of large cases (173) 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 $2,113 million in 2016 US dollars (median $490.6 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 (-24%). Twenty-five 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 the filing date. Twenty-nine percent of cases are filed in Delaware, and 18% are filed in the Southern District of New York (NY SD). 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 64% 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 have more prior work experience, and that male judges are more likely to have served in the military, are less likely to be democrats, and have more prior work experience.

3 Judge Random Assignment

An important identifying assumption for our empirical strategy is that judicial experience is unrelated to firm characteristics, and therefore that confounding factors do not affect case outcomes in the same time-varying manner as judges' job-specific experience. In this section, we compile direct evidence from U.S. bankruptcy courts, review prior research on random assignment, and perform two sets of empirical tests to support the notion that large bankruptcy cases are randomly assigned.

First, we conduct a thorough search on the official web site for each court in our sample to identify their case assignment policy. For courts that do not explicitly state their policy online, we emailed the chief clerk. We obtained policy statements from 58 courts which contain 89% of our sample cases. Appendix Table A1 provides a list of the courts and a summary of their case assignment procedure. Of these 58 courts, 57 have a policy to randomly assign cases among active bankruptcy judges.[10,11] In 13 courts, judge assignment is deterministic, either because there is only one judge (7 courts) or because each judge only takes cases from specific counties (6 courts). Results are qualitatively unchanged if we drop the 21 cases from these courts (see the online appendix). Courts' stated policies clearly support the notion of random assignment.

Second, there has been an increasing number of studies that exploit random assignment of bankruptcy judges for empirical identification (Chang and Schoar (2013); Dobbie and Song (2015); Bernstein et al. (2018, 2019)). These studies uniformly find evidence that bankruptcy case characteristics are orthogonal to judge characteristics. For example, Bernstein et al. (2019), employing a large sample of 28,000 unique bankruptcy filings from 1992 to 2005, show that judges' liquidation tendency is uncorrelated with case and establishment-level characteristics. Moreover, a number of studies exploit random assignment in district courts to identify judge effects in other settings (see Ashenfelter, Eisenberg, and Schwab (1995); Chen, Moskowitz, and Shue (2016); Cohen and Yang (2018)). Although legal scholars argue that cases may not be randomly assigned to judges at the Court of Appeals (Hall (2010); Chilton and Levy (2015)), there is no systematic empirical evidence of which we are aware that discredits random assignment at bankruptcy courts.

A caveat to existing studies is that their samples are dominated by small business filings and thus their findings may not extend to large cases. Experienced judges may compete for large cases, as overseeing these cases will potentially lead to national recognition and prestigious status for the judge (LoPucki (2005)). Courts could also potentially assign larger cases that require extensive effort to judges with more judicial experience, and large firms (or their lawyers) may have enough knowledge of the court system to strategically time their bankruptcy filing. We thus conduct two sets of empirical tests to investigate whether case assignment is orthogonal to judicial experience in our sample of large cases.

[10]The Clerk of Court in Wisconsin stated that Chapter 11 cases are not assigned to new judges for a period of "a few months," so we drop the six large cases filed in Wisconsin. The method of randomization varies by court and includes a computerized random draw procedure or a blind rotation system.

[11]Technically, judge random assignment occurs at the divisional office level, as cases are filed in a particular office of a bankruptcy district. Nearly all large cases are filed in the main divisional office of each district. For example, among cases filed in the SDNY in our sample, 93.3% are in Manhattan, 5.4% are in White Plains, and 1.2% are in Poughkeepsie.

If case assignment is independent of judicial experience, then each judge within a court should have an equal probability of being assigned a new large case, regardless of that judge's level of experience.[12] Our first test for random assignment is to estimate 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 two measures that capture judge i's court-level experience at the time case j was filed, namely *Log(Months)* and *First 2Y*. To provide a comprehensive analysis of case assignment, we also examine whether cases are assigned with respect to judge caseload (*Caseload large*, the number of large cases assigned to judge i and not yet confirmed at the time case j was filed), and industry-level experience (*Industry Experience*, the number of large confirmed cases from the same two-digit SIC industry previously assigned to judge i). We include case fixed effects and cluster standard errors by court.

If large cases are more likely to be assigned to experienced judges, then the coefficient β_1 should be positive for *Log(Months)* and negative for *First 2Y*. If courts assign large cases to judges with smaller caseloads, the coefficient β_1 should be negative for *Caseload large*, and if courts assign large cases to judges with experience ruling on cases from the same industry, 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 judges' tenure, caseload, and industry-level experience.

To identify the set of eligible judges when a case was filed, we combine our sample with corporate bankruptcy filings retrieved from LexisNexis to identify judges contemporaneously serving in that court (see Section 5 for a description of the LexisNexis bankruptcy sample). Because the Lexis Nexis data is incomplete prior to 1993, we exclude all large cases filed prior to 1993 from

[12] A shortcoming of this research design is that some courts only randomize large complex Chapter 11 filings over a subset of judges. For example, in 2016 the Southern District of Texas assigned 50 percent of complex Chapter 11 cases to Judge Isgur and 50 percent to Judge Jones, while the remaining two judges (Bohm and Brown) received smaller Chapter 11 cases but none of the complex cases. Some courts also use unequal weights (e.g., to compensate the Chief Judge for other required duties). As discussed below, there are significant empirical challenges in identifying the set of available judges.

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.[13] 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. Empirically we find that visiting judges are assigned only a small number of large Chapter 11 cases. 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 6,117 case-judge links representing 50 bankruptcy courts and 1,022 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 experience, current caseload, and industry experience are unrelated to large case assignment, consistent with assignment being independent of these judge experience measures. In Appendix Table A2 we examine whether the insignificant relationship between experience and case assignment persists in six subsamples: samples that exclude prepacks, jumbo cases with more than $500 million in assets, cases filed in either SDNY or Delaware, cases filed in only Delaware or cases filed in only the SDNY, and all cases filed outside these two courts. We note only two possible deviations from random assignment: large cases in Delaware are marginally less likely

[13]Visiting judges are sometimes used by bankruptcy courts that have abnormally large caseloads relative to their capacity. The eight bankruptcy courts we identified with visiting judges during our sample period are the Northern District of California, Delaware, the Southern District of Georgia, the Eastern District of Michigan, the Eastern District of Missouri, Nevada, the Southern District of New York, and the Southern District of Ohio.

to be assigned to a judge with a large caseload, and judges outside of Delaware and the Southern District of New York with relevant industry experience are more likely to be assigned large cases. Due to Delaware's reliance on both district and visiting judges prior to 2006, we redo the main analysis from Table 2 after dropping all cases filed in Delaware before 2006 and continue to find no evidence of non-random assignment (see Table A3).

