Biased Judges? Judge Characteristics and Bankruptcy Outcomes*

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Abstract

Using random assignment of judges, I study the effect of judge characteristics on bankruptcy outcomes of large U.S. public firms. Firms are more likely to emerge from bankruptcy under judges who grew up during the Great Depression (Depression-baby judges) and under Democratic judges, and less likely under economics-trained judges and judges who grew up in the firm's headquarters state. Firms whose cases are expected to be protracted spend less time in bankruptcy under Depression-baby and economics-trained judges, suggesting differences in case management across judges. I find no statistically significant relation between judge characteristics and post-emergence outcomes.

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1 Introduction

The bankruptcy institution governs the reallocation of resources of distressed firms. In a frictionless system, the identity of a judge ruling on a bankruptcy case should not affect its outcome. However, a growing literature suggests substantial variation in the application of bankruptcy law across judges (Bris et al. 2006; Chang and Schoar 2013; Bernstein et al. 2019). These studies identify statistically significant judge fixed effects in bankruptcy outcomes, but the sources of this heterogeneity remain less well understood. In this study, I examine whether judge characteristics help explain why firms are more likely to emerge from bankruptcy under some judges than others.

I study 1,090 Chapter 11 cases of large U.S. public firms filed between 1980 and 2022 and hand-collect biographical information on the judges assigned to these cases. Bankruptcy judges are randomly assigned to cases, an institutional feature exploited by prior studies (Chang and Schoar 2013; Dobbie and Song 2015; Bernstein et al. 2018; Iverson et al. 2023). I use this random assignment process to estimate the effects of assignment to judges with particular characteristics on bankruptcy outcomes. However, Hüther and Kleiner (2026) show that sophisticated parties, including experienced legal counsel, can influence judge assignment by timing their filings. Nevertheless, I find that observable case characteristics and the experience of debtor counsel explain little of the variation in assigned judges' characteristics, although a few imbalances remain. The outcome regressions control for these observable case characteristics.

I examine four judge characteristics related to personal experiences and political preferences. These characteristics may shape how judges weigh the interests of a bankrupt firm's workers and creditors, and thus judges' tendencies toward liquidation or emergence. The two groups' interests diverge: workers may favor emergence to preserve jobs (Graham et al. 2023), whereas secured creditors may favor liquidation to recover their claims (Ayotte and Morrison 2009; Jiang et al. 2012).

First, I consider whether judges grew up during the Great Depression and were thus exposed to widespread unemployment in their formative years (Malmendier and Tate 2005; Malmendier et al. 2011); I refer to these judges as Depression-baby judges. I hypothesize that this experience leads judges to place greater weight on the costs of job losses, making them more liquidation-averse.

Second, I consider political preferences, which may shape how judges balance debtor and creditor interests. Prior studies report that the Republican Party tends to support creditor-friendly legislation (Nash and Pardo 2012; Skeel 2014), and Rachlinski et al. (2006) and Rachlinski et al. (2017) suggest that Democratic judges tend to be more debtor-friendly than Republican judges in consumer bankruptcies. Based on this evidence, I hypothesize that Democratic judges place less weight on secured creditors' preference for liquidation, making them more liquidation-averse than Republican and nonpartisan judges, the comparison group throughout.

Third, I consider economics training, measured by attendance at a Manne economics program for federal judges or a J.D. from a law school with a strong law-and-economics culture. The field of law and economics emphasizes competition and economic efficiency in judicial decision-making (Posner 1987). For example, the field criticizes labor law and union protections that do not take economic efficiency into account (Epstein 1983; Posner 1984).[1] I hypothesize that economics-trained judges are less sympathetic to labor protections, making them more liquidation-inclined.

Finally, I consider whether the firm is headquartered in the state where the judge grew up, which I call the judge's home state. I refer to such a judge as a home judge and to such a case as a home case. Existing evidence documents home bias: economic agents show a preference for, or biased beliefs about, assets connected to their home region.[2] In

[1] Consistent with the law-and-economics agenda, Ash et al. (2026) find that circuit judges with law-and-economics training are more likely to vote against labor regulatory agencies.

[2] For a related literature on home bias, see Pool et al. (2012) for mutual fund managers' portfolio decisions; Yonker (2017) for CEOs' employment decisions; Jiang et al. (2018) for CEOs' choice of acquisition targets; Cornaggia et al. (2020) for credit analysts' rating behavior; and Lim and Nguyen (2020) for bank CEOs' lending decisions.

bankruptcy, the direction of home bias depends on which home-state stakeholders judges weigh more heavily. Home judges could be more liquidation-averse if they place greater weight on preserving home-state jobs. They could instead be more liquidation-inclined if they place greater weight on recoveries for home-state creditors, particularly secured creditors, who tend to prefer liquidation (Ayotte and Morrison 2009; Jiang et al. 2012).

The results show that the characteristics of the assigned judge affect the probability of emergence. I find that firms assigned to Depression-baby or Democratic judges are 9 and 2 percentage points more likely to emerge, respectively. Those assigned to economics-trained or home judges are 6 and 10 percentage points less likely to emerge, respectively. The magnitudes of these differences range from 3% to 14% of the unconditional emergence rate of 69%. These estimates account for other observed judge characteristics, including age and on-the-bench experience, that may be correlated with the characteristics of interest. They also account for differences across courts associated with firms' choice of where to file. Specifically, court-by-time fixed effects restrict the comparisons to cases filed in the same court and period. The home-judge specification additionally includes judge fixed effects, comparing each judge's home and non-home cases while holding constant the judge's overall tendency toward emergence.

Cross-sectional tests provide further evidence consistent with judges placing different weights on the interests of workers and creditors. Among firms with more employees, the point estimates imply a larger increase in emergence under Depression-baby judges and a larger decrease under economics-trained judges. These patterns are consistent with Depression-baby judges placing greater weight on preserving jobs and economics-trained judges placing less. The positive effect of Democratic judges increases with the share of secured debt in total debt but not with leverage, consistent with these judges counterbalancing secured creditors' preference for liquidation. For home judges, the reduction in emergence is more pronounced among geographically concentrated firms with more secured debt relative to total assets. Under the assumption that these firms'

operations and creditors are concentrated in their headquarters state, this pattern suggests that home judges weigh the interests of home-state secured creditors. By contrast, evidence on an employment channel for home judges is inconclusive.

I also examine whether these judge characteristics affect how long firms spend in bankruptcy. I find no statistically significant differences in average time across the four characteristics. Average effects can nevertheless conceal differences between cases likely to be resolved quickly and those likely to be protracted. To examine this possibility, I predict each case's length using observable firm characteristics, excluding judge characteristics. For cases expected to be protracted, the estimates suggest that firms spend less time in bankruptcy under Depression-baby and economics-trained judges than under other judges. This pattern is consistent with differences in how judges manage such cases.

Finally, I examine whether these judge effects reflect judicial bias by testing whether liquidation-averse judges allow economically unviable firms to emerge. If so, firms emerging under these judges should refile more often or perform worse after emergence. Among firms that emerge, I find no statistically significant differences in refiling within 3 years or in post-emergence return on assets across the four characteristics. These results are consistent with judge effects concentrated in marginal cases, in which the economic benefits of liquidation and emergence are similar, so that either outcome can be legally justified (Antill 2022). The test cannot, however, assess whether liquidation-inclined judges excessively liquidate viable firms: I do not observe how the assets of liquidated firms are used.

By identifying judge characteristics that help explain differences in bankruptcy outcomes, this study extends the literature documenting such differences using judge fixed effects (Bris et al. 2006; Chang and Schoar 2013; Dobbie and Song 2015; Bernstein et al. 2018; Bernstein et al. 2019; Bonfim and Nogueira 2026). My focus on variation within courts also complements studies of differences across courts in expertise and efficiency (Ayotte and Skeel 2004; Ponticelli and Alencar 2016; Iverson 2017; Ellias 2018; Li and Ponticelli 2022;

Müller 2022; Antill and Bellon 2023) and in their friendliness toward debtors or workers (LoPucki 2005; Araujo et al. 2023).

The closest study of U.S. bankruptcy judges is by Iverson et al. (2023), who examine on-the-bench experience, a characteristic distinct from the four studied here. They find that cases assigned to less experienced judges last longer and that firms emerging under these judges refile more often and have lower operating performance, although the probability of emergence does not change. In contrast, the characteristics examined here shift the probability of emergence, but I find no statistically significant differences in average duration. Among firms that emerge, I also find no statistically significant differences in refiling or post-emergence profitability. These post-emergence results differ from those of Chang and Schoar (2013), who find that firms assigned to pro-debtor judges, measured by judge fixed effects, have lower future sales and survival rates. Whereas their measure aggregates all sources of differences across judges, this study identifies observable judge characteristics that help explain those differences. In complementary work, Kim et al. (2025) find that Korean judges' gender, overseas education, and the political leaning of the regions where they attended high school affect the creditor-friendliness of their rulings. Their evidence suggests that judge characteristics matter for bankruptcy outcomes beyond the U.S. system. My study also examines post-emergence outcomes, which theirs does not.

The findings also have policy implications for the appointment of bankruptcy judges. Because judges' formative experiences, training backgrounds, and political preferences help explain differences in their liquidation tendencies, judicial appointments can shape bankruptcy outcomes. Considering diversity along these dimensions when appointing bankruptcy judges could limit the influence of a shared judicial outlook across a court's cases, complementing random assignment within the court.

2 Background on Chapter 11 and bankruptcy judges

2.1 Chapter 11 filings

Large U.S. firms in financial distress typically file a voluntary petition under Chapter 11 of the U.S. Bankruptcy Code, which allows them to reorganize under court supervision, whereas Chapter 7 provides for the liquidation of all assets. A firm can file in the bankruptcy court of any federal judicial district in which it is incorporated, has its principal place of business or principal assets, or has an affiliate with a pending bankruptcy case. Large firms may exploit this latitude to choose a favorable court among the 94 districts, a practice referred to as "forum shopping" (LoPucki and Whitford 1991; Eisenberg and LoPucki 1998–1999; Antill and Bellon 2023). Consistent with forum shopping, the District of Delaware and the Southern District of New York account for 600 of the 1,090 sample cases, and most firms filing in these two courts are headquartered in another state (Appendix Table IA.2).

The outcome of a Chapter 11 case is broadly either emergence from bankruptcy or liquidation. Under emergence, the firm continues to operate as a separate business after a reorganization plan is confirmed or after it is sold as a going concern under Section 363 of the Bankruptcy Code. Under liquidation, it shuts down or stops operating as a separate business, including when its assets are integrated into an acquirer's existing business, and employees often lose their jobs. Emergence is economically justified when the going-concern value of the business exceeds the value of its assets sold individually (Altman et al. 2019, p. 43).

2.2 The role of judges

Judges influence the bankruptcy process by, for example, deciding which motions filed by the debtor, creditors, and other parties to approve. Consistent with this active role, duration differs across judges (Bris et al. 2006; Iverson et al. 2023), judges differ in their tendencies

toward liquidation (Chang and Schoar 2013; Bernstein et al. 2019; Antill 2022; Hüther and Kleiner 2026), and debtor counsel tailor their practices to judge-specific preferences, thereby reducing duration (Goyal et al. 2026). Moreover, Iverson (2017, p. 5373) argues that judges face negligible monetary costs from allowing unviable firms to emerge, because "in practice it is nearly impossible to determine when this occurs."

Bankruptcy judges are appointed by the U.S. court of appeals for their circuit to 14-year terms in a specific bankruptcy court, and they handle all types of bankruptcy filings within that court.[3] They occasionally hear cases in other courts that have large workloads or need to avoid conflicts of interest among their own judges (Iverson et al. 2023).[4] Within a court, cases are typically assigned to judges at random.[5] Consistent with random assignment, prior studies find that judge assignment is uncorrelated with firm characteristics within a court (Chang and Schoar 2013; Bernstein et al. 2018; Bernstein et al. 2019), including in bankruptcies of large public firms, which are the focus of this study (Antill 2022; Iverson et al. 2023).

However, Hüther and Kleiner (2026) document that sophisticated parties, such as hedge funds and experienced legal counsel, can influence judge assignment by timing the filing date, a practice referred to as "judge shopping." Section 4.1 tests whether debtor counsel experience and other case characteristics predict the characteristics of the assigned judge.

[3] 28 U.S.C. § 152(a)(1).

[4] In the sample, 29 cases are assigned to visiting judges, that is, judges whose regular court at filing differs from the filing court. These cases remain in the main sample but are excluded from the eligible-judge tests in Section 4.1.

[5] If a court has multiple divisions, a case is randomly assigned to a judge within the division in which it is filed. This difference in the level of randomization is unlikely to invalidate the main identifying assumption, because Iverson et al. (2023) find that nearly all large corporate bankruptcies are filed in the main division of each court.

3 Data

3.1 Data and variable construction

3.1.1 Bankruptcy cases

The sample starts from the Florida-UCLA-LoPucki Bankruptcy Research Database (BRD) that covers filings through December 31, 2022. The BRD includes firms that filed an annual report (Form 10-K) with the Securities and Exchange Commission within 3 years prior to the bankruptcy filing and had assets worth $100 million or more, measured in 1980 dollars. This gives 1,218 cases. I drop 25 cases that are Chapter 7 at filing. When the BRD does not record the final outcome (emergence or liquidation), including for cases it still lists as pending, I determine it from court documents in Public Access to Court Electronic Records (PACER) and other sources. I drop 14 cases whose outcome I still cannot determine. Following Iverson et al. (2023) and Goyal et al. (2026), I drop 22 cases transferred to another court, 54 cases assigned to district judges rather than bankruptcy judges, and 13 cases filed in courts that have a single bankruptcy judge or do not assign cases randomly (Appendix IA.2 lists these courts). The final sample consists of 1,090 cases filed in 58 courts between 1980 and 2022. I also drop from the regressions 12 cases with missing firm-level controls (leverage or profitability).

For each case, the BRD records the filing date; the firm's assets, liabilities, and industry; the assigned judge; duration; and the final outcome (emergence or liquidation). I fill in missing accounting data from Compustat and obtain capital structure data, such as secured debt, from Capital IQ. From the debtor's counsel recorded in the BRD, I construct *Law firm experience*, the number of BRD cases filed in the prior 5 years in which the debtor's lead counsel also served as lead debtor counsel, and a supplementary indicator, *Most represented law firms* (Appendix IA.2).

3.1.2 Bankruptcy judges

I hand-collect biographical information on all 249 judges assigned to the sample cases from official court websites, legal databases, and web searches (see Appendix IA.2 for the list of sources). The data cover each judge's birth year, home state, gender, education, military service, bar admission state, and appointment date. I focus on four judge characteristics.

First, I identify judges born between 1920 and 1939 as Depression-baby judges (Malmendier and Tate 2005; Malmendier et al. 2011). If a judge's birth year is not directly observable, I infer it from the year of college graduation and assume graduation at age 22, following Greenwood and Nagel (2009). With this imputation, birth year is available for all 249 judges, of whom 65 are Depression-baby judges, including 5 with imputed birth years.

Second, I classify judges' political preferences into three categories: Democrats, Republicans, and "nonpartisans." Following Kuvvet (2021), I obtain voter registration records from 20 states and the District of Columbia. Donation records come from the U.S. Federal Election Commission (FEC). Appendix IA.2 describes both sources. Voter records classify a judge registered with the Democratic or Republican Party as a Democrat or Republican, respectively. A judge registered with the Independence Party is nonpartisan, as is a judge registered without party affiliation in New York or Pennsylvania, where this is an explicit choice. Only when voter records do not classify a judge do I use donation records[6]: a judge who donated only to the Democratic or Republican Party is classified with that party, and a judge who donated to both parties is nonpartisan. I treat political preferences as missing when neither source classifies the judge. I classify the political preferences of 127 judges: 89 Democrats, 27 Republicans, and 11 nonpartisans. *Democrat* equals one for Democratic judges and zero for Republican and nonpartisan judges.

Third, I identify judges as economics-trained if they meet at least one of two conditions: (1) the judge attended a Manne program in or before the filing year of the case, or (2)

[6]Voter records take priority because Kempf and Tsoutsoura (2021) argue that they separate political ideology from the motive to gain political influence and from social pressure better than donation records do.

following Cao (2023), the judge received a J.D. in 1970 or later from one of six law schools with a strong law-and-economics culture.[7] The Manne program, formally the Economics Institute for Federal Judges, is an intensive economics course for federal judges that the Law and Economics Center (LEC) first offered in 1976 (Butler 1999). Ash et al. (2026) measure attendance at the program between 1976 and 1999. I extend their measure to the LEC's later economics programs for judges held through 2010, the last year in the available records (Appendix IA.2). The results are similar when I count only attendance in or before 1999, as they do. Of the 249 judges, 29 are economics-trained in at least one of their cases.

Fourth, a case has a home judge when the assigned judge's home state is the firm's headquarters state at filing. A judge's home state is the state where the judge attended high school, which captures where the judge grew up; if this information is missing, I use the birth state. I obtain home state information for 205 judges.

