Friends with Bankruptcy Protection Benefits

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Abstract

We evaluate whether social networks limit the effectiveness of targeted debt relief programs. In our setting, individuals learn about the likelihood of debt relief from the recent experiences of workplace peers filing for Chapter 13 bankruptcy. Peers granted bankruptcy can discharge debts, while peers facing dismissal lose all protections. Exploiting the random assignment of judges to bankruptcy court, we determine that individuals with a "dismissed peer" are significantly less likely to file for bankruptcy or enter foreclosure. Our results highlight a novel channel relating social networks to household finances and demonstrate the spillover costs of granting individual debt relief.

Key words: Debt Relief, Personal Bankruptcy, Foreclosure, Peer Effects, Social Networks, Bankruptcy Judges, Random Assignment

JEL classification: K35, G4, G33, G32, M5, C9

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1 Introduction

In 1975, household debt in the U.S. comprised 51% of disposable income; by 2010, that ratio reached 124%. This rise has led to two primary concerns. First, debt overhang limits a household's incentives to invest (Melzer, 2017) or enter the labor market (Bernstein, 2016). Second, in tandem with the rise of leverage, delinquency and default rates also increased, and today over a third of households have debt in collections. As a public policy, debt relief offers a potential solution to these concerns through a combination of lower interest payments in the short-run and the partial discharge of debt for the long-run. Short-run reductions in interest rates can lead to lower rates of financial distress and increased consumption (Di Maggio et al., 2017; Fuster and Willen, 2017), while debt write-downs improve financial health and labor outcomes (Dobbie and Song, 2017).

Large-scale debt relief policies, however, can also lead to distorted borrower incentives. Expanding debt relief encourages households to default when payment is still possible (Mayer et al., 2014) and increase risk exposure (Fay et al., 2002).[1] These potential losses may partially explain why lenders are reluctant to modify household debt repayment plans (Adelino et al., 2013; Agarwal et al., 2017), even when debt overhang is costly for lenders due to deadweight losses (Bolton and Rosenthal, 2002). Driven by these concerns, recent policies have aimed to target those borrowers most at risk of insolvency, such as the 2005 Bankruptcy Abuse Prevention and Consumer Protection Act.[2] Ultimately, though, the effectiveness of debt relief programs depends on offering access exclusively to individual households that are unable—rather than unwilling—to cover debt payments.

In this paper, we evaluate whether social networks limit the success of targeted debt relief programs. Participants who benefit from debt relief may encourage peers to take advantage of these protections, undoing any attempts to exclude solvent households. Our focus on social networks is motivated by past evidence that peer effects influence a range of household debt outcomes including leverage (Bailey et al., 2017; Kalda, 2018), mortgage foreclosures (Guiso et al., 2013; Gupta, 2018),

[1]Gerardi et al. (2017) estimate that 38% of the mortgage foreclosures during the Great Recession were strategic defaults.

[2]Specifically, the 2005 Bankruptcy Abuse Prevention and Consumer Protection Act limits Chapter 7 filings to households making below the state-level median

debt refinancing (Maturana and Nickerson, 2016), and micro-finance repayments (Breza, 2012).[3] More broadly, research has found evidence that social networks impact the rate of stock market participation, as well as allocation decisions in brokerage and retirement accounts.[4] We contribute to this literature by isolating a novel channel relating peer effects and debt outcomes: updated expectations of the likelihood of debt relief. As a result, we are the first to examine whether peers influence participation in government policies offering financial relief.[5]

We focus our analysis on personal bankruptcy law, which allows consumers the opportunity to dissolve or modify current debts. The benefits of debt relief and reorganization under bankruptcy are substantial, and ultimately lead to increased employment rates, economic stability, and financial health (Dobbie and Song, 2015; Dobbie et al., 2017). In addition, bankruptcy protection represents a significant debt relief program, as one percent of U.S. households file bankruptcy in a given year and ten percent of households file at some point in their lifetime (Stavins, 2000). Given that the benefits from personal bankruptcy are twice those of all state unemployment insurance benefits and roughly 25% of all Medicaid subsidies (Lefgren and McIntyre, 2009), understanding the consequences of bankruptcy protection is of central economic importance.

To identify peer effects, we examine whether household behavior changes following a recent bankruptcy filing by someone in their social network. The effects of bankruptcy law on households critically depends on the degree to which it is enforced, and this uncertainty limits the acquisition and processing of information by potential filers. Due to these frictions, households may update expectations of the likelihood of debt relief through bankruptcy based on recent experiences of peers. While past research has illustrated the information content of corporate bankruptcy announcements (Borenstein and Rose, 1995; Eberhart et al., 1990; Lang and Stulz, 1992; Seyhun and Bradley, 1997), we analyze instead the information content of personal bankruptcies. In our set-

[3]While a past literature also suggests social networks influence the rate of bankruptcy in the economy (Cohen-Cole et al., 2008; Dick et al., 2008; Fay et al., 2002; Gross and Souleles, 2002; Miller, 2015; Scholnick, 2014), researchers have traditionally faced two obstacles. First, it is generally difficult to distinguish whether (i) a household is responding to the recent bankruptcy of a peer or (ii) both bankruptcies are driven by correlated shocks to either economic and financial conditions or bankruptcy policy. Second, the peer effects of bankruptcy may operate through two channels: households may (i) learn about the costs and benefits of bankruptcy protection or (ii) respond to the perceived social stigma of filing bankruptcy. Our experimental setting allows us to identify the causal role of learning from peer bankruptcies.

[4]This research includes Beshears et al. (2015); Brown et al. (2008); Bursztyn et al. (2014); Duflo and Saez (2002, 2003); Heimer (2016); Hong et al. (2004); Ouimet and Tate (2019).

[5]Previous research has shown that peers influence participation in welfare and paternity leave (Carneiro et al., 2016; Dahl et al., 2014). Our paper instead focuses on debt relief programs.

ting, creditor-friendly bankruptcy outcomes may highlight the relative costs of insolvency to outside households, while debtor-friendly outcomes may instead highlight the benefits of debt relief. As all peer have filed bankruptcy, it is unlikely households are learning about the associated stigma of filing bankruptcy; rather they are updating their expectations about the likelihood of court outcomes following filing. By evaluating the individual responses to peer bankruptcy outcomes, we are the first to identify a microeconomic channel for personal bankruptcy spillover.[6]

The fundamental challenge facing the literature on peer effects is the reflection problem outlined by Manski (1993). In our setting, individuals connected to dismissed households may differ in systematic ways unobservable to the econometrician. To overcome these concerns, we exploit the fact that U.S. bankruptcy courts use a blind rotation system to assign bankruptcy cases to judges. At any point in the court process, the bankruptcy judge has the authority to dismiss a case, ending all benefits of bankruptcy protection. There is substantial variation across judges in the interpretation of the law, resulting in different rates of dismissals (Dobbie and Song, 2015),[7] so similar households can be assigned judges with substantially different propensities to dismiss. By exploiting the random assignment of bankruptcy judges, we can measure the causal effect of a peer bankruptcy dismissal on the financial decisions of the household.

Individuals typically file for either Chapter 7 or Chapter 13 bankruptcy protections. Chapter 7 bankruptcy protects households from wage garnishment, but requires non-exempt assets to be forfeited. In contrast, Chapter 13 bankruptcy requires applicants to submit a plan to repay a portion of their current unsecured debts in exchange for protecting their assets and wages from garnishment. The judge assigned to the case decides whether to confirm or dismiss the plan based on Bankruptcy Code standards. Following a dismissal, the debtor loses any benefits of bankruptcy protection.

Given the costs of filing bankruptcy—including fees, time, and stigma—individuals who observe the dismissal of a peer's filing may be discouraged from seeking protection. Individuals who observe a peer granted the full benefits of protection, however, may be encouraged to take greater financial risk, leading to an increased likelihood of insolvency. All individuals in our sample are

[6]Our results contribute to the literature on bankruptcy spillovers. Past research identifies the effects of individual corporate bankruptcy on local employment (Benmelech et al., 2014; Bernstein et al., 2016; Hacamo and Kleiner, 2016), asset values (Benmelech and Bergman, 2011), or credit supply (Addoum et al., 2017; Hertzel and Officer, 2012).

[7]Researchers have found a similar role for judges in corporate bankruptcy, leading to differences in asset allocation, employee displacement, and neighboring firm revenue (Bernstein et al., 2016, 2017a; Hacamo and Kleiner, 2016).

connected to a peer who files for bankruptcy, but only a fraction of peers are granted the government protections; this is the random variation that we exploit.

We focus our analysis on workers employed by financial advisory firms and define peers as coworkers within the same branch at the date of bankruptcy filing. Financial advisers are regulated either by the Securities and Exchange Commission (SEC) or the Financial Industry Regulatory Authority (FINRA), and are required to report employment to their respective regulator. This allows us to construct detailed employment histories for each adviser. Advisers must also disclose major events including criminal convictions and disciplinary actions, customer disputes, and bankruptcies.[8]

We collect data on all Chapter 13 bankruptcies filed by advisers between 2008 and 2017. As illustrated in Figure 1, over 23,000 financial advisers experienced a personal bankruptcy including over 5,000 Chapter 13 filings. For each case, we identify the judge assigned to the case, and then calculate the two-year dismissal rate for the judge using their complete case history. Judges in our sample are assigned nearly 2,000 cases annually, and dismiss 32% of cases. Comparing outcomes within a given year and court district, we estimate that a 10 percentage point increase in the dismissal rate of the judge assigned the bankruptcy case increases the likelihood of a dismissal by 12 percentage points, indicating that the relevance criterion for an instrument is satisfied. We confirm that the exclusion restriction is satisfied by showing that judge dismissal rates are not correlated with characteristics of workers, branches, firms, or geographic locations. We also run a placebo test that randomly reassigns bankruptcy filers across the set of branches, and find no evidence that placebo judges predict dismissals. Overall, this provides strong evidence that judge dismissal rates are a valid and valuable instrument for our setting.

Studying over 40,000 coworkers, we estimate that the dismissal of a peer's Chapter 13 filing decreases the likelihood of an individual also entering bankruptcy within the following year by nearly two percentage points. Therefore, granting bankruptcy protection to a co-worker peer effectively doubles the four-year bankruptcy rate and highlights the effects of granting debt relief on

[8]Due to the regulatory nature of the financial advisory industry, researchers have also exploited the detailed employee-level data (Dimmock et al., 2015; Egan et al., 2019, 2017; Gurun et al., 2018). To our knowledge, we are the first to focus on the financial disclosures of financial advisers.

outside households within the social network. The difference in outcomes remains four years after the filing, highlighting the permanent effects of the initial filing outcome.

We develop two additional robustness tests to confirm our baseline results. First, we verify that the coefficients are not sensitive to controlling for worker, firm, or local characteristics. Second, we analyze subsets of the data sample where peer effects are more likely to be found. We find evidence of peer effects exclusively among homophillic peers (defined by gender and job tenure), and that our results are not driven by the largest branches, and continue to hold after excluding firm owners.

We differentiate between two alternate explanations of our results. In the first, *insolvent* workers may observe a dismissal and become discouraged with the benefits of bankruptcy protections, choosing instead to face wage garnishment, collection agencies, and foreclosure risk. Alternatively, *solvent* workers may observe the same dismissal and become pessimistic about the value of strategic defaults. To distinguish between these two mechanisms, we analyze the effects of bankruptcy outcomes on foreclosure rates and find that the dismissal of a peer leads to a two percentage point reduction in the likelihood of foreclosure, where the baseline rate of foreclosure is 40 bps. The results support an overall decline in strategic defaults and highlight a positive externality of bankruptcy dismissals. We find no evidence of an effect on insolvent workers.

We evaluate the implications of our results both beyond the financial advisory industry and on the local economy. First, to consider the possible effect outside of the financial industry, where bankruptcy filings would not typically be public knowledge, we examine whether bankruptcy outcomes could be indirectly observable to coworkers. Specifically, we estimate that dismissal in bankruptcy leads to a nine percentage point increase in adviser misconduct, our proxy for job performance. Assuming job performance is observable to coworkers, bankruptcy outcomes are at least indirectly observable. To our knowledge, we are the first to illustrate a causal effect between bankruptcy and job performance.

Second, we examine the aggregate effects of our results. While we show that bankruptcy dismissal leads to a lower rate of peer bankruptcy filings and foreclosures, previous research has found that dismissals lead to worse financial outcomes. To determine whether peer effects of dismissal may offset, or even dominate, any personal effects of dismissal from court, we evaluate the personal effects of bankruptcy outcomes on the rate of foreclosure. We estimate that dismissal from a Chapter 13 bankruptcy leads to an eight percentage point increase in the rate of personal foreclosure within four years.[9] Given our estimated peer effect of four percentage points (or half the personal effect of eight percentage points), the aggregated spillovers partially counteract the opposing personal effects of a dismissal.

Our results highlight an unexplored consequence of individual debt relief. First, bankruptcy judges should be aware of the full implications of bankruptcy decisions: creditor-friendly court rulings can deter outside households from insolvency, while debtor-friendly rulings can lead to additional financial distress. As individual bankruptcy judges have gained greater influence in recent years,[10] a thorough analysis of the consequences of judicial rulings is increasingly valuable. Second, while we focus our analysis on bankruptcy protection, the results may rationalize why lenders are reluctant to modify individual household debt repayment plans. Peer effects may be large enough to counteract any direct benefits, leading to additional creditor losses. Third, policy makers should recognize the potential spillover effects of government debt relief programs as additional losses imposed on lenders will ultimately reduce credit across all households (Gropp et al., 1997). Correctly distinguishing between solvent and insolvent households is therefore necessary to constrain spillovers.