Our second set of empirical tests evaluates whether there is any correlation between the assigned judge's experience and observable firm 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:

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

where $JudgeExp_{i,j}$ is one of the two measures of assigned judge i's tenure at the time case j was filed, and $Firm\ Characteristics_j$ include $Log(Assets)$ and $Log(NumFiling)$ to control for case complexity; $Leveragefiling$ and $ROAfiling$ to control for firm performance upon filing for Chapter 11; and the dummy variable *Prepack/Preneg* to control for cases that were prepackaged/prenegotiated. We include court fixed effects to control for unobservable firm heterogeneity that is correlated with court choice, potentially as a result of "forum shopping" where firms file in courts not in geographic proximity to their principal place of business or operations (Eisenberg and LoPucki (1999); Ayotte and Skeel (2004); LoPucki (2005)). We also include Industry (Fama French 12) fixed effects and cluster standard errors by court.

Table 2 Panel B presents coefficient estimates of equation 2. Using *Log(Months)* and *First 2Y* as the experience measures in columns (1) and (2), respectively, we find that the only firm characteristic correlated with judicial experience is *Prepack/Preneg*. The results suggest that more experienced judges are more likely to be assigned these cases.[14] We redo the analysis in columns (3) and (4) after dropping all prepackacked/prenegotiated cases and find insignificant coefficient estimates for

[14]Judges still must examine the feasibility of prepackaged/prenegotiated plans and rule on complex issues related to potential litigation, disputes, and objections filed by dissident stakeholders such as suppliers and employees. Prepackaged and prenegotiated cases in our sample take on average seven months to be confirmed.

all firm characteristics. We include a prepack indicator in all our main analyses. More importantly, we demonstrate the robustness of our findings by also dropping all prepackaged/prenegotiated cases.

We also examine the incremental effect of including firm characteristics on 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* specifications, the adjusted R^2 increases from 0.01 to 0.02 in column (2) and from 0.03 to 0.04 in column (4). 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, with the possible exception of prepackaged cases, are randomly assigned with respect to judicial experience.

4 Main Results

4.1 Baseline 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 OLS regressions and linear probability models of the following form:

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

using the $Outcome_{i,j}$ and $JudgeExp_{i,j}$ measures mentioned previously for each case j assigned to judge i. 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 case controls previously defined as well as industry fixed effects and court fixed effects, which allow 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. We cluster standard errors by court.

Panel A of Table 3 presents coefficient estimates for the analysis of *Duration*.[15] We find that a judge's time-on-the-bench significantly reduces bankruptcy duration. The coefficient estimate in column 3 can be interpreted as the elasticity, and suggests that being randomly assigned to a judge with twice as much time on the bench would result in a 5.4% decrease in bankruptcy duration, a decline of nearly one month relative to the mean *Duration* of 16.6 months. The coefficient on $First\,2Y$ in column (4) suggests 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 17.82% longer durations, which corresponds to an increase of 3 months relative to the sample mean.[16] Results presented in columns (5) and (6) after prepackaged and prenegotiated cases are removed are quantitatively similar to those in columns (3) and (4).

In Panel B of Table 3 we analyze *Ave Days(Ruling)* to understand how judges reduce *Duration*. We find that less experienced judges take longer to rule on motions. We estimate that a judge with twice as much experience issues orders on average 1.5 days faster (a 4.5% decrease relative to the sample average of 33.3 days), while judges with less than 2 years of experience take 3.4 days longer (a 10.2% increase). These economic magnitudes are comparable to the overall effects of experience on *Duration*, suggesting that a significant portion of the overall decrease in duration appears due to experienced judges' ruling faster. Results are qualitatively similar after dropping prepackaged/prenegotiated cases (columns (5) and (6)). Appendix Table A4 documents an insignificant association between the total number of motions filed and judge experience, inconsistent with a competing hypothesis that lawyers file more motions before inexperienced judges. The combined evidence suggests that reduced bankruptcy durations are driven by experience-driven improvements in judges' efficiency at handling specific tasks.

[15] We tabulate coefficient estimates for control variables for our primary specifications in Table A8.

[16] Because we use a log-linear model, the estimated impact of moving from a judge with less than 2 years experience to more than 2 years is $100[exp(\beta_1) - 1]$.

In Table 4 we analyze *Emergence* and *Refile3Y* and find that large cases assigned to judges with more time on the bench are significantly more likely to emerge. A one-standard-deviation increase in *Log(Months)* leads to a 3.1% increase (based on the coefficient estimate in column 3) in the probability that the firm emerges from Chapter 11 (rather than being liquidated), corresponding to 5.4% of the sample mean. Large cases assigned to judges in their first two years are 6.9% less likely to emerge, corresponding to 12.1% of the sample mean. Results are qualitatively similar after dropping prepackaged/prenegotiated cases (columns (5) and (6)). A higher rate of emergence could be consistent with more experienced judges being more lenient, allowing less viable firms to emerge from bankruptcy. In Panel B, however, we find no evidence that more experienced judges are associated with higher refiling rates. Taken together, the evidence in Table 4 suggests that experienced judges improve the likelihood that firms emerge from bankruptcy, but not at the cost of higher refiling rates.

In robustness tests, we remove the largest 20% cases in asset size and find similar results, alleviating concerns that a few extremely large cases drive our main results (see Appendix Table A5). Another concern is that judges on the long-end of the experience measure drive our results. This concern reflects a potential selection issue where better judges get reappointed and are therefore associated with more efficient outcomes.[17] As a robustness test, we only include cases assigned to judges during their first term and find qualitatively similar results (see Appendix Table A6). In addition, we remove 21 cases from 13 courts where judge assignment is deterministic (there is a single judge or each judge is given a specific geographic area within the district). Our main results remain robust in Appendix Table A7. Lastly, we present our main results with all control variables in Appendix Table A8.

A shortcoming of the emergence analysis in Table 4 is that the welfare implications are unclear due to our inability to observe what happens to the assets of liquidated firms. To provide suggestive evidence on creditors' welfare we examine debt recovery rates and changes in firms' market value of debt in Table 5. We find in all specifications that both *Total Recovery* and Δ *Debt MV* are significantly lower for cases assigned to judges with two or fewer years of experience, whereas

[17]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.

Total Recovery is significantly increasing in judge's total time on the bench (*Log(Months)*) in only column (1) of Panel A. The reduced significance for *Log(Months)* is potentially due to the reduced sample sizes in these regressions and a non-linear effect that concentrates in judges' first two years. In terms of economic magnitude, coefficient estimates in column (4) 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.

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.

