Financial Costs of Judicial Inexperience *

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Abstract

Exploiting the random assignment of corporate bankruptcy filings, we estimate financial costs of judicial inexperience. Despite bankruptcy judges' significant prior legal experience, formal education, and rigorous hiring process, cases assigned to new judges spend more time in bankruptcy, realize lower creditor recovery rates, and lower return on assets post bankruptcy, but similar refiling rates. Judges' learning curve for the average filing is one year but rises to four years for the most complex cases. Exposure to more corporate cases and a greater diversity of businesses accelerates judges' learning. Overall, the results are consistent with lower-quality restructuring by less experienced judges. Conservative estimates suggest that slight policy adjustments to the case assignment process could, in aggregate, reduce legal fees and increase creditor recoveries by approximately $10 billion for our sample period.

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

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1 Introduction

Many jobs require individuals to accumulate human capital through on-the-job training. While some skills can be acquired in classrooms and simulated scenarios, in many cases the only way for an individual 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. Costly first attempts are thus in many ways inevitable, but are typically borne by the worker or firm employing the worker. In this paper we provide evidence of financial costs that accrue to firms in bankruptcy as a result of judicial inexperience, and perhaps more importantly, suggest feasible policy adjustments that can reduce these costs.

The restructuring process plays an important role in the allocation of capital within financial markets, and federal bankruptcy judges are arguably the most important decision makers within that process, overseeing all major actions undertaken by firms in bankruptcy. Managerial compensation, plans of reorganization, section 363 asset sales, professional fees, creditor recoveries, and fresh-start accounting are all approved or affected by judges (Weiss and Wruck (1998); Heron et al. (2009); Gennaioli and Rossi (2010); Becker and Stromberg (2012); Chang and Schoar (2013); Goyal and Wang (2017); Bernstein et al. (2019b)). Although it is intuitive that all types of workers increase their efficiency with experience (i.e., learning by doing), the bankruptcy setting is unique in that the costs of judicial inexperience are primarily borne by the debtor firm and its creditors (rather than the judge or court), and that at least some of these costs are measurable. We find that despite their formal education, prior legal experience, and rigorous hiring process, judges require time to master the skill of "managing" corporate bankruptcies, and that the process of acquiring those skills imposes significant financial costs on firms already in financial distress.

Several unique institutional features of bankruptcy courts, not present in other settings, allow us to estimate how case-specific outcomes vary as judges accumulate valuable on-the-job experience. Most importantly, bankruptcy judges are randomly assigned to cases, which allows us to empirically estimate how case duration, speed of ruling, likelihood of emergence, refiling rates, post-bankruptcy firm performance, and creditors' recovery rates differ as judges accumulate job-specific experience.

Additionally, judges are appointed to 14-year renewable terms (reducing survival bias concerns), have flat compensation structures and nearly always complete their first term (reducing incentives to signal),[1] and typically end their career as a judge (reducing revolving door and risk-taking concerns). Furthermore, judges receive limited feedback on their job performance and a flow of new cases that does not take into account their caseload, potentially exacerbating the costs of inexperience. We exploit these features to document the speed with which judges learn to efficiently manage complex corporate restructurings, the resulting costs of judicial inexperience, factors that accelerate judges' learning, and the relative importance of prior work experience, education, and personal characteristics.

We begin our analysis by examining a comprehensive sample of 105,100 Chapter 11 bankruptcy filings between 1993 and 2012 overseen by 574 unique bankruptcy judges in 89 bankruptcy courts ("LexisNexis Sample"). Our identifying assumption is that case assignment is uncorrelated with judicial experience. Chang and Schoar (2013) and Bernstein et al. (2019b) provide evidence that bankruptcy cases are randomly assigned to judges. We also provide evidence consistent with random assignment. We first systematically validate, using courts' stated policies, that cases are randomly assigned. We also provide anecdotal evidence of large cases that were assigned to rookie judges. Next, we empirically document that judicial experience is unrelated to both the probability that a judge is assigned a case as well as case complexity.

We exploit this random assignment to estimate the effect of judicial experience on case duration, a proxy for the overall costs of bankruptcy. We find that cases assigned to less experienced judges spend more time in court. Specifically, we document an elasticity of -0.059, such that cases assigned to a judge with twice as much time on the bench (e.g., from 2 to 4 years) realize a 5.9% decrease in time spent in bankruptcy, a decline of nearly one month relative to the average duration in our sample (16.7 months).[2] An alternative measure of judicial experience based on Chapter 11 filings produces a slightly larger elasticity of -0.082. Longer case durations concentrate during judges' early years. Mapping out judges' learning curve, we find that cases assigned during a judge's first year experience 9% longer durations, and that case duration is significantly longer but

[1]Of the 262 judges in our sample appointed before 1998, only four left before the end of their first term (for a mixture of reasons) and two passed away.

[2]Although tenure is highly correlated with age, we note that aging is associated with *decreased* cognitive ability (Korniotis and Kumar (2011)), and thus should lead to decreasing, not improving, performance over time.

decreasing over the first 50 cases assigned to the judge. Our regression specifications include both court-year and judge fixed effects, allowing us to measure the effect of on-the-job experience while holding constant omitted time-varying, court-specific characteristics (such as the judge's cohort and characteristics of other cases filed in the same court year), and judge time-invariant characteristics.

LexisNexis provides a comprehensive sample of Chapter 11 filings, but contains limited data on case complexity, no data on creditor recoveries or post-bankruptcy performance, and is dominated by small, private businesses. We thus also examine a sample of 1,304 Chapter 11 filings by publicly traded firms with more than \$50 million in assets ("public firm sample") to better understand the costs of judicial inexperience. This sample of public firm bankruptcies allows us to estimate a learning curve for one of bankruptcy judges' most complex tasks and analyze outcomes not possible in the LexisNexis sample. The lower frequency of these cases also provides an opportunity to estimate task-specific learning in a setting lacking frequent repetition. We estimate that large public cases assigned to inexperienced judges spend an additional 4.5 months in bankruptcy. Only after four years (or approximately 200 private and public cases) do new judges have similar case durations as more experienced judges for these large firms. A four-year learning curve suggests that judges must be on the bench for a significant portion of their 14-year appointment before gaining the experience necessary to efficiently manage these highly complex cases.

We next benchmark the importance of on-the-bench experience to these judges' prior work experience and personal characteristics (educational background, gender, political affiliation, and military service). Although nearly all the sample judges worked previously as lawyers, none have prior judicial experience. Explicitly controlling for prior work and personal characteristics has little effect on our main findings, demonstrating that judges' *task-specific* human capital (e.g., ruling on motions, resolving disputes, managing large caseloads, etc.) plays an important role above and beyond existing *general* work and educational experience.

To better understand how experienced judges move cases through bankruptcy faster, we examine case dockets and find that judges in their first two years spend, on average, an additional 5.6 days on each motion, 16.8% above the sample mean. We do not find a significant relationship between judicial experience and the number of motions filed, suggesting that increased case duration for inexperienced judges is not due to new judges having to issue more rulings (i.e., lawyers do not file more motions when judges are inexperienced). Rather, new judges are less efficient at ruling on individual motions. Cases assigned to judges in their first two years are also 62% more likely to have more than three plans of reorganization filed, suggesting that failure to get all parties to agree to a plan likely contributes to the increased duration. We also find that judicial experience is most beneficial when judges are busiest (i.e., high caseloads).

We next study factors that might shorten judges' learning curve. Drawing on insights from the human capital and learning-by-doing literature, we predict that new judges accumulate task-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, particularly for highly complex cases which straddle multiple industries, is increasing in the diversity of business filings to which judges are exposed.

We test these predictions by analyzing all public firm cases (i.e., the most complex cases with the longest learning curves) assigned to judges with four or fewer years of judicial experience. Due to the unique composition of each court, each of these judges has different types of judicial 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 public 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 complex public cases more quickly. These results suggest that both the relevance of experience and diversity of tasks affect judges' learning curve.

Extended bankruptcy durations create both additional direct costs (e.g., legal fees) and indirect costs (e.g., loss of kep employees, suppliers, and customers). Firms with extended bankruptcies, however, could also potentially realize compensating benefits if the resulting outcome is ultimately superior (e.g., extra due diligence by inexperienced judges). We thus also examine additional outcomes available for the sample of public firms to better gauge the costs of judicial inexperience. First, we find that public cases assigned to inexperienced judges are 7.5% less likely to emerge from bankruptcy (i.e., more likely to be liquidated), but not more likely to refile for bankruptcy. Second, we find that these public cases realize 5.7% lower debt recovery rates and smaller increases in the market value of the defaulted debt throughout the restructuring process. Third, we find that the reorganized firms of inexperienced judges realize a return on assets in their first year out of bankruptcy that is 20.2 percentage points lower (273% below the the sample average of 7.4%). Although each of these individual measures captures a specific outcome, combined, the increased duration, lower likelihood of reorganization but comparable refiling rates, lower creditor recoveries, and lower post-bankruptcy firm performance are consistent with lower-quality restructuring by inexperienced judges. The evidence suggests that new judges require several years to efficiently and effectively manage complex corporate restructurings, and that the current process for acquiring those skills imposes significant costs on firms and their creditors.

To provide a sense of the aggregate costs of inexperience, we consider several counterfactual scenarios where cases are either endogenously assigned based on a judge's experience or randomly assigned among all judges with at least two years of experience. As discussed in more detail in the conclusion, reassigning just 85 highly complex public cases that were assigned to judges with two or fewer years of experience to a different random judge could reduce direct legal fees and increase credit recoveries by approximately \$10 billion.[3] Although there are certainly benefits to random assignment that must also be considered, including avoiding judicial capture by debtor firms, these "back-of-the-envelope" estimates suggest that both the direct and indirect costs of judicial inexperience may be substantial, and that there are feasible methods to reduce these costs.

Our study provides new insights into the bankruptcy process and costs of bankruptcy. Due in part to the significant direct and indirect costs associated with bankruptcy, a large literature examines predictors of bankruptcy (e.g., Altman (1968)). A closely related literature analyzes the effects of judges' discretion, specialization, behavioral mistakes, political ideology, and personal biases on rulings, case outcomes, litigation risk, and corporate tax planning (Sharfman (2005); Rachlinski et al. (2006); Posner (2008); Chang and Schoar (2013); LoPucki and Doherty (2015); Dobbie and Song (2015); Chen et al. (2016); Cohen and Yang (2018); Bernstein et al. (2019b); Huang et al. (2019); Chow et al. (2019)). We show that bankruptcy costs are impacted by time-varying

[3] Alternatives to random assignment must also address how judges will accumulate experience with highly complex restructurings. Our analyses suggest that prior experience with medium-sized restructurings possibly allows judges to accumulate sufficient human capital to subsequently efficiently handle the largest and most complex cases. This policy suggestion follows the approach used in Wisconsin, where complex cases are not assigned to new judges during their first few months (Wisconsin is the only court to currently practice any form of non-random assignment).

judicial characteristics, and that judges' on-the-bench experience is incremental to their prior work experience, education, and personal attributes. We also contribute to research on learning by doing and job-specific human capital, particularly for complex financial tasks. Prior studies provide a theoretical foundation for understanding investment in and accumulation of job- and task-specific human capital (Arrow (1962); Becker (1962); Prendergast (1993); Gibbons and Waldman (2004)), yet empirical evidence primarily focuses on relatively simple tasks performed over short horizons, with costs of inexperience that are primarily borne by the worker and difficult to measure.[4] Using a panel setting featuring high-level, highly educated economic decision makers randomly assigned to complex financial tasks, we document task-specific learning with multi-year learning curves, and that task variety and complexity accelerate the learning process. These findings suggest that other professionals managing complex financial tasks for the first time (e.g., new CEOs and CFOs, audit committee chairs, Chair of the Federal Reserve, Senate Finance Committee members, US Secretary of the Treasury, etc.) may face steep and potentially costly learning curves.

2 Institutional setting and empirical predictions

In this section we review the judge appointment process, chapter 11 filing process, empirical evidence of learning by doing, and motivation for our empirical predictions.

2.1 Judge Appointment

Each bankruptcy district has a fixed number of judgeships set by Congress.[5] When a judgeship becomes available, announcement of the vacancy is made in newspapers and bankruptcy practitioner publications. Applicants are required to be members of the bar in good standing and to have at least five years of experience practicing law, unless the circuit's judicial council determines that other relevant legal experience can be substituted. The vast majority of bankruptcy judges thus

[4]Examples include factory employees, taxi drivers, traffic stops by police officers, and farmers (Shaw and Lazear (2008); Levitt et al. (2013); DeAngelo and Owens (2017); West (2019); Haggag et al. (2017); Foster and Rosenzweig (1995)). Pisano et al. (2001) examine the adoption of new surgical procedures in health care, but focus on organizational features that promote adoption of new technologies rather than individual learning by doing. Harris and Sass (2011) and Cook and Mansfield (2016) estimate learning curves for teachers, a complex but non-financial task.

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

previously worked as lawyers before being appointed to the bench (Mabey (2005)). On average, there are 28 applicants for each judicial vacancy (Reddick and Knowlton (2013)).