2 Framework

2.1 U.S. bankruptcy law

We begin by briefly summarizing personal bankruptcy law.[11] Each year a small number of bankruptcy judges—roughly 350 individuals—preside over approximately 1.3 million annual U.S. bankruptcy filings across 94 federal courts.[12] A judge is appointed by the U.S. Court of Appeals and the number of judges is determined by Congress; appointments last for 14-year terms and are renewable. The same bankruptcy judges rule on both business and non-business cases.

[9] We note we are not the first to estimate the causal effect of bankruptcy dismissal on the foreclosure rate as Dobbie and Song (2015) estimate a nineteen percent effect five years after filing.

[10] Iverson (2017) notes that from 1980 to 2010, bankruptcy filings rose by 381% while the number of bankruptcy judges only increased by 53%.

[11] For a more complete explanation, we refer readers to Dobbie and Song (2015) and Dobbie and Song (2017).

[12] We measure the annual rate of bankruptcies over the past twenty years.

Personal bankruptcy law offers two types of protection: Chapter 7 and 13. In Chapter 7, households are protected from wage garnishment, but forfeit non-exempt assets. A bankruptcy judge will confirm or dismiss an application based on the financial affairs and income of the applicant. If the filing is confirmed, the debtor's non-exempt property is liquidated and split among creditors; in the case of a filing dismissal, the debtor exits from the bankruptcy. On average, 95% of Chapter 7 bankruptcies are approved.[13] Given the benefits of Chapter 7 bankruptcy protection, policy makers are well-aware that households may abuse the system by filing bankruptcy when full debt repayment is still feasible. In 2005 Congress enacted the Bankruptcy Abuse Prevention and Consumer Protection Act (BAPCPA). As part of the BAPCPA, households filing Chapter 7 are required to pass a means test, which requires household income over the past six months to be below the state median average.[14]

Under Chapter 13 bankruptcy, applicants submit a plan to repay a proportion of their current unsecured debts and in exchange protect their assets and wages from garnishment. Debtors are assigned a judge and a trustee (judicial offices generally share the same trustee unless the judge has a high caseload). The trustee works with the debtor to arrange a payment plan, and the judge then decides whether to confirm or dismiss the plan based on Bankruptcy Code standards. The judge may also dismiss the case at a later date if the debtor fails to make payments, or meet any other obligations. In the event of a dismissal, the applicant loses all benefits of protection. Nearly half of all Chapter 13 bankruptcies end in dismissal. Following a dismissal, the debtor may still refile for bankruptcy, but only after 180 days (Dobbie and Song, 2015).

There are both costs and benefits to filing for bankruptcy. In the short-term, filing for bankruptcy protection under Chapter 13 halts the debt collection efforts of creditors. These collection efforts can include foreclosure and wage garnishment. In the long-term, Chapter 13 bankruptcy protection allows the discharge and restructure of debts under the repayment plan. Unsecured debt not included in the repayment plan is discharged, while secured debt, including mortgages, can be restructured in the new plan. In comparison, the costs involve court and legal fees, time and effort, and stigma. While stigma is more difficult to quantify, legal costs for a Chapter 13 Bankruptcy are

[13] In our particular sample, we estimate an approval rate of 93 percentage points.

[14] Technically, the means test depends on the number of the individuals in the household.

generally estimated at $3,000-$4,000 for the average household and the process takes usually one to five years in total.

2.2 Empirical setting

We evaluate whether social networks limit the effectiveness of targeted debt relief, specifically Chapter 13 bankruptcy protection. As discussed above, Chapter 13 bankruptcy outcomes are idiosyncratic since they depend on the rulings of the assigned judge. The judge assigned to the case is able to both (i) dismiss the case, ending all bankruptcy protections, and (ii) reject a given repayment plan, affecting what debt is discharged in the bankruptcy. We argue the outcome uncertainty limits households' ability to fully acquire and process information regarding the costs and benefits of bankruptcy. In this environment, we conjecture that individuals update their expectations based on available resources, including the recent experiences of peers. The experiences of others may by especially valuable if (i) peers share similar characteristics and those same characteristics predict bankruptcy outcomes or (ii) informational frictions such as costly acquisition and processing limit access to more general information.

To identify causally peer influence, it is necessary to address the well-known endogeneity concerns associated with social networks (Hellerstein et al., 2015; Manski, 1993). This literature identifies three distinct reasons individuals belonging to the same peer group may behave similarly: (i) endogenous peer effects, (ii) exogenous contextual effects, and (iii) correlated effects driven by common characteristics of group members. As is standard in the literature, we attempt to isolate the first effect.

In our setting, the concern is that the ruling of the bankruptcy judge is not random, but depends on the financial condition of the debtor. Specifically, the majority of dismissed cases constitute abuse of the bankruptcy process where the judge rules that the debtor has the means to repay the debt (Hynes, 2004). This leads to two specific concerns. First, economic conditions may simultaneously impact the ruling of the judge and the financial decisions of peers. Porter (2011) reports that seventy percent of dismissed filers offer the desire to avoid foreclosure to explain their decision to file for bankruptcy. Since foreclosure is correlated across geographies due to local house price dynamics, real estate losses may simultaneously drive bankruptcy dismissal rates and worsening financial conditions of peers. Therefore, the dismissal of a peer could be correlated with a higher (rather than lower) rate of bankruptcy filings.

Second, as households sort into homophillic peer groups, the bankruptcy ruling of a peer is likely correlated with the decision to personally file for bankruptcy due to characteristics unobservable to the econometrician. For instance, individuals dismissed from court due to abuse of the bankruptcy process may be connected to peers who are also inclined to strategically default, similar to the results of Dimmock et al. (2015), who find that financial advisors are encouraged to engage in fraud based on the actions of workplace peers. Again, in this case, dismissal of a peer may predict a higher rate of peer bankruptcy filings.

To overcome both endogeneity concerns, we exploit the random assignment of bankruptcy judges to each filing within a court district. While judges rule based on bankruptcy law, there is substantial variation across judges in the interpretation of the law, resulting in different rates of bankruptcy protection being granted to households. Judges may lean towards protecting debtor rights or creditor rights, and these views will be both predictive of a given bankruptcy outcome and exogenous to the characteristics of peers. Our framework is unique from much of the literature, which has focused on particular settings where peers are randomly assigned (Hacamo and Kleiner, 2018; Lerner and Malmendier, 2013; Shue, 2013). In contrast, we develop a complementary strategy where peers are not randomly assigned, but their personal experiences are partially random due to the random assignment of a judge to the case.

Turning to our particular empirical methodology, we evaluate the effect of a peer's Chapter 13 bankruptcy dismissal on household financial outcomes. We focus on dismissal because it represents a significant loss to the debtor.[15] We introduce an instrumental variables framework that uses the past dismissal rate of the judge as an instrument for a peer's bankruptcy protection. We assume each judge has a unique interpretation of the law, and that her interpretation is relatively constant across time. As judges are assigned cases randomly within a court district, the mean dismissal rate for a given judge should predict the likelihood of a future dismissal. The past literature has used a similar framework to evaluate the effects of both personal bankruptcy (Dobbie and Song, 2015;

[15]Specifically, Dobbie and Song (2015) estimate that dismissal from Chapter 13 bankruptcy protection leads to a 25.1 percentage point decrease in earnings, and a 6.8 percentage point decrease in the employment rate. In addition, foreclosure rates increase by 19.1 percentage points, while mortality increases by 1.2 percentage points.

Dobbie et al., 2017) and corporate bankruptcy filings (Bernstein et al., 2016, 2017a; Hacamo and Kleiner, 2016).

Our empirical strategy implicitly assumes that (i) judges are not assigned cases systematically; and (ii) judges only affect bankruptcy outcomes through their decision of whether to grant protections. We argue the latter concern is unlikely as even the initial filer has limited contact with the assigned judge: debtors appear before the judge only at the confirmation hearing. We directly test the former concern below and find no evidence that cases are assigned to judges based on the characteristics of the filing household.

Since our identification strategy relies on the random assignment of judges, we estimate whether peers respond to the random (and therefore uninformative) component of the bankruptcy outcome. A peer's bankruptcy outcome may be informative if peers share similar characteristics and the bankruptcy outcomes depends at least partially on these characteristics. For instance, even if a financial adviser is able to observe the mean dismissal rate within her district, the outcome of a coworker may be valuable if financial advisors are treated differently from the rest of the population.

After observing the dismissal rate of all judges in the district, the dismissal rate of a particular judge within the district cannot be informative because judges are randomly assigned. Therefore, any response by peers is a response to pure randomness, not information. One explanation for this irrational behavior is that while information is technically available, acquiring and processing the information is also costly. These costs may limit the number of households that ultimately obtain the information; in addition, even households with the information may not sufficiently distinguish between the informative and uniformative components. Households may either not know the dismissal rate within the court or they may place some weight on the dismissal rate of a particular judge (relative to the other judges in the district) when making financial decisions. In either case we will observe peer effects in our setting.

We are not the first to suggest social networks influence the rate of bankruptcy in the economy (Cohen-Cole et al., 2008; Dick et al., 2008; Fay et al., 2002; Gross and Souleles, 2002; Miller, 2015; Scholnick, 2014). We make two contributions to this literature. First, it is generally difficult to distinguish whether (i) a household is responding to the recent bankruptcy of a peer or (ii) both bankruptcies are driven by correlated shocks to either economic and financial conditions or bankruptcy policy. We add to this literature by developing a novel strategy to identify whether households learn about bankruptcy through social networks based on the random assignment of bankruptcy judges to court cases. Second, even if it is possible to isolate a response to peer influence, it is unclear whether households are (i) learning about the bankruptcy process or (ii) responding to the perceived social stigma of filing bankruptcy. Our setting allows to isolate the learning channel: households respond to the differences in the bankruptcy outcomes of peers, rather than the bankruptcy filing of a peer. Assuming there are minimal differences in social stigma between discharging debt in bankruptcy and dismissal from bankruptcy court, we can exclude the effect of stigma.

2.3 Empirical methodology

In our baseline analysis, we analyze the effects of a peer's dismissal on an advier's decision to file bankruptcy. We consider two separate theoretical channels that link these two outcomes. First, there is a potential short-run effect on insolvent households. Observing a dismissal could lead insolvent households to hold a more negative view of bankruptcy benefits and therefore to opt out of filing bankruptcy. But without these protections, these households will instead face an increased risk of wage garnishment, collections agencies, and forced foreclosure. Second, there is an alternative effect on solvent households, whereby observing a bankruptcy dismissal leads to a more negative view of bankruptcy benefits, discouraging households to strategically file bankruptcy, and instead encouraging a sufficient coverage of debt payments (Dobkin et al., 2018; Keys, 2010). Both channels lead to a decreased rate of bankruptcy, leading to our first testable hypothesis.

Hypothesis I: The dismissal of a peer filing Chapter 13 bankruptcy leads to a decrease in the likelihood of also filing bankruptcy.

While both channels have similar implications for bankruptcy, they suggest opposing policy implications. If peer dismissal discourages insolvent households from seeking Chapter 13 bankruptcy protection, our results suggest a negative externality of social networks. Alternatively, if peer dismissal motivates outside households to reduce the likelihood of entering financial distress, our results provide a more positive outlook. In this context, it is necessary to determine whether a dismissal exclusively affects the decision to voluntarily file bankruptcy, or instead leads to an overall improved financial condition.

To distinguish between these two channels, we analyze the effects of peer bankruptcy outcomes on entering foreclosure. Simply put, bankruptcy law governs unsecured debt, while foreclosure law governs secured debt (Mitman, 2016). However, the decision to file for bankruptcy or enter foreclosure are highly correlated: Chapter 13 allows debtors to avoid foreclosure by including mortgage arrears as part of the repayment plan. Therefore, if dismissal steers insolvent households away from filing for bankruptcy relief, then we expect a simultaneous *increase* in the foreclosure rate. If solvent households instead reduce the likelihood of strategic defaults, then we should expect a simultaneous *decrease* in the foreclosure rate. We can therefore introduce a second testable hypothesis.

Hypothesis II: The dismissal of a peer filing Chapter 13 bankruptcy leads to a decrease in the likelihood of entering foreclosure.

With these hypotheses in mind, we introduce the second-stage equation, which estimates the effect of a peer dismissal on filing for bankruptcy (or alternatively, entering foreclosure):

$$Filing_i = \beta Peer\widehat{Dismissal}_i + \delta_t \times \phi_d + Controls_i + \epsilon_i \tag{1}$$

where i represents an employee, t represents the year, and d denotes the court district. The dependent variable is $Filing_i$, a binary variable that identifies the decision to file for bankruptcy, where bankruptcy is defined as filing either Chapter 7 or 13. In addition, we consider an alternate dependent variable, $Foreclosure_i$, defined as a binary variable that measures whether employee i entered foreclosure. The dependent variable $PeerDismissal_i$ is a binary variable denoting a coworker of employee i (employed within the same establishment at the time of filing) was previously dismissed from Chapter 13 bankruptcy court. In addition, since judges are only assigned within a court district, we include court district fixed effects interacted with year fixed effects. In certain specifications, we also include controls at the employee level, firm-level, or county level. As all employees within the same branch are subject to the same peer dismissal, we cluster our results at the branch level.

Turning to the first-stage regression, we estimate how the mean dismissal rate of the assigned judge predicts the dismissal of the workplace peer:

$$PeerDismissal_i = \gamma JudgeDismissalRate_i + \rho_t \times \theta_l + Controls_{it} + \eta_i \tag{2}$$

The independent variable of interest is $JudgeDismissalRate_i$ and is defined as the fraction of Chapter 13 bankruptcy cases dismissed by the assigned judge over the course of her career, excluding the current case, which we calculate as the dismissal rate across all personal bankruptcy filing during the 12 months prior to, and following, the filing date. As before, we also compare cases within a given year and court district. As in the first stage regression, we include a number of control variables and cluster at the branch level.