4.2 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 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 0.195 to a statistically insignificant 0.008 as judges' experience increases from their first two years to their fifth and sixth years. The coefficient estimates translate into 21.5% 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 for *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.[18]

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 that the average new bankruptcy judge has 18 years of relevant work experience. Previous work documents much shorter learning curves in other contexts. For example, Levitt et al. (2013) estimate a learning curve of approximately 12 weeks in an automobile assembly plant, while Jovanovic and Nyarko (1995) estimate learning curves ranging from two weeks for munitions manufacturing workers to one year for insurance sales. Our estimates show that learning curves can be significantly longer in more complex settings.[19] Finally, the curvature of the learning curve supports our identification assumption, as potentially

[18] In Appendix Table A9 we find qualitatively similar results if we include only cases assigned to judges during their first term to rule out the possibility that judges serving more than one term drive these learning curve results.

[19] Incentives to acquire job-specific capital also likely matter for the length of the learning curve. Judges are paid a flat salary and thus do not have a direct monetary incentive to process cases more efficiently. More effective judges, however, can establish potentially valuable reputations and lighten their caseloads. Judges could also desire a "quiet life" (Bertrand and Mullainathan (2003)), and thus move up the learning curve slower than other professionals.

confounding factors such as judges' biases and cognitive abilities are unlikely to affect case outcomes in the same time-varying manner as judges' tenure.[20]

4.3 Learning Accelerators

The results presented to this point demonstrate that judges with more judicial experience are able to resolve large bankruptcy cases faster and that the learning curve is approximately four years. 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 literature (Arrow (1962); Becker (1962); Lazear (2009)).

First, we posit that judges who accrue experience that is more relevant for large Chapter 11 cases in their early judicial career move up the learning curve faster. 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 thus gain experience that is more relevant for the large cases we study 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 business filings are able to more efficiently manage large Chapter 11 filings.

Second, 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). Management studies (e.g. Campion et al. (1994)) suggest that the exposure of employees and managers to a variety of tasks and experiences through job rotation stimulates faster development of their professional skills. We thus predict that judges with exposure to a greater diversity of business cases "move up the learning curve" faster.

To construct judge-specific empirical measures for relevant business filings and case diversity, we retrieve quarterly court-level filing statistics from the U.S. Courts Administrative Office. This data contains information on total filings across filing types (Chapters 7, 11, 13) and nature of

[20]Dobbie and Song (2015) and Bernstein et al. (2019) find that judges' biases with respect to case emergence is not time-varying. Given that the average judge in our sample is appointed at age 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.

debt (business or personal) from 1980. We estimate the number of both business and personal bankruptcies overseen by a judge in a given quarter as the total number of each case type filed in his/her court divided by the number of judges in the court that quarter. Given random case assignment, this is likely a close proxy to actual cases overseen by each judge (unobservable in our data). We then sum this judge-specific number from the beginning of a judge's tenure until the filing date of a given case to obtain a time-varying measure of each judge's experience with business and personal bankruptcies.

We empirically proxy for case diversity along two dimensions: the size and industry of bankrupt firms. 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. 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. We calculate *Diversity-Size* as one minus the Herfindahl concentration index of these size buckets.

We estimate a modified version of equation (3) and focus on case duration. Furthermore, 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 (308 cases) or first six years (443 cases). These subsamples are sufficiently large for empirical analysis, yet also contain judges with relatively little time on the bench who simultaneously exhibit significant variation in their types of experience.

In Table 7 Panel A column (1), we find that cases assigned to judges with four or fewer years on the bench 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, relevant experience of overseeing a high share of business cases 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 1.9 fewer months (about 11% of the sample average) in bankruptcy.[21]

In column (2) of both panels we find that judges in courts with more diversified local industry composition resolve large Chapter 11 cases faster relative to judges with similar tenure but located in courts with less diversified industry composition. A one-standard-deviation increase in *Diversity-Industry* leads to approximately 1.7 months (10% of the sample average) shorter duration. Similarly, column (3) of both panels shows that judges that oversee a broader mix of firm sizes are able to resolve large Chapter 11 cases faster. This result is statistically significant at the 1% level in both the 4-year and 6-year samples. A one-standard-deviation increase in *Diversity-Size* based on the estimate in Panel B is associated with a reduced duration of 1.7 months (10% of the sample average). 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 faster learning by judges. Collectively, our evidence suggests that exposure to more relevant tasks as well as task variety during judges' early years accelerates their ability to efficiently handle large Chapter 11 cases.

4.4 Judge Caseload

The combined evidence thus far is consistent with judges becoming more efficient at handling large cases as they gain job-specific experience. However, lawyers could also be learning over time about judges' decision-making and potentially explain these results. 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 (2018)). A rise in caseload typically 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

[21]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.

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 current 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, then we expect to see weaker effects of experience when the judge's caseload is high.

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) a judge would spend administering the particular mix of six bankruptcy case types (Chapters 7, 9, 11, 12, 13, and 15) filed in his/her bankruptcy district, and thus proxies for the overall time constraints the judge faces.

Table 8 splits our full sample by the sample median court caseload. 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 pronounced in periods with above-median caseloads (*High*). 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 using *First 2Y*. On average, the effect of experience is 2 to 3 times larger in the high caseload subsample. The evidence suggests that experience matters most when judges are busiest, which is more consistent with the learning-by-doing hypothesis rather than lawyers' learning judges' preferences and style.

5 Controlling for Judge Personal Characteristics

The results presented in Section 4 are consistent with the learning-by-doing hypothesis that bankruptcy judges accumulate job-specific skills over time and that such skills affect bankruptcy outcomes. Our empirical inferences are built on comparing case outcomes across judges over time in the same bankruptcy court. In this section, we test how fixed judge characteristics affect case outcomes, and whether our results are affected by including these additional controls.

5.1 Judges' Prior Work Experience and Personal Attributes

We perform similar empirical specifications as those presented in Table 3 Panel B after including additional judge characteristics. Specifically, we proxy for judges' prior professional experience using *Log(Years before Bench)* and four measures of personal characteristics (*Top 5 Lawschool*, *Male*, *Miltary*, *Democrat*). The results are presented in Table 9, 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. Previous work experience does not have a large effect on *Duration*, in sharp contrast to the effects of job-specific experience. To test whether judges with more prior work experience are able to move up the judicial learning curve more quickly, we interact the dummy variables *First 2Y*, *Year3-4*, and *Year5-6* with *Log(Years before Bench)* and run regressions similar to those in Table 6. In Appendix Table A10 we find 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 have a direct effect on bankruptcy outcomes, it helps accelerate judges' accumulation of job-specific skills.