3.2 Summary statistics

Table 1 presents summary statistics. Of all cases, 27% are assigned to Depression-baby judges. Among the 823 cases whose judge's political preferences are classified, 72% are assigned to Democratic judges, 8% to Republican judges, and the remaining 20% to non-partisan judges. Economics-trained judges are assigned to 10% of all cases, and 23% of the 994 cases with home state information have a home judge. With political preferences and home state missing more often than the other characteristics, tests of these two characteristics use smaller samples. The middle panel reports firm characteristics, measured by the closest available data to the filing date. The average firm in the sample is large, with total assets of \$5,192 million in 2023 dollars. It is also highly levered and not profitable, with an average leverage ratio of 1.20 and an average profitability of -0.05. The bottom panel reports bankruptcy outcomes. Of all firms, 69% emerge from bankruptcy, and cases

[7] These law schools are the University of Chicago, the University of Virginia, the University of Southern California, the University of Miami, Emory University, and George Mason University.

last 15.6 months on average. Of the emerged firms, 9% file for bankruptcy again within 3 years. Appendix Table IA.3 reports summary statistics at the judge level.

4 Empirical strategy

The empirical strategy estimates the effect of assignment to a judge with a given characteristic on bankruptcy outcomes. In the baseline specification, identification requires that, within a court and filing year, judge assignment be unrelated to observed and unobserved case characteristics that affect these outcomes. Section 2 describes the assignment procedures and prior evidence that motivate this assumption, as well as concerns about judge shopping. I therefore first assess the identifying assumption in the sample (Section 4.1) and then present the outcome regressions (Section 4.2).

4.1 Tests of random assignment

To complement the institutional and prior evidence in Section 2, I conduct two tests of the identifying assumption following Iverson et al. (2023). First, I run balance tests, which examine whether observable case characteristics predict the characteristics of the assigned judge. Table 2 reports regressions of each judge characteristic on firm size, leverage, profitability, the number of affiliated entities filing in the case, and the prepackaged-case indicator. All balance-test specifications include court-by-year and industry fixed effects, so the tests compare cases filed in the same court and year, after accounting for differences across industries.[8]

The balance tests also examine whether assignment is related to the experience of debtor counsel, because Hüther and Kleiner (2026) show that sophisticated parties, including experienced legal counsel, can influence judge assignment by timing bankruptcy

[8]The number of observations is smaller than the 1,090 cases reported in Table 1 because Stata's *reghdfe* command drops singleton observations, which are the sole member of a fixed-effect group (for example, a court-by-year cell containing only one case). These observations provide no within-group variation for estimating the coefficients and are removed to avoid bias in the cluster-robust standard errors.

filings (Section 2).[9] Accordingly, the even-numbered columns of Table 2 add *Law firm experience*, an ex ante measure of the recent bankruptcy experience of debtor counsel. Specifically, it is the number of BRD cases filed during the preceding 5 years in which the debtor's lead counsel also served as lead debtor counsel, similar to the ex ante law firm experience measures used in Antill and Bellon (2023) and Hüther and Kleiner (2026).

Table 2 shows that most estimated associations between case characteristics and the characteristics of the assigned judge are statistically insignificant. The within R^2 ranges from 0.003 to 0.008, indicating that the included characteristics explain less than 1% of the variation remaining after the fixed effects are absorbed. *Law firm experience* is statistically insignificant in every specification. The estimates are also small: a one-standard-deviation increase in *Law firm experience* (about 10 cases) changes the probability of assignment to a judge with any of the four characteristics by about 1 percentage point or less. Two other case characteristics are exceptions: prepackaged cases are more likely to be assigned to Depression-baby and Democratic judges, and more profitable firms are less likely to be assigned to Democratic judges. *Prepack* and *Profitability* enter as firm-level controls in the outcome regressions described in Section 4.2. Section 7.2 also excludes prepackaged cases and shows that the estimates for Depression-baby and Democratic judges without judge-level controls stay close to their baseline values.

As a robustness check, Appendix Table IA.5 repeats the balance tests using *Most represented law firms*, an indicator for whether the debtor's counsel team includes a law firm appearing in the rankings of leading bankruptcy counsel in Goyal et al. (2026).[10] This measure is statistically insignificant at the 5% level for all four judge characteristics; for economics-trained judges, its coefficient is negative and statistically significant at the 10% level. In unreported regressions, the outcome estimates reported in Section 5 are similar

[9] I use debtor counsel experience rather than hedge fund participation because coverage of hedge fund participation is limited, particularly for cases filed before 1990.

[10] See Appendix IA.2 for the specific law firms and the construction of this indicator. The rankings use filings from 1996 to 2019 and therefore do not measure experience strictly before each filing throughout the sample. I use this indicator as a supplementary measure of counsel sophistication.

when I add *Most represented law firms* as a control.

Second, I run eligible-judge tests, which examine whether judges with the characteristics examined in this study are more or less likely to receive a case among the eligible judges in the filing court. I construct case–judge pairs using judges' appointment and retirement years, excluding visiting and district judges and cases with 2 or fewer eligible judges.[11] The dependent variable, *Assigned*, equals one if the eligible judge is assigned to the case and zero otherwise. Each specification includes case fixed effects, holding constant all case characteristics and comparing eligible judges for the same case.

Table 3 shows that the estimated assignment probabilities of Democratic, economics-trained, and home judges are not statistically different from those of other eligible judges. The exception is Depression-baby judges, who are 2.6 percentage points less likely to be assigned a case. This difference is statistically significant at the 10% level and amounts to approximately 18% of the unconditional assignment probability of 14.2% (= 0.026/0.142). The within R^2 is at most 0.001 across the four specifications. Together, the tests show that observable case characteristics explain little of the variation in the characteristics of assigned judges, although they reveal a few imbalances.

These findings do not contradict Hüther and Kleiner (2026), who document that sophisticated parties can influence judge assignment and that cases represented by experienced legal counsel are assigned to judges who converted fewer past Chapter 11 cases to Chapter 7. Their tests characterize judges by their past rulings, whereas the tests here examine assignment with respect to specific observable judge characteristics. Because these characteristics need not capture all differences in judges' liquidation tendencies, sophisticated parties could influence assignment among judges with similar observed characteristics but different histories of rulings. Consistent with this distinction, *Law firm experience*, a measure close to theirs, does not predict the characteristics of the assigned judge in Table 2.[12]

[11] The eligible-judge roster includes only judges who appear in the BRD.

[12] The samples also differ: Hüther and Kleiner (2026) study the universe of U.S. Chapter 11 cases filed

4.2 Empirical specifications

The tests in Section 4.1 show little systematic sorting on the judge characteristics examined in this study, although they reveal a few imbalances. Under the identifying assumption, I estimate the following ordinary least squares (OLS) regression:

$$Y_{ircjt} = \alpha_r + \alpha_{ct} + \beta JudgeChar_{jt} + \delta Z'_{jt} + \gamma X'_{it} + \varepsilon_{ircjt}, \tag{1}$$

where Y_{ircjt} is the bankruptcy outcome of firm i in industry r that files for bankruptcy in court c in year t and whose case is assigned to judge j. The outcome is an indicator equal to one if the firm emerges from bankruptcy, the log of the number of months from filing to case disposition, or, for firms that emerge, *Refile* or *ROA (post)*.

The coefficient of interest is β, which captures the effect of assignment to a judge with a given characteristic. Depending on the test, $JudgeChar_{jt}$ is *Depression baby*, *Democrat*, or *Economics-trained* for judge j as of filing year t; *Economics-trained* varies with the filing year because it depends on whether the judge had attended a Manne program by then. In specifications with judge-level controls, Z'_{jt} contains *Male*, *Military*, *Log(Months as judge)*, and *Log(Judge age)*, which measure gender, military service, on-the-bench experience, and judge age at filing; Iverson et al. (2023) find on-the-bench experience relevant for explaining case duration.

Equation (1) includes two sets of fixed effects. First, court-by-year fixed effects (α_{ct}) absorb differences in outcomes across courts and filing years.[13] Such differences can arise because firms choose the court in which they file, through forum shopping (Section 2).[14] With these fixed effects, the estimates rest on judge-level variation in bankruptcy

between 2010 and 2020, including those of private firms, whereas this study covers large public firms that filed between 1980 and 2022.

[13] Section 7.3 replaces court-by-year fixed effects with court-by-period fixed effects, which use more of the variation in courts with few filings.

[14] For example, LoPucki (2005) argues that bankruptcy courts such as Delaware and the Southern District of New York are more debtor-friendly.

outcomes, which is the subject of this study, rather than on court-level variation. Second, industry fixed effects (α_r), based on the Fama–French 12 industry classification, absorb industry-level differences in bankruptcy outcomes. I cluster standard errors at the court level, following Antill (2022) and Iverson et al. (2023). Equation (1) thus compares the bankruptcy outcomes of firms that file in the same court and year but whose cases are assigned to different judges.

Following Iverson et al. (2023), I include the following firm-level controls in X'_{it}, measured with the available data closest to filing year t: (1) the log of the book value of assets (in 2023 dollars); (2) the leverage ratio; (3) profitability, measured as EBIT divided by total assets; (4) the log of the number of affiliated entities filing in the case; and (5) *Prepack*, an indicator for prepackaged cases.

For home judges, I modify Equation (1) and estimate the following OLS regression:

$$Y_{ircjt} = \alpha_r + \alpha_{ct'} + \alpha_j + \beta HomeJudge_{ij} + \delta Z'_{jt} + \gamma X'_{it} + \varepsilon_{ircjt}, \tag{2}$$

where $HomeJudge_{ij}$ is an indicator equal to one if firm i's case is assigned to judge j whose home state is the firm's headquarters state. The coefficient of interest, β, captures the effect of assignment to a home judge.

In Equation (2), I add judge fixed effects (α_j) to control for judges' overall liquidation tendency. The judge fixed effects ensure that I isolate within-judge variation.[15] In addition, unlike Equation (1), Equation (2) replaces the court-by-year fixed effects with court-by-decade fixed effects ($\alpha_{ct'}$) to ensure I have sufficient variation in the data: court-by-year fixed effects are highly restrictive in that they require a judge to be assigned to both home and non-home cases within the same court and year.[16] Equation (2) ensures that the identification strategy on home bias compares the bankruptcy outcomes of firms that filed

[15] Dobbie and Song (2015) and Bernstein et al. (2019) find that judge leniency with respect to emergence is not time-varying. Moreover, the judge fixed effects absorb *Male* and *Military*, which are time-invariant within a judge. Z'_{jt} thus contains only *Log(Months as judge)* and *Log(Judge age)* in this specification.

[16] In the full sample, the average (median) judge is assigned 4.4 (2) cases.

in the same court and decade and are assigned to the same judge, but whose headquarters states may or may not be the same as the judge's home state.

5 Judge characteristics and bankruptcy outcomes

This section asks whether bankruptcy outcomes depend on the characteristics of the judge assigned to the case. I examine three outcomes in turn: emergence from bankruptcy (Section 5.1), duration in bankruptcy (Section 5.2), and post-emergence outcomes (Section 5.3). Emergence is the main outcome, duration shows how judges manage cases along the way, and post-emergence outcomes test whether the judge effects on emergence reflect judicial bias.

5.1 Emergence from bankruptcy

I begin with a descriptive analysis of how emergence rates differ across the characteristics of the assigned judge. Figure 1 shows that the average emergence rate is 76% for firms whose cases are assigned to Depression-baby judges, compared with 67% for other judges. The corresponding rates are 71% for Democratic judges, 65% for nonpartisan judges, and 67% for Republican judges. Emergence is less common under economics-trained judges (63%, compared with 70% for other judges) and home judges (64%, compared with 71% for non-home judges). The dashed horizontal lines mark the full-sample mean of 69%. These descriptive differences motivate the regression analysis, which accounts for firm characteristics and fixed effects.

Table 4 shows results from the linear probability regressions in Equations (1) and (2), where the dependent variable is an indicator equal to one if a firm emerges from bankruptcy.[17] Under the identifying assumption in Section 4, the estimates capture the effect of assignment to a judge with a given characteristic.

[17] As in Table 2, singleton observations are excluded, reducing the estimation samples relative to the 1,090 cases reported in Table 1.

Firms whose cases are assigned to Depression-baby judges are 13 percentage points more likely to emerge (Column 1). The estimate falls to 9 percentage points after controlling for gender, military service, on-the-bench experience, and judge age (Column 2). Both estimates are statistically significant at the 5% level, and the latter amounts to approximately 13% of the unconditional emergence rate (= 0.09/0.69). The positive coefficient on *Depression baby* thus survives the control for judge age, mitigating the concern that the estimate in Column 1 simply captures the effect of older judges.

Firms whose cases are assigned to Democratic judges are 3 percentage points more likely to emerge, relative to Republican and nonpartisan judges (Column 3). This estimate is statistically significant at the 1% level. Adding judge-level controls reduces the estimate to 2 percentage points (Column 4), approximately 3% of the unconditional emergence rate, and it is no longer statistically significant. The baseline evidence for greater emergence under Democratic judges is therefore sensitive to the inclusion of other judge characteristics. Section 7.1 and Appendix Table IA.7 show that this sensitivity comes from controlling for judge age: in the estimation sample, no Republican judge is older than 63 at filing, so the data cannot separate political preferences from age among older judges. When I restrict the sample to judges aged 63 or younger, where both Democratic and Republican judges appear, I find a similar positive estimate of 2 percentage points, which is weakly significant at the 10% level with all judge-level controls.

Firms whose cases are assigned to economics-trained judges are 6 percentage points less likely to emerge in Column 6, which includes judge-level controls. This estimate is statistically significant at the 5% level and amounts to approximately 9% of the unconditional emergence rate. Without judge-level controls, the estimate is 8 percentage points but statistically insignificant (Column 5). These estimates are consistent with a greater liquidation tendency among economics-trained judges.

Firms whose cases are assigned to home judges are 10 percentage points less likely to emerge in both Columns 7 and 8, approximately 14% of the unconditional emergence rate.

These specifications include judge fixed effects and therefore use within-judge variation in whether the firm is headquartered in the judge's home state. While both estimates are significant at the 10% level, their economic magnitude is significant.

There are, however, two alternative interpretations of the home-judge result: the estimate could reflect local filing or judges' professional ties rather than where judges grew up. First, for judges who sit in their home state, home cases coincide with local filings—cases filed in the firm's headquarters state. For these judges, the home-judge comparison therefore also contrasts local and non-local firms, which may differ in ways that affect emergence. Second, home cases may involve professional ties: judges who previously practiced law in their home state may know the firm's managers, lenders, or counsel. To examine these alternative interpretations, I add *HQ and court in same state* and *Judge worked in HQ state* to the baseline home-judge specification. Appendix Table IA.8 shows that the home-judge estimate is larger in magnitude than the baseline estimate, although it is no longer statistically significant when both are included. These results suggest that local filing and professional ties do not fully account for the association between emergence and where judges grew up.

One concern in interpreting Table 4 is that judge characteristics may correlate with each other; Table 5 addresses this concern by including multiple characteristics jointly. I include *Depression baby*, *Democrat*, and *Economics-trained* in pairs (Columns 1–3) and then jointly (Column 4), before sequentially adding judge-level controls (Columns 5–7).[18] All specifications retain the firm-level controls and industry and court-by-year fixed effects. The joint specifications require nonmissing information on each included characteristic. Political preferences are the principal constraint: specifications including *Democrat* use 664 observations, compared with 830 when only *Depression baby* and *Economics-trained* enter. Comparisons of the *Depression baby* and *Economics-trained* estimates with Table 4

[18] I do not include *Home judge* in these joint specifications because its estimation requires judge fixed effects, which absorb *Depression baby* and *Democrat*, both constant for each judge, and most of the variation in *Economics-trained*, which changes only for the few judges who attended a Manne program after one of their earlier sample cases.

therefore also reflect differences in sample composition.

The estimates for Depression-baby and economics-trained judges largely persist in the joint specifications. Even in the fully controlled specification in Column 7, the estimates stay stable for Depression-baby and economics-trained judges. These results suggest that neither characteristic simply proxies for the other observed judge characteristics. The estimate for Democratic judges is more sensitive to the specification. In Column 1, including *Depression baby* reduces the *Democrat* coefficient from 0.03 in Table 4 to −0.01, which is statistically insignificant. In contrast, when *Democrat* enters with *Economics-trained* but without *Depression baby* in Column 3, its coefficient is 0.05 and significant at the 10% level. Conditioning on the older birth cohort thus attenuates the estimated Democratic-judge effect, resembling the attenuation in Table 4, Column 4, where judge-level controls, including judge age, reduce the coefficient to 0.02. As discussed above and in Section 7.1, this sensitivity is consistent with the absence of Republican judges older than 63 in the sample. The *Democrat* coefficient becomes smaller when all three characteristics enter jointly (Column 4) and rises to 0.04–0.05 once judge-level controls enter (Columns 5–7). The joint specifications therefore preserve the main patterns for Depression-baby and economics-trained judges, while the evidence for Democratic judges remains more sensitive to the treatment of other judge characteristics.