Applicants are evaluated by a merit selection panel, which is appointed by circuit's judicial council. The composition of merit review panels vary across circuit courts, but typically contain five to eight members and consist of a mix of sitting judges, law practitioners, and academics. Merit review panels examine all applications and, after additional interviews, recommend three to five "best qualified" candidates in ranked order. Although there is no universal set of qualifications that merit review panels examine, evidence in Reddick and Knowlton (2013) suggests that among the most important qualities are impartiality and fairness, strong background in bankruptcy law, organizational skill, decisiveness, and a commitment to the work. The recommendations of the merit review panel are passed on to active judges in the court of appeals who make the appointment and rarely deviate from the recommendations of the merit panel. Bankruptcy judges serve renewable 14-year terms. New judges are invited to attend two one-week orientation programs organized by the Federal Judicial Center and have opportunities to attend annual workshops and special focus programs sponsored by the Federal Judicial Center to enhance their judicial skills.[6]

2.2 Chapter 11 filings

Firms—especially large firms—have some choice in where they choose to file for bankruptcy. The US Code Title 28 Chapter 87 §1408 states that a debtor can file under Chapter 11 in one of the following four locations: (1) the debtor's place of domicile or residence, commonly referred to as the place of incorporation; (2) the debtor's principal place of business; (3) the location of the debtor's principal assets; (4) any district where a bankruptcy case is pending against the debtor's affiliate. For small firms, these four locations are all the same, and thus they cannot select their bankruptcy venue.

An increasing number of large firms file in a court that is not in geographic proximity to their principal place of business or operations, a controversial practice commonly known as "forum shopping." The US bankruptcy courts for the District of Delaware and the Southern District of New York have emerged as the most popular venues among the 94 bankruptcy courts for forum shoppers

[6] https://www.fjc.gov/education/programs-and-resources-judges

since 1990 as they are generally perceived to be debtor-friendly and more efficient (LoPucki (2005); Skeel (1998)). In our empirical tests, we include court-time fixed effects to control for unobservable firm heterogeneity that is correlated with court choice. Court-time fixed effects allow us to focus on variation in judicial experience within a given court. Although courts differ in collective experience and overall efficiency, random assignment of judges and variation in judicial experience within courts imply that firms can be assigned an inexperienced judge even within a popular venue.

2.3 Related literature and empirical predictions

Prior research suggests that many types of workers "learn by doing," in which they develop task-specific human capital and become increasingly efficient over time (Arrow (1962); Becker (1962); Lucas (1988); Lazear (2009)). While much of this research focuses on relatively simple tasks, there is also evidence that analysts, auditors, fund managers, and mid-level managers become more efficient at handling fairly complex financial tasks over time. Other research, however, casts doubts on these findings based on documented survival biases, the endogenous matching between worker and task, and confounding events (such as promotion incentives). Well-identified studies examining learning by doing for complex financial tasks are fairly sparse.

Due to the bankruptcy setting's unique institutional details, including random case assignment, 14-year appointments, lack of promotions or explicit incentives, and observable costs, we can provide estimates of the costs of judicial inexperience. Given their prior legal experience, rigorous hiring process, access to fellow judges, and resources/orientation provided by the Federal Judicial Center, new judges could feasibly "hit the ground running" with a minimal learning curve. However, there are also reasons to suspect that for judges' most complicated tasks, namely corporate restructurings, there is likely a learning curve. Although a new judge may have frequently interacted with and/or observed judges previously as an attorney, being a judge is fundamentally different from seeing a judge. Furthermore, Chapter 11 filings (particularly public firms) often involve large amounts of debt and a diverse set of both secured and unsecured creditors, equity committees, numerous subsidiaries, entrenched management, multiple law firms, and dozens of lawyers. Our primary prediction is that judges accumulate valuable expertise by overseeing Chapter 11 filings, such that experienced judges are able to more efficiently manage the bankruptcy process, and that this time-varying expertise is incremental to other fixed judge characteristics (such as previous work experience, gender, etc.).[7] Although the benefits of the bankruptcy setting provide high internal validity for our estimates of the costs of judicial inexperience, our results are generalizable in the sense that other financial professionals facing complex tasks for the first time also likely face potentially steep and costly learning curves.

We hypothesize several mechanisms whereby experienced judges more efficiently process Chapter 11 filings. First, experienced judges may require fewer actions to complete the restructuring. For instance, longer-tenured judges may be more decisive, or better foresee potential issues and prevent unnecessary disputes. Second, judges may be able to rule faster on motions, expediting the process and reducing the costs of bankruptcy. Finally, lawyers may be responsible for improvements in judicial efficiency. For instance, judges' case history may help lawyers better understand which motions the judge will approve, thereby reducing the number of unnecessary motions filed in court. Although this final mechanism is also consistent with learning by doing (albeit for matched groups of lawyers and judges), we conduct cross-sectional tests to further examine learning by judges. We expect that the effect of judicial experience on case outcomes should not differ by judge caseload if the results are due to lawyers learning, since a judge's caseload should not affect lawyers' incentives to learn a judge's style. In contrast, judicial experience is expected to matter more when judges face high caseloads, which would be more consistent with a judge learning explanation.

Finally, based on insights from the learning by doing and human capital literature (e.g., Arrow (1962); Becker (1962); Lazear (2009)), we examine two factors that potentially accelerate judges' learning curve. First, we posit that judges who accrue experience early in their judicial career that is more relevant for large Chapter 11 cases 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 complex Chapter 11 filings.

[7] Arora (2018) finds that director effort impacts the probability a firm emerges from bankruptcy, consistent with the notion that individuals can impact the bankruptcy process.

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 professional skills. For judges who only see small cases from the same industry, exposure to diverse cases potentially has limited usefulness. Large cases, however, are inherently complex and typically include subsidiaries that span multiple industries. We thus predict that judges exposed to a greater diversity of business cases "move up the learning curve" faster and can more efficiently handle complex cases.

3 Data and Variable Construction

3.1 Chapter 11 Sample

Our analysis begins with a comprehensive sample of all corporate Chapter 11 filings obtained from LexisNexis filed between 1993 and 2012. We begin with a sample of 133,050 total business Chapter 11 bankruptcy filings with valid judicial experience information, and remove 345 cases filed in Wisconsin (where the court's policy is not to randomize case assignment for new judges—see Section 4). We further remove 27,581 duplicate cases in which two subsidiaries are both assigned to the same judge and remain in court for the same period of time, and 24 "singletons" (judges or court-years with only one case). Our final LexisNexis sample consists of 105,100 cases assigned to 574 unique judges in 89 bankruptcy courts. Since nearly all of these filings are small, private companies, we cannot observe firm characteristics such as size or industry. To proxy for size, we define a variable *Log(Num Filings)*, where *Num Filings* is the number of individual bankruptcy filings created by subsidiaries of the same firm that enters Chapter 11. For this sample we observe starting and ending dates, the judge assigned to the case, and the filing court.[8] Because of the

[8] We can identify whether a case is dismissed by the court or converted to Chapter 7 by the judge but are not able to determine whether the firm is liquidated in Chapter 11 or reorganized.

large number of cases in the LexisNexis sample, we include court-year and judge fixed effects when analyzing these cases.

We also separately analyze a sample of Chapter 11 filings by large public firms which have more detailed case level information. Specifically, this sample contains all Chapter 11 filings by US public firms with a filing date between 1980 and 2012 and that have assets of at least $50 million, retrieved from UCLA LoPucki Bankruptcy Research Database (BRD) and New Generation Research's bankruptcydata.com.[9] We identify 1,424 such Chapter 11 filings, and collect detailed information on firm characteristics at the time of filing, plan confirmation and effective dates, restructuring outcomes (emergence, acquisition, liquidation in Chapter 11 or converted to Chapter 7), and the judge assigned to the case. We drop five cases that were not confirmed as of 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. Our final sample comprises 1,304 Chapter 11 filings assigned to 306 unique judges located in 74 bankruptcy courts. 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).

In both the LexisNexis and public firm samples, the main outcome variable we focus on is *Duration*. In the LexisNexis sample, *Duration* is defined as the natural logarithm of the number of months from the Chapter 11 filings until the case is (i) closed after completing a Chapter 11 restructuring (emergence or liquidation), (ii) converted to Chapter 7, or (iii) dismissed from court. In the public firm sample we observe the date on which a Chapter 11 plan is confirmed, and so define *Duration* as the natural logarithm of the number of months from the Chapter 11 filing date to plan confirmation date. In both samples, *Duration* proxies for the overall costs of restructuring.[10]

To investigate the mechanism through which judge experiences affects *Duration*, we gather docket information from PACER for 532 public firm 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,

[9] Specifically, we require that 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.

[10] 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.

reorganization plans, etc.) with the judicial order ruling on each motion. The average length of time that it takes a judge to rule on motions measures the efficiency of judges in resolving complex issues that arise in bankruptcy. 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. We also collect how many plans of reorganization are filed by the debtor, one of the most important documents submitted by management which outlines how creditors will be paid back and which must be approved by the creditors and confirmed by the judge.

We also test how judicial experience affects other bankruptcy case outcomes for the public firm sample. *Emergence* is an indicator variable set equal to one if a firm emerges from Chapter 11, and *Refile 3Y* is 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 efficient restructuring, although we caution that we cannot measure full economic efficiency due to an inability to observe what happens to the assets of liquidated firms. We measure creditors' payoff using the total recovery rate ($Total\,Recovery$), defined as the average recovery rate across all debt instruments listed in the reorganization or liquidation plan, and changes in the market value of debt from bankruptcy filing to plan confirmation ($\Delta\,Debt\,MV$) using Moody's Default & Recovery Database (DRD).[11] These two variables provide evidence on how the bankruptcy process impacts creditor welfare. We measure post-bankruptcy performance using *ROA Post*, calculated as net income scaled by total assets, which provides insights on how judicial experience impacts the subsequent profitability of restructured firms (Hotchkiss (1995); Kalay et al. (2007)).

3.2 Judge Experience and Personal Attributes

We compile bankruptcy judges' career history using resumes from bankruptcy courts, supplemented with information posted on 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

[11]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 smaller sample for these tests.

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.

Because learning is unobservable, we use judicial experience (time since appointment) as our primary measure of judge learning under the assumption that judges become more efficient the longer they have worked as a judge. We define two measures of judicial experience: *Log(Months)*, the natural logarithm of the number of months since a judge has been appointed to the bankruptcy court as of the case filing date, and, to capture any nonlinear effects, *First 2Y*, an indicator for cases assigned to judges in their first two years.[12] As a robustness test, we also validate our main results using a cumulative filing-based measure of judicial experience, available for more recently appointed judges. To measure judges' other professional experience, we use *Log(Years before Bench)*, the number of years of professional work experience since law school graduation.[13] We use four indicator variables for judges' personal characteristics: *Top5 Law School*,[14] *Male*, *Military*, and *Democrat*. See the Appendix for detailed variable definitions.

3.3 Summary Statistics

We summarize LexisNexis case characteristics in Panel A of Table 1. The average case spends 16.81 months in Chapter 11, and a total of only 7% of all cases are filed in the well-known bankruptcy centers of Delaware and the Southern District of New York. The median judge assigned to these cases has 9.6 years of experience, but there is significant variation in judicial experience, with 11% of all LexisNexis cases being assigned to judges in their first two years on the bench.

A much richer set of case characteristics are available for the public firm sample, summarized in Panel B of Table 1. The average judge has been on the bench for 9.5 years (standard deviation of 85.22 months), and 13% of the public firms (173) are assigned to judges who are in their first two years. For our sample of 1,304 public firms, the average case spent 16.57 months in Chapter

[12] Job tenure has been used by a number of prior studies to capture learning by doing and accumulation of job specific experience (see, for example, Shaw and Lazear (2008); Harris and Sass (2011); DeAngelo and Owens (2017)). We interchangeably use the terms "experience" and "tenure."

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

[14] 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.

11, and 57% of these cases emerged from Chapter 11. Conditional on emergence, 8% of the public firms refiled for Chapter 11 within 3 years. For 532 public firms with electronic dockets, the average case files 150 motions (some filed simultaneously) and each motion takes on average 33 days from filing to the issue of a corresponding order. The median number of plans filed by the debtor during bankruptcy is two, with 21% of cases filing more than three plans ($High\,Plans$). For public firms with 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%. The average ROA in the first year out of bankruptcy is 7.4% with a large standard deviation of 70.53%. Public firms have average assets at the time of filing of $2,113 million in 2016 US dollars (median $490.6 million), a liabilities-to-assets ratio of 1.01, and a -24% return on assets. Twenty-nine percent of cases are filed in Delaware, and 18% are filed in the Southern District of New York (NY SD).

In Panel C of Table 1, we document personal characteristics for the sample of judges who ever oversee a public firm, and in Panel D we document the corresponding correlation matrix. The average judge has 18 years of work experience before becoming a judge. Seventy-nine percent of these judges are male, 12% graduated from a top 5 law school, 23% served in the military, and 64% are affiliated with the Democratic party. Judges who went to a top law school tend to have more prior work experience. Ninety-two percent of judges worked previously as lawyers, and 10% of judges appointed after the year 2000 were previously listed as a bankruptcy lawyer on a Chapter 11 case docket.