Examining the effect of personal characteristics in Table 9, 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. (2018, 2019)). The economic magnitude is fairly significant, with male judges processing cases 17.3% faster. We do not find any significant relationship between *Duration* and *Top 5 Lawschool*, *Military*, or *Democrat*. Importantly, the adjusted R^2 does not change after including additional measures on judge experience and traits and the coefficient estimates for judge experience are quantitatively similar. The evidence together suggests that the effect of a judge's job-specific experience is not captured by time-invariant judge characteristics.

5.2 Judicial Experience and Small Business Chapter 11 Cases

Our analysis to this point examines the effect of judicial experience on large Chapter 11 bankruptcies. We focus on large corporations because these bankruptcies are more complex and thus judicial experience is expected to matter the most in these cases. In addition, detailed information on firm and judge characteristics is available for this sample. However, focusing on large cases has the drawback of limiting us to a relatively small sample size, which prevents us from including judge fixed effects in our baseline specifications to control for all time-invariant judicial characteristics (relatively few judges, especially outside Delaware and the Southern District of New York, see multiple large cases). In this subsection, we demonstrate that our results also hold for a larger sample that includes small Chapter 11 cases and, more importantly, that our results are robust to the inclusion of judge fixed effects.

In particular, we use the full set of business Chapter 11 cases assigned to any of the 306 judges in our baseline large-case sample, over the period 1992-2017. This data is obtained from LexisNexis Public Records searches and has near universal coverage, as described in Iverson (2018). In total, we identify 102,742 individual bankruptcy filings. Firm characteristics such as size or industry are not observable in this sample, since nearly all observations are small, private companies. We continue to include court fixed effects, but also include year and judge fixed effects to more tightly control for time effects and fixed judge characteristics. Specifically, we estimate regressions of the following form:

$$\text{Outcome}_{i,j} = \alpha + \beta_1 \text{JudgeExp}_{i,j} + \gamma \text{Judge FE} + \theta \text{Court FE} + \delta \text{Year FE} + \epsilon_{i,j} \tag{4}$$

Panel A of Table 10 shows how judge experience affects *Duration* in the LexisNexis sample.[22] We estimate that doubling judicial experience decreases *Duration* by 5%. Meanwhile, *Duration* is 8.8% longer for small business Chapter 11 cases assigned to judges in their first two years. As expected, the magnitude of these estimates are slightly smaller than our baseline results in Table 3, since this sample focuses on much less complex Chapter 11 cases.

We next examine the learning curve in the LexisNexis sample in Panel B of Table 10. The regression specification is similar to Table 6, except that we also include judge fixed effects. We find that for small business bankruptcies, the learning curve is only significant in the first two years of the judge's tenure, suggesting that judges move up the learning curve faster for these smaller and generally less complicated cases.

Finally, we use the LexisNexis sample to estimate a fixed effect for each judge, and include this estimate in our baseline specification from Table 3.[23] We first exclude all large, public cases from our baseline LexisNexis sample, and then estimate regression specification 4 for the sample of small cases. From this regression, the vector of estimated γ gives an estimate of each judge's fixed influence on case duration among small business Chapter 11 cases. We include each judge's estimated coefficient as a control variable in Panel C of Table 10 and find that this judge duration fixed effect is significantly associated with case duration in our baseline sample of large Chapter 11 filings. Thus, individual judges have fixed characteristics that strongly affect bankruptcy duration, consistent with Bris et al. (2006). The inclusion of the judge duration fixed effect, however, has no impact on our baseline estimates of judicial time-varying experience. Thus the experience effects we estimate are separate from previously documented judicial biases (Chang and Schoar (2013); Dobbie and Song (2015); Bernstein et al. (2019)). Taken together, the results in Table 10 show that on-the-job learning is important even for smaller Chapter 11 filings, and that our results are robust to the inclusion of fixed judge characteristics.

[22] *Duration* in the LexisNexis sample is defined as the time until a case is fully closed. For cases that are dismissed or converted to Chapter 7, *Duration* is defined as the time to dismissal or conversion.

[23] For the large case sample, this approach provides a less data demanding alternative to including a fixed effect for each judge.

6 Discussion and 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 show 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.

While on-the-job learning is clearly costly, evaluating the welfare effects of learning by doing are less clear. Benefits of judge randomization—the predominant current model—include avoiding judicial capture by debtor firms. While we cannot quantify these benefits, we estimate the costs of judge inexperience by envisioning several plausible counterfactual scenarios and calculating back-of-the-envelope costs relative to these counterfactuals. First, we estimate that aggregate legal fees would decrease by $16.2 billion due to reduced cased duration if the largest cases were assigned to the most experienced judge in the court where the case was filed. Reassigning just the 131 large cases currently assigned to inexperienced judges (two or fewer years experience) to the most experienced judge in that court would decrease legal fees by $3.5 billion. Alternatively, randomly assigning those 131 cases among experienced judges in that court would decrease legal fees by $812 million, but increase creditor recoveries by $12.1 billion.

Although we cannot fully pin down the welfare losses due to on-the-job-learning, understanding the costs of inexperience is an important first step in evaluating case assignment policy. In particular, our findings have implications for policies surrounding 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. More broadly, we show that the costs of on-the-job-learning can be large and the learning curve slow, even for highly educated workers with significant prior experience.

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Table 1: Summary Statistics

Panel A presents the summary statistics for the sample large 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)	1,304	2.41	2.53	0.96	1.02	3.53
Months in Ch11	1,304	16.57	12.50	15.40	2.77	34.20
Ave Days(Ruling)	532	33.33	29.69	24.09	16.05	53.16
Log(Num of Motion)	533	4.48	4.53	1.12	3.14	5.87
Num of Motion	533	149.36	93.00	162.10	23.00	353.00
Emergence	1,304	0.57	1.00	0.49	0.00	1.00
Refile 3Y	716	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 (%)	334	17.86	1.05	86.54	-80.90	149.10
Log(Months as Judge)	1,282	4.31	4.58	1.14	2.81	5.47
Months as Judge	1,282	114.49	97.37	85.22	16.67	236.73
First 2 Years	1,282	0.13	0.00	0.34	0.00	1.00
Assets (Mils)	1,304	2,113.01	490.60	5,729.02	119.82	4,003.54
Log(Num Filings)	1,253	1.38	1.10	1.31	0.00	3.22
Num Filings	1,253	10.70	3.00	20.82	1.00	25.00
ROA Filing (%)	1,235	-24.02	-11.21	40.45	-61.38	1.69
Leverage Filing	1,274	1.01	0.92	0.51	0.55	1.50
Prepack/Preneg	1,304	0.25	0.00	0.43	0.00	1.00
Delaware	1,304	0.29	0.00	0.46	0.00	1.00
NY SD	1,304	0.18	0.00	0.39	0.00	1.00