5.2 Duration in bankruptcy

Next, I examine whether assignment to judges with different liquidation tendencies affects duration. I first estimate Equation (1), and Equation (2) for home judges, with the log of the number of months from filing to case disposition as the dependent variable. Appendix Table IA.6 reports the estimates. The coefficients on the four judge characteristics range from −0.09 to 0.03 and are statistically indistinguishable from zero. These estimates

provide little evidence of an average difference in duration across judge characteristics.[19]

Average effects can nevertheless conceal differences in how judges handle cases with different expected durations. Cases expected to be resolved quickly leave judges little room to intervene; cases expected to be protracted leave more. I use predicted duration to distinguish these cases. If judges with a characteristic shorten protracted cases more than other judges do, the estimated effect of assignment to them should decline as predicted duration rises.

To examine this heterogeneity, I predict log duration using firm-level controls and industry fixed effects, excluding judge characteristics. I refer to this prediction, $\widehat{Duration}_i$, as predicted duration; it is measured in log months and constructed on a leave-one-out basis, so each case's prediction comes from a regression that excludes that case. I then add predicted duration and its interaction with each judge characteristic to the corresponding baseline duration regression. The interaction coefficient measures how the judge effect changes with predicted duration. Given that the prediction is nearly collinear with the firm controls and industry fixed effects included again in this regression, I do not interpret its main coefficient or the individual firm-control coefficients separately.

Table 6 shows that the interactions for Depression-baby and economics-trained judges are negative: -0.07 and -0.17, statistically significant at the 5% and 1% levels, respectively. In terms of economic magnitude, each doubling of predicted duration in months lowers duration under these judges, relative to other judges, by about 4.5% and 11% ($\exp(\beta_3 \times \log 2) - 1$), or about 0.6 and 1.6 months at the estimation-sample mean of 14.4 months. The estimated effects of assignment to these judges thus become more negative for cases expected to be protracted. The interactions for Democratic and home judges are also negative, but statistically indistinguishable from zero.

Figure 2 plots the estimated judge effects across predicted duration. For each binary characteristic, the effect is $\beta_1 + \beta_3 \widehat{Duration}_i$. Panels (a) and (c) show that the point estimates

[19] This finding is consistent with Antill (2022), who finds that judges' liquidation tendencies do not affect unconditional duration (Table B.4).

for Depression-baby and economics-trained judges become negative among cases expected to be protracted. For the median case that is not prepackaged, with a predicted duration of about 16 months, the estimates imply roughly 5% and 9% shorter duration, respectively ($\exp(\beta_1 + \beta_3 \widehat{Duration}_i) - 1$), although neither estimate is statistically significant at the 5% level at that point. The precision differs across the plotted range: the confidence interval for Depression-baby judges includes zero throughout, despite the statistically significant interaction, whereas the effect for economics-trained judges is significantly negative toward the upper end. For economics-trained judges, the effect is also significantly positive at the low end, indicating longer duration for cases expected to be resolved quickly.

In Panel (b) of the same figure, I separate non-Democratic judges into Republican and nonpartisan groups to examine heterogeneity across more detailed categories of political preferences. Democratic judges serve as the reference group. The gap between Democratic and Republican judges widens as predicted duration rises: cases expected to be protracted last less time under Democratic judges than under Republican judges, and the difference is statistically significant toward the upper end of the range. In contrast, estimated durations under Democratic and nonpartisan judges differ little throughout. These comparisons reveal differences not apparent from the pooled Democratic indicator. Finally, Panel (d) provides no statistically significant evidence of a home-judge effect over the plotted range.

These duration patterns do not mirror the emergence results. Depression-baby and economics-trained judges move emergence in opposite directions (Section 5.1), yet the estimated effects of assignment to either become more negative as predicted duration rises. I therefore interpret the duration results as differences in case management rather than as judges accelerating the outcomes they prefer.

5.3 Post-emergence outcomes

Finally, I examine whether judge characteristics are linked to better or worse post-emergence outcomes. The results so far show differences in emergence probabilities across judge characteristics and in duration effects across cases with different expected durations. Given the presence of these judge effects, it is natural to ask whether they reflect judicial *bias* by evaluating firms' post-emergence performance. For example, if judges with liquidation-averse characteristics allow clearly unviable firms to emerge, I expect the average post-emergence outcomes of firms whose cases are assigned to such judges to be worse.[20]

Table 7 shows no statistically significant effects of the four judge characteristics on post-emergence outcomes. The dependent variables are an indicator equal to one if a firm refiles within 3 years of emergence (*Refile*) and return on assets after emergence (*ROA (post)*). The estimated effects on refiling are 2, 3, 3, and 1 percentage points for Depression-baby, Democratic, economics-trained, and home judges, respectively, against a sample mean of 9%. The corresponding estimates for post-emergence ROA are -0.10, 0.03, 0.08, and -0.01, against a sample mean of 0.19. All eight estimates are statistically indistinguishable from zero, although their precision is limited. For example, the 95% confidence interval for the Democratic-judge effect on refiling ranges from -4.2 to 10.4 percentage points, so economically meaningful effects cannot be ruled out. The absence of statistically significant differences is consistent with Antill (2022), who argues that judges' liquidation tendency is more likely to affect cases where multiple forms of bankruptcy exits can be legally justified. The evidence is thus consistent with judge effects concentrated in marginal cases, where the economic benefits of liquidation and emergence are similar.

Figure 3 plots average post-emergence outcomes by judge assignment. Panel (a) shows higher average refiling rates among firms whose cases are assigned to Depression-baby

[20] The ideal test of post-bankruptcy outcomes would also examine asset utilization of liquidated firms. However, due to data limitations, I observe outcomes of emerged firms only.

or Democratic judges, while differences by economics training and home judge assignment are smaller. Panel (b) shows similar average post-emergence ROA across birth cohorts, economics training, and home judge assignment, with larger differences across political-preference groups. These unconditional comparisons should be read alongside the multivariate estimates in Table 7, which are statistically insignificant for all four judge characteristics.

6 Heterogeneity in case characteristics

In Section 5.1, I find evidence that judge characteristics affect the probability of emergence for bankrupt firms. Next, I test whether the effects of judge characteristics on the probability of emergence are more pronounced in some cases than in others. I perform cross-sectional tests to investigate whether judge preferences correlate with firm characteristics in determining case outcomes, in line with the earlier findings.

6.1 Labor channel

Do the liquidation tendencies of Depression-baby and economics-trained judges reflect their attitudes toward preserving employment in bankrupt firms? The hypotheses for both characteristics rest on how judges weigh job losses. If this labor-based channel drives their liquidation tendencies, the positive effect of Depression-baby judges and the negative effect of economics-trained judges on emergence should both be larger in magnitude among firms with more employees: liquidating these firms would carry higher perceived costs for Depression-baby judges and higher perceived benefits for economics-trained judges. To test this prediction, I split the sample at the median of 2,700 employees and re-estimate the emergence regressions with judge-level controls from Table 4 separately for firms with below-median employment (low-employment firms) and above-median employment (high-employment firms). I restrict this test to Depression-baby and

economics-trained judges because the hypotheses for the other two characteristics do not concern the firm's total employment: the hypothesis on political preferences rests on attitudes toward creditors (Section 6.2), and the home-judge hypothesis concerns creditors and employees *based* in the judge's home state (Section 6.3). Appendix Table IA.9 reports the split for all four characteristics; none of the estimates for Democratic or home judges is statistically significant.

Table 8 reports the results. Firms whose cases are assigned to Depression-baby judges are 5 percentage points more likely to emerge among low-employment firms and 10 percentage points more likely among high-employment firms (Columns 1–2). The latter estimate amounts to approximately 14% of the 69% unconditional emergence rate (= 0.10/0.69). While neither estimate is statistically significant, the larger point estimate among high-employment firms is consistent with the labor-based prediction.

Firms whose cases are assigned to economics-trained judges are 10 percentage points less likely to emerge among high-employment firms (Column 4), also approximately 14% of that rate. This estimate is statistically significant at the 1% level and larger than the full-sample estimate of 6 percentage points (Table 4, Column 6). Among low-employment firms, the estimate is -4 percentage points and statistically insignificant (Column 3).

For both characteristics, the point estimates are larger in magnitude among high-employment firms, as the labor-based hypothesis predicts. Only the economics-trained estimate among high-employment firms is statistically significant, however. The results are therefore consistent with a labor-based channel, particularly for economics-trained judges, but do not establish that the judge effects differ with firm employment.

6.2 Creditor channel

If Democratic judges are pro-debtor, and thus more liquidation-averse, the effect of assignment to them may be larger when creditors have more at stake, that is, when the firm is highly levered. It may also depend on the composition of debt: secured creditors

tend to prefer liquidation over emergence (Ayotte and Morrison 2009; Jiang et al. 2012), so Democratic judges' liquidation aversion may counterbalance this preference more strongly when secured debt accounts for a larger share of total debt.

To test these predictions, I add to the baseline specification, Equation (1), a debt measure and its interaction with *Democrat*: *Leverage ratio* in Column 1 and *Secured / total debt* in Columns 2 and 3. Both predictions imply a positive coefficient on the interaction.[21]

Table 9 provides no evidence that the effect of assignment to Democratic judges varies with leverage (Column 1): the coefficient on *Democrat* × *Leverage ratio* is 0.00. By contrast, the coefficient on *Democrat* × *Secured / total debt* is 0.16 without judge-level controls and 0.17 with them, statistically significant at the 1% and 5% levels, respectively (Columns 2 and 3). Due to the interaction, the coefficient on *Democrat* (−0.07 in Column 3) measures, for firms without secured debt, the difference in the probability of emergence between firms whose cases are assigned to Democratic judges and those whose cases are assigned to Republican and nonpartisan judges. Among firms whose cases are assigned to Republican and nonpartisan judges, those with a larger share of secured debt are less likely to emerge (the coefficient on *Secured / total debt* is −0.12), consistent with secured creditors' preference for liquidation; under Democratic judges, the interaction offsets this decline. These results suggest that Democratic judges are more liquidation-averse where secured debt accounts for a large share of total debt, consistent with their counterbalancing secured creditors' preference for liquidation.

6.3 Why do home judges rule differently?

The baseline estimates suggest that home judges are more liquidation-inclined (Table 4, Columns 7–8) but do not reveal which home-state interests drive this tendency. If home judges weigh the interests of home-state creditors, they should be more liquidation-

[21] Unlike in Section 6.1, I use an interaction rather than a sample split because relatively few cases have a judge with a classified political preference.

inclined when these creditors have a larger stake in the firm. This tendency should be stronger when the creditors are secured, because secured creditors tend to prefer liquidation (Section 6.2). Conversely, if they weigh home-state employment, they should be more liquidation-averse when the firm employs more workers in their home state. The locations of the firm's creditors and employees are not observed, so I approximate home-state stakeholders' interests by interacting *Home judge* with the firm's geographic concentration and with measures of its debt and employment:

$$\begin{aligned} Emerge_{ircjt} = {} & \alpha_r + \alpha_{ct'} + \alpha_j + \beta_1 HomeJudge_{ij} + \beta_2 GeoConc_i \\ & + \beta_3 StakeholderInterest_i + \beta_4 HomeJudge_{ij} \times GeoConc_i \\ & + \beta_5 HomeJudge_{ij} \times StakeholderInterest_i + \beta_6 GeoConc_i \times StakeholderInterest_i \\ & + \beta_7 HomeJudge_{ij} \times GeoConc_i \times StakeholderInterest_i \\ & + \gamma X'_{it} + \delta Z'_{jt} + \varepsilon_{ircjt}, \end{aligned} \tag{3}$$

where $GeoConc_i$ is *Geographic concentration*, the Herfindahl index of U.S. state-name mentions in firm i's last 10-K before filing; a higher value indicates operations concentrated in fewer states.[22] $StakeholderInterest_i$ is *Leverage ratio* or *Secured debt / total assets* for creditors' stake, and *Log(No. of employees)* for employment. A home judge's home state is, by definition, the firm's headquarters state. *Home judge* × *GeoConc* thus proxies for the concentration of the firm's operations in the judge's home state, assuming that geographically concentrated firms operate mainly in their headquarters state. The triple interaction, *Home judge* × *GeoConc* × *StakeholderInterest*, then proxies for the stake of home-state creditors or employees, assuming further that these stakeholders are located near the firm's operations. The second assumption is plausible for both groups: state names in 10-Ks often describe the locations of stores or manufacturing facilities (García and Norli 2012), where the firm's employees work, and banks are more likely to lend to geographically proximate

[22] Following García and Norli (2012), I construct the measure from the sample firms' 10-K filings from 1994 to 2022. Coverage is limited by the availability of electronic 10-Ks in EDGAR, which begins in 1994.

businesses (Sufi 2007). Controls and fixed effects are the same as in Table 4, Column 8. The coefficient of interest is β_7: the creditor channel predicts $\beta_7 < 0$ when $StakeholderInterest_i$ measures creditors' stake, and the employment channel predicts $\beta_7 > 0$ when it measures employment.

In Table 10, the home-judge effect on emergence is more negative for more geographically concentrated firms. The coefficient on *Home judge* × *GeoConc* is −0.40 and statistically significant at the 10% level (Column 1), suggesting that an increase in concentration is associated with a stronger negative home-judge effect. This pattern alone does not distinguish the creditor channel from the employment channel, because concentration raises the home-state stakes of both creditors and employees.

The stakeholder interactions provide more specific evidence. Neither *Home judge* × *Leverage ratio* nor its triple interaction with *GeoConc* is statistically significant, and both coefficients are close to zero (Columns 2–3). The triple interaction with *Secured debt / total assets*, in contrast, is −0.78 and statistically significant at the 10% level (Column 4). As with Democratic judges (Section 6.2), the evidence of heterogeneity comes from secured debt rather than from overall leverage. The employment triple interaction has the sign the employment channel predicts (+0.11) but is statistically insignificant (Column 5). Under the geographic proxy, these results suggest that home judges weigh the interests of home-state secured creditors, whereas the evidence on an employment channel is inconclusive.

7 Robustness checks

Three choices in the baseline analysis could affect the results: controlling for judge age in the Democratic-judge regressions, including prepackaged cases, and using court-by-year fixed effects. I examine each in turn.

7.1 Democratic judges and judge age

The estimate for Democratic judges in Table 4 falls from 0.03 to a statistically insignificant 0.02 when judge-level controls enter. Appendix Table IA.7 examines which control drives this decline. Column 1 omits judge-level controls, and Columns 2–4 add them sequentially, with gender and military service entering together in Column 2. The estimate for *Democrat* remains positive and statistically significant at the 1% level in Columns 1–3, ranging from 0.03 to 0.05, but falls to 0.02 once *Log(Judge age)* enters in Column 4.

The decline may reflect limited overlap in judge age across political-preference groups. In the unrestricted sample (Columns 1–4), no Republican judge is older than 63 at filing. Variation in judge age above 63 therefore comes only from Democratic and nonpartisan judges. The data thus cannot separate political preferences from age among older judges. To examine whether the estimate remains sensitive to the age controls within the range in which both Democratic and Republican judges appear, Columns 5 and 6 restrict the sample to cases whose judge is at most 63 years old at filing. In this sample, firms whose cases are assigned to Democratic judges are 2 percentage points more likely to emerge, approximately 3% of the unconditional emergence rate. The estimate is statistically significant at the 10% level with all judge-level controls (Column 5) and at the 5% level when the two age-related controls are dropped (Column 6).

Within the overlapping age range, the estimate for Democratic judges therefore does not depend on the age controls. It is smaller than the full-sample estimate with the same controls (0.02 in Column 6, compared with 0.05 in Column 2), which may partly reflect differences in sample composition. These results suggest a positive, if modest, effect of assignment to Democratic judges on emergence.

7.2 Excluding prepackaged cases

Table 2 shows that prepackaged cases are more likely to be assigned to Depression-baby and Democratic judges. Because firms that file prepackaged cases are more likely to emerge (Table 4), this imbalance may inflate the positive estimates for these judges. Although *Prepack* enters the baseline specification as a control, it absorbs only the average difference in emergence between prepackaged and other cases. Excluding prepackaged cases removes this imbalance altogether.

In Appendix Table IA.10, I repeat the analysis in Table 4 after excluding the 377 prepackaged cases, which reduces the estimation samples by more than 40%. Without judge-level controls, the estimates for Depression-baby and Democratic judges stay close to their baseline values, although the Democratic estimate is no longer statistically significant. These estimates show none of the decline expected if the assignment of prepackaged cases drove the baseline results. With judge-level controls, both estimates fall and are statistically insignificant, but they are imprecise: their standard errors are about three and four times their baseline values, respectively; the Democratic regressions have only 12 court clusters; and the 95% confidence interval for Depression-baby judges, from -21 to 25 percentage points, includes the baseline estimate. These specifications cannot distinguish a zero effect from one as large as the baseline. By contrast, the estimates for economics-trained and home judges become larger and are statistically significant at the 1% level in both specifications, so excluding prepackaged cases strengthens the evidence for these judges. For Depression-baby and Democratic judges, the evidence from this test is mixed: the estimates without judge-level controls are close to the baseline, but those with judge-level controls are inconclusive.

