Are Judges Randomly Assigned to Chapter 11 Bankruptcies? Not According to Hedge Funds

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Abstract

Analyzing 2010-2020 U.S. corporate bankruptcies, we find judicial assignment is not random, but predicted by hedge funds. In our setting, judges decide whether to convert Chapter 11 bankruptcies to liquidation, reducing unsecured creditor recovery. Relative to secured hedge funds, unsecured hedge fund creditors are assigned judges with lower past conversion rates and higher unsecured creditor recovery rates. Effects are greatest when hedge funds hold connections with the debtor's board or invested recently. Explaining these findings, we show judges are not assigned consecutive large cases, leading to predictability. Exploiting this pattern, we develop a novel recentered instrumental variable for causal identification.

Keywords: Chapter 11 Bankruptcy, Random Assignment of Judges, Hedge Funds, Unsecured Creditors

JEL Classification Codes: G23, G33, G34, K22

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"The bankruptcy system is supposed to work for everyone, but in many cases it works only for the powerful." —House Judiciary Committee Chairman Jerrold Nadler, July 28th, 2021

1 Introduction

Since Frank (1931), researchers have recognized that judicial outcomes are subject to the biases of the ruling judge. To alleviate concerns of fairness, courts in both the US and abroad claim to assign judges to individual court cases randomly (Abrams et al., 2012; Shayo and Zussman, 2011). From a policy perspective, randomization promotes public confidence in the judicial process by limiting forum shopping and the individual influence of any individual judge. From an academic perspective, recent empirical research in economics and finance exploits the random assignment of judges to causally identify of a wide range of legal outcomes.[1]

This paper revisits the claim of randomized judicial assignment in the context of U.S. Bankruptcy Court. In contrast to the past literature on forum shopping (LoPucki, 2010; LoPucki and Whitford, 1991), we instead analyze judicial assignment within the same district. Our research is motivated by legal scholarship arguing that debtors in recent cases are influencing judicial assignments (Levitin, 2021a,b), as well as renewed interest in these issues from policy makers and the public (Merle and Bernstein, 2019; Randles, 2020). Yet, despite the arguments, the extent of non-random judicial assignment is far from clear. For instance, after contacting all U.S. Bankruptcy Courts, Iverson et al. (2017) found that only one court (the Eastern District of Wisconsin) reports assigning cases to judges non-

[1] According to our own analysis, we count 22 papers published in the top economics journals (American Economic Review, Journal of Political Economy, and Quarterly Journal of Economics) since 2015 that exploit the random assignment of cases to judges. In addition, we count 6 papers published in the top finance journals (Journal of Finance, Journal of Financial Economics, and Review of Financial Studies) since 2018.

randomly.[2] In addition, a range of research including Chang and Schoar (2013), Bernstein et al. (2019b), Bernstein et al. (2019a), and Antill (2021) provides convincing evidence that debtor characteristics do not predict judicial assignments. Missing from this literature is any large-scale empirical evidence of non-random assignment. We aim to fill this gap.

To test the claim of randomized judicial assignment, we analyze the investments of active creditors made prior to a Chapter 11 bankruptcy filing. Past research finds active investors routinely influence a wide range of ex-post bankruptcy outcomes such as emergence and the structure of repayments (Ayotte et al.; Hotchkiss and Mooradian, 1997). Since bankruptcy judges have significant authority over these outcomes (Bris et al., 2006; Chang and Schoar, 2013), we argue investors have similar incentives to influence the assignment of cases. In addition, by focusing on investments made before the assignment of a bankruptcy judge, our technique is not suspect to standard critiques that predictability is merely an outcome of ex-post data mining; instead, in order for investors to systemically invest in firms that are later assigned a preferred judge, it must be possible to infer future judicial assignments.

Our analysis focuses on the investment decisions of hedge funds investing in private debt markets. Private debt investments have expanded dramatically as investments in private credit approached $600 billion globally by the end of 2016 and fund raising in private credit has grown 2.5 times the annual growth rate of private equity since 2010. Within this sector, distressed debt represents the largest investment strategy with 45% of all committed capital, and 43% of large corporate bankruptcies have one or more private debt funds acting as creditors Ivashina et al. (2016). As hedge funds are major investors in distressed firm debt (Aragon and Strahan, 2012), hedge funds routinely influence a wide range of bankruptcy outcomes including emergence and debt restructuring (Jiang et al., 2012; Lim, 2015). The prevalence of these investors allows us to explore a new channel of activism

[2] In line with prior findings, we also find no evidence that judges assignment depends on debtor size. We present these estimates in Figure 5.

in the distress debt market not yet studied by the hedge fund or bankruptcy literature: activist influence in judicial assignment process prior to filing.

We compare judges based on their individual propensity to convert Chapter 11 reorganizations to Chapter 7 liquidation similar to Bernstein et al. (2019b). While Chapter 11 results in a debtor developing a repayment plan for creditors, Chapter 7 leads to the debtor liquidating all assets (Bris et al., 2006; Chang and Schoar, 2013). Aggregating the judge conversion decisions for each judge over the prior three-year period, we develop a time-varying measure of a judge's propensity to convert a given case. We therefore evaluate whether filings involving a hedge fund creditor are consistently assigned a judge with a conversion rate different from filings without a hedge fund creditor. By focusing on the judge's past conversions, rather than the outcome of the current case, hedge funds must be influencing the assignment process itself and not the decisions of the judge following the assignment.

To identify non-random assignment, we exploit the fact that opposing regimes (reorganization vs. liquidation) lead to different repayment outcomes among creditors: secured creditors have a well-known liquidation bias (Ayotte and Morrison, 2009; Bergström et al., 2002; Vig, 2013), while unsecured creditors recover more under the repayment plan in reorganization (Antill, 2021; Bris et al., 2006). Further, Chang and Schoar (2013) find the judicial inclination to convert cases to liquidation is correlated with other pro-secured creditor decisions including (i) lifting an automatic stay and (ii) denying the extension of the exclusivity period. This distinction leads us to our empirical specification: we test whether unsecured hedge fund creditors are assigned a judge less likely to convert the case to a liquidation, relative to a similar debtor with a secured hedge fund creditor.

To begin our analysis, we collect data on the universe of U.S. Chapter 11 bankruptcy cases during 2010-2020 from court dockets. Second, we collect information on debtor characteristics including (i) industry, (ii) size, (iii) access to public equity markets, and (iv)

location. Third, for each filing, we also collect information on the bankruptcy outcomes including the (i) assigned judge, (ii) filing date and district, and (iii) conversion decision. Finally, we collect information on hedge fund debt investments in distressed firms, including debt terms, to determine whether a bankrupt firm had a hedge fund creditor at the time of filing.

Relative to other cases in the same year and court district, we estimate being assigned a judge with a 10 percentage point higher past conversion rate increases the likelihood a given case is converted to liquidation by 2.2 percentage points, equivalent to 22 percent of the mean conversion rate. To identify hedge fund creditors, we match cases to information on private debt agreements in the Preqin database. In total, we analyze nearly 20,000 case filings including nearly 600 cases with hedge funds acting as creditors at the time of bankruptcy filing.

In our baseline findings, we estimate that relative to a hedge fund acting as a secured creditor in the same court district and year, unsecured hedge funds are assigned a judge with a 3.3 percentage point lower mean conversion rate. As we estimate a mean judge conversion rate of 10%, we estimate a 33% reduction relative to the mean. The difference is statistically-significant at the one-percent level, holds after controlling for debtor characteristics, and is robust to excluding small- and medium-size debtors from the analysis. In addition, we find that unsecured hedge fund claimants are assigned a preferable judge more commonly when the hedge fund invested shortly before the bankruptcy filing, suggesting a portion of hedge funds choose to invest explicitly to influence the filing.

In order for creditor investments to predict future judicial assignment, creditors must be able to convince the debtor to file when optimal.[3] As equity holders and management have the same financial preferences for reorganization over liquidation as unsecured credi-

[3]Technically, a creditor may drive the debtor into an involuntary bankruptcy. However, involuntary bankruptcies are unable to explain our results as they comprise only 1% of corporate bankruptcies. We verify this argument in Table A1.

tors (Eckbo et al., 2016; White, 1989), we argue it is only unsecured creditors that should be able to influence the time of filing. In line with this argument we find no evidence that filings involving a secured hedge funds are assigned a different judge than otherwise similar cases. Furthermore, among the unsecured creditors, we show the effects are greatest when the hedge fund sits on the board of directors of the debtor at the time of filing, providing further support for the role of communication between debtor and creditor.

We next address three separate concerns with our analysis. First, it is possible our results are spurious. If true, we should find a judge's future conversion rate (after controlling for the past conversion rate) is also correlated with hedge fund investments. However, we find no evidence of any correlation, suggesting hedge funds are explicitly influencing judicial assignment based on information regarding past information judicial outcomes. Second, it is possible the assignment process is non-random for certain districts and this is public knowledge; our results may then be driven by this subset of districts. However, focusing on the subset of districts that explicitly state random assignment within their district (according to Iverson et al. (2017)), we continue to find hedge fund investments predict assignment. Third, our results may be insignificant from a policy standpoint. Therefore, to better understand the magnitudes of the coefficients, we compare our estimates to the advantage gained by unsecured hedge funds through traditional forum shopping. We estimate cases involving an unsecured hedge fund are filed in districts with a 3.4% lower judge conversion rate, nearly identical in magnitude to our baseline estimates of 3.3% discussed above. More directly, policy makers concerned over forum shopping should be similarly concerned over intra-district assignment mechanisms.

We next extend our analysis to an alternate bankruptcy outcome measure: the unsecured creditor recovery rate according to the confirmed plan. While we observe this measure for only a subsample of the full dataset, this measure allows us to examine variation within filings that are ultimately reorganized. As before, we estimate each judge's unse-

cured creditor recovery rate for previously assigned cases and continue to find (i) the past recovery rates of a given judge predict future recovery rates, (ii) unsecured hedge funds are far more likely to be assigned a judge with a high past unsecured recovery rate, and (iii) the coefficient is similar for the subsample of districts that explicitly state random assignment.

Given our results, it is natural to ask *how* hedge funds influence assignment. We first address one simple explanation: hedge funds gain access to preferable judges by influencing the choice of filing office within a district. In line with this argument, we find including office-district-year fixed effects explains roughly 45% of the unsecured hedge fund advantage. While significant, our results suggest hedge funds can still predict judicial assignment even within district offices. We then examine a separate argument: courts may be less inclined to assign multiple large cases to the same judge within a narrow window due to the required time and effort involved (Iverson et al., 2017). In line with this theory, we provide new evidence that large bankruptcy filings are negatively serially-correlated; being assigned a large bankruptcy in the previous week decreases the likelihood of being assigned a large bankruptcy filing this week. In contrast, small filings are not predictable from recent judicial assignments. In addition, we find evidence that unsecured hedge fund creditors exploit these patterns: while the filing dates of cases with unsecured hedge fund creditors can be partially explained by case assignments in the prior week, similar cases of secured hedge fund creditors do not exhibit these same patterns.

The results above suggest econometricians are capable of predicting judicial assignment similar to hedge funds. We therefore exploit these patterns to develop a recentered-instrument as discussed in Borusyak and Hull (2020) and Borusyak and Hull (2021). We recenter each judge's past conversion rate by controlling for: (i) the conversion rate of judges assigned large cases in the past 2-14 days, (ii) office-district-year fixed effects, and (ii) debtor controls. In line with the arguments above, we find recent assignments have no

predictive power over small cases, but are highly predictive of judicial assignment among larger debtors and this power increases with debtor size. We conclude by verifying the recentered-instrument continues to predict the conversion of larger cases, providing empirical researchers a valid technique to causally identify the effects of liquidation on large bankruptcy cases.

2 Literature Review

To our knowledge, we are the first to provide systematic evidence that judges are not randomly assigned to court cases. In addition, we believe we make contributions to three separate literatures. First, we contribute to past findings outlining the role of creditor activism in corporate distress (Chava and Roberts, 2008; Nini et al., 2009, 2012). More specifically, Hotchkiss and Mooradian (1997), Jiang et al. (2012), and Lim (2015) have already demonstrated hedge funds appear to influence bankruptcy outcomes:[4] our results instead provide a novel channel to explain *how* hedge funds can influence bankruptcy outcomes.

Second, a large recent literature exploits the random assignment of judges to bankruptcy filings. As our results question the validity of this assignment, we provide a new instrumental variable framework based on Borusyak and Hull (2020) and Borusyak et al. (2022) that recenters the instrument after controlling for predictability. Our strategy can be applied by future researchers to study bankruptcy outcomes as well as other settings where the assignment process may be partially predictable.

Third, our results add to research outlining policies to improve the bankruptcy judicial system. Legal and finance scholars have long recognized the potential for debtors to forum shop, or choose to file in a district with a pro-debtor bias (LoPucki, 2010; LoPucki

[4]For instance, when hedge funds are unsecured creditors, bankruptcies are more likely to (i) emerge as a standing firm, (ii) deviate from Absolute Priority Rule, and (iii) retain key employees.

and Whitford, 1991). Our results highlight a similar concern where parties attempt to influence judicial assignment within a district. In addition, Iverson et al. (2020) argues for an increase in the number of bankruptcy judges, based partially on evidence that judge busyness impacts decision-making (Iverson, 2018; Müller, 2022). Our findings highlight an additional benefit of more judges: reduced ability to predict assignment mechanisms.

3 Framework

3.1 Motivation

Bankruptcy is the legal process to resolve insolvency in the economy. Since 2000, there have been on average 35,000 U.S. corporate bankruptcies annually with a peak of over 60,000 in 2009. Bankruptcies can be either voluntary, where the debtor files the petition for protection, or involuntary, where the creditor files the petition, though only one percent of filings are involuntary. There are a total of 94 separate court districts, and parties can choose the district based on (i) place of incorporation, (ii) headquarters, or (ii) business revenues. Once a firm files for bankruptcy, the case is assigned to one of bankruptcy judges in that court district.

The U.S Bankruptcy code for corporations includes a role for both reorganization (Chapter 11) and liquidation (Chapter 7). Under Chapter 7, which composes roughly two-thirds of corporate bankruptcies, the assets of the firms are liquidated and the proceeds are used to pay creditors. The Chapter 7 process is largely overseen by an assigned trustee, who manages the payment of creditors. In contrast, firms filing Chapter 11 undergo a bargaining process between the debtor and creditors to restructure the firm and debt obligations. According to own analysis, Chapter 11 filings compose over 90% of all public firm bankruptcies.

Bankruptcy judges have significant influence on Chapter 11 bankruptcy outcomes, largely due to their authority to convert a Chapter 11 reorganization to Chapter 7 liquidation. In these instances, either the creditor or the assigned trustee files a petition to convert the case; the judge will then approve the petition if he/she believes the value of the debtor's assets are greatest under liquidation. Estimates from Bernstein et al. (2019b) find 40% of all Chapter 11 filings are converted by the assigned judge based on filings from 1992 to 2005. Antill (2021) argues that many conversions to liquidations are highly inefficient as they lead to lower recovery rates among creditors. Specifically, he finds that a statistician hired to compare the expected potential recovery rates across both regimes (reorganization and liquidation) and choose the better option would improve average recovery, across all cases, by 12 cents per dollar of debt claim.

These results suggest judges often make costly mistakes in the bankruptcy process, reducing recovery rates. A related concern is that judges vary in their decisions. Technically, judges are subject to a common criteria to decide whether to approve a conversion; however, in practice, these criteria appear largely up to individual interpretation. For instance, Bris et al. (2006) show that judge fixed effects account for 10% of conversion decisions. Similarly, Bernstein et al. (2019b) estimates the conversion rate separately for each bankruptcy judge and find that a one standard deviation in the conversion rate increases the likelihood of conversion by 7.5 percentage points compared to the unconditional propensity of 40.7 percentage points.

As judges differ in their inclination to convert a Chapter 11 to liquidation, creditors will have a preference for the assignment of one judge over another. However, this preference largely depends on whether the creditor is secured or unsecured. Finance researchers have long recognized that liquidation is optimal for secured creditors when the underlying collateral is more valuable than the partial debt repayments (Moore et al., 1993), Pulvino (1998), and Ayotte and Morrison (2009). In contrast, unsecured creditors benefit from re-

organizations: for instance, Bris et al. (2006) estimate an unsecured recovery rate of only 1.1% under Chapter 7 liquidation compared to a 52% recovery rate for unsecured debt under Chapter 11.[5] Similarly, (Ivashina et al., 2016) and Wang (2011) estimates creditors involved in filings converted to liquidation recover 22-25% less than creditors involved in reorganizations.