Panel B: Judge Characteristics

	N	Mean	Median	SD	P10	P90
Log(Years before Bench)	293	2.82	2.89	0.45	2.20	3.40
Years before Bench	294	18.41	17.50	7.79	8.00	30.00
Top 5 Law School	306	0.12	0.00	0.32	0.00	1.00
Male	306	0.79	1.00	0.41	0.00	1.00
Military	302	0.23	0.00	0.42	0.00	1.00
Democratic	204	0.64	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	0.12*	1.00			
Male	0.19**	0.04	1.00		
Military	0.08	-0.00	0.25***	1.00	
Democratic	0.07	0.04	-0.17*	-0.11	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
ln(Months)	0.003			
	(0.50)			
First 2Y		-0.009		
		(-0.64)		
Caseload Large			-0.003	
			(-0.43)	
Industry Experience				0.006
				(1.15)
Observations	5,910	5,910	6,117	5,843
Adjusted R^2	0.10	0.10	0.10	0.09
Case FE	Yes	Yes	Yes	Yes

Panel B: Firm Characteristics

	Full		No Prepacks	
	(1)	(2)	(3)	(4)
	Log(Months)	First 2Y	Log(Months)	First 2Y
Log(Assets)	0.022	0.002	0.039	-0.007
	(1.01)	(0.26)	(1.46)	(-0.80)
Log(Num Filing)	0.001	-0.003	-0.010	0.009
	(0.07)	(-0.29)	(-0.42)	(1.13)
Leverage Filing	0.078	-0.006	0.093	-0.016
	(0.75)	(-0.21)	(0.70)	(-0.48)
ROA Filing	0.159	-0.040	0.231	-0.044
	(1.20)	(-1.05)	(1.46)	(-1.07)
Prepack/Preneg	0.250***	-0.041**		
	(3.97)	(-2.52)		
Adjusted R^2	0.07	0.02	0.07	0.04
Observations	1,148	1,148	850	850
Court FE	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes
Adj R^2 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 ($First\ 2Y$). All regressions include $Time\ trend$ and Post-BAPCPA as time controls. Additional case controls include $Log(Assets)$, $Log(Num\ Filing)$, $Leverage\ filing$, and $ROA\ filing$. 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	(5) Log(Months)	(6) First 2Y
Experience Measure	-0.096***	0.208***	-0.054***	0.164**	-0.054***	0.141***
	(-6.19)	(3.10)	(-4.20)	(2.38)	(-3.63)	(3.10)
Adjusted R^2	0.08	0.07	0.41	0.41	0.18	0.18
Observations	1,268	1,268	1,148	1,148	850	850
Industry FE	No	No	Yes	Yes	Yes	Yes
Court FE	Yes	Yes	Yes	Yes	Yes	Yes
Case Controls	No	No	Yes	Yes	Yes	Yes
Sample	Full	Full	Full	Full	No Prepacks	No Prepacks

Panel B: Ave Days(Ruling)

	(1) Log(Months)	(2) First 2Y	(3) Log(Months)	(4) First 2Y	(5) Log(Months)	(6) First 2Y
Experience Measure	-0.964*	2.714	-1.458**	3.401**	-2.034**	3.435
	(-1.90)	(1.49)	(-2.24)	(2.17)	(-2.58)	(1.40)
Adjusted R^2	0.21	0.21	0.25	0.25	0.26	0.26
Observations	508	508	477	477	305	305
Industry FE	No	No	Yes	Yes	Yes	Yes
Court FE	Yes	Yes	Yes	Yes	Yes	Yes
Case Controls	No	No	Yes	Yes	Yes	Yes
Sample	Full	Full	Full	Full	No Prepacks	No Prepacks

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 ($First\ 2Y$). All regressions include *Time trend* and Post-BAPCPA as time controls. Additional case controls include $Log(Assets)$, $Log(Num\ Filing)$, *Leverage filing*, and *ROA filing*. 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) Log(Months)	(2) First 2Y	(3) Log(Months)	(4) First 2Y	(5) Log(Months)	(6) First 2Y
Experience Measure	0.042***	-0.065**	0.027**	-0.069*	0.038**	-0.093**
	(3.77)	(-2.35)	(2.15)	(-1.74)	(2.12)	(-2.44)
Adjusted R^2	0.06	0.05	0.20	0.20	0.14	0.14
Observations	1,268	1,268	1,148	1,148	850	850
Industry FE	No	No	Yes	Yes	Yes	Yes
Court FE	Yes	Yes	Yes	Yes	Yes	Yes
Case Controls	No	No	Yes	Yes	Yes	Yes
Sample	Full	Full	Full	Full	No Prepacks	No Prepacks

Panel B: Refile 3Y

	(1) Log(Months)	(2) First 2Y	(3) Log(Months)	(4) First 2Y	(5) Log(Months)	(6) First 2Y
Experience Measure	0.010	0.018	0.009	0.014	0.006	-0.002
	(1.25)	(0.55)	(0.89)	(0.36)	(0.50)	(-0.05)
Adjusted R^2	-0.01	-0.01	0.02	0.02	-0.00	-0.00
Observations	692	692	620	620	393	393
Industry FE	No	No	Yes	Yes	Yes	Yes
Court FE	Yes	Yes	Yes	Yes	Yes	Yes
Case Controls	No	No	Yes	Yes	Yes	Yes
Sample	Full	Full	Full	Full	No Prepacks	No Prepacks

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 ($First\ 2Y$). All regressions include $Time\ trend$ and Post-BAPCPA as time controls. Additional case controls include $Log(Assets)$, $Log(Num\ Filing)$, $Leverage\ filing$, and $ROA\ filing$. 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	2.098**	-7.336**	1.180	-4.948*
	(2.15)	(-2.33)	(1.19)	(-1.72)
Adjusted R^2	0.01	0.01	0.07	0.07
Observations	435	435	405	405
Industry FE	Yes	Yes	Yes	Yes
Court FE	Yes	Yes	Yes	Yes
Case Controls	No	No	Yes	Yes

Panel B: Δ Debt MV (%)

	(1) Log(Months)	(2) First 2Y	(3) Log(Months)	(4) First 2Y
Experience Measure	1.696	-15.593**	-0.232	-18.950***
	(0.65)	(-2.72)	(-0.06)	(-3.87)
Adjusted R^2	0.04	0.04	0.09	0.09
Observations	314	314	289	289
Industry FE	Yes	Yes	Yes	Yes
Court FE	Yes	Yes	Yes	Yes
Case Controls	No	No	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*). 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.

