Financial Costs of Judicial Inexperience: Evidence From Corporate Bankruptcies

Benjamin Iverson [ID]
Brigham Young University Marriott School of Business
biverson@byu.edu (corresponding author)

Joshua Madsen
University of Minnesota Carlson School of Management
jmmadsen@umn.edu

Wei Wang
Queen's University Smith School of Business
wwang@queensu.ca

Qiping Xu
University of Illinois Gies College of Business
qipingxu@illinois.edu

Abstract

Exploiting the random assignment of judges to corporate bankruptcy filings, we estimate financial costs of judicial inexperience. Despite new judges' prior legal experience, formal education, and rigorous hiring process, their public Chapter 11 cases spend 19% more time in bankruptcy, realize 31% higher legal and professional fees, and 21% lower creditor recovery rates. Examining possible mechanisms, we find that new judges take longer to rule on motions and cases assigned to these judges file more plans of reorganization. Conservative estimates suggest that minor policy adjustments could increase creditor recoveries by approximately $16.8 billion for the public firms in our sample.

I.   Introduction

The bankruptcy process has far-reaching effects, from the costs imposed on bankrupt firms to the allocation of capital within the economy. As arguably the most important decision makers within that process, bankruptcy judges need both finan-

cial and judicial expertise to negotiate compromises among key stakeholders and evaluate matters including financing, managerial compensation, professional fees, financial projections, plans of reorganization, and asset sales.[1] Some judges have extensive experience adjudicating these issues, while others do not. Whether and how judicial experience affects bankruptcy proceedings has important implications for the costs of financial distress and policies that seek to improve the legal system. In addition, understanding how judges learn on the job can shed light on how professionals may more quickly acquire the expertise necessary to efficiently manage complex tasks.

Theoretically, the extent to which judicial experience affects the speed and efficiency of the bankruptcy process is ambiguous. All new judges have substantial formal education, extensive legal experience (often in bankruptcy), and face a rigorous hiring process, so it is feasible that they could "hit the ground running" and are just as efficient as more seasoned judges. New judges could also bring fresh perspectives, relevant background experience, and energy, enabling them to manage corporate restructurings more efficiently than entrenched judges. Alternatively, new judges could face a learning curve and be initially less efficient, as addressing complex bankruptcy issues in a timely and efficient manner plausibly requires financial, managerial, and judicial skills that can only be acquired "on the job" (e.g., Arrow (1962), Becker (1962), and Lazear (2009)).

In this paper, we exploit several advantageous features of bankruptcy courts to estimate whether bankruptcy outcomes differ for cases assigned to inexperienced judges. First, and perhaps most importantly, bankruptcy judges are assigned to cases randomly. Second, judges are appointed to 14-year renewable terms (reducing survival bias concerns), have flat compensation structures and nearly always complete their first terms (reducing incentives to signal), and typically end their careers as judges (reducing revolving door and risk-taking concerns). Third, the flow of new cases does not take into account judges' caseloads, potentially exacerbating the effects of inexperience. Fourth, the bankruptcy process generates measurable outcomes that are plausibly affected by judicial experience. These features enable us to measure the speed with which judges gain the *job-specific* human capital necessary to efficiently manage complex corporate restructurings (i.e., navigate a learning curve) while holding constant fixed judge and court characteristics.

We begin our analysis by examining a comprehensive sample of 103,812 private business Chapter 11 filings recorded between 1993 and 2012 which were 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. (2019) provide strong evidence that corporate bankruptcy cases are assigned to judges randomly. We also provide evidence consistent with random assignment. First, we validate

---

systematically that cases are assigned randomly using courts' stated policies and correspondence with court clerks. Second, we empirically document that case characteristics are unrelated to an assigned judge's tenure. Third, we empirically document that the probability that a specific judge is assigned a specific case (out of all eligible judges) is unrelated to that judge's judicial experience.

Our baseline analysis estimates the effect of judicial inexperience 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. Cases assigned to a judge with twice as much time on the bench (e.g., from 2 to 4 years) realize a 6.5% decrease in time spent in bankruptcy, a decline of nearly 1 month relative to the average duration in our sample of 16.5 months.[2] Mapping out judges' learning curves, we find that these effects concentrate in judges' early years: cases assigned during a judge's first year spend 12% more time in court relative to cases assigned to judges with more than 10 years of experience. Our regression specifications include both bankruptcy court-year and judge fixed effects, allowing us to measure the effects of on-the-job experience while holding constant omitted time-varying, court-specific characteristics (such as a judge's cohort and characteristics of other cases filed in the same court-year), and fixed judge characteristics (such as a judge's previous work experience and potential biases).

LexisNexis provides a comprehensive sample of small, private Chapter 11 filings, but contains limited data on case characteristics and no data on creditor recoveries or postbankruptcy performance. We thus also examine a sample of 1,501 Chapter 11 filings by publicly traded firms (the "public-firm sample") to estimate a learning curve for more complex tasks and analyze outcomes not possible in the LexisNexis sample. The lower frequency with which judges see publicly traded firms also provides an opportunity to estimate task-specific learning in a setting that lacks frequent repetition. We find that cases involving public firms that are assigned to judges during the judges' first 2 years of service take almost 20% longer to resolve than cases involving public firms that are assigned to judges with more than 2 years of experience. Only after 4 years is the case duration of new judges comparable to those of the most experienced judges, suggesting that judges must be on the bench for a significant portion of their 14-year appointments before gaining the expertise necessary to efficiently manage complex cases involving these public firms.

Prolonged bankruptcy proceedings impose both additional direct costs (e.g., legal fees) and indirect costs (e.g., the loss of key employees, suppliers, or customers) on bankrupt firms. We estimate these costs for the public-firm sample in two ways. First, we analyze direct costs by collecting legal and professional fees from postbankruptcy 10K filings and court dockets. We find that legal fees are 30% higher for cases assigned to inexperienced judges. Second, we analyze the overall costs of financial distress by examining creditor recovery rates. We find that cases assigned to inexperienced judges realize recoveries that are 11.6 percentage points lower, 20.8% below the mean. Overall, we conclude that judicial

---

[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.

expertise plays an important role in determining the costs of financial distress for bankrupt firms.

The increased time in court, increased fees, and lower recovery rates attributable to judicial inexperience could have compensating benefits if the resulting bankruptcy outcomes are ultimately superior (e.g., more conscientious due diligence practiced by new judges). To evaluate this possibility, we examine postbankruptcy outcomes that are available for the sample of public firms to better gauge the effects of judicial inexperience. We find that cases assigned to experienced judges are equally likely to emerge from bankruptcy, but less likely to refile for bankruptcy within 3 years. Firms that restructure under experienced judges also realize higher revenue growth and returns on assets than similar firms whose cases are assigned to inexperienced judges. The combined evidence is consistent with inexperienced judges overseeing lower-quality restructurings.

To better understand how experienced judges move cases through bankruptcy faster, we examine a sample of public firms for which we have court dockets to examine particular judicial actions. We find that, once a motion has been made, judges in their first 2 years spend, on average, an additional 5 days to issue a ruling, which is 15.2% above the sample mean. In addition, cases assigned to inexperienced judges file on average 0.47 more plans of reorganization, an 18% increase over the sample mean. The results suggest that inexperienced judges require more time to process motions and have a harder time establishing a consensus. Unsurprisingly, both time to ruling on motions and the number of plans filed are positively correlated with bankruptcy duration, suggesting that these two mechanisms help explain our main findings.

To provide a sense of the aggregate costs of inexperience, we estimate several "back-of-the-envelope" calculations for counterfactual scenarios where public firms are either assigned endogenously based on a judge's experience or assigned randomly among all judges with at least 2 years of experience. We estimate that reassigning just 83 public firms that were assigned randomly to judges with 2 or fewer years of experience to a more experienced judge at the same court would reduce direct costs by $1–$2 billion and increase credit recoveries by $16.8 billion.[3] Although there are certainly benefits to random assignment that must be considered (e.g., avoiding judicial capture by debtor firms), our estimates suggest that the costs of judicial inexperience are substantial, and that there are feasible methods available that would reduce these costs.

Finally, we draw on insights from the learning-by-doing literature to study factors that accelerate judges' learning. Cases assigned to judges who have seen more past business cases have a shorter duration, whereas prior experience with nonbusiness cases has no association with case duration. We also find that, among

---

[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 might enable judges to accumulate sufficient human capital to subsequently handle the most complex cases efficiently. Our policy suggestion follows the approach used in Wisconsin, the only court to currently practice any form of nonrandom assignment, where complex cases are not assigned to new judges during their first few months.

judges with 6 or fewer years of judicial experience, those who have seen more diverse business filings, as measured by the diversity of industries and firm sizes located in their district, process public cases more quickly. The evidence suggests that new judges accumulate human capital more quickly when they see relevant filings in higher numbers and a greater diversity of filings.

Our study provides new insights into the costs of bankruptcy (Andrade and Kaplan (1998), Bris, Welch, and Zhu (2006), Almeida and Philippon (2007), Elkamhi, Ericsson, and Parsons (2012), and Dou, Taylor, Wang, and Wang (2021)). We contribute to this literature by identifying a new source of distress costs: judicial expertise. Our paper thus also contributes to the literature that analyzes how judges' discretion, specialization, behavioral mistakes, political ideology, and personal biases affect rulings, case outcomes, litigation risk, and corporate tax planning (Sharfman (2005), Rachlinski, Guthrie, and Wistrich (2006), Posner (2008), Chang and Schoar (2013), Dobbie and Song (2015), Chen, Moskowitz, and Shue (2016), Cohen and Yang (2018), Bernstein et al. (2019), Huang, Hui, and Li (2019), and Chow, Huang, Hui, and Shevlin (2021)). Different from these studies, we show that bankruptcy costs and outcomes are impacted by time-varying judicial characteristics that are distinct from judges' fixed characteristics. LoPucki and Doherty (2015) and Jaggia and Thosar (2019) examine correlations between bankruptcy outcomes and a variety of case characteristics, including judicial experience. Both papers find a correlation between judicial experience and bankruptcy outcomes for large Chapter 11 cases. We build on these papers by providing causal estimates, mapping out the judicial learning curve, and linking judicial expertise directly to the costs of bankruptcy as well as longer-term outcomes for restructuring firms.

We also contribute to research on the importance of financial and legal expertise. Related research studies expertise in the context of company boards, audit committees, analysts, auditors, mutual fund managers, and traders (Mikhail, Walther, and Willis (1997), Clement (1999), Mikhail, Walther, and Willis (2003), Low (2004), DeFond, Hann, and Hu (2005), Seru, Shumway, and Stoffman (2010), Erkens and Bonner (2013), Badolato, Donelson, and Ege (2014), Bradley, Gokkaya, and Liu (2017), Kempf, Manconi, and Spalt (2017), and Chychyla, Leone, and Minutti-Meza (2019)). Prior studies in economics provide a theoretical foundation for understanding the investment in and accumulation of job- and task-specific human capital (Arrow (1962), Becker (1962), Prendergast (1993), and Gibbons and Waldman (2004)). In contrast to many other financial professionals who are assigned more complex tasks as their careers progress and their qualifications increase, judges' task complexity does not vary with experience. As prior research has suggested, we find that performance improves with experience, albeit arguably using a tighter research design to address endogeneity concerns and estimate task-specific, multiyear learning curves. We also find that task variety and complexity accelerate the learning process. These results suggest that other professionals managing entirely new complex tasks for the first time may also face steep and potentially costly learning curves.

II.   Institutional Background

A.   Judicial Appointments

Each bankruptcy court has a fixed number of judgeships set by Congress.[4] Applicants for vacant judgeships are required to be members of the bar in good standing and to have at least 5 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 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 typically contains 5 to 8 members and consists of a mix of sitting judges, law practitioners, and academics. Among the most important qualities evaluated are impartiality and fairness, background in bankruptcy law, organizational skill, decisiveness, and commitment to bankruptcy work (Reddick and Knowlton (2013)). 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 merit panel's recommendations. Bankruptcy judges are appointed to specific courts (although occasionally, with permission, they "visit" other courts), and handle all types of bankruptcy filings within specific divisions of that court. Within our sample, the median court comprises three divisions and the median judge sees cases in two of those divisions. Bankruptcy judges serve renewable 14-year terms. New judges are invited to attend two 1-week orientation programs organized by the Federal Judicial Center and have opportunities to enhance their judicial skills by attending annual workshops and special-focus programs sponsored by the Federal Judicial Center (https://www.fjc.gov/education/programs-and-resources-judges).

