Consumer Bankruptcy and Financial Health[*]

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Abstract

This paper estimates the effect of Chapter 13 bankruptcy protection on financial health using a new dataset linking bankruptcy filings to credit bureau records. Our empirical strategy uses the leniency of randomly-assigned judges as an instrument for Chapter 13 protection. We find that Chapter 13 protection decreases an index measuring adverse financial events such as civil judgments and repossessions by 0.323 standard deviations and increases the probability of being a homeowner by 13.2 percentage points. Chapter 13 protection has little impact on open unsecured debt, but decreases the amount of debt in collections by $1,333.

America is a nation of debtors. The amount of debt held by the average American consumer increased from \$31,840 to \$45,500 between 2003 and 2013, with more than 14 percent of consumers having at least one debt in collections by 2013 (Federal Reserve Bank of New York 2014). Theoretical work has long suggested that excessive debt and financial distress can distort repayment, consumption, and labor supply decisions (e.g. Myers 1977, Krugman 1988). For example, borrowers with excessive debt have an incentive to avoid repayment through strategies with significant deadweight costs, such as leaving the formal banking system to avoid asset seizures or leaving the formal labor market to avoid wage garnishment. Consistent with this literature, recent empirical work shows that debt overhang reduces labor supply (Dobbie and Song 2015) and increases mortgage default rates (Melzer 2012). Perhaps the most important program meant to alleviate the adverse consequences of financial distress is the consumer bankruptcy system, the legal process to resolve unpaid debts in the United States.

The consumer bankruptcy system allows debtors to choose between Chapter 7 bankruptcy, which provides debt relief and protection from wage garnishment in exchange for a debtor's non-exempt assets, and Chapter 13 bankruptcy, which adds the protection of most assets in exchange for a partial repayment of debt. Each year, more than one million Americans file for bankruptcy protection, with nearly one in ten American households having filed for bankruptcy at some point in their lifetimes (Stavins 2000). Before filing, bankruptcy filers are nearly two and half times as likely to have a delinquent debt compared to the typical credit user, twice as likely to have a debt in collections, and three times as likely to have a lien or repossession. Even after filing, bankruptcy filers are much more likely to experience financial distress than the typical credit user.[1]

Despite over one percent of American households filing for bankruptcy each year, little is known about whether bankruptcy protection reduces or exacerbates financial distress. In theory, bankruptcy protection directly benefits debtors by improving their balance sheets and preventing the seizure of important assets such as a home or car. These direct benefits may in turn indirectly benefit debtors by increasing their credit scores, access to credit, or earnings. Yet, in practice, there

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[1]See Table 1 for details and additional summary statistics.

is little empirical evidence that bankruptcy protection provides any economically significant benefits to debtors. Cross-sectional comparisons suggest that bankruptcy filers work about the same number of hours and accumulate less wealth than non-filers (Han and Li 2007, 2011), and within-individual comparisons show that filers have less access to credit after receiving bankruptcy protection (Cohen-Cole, Duygan-Bump and Montoriol-Garriga 2013, Jagtiani and Li forthcoming). However, much of this prior work has been hampered by the lack of a plausible comparison group. Bankruptcy filers likely had worse outcomes even before filing, biasing cross-sectional estimates, and the most commonly reported causes of filing, such as job loss, also impact later outcomes, biasing within-individual estimates.[2]

This paper uses a new dataset linking bankruptcy filings to credit bureau records to estimate the impact of Chapter 13 bankruptcy protection on post-filing financial outcomes. We estimate the ex-post causal effect of Chapter 13 protection by comparing the outcomes of filers randomly assigned to bankruptcy judges with different propensities to grant bankruptcy protection. The identified parameter measures the treatment effect for filers whose bankruptcy decision is altered by the judge assignment due to disagreement on whether or not they should receive bankruptcy protection (i.e. the marginal recipients of bankruptcy protection). Using the same identification strategy, Dobbie and Song (2015) find that Chapter 13 protection increases earnings and decreases mortality risk.[3] The estimates in this paper capture the reduced form impact of bankruptcy protection on financial health through all potential mechanisms, including this previously documented earnings effect. While we cannot specifically identify each potential mechanism driving the effect of bankruptcy protection on financial health, our reduced form estimates are relevant to any policy

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[2]Dobbie, Goldsmith-Pinkham, and Yang (2015) estimate a variety of non-experimental specifications to reconcile our instrumental variable estimates with the estimates from this prior literature (e.g. Han and Li 2007, 2011, Cohen-Cole, Duygan-Bump and Montoriol-Garriga 2013, Jagtiani and Li forthcoming). We find that OLS estimates using a non-filer comparison group and within-individual estimates are both biased against finding any benefits of bankruptcy protection. In contrast, estimates using a dismissed filer comparison group are broadly consistent with our instrumental variable estimates, suggesting that selection into filing accounts for most of the bias in non-experimental specifications.

[3]Kling (2006) uses a similar empirical strategy to estimate the ex-post impact of sentence length on earnings, and subsequent papers have used similar methodologies to estimate the ex-post effects of foster care (Doyle 2007, 2008), juvenile incarceration (Aizer and Doyle 2015), corporate bankruptcy (Chang and Schoar 2008), temporary-help employment (Autor and Houseman 2010), and Disability Insurance (Maestas, Mullen, and Strand 2013, French and Song 2014).

reform that alters the costs of filing for bankruptcy, such as the 2005 Bankruptcy Abuse Prevention and Consumer Protection Act.[4]

There are two main contributions of this paper relative to Dobbie and Song (2015). First, we estimate the effect of Chapter 13 protection on a broad range of financial outcomes that shed new light on the well-being of debtors. We employ a new dataset constructed for the purposes of this study that links over 175,000 bankruptcy filings to credit bureau records. These data allow us to examine the reduced form effect of consumer bankruptcy on post-filing adverse financial events, unsecured debt, secured asset holdings, credit access, and credit score. Moreover, because we observe detailed information on distressed borrowers both before and after bankruptcy, we are able to provide new evidence on the long-term consequences of excessive debt and the extent to which bankruptcy protection mitigates these adverse consequences. Second, we describe the characteristics of filers who are more likely to be affected by judge assignment. We find that young filers are more likely to be affected by a lenient judge assignment but that there are no differences by baseline credit score or homeownership status. These results provide new evidence on the types of cases for which the instrumental variables estimates are most likely to apply and the types of filers who are most likely to be affected by changes in bankruptcy laws.

In our empirical analysis, we find that Chapter 13 protection is largely successful in alleviating the most direct adverse consequences of excessive debt. Over the first five post-filing years, Chapter 13 protection decreases an index measuring adverse financial events such as civil judgment and repossession by 0.323 standard deviations for the marginal recipient and significantly decreases seven of the eight individual measures of financial strain that compose the index. Chapter 13 protection has little impact on the amount of open unsecured debt for the marginal recipient, but the amount of debt in collections decreases by $1,333, a 31.6 percent change from the dismissed filer mean of $4,217. These results suggest that the marginal recipient of Chapter 13 protection reduces his or her unsecured debt through the bankruptcy system, while the marginal non-recipient is un-

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[4]Our estimates hold fixed any independent effects of bankruptcy filing, such as having a bankruptcy flag on a credit report (Han, Keys, and Li 2013), and any ex-ante impacts of bankruptcy, such as over-borrowing, moral hazard in the workplace (White 2011), entrepreneurial risk-taking (Fan and White 2003, Armour and Cumming 2008), or the crowding out of formal insurance (Mahoney 2015).

able to prevent his or her unsecured debts from being sold to a third-party debt collector. Chapter 13 protection also increases the probability that the marginal recipient retains his or her home by 13.2 percentage points, a 36.4 percent increase from the dismissed filer mean of 36.3 percent, but there are no discernible effects on the probability of having a car. The direct effects of Chapter 13 bankruptcy protection on financial strain, the amount of debt in collections, and the probability of retaining a home persist through the sixth to eighth post-filing years, suggesting sustainable and real improvements in marginal recipients' financial health.

Chapter 13 protection also has important impacts on credit access proxies and credit score, two financial outcomes not directly affected by bankruptcy protection. Over the first five post-filing years, Chapter 13 protection decreases the marginal recipient's revolving credit utilization by 16.3 percentage points, a 34.8 percent change from the dismissed filer mean, and decreases the number of non-mortgage inquiries by 0.300, a 18.9 percent change from the dismissed filer mean. Chapter 13 protection increases the marginal recipient's credit score by 17.0 points over the first five post-filing years, a 3.0 percent increase from the dismissed filer mean. Consistent with the financial health and debt results, we find that the indirect effect of Chapter 13 protection on credit scores also persists for several years after the bankruptcy procedure is completed.

We find suggestive evidence that protection from debt collectors and debt forgiveness are both important mechanisms that help explain our results, although large standard errors make definitive conclusions impossible. To test the importance of protection from debt collectors, we compare treatment effects in states that do and do not allow wage garnishment. Consistent with there being significant costs of debt collection, we find large and statistically significant effects of Chapter 13 protection in states that allow wage garnishment but small and imprecisely estimated effects in the four states that prohibit wage garnishment and where creditors have fewer options to collect unpaid debts from dismissed filers. However, only one of eight differences is statistically significant due to large standard errors. To test the importance of debt forgiveness, we compare treatment effects in states with higher and lower Chapter 7 homestead exemption levels. Since Chapter 13 requires that creditors are repaid at least as much as they would have received in Chapter 7, homeowners who

file for Chapter 13 in high exemption states are required to repay creditors less than filers in low exemption states. Consistent with the benefits of Chapter 13 protection increasing in the amount of debt that is forgiven, we find that the effects of Chapter 13 protection are larger for homeowners in states with high Chapter 7 exemption levels compared to homeowners in low Chapter 7 exemption states. However, only two of eight differences are statistically significant due to the imprecision of our estimates.

The remainder of the paper is structured as follows. Section I provides a brief overview of the consumer bankruptcy system in the United States. Section II describes our data and provides summary statistics. Section III describes our empirical strategy. Section IV estimates the impact of Chapter 13 bankruptcy protection on post-filing financial outcomes. Section V discusses the potential mechanisms underlying our estimates, and Section VI concludes.

I. Chapter 13 Bankruptcy Protection

A. Overview

Under Chapter 13 bankruptcy, filers propose a three- to five-year plan to partially repay their unsecured debt in exchange for a discharge of the remaining unsecured debt, a hold on debt collection, and the retention of most assets.[5,6] Chapter 13 requires filers to use all of their disposable income, defined as their predicted income less predicted expenses, to repay creditors. Creditors must receive at least as much as they would have received if the filer's assets were liquidated under Chapter 7, a requirement known as the "best interest of creditors" test. Chapter 13 filers are also required to fully repay priority claims, such as child support and alimony, unless the claimant agrees to a reduced payment. If a filer wants to keep any collateral securing a claim, he or she must keep

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[5]During our sample period, Chapter 13 filers were able to choose the length of their repayment plan. In our estimation sample (described in Section II), discharged filers took an average of 3.7 years to complete their plan. In a random sample of bankruptcy filings where we coded detailed information on plan terms (described in Section I.B), filers proposed an average plan length of 4.1 years and proposed to repay 36.4 percent of their unsecured debt.