3 Data

3.1 Data sources

Financial adviser data. We focus on the financial advisory industry because of the availability of detailed data on employment history and both financial and nonfinancial information, including disclosures of bankruptcy or misconduct. Financial advisers must register with the either the SEC or FINRA, and are required to disclose detailed information on dates and locations of current and past employment as well as disclosures of: (i) qualifications, including current and past industry exams and professional designations; and (ii) any disclosure events. According to each SEC Investment Adviser Public Disclosure (IAPD), disclosure events include "criminal charges and convictions, formal investigations and disciplinary actions initiated by regulators, customer disputes and arbitrations, and financial disclosures such as bankruptcies and unpaid judgements or liens."

We extract detailed information on all financial disclosures from the SEC's Investment Adviser Public Disclosure (IAPD) and FINRA's BrokerCheck web sites. For personal bankruptcies, these disclosures include the Chapter, filing data, court district, case docket number, case outcome, and disposition date. For other financial compromises-primarily foreclosures- the data includes the action date, the disposition, name of the creditor, original amount owed, and the terms reached with the creditor.

To identify workplace peers, we collect employment histories detailing the name and location of each employer, as well as start and end dates. Peers are defined as all other advisers working in the same branch. We also extract information on industry exams and professional designations. Finally, we follow Egan et al. (2019, 2017) to measure workplace misconduct and identify adviser gender.

While there may be limitations of focusing exclusively on the financial adviser industry, we argue that focusing on financial advisers should only understate the size of peer effects. Financial advisers, by definition, are likely more financially-sophisticated than the rest of the population. As a result, advisers should base their beliefs concerning the value of bankruptcy protection less on personal experiences and more on data-driven evidence, at least compared to the rest of the population. Our results therefore suggest only a lower bound when estimating peer effects from bankruptcy protection. However, advisers are limited in their financial knowledge (Linnainmaa et al., 2017), suggesting that there is place for peer effects.

Bankruptcy dockets. We attempt to match each bankruptcy filing in our sample to the actual docket from Reuters Westlaw using the docket number, court district, last name of filer, and date of bankruptcy filing. We collect 5,135 Chapter 13 cases from the Financial advisory data and are able to match 4,977 or 97% of the original bankruptcy cases. After cleaning all judge names in this sample, we then collect all cases assigned to the judge during the year prior to the filing and the year following the filing. We calculate the number of these Chapter 13 bankruptcy cases dismissed by each judge.

Additional data sources. We use Westlaw data to count the annual number of bankruptcy filings within each county. We then estimate the rate of personal bankruptcy filings by dividing the number of filings by the county population. Per capita income and population at the county-level are collected from the Bureau of Labor Statistics. County-level house prices are collected from Zillow.

3.2 Data summary

Bankruptcy filings statistics. We first summarize the set of all Bankruptcy filings in Panel A of Figure 1. In the initial dataset, we observe a total of 23,363 bankruptcy cases. Given we observe a total of 1,039,115 individuals in total, we estimate an average bankruptcy rate of 2.25%. Chapter 7 cases comprise 76.4% of cases, while Chapter 13 comprise 22.0% of cases.[16] We note two facts. First, we find a high rate of bankruptcy filings during the Financial Crisis: nearly 5,000 cases in both 2009 and 2010, compared to under 500 cases in 2017. Second, we find strong evidence that the ratio of Chapter 13 to Chapter 7 cases rise over the course of our sample: Chapter 13 compose 20% of cases in 2009 and 47% in 2017.[17] One potential explanation for this rise is that households are required to meet a means test based on your income relative to the average income in the state. If financial advisers experienced a relative rise in income growth following the Financial Crisis, we would expect a higher proportion of Chapter 13 bankruptcies and lower proportion of Chapter 7 bankruptcies.

We compare these results to rates of bankruptcy among the general public. First, between 2007 and 2017, we estimate an average of 11.7 million personal bankruptcy filings. In other words, roughly 5% of U.S. adults entered bankruptcy during the last ten years. Therefore, financial advisers are roughly half as likely to file bankruptcy as the rest of the adult population. Second, Chapter 7 filings compose 67% of all bankruptcy, while the remaining 33% are Chapter 13. Therefore, financial advisers are slightly more likely to choose Chapter 7 bankruptcy relative to the rest of the population. Third, Chapter 13 bankruptcies composed 29% of bankruptcies in 2009 and 36% by 2015; therefore, the ratio of Chapter 13 to Chapter 7 cases also rose in the overall population.

We focus our analysis on the Chapter 13 filings. While Chapter 7 cases comprise the majority of cases both in our sample and in all consumer bankruptcy cases in general, bankruptcy judges have a limited role in Chapter 7 outcomes, making our identification strategy not possible for these cases. Moreover, within the filings already completed, only 2.5% of the Chapter 7 cases are dismissed, leading to little heterogeneity in outcomes.

[16] In addition, 357 cases are classified as Chapter 11 and 22 are classified as Other.

[17] We exclude 2008 and 2018 as our data does not cover the full twelve months of either year.

After including only Chapter 13 filings, we are left with 5,135 cases. Within these cases, 1,264 cases (24.6%) are still in progress, meaning they were likely filed in the last several years. A total of 1,308 (25.5%) of the cases are classified as dismissed in court. Among disposed cases, dismissals are more common during the later years in the sample. One explanation is that cases in progress (which are more common during later years) are more likely to be completed, eventually leading to lower dismissal rates.

Turning to the regression dataset, we restrict the sample in five ways. First, we exclude Chapter 13 bankruptcies filed after 2014 in order to evaluate the long-term effects of the bankruptcy outcome. Second, we restrict our focus to bankruptcies filed while the adviser is employed with an advisory firm. As advisors are required to disclose bankruptcy filings for the prior ten years, many of the bankruptcies in the sample are filed while the individual is not yet an adviser. Third, the advisory firm must employ fewer than a thousand advisors in order for us to clearly identify peer effects. Fourth, we only keep the first observed bankruptcy filing for each individual in the sample to ensure bankruptcy outcomes are not correlated across time due to unobservable worker characteristics. Fifth, we include only the first observed bankruptcy filing within each branch to ensure individuals are not treated more than once. The final set of Chapter 13 bankruptcies are presented in Panel B of Figure 1.

Bankruptcy outcome statistics. We summarize the set of 1,099 initial Chapter 13 bankruptcy filings during 2008 and 2014 in Table 1. Among the Chapter 13 filings, 23% are dismissed. This is lower than the mean judge dismissal rate of 32%, indicating a lower dismissal of financial advisers compared to the rest of the population. To estimate the judge dismissal rate, we collect data on all cases during the twelve months prior to the filing and the twelve months following the filing. The mean judge in our sample is assigned to 3,968 cases during these two years, or 1,984 cases annually. 35% of filers are female. The mean worker has been with the firm for four years, and has eleven years of work experience.

Turning to the local statistics, the county level bankruptcy rate (defined at the number of personal bankruptcies per capita) is 0.49%. The mean per capita income is $49,000, mean county population is 1.37 million individuals, and the mean house price is at $238,000 with a lower median of \$172,000. We estimate foreclosure rates of 1.5% within three years and 1.8% within five years among the workers filing Chapter 13 bankruptcy. Comparing Table 1 and 2, which we discuss below, we can see that these rates of foreclosures are significantly higher for those entering bankruptcy relative to their workplace peers.

In Figure 2 we plot the number of employees in each branch. In our sample, the median (mean) branch employees 9 (23) financial advisors and over 30% of branches employ fewer than five financial advisers at the time of the filing. By comparison, branches with over a hundred employees compose under ten percent of the sample. Overall, a majority of branches are small enough to ensure peer interaction.

To better understand the underlying factors that drive personal bankruptcy, we collect and analyze the written financial statements within each financial disclosure. For each financial disclosure, a financial adviser is allowed, but not required, to offer an explanation of the particular event; 31% do so. To analyze this text, we consider five primary drivers of personal bankruptcy identified in past literature, and then identify key words within each statement to match the disclosure to a particular driver. The five drivers we consider are (i) loss of employment income (Keys, 2010), (ii) losses in real estate (Mitman, 2016), (iii) failure of a personal business (Fan and White, 2003), (iv) health issues leading to medical costs Mahoney (2015), and (v) divorce (Traczynski, 2011).[18]

Results are displayed in Panel A of Figure 4. About one-fifth of statements mention the loss of employment or real estate losses and the threat of foreclosures, while about 10% to 13% of statements mention either business losses, medical issues, or a recent divorce. By comparison, 23% of the statements mention none of the explanations above. (An adviser may mention mention multiple drivers with same statement.) In addition, we find that loss of employment income, real estate, and business ownership were especially common drivers during the early years of the sample in the aftermath of the 2008 financial crisis. Meanwhile, medical issues have become more common in the later years of the sample, and divorce as a primary driver has stayed relatively constant over the time period. Overall, the evidence suggests that financial advisors are subject to the same financial shocks as any other household.

[18]We note the category loss of employment income includes (i) income loss of a spouse, and (ii) a partial decrease in employment income. Therefore, the individuals in our sample may be classified as filing bankruptcy due to loss of employment income even while they remain employed with an advisory firm.

Next we analyze whether the adviser statements predict the likelihood of a Chapter 13 bankruptcy dismissal. Since judges decide bankruptcy outcomes based on the ability of each individual to cover debt payments, we expect the underlying motivations of the bankruptcy are predictive of dismissal. As before, we focus on financial disclosures that identify at least one of the five drivers. We expect that negative and unforseen income shocks (due to declines in employment income, business income declines, health issues, or divorce) lead to an inability to cover debt payments, decreasing the likelihood of dismissal from bankruptcy court. In contrast, real estate losses should not affect the ability to repay; instead, individuals who file to protect from foreclosure should be more likely dismissed in bankruptcy court.

In Panel B of Figure 4, we show that a bankruptcy judge is less likely to dismiss a bankruptcy case when the reported driver of bankruptcy is job loss (two percent less), business income (four percent less), health issues (fourteen percent less), or divorce (two percent less). In comparison, if the filing is driven by a decrease in the value of real estate, dismiss rates increase from 34% to 52%. As expected, the stated motivation for the bankruptcy is correlated with the court outcome.

Workforce peer statistics. Table 2 we summarize our dataset of workforce peers. We identify a total of 40,433 coworkers in the sample. The mean judge dismissal rate in this panel is 32% (as in the sample in Table 1). In addition, 29% of the workers are women, with a mean job tenure of four years and mean work experience of ten years. Compared to the workers filing bankruptcy, coworkers have fewer years with the firm and are less likely female.

We estimate a bankruptcy rate of 1% (1.3%) within three (five) years. We find a similar foreclosure rate of 1% (1.3%) within three (five) years. Based on the summary statistics, we identify 527 observations where an employee files for bankruptcy within five years of the filing of the workplace peer. In addition, we identify a similar 544 observations where the employee enters foreclosure.

Judge assignment. Given our identification strategy, we next summarize the judge-specific dismissal rate. For each Chapter 13 bankruptcy in our sample, we identify the judge assigned to the case and then match each case to the judge-specific dismissal rate. The "dismissal rate" is the percentage of Chapter 13 filings dismissed by the judge during the year prior to the filing and in the year following the filing. The dismissal rate is calculated using all cases that he/she sees during the period. It is not limited to cases involving financial advisers in our data. The average judge in our sample sees 3,968 cases over a two year period. According to Table 1 we estimate a mean (median) dismissal rate of 23%.

Bankruptcy judges are randomly assigned within their court district, so our empirical framework compares bankruptcy outcomes within a particular district and year. To ensure the sample size is appropriate, we plot the frequency of bankruptcies within each court district-year pairing in Figure 3. We find that only 120 cases (11% of the sample) are filed in a court-district-year pair with only a single case. In contrast, over half of the cases are in a court district-year with at least five assigned cases in total. Within our sample, the courts with the greatest number of cases are: Florida-Middle, California-Central, Florida-Southern, New Jersey, and Illinois-Northern. We confirm the majority of district-year pairs contain multiple pairings.

We present a histogram of the judge dismissal rate for each of the 1,099 bankruptcy filings in Figure 5. In Panel A we plot the raw dismissal rate. We estimate that roughly 50% of judges have a dismissal rate between 27% and 37%. The bottom 10% of judges have a dismissal rate of 21%, while the top 10% have a dismissal rate of 41%. In Panel B we demean each dismissal rate plot within the district and year.[19] We identify few outliers that could potentially drive the results.

Next, we measure the judge dismissal rate for each case across a two-year window. This assumes that judge dismissal decisions are auto-correlated across time. If this were not the case, then we would not be able to exploit the random assignment of bankruptcy judges in our first-stage analysis. To confirm correlation across dismissal outcomes, we compare the judge dismissal rate in the first year of the sample and the judge dismissal rate in the final year of the sample. After excluding instances where the judge occurs only once in the sample, we have a total of 195 judges with which to conduct our analysis.

As shown in Panel A of Figure 6, we find a positive correlation between the two variables. Specifically, under a simple linear regression, we estimate a coefficient of 0.68 between the two variables. In other words, a judge with a ten percentage point higher dismissal rate in the first year of the sample is predicted to have roughly a seven percentage point higher dismissal rate in the final year of the sample. In addition, in Panel B of Figure 6 we plot the relationship between the

[19] We note that the spike at 0% is from the 120 singleton observations, which are effectively removed from the estimation due to the inclusion of district-year fixed effects.

judge's first and final year after controlling for each court district-year pair. While the results are now centered around zero, we still establish a positive relationship between the two variables: a ten percent increase in the first-year dismissal rate predicts a 8.7% in the final year dismissal rate with a T-statistic of 9.6. Therefore, judge decisions appear highly correlated across the sample.

Finally, in Table 3 we confirm that the dismissal rate appears random across employees. Specifically, we compare the dismissal rate to worker, branch, and local characteristics. For each variable, we run a separate regression to determine the relationship between the variable of interest and the judge dismissal rate. We find no evidence that any variables predict the dismissal rate, suggesting the assignment of judges is random in our setting.