In addition, past research from Chang and Schoar (2013) finds a high correlation in pro-secured creditor decisions for the same judge. For instance, judges who are more likely to convert a case to liquidation are also far more likely to lift the automatic stay (which allows secured creditors to remove assets from the firm) or deny the extension of an exclusivity period (which allows creditors to submit their own restructuring plan). Our focus on conversion rates can then be interpreted as a more general proxy of a judge's preference for secured creditors relative to other judges.

Despite the benefits of being assigned a creditor-friendly judge, it is far from obvious creditors can influence judicial assignment within a court district. In fact, past research has argued assignment is fully-randomized and, therefore, unpredictable. For instance, Iverson et al. (2017) contacted all U.S. Bankruptcy Courts regarding the assignment process; of the 81 courts that responded, only one court (the Eastern District of Wisconsin) reports assigning cases to judges non-randomly. In addition, a number of papers including Chang and Schoar (2013), Bernstein et al. (2019b), Bernstein et al. (2019a), and Antill (2021) provides convincing evidence that assignment is not based on observable measures as debtor characteristics fail to predict judicial assignments.

[5]While these numbers are for all Chapter 7 filings, rather than conversions, recovery rates for Chapter 7 conversions are not statistically-difference from pure Chapter 7 cases.

3.2 Hypothesis Development

Based on the discussion above, both academics and practitioners disagree on whether or not the judicial assignment process is randomized within a court district or office. However, providing empirical evidence that judicial assignment is not random is complicated. Any correlation between filing characteristics and judicial assignment may simply be an outcome of ex-post data mining.

An alternate path forward is to test whether highly-sophisticated and active investors may be able to predict these assignments for their own financial benefit. The benefit of this strategy is that investors must be able to predict bankruptcy assignments ex-ante. There are three reasons to believe hedge funds may be among this set of sophisticated investors. First, research has already found hedge funds hold superior predictive powers (Cao et al., 2013; Jiang et al., 2007), largely explaining their superior performance. Second, hedge funds are highly active in the bankruptcy process, impacting a variety of outcomes including the likelihood of emergence of the firm (Jiang et al., 2012). Third, hedge funds trade frequently prior to the filing in an effort to concentrate their ownership and influence on bankruptcy outcomes including higher recovery rates for claimants (Ivashina et al., 2016).

Even if sophisticated creditors can predict judicial assignment, this ability has limited benefits if they cannot also influence the timing of the filing to increase the likelihood of a creditor-friendly assignment. Given 99% of corporate bankruptcy filings are voluntary, the exact timing of the bankruptcy filing is technically decided by the debtor, not the creditors, limiting their influence. Creditors can therefore only influence the timing indirectly by encouraging the debtor to file.

In this environment, we should expect creditors to have power in influencing bankruptcy timings only when their preferences align with the debtor. As equity holders are paid last, even after unsecured creditors, they have strong financial preferences for reorganization

(White, 1989). In addition to owning equity, incumbent CEOs also suffer substantial compensation loss under liquidation, furthering increasing their preference for reorganization (Eckbo et al., 2016). We should therefore expect that only unsecured creditors that can influence the timing of the filing, while secured creditors are limited in their abilities. Collectively, these arguments provide us with two testable hypotheses:

Hypothesis I: Relative to similar cases in the same court district, Chapter 11 filings involving an unsecured hedge fund creditor are less likely to be assigned a judge with strong inclinations to convert the case to Chapter 7.

Hypothesis II: Relative to similar cases in the same court district, Chapter 11 filings involving a secured hedge fund creditor are equally likely to be assigned a judge with strong inclinations to convert the case to Chapter 7.

3.3 Empirical Specification

We next develop our empirical methodology to test these hypotheses. Focusing exclusively on firms entering bankruptcy, our baseline specification allows us to test whether a bankrupt firm that borrows unsecured debt from a hedge fund is assigned a different judge than a bankrupt firm with a hedge fund acting as a secured creditor. To measure differences across judges, we estimate the conversion rate, or the fraction of Chapter 11 corporate bankruptcy cases assigned to a given judge that are converted to Chapter 7 liquidation. There are two benefits to explicitly distinguishing between hedge funds acting as unsecured vs. secured creditors. First, while secured hedge fund creditors will have a bias towards liquidation, unsecured hedge fund creditors will benefit from the reorganization. Second, as equity holders and managers benefit from reorganization over liquidation (similar to unsecured creditors), we should expect it is only unsecured creditors that can

influence the timing of the filing. Therefore, our baseline regression is:

$$\begin{aligned} \text{Judge Conversion Rate}_{it} &= \beta_1 \text{Unsecured Hedge Fund}_{it} + \beta_2 \text{Hedge Fund}_{it} \\ &+ \text{Court District FE} \times \text{Year FE} \\ &+ \mathbf{\Theta Debtor\ Controls} + \eta_{it} \end{aligned} \tag{1}$$

where i denotes each filing and t denotes the year. The dependent variable in our linear regression is $Judge\ Conversion\ Rate$. At each date, we estimate the conversion rate over the prior three-year period.[6] As our outcome variable is based on past judicial outcomes and not the current case, any relationship between dependent and independent variables cannot be explained by activist hedge funds influencing the judge's decisions on the current case. In this way, our focus differs from Jiang et al. (2012) and Lim (2015), who find hedge funds influence the outcomes of the case following the assignment of the judge.

The principal dependent variable is a simple binary variable, $Unsecured\ Hedge\ Fund$, which denotes at least one unsecured creditor prior to filing was a hedge fund. Based on our first hypothesis, we expect the prevalence of an unsecured hedge fund creditor will lead to the assignment of a judge with a lower conversion rate, or $\beta_1 < 0$.

In addition, we directly control for the influence of any hedge fund creditor, denoted $Hedge\ Fund$ in the equation, which denotes at least one creditor (secured or unsecured) prior to filing was a hedge fund. By including this control, we therefore estimate the additive influence of an unsecured hedge fund creditor relative to a secured hedge fund creditor. Assuming filings with unsecured hedge funds are similar to filings with secured hedge funds, a fact we confirm below, we can argue the primary difference between any effect is due to financial incentives of secured creditors relative to unsecured creditors. Based our

[6] In additional robustness tests, we introduce an alternate dependent variable, which measures a judge's mean unsecured creditor recovery rate for prior cases according to the approved reorganization plan.

second hypothesis, we expect the prevalence of a secured hedge fund creditor will have minimal effect on the judicial assignment, or $\beta_2 = 0$.

As we confirm in the analysis below, debtors with hedge fund creditors are different from other debtors. However, because our sample includes both public and private borrowers, we are unable to include a full set of potential control variables. Instead, we include three sets of controls. First, as hedge funds disproportionately invest in larger firms, we control for debtor size by including both (i) liability size fixed effects and (ii) asset size fixed effects. We create size fixed effects by splitting borrowers into ten bins. Second, we control for industry fixed effects. Third, we control for the headquarter location at the state-level. In addition, as judges are assigned at the district-level, we include court district fixed-effects interacted with filing year fixed effects. Last, we cluster errors at the level of the court district-year.

4 Data

4.1 Data Sources

4.1.1 Debtor Data

To test the hypotheses developed above, we first collect information on bankruptcy filings from two primary sources: (i) BankruptcyData.com and (ii) the Federal Justice Center Integrated Database. BankruptcyData provides both academics and practitioners access to business bankruptcy filings. Subscribers can query and export data from the database of business bankruptcy filing information. For our purposes, the BankruptcyData provides information on: (i) docket number, (ii) assigned judge, (ii) whether the debtor is public, (iv) debtor revenue, and (v) debtor NAICS industry.

Missing from this data is information on the outcome of the case. To overcome this challenge we collect additional information from the Federal Justice Center (FJC). The FJC, under an arrangement with the Administrative Office of the U.S. Courts (AOUSC), provides public access to the Integrated Database (IDB). The FJC receives regular updates of the case-related data that are routinely reported by the courts to the AOUSC. The FJC then post-processes the data, consistent with the policies of the Judicial Conference of the United States governing access to these data, into a unified longitudinal database, the IDB. For our purposes, the IDB provides information on whether the Chapter 11 reorganization was converted to a Chapter 7 liquidation. In addition, we collect information on the unsecured creditor recovery rate according to the confirmed reorganization plan. Last, we collect information on debtor assets and liabilities.

For each database above, we collect all Chapter 11 corporate bankruptcy cases filed between 2007 and 2020. We then match case filings from these two datasets using (i) docket number, (ii) filing date, and (iii) court district. While our empirical analysis detailed below focuses on cases filed between 2010 and 2020, we also collect information on cases filed in 2007-2009 to estimate each judge's conversion-rate (and unsecured recovery rate) over the prior three years.

4.1.2 Creditor Data

Up to the this point, we have no information on the creditors involved in each bankruptcy. Therefore, we match each bankrupt firm to its list of hedge funds acting as creditors from the Preqin Private Debt database. Preqin collects deal-level data through direct contact with industry professionals including fund managers, investors, and service providers. In addition to firms self-reporting information, Preqin's research analysts also monitor regulatory filings, make FOIA requests, and track industry news sources on a daily basis. We match the Preqin data to the filings using firm name and headquarter address. Preqin pro-

vides us creditor-level information, specifically whether the bankrupt firm was provided credit by a hedge fund and, if so, the characteristics of the debt contract.

For each filing with a hedge fund creditor, we match the filing to all other bankruptcies based on assets, liability, industry, and headquarter location. We have twelve buckets of asset and liability sizes and ten industry classifications. Observations are dropped if asset, liability, industry and headquarter buckets do not have a corresponding hedge fund match. After dropping these non-matched observations and bankruptcies assigned to judges with less than 4 previous cases, our total sample consists of 12,343 unique cases of chapter 11 filings including 569 cases with a hedge fund acting as creditor at the time of bankruptcy filing. That means, we have on average 30 non-hedge matches for each hedge fund filing. For our hedge fund matches we observe on average four hedge funds per bankruptcy. If there is more than one debt investment with a hedge fund before bankruptcy (i.e., more than one hedge fund invested in debt claims of a company before bankruptcy, or one hedge fund invested multiple times before bankruptcy) we include the hedge fund investment closest to the filing date. We choose to focus on one hedge fund investment per bankruptcy to avoid biases in our analyses due to duplicate observations of bankruptcies. To identify affiliates, we first look for companies that (i) are filed on the same day, (ii) are filed in the same district, (iii) share the same address and (iv) are assigned the same judge.

As discussed above, our analysis identifies creditors of bankrupt firms from the Preqin database. One concern with this approach is that we cannot confirm the creditors remain invested with the borrower at the time of the bankruptcy filing. To alleviate this issue, we collect the full creditor list for a subsample of the Chapter 11 bankruptcies in our dataset. Specifically, we focus on the set of corporate bankruptcies filed in 2019-2020, the final years of our sample; we choose to focus on a subsample as this data is collected directly from bankruptcy dockets and costly to collect for the full dataset. After collecting the data, we then match all creditors from the bankruptcy docket to the hedge funds available from

Preqin based on firm name and address. We estimate that 65% of the hedge funds invested in the borrower prior to the bankruptcy filing remain invested at the day of filing.

4.2 Filings with and without Hedge Fund Creditors

To summarize the data, we first plot the distribution of Chapter 11 filings by filing year in Figure 1. The visible increase in cases with a hedge fund creditor since 2010 is attributable to post-financial-crisis regulation including the adoption of Basel III and the passage of the Dodd-Frank Wall Street Reform and Consumer Protection Act, which placed increase capital requirements on the global banking system. As banks retreated, hedge funds stepped in to meet the capital needs of distressed companies. Similarly on the demand side, institutional investor appetite for private credit has grown in the last decade. Filings are highly cyclical both for filings with unsecured hedge funds (blue bars) and with secured hedge fund creditor (red bars). Last, Figure 1 shows that bankruptcies with secured and unsecured hedge funds are similarly distributed over the years with visibly more unsecured hedge funds linked to bankruptcies in 2020 compared to secured hedge funds.

Next, we compare the industrial composition of debtors in our data in (Figure 2). We find non-hedge fund creditors disproportionately invest in distressed firms operating in the financial sector. This discrepancy is largely explained by the underrepresentation of hedge funds as creditors in financial institutions during and shortly after the financial crisis. We find no notable differences in industries of bankruptcies between secured and unsecured hedge fund creditors, as shown in Panel B.

—Please see Figure 2—

Figure 3 shows that the vast majority of our observations are clustered in a few court districts. This is valuable for our empirical analysis, given our specification only compares

filings within the same court district and year. Specifically, we find the majority of cases involving hedge funds are disproportionately filed in: Delaware, New York - Southern, Texas - Northern, Texas - Southern, and Virginia Eastern. We find that bankruptcies with unsecured hedge funds are filed in Delaware to a larger extent than bankruptcies with secured hedge funds (see Panel B).

—Please see Figure 3—

Descriptive statistics of observable company characteristics at Chapter 11 filing date are reported in Table 1. Panel A shows statistics of bankruptcies with and without hedge funds. The mean firm with a hedge fund creditor at bankruptcy filing has $124 million in liabilities, $53 million in assets, $83 in unsecured claims. Both the $t-$statistic and the Wilcoxon statistic indicate that hedge funds target larger distressed firms and thus are part of bigger cases as compared to bankruptcies without a hedge fund creditor. Observable company characteristics are limited since the majority of these companies are private: 14% of the firms with a hedge fund creditor in the sample are public compared to 3% of firms without a hedge fund creditor.

—Please see Table 1—

Panel B of Table 1 compares bankruptcies with unsecured and secured hedge funds. The mean firm with an unsecured hedge fund creditor at bankruptcy filing appears to be slightly smaller than the mean firm with a secured hedge fund creditor, though these differences are only statistically-different in the case of liabilities. The percentages of investments in public companies are basically identical between secured and unsecured hedge funds.

The summary statistics above suggest that there remains significant differences between filings involving a hedge fund creditor and those without a hedge fund creditor.

However, this does not invalidate our analysis as our specification primarily compares filings involving hedge funds, but differ in whether the hedge fund is secured or unsecured. As shown in Table 1 and Figure 2 the mean firm appears to be similar with unsecured and secured hedge fund creditors, further validating our specification.

4.3 Hedge Fund Investments in Bankrupt and Non-Bankrupt Firms

In addition to comparing bankruptcy filings between unsecured and secured hedge fund creditors, it is valuable to examine the differences between hedge funds that could be matched to bankruptcies and those which could not be matched to a filing.

Table 2 compares the hedge funds included in Preqin that could be matched with Chapter 11 bankruptcy cases to hedge funds that did not invest in a bankrupt firm. We find minimal differences in the distribution of investment times between the included and excluded hedge fund data. Since the market of private debt funds grew rapidly since 2010, we see an overall increase in debt investments in more recent years. Most of these deals are large in size, similar to Jiang et al. (2012) who report that hedge funds are among the largest creditors at filing.

We find a slightly higher number of deals where the hedge fund is an unsecured claimant than in the matched sample. A large fraction of hedge funds acting as unsecured claimants in these bankruptcies is in line with Lim (2015) who finds that a majority of hedge funds obtain a creditor position by purchasing unsecured claims. Looking at raw numbers we see that hedge funds invest closer to the bankruptcy filing. In more than half of the bankruptcy cases with hedge fund involvement hedge funds invested no more than 5 years prior to bankruptcy filing.

—Please see Table 2—

4.4 Judge-Specific Conversion Rate

Our analysis tests whether bankruptcies with unsecured hedge fund creditors are assigned judges less likely to convert the case to Chapter 7 relative to bankruptcies with secured hedge fund creditors. Implicit in this hypothesis is that the past conversion rate of a given judge is predictive of the likelihood of future conversions. We test this assumption below.

To start, Figure 4 describes the distribution of judge conversion rates (Panel A) and the mean unsecured creditor recovery rate for each judge (Panel B). We document substantial variation in conversion rates across judges: within a given year, highlighting the role of judicial assignment in explaining bankruptcy outcomes.