[6]Debtors also have the option of filing under Chapter 7, which discharges unsecured debts and stops collection efforts in exchange for any non-exempt assets. Chapter 7 bankruptcy does not allow debtors to retain non-exempt assets or collateral securing delinquent debt. Our analysis focuses on the effects of Chapter 13 protection due to limited variation in the treatment of Chapter 7 cases. See Dobbie and Song (2015) for additional details and a discussion of the differences between Chapter 7 and Chapter 13.

up to date on all current payments and include any arrears in the repayment plan. The filer can also choose to give up the collateral and discharge the remaining debt. Thus, Chapter 13 allows filers to avoid a costly home foreclosure or the repossession of a car by including any arrears in the repayment plan, with the original debt contract reinstated on the completion of the Chapter 13 repayment plan.[7]

Chapter 13 cases begin with the debtor filing a bankruptcy petition, a statement of financial affairs, a list of creditors and amounts owed, a copy of his or her most recent tax return, executory contracts and unexpired leases, and schedules of current income, expenditures, and assets and liabilities. During our sample period, a debtor must pay a filing fee of $185 and an attorney's fee of about $3,000. Following the filing of the bankruptcy petition and payment of the filing fee, a trustee and bankruptcy judge are generally assigned within a few days. A proposed Chapter 13 repayment plan must then be filed with the petition or no later than 15 days after the petition is filed.[8] The bankruptcy trustee then holds a meeting with the debtor and any interested creditors in order to resolve problems with the proposed repayment plan.[9] In order to preserve their independent judgment, bankruptcy judges are prohibited from attending the creditors' meeting. After this meeting, a debtor has the opportunity to file an amended repayment plan. At a subsequent confirmation hearing, the bankruptcy judge decides whether the repayment plan is feasible and meets the standards for confirmation set forth in the Bankruptcy Code. If the judge confirms the repayment plan, the debtor makes biweekly or monthly payments to the trustee until the plan is complete.

Even if a plan is initially confirmed, it may later be dismissed if the debtor fails to make payments, fails to pay post-filing domestic support obligations, or fails to make required tax filings during the case. To the extent that a debtor's financial circumstances change following the confir-

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[7]In a sample of Delaware cases, 71 percent of filers included mortgage arrears in their repayment plans, 41 percent included car loans, and 38 percent included priority debt (White and Zhu 2010). Survey evidence suggests that approximately 70 percent of filers choose Chapter 13 in order to avoid foreclosure (Porter 2011).

[8]The 2005 Bankruptcy Abuse Prevention and Consumer Protection Act (BAPCPA) increased filing fees and imposed mandatory credit counseling. Beginning December 1, 2009, the repayment plan also had to be filed within 14 days of the petition.

[9]There is typically one Chapter 13 bankruptcy trustee who works with all judges in an office. If an office has a particularly high Chapter 13 caseload, judges may have their own Chapter 13 trustee. As a result, it is not possible to isolate the independent impact of trustees on the probability of receiving bankruptcy protection using our empirical methodology.

mation of the repayment plan due to job loss or health shocks, he or she also has an opportunity to request a plan modification. If a plan modification is requested, creditors again have an opportunity to object, followed by a confirmation hearing by the judge. The judge may also dismiss or convert the case to Chapter 7 if the filer fails to satisfy the plan's requirements. If a Chapter 13 filing is dismissed without prejudice, a debtor may refile for either Chapter 7 or Chapter 13 after 180 days.

We estimate the benefits of Chapter 13 protection, net the costs of repayment, compared to the best outside option for the marginal dismissed filer. During our sample period, we find that approximately 27 percent of dismissed filers convert or refile for Chapter 7 bankruptcy within one year, with another one percent refiling under Chapter 7 at some point after one year. Conditional on converting or refiling under Chapter 7, 95 percent of dismissed Chapter 13 filers are able to discharge at least some of their debt through Chapter 7. About another 13 percent of dismissed filers refile under Chapter 13 and are dismissed a second time, with about 2.5 percent of dismissed filers refiling under Chapter 13 successfully. The remaining 57 percent of dismissed Chapter 13 filers never file for bankruptcy protection again.

B. Bankruptcy Judges

Bankruptcy judges are federal judges appointed to 14-year terms by the Court of Appeals in their judicial district. There are a total of 94 federal bankruptcy courts in the United States, including at least one bankruptcy court in each state, the District of Columbia, and Puerto Rico. Each bankruptcy court hears all cases originating from counties in its jurisdiction and are often further divided into offices that hear all cases originating from a subset of counties in the court's jurisdiction. Bankruptcy judges often hear cases across multiple offices within their court but only hear cases filed in their bankruptcy court. These cases are typically assigned to judges using a random number generator or a blind rotation system within each office.[10]

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[10]The median court in our sample is divided into three offices, with little systematic pattern to the number of offices in each court. There is considerable variation in the number of bankruptcy judges in each bankruptcy court and office, with courts serving more populous regions tending to have more judges. Of the 205 offices we observe in our data, 110 have only one Chapter 13 judge, 52 have two Chapter 13 judges, 25 have three Chapter 13 judges, and 18 have four or more Chapter 13 judges. See Dobbie and Song (2015) for additional details.

The assigned bankruptcy judge decides all matters connected to a case, including whether the repayment plan is feasible and meets the standards for confirmation set forth in the Bankruptcy Code. Common reasons for dismissal include the debtor being able to repay his or her debts without bankruptcy protection, the repayment plan repaying creditors too little, or the repayment plan being infeasible given the debtor's predicted income and expenses (Hynes 2004). In Section III, we discuss how we use systematic differences in the probability that a judge dismisses a filing to estimate the causal impact of bankruptcy protection. The variation in judge behavior that we measure is likely to be driven by differences in how judges interpret the above criteria, which can occur at the initial confirmation hearing or at subsequent confirmation hearings. If a debtor defaults on plan payments post-confirmation, a judge may dismiss the plan immediately or allow the debtor to file a plan modification, giving the debtor some time to make up late payments.

Our empirical strategy assumes that judges only impact future outcomes through the probability of receiving bankruptcy protection. This exclusion restriction would be violated if judges affect debtor outcomes in other ways. For example, the exclusion restriction would be violated if more lenient judges give particularly helpful financial advice during the confirmation meeting or if debtors and their lawyers file different repayment plans when assigned to more lenient judges. The assumption that judges only systematically affect debtor outcomes through bankruptcy is fundamentally untestable, and our estimates should be interpreted with this potential caveat in mind. However, we argue that the exclusion assumption is not unreasonable in our setting. Despite the central role of bankruptcy judges, debtors typically have only limited interaction with the assigned judge. Chapter 13 filers appear before the bankruptcy judge only at the plan confirmation hearing, and judges are prohibited from attending the creditors' meeting or offering any legal advice. All administrative aspects of the bankruptcy process are conducted by the bankruptcy trustee and not the judge. Thus, it seems unlikely that judges would significantly impact debtors other than through their probability of receiving Chapter 13 protection.

We can also partially test the validity of the exclusion restriction by examining whether filers and their lawyers strategically alter the terms of their repayment plans in response to the identity

of the judge. We analyze a random sample of bankruptcy cases and collect detailed information on plan terms and pre- and post-confirmation modifications from electronic filings on Public Access to Court Electronic Records (PACER).[11] Columns 1 and 2 of Online Appendix Table 1 present filing characteristics separately for filers assigned to judges with above and below median leniency. Column 3 reports results from a series of OLS regressions of each filing characteristic on an indicator for being assigned to a judge with above median leniency and office-by-filing-month fixed effects with standard errors clustered by office. There are no economically or statistically significant differences in the initial plan length, the initial repayment rate, the probability of having a pre-confirmation plan amendment, or the probability of having a pre-confirmation plan modification. These results suggest that filers and their lawyers are not significantly tailoring their bankruptcy plans in response to the assigned judge. None of the results suggest that the exclusion restriction is likely to be invalid in our setting.

C. Potential Benefits of Chapter 13 Protection

There are at least three reasons that debtors may directly benefit from Chapter 13 bankruptcy protection. First, filing for and obtaining bankruptcy protection puts a hold on current and future debt collection efforts.[12] Bankruptcy protection may therefore decrease the incentive to avoid repayment through strategies with significant deadweight costs, such as leaving the formal banking system to avoid seizure of assets or leaving the formal labor market to avoid wage garnishment.[13]

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[11]We construct our random sample in three main steps. First, we restrict our sample to the offices for which we have filings during all years in our study (2002-2005), leaving us with 29 offices. Second, we categorize each office into quartiles of variation in our measure of judge leniency as measured using our full sample of 253,863 filings. We randomly select one office from each of the quartiles to ensure a representative distribution of our source of variation, leaving us with five offices in four bankruptcy courts: Atlanta, Tulsa, Newport News, Miami, and San Diego. Other than the San Diego office, which has four bankruptcy judges, the remaining four offices have two judges each. Third, from the estimation sample of bankruptcy filings linked to TransUnion credit records, we randomly select ten bankruptcy filings in December 2003 from each judge-office combination, resulting in 120 filings. We choose filings in December 2003 because bankruptcy filings were available for electronic public access on PACER beginning December 1, 2003.

[12]Dismissed filers receive a temporary stay on collections activity that lasts until the filing is dismissed. Estimates on debt collections activity are therefore likely to be biased downwards, at least in the short run.

[13]Creditors have a number of options to collect unpaid debts if a debtor has not filed for bankruptcy protection or after a case is dismissed, including wage garnishment, collection letters or phone calls, in-person visits at home or work, and seizing of assets through a court order. Debtors can make these collection efforts more difficult by ignoring collection letters and calls, changing their telephone number, or moving without leaving a forwarding address. Debtors

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Second, Chapter 13 protection discharges any unsecured debts not repaid under the proposed plan, significantly improving a debtor's balance sheet. Third, Chapter 13 bankruptcy allows debtors to restructure secured debts, such as a car or mortgage loan. Creditors are allowed to seize assets securing a delinquent loan if a debtor has not filed for bankruptcy protection or after a case has been dismissed, suggesting that Chapter 13 may allow debtors to retain important assets and avoid a potentially costly repossession or foreclosure.

There are also several potential indirect benefits of bankruptcy protection. Most importantly, bankruptcy protection may benefit debtors by increasing their access to credit through an improved balance sheet and fewer adverse collection events reported on a credit record. This may allow debtors to avoid more costly forms of credit, such as pawn or payday loans. Bankruptcy protection may also prevent any sharp drops in consumption that have important long-term consequences, such as becoming sick due to the lack of medical care. Finally, bankruptcy protection may increase economic stability by allowing debtors to avoid foreclosure or eviction.

There are also many reasons to believe that Chapter 13 protection will have little impact on debtors. First, it is possible that the bankruptcy process may exacerbate financial distress by forcing filers to devote all of their disposable income to the repayment plan. It is also possible that debtors are able to avoid most debt collection efforts at a relatively low cost or that collections strategies do not significantly affect most debtors. Finally, bankruptcy protection will have little impact if filers' financial distress stems from broader economic conditions or immutable individual characteristics such as low human capital.

Our paper presents causal estimates of the net impact of receiving bankruptcy protection on measures of financial well-being. While we are unable to identify the separate impact of all the potential channels through which bankruptcy protection affects financial health, our reduced form estimates will likely reflect a number of the potential mechanisms discussed in this section. In Section IV.V, we explore two mechanisms that may explain our findings: protection from debt

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can also leave the formal banking system to hide their assets from seizure, change jobs to force creditors to reinstate a garnishment order, or work less so that their earnings are not subject to garnishment. See Hynes, Dawsey, and Ausubel (2013) for additional discussion of the debt collection process.

collectors and debt forgiveness.