B.   Chapter 11 Filings

Firms (especially large firms) have some choice as to where they choose to file for bankruptcy. The U.S. Code Title 28 Chapter 87 §1,408 states that a debtor can file under Chapter 11 in one of the following four locations: i) the debtor's place of domicile or residence, commonly referred to as the place of incorporation; ii) the debtor's principal place of business; iii) the location of the debtor's principal assets; iv) 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 venues.

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." U.S. bankruptcy courts for the District of Delaware and the Southern District of New York have since 1990 emerged as the most popular venues among the 94 bankruptcy courts for forum shoppers (Skeel

---

[4]The Judicial Conference of the United States conducts a study of judgeship needs every other year, and makes recommendations to Congress. Because creating new judgeships requires passage of a bill by Congress, however, new judgeships are rarely created.

(1998), LoPucki (2005)). In our empirical tests, we include division-time fixed effects to control for unobservable firm heterogeneity that is correlated with court choice over time. Although courts differ in collective experience and overall efficiency, random assignment of judges and variation in judicial experience within court divisions imply that firms can be assigned inexperienced judges even within popular venues.

III.   Data and Variable Construction

A.   Chapter 11 Sample

Our analysis begins with a comprehensive sample of private business Chapter 11 filings (obtained from LexisNexis). The sample covers the years 1993–2012. The initial sample contains 133,050 business Chapter 11 bankruptcy filings with judicial experience information. We remove 345 cases filed in Wisconsin (where the court's policy is not to randomize case assignment for new judges – see Section IV), 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, 24 cases where there is a single case assigned to a judge or filed in a court-year, and 816 public firm filings. Our final LexisNexis sample consists of 104,284 cases assigned to 574 unique judges in 89 bankruptcy courts. Because we cannot observe firm characteristics such as size or industry, we proxy for size using log(NUM_FILINGS), the log number of subsidiary bankruptcy filings associated with a given case, which is the only case characteristic available for this sample.

We also separately analyze a sample of Chapter 11 filings by public firms for which more detailed case-level information is available. Specifically, this sample contains all Chapter 11 filings by U.S. public firms with filing dates between 1980 and 2012 and confirmation dates before 2016, retrieved from New Generation Research's bankruptcydata.com.[5] We identify 1,607 such Chapter 11 filings with detailed information on assigned judges, filing courts and divisions, firm characteristics when filings occur, plan confirmation dates, and plan effective dates. We drop 61 cases overseen by a district judge, 39 cases that were transferred to other courts, and 6 cases filed in Wisconsin. Our final public-firm sample comprises 1,501 Chapter 11 filings assigned to 336 unique judges located in 78 bankruptcy courts.

In both the LexisNexis and public-firm samples, the main outcome variable is log(DURATION). For the LexisNexis sample, log(DURATION) is defined as the natural logarithm of the number of months from filing date until a case is i) closed after completing a Chapter 11 restructuring (emergence or liquidation), ii) converted to Chapter 7 (liquidation), or iii) dismissed from court. In the public-firm sample, where more information is available, we define log(DURATION) as the natural logarithm of the number of months from the filing date until i) the date when

---

[5]Specifically, we require these firms to have filed financial statements with the SEC in any of the 3 years before bankruptcy. We end our filing dates in 2012 to avoid potential survival bias in measuring both case resolutions and any subsequent refilings. Upon observing inconsistency between the two databases we resort to Public Access to Court Electronic Records (PACER) for verification.

the reorganization/liquidation plan is confirmed or ii) the date when the plan is converted to Chapter 7 (liquidation), the period of time over which the reorganization is determined and the most important judicial decisions are often made.[6]

Although our analysis focuses on log(DURATION) as a proxy for the overall costs of restructuring as it is measurable for all cases in our sample, to analyze these costs more directly we use two additional measures for the public-firm sample. First, we collect legal and professional fees from postbankruptcy 10K filings (for those firms that remain public) and supplement these data with court-approved legal and professional fees from PACER court dockets, which are typically available for cases filed after 2002. Second, we measure creditors' costs using the family recovery rate (FAMILY_RECOVERY(%)), defined as the enterprise value of a corporate family relative to its total liabilities at default resolution.[7]

We also collect information on other bankruptcy outcomes for the public-firm sample. We identify whether a firm emerged from bankruptcy (EMERGENCE) and whether firms that emerged filed again for bankruptcy within 3 years (REFILE_3Y). We also calculate two measures of postemergence performance for those firms that remain public: the postemergence change in sales relative to sales at filing (Δ_SALES), and postbankruptcy net income scaled by total assets (ROA_POST). These variables provide an indication of efficient restructuring and how judicial experience impacts the subsequent profitability of restructured firms (Hotchkiss (1995), Denis and Rodgers (2007), and Kalay, Singhal, and Tashjian (2007)).[8] We identify the number of reorganization/liquidation plans filed by debtors (available from New Generation Research and supplemented using available electronic court dockets) for those public firms that are not liquidated through conversion to Chapter 7 (under which no plan is filed).

Finally, we gather information regarding motions filed for cases with electronic dockets. Bankruptcy dockets allow us to link all motions filed (e.g., compensation issues, postpetition financing, asset sales and liquidation, creditor valuation disputes, 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 judge's efficiency in resolving complex issues that arise in bankruptcy. We calculate AVG_DAYS_RULING as the average number of days between the motion filing date 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.

---

[6]The LexisNexis data does not provide the date on which a reorganization plan is confirmed or becomes effective. We conduct robustness tests for the public-firm sample using two alternative definitions of the end of a case (the effective date and the resolution date) and find similar results (see Section V.A).

[7]Bankruptcy costs include both legal and administration fees as well as opportunity costs (e.g., loss of customers, suppliers, or employees). These costs are significantly higher in prolonged cases (Dou et al. (2021)). Spillover effects make the real economic costs of bankruptcy even larger (Hertzel, Li, Officer, and Rodgers (2008), Boone and Ivanov (2012), Kolay, Lemmon, and Tashjian (2016), and Bernstein et al. (2019)).

[8]We caution, however, that we cannot measure full economic efficiency because we are unable to observe what happens to the assets of liquidated firms and that sample sizes for these additional outcomes are restricted to cases with available data and thus are subject to selection concerns.

B.   Judicial Experience

We compile bankruptcy judges' career histories using resumes from bankruptcy courts, supplemented with information posted on LinkedIn, LexisNexis personal reports database, press releases, and other online and library resources. Because learning is unobservable, we use judicial experience (time since appointment) as our primary measure of judges' learning under the assumption that judges become more efficient the longer they have worked as judges. 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 a case filing date, and, to capture any nonlinear effects, FIRST_2Y, an indicator that equals 1 for cases that are assigned to judges in their first 2 years on the bench.[9] As a robustness test, we also validate our main results using cumulative filing-based measures of judicial experience, which are available for more recently appointed judges. See the Supplementary Material Appendix for detailed variable definitions.

C.   Summary Statistics

We summarize the characteristics of the 104,284 LexisNexis cases in Panel A of Table 1. The average case spends 16.52 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 average judge assigned to these cases has been on the bench for 130 months (10.8 years, standard deviation of 91 months), and 11% of LexisNexis cases are assigned to judges in their first 2 years on the bench. Of the 262 judges in our sample appointed before 1998, only 4 left the bench before the end of their first terms (for a mixture of reasons) and 2 passed away, consistent with judges generally finishing their first terms.

A much richer set of case characteristics is available for the sample of 1,501 public firms, as summarized in Panel B of Table 1. For this sample, the average judge has been on the bench for 119 months (9.9 years, with a standard deviation of 83.99 months), and 12% of the public firms (181) are assigned to judges who are in their first 2 years on the bench. The average public firm spends 16 months in Chapter 11. For firms with disclosed legal and professional fees, the average fees total $22 million. The median ratio of fees to prefiling assets is 2% and the average ratio is 4%. For firms with recovery information, the average family recovery rate across rated debt instruments is 55.5%.[10] Fifty-three percent of the public firms emerge from bankruptcy, and conditional on emergence, 7% refiled for Chapter 11 within 3 years. For firms that remain public postbankruptcy, the average change in sales is −20% and the average ROA in the first year out of bankruptcy is −2%. For those cases with electronic dockets, the average number of motions is 127 (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 debtors that are

---

[9]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), and DeAngelo and Owens (2017)). We interchangeably use the terms "experience" and "tenure."

[10]Moody's DRD provides detailed information for only debt instruments rated by Moody's, resulting in a smaller number of observations for recovery rate.

TABLE 1

Summary Statistics

Panel A of Table 1 presents summary statistics for the LexisNexis sample and Panel B presents summary statistics for the public-firm sample. Detailed variable definitions are provided in the Supplementary Material Appendix.

	N	Mean	Median	SD	P10	P90
Panel A. LexisNexis Sample						
Judge Experience						
FIRST_2Y	104,284	0.11	0.00	0.31	0.00	1.00
log(MONTHS)	104,284	4.50	4.76	1.09	3.12	5.56
Case Outcomes						
MONTHS_CH11	104,284	16.52	10.95	17.64	2.11	37.73
DURATION	104,284	2.28	2.39	1.13	0.74	3.63
Case Controls						
log(NUM_FILINGS)	104,284	0.06	0.00	0.27	0.00	0.00
DELEWARE	104,284	0.02	0.00	0.13	0.00	0.00
NYSD	104,284	0.05	0.00	0.22	0.00	0.00
Panel B. Public Firm Sample						
Judge Experience						
FIRST_2Y	1,501	0.12	0.00	0.33	0.00	1.00
log(MONTHS)	1,501	4.39	4.65	1.07	2.97	5.47
MONTHS_AS_JUDGE	1,501	119.01	104.67	83.99	19.40	236.80
Case Outcomes						
MONTHS_CH11	1,500	16.06	12.12	14.83	2.88	32.43
DURATION	1,500	2.39	2.49	0.94	1.06	3.48
log(BANKRUPTCY_FEES)	350	2.01	1.82	1.52	0.15	4.39
BANKRUPTCY_FEES($ Mil)	350	22.33	5.19	38.19	0.17	79.92
BANKRUPTCY_FEES/ASSETS	350	0.04	0.02	0.06	0.00	0.10
FAMILY_RECOVERY(%)	433	55.52	56.29	28.51	15.37	100.00
EMERGENCE	1,501	0.53	1.00	0.50	0.00	1.00
REFILE_3Y	761	0.07	0.00	0.25	0.00	0.00
Δ_SALES	261	−0.20	−0.18	0.37	−0.69	0.25
ROA_POST	370	−0.02	−0.03	0.41	−0.50	0.53
log(NUM_MOTIONS)	649	4.22	4.32	1.24	2.77	5.74
NUM_MOTIONS	649	127.28	75.00	151.63	16.00	311.00
AVG_DAYS_RULING	647	33.13	29.73	19.15	15.87	54.23
NUM_PLANS	742	2.54	2.00	1.34	1.00	4.00
D(HIGH_PLANS)	742	0.19	0.00	0.39	0.00	1.00
Case Controls						
log(ASSETS)	1,501	5.65	5.79	1.94	3.03	8.10
ASSETS(Mils)	1,501	1,521.27	327.16	4,144.95	19.71	3,296.77
log(NUM_FILINGS)	1,501	1.47	1.10	1.07	0.69	3.04
NUM_FILINGS	1,501	8.53	2.00	18.59	1.00	20.00
LEVERAGE_FILING	1,501	1.06	0.90	0.76	0.48	1.69
ROA_FILING	1,501	−0.37	−0.14	0.66	−1.06	0.02
PREPACK_PRENEG	1,501	0.22	0.00	0.41	0.00	1.00
DEBT_CONCENTRATION	891	0.69	0.66	0.22	0.40	1.00
BUS_FILINGS/TOTAL_FILINGS	1,462	0.11	0.08	0.09	0.03	0.27
DIVERSITY_INDUSTRY	1,500	0.96	0.96	0.00	0.95	0.96
DIVERSITY_SIZE	1,500	0.64	0.64	0.03	0.60	0.67
PAST_TOTAL_FILINGS	1,462	28.28	19.82	25.29	3.70	65.21
DELEWARE	1,501	0.25	0.00	0.43	0.00	1.00
NYSD	1,501	0.16	0.00	0.37	0.00	1.00

not converted to Chapter 7 during bankruptcy is 2, with a 90th percentile of four plans. Public firms filing for bankruptcy have average assets at the time of filing of $1,521 million in 2016 U.S. dollars (with a median of $327 million), a liabilities-to-assets ratio of 1.06, and a –37% return on assets. Twenty-five percent of these cases are filed in Delaware and 16% are filed in the Southern District of New York (NYSD).