—Please see Figure 4—

We next estimate whether the conversion rate of the assigned judge over the prior three years predicts the likelihood of conversion to Chapter 7. We estimate a median conversion rate of 14% and a rate of 7% (23%) at the 25th (75th) percentile. As judges are assigned at the district-level, we include court district fixed-effects interacted with filing-year fixed effects. Since we match bankruptcy filings with a hedge fund creditor to filings based on assets, liabilities, industry, and headquarter location, we control for all four observables with fixed effects (columns 2-4 of Table 3).

—Please see Table 3—

According to columns 1 and 2 of Table 3, we find a 10 percentage-point increase in the past three-year conversion-rate predicts a 2.2 percentage-point increase in the likelihood the judge converts a given current case. The relationship is statistically-significant as we estimate a t-statistic of 6.1. For comparison, Bernstein et al. (2019b) estimate a 10 percentage-point increase in a judge's conversion rate increases the likelihood of converting a given

case by 5.8 percentage points. The difference between these estimates is likely driven by different time samples as Bernstein et al. (2019b) focuses on the period 1992-2005 prior to the implementation of the Bankruptcy Abuse Prevention and Consumer Protection Act of 2005 (BAPCPA).

We next confirm our results hold even when focusing on larger borrowers more likely to have hedge fund creditors. In column 3, we focus on the subsample of borrowers with liabilities above the median (estimated at $600,000 for our sample), while in column 4, we focus on firms with assets above the median size (estimated at $400,000). For this subsample of filers, the coefficient actually increases slightly: we estimate a 10 percentage point increase in the past conversion-rate predicts a 2.7 percentage-point increase in the likelihood the case is converted.

Last, we verify the past literature by confirming standard debtor controls fail to predict judicial assignment. Specifically, we split all cases into ten subsets based on asset size (as well as liability size). We then estimate whether larger cases are systematically assigned a judge with a different conversion rate compared to smaller cases filed in the same district and year. The results are provided in Figure 5; Panel A split debtors based on asset size, while Panel B splits debtors by liabilities. Regardless of how we define size, we find no evidence size predicts judicial assignment, providing little evidence assignment is non-random.

—Please see Figure 5—

5 Results

We split our results into eight sections. First, we investigate whether filers with unsecured hedge fund creditors are assigned judges less likely to convert the case to Chapter 7. Sec-

ond, we consider alternative assignment mechanisms. Third, we evaluate alternative measures of judge conversion rates. Fourth, we evaluate heterogeneity in effects across filings. Fifth, we extend the analysis to an alternate bankruptcy outcome: unsecured creditor recovery rates according to the bankruptcy plan. Sixth, we confirm judge assignment is indeed predictable based on the recent past assignment of large bankruptcy cases. Seventh, we test whether hedge funds exploit the predictable patterns of judge assignments to time the date of filing. Eighth, we investigate the implications our of findings for instrumental variables approaches to identify the causal effect of liquidation.

5.1 Baseline Analysis

Given the past conversion rate of a judge is highly predictive of future decisions, we turn to our primary research question: are filings with hedge fund creditors assigned more creditor-friendly judges? As we can observe information about the debt tranche from the Preqin database, we split the hedge funds between secured and unsecured claimants. As pointed out by Jiang et al. (2012) and Lim (2015), unsecured creditors have a strong preference for reorganization because unsecured recovery rates are low following the payment of secured creditors in liquidation.

We present our findings in Panel A of Table 4. We set $Hedge\ Fund$ equal to one if at least one creditor is a hedge fund at the time of filing. We set $Unsecured\ Hedge\ Fund$ equal to one if at least one unsecured creditor is a hedge fund. In column 1, we include court district fixed effects interacted with year fixed effects. Relative to secured creditors, we estimate a filing with an unsecured hedge fund creditor is assigned a judge with a 3.3 percentage-point lower conversion rate (a 33% relative effect to the mean). The result is statistically-significant with a t-statistic of 3.77. In column 2, we also include fixed effects for firm asset size, liability size, industry, and headquarter location and find similar results.

—Please see Table 4—

In addition, it's important to note that our results hold regardless of the control firms included in the analysis. Since firms with hedge funds creditors are shown to be large (see Table 1), we confirm our results continue to hold after excluding small borrowers as measured by asset or liabilities below the median in columns 3 and 4. We again estimate that relative to secured creditors, filings with an unsecured hedge fund creditor are assigned a judge with a 3.3 percentage-point lower conversion rate.

Overall, the results support our first hypothesis: relative to filings involving secured hedge fund creditors, filings involving unsecured hedge fund creditors are assigned judges less inclined to convert a case to liquidation. We next test our second hypothesis: secured hedge fund creditors are assigned similar judges as similar filings not involving a hedge fund creditor. According to columns 1 through 4 of Panel A of Table 4, we estimate secured hedge funds are assigned a judge with a slightly higher (rather than lower) inclination to convert a case, though no effects are statistically-insignificant at the 10%-level. Therefore, our results support this second hypothesis.

5.2 Assignment Mechanism

5.2.1 Only Including Courts that Explicitly Claim Random Assignment

The results above suggest hedge fund investments predict future case assignments. This finding is only surprising if courts explicitly state their assignment is random. Our next analysis focuses on the subset of court districts that explicitly report randomizing judicial assignment at the level of the court district. We identify these court districts from Iverson et al. (2017), who contact court districts regarding details of their assignment process. By focusing on this subset of court districts, our sample size declines to 11,043 filings.

—Please see Table 5—

We present our finding in Panel A of Table 5. Column 1 includes court district-by-year fixed effects, while column 2 also includes borrower size fixed effects, as well as industry and headquarter location fixed effects. We estimate that relative to other filings involving a hedge fund, filings involving an unsecured hedge fund are assigned a judge with a 2.3 percentage-point lower past conversion rate. While the coefficient is smaller than estimated in Panel A of Table 4 (3.3 percentage-points), it remains statistically-significant at the 1%-level. In columns 3 and 4, we focus on borrowers above the median size (based on liabilities or assets); again the effect remains statistically-significant. Overall, the results suggest that while unsecured hedge are perhaps less capable of influencing judicial assignment among districts that explicitly claim randomized assignment, influence remains possible.

5.2.2 Controlling for Filing Office within Districts

A related explanation for our results is that while court districts state their assignment process occurs at the court district-level, the filing office still influences the assignment process. To test this hypothesis, we redo our baseline analysis, but include court district-by-office-by year fixed effects to explicitly compare bankruptcies filed within the same office. For reference, there are a total of 278 offices across the 93 districts included in our analysis.

—Please see Table 6—

We present our findings in Panel A of Table 6. According to columns 1 and 2, we estimate that relative to other filings involving a hedge fund, filings involving an unsecured hedge fund are assigned a judge with a 1.5-1.7 percentage-point lower past conversion rate, and the effects remains statistically-significant at the 1%-level. In columns 3 and 4, we focus on borrowers above the median size (based on liabilities or assets) and the coefficient

increases slightly to 1.7-1.8 percentage points. Overall, the findings suggest that roughly 45% of the hedge fund influence is explained by influencing the filing office of the case; in other words, over half of the hedge fund advantage remains even after comparing cases filed within the office.

5.3 Alternate Measures of Conversion Rates

Implicit in this analysis is that hedge funds can observe recent judicial conversion rates and will then influence judicial outcomes based on these observations. If this interpretation is correct, a judge's future decisions will not be correlated with hedge fund investments as future decisions are by definition not observable in the present. We test this argument in Panel B of Table 6.

To begin, we must first account for the finding presented in Table 3 that judge's differ in their propensity to convert cases. We therefore regress each judge's conversion over the three future years on the judge's conversion over the past three years to estimate the residual or unexplained component of future conversion rates. This residual is now our measure of the future 3-year conversion rate for a given judge unexplained by their past decisions. Using our standard framework, we then estimate whether filings involving an unsecured hedge fund investor are assigned judges with a different future conversion rate than similar filings with a secured hedge fund investor. As before, we continue to include court district fixed effects interacted with year fixed effects, as well as fixed effects for asset size, liability size, industry, and headquarter location. Across all specifications, we find no evidence that hedge fund investments (secured or unsecured) are correlated with the future outcomes of their assigned judge. The results support the theory that hedge funds are influencing case assignments based on their information regarding past judicial outcomes.

Second, we originally estimated a judge's propensity to convert Chapter 11 bankruptcy to Chapter 7 based on their mean conversion rate over the prior three-year period. For robustness, we confirm all results hold when we estimate judge conversion rates over the prior five-year period (see Table A1, Panel A). Our results are quantitatively-similar to the results in our baseline analysis using three-year conversion rates.

Last, we develop a binary conversion rate measure that takes a value if one if the assigned judge to the case holds the highest (lowest) three-year conversion rate among all judges assigned cases in that district and year. In Panel A of Table A2 of the appendix, we focus on the judge with highest conversion rate, while Panel B we focus on the judge with the lowest rate of conversion. In line with our prior findings, we find cases involving an unsecured hedge fund creditor are 12.2 percentage points less likely assigned the judge the highest conversion rate and 22.8 percentage points more likely to be assigned the judge with the lowest rate of conversion. The results are similar when focusing on the larger cases in the sample.

5.4 Heterogeneous Effects across Filing Characteristics

5.4.1 Do the Findings Differ Based on Time since Origination?

Assuming hedge funds invest in distressed firms in order to influence the filing date (and therefore the bankruptcy outcome), we should find a stronger relationship among more recent investors. We test this hypothesis below. We begin by excluding all filings with secured hedge fund creditors. We then split the remaining filings with unsecured hedge fund creditors into two equal groups: cases with a hedge fund investing just prior to the filing date (below the median time until filing) equal one and cases with a hedge fund investing long before the filing date (defined as above the median time until filing) are set to zero. We set $Unsecured\ Hedge\ Fund\ just\ before\ filing$ equal to one if at least one

unsecured hedge fund creditor invested just prior to the filing date (below the median time until filing). We set $Hedge\ Fund$ equal to one if at least one creditor is a hedge fund at the time of filing. We then estimate which group of unsecured hedge funds is more likely to be assigned a favorable judge.

We present our findings in Panel A of Table 7. Column 1 includes court district fixed effects interacted with year fixed effects, while column 2 also includes fixed effects for the firm's assets and liabilities, industry, and headquarter location. Across both specifications, we estimate unsecured hedge fund creditors investing shortly before the filing date are assigned a judge with a 2 percentage-point lower conversion rate compared to unsecured creditors investing long before filing. This difference continues to hold when we exclude smaller borrowers as measured by liabilities (column 3) or assets (column 4). Overall, the findings align with the hypothesis that hedge funds with more recent investments are more likely to influence judicial assignments.

—Please see Table 7—

To confirm our interpretation is correct, we conduct a similar analysis of secured hedge fund creditors. We first split secured hedge fund creditors into two equal groups based on the time between the initial debt investment and the bankruptcy filing date. We then evaluate the assignment of judges to secured hedge fund creditors in Panel B of Table 7. We find no evidence filings with a secured hedge fund creditors are assigned a different judge than filings without a hedge fund creditor, regardless of when the initial investment was first made. The results again confirm our second hypothesis that secured creditors are limited in their abilities to influence the timing of filing.

5.4.2 How Relevant are Relationships between the Creditor and Debtor?

Thus far, we find evidence that unsecured creditors, rather than secured creditors, influence judicial assignment. We argue this distinction is due to the fact that (i) unsecured creditors have the same preferences for reorganization as debtor managers and equity holders, and (ii) the debtor chooses the exact timing of the filing. If this interpretation is correct, our results should be stronger when unsecured creditors can more readily influence the choices of the debtor.

We test this theory by evaluating how relationships between the hedge fund creditor and debtor impact judicial assignment outcomes. Following Gilson (1990) and Kaplan and Minton (1994), we identify relationships as filings where the hedge fund holds a seat on the debtor's board of directors prior to the date of filing.We observe board composition from BoardEx, which provides details on public company boards and and senior managers. Given BoardEx does not include information on private firm, our analysis is restricted to hedge funds investing in public companies. Among public firm debtors, roughly half are directly connected to the hedge fund.

—Please see Table 8 —

We present our findings in Panel A of Table 8. When focusing on all cases, we estimate that relative to a similar unsecured hedge fund, unsecured hedge funds with a seat on the debtor's board are assigned a judge with a 0.4 percentage-point lower conversion rate, though the effect is not statistically-significant. However, when focusing exclusively on the subset of larger cases (based on liabilities or assets) in columns 3 and 4, we estimate a difference of 1.2-1.5 percentage points and the effect is statistically-significant. The smaller coefficient among smaller firms is likely due to small sample size as we can only identify board connections for public firms. In contrast, we find no evidence these dynamics are

present among secured creditors according to Panel B. Overall, the results provide further evidence for the role of relationships between unsecured creditors and debtors in driving our findings.

An alternative explanation of our findings is that our hedge fund creditors influence the timing of the filing directly through an involuntary filing. We argue involuntary bankruptcies are unlikely to be driving our results as only 1% of the filings in our sample are involuntary. To confirm this argument we replicate our findings after excluding all involuntary bankruptcies and present our findings in Table A1 Panel B of the Appendix. Results remain nearly unchanged to findings presented in Table 4.

5.5 Recovery Rate Analysis

A concern with our analysis thus far is that we focus exclusively on a single Chapter 11 bankruptcy outcome: conversion to Chapter 7. As discussed above, we focus on conversion rates as (i) judges have significant authority to convert cases and (ii) conversions have substantial impacts on creditor recoveries. However, the limitation with this strategy is that judges can influence case outcomes even when the case is not converted to liquidation. In this next section, we instead consider an alternative measure: the unsecured creditor recovery rate for reorganized cases. Our measure is collected from the Federal Justice Center (FJC) Integrated Database (IDB) and measures "the percentage dividend to be paid to the general class of unsecured debtors under the confirmed plan". We note the unsecured creditor recovery rate is only available for 3,174 of the 48,047 filings in our full dataset. Similar to our conversion analysis, we estimate a judge's mean recovery rate over cases in the prior three years and plot these estimates across judge in Panel B of Figure 4, as mentioned in subsection 4.2 . We estimate a median recovery rate of 22.9%, compared to a recovery rate of 6.7% (41.7%) at the 25th (75th) percentile.

Following the outline above, our analysis follows in two steps. First, we confirm a judge's mean recovery rate in prior cases is predictive of the recovery rate for future cases. We present our findings in Panel B of Table 3. According to the second column, we estimate a 10 percentage-point increase in the past recovery rates predicts a 2.81 percentage-point increase in the recovery rate of a current filing and the results is statistically-significant with a t-statistic of 2.22. This relationship is larger in both magnitude and statistical-significance when we focus exclusively on larger corporate filers (as measured by assets and liabilities).

Second, we evaluate whether unsecured hedge fund creditors are assigned a creditor-friendly judge relative to a secured hedge fund creditor. We present these results in Panel B of Table 4. According to the second column, we estimate that relative to secured hedge fund creditors, hedge funds acting as unsecured creditors are assigned a judge with a 28 percentage-point higher past recovery rate and the result is statistically-significant with a t-statistic of 2.0. When we focus on the subsample of larger borrowers (as measured by assets or liabilities above the median), we estimate an effect of 20-23% higher recovery rate and the result is statistically-significant at the 1%-level and 5%-level.

Third, we test whether our results continue to hold among the subsample of districts that explicitly claim random assignment at the district-level. We present these findings in Panel B of Table 5. We estimate that relative to secured hedge fund creditors, hedge funds acting as unsecured creditors are assigned a judge with a 27-30 percentage-point higher past recovery rate and the result is statistically-significant at 5%-level. Results are similar among the larger borrowers.

5.6 Comparison to Forum Shopping

While we evaluate whether parties can influence assignment with a court district, a prior literature focuses on forum shopping: the act of choosing a district to gain a more favor-

able court outcome. To better understand the magnitude of our effects, we compare our estimates to the advantages gained by unsecured hedge funds through forum shopping. More formally, we evaluate the specification:

$$\begin{aligned} \text{District Conversion Rate}_{it} &= \beta_1 \text{Unsecured Hedge Fund}_{it} + \beta_2 \text{Hedge Fund}_{it} \\ &+ \text{Year FE} + \text{Asset Size FE} + \text{Liability Size FE} \\ &+ \text{Industry FE} + \text{Headquarter FE} + \eta_{it} \end{aligned} \tag{2}$$

where i denotes each filing and t denotes the year. The dependent variable in our linear regression is then $District\ Conversion\ Rate$, measured as the conversion rate across all cases assigned to that district in the prior three years.

