The Impact of Debt Relief Generosity and Liquid Wealth on Household Bankruptcy

Sasha Indarte*
Duke University

June 30, 2019

Abstract

The debt relief households obtain in bankruptcy provides insurance against wealth losses, but also distorts borrower incentives to repay debt, discouraging lending. Understanding how bankruptcy filings respond to changes in cash-flows and "strategically" to relief generosity is important for assessing these trade-offs. This paper presents new evidence on the causes of bankruptcy using data on millions of mortgage borrowers. First, I exploit a kink in debt relief generosity induced by asset exemption laws in a regression kink design (RKD) to estimate a small positive effect of an increase in generosity on filing. Second, I exploit quasi-experimental variation in mortgage payment reductions to estimate a large negative effect of an increase in cash-flows on filing. The RKD isolates the strategic motive by holding wealth fixed and varying the payoff from filing while the payment reductions affect filing by increasing cash-flows that are not generally seizable in bankruptcy. Using a simple model of household bankruptcy, I show that the relatively weak strategic motive implies consumption gains to filers must be large but that other costs of bankruptcy, such as social stigma or from credit market exclusion, must also be large. My findings are consistent with a lack of insurance against cash-flow shocks driving bankruptcy and imply that increases in the generosity of bankruptcy only weakly incentivize further filing.

*Email: **sasha.indarte@duke.edu**, website: **https://sashaindarte.github.io**.

I am deeply grateful to David Berger, Marty Eichenbaum, Guido Lorenzoni, Paul Mohnen, John Mondragon, and Matt Notowidigdo for their guidance and encouragement while working on this project. For insightful discussions and comments, I thank Scott Baker, Gideon Bornstein, Ivan Canay, Larry Christiano, Anthony DeFusco, Eileen Driscoll, Jan Eberly, Kilian Huber, Stephanie Johnson, Alice Jun, David Matsa, Brian Melzer, Charlie Nathanson, Matt Rognlie, Paola Sapienza, John Shea, Max Tabord-Meehan, Fabrice Tourre, Nick Turner, Harald Uhlig, and Eric Zwick as well as seminar participants at Northwestern, Kellogg, Rice (Jones), Notre Dame (Mendoza), Boston College (Carroll), the Fed Board, LSE, LBS, NYU (Stern), the New York Fed, UBC, Maryland, HEC Montreal, and Bocconi.

Financial support from the Becker Friedman Institutes's Macro Financial Modeling Initiative is gratefully acknowledged. This research was supported in part through the computational resources and staff contributions provided for the Quest high performance computing facility at Northwestern University which is jointly supported by the Office of the Provost, the Office for Research, and Northwestern University Information Technology. I am grateful for the research support and data made available through the Guthrie Center for Real Estate Research at the Kellogg School of Management.

1 Introduction

During the Great Recession, millions of households sought debt relief through bankruptcy. In 2010, bankruptcy offered households $454 billion in debt forgiveness, exceeding transfers from unemployment insurance at its peak that same year ($139 billion).[1] Over 1.5 million households filed for bankruptcy in 2010 alone, and nearly one in ten Americans have filed at least once in their life (Stavins, 2000; Keys, 2018). By allowing households to discharge debt, bankruptcy implicitly provides a form of wealth insurance. This insurance is potentially welfare-improving if households face incomplete credit and insurance markets. But generous debt relief also distorts borrower incentives to repay, which encourages more filing and ultimately discourages lending.

The desirability of generous bankruptcy depends fundamentally on what causes households to file.[2] Is bankruptcy driven by cash-flow shocks and liquidity constraints? Or is bankruptcy driven by a "strategic motive" to increase wealth by discharging debt? A strong "cash-flow motive" can arise when liquidity constraints create pressure to file in order to increase consumption by erasing debt obligations and stopping creditors from garnishing wages. When the strategic motive is strong, the social cost of generous bankruptcy is higher as creditors will experience greater losses and therefore become reluctant to lend.

Despite the scale of bankruptcy, there is limited evidence on the strength of strategic and cash-flow filing motives. This paper fills this gap by using detailed micro data and two quasi-experimental research designs to document new evidence on the causes of household bankruptcy. The first research design holds borrower wealth outside of bankruptcy constant and varies the generosity of the debt relief households receive in bankruptcy. By only varying the relative payoff (wealth gain) from filing, this analysis isolates the strategic motive. The second research design exploits variation in the size of reductions in minimum mortgage payments. In bankruptcy, mortgages are rarely discharged and payment reductions are generally not seizable by creditors. This second design approximates an experiment that isolates cash-flow motives by varying cash-flows while holding fixed the payoff from filing. I find that filing responds *weakly* to changes in the generosity of bankruptcy but is *strongly* deterred by reductions in minimum debt payments. This suggests the strategic filing motive is weak and that a lack of insurance against cash-flow shocks is an important driver of bankruptcy.

The first analysis focuses on the strategic motive by estimating the causal effect of a reduction in the generosity of bankruptcy on filing rates using a regression kink design (RKD). The RKD exploits a kink in the debt relief households receive in bankruptcy induced by states' homestead exemption laws. These laws limit the amount of home equity filers can protect in bankruptcy, where filers must pay creditors the value of any home equity in excess of the limit.

It is challenging to isolate the causal effect of generous bankruptcy on household filing. Cross-

[1] Sources: (1) annual BAPCPA report, tables 1A and 1D, summing net scheduled (eligible for discharge) debt for Chapter 7 and 13 filers; (2) Bureau of Economic Analysis (state and federal unemployment transfers).

[2] The generosity of bankruptcy refers to the amount of debt a filer is able to discharge net of required payments out of assets to their creditors.

sectional comparisons of filing rates and exemptions laws could *understate* the effect on household behavior, as exemption laws also indirectly affect filing by reducing the supply of unsecured credit.[3] On the other hand, within state comparisons of households with different amounts of seizable assets could *overstate* the effect of generous debt relief. Unobserved factors that reduce wealth can incentivize bankruptcy both directly and indirectly by reducing the cost of bankruptcy.

The RKD isolates the causal effect of debt relief generosity by measuring the *change* in the relationship between filing rates and home equity at the exemption limit. As long as no other factors that affect filing also *kink* at the exemption limit, the change in this relationship identifies the effect of having to pay creditors an additional dollar in bankruptcy. An attractive feature of the RKD is that this assumption of no kink is much weaker than assuming exogeneity with respect to other factors that influence bankruptcy. I implement the RKD using a monthly mortgage-level dataset from CoreLogic that reports the price of the home at origination and tracks mortgage balances and bankruptcy filings for nearly half of all mortgages originated in the US.[4] To my knowledge, this paper is the first to use the bankruptcy filing data available in CoreLogic's data and to exploit homestead exemption laws in an RKD.

The second analysis focuses on the cash-flow motive and estimates the causal effect of mortgage payment reductions on filing. I exploit quasi-experimental variation in the size of payment reductions received by households with adjustable-rate mortgages (ARMs) in an instrumental variables (IV) strategy. ARMs feature a fixed interest rate for typically the first five years and then reset to a new interest rate based on the current level of a preselected "index rate." In 2008, an unprecedented spread opened between two popular index rates: the one-year Libor and Treasury rates. The spread resulted in otherwise similar mortgages receiving very different payment reductions. At the 218 basis point peak in the Libor-Treasury spread in September 2008, resetting mortgages indexed to Libor on average paid $4,634 more over the next year than mortgages indexed to Treasury. Using data on ARMs originated prior to 2008 from CoreLogic, I estimate the effect of mortgage payment size on filing in the year after an ARM's reset using the value of the index rate (Libor or Treasury) at the time of the reset as an instrument.

The ideal experiment to isolate the effect of cash-flows, net of any strategic motives, would vary cash-flows not seizable in bankruptcy. The ARM IV strategy is an approximation of this ideal in two ways. First, although payment changes last for one year, a given change can affect filing both through that year's cash-flows and by changing the expectations of future payments. Additionally, households may not "receive" this cash-flow increase if they stop making mortgage payments. This could arise from delinquency or prepaying the mortgage (from a sale or refinancing). To address

[3]Unsecured credit refers to loans not backed by collateral (credit cards, medical debt, etc.) as opposed secured credit such as mortgage debt. Bankruptcy is typically only used to discharge unsecured debt as the discharge does not erase a creditor's claim to collateral. The negative relationship between unsecured lending and generous bankruptcy laws is well-documented in the cross-section (Gropp et al., 1997; Pence, 2006; White, 2007; Mitman, 2016; Severino and Brown, 2017) and analyses of the 2005 bankruptcy reform also find that its reduction in generosity led to lower interest rates (Gross et al., 2018b). Restricted credit access leads to less accumulation of unsecured debt and ultimately weakens incentives to file.

[4]I follow Di Maggio et al. (2017) and update home values over time using Zillow's ZIP-level house price index. This introduces measurement error which I address in my implementation.

these issues, I combine additional data from CoreLogic on these borrower behaviors to estimate the expected net present value (NPV) of mortgage payments after a reset under various assumptions over discount rates. With this estimate of the NPV, I can scale the IV estimate to identify the direct effect of a change in the current year's cash-flows.

The main finding is that household bankruptcy is much more responsive to a given change in cash-flows than an equivalent reduction in the generosity of bankruptcy. A $1,000 decrease in the generosity of bankruptcy leads to a 3.42% decrease in annual filings (a 0.02 percentage point drop in the rate). On the other hand, a $1,000 decrease in total mortgage payments over the next year leads to at least a 12.61% decrease in annual filings (a 0.09 percentage point drop), a response more than four times greater.[5] Sensitivity to generosity is most pronounced among households in ZIP codes with lower income, higher shares of households claiming unemployment insurance, and larger falls in house prices during the housing crisis.

Combining these estimates with a model of household bankruptcy, I show that the estimates imply that the marginal filer (1) experiences a large consumption gain when filing and (2) faces large costs from bankruptcy outside of its immediate financial cost. The model relates the relative strength of strategic and cash-flow motives to the ratio of marginal utility in and out of bankruptcy for the marginal filer. Intuitively, when the strategic motive is weak relative to the cash-flow motive, marginal utility is low in bankruptcy compared to outside of bankruptcy (for the marginal filer). This implies that the immediate consumption increase is large. Because the marginal filer is by definition indifferent, this large consumption gain implies that other costs of bankruptcy must also be large. Disutility stemming from a moral aversion to default is one potential cost.[6] Another likely cost are the dynamic costs from the long-term credit market exclusion that bankruptcy filers experience.[7]

This paper contributes to four strands of literature. First I add to the literature studying the causes of household bankruptcy. I build on previous studies of the effect of generous bankruptcy on filings (Fay, Hurst and White, 2002; Mitman, 2016) by using a larger and more representative dataset and a new research design (the RKD) to isolate the household's direct response to reduced generosity. Prior work on the relationship between liquidity and bankruptcy finds evidence that a lack of liquidity to afford upfront filing fees prevents some households from filing for bankruptcy, finding that receiving tax rebates made households *more* likely to file (Gross, Notowidigdo and Wang, 2014). The population in my sample (homeowners) is likely less liquidity-constrained than the broader population and the typical payment reduction in the ARM IV analysis is much larger than the tax rebates ($2,000 versus $200 within a year). Both of these features make it less likely that

[5] This 12.61% figure corresponds to the effect of an increase in cash-flows over the next year, net of wealth effects from changes in expected mortgage payments. I also reweight the sample when obtaining this estimate to resemble the RKD sample in terms of observable characteristics, making the estimates across these subsamples more comparable.

[6] A large cost would be consistent with survey evidence that 82% of households report it being immoral to default when capable of repaying Guiso et al. (2013). Experiments also find that appeals to morality make delinquent credit card borrowers more likely to begin making minimum debt payments (Bursztyn et al., 2018).

[7] See for example Gross, Notowidigdo and Wang (2018a); Dobbie, Keys and Mahoney (2017); Dobbie, Goldsmith-Pinkham, Mahoney and Song (2019); Herkenhoff, Phillips and Cohen-Cole (2016); Musto (2004).

the filing behavior I observe is driven mainly by overcoming this barrier to affording bankruptcy, instead reflecting the relaxation of liquidity constraints that allow the household to consume more and lessening the "need" to file for bankruptcy.