4 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, provide anecdotal evidence, review prior research on random assignment, and perform two sets of empirical tests to support the notion that corporate 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 81 courts which contain 94% of the LexisNexis cases and 97% of the public firm cases. Table 2 provides a list of the courts and a summary of their case assignment procedure. Of these 81 courts, only one court uses a policy involving non-random assignment.[15,16] Several courts indicate they use the Federal Judiciary's comprehensive CM/ECF system "that has a 'card deck' for each chapter with each judge having the same number of cards in the deck...allowing random assignment but keeping the number of cases per judge equal" (email from court clerk for the district of New Mexico dated Dec 9, 2019).[17] In 26 courts, judge assignment is deterministic, either because there is only one judge (16 courts) or because each judge only takes cases from specific counties within their district (10 courts). Courts' stated policies clearly support the notion of random assignment.

Second, anecdotal evidence supports the notion of random assignment, even for large public firms. At the time of its bankruptcy filing in November 2011, AMR Corporation, the parent company of American Airlines, had $25 billion in assets (inflation adjusted). AMR filed in the Southern District of New York, a popular venue, and was assigned to Judge Sean H. Lane, who was appointed to the bench only fourteen months earlier. Prior to his appointment, Judge Lane worked in the US Attorney's Office. Even the largest U.S. bankruptcy to date, Lehman Brothers, was assigned to a relatively inexperienced judge. Judge James M. Peck was appointed to the bench just 32 months prior to Lehman's filing. Although Judge Peck had more than 30 years of legal experience prior to his appointment (including a focus on bankruptcy law), he had the second least amount of judicial experience of the 10 judges serving on the bench at the time of Lehman's filing. First Republic Bank Corp ($68 billion in assets), Adelphia Communications ($29 billion), Federated Department Stores ($15 billion), and many more large firms were assigned to judges in

[15] 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 all cases filed in Wisconsin. The method of randomization varies by court and includes a computerized random draw procedure or a blind rotation system.

[16] Technically, judge random assignment occurs at the divisional office level, as cases are filed in a particular office of a bankruptcy district. Nearly all public cases are filed in the main divisional office of each district. For example, among public 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.

[17] https://www.pacer.gov/cmecf/

their first two years. Anecdotal evidence confirms that large firms can be assigned inexperienced judges, consistent with random assignment.

Third, there has been an increasing number of studies that exploit the random assignment of bankruptcy judges for empirical identification (Chang and Schoar (2013); Dobbie and Song (2015); Bernstein et al. (2019a,b)). These studies uniformly find evidence that bankruptcy case characteristics are orthogonal to judge characteristics. For example, Bernstein et al. (2019b), 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. Experienced judges may compete for large public 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 public 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 both our LexisNexis sample and public firm sample.

If case assignment is independent of judicial experience, then each judge within a court should have an equal probability of being assigned a new case. 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 hold constant all case-specific characteristics, we include case fixed effects. Thus our analysis exploits within-case variation in the judicial experience of judges serving in the court at the time case j was filed. If cases are more likely to be assigned to experienced judges, then the coefficient β_1 will be positive for *Log(Months)* and negative for *First 2Y*. A lack of any significant relationship is consistent with random assignment with respect to judicial experience. We cluster standard errors by court.

To identify the set of eligible judges when a case was filed we use the LexisNexis judges' appointment and retirement dates, and thus conduct the tests for both the LexisNexis and public firm samples over the period 1993–2012. Identifying eligible judges is complicated, however, by at least two features of bankruptcy courts. First, 49 bankruptcy courts in our sample occasionally assign cases to judges appointed to a different court. These "visiting judges" are used to distribute workloads and handle cases where there are conflicts of interest for all of the court's own judges. 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 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 Chapter 11 case.[18]

We address these issues by dropping all cases assigned to a visiting or district judge and exclude 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). We further drop cases with only four or fewer eligible judges due to difficulties empirically evaluating random assignment in small samples. Our final LexisNexis randomization sample consists of 66,915 cases filed in 35 courts and assigned to 372 different judges. These cases had on average 9.3 *potential* judges (median of 7) serving at the same time that could have been assigned the case, resulting in 614,545 case-judge pairs. Our public firm

[18]An additional 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).

sample consists of 642 cases filed in 27 courts and assigned to 168 different judges. These public cases have on average 8.9 potential judges (median of 8), resulting in 5,646 case-judge pairs.

Table 3 Panel A presents the results of estimating equation (1). Columns (1) and (2) analyze the LexisNexis sample, and columns (3) and (4) the public firm sample. The unconditional probability of being assigned a case (mean of the dependent variable) is 0.109 for the LexisNexis sample and 0.114 for the public firm sample. In both samples, we find that *Log(Months)* and *First2Y* are statistically unrelated to case assignment. The coefficient estimates are not only insignificant, but also economically small relative to the mean of the dependent variable (i.e., the estimate in column (1) is 1.8% of the dependent variable), also consistent with case assignment that is independent of judicial experience. Importantly, we document significant within-case variation in judicial experience. For the LexisNexis sample, the average within-case standard deviation of *Months as Judge* is 88.2 months, with a standard deviation of 36.5 months. For the public firm sample the corresponding mean and standard deviation are 85.4 and 31.1, respectively. This significant within-case variation in judicial experience suggests that the lack of a significant relationship is not due to lack of variation in the explanatory variable of interest.

Our second set of empirical tests evaluates whether there is any correlation between the assigned judge's experience and observable firm characteristics. If cases are assigned randomly with respect to experience, then firm characteristics should be uncorrelated with the assigned judge's experience. For both the LexisNexis and public firm samples we estimate regressions of the following form:

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

where $JudgeExp_{i,j}$ is one of the two measures of the assigned judge i's tenure at the time case j was filed. For the LexisNexis sample we use $Log(Num\,Filings)$, as it is the only available firm characteristic available for this sample. For the public firm sample, we include $Log(Num\,Filings)$, as well as $Log(Assets)$, $Leverage\,filing$, and $ROA\,filing$. These variables allow us to examine whether variation in firm performance and complexity at the time of filing are related to the experience level of the assigned judge. We include court-year fixed effects in the LexisNexis sample and court-period fixed effects in the public firm sample 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)).[19] For the public firm sample we also include Industry (Fama French 12) fixed effects. We cluster standard errors by court.

Table 3 Panel B presents coefficient estimates of equation 2. Columns (1) and (2) analyze the LexisNexis sample, and columns (3) and (4) the public firm sample. Across both samples, we find that each firm attribute is insignificantly associated with the assigned judge's level of judicial experience, suggesting that cases with these characteristics are not systematically assigned to judges with certain levels of experience.

5 Judicial Experience and Case Duration

In this section, we analyze the relationship between judicial experience and bankruptcy duration. We first present baseline results using both the LexisNexis and public firm samples. We then focus on the public firm sample to examine several mechanisms through which experience affects case duration and test for factors that can accelerate judges' learning curve.

5.1 Baseline Results

To test the impact of judicial experience on Chapter 11 case durations, we estimate OLS regressions of the following form:

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

using the $Duration_{i,j,t}$ and $JudgeExp_{i,j,t}$ measures mentioned previously for each case j assigned to judge i in year t.[20] In the LexisNexis sample, we include court-year fixed effects to control for trends in bankruptcy outcomes within each court (Bharath et al. (2010)) and judge fixed effects as previous

[19] We discuss the justification for including court-period fixed effects for the public firm sample in Section 5.1.

[20] 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 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 5.3.

work has documented that fixed judge characteristics play an important role in affecting bankruptcy outcomes (Chang and Schoar (2013); Dobbie and Song (2015); Bernstein et al. (2019b)). Because the public firm sample has far fewer observations for each court, we lack the statistical power to include court-year fixed effects. To approximate these fixed effects as closely as possible, we include court-period fixed effects to control for trends in individual courts (including changes in the judge's cohort) as well as for fixed differences in cases across courts.[21] Similarly, in the public firm sample we are unable to include judge fixed effects because most judges see only one or two cases in this sample. To control for any fixed judge characteristic, we thus estimate equation (3) using the LexisNexis sample but exclude all public firms. We then extract the judge fixed effect coefficients from the regression as proxies for each judge's fixed impact on case durations, and include these estimates as a single control variable in the regressions for the public firm sample ($Judge\,Duration\,FE$). In addition, we include $Log(Num\,Filings)$, $Log(Assets)$, $Leverage\,filing$, and $ROA\,filing$ as controls for the public firm sample, as well as industry fixed effects.

Panel A of Table 4 presents coefficient estimates of equation (3), with columns (1) and (2) utilizing the full LexisNexis sample, and columns (3) and (4) analyzing the public firm sample.[22] We find that a judge's time on the bench significantly reduces bankruptcy duration. The coefficient estimates in columns (1) and (3) can be interpreted as elasticities, and suggest that being randomly assigned to a judge with twice as much time on the bench (e.g., 2 vs. 4 years) is associated with a 5.8% decrease in bankruptcy duration in the LexisNexis sample (a decline of 1 month relative to the mean of 16.8 months), and a 7.6% decline in the public firm sample (a decline of 1.3 months relative to the mean of 16.6 months). The coefficient on $First\,2Y$ in columns (2) and (4) suggests that this effect concentrates during judges' early years, with an economically significant impact of *inexperience* on duration: average cases assigned to judges in their first two years have 6% longer durations across all Chapter 11 bankruptcies, and 27% longer durations among public firms (increases of 1.0 and 4.5 months, respectively).[23] Consistent with the intuition that on-the-job

[21]We use three time periods that mark important changes in the bankruptcy landscape over time (1980-1994, 1995-2005, and 2005-2012). Delaware rose to prominence as a major bankruptcy court in 1995 (LoPucki (2005)), and the Bankruptcy Abuse Prevention and Consumer Protection Act (BAPCPA) was passed in 2005.

[22]We tabulate coefficient estimates for all control variables in the public firm sample in Appendix Table A1.

[23]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 $100[exp(\beta_1) - 1]$.

experience matters most for highly complex cases, we find larger effects in the public firm sample relative to the LexisNexis sample using both measures of judicial experience.

In Panel B of Table 4 we use an alternative approach to examine the role of fixed judge characteristics. This panel focuses exclusively on the public firm sample, for which we have collected information on each judge's background. We proxy for judges' prior professional experience using *Log(Years before Bench)* and four measures of personal characteristics (*Top 5 Lawschool*, *Male*, *Miltary*, *Democrat*). We find that including these additional characteristics as controls in place of the estimated judge duration fixed effect does not affect the point estimates nor reduce the significance of our time-based judicial experience measures. Previous work experience does not have a large effect on *Duration*, in sharp contrast to the effects of judicial experience.[24] Among other personal characteristics, we find that time in bankruptcy is shorter when cases are assigned to male judges, consistent with judge time-invariant preferences (Chang and Schoar (2013); Dobbie and Song (2015); Bernstein et al. (2019a,b)). 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 coefficient estimates for judicial experience are qualitatively similar. The evidence suggests that the judicial experience effects we estimate are separate from previously documented judicial biases.

We perform a number of robustness tests which are reported in the appendix. One concern is that the largest firms in our sample potentially engage in "forum shopping" and selectively choose to file in a court where they expect to have a sympathetic or experienced judge. Importantly, court-year and court-period fixed effects help address this concern by only comparing cases filed in the same court and same time period to each other. In addition, we find similar results when we remove firms with likely the largest incentives to forum shop: firms with more than one subsidiary (LexisNexis sample) and the largest 20% of cases in asset size (public firm sample). See Appendix Table A3. We also find similar results if we drop cases filed in either Delaware or the Southern District of New York, the two courts at which most forum shopping occurs (see Appendix Table A4).

[24] We do find, however, that judges with more prior work experience are able to move up the judicial learning curve more quickly. To test this, we interact the dummy variables *Year1-2*, *Year3-4*, and *Year5-6* with *Log(Years before Bench)* and run regressions similar to those in Figure 2. In Appendix Table A2, we find that the coefficients for all interacted variables are largely negative and statistically significant at the 5% or 10% level. The combined evidence suggests that although prior work experience does not have a direct effect on bankruptcy outcomes, it does help accelerate judges' accumulation of job-specific skills.

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.[25] As a robustness test, we only include cases assigned to judges during their first term and find qualitatively similar results (see Appendix Table A5). In addition, we remove a handful of cases from 13 courts where judge assignment is deterministic (i.e., courts with only one judge or courts that assign cases to judges based on specific geographic areas). Our main results remain robust in Appendix Table A6.

Our main analysis examines both the elasticity of *Duration* with respect to judicial experience as well as the average *Duration* associated with inexperienced judges (i.e., judges with two or fewer years on the bench). We next expand this analysis to examine average *Duration* 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 in which period of a judge's tenure a case was filed, and include these dummy variables as measures of judicial experience, where the omitted category, and thus benchmark, is the average outcome of cases assigned to the most experienced judges. By testing for differences across the coefficient estimates on these judicial experience indicators, we are able to estimate when case outcomes of new judges become indistinguishable from the case outcomes of more experienced judges.