We provide our findings in Table 9. According to Panel A, we estimate that relative to a case involving a secured hedge fund, cases involving an unsecured hedge fund file in a district with a 3.4-4.3% lower past conversion rate. In Panel B, we estimate that relative to a case involving a secured hedge fund, cases involving an unsecured hedge fund file in a district with a 8-14% higher unsecured recovery rate, though the effect is not statistically-significant. Comparing this coefficient to our within-district analysis in Table 4, we find hedge fund influence of judicial assignment within a district is of a similar magnitude as more traditional forum shopping.

—Please see Table 9—

5.7 Hedge Fund Strategy

5.7.1 Serial Correlation of Cases

Given the findings thus far, it is necessary to understand how unsecured hedge fund creditors systematically invest in debtors assigned a judge unlikely to convert the case to liquidation. One simple explanation is that these cases are filed in different office than other cases; while we do find evidence of this theory in Table 6, unsecured hedge funds are still assigned a judge with a 1.7 percentage point lower conversion rate even when compared to similar cases filed in the same office. In other words, another strategy must be at play.

We next evaluate a separate theory: hedge funds can predict judicial assignments, making the assignment process non-random by definition. More specifically, we hypothesize a given judge is less likely to be assigned a large bankruptcy if they were recently assigned a prior large bankruptcy. This argument is motivated by descriptions of the assignment process provided by some districts. For instance, the North District of Iowa claims random assignment, but then explains that, "one simple method [of assignment] is to rotate the names of available judges." Alternatively, the Northern District of Illinois describes one method of assignment is to shuffle a deck of cards featuring the name of each active judge an equal number of times and then choose assignment based on the next card. In both accounts, assignment is predictable once participants can observe recent case filings readily available from court dockets.

To test the validity of this theory, we estimate the linear probability model:

$$\begin{aligned} \text{Case}_{jt} &= \beta \text{Large Case}_{jt-1} + \text{Judge FE} + \boldsymbol{\Theta} \text{Debtor Controls} \\ &+ \text{Court FE} \times \text{Office FE} \times \text{Year FE} + \varepsilon_{jt} \end{aligned} \tag{3}$$

where j denotes the judge who is assigned a case in week t. The dependent variable $Case$ is a binary variable equaling one if a case was assigned to judge j in week t. The independent variable $Large\ Case$ is defined as a binary variable equal to one if the assets (Panel A of Table 10) or liabilities (Panel B of Table 10) in the judge's prior case were below the median (column 1), above the median (column 2)/75th (column 3)/90th (column 4) percentile. Under the hypothesis that judges are less likely to be assigned cases directly after being assigned a large case, we expect $\beta < 0$. To control for differences in debtors, we include fixed-effects for liability, asset size, industry, and headquarter location. In addition, we include judge fixed-effects to focus on variation across time periods for the same judge. As we are interested in explaining variation not explained by the choice of office, we include district-by-office-by-year fixed effects.

—Please see Table 10—

Column (1) of Table 10 shows that the assignment of a new case is statistically unrelated to a previous week's case assignment if that case was small (assets or liabilities below the median). In contrast, larger case assignments made to a judge in the prior week decrease the likelihood of being assigned a Chapter 11 filing this week, and this effect increases with the size of the case in week $t-1$ (columns 2-4). The results are statistically-significant at the 1%-level and qualitatively similar whether case size is measured from liabilities or assets.

The result are qualitatively similar when we conduct the analysis under a piecewise exponential multiple-failure survival framework, where h_{ijt} is the hazard for Chapter 11 filing i assigned to judge j in week t. We choose a proportional hazard model with fixed effects as standard logistic regression models are subject to incremental parameter bias. In line with the linear probability model, we subdivide time at the weekly-level. We assume that the baseline hazard is constant in each week, leading to a piecewise exponential model (see Table A3 of the appendix).

5.7.2 Timing of Bankruptcy Filings

The specification above evaluates whether judicial assignment are predicted by assignments in the prior week. We next test whether hedge funds influence the timing of filings based on this predictability. Implicit in this analysis is the assumption that unsecured hedge funds encourage debtors to submit the filing directly after a pro-liquidation judge is assigned a large case. Our specification is therefore:

$$\begin{aligned} \text{Unsecured HF filing}_{ht} &= \beta \text{Large Case Assigned to High Conversion Rate Judge}_{ht-1} \quad (4) \\ &+ \boldsymbol{\Theta} \text{Debtor Controls} + \text{Hedge Fund FE} + \varepsilon_{ht} \end{aligned}$$

where h denotes the unsecured hedge fund linked to a filing in week t. Thus, the dependent variable $Unsecured\ HF\ filing$ is a binary variable equaling one if an unsecured hedge fund is part of the creditors in a case filed in week t. The independent variable $Large\ Case\ Assigned\ to\ High\ Conversion\ Rate\ Judge$ is defined as a binary variable equal to one if a judge with a conversion rate above the median was assigned a large case in the prior week. To control for differences in debtors, we include fixed-effects for liability, asset size, industry, and headquarter location. In addition, we include hedge fund fixed-effects to focus on variation across time periods for the same hedge fund. Finally, we include district-by-office-by year fixed effects to explicitly compare bankruptcies filed within the same office.

—Please see Table 11—

Table 11 reports coefficients of these linear probability models. Column (1) shows that a Chapter 11 filing involving an unsecured hedge fund is not more likely if a high conversion-rate judge has been assigned a small case (assets or liabilities below the median) in the prior week. We find the probability of filing increases when a large case was

assigned to a judge with a high conversion rate in the prior week $t-1$ (columns 2-4). The results are statistically-significant at the 1%-level. Last, we find no similar relationship holds for hedge funds acting as secured creditors as shown in Table A4 of the appendix.

In addition, we run the analysis under a piecewise exponential multiple-failure survival framework and present the results in Table A5 and Table A6 from the appendix. We define h_{iht} as the hazard for Chapter 11 filing i of hedge fund h in week t. We continue to find cases involving an unsecured hedge fund are more likely to be filed in the week after a large case is assigned a low-conversion rate judge, but no similar relationship among secured hedge funds. Overall, the results support the hypothesis that hedge funds exploit the predictability of assignments by influencing the timing of the bankruptcy filing.

5.8 Implications for Instrumental Variables

A large recent literature including (Bernstein et al., 2019b; Bris et al., 2006; Chang and Schoar, 2013), and Antill (2021) exploit the random assignment of bankruptcy judges to Chapter 11 filings to study the causal implications of bankruptcy rulings. We next evaluate the implications of our findings for future researchers relying on this same identification strategy.

While we find evidence judicial assignment is partially predictable, and therefore not random, it does not follow that this empirical approach is never valid. First, Table 10 finds no evidence of predictability among smaller Chapter 11 bankruptcy cases. Second, larger cases may still be valid to study if we can separate the random and non-random component of the assignment process. In line with this argument, Figure 5 finds that larger cases are not systemically assigned different judges from smaller cases filed in the same district.

Our approach to develop a valid instrument of judicial assignment is based on the re-centering approach developed in Borusyak and Hull (2020) and extended in Borusyak

and Hull (2021). The authors develop an econometric approach when some, but not all, treatments are generated by a true or natural experiment. Their solution starts with an instrument z_i which predicts variation in x_i by combining exogenous shocks from the experiment with a non-random measure of shock exposure. The authors then adjust z_i by its expected counterfactual outcome to purge any omitted variable bias arising from the variation in the non-random shock exposure.

Applying the idea to our own setting, we argue the exposure of a given case to the randomized judicial assignment mechanism is dependent on the assignments of recent large cases. We must therefore adjust the assigned judge's past conversion rate by the counterfactual outcome when studying conversion to liquidation. To estimate the counterfactual outcome, $Expected\ Judge\ ConversionRate_{it}$ for case i on date t, we regress the assigned judge's conversion rate on (i) the mean judge conversion rate among all large assigned in the past 2-14 days, (ii) office-district-year fixed effects, and (iii) debtor controls, and then collect the estimated coefficients on each set of variables. The specification is therefore:

$$\begin{aligned} \text{Exogenous Variation in Judge Conversion Rate}_{it} &= \text{Judge Conversion Rate}_{it} \\ &- \text{Expected Judge Conversion Rate}_{it} \end{aligned}$$

We control for recent judicial assignments based on the results from Table 10 that large cases assignments are predictable based on recent filings. We only compare cases filed within the same district office based on evidence from Table 6 that some assignment occurs at the office, rather than district, level.

We note our approach differs from the approach outlined in Borusyak and Hull (2020) and Borusyak and Hull (2021) as they estimate the expected instrument by averaging across counterfactual outcomes. While this approach is valid when the counterfactual is not time-varying, our setting assumes the counterfactual depends on recent case as-

signments and therefore must vary across time. These approaches are equivalent when the econometrician knows the assignment process. When the process is not known, as is our case, our approach is only an approximation to the original econometric approach. Nonetheless, we hope this technique provides at least a partial solution for empirical researchers when studying corporate bankruptcy outcomes.

We present our results in Table 12. We must make three decisions for our analysis. First, we must decide the subset of firms included in our analysis. Assuming small cases are not predictable, we focus on filings with assets above $1 million, in column (2) and then increase this restriction to $2 million (column 3), $3 million (column 4), $4 million (column 6), and $5 million (column 6). The advantage of focusing on larger cases is that assignment is more likely non-random, but at the expense of including fewer cases in the estimation. Second, we must decide how recent large cases influence future assignment. In Panel A, we assume only cases filed in the prior 2 days impact the assignment process, while Panel B (C) assumes cases filed in the past 7 days (14 days) impact assignment. Focusing on a narrow time frame allows us to focus exclusively on recent cases, but at the cost of fewer observations, especially in districts where large cases are relatively rare. Third, and in line with the first decision, we assume it is only the recent assignment of cases larger than $1 million in assets that impact future assignments.

—Please see Table 12—

Column (1) of each panel confirms that recent large case assignments have no predictive power on small cases, regardless of whether the past case was assigned in the last two days (Panel A), seven days (Panel B), or fourteen days (Panel C). In contrast, we find that the conversion rate of judges assigned large cases in the past two days is negatively correlated with the conversion rate of the judge assigned with a large case today. The effect is increasing with the asset size of the case today so that the coefficient is estimated at -5.4

percentage points for a cases with at least $1 million in assets and increases to -14.2 percentage points for cases with $5 million or more in assets (Panel A). In addition, this effect is smaller for cases filed in the past seven days (Panel B), and decreases further for cases filed in the past two weeks (Panel C). In unreported results, we find little evidence cases assigned more than two weeks ago predict future assignments.

With these coefficients, we can evaluate the potential benefits of influencing the timing of the filing. We first estimate a 6 percentage point difference in weekly conversion rates within a court district in an average month. Assuming filers can choose the week of filing within the month, we then estimate filers can decrease the conversion rate of the assigned judge among the largest cases (those with assets above $5 million) by roughly 0.6 percentage point according to Panel B ($10.4\% \times 6\%$). Assuming filers can instead choose the optimal two-day period (instead of week) to file, filers are assigned a judge with a 1 percentage point lower conversion rate based on the coefficients in Panel A.

Overall, the result suggest (i) large cases are predicted by recent filings, and (ii) predictive power is larger for larger cases and more recent past filings. Based on these findings, we conclude this section by verifying our recentered instrument remains valuable for future empirical research. We present our findings in Table A7 of the appendix. For simplicity, we focus on cases above $1 million in assets (Panel A) and $5 million in assets (Panel B), though the results hold for cases with assets from $1 million to $5 million as in Table 12). Columns (1) and (2) consider the standard instrumental specification in this literature, which estimates the relationship between the past conversion rate of the assigned judge and the decision to convert case i to liquidation. Column (3) and (4) instead estimate the relationship between the recentered past conversion rate of the assigned judge (estimated from the coefficients in Table 12) and the decision to convert case i. All columns include district-by-year fixed effects and the second and fourth columns also includes debtor characteristics.

—Please see Table A7—

We find the recentered instrumental variable approach (Columns 3-4) provides a nearly identical coefficient and T-statistic as the traditional instrumental variables strategy (Columns 1-2). The results confirm the value of the recentered IV approach for future researchers interested in identifying the causal effects of liquidation on corporate bankruptcy outcomes. More broadly, the results highlight the value of the approach for any setting exploiting judicial assignment in which the assignment process may be partially predictable.

6 Conclusion

The random assignment of judges to court cases promotes fairness, minimizes forum shopping, and is routinely exploited for causal identification by economists. Analyzing U.S. corporate bankruptcy filings between 2010 and 2020, we provide the first evidence that assignment is not random, but predicted by the lending decisions of hedge funds. In our setting, judges can decide whether to convert a Chapter 11 bankruptcy to a Chapter 7 liquidation; while secured creditors have a preference for liquidation, unsecured creditors generally recover more under reorganization. Exploiting this distinction, we show that relative to secured hedge funds, unsecured hedge fund creditors are significantly less likely to be assigned a judge with a tendency to convert Chapter 11 cases. Effects are largest when the hedge fund has connections with the debtor's board or invested recently, and the gains are similar to the benefits of forum shopping. Explaining these findings, we show judges are not assigned multiple large cases within a small time window, allowing hedge funds to influence the filing date and ultimately judicial assignment. A recentered IV approach that excludes the predictable component of judicial assignment provides researchers an alternative identification strategy.

The results above find strong evidence judicial assignment is not random, but can be influenced by sophisticated parties. The first, and simplest, solution to improve judicial fairness is for policy makers to develop a truly randomized process. The downside of this proposal is that some judges may at times be assigned several large filings in a short time period. This is a concern as past research finds judge busyness impacts judicial decisions (Iverson, 2018; Müller, 2022). Alternatively, and following the suggestions of Iverson et al. (2020), policy makers can instead increase the number of bankruptcy judges. In this scenario, creditors will lose their predictability powers even if assignment is not fully randomized. Policy makers intent on a more fair judicial system should consider both proposals.