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

	(1) Duration	(2) Ave Days(Ruling)
Year1-2	0.195***	3.914**
	(3.30)	(2.07)
Year3-4	0.153**	3.233
	(2.47)	(1.62)
Year5-6	0.008	1.502
	(0.16)	(0.82)
Observations	1,148	482
Adjusted R^2	0.41	0.25
Industry FE	Yes	Yes
Court FE	Yes	Yes
Case Controls	Yes	Yes
P(Y12=Y34)	0.669	0.716
P(Y12=Y56)	0.047	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 past 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
	(0.09)	(0.86)	(1.42)
Bus Filings/Total Filings	-0.12**	-0.10**	-0.11***
	(-2.63)	(-2.27)	(-2.80)
Diversity-Industry		-0.10**	
		(-2.11)	
Diversity-Size			-0.14***
			(-3.96)
Observations	308	308	308
Adjusted R^2	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
	(-1.07)	(-0.23)	(-0.05)
Bus Filings/Total Filings	-0.11***	-0.09**	-0.10***
	(-2.83)	(-2.29)	(-2.79)
Diversity-Industry		-0.10***	
		(-2.83)	
Diversity-Size			-0.11***
			(-3.56)
Observations	443	443	443
Adjusted R^2	0.46	0.47	0.47
Filing Year FE	Yes	Yes	Yes
Case Controls	Yes	Yes	Yes

Table 8: Sample Splitting by Judge 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.

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

Panel A: Log(Months as Judge)

	Duration		Ave Days(Ruling)	
	(1)	(2)	(3)	(4)
	High	Low	High	Low
Log(Months)	-0.052**	-0.035	-2.119***	-0.783
	(-2.40)	(-1.21)	(-3.17)	(-0.49)
Adjusted R^2	0.36	0.46	0.09	0.33
Observations	568	562	239	236
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	0.198***	0.088	6.642***	1.870
	(3.42)	(0.55)	(3.66)	(0.69)
Adjusted R^2	0.36	0.46	0.09	0.33
Observations	568	562	239	236
Case Controls	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes
Court FE	Yes	Yes	Yes	Yes

Table 9: Judge 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.

$$\text{Duration}_{i,j} \quad = \quad \alpha + \beta_1 \text{JudgeExp}_{i,j} + \beta_2 \text{Characteristics}_i + \beta_3 \text{Controls} + \delta \text{FEs} + \epsilon_{i,j}$$

	(1) Log(Months)	(2) First 2Y	(3) Log(Months)	(4) First 2Y	(5) Log(Months)	(6) First 2Y
Experience Measure	-0.054***	0.164**	-0.050***	0.158**	-0.044***	0.133
	(-4.20)	(2.38)	(-4.01)	(2.19)	(-3.34)	(1.67)
Log(Years before Bench)			0.019	0.039	0.096	0.115*
			(0.40)	(0.73)	(1.56)	(1.76)
Top5 Lawschool					-0.046	-0.039
					(-0.65)	(-0.55)
Male					-0.186***	-0.189***
					(-3.81)	(-3.97)
Military					0.039	0.029
					(0.61)	(0.44)
Democrats					-0.007	-0.013
					(-0.11)	(-0.19)
Adjusted R^2	0.41	0.41	0.41	0.41	0.41	0.41
Observations	1,148	1,148	1,142	1,142	1,141	1,141
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 10: Judge Fixed Effects

This table Panel A presents regression estimates of the log number of months a case spends under Chapter 11 (*Duration*) for the LexisNexis sample. 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 ($First\ 2Y$). Panel B presents estimates of the learning curve. Panel C presents regression estimates on *Duration* for public firm sample, controlling for judge duration fixed effects estimated using the specification in Panel A column (1) with all public firm observations removed from the LexisNexis Sample. Standard errors are clustered at the court level. We include t-stats in the parentheses and *, **, *** indicate 10%, 5%, and 1% statistical significance, respectively.

Panel A: Duration (LexisNexis Sample)

	(1) Log(Months)	(2) First 2Y
Experience Measure	-0.051**	0.084**
	(-2.35)	(2.07)
Adjusted R^2	0.16	0.16
Observations	102,742	102,742
Court-FE	Yes	Yes
Year-FE	Yes	Yes
Judge FE	Yes	Yes

Panel B: Learning Curve (LexisNexis Sample)

	(1) Duration
Year1-2	0.079**
	(2.09)
Year3-4	-0.019
	(-0.47)
Year5-6	0.015
	(0.45)
Adjusted R^2	0.16
Observations	102,742
Court-FE	Yes
Year-FE	Yes
Judge FE	Yes

Panel C: Duration (Public Firm Sample)

	(1) Log(Months)	(2) First 2Y
Experience Measure	-0.050***	0.161**
	(-4.27)	(2.09)
Judge Duration FE	0.134**	0.139**
	(2.60)	(2.51)
Adjusted R^2	0.39	0.39
Observations	1,093	1,093
Industry FE	Yes	Yes
Court FE	Yes	Yes
Case Controls	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: Court Random Assignment

This table summarizes judge assignment procedures for 58 courts who responded to our inquiries. Courts marked "Single Judge" have only one judge for the entire district and thus all cases are assigned to that judge. Courts marked "By location" have multiple judges, but each judge is given a specific geographic area within the district. Wisconsin is the only court that gives consideration to a judges experience, stating that new judges are not assigned Ch. 11 cases for "a few months." Accordingly, Wisconsin cases are removed from the sample. The remaining courts all use some form of randomization to assign cases.