IV.   Judicial Assignments

The identifying assumption for our empirical strategy is that case assignment is uncorrelated with a judge's time on the bench and, therefore, that confounding

factors do not affect case outcomes in the same time-varying manner as judges' job-specific experience.[11] In this section, we compile direct evidence from U.S. bankruptcy courts, provide anecdotal evidence, review prior research on random assignment, and perform empirical tests to validate that corporate bankruptcy cases are randomly assigned with respect to judicial tenure.

First, we conduct a thorough search of the official website for each court in our sample to identify case assignment policies. For courts that do not explicitly state their policies online, we emailed chief clerks. We obtained policy statements from 81 courts that adjudicate 94% of the LexisNexis cases and 97% of the public-firm cases. Supplementary Material Table A1 provides a list of the courts and a summary of their case assignment procedures. Of these 81 courts, only one court uses a policy involving nonrandom assignment.[12] Each bankruptcy district contains multiple divisional offices, and cases are filed in these specific offices. Therefore, each case will be randomly assigned to one of the judges that operates in each divisional office.[13] Several courts indicate that they use the Federal Judiciary's comprehensive CM/ECF system, which "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). Courts' stated policies clearly support the notion of random assignment.

Second, anecdotal evidence supports the notion of random assignment, even for public firms. In the public-firm sample, 12.8% of cases were filed in a court where at least one of that court's judges had been on the bench for 2 years or less. Consistent with random assignment, 12.1% of public cases were assigned to one of these judges. For example, AMR Corporation, with $25 billion in assets (adjusted for inflation), filed in the Southern District of New York in Nov. 2011 and was assigned to Judge Sean H. Lane, who had been appointed to the bench only 14 months earlier. Even the largest U.S. bankruptcy to date, Lehman Brothers, was assigned to a relatively inexperienced judge. Judge James M. Peck had been appointed to the bench just 32 months prior to Lehman's filing and had the second briefest judicial experience of the 10 judges serving on the bench at the time. 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 who were serving in their first 2 years. Anecdotal evidence confirms that large firms can be assigned to inexperienced judges, consistent with random assignment.

---

[11]Our tests require only that assignments be uncorrelated with a judge's time on the bench. While all evidence suggests that corporate bankruptcy cases are fully randomly assigned, nonrandom assignment with respect to specific cases and fixed judge characteristics (e.g., a judge's background) does not invalidate our identification assumption.

[12]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.

[13]Nearly all public cases are filed in the main divisional office of each district. For example, among public cases filed in the NYSD in our sample, 93.3% are filed in Manhattan, 5.4% are filed in White Plains, and 1.2% are filed in Poughkeepsie.

Third, an increasing number of studies exploit the random assignment of bankruptcy judges for empirical identification (Chang and Schoar (2013), Dobbie and Song (2015), and Bernstein et al. (2019)). These studies uniformly find evidence that bankruptcy case characteristics are uncorrelated with judge characteristics. For example, Bernstein et al. (2019), employing a large sample of 28,000 unique bankruptcy filings from 1992 to 2005 similar to the LexisNexis sample used in this paper, 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 et al. (2016), and 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 in bankruptcy courts. Importantly, we note that forum shopping does not invalidate our research design, because we include division-time and judge fixed effects in our specifications to exploit random assignment within a given bankruptcy court division during a specific time period.

Existing studies' samples, like our LexisNexis sample, are dominated by small business filings. Thus the assumption of random assignment for the LexisNexis sample is strongly supported by prior research. There is, however, less empirical evidence of random assignment for publicly traded firms, although there is no systematic evidence to suggest assignment is not random. Nonetheless, experienced judges might compete for public cases, as overseeing these cases could lead to national recognition and prestigious status for a particular judge (LoPucki (2005)). Courts could also assign public cases that require extensive effort to judges who have considerable judicial experience, and those firms may have enough knowledge of the court system to strategically receive their desired judges, an even more controversial practice known as "judge shopping" (Norwood (1995), Wasby (1995), and Eisenberg and LoPucki (1999)).

To provide additional evidence that case assignment is uncorrelated with judicial experience, we conduct two sets of tests using both the LexisNexis and public-firm samples. First, we examine whether case characteristics are associated with an assigned judge's tenure. Specifically, we estimate the following regression:

$$(1) \quad \text{JUDGE\_EXP}_{i,j} = \alpha + \beta \times \text{CASE\_CHARACTERISTICS}_i + \theta_{\text{Division-time}} + \varepsilon_{i,j},$$

where $\text{JUDGE\_EXP}_{i,j}$ is either the log number of months judge j has been on the bench at the time case i was filed and assigned to the judge or an indicator of whether the judge has been on the bench for 2 years or less time as of the filing date. CASE_CHARACTERISTICS (controls) include log(NUM_FILINGS) and division-year fixed effects when analyzing the LexisNexis sample, and log(NUM_FILINGS), log(ASSETS), LEVERAGE_FILING, ROA_FILING, and PREPACK_PRENEG as well as division-decade and industry (Fama French 12) fixed effects when analyzing the public-firm sample. If cases are randomly assigned, we expect that case characteristics will jointly be insignificantly associated with our measures of judicial experience.

TABLE 2

Case Characteristics and Judicial Experience

Table 2 shows the estimates of the association between case characteristics and judicial experience. In columns 1–2, the sample comprises all private business Chapter 11 filings from 1993 through 2012 (LexisNexis sample) and in columns 3–4, the sample comprises all business Chapter 11 filings by public firms from 1980 through 2012 (Public Sample). The dependent variable (as indicated in the column header) is judge j's judicial experience as of the filing date for case i, measured as either the log number of months the judge has been on the bench (log(MONTHS)) or an indicator of whether the judge has been on the bench for 2 years or less (FIRST_2Y). We include log(NUM_FILINGS) and division-year fixed effects in columns 1 and 2 and log(NUM_FILINGS), log(ASSETS), LEVERAGE_FILING, ROA_FILING, and PREPACK_PRENEG as well as division-decade and industry (Fama French 12) fixed effects in columns 3 and 4. F-statistics and p-values for a test of the joint significance of the case characteristics are tabulated at the bottom of each column. Detailed variable definitions are provided in the Supplementary Material Appendix. Standard errors (clustered by court division) are reported in parentheses, and *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

$$\text{LNSample}: \text{JUDGE\_EXP}_{i,j} = \alpha + \beta \times \text{CASE\_CHARACTERISTICS} + \theta_{\text{Division}\times\text{Year}} + \varepsilon_{i,j}$$
$$\text{Public Sample}: \text{JUDGE\_EXP}_{i,j} = \alpha + \beta \times \text{CASE\_CHARACTERISTICS} + \theta_{\text{Division}\times\text{Decade}} + \rho_{\text{Industry}} + \varepsilon_{i,j}$$

	LexisNexis Sample		Public Firm Sample	
	log(MONTHS)	FIRST_2Y	log(MONTHS)	FIRST_2Y
	1	2	3	4
log(NUM_FILINGS)	−0.020	−0.001	0.014	−0.007
	(0.012)	(0.004)	(0.027)	(0.008)
log(ASSETS)			0.019	0.003
			(0.013)	(0.004)
LEVERAGE_FILING			0.071	−0.006
			(0.056)	(0.018)
ROA_FILING			0.100	−0.022
			(0.072)	(0.026)
PREPACK_PRENEG			0.141**	−0.025
			(0.058)	(0.016)
No. of Obs.	103,813	103,813	1,331	1,331
R^2	0.43	0.42	0.24	0.14
Within R^2	0.00	0.00	0.01	0.00
Division-time FE	Yes	Yes	Yes	Yes
Industry FE	No	No	Yes	Yes
F-stat	2.66	0.06	1.32	1.76
p-Value	0.10	0.81	0.27	0.13

We tabulate coefficient estimates of equation (1) for both the LexisNexis in columns 1 and 2 of Table 2 and public-firm sample in columns 3 and 4. F-statistics and the associated p-values for tests of the joint significance of the included case characteristics are tabulated in the table notes. We find that although one case characteristic (PREPACK_PRENEG), is individually significant in one specification for the public-firm sample, in all specifications the case controls jointly are unassociated with the assigned judge's judicial experience. Furthermore, the within R^2 in all four specifications is around 0, demonstrating that firm characteristics account for almost no variation within each division-time unit. Overall, the results are consistent with case assignment that is unassociated with judicial experience.

Second, we test whether each judge within a division is equally likely to be assigned a case. We identify all eligible judges serving in the court division where a given case was filed and estimate linear probability models of the following form:

$$(2) \qquad \text{ASSIGNED}_{i,j} = \alpha + \beta_1 \times \text{JUDGE\_EXP}_{i,j} + \theta_i + \varepsilon_{i,j},$$

where each observation is a unique case-judge pair using all eligible judges in a court division, and ASSIGNED$_{i,j}$ is an indicator variable that equals 1 if eligible

judge j was assigned case i and 0 otherwise. For each case, $\text{ASSIGNED}_{i,j}$ thus equals 1 for exactly one eligible judge and equals 0 for all other eligible judges.[14] $\text{JUDGE\_EXP}_{i,j}$ is one of two measures that capture eligible judge j's experience at the time case i was filed, namely log(MONTHS) or FIRST_2Y. To hold constant all case characteristics (including the set of available judges), we include case fixed effects (θ_i). Thus our analysis exploits within-case variation in the judicial experience of the eligible judges serving in the court division at the time case i was filed (i.e., the setting that best approximates the judicial assignment). With this fixed-effect structure, we drop cases with only 2 or fewer eligible judges, as we need multiple eligible judges per case. If corporate bankruptcy cases are more (or less) likely to be assigned to experienced judges, then the coefficient β_1 will be significant for either log(MONTHS) or FIRST_2Y.[15] A lack of any significant relationship is consistent with random assignment with respect to judicial experience. We cluster standard errors by court division.

Our final LexisNexis randomization sample consists of 75,977 cases filed between 1993 and 2012 in 62 courts and assigned to 478 judges. These cases had on average 5.7 judges (with a median of 5) serving at the same time who *could have been assigned the case*, resulting in 430,873 case-judge pairs. Our public-firm sample consists of 777 cases filed between 1993 and 2012 in 43 courts and assigned to 196 judges. These public cases have on average 6.3 judges (with a median of 6) who could have been assigned to them, resulting in 4,819 case-judge pairs.

Table 3 presents the results of estimating equation (2). The unconditional probability of being assigned a case (the mean of the dependent variable) is 0.18 for the LexisNexis sample and 0.16 for the public-firm sample. We find that log(MONTHS) and FIRST_2Y are statistically unrelated to case assignment in both samples. The coefficient estimates are not only insignificant, but also economically small relative to the mean of the dependent variable (e.g., the estimate in column 1 is 2.9% of the dependent variable), also consistent with case assignment that is uncorrelated with judicial experience. Importantly, there is significant within-case variation in judicial experience. For the LexisNexis sample, the average within-case standard deviation of MONTHS_AS_JUDGE is 84.6 months, with a standard deviation of 42 months. For the public-firm sample, the corresponding mean and standard deviation are 77.9 and 33.7 months, respectively. This significant within-case variation in judicial experience suggests that the lack of a significant relationship cannot be attributed to a lack of variation in the explanatory variable of interest.

Overall, based on court surveys, prior literature, anecdotal evidence, and empirical analysis, we conclude that judge assignment is random with respect to judicial experience. We next exploit this random assignment to evaluate the consequences of judicial experience.

---

[14]We use appointment and retirement dates to determine the set of eligible judges, excluding visiting and district judges.

[15]For example, if new judges are disproportionately assigned to consumer or small business Chapter 7 cases, we would find a negative relationship between experience and assignment to corporate Chapter 11 cases.