7.3 Alternative specifications

One might worry that court-by-year fixed effects are too restrictive, despite their advantages in addressing concerns about forum shopping. In particular, court-by-year fixed effects absorb variation in outcomes in smaller courts that have only one or two cases in the estimation sample. I address this concern by replacing court-by-year fixed effects with court-by-period fixed effects, which compare firms that file in the same court within a period of several years. The periods follow Goyal et al. (2026), who divide 1996–2019 into periods of five to seven years to reflect broad shifts in bankruptcy filing conditions. I extend this classification to the longer sample, which yields the periods 1980–1984, 1985–1989, 1990–1995, 1996–2002, 2003–2009, 2010–2014, 2015–2019, and 2020–2022. For home judges, court-by-period fixed effects replace court-by-decade fixed effects. Because the periods are shorter than decades, this change tightens rather than relaxes the home-judge specification.

Appendix Table IA.11 repeats the analyses of emergence (Panel A), the interaction with predicted duration (Panel B), and post-emergence outcomes (Panel C). The results for economics-trained and home judges are similar to the baseline: firms whose cases are assigned to these judges remain less likely to emerge, and the interaction between *Economics-trained* and predicted duration remains negative and statistically significant. The results for Depression-baby and Democratic judges are weaker. The Depression-baby estimate for emergence remains statistically significant without judge-level controls but not with them, and its interaction with predicted duration is 0.01 and statistically indistinguishable from zero. The estimates for Democratic judges on emergence are close to zero and statistically insignificant. As in the baseline, post-emergence outcomes show no statistically significant relation with judge characteristics. The evidence for economics-trained and home judges and the null post-emergence results are therefore robust to this alternative specification, whereas the evidence for Depression-baby and

Democratic judges is sensitive to it. Court-by-period fixed effects also retain cases that drop as singletons under the baseline court-by-year fixed effects. As a result, the estimation samples also differ, and this sensitivity can reflect either the alternative fixed effects or the added cases.

I nevertheless retain court-by-year fixed effects in the baseline specification for Depression-baby, Democratic, and economics-trained judges. Judges are randomly assigned among those sitting in the court at the time of filing, so court-by-year fixed effects compare firms that face the same pool of judges. Court-by-period fixed effects also compare firms that file in different years of the same period, when the court's bench and filing conditions may differ, and this variation does not come from random assignment. The cost is that the baseline specification relies mainly on larger courts, which hear several cases each year.

8 Conclusion

Exploiting random assignment of judges within bankruptcy courts, I find that judge characteristics affect whether large U.S. public firms emerge from Chapter 11. Firms assigned to Depression-baby and Democratic judges are more likely to emerge, whereas those assigned to economics-trained and home judges are less likely to emerge. The home-judge effect is stronger among geographically concentrated firms with more secured debt relative to total assets. If these firms' operations and creditors are located mainly in their headquarters state, this pattern suggests that home judges give greater weight to home-state secured creditors.

I find no statistically significant differences in average duration across judge characteristics. However, the estimated effects of assignment to Depression-baby and economics-trained judges become more negative as predicted duration rises. This pattern is consistent with differences in the management of cases expected to be protracted.

Among firms that emerge, I find no statistically significant differences in refiling or post-emergence profitability. These results are consistent with judge effects concentrated in marginal cases, in which liquidation and emergence offer similar economic benefits, so that either outcome can be justified. The analysis cannot, however, assess whether liquidation-inclined judges excessively liquidate viable firms, because I do not observe how liquidated firms' assets are used.

The findings suggest that judicial appointments can shape bankruptcy outcomes. Judges' formative experiences, training backgrounds, and political preferences help explain differences in their liquidation tendencies. Considering diversity along these dimensions when the U.S. courts of appeals appoint bankruptcy judges could limit the influence of a shared judicial outlook across a court's cases, complementing random assignment within the court.

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Figure 1: Emergence by assigned judges' characteristics

This figure plots emergence rates conditional on assignment to Depression-baby judges, judges in each political-preference category, economics-trained judges, and home judges. The dashed horizontal lines mark the full-sample mean. *Depression baby* indicates judges born between 1920 and 1939. *Democrat*, *Nonpartisan*, and *Republican* indicate the three categories of judges' political preferences, classified from voter registration and political donation records (Section 3.1.2). *Economics-trained* indicates judges who attended a Manne program in or before the filing year, or received a J.D. in 1970 or later from one of six law schools with a strong law-and-economics culture. *Home judge* indicates whether a judge's home state is the same as the state where the firm is headquartered.

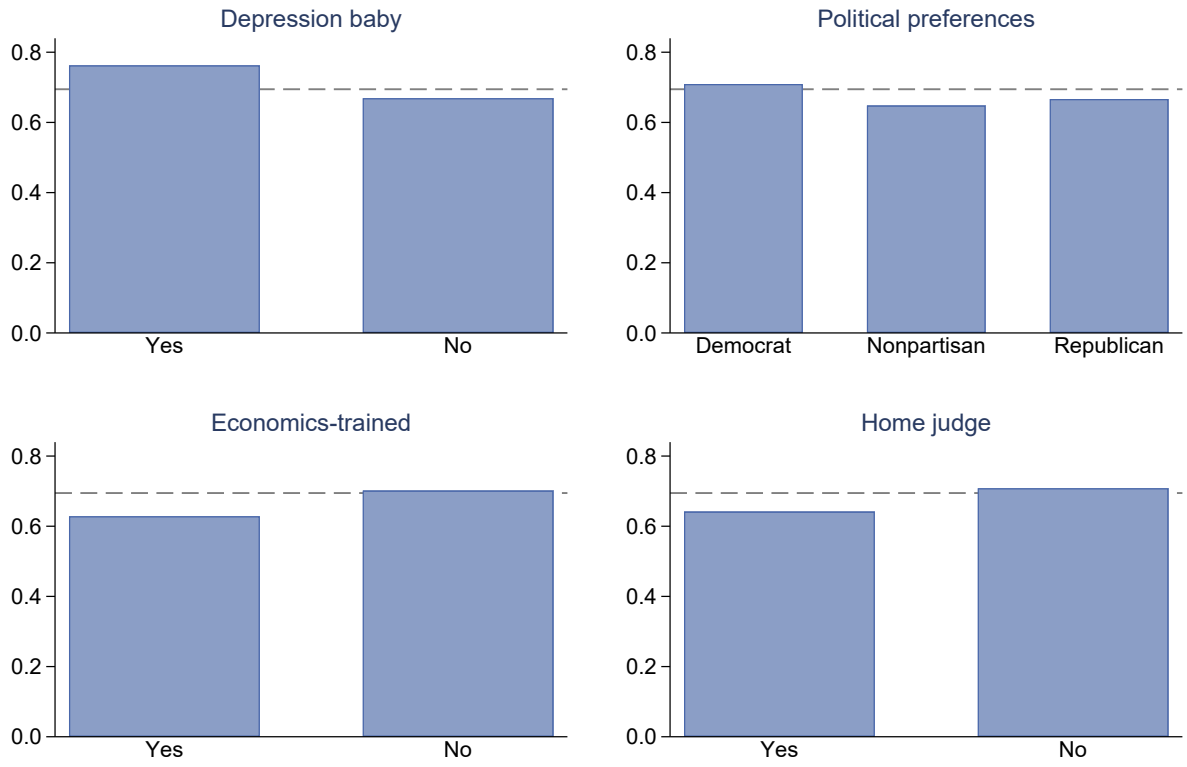


Figure 2: Judge characteristics and duration in bankruptcy across predicted duration

This figure plots the estimated effect of assignment to a judge with each characteristic on log duration as a function of predicted duration (in log months). It uses the controls and fixed effects in Table 6, with political preferences entered as three categories in Panel (b). Predicted duration is constructed on a leave-one-out basis from firm-level controls and industry fixed effects, excluding judge characteristics. Panels (a), (c), and (d) show the differences between cases assigned to judges with and without the *Depression baby*, *Economics-trained*, and *Home judge* characteristics, respectively. Panel (b) shows the differences for nonpartisan and Republican judges relative to Democratic judges. Shaded areas show pointwise 95% confidence intervals based on standard errors clustered at the court level, treating predicted duration as fixed. Each line covers only the range of predicted duration common to all groups compared in the panel, based on each group's minimum and maximum in the estimation sample. The dashed horizontal lines mark zero. Appendix Table IA.1 defines all variables.

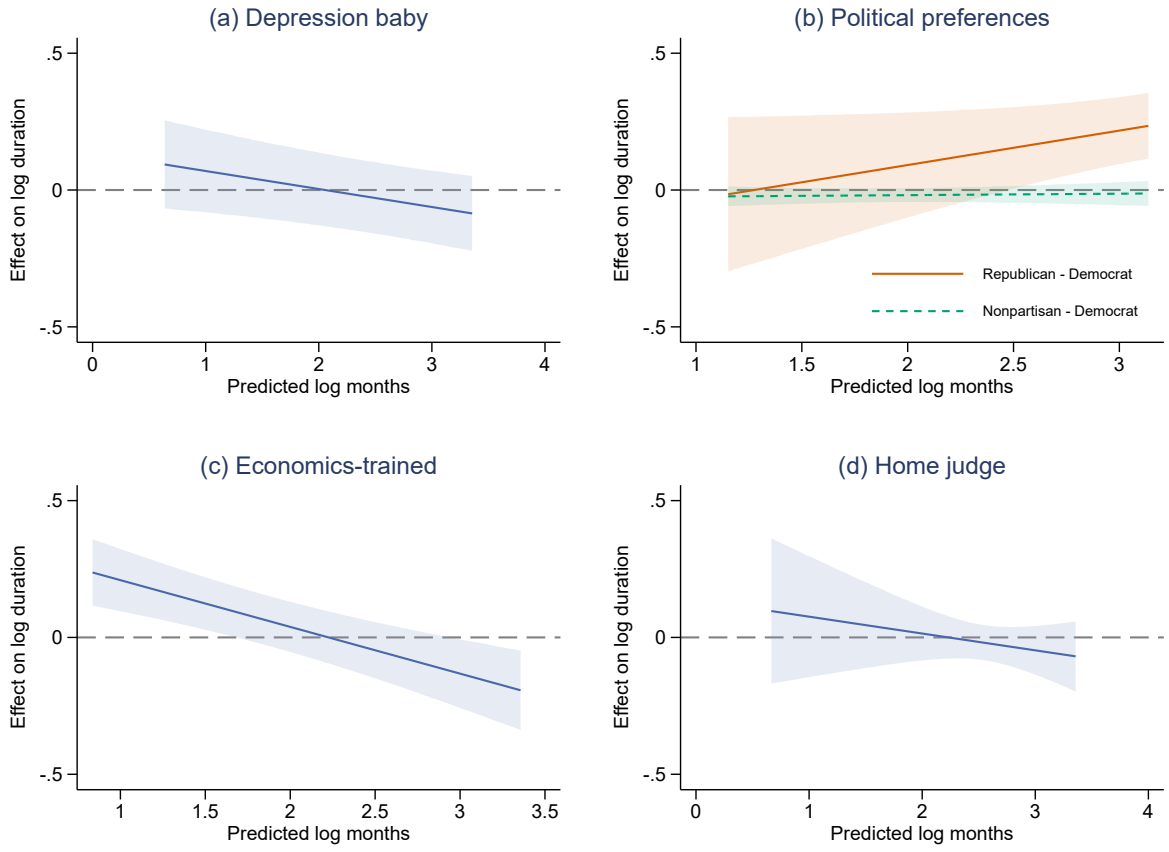


Figure 3: Post-emergence outcomes by assigned judges' characteristics

This figure plots average post-emergence outcomes conditional on assignment to Depression-baby judges, judges in each political-preference category, economics-trained judges, and home judges. Panels (a) and (b) report the average refiling rate and return on assets after emergence, respectively. The dashed horizontal lines mark the full-sample mean in each panel, computed over all firms with available data. Appendix Table IA.1 defines all variables.

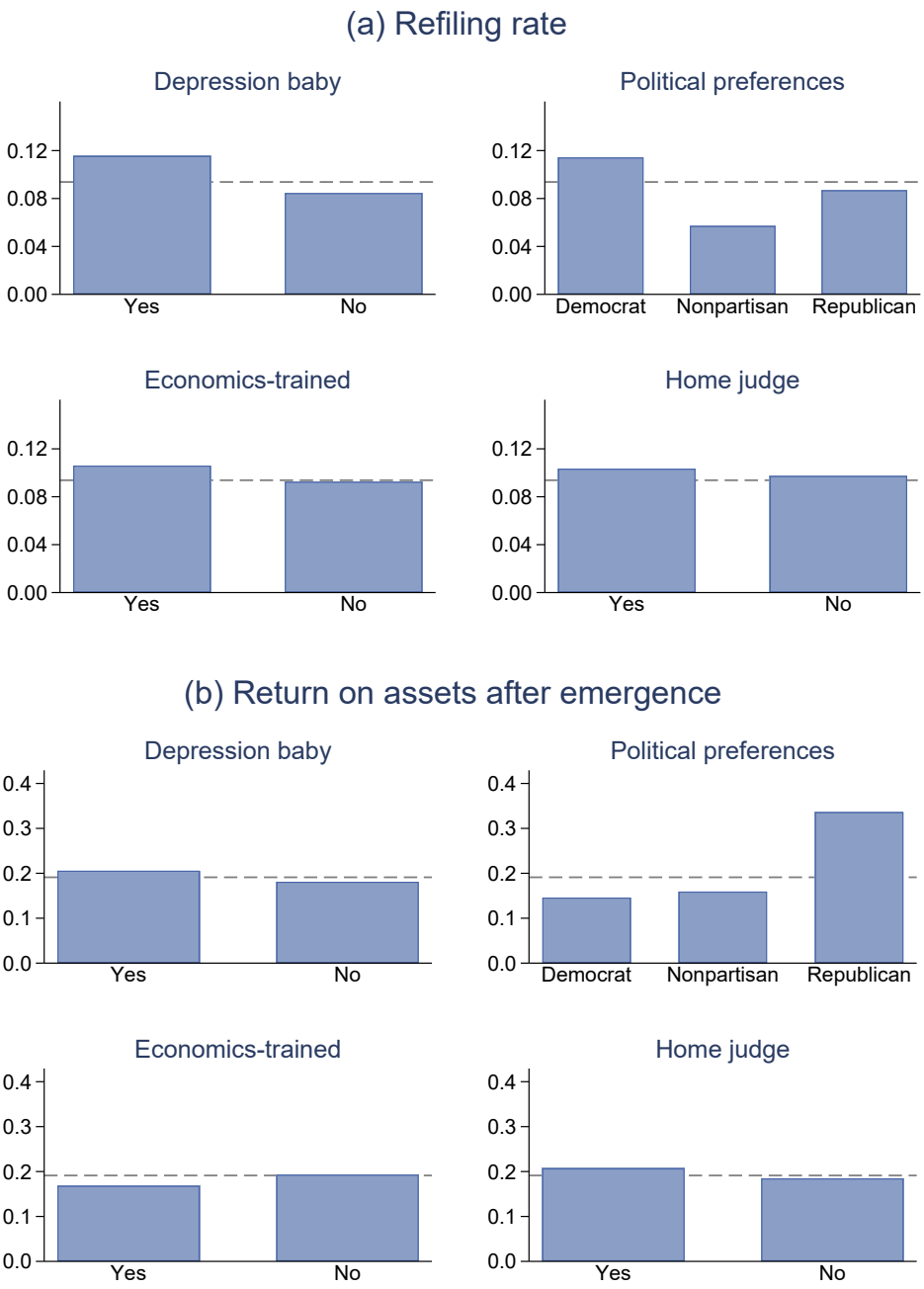


Table 1: Summary statistics

This table reports the number of observations, mean, standard deviation, 25th percentile, median, and 75th percentile of the main variables for 1,090 large U.S. Chapter 11 cases filed in 1980–2022. Judge characteristics are measured at filing and summarized at the case level. Total assets are in millions of 2023 dollars. Appendix Table IA.1 defines all variables.