Court	Assignment Method	No. Cases	Source	Court	Assignment Method	No. Cases	Source
AK	Single Judge	1	-	MT	Single Judge	1	-
AL, N	Random	2	Email from Clerk	NE	Random	1	Email from Clerk
AR, E	Random	2	Local rules	NJ	Random	37	Local rules
AZ	Random	15	Local rules	NV	Random	17	Email from Clerk
CA, C	Random	62	Phone call to court	NY, E	Random	6	Local rules
CA, E	Random	2	Email from Clerk	NY, N	By location	2	Email from Clerk
CA, N	Random	39	Email from Clerk	NY, S	Random	237	Local rules
CA, S	Random	7	Email from Clerk	NY, W	Random	3	Local rules
CO	Random	15	Local rules	OH, N	Random	15	Local rules
CT	Random	5	Local rules	OH, S	Random	15	Email from Clerk
DE	Random	383	Judge Shannon	OK, W	Random	6	Local rules
FL, M	Random	21	News article	OR	Random	4	Email from Clerk
FL, S	Random	32	Local rules	PA, E	Random	1	Local rules
GA, M	Random	1	Email from Clerk	PA, M	Random	2	Local rules
GA, S	By location	4	Email from Clerk	PA, W	Random	5	Local rules
HI	Single Judge	2	-	RI	Single Judge	1	-
ID	Random	1	Local rules	SC	By location	4	Local rules
IL, N	Random	40	Local rules	TN, M	Random	6	Email from Clerk
IN, S	Random	8	Email from Clerk	TN, W	Random	2	Email from Clerk
KS	By location	3	Local rules	TX, E	Random	3	Email from Clerk
KY, E	Random	3	Local rules	TX, S	Random	46	Phone call to court
LA, M	Single Judge	1	-	TX, W	Random	19	Email from Clerk
LA, W	By location	5	Email from Clerk	UT	Random	4	Email from Clerk
MD	Random	13	Local rules	VA, E	Random	15	Email from Clerk
MI, E	Random	16	Local rules	VA, W	Random	2	Email from Clerk
MI, W	Random	4	Local rules	VT	Single Judge	1	-
MN	Random	4	Local rules	WA, W	Random	7	Local rules
MO, E	Random	13	Email from Clerk	WI, E	Non-random	6	Email from Clerk
MO, W	Random	5	Local rules	WV, S	Single Judge	1	-
MS, S	By location	3	Email from Clerk				

Table A2: 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 six subsamples: cases that were not filed as a prepack ($No\,Prepack$, column 1); cases with total assets (current dollars) larger than or equal to $500 million at time of filing ($Jumbo$, column 2); cases filed in either the southern district of New York or Delaware ($NYSD/DE$, column 3); cases filed in just Delaware (DE, column 4); cases filed in just the NYSD ($NYSD$, column 5); and cases filed in a court other than Delaware or NYSD ($Other$, column 6). The judge experience measure include: the log number of months the judge has been on the bench in Panel A ($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.

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

Panel A

	(1) No Prepack	(2) Jumbo	(3) NYSD/DE	(4) DE	(5) NYSD	(6) Other
ln(Months)	0.001	-0.005	-0.004	0.005	-0.008	0.012
	(0.18)	(-0.54)	(-0.52)	(0.31)	(-0.96)	(1.29)
Observations	5,055	2,833	2,962	1,193	1,769	2,948
R-Squared	0.10	0.10	0.12	0.12	0.00	0.07
Case FE	Yes	Yes	Yes	Yes	Yes	Yes
Avg Dep Var	0.17	0.17	0.18	0.28	0.11	0.16

Panel B

	(1) No Prepack	(2) Jumbo	(3) NYSD/DE	(4) DE	(5) NYSD	(6) Other
First 2Y	-0.008	0.011	0.007	-0.004	0.012	-0.037
	(-0.43)	(0.44)	(0.32)	(-0.07)	(0.52)	(-1.16)
Observations	5,055	2,833	2,962	1,193	1,769	2,948
R-Squared	0.10	0.10	0.12	0.12	0.00	0.07
Case FE	Yes	Yes	Yes	Yes	Yes	Yes
Avg Dep Var	0.17	0.17	0.18	0.28	0.11	0.16

Randomization Robustness Tests (cont)

Panel C

	(1) No Prepack	(2) Jumbo	(3) NYSD/DE	(4) DE	(5) NYSD	(6) Other
Caseload large	-0.003	0.001	-0.007	-0.013*	0.002	0.027
	(-0.48)	(0.17)	(-1.42)	(-1.71)	(0.41)	(1.68)
Observations	5,244	2,909	2,962	1,193	1,769	3,155
R-Squared	0.10	0.10	0.12	0.13	0.00	0.08
Case FE	Yes	Yes	Yes	Yes	Yes	Yes
Avg Dep Var	0.17	0.17	0.18	0.28	0.11	0.16

Panel D

	(1) No Prepack	(2) Jumbo	(3) NYSD/DE	(4) DE	(5) NYSD	(6) Other
Industry experience	0.011	-0.015	-0.000	0.002	-0.007	0.089**
	(1.49)	(-1.23)	(-0.04)	(0.16)	(-0.58)	(2.43)
Observations	5,021	2,810	2,868	1,099	1,769	2,975
R-Squared	0.10	0.09	0.10	0.12	0.00	0.09
Case FE	Yes	Yes	Yes	Yes	Yes	Yes
Avg Dep Var	0.17	0.17	0.18	0.28	0.11	0.16

Table A3: 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
ln(Months)	0.003			
	(0.54)			
First 2Y		-0.010		
		(-0.63)		
Caseload large			0.001	
			(0.11)	
Industry experience				0.006
				(1.02)
Observations	5,504	5,504	5,711	5,531
R-Squared	0.05	0.05	0.05	0.05
Case FE	Yes	Yes	Yes	Yes

Table A4: Number of Motions

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 ($First\ 2Y$). 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.

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

	(1) Log(Months)	(2) First 2Y	(3) Log(Months)	(4) First 2Y	(5) Log(Months)	(6) First 2Y
Experience Measure	0.025	-0.082	0.053	-0.116	0.010	0.072
	(0.46)	(-0.68)	(0.88)	(-0.68)	(0.15)	(0.51)
Adjusted R^2	0.11	0.11	0.50	0.50	0.52	0.52
Observations	514	514	483	483	310	310
Industry FE	No	No	Yes	Yes	Yes	Yes
Court FE	Yes	Yes	Yes	Yes	Yes	Yes
Case Controls	No	No	Yes	Yes	Yes	Yes
Sample	Full	Full	Full	Full	No Prepacks	No Prepacks

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), a dummy variable indicating a firm emerges from Chapter 11 (*Emergence*) in columns (3)–(4), and a dummy variable indicating a firm refiles for Chapter 11 within three years after emergence ($Refile3Y$) in columns (5)–(6). The two judge experience measures include: the log number of months the judge has been on the bench ($Log(Months)$) and an indicator for cases assigned during the first two years of a judge's tenure ($First\,2Y$). 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.

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

	Duration		Emergence		Refile 3Y	
	(1)	(2)	(3)	(4)	(5)	(6)
	Log(Months)	First 2Y	Log(Months)	First 2Y	Log(Months)	First 2Y
Experience Measure	-0.059***	0.193**	0.032**	-0.069	0.013	0.011
	(-3.99)	(2.16)	(2.27)	(-1.51)	(1.32)	(0.26)
Adjusted R^2	0.38	0.38	0.20	0.20	0.02	0.02
Observations	905	905	905	905	469	469
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 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), a dummy variable indicating a firm emerges from Chapter 11 (*Emergence*) in columns (3)–(4), and a dummy variable indicating a firm refiles for Chapter 11 within three years after emergence ($Refile3Y$) in columns (5)–(6). The two judge experience measures include: the log number of months the judge has been on the bench ($Log(Months)$) and an indicator for cases assigned during the first two years of a judge's tenure ($First\,2Y$). 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.