Second, I add to the literature on the role of strategic and cash-flow motives in household default. This literature focuses on mortgage delinquency and there is a lack of consensus on the strength of strategic motives (for evidence of a weak motive see Scharlemann and Shore, 2016, Gerardi et al., 2017, and Ganong and Noel, 2019; for a strong motive see Mayer et al., 2014, Haughwout et al., 2016, and Dobbie and Song, 2018).[8] Results for mortgage delinquency do not obviously generalize to bankruptcy. Mechanically, liquidity shocks could have a strong effect on delinquency by making repayment infeasible. However, bankruptcy is not mechanically triggered in the same way. Filing is most often a decision by the filer, as creditors rarely initiate bankruptcy proceedings. The costs of mortgage delinquency may also be more backloaded. Foreclosures typically take over a year to complete, during which the delinquent household can remain in their home.

Another important difference from debt relief programs studied in prior work is that bankruptcy is available to a broader population. Qualifying for debt reductions in HAMP (e.g., Ganong and Noel, 2019) or the credit card debt modification program of Dobbie and Song (2018) required delinquency or evidence of financial hardship. This makes these studies informative about the strength of strategic and cash-flow motives among financially distressed borrowers, the relevant population for many policies, but it is not obvious if the average household would behave similarly. Moreover, when qualifying for debt relief is easy to manipulate through delinquency, selection could lead more strategic filers to comprise a larger share of the financially distressed population receiving debt relief (Mayer et al., 2014). While bankruptcy may be more appealing to distressed households, Chapter 13 is available to all households and the more generous Chapter 7 is available to those with income below their state's median. Studying a broader population is useful for gaining a sense of how strategic households are on average.

Third, this paper documents new facts on the causes of bankruptcy that are useful for disciplining the model-based literature studying the macroeconomic effects of bankruptcy policy. This literature explores the positive and normative effects of changes to the design of bankruptcy in dynamic stochastic general equilibrium models (e.g., Athreya, 2002, 2006; Livshits et al., 2007; Chatterjee et al., 2007; Chatterjee and Gordon, 2012; Nakajima and Ríos-Rull, 2014; Mitman, 2016; Dávila, 2016; Gordon, 2017). The extent to which households lack insurance against wealth losses and the responsiveness of filing to changes in bankruptcy generosity are important elasticities that shape the desirability of generous bankruptcy in these models. The weak strategic motive that I document implies a smaller social cost of generous bankruptcy. On the other hand, the strong cash-flow motive is consistent with households lacking insurance against welfare shocks – a market failure that generous bankruptcy could potentially mitigate. Moreover, I build on the theoretical

[8]To my knowledge, the only prior work to investigate similar questions in bankruptcy are Dobbie and Song (2018) and Fay, Hurst and White (2002). This empirical part of this paper differs from Dobbie and Song (2018) by focusing on a broader population and by using different identification strategies. Compared to Fay, Hurst and White (2002), the RKD uses a different identification strategy and a new, large dataset to study the effect of bankruptcy's generosity on filing.

work of Dávila (2016) by showing that the *relative* strength of these motives has implications for consumption dynamics when filing and the costs of bankruptcy outside of its immediate financial cost.

Lastly, this paper also contributes to the RKD literature (e.g., Calonico et al., 2014; Card et al., 2015). Measurement error introduces non-standard challenges in an RKD, and identification for the usual framework used to address it (the "fuzzy" RKD of Card et al., 2015) can fail when (1) measurement error is a continuous variable or (2) the researcher lacks independent measures of the explanatory and running variables. In my setting, measurement error arises from the imputation of home prices and I can only measure the explanatory variable (seizable equity) with a mis-measured running variable (home equity). To address this, I characterize the bias of a parametric analog of the standard sharp RK estimator relative to the local average response identified by the sharp RK estimand. Measurement error induces a familiar attenuation bias in the estimate but also another bias that grows with the proportion of observations assigned to the "wrong" side of the kink. I propose a measurement-error-corrected estimator for cases when the researcher has a subset of correctly measured data. I implement this estimator using a subset of the data containing home sales, assuming that home sales reflect correctly-measured home values. In a robustness analysis, I also extend the RKD permutation test of Ganong and Jäger (2018), an alternative approach to inference in an RKD, to a multi-kink setting.

The paper is organized as follows. Section 2 describes relevant institutional features of consumer bankruptcy. Section 3 describes the data used in both empirical analyses. Section 4 presents RKD estimation results for the effect of generosity on filing and the RKD measurement error consistency results (with econometric details available in Section E of the appendix). Section 5 presents the instrumental variables estimation results for the effect of cash-flows on filing. Section 6 presents the model of bankruptcy. Section 7 concludes.

2 Consumer Bankruptcy in the US

2.1 Overview

Consumer bankruptcy is a legal process that allows households to discharge debt while making partial payments to creditors. Households primarily use bankruptcy to erase unsecured debt, such as credit card and medical debt.[9] Discharging debt effectively transfers wealth from creditors to debtors. The option to file for bankruptcy creates an implicit form of wealth insurance by allowing households to discharge debt in response to events like job loss and illness.

Bankruptcy provides US households substantial debt relief. About 1% of US households file for bankruptcy each year, resulting in around one million filings per year. In a typical year, bankruptcy

[9] Bankruptcy is rarely used to discharge secured debt, such as mortgages or auto loans, because bankruptcy discharges the debt obligation but it does not erase the the creditor's lien on the collateral securing the loan. In fact, households appear to use bankruptcy to avoid default on secured loans (and, ultimately, foreclosure Mitman, 2016) by lowering unsecured debt payments so as to catch up/stay current on mortgages.

offers \$188.9 billion in debt forgiveness to households.[10] This is larger than the amount of unemployment insurance received by households during its 2010 peak (\$139 billion).

When filing for bankruptcy, households make payments to creditors based on the amount of assets they have above exemption limits. Under Chapter 7, a household's net financial cost of filing for bankruptcy is

$$\text{Seizable Assets} - \text{Dischargeable Debt} + \text{Legal and Filing Fees.}$$

Seizable assets are determined by state-specific exemption laws that outline the type and amount of assets protected from creditors when filing under Chapter 7. State and federal laws also prevent some types of debt from being discharged (e.g., child support and tax arrears). Filers also face court fees and, if they hire a lawyer, additional legal fees.[11] Exemption laws are one of the primary tools available to policymakers to influence how generous/costly bankruptcy is for households. By increasing exemption limits, policymakers can reduce the costs of bankruptcy to households, increasing the generosity of this source of debt relief.

Costs to households under Chapter 13 are closely related to those in Chapter 7.[12] In Chapter 13, households make monthly payments to creditors over a three to five year period, whereas in Chapter 7 households make a one-time payment. When filing under Chapter 13, households complete a detailed income and expense report to compute their disposable income. Chapter 13 payments are typically set equal to disposable income, but in principle they can be set higher. Federal law requires that total payments received by creditors under Chapter 13 must be at least as high as what they would receive under Chapter 7. Thus the Chapter 7 financial cost above serves as a lower bound for the financial cost of households filing for Chapter 13.[13]

2.2 The Homestead Exemption

Homestead exemptions, which protect home equity, are one of the most important policies shaping the generosity of the debt relief filers receive in bankruptcy. Most of the variation in households' potential debt relief is due to the homestead exemption (Auclert et al., 2019). Real estate is also the largest asset on most households' balance sheets, comprising 74% of filing households' assets on average.

[10] The figure here is the median amount of total, annual net scheduled debt among Chapter 7 and Chapter 13 filers since 2007. Note that net scheduled debt reflects the amount of debt that filers are eligible to discharge in bankruptcy based on the type of debt, which is not necessarily the actual amount that is discharged. Judges have some discretion to prevent the discharge of particular debt, which is typically used in cases were there is suspected fraud. Net scheduled debt per year since 2007 is depicted in Figure A.2 in the appendix.

[11] Typically these total around \$2,000, which can prove prohibitively expense and prevent some of the most distressed households from filing for bankruptcy (Gross, Notowidigdo and Wang, 2014).

[12] Another bankruptcy option available to businesses is Chapter 11. Occasionally individuals who run a business file under Chapter 11. This paper focuses on Chapter 7 and 13 filings as Chapter 11 filings (1) constitute a small fraction of nonbusiness filings (0.15% in 2017) and (2) better resemble corporate debt relief than household debt relief.

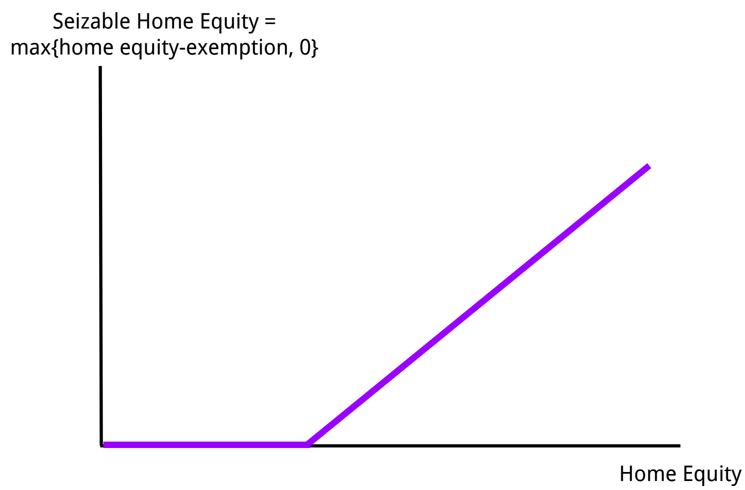
[13] In order to qualify for Chapter 7, households must have income below their state's median. This means test has been used since October 2005, when the Bankruptcy Abuse Prevention and Consumer Protection Act (BAPCPA) came into effect.

Homestead exemptions induce a kink in *seizable equity* as a function of home equity:

$$\text{Seizable Equity} = \max\{\text{Home Equity} - \text{Homestead Exemption}, 0\}.$$

Below their state's exemption limit, an additional dollar of home equity has no effect on their seizable equity and thus no effect on one of the major financial cost components of bankruptcy. But above the exemption, every additional dollar of home equity is an additional dollar that must be repaid to creditors in bankruptcy. This kink is illustrated below in Figure 1. This paper exploits the kink induced by homestead exemption laws in households' cost of bankruptcy as a quasi-experimental source of variation in households' financial cost of bankruptcy.

Figure 1: The Kink in Seizable Equity

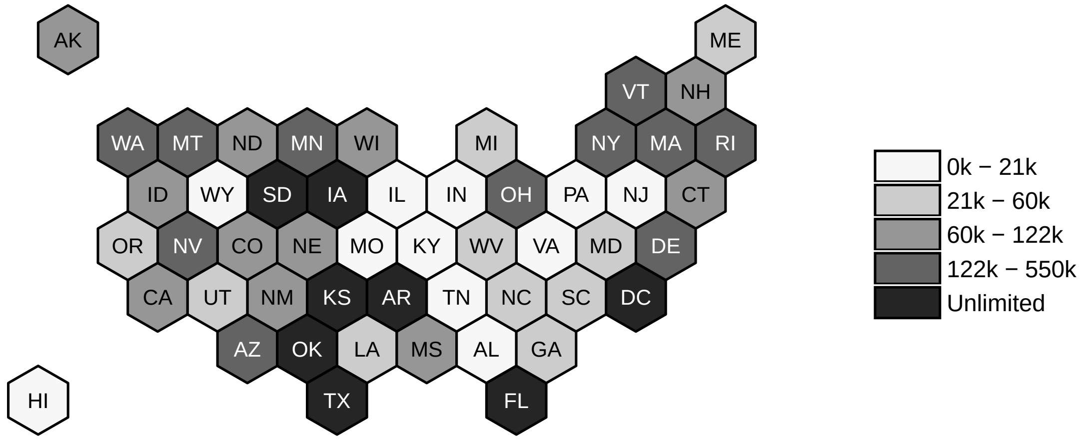


Notes: This figure depicts the kinked relationship between seizable equity and home equity. Seizable equity is zero below the exemption limit. Above the limit, seizable equity increases one-for-one with home equity.