Variable	Obs	Mean	SD	P25	Median	P75
Judge characteristics at filing						
Depression baby	1,090	0.27	0.44	0.00	0.00	1.00
Democrat	823	0.72	0.45	0.00	1.00	1.00
Republican	823	0.08	0.28	0.00	0.00	0.00
Economics-trained	1,090	0.10	0.30	0.00	0.00	0.00
Home judge	994	0.23	0.42	0.00	0.00	0.00
Male	1,090	0.76	0.43	1.00	1.00	1.00
Military	1,090	0.18	0.38	0.00	0.00	0.00
Months as judge	1,090	118.7	85.9	50.6	101.2	177.9
Judge age	1,090	57.1	8.8	51.0	57.5	63.0
Firm characteristics at filing						
Total assets	1,090	5,192	33,236	595	1,128	2,852
Leverage ratio	1,087	1.20	1.13	0.80	0.98	1.24
Profitability	1,078	-0.05	0.21	-0.07	0.00	0.04
Number of filings	1,090	9.90	21.69	1.00	2.00	9.00
Prepack	1,090	0.35	0.48	0.00	0.00	1.00
Law firm experience	1,086	7.03	10.13	0.00	2.00	10.00
Number of employees	1,090	7,442	16,803	900	2,700	7,200
Secured / total debt	934	0.51	0.36	0.17	0.50	0.85
Secured debt / total assets	943	0.35	0.35	0.06	0.27	0.51
Geographic concentration	858	0.31	0.24	0.14	0.25	0.40
Bankruptcy outcomes						
Emerge	1,090	0.69	0.46	0.00	1.00	1.00
Months in Ch. 11	1,090	15.62	16.65	4.87	11.06	19.69
Refile within 3 years	757	0.09	0.29	0.00	0.00	0.00
Return on assets (post)	377	0.19	0.46	-0.06	0.12	0.47

Table 2: Case characteristics and judge characteristics

This table presents estimates of the association between case characteristics and the characteristics of the judge assigned to the case. The estimates come from linear probability models in the specification provided below. The dependent variable is an indicator variable for one of four characteristics of the assigned judge, as indicated in the column header. In Columns 1 and 2, the dependent variable is *Depression baby*, defined as judges born between 1920 and 1939. In Columns 3 and 4, it is *Democrat*, defined as judges whose political preferences are classified as Democratic; the comparison group consists of Republican and nonpartisan judges. In Columns 5 and 6, it is *Economics-trained*, defined as judges who attended a Manne program in or before the filing year, or received a J.D. in 1970 or later from one of six law schools with a strong law-and-economics culture. In Columns 7 and 8, it is *Home judge*, an indicator equal to one if the firm is headquartered in the judge's home state. Case characteristics are *Log(Total assets)*, *Leverage ratio*, *Profitability*, *Log(Number of filings)*, and *Prepack*. Even-numbered columns add *Law firm experience*, the number of BRD cases filed in the prior 5 years in which the debtor's lead bankruptcy counsel was also lead debtor counsel. Each column uses all cases with available data for its specification. Columns 3–4 and 7–8 have fewer observations because political preferences and home state are unavailable for some judges, and Columns 2, 6, and 8 each lose one case that lacks information on the debtor's legal counsel. All columns include industry (Fama–French 12) and court-by-year fixed effects. *Within* R^2 is the share of the remaining variation in the dependent variable that case characteristics explain after the fixed effects are absorbed. Standard errors are clustered at the court level, and t-statistics are in parentheses. *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels, respectively. Appendix Table IA.1 defines all variables.

$$JudgeChar_{ircjt} = \alpha_r + \alpha_{ct} + \beta' CaseChar_{it} + \varepsilon_{ircjt}$$

	Depression baby		Democrat		Economics-trained		Home judge	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Log(Total assets)	-0.007	-0.008	-0.003	-0.004	-0.003	-0.002	0.004	0.005
	(-0.934)	(-1.162)	(-0.315)	(-0.979)	(-0.311)	(-0.228)	(0.421)	(0.532)
Leverage ratio	0.015	0.014	-0.013	-0.014	-0.013	-0.012	0.014	0.015
	(1.103)	(1.021)	(-1.093)	(-0.969)	(-0.816)	(-0.759)	(0.684)	(0.695)
Profitability	-0.014	-0.017	-0.115*	-0.111**	-0.070	-0.069	0.059	0.054
	(-0.578)	(-0.785)	(-1.975)	(-2.170)	(-1.005)	(-0.984)	(0.659)	(0.653)
Log(Number of filings)	-0.001	-0.000	0.018	0.018	0.005	0.005	-0.005	-0.005
	(-0.114)	(-0.015)	(1.255)	(1.384)	(0.614)	(0.584)	(-0.388)	(-0.380)
Prepack	0.036**	0.031**	0.048*	0.046**	0.033	0.037	-0.007	-0.004
	(2.555)	(2.697)	(2.015)	(2.479)	(1.341)	(1.664)	(-0.290)	(-0.139)
Law firm experience		0.000		0.001		-0.000		-0.001
		(0.228)		(0.270)		(-0.264)		(-0.513)
Industry fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Court-time fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	830	829	664	664	830	829	762	761
R^2	0.482	0.484	0.320	0.321	0.267	0.269	0.463	0.460
Within R^2	0.006	0.005	0.008	0.008	0.006	0.006	0.003	0.004

Table 3: Judge assignment and judge characteristics

This table presents linear probability model estimates of judge assignment using the set of eligible judges in the court in which each case is filed. Each observation is an eligible case–judge pair. The sample includes all eligible case–judge pairs based on judge appointment and retirement years. The eligible judges are those who appear in the BRD, that is, those who are ever assigned to a large public bankruptcy case during their tenure. I 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, $Assigned_{ij}$, is an indicator variable equal to one if eligible judge j is assigned to case i and zero otherwise; for each case, it equals one for exactly one eligible judge. The explanatory variable is eligible judge j's characteristic as of the filing date of case i, as indicated in the column header. Mean of *Assigned* is the unconditional probability of assignment. All columns include case fixed effects, so the estimates exploit variation in judge characteristics across the eligible judges for the same case. Standard errors are clustered at the court level, and t-statistics are in parentheses. *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels, respectively. Appendix Table IA.1 defines all variables.

$$Assigned_{ij} = \alpha_i + \beta JudgeChar_{ij} + \varepsilon_{ij}$$

	(1) Depression baby	(2) Democrat	(3) Economics-trained	(4) Home judge
Depression baby	-0.026*			
	(-1.768)			
Democrat		0.014		
		(0.887)		
Economics-trained			0.004	
			(0.378)	
Home judge				-0.010
				(-0.436)
Case fixed effects	Yes	Yes	Yes	Yes
Observations	5,460	3,908	5,460	4,901
Mean of *Assigned*	0.142	0.145	0.142	0.141
Within R^2	0.001	0.000	0.000	0.000

Table 4: Judge characteristics and emergence from bankruptcy

This table reports estimates of the effect of judge characteristics on the probability that a firm emerges from bankruptcy. The estimates come from linear probability models in specifications that are provided below. The dependent variable is an indicator variable equal to one if a firm emerges from bankruptcy. Across columns, I use indicator variables for four different judge characteristics. In Columns 1 and 2, the main explanatory variable is *Depression baby*, defined as judges born between 1920 and 1939. In Columns 3 and 4, it is *Democrat*, defined as judges whose political preferences are classified as Democratic; the comparison group consists of Republican and nonpartisan judges. Appendix Table IA.7 examines the sensitivity of the *Democrat* estimate in Column 4 to age-related controls and to restricting judge age. In Columns 5 and 6, it is *Economics-trained*, defined as judges who attended a Manne program in or before the filing year, or received a J.D. in 1970 or later from one of six law schools with a strong law-and-economics culture. In Columns 7 and 8, it is *Home judge*, an indicator equal to one if the firm is headquartered in the judge's home state. Odd-numbered columns include firm-level controls only, and even-numbered columns add judge-level controls Z'_{jt} (*Male*, *Military*, *Log(Months as judge)*, and *Log(Judge age)*); in Column 8, *Male* and *Military* are absorbed by judge fixed effects. All columns include firm-level controls and industry (Fama–French 12) fixed effects. In addition, Columns 1–6 include court-by-year fixed effects, and Columns 7–8 include court-by-decade and judge fixed effects. Standard errors are clustered at the court level, and t-statistics are in parentheses. *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels, respectively. Appendix Table IA.1 defines all variables.

Columns 1–6: $Emerge_{ircjt} = \alpha_r + \alpha_{ct} + \beta JudgeChar_{jt} + \delta Z'_{jt} + \gamma X'_{ijt} + \varepsilon_{ircjt}$

Columns 7–8: $Emerge_{ircjt} = \alpha_r + \alpha_{ct'} + \alpha_j + \beta HomeJudge_{ij} + \delta Z'_{jt} + \gamma X'_{ijt} + \varepsilon_{ircjt}$

	Depression baby		Democrat		Economics-trained		Home judge	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Judge characteristic	0.13**	0.09**	0.03***	0.02	-0.08	-0.06**	-0.10*	-0.10*
	(2.73)	(2.28)	(3.74)	(1.21)	(-1.61)	(-2.10)	(-2.00)	(-1.83)
Male		-0.04		-0.06***		-0.02		
		(-1.34)		(-4.51)		(-0.70)		
Military		0.01		-0.00		0.01		
		(0.36)		(-0.16)		(0.22)		
Log(Months as judge)		0.00		0.00		0.01		0.04***
		(0.03)		(0.29)		(0.38)		(2.77)
Log(Judge age)		0.17*		0.26***		0.29***		0.06
		(1.78)		(3.44)		(3.67)		(0.16)
Log(Total assets)	0.05***	0.05***	0.04***	0.04***	0.05***	0.05***	0.06***	0.06***
	(3.57)	(3.56)	(5.38)	(5.40)	(3.58)	(3.59)	(4.35)	(4.13)
Leverage ratio	0.03***	0.03***	0.02*	0.02	0.03***	0.03***	0.03***	0.03***
	(3.89)	(3.65)	(2.14)	(1.57)	(5.08)	(3.86)	(3.96)	(3.70)
Profitability	0.10	0.10	0.06	0.05	0.10	0.10	0.09	0.08
	(1.22)	(1.26)	(0.72)	(0.73)	(1.05)	(1.16)	(1.18)	(1.15)
Log(Number of filings)	-0.02	-0.02	-0.03***	-0.03***	-0.02	-0.02*	-0.01	-0.01
	(-1.57)	(-1.69)	(-3.09)	(-3.32)	(-1.63)	(-1.76)	(-0.43)	(-0.42)
Prepack	0.26***	0.26***	0.26***	0.25***	0.27***	0.26***	0.28***	0.28***
	(11.08)	(11.03)	(11.40)	(11.32)	(11.28)	(11.42)	(6.35)	(6.27)
Industry fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Court-time fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Judge fixed effects	No	No	No	No	No	No	Yes	Yes
Observations	830	830	664	664	830	830	878	878
R^2	0.349	0.350	0.331	0.337	0.343	0.350	0.354	0.358

Table 5: Judge characteristics and emergence from bankruptcy: Joint specifications

This table presents the effect of judge characteristics on the probability that a firm emerges from bankruptcy when several judge characteristics enter the same regression. The estimates come from linear probability models in the specification provided below. The dependent variable is an indicator variable equal to one if a firm emerges from bankruptcy. Columns 1–3 include pairs of *Depression baby*, *Democrat*, and *Economics-trained*, and Column 4 includes all three. Columns 5–7 add judge-level controls Z'_{jt} sequentially: *Male* and *Military* in Column 5, *Log(Months as judge)* in Column 6, and *Log(Judge age)* in Column 7. *Home judge* is not included because its specification requires judge fixed effects. Columns that include *Democrat* use only cases whose judge's political preferences are classified. All columns include firm-level controls and industry (Fama–French 12) and court-by-year fixed effects. Standard errors are clustered at the court level, and *t*-statistics are in parentheses. *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels, respectively. Appendix Table IA.1 defines all variables.

$$Emerge_{irc\,jt} = \alpha_r + \alpha_{ct} + \beta_1 DepressionBaby_j + \beta_2 Democrat_j + \beta_3 EconTrained_{jt} + \delta Z'_{jt} + \gamma X'_{ijt} + \varepsilon_{irc\,jt}$$

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Depression baby	0.11***	0.13**		0.10***	0.12***	0.11***	0.13**
	(6.42)	(2.77)		(3.26)	(3.04)	(3.80)	(2.89)
Democrat	-0.01		0.05*	0.01	0.04	0.04*	0.05*
	(-0.99)		(2.14)	(0.24)	(1.74)	(1.98)	(1.97)
Economics-trained		-0.09**	-0.09	-0.08	-0.10**	-0.10***	-0.11**
		(-2.15)	(-1.27)	(-1.35)	(-2.59)	(-3.20)	(-2.75)
Male					-0.08***	-0.08***	-0.08***
					(-3.44)	(-3.81)	(-3.71)
Military					-0.03	-0.03	-0.03
					(-1.00)	(-1.01)	(-0.95)
Log(Months as judge)						0.01	0.01
						(0.64)	(0.81)
Log(Judge age)							-0.08
							(-0.49)
Log(Total assets)	0.04***	0.05***	0.04***	0.04***	0.04***	0.04***	0.04***
	(5.14)	(3.56)	(5.25)	(5.13)	(5.42)	(5.23)	(5.03)
Leverage ratio	0.02*	0.03***	0.02**	0.02*	0.02*	0.02	0.02
	(1.83)	(4.30)	(2.36)	(1.95)	(1.89)	(1.72)	(1.71)
Profitability	0.05	0.10	0.05	0.05	0.05	0.05	0.05
	(0.69)	(1.06)	(0.59)	(0.59)	(0.63)	(0.63)	(0.63)
Log(Number of filings)	-0.03***	-0.02	-0.03***	-0.03***	-0.03***	-0.03***	-0.03**
	(-3.08)	(-1.59)	(-3.21)	(-3.18)	(-3.20)	(-3.06)	(-2.95)
Prepack	0.26***	0.26***	0.26***	0.26***	0.25***	0.25***	0.25***
	(11.85)	(11.80)	(11.63)	(11.99)	(11.24)	(11.19)	(10.92)
Industry fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Court-time fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	664	830	664	664	664	664	664
R^2	0.336	0.351	0.334	0.337	0.341	0.341	0.341

Table 6: Judge characteristics and duration in bankruptcy: Interaction with predicted duration

This table reports OLS estimates of judge effects across predicted duration. The dependent variable, Y_{ircjt}, is log months from filing to case disposition. $\widehat{Duration}_i$ is a leave-one-out prediction from firm-level controls and industry fixed effects, excluding judge characteristics. Each column examines the characteristic in its header; *Democrat* compares Democratic with Republican and nonpartisan judges. All columns include firm-level controls (X_{it}), judge-level controls (Z_{jt}), and industry (Fama–French 12) fixed effects. Columns 1–3 also include court-by-year fixed effects; Column 4 includes court-by-decade and judge fixed effects, which absorb time-invariant judge controls. The predicted-duration and firm-control coefficients are not interpreted separately because of near collinearity. Standard errors are clustered at the court level, treating predicted duration as fixed, and t-statistics are in parentheses. *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels, respectively. Appendix Table IA.1 defines all variables.

$$\text{Columns 1–3: } Y_{ircjt} = \alpha_r + \alpha_{ct} + \beta_1 JudgeChar_{jt} + \beta_2 \widehat{Duration}_i + \beta_3 JudgeChar_{jt} \times \widehat{Duration}_i + \gamma X'_{it} + \delta Z'_{jt} + \varepsilon_{ircjt}$$

$$\text{Column 4: } Y_{ircjt} = \alpha_r + \alpha_{ct'} + \alpha_j + \beta_1 HomeJudge_{ij} + \beta_2 \widehat{Duration}_i + \beta_3 HomeJudge_{ij} \times \widehat{Duration}_i + \gamma X'_{it} + \delta Z'_{jt} + \varepsilon_{ircjt}$$

	(1) Depression baby	(2) Democrat	(3) Economics-trained	(4) Home judge
Judge characteristic	0.14	0.04	0.38***	0.14
	(1.55)	(1.05)	(4.63)	(0.79)
Predicted Log(Months in Ch. 11)	-35.32***	-34.51***	-35.29***	-34.42***
	(-11.86)	(-11.01)	(-11.99)	(-10.21)
Judge char. × Predicted Log(Months in Ch. 11)	-0.07**	-0.03	-0.17***	-0.06
	(-2.71)	(-1.00)	(-4.64)	(-0.92)
Male	-0.05**	-0.02	-0.06*	
	(-2.09)	(-0.56)	(-1.81)	
Military	0.04	0.08**	0.04	
	(1.37)	(2.30)	(1.55)	
Log(Months as judge)	0.00	-0.02	-0.00	-0.02
	(0.08)	(-1.50)	(-0.07)	(-1.28)
Log(Judge age)	0.14	0.25*	0.11	-0.18
	(0.93)	(2.02)	(1.37)	(-0.67)
Log(Total assets)	2.72***	2.67***	2.72***	2.66***
	(12.36)	(11.35)	(12.45)	(10.62)
Leverage ratio	-0.06	-0.07	-0.06	-0.09*
	(-1.37)	(-1.40)	(-1.31)	(-1.87)
Profitability	8.86***	8.64***	8.85***	8.63***
	(11.39)	(10.84)	(11.59)	(10.10)
Log(Number of filings)	-0.40***	-0.39***	-0.40***	-0.38***
	(-9.89)	(-8.85)	(-10.04)	(-9.30)
Prepack	-52.60***	-51.40***	-52.55***	-51.30***
	(-12.12)	(-11.27)	(-12.25)	(-10.49)
Industry fixed effects	Yes	Yes	Yes	Yes
Court-time fixed effects	Yes	Yes	Yes	Yes
Judge fixed effects	No	No	No	Yes
Observations	830	664	830	878
R^2	0.863	0.858	0.864	0.850

Table 7: Judge characteristics and post-emergence outcomes

This table presents the effect of judge characteristics on post-emergence outcomes. Odd-numbered columns report estimates from linear probability models and even-numbered columns report estimates from OLS regressions, in specifications that are provided below. The dependent variable is either an indicator equal to one if a firm refiles within 3 years of emergence (*Refile*, odd-numbered columns) or return on assets after emergence (*ROA (post)*, even-numbered columns). Across columns, I use indicator variables for four different judge characteristics: *Depression baby* in Columns 1 and 2, *Democrat* in Columns 3 and 4, *Economics-trained* in Columns 5 and 6, and *Home judge* in Columns 7 and 8. All columns include judge-level controls, firm-level controls, and industry (Fama–French 12) fixed effects. In addition, Columns 1–6 include court-by-year fixed effects and Columns 7–8 include court-by-decade and judge fixed effects. Standard errors are clustered at the court level, and *t*-statistics are in parentheses. *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels, respectively. Appendix Table IA.1 defines all variables.