We first estimate the learning curve for the LexisNexis sample using the following regression specification:

$$\begin{aligned}\text{Duration}_{i,j,t} =& \alpha + \sum_{k=1}^{4} \beta_k \text{Year}^k_{i,j,t} + \beta_5 \text{Year5-6}_{i,j,t} + \beta_6 \text{Year7-8}_{i,j,t} + \beta_7 \text{Year9-10}_{i,j,t} \\ &+ \delta \text{Judge FE} + \theta \text{Cour-Year FE} + \epsilon_{i,j,t}\end{aligned} \tag{4}$$

In Figure 1, we plot the β coefficient estimates and 90% confidence intervals for each of the judicial experience dummy variables, individually for years 1, 2, 3, and 4, and then in two-year periods for years 5-6, 7-8, and 9-10. Judges with more than ten years of experience form the benchmark control group. We find that the effects of judicial inexperience are concentrated early in a judge's

[25] 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.

tenure, with cases assigned in a judge's first year experiencing 8.8% longer durations than cases assigned to more experienced judges. After the first year, we find only insignificant coefficient estimates. In Appendix Figure A1, we estimate judges' learning curve for the LexisNexis sample using the previous count of Chapter 11 cases, and find that case durations are significantly longer but declining over the first 50 cases assigned to the judge (see also Appendix Table A7 columns (1) and (2) for coefficient estimates of a modified version of equation 3 that uses case counts as the judge experience measure). Thus, judges who have been on the bench for more than a year or have seen approximately 50 cases realize similar case durations as more experienced judges.

In Figure 2 we plot the learning curve coefficient estimates for the public firm sample using the following specification:

$$\begin{aligned}\text{Duration}_{i,j,t} =& \alpha + \beta_1 \text{Year1-2}_{i,j,t} + \beta_2 \text{Year3-4}_{i,j,t} + \beta_3 \text{Year5-6}_{i,j,t} + \beta_4 \text{Year7-8}_{i,j,t} \\ & + \beta_5 \text{Year9-10}_{i,j,t} + \gamma \text{Controls} + \delta \text{Industry FE} \\ & + \theta \text{Court-Period FE} + \rho \text{Judge Duration FE} + \epsilon_{i,j,t}\end{aligned} \tag{5}$$

Because these bankruptcy cases are more complex and because most judges are not assigned to public cases very frequently, we anticipate a significantly longer learning curve for this sample. Consistent with this expectation, we document a clear and lengthy declining trend. The coefficient estimates translate into 29% longer durations (4.9 months) in the first two years and 12.2% longer durations (2.0 months) in years 3–4, respectively. Statistically, we find no difference between the coefficients for *Year1-2* and *Year3-4*, suggesting only slight improvements in efficiency during this time period. The coefficient estimates on the remaining dummies are insignificant, suggesting similar durations as cases assigned to more experienced judges. In Appendix Figure A2, we estimate a similar learning curve using previous counts of Chapter 11 filings as an alternative measure for judicial experience and find that until judges have seen approximately 200 filings (public or private), their complex public cases realize significantly longer duration (see also Appendix Table A7 columns (3) and (4)).

We note that the learning curve is flat in year two (or around 50 Chapter 11 cases) for the LexisNexis sample, whereas it can take up to four years (or 200 Chapter 11 cases) for a judge to manage complex public cases in a manner similar to more experienced judges. These relatively different learning curves are consistent with faster learning for frequent, simpler tasks, and slower learning for infrequent, complex tasks. Previous work documents even 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, and Jovanovic and Nyarko (1995) estimate learning curves ranging from two weeks for munitions manufacturing workers to one year for insurance sales. With this perspective, even a learning curve of one year or 50 cases highlights significant differences between learning curves for relatively straight forward vs. complex tasks.[26]

Given that the average judge in our sample is appointed at age 47, one might expect a reversal in the learning curve for the longest-tenured judges, due possibly to a deterioration in cognitive ability or a lack of performance incentives as judges near retirement. The data do not support this. Instead, judges appear to maintain similar levels of productivity throughout the end of their terms. Finally, we note that the shape of the learning curves in Figures 1, 2, A1, and A2 supports our identification assumption, as potentially confounding factors such as judges' biases are unlikely to affect case outcomes in the same time-varying manner as judicial experience.[27]

5.2 Mechanism

We next investigate mechanisms whereby experienced judges accelerate the bankruptcy process. In this subsection, we use data on motions filed during a case to examine how experienced judges resolve cases faster. First, we test whether there are fewer motions filed in cases assigned to more experienced judges. In columns (1) and (2) of Table 5, we find no significant relationship between judicial experience and the number of motions filed. Experienced judges do not appear to accelerate bankruptcies by reducing the total actions taken by firms and their lawyers during bankruptcy.

Second, we examine whether experienced judges are quicker to rule on motions. To test this hypothesis, we find the judge's order that is associated with each motion filed in court (many filed simultaneously) and calculate *Ave Days(Ruling)* across all motions (except for first-day motions)

[26]Incentives also likely matter for the length of the learning curve. Judges are paid a flat salary and thus have no direct monetary incentive to process cases faster. Judges possibly establish potentially valuable reputations or lighten their caseloads by processing cases more quickly, but could also desire a "quiet life" (Bertrand and Mullainathan (2003)), and only slowly move up the learning curve.

[27]Dobbie and Song (2015) and Bernstein et al. (2019b) find that judges' biases with respect to case emergence are not time-varying.

filed in a bankruptcy case. The results, presented in columns (3) and (4) of Table 5, demonstrate that inexperienced 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 5.6 days longer (a 16.7% increase). These economic magnitudes are comparable to the overall effects of judicial experience on $Duration$, suggesting that a significant portion of the overall decrease in duration appears to be due to experienced judges' ability to rule faster on motions.

We next test whether there are fewer reorganization plans filed in cases with experienced judges, which would suggest that experienced judges are able to establish consensus among all parties faster. In Appendix Table A8 we find that cases assigned to judges in their first two years are 62% more likely to have three or more plans of reorganization than more experienced judges.[28] A failure to quickly get all parties to agree to a reorganization plan plausibly contributes to the overall increase in duration for inexperienced judges.

An alternative explanation for our findings is that lawyers, not the judges themselves, learn over time by observing judges' decision-making. While we cannot fully rule out this hypothesis, the insignificant association between judicial experience and the number of motions filed suggests that lawyers are not changing their actions in an observable way. To more fully explore this alternative explanation, we next examine the relative importance of judges' job-specific experience during periods of differing caseloads to provide suggestive evidence on the mechanism driving our results.

Because the number of judges in a court is fixed, when more firms and individuals file for bankruptcy, 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 conflicts. These cases require judges' close attention and often daily rulings. During periods of elevated caseloads, judicial 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 judicial experience on case outcomes is driven by lawyers learning about judges' decision making, the effect of judicial experience on case outcomes should

[28] Calculated as the coefficient on $First\ 2y$ of 0.131 divided by 0.21, the sample average of $High\ Plan$. Inexperienced judges thus have more plans filed (Table A8) but not significantly more motions filed (Table 5), likely because filing more plans does not require filing significantly more motions.

not differ by caseload, since lawyers have incentives to learn about judge's past rulings regardless of the current court caseload and have the ability to turn down cases if they are too busy.

We measure the current caseload of each judge as the weighted number of bankruptcy filings in the court-quarter when a firm files for Chapter 11.[29] This weighted caseload measure approximates the number of hours (per year) a judge would spend administering the bankruptcy cases filed in his/her bankruptcy district, and thus proxies for the overall time constraints the judge faces. We conduct this test using the public firm sample, where a complex cases is likely more demanding for a judge with an already high caseload.

Table 6 splits the public firm sample by the sample median court caseload. We continue to include court-case controls, as well as court-period, judge duration, and industry fixed effects. We find that judges' judicial experience has a larger impact in periods with above-median caseloads (*High*). Panel A shows that judicial experience significantly reduces *Duration* 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 examining the impact on *Ave Days(Ruling)*. In terms of *Duration*, the effect of experience is 2 to 3 times larger in the high caseload subsample, and when examining *Ave Days(Ruling)* estimated effects are 1.5 to 10 times larger in the high caseload sample. The evidence suggests that experience matters most when judges are busiest, which is more consistent with judges accumulating valuable on-the-job experience rather than lawyers learning judges' preferences and style.

5.3 Learning Accelerators

The results presented thus far demonstrate that judges with more judicial experience resolve bankruptcy cases faster, with particularly lengthy learning curves for complex public firms. The slow learning process for complex bankruptcies suggests that significant costs could be avoided if judges could move up the learning curve more quickly. In this section, we use the public firm sample to examine whether the types and/or diversity of filings a judge sees impacts their learning curve, based on insights from the learning by doing and human capital literature (see Section 2.3).

[29] The weights come from Bermant et al. (1991), who suggest specific hours that judges approximately spend on six distinct types of bankruptcy cases (Chapters 7, 9, 11, 12, 13, and 15).

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 court filings across filing types (Chapters 7, 11, 13) and the nature of 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.

Using these measures, we estimate a modified version of equation (3). 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.

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 handle large Chapter 11 cases efficiently.

6 Other Bankruptcy Outcomes

Our main analysis focuses on the effect of judicial experience on *Duration*, a proxy for overall bankruptcy costs. Although there are certainly more costs (both direct and indirect) associated with a lengthier bankruptcies, it is not clear whether lengthier bankruptcies are less efficient restructurings. Longer bankruptcies could reflect higher and more careful judicial scrutiny, resulting in more optimal reorganizations/liquidations, and shorter bankruptcies could impose additional costs on firms and creditors if judges are "kicking the can down the road." We therefore also study how judicial experience affects other bankruptcy outcomes to better understand the overall economic costs of judicial inexperience. In this section, we use detailed information on case outcomes available in the public firm sample to estimate equations similar to equation (3), using as dependent variables $Emergence$, $Refile3Y$, $Total\ Recovery$, $\Delta\, Debt\ MV$, and $ROA\ Post$ to provide a more complete assessment of the costs of judicial inexperience for bankrupt firms.

In Table 8 Panel A we analyze $Emergence$ and find that large cases assigned to judges with more time on the bench are significantly more likely to emerge. Being randomly assigned to a judge with twice as much time on the bench (e.g., 2 vs. 4 years) leads to a 3% increase in the likelihood of emergence (5.2% of the sample $Emergence$ mean of 0.57). Public cases assigned to judges in their first two years are 7.5% less likely to emerge, corresponding to 13.2% of the sample mean.[30] 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 ($Refile3Y$). Taken together, the evidence in Table 8 Panels A and B suggests that experienced judges improve the likelihood that firms emerge from bankruptcy, but not at the cost of higher refiling rates.

To provide suggestive evidence on creditors' welfare we examine debt recovery rates and changes in firms' market value of debt during bankruptcy in Table 8 Panels C and D. We find in all specifications that both $Total\ Recovery$ and $\Delta\, Debt\ MV$ are significantly lower for cases assigned to judges with two or fewer years of experience, and that $Total\ Recovery$ is significantly increasing in judges' total time on the bench (*Log(Months)*) in column (1) of Panel C. The reduced significance for *Log(Months)* in Panel D column (1) 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 suggest that creditors recover 5.7% less at plan confirmation and that their bonds experience 21% 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.

Finally, we examine the performance of public firms post bankruptcy. In Panel E, we find that ROA in the first year post bankruptcy is increasing in the judge's experience. Doubling the judges' years on the bench is associated with a 5.1% increase in ROA, 69% of the average ROA of 7.4%.

[30] We continue to include $Judge\, Duration\, FE$ (estimated using the LexisNexis sample) to address fixed judge characteristics when analyzing other bankruptcy outcomes in this section, but note that results are similar if we drop this control or alternatively include controls for judge personal attributes (e.g., gender).

The cost of inexperience is particularly stark. The reorganized firms of inexperienced judges realize a ROA one year out of bankruptcy that is 20 percentage points lower than the sample average.[31] Experienced judges appear to better position their firms for superior performance post bankruptcy.

Overall, the evidence suggests that as judges accumulate valuable on-the-bench experience they become more efficient, with their large public cases realizing shorter time in bankruptcy with faster rulings on motions, higher likelihoods of emerging from bankruptcy with similar refiling probabilities, higher financial performance, and better recovery rates for creditors. Taken together, the findings suggest likely large financial costs for bankrupt firms assigned to inexperienced judges. We next discuss back-of-the-envelope estimates of the costs of judicial inexperience, with the caveat that we cannot fully pin down the overall welfare effects of judicial inexperience because there are benefits of judge random assignment that we do not measure.

7 Discussion and Conclusion

Exploiting the random assignment of Chapter 11 filings to bankruptcy judges, we document that judicial inexperience imposes significant financial costs on firms in bankruptcy. We verify that the assignment of Chapter 11 bankruptcy cases is independent of judicial experience, and find that cases assigned to new judges spend more time in bankruptcy, due principally to the judge's inability to quickly rule on individual motions. Among large, public firms, we find that cases assigned to new judges are also less likely to be kept as a going-concern but are not more likely to refile for bankruptcy after emergence, and that theses cases realize lower creditor recovery rates and a lower return on assets post bankruptcy. The findings are collectively consistent with new judges being less efficient at managing the restructuring process for large complex firms.

Our estimates of judges' learning curve suggest that it takes up to four years for a judge to efficiently manage complex Chapter 11 filings. Exposure to business filings and a greater diversity of case types accelerates judges' learning curve. Judges' non-judicial experience and personal attributes are not consistently related to bankruptcy outcomes and do not explain these findings.

[31] In untabulated analysis, we find no evidence that post-bankruptcy leverage ratios significantly differ for inexperienced judges, suggesting that the decrease in firm performance is not driven by these firms having more debt.