II. Data

A. Data Sources and Sample Construction

Our empirical analysis uses data from individual bankruptcy filings merged to credit bureau records from TransUnion. The bankruptcy records come from the 72 (out of 94) federal bankruptcy courts that allow full electronic access to their dockets. These data include approximately 75 percent of all filings during our sample period. Each record includes information on the filer's name, address, bankruptcy chapter, filing date, court, office, outcome, and the name of the judge and trustee assigned to the case.

Following Dobbie and Song (2015), we make four restrictions to the bankruptcy data. First, we drop filings from 110 offices that only have a single Chapter 13 bankruptcy judge and filings from counties that assign all cases to a single judge, as in both scenarios there is no variation in judge leniency that allows us to estimate the impact of Chapter 13 protection. Second, we drop office-by-year bins where a retiring judge's cases were reassigned with no documentation as to the original judge. Third, we restrict the sample to debtors who filed for Chapter 13 bankruptcy for the first time between June 2002 and December 2005 as defined using administrative bankruptcy records, ensuring that we obtain at least five years of post-filing outcomes and at least one year of pre-filing baseline outcomes for all debtors. This restriction also ensures that filings occurred before the 2005 Bankruptcy Reform Act came into effect. Finally, we drop office-by-year-by-judge bins with fewer than ten cases where we are unlikely to be able to accurately measure judge leniency. These sample restrictions leave us with 253,863 filings.

We match these 253,863 filings to credit bureau records from TransUnion using name and address at the time of filing. We were able to successfully match 68.9 percent of our estimation sample to the TransUnion data. Our match rate is similar to Finkelstein et al. (2012), who matched 68.5 percent of Oregon Medicaid applicants to TransUnion data using name, address, and date of birth. The probability of being matched to the credit report data is not significantly related to judge

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leniency (see Panel H of Table 1).

The TransUnion data are available from June 2002 to June 2010. We observe each individual in the credit bureau data annually in June. The TransUnion data are derived from public records, collections agencies, and trade lines data from lending institutions. The data also include geographic location at the ZIP code level and age. No other demographic information is available at the individual level. See Avery et al. (2003) and Finkelstein et al. (2012) for additional details on the TransUnion data.

Our estimation sample includes the 253,863 filings matched to at least one post-filing year of credit bureau data. This sample consists of 175,076 filers from 39 offices and 29 bankruptcy courts. The sample includes 348 office-by-year-by-judge observations – the level of variation that drives our empirical design. The number of cases in each office-by-year-by-judge bin ranges from 31 to 2,040, with a median of 799. Online Appendix Table 2 provides additional details on each of the offices in our estimation sample.

B. Measures of Financial Outcomes

We use the linked dataset to estimate the impact of Chapter 13 bankruptcy protection on financial strain, unsecured debt, asset holdings, and credit access.[14] This section briefly describes how we construct the measures used in our main analysis. The online data appendix provides additional details on all of the measures used in our analysis.

Financial strain is measured using a summary index of eight separate events that are meant to broadly capture financial distress associated with collections activity: delinquency, creditor charge-offs, collections, bankruptcy, foreclosure, civil judgments, liens, and repossessions within the last 12 months. Following Fryer and Katz (2013), we first standardize each component in the financial strain index for each post-filing year using the mean and standard deviation of the component for

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[14]Unfortunately, the data do not allow us to distinguish between accounts specifically included in bankruptcy versus those that are excluded. However, whether an account is dischargeable in bankruptcy may be endogenous to judge assignment. Our main results therefore analyze measures of debt and assets holdings across all accounts, which may capture a mechanical effect of bankruptcy protection during plan completion. However, results in years post-plan completion will not be affected by this issue.

the dismissed filer group in the baseline year. Next, we sum the eight components in each post-filing year, restandardizing using the mean and standard deviation of the index for the dismissed filer group in the baseline period. To exclude the mechanical effect of filing on charge-offs and new bankruptcies in the year of filing, the financial strain index in the year of filing is composed of the other six measures of adverse financial events. Finally, we average these annual index measures over the post-filing years. Because each of the financial strain components represent adverse events that negatively impact access to credit, a higher score on the index represents worse outcomes. See the data appendix for additional details on each component of the financial strain index.

Unsecured debt and collections activity are measured using the current balance of open revolving loans and the amount of debt currently in collections. Revolving loans includes all current bank cards, retail cards, and check credit accounts. Collections debt includes all loans that have been transferred to a collection agency or that are in the process of collection. Our collections measure does not include debts sent to collection agencies that do not report to credit bureaus and therefore represents a lower bound on total collections activity. Our unsecured debt data also do not include information on some non-bank and non-retail forms of unsecured credit, such as pawn and payday loans. Moreover, the TransUnion data do not include information on the cost of revolving debt. We are therefore unable to estimate the impact of Chapter 13 on these outcomes.

Retention of secured assets is measured using indicators for having an open mortgage loan within the past 12 months and having an open auto loan within the past six months, and the current balance of all open mortgages and open auto loans. All of the debt balance measures are captured in June of each year. Having an active mortgage or auto loan proxies for ownership of these assets but is an underestimate of actual ownership as some filers have likely fully paid off their mortgage or auto loans.

We measure credit access using the total utilization on revolving accounts and the number of non-mortgage inquiries in the last six months. TransUnion does not provide credit line information for each category of non-mortgage debt, so we proxy for credit supply using revolving trades, the largest category of non-mortgage credit among all credit users and our estimation sample. Revolv-

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ing trades include any bank card accounts, retail accounts, and check credit accounts. Results are qualitatively similar using bank card trades, a subset of revolving trades. Utilization is defined as the current balance divided by the credit limit, where TransUnion measures the credit limit using either the reported credit limit or the highest amount ever owed on an account if the credit limit is unreported. Avery et al. (2003) discuss this imputation procedure, concluding that the credit limit variable is likely a lower bound. Accordingly, utilization measures likely reflect an upper bound for accounts where the credit limit is imputed. Importantly, estimates using utilization may be biased if Chapter 13 protection impacts the highest amount ever owed on an account, as credit limits will appear higher for these individuals. Our utilization estimates should be interpreted with this potential measurement bias in mind. Our second measure of credit access is the number of non-mortgage inquiries. Inquiries are made to ensure that an applicant for credit, apartment rental, insurance, or employment meets minimum standards, and is considered a proxy for excess credit demand.

C. Descriptive Statistics

Table 1 presents summary statistics for our data. Column 1 reports summary statistics for a random sample of the population of credit users in the TransUnion database.[15] Column 2 reports summary statistics for individuals in this random sample that file for bankruptcy protection during our sample period. The TransUnion data does not report chapter of filing, so these individuals include a mix of Chapter 7, Chapter 11, Chapter 12, and Chapter 13 filers. Because very different types of individuals file under various bankruptcy chapters, bankruptcy filers in the national sample are likely to differ in substantial ways from Chapter 13 filers.[16] Columns 3 and 4 report summary statistics for Chapter 13 filers in our estimation sample assigned to judges with below and above

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[15]See Dobbie and Goldsmith-Pinkham (2014) for additional details on the credit user sample. The data contain approximately two percent of all credit users in the United States during this time period.

[16]The TransUnion data do not provide information on the date of bankruptcy filing or the chapter of bankruptcy, but each calendar year pull provides information on the number of bankruptcy filings in the last 12 months. From this bankruptcy filing flag, we define bankruptcy filers as those individuals who filed for bankruptcy for the first time in the last 12 months, based on credit report data between 2003 and 2006. Individuals whose bankruptcy flag is turned on in multiple years between 2003 and 2006 are excluded.

median judge leniency, as defined in Section III.

Bankruptcy filers are younger and more likely to own a home than the typical credit user in the United States. The typical bankruptcy filer in the national sample is 43.7 years old, compared to 48.5 years old for all credit users. Fifty-two percent of bankruptcy filers own a home. In comparison, 47.0 percent of all credit users own a home. In our estimation sample, 65.5 percent of Chapter 13 filers are homeowners and the average age is 44.8 years old. Bankruptcy filers are also disproportionately from lower income ZIP codes. The typical bankruptcy filer in the national sample resides in a ZIP code with an average annual income of $39,907 compared to $46,192 for the typical credit user. In our estimation sample, bankruptcy filers reside in ZIP codes with an average annual income of $38,143.

Perhaps not surprisingly, bankruptcy filers are far more likely to suffer an adverse financial event than the typical credit user even before filing. In the national sample, 41.3 percent of bankruptcy filers have at least one delinquency before filing, 29.6 percent have at least one debt in collections, 18.8 percent have at least one creditor charge-off, 3.4 percent have at least one civil judgment, 1.0 percent have experienced a foreclosure, 1.1 percent have at least one property lien, and 1.2 percent have at least one repossession. Chapter 13 filers in our estimation sample are even more likely to have suffered an adverse financial event before filing compared to the typical credit user, with 67.7 percent having had a delinquency in the past 12 months, 46.3 percent having a debt in collections, 30.9 percent having a charge-off, 6.3 percent having a judgment, 5.1 percent having a foreclosure, 2.1 percent having a lien, and 2.1 percent having a repossession. In comparison, only 14.8 percent of all credit users have a delinquency in the past 12 months, 13.7 percent have a debt in collections, 6.5 percent have a charge-off, 0.9 percent have a judgment, 0.3 percent have a foreclosure, 0.4 percent have a lien, and 0.3 percent have a repossession.

Bankruptcy filers also have significantly higher unsecured debt and collections activity compared to the typical credit user. Bankruptcy filers in the national sample have $13,083 in revolving debt and $1,432 of debt in collections. Chapter 13 filers in our estimation sample have $10,460 in revolving debt and $2,460 of debt in collections. In comparison, the typical credit user has $6,011

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in revolving debt and $601 of debt in collections.

Bankruptcy filers are more likely to have an open mortgage than the typical credit user. In the national sample, 43.4 percent of bankruptcy filers have at least one open mortgage, compared to 36.7 percent for all credit users. In our estimation sample, 57.9 percent of Chapter 13 filers have at least one open mortgage. Note that active mortgage rates are generally lower than homeownership rates in both the national sample and estimation sample, suggesting that approximately seven to ten percent of homeowners have already paid off their mortgages.

While bankruptcy filers in the national sample are more likely to have a mortgage, they have mortgage balances that are $2,612 lower than the typical credit user, while Chapter 13 filers in our estimation sample have mortgage balances that are $12,615 more than the typical credit user. Home mortgage balances are likely higher among Chapter 13 filers than bankruptcy filers in the national sample because national bankruptcy filers comprise those who file under Chapter 7 as well as Chapter 13, and Chapter 7 filers are less likely to be homeowners.

Bankruptcy filers are also 17.1 percent more likely to have an open auto loan compared to the typical credit user, with Chapter 13 filers in our estimation sample 19.1 percent more likely to have an active auto loan than the typical credit user. Accordingly, bankruptcy filers in the national sample have auto balances that are $3,412 more than the typical credit user. Chapter 13 filers in our estimation sample have auto balances $3,892 more than the typical credit user.