TABLE 3

Judicial Assignments and Experience

Table 3 shows the linear probability model estimates of judge case assignments using the set of eligible judges serving in the court division where each case was filed. The sample includes all eligible judge–case pairs based on judge appointment and retirement dates. We exclude as eligible judges all visiting and district judges and drop cases assigned to visiting or district judges as well as cases with 2 or fewer eligible judges. The dependent variable ($ASSIGNED_{i,j}$) is an indicator equal to 1 if eligible judge j was assigned to case i and 0 otherwise. Thus for each case i, $ASSIGNED_{i,j}$ equals 1 for only one of the eligible n judges serving in court division d at the time the case was filed and equals 0 for all remaining judges. We regress this assignment indicator on two separate measures of judicial experience: the log number of months eligible judge j has been on the bench when case i is filed (log(MONTHS)) and an indicator of whether eligible judge j has been on the bench for 2 years or less when case i was filed (FIRST_2Y). We include case fixed effects. Standard errors (clustered by court division) are reported in parentheses. Columns 1 and 2 present results for the LexisNexis sample and columns 3 and 4 present results for the public-firm sample.

$$ASSIGNED_{i,j} = \alpha + \beta_1 \cdot JUDGE\_EXP_{i,j} + \phi_i + \varepsilon_{i,j}$$

	Lexis Nexis Sample		Public Firm Sample	
	log(MONTHS)	FIRST_2Y	log(MONTHS)	FIRST_2Y
	1	2	3	4
Experience Measure	−0.006	0.010	0.010	−0.054
	(−1.57)	(0.97)	(0.82)	(−1.17)
No. of Obs.	430,873	430,873	4,819	4,819
Within R^2	0.0003	0.0001	0.0009	0.0022
Case FE	Yes	Yes	Yes	Yes

V.   Judicial Experience, Case Duration, and Bankruptcy Costs

In this section, we present our analysis of the relation between judicial experience and bankruptcy outcomes. We first analyze the effect of judicial experience on bankruptcy duration using both the LexisNexis and public-firm samples. We then focus on the public-firm sample to examine the effects of judicial experience on bankruptcy costs and other outcomes. Third, we explore several mechanisms through which experience affects case duration. Finally, we test for factors that could accelerate judges' learning curves.

A.   Judicial Experience and Case Duration

Our primary prediction is that judges accumulate valuable expertise by overseeing Chapter 11 filings, such that experienced judges are able to manage the bankruptcy process more efficiently than inexperienced judges and that this time-varying expertise is incremental to other fixed judge characteristics (such as previous work experience, gender, and so on). To test the impact of judicial experience on Chapter 11 case duration, we estimate OLS regressions of the following form:

(3) $\log(DURATION_i) = \alpha + \beta \times JUDGE\_EXP_{i,j} + \gamma \times CONTROLS_i + \delta FEs + \varepsilon_i,$

using the $\log(DURATION_i)$ and $JUDGE\_EXP_{i,j}$ measures defined previously for each case i assigned to judge j. In the LexisNexis sample, we include division-year fixed effects to control for trends in bankruptcy outcomes within each court division. We also include judge fixed effects, as previous work documents that fixed judge characteristics can explain bankruptcy outcomes. Because the public-firm sample includes far fewer observations for each court, we lack the statistical power

TABLE 4

Bankruptcy Duration and Judicial Experience

Table 4 shows estimates of the effects of judicial experience on corporate bankruptcy duration. In columns 1 and 2, the sample comprises all corporate Chapter 11 filings from LexisNexis filed between 1993 and 2012 (LexisNexis sample) and in columns 3–6, the sample comprises all corporate Chapter 11 filings by public firms filed between 1980–2012 (Public Firm Sample). We estimate the regressions provided below, where log(DURATION$_i$) is the log number of months case i spends under Chapter 11. The main explanatory variable is one of two measures of judicial experience as of the case filing date (indicated in the column header): the log number of months over which judge j assigned to case i has been on the bench (log(MONTHS)) and an indicator of whether judge j has been on the bench for 2 years or less (FIRST_2Y). We include a control for log(NUM_FILINGS) as well as judge and division-year fixed effects in columns 1 and 2, and controls for log(NUM_FILINGS), log(ASSETS), LEVERAGE_FILING, ROA_FILING, and PREPACK_PRENEG as well as judge, division-decade, and industry (Fama French 12) fixed effects in columns 3 and 4. In columns 5 and 6, we estimate a predicted duration for the public-firm sample after omitting the judicial experience measure, and then regress this predicted duration on each of the measures of judicial experience as well as division-decade and judge fixed effects. Detailed variable definitions are provided in the Supplementary Material Appendix. Standard errors (clustered by court division) are in parentheses, and *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

$$\text{LNSample:} \ \log(\text{DURATION})_i = \alpha + \beta \times \text{JUDGE\_EXP}_{i,j} + \gamma \times \text{CONTROLS}_i + \delta_j + \theta_{\text{Division}\times\text{Year}} + \varepsilon_i$$
$$\text{Public Sample:} \ \log(\text{DURATION})_i = \alpha + \beta \times \text{JUDGE\_EXP}_{i,j} + \gamma \times \text{CONTROLS}_i + \delta_j + \theta_{\text{Division}\times\text{Decade}} + \rho_{\text{Industry}} + \varepsilon_i$$

	LexisNexis Sample		Public Firm Sample			
	log(DURATION)		log(DURATION)		Falsification Predicted log(DURATION)	
	log(MONTHS)	FIRST_2Y	log(MONTHS)	FIRST_2Y	log(MONTHS)	FIRST_2Y
	1	2	3	4	5	6
Experience Measure	−0.065*** (0.011)	0.062** (0.028)	−0.127*** (0.026)	0.178** (0.085)	−0.041 (0.032)	−0.020 (0.038)
log(NUM_FILINGS)	0.354*** (0.043)	0.355*** (0.043)	0.068*** (0.018)	0.069*** (0.019)		
log(ASSETS)			0.048** (0.022)	0.046** (0.023)		
LEVERAGE_FILING			−0.046 (0.038)	−0.047 (0.039)		
ROA_FILING			0.023 (0.046)	0.018 (0.047)		
PREPACK_PRENEG			−1.128*** (0.069)	−1.136*** (0.066)		
No. of Obs.	103,812	103,812	1,255	1,255	1,255	1,255
R^2	0.17	0.17	0.49	0.49	0.50	0.50
Division-time FE	Yes	Yes	Yes	Yes	Yes	Yes
Judge FE	Yes	Yes	Yes	Yes	Yes	Yes
Industry FE	No	No	Yes	Yes	No	No

to include division-year fixed effects.[16] To approximate these fixed effects, we include division-decade fixed effects to control for trends in individual court divisions (including changes in a judge's cohort) as well as for fixed differences in cases across court divisions. For the public-firm sample, we also include judge and Fama-French 12-industry fixed effects as well as case-level controls (log (NUM_FILINGS), log(ASSETS), LEVERAGE_FILING, ROA_FILING, and PREPACK_PRENEG). We cluster standard errors by court division.

Table 4 presents coefficient estimates of equation (3), with analysis of the LexisNexis sample reported in columns 1 and 2 and analysis of the public-firm sample reported in columns 3 and 4. We find that cases assigned to judges with more time on the bench spend significantly less time in court. The coefficient estimates reported in columns 1 and 3 can be interpreted as elasticities, suggesting that being

---

[16]Including division-year fixed effects in the public-firm sample reduces the sample by 28% and eliminates 24 of the 70 court divisions. Coefficient estimates on the variables of interest are similar but standard errors are 1.6 to 2.3 times larger with this more restrictive research design.

randomly assigned to a judge with twice as much time on the bench (e.g., 2 vs. 4 years) is associated with a 6.5% decrease in bankruptcy duration in the Lexis-Nexis sample (a decline of 1 month relative to the mean of 16.7 months), and a 12.7% decline in the public-firm sample (a decline of 2 months relative to the mean of 16.1 months). The coefficient estimates on FIRST_2Y reported in columns 2 and 4 suggest that this effect concentrates during judges' early years: average cases assigned to judges in their first 2 years (relative to cases assigned to judges with more than 2 years experience) last 6% longer in the LexisNexis sample and 19% longer in the public-firm sample (increases of 1.0 and 3.1 months, respectively).[17] Consistent with the intuition that on-the-job experience matters most for highly complex cases, we find larger effects in the public-firm sample than in the LexisNexis sample using both measures of judicial experience.

We next expand this analysis to examine the average log(DURATION) at various levels of judicial experience, allowing us to map out judges' learning curves and better understand how long it takes a judge to become "experienced." We first estimate the learning curve for the LexisNexis sample using the following regression specification:

$$(4) \quad \log\left(\mathrm{DURATION}_i\right) = \alpha + \sum_{k=1}^{4} \beta_k \times \mathrm{YEAR}_{i,j}^k + \beta_5 \times \mathrm{YEAR}5 - 6_{i,j} + \beta_6$$

$$\times \mathrm{YEAR}7 - 8_{i,j} + \beta_7 \times \mathrm{YEAR}9 - 10_{i,j} + \gamma$$

$$\times \mathrm{CONTROLS}_i + \delta_j + \theta_{\mathrm{Division} \times \mathrm{Year}} + \varepsilon_i,$$

where δ represents a judge fixed effect, θ represents division-year fixed effects, and we include log(NUM_FILINGS) as a control. In Graph A of Figure 1, we plot the β coefficient estimates and 95% confidence intervals for each of the judicial experience dummy variables, individually for years 1 through 4, and then in 2-year periods for years 5–6, 7–8, and 9–10. Judges with more than 10 years of experience form the benchmark control group. We find that the effects of judicial inexperience concentrate early in a judge's tenure, with cases assigned in a judge's first year lasting 12% longer than cases assigned to more experienced judges. After the first year, coefficient estimates are insignificant.

In Graph B of Figure 1, we plot the learning-curve coefficient estimates for the public-firm sample, using the following specification:

$$(5) \quad \log\left(\mathrm{DURATION}_i\right) = \alpha + \beta_1 \times \mathrm{YEAR}1 - 2_{i,j} + \beta_2 \times \mathrm{YEAR}3 - 4_{i,j} + \beta_3 \times \mathrm{YEAR}5$$

$$- 6_{i,j} + \beta_4 \times \mathrm{YEAR}7 - 8_{i,j} + \beta_5 \times \mathrm{YEAR}9 - 10_{i,j} + \gamma$$

$$\times \mathrm{CONTROLS}_i + \delta_j + \theta_{\mathrm{Division} \times \mathrm{Decade}} + \rho_{\mathrm{Industry}} + \varepsilon_i,$$

where δ represents a judge fixed effect, θ represents division-decade fixed effects, ρ represents industry fixed effects, and we continue to include the full set of case
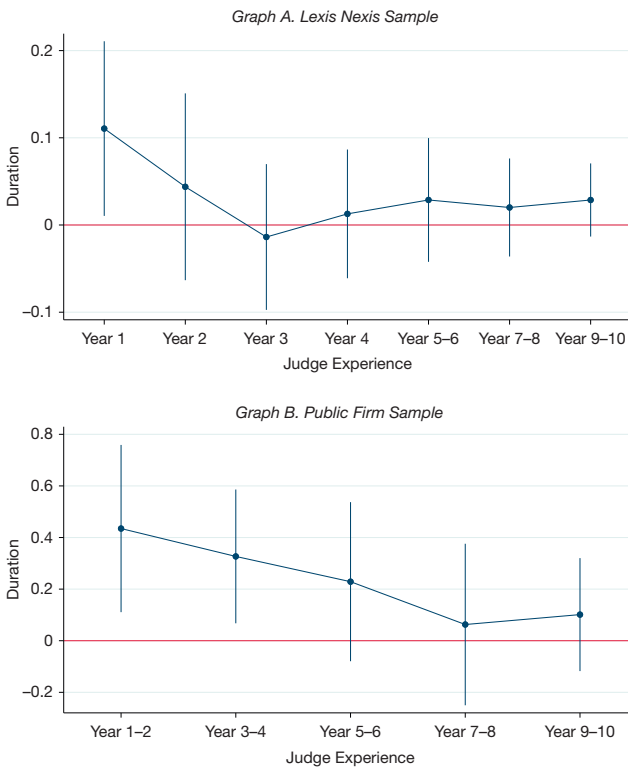
---

[17]Because we use a log-linear model, the estimated impact of moving from a judge with less than 2 years of experience to one with more than 2 years of experience in columns 2 and 4 is $100[\exp(\beta_1) - 1]$.