4 Results

4.1 Baseline analysis

Judge assignment and bankruptcy outcomes. To identify the causal effect of debt relief outcomes on social networks, we exploit the random assignment of judges to bankruptcy cases. To confirm this identification strategy, in Table 4 we evaluate the relationship between the Chapter 13 filing outcome—specifically, whether the case is dismissed—and the mean dismissal rate of the judge assigned to the case. The judge dismissal rate is measured as the percent of Chapter 13 filings dismissed by the judge during the two years surrounding the filing date. As judges are only assigned within a given court district, we include year fixed effects interacted with court district fixed effects.

In the first column, the coefficient estimate on "Judge Dismissal Rate" is 1.21, which indicates that a 10% increase in the judge's mean dismissal rate leads to about a 12% increase in the likelihood of a dismissal. The t-statistic is 4.9, so the correlation is highly statistically significant, and is therefore unlikely to suffer from problems associated with weak instruments (Stock and Andrews, 2005). In each additional column, we add controls for the worker (gender, job tenure, and work experience), branch (number of employees), and location (county-level filings rates, population, income, and house prices). After including all controls, we estimate that a 10 percentage point increase in the judge dismissal rate leads to a 12.2 percentage point increase in the likelihood of a dismissal and the results are again highly significant with a t-statistic of 4.8. This confirms the validity of our instrument.

Peer outcomes and bankruptcy. By exploiting the random assignment of judges to bankruptcy court, we are now able to causally identify bankruptcy spillover effects. We use the first-stage regression, equation (1), to develop a measure of bankruptcy outcomes that is orthogonal to coworker characteristics. In Panel A of Table 5 we estimate the relationship between a peer's Chapter 13 filing outcome and the decision to file for bankruptcy. We include court district-year fixed effects and the standard set of controls discussed above. We estimate the effect separately for each year relative to the year of bankruptcy filing.

According to Panel A, we estimate that the dismissal of a peer decreases the probability of filing for bankruptcy by 1.8% within the first year. The result is statistically significant at the 5% level. While a 1.8% effect may appear small, recall that the four-year bankruptcy rate is 1.2%. The effect dissipates two years after filing and is no longer statistically significant. In addition, we illustrate that the effect does not exist prior to the year of bankruptcy filing, again confirming the validity of the instrument.

Foreclosure outcomes. A fundamental question in our analysis is *which* peers are responding to bankruptcy outcomes as the policy implications of our results depend critically on this. On the one hand, if peer dismissal discourages insolvent households from seeking Chapter 13 bankruptcy protection, our results imply a negative externality of social networks. On the other, if peer dismissal discourages solvent households from strategically defaulting, our results provide a more positive outlook.

We now attempt to differentiate between these two differing explanations by evaluating if households discouraged by a peer dismissal instead enter into foreclosure. As discussed earlier, the risk of foreclosure is a primary motivation for filing bankruptcy: seventy percent of dismissed filers explained that the decision to file for bankruptcy was to avoid foreclosure (Porter, 2011). Therefore, if the dismissal of a peer is discouraging insolvent households from filing for bankruptcy relief, we expect a simultaneous increase in the foreclosure rate. If the dismissal instead leads solvent households to a more negative view of the benefits of bankruptcy protection, we should expect a decrease in the foreclosure rate as these households reduce strategic defaults. Using this relationship, we evaluate if households discouraged from a peer dismissal instead enter into foreclosure.

As shown in Panel B of Table 5 we estimate that a peer's Chapter 13 dismissal leads to a 1.9% decrease in the rate of foreclosure within the following year. In contrast to the bankruptcy results in Panel A, we also find a 2.2% decline in the year of the filing. As before, the results find no evidence of any dismissal effect prior to the filing year. Overall, the result lends support to the hypothesis that in response to a peer's dismissal, employees are less inclined to strategically default.

Combining these two outcomes, we estimate peer influence on either filing or foreclosure. We find that a dismissal leads to a 3.8% increase in the likelihood of either bankruptcy or foreclosure. As before, we find no effect prior to the filing.

Cumulative peer effects. Our results provide evidence of an immediate effect of bankruptcy dismissal on peer financial outcomes. A possible explanation is that peer outcomes alter only the timing of the bankruptcy. In this case, we should observe only a temporary delay in filings/foreclosure following dismissal, but no long-term differences in the rate of filing or foreclosure. Therefore, we next estimate the cumulative likelihood of filing personal bankruptcy within four years of a peer's filing.

Following a dismissal, we estimate a 3.9% decline of filing within four years of the initial filing according to Panel A of Table 6. Overall, we find no evidence that the dismissal merely delays a bankruptcy filing. To put these estimates in perspective, we note the likelihood of entering bankruptcy within four years is 1.1% according to Table 2. Similarly, in Panel B we estimate a 0.39% foreclosure rate of the four years. Compared to our estimated coefficient of 1.1, we again observe a significant effect relative to the mean. For the remainder of the paper, we combine the bankruptcy and foreclosure results into a single measure; however, all results remain significant when considering either variable individually.

Chapter 13 and Chapter 7 bankruptcy outcomes. As discussed, we analyze the filing outcomes of Chapter 13 bankruptcies as Chapter 7 bankruptcies have little role for assigned judges and there is little variation in bankruptcy outcomes; our analysis, however, measures peer influence on total bankruptcy filings, both Chapter 13 and 7. Therefore, we next distinguish between these outcome variables.

It is not obvious that we should expect a similar effect on the rate of Chapter 13 and Chapter 7 filings. Assuming peers are primarily learning about the potential downside of Chapter 13 relative to Chapter 7, then we should expect a dismissal leads to a decrease in the likelihood of filing Chapter 13. In comparison, Chapter 7 filings may either increase or decrease in response to a peer dismissal. They may increase if insolvent employees substitute away from Chapter 13 filings. Alternatively, if peers more generally update beliefs about the potential benefits of strategic default, then we should find a dismissal leads to a decrease in the likelihood of Chapter 7 filings. In the latter case, the effects may be especially large as Chapter 7 filings compose over 76% of all bankruptcy filings in the sample.

In Table 7 we consider the separate impact of a peer's filing outcome on Chapter 13 filings (Panel A) and Chapter 7 filings (Panel B). According to our estimates, a dismissal leads to a 1.2% decrease in the likelihood of filing Chapter 13 within the following four years, compared to a 3.0% decrease in the rate of a Chapter 7 filing. The impact on Chapter 13 filings is not statistically significant within the first year, but is significant for the Chapter 7 filings. We note, however, that Chapter 13 filings are much less frequent than Chapter 7 and so power is likely an issue in these tests. Nonetheless, we find no evidence that employees are substituting away from Chapter 13 bankruptcies, as we observe a decrease in the rate of Chapter 7 filings. Rather, the results support the hypothesis that employees update expectations of debtor protections, leading to a decrease in the rate of all strategic defaults.

In untabulated results, we also evaluate whether a peer dismissal alters the proportion of Chapter 13 cases relative to Chapter 7 cases. Assuming workers are updating expectations about the relative benefits of filing Chapter 13, we expect that a peer dismissal leads to a relative increase in the rate of Chapter 7 filings. However, we find no evidence of a change in the proportion, again suggesting that workers are updating expectations about the more general benefits of strategic default.

4.2 Robustness analysis

Placebo test. One potential concern with our setting is that any match between a given judge and a particular case outcome will result in a significant relationship. Therefore, we next conduct a placebo test where workers are randomly assigned to a different judge and court district in the sample. We then re-estimate the effect of the placebo judge on the case dismissal in each of 500 replications.

In Panel A of Figure 7 we plot a histogram of the estimated coefficients across all 500 placebo assignments, while in Panel B we plot the histogram of the t-statistics. First, the coefficients are centered around zero, and in only 1.2% of cases do we estimate a coefficient above 0.1. Second, the t-statistics are centered around zero, and in less then 1% of cases we do estimate a t-statistic above 1.96. Combined, the figures confirm the results are not the result of the particular empirical specification.

Employee, firm, and location controls. The results above include a range of control variables in the analysis. Assuming judges are randomly assigned to bankruptcy court, there is no concern that unobservable factors drive both the dismissal rate of a given judge and the decision of a workplace peer to enter bankruptcy. For evidence of this claim, we previously illustrated that there is no relationship between the judge dismissal rate and the characteristics of workplace peers in Table 3. Therefore, control variables are not necessary for the results.

In Table 8, we confirm that controlling for additional variables has no significant effect on our estimation. We individually include controls at the employee-level (gender, years of work experience, and job tenure), firm-level (number of employees at the branch and the number of branches within the firm), and county-level (rate of bankruptcy filings, income, population, and house prices). Across all nine specifications, we find similar coefficients regardless of the particular controls variables included.

Peer effects across worker subsets. To increase our confidence that the effects are likely the result of peer interaction, we estimate the coefficient for subsets of the sample in Table 9. In the first analysis, we evaluate whether the effects appear greater across homophillic peers. Prior evidence suggests individuals choose to form sub-groups with similar individuals (Carrell et al., 2013); assuming our results are due to peer interaction, we should therefore find larger peer effects among similar employees.

We consider two separate definitions of homophily. First, we define homophillic peers as two workers who share the same gender (either both male or both female). After splitting the sample, we identify 26,446 observations where peers share the same gender and the remaining 13,987 cases where genders differ. According to the results, peer effects are only significant when both workers are share the same gender. Second, we define homophillic peers as two workers starting with the firm within the same three years. Workers entering in the same cohort are more likely to be in similar stages of the career ladder and engaged in similar job responsibilities. Again, we estimate a strong peer effect among workers starting in the same cohort, but no evidence of peer effects for workers in separate cohorts.

Peer effects across firm size and ownership. In Panel B, we consider subsets of the population based on branch size and ownership. There are two separate concerns regarding branch size. First, since two given employees are more likely to interact regularly within smaller branches, excluding larger branches may better identify peer effects. Second, since workers in the smallest firms may be financially-dependent on one another, a shock to one worker may have ripple effects on another worker not due to peer effects. Therefore, we should exclude the smallest branches to better identify peer effects. First, we exclude all branch employing more than 250 employees; second, we exclude all branches employing under ten employees. Under both analyses, we confirm there remains a negative and statistically significant relationship between peer dismissal and financial outcomes.

A separate concern is that workers may be financially-dependent on their firm's owners's financial health for job security and wage growth. Owner bankruptcies may therefore affect worker finances outside of a peer effects channel. Similarly, firm owners may be financially-dependent on workers for productivity. If financial distress leads to lower work output, owners finances may be impacted on the bankruptcy outcomes of workers. Therefore, we exclude (i) owners initially filing Chapter 13 bankruptcy and (ii) owners with a coworker filing Chapter 13 bankruptcy. The results remain unchanged and statistically significant after excluding both subsets of the data. Overall, the results offer additional evidence the effect is driven by peer interactions.

Alternative fixed effects. As judges are only randomly assigned within a given year and district, we have included district-year fixed effects across all empirical specifications. However, the large number of fixed effects relative to observations may bias our results as discussed in Kolesar (2013). For context, we graph the number of observations within each year-district bin in Figure 3. We develop two potential strategies to overcome this concern and present our findings in Panel C of Table 9.

First, we provide estimates excluding establishments in district-year pairs with very few observations. As the median number of establishments in a district-year is five, we confirm our results are largely unchanged after requiring there to be at least five establishments per district-year. Second, rather than include joint district-year fixed effects, we instead control for the mean dismissal rate within each district and year. By including the mean dismissal rate for the district-year, we are controlling for the expected dismissal rate facing the new household. If the household still responds to a peer's dismissal outcome, we are able to clearly identify peer influence. We also include both year fixed effects and district fixed effects, to control for general macroeconomic conditions and differences in characteristics across districts. We estimate the dismissal of a peer reduces the likelihood of a financial event by 3.1 percentage points. The results suggest our results are not due to the large number of fixed effects included in our primary empirical specification.

Geographic differences in Chapter 13 and 7 filing rates. As discussed, Chapter 13 bankruptcies are relatively uncommon compared to Chapter 7 bankruptcies. During our time sample of 2008-2014, Chapter 13 bankruptcies composed roughly 30.7% of nonbusiness bankruptcies, while Chapter 7 bankruptcies composed the remaining 69.3%. However, these differences are differ substantially across districts; while under ten percent of households bankruptcies in Northern Iowa are Chapter 13 filings, they compose over eighty percent in the Western Louisiana district. Given our focus on Chapter 13 bankruptcies, it is valuable to confirm the results are not driven by districts with a high proportion of Chapter 13 filings. As southern districts observe a high proportion of Chapter 13 bankruptcies (as a percent of all households bankruptcies), we exclude all districts from the South-Eastern United States.[20] In unreported results, we confirm a peer's dismissal from Chapter 13 bankruptcies leads to a lower rate of coworker bankruptcies filings and foreclosures after excluding these districts.

4.3 Implications

We conclude this section by analyzing the broader implications of this study. First, we evaluate the relevance of the results beyond the financial advisory industry. Second, we discuss implications for the local economy.

Implications beyond the financial advisory industry. We analyze workers in financial advisory firms because they are required to disclose financial events such as foreclosures and bankruptcies. However, what we classify as peer effects may instead be "disclosure effects": workers are learning about bankruptcy outcomes not through interaction, but analysis of a coworker's public information. In this case, our results may not hold in industries without required disclosures.

To partially overcome this concern, we argue that personally bankruptcy outcomes are at least indirectly observable at the workplace. Specifically, we offer novel evidence that bankruptcy dismissal leads to worse job performance. To measure changes in work performance, we measure the rate of professional misconduct. Our focus on the relationship between bankruptcy outcomes and job performance is motivated by bankruptcy law: following dismissal, creditors are again able to garnish debtor wages for the purpose of debt repayment. The decrease in effective wages results in lower labor incentives, potentially leading to worse job performance. Assuming coworkers are able to observe workplace performance across all industries—not simply in the financial advisory sector—we offer can evidence that bankruptcy dismissal is at least indirectly observable.