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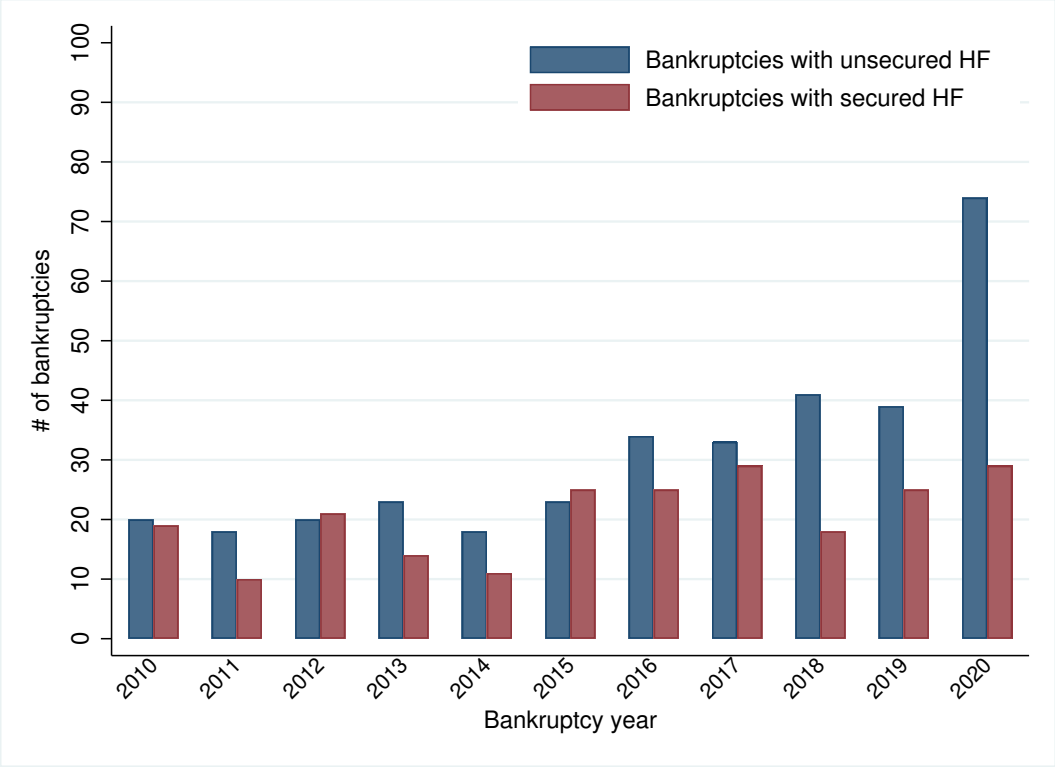
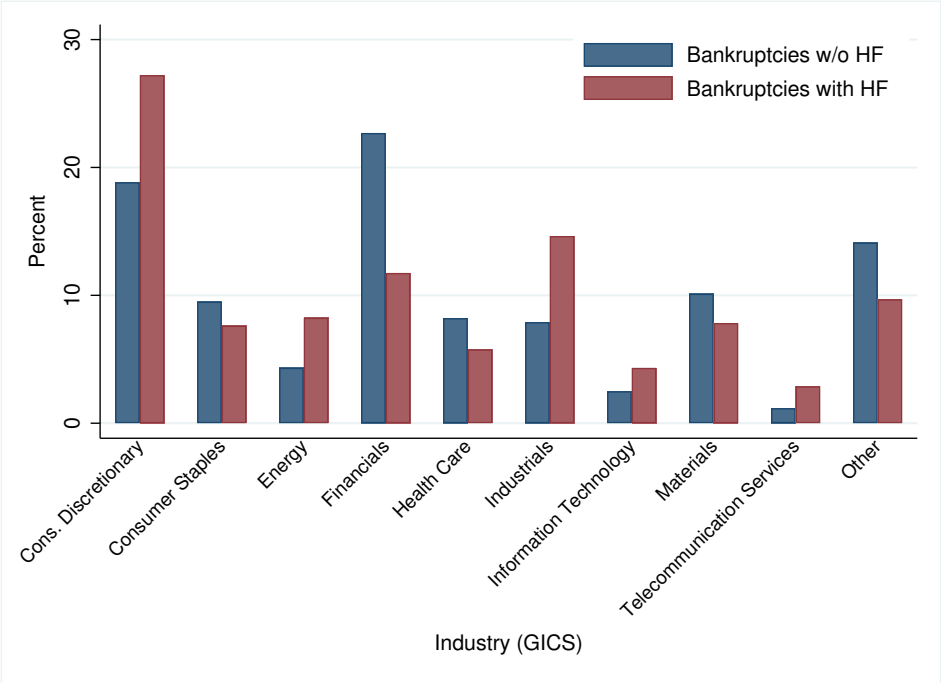
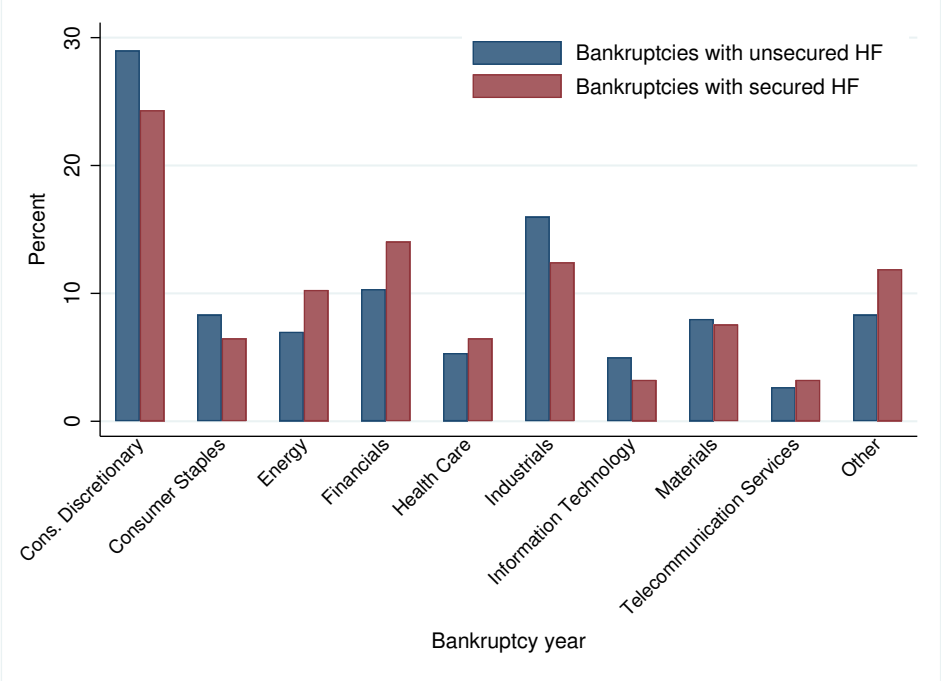


Figure 1: Histogram of Hedge Fund Investors (Unsecured, Secured) across Time

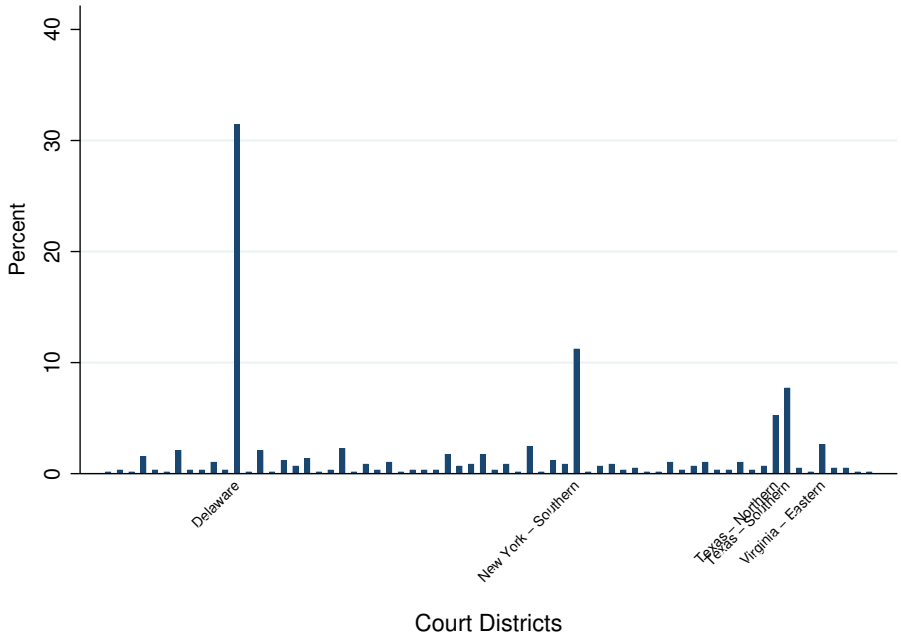


(a) **Panel A:** Bankruptcies with(out) Hedge Fund

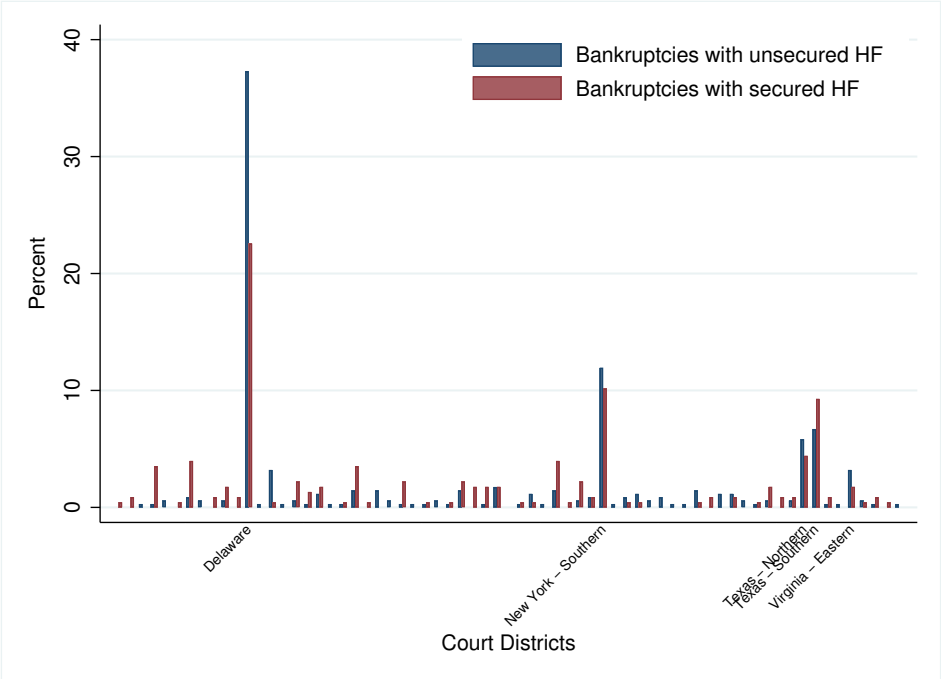


(b) **Panel B:** Bankruptcies with (Un)secured Hedge Fund

Figure 2: Filings across Industry

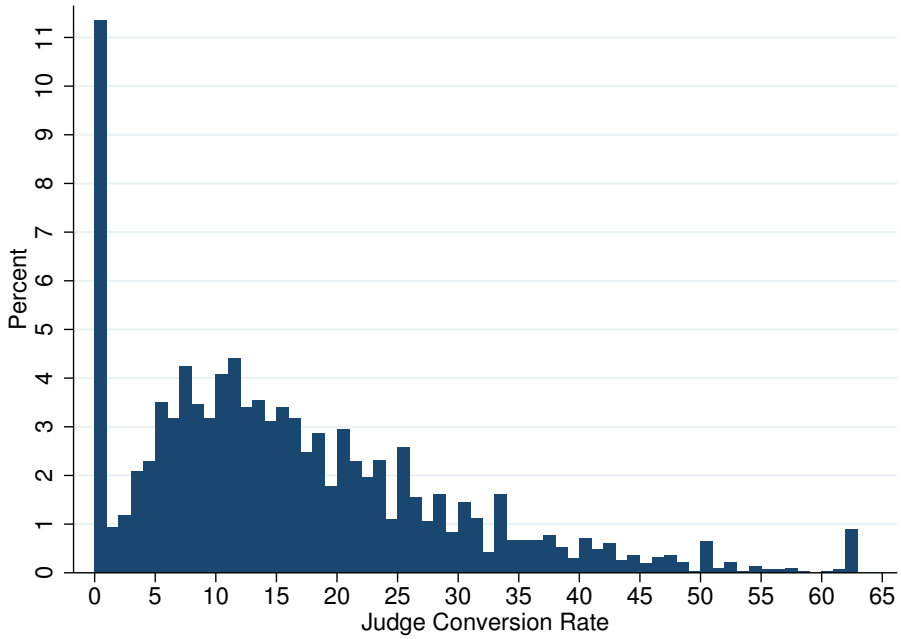


(a) **Panel A:** Bankruptcies with(out) Hedge Fund

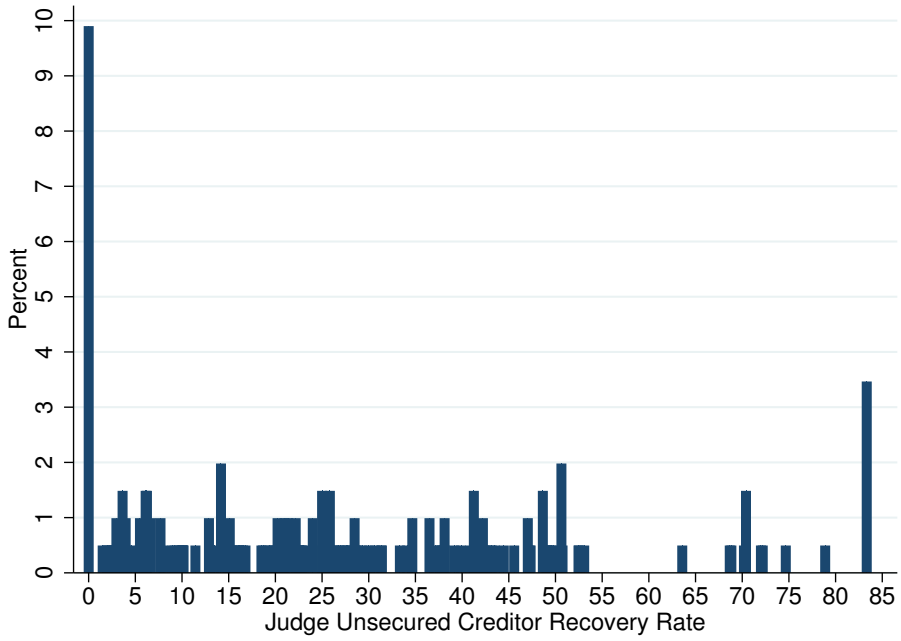


(b) **Panel B:** Bankruptcies with (Un)secured Hedge Fund

Figure 3: Filings across Court District

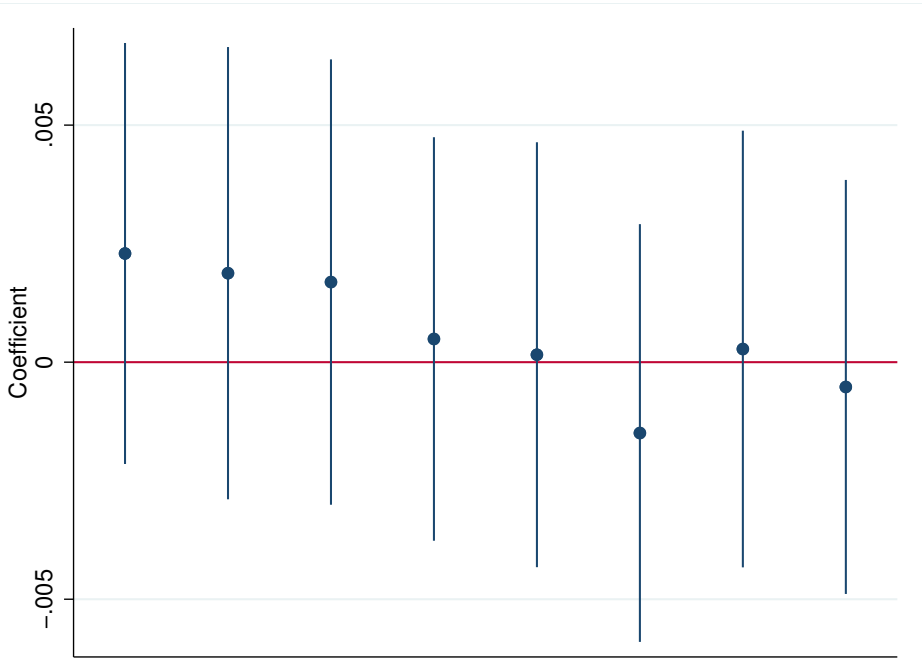


(a) **Panel A:** Mean Conversion Rate for each Judge

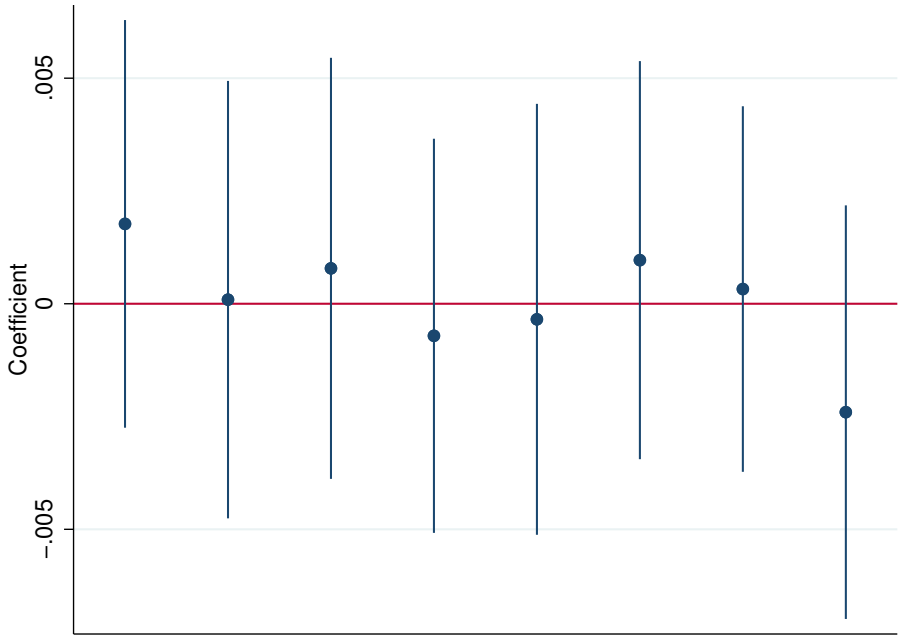


(b) **Panel B:** Mean Unsecured Creditor Recovery Rate for each Judge

Figure 4: Histogram of Judges' Conversion Rate and Unsecured Creditor Recovery Rate



(a) **Panel A:** Asset Size Deciles



(b) **Panel B:** Liability Size Deciles

Figure 5: Differences in Judicial Assignment across Debtor Size

In this figure, we evaluate whether a given bankruptcy outcome can be predicted by the asset/liability size of the debtor. In our regression specification, the dependent variable is *Convert to Chapter 7*, a binary variable that denotes whether the bankruptcy was converted to a Chapter 7 liquidation. In Panel A, the independent variables are fixed effects for each asset size decile, while in the Panel B the independent variables are fixed effects for each liability size decile. The regression also includes district-by-year fixed effects. Confidence bounds are estimated at the 5%-level. We cluster standard errors at the court district-year level.