FIGURE 1

Judges' Learning Curves

Figure 1 depicts the effects of judicial experience on bankruptcy duration. In Graph A, we analyze the LexisNexis sample and in Graph B, we analyze the public-firm sample. We estimate the regressions provided below, where log(DURATION$_i$) is the log number of months case i spends under Chapter 11. The β coefficients (plotted circles) capture the effects of incremental years on the bench for judge j. In Graph A, we include log(NUM_FILINGS) as a control as well as judge and division-year fixed effects. In Graph B, we include log(NUM_FILINGS), log(ASSETS), LEVERAGE_FILING, ROA_FILING, and PREPACK_ PRENEG as controls as well as division-decade, judge, and industry (Fama French 12) fixed effects. Detailed variable definitions are provided in the Supplementary Material Appendix. Standard errors are clustered by court division and bars plot 95% confidence intervals.

$$\text{LNSample} : \log(\text{DURATION}_i) = \alpha + \sum_{k=1}^{4} \beta_k \times \text{YEAR}_{i,j}^k + \beta_5 \times \text{YEAR5} - 6_{i,j} + \beta_6 \times \text{YEAR7} - 8_{i,j} + \beta_7 \times \text{YEAR9} - 10_{i,j}$$
$$+ \gamma \times \text{CONTROLS}_i + \delta_j + \theta_{\text{Division} \times \text{Year}} + \varepsilon_i$$

$$\text{Public Sample} : \log(\text{DURATION}_i) = \alpha + \beta_1 \times \text{YEAR1} - 2_{i,j} + \beta_2 \times \text{YEAR3} - 4_{i,j} + \beta_3 \times \text{YEAR5} - 6_{i,j} + \beta_4 \times \text{YEAR7} - 8_{i,j}$$
$$+ \beta_5 \times \text{YEAR9} - 10_{i,j} + \gamma \times \text{CONTROLS}_i + \delta_j + \theta_{\text{Division} \times \text{Decade}} + \rho_{\text{Industry}} + \varepsilon_i$$

Graph A. Lexis Nexis Sample

Graph B. Public Firm Sample

controls. 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 54% longer duration (8.6 months) for cases assigned to judges in their first 2 years and 39% longer duration (6.2 months) for cases assigned to judges in their third and fourth years, 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

experience dummies are insignificant, suggesting that the duration of cases assigned to judges in their fifth through tenth years is similar to those of cases assigned to judges with more than 10 years of experience.

We note that the learning curve is flat in year 2 for the LexisNexis sample, whereas it can take up to 4 years for a judge to manage public cases in a manner that is similar to how more experienced judges manage such cases. These relatively different learning curves are consistent with faster learning associated with frequent, simpler tasks and slower learning associated with infrequent, more complex tasks. Previous work documents even shorter learning curves in other contexts. For example, Levitt, List, and Syverson (2013) estimate a learning curve of approximately 12 weeks in an automobile assembly plant, and Jovanovic and Nyarko (1995) estimate learning curves ranging from 2 weeks for munitions manufacturing workers to 1 year for insurance sales. From this perspective, even a learning curve of 1 year highlights significant differences between learning curves for relatively straightforward tasks as opposed to learning curves for complex tasks.[18]

We next perform falsification tests for the public-firm sample, much like those implemented in Christensen, Hail, and Leuz (2016) and Ljungqvist, Zhang, and Zuo (2017), to show that the empirical relation between judicial experience and duration is not driven by omitted unobserved heterogeneity. We report results in columns 5 and 6 of Table 4. Specifically, in the first stage, we regress case duration on the case characteristics and fixed effects, omitting the judicial-experience measures. In the second stage, we regress the predicted duration from this first stage on the judicial experience measures, omitting the case-characteristic variables, and tabulate the coefficients. If an omitted variable is correlated with both case duration and the assigned judge's judicial experience, we would expect to observe coefficients on the judicial experience variables that are similar to those reported in columns 3 and 4. Small and insignificant coefficients would suggest that confounding variables are unlikely to explain our baseline estimates. In columns 5 and 6, we find that the coefficients are smaller, insignificant, and that one has the opposite sign.

We conduct a number of tests to demonstrate the robustness of the duration results and report the results of these tests in Supplementary Material Table A2. In Panel A, we remove firms with the strongest ability to forum shop: firms with more than one subsidiary in the LexisNexis sample and the largest 20% of cases in asset size in the public-firm sample. In Panels B and C, we drop cases filed in either Delaware or the Southern District of New York, the 2 courts that are the targets of most forum shopping. In Panel D, we include only cases assigned to judges during their first terms to address a concern that the results could be driven by judges who are reappointed and thus have more experience.[19] In Panel E, we remove a handful

---

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

[19]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.

of cases from 13 courts where judge assignment is deterministic (i.e., courts with only one judge in a given year or courts that assign cases to judges based on specific geographic areas), and in Panel F, we remove all prepackaged/prenegotiated cases from the public-firm sample, as the results reported in Table 1 indicate that this case characteristic may be associated with judicial experience. The results are similar in each of these subsamples.

We also test whether our results are sensitive to the definition of log(DURATION). Supplementary Material Table A3 shows results for a subset of the public-firm sample where log(DURATION) is measured using the effective date of the reorganization plan (the date on which the plan went into effect) or the resolution date (the date on which the firm emerged from bankruptcy). Results are similar using these alternative measures.

The results reported in Supplementary Material Table A4 suggest that our main results are also robust to an alternative measure of judicial experience. In this table, we use the previous count of overall Chapter 11 cases or the count of public firm cases assigned to a judge rather than time on the bench as a measure of experience. We find that the duration of cases in both samples is decreasing in the log counts of both LexisNexis and public firm cases.

Finally, Ivashina, Iverson, and Smith (2016) show that debt concentration can be an important determinant of bankruptcy outcomes. We proxy for debt concentration by computing a Herfindahl index of debt types from CapitalIQ and find that our results are not affected by including this control, which is available for only a subset of our cases (see Supplementary Material Table A5).[20]

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, perhaps because of a deterioration in cognitive ability or a lack of performance incentives as judges near retirement. The data do not support this hypothesis. Instead, judges appear to maintain similar levels of productivity through the ends of their terms. Finally, we note that the shapes of the learning curves plotted in Figure 1 support 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.[21]

B.   Judicial Experience and Bankruptcy Costs

In the previous section, we document that cases assigned to less experienced judges spend more time in court. Prior research suggests that increased time in court is costly (e.g., Dou et al. (2021)), although direct, large-sample evidence is missing from the literature. In this section, we fill this gap and provide direct evidence that judicial inexperience is associated with higher bankruptcy costs by analyzing legal and professional fees and creditor recovery rates. We note that these tests are restricted to the sample of public firms with available data (e.g., disclosed legal fees or rated debt).

---

[20]Specifically, we measure the share of debt in each of six categories: revolving loans, term loans, secured bonds and notes, capital leases, other secured debt, and unsecured bonds and notes.

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

TABLE 5

Bankruptcy Costs

Table 5 shows the estimates of the effects of judicial experience on corporate bankruptcy costs. We analyze direct bankruptcy costs using the log number of professional and legal fees (log(BANKRUPTCY_FEES)) in columns 1 and 2, and overall bankruptcy costs using family recovery rates (FAMILY_RECOVERY(%)) in columns 3 and 4. We measure judicial experience using log(MONTHS) and FIRST_2Y. Division-decade, industry, and judge fixed effects are included in each regression, and additional case controls include log(ASSETS), log(NUM_FILINGS), LEVERAGE_FILING, ROA_FILING, and PREPACK_PRENEG. Detailed variable definitions are provided in the Supplementary Material Appendix. Standard errors (clustered by court division) are reported in parentheses, and *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

$$OUTCOME_i = \alpha + \beta_1 \times JUDGE\_EXP_{i,j} + \gamma \times CONTROLS_i + \delta_j + \theta_{Division \times Decade} + \rho_{Industry} + \varepsilon_i$$

	log(BANKRUPTCY_FEES)		FAMILY_RECOVERY(%)	
	log(MONTHS)	FIRST_2Y	log(MONTHS)	FIRST_2Y
	1	2	3	4
Experience Measure	−0.126**	0.313***	5.474***	−11.603***
	(0.052)	(0.053)	(1.467)	(2.279)
log(ASSETS)	0.528***	0.530***	−0.176	−0.196
	(0.043)	(0.044)	(1.047)	(1.051)
log(NUM_FILINGS)	0.205***	0.204***	−2.726	−2.568
	(0.040)	(0.038)	(3.471)	(3.569)
LEVERAGE_FILING	0.061	0.060	0.945	1.255
	(0.057)	(0.055)	(3.622)	(3.416)
ROA_FILING	0.126**	0.130***	3.741	2.499
	(0.046)	(0.044)	(11.359)	(12.069)
PREPACK_PRENEG	−0.338***	−0.339***	5.827**	6.272**
	(0.089)	(0.085)	(2.424)	(2.390)
No. of Obs.	259	259	302	302
R^2	0.84	0.84	0.26	0.26
Division-time FE	Yes	Yes	Yes	Yes
Judge FE	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes

We estimate a modified version of equation (3) using measures of bankruptcy costs as the dependent variable and report the results in Table 5. For columns 1–2, we use log(BANKRUPTCY_FEES) as the dependent variable, and find that judicial experience leads to lower professional fees. In terms of economic magnitude, doubling a judge's tenure leads to a 12.6% reduction in total legal fees, and cases assigned to judges in their first 2 years experience 37% higher fees.[22] These estimates are overall similar to those that indicated the effects of judicial inexperience on bankruptcy duration for public firms that we document in Table 4 (i.e., a 12.7% reduction in duration for doubling a judge's tenure and 19% more time in bankruptcy for cases assigned to inexperienced judges).

We next examine how judicial experience impacts overall debt recovery rates, a measure of total bankruptcy costs. Columns 3 and 4 of Table 5 report the results. We find that FAMILY_RECOVERY(%) increases with judges' total time on the bench and is lower for cases assigned to judges with 2 or fewer years of experience. In terms of economic magnitude, doubling a judge's tenure leads to a 5.5 percentage point increase in the family recovery rate, and cases assigned to judges' in their first 2 years on the bench realize an average recovery rate that is

---

[22]We also collect and analyze legal fees for a sample of 563 private cases filed in the Southern District of New York, where we obtained a PACER fee waiver, and report the results in Supplementary Material Table A6. The estimated coefficients for the effects of judicial experience are similar, albeit with weaker significance, for this sample.

11.6 percentage points lower (i.e., 21% lower than the mean). Our evidence is consistent with the notion that less experienced judges affect creditors' welfare negatively.[23]

To investigate the likelihood that costs generally increase with case duration, we analyze the relation between these two variables and report the results in Supplementary Material Table A7. We find that bankruptcy fees increase with and family recovery rates decrease with case duration. This is consistent with the notion that distress costs increase with case duration and supports prior studies that proxy for bankruptcy costs using case duration (Franks and Torous (1989), Thorburn (2000), and Bris et al. (2006)). We thus expect that the increased time in court that results from cases being assigned to inexperienced judges generates higher distress costs, even though we cannot directly measure these costs for our full sample.

Although there are likely additional costs (both direct and indirect) associated with lengthier bankruptcies, it is not clear whether lengthier bankruptcies are associated with less efficient restructurings. Longer bankruptcies could reflect closer 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.

To shed light on the economic efficiency of the bankruptcy restructuring, we analyze detailed information regarding case outcomes that are available in the public-firm sample. Specifically, we estimate a modified version of equation (3), using as dependent variables EMERGENCE, REFILE_3Y, Δ_SALES, and ROA_POST. We continue to include judge, division-decade, and industry fixed effects as well as all case controls. Although these outcomes are not available for all public cases (e.g., REFILE_3Y requires a firm to have emerged from bankruptcy, and Δ_SALES and ROA_POST require that a firm has emerged from bankruptcy and has remained public), this analysis provides additional insights into the costs of judicial inexperience.

We present the results in Table 6. We find that judicial experience is insignificantly associated with the likelihood that a firm emerges from bankruptcy (see columns 1–2). We also find that REFILE_3Y is lower for cases assigned to more experienced judges (see column 3), suggesting that inexperienced judges do not solve the issues that drive firms into bankruptcy in the first place.[24] Columns 5–8 confirm that postemergence performance is poorer when a firm is assigned to an

---

[23]Dou et al. (2021) estimate a structural model that incorporates two specific frictions related to bargaining among creditors, namely information asymmetry and conflicts of interest. They do not examine other real frictions such as judicial experience. Their estimates show that, when both asymmetric information and conflicts of interest are removed, creditors' recovery rates improve from 35.1% to 42.9%, a 7.8 percentage point increase. In contrast, our study shows that creditor recovery rates for cases assigned to judges' in their first 2 years are 11.6 percentage points lower than in cases assigned to more experienced judges. Although their estimates are based on a specific model while we provide a reduced-form estimation, comparing our estimates with theirs suggests that the costs of judicial inexperience are sizable.

[24]The coefficient on FIRST_2Y in column 4 is not statistically significant, with a *p*-value of 0.22, but it is directionally consistent with the notion that cases assigned to inexperienced judges exhibit marginally higher recidivism rates.