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

	Duration		Emergence		Refile 3Y	
	(1)	(2)	(3)	(4)	(5)	(6)
	Log(Months)	First 2Y	Log(Months)	First 2Y	Log(Months)	First 2Y
Experience Measure	-0.043**	0.122	0.030**	-0.050	-0.005	0.031
	(-2.36)	(1.52)	(2.63)	(-1.17)	(-0.39)	(0.78)
Adjusted R^2	0.40	0.40	0.20	0.20	0.01	0.01
Observations	832	832	832	832	442	442
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 A7: Robustness Check: Single Judge Court or By Location Assignment

This table presents regression estimates of judge experience measures on case outcomes, excluding courts that only have one judge or courts that assign cases by location, as listed in Appendix Table A1 The outcome variables include: the log number of months a case spends under Chapter 11 ($Duration$) in columns (1)–(2), a dummy variable indicating a firm emerges from Chapter 11 ($Emergence$) in columns (3)–(4), and a dummy variable indicating a firm refiles for Chapter 11 within three years after emergence ($Refile3Y$) in columns (5)–(6). The two judge experience measures include: the log number of months the judge has been on the bench ($Log(Months)$) and an indicator for cases assigned during the first two years of a judge's tenure ($First\,2Y$). 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.

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

	Duration		Emergence		Refile 3Y	
	(1)	(2)	(3)	(4)	(5)	(6)
	Log(Months)	First 2Y	Log(Months)	First 2Y	Log(Months)	First 2Y
Experience Measure	-0.054***	0.166**	0.028**	-0.070*	0.008	0.016
	(-4.22)	(2.35)	(2.20)	(-1.73)	(0.82)	(0.40)
Adjusted R^2	0.41	0.41	0.20	0.20	0.02	0.02
Observations	1,127	1,127	1,127	1,127	609	609
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 A8: Robustness: Main Results with Control Variables

This table presents regression estimates of judge experience measures on case outcomes, with coefficient estimates of control variables tabulated. The outcome variables include: the log number of months a case spends under Chapter 11 (*Duration*) in columns (1)–(2), a dummy variable indicating a firm emerges from Chapter 11 (*Emergence*) in columns (3)–(4), and a dummy variable indicating a firm refiles for Chapter 11 within three years after emergence ($Refile3Y$) in columns (5)–(6). The two judge experience measures include: the log number of months the judge has been on the bench ($Log(Months)$) and an indicator for cases assigned during the first two years of a judge's tenure ($First\,2Y$). 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.

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

	Duration		Emergence		Refile 3Y	
	(1)	(2)	(3)	(4)	(5)	(6)
	Log(Months)	First 2Y	Log(Months)	First 2Y	Log(Months)	First 2Y
Experience Measure	-0.054***	0.164**	0.027**	-0.069*	0.009	0.014
	(-4.20)	(2.38)	(2.15)	(-1.74)	(0.89)	(0.36)
Log(Assets)	0.084***	0.082***	0.059***	0.060***	-0.008	-0.008
	(4.31)	(4.21)	(4.73)	(4.78)	(-1.41)	(-1.36)
Log(Num Filings)	0.043***	0.044***	0.023**	0.023***	0.012	0.013
	(3.45)	(3.35)	(2.53)	(2.67)	(1.47)	(1.52)
Leverage Filing	-0.148**	-0.152***	0.154***	0.156***	0.038	0.039
	(-2.66)	(-2.70)	(6.15)	(6.59)	(1.40)	(1.45)
ROA Filing (%)	-0.001***	-0.001***	0.000	0.000	0.000	0.000
	(-2.68)	(-2.83)	(0.85)	(0.91)	(0.53)	(0.60)
Prepack/Preneg	-1.190***	-1.196***	0.297***	0.300***	0.051*	0.052**
	(-17.76)	(-17.44)	(14.30)	(14.64)	(1.99)	(2.05)
Time Trend	-0.023***	-0.024***	-0.020***	-0.019***	-0.004	-0.004
	(-3.35)	(-3.45)	(-5.84)	(-5.67)	(-1.12)	(-1.02)
Post-BAPCPA	0.118**	0.128**	0.079	0.074	0.030	0.028
	(2.21)	(2.29)	(1.52)	(1.39)	(0.76)	(0.71)
Adjusted R^2	0.41	0.41	0.20	0.20	0.02	0.02
Observations	1,148	1,148	1,148	1,148	620	620
Industry FE	Yes	Yes	Yes	Yes	Yes	Yes
Court FE	Yes	Yes	Yes	Yes	Yes	Yes

Table A9: 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 ($AveDays(Ruling)$). 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.

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

	(1) Duration	(2) Ave Days(Ruling)
Year1-2	0.160**	3.945*
	(2.18)	(2.02)
Year3-4	0.144**	2.839
	(2.27)	(1.38)
Year5-6	-0.003	0.229
	(-0.07)	(0.19)
Observations	832	338
Adjusted R^2	0.40	0.09
Industry FE	Yes	Yes
Court FE	Yes	Yes
Case Controls	Yes	Yes
P(Y12=Y34)	0.879	0.499
P(Y12=Y56)	0.070	0.071
P(Y34=Y56)	0.021	0.185
P(Y12=Y34=Y56)	0.022	0.185

Table A10: 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*). 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.

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

	(1) Log(Months in Ch11)	(2) Log(Months in Ch11)
Year1-2	1.062*	0.547***
	(1.70)	(3.25)
Year3-4	0.582**	0.418***
	(2.19)	(3.62)
Year5-6	0.839*	0.327
	(1.71)	(1.23)
Year1-2*Log(Years before Bench)	-0.294	
	(-1.35)	
Year3-4*Log(Years before Bench)	-0.147*	
	(-1.73)	
Year5-6*Log(Years before Bench)	-0.286*	
	(-1.74)	
Log(Years before Bench)	0.096	
	(1.61)	
Year1-2*Long experience before Bench		-0.242*
		(-1.76)
Year3-4*Long experience before Bench		-0.177**
		(-2.44)
Year5-6*Long experience before Bench		-0.213
		(-1.22)
Long experience before Bench		0.066
		(1.30)
Adjusted R^2	0.41	0.41
Observations	1,142	1,142
Industry FE	Yes	Yes
Court FE	Yes	Yes
Case Controls	Yes	Yes