Homestead exemption generosity differs dramatically across states (see figure 2). In 2017, exemptions ranged from \$0 in New Jersey, to \$550,000 in Nevada, to an unlimited amount in Texas.[14] Homestead exemptions also vary significantly across time, for example Ohio increased their homestead exemption for single households from \$5,000 to \$20,200 in 2008, and later from \$21,625 to \$132,900 in 2013. The RKD does *not* use variation in exemption levels for identification. However, the presence of multiple kinks can potentially improve estimation precision significantly in the RKD. Because home equity is likely related to many other factors (e.g., house prices) through which seizable equity correlates with bankruptcy, the RKD can better isolate the direct effect of seizable equity by controlling for home equity. Intuitively, with multiple kinks it is possible to compare households with similar levels of home equity but different distances from their state's exemption limit and thus different costs of filing for bankruptcy.

[14]Lastly, in some states, married households filing jointly can double the amount of their homestead exemption. I do not observe households' marital status in my data, nor the number of cosigners on the mortgage. To avoid the econometric challenges created by not knowing which kink a household effectively faces, the empirical analysis uses

Figure 2: Homestead Exemptions By State (2017)



Notes: This figure color-codes states based on their 2017 homestead exemption limits. For the states that allow doubling, the exemption level for single households is used.

Why do homestead exemptions differ so much across states? Cross-state differences in generosity have remained relatively stable in the US over the past two centuries. Early exemptions laws appear to be mainly a function of historical events, politics, and economic conditions Skeel (2001). In fact, historical homestead exemption levels better predict current levels than contemporaneous economic conditions (Hynes et al., 2004; Auclert et al., 2019). Most changes to exemption levels are done to keep pace with inflation. Large changes are rarer, occurring around two to three times for most states over 2000-2017.

The historical origin of current homestead exemption levels suggests that exemption levels are plausibly exogenous with respect current to bankruptcy filing. It is unlikely that there is reverse causality from filings to exemption levels nor omitted factors influencing both filing and exemptions simultaneously. However, exemption levels can influence filing indirectly. One well-documented channel is that unsecured creditors restrict lending to households in states with more generous bankruptcy exemptions (Gropp, Scholz and White, 1997; Pence, 2006; White, 2007; Mitman, 2016; Severino and Brown, 2017). When households accumulate less unsecured debt their benefit from filing diminishes. This indirect channel appears powerful as cross-sectional comparisons find that filing rates are *lower* among households in *more* generous states.[15] Cross-sectional comparisons of filing in states with different exemption levels reflect a general equilibrium outcome, but not tell us the direct effect of a marginal dollar of debt relief on a household's probability of filing. This paper estimates this direct effect using a new approach: an RKD exploiting the kink induced by the homestead exemption in households' cost of bankruptcy.

data only from states where households cannot double exemption limits.

[15]See Mitman (2016) for state-level evidence on this fact and Table 3 for household-level evidence.

3 Data

The main dataset used in the empirical analysis is a household-level panel dataset available from CoreLogic. The data allow me to track households' home equity and bankruptcy filings at the monthly level. Below I describe the structure of this data, the construction of key variables, and discuss summary statistics and the representativeness of the data. Appendix C contains additional information on the datasets and variable definitions.

3.1 Key Variables and Data Sources

Bankruptcy and Home Equity Data

I obtain household-level measures of home equity from CoreLogic's Loan Level Market Analytics (LLMA) database. The LLMA data contain detailed information on mortgage characteristics at origination and monthly loan performance over the life of the loan for a large sample of anonymized borrowers. CoreLogic collects this data from 25 of the largest mortgage servicers in the US. The LLMA data track approximately 5.7 million mortgages each year and in a typical year include 45% of mortgages originated in the US over the sample period (2000-2016).

To measure home equity, I take the difference between the end-of-month unpaid mortgage balance and an imputed home value. The LLMA data contain the sale price. I update the value of the home over time using Zillow's ZIP-level monthly home value indexes as in Di Maggio et al. (2017).[16] This imputation is subject to measurement error. Additionally, the LLMA data contain limited information on other loans collateralized by the same property (e.g., home equity loans). Measurement error poses unique challenges in a regression kink design, which I address in detail in Section 4.3.

The second variable I use from the LLMA database is a monthly flag for whether the mortgage borrower is currently in bankruptcy. In the US, about 1% of households file for bankruptcy each year. The filing rate is slightly lower for households in the LLMA database (0.72% file per year). This lower rate is not surprising as housing wealth should deter households from filing. Moreover, households able to buy a home are less likely to be subject to the financial distress that would make them more likely to file. While it is a limitation to only study homeowners, they are the right subset of the population on which to focus to assess the direct effects of changes in homestead exemptions on filing.

To my knowledge, this paper is the first to exploit the extensive information on bankruptcy available in the LLMA data.[17] One reason it is difficult to study the causes of household bankruptcy is that few household surveys record bankruptcies. An exception is the PSID, which reports detailed household balance sheets and bankruptcy filings during 1984-1995 (one well-known use is Fay, Hurst and White, 2002). While the rich information in the PSID is useful, the few bankruptcies (numbering in the low hundreds) limit the power of many empirical strategies.

[16] See appendix C.2 for more information on this procedure.
[17] The full sample over 2000 to 2016 documents 3.6 million filings.

The unit of observation in the LLMA database is a mortgage-month. I collapse this panel to a quarterly frequency. The average monthly filing rate is 0.007%, which makes bankruptcy relatively infrequent even in such a large dataset and could potentially lead to an underpowered statistical analysis. A quarterly frequency is preferable because it includes many more bankruptcies per period while still making it possible to measure a household's financial cost relative to the time when they're choosing whether to file bankruptcy. Throughout, I will also refer to the non-time dimension of the panel as "households" rather than "mortgages" as the focus of this study is the behavior of a household holding a given mortgage.[18]

Homestead Exemption Data

I construct a quarterly panel of states' homestead exemption levels by manually collecting this information from the original state statutes.[19] In some states, households have options to double their exemption if married and filing jointly and also to file using an alternative federal set of exemptions. I recorded these features of the state laws and exclude states where households can double, as this may threaten identification (discussed further below). The option to use federal exemptions is important for identifying the relevant exemption level as federal exemptions are more generous than some states' exemptions and can therefore be preferable to the state's exemption system.

Additional Variables

To shed light on what drives the small response of bankruptcy filings to generosity, I investigate which borrower characteristics are associated with greater sensitivity to the generosity of bankruptcy in a heterogeneity analysis. CoreLogic reports the household's FICO credit score and loan-to-value (LTV, the ratio of the home's value to its price) ratio at the time of origination. Low FICO scores reflect greater expected credit risk, which can limit credit access and make this score a useful proxy for how credit constrained a household it is. The LTV ratio is a useful proxy for household wealth as home equity reflects the net position on the largest asset and liability for most households.

To capture local economic conditions, I use data on unemployment, income, and house price growth.[20] At the county-level, I use quarterly data on unemployment rates and annual data on median incomes. To measure economic conditions at a more granular level, I also use annual ZIP-level measures of real median income and the fraction of households that receive unemployment benefits in a given year from the IRS Statistics of Income (SOI). From Zillow, I obtain monthly, ZIP-level data on house prices. While I use beginning of quarter house prices from the exact quarter

[18] It is possible that a household could appear twice in the data if, for example, they sold their home, paid off their mortgage, and purchased a new home with a new mortgage. It's not possible in the LLMA data to identify if a household reappears with a different mortgage.

[19] I identified the relevant statutes using Elias (2011).

[20] Throughout I deflate nominal variables using the CPI with a base year of 2010. For details on the data sources and the construction of these variables, see appendix C.4.

in consideration for computing home equity, the heterogeneity analysis uses annual growth rates to capture longer-term house price movements. The IRS data necessary to compute the measures here are only available beginning in 2005, but other covariates are available throughout the sample period (2000-2016).

Sample Restrictions

I impose several sample restrictions to enhance the internal validity of the research design. First, I restrict my main analysis to states where the homestead exemption does not vary with the marital status of the filer. Second, I only include households with positive home equity. Positive home equity households can be either above or below their state's cutoff. But negative equity households by definition cannot have positive seizable equity. A sharp change in the probability of having negative equity could violate the RKD identifying assumptions. Intuitively, we could conflate a kink in another characteristic that also affects filing (here, whether households have negative equity) with the kink in seizable equity. I therefore exclude households with negative equity from the sample to preserve internal validity. Third, I restrict attention to owner-occupied homes as investment properties *cannot* be protected in bankruptcy. I also keep only first mortgages as these capture the bulk of a household's mortgage debt.[21] Lastly I also draw a random sample of households for computational tractability. Appendix C.1 discusses in greater detail the motivation for these restrictions and more minor ones made based on data availability and to eliminate outliers.

3.2 Summary Statistics

Representativeness of CoreLogic's LLMA

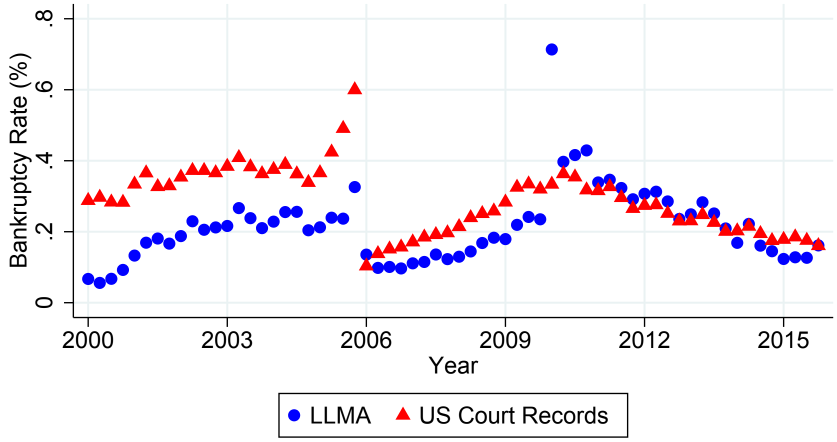
The LLMA data contain over a billion month-mortgage observations, drawing on data from around 45% of all residential mortgage originated in the US each year.[22] The sample is also geographically diverse. On average, in each year 99.97% of population-weighted counties are represented (98.97% by ZIP code).The average household has 48 months of data in the LLMA.

The 0.72% annual bankruptcy rate in the LLMA sample is lower than that of the total population (1.12%). But the filing rate in the LLMA sample closely tracks the national filing rate over time, as seen below in Figure 3. A lower rate for the LLMA sample is not surprising because these homeowners are on average wealthier and tend to have higher income compared to renters, making bankruptcy less attractive to them.

[21] I use first mortgages to identify households, but whenever information about other mortgages is available I count this debt towards the household's total mortgage balance.

[22] Coverage appears to decrease after 2011 as a result of the financial crisis negatively impacting a number of the mortgage services that previously provided data. Year-by-year coverage is depicted in Figures A.5 and A.6 in Section A.2 of the appendix.

Figure 3: Quarterly Household Bankruptcy Rates in the LLMA Data vs. the National Rate



Notes: This figures depicts the quarterly filing for households in the LLMA sample and for the total population. I compute the total population rate using quarterly Chapter 7 and Chapter 13 bankruptcy counts from the American Bankruptcy Institute (ABI) and Census data on the number of households in the US. The ABI's data source are the universe of US Bankruptcy Court records.

RKD Borrower Characteristics

The first column of Table 1 presents average characteristics for the sample used in the RKD. The next two columns split the sample based on whether the household is filing for bankruptcy, and the last two columns split it based on whether the household is currently above or below their state's exemption limit. The sample contains 176,384 bankruptcy filings and there are approximately 25% more households below the exemption limit than above.