This new analysis does not analyze peer effects so we are able to now extend our data sample. We initially included only the first filing within the branch; we now extend the sample to allow multiple filings within the same branch. The number of Chapter 13 filings now analyzed increases from 1,099 to 2,840 observations. We define misconduct following Egan et al. (2019) as any of the

[20] Within states located in the southeast United States (North Carolina, South Carolina, Tennessee, Georgia, Florida, Alabama, Arkansas, Mississippi, Louisiana, Texas), Chapter 13 bankruptcies compose over 54% of all household bankruptcies.

following disclosures: (i) Customer Dispute, either Award/Judgement or Settled, (ii) Employment Separation after Allegations, (iii) Criminal-Final, and (iv) Civil-Final.

According to Panel A of Table 10, we estimate that bankruptcy dismissal is associated with a 9% increase in the misconduct rate within four years after filing. The results help confirm that bankruptcy outcomes are at least indirectly observable in the workplace. In addition, we note our results have implications beyond this study. First, we are the first to highlight the benefits of bankruptcy protection directly on job performance, as opposed to wages. Second, our results contribute to a recent literature documenting the effect of financial distress on work output.[21]

Implications for the Local Economy. This paper highlights the positive consequences of bankruptcy dismissals on the financial condition of workplace peers. In this way, our results contrast with the prior literature, which finds a decrease in financial health following dismissal from court (Dobbie and Song, 2015; Dobbie et al., 2017). From the standpoint of the filing household, bankruptcy protection is an obvious benefit relative to dismissal. However, at the local level, the costs and benefits are less clear. Peer effects may actually aggregate up and lead to worse financial outcomes when compared to the single effect on the filing household. To understand the broader implications of our study on the local economy, it is then natural to understand which effect—personal or peer—dominates.

To analyze the implications of a personal bankruptcy outcomes, we evaluate the causal effect of dismissal on entering foreclosure. We again analyze the full 2,840 Chapter 13 bankruptcy filings as we can include multiple filings within the same branch. According to Panel B of Table 10 we estimate that a Chapter 13 bankruptcy dismissal increases the four-year foreclosure rate by 8.1% relative to receiving bankruptcy protections. The estimate is smaller than the previous estimate of 19 percentage points according to Dobbie and Song (2015), though this is likely driven by our particular focus on financial advisers. Within our current analysis, the result suggests the personal effect is roughly twice the size of the peer effects, highlighting the significance of aggregate peer influence on the local economy.

[21] For instance, Maturana and Nickerson (2017) evaluates how teachers suffering a personal bankruptcy leads lower student performance in the classroom. In a related paper, Bernstein et al. (2017b) documents that wealth shocks due to house price declines impacts the productivity of inventors across public firms. Closest to our paper, Dimmock et al. (2018) evaluate the role of house price shocks on financial adviser misconduct.

Finally, we note that these estimates provide only a lower bound for the true aggregate spillover effects of personal bankruptcies. As discussed in Dahl et al. (2014), individual peer effects are amplified over time within a firm through a snowball effect. In our setting, observing a dismissed bankruptcy decreases the likelihood of filing bankruptcy. As a portion of these new bankruptcies will be discharged-roughly a third according to the data- there will be second-order peer effects throughout the economy. While we are unable to directly quantify the effects in our particular setting, past research estimates these higher-order effects can dominate the original peer interaction.

5 Conclusion

In this paper, we evaluate whether social networks limit the effectiveness of targeted debt relief. By exploiting the random assignment of bankruptcy judges to court cases, we isolate the causal effect of a peer's dismissal on the decision to also file for bankruptcy. Individuals with a peer facing dismissal are 2% less likely to file for bankruptcy within the following two years and 2% less likely to enter foreclosure. The results are robust to a range of specifications, are strongest for homophillic peers, and are not driven by firm owners. We offer preliminary evidence that the aggregated peer effects partially counteract the benefits of bankruptcy protections on the local economy.

While past research suggests social networks may influence the rate of bankruptcy in the economy, we believe we make two contributions to this literature. First, it is generally difficult to distinguish whether (i) a household is responding to the recent bankruptcy of a peer or (ii) both bankruptcies are driven by correlated shocks to either economic and financial conditions or bankruptcy policy. We add to this literature by identifying the role of social interactions through the random assignment of bankruptcy judges. Second, the peer effects of bankruptcy may operate through two channels: households may (i) learn about the costs and benefits of bankruptcy protection or (ii) respond to the perceived social stigma of filing bankruptcy. To our knowledge, this is the first paper to isolate the learning channel. In our experimental setting, households respond to differences in the bankruptcy outcomes of peers, rather than the bankruptcy filing of a peer. Assuming there are minimal differences in social stigma between discharging debt in bankruptcy and dismissal from bankruptcy court, our framework allows us to isolate how households learn about the expected benefits of debt relief from peers.

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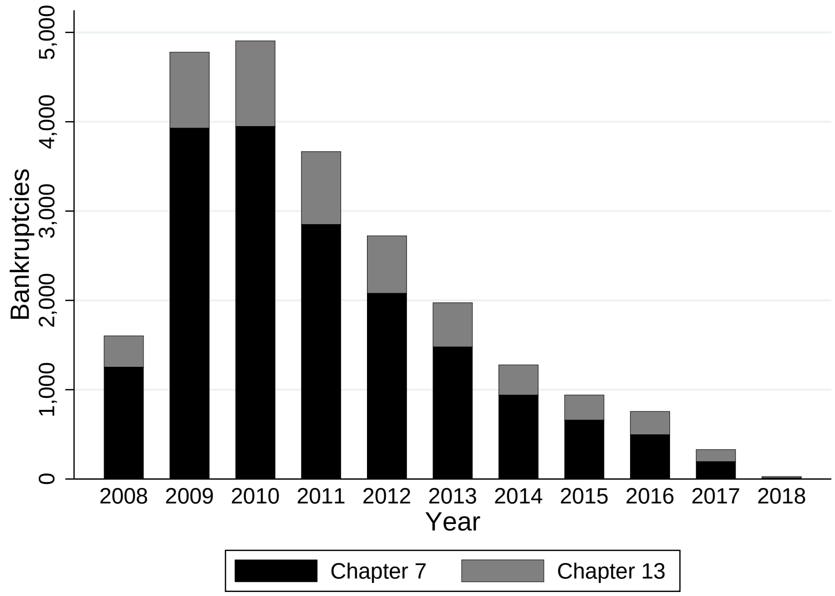
Figure 1: Frequency of Personal Bankruptcies

The figure in Panel A graphs the number of Chapter 13 and Chapter 7 bankruptcies among SEC-Registered Financial Advisers. The figure in Panel B graphs our regression sample of Chapter 13 bankruptcies and the dismissal rates.

Panel A: Financial Adviser Personal Bankruptcies



Panel B: Chapter 13 Bankruptcies within the Regression Sample

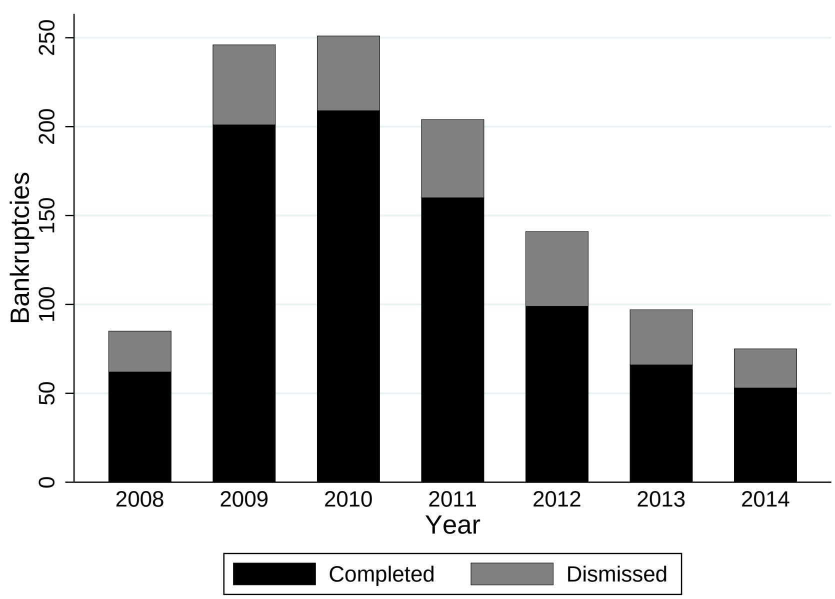


Figure 2: Establishment Size

This figure graphs the employment size of each establishment in the regression sample. We measure the percent of observations within each bin. We winsorize the plot at a hundred employees.

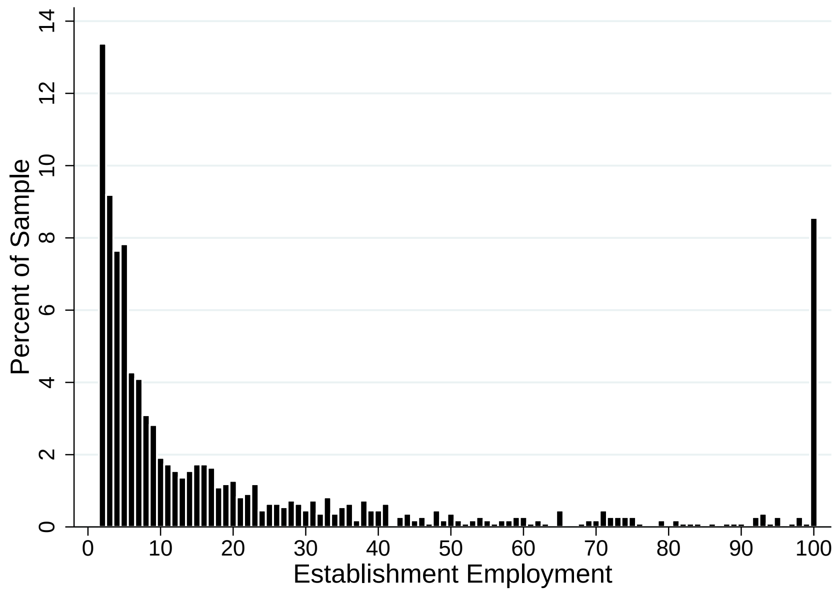


Figure 3: Chapter 13 Bankruptcies within each Court District and Year

This figure graphs the percent of Chapter 13 bankruptcies within each district-year pair.

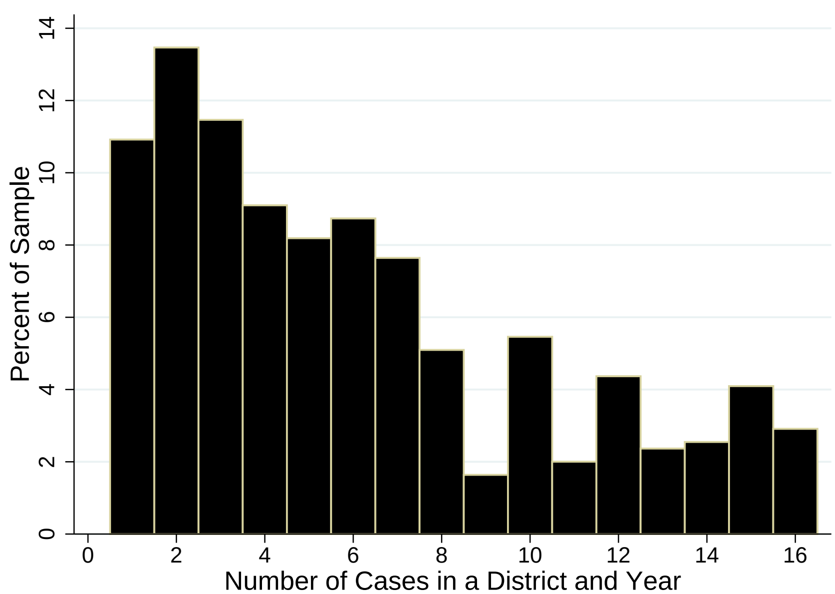
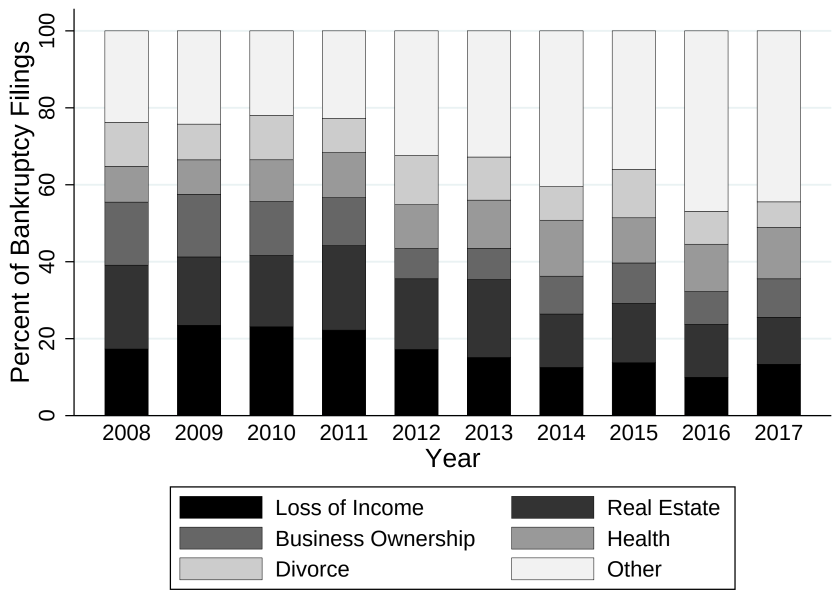


Figure 4: Drivers of Bankruptcy

Panel A graphs the underlying driver of each bankruptcy according to the advisor's written disclosure statements. We analyze the textual statements and split drivers into five categories: loss of employment income, real estate loss, business income loss, health issues, and divorce. We then measure the rate of each driver separately for each year in the sample. Panel B graphs the underlying driver of each bankruptcy according to the advisor's written disclosure statements. We analyze the textual statements and split drivers into the five categories as above. We then measure the rate of each driver for completed cases and cases dismissed from court.