Columns 1–6: $PostOutcomes_{ircjt} = \alpha_r + \alpha_{ct} + \beta JudgeChar_{jt} + \delta Z'_{jt} + \gamma X'_{ijt} + \varepsilon_{ircjt}$

Columns 7–8: $PostOutcomes_{ircjt} = \alpha_r + \alpha_{ct'} + \alpha_j + \beta HomeJudge_{ij} + \delta Z'_{jt} + \gamma X'_{ijt} + \varepsilon_{ircjt}$

	Depression baby		Democrat		Economics-trained		Home judge	
	(1) Refile	(2) ROA (post)	(3) Refile	(4) ROA (post)	(5) Refile	(6) ROA (post)	(7) Refile	(8) ROA (post)
Judge characteristic	0.02	-0.10	0.03	0.03	0.03	0.08	0.01	-0.01
	(0.82)	(-1.12)	(0.97)	(0.18)	(0.63)	(0.93)	(0.27)	(-0.08)
Male	0.04	-0.22**	0.05**	-0.23*	0.04	-0.23*		
	(1.21)	(-2.20)	(2.40)	(-2.53)	(1.53)	(-2.07)		
Military	0.08	-0.02	0.09	0.13**	0.09	-0.02		
	(1.33)	(-0.26)	(0.92)	(2.77)	(1.38)	(-0.17)		
Log(Months as judge)	-0.00	0.08***	0.01	0.04**	-0.00	0.08***	-0.03	0.00
	(-0.12)	(3.68)	(0.61)	(3.39)	(-0.27)	(3.43)	(-1.11)	(0.05)
Log(Judge age)	-0.03	0.17	-0.07	0.11	0.02	-0.03	0.58	1.02
	(-0.24)	(0.55)	(-0.43)	(0.65)	(0.15)	(-0.11)	(0.80)	(1.23)
Log(Total assets)	-0.01**	0.06***	-0.01***	0.07***	-0.01**	0.06***	-0.01*	0.07***
	(-2.59)	(3.37)	(-3.73)	(4.57)	(-2.60)	(3.79)	(-1.95)	(5.53)
Leverage ratio	0.00	0.16**	0.00	0.23**	0.00	0.17**	-0.00	0.22***
	(0.14)	(2.61)	(0.21)	(3.69)	(0.21)	(2.51)	(-0.10)	(4.17)
Profitability	-0.03	-0.52***	-0.03	-0.51**	-0.03	-0.52***	0.01	-0.30**
	(-0.67)	(-3.29)	(-0.63)	(-3.78)	(-0.69)	(-3.20)	(0.20)	(-2.52)
Log(Number of filings)	0.01	0.02	0.01	0.00	0.01	0.02	0.00	0.03**
	(1.28)	(1.02)	(0.76)	(0.08)	(1.23)	(0.99)	(0.39)	(2.36)
Prepack	0.08***	-0.05	0.09***	-0.03	0.08***	-0.05	0.08***	-0.12
	(3.71)	(-0.57)	(4.65)	(-0.24)	(3.86)	(-0.61)	(5.32)	(-1.47)
Industry fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Court-time fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Judge fixed effects	No	No	No	No	No	No	Yes	Yes
Observations	555	251	468	208	555	251	565	253
R^2	0.331	0.445	0.345	0.442	0.331	0.445	0.263	0.400

Table 8: Heterogeneous effects of Depression-baby and economics-trained judges: Number of employees

This table reports estimates of the effects of Depression-baby and economics-trained judges on the probability that a firm emerges from bankruptcy, separately for firms with below-median (Low) and above-median (High) employment. Appendix Table IA.9 reports the same split for all four judge characteristics. The sample is split at the full-sample median of 2,700 employees, measured from the last 10-K before filing. The dependent variable is an indicator variable equal to one if a firm emerges from bankruptcy. Within each subsample, the specification is identical to that in Table 4, Column 2 (Columns 1–2) or Column 6 (Columns 3–4). All columns include judge-level controls, firm-level controls, and industry (Fama–French 12) and court-by-year fixed effects. The two subsamples together contain fewer observations than the corresponding columns of Table 4: splitting the sample leaves more fixed-effect cells with a single observation, which are dropped. Standard errors are clustered at the court level, and t-statistics are in parentheses. *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels, respectively. Appendix Table IA.1 defines all variables.

	Depression baby		Economics-trained	
	(1)	(2)	(3)	(4)
Number of employees	Low	High	Low	High
Judge characteristic	0.05	0.10	-0.04	-0.10***
	(1.05)	(1.58)	(-0.75)	(-3.13)
Male	-0.05	-0.05*	-0.03	-0.02
	(-1.19)	(-1.95)	(-0.89)	(-0.99)
Military	-0.00	0.01	-0.01	0.00
	(-0.06)	(0.10)	(-0.19)	(0.04)
Log(Months as judge)	-0.03	0.03**	-0.03	0.05**
	(-0.97)	(2.12)	(-0.96)	(2.79)
Log(Judge age)	0.32*	-0.21	0.37**	-0.07
	(2.18)	(-1.08)	(3.06)	(-1.07)
Log(Total assets)	0.06*	0.04**	0.06*	0.03**
	(2.03)	(2.34)	(2.12)	(2.11)
Leverage ratio	0.03**	0.02	0.03**	0.01
	(2.63)	(1.49)	(2.81)	(1.18)
Profitability	0.02	0.66*	0.01	0.64*
	(0.27)	(1.94)	(0.20)	(1.88)
Log(Number of filings)	-0.01	-0.04**	-0.01	-0.04**
	(-1.19)	(-2.43)	(-1.21)	(-2.56)
Prepack	0.29***	0.22***	0.29***	0.23***
	(7.70)	(10.55)	(7.77)	(10.23)
Industry fixed effects	Yes	Yes	Yes	Yes
Court-time fixed effects	Yes	Yes	Yes	Yes
Observations	375	381	375	381
R^2	0.427	0.415	0.427	0.416

Table 9: Heterogeneous effects of Democratic judges: Debt ratios

This table reports estimates from linear probability models of how the effect of assignment to Democratic judges on emergence varies with the firm's debt. The dependent variable is an indicator variable equal to one if a firm emerges from bankruptcy. The specification adds to Equation (1) an interaction of *Democrat* with *Leverage ratio*, the ratio of total liabilities to total assets (Column 1), or with *Secured / total debt*, the ratio of secured debt to total debt (Columns 2 and 3), together with the debt measure itself. Column 3 adds judge-level controls. All columns include firm-level controls and industry (Fama–French 12) and court-by-year fixed effects. Standard errors are clustered at the court level, and *t*-statistics are in parentheses. *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels, respectively. Appendix Table IA.1 defines all variables.

	(1) Leverage	(2) Secured debt	(3) Secured debt
Democrat	0.03	-0.06**	-0.07**
	(1.33)	(-2.33)	(-2.74)
Leverage ratio	0.02***	0.02*	0.02
	(3.61)	(2.08)	(1.60)
Democrat × Leverage ratio	0.00		
	(0.04)		
Secured / total debt		-0.11***	-0.12**
		(-3.61)	(-2.84)
Democrat × Secured / total debt		0.16***	0.17**
		(3.16)	(2.94)
Male			-0.06***
			(-3.62)
Military			-0.03
			(-1.51)
Log(Months as judge)			0.01
			(0.53)
Log(Judge age)			0.22**
			(2.45)
Log(Total assets)	0.04***	0.04***	0.04***
	(5.04)	(5.16)	(5.33)
Profitability	0.06	0.06	0.06
	(0.70)	(0.78)	(0.80)
Log(Number of filings)	-0.03***	-0.03**	-0.03**
	(-3.15)	(-2.39)	(-2.56)
Prepack	0.26***	0.28***	0.28***
	(11.04)	(13.94)	(12.74)
Industry fixed effects	Yes	Yes	Yes
Court-time fixed effects	Yes	Yes	Yes
Observations	664	592	592
R^2	0.331	0.352	0.358

Table 10: Heterogeneous effects of home judges: Geographic concentration

This table reports linear probability estimates of how the effect of home judges on emergence varies with the firm's geographic concentration, debt, and employment, based on Equation (3). The dependent variable equals one if the firm emerges from bankruptcy. *GeoConc* denotes *Geographic concentration*, the Herfindahl index of U.S. state-name mentions in the firm's last 10-K filed within 3 years before filing (García and Norli 2012); a higher value indicates operations concentrated in fewer states. Columns 1 and 2 interact *Home judge* with *GeoConc* and *Leverage ratio*, respectively; Columns 3–5 estimate Equation (3) with $StakeholderInterest_i$ equal to *Leverage ratio*, *Secured debt / total assets*, and *Log(No. of employees)*, respectively. All columns include judge-level controls (*Log(Months as judge)* and *Log(Judge age)*), firm-level controls, and industry (Fama–French 12), court-by-decade, and judge fixed effects (which absorb *Male* and *Military*); of the controls, only *Leverage ratio* is reported. Standard errors are clustered at the court level, and *t*-statistics are in parentheses. *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels, respectively. Appendix Table IA.1 defines all variables.

Moderators	(1) Concentration	(2) Leverage	(3) Concentration × Leverage	(4) Concentration × Secured debt	(5) Concentration × Employment
Home judge	0.06	-0.09	0.09	-0.02	0.40
	(0.62)	(-0.98)	(0.47)	(-0.15)	(0.95)
GeoConc	0.01		0.02	-0.13	0.08
	(0.13)		(0.32)	(-1.65)	(0.28)
Home judge × GeoConc	-0.40*		-0.41	-0.06	-1.18
	(-1.82)		(-0.96)	(-0.29)	(-1.23)
Leverage ratio	0.03***	0.04***	0.04	0.03*	0.03***
	(3.75)	(3.10)	(1.52)	(2.05)	(3.31)
Home judge × Leverage		-0.01	-0.02		
		(-0.42)	(-0.23)		
GeoConc × Leverage			-0.01		
			(-0.26)		
Home judge × GeoConc × Leverage			0.00		
			(0.01)		
Secured debt / total assets				-0.05	
				(-0.66)	
Home judge × Secured debt / total assets				0.10	
				(1.02)	
GeoConc × Secured debt / total assets				0.53***	
				(3.17)	
Home judge × GeoConc × Secured debt / total assets				-0.78*	
				(-1.78)	
Log(No. of employees)					0.02
					(1.04)
Home judge × Log(No. of employees)					-0.05
					(-0.82)
GeoConc × Log(No. of employees)					-0.01
					(-0.19)
Home judge × GeoConc × Log(No. of employees)					0.11
					(0.88)
Judge-level controls	Yes	Yes	Yes	Yes	Yes
Firm controls	Yes	Yes	Yes	Yes	Yes
Industry fixed effects	Yes	Yes	Yes	Yes	Yes
Court-time fixed effects	Yes	Yes	Yes	Yes	Yes
Judge fixed effects	Yes	Yes	Yes	Yes	Yes
Observations	691	878	691	602	691
R^2	0.360	0.358	0.361	0.389	0.363

Internet Appendix to

"Biased Judges? Judge Characteristics and Bankruptcy Outcomes"

Donghyun Kang[1]

Internet Appendix Contents

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IA.1 Variable definitions

Table IA.1: Variable definitions

Variable	Definition
A. Judge characteristics (measured at filing)	
Depression baby	Indicator variable equal to one if the judge was born between 1920 and 1939.
Democrat	Indicator variable equal to one if the judge's political preferences are Democratic and zero if they are Republican or nonpartisan. Political preferences are classified from voter registration records and, if these do not classify the judge, from Federal Election Commission (FEC) donation records (Section 3.1.2 and Appendix IA.2). Missing if neither record classifies the judge.
Economics-trained	Indicator variable equal to one if the judge attended a Manne program in or before the filing year, or received a J.D. in 1970 or later from one of six law schools with a strong law-and-economics culture (Section 3.1.2 and Appendix IA.2).
Home judge	Indicator variable equal to one if the judge's home state is the firm's headquarters state at filing. The home state is the state of the judge's high school or, if unknown, the birth state.
Male	Indicator variable equal to one if the judge is male.
Military	Indicator variable equal to one if the judge served in the military.
Months as judge	Number of months from the judge's appointment date to the filing date.
Judge age	Judge's age in years on the filing date.
B. Alternative home-judge measures	
HQ and court in same state	Indicator variable equal to one if the firm's headquarters state at filing is the state of the filing court.
Judge worked in HQ state	Indicator variable equal to one if the judge's bar admission state (filing-court state if unknown) is equal to the firm's headquarters state. The bar admission state proxies for the state where the judge primarily worked before joining the bench.

Table IA.1: Variable definitions (continued)

Variable	Definition
C. Firm characteristics (measured at filing)	
Total assets	Total assets at filing, in millions of 2023 dollars.
Leverage ratio	Total liabilities divided by total assets at filing, winsorized at the 0.5th and 99.5th percentiles.
Profitability	EBIT divided by total assets at filing, winsorized at the 0.5th and 99.5th percentiles.
Number of filings	Number of affiliated entities filing in the case, set to one if missing.
Prepack	Indicator variable equal to one if the case is prepackaged or prenegotiated.
Law firm experience	Number of BRD cases filed in the prior 5 years in which the debtor's lead bankruptcy counsel was also lead debtor counsel.
Most represented law firms	Indicator variable equal to one if any law firm on the debtor's counsel team (lead or co-counsel) is among the top debtor law firms listed in Goyal et al. (2026, Appendix A) (Appendix IA.2).
Number of employees	Number of employees at filing.
Secured / total debt	Secured debt divided by total debt at filing.
Secured debt / total assets	Secured debt divided by total assets at filing.
Geographic concentration	Herfindahl index of U.S. state-name mentions in the firm's last 10-K filed within 3 years before filing, following García and Norli (2012).
D. Bankruptcy outcomes	
Emerge	Indicator variable equal to one if the firm emerges from bankruptcy.
Months in Ch. 11	Number of months from the filing date to the plan confirmation date or the date the case is converted to a Chapter 7 liquidation.
Refile	Indicator variable equal to one if the emerged firm files for bankruptcy again within 3 years of emergence.
Return on assets (post)	Net income divided by total assets from the first 10-K filed after emergence, winsorized at −100% and 100%.

IA.2 Data construction

Courts excluded from the sample I drop 13 cases filed in courts that have a single bankruptcy judge or do not assign cases randomly (Section 3.1). The single-judge courts are the District of Alaska, the District of Columbia, the District of Hawaii, the Middle District of Louisiana, the District of New Hampshire, the District of Vermont, the Western District of Wisconsin, and the Southern District of West Virginia. The Eastern District of Wisconsin does not assign cases randomly (Iverson et al. 2023).

Debtor counsel *Most represented law firms* equals one if any firm on the debtor's counsel team (as recorded in the BRD) is among the most represented lead counsel firms in Appendix A of Goyal et al. (2026, p. 69). That appendix lists the ten most frequently appearing lead counsel firms in their sample of 1,799 Chapter 11 filings by public, non-financial firms with $10 million or more in assets from 1996 to 2019, ranked by aggregate debtor assets (Panel A) and by number of cases (Panel B). The union of the two top-ten lists comprises 13 firms: Weil, Gotshal & Manges; Kirkland & Ellis; Skadden, Arps, Slate, Meagher & Flom; Jones Day; Sidley Austin; Latham & Watkins; Willkie Farr & Gallagher; Pachulski Stang Ziehl & Jones; Munger, Tolles & Olson; Squire Patton & Boggs; and the three Wilmington, Delaware firms Richards, Layton & Finger; Young Conaway Stargatt & Taylor; and Morris, Nichols, Arsht & Tunnell.