Filers tend to live in places with slightly weaker economies and were more financially constrained when they originated their mortgage (lower FICO and higher LTV). Filers also live in states with slightly lower exemptions on average, consistent with creditors restricting unsecured lending to people in states with generous bankruptcy. The filing rate is nearly double for households below the exemption limit. But households below the limit also tend to live in areas doing slightly worse economically and were slightly more constrained when originating their mortgage. These patterns are not surprising. Lower house prices erode seizable home equity, and lower home equity can also depress consumption, income, and employment when it limits households' ability to borrow against their homes (Mian and Sufi, 2014; Adelino, Schoar and Severino, 2015; Mondragon, 2018). These patterns underscore why cross-sectional comparisons of filing versus seizable equity could be misleading about the direct effect of costly bankruptcy on a household's likelihood of filing. Low seizable home equity is correlated with economic conditions which could independently encourage more bankruptcy (higher unemployment, lower FICO scores, etc.).

Table 1: Means for Main Variables of Interest (RKD Analysis)

	(1)	(2)	(3)	(4)	(5)
		Filing for Bankruptcy?		Above or Below Exemption?	
	All	Filing	Non-filing	Below	Above
Bankruptcy					
File (%)	0.72	100	0	0.88	0.48
Never Filed (%)	99.91	91.84	99.93	99.92	99.91
Years Since Last Bankruptcy	2.71	0	9.37	2.11	3.74
Home Equity					
Home Equity	104.52	59.77	104.6	71.71	145.34
Equity Distance	-47.92	-85.53	-47.85	-156.34	86.94
State's Homestead Exemption	150.29	144.0	152.26	224.84	57.56
Borrower/Mortgage					
Mortgage Age (months)	40.62	51.82	40.60	37.74	44.20
Origination LTV	77.72	80.14	77.72	79.00	76.14
FICO at Origination	719.16	666.48	719.25	716.66	722.33
Local Economy					
Unemployment Rate	5.89	6.64	5.88	6.09	5.64
House Price Growth	1.90	0.70	1.90	1.38	2.53
Median Income	85.34	83.64	85.34	84.01	87.03
ln(Median Income)	11.35	11.33	11.35	11.33	11.36
Observations	99,233,172	176,384	99,056,788	55,009,070	44,224,102

Notes: This table presents means for the full sample and two different groups of subsamples. The groups are split based on whether the household is currently filing for bankruptcy and whether the household's home equity is above or below their state's exemption limit. All dollar amounts are reported in thousands of 2010 dollars. The data are quarterly but I annualize the bankruptcy rate. The "Years Since Last Bankruptcy" variable is the average across the subset of households that have filed for bankruptcy in the past. Equity distance is the difference in the household's home equity and their state's exemption limit. The unemployment rate and median income are measured at the county-level. House price growth is measured as 100$\times$ the difference in ZIP-level house prices over the previous year (i.e., from the beginning of $t-4$ to the end of $t-1$). Median income is also adjusted to reflect regional cost of living differences (the details of this adjustment are discussed in the main text). Additional summary statistics are available in Section B.1 of the appendix.

4 The Effect of Reduced Bankruptcy Generosity on Filings

In this section, I investigate how the financial cost of bankruptcy affects a household's decision to file for bankruptcy. To do so, I exploit the kink induced by homestead exemption laws in households' cost of bankruptcy as a function of their home equity. Using a regression kink design (RKD), I find that higher seizable home equity has a negative, but small, effect on a household's probability of filing. The RKD estimate is smaller than those found in earlier work using household-level data (Fay, Hurst and White, 2002) and has the opposite sign of the effect found in aggregate, cross-sectional comparisons (Mitman, 2016).

4.1 Identification Strategy

This subsection describes the sharp regression kink design (RKD) used to estimate the response of household filings to the generosity of bankruptcy. Quantifying the causal effect of bankruptcy generosity on households' bankruptcy filing is difficult for two main reasons: omitted variables and an exclusion restriction. With detailed balance sheet and income data, it would be possible to exactly quantify households' seizable assets (as in Fay, Hurst and White, 2002). But regressing filing decisions directly on a measure of this component of households' financial cost would likely be biased. This is because households that tend to accumulate few assets are more likely to file for bankruptcy for reasons other than having a low cost of bankruptcy (e.g., volatile income). This force would bias OLS estimates downwards.

Empirical strategies that take exemption laws as exogenous (e.g., Auclert et al., 2019; Mahoney, 2015) and instrument for households' cost of bankruptcy recover a net effect that includes local general equilibrium (GE) channels in addition to the direct response of households. Exemptions affect filing not only through their direct effect on a household's seizable equity, but also by altering the amount of debt they accumulate by making creditors less willing to lend.

While this GE effect may be of independent interest, it is not useful for assessing the relative importance of strategic vs. cash-flow motives in default. In fact, using this IV strategy yields a *positive* response of filing to costly bankruptcy (shown in Section 4.4.2). Contrasting this net effect with the small, *negative* behavioral response of filing estimated using the RKD, this suggests that the general equilibrium response of lending to generous bankruptcy is important.

Another advantage of the RKD relative to an instrumental variables (IV) identification strategy is that its identifying assumptions are much weaker. IV requires that the instrument is uncorrelated with other factors influencing bankruptcy filings. But the identifying assumptions of an RKD do *not* require that the running variable is exogenous with respect to the outcome. An RKD simply requires that other factors affecting filing do not kink at the exemption limit.

Next I describe the RKD, its identifying assumptions, and tests for failures of the key assumptions. I then discuss the non-standard identification challenges that measurement error in the running variable of an RKD (or RDD) creates. To address this issue, I present a new characterization of the RKD estimator's bias relative to the sharp RKD estimand in the presence of measurement error. This characterization also enables me to correct for this bias by exploiting a subset of the data for which there is plausibly no measurement error. Estimation results are presented in Section 4.4.

Regression Kink Design

To identify how changes in a household's cost of bankruptcy affect their likelihood of filing, I employ a regression kink (RK) design. Let $B \in \{0, 1\}$ indicate whether or not a household files for bankruptcy (where $B = 1$ denotes filing). The running variable is the difference between the household's home equity and their state's homestead exemption, which I'll refer to as "equity distance" and denote by $D \in \mathbb{R}$. We're interested in how changes in *seizable* home equity, defined as $S \equiv \max\{D, 0\}$, affect the household's probability of filing ($\mathbb{E}(B)$).

The regression kink design exploits the discontinuity in the slope of seizable equity S with respect to equity distance D to identify the treatment effect of a dollar increase in seizable equity on the household's probability of filing. The sharp RK estimand is

$$\tau = \frac{\lim_{D_0 \to 0^+} \beta(D_0) - \lim_{D_0 \to 0^-} \beta(D_0)}{\lim_{D_0 \to 0^+} S'(D_0) - \lim_{D_0 \to 0^-} S'(D_0)} \tag{1}$$

where $\beta(D_0)$ is the slope of the conditional probability of filing with respect to equity distance evaluated at the point where equity distance equals D_0, i.e.

$$\beta(D_0) = \left.\frac{d\mathbb{E}(B|D = \widetilde{D})}{d\widetilde{D}}\right|_{\widetilde{D}=D_0}. \tag{2}$$

Because the right and left limits of $S'(D)$ approach 1 and 0 as $D \to 0$, respectively, the denominator of τ is simply 1.

Under the assumptions of Card et al. (2015) (discussed more below), τ non-parametrically identifies the local average response of filing to changes in seizable equity:

$$\tau = \frac{\partial \mathbb{E}(B|D = 0)}{\partial S}. \tag{3}$$

Importantly, the parameter above is a partial derivative. It reflects purely the *direct* effect of changes in seizable equity on households' filing propensity. This means we identify the partial equilibrium, behavioral response of households, not an effect confounded by endogenous covariates nor general equilibrium effects. Intuitively, the RK estimand is able to isolate this direct effect by differencing out the other channels through which equity distance (D) affects filing (B). As long as these other effects are continuous at the cutoff $D = 0$, the difference in $\frac{d\mathbb{E}(B|D=\widetilde{D})}{d\widetilde{D}}$ above and below the cutoff is solely attributable to $\frac{\partial \mathbb{E}(B|D=\widetilde{D})}{\partial S} S'(\widetilde{D})$.

Following Card et al. (2015), I non-parametrically estimate the sharp RK parameter $\widehat{\tau}$ using local polynomials. My benchmark specification uses a local quadratic estimator, which is also standard in the RK literature because of its nice estimation properties.[23] My preferred specification uses a uniform kernel as in Card et al. (2015), but the results are similar when using a triangular or Epanechnikov kernel instead. I construct (approximation) bias-corrected robust confidence intervals and optimally select the estimation bandwidth using the MSE-minimizing procedures

[23]Similarly to a regression discontinuity design, it is desirable to use a polynomial order that is one degree higher than the "order" of the derivative of interest. In an RD, the object of interest is a difference in means (0^{th} order derivative) whereas in an RK it is the difference in slopes (1^{st} order derivative), and the preferred estimators are local linear and quadratic estimators (respectively). Even-ordered polynomials are preferable for an RK as moving from an odd-ordered polynomial to next highest even-ordered polynomial reduces bias but with no increase in estimator variability. Moving from an even-ordered to odd-ordered polynomial, however, will reduce bias as well but at the cost of increased estimator variability. This odd property is a result of the assumed symmetry of the kernel used in estimation (Fan, 1992). Because the bandwidth already controls model complexity, Fan and Gijbels (1996) recommend using the lowest even-ordered polynomial for an RK.

of Calonico et al. (2014).[24] Because the measure of home equity is likely subject to measurement error, I employ a new parametric estimator that corrects for bias due to measurement error. Section 4.3 presents this estimator, which I show is consistent for the true effect of interest under a set of stronger parametric assumptions than the setting of Card et al. (2015).

4.2 Internal Validity

The key identification assumption in an RKD is that the density of all other variables that affect the outcome (filing), conditional on the running variable (equity distance), is smooth at the kink. Smooth means that there is no discontinuity or kink in this density (Card et al., 2015). This assumption could fail if another characteristic that influenced filing was a kinked function of equity distance. In this case, the difference between the left and right slopes would reflect the effect of both seizable equity and the unobserved kinked factor.

An attractive feature of RKDs is that assuming other factors are not kinked functions of equity distance is much weaker than assuming that they are exogenous. For example, it would not be a problem for the RKD if house price decreases tend to lower both home equity (and thus equity distance and seizable equity) and increase unemployment, which also motivates households to file for bankruptcy. As long as the probability of being unemployed doesn't jump or kink at the point where equity distance is zero, this type of endogeneity does not violate the identifying assumptions. Moreover, the smooth density assumption gives rise to two testable predictions, making it possible to test for evidence of failure of this key assumption.

Other Factors Do Not Kink at the Cutoff

The first prediction is that pre-determined factors that affect filing are smooth (no jump and no kink) with respect to equity distance at the kink (Card et al., 2015). To jointly test this prediction for multiple covariates, I follow Ganong and Noel (2019) and Berger, Turner and Zwick (2019) and test for a jump and kink in households' predicted probability of filing. The predicted value is generated from a linear probability model that regresses a binary indicator for filing on a set of pre-determined covariates.

The household-level variables I include are the FICO score at origination of their mortgage in the sample, the origination loan-to-value (LTV) ratio, and dummy variables for the date of origination. At the ZIP-level I include log house price growth over the year prior to the current quarter. I also include county-time fixed effects to absorb all constant and time-varying county-level factors that affect filing.

[24] Conventional methods for confidence interval construction tend to over reject the null hypothesis of no treatment effect. Confidence intervals constructed in the style of Calonico et al. (2014) account for the additional uncertainty introduced when using an estimate of the approximation bias to correct for this bias. The method of Calonico et al. (2014) has close to nominal size, though lower power than conventional methods (Ganong and Jäger, 2018). I also present results for an alternative inference approach that uses the permutation test procedures of Ganong and Jäger (2018) in Section 4.5.

To test for a jump and kink in the predicted filing probability, I estimate an RDD and RKD using the predicted rate as the outcome variable.[25] The estimated level discontinuity (jump) and change in slopes (kink) are small and not statistically different from zero. This suggests these predetermined covariates are smooth through the cutoff. The point estimate for the jump implies that moving from below to above the cutoff reduces the predicted probability of filing by 0.15 percentage points (with a p-value of 0.47). The estimated difference in the right and left slopes at the cutoff is a decrease of 0.04 percentage points per $1,000 change in the cost of bankruptcy (with a p-value of 0.28). These results, as well as those computed using individual covariates, are displayed in Appendix Table D.1. In the appendix Figure D.1 depicts the smooth relationship between equity distance and the predicted versus actual filing rate; Figure D.2 and Table D.1 report results for individual covariates.