Bankruptcy filers in the national sample have higher utilization on revolving accounts and more credit inquiries than the typical credit user, suggesting that bankruptcy filers have excess credit demand conditional on credit supply. Specifically, bankruptcy filers in the national sample have utilization rates that are 35.9 percentage points higher than the average credit user and also have 1.0 more non-mortgage inquiries in the last six months than the typical credit user. In our estimation sample, Chapter 13 filers have 45.4 percentage points higher utilization on revolving accounts than the typical credit user and 1.5 more non-mortgage inquiries.

We also find that bankruptcy filers have lower credit scores than the typical credit user in the United States. Average pre-filing credit scores are 630.1 for bankruptcy filers in the national

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sample. In comparison, average credit scores are 739.5 for all credit users. In our estimation
sample, the average credit score is 580.7.

III. Research Design

Overview: Consider a model that relates post-filing outcomes such as credit score to the receipt of
Chapter 13 bankruptcy protection:

$$y_{it} = \alpha + \beta \mathbf{X}_i + \gamma Bankruptcy_i + \varepsilon_{it} \tag{1}$$

where i denotes individuals, t is the year of observation, γ is the causal impact of bankruptcy pro-
tection, $\mathbf{X}_i$ includes controls such as age and lagged outcomes, and ε_{it} is noise. Our key empirical
problem is that OLS estimates of Equation (1) may be biased if bankruptcy protection is correlated
with the unobservable determinants of later outcomes, explored later in this section.

We estimate the impact of Chapter 13 protection on debtors using judge leniency as an instru-
ment for bankruptcy protection. Our empirical strategy exploits the fact that judges are randomly
assigned to filings and that those bankruptcy judges have differing tendencies to grant Chapter 13
protection. In this specification, we interpret any difference in post-filing outcomes as the causal
effect of the change in the probability of receiving bankruptcy protection operating through judge
assignment. The second stage estimating equation is:

$$y_{it} = \alpha + \alpha_{ot} + \beta \mathbf{X}_i + \gamma Bankruptcy_i + \varepsilon_{it} \tag{2}$$

where α_{ot} are office-by-filing-month fixed effects and $\mathbf{X}_i$ includes baseline age bins, homeowner-
ship, financial strain, revolving, mortgage, auto, and collections debt, indicators for open mortgage
and open auto loans, revolving utilization, non-mortgage inquiries, and ZIP code income. $\mathbf{X}_i$ also
includes indicators for missing age and baseline characteristics. The baseline controls included in
$\mathbf{X}_i$ are orthogonal to judge leniency due to the random assignment of judges discussed above. We
verify this fact in Table 1. In large samples, the inclusion of baseline controls should therefore

have little impact on estimates of γ. In small samples, however, including baseline controls may increase precision. We report estimates both with and without baseline controls throughout the paper.

The corresponding first stage estimating equation associated with Equation (2) is:

$$Bankruptcy_{it} = \alpha + \alpha_{ot} + \beta \mathbf{X}_i + \delta \sigma_j + \varepsilon_{it} \tag{3}$$

where σ_j is the systematic component of judge behavior and δ represents the impact of judge behavior on the probability of receiving bankruptcy protection. We cluster standard errors at the office level in both the first and second stage regressions to account for any serial correlation across filers at the level of randomization. Results are qualitatively similar if we cluster at the office-by-judge or office-by-filing-month level.

Following the previous literature (e.g. Kling 2006, Chang and Schoar 2008, Doyle 2007, 2008, Autor and Houseman 2010, French and Song 2014, Aizer and Doyle 2015, Maestas, Mullen, and Strand 2013, and Dobbie and Song 2015), we predict the systematic component of judge behavior σ_j with judge leniency Z_{ioj}, defined as the leave-one-out fraction of filings granted by judge j in office o minus the leave-one-out fraction granted in office o:

$$Z_{ioj} = \frac{1}{n_{oj}-1} \left(\sum_{k=1}^{n_{oj}} (B_k) - B_i \right) - \frac{1}{n_o-1} \left(\sum_{k=1}^{n_o} (B_k) - B_i \right) \tag{4}$$

where i again denotes individuals, o denotes offices, j is the assigned judge, B_i is an indicator for receiving bankruptcy protection, n_{oj} is the number of cases seen by a judge in office o, and n_o is the number of cases seen by an office. We calculate judge leniency across all years and using all filings in the full sample of filings, including those not matched to TransUnion credit records. Results are similar if we allow judge leniency to vary across years or if we calculate judge leniency only within filings matched to the TransUnion records. Note that Z_{ioj} is defined as a leave-one-out measure, specifically omitting the bankruptcy result for the i-th individual when estimating the average leniency of the judge assigned to individual i. As a result, there is no mechanically

induced dependence between the outcome for individual i and our judge leniency measure. This method is analogous to the solution proposed in Angrist, Imbens, and Krueger (1999) and Kolesar (2013). In Section IV.E, we explore whether estimation error in both our judge leniency measure and financial health outcomes biases the standard errors in our main specifications.

Our preferred measure of judge leniency uses the final decision on each bankruptcy filing, not whether a plan is initially confirmed or dismissed. We focus on this measure of judge leniency for two reasons. First, the resulting two-stage least squares estimates can be interpreted as the causal effect of receiving bankruptcy protection, which has clearer policy implications than plan confirmation. Second, we do not observe the reason for case dismissal in our data and are therefore unable to measure plan confirmation directly. In Section IV.E, we present estimates that use judge leniency measured over the first 90 days, a proxy for plan confirmation. These results are nearly identical to our preferred estimates discussed below. See Section IV.E for additional details on this alternative measure of judge leniency and other robustness checks.

Judge Variation: Consistent with Dobbie and Song (2015), we find considerable variation in the treatment of Chapter 13 cases within an office. The standard deviation of Z_{ioj} is 0.024 for Chapter 13 filers in our sample. There is also significant persistence in our measure of judge behavior. Online Appendix Figure 1 plots current and lagged judge discharge rates, with each point representing a separate judge-by-office-by-year observation. Discharge rates are highly correlated across time, with an OLS regression relating each judge-by-office-by-year discharge rate to the lagged discharge rate yielding a coefficient of 0.902. These results suggest that we are capturing systematic differences in judge behavior, not random year-to-year noise.

Using our measure of judge leniency Z_{ioj} as an instrument for the receipt of Chapter 13 bankruptcy protection, two-stage least squares estimates from Equation (2) measure the local average treatment effect of Chapter 13 protection for filers whose bankruptcy outcomes are altered by judge assignment. Thus, the identified parameter measures the treatment effect for filers whose bankruptcy decision is altered by the judge assignment due to disagreement on whether or not they should receive bankruptcy protection (i.e. the marginal recipients of bankruptcy protection). As

is always the case with local average treatment effects, the estimated parameter may not be relevant to substantially different populations, such as individuals who are not in financial distress or individuals unable or uninterested in filing for bankruptcy protection.

Three conditions must hold to interpret our two-stage least squares estimates as the local average causal impact of bankruptcy protection: (1) judge assignment is associated with bankruptcy protection, (2) judge assignment only impacts debtor outcomes through the probability of receiving bankruptcy protection, and (3) the impact of judge assignment on the probability of receiving bankruptcy protection is monotonic across filers. Online Appendix Figure 2 tests the first assumption by plotting average discharge against our leave-one-out measure of judge leniency. The estimation sample includes first-time filers between 2002 and 2005 in the 39 offices in the 29 courts that randomly assign Chapter 13 filings to judges. Online Appendix Figure 2 is constructed by calculating the mean residuals from a regression of an indicator for receiving Chapter 13 protection on office-by-filing-month fixed effects. For ease of interpretation, we add the mean discharge rate to the mean residual in each judge-by-year bin. The plotted line and corresponding coefficient show the best linear fit estimated on the underlying individual-level data, controlling for office-by-filing-month fixed effects and with standard errors clustered at the office level. Table 2 presents analogous individual-level estimates with and without controls.

Instrument Validity: Online Appendix Figure 2 and Table 2 indicate that judge leniency is highly predictive of the probability of receiving bankruptcy protection. With no controls, a one percentage point increase in Z_{ioj} increases the probability that a debtor receives bankruptcy protection by 0.889 percentage points. Controlling for all baseline characteristics in column 6, our measure of judge leniency remains highly predictive of the probability of receiving bankruptcy protection, with a one percentage point increase in Z_{ioj} increasing the probability that a debtor receives bankruptcy protection by 0.819 percentage points. Thus, a one standard deviation (2.4 percentage point) increase in judge leniency increases the likelihood of receiving bankruptcy protection by about 2.0 percentage points, corresponding to a 4.5 percent change from the mean discharge rate of 44.6 percent.

Consistent with the first stage results in Dobbie and Song (2015), the probability of receiving Chapter 13 protection does not increase one-for-one with our measure of judge leniency, likely because of measurement error that attenuates the effect toward zero. For instance, the accuracy of our leave-one-out measure will be reduced if judge leniency drifts over the course of the year or fluctuates with case characteristics. Nevertheless, our first stage results confirm that our measure of judge leniency is highly predictive of case outcomes.

The coefficients on our baseline controls are of independent interest for understanding the types of individuals more or less likely to receive Chapter 13 protection. The probability of receiving bankruptcy protection is increasing in filer age and ZIP code income. Homeowners are also more likely to receive Chapter 13 protection than non-homeowners. The probability of receiving Chapter 13 protection is decreasing in financial strain and the amount of debt in collections. The probability of receiving bankruptcy protection is also decreasing in mortgage and auto debt, although individuals with open mortgage and auto loans are more likely to receive Chapter 13 protection. Filers with higher unsecured debt are more likely to receive bankruptcy protection. Finally, the probability of receiving Chapter 13 is decreasing with the number of credit inquiries in the last six months.

Following Doyle (2008), we also present first stage results for different subsamples to shed light on the characteristics of filers who are more likely to be affected by judge assignment. In the case of a binary instrument, the relative likelihood that a complier has a given characteristic is equal to the first stage coefficient for that group divided by the first stage coefficient for the full sample. Similar logic applies to the case of a continuous instrument. Given that filers likely differ in how much they benefit from Chapter 13 protection, these results provide new evidence on the types of cases for which the instrumental variables estimates are most likely to apply; that is, those filers for whom judges most disagree on whether to grant bankruptcy protection. These results also provide insight into the likely impacts of debt-relief policy interventions that target different types of debtors.

Online Appendix Table 3 shows the first stage estimates for subgroups of interest. We also

present the ratio of the subgroup first stage coefficient to the overall first stage coefficient from column 6 of Table 2. The most striking first stage result is for filers 25 to 39 years old at the time of filing, who have a first stage coefficient that is 15.4 percent higher than the overall first stage, although the ratio is not significantly different from one due to variability in the data. Conversely, filers who are 60 and up have a first stage coefficient that is only 49.2 percent of the overall first stage, significantly different from one. First stage results are not substantially different between filers with below and above median baseline credit scores and by baseline homeownership status. These results suggest that young filers are most likely to be affected by a lenient judge assignment, indicating greater judicial disagreement over these types of filers.[17]

Our second identifying assumption is that judge assignment only impacts debtor outcomes through the probability of receiving bankruptcy protection. This assumption would be violated if judge leniency is correlated with unobservable determinants of future outcomes. We partially test this assumption by assessing whether observable filer characteristics differ based on whether filers are assigned to a judge with either a high or low propensity to grant Chapter 13 protection. Following Aizer and Doyle (2015), columns 3 and 4 of Table 1 present summary statistics separately for filers assigned to judges with above and below median leniency. Column 5 reports results from a series of OLS regressions of each observable filer characteristic on an indicator for being assigned to a judge with above median leniency and office-by-filing-month fixed effects with standard errors clustered by office. Consistent with our identifying assumptions, there is only one statistically significant difference in the 24 variables we consider.