Panel A: Relative Frequency of Drivers



Panel B: Drivers of Bankruptcy by Court Outcomes

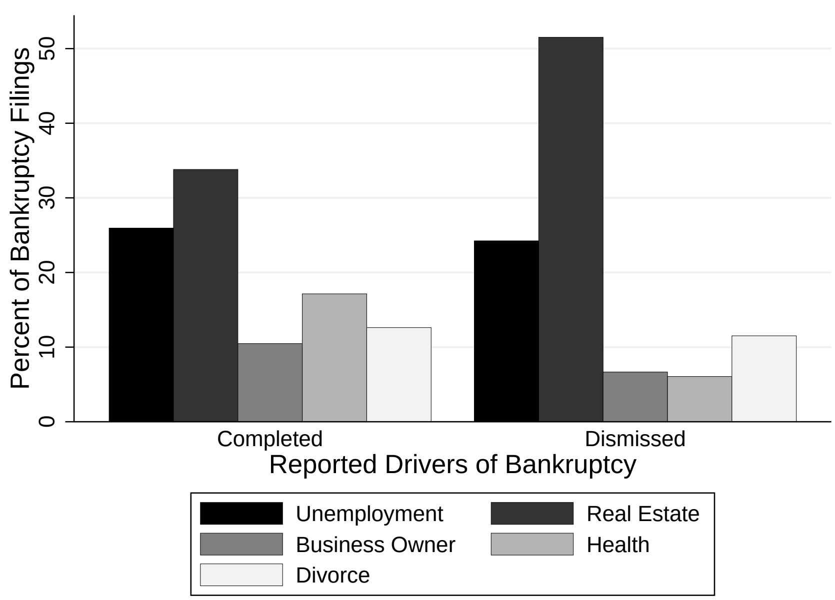
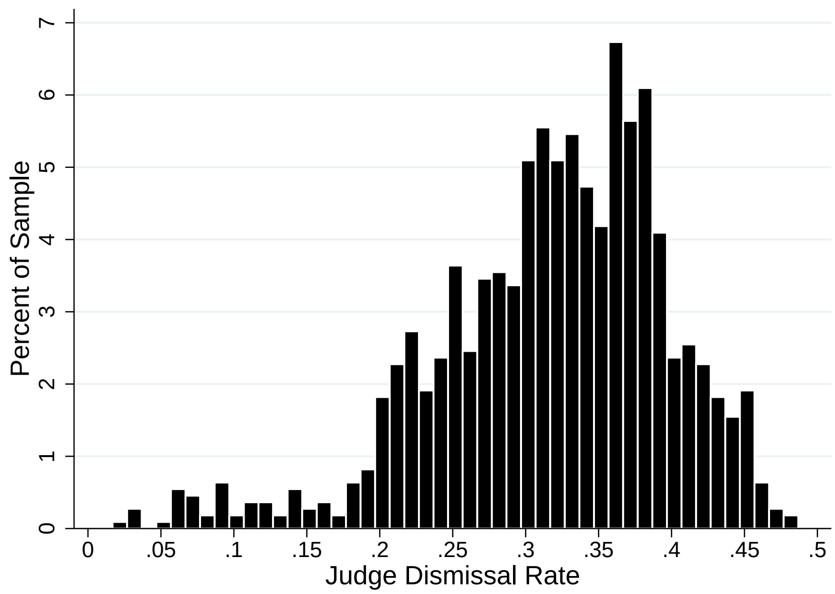


Figure 5: Histogram of Judge Dismissal Rates

Panel A graphs the dismissal rate for each Chapter 13 bankruptcy filing. We measure the percent of observations within each bin. Panel B graphs the dismissal rate for each Chapter 13 bankruptcy filing demeaned at the court district-year level.

Panel A: Judge Dismissal Rate



Panel B: Demeaned Judge Dismissal Rate

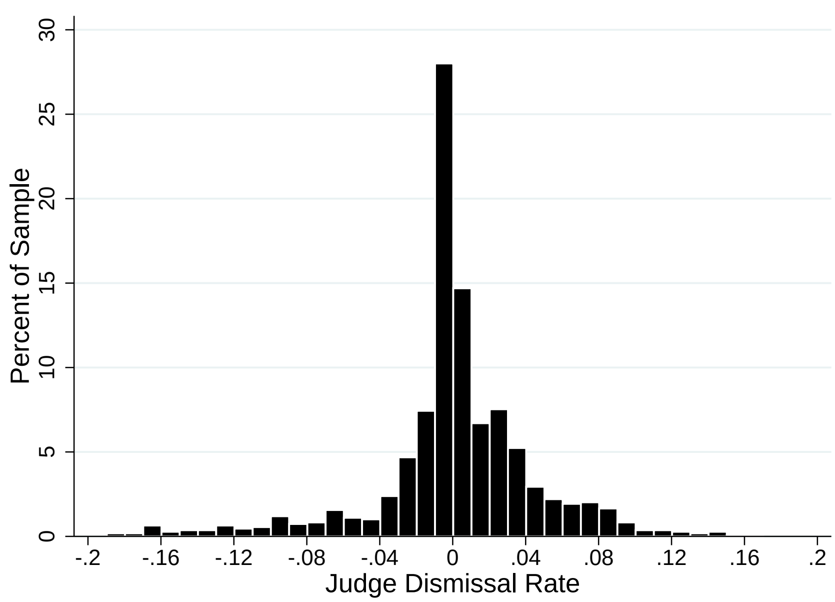
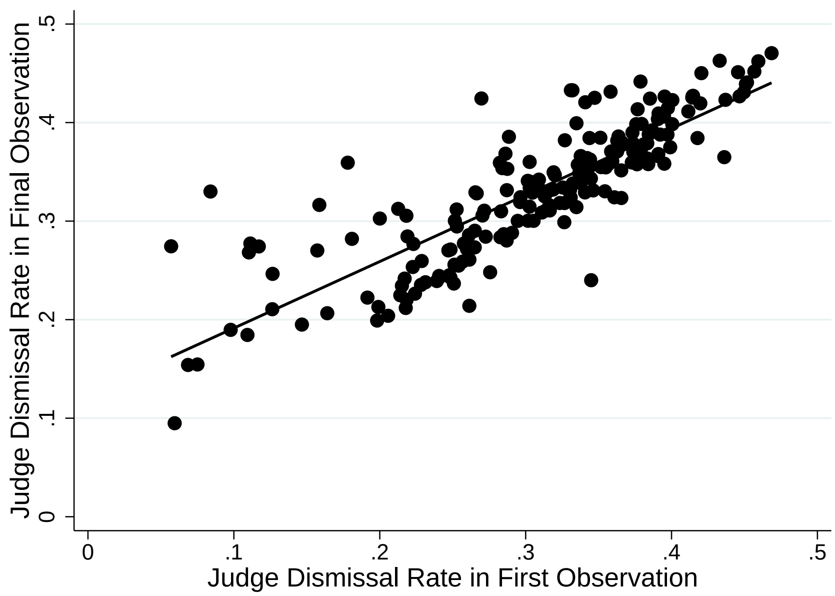


Figure 6: Predictictability of Judge Dismissal Rates

Panel A graphs the predictability of judge dismissal rates. Panel B graphs the predictability of judge dismissal rates demeaned at the court district-year level. For each judge, we collect the judge dismissal rate in the first year of the sample and the last year of the sample. The independent variable is the first-year judge dismissal rate and the dependent variable is the final year judge dismissal rate. We also graph the linear relationship between the two variables.

Panel A: Judge Dismissal Rate Predictability



Panel B: Demeaned Judge Dismissal Rate Predictability

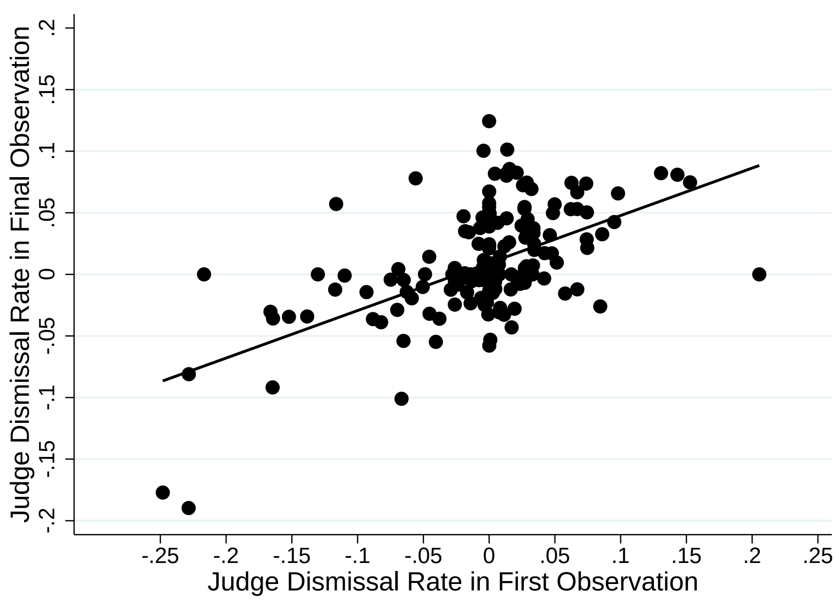
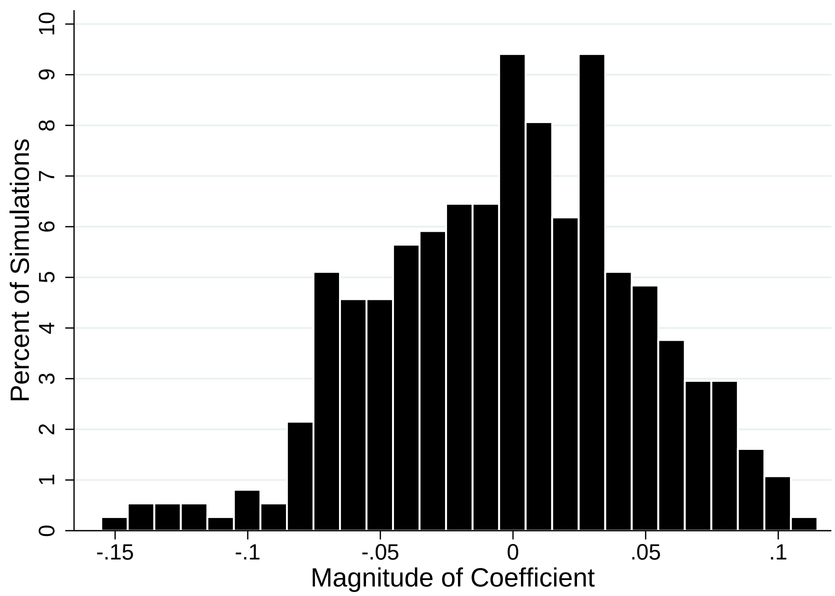


Figure 7: Estimates from Placebo Test

Panel A presents a histogram of the coefficients from the placebo test. Panel B presents a histogram of the T-statistics from the placebo test. Under the placebo test, we randomly assign each Chapter 13 bankruptcy to a judge and court within the sample. We then estimate the relationship between the placebo judge dismissal rate and each bankruptcy outcome. We run this experiment in five-hundred instances and sort the estimated coefficients and T-statistics into bins. We measure the percent of observations within each bin.

Panel A: Coefficients from the Placebo Test



Panel B: T-Statistics from the Placebo Test

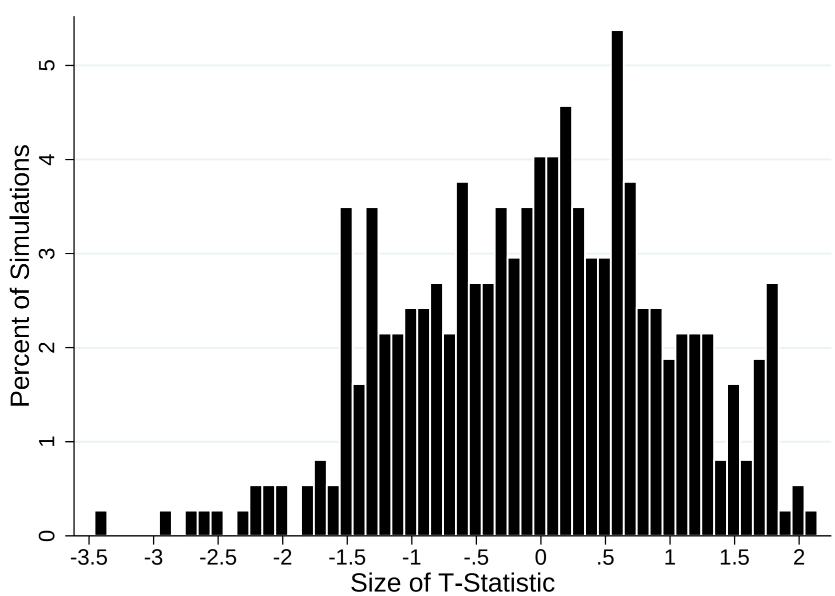


Table 1: Summary Statistics on Initial Chapter 13 Bankruptcy Filings

The table summarizes the sample of 1,099 SEC or FINRA registered financial advisers initially filing Chapter 13 bankruptcy from 2008 and 2014. The sample includes bankruptcies filed while the adviser is employed with an advisory firm with fewer than a thousand advisors. Only the first observed bankruptcy filing for an individual and/or for a particular branch are included in the sample. Therefore, there is one financial adviser per branch included in the sample.