Smooth Running Variable Density

The second testable prediction focuses on the possibility that agents can manipulate their seizable equity around the cutoff. This could arise if households manipulate their home equity in order to remain below their state's homestead exemption limit. If the ability or desire to manipulate home equity is correlated with other characteristics that influence the decision to file, then the RK estimate ($\widehat{\tau}$) would be biased. To check for evidence of manipulation, we can investigate whether or not the empirical distribution of the running variable (equity distance) is smooth at the cutoff (McCrary, 2008). If households can manipulate their home equity under exemption limits, we would expect to see excess mass to the left of the cutoff and missing mass immediately to right. Whether due to manipulation or another force, a non-smooth distribution of equity distance indicates failure of the sharp RKD smooth density assumption (Card et al., 2015).

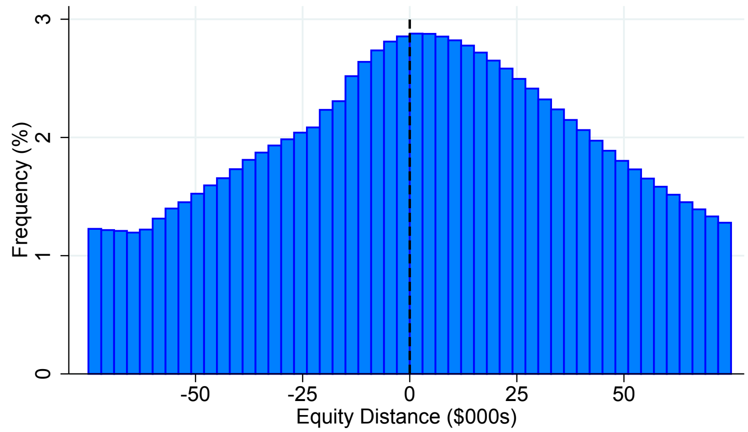
Visually inspecting this histogram, plotted below in Figure 4, the empirical distribution appears to be smooth at the cutoff. We can formally test for a discontinuity in the density by using a large number of bins to approximate the density function and by estimating an RDD using the empirical frequency as the outcome. This test yields an estimate of a 1.21% relative jump in the density at the cutoff with a p-value of 0.24.[26] With this, we fail to reject that there is no jump in the distribution of equity distance at the cutoff. Testing for a kink in the density yields an estimate of a relative slope change of -0.82% per $1,000 in equity distance, with a p-value of 0.003.While statistically significant, the measured kink is economically small. This suggests that if there is any bias related to this slightly kinked density, it is small.

There are a number of reasons that households are unlikely to manipulate their home equity. First, households have limited ability to control their house price directly. A homeowner could damage their property to lower its value, but this is a risky strategy as bankruptcy judges have wide discretion to dismiss cases on the basis of fraudulent attempts to lower one's bankruptcy

[25]I make the same estimation choices as in my preferred specification. This includes using a local quadratic estimator (local linear for the RDD) , a uniform kernel, linearly controlling for home equity, and an bandwidth optimally chosen using the method of Calonico et al. (2014).

[26]This percent is relative to the mean frequency among the bins.

Figure 4: Smoothness of the Empirical Distribution of Equity Distance



Notes: This graph plots a histogram of equity distance for the main sample, within $75k of the cutoff.

cost. A less extreme approach would be to reduce time and effort in upkeep, slowly letting a home depreciate. But this process would likely be too slow for a financially distressed household seriously considering bankruptcy.

Second, a household could lower their home equity by refraining from making mortgage payments. This is unlikely to be an attractive option. This would also slowly change home equity as mortgage payments are typically about $1000. This means it would take a household within thousands of dollars of their exemption limit several months to move under the cutoff, even with constant house prices. Moreover, persistent delinquency significantly increases the chances a mortgage lender will begin foreclosing on the property. If the goal of the household is to remain in their home by getting under the exemption limit, delinquency will in turn make this much harder to achieve.[27] A household could also attempt to lower their home equity by taking out an additional loan against their property, but a financially distressed borrower would have hard time qualifying for such a loan.

4.3 Measurement Error and Regression Kink Designs

The measure of home equity is likely subject to measurement error, and this type of measurement error poses unique challenges in a regression kink design. Ideally we would observe how each borrower's home would actually be valued in a bankruptcy court. While the process I use to impute the home price is conceptually similar to how courts actually value properties (using the recent sales prices of similar, nearby properties), it is unlikely that it perfectly measures the home value as a court would.

The dominant framework used in RKD/RDDs to address measurement error in the running

[27] One especially attractive feature of bankruptcy is the automatic stay, which can prevent foreclosure. The automatic stay stops creditors from *initiating* a foreclosure while the household is filing. But creditors that have already begun the foreclosure process can request (and are often permitted) to continue a foreclosure already in progress (Elias, 2011).

variable (home equity here) and the explanatory/policy variable (seizable equity), or a non-deterministic relationship between these two variables, is the fuzzy RKD/RDD (Card et al., 2015). This framework is a powerful and versatile solution to a noisy rule linking the running and policy variables. However, for the fuzzy RKD/RDD estimand to non-parametrically identify a local average response requires (1) a policy variable not directly computed from the mis-measured running variable and (2) an assumption of a point mass of perfectly measured running variable observations. This first assumption fails in this paper's setting as I do not have an independent measure of seizable equity (I can only compute it from the mis-measured home equity variable). This assumption can fail in other settings of interest to researchers, for example means-tested policies and mis-measured income. To my knowledge, this paper is the first to highlight the validity of this first assumption as a potential threat to identification for fuzzy RKD/RDDs. The second assumption is also strong for many settings. Moreover, under this second assumption the fuzzy RKD/RDD parameter identifies a local average response conditional on a observations both having zero measurement error and being at cutoff. But the sharp RKD/RDD identifies a response only conditional on the latter.

Both the sharp and fuzzy RKD/RDD estimand non-parametrically identify a local average response conditional on being at the cutoff, but a limitation of the fuzzy parameter is that it is *also* conditional on observations having zero measurement error.

To assess the bias created by this type of measurement error, I make additional parametric assumptions on the relationship between the outcome and both the running variable and policy variable. Specifically, I assume that the outcome is a quadratic function of the true values of the running and policy variables and unobserved factors additively affect filing.[28] I assume that measurement error is mean-zero and uncorrelated with the running variable and unobserved factors, but I allow for the running variable to be *correlated* with the unobserved factors affecting filing. Under these assumptions, I first show in Section E of the appendix that parameters of this model identify the same local average response identified by the sharp RK estimand of Card et al. (2015). I then propose a parametric least-squares estimator for the local average response and characterize its bias relative to the local average response. The parametric RKD estimator is

$$\hat{\tau}^{PRK} = \frac{\widehat{\beta}_1^+ - \widehat{\beta}_1^-}{S'(D)^+ - S'(D)^-} \tag{4}$$

where $\widehat{\beta}_1^+$ and $\widehat{\beta}_1^-$ are coefficients on the linear terms in the least squares problem

$$\min_{\{\beta_j^+\}} \left\{ \sum_{i=1}^{N^+} \left[B_i^+ - \sum_{j=0}^{2} \beta_j^+ (D_i^+)^j \right] \right\}^2, \qquad \min_{\{\beta_j^-\}} \left\{ \sum_{i=1}^{N^-} \left[B_i^- - \sum_{j=0}^{2} \beta_j^- (D_i^-)^j \right] \right\}^2. \tag{5}$$

The superscripts $+$ and $-$ denote observations with equity distance above and below the cutoff

[28] This seems to be a reasonable approximate for my setting as the plot of the kinked relationship between filings and equity distance appears well-approximated by quadratic functions (see Figure 5).

(zero) and $S'(D)^+ - S'(D)^-$ is the known change in the slope in the rule relating seizable equity to equity distance (which is equal to one in my application).

Under simplifying assumptions,[29] the probability limit of the parametric RKD estimator is

$$\widehat{\beta}_1^+ - \widehat{\beta}_1^- \xrightarrow{p} \left(1 - \frac{\sigma_\mu^2}{\sigma^2}\right)(1 - \pi^+ - \pi^-)(\beta_1^+ - \beta_1^-).$$

Above, $\widehat{\beta}_1^+ - \widehat{\beta}_1^-$ is the difference in the coefficients on the linear term in the estimated quadratic regressions (i.e., the true slope change at the kink), which identifies the local average response. The estimator is biased relative to the actual slope change. The first term reflects attenuation bias which scales down the magnitude of the estimator relative to the actual slope change, where σ^2 the variance of the mis-measured running variable and σ_μ^2 is the variance of the measurement error. But an additional source of bias arises from assigning observations to the "wrong" side of the cutoff. The variables $\pi^+, \pi^- \in (0,1)$ denote the probability an observation had a positive mis-measured value but actually had a negative value (and vice versa for π^-). This biases that estimator towards the opposite sign, and could even flip the sign if more observations were assigned to the wrong side than not.

If a researcher had a subset of the data with both the mis-measured and correctly measured running variables, they could correct for this bias using the following estimator:

$$\widehat{\tau}^{PRK-ME} = \frac{\widetilde{\beta}_1^+ - \widetilde{\beta}_1^-}{S'(D)^+ - S'(D)^-} \tag{6}$$

$$\widetilde{\beta}_1^+ - \widetilde{\beta}_1^- \equiv \left[\left(1 - \frac{\widehat{\sigma}_\mu^2}{\widehat{\sigma}^2}\right)(1 - \widehat{\pi^+} - \widehat{\pi^-})\right]^{-1} \left(\widehat{\beta}_1^+ - \widehat{\beta}_1^-\right) \xrightarrow{p} (\beta_1^+ - \beta_1^-)$$

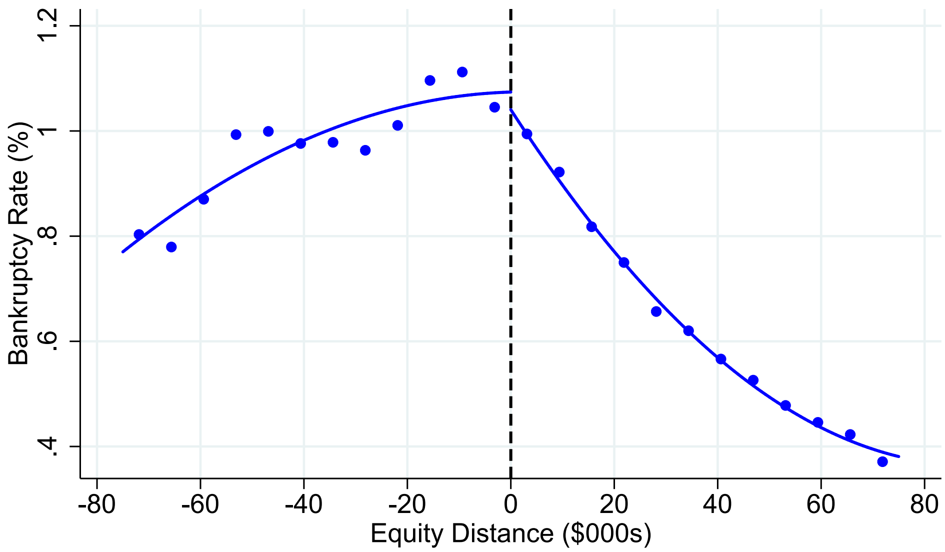
where $\widehat{\sigma}^2, \widehat{\sigma}_\mu^2, \widehat{\pi^+}$, and $\widehat{\pi^-}$ are simply the sample variance of the mis-measured running variable and measurement error and the sample averages of how often observations are assigned to the wrong side. In my application, I use a subsample of households selling the homes in the quarter of interest and treat the observed home sale price as a correctly measured observation. I can then compare it to my imputed home price to obtain the four extra estimators needed to correct for the biases induced by measurement error. I present estimates obtained using this measurement-error-adjusted estimator, alongside results from the standard local polynomial estimator, in the following section. Appendix Section E provides additional evidence on the quality of the imputation using the home sale subsample.