Table 1: Summary statistics of Bankruptcies with(out) Hedge Fund

This table presents characteristics of bankruptcies with a hedge fund (HF) (569 observations) and without a HF creditor (16,656 observations) (Panel A) and characteristics of bankruptcies with unsecured hedge funds and secured hedge funds where bankruptcies occurred between 2010 and 2020. The first seven columns of Panel A report the mean, standard deviation, min, p25, median, p75, and max of the characteristics for bankruptcies with a HF (unsecured hedge fund in Panel B). Columns 6, 7 and 8 of Panel B report the mean of characteristics for bankruptcies without a HF (unsecured hedge fund in Panel B), $t-$statistics of differences in mean, Wilcoxon rank-sum tests, and Pearson χ^2-tests (in braces) of the null hypotheses that distributions of the two samples are identical. Assets, liabilities, unsecured claims, and debt investments are reported in million US dollars LTM before the bankruptcy filing date.

Panel A: Bankruptcies with(out) Hedge Funds

	Bankruptcies with HF							Bankruptcies w/o HF		
	Mean	SD	Min	p25	Median	p75	Max	Mean	$t-$stat of Diff.	Wilcoxon (χ^2-test)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Liabilities	124	247	0	0	2	42	5,931	27	-9.23	-5.50
Assets	53	144	0	0	0.037	4	9,866	11	-6.85	1.84
Unsecured Claims	83	199	0	0.048	1	14	5,931	14	-6.46	-6.02
Firm is public?				14%					3%	(-15.53)

Panel B: Bankruptcies with (Un)secured Hedge Funds

	Bankruptcies with unsecured HF							Bankruptcies with sec. HF		
	Mean	SD	Min	p25	Median	p75	Max	Mean	$t-$stat of Diff.	Wilcoxon (χ^2-test)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Liabilities	55	491	0	0	4	93	5,931	169	2.73	2.80
Assets	19	438	0	0	0.052	8	9,866	75	1.51	1.62
Unsecured Claims	23	502	0	0.038	2	28	5,931	122	1.66	1.98
Debt investment	174	1602	1	18	80	250	21,475	166	0.73	1.32
Firm is public?				14%					13%	(0.39)

Table 2: Details of Hedge Fund Data

This table reports the number of hedge funds (HFs) and HF deals matched with chapter 11 bankruptcies and the universe of HF/deal data in Preqin as of 01/1/2021, with investment years between 1996 and 2020. If there was more than one HF deal before bankruptcy (i.e., more than one HF invested in debt claims of a company before bankruptcy, or one HF invested multiple times before bankruptcy) we only consider the HF investment closes to bankruptcy.

	Sample: Fund and deal data	Preqin: excl. sample data
# of funds	138	1,743
# of HF deals	569	16,556
Inv. times		
— 1996-1999	4	221
— 2000-2004	30	909
— 2005-2010	179	3619
— 2011-2015	223	4753
— 2016-2020	133	6915
Debt investment		
— # of deals with size $>$ 1b	49	1,054
— # of deals with size 300m $<$ size $\leq$ 1b	245	8,635
— # of deals with 50m $<$ size $\leq$ 300m	129	2,793
— # of deals with 10m $<$ size $\leq$ 50m	93	2,631
— # of deals with size $<$ 10m	53	1,443
Tranche		
— Percent of deals with unsecured debt	60%	54%
Inv. time to bankruptcy		
— more than 10 years	65	
— between 5 years and 10 years	137	
— between 0 and 5 years	366	

Table 3: Do Judge Conversion/Recovery Rates Predict Future Case Outcomes?

In this table, we evaluate whether a given bankruptcy outcome can be predicted by the assigned judge's prior case outcomes. In Panel A, the dependent variable is *Convert to Chapter 7*, a binary variable that denotes whether the bankruptcy was converted to a Chapter 7 liquidation. The primary independent variable is *Judge Conversion Rate*, the judge's conversion-rate over the prior three-year period. In Panel B, the dependent variable Judge Recovery Rate, a continuous variable denoting the recovery rate for unsecured debt according to the confirmed reorganization plan. The primary independent variable is *Judge Recovery Rate*, the judge's recovery-rate over the prior three-year period. We use * to denote significance at the 10% level, ** to denote significance at the 5% level, and *** to denote significance at the 1% level. We cluster standard errors at the court district-year level.

Panel A: Conversion to Chapter 11

	(1) All	(2) All	(3) L $\geq$ p50 ($600k)	(4) A $\geq$ p50 ($400k)
Judge Conversion Rate	0.222***	0.220***	0.255***	0.257***
	(0.037)	(0.037)	(0.043)	(0.042)
Asset FE	No	Yes	Yes	Yes
Liability FE	No	Yes	Yes	Yes
Headquarter FE	No	Yes	Yes	Yes
Industry FE	No	Yes	Yes	Yes
Court FE $\times$ year FE	Yes	Yes	Yes	Yes
Observations	48894	48354	33400	31977
Adj. R^2	0.077	0.088	0.075	0.073

Panel B: Unsecured Creditor Recovery Rate

	(1) All	(2) All	(3) L $\geq$ p50 ($600k)	(4) A $\geq$ p50 ($400k)
Recovery Rate	0.281**	0.323***	0.342**	0.353**
	(0.125)	(0.120)	(0.141)	(0.142)
Asset FE	No	Yes	Yes	Yes
Liability FE	No	Yes	Yes	Yes
Headquarter FE	No	Yes	Yes	Yes
Industry FE	No	Yes	Yes	Yes
Court FE $\times$ year FE	Yes	Yes	Yes	Yes
Observations	3185	3179	2301	2237
Adj. R^2	0.468	0.483	0.357	0.359

Table 4: Are Unsecured Hedge Fund Creditors Assigned More Favorable Judges?

In this table, we evaluate whether filings with an unsecured hedge fund creditor are assigned different judges than a similar filing with a secured hedge fund creditor. In Panel A, the dependent variable is *Judge Conversion Rate*, a continuous variable that denotes the conversion rate of the assigned judge over the prior three years. In Panel B, the dependent variable is *Judge Recovery Rate*, a continuous variable that denotes the unsecured creditor recovery rate of the assigned judge over the prior three years. The primary independent variable is *Unsecured Hedge Fund*, a binary variable denoting whether the filing is associated with a hedge fund acting as an unsecured creditor. The independent variable, *Unsecured Hedge Fund*, is a binary variable denoting whether the filing is associated with a hedge fund acting as a creditor (secured or unsecured). We use * to denote significance at the 10% level, ** to denote significance at the 5% level, and *** to denote significance at the 1% level. We cluster standard errors at the court district-year level.

Panel A: Judge Conversion Rate

	(1) All	(2) All	(3) L $\geq$ p50 ($600k)	(4) A $\geq$ p50 ($400k)
Unsecured Hedge Fund	-0.033***	-0.032***	-0.031***	-0.032***
	(0.008)	(0.008)	(0.011)	(0.011)
Hedge Fund	0.004	0.002	0.005	0.005
	(0.007)	(0.007)	(0.009)	(0.010)
Asset FE	No	Yes	Yes	Yes
Liability FE	No	Yes	Yes	Yes
Headquarter FE	No	Yes	Yes	Yes
Industry FE	No	Yes	Yes	Yes
Court FE $\times$ year FE	Yes	Yes	Yes	Yes
Observations	12343	12078	8725	8347
Adj. R^2	0.499	0.496	0.498	0.494
Mean of Dep. Variable	0.110	0.112	0.118	0.118

Panel B: Judge Recovery Rate

	(1) All	(2) All	(3) L $\geq$ p50 ($600k)	(4) A $\geq$ p50 ($400k)
Unsecured Hedge Fund	27.842**	22.087**	23.178***	19.978**
	(12.564)	(10.041)	(8.189)	(9.895)
Hedge Fund	-9.822	-8.330	-9.200	-9.312
	(10.922)	(9.686)	(8.317)	(8.812)
Asset FE	No	Yes	Yes	Yes
Liability FE	No	Yes	Yes	Yes
Headquarter FE	No	Yes	Yes	Yes
Industry FE	No	Yes	Yes	Yes
Court FE $\times$ year FE	Yes	Yes	Yes	Yes
Observations	674	673	454	433
Adj. R^2	0.699	0.725	0.708	0.727
Mean of Dep. Variable	24.529	24.562	28.413	28.587

Table 5: Are Judge Assignments Really Random in these Districts ?

In this table, we evaluate whether filings with an unsecured hedge fund creditor are assigned different judges than a similar filing with a secured hedge fund creditor. In contrast to Table 4 we only include the 89 bankruptcy districts which are stated to be randomly assigned to one of the bankruptcy judges according to Iverson (2019). In Panel A, the dependent variable is *Judge Conversion Rate*, a continuous variable that denotes the conversion rate of the assigned judge over the prior three years. In Panel B, the dependent variable is *Judge Recovery Rate*, a continuous variable that denotes the unsecured creditor recovery rate of the assigned judge over the prior three years. The primary independent variable is *Unsecured Hedge Fund*, a binary variable denoting whether the filing is associated with a hedge fund acting as an unsecured creditor. The independent variable, *Unsecured Hedge Fund*, is a binary variable denoting whether the filing is associated with a hedge fund acting as a creditor (secured or unsecured). We use * to denote significance at the 10% level, ** to denote significance at the 5% level, and *** to denote significance at the 1% level. We cluster standard errors at the court district-year level.

Panel A: Judge Conversion Rate

	(1) All	(2) All	(3) L $\geq$ p50 ($600k)	(4) A $\geq$ p50 ($400k)
Unsecured Hedge Fund	-0.023***	-0.020**	-0.018**	-0.016**
	(0.007)	(0.008)	(0.007)	(0.006)
Hedge Fund	0.002	0.002	0.006	0.005
	(0.006)	(0.006)	(0.010)	(0.010)
Asset FE	No	Yes	Yes	Yes
Liability FE	No	Yes	Yes	Yes
Headquarter FE	No	Yes	Yes	Yes
Industry FE	No	Yes	Yes	Yes
Court FE $\times$ year FE	Yes	Yes	Yes	Yes
Observations	11043	9251	6569	6269
Adj. R^2	0.486	0.484	0.481	0.476
Mean of Dep. Variable	0.111	0.111	0.117	0.118

Panel B: Judge Recovery Rate

	(1) All	(2) All	(3) L $\geq$ p50 ($600k)	(4) A $\geq$ p50 ($400k)
Unsecured Hedge Fund	27.362**	29.690**	15.892**	15.817*
	(11.812)	(11.489)	(7.856)	(8.580)
Hedge Fund	-21.623**	-21.560**	-14.746**	-14.436*
	(10.721)	(9.630)	(7.364)	(7.605)
Asset FE	No	Yes	Yes	Yes
Liability FE	No	Yes	Yes	Yes
Headquarter FE	No	Yes	Yes	Yes
Industry FE	No	Yes	Yes	Yes
Court FE $\times$ year FE	Yes	Yes	Yes	Yes
Observations	616	531	356	340
Adj. R^2	0.704	0.750	0.706	0.719
Mean of Dep. Variable	23.195	21.906	26.083	26.028

Table 6: Are our Findings Robust to Alternative Data and Specifications?

In this table, we evaluate whether filings with an unsecured hedge fund creditor are assigned different judges than a similar filing with a secured hedge fund creditor. In Panel A, the dependent variable is *Judge Conversion Rate*, a continuous variable that denotes the conversion rate of the assigned judge over the prior three years. In Panel B, the dependent variable is *Residual Judge Conversion Rate*, a continuous variable that denotes the conversion rate of the assigned judge over the future three years after controlling for the past three years. That means, Panel B reports results for the residual of the 3-year future conversion rate regressed on the 3-year past conversion rate as dependent variable. The primary independent variable is *Unsecured Hedge Fund*, a binary variable denoting whether the filing is associated with a hedge fund acting as an unsecured creditor. The independent variable, *Hedge Fund*, is a binary variable denoting whether the filing is associated with a hedge fund acting as a creditor (secured or unsecured). Panel A displays estimates with Court × Office × Year fixed effects. We use * to denote significance at the 10% level, ** to denote significance at the 5% level, and *** to denote significance at the 1% level. We cluster standard errors at the court district-year level.

Panel A: Court FE × Office FE × Year FE

	(1) All	(2) All	(3) L ≥ p50 ($600k)	(4) A ≥ p50 ($400k)
Unsecured Hedge Fund	-0.015***	-0.017**	-0.018**	-0.017***
	(0.006)	(0.007)	(0.007)	(0.005)
Hedge Fund	0.000	0.005	0.010	0.009
	(0.006)	(0.006)	(0.011)	(0.012)
Asset FE	No	Yes	Yes	Yes
Liability FE	No	Yes	Yes	Yes
Headquarter FE	No	Yes	Yes	Yes
Industry FE	No	Yes	Yes	Yes
Court FE × year FE	Yes	Yes	Yes	Yes
Observations	12259	12259	8734	8359
Adj. R^2	0.647	0.648	0.637	0.644
Mean of Dep. Variable	0.111	0.111	0.117	0.118

Panel B: Residual of 3-year Future Conversion Rate

	(1) All	(2) All	(3) L ≥ p50 ($600k)	(4) A ≥ p50 ($400k)
Unsecured Hedge Fund	0.002	-0.004	0.005	0.004
	(0.009)	(0.011)	(0.011)	(0.011)
Hedge Fund	-0.000	0.004	-0.006	-0.002
	(0.008)	(0.010)	(0.010)	(0.011)
Asset FE	No	Yes	Yes	Yes
Liability FE	No	Yes	Yes	Yes
Headquarter FE	No	Yes	Yes	Yes
Industry FE	No	Yes	Yes	Yes
Court FE × year FE	Yes	Yes	Yes	Yes
Observations	9566	8000	5837	5573
Adj. R^2	0.387	0.400	0.410	0.412
Mean of Dep. Variable	-0.046	-0.047	-0.045	-0.045

Table 7: Does Judicial Assignment Depend on the Time since Initial Hedge Fund Investment?

In this table, we evaluate whether assignment of a favorable judge depends on whether the unsecured (secured) hedge fund invested shortly prior to bankruptcy filing. The dependent variable is *Judge Conversion Rate*, a continuous variable that denotes the conversion rate of the assigned judge over the prior three years. The primary independent variable is *Unsecured* (*Secured*) *HF investing just before filing*, a binary variable denoting whether the associated unsecured secured) hedge fund associated with the filing invested below the median time to filing. The independent variable, *Unsecured* (*Secured*) *Hedge Fund*, is a binary variable denoting whether the filing is associated with a hedge fund acting as a creditor. We use * to denote significance at the 10% level, ** to denote significance at the 5% level, and *** to denote significance at the 1% level. We cluster standard errors at the court district-year level.