Our findings have implications for policies surrounding the bankruptcy filing process (e.g., proposed Bankruptcy Venue Reform Act), assignment of cases to judges, and recruitment and training of judges. More broadly, our estimates show that learning curves can be long and costs of inexperience high even for educated workers with prior experience. Understanding these costs and how individuals move up the learning curve has important implications for how organizations hire and train workers involved with complex tasks, and for how such tasks are assigned to employees.

While on-the-job learning is clearly costly, a unique feature of bankruptcy courts is that these costs are generally not borne by judges, but rather by firms already in financial distress. These costs, however, are not unavoidable and can be reduced through feasible policy adjustments. We envision several counterfactual scenarios, and estimate "back-of-the-envelope" possible reductions in aggregate costs for each of these counterfactuals relative to the current policy of complete random assignment. Because these estimates require information on firm size, we use the public firm sample and thus provide a significant lower bound on the total costs of inexperience.[32]

First, based on estimates of total legal fees by LoPucki and Doherty (2004) and Bris et al. (2006), we estimate that aggregate legal fees across the public firm sample would decrease by \$21.2 billion due to reduced case duration if all large Chapter 11 cases in our sample were assigned to the most experienced judge in the court where the case was filed.[33] An alternative and more selective approach would focus on only those cases that were assigned an inexperienced judge. In our sample, 85 large public cases filed after 1992 were assigned to a judge with less than 24 months of on-the-bench experience, when another more experienced judge was available. Reassigning these 85 cases to the most experienced judge in that court would decrease legal fees by \$3.2 billion. Alternatively, randomly assigning those 85 cases among all experienced judges in that court (i.e., judges with more than two years of experience) would decrease legal fees by \$836 million (based on the coefficient estimate in Table 4 Panel A column (4)). Importantly, all these counterfactual cost reductions focus on decreases in legal fees alone due to reduced bankruptcy durations, and thus ignore any indirect costs of bankruptcy. Using the coefficient estimates from the recovery rate regression (Table 8 Panel

[32]The costs of judicial inexperience must also be compared to the benefits of judge random assignment—the predominant current model—which include avoiding judicial capture by debtor firms. Although we cannot quantify these benefits, we nonetheless present estimates of the costs of judicial inexperience for consideration.

[33]Estimates are based on the coefficient estimate in Table 4 Panel A column (3), the increase in judicial experience according to the set of available judges at the time the case was filed, and an assumption of legal fees representing 2% of assets (LoPucki and Doherty (2004); Bris et al. (2006)).

C column (2)), we estimate that randomly redistributing these 85 cases among judges with more than two years of experience would increase creditor recoveries by $10.1 billion.

An important caveat to the above costs is that any policy must consider how new judges will obtain the experience necessary to efficiently manage complex corporate restructurings. As discussed in section 5.1, by the time most judges are assigned a large complex case they have already been judges for several years. Untabulated results suggest that specific experienced with large complex public cases is insignificantly associated with case durations, presumably because most judges seeing their first public firm have relevant experience with small- and medium-sized corporate restructurings. These findings are consistent with the largest costs of inexperience accruing during judges' first years on the bench, and that delaying the assignment of the largest and most complex cases until a judge has experience with slightly less complex cases (the model already used in the Eastern District of Wisconsin) can result in significant increases in efficiency and decreases in the financial costs borne by bankrupt firms and their creditors.

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Figure 1: Judges' Learning Curve: Duration (LexisNexis Sample)

This figure depicts the coefficient estimates (circles) and 90% confidence intervals from a regression allowing *Duration* to vary by the number of years the judge has been on the bench. Specifically, we run the regression below and plot the β coefficient estimates:

$$\begin{aligned} \text{Duration}_{i,j,t} &= \alpha + \sum_{k=1}^{4} \beta_k \text{Year}^k_{i,j,t} + \beta_5 \text{Year5-6}_{i,j,t} + \beta_6 \text{Year7-8}_{i,j,t} + \beta_7 \text{Year9-10}_{i,j,t} + \\ &+ \; + \delta \text{Judge FE} + \theta \text{Cour-Year FE} + \epsilon_{i,j,t} \end{aligned}$$

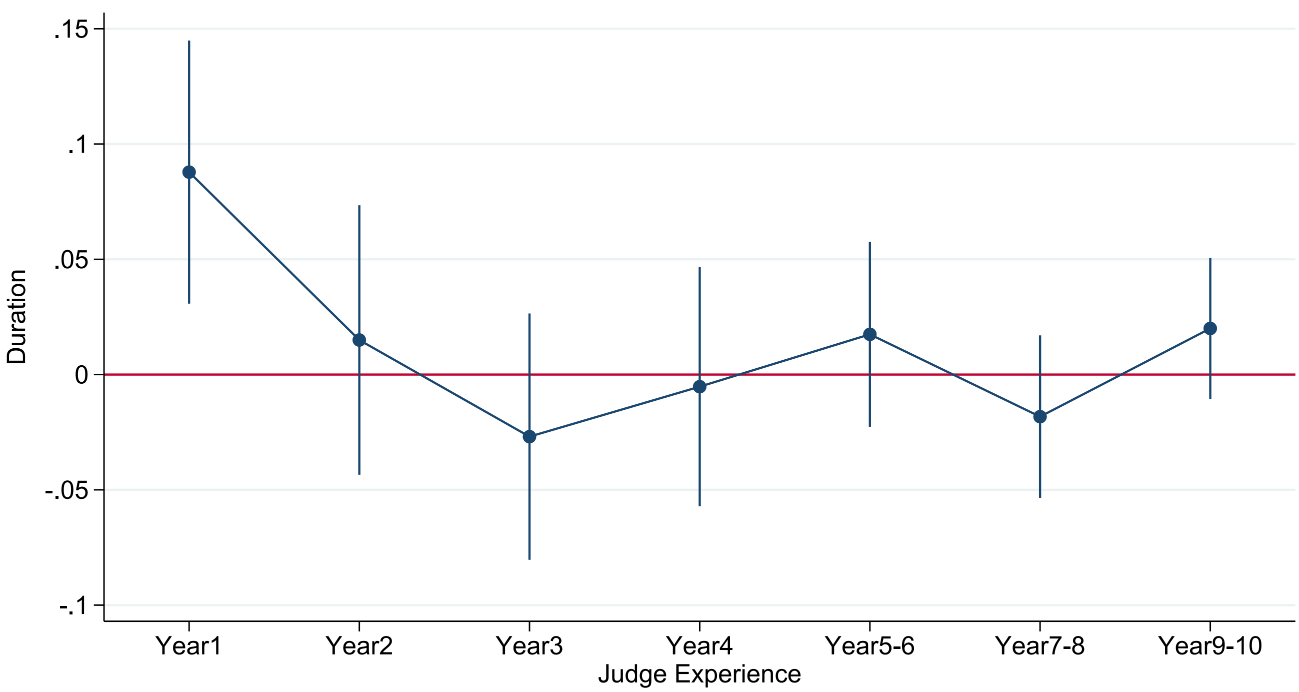

Figure 2: Judges' Learning Curve: Duration (Public Firm Sample)

This figure depicts the coefficient estimates (circles) and 90% confidence intervals from a regression allowing *Duration* to vary by the number of years the judge has been on the bench. Specifically, we run the regression below and plot the β coefficient estimates:

$$\begin{aligned} \text{Duration}_{i,j,t} &= \alpha + \beta_1 \text{Year1-2}_{i,j,t} + \beta_2 \text{Year3-4}_{i,j,t} + \beta_3 \text{Year5-6}_{i,j,t} + \beta_4 \text{Year7-8}_{i,j,t} + \beta_5 \text{Year9-10}_{i,j,t} + \\ &+ \gamma \text{Controls} + \delta \text{Industry FE} + \theta \text{Court-Period FE} + \rho \text{Judge Duration FE} + \epsilon_{i,j,t} \end{aligned}$$

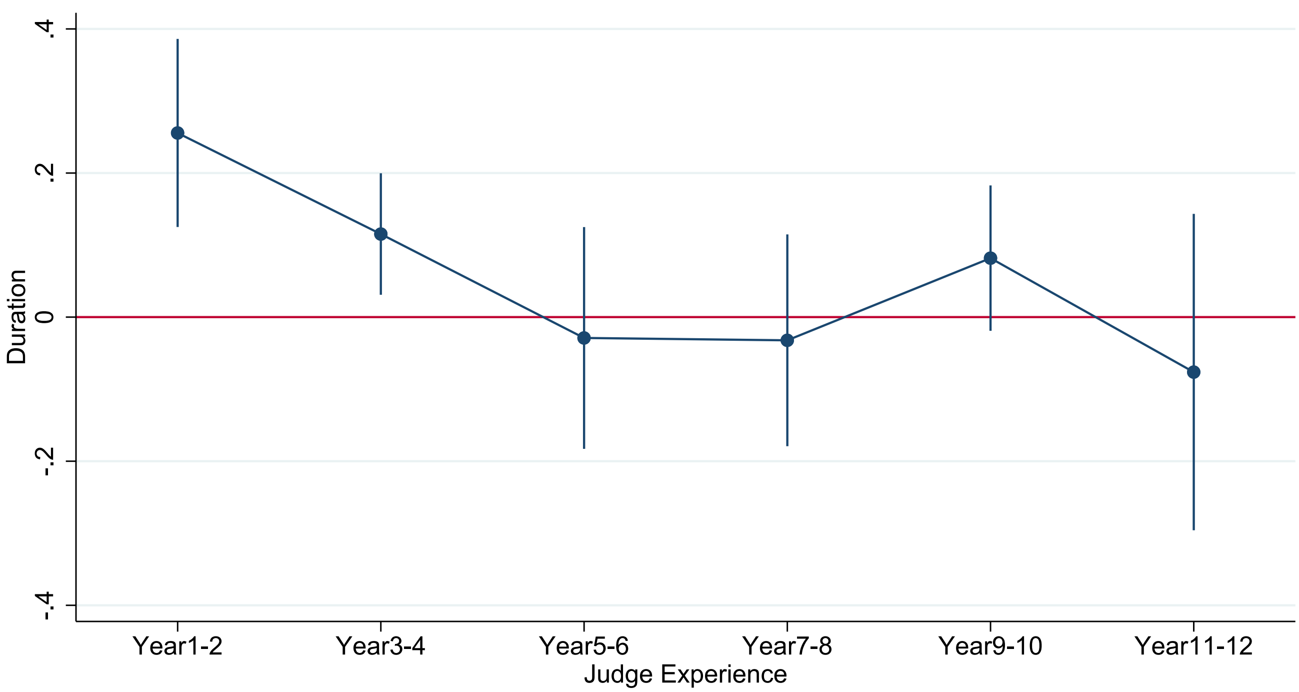

Table 1: Summary Statistics

Panel A presents summary statistics for the LexisNexis sample, including judicial experience at case assignment and case duration. Panel B presents summary statistics for the public firm sample, including judicial experience measures at case assignment, case characteristics, and final outcomes. Panel C presents summary statistics for judge personal attributes and Panel D tabulates the correlation matrix across various judge personal attributes.

Panel A: LexisNexis Sample

	N	Mean	Median	SD	P10	P90
Judge Experience						
Log(Months as Judge)	105,099	4.50	4.76	1.09	3.12	5.56
Months as Judge	105,099	130.83	115.27	93.29	21.67	258.50
First 2 Years	105,099	0.11	0.00	0.31	0.00	1.00
Case Outcomes						
Log(Month in Ch11)	105,099	2.28	2.40	1.15	0.74	3.65
Months in Ch11	105,099	16.81	11.05	18.45	2.11	38.32
Case Characteristics						
Log(Num Filings)	105,099	0.07	0.00	0.33	0.00	0.00
Delaware	105,099	0.02	0.00	0.14	0.00	0.00
NYSD	105,099	0.05	0.00	0.22	0.00	0.00

Panel B: Public Firm Sample

	N	Mean	Median	SD	P10	P90
Judge Experience						
First 2 Years	1,282	0.13	0.00	0.34	0.00	1.00
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
Case Outcomes						
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
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
Log(Num of Motion)	532	4.49	4.53	1.10	3.18	5.87
Num of Motion	532	149.64	93.00	162.13	24.00	353.00
Ave Days(Ruling)	532	33.33	29.69	24.09	16.05	53.16
Num Plans	505	2.53	2.00	1.34	1.00	4.00
High Plans	505	0.21	0.00	0.41	0.00	1.00
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
ROA Post (%)	393	7.41	-2.11	70.53	-39.28	54.98
Case Characteristics						
Log(Assets)	1,304	6.41	6.20	1.39	4.79	8.29
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
Leverage Filing	1,274	1.01	0.92	0.51	0.55	1.50
ROA Filing (%)	1,235	-24.02	-11.21	40.45	-61.38	1.69
Bus/Total Filings	1,303	0.12	0.10	0.09	0.03	0.27
Diversity-Industry	1,304	0.96	0.96	0.00	0.95	0.96
Diversity-Size	1,304	0.64	0.64	0.03	0.60	0.67
Past Total Filings	1,303	24.56	16.01	23.08	3.23	58.51
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 C: Judge Personal Attributes (for judges in the public firm sample)

	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
Democrat	204	0.64	1.00	0.48	0.00	1.00

Panel D: Judge Personal Attributes 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: Court Random Assignment

This table summarizes judge assignment procedures for 81 courts who either responded to our inquiries or stated case assignment policies on their website. Courts marked “Single Judge” have only one judge for the entire district. Courts marked “By location” have multiple judges, but each judge is given cases from only a specific geographic area within the district. The Eastern District of Wisconsin is the only court that gives consideration to a judge’s experience, stating that new judges are not assigned Ch. 11 cases for “a few months.” Accordingly, cases from this court are removed from the sample.