[29] This includes symmetry in distribution of the running variable as in Griliches and Ringstad (1970) and assuming that the covariance between the true and mis-measured running variable is the same regardless of their signs. I show the more general results, relaxing these assumptions, in Section E of the appendix.

4.4 Empirical Results

Figure 5 depicts a reduced-form version of the RKD by plotting the filing rate for households in equally-spaced equity distance bins around the cutoff. In the figure, when a household's home equity begins to exceed their state's exemption limit, the relationship between home equity and bankruptcy filing changes sharply. Above the cutoff, the effect of additional home equity becomes more negatively associated with their likelihood of filing for bankruptcy.

Figure 5: The Effect of Seizable Equity on Bankruptcy Filings



Notes: In this plot, the points are mean quarterly filing rates for equity distance bins. The lines are generated by fitting a quadratic polynomial to the individual observations on each side of the kink. The difference in the slopes evaluated at the kink corresponds to an RKD estimate. The benchmark specification differs in having its bandwidth optimally chosen, controlling for home equity, and by correcting for approximation bias as in Calonico et al. (2014).

The filing rate for households below their state's homestead exemption limit is higher on average than for households above the limit, but it is also increasing as households approach the cutoff from below. One might have expected this relationship to be flat below the cutoff as seizable equity is flat at $0 for households below their limit. However, other factors influencing filing, such as unsecured borrowing, are also correlated with equity distance. Households in states with generous bankruptcy exemptions tend to accumulate less unsecured debt and face higher borrowing costs (Pence, 2006; White, 2007; Severino and Brown, 2017). All else equal, households with less unsecured debt will have less to gain from filing for bankruptcy, making them less likely to file.

Another potential reason for the upward slope below the cutoff is that households may worry about their bankruptcy option no longer being "in the money" if they expect their home equity to continue to grow. As households get closer to the limit, their expected future costs of filing rises if they expect their home equity to rise (from house price appreciation or continuing to pay down their mortgage). This response would be part of the direct effect of equity distance on filing (i.e., $\frac{\partial \mathbb{E}(B)}{\partial D}$ as opposed to $\frac{d\mathbb{E}(B)}{dD}$) that is differenced out in the RKD estimation. The key assumption implying that this effect is differenced out is that the direct effect of equity distance on the filing rate does not kink at the cutoff. Intuitively, if households $1 below and above the cutoff have similar expectations about the future value of their home equity, then this assumption is unlikely to be violated.

Estimating the RKD, I find that a reduction in the generosity of bankruptcy has a small, negative effect on a household's probability of filing. According to the estimate under the preferred specification, a $1,000 increase in seizable equity implies a statistically significant 3.42% decrease in the annual filing rate relative to the sample average of 0.72%. The implied level decrease in the filing rate is 0.025 percentage points. Table 2 reports the estimation results under various specification choices. Column 1 reports results for the benchmark specification that uses a local quadratic estimator and linearly controls for home equity. The preferred specification (last column) uses the parametric measurement-error-adjusted estimator in equation (6) and makes the same polynomial order choices as the benchmark estimator. Correcting for measurement error results in approximately doubling the coefficient obtained under the benchmark specification.

Table 2: The Effect of Bankruptcy Costs on Filing (RKD Estimates)

	(1)	(2)	(3)	(4)	(5)
RK est. $\left(\widehat{\frac{\partial p}{\partial s}}\right)$	-1.64***	-1.43***	-1.48***	-2.16***	-3.42***
Standard Error	(0.21)	(0.27)	(0.28)	(0.33)	(0.44)
Bandwidth	67.07	49.56	89.33	44.16	67.07
Meas. Error Adj.					✓
RKD Poly. Order	2	2	3	2	2
Home equity control order	1	3	3	0	1
LHS Obs.	21,385,861	17,570,883	25,889,231	16,162,484	21,385,861
RHS Obs.	24,640,279	20,054,807	29,033,290	18,388,688	24,640,279

Notes: The estimates correspond to the percent change in filing rate, relative to the sample average of 0.72, in response to a $1,000 increase in seizable home equity. This table reports RKD estimation results for the main sample. Results for the benchmark specification are in column 1. The bandwidth is optimally chosen for each specification using the MSE-minimizing procedure of Calonico et al. (2014) and is displayed in thousands of 2010 dollars. Bias-corrected robust standard errors are computed as in Calonico et al. (2014) in each specification. The effective number of observations used to estimate the left and right-hand-side (LHS and RHS) slopes are displayed below the bandwidth. "RKD Order" refers to the polynomial order of the running variable used in estimation (i.e., the degree of the local polynomial). "Home Equity Control Order" refers to the polynomial order of the home equity control. The coefficients and standard errors are scaled by 1e5 (meaning that, for example, column 1 implies that a $10,000 increase in seizable equity reduces the probability of filing by 0.0291 percentage points.). Statistical significance: 0.1^{+}, 0.05*, 0.01**, and 0.01***.

Including higher-order polynomial terms for the running variable (equity distance) and home equity has little effect on the non-parametric estimates. Using home equity as a control also helps improve the power of the estimator (columns 1 versus 4). Intuitively, controlling for home equity means that the RKD compares households with similar levels of home equity but on different sides of the cutoff. This control could help account for a lot of the economic content in equity distance that affects household bankruptcy decisions, which should make it a powerful control for isolating the effect of interest. Results are similar when using a triangular or Epanechnikov kernel.[30]

4.4.1 Heterogeneity in Filing Sensitivity

Borrower Characteristics: Are some households more or less responsive to changes in bankruptcy's generosity? I analyze this question two different ways. First I split the sample based on whether or not households are above or below median on a chosen characteristic and separately estimate the RKD within these subsamples (Table B.6). Second, I re-estimate the RKD on the full sample with a parametric estimation procedure that allows me to interact the effect of generosity on filing with borrower characteristics (Table B.7).[31] Overall these analyses point towards financial and economic distress making household filing more sensitive to the generosity of bankruptcy.

The sample-splitting approach finds that mortgage origination characteristics that imply the household is more likely to be financially constrained increase their sensitivity to the generosity of debt relief received in bankruptcy. Households with above median loan-to-origination (LTV) values at origination are about 70% more sensitive to changes in the generosity of bankruptcy. The filing rates for those with below median FICO scores are nearly ten times more sensitive to a given change in debt relief generosity. A stronger response comes from a population either being (1) more likely to be near the brink of bankruptcy or (2) from the rule governing their bankruptcy decision being more sensitive to the exemption level. The former is likely true for higher-leverage (high LTV) and low-FICO households, as they will generally have a harder time accessing credit because of their perceived risk. This is consistent with credit constraints playing an important role in driving bankruptcy. The decision rule for financially constrained households could also be more responsive if bankruptcy enables them to increase their consumption relatively more (compared to less-constrained households). The model in Section 6 formalizes the intuition described here.

At the county-level, there does not appear to be a strong relationship between sensitivity to generosity and income or unemployment. However, at the ZIP-level, we see much starker differences. ZIP codes with median ZIP-level income below the sample median have almost an 80% stronger response to changes in generosity. Similarly to LTV and FICO, when households lack insurance against cash-flow shocks, low income could push the household closer to the threshold at which they prefer to file for bankruptcy. To the extent that these households have significant debt and fewer assets, a $1,000 change in seizable equity could be a larger percentage change in their cost of bankruptcy, which could make the rule governing their bankruptcy decision more sensitive to a given change in debt relief.

The interpretation for the ZIP-level fraction of households claiming unemployment insurance

[30] Available upon request.

[31] This second approach uses the parametric estimator defined in equation 4, estimating it in one equation by interacting the linear term of interest with an indicator for whether or not an observations is above the cutoff, as in Berger et al. (2019). I interact the term of interest (equity distance $\times \mathbf{1}$(above cutoff)) with the covariates of interest and include linear controls for these covariates.

(UI) is more subtle. While a high fraction of UI claims could reflect high unemployment, UI duration varies significantly across states and time, and high claim rates could reflect higher take-up in places with more generous UI. Filing rates are lower where sources of insurance such as Medicaid are more generous (Gross and Notowidigdo, 2011), and mortgage delinquency falls when UI becomes more generous (Hsu et al., 2018). When other sources of insurance are more generous, the utility gain from filing for a distressed household is weakly smaller, all else equal.[32] Therefore it may be that UI is more generous where claim rates are higher, and the filing in these ZIP codes is more responsive to marginal changes in generosity. Additionally, to qualify for UI an individual must have worked for a minimum amount of time before losing their job. ZIP codes where more households have struggled to hold a job could also have lower claim rates because their residents have had worse labor market outcomes. Implications from the second approach using interactions in estimation gives similar results for these variables associated with income, unemployment, and credit access.

I also find that households more likely to file for bankruptcy based on their observable characteristics are the most sensitive to changes in generosity. Splitting the sample based on their predicted probability of filing (the probability used earlier to check for smoothness of other factors related to bankruptcy) show that high-risk households are the most sensitive to changes in generosity. Interestingly, this differs from prior evidence on mortgage delinquency that found low-risk households were the *most* responsive to a change in the generosity of the debt relief they could obtain through delinquency (Mayer et al., 2014).

Composition Adjustment: An important limitation of the heterogeneity analysis is that we do not have exogenous variation in household characteristics. While we can establish that, for example, filing in lower income ZIP codes is more sensitive to the cost of bankruptcy, we cannot be sure that low income is *causing* this weaker sensitivity. Other factors correlated with high income could instead be the source of reduced sensitivity.

As a robustness check, I redo the subsample estimations and reweight the observations so that the subsamples match along a vector of observables (as in DiNardo, Fortin and Lemieux, 1996; Gross, Notowidigdo and Wang, 2018a). The results for the above analysis of borrower characteristics are little changed by this additional step.[33] To construct the weights, I first estimate a probit regression where the outcome is an indicator for membership in the "high" group. The explanatory variables are county-level median income and unemployment rates, origination FICO scores and LTV ratios, and annual house price growth.[34] This probit regression yields a *predicted* probability $\widehat{p}_i$ of being in the high group for each observations. The weight used for observation i in the

[32] This assumes a decreasing marginal utility in consumption.

[33] Available upon request.

[34] When one of these variables is defining the relevant partition, I omit it from the regression used to construct the weights.

partitioned RKD estimations are

$$w_i = \frac{\widehat{p}_i}{1-\widehat{p}_i} \times \left[\frac{\sum_i^N \mathbf{1}(i \text{ is in high group})/N}{1-\sum_i^N \mathbf{1}(i \text{ is in high group})/N} \right].$$

I also employ this composition adjustment below to analyze heterogeneity across time periods.

Time Series Heterogeneity: I examine how the sensitivity to generosity varies over time by splitting the sample into time periods and re-estimating the RKD within each sub-period. Table B.8 presents the results from two different ways of splitting the sample. First I partition it into three periods: pre-recession (2006 Q1 to 2007 Q4), recession (2008 Q1 to 2010 Q4), and post-recession (2010 Q1 to 2010 Q4). The point estimate in the recession is more than double than the estimate for the two periods before and after the recession. The difference between the recession sensitivity and the other periods is statistically significant (at the 5% and 10% levels, respectively for the pre and post-periods). After adjusting for the composition (including county-level income unemployment, ZIP-level house price growth, and origination FICO and LTV), the differences become smaller across periods and not statistically different from each other.

Overall this suggests that filing is more sensitive to generosity in downturns, mainly due to deterioration in local economic conditions and borrower characteristics. Greater sensitivity to generosity also implies that the cost of generous bankruptcy is higher in recessions, but the full welfare implications are unclear as the value of the insurance bankruptcy provides may also be larger in recessions. This result differs from studies of unemployment insurance, which find that unemployment durations are *less* responsive to benefit generosity in recessions (Kroft and Notowidigdo, 2016), suggesting that the social cost of generous UI is procyclical. This shows that the strength of the distortionary effects of insurance on household behavior can comove differently with the business cycle depending on the type of insurance.