Panel A: Unsecured Creditor

	(1) All	(2) All	(3) L $\geq$ p50 ($600k)	(4) A $\geq$ p50 ($400k)
Unsecured HF investing just before filing	-0.020***	-0.021***	-0.020***	-0.021***
	(0.005)	(0.005)	(0.007)	(0.007)
Unsecured Hedge Fund	-0.019**	-0.019**	-0.015	-0.014
	(0.008)	(0.008)	(0.011)	(0.011)
Asset FE	No	Yes	Yes	Yes
Liability FE	No	Yes	Yes	Yes
Headquarter FE	No	Yes	Yes	Yes
Industry FE	No	Yes	Yes	Yes
Court FE $\times$ year FE	Yes	Yes	Yes	Yes
Observations	12117	11853	8582	8215
Adj. R^2	0.498	0.494	0.495	0.491
Mean of Dep. Variable	0.110	0.111	0.117	0.118

Panel B: Secured Creditor

	(1) All	(2) All	(3) L $\geq$ p50 ($600k)	(4) A $\geq$ p50 ($400k)
Secured HF investing just before filing	0.002	-0.001	-0.003	-0.006
	(0.011)	(0.011)	(0.014)	(0.013)
Secured Hedge Fund	0.002	0.005	0.014	0.021
	(0.014)	(0.014)	(0.017)	(0.018)
Asset FE	No	Yes	Yes	Yes
Liability FE	No	Yes	Yes	Yes
Headquarter FE	No	Yes	Yes	Yes
Industry FE	No	Yes	Yes	Yes
Court FE $\times$ year FE	Yes	Yes	Yes	Yes
Observations	12010	11750	8509	8146
Adj. R^2	0.497	0.494	0.496	0.492
Mean of Dep. Variable	0.112	0.113	0.119	0.120

Table 8: Does Judicial Assignment Depend on the Relationship between the Hedge Fund and Debtor?

In this table, we evaluate whether assignment of a favorable judge depends on whether the unsecured (secured) hedge fund holds a prior connection with the board of the debtor. The dependent variable is *Judge Conversion Rate*, a continuous variable that denotes the conversion rate of the assigned judge over the prior three years. The primary independent variable is *Unsecured* (*Secured*) *HF with Board Connections*, a binary variable denoting whether the associated unsecured secured) hedge fund associated with the filing has a prior connection with the board of the debtor. This is the case for about 50 percent of public borrowers. The independent variable, *Unsecured* (*Secured*) *Hedge Fund*, is a binary variable denoting whether the filing is associated with a hedge fund acting as a creditor. The independent variable, *Public Borrower*, is a binary variable denoting whether the filing is associated with a hedge fund invested in a public company. We use * to denote significance at the 10% level, ** to denote significance at the 5% level, and *** to denote significance at the 1% level. We cluster standard errors at the court district-year level.

Panel A: Unsecured Creditor

	(1) All	(2) All	(3) L $\geq$ p50 ($600k)	(4) A $\geq$ p50 ($400k)
UHF with Board Connection	-0.004	-0.004	-0.012**	-0.015***
	(0.004)	(0.004)	(0.006)	(0.006)
Unsecured Hedge Fund (UHF)	-0.029***	-0.029***	-0.025***	-0.023***
	(0.006)	(0.006)	(0.007)	(0.007)
Public Borrower	0.003	0.003	0.002	0.002
	(0.003)	(0.003)	(0.004)	(0.004)
Asset FE	No	Yes	Yes	Yes
Liability FE	No	Yes	Yes	Yes
Headquarter FE	No	Yes	Yes	Yes
Industry FE	No	Yes	Yes	Yes
Court FE $\times$ year FE	Yes	Yes	Yes	Yes
Observations	12343	12343	8790	8412
Adj. R^2	0.499	0.500	0.501	0.496
Mean of Dep. Variable	0.110	0.110	0.117	0.118

Panel B: Secured Creditor

	(1) All	(2) All	(3) L $\geq$ p50 ($600k)	(4) A $\geq$ p50 ($400k)
SHF with Board Connection	-0.007	-0.007	-0.019	-0.019
	(0.015)	(0.015)	(0.016)	(0.017)
Secured Hedge Fund (SHF)	0.007	0.007	0.010	0.011
	(0.008)	(0.008)	(0.010)	(0.011)
Public Borrower	0.001	0.001	0.001	0.001
	(0.003)	(0.003)	(0.004)	(0.005)
Asset FE	No	Yes	Yes	Yes
Liability FE	No	Yes	Yes	Yes
Court FE $\times$ year FE	Yes	Yes	Yes	Yes
Observations	12343	12343	8790	8412
Adj. R^2	0.498	0.498	0.500	0.496
Mean of Dep. Variable	0.110	0.110	0.117	0.118

Table 9: Do Unsecured Hedge Fund Creditors File in more Favorable Districts?

In this table, we evaluate whether filings with an unsecured hedge fund creditor are filed in a different court district than a similar filing with a secured hedge fund creditor. In Panel A, the dependent variable is *Court-District Conversion Rate*, a continuous variable that denotes the conversion rate of the court district over the prior three years. In Panel B, the dependent variable is *Court District Recovery Rate*, a continuous variable that denotes the unsecured creditor recovery rate of the court district over the prior three years. The primary independent variable is *Unsecured Hedge Fund*, a binary variable denoting whether the filing is associated with a hedge fund acting as an unsecured creditor. The independent variable, *Hedge Fund*, is a binary variable denoting whether the filing is associated with a hedge fund acting as a creditor (secured or unsecured). We use * to denote significance at the 10% level, ** to denote significance at the 5% level, and *** to denote significance at the 1% level. We cluster standard errors at the court district-year level.

Panel A: District Conversion Rate

	(1) All	(2) All	(3) L $\geq$ p50 ($600k)	(4) A $\geq$ p50 ($400k)
Unsecured Hedge Fund	-0.043***	-0.034**	-0.039***	-0.040***
	(0.012)	(0.012)	(0.009)	(0.011)
Hedge Fund	0.003	0.007	0.011	0.012
	(0.012)	(0.009)	(0.009)	(0.009)
Asset FE	No	Yes	Yes	Yes
Liability FE	No	Yes	Yes	Yes
Headquarter FE	No	Yes	Yes	Yes
Industry FE	No	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Observations	16621	16350	11799	11311
Adj. R^2	0.095	0.315	0.321	0.320
Mean of Dep. Variable	0.112	0.113	0.118	0.119

Panel B: District Recovery Rate

	(1) All	(2) All	(3) L $\geq$ p50 ($600k)	(4) A $\geq$ p50 ($400k)
Unsecured Hedge Fund	13.949	8.121	2.199	-2.790
	(8.717)	(6.500)	(3.825)	(3.721)
Hedge Fund	-6.138	-3.273	-2.406	-0.227
	(5.954)	(4.754)	(3.642)	(2.299)
Asset FE	No	Yes	Yes	Yes
Liability FE	No	Yes	Yes	Yes
Headquarter FE	No	Yes	Yes	Yes
Industry FE	No	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Observations	807	806	572	543
Adj. R^2	0.080	0.694	0.663	0.669
Mean of Dep. Variable	23.201	23.160	25.691	25.798

Table 10: Do Past Judicial Assignments Predict Future Assignments?

In this table, we analyze whether a judge who has been assigned a large case in week $t-1$ is less likely to be a case assigned a case in week t. We conduct a panel data analysis with fixed effects, with a binary dependent variable that takes the value of one if a judge is assigned a case in week t, and zero otherwise. The independent variable, *A<p50,* (*A≥p50*) (*A≥p75*), (*A≥p90*), is a binary variable that takes the value of one if the case is below the median, or above the median (p75, p90) asset case (Panel A). The independent variable, *L<p50,* (*L≥p50*) (*L≥p75*), (*L≥p90*), is a binary variable that takes the value of one if the case is below the median, or above the median (p75, p90) liability case (Panel B). We use * to denote significance at the 10% level, ** to denote significance at the 5% level, and *** to denote significance at the 1% level. We cluster standard errors at the court district-office-year level.

Panel A: Case Size Based on Assets

	(1)	(2)	(3)	(4)
$<$ 400k asset case in week $(t-1)$/A$<$ p50	0.006			
	(0.009)			
$\geq$ 400k asset case in week $(t-1)$/A$\geq$ p50		-0.012**		
		(0.006)		
$\geq$ 2m asset case in week $(t-1)$/A$\geq$ p75			-0.016***	
			(0.006)	
$\geq$ 5m asset case in week $(t-1)$/A$\geq$ p90				-0.026***
				(0.008)
Judge FE	Yes	Yes	Yes	Yes
Liability FE	Yes	Yes	Yes	Yes
Asset FE	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes
Headquarter FE	Yes	Yes	Yes	Yes
Court FE $\times$ office $\times$ year FE	Yes	Yes	Yes	Yes
# of weeks	77215	77215	77215	77215

Panel B: Case Size Based on Liabilities

	(1)	(2)	(3)	(4)
$<$ 600k liability case in week $(t-1)$/L$<$ p50	0.004			
	(0.008)			
$\geq$ 600k liability case in week $(t-1)$/L$\geq$ p50		-0.012**		
		(0.006)		
$\geq$ 1m liability case in week $(t-1)$/L$\geq$ p75			-0.016***	
			(0.006)	
$\geq$ 10m liability case in week $(t-1)$L$\geq$ p90				-0.029***
				(0.009)
Judge FE	Yes	Yes	Yes	Yes
Liability FE	Yes	Yes	Yes	Yes
Asset FE	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes
Headquarter FE	Yes	Yes	Yes	Yes
Court FE $\times$ office $\times$ year FE	Yes	Yes	Yes	Yes
# of weeks	77215	77215	77215	77215

Table 11: Do Unsecured Hedge Funds Time Filing Dates?

In this table, we analyze whether an **unsecured** hedge fund is more likely part of a bankruptcy filing in week t if an unfavorable judge has been assigned a large case in the prior week (based on assets (Panel A) or on liabilities (Panel B)). The dependent variable equals 1 if an unsecured hedge fund is a creditor in a case filing in week t. The independent variables, *L<p50, L≥ p50, L≥ p75, L≥ p90*, are binary variables that takes the value of one if the case is below the median, above the median, above the 75^{th} percentile, or 90^{th} percentile in the prior week. The independent variable, *High judge conv. rate*, is a binary variable that takes the value of one if the judge's last three years conversion rate is above the 75^{th} percentile in the prior week. We use * to denote significance at the 10% level, ** to denote significance at the 5% level, and *** to denote significance at the 1% level. We cluster standard errors at the court district-office-year level.

Panel A: Case Size Based on Assets

High judge conv. rate & A< p50	0.004			
	(0.004)			
High judge conv. rate & A≥ p50		0.011***		
		(0.003)		
High judge conv. rate & A≥ p75			0.016***	
			(0.003)	
High judge conv. rate & A≥ p90				0.017***
				(0.003)
Hedge Fund FE	Yes	Yes	Yes	Yes
Liability FE	Yes	Yes	Yes	Yes
Asset FE	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes
Headquarter FE	Yes	Yes	Yes	Yes
Court FE × office × year FE	Yes	Yes	Yes	Yes
# of weeks	19073	19073	19073	19073

Panel B: Case Size Based on Liabilities

High judge conv. rate & L< p50	0.003			
	(0.004)			
High judge conv. rate & L≥ p50		0.015***		
		(0.003)		
High judge conv. rate & L≥ p75			0.016***	
			(0.003)	
High judge conv. rate & L≥ p90				0.018***
				(0.004)
Hedge Fund FE	Yes	Yes	Yes	Yes
Liability FE	Yes	Yes	Yes	Yes
Asset FE	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes
Headquarter FE	Yes	Yes	Yes	Yes
Court FE × office × year FE	Yes	Yes	Yes	Yes
# of weeks	19073	19073	19073	19073

Table 12: Can we estimate the Counterfactual Conversion Rate of Large Cases?

In this table, we estimate the counterfactual conversion rate of a given large case filing. The dependent variable is *Judge Conversion Rate*, the judge's conversion-rate over the prior three-year period. The dependent variable *District Conversion Rate over the prior X days*, which estimates the mean judge conversion rate for all cases with assets above $1 million assigned in the prior X days. Panel A focuses on the prior 2 days, Panel B focuses on the prior 7 days, and Panel C focuses on the prior 14 days. The first column includes the subset of filings with under $1 million in assets, the second column includes the subset of filings with over $1 million in assets, the third column includes the subset of filings with over $2 million in assets, the third column includes the subset of filings with over $3 million in assets, the third column includes the subset of filings with over $4 million in assets, and the sixth column includes the subset of filings with over $5 million in assets. We use * to denote significance at the 10% level, ** to denote significance at the 5% level, and *** to denote significance at the 1% level. We cluster standard errors at the court district-year level.

Panel A: Predicted based on Prior 2 Days

	Asset Size					
	<$1m	≥$1m	≥$2m	≥$3m	≥$4m	≥$5m
District Conversion Rate over Prior 2 Days	0.024	-0.053*	-0.086**	-0.109**	-0.131**	-0.142**
	(0.017)	(0.031)	(0.038)	(0.050)	(0.057)	(0.069)
Asset FE	Yes	Yes	Yes	Yes	Yes	Yes
Liability FE	Yes	Yes	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes	Yes	Yes
Headquarter FE	Yes	Yes	Yes	Yes	Yes	Yes
Court FE × Office × year FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	4867	3880	2760	2135	1748	1483
Adj. R-squared	.652	.686	.681	.678	.677	.663
Mean of Dep. Variable	.2	.209	.207	.204	.203	.203

Panel B: Predicted based on Prior 7 Days

	Asset Size					
	<$1m	≥$1m	≥$2m	≥$3m	≥$4m	≥$5m
District Conversion Rate over 7 Days	-0.005	-0.039*	-0.069***	-0.071**	-0.089***	-0.104***
	(0.012)	(0.023)	(0.026)	(0.031)	(0.032)	(0.036)
Asset FE	Yes	Yes	Yes	Yes	Yes	Yes
Liability FE	Yes	Yes	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes	Yes	Yes
Headquarter FE	Yes	Yes	Yes	Yes	Yes	Yes
Court FE × Office × year FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	11417.00	8907.00	6327.00	4894.00	4018.00	3430.00
Adj. R-squared	0.68	0.71	0.71	0.70	0.71	0.70
Mean of Dep. Variable	0.19	0.20	0.20	0.20	0.20	0.20

Panel C: Predicted based on Prior 14 Days

	Asset Size					
	<$1m	≥$1m	≥$2m	≥$3m	≥$4m	≥$5m
District Conversion Rate over Prior 14 Days	-0.014	-0.016	-0.036*	-0.034	-0.048*	-0.066**
	(0.014)	(0.015)	(0.021)	(0.023)	(0.026)	(0.028)
Asset FE	Yes	Yes	Yes	Yes	Yes	Yes
Liability FE	Yes	Yes	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes	Yes	Yes
Headquarter FE	Yes	Yes	Yes	Yes	Yes	Yes
Court FE × Office × year FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	15554	11819	8382	6498	5345	4541
Adj. R-squared	.686	.714	.714	.711	.71	.699
Mean of Dep. Variable	.191	.201	.198	.196	.194	.192

Appendix

Table A1: Results under Alternate Measures of Judge Conversion Rates

In this table, we evaluate whether filings with an unsecured hedge fund creditor are assigned different judges than a similar filing with a secured hedge fund creditor. We measure judge conversion-rate over the prior five years (Panel A), and exclude all involuntary bankruptcies from the analysis (Panel B). In Panel A, the dependent variable is *Judge Five-Year Conversion Rate*, a continuous variable that denotes the conversion rate of the assigned judge over the prior five years. In Panel B, the dependent variable is *Judge Conversion Rate*, a continuous variable that denotes the conversion rate of the assigned judge over the prior three years. The primary independent variable is *Unsecured Hedge Fund*, a binary variable denoting whether the filing is associated with a hedge fund acting as an unsecured creditor. The independent variable, *Hedge Fund*, is a binary variable denoting whether the filing is associated with a hedge fund acting as a creditor (secured or unsecured). We use * to denote significance at the 10% level, ** to denote significance at the 5% level, and *** to denote significance at the 1% level. We cluster standard errors at the court district-year level.