	N	Mean	Median	Std	Min	Max
Dismissal	1099	0.23	0	0.42	0	1
Judge Dismissal Rate	1099	0.32	0.33	0.080	0.017	0.48
Female	1099	0.35	0	0.48	0	1
Job Tenure	1099	4.13	2	4.70	0	40
Work Experience	1099	10.7	10	7.71	0	42
Establishment Employment	1099	37.0	9	87.3	2	978
Local Bankruptcy Rate	1099	0.0049	0.0047	0.0023	0.00086	0.014
Local Income	1099	48974.2	43854	19083.5	20616	150137
Local Population	1099	1372342.0	906949	1679676.2	17067	10066615
Local House Price	1099	238279.8	171500	188356.8	26200	1221700
Foreclosure within One Year	1099	0.0073	0	0.085	0	1
Foreclosure within Two Years	1099	0.012	0	0.11	0	1
Foreclosure within Three Years	1099	0.015	0	0.12	0	1
Foreclosure within Four Years	1099	0.017	0	0.13	0	1
Foreclosure within Five Years	1099	0.018	0	0.13	0	1
Year	1099	2010.6	2010	1.66	2008	2014

Table 2: Summary Statistics on Coworkers

The table summarizes the sample of 40,433 coworkers (peers) of the initial bankruptcy filing financial advisers summarized in Table 1. Each SEC or FINRA registered financial adviser in the sample works at the same branch as one of the workplace peers filing Chapter 13 bankruptcy in the sample summarized in Table 1 during the time of the filing. Since there is only one observation per branch, the observations are at the peer-branch level.

	N	Mean	Median	Std	Min	Max
Peer Dismissal	40433	0.19	0	0.40	0	1
Judge Dismissal Rate	40433	0.32	0.33	0.079	0.017	0.48
Female	40433	0.29	0	0.45	0	1
Work Experience	40433	9.70	8	7.71	0	61
Job Tenure	40433	4.20	2	4.90	0	43
Bankruptcy within One Year	40433	0.0047	0	0.068	0	1
Bankruptcy within Two Years	40433	0.0080	0	0.089	0	1
Bankruptcy within Three Years	40433	0.010	0	0.10	0	1
Bankruptcy within Four Years	40433	0.012	0	0.11	0	1
Bankruptcy within Five Years	40433	0.013	0	0.11	0	1
Foreclosure within One Year	40433	0.0043	0	0.065	0	1
Foreclosure within Two Years	40433	0.0079	0	0.088	0	1
Foreclosure within Three Years	40433	0.010	0	0.10	0	1
Foreclosure within Four Years	40433	0.012	0	0.11	0	1
Foreclosure within Five Years	40433	0.013	0	0.12	0	1
Year	40433	2010.3	2010	1.79	2008	2014

Table 3: Correlation between Judge Dismissal Rate and Treated Characteristics

This table reports the correlation between the judge dismissal rate and the characteristics of the filing worker using the sample outlined in Table 1. We include court district-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.

	Judge Dismissal Rate			
	(1)	(2)	(3)	(4)
Job Tenure	-0.000			
	(-0.25)			
Firm Size		-0.000		
		(-0.57)		
Local Bankruptcy Rate			0.572	
			(0.33)	
Local Income				-0.000
				(-0.43)
District-Year FE	Yes	Yes	Yes	Yes
N	1099	1099	1099	1099
R-squared	.61	.61	.61	.61

Table 4: Judge Dismissal Rate on Bankruptcy Dismissal

This table reports the impact of the judge dismissal rate on the likelihood of a Chapter 13 bankruptcy dismissal using the sample outlined in 2. The dismissal rate is measured as the percentage of Chapter 13 filings dismissed by the assigned judge over the two years surrounding the filing. We include court district-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. Standard errors are clustered at the establishment-year level.

	Judge Dismissal Rate								
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Judge Dismissal Rate	1.207***	1.208***	1.231***	1.228***	1.229***	1.228***	1.227***	1.233***	1.216***
	(4.86)	(4.86)	(4.94)	(4.93)	(4.92)	(4.92)	(4.91)	(4.88)	(4.80)
Female	0.013	0.013	0.028	0.029	0.030	0.030	0.032	0.032	0.035
	(0.22)	(0.22)	(0.46)	(0.47)	(0.48)	(0.48)	(0.52)	(0.52)	(0.56)
Job Tenure		0.001	-0.002	-0.002	-0.002	-0.002	-0.002	-0.002	-0.002
		(0.14)	(-0.48)	(-0.48)	(-0.47)	(-0.47)	(-0.40)	(-0.41)	(-0.39)
Work Experience			0.004	0.004	0.004	0.004	0.004	0.004	0.004
			(1.24)	(1.24)	(1.19)	(1.20)	(1.12)	(1.13)	(1.26)
Firm Size				-0.000	-0.000	-0.000	-0.000	-0.000	-0.000
				(-0.55)	(-0.56)	(-0.55)	(-0.65)	(-0.65)	(-0.65)
No. Establishments					-0.000	-0.000	-0.000	-0.000	-0.000
					(-0.09)	(-0.11)	(-0.05)	(-0.06)	(-0.03)
Local Bankruptcy Rate						4.384	9.886	10.124	11.882
						(0.36)	(0.74)	(0.75)	(0.87)
Local Income							0.000	0.000	-0.000
							(0.93)	(0.94)	(-0.41)
Local Population								-0.000	-0.000
								(-0.15)	(-0.25)
Local House Price									0.000
									(1.06)
District-Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N	1099	1099	1099	1099	1099	1099	1099	1099	1099
R-squared	.33	.33	.34	.34	.34	.34	.34	.34	.34

Table 5: Peer Effects of Bankruptcy Dismissal

This table reports the causal impact of the dismissal of a peer's Chapter 13 bankruptcy filing on financial outcomes using an instrumental variables approach. The sample utilized is outlined in Table 2. We instrument for dismissal by exploiting the random assignment of judges. Panel A evaluates the effect on the likelihood to file bankruptcy. Panel B evaluates the effect on the likelihood of entering foreclosure. Panel C evaluates the combined effect of either filing bankruptcy or entering foreclosure. Each column measures the effect surrounding the year T of filing. The first column measures the effect two years prior to filing, $T-2$, the second column measures the effect one year prior to filing, $T-1$, etc. We use * to denote significance at the 10% level, ** to denote significance at the 5% level, and *** to denote significance at the 1% level. Standard errors are clustered at the establishment-year level.

Panel A: Peer Effects on Bankruptcy

	Years Around Peer Filings				
	T-2	T-1	T	T+1	T+2
Peer Dismissal	-0.007	-0.010	0.003	-0.018**	-0.004
	(-0.99)	(-0.90)	(0.34)	(-2.13)	(-0.62)
Controls	Yes	Yes	Yes	Yes	Yes
District-Year FE	Yes	Yes	Yes	Yes	Yes
N	40433	40433	40433	40433	40433
R-squared	.012	.015	.015	.013	.0059

Panel B: Peer Effects on Foreclosure

	Years Around Peer Filings				
	T-2	T-1	T	T+1	T+2
Peer Dismissal	-0.001	0.001	-0.022**	-0.019**	-0.019*
	(-0.13)	(0.08)	(-2.12)	(-2.11)	(-1.96)
Controls	Yes	Yes	Yes	Yes	Yes
District-Year FE	Yes	Yes	Yes	Yes	Yes
N	40433	40433	40433	40433	40433
R-squared	.018	.011	.0024	.0068	.0084

Panel C: Peer Effects on Combined Bankruptcy and Foreclosure

	Years Around Peer Filings				
	T-2	T-1	T	T+1	T+2
Peer Dismissal	-0.008	-0.009	-0.018	-0.038***	-0.024**
	(-0.77)	(-0.63)	(-1.39)	(-2.86)	(-2.16)
Controls	Yes	Yes	Yes	Yes	Yes
District-Year FE	Yes	Yes	Yes	Yes	Yes
N	40433	40433	40433	40433	40433
R-squared	.017	.013	.011	.0081	.0089

Table 6: Cumulative Peer Effects of Bankruptcy Dismissal

This table reports the cumulative impact of the dismissal of a peer's Chapter 13 bankruptcy filing on financial outcomes using the sample outlined in Table 2. We instrument for dismissal by exploiting the random assignment of judges. Panel A evaluates the effect on the likelihood of filing bankruptcy. Panel B evaluates the effect on the likelihood of entering foreclosure. Panel C evaluates the combined effect of either filing bankruptcy or entering foreclosure. The first column defines the dependent variable as filing within the following year; the second column defines the dependent variable as filing within the following two years, etc. We use * to denote significance at the 10% level, ** to denote significance at the 5% level, and *** to denote significance at the 1% level. Standard errors are clustered at the establishment-year level.

Panel A: Peer Effects on Bankruptcy

	Years Following Peer Bankruptcy			
	1 Year	2 Years	3 Years	4 Years
Peer Dismissal	-0.018**	-0.021*	-0.029**	-0.039**
	(-2.13)	(-1.91)	(-2.21)	(-2.45)
Controls	Yes	Yes	Yes	Yes
District-Year FE	Yes	Yes	Yes	Yes
N	40433	40433	40433	40433
R-squared	.013	.012	.011	.011

Panel B: Peer Effects on Foreclosure

	Years Following Peer Bankruptcy			
	1 Year	2 Years	3 Years	4 Years
Peer Dismissal	-0.019**	-0.028**	-0.041**	-0.039**
	(-2.11)	(-2.06)	(-2.49)	(-2.45)
Controls	Yes	Yes	Yes	Yes
District-Year FE	Yes	Yes	Yes	Yes
N	40433	40433	40433	40433
R-squared	.0068	.011	.01	.016

Panel C: Peer Effects on Combined Bankruptcy and Foreclosure

	Years Following Peer Bankruptcy			
	1 Year	2 Years	3 Years	4 Years
Peer Dismissal	-0.038***	-0.047***	-0.065***	-0.072***
	(-2.86)	(-2.80)	(-3.18)	(-3.29)
Controls	Yes	Yes	Yes	Yes
District-Year FE	Yes	Yes	Yes	Yes
N	40433	40433	40433	40433
R-squared	.0081	.013	.011	.014

Table 7: Peer Effects of Bankruptcy Dismissal on Chapter 13 and 7

This table reports the cumulative impact of the dismissal of a peer's Chapter 13 bankruptcy filing on financial outcomes using the sample outlined in Table 2. We instrument for dismissal by exploiting the random assignment of judges. Panel A evaluates the effect on the likelihood of filing Chapter 13 bankruptcy. Panel B evaluates the effect on the likelihood of filing Chapter 7 bankruptcy. Panel C evaluates the combined effect of either filing bankruptcy or entering foreclosure. The first column defines the dependent variable as filing within the following year; the second column defines the dependent variable as filing within the following two years, etc. We use * to denote significance at the 10% level, ** to denote significance at the 5% level, and *** to denote significance at the 1% level. Standard errors are clustered at the establishment-year level.

Panel A: Peer Effects on Chapter 13 Bankruptcy

	Years Following Peer Bankruptcy			
	1 Year	2 Years	3 Years	4 Years
Peer Dismissal	-0.003	-0.006	-0.010*	-0.012*
	(-0.95)	(-1.09)	(-1.73)	(-1.89)
Controls	Yes	Yes	Yes	Yes
District-Year FE	Yes	Yes	Yes	Yes
N	40433	40433	40433	40433
R-squared	.007	.0069	.0073	.0066

Panel B: Peer Effects on Chapter 7 Bankruptcy

	Years Following Peer Bankruptcy			
	1 Year	2 Years	3 Years	4 Years
Peer Dismissal	-0.016**	-0.018**	-0.021**	-0.030**
	(-2.18)	(-2.07)	(-2.01)	(-2.46)
Controls	Yes	Yes	Yes	Yes
District-Year FE	Yes	Yes	Yes	Yes
N	40433	40433	40433	40433
R-squared	.015	.014	.013	.013

Table 8: Peer Effect of Dismissal on Bankruptcy/Foreclosure with Controls

This table reports the causal impact of the dismissal of a peer's Chapter 13 bankruptcy filing on either entering bankruptcy or foreclosure using the sample outlined in Table 2. We instrument for dismissal by exploiting the random assignment of judges. The dependent variable identifies whether the coworker enter foreclosure or bankruptcy within the following year. Each column an addition controls variable at the employee, firm, and local level. We use * to denote significance at the 10% level, ** to denote significance at the 5% level, and *** to denote significance at the 1% level. Standard errors are clustered at the establishment-year level.