Court	Assignment Method	No. Lexis Nexis Cases	No. Large Cases	Source	Court	Assignment Method	No. Lexis Nexis Cases	No. Large Cases	Source
AK	Single Judge	201	1	-	NC, E	Random	864	5	Local rules
AL, M	Random	223	3	Phone call to court	NC, W	Random	684	0	Phone call to court
AL, N	Random	979	2	Email from Clerk	ND	Single Judge	51	0	-
AL, S	Random	342	1	Email from Clerk	NE	Random	368	1	Email from Clerk
AR, E	Random	350	2	Local rules	NH	Single Judge	443	2	-
AR, W	By location	260	0	Local rules	NJ	Random	4996	37	Local rules
AZ	Random	3587	15	Local rules	NM	Random	531	0	Email from Clerk
CA, C	Random	7499	62	Phone call to court	NV	Random	2102	17	Email from Clerk
CA, E	Random	1555	2	Email from Clerk	NY, E	Random	3500	6	Local rules
CA, N	Random	2816	39	Email from Clerk	NY, N	By location	700	2	Email from Clerk
CA, S	Random	1062	7	Email from Clerk	NY, S	Random	5352	237	Local rules
CO	Random	1358	15	Local rules	NY, W	Random	853	3	Local rules
CT	Random	1381	5	Local rules	OH, N	Random	973	15	Local rules
DC	Single Judge	593	2	-	OH, S	Random	961	15	Email from Clerk
DE	Random	2160	383	Judge Shannon	OK, E	Single Judge	108	0	-
FL, M	Random	4353	21	News article	OK, N	Random	159	0	Phone call to court
FL, N	Single Judge	332	0	Email from Clerk	OK, W	Random	443	6	Local rules
FL, S	Random	3371	32	Local rules	OR	Random	534	4	Email from Clerk
GA, M	Random	473	1	Email from Clerk	PA, E	Random	2307	1	Local rules
GA, S	By location	477	4	Email from Clerk	PA, M	Random	864	2	Local rules
HI	Single Judge	327	2	-	PA, W	Random	1637	5	Local rules
IA, N	Single Judge	127	0	-	RI	Single Judge	294	1	-
IA, S	Random	186	0	Email from Clerk	SC	By location	804	4	Local rules
ID	Random	450	1	Local rules	SD	Single Judge	91	1	-
IL, C	By location	334	0	Email from Clerk	TN, M	Random	1048	6	Email from Clerk
IL, N	Random	1828	40	Local rules	TN, W	Random	717	2	Email from Clerk
IN, S	Random	1136	8	Email from Clerk	TX, E	Random	788	3	Email from Clerk
KS	By location	556	3	Local rules	TX, N	Random	3356	57	Phone call to court
KY, E	Random	324	3	Local rules	TX, S	Random	3581	46	Phone call to court
LA, M	Single Judge	257	1	-	TX, W	Random	2012	19	Email from Clerk
LA, W	By location	585	5	Email from Clerk	UT	Random	855	4	Email from Clerk
MA	Random	2816	22	Email from Clerk	VA, E	Random	2141	15	Email from Clerk
MD	Random	2417	13	Local rules	VA, W	Random	416	2	Email from Clerk
MI, E	Random	2255	16	Local rules	VT	Single Judge	123	1	-
MI, W	Random	691	4	Local rules	WA, E	By location	402	2	Local rules
MN	Random	914	4	Local rules	WA, W	Random	2302	7	Local rules
MO, E	Random	460	13	Email from Clerk	WI, E	Non-random	345	6	Email from Clerk
MO, W	Random	654	5	Local rules	WV, N	Single Judge	223	1	-
MS, N	By location	264	0	Local rules	WV, S	Single Judge	388	1	-
MS, S	By location	469	3	Email from Clerk	WY	Single Judge	156	0	-
MT	Single Judge	204	1	-					

Table 3: Randomization

Panel A presents linear probability model estimates of judge case assignment using the set of eligible judges when a case was filed (see Section 4 for detailed discussion of the sample). The dependent variable, $Assigned_{i,j}$, is an indicator equal to one if judge i was assigned to case j, and zero otherwise. We regress this assignment indicator on two separate measures of judicial experience/activity: the log number of months the judge has been on the bench ($Log(Months)$), and an indicator for the first two years of a judge's tenure (*First 2Y*). Columns (1) and (2) presents results for the LexisNexis sample, while Columns (3) and (4) present results for the public firm sample. Panel B presents regression estimates of the assigned judge's experience on characteristics of the filing firm as of the Chapter 11 filing date. Standard errors are clustered at the court level. We include t-stats in parentheses and *, **, *** indicate 10%, 5%, and 1% statistical significance, respectively.

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

Panel A: Randomization Test

	Lexis Nexis Sample		Public Firm Sample	
	(1)	(2)	(3)	(4)
	Log(Months)	First 2Y	Log(Months)	First 2Y
Experience Measure	-0.002	0.003	0.002	-0.006
	(-1.54)	(0.92)	(0.43)	(-0.54)
Observations	614,545	614,545	5,646	5,646
Within R-Squared	0.0001	0.0000	0.0000	0.0000
Case FE	Yes	Yes	Yes	Yes

$$\text{Experience}_{i,j,t} \quad = \quad \alpha + \beta_1 \text{Firm Characteristics}_j + \text{FEs} + \epsilon_{i,j,t}$$

Panel B: Case Characteristics

	Lexis Nexis Sample		Public Firm Sample	
	(1)	(2)	(3)	(4)
	Log(Months)	First 2Y	Log(Months)	First 2Y
Log(Num Filings)	-0.012	-0.000	-0.006	-0.001
	(-1.13)	(-0.13)	(-0.30)	(-0.16)
Log(Assets)			0.024	0.001
			(1.11)	(0.21)
Leverage Filing			0.110	-0.012
			(0.98)	(-0.37)
ROA Filing (%)			0.002	-0.000
			(1.18)	(-1.05)
Adjusted R^2	0.24	0.23	0.06	0.02
Observations	105,100	105,100	1,148	1,148
Court-Year FE	Yes	Yes		
Court-Period FE			Yes	Yes
Industry FE			Yes	Yes

Table 4: Bankruptcy Duration

This table presents regression estimates for the log number of months a case spends under Chapter 11 (*Duration*). The main explanatory variable is one of two measures of judicial experience as of the case filing date: the log number of months the judge has been on the bench (*Log*(*Months*)) and an indicator for the first two years of a judge's tenure (*First 2Y*). In Panel A we include court-year and judge fixed effects in columns (1) and (2) (LexisNexis sample), and in columns (3) and (4) (public firm sample), we include court-period fixed effects, industry fixed effects, and estimates of each judge's fixed effect on case duration (*Judge Duration FE*, estimated from the LexisNexis sample), and controls for *Log*(*Assets*), *Log*(*Num Filings*), *Leverage filing*, and *ROA filing*. In Panel B we analyze the public firm sample and include additional fixed judge characteristics. Standard errors are clustered at the court level, t-stats are in parentheses, and *, **, *** indicate 10%, 5%, and 1% statistical significance, respectively.

$$\text{Duration}_{i,j,t} \quad = \quad \alpha + \beta_1 \text{JudgeExp}_{i,j,t} + \text{FEs} + \epsilon_{i,j,t}$$

Panel A: Duration with Judge Fixed Effects

	Lexis Nexis Sample		Public Firm Sample	
	(1)	(2)	(3)	(4)
	Log(Months)	First 2Y	Log(Months)	First 2Y
Experience Measure	-0.059***	0.057**	-0.076***	0.237***
	(-5.47)	(2.51)	(-4.72)	(3.40)
Adjusted R^2	0.11	0.11	0.15	0.15
Observations	105,100	105,100	1,088	1,088
Court-Year FE	Yes	Yes		
Judge FE	Yes	Yes		
Judge Duration FE			Yes	Yes
Court-period FE			Yes	Yes
Industry FE			Yes	Yes
Case Controls			Yes	Yes

Panel B: Judge Personal Attributes (Public Firm Sample)

	(1)	(2)
	Log(Months)	First 2Y
Experience Measure	-0.069***	0.206***
	(-5.12)	(3.37)
Log(Years before Bench)	-0.065	-0.041
	(-1.10)	(-0.76)
Top5 Lawschool	0.088	0.102
	(1.16)	(1.32)
Male	-0.129**	-0.134**
	(-2.50)	(-2.59)
Military	0.105	0.090
	(1.54)	(1.31)
Democrats	0.086	0.082
	(1.52)	(1.35)
Adjusted R^2	0.17	0.17
Observations	1,100	1,100
Court-period FE	Yes	Yes
Industry FE	Yes	Yes
Case Controls	Yes	Yes

Table 5: Mechanism: Motion Filing

This table presents regression estimates for the effect of judicial experience on the log number of motions filed for a large public case and the average days from motion filing (excluding filing date motions) to the passing of a corresponding order ($AveDays(Ruling)$). We measure judicial experience using $Log(Months)$ and $First\,2Y$. Court-period, industry, and judge duration fixed effects are included in each regression, and additional case controls include $Log(Assets)$, $Log(Num\ Filings)$, $Leverage\ filing$, and $ROA\ filing$. Standard errors are clustered at the court level. We include t-stats in parentheses and *, **, *** indicate 10%, 5%, and 1% statistical significance, respectively.

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

	Number of Motions		Ave Days(Ruling)	
	(1)	(2)	(3)	(4)
	Log(Months)	First 2Y	Log(Months)	First 2Y
Experience Measure	-0.005	0.084	-1.502**	5.561***
	(-0.07)	(0.38)	(-2.09)	(3.09)
Adjusted R^2	0.35	0.35	0.06	0.06
Observations	462	462	462	462
Court-period FE	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes
Judge Duration FE	Yes	Yes	Yes	Yes
Case Controls	Yes	Yes	Yes	Yes

Table 6: Bankruptcy Duration: Splits by Judge Caseload

This table presents regression estimates for the effects of judicial experience on *Duration* and $AveDays(Ruling)$ after splitting the public firm sample by bankruptcy court caseloads. *High* includes cases with court caseloads above the median at the filing date, and *Low* includes cases with court caseloads below the median at the filing date. Court-period, industry, and judge duration fixed effects fixed effects are included in each regression, and additional case controls include $Log(Assets)$, $Log(Num\ Filings)$, $Leverage\ filing$, and $ROA\ filing$. Standard errors are clustered at the court level. We include t-stats in parentheses and *, **, *** indicate 10%, 5%, and 1% statistical significance, respectively.

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

Panel A: Duration

	Log(Months)		First 2Y	
	(1)	(2)	(3)	(4)
	High	Low	High	Low
Experience Measure	-0.051*	-0.021	0.215***	0.101
	(-1.70)	(-0.75)	(2.82)	(0.75)
Adjusted R^2	0.36	0.45	0.36	0.45
Observations	540	514	540	514
Industry FE	Yes	Yes	Yes	Yes
Court-Period FE	Yes	Yes	Yes	Yes
Judge Duration FE	Yes	Yes	Yes	Yes
Case Controls	Yes	Yes	Yes	Yes

Panel B: Ave Days(Ruling)

	Log(Months)		First 2Y	
	(1)	(2)	(3)	(4)
	High	Low	High	Low
Experience Measure	-2.305***	-0.290	7.202***	5.173***
	(-3.04)	(-0.42)	(4.86)	(3.00)
Adjusted R^2	0.07	0.10	0.07	0.11
Observations	233	219	233	219
Industry FE	Yes	Yes	Yes	Yes
Court-Period FE	Yes	Yes	Yes	Yes
Judge Duration FE	Yes	Yes	Yes	Yes
Case Controls	Yes	Yes	Yes	Yes

Table 7: Learning Accelerators

This table presents regression estimates for the effects of different types of judicial experience on *Duration*. We measure judicial experience type using the mix of cases previously seen by each judge based on historical court filings. We also measure judicial experience type using census data to calculate the diversity of local businesses at each court. Panel A includes all public firm cases assigned to judges during their first four years on the bench, and Panel B includes all public firm cases assigned to judges during their first six years on the bench. All explanatory variables are standardized. Case controls, filing year fixed effects, and judge duration fixed effects are included in each regression. Standard errors are clustered at the court level. We include t-stats in parentheses and *, **, *** indicate 10%, 5%, and 1% statistical significance, respectively.