As discussed in Section I.B, the exclusion restriction would also be violated if judge leniency is correlated with repayment plan terms. However, we find no evidence that initial plan terms or pre- and post-modification are statistically different across judges (see Online Appendix Table 1), further suggesting that the exclusion restriction is likely valid in our setting.

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[17]In unreported results, we also characterize the number of compliers and their characteristics following the approach developed by Abadie (2003) and extended by Dahl et al. (2014). The results under this approach are qualitatively similar to the results following Doyle (2008). Under our linear specification, we find that 12.9 percent of our sample are compliers, 49.8 percent are always takers, and 38.3 percent are never takers. Compliers are younger at filing compared to the average filer. Compliers are not systematically different from the average filer by baseline financial strain, homeownership status, or credit score, however.

Our third identifying assumption is that there is a monotonic impact of judge assignment on the probability of receiving bankruptcy protection. The monotonicity assumption implies that being assigned to a more (less) lenient judge does not decrease (increase) the likelihood of receiving Chapter 13 protection. Following Dobbie and Song (2015), we partially test the validity of the monotonicity assumption by examining how judges treat filings from observably different filers. Any significant differences in the way that judges treat these filings would suggest that the monotonicity assumption is violated. Online Appendix Figure 3 plots judge leniency measures calculated separately by age at filing, baseline credit score, baseline homeownership, and baseline financial strain. Each plot reports the coefficient and standard error from an OLS regression relating each measure of judge leniency. Consistent with our monotonicity assumption, we find that judge tendencies are very similar across observably different filers.

In unreported results, we also examine whether measures of judge leniency for subcategories of individuals, such as young versus old filers, are additionally predictive beyond the average leniency for a judge. If the monotonicity assumption holds, these subgroup specific measures of judicial leniency should not be predictive of case outcomes after we condition on average judge leniency. Consistent with our monotonicity assumption, we find that only the average measure of judge leniency is a statistically significant predictor of case outcomes. None of the subgroup specific measures of judicial leniency are statistically significant, and a joint test of significance yields a p-value of 0.776. Using principal component analysis, we also find no evidence of clustering in each judge's subgroup specific leniency measures. These results further suggest that judicial leniency does not vary across different types of filers. None of our results suggest that the monotonicity assumption is likely to be invalid in our setting.

Understanding our LATE: Our two-stage least squares estimates represent the LATE for filers whose bankruptcy outcomes are altered by judge assignment, i.e., marginal recipients of bankruptcy protection. In particular, we estimate the causal impact of receiving Chapter 13 bankruptcy protection on the marginal recipient's financial health. Our results complement earlier work by Dobbie and Song (2015) showing that Chapter 13 protection increases earnings and reduces mortality risk

among marginal recipients. In the context of this prior work, our causal estimates should be interpreted as capturing the reduced form effect of obtaining bankruptcy protection on financial health, both through the direct channel of bankruptcy as well as any indirect effects such as increased earnings. For example, bankruptcy protection may impact financial health directly by discharging the majority of a filer's unsecured debt, and indirectly by increasing a filer's earnings net of creditor garnishment. While we cannot specifically disentangle the direct and indirect effects of bankruptcy protection on financial health, our reduced form causal estimates are relevant to any policy reforms that alter the costs of filing for bankruptcy at the margin.

IV. Results

We begin by exploring the impact of Chapter 13 protection on financial strain associated with debt collection, the amount of unsecured debt, and retention of assets. We then estimate the indirect effects of Chapter 13 protection on credit access. We conclude by examining the most likely mechanisms driving our results and testing the robustness of our empirical design.

A. Financial Strain

Panel A of Table 3 reports two-stage least squares estimates for our financial strain index that combines eight adverse financial events as described above. Our estimation sample consists of Chapter 13 bankruptcy filers originating from offices that randomly assign filers to judges between 2002 and 2005 that are linked to the credit report data in the year of filing. In columns 1 through 3, we present results in the first five post-filing years. Note that discharged filers in our estimation sample took approximately 3.7 years to complete their repayment plans. As a result, our main findings using the year of filing to the fifth post-filing year capture both mechanical effects of filing for Chapter 13, such as a stop on collections activity, as well as non-mechanical effects after plan completion. Moreover, even during the repayment plan period, our results will capture non-mechanical effects of receiving bankruptcy protection to the extent that marginal dismissed filers are unable to immediately re-file for bankruptcy. Furthermore, in columns 4 through 6, we

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examine the persistence of our findings using outcomes for an unbalanced panel of filers in the sixth through eighth post-filing years. These results allow us to identify whether the marginal recipients of bankruptcy protection are prone to recidivism or whether the benefits of bankruptcy protection generate long-term relief.

Columns 1 and 4 report the mean financial strain index for dismissed filers. Columns 2 and 5 present two-stage least squares estimates using our leave-one-out measure of judge leniency controlling only for office-by-filing-month fixed effects. Columns 3 and 6 add controls for baseline age bins, homeownership, financial strain, revolving, mortgage, auto, and collections debt, indicators for open mortgage and open auto loans, revolving utilization, non-mortgage inquiries, and ZIP code income. We report standard errors clustered at the office level throughout. Results for each individual component of the financial strain index and details on these measures can be found in the data appendix.

We find that Chapter 13 significantly improves financial well-being by reducing the likelihood of adverse financial events and creditor actions. Over the first five post-filing years, Chapter 13 protection decreases the marginal recipient's level of financial strain by 0.323 to 0.369 standard deviations. In Online Appendix Table 4, we show that Chapter 13 protection decreases the marginal recipient's annual probability of having a debt in collections by 15.4 percentage points, a 26.4 percent decrease from the dismissed filer mean of 58.4 percent. Credit charge-offs decrease by 6.8 percentage points, a 31.5 percent decrease from the dismissed filer mean of 21.6 percent. Subsequent bankruptcy filings (of all chapters) decrease by 6.5 percentage points, a 59.6 percent decrease from the dismissed filer mean of 10.9 percent. Foreclosures decrease by 1.6 percentage points, a 22.9 percent decrease from the dismissed filer mean of 7.0 percent. Creditor judgments decrease by 3.1 percentage points, a 47.0 percent decrease from the dismissed filer mean of 6.6 percent. Liens decrease by 3.3 percentage points, a 97.1 percent decrease from the dismissed filer mean of 3.4 percent. Repossessions decrease by 1.6 percentage points, a 84.2 percent decrease from the dismissed filer mean of 1.9 percent. Conversely, there is no impact of Chapter 13 protection on delinquency, defined as the probability of any trade being at least thirty days past due. The

effects of bankruptcy protection on the marginal recipient's financial strain also persist long after plan completion. In the sixth through eighth post-filing years, Chapter 13 protection reduces the marginal recipient's financial strain by 0.137 to 0.216 standard deviations.[18]

Table 4 presents two-stage least squares results from our preferred specification separately by age, baseline credit score, and baseline homeownership. Chapter 13 reduces financial strain by 0.415 standard deviations for homeowners compared to just 0.188 standard deviations for non-homeowners, though the difference is not statistically significant. There are no economically or statistically significant differences by age or baseline credit score.

Consistent with Dobbie and Song (2015), we find that our results are driven by a deterioration of outcomes among dismissed filers rather than gains among granted filers (see Online Appendix Figure 4). Taking repossession as an illustrative example, we see that both dismissed and granted filers are more likely to experience a repossession than non-filers even before filing. In the four years before filing, repossession rates average 0.7 percent and 1.4 percent among granted and dismissed filers, respectively. Repossession rates increase to 1.1 percent for granted filers and 1.6 for dismissed filers in the year before filing, before peaking at 2.4 and 3.4 percent, respectively, in the year of filing. Repossession rates for dismissed filers remain elevated at approximately 1.4 percent throughout our sample period, while the rates for granted filers fall to about 0.5 percent, comparable to non-filers. These results are consistent with bankruptcy protection mitigating the long-term consequences of financial shocks that might otherwise harm debtors, but not conferring benefits in the absence of a financial shock.

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[18]Online Appendix Table 4 also presents results for each component of the financial strain index controlling for the Family-Wise Error Rate, or the probability of making one or more false discoveries when performing multiple hypothesis tests, using a step-down algorithm similar to those described by Romano and Wolf (2005) and Romano, Shaikh, and Wolf (2008). For a given family of k-hypothesis tests, the algorithm uses resampling and step-down methods to estimate the dependence structure of the test statistics and provides corrected p-values. We also present results from the more conservative Holm step-down method described in Romano, Shaikh and Wolf (2010), which controls the Family-Wise Error Rate without taking into account the dependence structure of the test statistics. The corrected p-values yield almost identical results. Online Appendix Tables 5 and 6 present additional financial strain results. Online Appendix Table 5 reports results using the cumulative probability of an event occurring at least once in the first five post-filing years for each of the eight adverse financial events and the total number of adverse events in the first five post-filing years. In Online Appendix Table 6, we find that Chapter 13 protection decreases both the number of paid and unpaid collections but has no impact on the number of medical collections. The decrease in judgments is due to a decrease in unpaid judgments.

We conclude this section by comparing the magnitude of our two-stage least squares estimates to the deterioration of outcomes for dismissed filers. This calculation provides a back-of-the-envelope approximation of the extent to which Chapter 13 protection mitigates the adverse consequences of financial distress. Specifically, we calculate the change in outcomes for dismissed filers by subtracting the average post-filing outcomes for the first five post-filing years from the pre-filing outcomes in the third to fourth years pre-filing. This calculation implies that in the first five post-filing years, Chapter 13 protection mitigates approximately 93.1 percent of the deterioration in the financial strain index. For the individual components of the financial strain index, Chapter 13 protection mitigates approximately 28.6 percent of the deterioration in the probability of having a foreclosure and approximately 100 percent of the potential deterioration in the probability of having collections debt, a charge-off, a new bankruptcy, a judgment, a lien, and a repossession.

B. Unsecured Debt and Collections

Panel B of Table 3 reports two-stage least squares estimates on open unsecured debt and collections debt. Our estimates measure the impact of bankruptcy protection on the marginal recipient's debt, conditional on filing for bankruptcy. Each dependent variable is the average amount of debt reported in each category. We report results using the year of filing to the fifth post-filing year in columns 1 through 3, and results from the sixth to eighth post-filing year in columns 4 through 6.

There is little impact of Chapter 13 protection on open unsecured debt. Point estimates are small and not statistically different from zero. However, in the first five post-filing years, the marginal recipient of Chapter 13 has $1,333 to $1,842 less debt in collections, a 31.6 to 43.6 percent decrease from the dismissed filer mean of $4,217. These results extend after plan completion, with the marginal recipient of Chapter 13 having $1,986 to $2,534 less debt in collections by the sixth through eighth post-filing years, a 42.6 to 54.3 percent decrease from the dismissed filer mean of $4,666. The impact of Chapter 13 on collections debt is higher for filers with high baseline credit scores but does not vary by age or homeownership.[19]

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[19]Panel B of Online Appendix Table 6 presents results for student debt, a form of unsecured debt that is not discharged under Chapter 13 protection. We find no impact of Chapter 13 on active or deferred student debt.