	Bankruptcy/Foreclosure within One Year								
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Peer Dismissal	-0.040***	-0.039***	-0.039***	-0.041***	-0.042***	-0.042***	-0.040***	-0.038***	-0.038***
	(-2.75)	(-2.74)	(-2.75)	(-2.71)	(-2.73)	(-2.74)	(-2.89)	(-2.89)	(-2.86)
Female	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000
	(0.04)	(0.04)	(0.04)	(0.07)	(0.06)	(0.03)	(0.06)	(0.02)	(0.01)
Job Tenure		-0.000**	-0.000**	-0.000**	-0.000**	-0.000**	-0.000**	-0.000**	-0.000**
		(-2.46)	(-2.10)	(-2.09)	(-2.12)	(-2.06)	(-2.02)	(-2.09)	(-2.15)
Work Experience			0.000	-0.000	-0.000	-0.000	-0.000	-0.000	-0.000
			(0.02)	(-0.11)	(-0.11)	(-0.15)	(-0.23)	(-0.17)	(-0.13)
Firm Size				-0.000**	-0.000**	-0.000**	-0.000**	-0.000**	-0.000**
				(-2.24)	(-2.14)	(-2.06)	(-2.08)	(-1.98)	(-2.02)
No. Establishments					0.000	0.000	0.000	0.000	0.000
					(0.08)	(0.16)	(0.25)	(0.36)	(0.42)
Local Bankruptcy Rate						1.322	1.698**	1.866**	2.042**
						(1.53)	(2.00)	(2.27)	(2.35)
Local Income							0.000	0.000	-0.000
							(0.51)	(0.58)	(-0.59)
Local Population								-0.000*	-0.000*
								(-1.82)	(-1.87)
Local House Price									0.000
									(0.93)
District-Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N	40433	40433	40433	40433	40433	40433	40433	40433	40433
R-squared	.0061	.0064	.0064	.0056	.0056	.0057	.0064	.0076	.0081

Table 9: Isolating the Peer Effect of Dismissal on Bankruptcy/Foreclosure

This table reports attempt to isolate the dismissal of a peer's Chapter 13 bankruptcy filing on the likelihood of entering foreclosure or bankruptcy using the sample outlined in Table 2. We instrument for dismissal by exploiting the random assignment of judges. The dependent variable identifies whether the coworkers enters foreclosure or bankruptcy within the following year. Panel A splits the sample between similar peers and dissimiilar peers. The first column identifies peer effects based on whether the employee and peer filing bankruptcy share the same gender; the second column instead considers cases where the gender is different. In the third and fourth column, we split the sample based on whether peers belong to the same cohort (started at the firm within the three years) or different cohorts. Panel B distinguishes the sample based on establishment size and ownership. The first column considers employees at the largest establishments (measured as above 250 employees); the second column excludes employees at the smallest establishments (measured as under ten employees). The third column excludes all observations where a firm owner initially filing Chapter 13 bankruptcy. The fourth column excludes all observations where the peer coworker is a part owner of the firm. Panel C offer two alternate methodologies to decrease the number of fixed effects. The first (second) column include only observations in district-year pairs with at least three (five) bankruptcy filings, respectively. The third and fourth columns include year fixed effects and district fixed effects. The fourth column also controls for the mean dismissal rate within the district-year pair. We use * to denote significance at the 10% level, ** to denote significance at the 5% level, and *** to denote significance at the 1% level. Standard errors are clustered at the establishment-year level.

Panel A: Personal Characteristics

	Gender		Cohort	
	Same	Different	Same	Different
Peer Dismissal	-0.048***	0.068	-0.059***	-0.010
	(-2.91)	(0.83)	(-2.93)	(-0.53)
Controls	Yes	Yes	Yes	Yes
District-Year FE	Yes	Yes	Yes	Yes
N	26446	13987	21350	19083
R-squared	.0034	.024	.0069	.028

Panel B: Establishment Size and Ownership

	Employment Size		Excluding Owners	
	Under 250	Over 10	Excluding Filings	Excluding Peers
Peer Dismissal	-0.030**	-0.045***	-0.036***	-0.032***
	(-2.15)	(-2.83)	(-2.85)	(-3.19)
Controls	Yes	Yes	Yes	Yes
District-Year FE	Yes	Yes	Yes	Yes
N	25212	38493	39189	39414
R-squared	.016	.0017	.0095	.011

Panel C: District-Year Fixed Effects

	Min Observations within District-Year		Excluding District-Year FE	
	Three or More	Five or More	(1)	(2)
Peer Dismissal	-0.039***	-0.036***	-0.030*	-0.031*
	(-2.86)	(-2.76)	(-1.68)	(-1.70)
District-Year Dismissal Rate				0.016
				(0.56)
Controls	Yes	Yes	Yes	Yes
Year FE	No	No	Yes	Yes
District FE	No	No	Yes	Yes
District-Year FE	Yes	Yes	No	No
N	29579	22507	40433	40433
R-squared	.01	.011	.0055	.0054

Table 10: Misconduct and Foreclosure following Dismissal

The table evaluates the full sample of 2,840 SEC or FINRA registered financial advisers initially filing Chapter 13 bankruptcy from 2008 and 2014 while employed with a financial advisory firm. Panel A reports the impact of a Chapter 13 bankruptcy dismissal on the probability of personally committing workplace misconduct. We instrument for dismissal by exploiting the random assignment of judges. Panel B reports the impact of a Chapter 13 bankruptcy dismissal on the probability of personally entering foreclosure. We instrument for dismissal by exploiting the random assignment of judges. The first column defines the dependent variable as committing misconduct within the following year; the second column defines the dependent variable as misconduct within the following two years, etc. We use * to denote significance at the 10% level, ** to denote significance at the 5% level, and *** to denote significance at the 1% level.

Panel A: Financial Misconduct following Bankruptcy Dismissal

	Years Following Personal Bankruptcy			
	1 Year	2 Years	3 Years	4 Years
Dismissal	0.021	0.046	0.076	0.086*
	(0.67)	(1.13)	(1.59)	(1.66)
Controls	Yes	Yes	Yes	Yes
District-Year FE	Yes	Yes	Yes	Yes
N	2840	2840	2840	2840
R-squared	.11	.12	.13	.13

Panel B: Foreclosure following Bankruptcy Dismissal

	Years Following Personal Bankruptcy			
	1 Year	2 Years	3 Years	4 Years
Dismissal	0.040*	0.047	0.079**	0.081**
	(1.85)	(1.63)	(2.37)	(2.29)
Controls	Yes	Yes	Yes	Yes
District-Year FE	Yes	Yes	Yes	Yes
N	2840	2840	2840	2840
R-squared	.11	.12	.11	.12

Appendix

Financial Disclosures and Job Turnover. Strategic default is the decision by a borrower to stop meeting debt payments even though the borrower has the financial ability. Strategic default may be a rational decision when the expected benefits of default outweigh the expected costs. Past research has found evidence that strategic defaults account for a significant fraction of defaults among households (Gerardi et al., 2017; Guiso et al., 2013).[22]

Financial advisers face the standard costs and benefits of filing bankruptcy of entering foreclosure. However, they also face additional costs as both events must be disclosed to prospective clients for the following ten years. Assuming clients view these events as negative signals of the abilities of the adviser, events may lead to the loss of clients and ultimately worse career outcomes. If these costs are too great, then we should find limited evidence of strategic defaults leading to bankruptcy and foreclosure. In this setting, defaults instead occur only when the adviser is not capable of making debt payments; as a result, learning about the costs and benefits of debt protections should have minimal effects on debt outcomes.

In this section, we develop two methods to evaluate whether the costs of financial disclosure necessarily outweigh the potential benefits of strategic default. First, we measure the relationship between disclosing a financial event and job turnover in the following year. Assuming clients respond negatively to the financial disclosure of an adviser, we should expect advisers experience a loss in revenue. As the adviser is less valuable to the firm, we would also expect a rise in turnover rates. While we do not have direct information on the number of clients or annual revenue of each financial adviser in our sample, we do have complete information on the employment history of each adviser across all firms in our sample. According to our hypothesis, a high rate of turnover suggests that defaults are costly because it is necessary to disclose each event; a low rate of turnover suggests that the benefits of strategic default may outweigh the costs. Turning to the empirical specification:

$$Turnover_{it+1} = \beta_1 Filing_{it} + \beta_2 Foreclosure_{it} + +\theta_i + \delta_t + Controls_{it} + \epsilon_{it} \tag{3}$$

[22] For instance, Gerardi et al. (2017) estimate that 38% of the mortgage foreclosures during the Great Recession were strategic defaults.

The dependent variable is $Turnover_{i,t+1}$, a binary variable measuring whether employee i left his employer within the following year $t + 1$. We consider two separate financial disclosures: $Filing_{it}$, which identifies a bankruptcy filing and and $Foreclosure_{it}$, which identifies a foreclosure. In addition, we include several controls. First, we include year fixed effects to control for changes in turnover across the business cycle. Second, as turnover rates likely depend on the employee, we include employee fixed effects to control for unobservable differences and age to control for time-varying factors. Third, we control for local conditions including filing rates, population, income, and house prices. We cluster at the employee level.

According to Table A1, we estimate that without any controls, filing bankruptcy (entering foreclosure) is associated with a 8.3 (12.5) percentage point increase in turnover the following year. However, after including year fixed effects, employee fixed effects and controls, we estimate a significantly smaller effect: filing bankruptcy is associated with a 1.3 percentage point increase, while entering foreclosure is associated with a 2.5 percentage point increase in turnover. Given the annual turnover rate is 24% in our sample, filing (entering foreclosure) is associated with a five (ten) percentage point increase relative to the mean. Overall, we find limited evidence that the costs of financial disclosures are too great to ever warrant strategic default.

We argue our results are likely an upper bound on the effects of financial disclosure on forced turnover as we are not able to distinguish between forced and unforced exits. Unforced turnover is likely to be correlated with financial disclosures for two reasons. First, firms in financial trouble may worsen an employee's financial condition and increase the likelihood they exit for better job opportunities. Second, both filing bankruptcy and entering foreclosure impact homeownership, potentially leading workers to move locations and therefore employers. This may explain why the foreclosure effect is double the bankruptcy filing effect. By including both forced and unforced turnover, we are likely overstating the effects of disclosure on forced exit.

Strategic Default. In our second analysis, we offer evidence that quasi-exogenous variation in the benefits of bankruptcy protection predict the likelihood of filing bankruptcy among advisers. Specifically, we follow the past literature examining state-level variation in the homestead exemption under Chapter 7 bankruptcy (Fay et al., 2002; Gropp et al., 1997). Under Chapter 7, households are required to forfeit all non-exempt assets to pay creditors; however, the definition of non-exempt assets differs across states. For instance, Tennessee, Virginia, and Alabama do not allow exemptions above $20,000, including real estate. In comparison, seven states allow for unlimited homestead exemptions.

A potential concern is that the size of the exemption is not random across states. Specifically, the exemption rate may be correlated with the relative financial distress of the population. Therefore, we analyze how an unexpected financial shock impacts advisers in states with high exemption rates relative to states with low exemption rates. If an equivalent shock leads to higher filing rates when exemption rates are high, our results offer evidence of strategic default. Our empirical specification is therefore:

$$Chapter7_i = \beta_1 High_i \times \Delta HP_i + \beta_2 Mid_i \times \Delta HP_i + \gamma_1 High_i + \gamma_2 Mid + \theta \Delta HP_i + Controls_i + \epsilon_i \quad (4)$$

In our specification i represents an employee. The dependent variable is $Chapter7_i$, a binary variable that identifies the decision to file for Chapter 7 bankruptcy at any point between 2007 and 2012. We also consider three separate dependent variables. First, we split the homestead exemption into terciles: Low_i, Mid_i, and $High_i$. Given the concern that exemption levels may be driven by the state of the economy, we define the exemption levels as of 2007. Second, we define ΔHP_i as the county-level house price decline between 2007 and 2010. We note that ΔHP_i denotes the percent loss; therefore a positive value denotes a decrease in house price between 2007 and 2010. Third, we interact each homestead exemption tercile with the decline in house price. Therefore the interaction term measures the effect of an equivalent house price loss in high exemption states relative to low exemption states. Across all specifications, we control for the 2007 bankruptcy filing rate, per capita income, population, and house price, all at the county-level. We also include employee controls for gender and age. We cluster at the state-level as exemption levels are all the same for employees within the same state.

In Column (1) of Table A2 we offer evidence that the bankruptcy filing rate is 0.4% percentage points higher in the highest exemption states relative to the lowest exemption states. In Column (2), we confirm the results hold after controlling for house price declines. In Column (3), we estimate that a ten percent decline in local house prices has no effect on the Chapter 7 bankruptcy filing rate in low exemption states; however, it leads to a 0.7 percentage point increase in mid-level exemption states and a 0.4 percentage point increase in high exemption states. Across all specifications, we find evidence that high exemption rates increase the Chapter 7 filing rate among advisers. If the costs of financial disclosure always outweigh the benefits of strategic default, we should instead find that advisors do not respond to default incentives across states. Instead, the results suggest that advisers do default strategically based on the expected benefits.

Table A1: Financial Disclosures on Job Turnover

1039115 The table evaluates the annual employment histories of the 1,039,115 SEC or FINRA registered financial advisers employed between 2008 and 2014 with a financial advisory firm. This table reports the impact of the judge dismissal rate on the likelihood of a Chapter 13 bankruptcy dismissal. The dismissal rate is measured as the percentage of Chapter 13 filings dismissed by the assigned judge over the two years surrounding the filing. We include court district-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. Standard errors are clustered at the establishment-year level.

	Job Turnover			
	(1)	(2)	(3)	(4)
Bankruptcy	0.061***	0.053***	0.005	0.004
	(12.72)	(11.10)	(1.18)	(1.02)
Foreclosure	0.130***	0.127***	0.031***	0.029***
	(25.32)	(24.91)	(6.97)	(6.49)
Year FE	No	Yes	Yes	Yes
Worker FE	No	No	Yes	Yes
Controls	No	No	No	Yes
N	4263918	4263918	4263918	4111689
R-squared	.00036	.0072	.43	.44

Table A2: Homestead Exemptions on Filing

The table evaluates the financial outcomes of the 431,968 SEC or FINRA registered financial advisers employed in 2008 with a financial advisory firm. This table reports the impact of the judge dismissal rate on the likelihood of a Chapter 13 bankruptcy dismissal. The dismissal rate is measured as the percentage of Chapter 13 filings dismissed by the assigned judge over the two years surrounding the filing. We include court district-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. Standard errors are clustered at the establishment-year level.

	Chapter 7 Bankruptcy Filing		
	(1)	(2)	(3)
High X HP Loss			0.036**
			(2.28)
Mid X HP Loss			0.068***
			(8.08)
High Exemption	0.004**	0.004**	-0.001
	(2.35)	(2.41)	(-0.48)
Mid Exemption	0.002	0.004	-0.005***
	(0.70)	(1.52)	(-2.69)
House Price Loss		0.028***	0.005
		(3.45)	(0.79)
Worker Controls	Yes	Yes	Yes
N	431968	431968	431968
R-squared	.0022	.0026	.0029