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

Panel A: First Four Years

	(1)	(2)	(3)
Past Total Filings	0.01	0.04	0.05
	(0.26)	(0.94)	(1.43)
Bus Filings/Total Filings	-0.12**	-0.10**	-0.11***
	(-2.68)	(-2.24)	(-2.82)
Diversity-Industry		-0.10*	
		(-1.70)	
Diversity-Size			-0.16***
			(-3.49)
Observations	306	306	306
Adj R-Squared	0.41	0.41	0.43
Filing Year FE	Yes	Yes	Yes
Judge Duration FE	Yes	Yes	Yes
Case Controls	Yes	Yes	Yes

Panel B: First Six Years

	(1)	(2)	(3)
Past Total Filings	-0.03	-0.01	-0.00
	(-1.02)	(-0.24)	(-0.06)
Bus Filings/Total Filings	-0.11***	-0.09**	-0.10***
	(-2.82)	(-2.30)	(-2.86)
Diversity-Industry		-0.09**	
		(-2.30)	
Diversity-Size			-0.11***
			(-2.81)
Observations	439	439	439
Adj R-Squared	0.45	0.46	0.46
Filing Year FE	Yes	Yes	Yes
Judge Duration FE	Yes	Yes	Yes
Case Controls	Yes	Yes	Yes

Table 8: Additional Case Outcomes

This table presents linear probability model estimates in Panel A of the likelihood that a firm emerges from Chapter 11 (*Emergence*), and in Panel B of the likelihood that a firm that emerged from Chapter 11 refiles for bankruptcy within 3 years ($Refile\,3Y$). In Panel C we present OLS estimates for the average recovery rate across all debt instruments listed at plan confirmation ($Total\,Recovery$), in Panel D for changes in the debt market value from default to plan confirmation ($\Delta Debt\,MV$), and in Panel E for post-bankruptcy return on assets ($ROA\,Post$). The main explanatory variable of interest is one of two measures of judicial experience ($Log(Months)$ or $First\,2Y$). Court-period, industry and judge duration fixed effects are included in each regression, and additional case controls include $Log(Assets)$, $Log(Num\ Filings)$, $Leverage\ filing$, and $ROA\ filing$. Standard errors are clustered at the court level. We include t-stats in parentheses and *, **, *** indicate 10%, 5%, and 1% statistical significance, respectively.

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

Panel A: Emergence

	(1) Log(Months)	(2) First 2Y
Experience Measure	0.030***	-0.075*
	(3.27)	(-1.77)
Adjusted R^2	0.16	0.16
Observations	1,088	1,088
Industry FE	Yes	Yes
Court-Period FE	Yes	Yes
Judge Duration FE	Yes	Yes
Case Controls	Yes	Yes

Panel B: Refile 3Y

	(1) Log(Months)	(2) First 2Y
Experience Measure	-0.002	0.036
	(-0.17)	(1.00)
Adjusted R^2	0.01	0.01
Observations	572	572
Industry FE	Yes	Yes
Court-Period FE	Yes	Yes
Judge Duration FE	Yes	Yes
Case Controls	Yes	Yes

Panel C: Total Recovery (%)

	(1) Log(Months)	(2) First 2Y
Experience Measure	1.785*	-5.728*
	(1.84)	(-1.73)
Adjusted R^2	0.04	0.04
Observations	396	396
Industry FE	Yes	Yes
Court-Period FE	Yes	Yes
Judge Duration FE	Yes	Yes
Case Controls	Yes	Yes

Panel D: Δ Debt MV (%)

	(1) Log(Months)	(2) First 2Y
Experience Measure	3.906	-21.164***
	(0.89)	(-3.68)
Adjusted R^2	0.09	0.10
Observations	266	266
Industry FE	Yes	Yes
Court-Period FE	Yes	Yes
Judge Duration FE	Yes	Yes
Case Controls	Yes	Yes

Panel E: Post Bankruptcy Return on Assets (%)

	(1) Log(Months)	(2) First 2Y
Experience Measure	5.118***	-20.243***
	(2.90)	(-5.15)
Adjusted R^2	0.08	0.09
Observations	290	290
Industry FE	Yes	Yes
Court-Period FE	Yes	Yes
Judge Duration 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 of a judge's term
Case Outcomes	
Ave Days(Ruling)	The average days from motion filing (excluding filing date motions) to the passing of a corresponding order
Emergence	A dummy variable=1 for firms emerged from Chapter 11
Log(Months in Ch11)	Log(number of months a case spent in Chapter 11)
Log(Num Motion)	Log(Number of motions filed with a case)
Num Plans	Number of plans of reorganization filed during bankruptcy
High Plans	A dummy variable=1 if more than 3 plans of reorganization are filed during bankruptcy
Refile 3Y	A dummy variable=1 if a firm refiles for Chapter 11 within 3 years after emergence
Total Recovery(%)	The average recovery rate across all debt instruments listed in the reorganization or liquidation plan that is confirmed by the judge
Δ Debt MV (%)	Change in the market value of debt from default to plan confirmation
ROA Post	Income before extraordinary items (Compustat variable IB) scaled by total assets (Compustat variable AT) measured during the first year for which financial statements are available post bankruptcy.
Case Characteristics	
Log(Assets)	Log of assets dollar value at filing (in 2016 dollars)
Log(Num Filings)	Log(Number of subsidiaries associated with a case at filing)
Leverage Filing	$\frac{liabilities}{Assets}$ at filing
ROA Filing (%)	$\frac{NetIncome}{Assets}$ at filing
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
Delaware	A dummy variable=1 for cases filed in Delaware court
NY SD	A dummy variable=1 for cases filed in the Southern District of New York
Judge Characteristics	
Judge Duration FE	The extracted judge fixed effect coefficient estimates for the outcome *Log(Months in Ch 11)*. Estimated using the LexisNexis sample and all non-public cases.
Log(Years before Bench)	Log(number of years after law school and before appointed as a bankruptcy judge)
Top 5 Law School	A dummy variable=1 if a law school is ranked in the top 5 by 2009 U.S. News
Male	A dummy variable=1 for male judges
Military	A dummy variable=1 for judges with military service before bankruptcy judgeship
Democrat	A dummy variable=1 for a judge affiliated with the Democratic party

Figure A1: Judges' Learning Curve: Duration (LexisNexis Sample)

This figure depicts the coefficient estimates (circles) and 90% confidence intervals from a regression allowing *Duration* to vary by the number of cases the judge has seen on the bench. Specifically, we run the regression below and plot the β coefficient estimates:

$$\text{Duration}_{i,j,t} = \alpha + \sum_{k=10}^{100} \beta_k \text{Case Count}^k_{i,j,t} + \delta \text{Judge FE} + \theta \text{Cour-Year FE} + \epsilon_{i,j,t}$$

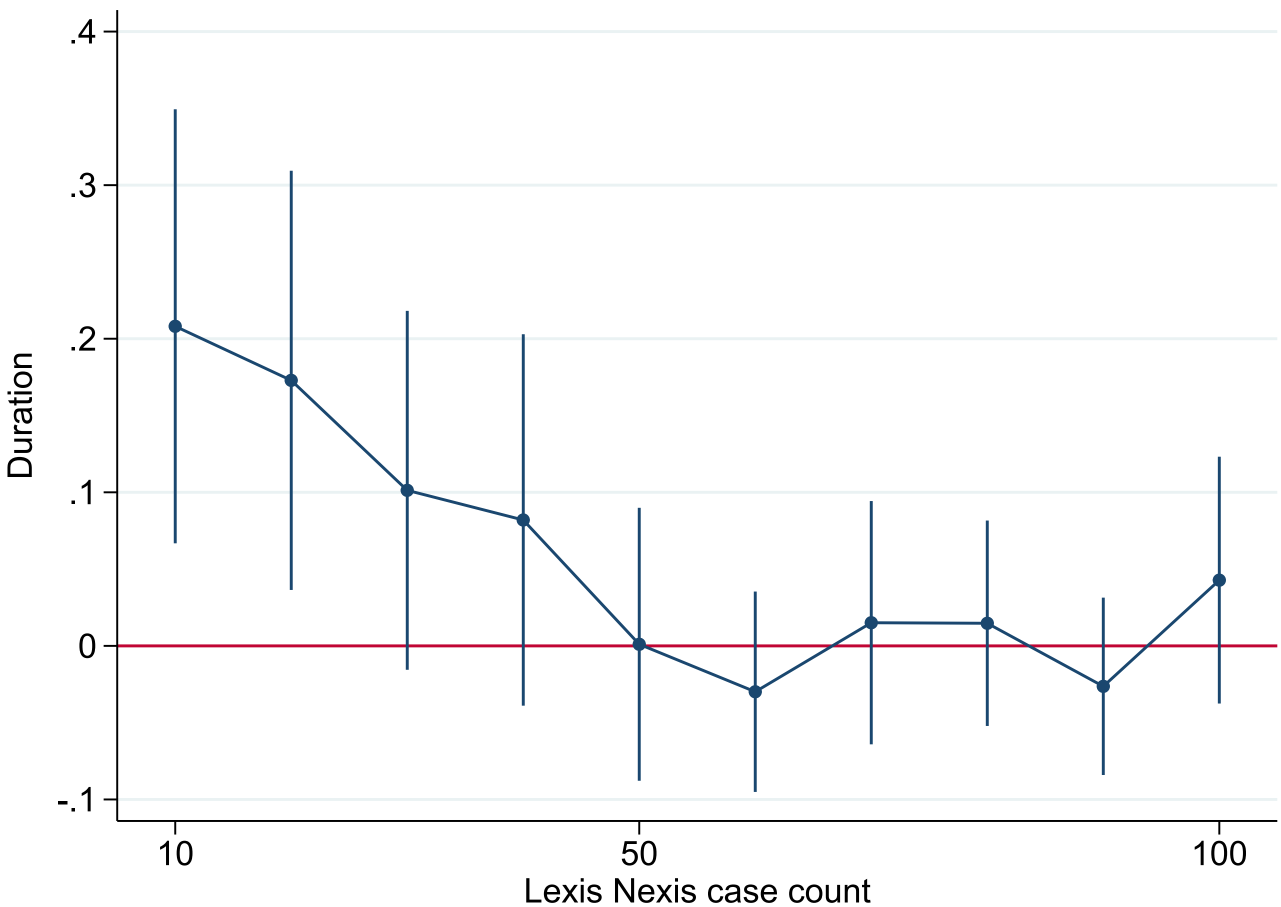

Figure A2: Judges' Learning Curve: Duration (Public Firm Sample)

This figure depicts the coefficient estimates (circles) and 90% confidence intervals from a regression allowing *Duration* to vary by the number of cases the judge has seen on the bench. Specifically, we run the regression below and plot the β coefficient estimates:

$$\begin{aligned}\text{Duration}_{i,j,t} &= \alpha + \sum_{k=100}^{500} \beta_k \text{Case Count}^k_{i,j,t} + \gamma \text{Controls} \\ &+ \delta \text{Industry FE} + \theta \text{Court-Period FE} + \rho \text{Judge Duration FE} + \epsilon_{i,j,t}\end{aligned}$$

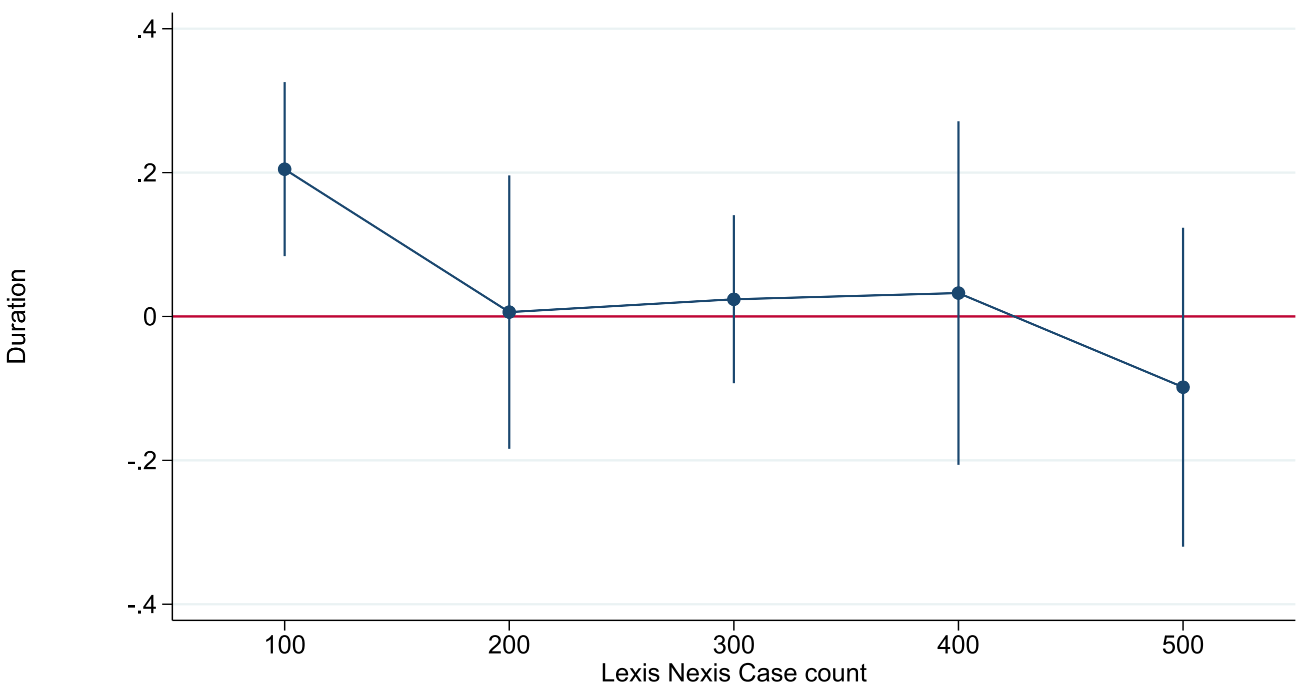

Table A1: Public Firm Sample: Results with Control Variables

This table presents regression estimates of judicial experience measures on public firm case outcomes, with coefficient estimates of control variables tabulated. The outcome variables include *Duration* in columns (1)–(2), $LogNumberofMotions$ in columns (3)–(4), and $AveDays(Rulings)$ in columns (5)–(6). We measure judicial experience using $Log(Months)$ and $First\,2Y$. Court-period, industry and judge duration fixed effects are included in each regression and additional case controls include $Log(Assets)$, $Log(Num\ Filings)$, $Leverage\ filing$, and $ROA\ filing$. Standard errors are clustered at the court level. We include t-stats in parentheses and *, **, *** indicate 10%, 5%, and 1% statistical significance, respectively.