TABLE 6

Emergence and Postemergence Performance

Table 6 shows the estimates of the effects of judicial experience on corporate bankruptcy outcomes. We analyze the likelihood that a firm emerges from bankruptcy (EMERGENCE) in columns 1–2, the likelihood that a firm that emerges from bankruptcy refiles for bankruptcy within 3 years (REFILE_3Y) in columns 3–4, postemergence change in sales relative to sales at filing (Δ_SALES) in columns 3–4, and postemergence return on assets (ROA_POST) in columns 7–8. The sample includes all public firms with available data for each outcome. We measure judicial experience using log(MONTHS) and FIRST_2Y. Division-decade, industry, and judge fixed effects are included in each regression, and additional case controls include log(ASSETS), log(NUM_FILINGS), LEVERAGE_FILING, ROA_FILING, and PREPACK_PRENEG. Detailed variable definitions are provided in the Supplementary Material Appendix. Standard errors (clustered by court division) are reported in parentheses, and *, **, *** indicate significance at the 10%, 5%, and 1% levels, respectively.

$$\text{OUTCOME}_i = \alpha + \beta_1 \times \text{JUDGE\_EXP}_{i,j} + \gamma \times \text{CONTROLS}_i + \delta_j + \theta_{\text{Division} \times \text{Decade}} + \rho_{\text{Industry}} + \varepsilon_i$$

	EMERGENCE		REFILE_3Y		Δ_SALES		ROA_POST	
	log(MONTHS)	FIRST_2Y	log(MONTHS)	FIRST_2Y	log (MONTHS)	FIRST_2Y	log(MONTHS)	FIRST_2Y
	1	2	3	4	5	6	7	8
Experience Measure	0.008	0.037	−0.018***	0.023	0.058**	−0.208***	0.067**	−0.190**
	(0.015)	(0.057)	(0.006)	(0.018)	(0.025)	(0.045)	(0.029)	(0.089)
log(ASSETS)	0.046***	0.046***	0.007	0.007	0.037*	0.039**	0.061	0.060
	(0.008)	(0.008)	(0.005)	(0.005)	(0.018)	(0.017)	(0.038)	(0.038)
log(NUM_FILINGS)	0.033**	0.033**	0.024*	0.023	−0.005	−0.006	−0.030	−0.032
	(0.015)	(0.015)	(0.014)	(0.014)	(0.026)	(0.024)	(0.039)	(0.039)
LEVERAGE_FILING	0.091***	0.090***	0.042	0.041	0.174***	0.183***	0.197	0.193
	(0.020)	(0.021)	(0.025)	(0.025)	(0.041)	(0.044)	(0.129)	(0.127)
ROA_FILING	0.013	0.014	0.015	0.014	0.141***	0.108**	0.242	0.240
	(0.033)	(0.033)	(0.028)	(0.028)	(0.041)	(0.045)	(0.195)	(0.190)
PREPACK_PRENEG	0.284***	0.285***	0.052*	0.051*	0.028	0.028	−0.046	−0.038
	(0.027)	(0.027)	(0.030)	(0.030)	(0.043)	(0.039)	(0.080)	(0.085)
No. of Obs.	1,256	1,256	542	542	156	156	211	211
R^2	0.37	0.37	0.24	0.24	0.49	0.50	0.39	0.40
Division-time FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Judge FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

inexperienced judge. In columns 5 and 6, we analyze Δ_SALES, and in columns 7 and 8, we analyze ROA_POST. We find that doubling judicial experience is associated with a 5.8 percentage-point increase in the change in sales revenue (see column 5), and that cases assigned to inexperienced judges result in a change in sales revenue that is 21 percentage points lower (see column 6). Similarly, we find that doubling judicial experience is associated with a 6.7 percentage-point increase in postemergence ROA (see column 7), and that cases assigned to inexperienced judges realize nearly 19 percentage points lower postemergence ROAs (see column 8).[25] Experienced judges appear to better position firms assigned to them for superior postemergence performance, although we caution that sample sizes are smaller for these outcome variables and limited to select sets of firms.

C. Mechanism

We next investigate mechanisms whereby experienced judges can accelerate the bankruptcy process. For all tests discussed in this subsection, we continue to analyze the public-firm sample (i.e., the sample involving judges with the

---

[25] We winsorize ROA_POST at ±100% to ensure that this result is not driven by a small number of outliers.

TABLE 7

Bankruptcy Motions

Table 7 shows the estimates of the effects of judicial experience on motions filed during bankruptcy for public firms with available court-docket data from PACER over the years 2002–2012. We analyze the log number of motions filed log(MOTIONS)) in columns 1–2 and the average number of days from the motion filing date to the passing of a corresponding order, excluding first-day motions (AVG_DAYS_RULING) in columns 3–4. We measure judicial experience using log(MONTHS) and FIRST_2Y. Division-decade, industry, and judge fixed effects are included in each regression, and additional controls include log(ASSETS), log(NUM_FILINGS), LEVERAGE_FILING, ROA_FILING, and PREPACK_PRENEG. Detailed variable definitions are provided in the Supplementary Material Appendix. Standard errors (clustered by court division) are reported in parentheses, and *, **, *** indicate significance at the 10%, 5%, and 1% levels, respectively.

$$\text{OUTCOME}_i = \alpha + \beta_1 \times \text{JUDGE\_EXP}_{i,j} + \gamma \times \text{CONTROLS}_i + \delta_j + \theta_{\text{Division} \times \text{Decade}} + \rho_{\text{Industry}} + \varepsilon_i$$

	log(MOTIONS)		AVG_DAYS_RULING	
	log(MONTHS)	FIRST_2Y	log(MONTHS)	FIRST_2Y
	1	2	3	4
Experience Measure	−0.012	0.153	−2.696**	4.833***
	(0.110)	(0.124)	(1.203)	(1.165)
log(ASSETS)	0.261***	0.261***	0.112	0.130
	(0.024)	(0.024)	(0.292)	(0.285)
log(NUM_FILINGS)	0.225***	0.225***	0.873*	0.840*
	(0.062)	(0.062)	(0.474)	(0.488)
LEVERAGE_FILING	0.009	0.009	−2.693**	−2.685**
	(0.060)	(0.060)	(1.272)	(1.252)
ROA_FILING	−0.016	−0.011	−1.790	−1.778
	(0.063)	(0.067)	(1.389)	(1.358)
PREPACK_PRENEG	−0.835***	−0.830***	−6.431***	−6.514***
	(0.051)	(0.051)	(1.171)	(1.219)
No. of Obs.	502	502	502	502
R^2	0.71	0.71	0.27	0.27
Division-time FE	Yes	Yes	Yes	Yes
Judge FE	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes

steepest learning curves and the richest data) using regressions that are similar to equation (3).

We first examine whether more motions are filed in cases with inexperienced judges, which could slow down court processes. In columns 1 and 2 of Table 7, we use log(MOTIONS) as the dependent variable and find that the coefficient estimate on each measure of judicial experience is insignificant. Experienced judges do not appear to accelerate bankruptcies by reducing the total number of actions taken during bankruptcy proceedings.

We next examine whether experienced judges are quicker to rule on motions, possibly expediting the bankruptcy process. In columns 3 and 4 of Table 7, we use AVG_DAYS_RULING as the dependent variable and find that inexperienced judges take more time to rule on motions. We estimate that a judge with twice as much experience on average issues orders almost 3 days faster, an 8% reduction relative to the sample average of 33 days. In contrast, judges with less than 2 years of experience take on average 4.8 more days to rule on motions, a 15% increase. These economic magnitudes are comparable to the overall effects of judicial experience on log(DURATION) and distress costs, suggesting that a significant portion of the overall increase in duration and fees for cases assigned to inexperienced judges reflects the judges' inability to rule quickly on motions.

Finally, we examine whether experienced judges require fewer actions to complete restructuring. Longer-tenured judges may be more decisive or better able

TABLE 8
Reorganization Plans

Table 8 shows the estimates of the effects of judicial experience on the number of reorganization/liquidation plans filed during bankruptcy proceedings for public firms with available data and that are not converted to Chapter 7 (where no plan is filed). We analyze the overall number of plans (NUM_PLANS) in columns 1–2 and an indicator of whether more than three plans are filed (D(HIGH_PLANS)) in columns 3–4. We measure judicial experience using log(MONTHS) and FIRST_2Y. Division-decade, industry, and judge fixed effects are included in each regression, and additional case controls include log(ASSETS), log(NUM_FILINGS), LEVERAGE_FILING, ROA_FILING, and PREPACK_PRENEG. Detailed variable definitions are provided in the Supplementary Material Appendix. Standard errors (clustered by court division) are in parentheses, and *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

$$\text{OUTCOME}_i = \alpha + \beta_1 \times \text{JUDGE\_EXP}_{i,j} + \gamma \times \text{CONTROLS}_i + \delta_j + \theta_{\text{Division}\times\text{Decade}} + \rho_{\text{Industry}} + \varepsilon_i$$

	NUM_PLANS		D(HIGH_PLANS)	
	log(MONTHS)	FIRST_2Y	log(MONTHS)	FIRST_2Y
	1	2	3	4
Experience Measure	−0.078*	0.468***	−0.052**	0.195***
	(0.043)	(0.172)	(0.020)	(0.046)
log(ASSETS)	0.091	0.091	0.037	0.037
	(0.079)	(0.080)	(0.027)	(0.027)
log(NUM_FILINGS)	0.009	0.008	0.019	0.019
	(0.094)	(0.092)	(0.021)	(0.020)
LEVERAGE_FILING	−0.074	−0.079	−0.024	−0.025
	(0.078)	(0.080)	(0.025)	(0.026)
ROA_FILING	0.058	0.067	0.010	0.013
	(0.135)	(0.138)	(0.036)	(0.038)
PREPACK_PRENEG	−0.266*	−0.255*	−0.034	−0.031
	(0.138)	(0.132)	(0.036)	(0.034)
No. of Obs.	638	638	638	638
R^2	0.31	0.31	0.26	0.27
Industry FE	Yes	Yes	Yes	Yes
Division-time FE	Yes	Yes	Yes	Yes
Judge FE	Yes	Yes	Yes	Yes

to foresee issues that might arise and thereby prevent unnecessary disputes. To test this hypothesis, we examine whether fewer plans of reorganization or liquidation are filed in cases assigned to experienced judges. This plan is perhaps the most important motion filed during bankruptcy, and the bankruptcy process cannot conclude until all parties agree to such a plan (or are forced to by the judge). In columns 1 and 2 of Table 7, we use the dependent variable NUM_PLANS, and in columns 3 and 4 we use a dummy indicator that takes the value of 1 if more than three plans were filed (D(HIGH_PLANS)). We find that cases assigned to judges in their first 2 years have 0.47 more plans, an 18% increase over the sample mean, and are significantly more likely to have more than three plans filed. In contrast, fewer plans are filed in cases filed with more experienced judges. A failure to get all parties to agree to a reorganization/liquidation plan quickly plausibly contributes to longer duration and higher fees as creditors and firms must review and respond to each proposed plan.

A larger number of plans and increased time to issue rulings can both increase the total time in bankruptcy. To illustrate this point, in Supplementary Material Table A8, we regress case duration on AVG_DAYS_RULING and NUM_PLANS. We standardize both right-hand-side variables to a mean of 0 and a standard deviation of 1 to facilitate comparisons between these two factors. We find that both mechanisms have a similar economic impact on bankruptcy duration regardless of the fixed-effect structure used: a 1-standard-deviation increase in motion

ruling is associated with a 32%–39% increase in duration, and a 1-standard-deviation increase in reorganization plans is associated with a 25% increase in duration.

D.   Learning Accelerators

Given the high costs that inexperience imposes on bankrupt firms, it is worthwhile to consider ways to accelerate learning for bankruptcy judges. Using insights from the learning-by-doing and human capital literature (e.g., Arrow (1962), Becker (1962), and Lazear (2009)), we examine two factors that potentially accelerate judicial learning. First, we posit that judges who accrue *relevant* experience early in their judicial careers move up the learning curve faster. In particular, a judge who oversees more corporate cases early in her career will likely be better prepared to manage a large Chapter 11 case than one who has been assigned primarily to consumer bankruptcy cases.[26] Second, while exposure to relevant tasks is useful, there are likely diminishing returns to seeing a large number of similar business cases. Seeing cases from a diversity of industries will potentially provide more useful experience than seeing a similar number of cases from the same industry. We expect this to be particularly true for public-firm bankruptcies, which are inherently complex and typically include subsidiaries that span multiple industries. We thus predict that judges who are exposed to a greater diversity of business cases "move up the learning curve" faster and handle complex cases more efficiently.