Panel A: Five-Year Conversion Rates

	(1) All	(2) All	(3) L $\geq$ p50 ($600k)	(4) A $\geq$ p50 ($400k)
Unsecured Hedge Fund	-0.029***	-0.028***	-0.026***	-0.023***
	(0.008)	(0.008)	(0.011)	(0.010)
Hedge Fund	0.007	0.009	0.007	0.006
	(0.006)	(0.006)	(0.010)	(0.009)
Asset FE	No	Yes	Yes	Yes
Liability FE	No	Yes	Yes	Yes
Headquarter FE	No	Yes	Yes	Yes
Industry FE	No	Yes	Yes	Yes
Court FE $\times$ year FE	Yes	Yes	Yes	Yes
Observations	12210	10211	7363	7056
Adj. R^2	0.518	0.518	0.515	0.511
Mean of Dep. Variable	0.120	0.120	0.126	0.127

Panel B: Only Voluntary Bankruptcies

	(1) All	(2) All	(3) L $\geq$ p50 ($600k)	(4) A $\geq$ p50 ($400k)
Unsecured Hedge Fund	-0.033***	-0.030***	-0.037***	-0.038***
	(0.008)	(0.009)	(0.013)	(0.013)
Hedge Fund	0.004	0.005	0.015	0.015
	(0.007)	(0.008)	(0.012)	(0.011)
Asset FE	No	Yes	Yes	Yes
Liability FE	No	Yes	Yes	Yes
Court FE $\times$ year FE	Yes	Yes	Yes	Yes
Observations	12308	10296	7113	7423
Adj. R^2	0.499	0.496	0.492	0.496
Mean of Dep. Variable	0.110	0.110	0.117	0.116

Table A2: Results under Binary Judge Conversion Rate Measures

In this table, we evaluate whether filings with an unsecured hedge fund creditor are less (most) likely to be assigned the judge with the highest (lowest) conversion rate in the district. In Panel A, the dependent variable is a binary variable equaling one if a judge in a court × year has the highest *Judge Conversion Rate*, a continuous variable denotes the conversion rate of the assigned judge over the prior three years. In Panel B, the dependent variable is a binary variable equaling one if a judge in a court × year has the lowest *Judge Conversion Rate*. The primary independent variable is *Unsecured Hedge Fund*, a binary variable denoting whether the filing is associated with a hedge fund acting as an unsecured creditor. The independent variable, *Unsecured Hedge Fund*, is a binary variable denoting whether the filing is associated with a hedge fund acting as a creditor (secured or unsecured). We use * to denote significance at the 10% level, ** to denote significance at the 5% level, and *** to denote significance at the 1% level. We cluster standard errors at the court district-year level.

Panel A: Highest Judge Conversion Rate

	(1) All	(2) All	(3) L ≥ p50 ($600k)	(4) A ≥ p50 ($400k)
Unsecured Hedge Fund	-0.124***	-0.122***	-0.077*	-0.080*
	(0.033)	(0.032)	(0.045)	(0.047)
Hedge Fund	0.057	0.058	0.026	0.036
	(0.037)	(0.036)	(0.044)	(0.046)
Asset FE	No	Yes	Yes	Yes
Liability FE	No	Yes	Yes	Yes
Headquarter FE	No	Yes	Yes	Yes
Industry FE	No	Yes	Yes	Yes
Court FE × year FE	Yes	Yes	Yes	Yes
Observations	12343	12078	8725	8347
Adj. R^2	0.240	0.242	0.257	0.256

Panel B: Lowest Judge Conversion Rate

	(1) All	(2) All	(3) L ≥ p50 ($600k)	(4) A ≥ p50 ($400k)
Unsecured Hedge Fund	0.232***	0.228***	0.209***	0.198***
	(0.049)	(0.046)	(0.068)	(0.068)
Hedge Fund	-0.093**	-0.087**	-0.091*	-0.106*
	(0.041)	(0.042)	(0.055)	(0.055)
Asset FE	No	Yes	Yes	Yes
Liability FE	No	Yes	Yes	Yes
Headquarter FE	No	Yes	Yes	Yes
Industry FE	No	Yes	Yes	Yes
Court FE × year FE	Yes	Yes	Yes	Yes
Observations	12343	12078	8725	8347
Adj. R^2	0.083	0.086	0.079	0.078

Table A3: Case Assignments for Judges under a Piecewise Exponential Model

This table reports estimates of piecewise exponential models. In contrast to the general hazard rate model we make mild assumption about the baseline hazard to be able to include fixed effects without facing an incremental parameter bias. Specifically, we subdivide time into weeks and assume that the baseline hazard is constant in each week, leading to a piecewise exponential model. Our model is of the form: $h_{ijt} = h_t exp\left\{\boldsymbol{x}'_{ijt}\boldsymbol{\beta}\right\}$, where h_{ijt} is the hazard for Chapter 11 filing i assigned to judge j in week t, and $exp\left\{\boldsymbol{x}'_{ijt}\boldsymbol{\beta}\right\}$ is the relative risk for filing with covariate values $\boldsymbol{x}_{ijt}$, compared to the baseline at any given time. Each failure event is a case assigned to judge j so that each case is at risk from the first to the last case of judge j in our sample. We report coefficients instead of hazard ratios. That means, we estimate a log-linear model: $log\, h_{ijk} = log\, h_k + \boldsymbol{x}'_{ijk}\boldsymbol{\beta}$. We cluster standard errors at the court district-office-year level.

Panel A: Case Size Based on Liabilities

	(1)	(2)	(3)	(4)
$<$ 600k liability case in week $(t-1)$/L$<$ p50	0.042			
	(0.044)			
$\geq$ 600k liability case in week $(t-1)$/L$\geq$ p50		-0.095**		
		(0.038)		
$\geq$ 1m liability case in week $(t-1)$/L$\geq$ p75			-0.104***	
			(0.040)	
$\geq$ 10m liability case in week $(t-1)$L$\geq$ p90				-0.169***
				(0.060)
Judge FE	Yes	Yes	Yes	Yes
Liability FE	Yes	Yes	Yes	Yes
Asset FE	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes
Headquarter FE	Yes	Yes	Yes	Yes
Court FE $\times$ office $\times$ year FE	Yes	Yes	Yes	Yes
# of weeks	77215	77215	77215	77215

Panel B: Case Size Based on Assets

	(1)	(2)	(3)	(4)
$<$ 400k asset case in week $(t-1)$/A$<$ p50	0.044			
	(0.049)			
$\geq$ 400k asset case in week $(t-1)$/A$\geq$ p50		-0.091**		
		(0.038)		
$\geq$ 2m asset case in week $(t-1)$/A$\geq$ p75			-0.095**	
			(0.053)	
$\geq$ 5m asset case in week $(t-1)$/A$\geq$ p90				-0.143***
				(0.038)
Judge FE	Yes	Yes	Yes	Yes
Liability FE	Yes	Yes	Yes	Yes
Asset FE	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes
Headquarter FE	Yes	Yes	Yes	Yes
Court FE $\times$ office $\times$ year FE	Yes	Yes	Yes	Yes
# of weeks	77215	77215	77215	77215

Table A4: Secured Hedge Fund Bankruptcy Filing Decisions

In this table, we analyze whether an **secured** hedge fund is more likely part of a bankruptcy filing in week t if a favorable judge has been assigned a large case in the prior week (based on assets (Panel A) or on liabilities (Panel B)). The dependent variable equals 1 if an unsecured hedge fund is a creditor in a case filing in week t. The independent variables, *L<p50, L≥ p50, L≥ p75, L≥ p90*, are binary variables that takes the value of one if the case is below the median, above the median, above the 75^{th} percentile, or 90^{th} percentile in the prior week. The independent variable, *High judge conv. rate*, is a binary variable that takes the value of one if the judge's last three years conversion rate is above the 75^{th} percentile in the prior week. We use * to denote significance at the 10% level, ** to denote significance at the 5% level, and *** to denote significance at the 1% level. We cluster standard errors at the court district-office-year level.

Panel A: Case Size Based on Assets

High judge conv. rate & L< p50	0.001			
	(0.002)			
High judge conv. rate & L≥ p50		-0.002		
		(0.001)		
High judge conv. rate & L≥ p75			-0.001	
			(0.001)	
High judge conv. rate & L≥ p90				0.014
				(0.010)
Hedge Fund FE	Yes	Yes	Yes	Yes
Liability FE	Yes	Yes	Yes	Yes
Asset FE	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes
Headquarter FE	Yes	Yes	Yes	Yes
Court FE × office × year FE	Yes	Yes	Yes	Yes
# of weeks	16245	16245	16245	16245

Panel B: Case Size Based on Liabilities

High judge conv. rate & A < p50	0.002			
	(0.002)			
High judge conv. rate & A≥ p50		-0.003		
		(0.003)		
High judge conv. rate & A≥ p75			-0.003	
			(0.002)	
High judge conv. rate & A≥ p90				0.011
				(0.009)
Hedge Fund FE	Yes	Yes	Yes	Yes
Liability FE	Yes	Yes	Yes	Yes
Asset FE	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes
Headquarter FE	Yes	Yes	Yes	Yes
Court FE × office × year FE	Yes	Yes	Yes	Yes
# of weeks	16245	16245	16245	16245

Table A5: Unsecured Hedge Fund Bankruptcy Filing Decisions under a Piecewise Exponential Model

This table reports estimates of piecewise exponential models. In contrast to the general hazard rate model we make mild assumption about the baseline hazard to be able to include fixed effects without facing an incremental parameter bias. Specifically, we subdivide time into weeks and assume that the baseline hazard is constant in each week, leading to a piecewise exponential model. Our model is of the form: $h_{ijt} = h_t exp\left\{\boldsymbol{x}'_{ijt}\boldsymbol{\beta}\right\}$, where h_{ijt} is the hazard for Chapter 11 filing i of **unsecured** hedge fund j in quarter t, and $exp\left\{\boldsymbol{x}'_{ijt}\boldsymbol{\beta}\right\}$ is the relative risk for filing with covariate values $\boldsymbol{x}_{ijk}$, compared to the baseline at any given time. The failure event is a Chapter 11 filing by hedge fund j so that each portfolio company is at risk from the time of investment by the hedge fund j. We report coefficients instead of hazard ratios. That means, we estimate a log-linear model: $log\ h_{ijt} = log\ h_t + \boldsymbol{x}'_{ijk}\boldsymbol{\beta}$. We cluster standard errors at the court district-office-year level.

Panel A: Case Size Based on Assets

High judge conv. rate & L< p50	1.384			
	(0.879)			
High judge conv. rate & L≥ p50		8.034***		
		(1.702)		
High judge conv. rate & L≥ p75			8.275***	
			(1.703)	
High judge conv. rate & L≥ p90				8.322***
				(1.810)
Hedge Fund FE	Yes	Yes	Yes	Yes
Liability FE	Yes	Yes	Yes	Yes
Asset FE	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes
Headquarter FE	Yes	Yes	Yes	Yes
Court FE × office × year FE	Yes	Yes	Yes	Yes
# of weeks	19073	19073	19073	19073

Panel B: Case Size Based on Liabilities

High judge conv. rate & A< p50	1.372			
	(0.875)			
High judge conv. rate & A≥ p50		8.487***		
		(1.782)		
High judge conv. rate & A≥ p75			8.528***	
			(1.795)	
High judge conv. rate & A≥ p90				8.518***
				(1.816)
Hedge Fund FE	Yes	Yes	Yes	Yes
Liability FE	Yes	Yes	Yes	Yes
Asset FE	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes
Headquarter FE	Yes	Yes	Yes	Yes
Court FE × office × year FE	Yes	Yes	Yes	Yes
# of weeks	19073	19073	19073	19073

Table A6: Secured Hedge Fund Bankruptcy Filing Decisions under a Piecewise Exponential Model

This table reports estimates of piecewise exponential models. In contrast to the general hazard rate model we make mild assumption about the baseline hazard to be able to include fixed effects without facing an incremental parameter bias. Specifically, we subdivide time into weeks and assume that the baseline hazard is constant in each week, leading to a piecewise exponential model. Our model is of the form: $h_{ijt} = h_t exp\left\{\boldsymbol{x}'_{ijt}\boldsymbol{\beta}\right\}$, where h_{ijt} is the hazard for Chapter 11 filing i of **secured** hedge fund j in quarter t, and $exp\left\{\boldsymbol{x}'_{ijt}\boldsymbol{\beta}\right\}$ is the relative risk for filing with covariate values $\boldsymbol{x}_{ijk}$, compared to the baseline at any given time. The failure event is a Chapter 11 filing by hedge fund j so that each portfolio company is at risk from the time of investment by the hedge fund j. We report coefficients instead of hazard ratios. That means, we estimate a log-linear model: $log\ h_{ijt} = log\ h_t + \boldsymbol{x}'_{ijk}\boldsymbol{\beta}$. We cluster standard errors at the court district-office-year level.

Panel A: Case Size Based on Assets

High judge conv. rate & L< p50	0.041			
	(0.072)			
High judge conv. rate & L≥ p50		0.775		
		(1.057)		
High judge conv. rate & L≥ p75			0.515	
			(0.417)	
High judge conv. rate & L≥ p90				0.972
				(1.045)
Hedge Fund FE	Yes	Yes	Yes	Yes
Liability FE	Yes	Yes	Yes	Yes
Asset FE	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes
Headquarter FE	Yes	Yes	Yes	Yes
Court FE × office × year FE	Yes	Yes	Yes	Yes
# of weeks	16245	16245	16245	16245

Panel B: Case Size Based on Liabilities

High judge conv. rate & A< p50	0.052			
	(0.070)			
High judge conv. rate & A≥ p50		0.771		
		(1.053)		
High judge conv. rate & A≥ p75			0.508	
			(0.412)	
High judge conv. rate & A≥ p90				0.976
				(1.043)
Hedge Fund FE	Yes	Yes	Yes	Yes
Liability FE	Yes	Yes	Yes	Yes
Asset FE	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes
Headquarter FE	Yes	Yes	Yes	Yes
Court FE × office × year FE	Yes	Yes	Yes	Yes
# of Quarters	16245	16245	16245	16245

Table A7: The Effect of Judicial Assignment on Conversion under the Recentered Instrument

In this table, we estimate the relationship between the assigned judge's past conversion rate and the likelihood a given case is converted to liquidation. Panel A includes the subset of cases with at least $1 million in assets, while Panel B include cases with at least $5 million in assets. The dependent variable is *Convert to Chapter 7*, a binary variable that denotes whether the bankruptcy was converted to a Chapter 7 liquidation. In the first and second column, the primary independent variable is *Judge Conversion Rate*, the judge's conversion-rate over the prior three-year period. In the third and fourth column, the primary independent variable is *Recentered Judge Conversion Rate*, the judge's conversion-rate over the prior three-year period relative to the counterfactual judge's conversion rate. We use * to denote significance at the 10% level, ** to denote significance at the 5% level, and *** to denote significance at the 1% level. We cluster standard errors at the court district-year level.

Panel A: Assets $>$ $1 Million

	(1)		(2)	
Judge Conversion Rate	0.227***	0.227***		
	(0.047)	(0.047)		
Recentered Judge Conversion Rate			0.224***	0.227***
			(0.047)	(0.047)
Asset FE	No	Yes	No	Yes
Liability FE	No	Yes	No	Yes
Industry FE	No	Yes	No	Yes
Headquarter FE	No	Yes	No	Yes
Court FE $\times$ year FE	Yes	Yes	Yes	Yes
Observations	11078	11078	11078	11078
Adj. R^2	0.050	0.055	0.050	0.055
Mean of Dep. Variable	0.16	0.16	0.16	0.16

Panel B: Assets $>$ $5 Million

	(1)		(2)	
Judge Conversion Rate	0.228***	0.236***		
	(0.080)	(0.081)		
Recentered Judge Conversion Rate			0.233***	0.237***
			(0.080)	(0.081)
Asset FE	No	Yes	No	Yes
Liability FE	No	Yes	No	Yes
Industry FE	No	Yes	No	Yes
Headquarter FE	No	Yes	No	Yes
Court FE $\times$ year FE	Yes	Yes	Yes	Yes
Observations	4099	4099	4099	4099
Adj. R^2	0.035	0.041	0.035	0.041
Mean of Dep. Variable	0.15	0.15	0.15	0.15