Biographical information I collect judges' biographical information from several sources. First, I obtain biographical information (e.g., home state, educational background, and years of appointment and retirement) from official court websites. If such information is not available on court websites, I search legal databases and other biographical sources, such as the BR/MAG Judge Database of the Legal Data Lab at the University of Virginia School of Law (Ashley et al. 2025), the American College of Bankruptcy, Martindale.com, LexisNexis, Westlaw Profiler, Bloomberg Law's People

Search, the Almanac of the Federal Judiciary (via Westlaw), Ballotpedia.com, LinkedIn, Marquis Who's Who Biographies, and other online resources. In addition, I identify the birthplace of judges born before 1950 from 1940 and 1950 Census records.[1] I also use college yearbooks, which list each graduating student's hometown, to identify the judge's hometown at college graduation. Finally, I contact judges for home state information.

Second, to supplement home state information that is not available from the previous steps, I conduct web searches. Specifically, I combine the name of a judge with common keywords that identify a person's hometown, such as *native*, *hometown*, *birth*, *born*, *grew*, *childhood*, or *high school*. Common sources include obituaries, judges' autobiographical interviews, and newspaper articles. To ensure I identify the correct person and the correct home state, I read each article and verify the match.

Political preferences Following Kuvvet (2021), I obtain voter registration records that are publicly accessible through VoterRecords.com and Stephen P. Morse's "One-Step Search Tool" generator. These cover the District of Columbia and 20 states: Alaska, Arkansas, Colorado, Connecticut, Delaware, Florida, Idaho, Louisiana, Michigan, Mississippi, Nevada, New Jersey, New York, North Carolina, Ohio, Oklahoma, Pennsylvania, Rhode Island, Utah, and Washington. Notable omissions are California and Texas. FEC donation data are available from 1979. I link the judges in the sample to FEC donors by name and occupation, following Hong and Kostovetsky (2012) and Lee et al. (2014).

In the voter data, New York and Pennsylvania record "no party affiliation" as a separate category, distinct from a blank party field. Registering without a party is a deliberate choice in these states: the registration form offers "no affiliation" as an option alongside the parties, and because both states hold closed primaries, a voter registered without a party affiliation cannot vote in party primaries. The results are similar when I instead code these judges' political preferences as missing.

Of the 11 nonpartisan judges, 7 donated to both parties, 3 are registered without party

[1] For details on using the Census data, see Appendix B in Chuprinin and Sosyura (2018).

affiliation, and 1 is registered with the Independence Party.

Economics training Ash et al. (2026) measure attendance at the Manne program between 1976 and 1999; I extend their measure to later attendance. The program had already changed format within their period without changing its content: in 1998, the LEC shortened the course from 14 to 10 days, "while continuing to cover the same amount of material" (Butler 1999, p. 361). After 1999, the LEC continued to offer economics programs for judges, and I count attendance at those whose titles contain the word "economic" or "economics": "Economic Analysis of Law for Judges," "Economics of Private Law," "Economics of Public Law Institute for Judges," "Frontiers of Law and Economics," and "International Law from an Economic Perspective for Judges."

I obtain attendance from the cumulative list of participating judges in Butler (1999) and, for later programs, from the LEC's program records, which list the judges attending each program. George Mason University, which houses the LEC, released these records in response to a public records request.[2] The records cover programs held between 1986 and 2010, and I also use them to cross-check Butler's list. I do not observe attendance at programs held after 2010, although the sample runs through 2022.

[2]The records are available at https://www.muckrock.com/foi/virginia-128/judge-attendance-at-events-conferences-and-seminars-36073/.

IA.3 Additional summary statistics

Table IA.2: Court-level summary statistics

This table presents summary statistics for the 1,090 Chapter 11 cases filed between 1980 and 2022 in the 58 bankruptcy courts in the sample. Each row refers to a bankruptcy court, where *No. of filings* denotes the total number of filings over the sample period, *No. of judges* denotes the number of unique judges assigned to cases filed in that court, and *% Outside* denotes the share of cases in which firms file outside their headquarters state. When calculating *No. of judges*, I include visiting judges who preside outside the court to which they ordinarily belong; hence, a single judge may be counted multiple times across courts.

Court	No. of filings	No. of judges	% Outside	Court	No. of filings	No. of judges	% Outside
AL MD	3	2	0	MO ED	13	4	15
AL ND	3	3	0	MO WD	5	2	20
AL SD	1	1	100	MS SD	3	2	0
AR ED	1	1	0	NC ED	5	2	0
AZ	9	6	33	NE	1	1	100
CA CD	34	18	12	NJ	23	10	35
CA ED	1	1	0	NV	10	3	40
CA ND	20	9	5	NY ED	1	1	100
CA SD	4	2	0	NY ND	1	1	0
CO	8	5	0	NY SD	224	28	64
CT	1	1	0	NY WD	1	1	0
DE	376	22	99	OH ND	13	9	23
FL MD	11	4	9.1	OH SD	10	5	10
FL SD	12	5	25	OK WD	5	4	20
GA ND	10	5	40	OR	2	1	0
GA SD	1	1	0	PA MD	2	2	0
ID	1	1	100	PA WD	3	3	0
IL ND	32	16	25	SC	2	2	0
IN ND	1	1	100	TN ED	1	1	0
IN SD	6	4	0	TN MD	3	3	0
KS	3	3	0	TX ND	38	10	18
KY ED	1	1	100	TX SD	96	10	29
LA ED	6	3	33	TX WD	9	5	11
LA WD	2	1	0	UT	3	3	33
MA	11	7	18	VA ED	24	5	46
MD	10	5	30	VA WD	1	1	0
ME	1	1	100	WA ED	2	1	0
MI ED	15	7	0	WA WD	2	2	50
MN	2	1	0	WV ND	1	1	0

Table IA.3: Summary statistics at the judge level

This table presents the number of observations, mean, standard deviation, 25th percentile, median, and 75th percentile of judge characteristics for the 249 judges in the sample, with equal weight per judge. Observation counts vary with data availability. Characteristics that vary across a judge's cases—economics training, home-judge status, age, and months as judge—are measured at that judge's latest filing in the sample. Appendix Table IA.1 defines all variables.

Variable	Obs	Mean	SD	P25	Median	P75
Depression baby	249	0.26	0.44	0.00	0.00	1.00
Democrat	127	0.70	0.46	0.00	1.00	1.00
Republican	127	0.21	0.41	0.00	0.00	0.00
Economics-trained	249	0.12	0.32	0.00	0.00	0.00
Home judge	205	0.39	0.49	0.00	0.00	1.00
Male	249	0.77	0.42	1.00	1.00	1.00
Military	249	0.30	0.46	0.00	0.00	1.00
Months as judge	249	144.5	95.7	68.6	134.9	206.8
Judge age	249	58.1	8.8	52.0	58.0	64.0

Table IA.4: Summary statistics of alternative home-judge measures

This table reports the number of observations, mean, standard deviation, 25th percentile, median, and 75th percentile of the two alternative home-judge measures in Table IA.8 for the 1,090 cases in the sample. *HQ and court in same state* is an indicator variable equal to one if the firm's headquarters state at filing is the state of the filing court. *Judge worked in HQ state* is an indicator variable equal to one if the state where the judge primarily worked before joining the bench, proxied by the judge's bar admission state, is the firm's headquarters state. Appendix Table IA.1 defines all variables.

Variable	Obs	Mean	SD	P25	Median	P75
HQ and court in same state	1,090	0.43	0.49	0.00	0.00	1.00
Judge worked in HQ state	1,090	0.42	0.49	0.00	0.00	1.00

IA.4 Additional results

Table IA.5: Case characteristics and judge characteristics: Alternative measure of legal counsel experience

This table repeats the analysis in Table 2, except that the legal-counsel measure is *Most represented law firms*, an indicator variable equal to one if any law firm on the debtor's counsel team (lead or co-counsel) is among the most represented debtor law firms in Goyal et al. (2026, Appendix A), rather than *Law firm experience*. The estimates come from linear probability models in the specification provided below. The dependent variable is an indicator variable for the assigned judge's characteristic indicated in the column header. Case characteristics are *Log(Total assets)*, *Leverage ratio*, *Profitability*, *Log(Number of filings)*, and *Prepack*. Each column uses all cases with available data for its specification, which requires information on the debtor's legal counsel. All columns include industry (Fama–French 12) and court-by-year fixed effects. Standard errors are clustered at the court level, and t-statistics are in parentheses. *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels, respectively. Appendix Table IA.1 defines all variables.

$$JudgeChar_{irc\,j\,t} = \alpha_r + \alpha_{ct} + \beta' CaseChar_{it} + \varepsilon_{irc\,j\,t}$$

	(1) Depression baby	(2) Democrat	(3) Economics-trained	(4) Home judge
Log(Total assets)	-0.007	-0.002	-0.000	0.005
	(-0.919)	(-0.288)	(-0.034)	(0.510)
Leverage ratio	0.015	-0.012	-0.011	0.015
	(1.009)	(-0.893)	(-0.685)	(0.700)
Profitability	-0.019	-0.117**	-0.071	0.058
	(-0.844)	(-2.168)	(-1.029)	(0.646)
Log(Number of filings)	-0.000	0.018	0.005	-0.005
	(-0.008)	(1.311)	(0.598)	(-0.391)
Prepack	0.032***	0.049**	0.040*	-0.005
	(3.244)	(2.592)	(1.783)	(-0.188)
Most represented law firms	-0.009	-0.017	-0.030*	-0.014
	(-0.392)	(-0.342)	(-1.869)	(-0.679)
Industry fixed effects	Yes	Yes	Yes	Yes
Court-time fixed effects	Yes	Yes	Yes	Yes
Observations	829	664	829	761
R^2	0.484	0.321	0.271	0.460
Within R^2	0.005	0.008	0.008	0.003

Table IA.6: Judge characteristics and duration in bankruptcy

This table explores the effect of judge characteristics on case duration, irrespective of predicted duration. It estimates Equation (1) in Columns 1–6 and Equation (2) in Columns 7 and 8 by OLS, with duration as the dependent variable. The dependent variable is the log of the number of months a case spends in bankruptcy. The main explanatory variable is *Depression baby* in Columns 1 and 2, *Democrat* in Columns 3 and 4, *Economics-trained* in Columns 5 and 6, and *Home judge* in Columns 7 and 8. Even-numbered columns add judge-level controls. All columns include firm-level controls and industry (Fama–French 12) fixed effects. Columns 1–6 also include court-by-year fixed effects, and Columns 7 and 8 include court-by-decade and judge fixed effects. Standard errors are clustered at the court level, and *t*-statistics are in parentheses. *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels, respectively. Appendix Table IA.1 defines all variables.

Columns 1–6: $Log(Months\ in\ Ch.\ 11)_{ircjt} = \alpha_r + \alpha_{ct} + \beta JudgeChar_{jt} + \delta Z'_{jt} + \gamma X'_{ijt} + \varepsilon_{ircjt}$

Columns 7–8: $Log(Months\ in\ Ch.\ 11)_{ircjt} = \alpha_r + \alpha_{ct'} + \alpha_j + \beta HomeJudge_{ij} + \delta Z'_{jt} + \gamma X'_{ijt} + \varepsilon_{ircjt}$

	Depression baby		Democrat		Economics-trained		Home judge	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Judge characteristic	-0.04	-0.03	-0.04	-0.04	-0.03	0.03	-0.09	-0.09
	(-1.07)	(-0.63)	(-1.16)	(-0.42)	(-0.31)	(0.58)	(-1.12)	(-1.22)
Male		-0.18***		-0.14**		-0.18***		
		(-3.21)		(-2.42)		(-3.44)		
Military		0.15*		0.20		0.15*		
		(1.89)		(1.23)		(1.97)		
Log(Months as judge)		-0.03		-0.06***		-0.03		-0.02
		(-1.29)		(-6.44)		(-1.42)		(-0.72)
Log(Judge age)		0.31		0.50**		0.26*		-1.05**
		(1.62)		(2.88)		(1.76)		(-2.65)
Log(Total assets)	0.12***	0.12***	0.12***	0.13***	0.12***	0.12***	0.12***	0.12***
	(7.00)	(6.92)	(6.89)	(7.16)	(7.00)	(6.89)	(4.90)	(5.35)
Leverage ratio	0.03	0.03	0.03*	0.03	0.02	0.03	0.02	0.03
	(1.13)	(1.20)	(1.78)	(1.64)	(1.15)	(1.21)	(0.80)	(0.94)
Profitability	0.16*	0.16*	0.14*	0.15	0.16*	0.17*	0.14**	0.14**
	(1.92)	(1.97)	(1.79)	(1.72)	(1.78)	(1.95)	(2.71)	(2.38)
Log(Number of filings)	0.04***	0.04***	0.05**	0.05***	0.04***	0.04***	0.05***	0.05***
	(3.95)	(4.81)	(2.70)	(3.03)	(4.05)	(4.75)	(4.47)	(4.83)
Prepack	-1.19***	-1.19***	-1.20***	-1.18***	-1.19***	-1.19***	-1.28***	-1.27***
	(-24.63)	(-25.31)	(-30.47)	(-24.23)	(-24.97)	(-25.36)	(-18.86)	(-17.56)
Industry fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Court-time fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Judge fixed effects	No	No	No	No	No	No	Yes	Yes
Observations	830	830	664	664	830	830	878	878
R^2	0.651	0.655	0.643	0.648	0.651	0.655	0.631	0.634

Table IA.7: Democratic judges and emergence from bankruptcy: Overlapping judge ages

This table examines whether the effect of Democratic judges on the probability of emergence reflects differences in judge age. The estimates come from linear probability models in the specification provided below. The dependent variable is an indicator variable equal to one if a firm emerges from bankruptcy, and the main explanatory variable is *Democrat*. Columns 1–4 use the full sample and add judge-level controls Z'_{jt} sequentially: *Male* and *Military* in Column 2, *Log(Months as judge)* in Column 3, and *Log(Judge age)* in Column 4. Columns 1 and 4 reproduce Table 4, Columns 3 and 4, respectively. Columns 5 and 6 restrict the sample to cases whose judge is at most 63 years old at filing, the highest age at which both Democratic and Republican judges appear in the estimation sample of Column 4. Column 6 uses the same sample as Column 5 but drops *Log(Months as judge)* and *Log(Judge age)*. Nonpartisan judges are retained in all the specifications. All columns include firm-level controls and industry (Fama–French 12) and court-by-year fixed effects. Standard errors are clustered at the court level, and *t*-statistics are in parentheses. *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels, respectively. Appendix Table IA.1 defines all variables.

$$Emerge_{ircjt} = \alpha_r + \alpha_{ct} + \beta Democrat_j + \delta Z'_{jt} + \gamma X'_{ijt} + \varepsilon_{ircjt}$$

	Unrestricted				Age at most 63	
	(1)	(2)	(3)	(4)	(5)	(6)
Democrat	0.03***	0.05***	0.04***	0.02	0.02*	0.02**
	(3.74)	(5.58)	(3.93)	(1.21)	(2.10)	(2.16)
Male		-0.04***	-0.04***	-0.06***	-0.06***	-0.06***
		(-3.62)	(-3.54)	(-4.51)	(-4.86)	(-4.61)
Military		-0.01	-0.01	-0.00	-0.04	-0.04
		(-0.42)	(-0.35)	(-0.16)	(-0.78)	(-0.74)
Log(Months as judge)			0.02**	0.00	0.01	
			(2.18)	(0.29)	(0.26)	
Log(Judge age)				0.26***	0.05	
				(3.44)	(0.53)	
Log(Total assets)	0.04***	0.04***	0.04***	0.04***	0.04***	0.04***
	(5.38)	(5.56)	(5.20)	(5.40)	(4.08)	(3.91)
Leverage ratio	0.02*	0.02*	0.02	0.02	0.03***	0.03***
	(2.14)	(2.14)	(1.75)	(1.57)	(4.63)	(4.66)
Profitability	0.06	0.06	0.06	0.05	-0.14	-0.14
	(0.72)	(0.76)	(0.77)	(0.73)	(-1.17)	(-1.15)
Log(Number of filings)	-0.03***	-0.03***	-0.03**	-0.03***	-0.04**	-0.04**
	(-3.09)	(-3.11)	(-2.92)	(-3.32)	(-2.52)	(-2.59)
Prepack	0.26***	0.25***	0.25***	0.25***	0.26***	0.26***
	(11.40)	(10.91)	(10.65)	(11.32)	(10.89)	(11.54)
Industry fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Court-time fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Observations	664	664	664	664	476	476
R^2	0.331	0.332	0.334	0.337	0.391	0.390

Table IA.8: Home judges and emergence from bankruptcy: Alternative explanations

This table examines whether the effect of home judges on the probability of emergence reflects alternative explanations. The estimates come from linear probability models whose specification is identical to that of the baseline home-judge regression in Equation (2), as estimated in Table 4, Column 8, except for the addition of two control variables. The dependent variable is an indicator variable equal to one if a firm emerges from bankruptcy. Column 1 (*Local filing*) adds *HQ and court in same state*, an indicator variable equal to one if the firm's headquarters state at filing is the state of the filing court. Column 2 (*Professional ties*) adds *Judge worked in HQ state*, an indicator variable equal to one if the state where the judge primarily worked before joining the bench, proxied by the judge's bar admission state, is the firm's headquarters state. Column 3 includes both. Appendix Table IA.4 summarizes both measures. All columns include judge-level controls Z'_{jt} (*Log(Months as judge)* and *Log(Judge age)*), firm-level controls, and industry (Fama–French 12), court-by-decade, and judge fixed effects. Standard errors are clustered at the court level, and t-statistics are in parentheses. *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels, respectively. Appendix Table IA.1 defines all variables.