We construct judge-specific empirical measures for relevant business filings using quarterly court-level filing statistics from the U.S. Courts Administrative Office. Because cases are randomly assigned to judges, we can closely proxy the number of business and personal bankruptcy cases assigned to a judge in a given quarter as the total number of business or personal cases divided by the total number of judges in the court in that quarter. We 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 nonbusiness bankruptcies, and then standardize the measure to a mean of 0 and a standard deviation of 1. We compare the effects of judges' previous experience with business or nonbusiness cases on the duration of their public firms by estimating the following specification:

$$(6) \quad \log(\text{DURATION}_i) = \alpha + \beta_1 \times \text{BUSINESS}_{i,j} + \beta_2 \times \text{NONBUSINESS}_{i,j} + \gamma$$
$$\times \text{CONTROLS}_i + \delta_j + \theta_{\text{Division} \times \text{Decade}} + \rho_{\text{Industry}} + \varepsilon_i$$

We present the effects of judges' business or nonbusiness experience in Figure 2. Graph A shows a binned scatterplot of β_1 and Graph B a binned scatterplot of β_2. Panel A indicates that cases assigned to judges with more business bankruptcy experience have significantly shorter duration. A 1-standard-deviation

---

[26]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 to the large corporate cases than do judges who spend the majority of their time on nonbusiness bankruptcies.

FIGURE 2

Business- and Nonbusiness-Case Experience

Figure 2 depicts the effects of judges' previous business- and nonbusiness-case counts on bankruptcy duration for the public-firm sample using binned scatterplots. We estimate the regression provided below, where DURATION$_i$ is the log number of months case i spends under Chapter 11, BUSINESS$_{i,j}$ is the cumulative number of business cases judge j assigned to case i has previously seen, and NONBUSINESS$_{i,j}$ is the number of nonbusiness cases judge j has previously seen. Both BUSINESS and NONBUSINESS are standardized to a mean of 0 and a standard deviation of 1. We include as additional controls log(NUM_FILINGS), log(ASSETS), LEVERAGE_FILING, ROA_FILING, and PREPACK_PRENEG as well as division-decade, judge, and industry (Fama French 12) fixed effects. Detailed variable definitions are provided in the Supplementary Material Appendix. Standard errors are clustered by court division.

$$\log(\text{DURATION}_i) = \alpha + \beta_1 \times \text{BUSINESS}_{i,j} + \beta_2 \times \text{NONBUSINESS}_{i,j} + \gamma \times \text{CONTROLS}_i + \delta_j + \theta_{\text{Division} \times \text{Decade}} + \rho_{\text{Industry}} + \varepsilon_i$$

Graph A. Business   Graph B. Non-Business
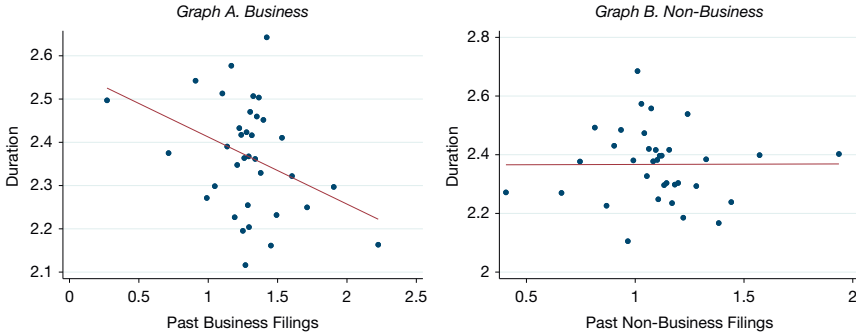
increase in the business-case count reduces case duration by 13.6% (2.2 months) with a p-value of 0.037. In contrast, we do not observe a similar effect in Panel B for past nonbusiness-case experience: the coefficient estimate for β_2 is –3.6% with a p-value of 0.579. The evidence presented in Figure 2 suggests that it is specifically relevant experience that helps judges move up the learning curve more quickly for large Chapter 11 cases. In light of this evidence, there are at least two policies that could be considered to help reduce bankruptcy costs. First, some specialization among judges could accelerate their learning curves. Second, because the business filing measure includes exposure to smaller corporate cases, our results suggest that a policy that assigns new judges initially only to small cases could improve their ability to handle more complex cases later.

Our second hypothesis is that seeing a diversity of business bankruptcy cases enables judges to learn more quickly. We measure case diversity along two dimensions: industry and size. We create both diversity measures using the Census County Business Patterns data set covering the years 1986 through 2015. To measure industry diversity, we first calculate the share of business establishments in a bankruptcy court in each 2-digit SIC industry and convert this to a diversity measure (DIVERSITY-INDUSTRY), defined as the reciprocal of 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 the size distribution of businesses that file for bankruptcy in a district is similar to the distribution of the overall set of businesses in the area. We calculate DIVERSITY-SIZE as the reciprocal of the Herfindahl concentration index of these size buckets.

TABLE 9

Learning Accelerators

Table 9 shows the relation between court diversity characteristics and DURATION. The dependent variable is DURATION$_i$, the log number of months case i spends under Chapter 11. The explanatory variables of interest are DIVERSITY-INDUSTRY, the diversity of industries located within a court's geographic boundaries based on census data, and DIVERSITY-SIZE, the diversity of business establishments within the court's geographic boundaries based on census data. We include all public-firm cases assigned to judges during their first 4 years on the bench in columns 1–2 and all public-firm cases assigned to judges during their first 6 years on the bench in columns 3–4. All explanatory variables are standardized to a mean of 0 and a standard deviation of 1. We include as controls log(ASSETS), log(NUM_FILINGS), LEVERAGE_FILING, ROA_FILING, and PREPACK_PRENEG as well as filing-year fixed effects. Detailed variable definitions are provided in the Supplementary Material Appendix. Standard errors (clustered by court division) are reported in parentheses, and *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

$$\log(\text{DURATION}_i) = \alpha + \beta_1 \times \text{DIVERSITY}_i + \gamma \times \text{CONTROLS}_i + \theta_{\text{FilingYear}} + \varepsilon_i$$

	First Four Year		First Six Year	
	1	2	3	4
DIVERSITY_INDUSTRY	−0.069*		−0.077**	
	(0.039)		(0.036)	
DIVERSITY_SIZE		−0.090***		−0.069*
		(0.033)		(0.037)
log(ASSETS)	0.073***	0.076***	0.055***	0.057***
	(0.021)	(0.023)	(0.014)	(0.014)
log(NUM_FILINGS)	0.062	0.060	0.055**	0.056**
	(0.040)	(0.040)	(0.025)	(0.025)
LEVERAGE_FILING	−0.008	−0.015	−0.040	−0.046
	(0.049)	(0.049)	(0.048)	(0.047)
ROA_FILING	0.001	−0.015	0.016	0.005
	(0.060)	(0.060)	(0.052)	(0.051)
PREPACK_PRENEG	−1.125***	−1.137***	−1.182***	−1.183***
	(0.080)	(0.074)	(0.104)	(0.104)
No. of Obs.	373	373	543	543
R^2	0.42	0.42	0.44	0.44
Filing year FE	Yes	Yes	Yes	Yes
Case controls	Yes	Yes	Yes	Yes

Using these measures, we estimate a modified version of equation (3). To examine how variation in type of experience affects case outcomes holding judge tenure constant, we restrict this analysis to large cases assigned to judges in either their first 4 years (370 cases) or first 6 years (538 cases) on the bench. We standardize all diversity measures to a mean of 0 and a standard deviation of 1 and cluster standard errors by court division.

In Table 9, we find that judges in courts whose jurisdictions feature more highly diversified local industry compositions resolve Chapter 11 cases faster than do judges with similar tenures who are located in courts with less diversified industry compositions. A 1-standard-deviation increase in DIVERSITY-INDUSTRY leads to a 6.9% reduction in bankruptcy duration among all cases assigned to judges during their first 4 years on the bench. Judges who oversee a broader mix of firms as measured by size are also able to resolve Chapter 11 cases faster. A 1-standard-deviation increase in DIVERSITY-SIZE, based on the estimate reported in column 2, is associated with a 9.0% reduction in bankruptcy duration. We document similar effects in columns 3 and 4 using all cases assigned to judges during their first 6 years. Collectively, our evidence suggests that exposure to more relevant tasks as well as task variety during judges' early years accelerates their ability to manage complex public Chapter 11 cases efficiently.

VI.   Discussion and Conclusion

Exploiting the random assignment of Chapter 11 filings to bankruptcy judges, we provide evidence that judicial inexperience imposes significant financial costs on firms that are in bankruptcy. We find that cases assigned to new judges spend more time in bankruptcy, principally reflecting delays in rulings on individual motions and failure to attain speedy consensus from creditors on reorganization or liquidation plans. Among public firms, we find that cases assigned to new judges incur higher professional fees, realize lower creditor recovery rates, and are followed by worse postemergence financial performance. The findings are collectively consistent with the notion that new judges are less efficient at managing the restructuring process for complex firms. Our estimates of judges' learning curves suggest that it takes up to 4 years for a judge to manage complex Chapter 11 filings efficiently. Exposure to business cases and a greater diversity of case types accelerates judges' ascents up their learning curves.

While on-the-job learning is clearly costly, one feature of bankruptcy courts is that these costs are generally not borne by judges but by firms that are 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" reductions in aggregate costs for each of these counterfactuals relative to the current policy of complete random assignment. Because these estimates require information regarding firm size, we use the public-firm sample and thus provide a significantly lower bound on the total costs of inexperience.[27]

First, using the 350 cases for which we were able to obtain legal fees, we estimate that total legal fees equal approximately 4.04% (2.15%) of total assets for the average (median) case assigned to a judge with more than 2 years of experience.[28] Based on the results reported in Table 4, we estimate that these direct costs increase by 31.3% when assigned to an inexperienced judge, which drives up fees to 5.32% (2.82%) of assets for the average (median) case. Extrapolating these fee percentages to the 83 public cases assigned to inexperienced judges in our sample and aggregating them yields an estimate of $1.17 to $2.24 billion in direct distress costs that result from judicial inexperience.

The above exercise estimates only increase in professional fees that are attributable to judicial inexperience. We can use the estimates reported in column 4 of Table 5 to get a sense of how inexperience affects the overall recovery rate associated with public cases. Specifically, we estimate that recovery rates would rise by 11.6 percentage points if cases were reassigned to judges with more than 2 years of experience. For the 83 cases assigned to inexperienced judges in our sample, this results in $16.8 billion lost to judicial inexperience. This estimate is substantially higher than the estimate of increased professional fees, suggesting

---

[27]The costs of judicial inexperience must also be compared with the benefits of the random assignment of judges (the predominant current model) which include avoiding judicial capture by debtor firms. Although we cannot quantify these benefits and thus cannot estimate the overall welfare effects, we nonetheless present estimates of the costs of judicial inexperience for consideration.

[28]These estimates are similar to estimates reported in prior work that total legal fees equal approximately 2% of total assets (LoPucki and Doherty (2004), Bris et al. (2006)).

that the indirect costs of judicial inexperience are significantly higher than the direct costs.

Finally, we can estimate the costs of judicial inexperience in a slightly less direct manner by assuming that legal fees scale linearly with case duration. Under this assumption, estimates reported in Graph B of Figure 1 suggest that case duration and, hence, legal fees, would be reduced by 35% if a case was reassigned from a judge with less than 2 years of experience to a judge with more than 7 years of experience.[29] In a counterfactual world in which public bankruptcies are always assigned to the most experienced judge in a given district, we estimate that legal fees would decrease by $8.5 billion by reassigning the 777 public-firm cases in our sample that were not assigned to the most experienced judge.[30] An alternative and more selective approach would focus on only 83 of these cases assigned to judges with less than 2 years of experience. Reassigning these 83 cases to the most experienced judge in a given court would reduce legal fees by $1.3 billion, which is similar in magnitude to our estimate based on changes in direct fees for these cases. Importantly, these counterfactual estimates focus only on the direct costs of bankruptcy that are attributable to legal fees. As illustrated above, the indirect costs are likely significantly higher.

Any policy must also consider how new judges will obtain the experience necessary to manage complex corporate restructurings efficiently. By the time most judges are assigned public cases, they have already served as judges for several years. Our results suggest that experience with prior business filings, including small private cases, can help reduce the duration of bankruptcies for large public firms. These findings suggest 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.

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

Supplementary Material

To view supplementary material for this article, please visit http://doi.org/10.1017/S002210902200062X.