I also split the sample into periods before, during, and after the 2005 bankruptcy reform (BAPCPA). BAPCPA was motivated by concerns over strategic filing, specifically wealthy households capable of repaying their debt but unwilling to do so. The reform increased filing fees for nearly all households, imposed residency requirements that prevented some households from using the more generous bankruptcy exemptions in their current state, and also imposed a means-test, barring households with income above their state's median from filing under Chapter 7 (they would instead have to file under Chapter 13). While the other aspects of the reform applied more broadly, the means-test targeted wealthier households.

I split the sample into: pre-BAPCPA (2000 Q1 to 2005 Q2), the rush-to-file (2005 Q3 to 2005 Q4), and post-BAPCPA (2006 Q1 to 2016 Q1) periods. In the last two quarters just before BAPCPA came into effect, there was a huge uptick in filings as some households rushed to file as bankruptcy became permanently less generous. The scale of the run-up can be seen in Figure A.1. Prior to the reform, a \$1,000 increase in generosity led to a 1.57% increase in filings, but in the rush-to-file the same $1,000 change led to a 7.58% rise in filings.[35] These imply 0.01 and 0.05 percentage point increases in a household's probability of filing in response to a $1,000 increase in generosity (respectively).

Does this large sensitivity during the rush-to-file tell us that in fact the strategic motive was strong? Not necessarily. In this period, many households anticipated a large and persistent increase in their cost of bankruptcy, eroding the option value of waiting to file. Households cannot receive another discharge in bankruptcy for typically several years after filing; when the value of this option falls the dynamic costs of bankruptcy in the present become smaller. As the model will later formalize, smaller dynamic costs means that the marginal filer is someone getting a smaller relative consumption gain when filing, so they are more sensitive to an additional dollar in bankruptcy and have a stronger strategic motive.

The sensitivity before and after the reform is similar (1.57 versus 1.43); after re-weighting to adjust the composition to be similar across years, the point estimate is relatively lower after the reform (3.11 versus 2.16), but we still cannot statistically reject that sensitivities are the same before and after. This suggests that BAPCPA did not result in the marginal filing becoming any less strategic in that they are similarly sensitive to changes in the generosity of bankruptcy. If the weak strategic motive on average is driven by the generosity of the homestead exemption being small in comparison to the other costs of bankruptcy (stigma or dynamic cost such as credit market exclusion), it is then not surprising that BAPCPA's reduction in generosity had a relatively small effect on household's sensitivity to seizable home equity. Moreover, BAPCPA targeted its cost increases towards high-income households, who we found earlier to be less sensitive to debt relief generosity. This suggests BAPCPA may have been misguided to the extent that it assumed wealthy households had a strong strategic motive. This low sensitivity of high-income households helps account for recent findings that BAPCPA did not change the income distribution of filers (Gross et al., 2018b).

4.4.2 Comparison with Other Estimates

The regression kink design estimates a smaller filing response to reduced bankruptcy generosity than prior research. Fay, Hurst and White (2002) was the first paper to directly examine the effect of the financial cost of bankruptcy on filings. The authors compute households' financial cost of bankruptcy from detailed balance sheet data in the PSID and state exemption laws. Using probit regressions, they estimate that a $1,000 decrease in the cost of bankruptcy raises the annual filing rate by 0.012 percentage points, which is smaller than my estimate of 0.029.[36] However, the filing rate in the PSID is far below the national average, typically by more than half.[37] In relative terms,

[35] After adjusting the compositions, the differences are 3.11% and 12.7%, respectively.

[36] I obtained this figure by inflation-adjusting the estimate reported in table 5 of Fay, Hurst and White (2002) using the CPI with a base year of 2010 (as in the RKD estimation). The authors' sample spans 1984-1995 and I use the average CPI-level during this period. Using the CPI from 1984 and 1995 implies annual filing rate increases of 0.010 and 0.015 percentage points in response to a $1,000 decrease in the cost of bankruptcy, which are 3.29% and 4.92% increases relative to their sample's annual filing rate of 0.30%, respectively.

[37] See table 1 in Fay, Hurst and White (2002).

Fay, Hurst and White (2002) estimate a 4.1% increase in the filing rate versus the 3.42% increase in filings implied by the measurement-error-adjusted estimator.

The most likely reasons for the differences in our estimates are omitted variables bias in Fay, Hurst and White (2002) and differences in our samples. Cross-sectional comparisons of filing rates between households with low versus high costs of bankruptcy can be misleading as factors such as higher unemployment risk or chronic medical conditions can influence both a household's decision to file and their wealth. Direct OLS/probit estimates may overstate the deterrent effect of high bankruptcy costs. Unobserved sources of financial distress will encourage filing and also lead the household to accumulate fewer assets, and therefore have a lower cost of bankruptcy.[38]

The findings of Fay, Hurst and White (2002) are also likely a lower bound, in terms of magnitude, for what one would obtain with a similar research design and a more representative sample. The heterogeneity analysis illustrated that characteristics associated with lower filing rates were also strongly associated with lower filing sensitivity to bankruptcy costs. This means that the PSID sample may be skewed towards households with a lower than average sensitivity.

Another source of bias arises from the effects of bankruptcy generosity on filing through reductions in the supply of unsecured credit (Gropp, Scholz and White, 1997; Pence, 2006; White, 2007; Mitman, 2016; Severino and Brown, 2017). When bankruptcy generosity makes filing more tempting, lenders will require a higher interest rate to break even or may ration credit more strictly, discouraging borrowing. With less unsecured debt, households will have less to gain from filing for bankruptcy. This GE channel should bias OLS/probit estimates upwards relative to the direct response of households (estimated in the RKD); it also should bias upwards IV estimators that use exemption levels as an instrument for households' cost of bankruptcy. Although exemption levels are arguably exogenous in the sense of no reverse causality or being caused by factors that influence filing, the bias stems from failure of the exclusion restriction to hold.[39]

I apply these alternative estimation approaches to the LLMA data (see Table 3). Using OLS to estimate a regression of the filing indicator on seizable equity with no additional controls or fixed effects yields a negative coefficient relatively similar to the RKD estimate. This estimate should be subject to both upward and downward bias. Adding in state-time fixed effects removes any state-level effects on filing, including the state exemption level. The coefficient grows in magnitude when adding state-time fixed effects, as one would expect if the creditor supply response was biasing the coefficient upwards.

To better isolate the creditor response channel, I regress the household-level filing indicator on the state exemption level. This approach implies that a \$1,000 increase in exemption generosity *reduces* filings 0.62%. Similarly, instrumenting for seizable equity with the homestead exemption

[38] For example, suppose that household bankruptcy is 100% driven by liquidity shocks (e.g., job loss or health expenditures) and that the cost of bankruptcy has no direct influence on filing. We would still find a negative correlation between filing and the household's financial cost as the households that experience larger and more frequent shocks will be more apt to file and also accumulate fewer assets.

[39] Bankruptcy exemption generosity appears to largely be a function of idiosyncratic historical events in the 19$^{\text{th}}$ century (Skeel, 2001; Hynes et al., 2004; Mahoney, 2015; Auclert et al., 2019). Most updates in recent decades to the homestead exemptions have been to keep pace with inflation.

Table 3: RKD, OLS, and IV Estimates of Filing Response to Costly Bankruptcy

	(1) RKD	(2) OLS	(3) OLS	(4) OLS	(5) IV
Seizable Equity	-1.64***	-1.42***	-1.94***		7.59***
	(0.21)	(0.19)	(0.19)		(0.75)
Home. Exemp.				-0.62***	
				(0.06)	
Stage 1 F-Stat.					76.61
Observations	46,026,140	46,026,146	46,026,146	46,026,146	46,026,146
State x Time FE			✓		
Time FE			✓		✓

Notes: This table depicts the results of several approaches to estimating the effect of costly bankruptcy on household filing. Column 1 reproduces the regression kink design (RKD) estimate. The RKD estimate captures the direct effect of bankruptcy generosity on household filing probabilities. Columns 2-3 report OLS estimation results from regressing a filing indicator on the household's seizable equity. Column 3 adds state-time fixed effects to remove the effect of exemption levels on filing. Column 4 gives the OLS estimate from regressing the filing indicator on the household's state exemption limit. Taking exemption level as exogenous, columns 4's estimate is the total derivative counterpart to the partial derivative estimated using the RKD (i.e., $\frac{dP(\text{file})}{d\text{exemption}}$). Lastly, column 5 gives the results from the two-stage least squares estimation in which I instrument for the household's seizable equity using the exemption limit. In columns 2-5 I cluster standard errors at the county level. Statistical significance: 0.1^{+}, 0.05*, 0.01**, and 0.01***.

also yields estimates with signs opposite from the RKD. This suggests that, in general equilibrium, the negative credit supply response more than offsets the direct effect on household filing incentives. This relatively strong reaction, in contrast to the weak strategic response of households, helps explain recent findings that the 2005 bankruptcy reform raised borrowing costs but did not change the characteristics of the people filing for bankruptcy (Gross et al., 2018b).

4.5 Additional Robustness

Permutation Test

An alternative approach to inference in an RKD is through a permutation test (Ganong and Jäger, 2018). To perform this test, I repeatedly sample placebo exemption limits from the empirical distribution of limits (e.g., assigning Wisconsin's history of limits to Washington). For each draw, I compute every household's distance to the placebo exemption and re-run the RKD estimation using these "false" measures of equity distance.[40] Intuitively, the test compares the extremeness of the actual RKD estimate to those computed for the placebo data.

Formally, the permutation test assesses the null hypothesis of no treatment effect (i.e., seizable equity does not affect bankruptcy filings). A key assumption in this test is that the actual exemption

[40] In the estimation on the placebo samples, I make the same choices as in the main analysis (uniform kernel, quadratic specification, linearly controlling for home equity, and choosing the bandwidth as in Calonico et al., 2014). For comparability, I use the standard non-parametric RK estimator.

limits are drawn from a known distribution of placebo exemption limits.[41] The main advantage of the permutation test, relative to other approaches, is that it has exact size in finite sample. Simulations in Ganong and Jäger (2018) document the downside of the permutation test, which is that it is underpowered relative to inference done with the bias-corrected confidence intervals of Calonico et al. (2014). An advantage of both the methods of Ganong and Jäger (2018) and Calonico et al. (2014) is that they perform better than standard asymptotic inference in distinguishing kinks from highly nonlinear relationships between the outcome and running variable (Ganong and Jäger, 2018).

I run this test using 1,000 random draws of exemption regimes. Figure D.3 displays the distribution of coefficients and t-statistics obtained for the placebo samples. The dashed line indicates the actual RKD coefficient and t-statistic, which are both relatively extreme compared to their placebo counterparts. The p-value from this test is 0.098, which rejects the null hypothesis of no effect at the 10% level.[42]

5 The Effect of Debt Payment Reductions on Bankruptcy Filings

This section examines the effect of mortgage payment reductions on household bankruptcy filings to learn about the role of cash-flow shocks in driving filing. I exploit rules governing interest rate resets for adjustable-rate mortgages (ARMs) in an instrumental variables strategy as a source of exogenous variation in the size of payment reductions. One challenge in interpreting the results is that payment reductions can affect filing decisions through not only their immediate impact on cash-flows, but also through expectations of lower payments in the future (a wealth effect). To aide in interpretation and help relate the estimate of the effect of cash-flows to the RKD estimate of the effect of generosity, I estimate the expected net present value (NPV) of payment reductions for a given payment change. I find that filings are very sensitive to changes in cash-flows, much more so than the generosity of debt relief.

5.1 Empirical Strategy: Exploiting ARM Index Rates

A key challenge in learning about the causal effect of cash-flows on bankruptcy filings is endogeneity. For debt payments in particular, a large mortgage payment could indicate that a household was financially sound enough to qualify for a large mortgage. To the extent that better financial

[41] I adapt the permutation test of Ganong and Jäger (2018) to a multiple-kink setting by recasting the assumptions about the kink distribution in terms of a multivariate distribution of state-specific exemption histories. In my application of this test, I randomly sample an entire *history* of exemption limits from one state and reassign it to another. I randomize this way, instead of assigning a potentially different exemption every year, to maintain the empirically observable persistence in exemption limits over time. A second important choice is that this procedure assigns a placebo limit for *each* state, rather than assigning a common placebo exemption limit across all states. One could apply the procedure of Ganong and Jäger (2018) treating zero as the kink after normalizing the running variable to be in terms of distance from the cutoff. But this assumes perfectly correlated shifts in exemption limits across draws, which is a restrictive assumption about the distribution of exemption limits.