$$\text{Outcomes}_{i,j,t} \quad = \quad \alpha + \beta_1 \text{JudgeExp}_{i,j,t} + \text{FEs} + \epsilon_{i,j,t}$$

	Duration		Number of Motion		Ave Days(Ruling)	
	(1)	(2)	(3)	(4)	(5)	(6)
	Log(Months)	First 2Y	Log(Months)	First 2Y	Log(Months)	First 2Y
Experience Measure	-0.072***	0.231***	-0.005	0.084	-1.502**	5.561***
	(-4.85)	(3.42)	(-0.07)	(0.38)	(-2.09)	(3.09)
Log(Assets)	0.075***	0.073***	0.313***	0.312***	0.714*	0.698**
	(3.89)	(3.76)	(10.32)	(10.24)	(2.04)	(2.17)
Log(Num Filings)	0.070***	0.070***	0.186***	0.187***	0.700***	0.652***
	(4.48)	(4.39)	(3.08)	(2.99)	(3.18)	(2.83)
Leverage Filing	-0.244***	-0.250***	-0.083	-0.081	-3.839*	-3.906*
	(-3.70)	(-3.82)	(-1.08)	(-1.00)	(-1.88)	(-1.94)
ROA Filing (%)	-0.001	-0.001	-0.002***	-0.002***	-0.041	-0.042
	(-1.16)	(-1.17)	(-3.28)	(-3.09)	(-1.02)	(-1.03)
Adjusted R^2	0.16	0.16	0.35	0.35	0.06	0.06
Observations	1,105	1,105	462	462	462	462
Industry FE	Yes	Yes	Yes	Yes	Yes	Yes
Court-period FE	Yes	Yes	Yes	Yes	Yes	Yes
Judge Duration FE	Yes	Yes	Yes	Yes	Yes	Yes
Case Controls	Yes	Yes	Yes	Yes	Yes	Yes

Table A2: 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*). We measure judicial experience using indicators for the number of years the judge has been at the court *(Years 1-2*, *Years 3-4*, and *Years 5-6*), and include interactions of these indicators with *Log(Years before Bench)* in column (1) and an indicator for above-median experience before being appointed to the bench in column (2). Court-period and industry fixed effects are included in each regression and additional case controls include *Log(Assets)*, *Log(Num Filings)*, *Leverage filing*, and *ROA filing*. Standard errors are clustered at the court level. We include t-stats in parentheses and *, **, *** indicate 10%, 5%, and 1% statistical significance, respectively.

	(1) Log(Months in Ch11)	(2) Log(Months in Ch11)
Year1-2	1.319***	0.627***
	(2.82)	(4.39)
Year3-4	0.808**	0.463***
	(2.62)	(4.06)
Year5-6	1.005*	0.380
	(1.70)	(1.10)
Year1-2*Log(Years before Bench)	-0.361**	
	(-2.21)	
Year3-4*Log(Years before Bench)	-0.212**	
	(-2.05)	
Year5-6*Log(Years before Bench)	-0.331*	
	(-1.68)	
Log(Years before Bench)	0.037	
	(0.64)	
Year1-2*Long experience before Bench		-0.264**
		(-2.52)
Year3-4*Long experience before Bench		-0.192**
		(-2.19)
Year5-6*Long experience before Bench		-0.232
		(-1.01)
Long experience before Bench		0.041
		(0.95)
Adjusted R^2	0.39	0.39
Observations	1,142	1,142
Court-Period FE	Yes	Yes
Industry FE	Yes	Yes
Case Controls	Yes	Yes

Table A3: Robustness Check: Removing the Largest Cases

This table presents regression estimates for the effect of judicial experience on case duration after removing cases filed with more than one subsidiaries in the LexisNexis Sample or removing the largest 20% of the cases based on asset values for the public firm sample. We measure judicial experience using $Log(Months)$ and $First\,2Y$. For the public firm sample, court-period and industry fixed effects are included in each regression and additional case controls include $Log(Assets)$, $Log(Num\ Filings)$, $Leverage\ filing$, and $ROA\ filing$. Standard errors are clustered at the court level. We include t-stats in parentheses and *, **, *** indicate 10%, 5%, and 1% statistical significance, respectively.

$$\text{Duration}_{i,j,t} \quad = \quad \alpha + \beta_1 \text{JudgeExp}_{i,j,t} + \text{FEs} + \epsilon_{i,j,t}$$

	Lexis Nexis Sample		Public Firm Sample	
	(1)	(2)	(3)	(4)
	Log(Months)	First 2Y	Log(Months)	First 2Y
Experience Measure	-0.061***	0.061***	-0.089***	0.280***
	(-5.21)	(2.53)	(-4.94)	(3.26)
Adjusted R^2	0.11	0.11	0.13	0.13
Observations	99,178	99,178	863	863
Court-Year FE	Yes	Yes		
Judge FE	Yes	Yes		
Judge Duration FE			Yes	Yes
Court-period FE			Yes	Yes
Industry FE			Yes	Yes
Case Controls			Yes	Yes

Table A4: Robustness Check: Removing NYSD or Delaware

This table presents regression estimates for the effect of judicial experience on case duration after removing cases filed in NYSD or Delaware district. We measure judicial experience using $Log(Months)$ and $First\,2Y$. For the public firm sample, court-period and industry fixed effects are included in each regression and additional case controls include $Log(Assets)$, $Log(Num\ Filings)$, $Leverage\ filing$, and $ROA\ filing$. Standard errors are clustered at the court level. We include t-stats in parentheses and *, **, *** indicate 10%, 5%, and 1% statistical significance, respectively.

$$\text{Duration}_{i,j,t} \quad = \quad \alpha + \beta_1 \text{JudgeExp}_{i,j,t} + \text{FEs} + \epsilon_{i,j,t}$$

Panel A: Removing NYSD

	Lexis Nexis Sample		Public Firm Sample	
	(1)	(2)	(3)	(4)
	Log(Months)	First 2Y	Log(Months)	First 2Y
Experience Measure	-0.062***	0.065***	-0.070***	0.186**
	(-5.64)	(2.76)	(-2.73)	(2.42)
Adjusted R^2	0.11	0.11	0.12	0.12
Observations	99,747	99,747	878	878
Court-Year FE	Yes	Yes		
Judge FE	Yes	Yes		
Judge Duration FE			Yes	Yes
Court-period FE			Yes	Yes
Industry FE			Yes	Yes
Case Controls			Yes	Yes

Panel B:Removing Delaware

	Lexis Nexis Sample		Public Firm Sample	
	(1)	(2)	(3)	(4)
	Log(Months)	First 2Y	Log(Months)	First 2Y
Experience Measure	-0.058***	0.055**	-0.070***	0.290***
	(-5.26)	(2.34)	(-3.15)	(2.97)
Adjusted R^2	102,939	102,939	0.13	0.13
Observations	0.10	0.10	736	736
Court-Year FE	Yes	Yes		
Judge FE	Yes	Yes		
Judge Duration FE			Yes	Yes
Court-period FE			Yes	Yes
Industry FE			Yes	Yes
Case Controls			Yes	Yes

Table A5: Robustness Check: First-term judges

This table presents regression estimates for the effect of judicial experience on case duration including only cases filed with each judge's first term. We measure judicial experience using $Log(Months)$ and $First\,2Y$. For the public firm sample, court-period and industry fixed effects are included in each regression and additional case controls include $Log(Assets)$, $Log(Num\ Filings)$, $Leverage\ filing$, and $ROA\ filing$. Standard errors are clustered at the court level. We include t-stats in parentheses and *, **, *** indicate 10%, 5%, and 1% statistical significance, respectively.

$$\text{Duration}_{i,j,t} \quad = \quad \alpha + \beta_1 \text{JudgeExp}_{i,j,t} + \text{FEs} + \epsilon_{i,j,t}$$

	Lexis Nexis Sample		Public Firm Sample	
	(1)	(2)	(3)	(4)
	Log(Months)	First 2Y	Log(Months)	First 2Y
Experience Measure	-0.078***	0.056**	-0.082***	0.248***
	(-6.28)	(2.11)	(-3.46)	(3.83)
Adjusted R^2	0.11	0.11	0.16	0.16
Observations	73,244	73,244	790	790
Court-Year FE	Yes	Yes		
Judge FE	Yes	Yes		
Judge Duration FE			Yes	Yes
Court-period FE			Yes	Yes
Industry FE			Yes	Yes
Case Controls			Yes	Yes

Table A6: Robustness Check: Single Judge Court or By Location Assignment

This table presents regression estimates of judicial experience measures on case duration, excluding courts that only have one judge or courts that assign cases by location, as listed in Table 2. We measure judicial experience using $Log(Months)$ and $First\,2Y$. For the public firm sample, court-period and industry fixed effects are included in each regression and additional case controls include $Log(Assets)$, $Log(Num\ Filings)$, $Leverage\ filing$, and $ROA\ filing$. Standard errors are clustered at the court level. We include t-stats in parentheses and *, **, *** indicate 10%, 5%, and 1% statistical significance, respectively.

$$\text{Duration}_{i,j,t} \quad = \quad \alpha + \beta_1 \text{JudgeExp}_{i,j,t} + \text{FEs} + \epsilon_{i,j,t}$$

	Lexis Nexis Sample		Public Firm Sample	
	(1)	(2)	(3)	(4)
	Log(Months)	First 2Y	Log(Months)	First 2Y
Experience Measure	-0.059***	0.059***	-0.080***	0.251***
	(-5.43)	(2.54)	(-5.13)	(3.59)
Adjusted R^2	0.11	0.11	0.15	0.15
Observations	99,713	99,713	1,073	1,073
Court-Year FE	Yes	Yes		
Judge FE	Yes	Yes		
Judge Duration FE			Yes	Yes
Court-period FE			Yes	Yes
Industry FE			Yes	Yes
Case Controls			Yes	Yes

Table A7: Robustness Check: Alternative Experience Measure

This table presents regression estimates for the effect of judicial experience on case duration. We measure judicial experience base on the number of LexisNexis cases a judge has seen until the filing date of a case. For the public firm sample, court-period and industry fixed effects are included in each regression and additional case controls include *Log(Assets)*, *Log(Num Filings)*, *Leverage filing*, and *ROA filing*. Standard errors are clustered at the court level. We include t-stats in parentheses and *, **, *** indicate 10%, 5%, and 1% statistical significance, respectively.

$$\text{Duration}_{i,j,t} \quad = \quad \alpha + \beta_1 \text{JudgeExp}_{i,j,t} + \text{FEs} + \epsilon_{i,j,t}$$

	Lexis Nexis Sample		Public Firm Sample	
	(1)	(2)	(3)	(4)
	Log(Case Count)	First 20	Log(Case Count)	First 200
Experience Measure	-0.082***	0.119***	-0.076***	0.191**
	(-2.93)	(3.12)	(-2.81)	(2.42)
Adjusted R^2	0.11	0.11	0.14	0.14
Observations	46,421	46,421	597	597
Court-Year FE	Yes	Yes		
Judge FE	Yes	Yes		
Judge Duration FE			Yes	Yes
Court-period FE			Yes	Yes
Industry FE			Yes	Yes
Case Controls			Yes	Yes

Table A8: Reorganization Plans

This table presents linear probability model estimates of the likelihood the case has more than three plans of reorganization filed during bankruptcy (*High Plans*). Court-period, industry and judge duration fixed effects are included in each regression, and additional case controls include *Log(Assets)*, *Log(Num Filings)*, *Leverage filing*, and *ROA filing*. Standard errors are clustered at the court level. We include t-stats in parentheses and *, **, *** indicate 10%, 5%, and 1% statistical significance, respectively.

$$\text{High Plans}_{i,j,t} \quad = \quad \alpha + \beta_1 \text{JudgeExp}_{i,j,t} + \beta_2 \text{Controls} + \text{FEs}\epsilon_{i,j,t}$$

	(1)	(2)
	Log(Months)	First 2Y
Experience Measure	-0.016*	0.131***
	(-1.81)	(3.17)
Adjusted R^2	0.08	0.09
Observations	451	451
Industry FE	Yes	Yes
Court-Period FE	Yes	Yes
Judge Duration FE	Yes	Yes
Case Controls	Yes	Yes