Comparison of means shows that open unsecured debt falls for both granted and dismissed filers post-bankruptcy. Collections debt increases for both groups before filing but falls to pre-filing levels for granted filers only in the post-filing years. Taken together with our above results, these trends suggest that the marginal recipient of Chapter 13 protection reduces his or her unsecured debt through the bankruptcy system, while the marginal non-recipient is unable to prevent his or her unsecured debts from being sold to a third-party debt collector. Using these trend results, we estimate that Chapter 13 protection mitigates approximately 64.6 percent of the potential increase in collections debt.[20]

C. Secured Assets

Panel C of Table 3 reports two-stage least squares estimates for the probability of having an open mortgage, the average amount of mortgage debt, the probability of having an open auto loan, and the amount of auto debt.

In the first five post-filing years, we find that Chapter 13 protection significantly increases the probability of having a mortgage by 11.0 to 13.2 percentage points, a 30.3 to 36.4 percent increase from the dismissed filer mean of 36.3 percent. Chapter 13 protection also increases the marginal recipient's mortgage debt by $12,196 to $14,535, a 45.4 to 54.1 percent increase from the dismissed filer mean of $26,833. Unsurprisingly, the impact of Chapter 13 on both homeownership and mortgage debt is higher for baseline homeowners. Effects are also larger for filers who are 60 or older at the time of filing.

The increase in mortgage debt may be the result of dismissed filers decreasing their mortgage debt by downsizing or becoming renters, or by granted filers increasing their mortgage debt by buying a new home. Panel C of Online Appendix Table 6 sheds light on this issue by estimating the impact of Chapter 13 protection on the probability of living in the same residence, the probability of moving to a rental, and the probability of moving to a home. Each dependent variable is measured

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[20]Our estimates of the effect of bankruptcy discharge on unsecured debt capture the overall change in all debts, including those accumulated prior to bankruptcy. According to TransUnion, unsecured debt remains on all accounts even after filing, and is only reported as zero after a discharge is granted. Hence, the change in unsecured debt is due to the receipt of debt discharge after plan completion and not a change in measurement due to filing itself.

in the fifth post-filing year, with identical results for earlier years. Chapter 13 protection increases the probability of staying in the same residence by 24.8 to 27.0 percentage points and decreases the probability of moving to a rental by 24.8 to 26.7 percentage points. These results are consistent with Chapter 13 decreasing the probability that filers sell or lose their homes. In fact, by six to eight years post-filing, Chapter 13 protection significantly increases the probability of having a mortgage by 25.9 to 26.2 percentage points, an over 100 percent increase from the dismissed filer mean of 18.4 percent, and increases the amount of mortgage debt by \$34,967 to \$37,300.

Comparison of means provides additional evidence on this issue. The probability of having a mortgage increases in the years before filing for both granted and dismissed filers, peaking the year before filing at 59.8 percent and 56.2 percent, respectively. For granted filers, the probability of having a mortgage falls modestly after filing to around 45 percent. For dismissed filers, the probability of having a mortgage falls all the way to 21.8 percent by the fifth year after filing. These trend results suggest that Chapter 13 protection alleviates more than 100 percent of the potential fall in homeownership rates.

We also explore the impact of Chapter 13 protection on whether a debtor has an open auto loan and average auto debt. Because at least some car owners do not have an open car loan, our measures of car ownership are lower bounds. Chapter 13 protection does not have a statistically significant effect on the probability of having a car loan or on average auto debt in the first five post-filing years. In Online Appendix Figure 4, we find that the probability of having a car loan falls in the year of filing for both granted and dismissed filers, suggesting that most debtors give up their cars in the short-run. These results suggest that the retention of a home may be a more important priority for Chapter 13 filers. This interpretation is consistent with survey results showing that over 70 percent of dismissed filers choose to file under Chapter 13 bankruptcy to avoid foreclosure (Porter 2011). However, by six to eight years post-filing, the marginal recipient of bankruptcy protection is 12.7 to 12.8 percentage points more likely to have a car loan, a 67.4 to 66.8 percent increase from the dismissed filer mean, suggesting that dismissed filers are more likely to lose their cars several years after filing.

D. Credit Access

Panel D of Table 3 reports two-stage least squares estimates for the total utilization on revolving accounts and the number of non-mortgage inquiries. We report results using the year of filing to the fifth post-filing year in columns 1 through 3 and results from the sixth to eighth post-filing year in columns 4 through 6. As discussed previously, while these outcomes have a less clear economic interpretation than our other outcomes, they are still suggestive of potential economic benefits to bankruptcy protection.

We find that Chapter 13 protection decreases revolving credit utilization, as measured by the balance to credit limit ratio. These revolving trades include bank credit cards, retail credit cards, and check credit accounts. Revolving accounts are the most common type of credit accounts, representing 63 percent of all credit accounts and about 71 percent of all open accounts (Avery et al. 2003). To the extent that credit cards are uncommon among bankruptcy filers, revolving utilization may not be the best proxy of credit access and should be interpreted with this caveat in mind. Indeed, 37.6 percent of filers in our estimation sample have zero revolving trades in the baseline year. Nevertheless, credit limits are high among the other 62.4 percent of filers with revolving trades. Bankruptcy filers in our estimation sample have an average credit limit across all revolving trades of $17,842.

In the first five years post-filing, utilization decreases by 15.1 to 16.3 percentage points, a 32.3 to 34.9 percent decrease from the dismissed filer mean of 46.7 percent. Chapter 13 protection also decreases the number of non-mortgage inquiries in the past six months by 0.300 to 0.410, a 18.9 to 25.9 percent decrease from the dismissed filer mean of 1.584. The impact of Chapter 13 on credit utilization is larger among younger filers, and the impact on non-mortgage inquiries is larger among baseline homeowners. Overall, these results suggest that Chapter 13 protection increases credit access.[21] Differences in utilization and non-mortgage inquiries between marginal recipients and non-recipients are no longer significant in the sixth through eighth post-filing years.

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[21] The increase in credit access is most likely the result of improved financial health documented above, as opposed to any limits on future bankruptcy filing. While discharged Chapter 13 filers have to wait six years before filing under Chapter 7, filers are still in the process of making plan payments during three to five years post-filing.

A comparison of means shows that utilization rates and non-mortgage inquiries fall (i.e. improves) for both granted and dismissed filers after filing, with larger falls for granted filers. For dismissed filers, average revolving utilization decreases by 19.3 percent and average non-mortgage inquiries decrease by 0.8 from the pre- to post-filing periods. These results are consistent with the pre-filing levels of credit usage being unsustainable for all filers. This suggests that our two-stage least squares estimates may be more correctly interpreted as Chapter 13 protection decreasing unmet credit demand, rather than increasing credit supply. Following our earlier back-of-the-envelope calculations, our estimates suggest that Chapter 13 protection further augments the fall in utilization by an additional 78.9 percent and further augments the fall in the number of inquiries by an additional 37.4 percent.

Panel E of Online Appendix Table 6 reports results for credit score, an aggregate measure of credit risk that is likely to mediate our credit access results discussed above. The two-stage least squares results with no controls suggest that Chapter 13 protection increases the marginal recipient's post-filing credit score by 28.5 points, a 5.0 percent increase from the dismissed filer mean of 565.4 points. With controls, the estimated impact is 17.0 points, a 3.0 percent increase from the dismissed filer mean. Improvements in credit score persist in the sixth to eighth post-filing years. In unreported results, we find that the effect of Chapter 13 bankruptcy protection on post-filing credit score becomes small and statistically insignificant after controlling for post-filing financial strain, unsecured debt and collections, secured assets, and credit access, suggesting that the indirect effect of Chapter 13 protection on credit score is largely the result of its impact on these other financial outcomes.

E. Additional Robustness Tests

Online Appendix Table 7 explores the robustness of our main results to alternative measures of judge leniency. Column 1 replicates our preferred estimates from column 3 of Table 3 using the leave-one-out measure of judge leniency as an instrument for Chapter 13 protection. Column 2 uses a leave-month-out version of judge leniency as an instrument for Chapter 13 protection that

purges any remaining correlation between a filer's outcomes and our instrument introduced by the estimation of the office-by-filing-month fixed effects in our first and second stage regressions. Column 3 uses a leave-one-out measure of judge leniency calculated using decisions after only 90 days to proxy for the initial judicial decision to confirm or dismiss a filing.[22] Another potential concern is that by constructing our judge instrument and outcome measures using the same sample, we may mechanically induce dependence across observations. To eliminate this induced correlation, column 4 uses a randomly selected subset of 25 percent of filers to calculate a leave-month-out measure of judge leniency that is used as an instrument in the mutually exclusive subset of filers. We also test the robustness of our results using judge fixed effects directly rather than our reduced form measure of judge leniency. Columns 5 through 7 present results that use judge fixed effects as instruments for bankruptcy protection estimated using two-stage least squares, LIML, and jackknife IV, respectively. Results across all specifications are nearly identical to our preferred specifications. None of the estimates suggest that our preferred estimates are invalid.

In Online Appendix Table 8, we explore whether estimation error in both our judge leniency measure and financial strain index biases the standard errors in our main specifications. To account for this estimation error in our judge leniency measure and financial strain index, we cluster bootstrap our specifications following Cameron, Gelbach, and Miller (2008). This procedure involves sampling at the office level, with replacement, and then generating the judge leniency and financial strain measures within this sampled data. We then run our two-stage least squares regressions within the sample data, extracting the parameter values to generate a distribution of t-statistics values to calculate our standard errors. We report results from this bootstrap-t procedure with 1,000 simulations for our main results from Table 3, reporting for each result whether we reject the null. Results are very similar with our estimation error corrected p-values and have little impact on the statistical significance of our results.

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[22]We calculate judge leniency using decisions after 90 days because the bulk of dismissals occur within 90 days of filing. Under the Bankruptcy Code, the Chapter 13 trustee must hold a meeting of creditors between 21 and 50 days after the debtor files for bankruptcy. A judge is required to hold a confirmation hearing for the proposed repayment plan no later than 45 days after the meeting of the creditors. Our results are qualitatively similar using decisions after 60 and 120 days post-filing.