---

[29] The coefficient estimate reported in Graph B of Figure 1 for Year $1-2$ is 0.4335. Because these are log-linear specifications, the reduction in duration that would be expected when moving a case to an experienced judge is $100*(e^{(0-.4335)}-1) = -35\%$.

[30] This estimate assumes the more conservative median legal fee of 2.15% of total assets.

References

Almeida, H., and T. Philippon. "The Risk-Adjusted Cost of Financial Distress." *Journal of Finance*, 62 (2007), 2557–2586.

Andrade, G., and S. N. Kaplan. "How Costly is Financial (Not Economic) Distress? Evidence from Highly Leveraged Transactions that Became Distressed." *Journal of Finance*, 53 (1998), 1443–1493.

Arrow, K. J. "The Economic Implications of Learning by Doing." *Review of Economic Studies*, 29 (1962), 155–173.

Ashenfelter, O.; T. Eisenberg; and S. Schwab. "Politics and the Judiciary: The Influence of Judicial Background on Case Outcomes." *Journal of Legal Studies*, 24 (1995), 257–281.

Badolato, P. G.; D. C. Donelson; and M. Ege. "Audit Committee Financial Expertise and Earnings Management: The Role of Status." *Journal of Accounting and Economics*, 58 (2014), 208–230. 2013 Conference Issue.

Becker, G. S. "Investment in Human Capital: A Theoretical Analysis." *Journal of Political Economy*, 70 (1962), 9–49.

Becker, B., and P. Stromberg. "Fiduciary Duties and Equity-Debtholder Conflicts." *Review of Financial Studies*, 25 (2012), 1931–1969.

Bernstein, S.; E. Colonnelli; and B. Iverson. "Asset Allocation in Bankruptcy." *Journal of Finance*, 74 (2019), 5–53.

Bertrand, M., and S. Mullainathan. "Enjoying the Quiet Life? Corporate Governance and Managerial Preferences." *Journal of Political Economy*, 111 (2003), 1043–1075.

Boone, A. L., and V. I. Ivanov. "Bankruptcy Spillover Effects on Strategic Alliance Partners." *Journal of Financial Economics*, 103(2012), 551–569.

Bradley, D.; S. Gokkaya; and X. Liu. "Before an Analyst Becomes an Analyst: Does Industry Experience Matter?" *Journal of Finance*, 72 (2017), 751–792.

Bris, A.; I. Welch; and N. Zhu. "The Costs of Bankruptcy: Chapter 7 Liquidation Versus Chapter 11 Reorganization." *Journal of Finance*, 61 (2006), 1253–1303.

Chang, T., and A. Schoar. "Judge Specific Differences in Chapter 11 and Firm Outcomes." Working Paper, available at https://papers.ssrn.com/sol3/papers.cfm?abstract_id=890699 (2013).

Chen, D. L.; T. J. Moskowitz; and K. Shue. "Decision Making under the Gambler's Fallacy: Evidence from Asylum Judges, Loan Officers, and Baseball Umpires." *Quarterly Journal of Economics*, 131 (2016), 1181–1242.

Chilton, A. S., and M. K. Levy. "Challenging the Randomness of Panel Assignment in the Federal Courts of Appeals." *Cornell Law Review*, 101 (2015), 1–56.

Chow, T.; A. Huang; K. W. Hui; and T. J. Shevlin. "Judge Ideology and Corporate Tax Planning." Working Paper, available at https://papers.ssrn.com/sol3/papers.cfm?abstract_id=3513154 (2021).

Christensen, H.; L. Hail; and C. Leuz. "Capital-Market Effects of Securities Regulation: Prior Conditions, Implementation, and Enforcement." *Review of Financial Studies*, 29 (2016), 2885–2924.

Chychyla, R.; A. J. Leone; and M. Minutti-Meza. "Complexity of Financial Reporting Standards and Accounting Expertise." *Journal of Accounting and Economics*, 67 (2019), 226–253.

Clement, M. B. "Analyst Forecast Accuracy: Do Ability, Resources, and Portfolio Complexity Matter?" *Journal of Accounting and Economics*, 27 (1999), 285–303.

Cohen, A., and C. Yang. "*Judicial Politics and Sentencing Decisions*." Technical report, National Bureau of Economic Research (2018).

DeAngelo, G., and E. G. Owens. "Learning the Ropes: General Experience, Task-Specific Experience, and the Output of Police Officers." *Journal of Economic Behavior and Organization*, 142 (2017), 368–377.

DeFond, M. L.; R. N. Hann; and X. Hu. "Does the Market Value Financial Expertise on Audit Committees of Boards of Directors?." *Journal of Accounting Research*, 43 (2005), 153–193.

Denis, D., and K. Rodgers. "Chapter 11: Duration, Outcome, and Post Reorganization Performance." *Journal of Financial and Quantitative Analysis*, 42 (2007), 101–118.

Dobbie, W., and J. Song. "Debt Relief and Debtor Outcomes: Measuring the Effects of Consumer Bankruptcy Protection." *American Economic Review*, 105 (2015), 1272–1311.

Dou, W.; L. Taylor; W. Wang; and W. Wang. "Dissecting Bankruptcy Frictions." *Journal of Financial Economics*, 142 (2021), 975–1000.

Eckbo, E.; K. Li, and W. Wang. "Rent Extraction by Super-Priority Lenders." Working Paper, Dartmouth College (2020).

Eisenberg, T., and L. LoPucki. "Shopping for Judges: An Empirical Analysis of Venue Choice in Large Chapter 11 Reorganizations." *Cornell Law Review*, 84 (1999), 967–1003.

Elkamhi, R.; J. Ericsson; and C. A. Parsons. "The Cost and Timing of Financial Distress." *Journal of Financial Economics*, 105 (2012), 62–81.

Erkens, D. H., and S. E. Bonner. "The Role of Firm Status in Appointments of Accounting Financial Experts to Audit Committees." *Accounting Review*, 88 (2013), 107–136.

Franks, J. R., and W. N. Torous. "An Empirical Investigation of U.S. Firms in Reorganization." *Journal of Finance*, 44 (1989), 747–770.

Gennaioli, N., and S. Rossi. "Judicial Discretion in Corporate Bankruptcy." *Review of Financial Studies*, 23 (2010), 4078–4114.

Gibbons, R., and M. Waldman. "Task-Specific Human Capital." *American Economics Review: Papers and Proceedings*, 94 (2004), 203–207.

Goyal, V. K., and W. Wang. "Provision of Management Incentives in Bankrupt Firms." *Journal of Law, Finance, and Accounting*, 2 (2017), 87–123.

Hall, M. "Randomness Reconsidered: Modelling Random Judicial Assignment in the U.S. Court of Appeals." *Journal of Empirical Legal Studies*, 3 (2010), 574–589.

Harris, D. N., and T. R. Sass. "Teacher Training, Teacher Quality and Student Achievement." *Journal of Public Economics*, 95 (2011), 798–812.

Heron, R. A.; E. Lie; and K. J. Rodgers. "Financial Restructuring in Fresh-Start Chapter 11 Reorganizations." *Financial Management*, 38 (2009), 727–745.

Hertzel, M.; Z. Li; M. Officer; and K. Rodgers. "Inter-Firm Linkages and the Wealth Effects of Financial Distress along the Supply Chain." *Journal of Financial Economics*, 87 (2008), 374–387.

Hotchkiss, E. "Postbankruptcy Performance and Management Turnover." *Journal of Finance*, 50 (1995), 3–21.

Huang, A.; K. W. Hui; and R. Z. Li. "Federal Judge Ideology: A New Measure of Ex Ante Litigation Risk." *Journal of Accounting Research*, 57 (2019), 431–489.

Ivashina, V.; B. Iverson; and D. C. Smith. "The Ownership and Trading of Debt Claims in Chapter 11 Restructurings." *Journal of Financial Economics*, 119 (2016), 316–335.

Jaggia, S., and S. Thosar. "An Evaluation of Chapter 11 Bankruptcy Filings in a Competing Risks Framework." *Journal of Economics and Finance*, 43 (2019), 569–581.

Jovanovic, B., and Y. Nyarko. "A Bayesian Learning Model Fitted to a Variety of Empirical Learning Curves." *Brookings Papers on Economic Activity, Microeconomics*, (1995), 247–305.

Kalay, A.; R. Singhal; and E. Tashjian. "Is Chapter 11 Costly?" *Journal of Financial Economics*, 84 (2007), 772–796.

Kempf, E.; A. Manconi; and O. Spalt. "Learning by Doing: The Value of Experience and the Origins of Skill for Mutual Fund Managers." Working Paper, Harvard Business School (2017).

Kolay, M.; M. Lemmon; and E. Tashjian. "Spreading the Misery? Sources of Bankruptcy Spillover in the Supply Chain." *Journal of Financial and Quantitative Analysis*, 51 (2016), 1955–1990.

Korniotis, G. M., and A. Kumar. "Do Older Investors Make Better Investment Decisions?" *Review of Economics and Statistics*, 93 (2011), 244–265.

Lazear, E. "Firm-Specific Human Capital: A Skill Weights Approach." *Journal of Political Economy*, 117 (2009), 914–940.

Levitt, S. D.; J. A. List; and C. Syverson. "Toward an Understanding of Learning by Doing: Evidence from an Automobile Assembly Plant." *Journal of Political Economy*, 121 (2013), 643–681.

Ljungqvist, A.; L. Zhang; and L. Zuo. "Sharing Risk with the Government: How Taxes Affect Corporate Risk Taking." *Journal of Accounting Research*, 55 (2017), 669–707.

LoPucki, L., *Courting Failure: How Competition for Big Cases Is Corrupting the Bankruptcy Courts*. Ann Arbor, Michigan: The University of Michigan Press (2005).

LoPucki, L., and J. Doherty. "The Determinants of Professional Fees in Large Bankruptcy Reorganization Cases." *Journal of Empirical Legal Studies*, 1 (2004), 111–141.

LoPucki, L. M., and J. W. Doherty. "Bankruptcy Survival." *UCLA Law Review*, 62 (2015), 969.

Low, K.-Y. "The Effects of Industry Specialization on Audit Risk Assessments and Audit-Planning Decisions." *Accounting Review*, 79 (2004), 201–219.

Mabey, R. "The Evolving Bankruptcy Bench: How Are the "Units" Faring?" *Boston College Law Review*, 47 (2005, 105–124.

Mikhail, M. B.; B. R. Walther; and R. H. Willis. "Do Security Analysts Improve their Performance with Experience?" *Journal of Accounting Research*, 35 (1997), 131–157.

Mikhail, M. B.; B. R. Walther; and R. H. Willis. "The Effect of Experience on Security Analyst Underreaction." *Journal of Accounting and Economics*, 35 (2003), 101–116.

Norwood, K. J. "Shopping for a Venue: The Need for More Limits on Choice." *University of Miami Law Review*, 50 (1995), 267.

Posner, E. "Does Political Bias in the Judiciary Matter? Implications of Judicial Bias Studies for Legal and Constitutional Reform." *University of Chicago Law Review*, 75 (2008), 853–883.

Prendergast, C. "The Role of Promotion in Inducing Specific Human Capital Acquisition." *Quarterly Journal of Economics*, 108 (1993), 523–534.

Rachlinski, J. J.; C. Guthrie; and A. J. Wistrich. "Inside the Bankruptcy Judge's Mind." *Boston University Law Review*, 86 (2006), 1227.

Reddick, M., and N. Knowlton. "A Credit to the Courts: The Selection, Appointment, and Reappointment Process for Bankruptcy Judges." Technical Report (2013).

Seru, A.; T. Shumway; and N. Stoffman. "Learning by Trading." *Review of Financial Studies*, 23 (2010), 705–739.

Sharfman, K. "Judicial Valuation Behavior: Some Evidence from Bankruptcy." *Florida State University Law Review*, 32 (2005), 387–400.

Shaw, K., and E. P. Lazear. "Tenure and Output." *Labour Economics*, 15 (2008), 704–723.

Skeel, D. A. "Bankruptcy Judges and Bankruptcy Venue: Some Thoughts on Delaware." Penn Law Legal Scholarship Repository 719 (1998).

Thorburn, K. "Bankruptcy Auctions: Costs, Debt Recovery, and Firm Survival." *Journal of Financial Economics*, 58 (2000), 337–368.

Wasby, S. X. "Forcing Recusal as a Form of Judge Shopping." *Justice System Journal*, 18 (1995), 204–206.

Weiss, L., and K. Wruck. "Information Problems, Conflicts of Interest, and Asset Stripping." *Journal of Financial Economics*, 48 (1998), 55–97.