	(1) Local filing	(2) Professional ties	(3) Both
Home judge	-0.17**	-0.16*	-0.15
	(-2.14)	(-1.76)	(-1.69)
HQ and court in same state	0.09		0.18***
	(1.32)		(7.76)
Judge worked in HQ state		0.07	-0.10
		(0.89)	(-1.42)
Log(Months as judge)	0.04**	0.04***	0.04***
	(2.72)	(2.75)	(2.75)
Log(Judge age)	0.07	0.06	0.08
	(0.20)	(0.17)	(0.22)
Log(Total assets)	0.06***	0.06***	0.06***
	(4.13)	(4.18)	(4.08)
Leverage ratio	0.03***	0.03***	0.03***
	(3.73)	(3.73)	(3.62)
Profitability	0.07	0.07	0.07
	(0.99)	(1.05)	(1.05)
Log(Number of filings)	-0.01	-0.01	-0.00
	(-0.38)	(-0.42)	(-0.34)
Prepack	0.28***	0.28***	0.28***
	(6.04)	(6.22)	(6.00)
Industry fixed effects	Yes	Yes	Yes
Court-time fixed effects	Yes	Yes	Yes
Judge fixed effects	Yes	Yes	Yes
Observations	878	878	878
R^2	0.359	0.358	0.360

Table IA.9: Heterogeneous effects of judge characteristics: Number of employees

This table repeats the analysis in Table 8 for all four judge characteristics, estimating the effect of each on the probability that a firm emerges from bankruptcy separately for firms with below-median (Low) and above-median (High) employment. Columns 1–2 and 5–6 reproduce Table 8, and Columns 3–4 and 7–8 report the corresponding estimates for Democratic and home judges. The sample is split at the full-sample median of 2,700 employees, measured from the last 10-K before filing. The dependent variable is an indicator variable equal to one if a firm emerges from bankruptcy. Within each subsample, the specification is identical to the corresponding even-numbered column of Table 4. All columns include judge-level controls, firm-level controls, and industry (Fama–French 12) fixed effects. In addition, Columns 1–6 include court-by-year fixed effects, and Columns 7–8 include court-by-decade and judge fixed effects, which absorb *Male* and *Military*. Standard errors are clustered at the court level, and *t*-statistics are in parentheses. *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels, respectively. Appendix Table IA.1 defines all variables.

	Depression baby		Democrat		Economics-trained		Home judge	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Number of employees	Low	High	Low	High	Low	High	Low	High
Judge characteristic	0.05	0.10	0.07	-0.00	-0.04	-0.10***	-0.07	-0.09
	(1.05)	(1.58)	(1.67)	(-0.08)	(-0.75)	(-3.13)	(-0.74)	(-1.37)
Male	-0.05	-0.05*	-0.12***	-0.02	-0.03	-0.02		
	(-1.19)	(-1.95)	(-5.85)	(-1.28)	(-0.89)	(-0.99)		
Military	-0.00	0.01	0.00	0.00	-0.01	0.00		
	(-0.06)	(0.10)	(0.00)	(0.12)	(-0.19)	(0.04)		
Log(Months as judge)	-0.03	0.03**	-0.02	0.02	-0.03	0.05**	0.00	0.10***
	(-0.97)	(2.12)	(-0.94)	(1.47)	(-0.96)	(2.79)	(0.01)	(5.88)
Log(Judge age)	0.32*	-0.21	0.30***	0.00	0.37**	-0.07	0.46	-0.71
	(2.18)	(-1.08)	(5.28)	(0.01)	(3.06)	(-1.07)	(0.71)	(-1.17)
Log(Total assets)	0.06*	0.04**	0.04*	0.03***	0.06*	0.03**	0.05	0.04**
	(2.03)	(2.34)	(2.28)	(3.61)	(2.12)	(2.11)	(1.45)	(2.70)
Leverage ratio	0.03**	0.02	0.02	0.01	0.03**	0.01	0.04***	-0.01
	(2.63)	(1.49)	(0.99)	(0.48)	(2.81)	(1.18)	(4.21)	(-1.32)
Profitability	0.02	0.66*	-0.05	0.72	0.01	0.64*	0.08	0.45
	(0.27)	(1.94)	(-0.79)	(1.58)	(0.20)	(1.88)	(1.06)	(1.62)
Log(Number of filings)	-0.01	-0.04**	-0.00	-0.06***	-0.01	-0.04**	0.02	-0.04*
	(-1.19)	(-2.43)	(-0.09)	(-4.64)	(-1.21)	(-2.56)	(1.22)	(-2.00)
Prepack	0.29***	0.22***	0.26***	0.24***	0.29***	0.23***	0.31***	0.24***
	(7.70)	(10.55)	(10.75)	(7.83)	(7.77)	(10.23)	(4.56)	(7.66)
Industry fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Court-time fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Judge fixed effects	No	No	No	No	No	No	Yes	Yes
Observations	375	381	317	317	375	381	388	386
R^2	0.427	0.415	0.408	0.436	0.427	0.416	0.397	0.400

Table IA.10: Judge characteristics and emergence from bankruptcy: Excluding prepackaged and prenegotiated cases

This table repeats the analysis in Table 4 after excluding prepackaged and prenegotiated cases. The coefficient of *Prepack* is omitted because it equals zero for all cases. The dependent variable is an indicator variable equal to one if a firm emerges from bankruptcy. All columns include firm-level controls and industry (Fama–French 12) fixed effects. In addition, Columns 1–6 include court-by-year fixed effects, and Columns 7–8 include court-by-decade and judge fixed effects. Standard errors are clustered at the court level, and *t*-statistics are in parentheses. *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels, respectively. Appendix Table IA.1 defines all variables.

	Depression baby		Democrat		Economics-trained		Home judge	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Judge characteristic	0.15**	0.02	0.03	-0.04	-0.22***	-0.22***	-0.17***	-0.16***
	(2.59)	(0.17)	(1.02)	(-0.64)	(-3.54)	(-4.65)	(-3.09)	(-3.00)
Male		0.03		-0.02		0.05		
		(0.36)		(-0.62)		(0.84)		
Military		0.07		0.10*		0.06		
		(1.25)		(1.93)		(0.92)		
Log(Months as judge)		0.03		0.02		0.04**		0.06
		(1.46)		(0.57)		(2.30)		(1.37)
Log(Judge age)		0.24		0.36		0.15		-0.47
		(0.73)		(1.34)		(1.52)		(-0.47)
Log(Total assets)	0.05**	0.05**	0.04***	0.03**	0.05***	0.04**	0.07***	0.06***
	(2.80)	(2.39)	(3.73)	(2.33)	(2.85)	(2.19)	(4.20)	(3.93)
Leverage ratio	0.04**	0.04**	0.04*	0.03	0.04***	0.03**	0.04***	0.04**
	(2.61)	(2.39)	(2.00)	(1.61)	(3.88)	(2.79)	(3.03)	(2.63)
Profitability	0.12	0.11	0.11	0.10	0.11	0.10	0.12	0.11
	(0.90)	(0.88)	(0.82)	(0.71)	(0.73)	(0.67)	(0.88)	(0.83)
Log(Number of filings)	-0.02	-0.02	-0.04***	-0.04***	-0.02	-0.02	-0.01	-0.01
	(-1.09)	(-1.07)	(-6.12)	(-5.16)	(-0.99)	(-1.06)	(-0.51)	(-0.49)
Industry fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Court-time fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Judge fixed effects	No	No	No	No	No	No	Yes	Yes
Observations	480	480	359	359	480	480	521	521
R^2	0.375	0.382	0.373	0.384	0.375	0.393	0.353	0.358

Table IA.11: Judge characteristics and bankruptcy outcomes: Court-by-period fixed effects

This table repeats the analyses of emergence in Table 4 (Panel A), duration in Table 6 (Panel B), and post-emergence outcomes in Table 7 (Panel C), except that I use court-by-period fixed effects in place of court-by-year fixed effects (Columns 1–6 in Panels A and C; Columns 1–3 in Panel B) and court-by-decade fixed effects (Columns 7–8 in Panels A and C; Column 4 in Panel B). The filing periods are 1980–1984, 1985–1989, 1990–1995, 1996–2002, 2003–2009, 2010–2014, 2015–2019, and 2020 onward. Columns with *Home judge* also include judge fixed effects. All other aspects of the specifications are identical to those in the corresponding tables. The number of observations differs from the baseline tables because singleton observations are dropped: fewer in the columns that replace court-by-year fixed effects, because periods span several years, and more in the home-judge columns, because periods are shorter than decades. All columns include firm-level controls and industry (Fama–French 12) fixed effects. Standard errors are clustered at the court level, and t-statistics are in parentheses. *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels, respectively. Appendix Table IA.1 defines all variables.

Panels A and C, Columns 1–6: $Y_{ircjt} = \alpha_r + \alpha_{c\tau} + \beta JudgeChar_{jt} + \delta Z'_{jt} + \gamma X'_{ijt} + \varepsilon_{ircjt}$

Panels A and C, Columns 7–8: $Y_{ircjt} = \alpha_r + \alpha_{c\tau} + \alpha_j + \beta HomeJudge_{ij} + \delta Z'_{jt} + \gamma X'_{ijt} + \varepsilon_{ircjt}$

Here, $\alpha_{c\tau}$ denotes court-by-period fixed effects. Panel B adds $\widehat{Duration}_i$ and its interaction with the judge characteristic, as in Table 6.

Panel A: Emergence from bankruptcy

	Depression baby		Democrat		Economics-trained		Home judge	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Judge characteristic	0.09**	0.04	0.01	-0.01	-0.10**	-0.09**	-0.14**	-0.14**
	(2.50)	(0.94)	(0.52)	(-0.57)	(-2.10)	(-2.54)	(-2.39)	(-2.32)
Male		-0.00		-0.03**		0.01		
		(-0.06)		(-2.35)		(0.49)		
Military		0.01		0.01		0.00		
		(0.26)		(0.19)		(0.11)		
Log(Months as judge)		0.00		0.00		0.01		0.05***
		(0.08)		(0.07)		(0.61)		(3.13)
Log(Judge age)		0.14		0.24**		0.17**		-1.04*
		(1.22)		(2.78)		(2.06)		(-1.88)
Log(Total assets)	0.05***	0.05***	0.04***	0.04***	0.05***	0.05***	0.06***	0.06***
	(4.04)	(4.06)	(5.07)	(4.91)	(3.99)	(3.96)	(4.35)	(4.21)
Leverage ratio	0.04***	0.04***	0.03***	0.03**	0.04***	0.04***	0.03**	0.03**
	(3.09)	(3.06)	(3.18)	(2.79)	(3.12)	(2.97)	(2.46)	(2.47)
Profitability	0.05	0.05	0.04	0.04	0.04	0.05	0.01	0.00
	(0.80)	(0.82)	(0.61)	(0.56)	(0.66)	(0.72)	(0.18)	(0.03)
Log(Number of filings)	-0.01	-0.01	-0.02*	-0.02*	-0.01	-0.01	-0.02	-0.02
	(-0.89)	(-0.93)	(-1.85)	(-1.97)	(-1.00)	(-1.01)	(-1.32)	(-1.29)
Prepack	0.29***	0.29***	0.28***	0.28***	0.29***	0.29***	0.28***	0.28***
	(9.06)	(9.09)	(8.23)	(8.48)	(9.51)	(9.54)	(6.70)	(6.74)
Industry fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Court-period fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Judge fixed effects	No	No	No	No	No	No	Yes	Yes
Observations	1001	1001	758	758	1001	1001	851	851
R^2	0.284	0.285	0.280	0.284	0.283	0.287	0.392	0.395

Table IA.11: Judge characteristics and bankruptcy outcomes: Court-by-period fixed effects (continued)

Panel B: Duration in bankruptcy, interaction with predicted duration

	(1) Depression baby	(2) Democrat	(3) Economics-trained	(4) Home judge
Judge characteristic	-0.07	-0.03	0.42***	0.21
	(-0.63)	(-0.93)	(4.79)	(1.07)
Predicted Log(Months in Ch. 11)	-35.68***	-34.72***	-35.69***	-34.53***
	(-11.13)	(-10.73)	(-11.30)	(-9.97)
Judge char. × Predicted Log(Months in Ch. 11)	0.01	-0.00	-0.18***	-0.09
	(0.15)	(-0.06)	(-4.11)	(-1.22)
Male	-0.02	0.00	-0.03	
	(-0.92)	(0.37)	(-1.61)	
Military	0.00	0.04	0.00	
	(0.05)	(1.25)	(0.16)	
Log(Months as judge)	-0.02	-0.03	-0.02	-0.01
	(-1.18)	(-1.39)	(-1.31)	(-0.58)
Log(Judge age)	0.28**	0.26	0.20*	0.32
	(2.05)	(1.51)	(1.70)	(0.63)
Log(Total assets)	2.75***	2.68***	2.75***	2.67***
	(11.49)	(10.84)	(11.70)	(10.33)
Leverage ratio	-0.08	-0.05	-0.07	-0.09*
	(-1.69)	(-0.85)	(-1.66)	(-1.83)
Profitability	9.01***	8.75***	9.01***	8.70***
	(10.89)	(10.48)	(11.13)	(9.76)
Log(Number of filings)	-0.40***	-0.39***	-0.40***	-0.38***
	(-9.34)	(-8.17)	(-9.43)	(-8.77)
Prepack	-53.15***	-51.74***	-53.19***	-51.47***
	(-11.39)	(-10.98)	(-11.59)	(-10.24)
Industry fixed effects	Yes	Yes	Yes	Yes
Court-period fixed effects	Yes	Yes	Yes	Yes
Judge fixed effects	No	No	No	Yes
Observations	1001	758	1001	851
R^2	0.843	0.842	0.844	0.849

Table IA.11: Judge characteristics and bankruptcy outcomes: Court-by-period fixed effects (continued)

Panel C: Post-emergence outcomes

	Depression baby		Democrat		Economics-trained		Home judge	
	(1) Refile	(2) ROA (post)	(3) Refile	(4) ROA (post)	(5) Refile	(6) ROA (post)	(7) Refile	(8) ROA (post)
Judge characteristic	-0.03	-0.10	0.05	-0.03	0.01	0.03	0.02	0.05
	(-0.76)	(-1.41)	(1.69)	(-0.37)	(0.18)	(0.40)	(0.42)	(0.34)
Male	0.02	-0.17***	0.03	-0.12**	0.02	-0.17***		
	(0.62)	(-3.68)	(1.11)	(-3.22)	(0.49)	(-3.74)		
Military	0.00	-0.11	-0.05	-0.06**	0.00	-0.12*		
	(0.07)	(-1.67)	(-0.78)	(-2.96)	(0.06)	(-1.95)		
Log(Months as judge)	-0.01	0.02	0.01	0.03	-0.01	0.01	-0.01	0.02
	(-0.97)	(0.57)	(0.54)	(0.64)	(-1.07)	(0.44)	(-0.21)	(0.31)
Log(Judge age)	0.13	0.36*	-0.06	0.04	0.08	0.18	0.06	2.31*
	(1.38)	(1.84)	(-0.51)	(0.19)	(1.67)	(1.22)	(0.05)	(2.04)
Log(Total assets)	-0.02***	0.05***	-0.02***	0.06**	-0.02***	0.05***	-0.02*	0.07***
	(-2.79)	(4.14)	(-3.10)	(2.99)	(-2.86)	(4.28)	(-1.90)	(5.74)
Leverage ratio	-0.00	0.16***	0.00	0.23***	-0.00	0.17***	0.00	0.19***
	(-0.10)	(3.60)	(1.13)	(5.15)	(-0.08)	(3.42)	(0.46)	(4.03)
Profitability	0.01	-0.30**	-0.00	-0.29*	0.01	-0.30**	0.01	-0.26
	(0.10)	(-2.14)	(-0.03)	(-2.16)	(0.12)	(-2.16)	(0.15)	(-1.46)
Log(Number of filings)	0.00	0.03*	0.00	0.02	0.00	0.03*	0.00	0.04**
	(0.42)	(1.94)	(0.14)	(1.21)	(0.46)	(2.08)	(0.00)	(2.89)
Prepack	0.07***	-0.07	0.09***	-0.05	0.07***	-0.08	0.07***	-0.06
	(4.97)	(-0.94)	(6.38)	(-0.47)	(4.98)	(-0.92)	(3.95)	(-0.63)
Industry fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Court-period fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Judge fixed effects	No	No	No	No	No	No	Yes	Yes
Observations	681	320	517	236	681	320	548	251
R^2	0.183	0.323	0.182	0.309	0.183	0.321	0.274	0.410