V. Potential Mechanisms

In this section, we explore two potential mechanisms that might explain our findings: (1) protection from debt collectors and (2) debt forgiveness.[23]

We test the importance of the Chapter 13 provision that puts a hold on debt collection efforts using across-state variation in state garnishment laws. In the four states that do not allow wage garnishment – Florida, Pennsylvania, South Carolina, and Texas – creditors have fewer options to collect unpaid debts from dismissed filers. Treatment effects in states that allow wage garnishment include the effect of the hold on debt collection, debt forgiveness, and asset retention. Treatment effects in states that do not allow wage garnishment only include the effect of debt forgiveness and asset retention. If the two sets of estimates are different, this implies that the hold on debt collection is empirically important.[24] Table 5 presents two-stage least squares results for filers in states that do and do not allow wage garnishment. Consistent with our hypothesis, we find large and statistically significant effects of Chapter 13 protection in states that allow wage garnishment and small and imprecisely estimated effects in the four states that prohibit wage garnishment. In particular, we find suggestive evidence that the impact of bankruptcy protection on financial strain associated with debt collections activity is larger in states that allow wage garnishment. However, only one of the eight differences is statistically significant due to the imprecision of the point estimates in states that do not allow wage garnishment. These results are therefore consistent with there being significant costs of debt collection that may help explain the deterioration of outcomes

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[23]The retention of assets is a third potential mechanism that we are unable to fully test. One partial test of this hypothesis is to compare treatment effects for baseline homeowners to baseline renters. In Table 4, we find positive benefits of Chapter 13 protection for both homeowners and non-homeowners, but results are somewhat larger for homeowners. These results suggest that retention of important assets, such as a home, is a modest but important mechanism explaining our results. An alternative test of this mechanism is to compare treatment effects for homeowners and non-homeowners in states with and without judicial foreclosure. In states without judicial foreclosure, creditors can initiate foreclosure proceedings more easily. It is plausible that Chapter 13 protection may have a larger impact for homeowners in these states if the retention of the home is an important driver of our results. Online Appendix Table 9 presents these results. Consistent with the results discussed above, the point estimates suggest that the retention of assets is a somewhat important driver of our results. However, the imprecision of the estimates makes definitive conclusions difficult.

[24]It is plausible that the decision to file for Chapter 13 is influenced by state wage garnishment laws. If the effects of Chapter 13 protection are different for these filers, our estimates will also incorporate this heterogeneity of treatment effects. The same logic applies to our Chapter 7 exemption results in Table 6.

among dismissed filers, but these estimates are far from conclusive evidence.

Next, we test the importance of debt forgiveness using across-state variation in Chapter 7 homestead exemption levels. Recall that the Chapter 13 repayment plan must pay unsecured creditors at least as much as they would receive under Chapter 7. Moreover, the amount that unsecured creditors receive under Chapter 7 depends on state home exemption levels. If debt forgiveness is empirically important, the effect of Chapter 13 protection should therefore be larger in states that have high exemptions where filers are able to discharge a larger fraction of their debt. Table 6 presents two-stage least squares results for baseline homeowners and renters in states with above and below median home exemption levels ($15,000). The results are broadly consistent with the idea that benefits of Chapter 13 protection are larger when more debt is forgiven. However, once again, only two of the eight differences are statistically significant due to large standard errors. The effect of Chapter 13 protection on financial strain is 0.187 standard deviations larger for homeowners in high exemption states compared to homeowners in low exemption states (p-value=0.078), and the effect on mortgage balance is $26,724 larger (p-value=0.008). For renters, we find that the effect of Chapter 13 protection on auto balance is larger in high exemption states because states with high home exemptions also have high auto exemptions.[25] These results suggest that debt forgiveness also plays an important role in explaining our findings, though again the evidence is far from conclusive.

VI. Conclusion

In this paper, we exploit the random assignment of bankruptcy filers to judges to estimate the impact of Chapter 13 bankruptcy protection on post-filing financial outcomes. We find that Chapter 13 protection reduces financial strain, increases the probability of being a homeowner, and reduces the amount of debt in collection. Chapter 13 protection also increases credit scores and credit access proxies. The effects of Chapter 13 protection are largest in states with more creditor-friendly laws and states with higher Chapter 7 exemption levels, suggesting that protection from debt col-

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[25]The correlation between a state's homestead exemptions level and auto exemptions level is 0.43.

lectors and debt forgiveness are key drivers of our results.

Our results complement earlier work by Dobbie and Song (2015) showing that Chapter 13 protection increases earnings and reduces mortality risk among marginal recipients. The results in this paper suggest that Chapter 13 protection also has important impacts on financial well-being and economic stability. Importantly, and contrary to much of the prior literature, we find that Chapter 13 decreases the likelihood of adverse financial events, allows debtors to retain important assets such as a home, and increases both credit access measures and credit scores. These results suggest that the benefits of bankruptcy protection are much broader than previously realized. We also find that many of the direct and indirect effects of Chapter 13 protection persist beyond plan completion, suggesting that improvements in financial well-being are long-term and not merely a mechanical consequence of the bankruptcy procedure.

Both papers also find that the benefits of bankruptcy protection are driven by a deterioration of outcomes among dismissed filers, rather than gains among granted filers. These results provide new evidence on the mechanisms through which excessive debt and financial distress distort borrower behavior. In particular, our results suggest that both excessive debt and the debt collection process have significant long-term consequences and that bankruptcy protection can ameliorate many of these adverse consequences.

The findings from this paper and Dobbie and Song (2015) will also help inform ongoing efforts to evaluate the welfare impact of the consumer bankruptcy system. These evaluations typically use quantitative models to weigh the trade-off between the ex-post consumption smoothing benefits provided by bankruptcy protection estimated in this paper, with the ex-ante increased borrowing costs suggested by economic theory (e.g. Athreya 2002, Li and Sarte 2006, Livshits, MacGee, and Tertilt 2007, Chatterjee and Gordon 2012). An important limitation of this literature has been the lack of empirical evidence on the magnitude of the benefits provided by bankruptcy protection for the marginal recipient. Our results suggest that the ex-post benefits of consumer bankruptcy on important outcomes, such as credit access and debt repayment, are significantly larger than previously assumed by this literature. Moreover, we find that consumer bankruptcy also impacts a

number of outcomes previously assumed to be fixed, such as asset holdings and labor supply. We therefore view the incorporation of our empirical estimates into a general equilibrium model of the credit market as an important area for future research.

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Table 1
Descriptive Statistics and Randomization Balance

	All Credit Users		Judge Sample		
	Full Sample	Bankruptcy Filers	Harsh Judge	Lenient Judge	p-value
Panel A: Judge Leniency	(1)	(2)	(3)	(4)	(5)
Judge Leniency	-	-	-0.013	0.012	0.000
Panel B: Baseline Characteristics					
Age	48.549	43.699	44.843	44.863	0.229
Homeowner	0.470	0.520	0.668	0.643	0.175
ZIP code Income	46.192	39.907	38.463	37.669	0.943
Panel C: Baseline Financial Events					
Delinquency	0.148	0.413	0.681	0.675	0.962
Collection	0.137	0.296	0.460	0.467	0.897
Charge-off	0.065	0.188	0.308	0.310	0.630
Bankruptcy	0.010	0.007	0.046	0.048	0.318
Judgment	0.009	0.034	0.067	0.060	0.403
Foreclosure	0.003	0.010	0.055	0.048	0.632
Lien	0.004	0.011	0.021	0.021	0.445
Repossession	0.003	0.012	0.022	0.020	0.491
Panel D: Baseline Unsecured Debt and Collections					
Revolving Balance	6.011	13.083	10.939	10.007	0.440
Collection Balance	0.601	1.432	2.421	2.497	0.676
Panel E: Baseline Secured Assets					
Have a Mortgage	0.367	0.434	0.591	0.567	0.274
Mortgage Balance	42.460	39.848	56.804	53.437	0.213
Have an Auto Loan	0.283	0.454	0.479	0.468	0.778
Auto Balance	4.391	7.803	8.359	8.207	0.913
Panel F: Baseline Credit Access					
Revolving Utilization	25.495	61.443	70.869	70.968	0.858
Non-Mortgage Inquiries	0.807	1.841	2.355	2.362	0.186
Panel G: Baseline Credit Score					
Credit Score	739.538	630.096	581.373	580.155	0.730
Panel H: Data Characteristics					
Matched to Credit Report	-	-	0.692	0.687	0.823
Missing Baseline Outcomes	-	0.187	0.028	0.029	0.037
Observations	3308824	56906	85173	89903	175076

Notes: This table reports summary statistics. The all credit user sample consists of a two percent random sample of credit users in the United States from 2002-2005. Bankruptcy filers consist of individuals who filed for any bankruptcy chapter from 2002-2006. The judge sample consists of Chapter 13 bankruptcy filers originating from offices that randomly assigns filers to judges between 2002-2005 that are linked to credit report data in the year of filing. Column 5 reports p-values calculated from separate regression models of each baseline characteristic on an indicator for being assigned to a judge with above median leniency. Column 5 also controls for office-by-filing-month fixed effects and clusters standard errors at the office level. See the data appendix for details on the data and variable construction.

Table 2
Judge Leniency and Chapter 13 Protection

	(1)	(2)	(3)	(4)	(5)	(6)
Judge Leniency	0.88872***	0.84047***	0.82620***	0.83218***	0.84044***	0.81910***
	(0.04873)	(0.05172)	(0.04909)	(0.05408)	(0.05100)	(0.05111)
Age at Filing		0.00332***	0.00197***	0.00349***	0.00310***	0.00178***
		(0.00017)	(0.00019)	(0.00016)	(0.00016)	(0.00018)
Homeowner		0.03570***	0.00770	0.02983***	0.04015***	0.01380**
		(0.01097)	(0.01167)	(0.00462)	(0.01072)	(0.00534)
ZIP code Income		0.00067***	0.00005	0.00073***	0.00071***	0.00024
		(0.00022)	(0.00017)	(0.00022)	(0.00022)	(0.00017)
Financial Strain Index		−0.08902***	−0.06781***	−0.08596***	−0.08783***	−0.06211***
		(0.00691)	(0.00551)	(0.00669)	(0.00667)	(0.00493)
Revolving Balance			0.00510***			0.00526***
			(0.00024)			(0.00022)
Collection Balance			−0.00383***			−0.00328***
			(0.00050)			(0.00047)
Have a Mortgage				0.02975**		0.03738***
				(0.01172)		(0.01097)
Mortgage Balance				−0.00032***		−0.00049***
				(0.00006)		(0.00006)
Have an Auto Loan				0.09052***		0.08470***
				(0.00537)		(0.00522)
Auto Balance				−0.00041*		−0.00043*
				(0.00023)		(0.00024)
Revolving Utilization					0.00011***	−0.00003
					(0.00002)	(0.00002)
Non-Mortgage Inquiries					−0.01104***	−0.01295***
					(0.00099)	(0.00079)
Dependent Variable Mean	0.446	0.446	0.446	0.446	0.446	0.446
Observations	175076	175076	175076	175076	175076	175076

Notes: This table reports first stage results. The sample consists of Chapter 13 bankruptcy filers originating from offices that randomly assigns filers to judges between 2002-2005 that are linked to credit report data in the year of filing. Judge leniency is the leave-one-out mean rate of granting Chapter 13 bankruptcy protection for the assigned judge minus the leave-one-out mean rate of granting bankruptcy protection for the office. All characteristics are measured one year prior to the bankruptcy filing. All regressions control for office-by-filing-month fixed effects and cluster standard errors at the office level. See the data appendix for details on the data and variable construction. *** = significant at 1 percent level, ** = significant at 5 percent level, * = significant at 10 percent level.

Table 3
Chapter 13 Protection and Financial Well-Being

| | Years 1-5 Post-filing | | | Years 6-8 Post-filing | | |
| | Dismissed Mean | 2SLS Results | | Dismissed Mean | 2SLS Results | |
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Adverse Financial Events						
Financial Strain Index	−0.068	−0.369***	−0.323***	−0.416	−0.216***	−0.137**
	(0.575)	(0.070)	(0.071)	(0.777)	(0.055)	(0.058)
Panel B: Unsecured Debt and Collections						
Revolving Balance	2.563	0.199	−0.871	1.014	0.697*	0.167
	(6.065)	(0.720)	(0.727)	(3.878)	(0.360)	(0.355)
Collection Balance	4.217	−1.842***	−1.333***	4.666	−2.534***	−1.986***
	(5.898)	(0.449)	(0.433)	(7.376)	(0.563)	(0.629)
Panel C: Secured Assets						
Have a Mortgage	0.363	0.110**	0.132***	0.184	0.259***	0.262***
	(0.369)	(0.045)	(0.021)	(0.371)	(0.032)	(0.027)
Mortgage Balance	26.833	12.196***	14.535***	15.861	34.967***	37.300***
	(41.359)	(4.132)	(5.075)	(48.046)	(11.495)	(13.747)
Have an Auto Loan	0.178	0.046*	0.021	0.190	0.127***	0.128***
	(0.263)	(0.028)	(0.031)	(0.367)	(0.041)	(0.045)
Auto Balance	4.067	−0.500	−0.880	3.518	0.641	0.524
	(5.797)	(0.500)	(0.560)	(6.972)	(0.655)	(0.704)
Panel D: Credit Access						
Revolving Utilization	46.729	−15.132***	−16.289***	34.362	−8.942	−8.690
	(46.437)	(3.884)	(3.403)	(53.460)	(8.636)	(9.230)
Non-Mortgage Inquiries	1.584	−0.410***	−0.300**	0.974	−0.107	−0.043
	(1.487)	(0.121)	(0.122)	(1.375)	(0.184)	(0.197)
Controls	–	No	Yes	–	No	Yes
Observations	97006	175076	175076	83792	151655	151655

Notes: This table reports two-stage least squares results of the impact of Chapter 13 bankruptcy protection on post-filing outcomes. The sample consists of Chapter 13 bankruptcy filers originating from offices that randomly assign filers to judges between 2002-2005 who are linked to credit report data in the year of filing. Columns 1-3 present results for the year of filing to fifth year post-filing and columns 4-6 present results for the sixth year post-filing to eighth year post-filing. Columns 1 and 4 report the post-filing mean and standard deviation for dismissed filers. Columns 2-3 and 5-6 instrument for Chapter 13 protection using our judge instrument. All regressions control for office-by-filing-month fixed effects and cluster standard errors at the office level. Columns 3 and 6 add controls for baseline age bins, homeownership, ZIP code income, financial strain index, revolving balance, collection balance, mortgage balance, auto balance, indicators for mortgage and auto loans, revolving utilization, and non-mortgage inquiries as controls. See the data appendix for details on the data and variable construction. *** = significant at 1 percent level, ** = significant at 5 percent level, * = significant at 10 percent level.

Table 4

Chapter 13 Results by Filer Characteristics

	Age at Filing			Baseline Credit Score		Baseline Homeowner	
	25 to 39	40 to 59	60 and up	High	Low	Yes	No
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Financial Strain	-0.402***	-0.321***	-0.360	-0.347***	-0.336***	-0.415***	-0.188**
	(0.084)	(0.071)	(0.392)	(0.066)	(0.072)	(0.050)	(0.091)
	[0.040]	[-0.041]	[-0.248]	[-0.132]	[0.012]	[0.010]	[-0.184]
Revolving Balance	0.041	-1.172	1.017	-1.079	-0.079	-0.821	-0.445
	(0.691)	(1.078)	(2.138)	(0.961)	(0.757)	(0.736)	(0.879)
	[2.171]	[2.961]	[4.282]	[3.161]	[2.226]	[3.061]	[1.676]
Collection Balance	-1.723**	-1.332***	-1.396	-2.206***	-0.386	-1.509**	-1.102**
	(0.746)	(0.407)	(1.537)	(0.399)	(1.070)	(0.641)	(0.504)
	[4.737]	[4.160]	[2.950]	[3.354]	[4.737]	[3.935]	[4.661]
Mortgage Balance	11.234	18.742***	27.430***	16.099***	14.894*	20.617***	4.854
	(7.264)	(4.938)	(10.685)	(4.598)	(8.702)	(9.672)	(6.043)
	[25.937]	[31.594]	[28.102]	[29.721]	[26.420]	[39.126]	[6.404]
Auto Balance	-1.581**	-0.687	1.824	-0.651	-0.919*	-0.884	-0.866
	(0.766)	(0.757)	(1.769)	(0.786)	(0.480)	(0.590)	(0.965)
	[4.797]	[4.188]	[3.129]	[4.508]	[3.920]	[4.224]	[3.810]
Revolving Utilization	-21.320***	-13.864***	-6.868	-16.015***	-14.252**	-16.641***	-14.666**
	(6.616)	(3.027)	(10.075)	(4.910)	(5.515)	(2.631)	(7.009)
	[49.679]	[48.149]	[45.564]	[41.543]	[52.114]	[46.686]	[47.368]
Non-Mortgage Inquiries	-0.281	-0.362***	0.294	-0.450**	-0.189	-0.437**	-0.094
	(0.215)	(0.077)	(0.621)	(0.208)	(0.378)	(0.192)	(0.161)
	[1.843]	[1.552]	[1.221]	[1.422]	[1.714]	[1.595]	[1.548]
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	54442	81206	19100	84154	82241	111432	58627

Notes: This table reports first stage and two-stage least squares results by baseline characteristics. The sample consists of Chapter 13 bankruptcy filers originating from offices that randomly assign filers to judges between 2002-2005 who are linked to credit report data in the year of filing. The post-filing mean for dismissed filers is reported in brackets for each subgroup. We instrument for Chapter 13 protection using the leave-one-out mean rate of granting Chapter 13 bankruptcy protection for the office. Subgroup instruments are constructed using the leave-one-out mean rate of granting Chapter 13 bankruptcy protection for the assigned judge minus the leave-one-out mean rate of granting Chapter 13 bankruptcy protection for the office. Subgroup instruments are constructed using the matched estimation sample. All regressions control for baseline age bins, homeownership, ZIP code income, financial strain index, revolving balance, collection balance, mortgage balance, auto balance, indicators for mortgage and auto loans, revolving utilization, non-mortgage inquiries, office-by-filing-month fixed effects, and cluster standard errors at the office level. See the data appendix for details on the data and variable construction. *** = significant at 1 percent level, ** = significant at 5 percent level, * = significant at 10 percent level.

Table 5
Chapter 13 Results by State Wage Garnishment Laws

	Garnishment Allowed		
	Yes	No	p-value
	(1)	(2)	(3)
Financial Strain	−0.326***	−0.162	0.494
	(0.071)	(0.234)	
	[−0.058]	[−0.167]	
Revolving Balance	−1.016	6.716	0.118
	(0.637)	(4.967)	
	[2.477]	[3.407]	
Collection Balance	−1.301***	−2.964	0.409
	(0.443)	(1.996)	
	[4.190]	[4.482]	
Mortgage Balance	14.244***	29.708	0.604
	(5.309)	(29.802)	
	[26.517]	[29.940]	
Auto Balance	−0.996*	5.198	0.238
	(0.515)	(5.305)	
	[4.051]	[4.223]	
Revolving Utilization	−16.758***	8.812	0.364
	(3.602)	(27.600)	
	[46.375]	[50.211]	
Non-Mortgage Inquiries	−0.326***	1.056	0.096
	(0.126)	(0.834)	
	[1.584]	[1.584]	
Controls	Yes	Yes	–
Observations	154611	20465	–

Notes: This table reports two-stage least squares results of the impact of Chapter 13 bankruptcy protection for states that do and do not allow wage garnishment. The sample consists of Chapter 13 bankruptcy filers originating from offices that randomly assign filers to judges between 2002-2005 who are linked to credit report data in the year of filing. The post-filing mean for dismissed filers is reported in brackets for each subgroup. We instrument for Chapter 13 protection using the leave-one-out mean rate of granting Chapter 13 bankruptcy protection for the assigned judge minus the leave-one-out mean rate of granting bankruptcy protection for the office. Subgroup instruments are constructed using the matched estimation sample. All regressions control for baseline age bins, homeownership, ZIP code income, financial strain index, revolving balance, collection balance, mortgage balance, auto balance, indicators for mortgage and auto loans, revolving utilization, non-mortgage inquiries, office-by-filing-month fixed effects, and cluster standard errors at the office level. See the data appendix for details on the data and variable construction. *** = significant at 1 percent level, ** = significant at 5 percent level, * = significant at 10 percent level.

Table 6

Chapter 13 Results by State Homestead Exemption Laws

	Homeowners			Renters		
	High Home Ex.	Low Home Ex.	p-value	High Home Ex.	Low Home Ex.	p-value
	(1)	(2)	(3)	(4)	(5)	(6)
Financial Strain	−0.454***	−0.267**	0.078	−0.213***	−0.161	0.787
	(0.030)	(0.108)		(0.076)	(0.192)	
	[0.032]	[−0.022]		[−0.141]	[−0.214]	
Revolving Balance	−1.139**	0.366	0.405	−0.634	−0.239	0.789
	(0.535)	(1.797)		(1.106)	(1.094)	
	[3.333]	[2.677]		[2.010]	[1.435]	
Collection Balance	−1.681***	−0.869	0.681	−1.198**	−1.009	0.833
	(0.593)	(1.928)		(0.587)	(0.822)	
	[4.089]	[3.718]		[4.699]	[4.634]	
Mortgage Balance	26.724***	−1.546	0.008	6.452	6.179	0.972
	(7.884)	(8.119)		(6.667)	(4.977)	
	[42.984]	[33.692]		[8.444]	[4.934]	
Auto Balance	−0.904	−0.628	0.790	−1.437**	0.596	0.067
	(0.703)	(0.819)		(0.718)	(0.991)	
	[4.327]	[4.080]		[4.449]	[3.350]	
Revolving Utilization	−17.233***	−16.044*	0.895	−10.084*	−27.058***	0.106
	(1.831)	(9.139)		(5.936)	(9.434)	
	[46.774]	[46.561]		[47.841]	[47.027]	
Non-Mortgage Inquiries	−0.500**	−0.178	0.411	−0.209*	0.147	0.160
	(0.249)	(0.322)		(0.112)	(0.269)	
	[1.637]	[1.537]		[1.596]	[1.514]	
Controls	Yes	Yes	–	Yes	Yes	–
Observations	61334	50098	–	25900	32727	–

Notes: This table reports two-stage least squares results of the impact of Chapter 13 bankruptcy protection for states with above median and below median homestead exemption amounts, separately by homeownership status. The sample consists of Chapter 13 bankruptcy filers originating from offices that randomly assign filers to judges between 2002-2005 who are linked to credit report data in the year of filing. The post-filing mean for dismissed filers is reported in brackets for each subgroup. We instrument for Chapter 13 protection using the leave-one-out mean rate of granting Chapter 13 bankruptcy protection for the assigned judge minus the leave-one-out mean rate of granting bankruptcy protection for the office. Subgroup instruments are constructed using the matched estimation sample. All regressions control for baseline age bins, homeownership, ZIP code income, financial strain index, revolving balance, collection balance, mortgage balance, auto balance, indicators for mortgage and auto loans, revolving utilization, non-mortgage inquiries, office-by-filing-month fixed effects, and cluster standard errors at the office level. See the data appendix for details on the data and variable construction. *** = significant at 1 percent level, ** = significant at 5 percent level, * = significant at 10 percent level.