[42] This an exact test, so I compute the two-sided p-value as double the percent of estimates at least as extreme (in my case, below) the actual exemption limit.

health makes these households less likely to file, cross-sectional comparisons would understate the causal effect of debt payment size on filing.

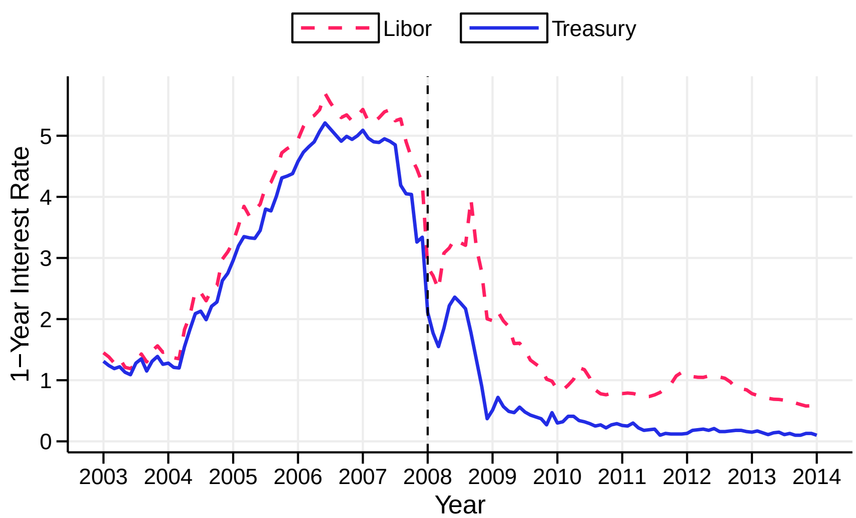
The empirical strategy adopted here uses a subset of the CoreLogic data to study the filing behavior of households with ARMs and uses plausibly exogenous variation in the size of their mortgage payment reductions. ARMs have a fixed interest rate for an initial period after origination and then reset to a new rate at a pre-specified date. The rule determining the new interest rate is also specified at origination, but inputs to this rule depend on the value of a chosen "index rate" at the time of the reset. The new rate is the sum of a "margin" (selected at origination) and the current value of the index rate:

$$\text{new interest rate} = \text{margin} + \text{current value of index rate.}$$

The margin is typically 2-2.5% and popular choices for index rates during 2003-2008 (the period I study) were the one-year Libor and Treasury rates.

During 2003-2008, the Libor and Treasury rates had a nearly constant spread of about 25-50 basis points (see Figure 6). But beginning in 2008, an unprecedented, large spread opened up between the two rates. The spread in this unusual period resulted in a natural experiment in which otherwise similar mortgages received different payment reductions when resetting. The differences in payments were substantial. At the spread's 218 basis point peak in September 2008, the average resetting ARM paid $4,634 more over the next year if indexed to Libor instead of the Treasury rate.

Figure 6: One-Year Libor and Treasury Rates



Notes: This graph plots one-year Libor and Treasury rates at a monthly frequency. Source: FRED.

I exploit this natural experiment in an instrumental variables strategy. The parameter of interest is the coefficient β in the second stage equation:

$$B_{ict} = \beta \text{MPay}_{ic} + \alpha_c + \alpha_t + \gamma X_{ict} + \epsilon_{ict}$$

where $B_{ict} = 1$ if household i in county c files for bankruptcy in month t. Each household has twelve observations on filing behavior for the twelve months including and following the reset.[43] The covariate of interest is MPay_{ic}, which is the component of the new mortgage payment determined by the index rate (as opposed to the margin) upon reset. The baseline specification includes county and time time fixed effects and a vector of borrower-level controls (including origination characteristics and contemporaneous variables). To estimate β, I instrument for MPay_{ic} using the value of the index rate (Libor or Treasury) at the time of the reset. The first-stage equation is

$$\text{MPay}_{ic} = \pi \text{IndexRate}_{ic} + \omega_c + \omega_t + \zeta X_{ict} + \eta_{ict} \tag{7}$$

where IndexRate_{ic} is the value of the index rate for household i's mortgage at the time of its first reset. This instrument is a household-specific variable and does not vary over time within households. I therefore cluster by county to allow for correlation in omitted factors not only within households, but within regions as well.

The baseline specification includes county and time fixed effects. Time fixed effects are important to include as they absorb macro-level factors that also influence filing. This means that the IV estimator identifies the effect of payments on filing off of variation between the index rates *within* a given time period. The controls include characteristics from the time of origination such as the margin on the loan, the original payment level, the borrower's FICO score, and the origination loan-to-value (LTV) ratio. The time-varying controls are the log mortgage balance and log home equity in time t.[44] Controlling for home equity is important as it can affect filing through seizable equity. Additionally, interest rate reductions can have a small effect on the rate at which the mortgage balance is paid off over the next year.[45] Including this control helps to separate the direct effect of payment reductions on filing from any indirect effects through changes in seizable home equity and the generosity of the debt relief the household would receive in bankruptcy.

Identifying Variation: The key identifying assumption is an exclusion restriction: the index rate only affects bankruptcy filing through changes in the household's mortgage payment level. To assess this assumption, it is helpful to consider what drives variation in the index rate choice. Mortgage lenders have persistent relationships with mortgage-backed securities (MBS) investors that purchase their mortgages, and purchasers differ in the denomination of their cost of funds (Libor or Treasury). Investors generally preferred to purchase MBS with a payment structure matching that of the denomination of their cost of funds. In fact, Gupta (2018) shows that lender identity explains over 50% of the variation in index rate choice.

[43] My preferred specification uses twelve months of data for each household to boost statistical power. I do so by including time-varying controls that can help better account for other sources variation in mortgage payments and bankruptcy filings. My results are robust to using flattened version of the panel that only uses that annual filing rate as the outcome (available upon request).

[44] Because home equity can be negative, the control I use is $\text{Sign(Home Equity)}_{ict}) \times \ln(|\text{Home Equity}_{ict}|)$.

[45] For the many mortgages in the sample that are "interest-only" during the first reset, this is not an issue. But for households making both principal and interest payments, a rate decrease can induce slight variation in the rate at which the mortgage is balance is paid down over the next year (typically on the order of hundreds of dollars per year).

Prior to 2008, the spread was fairly constant and the difference in ARM margins for new originations was close in size to the typical Libor-Treasury spread (25-50 basis points). This suggests borrowers and lenders during 2003-2008 did not anticipate the upcoming widening spread and would not have a had a strong reason to prefer Libor indexation. Looking for systematic difference in Libor versus Treasury loans originated in this time, the main difference is that Libor loans tended to have a larger mortgage balances and slightly. This makes it important to control for the balance size in the IV estimation. But other mortgage characteristics and local economic conditions (county unemployment, income, etc.) are very similar for Libor and Treasury-indexed mortgages.[46] It does not appear that households systematically selected into Libor-indexed ARMs.

Why did the spread between the Libor and Treasury rates emerge? The Libor rate is calculated from daily self-reports from the largest global banks of their expected borrowing costs on the interbank market. This makes Libor much more closely linked to distress in the interbank lending market than the Treasury rate. During the financial crisis Libor rose as interbank lending dried up; in September 2008, then Governor of the Bank of England Mervyn King described Libor as "the rate at which banks do not lend to each other."

Sample Restrictions: I restrict the CoreLogic sample to ARMs originated during 2003-2008. Truncating the sample to ARMs originated prior to the widening of the spread in 2008 is preferable to preserve internal validity as there would have been little incentive for households of a particular type to select into Libor-indexed ARMs. Another reason to prefer using ARMs resetting post-2008 data is that most households received rate decreases, whereas in normal economic times ARMs typically reset from a low teaser rate to a *higher* new rate. I restrict the sample to only include resets with rate decreases (which are common post-2008) because increases create an incentive to refinance which can introduce selection bias.[47] Due to limited data availability, I begin the sample in 2003. This also ensures that the ARMs in the sample reset in 2008 or later as the time until the first reset is either five, seven, or ten years (comprising 77%, 22%, and 1% of the sample, respectively). All ARMs are indexed to a one-year interest rate (either Libor or Treasury) and reset at annual frequency. I drop ARMs missing information on their reset rule, which leaves a sample of nearly 100,000 ARMs with over one million monthly observations. Tables B.9 and B.10 in the appendix present summary statistics for these loans, splitting the sample based on the index rate (Libor or Treasury).

Related Approaches: This research design is related to that of Fuster and Willen (2017) and Di Maggio et al. (2017), which study the effects of ARM resets on mortgage delinquency and consumption in event-study and difference-in-difference frameworks. These papers exploit variation in the

[46]See Tables B.9 and B.9 in the appendix for summary statistics and Table D.4 for a regression of an indicator for Libor indexation on mortgage and regional characteristics.

[47]As discussed in Fuster and Willen (2017), financially constrained households (e.g., unemployed or with negative equity) will have a harder time qualifying for a refinance. The leads the pool of non-refinancing borrowers to "worsen" in terms of financial health given a higher rate increase. This selection bias would lead us to overstate the effect of high rates/payments on default.

timing of household's resets; an advantage of the IV approach here is that it exploits *within*-period variation in payment reduction sizes. The research design here is most similar to that of Gupta (2018), which also uses ARM index rate choice and other contract features as sources of variation in interest rate reductions. A key difference is that the explanatory variable I am interested in is the size of a cash-flow change rather than the current interest rate. This presents additional challenges because the size of the mortgage payment, even the component directly determined by the index rate choice, is endogenous with filing as larger payments are associated with larger mortgages. Moreover, separating the pure cash-flow effect from wealth effects due to changes in expectations over permanent income also creates an additional challenge. The approach here illustrates how to adapt strategies in the style of Gupta (2018) to study the effect of cash-flow/income shocks.

5.2 Estimation Results

Table 4 reports the IV estimation results. The estimate under my preferred specification (column 4) implies that a \$1,000 decrease in annual mortgage payments leads to a statistically significant 30% drop in the annual bankruptcy filing rate. This corresponds to a 0.28 percentage point decrease in the probability of filing over the next year relative to the sample's average filing rate. The first-stage estimate in column 4 implies that a one percentage point lower index rate at the time of reset on average leads to a statistically significant \$1,397 reduction in annual mortgage payments. The first-stage F-statistics for the excluded instrument are consistently above ten, rejecting that the instrument is weak.

Augmenting the baseline specification to include additional fixed effects has little effect on the IV estimate. Column 1 reports the baseline and columns 2 adds loan age fixed effects. Households are likely in relatively good financial health at the time they obtain their mortgage, making them less likely to file for bankruptcy early in the life of the mortgage. The loan age fixed effect helps control for how the risk of bankruptcy grows over time. Column 3 allows for a loan-age specific time trend. This means the IV estimate implicitly compares mortgages both originated and resetting at the same time but resetting to different payment levels as a result of the Libor-Treasury spread. My preferred specification in column 4 adds a county-specific time trend, which absorbs time-varying regional factors that also influence bankruptcy.

The first and second stage estimates are robust to a number of changes in the econometric specification.[48] This includes taking the log of the mortgage payment component determined by the index rate, using a binary indicator for Libor indexation as the instrument, and collapsing the data to a longitudinal panel with the annual bankruptcy rate as the outcome (rather than using each month's observation). Using the entire new mortgage payment, as opposed to just the component determined by the index rate also yields similarly point estimates, but the first stage is weaker.[49]

[48] Available by request.

[49] A weaker first stage is not surprising as this effectively introduces additional noise into the first stage.

Table 4: IV Estimates of Filing Response to Liquidity

	(1)	(2)	(3)	(4)
	Panel A: Second Stage (outcome = bankruptcy)			
MPay_{ic}	30.72***	27.49***	33.49***	29.98***
	(7.36)	(7.64)	(8.48)	(8.71)
	Panel B: First Stage (outcome = $MPay_{ic}$)			
Index Rate_{ict}	1,275***	1,253***	1,384***	1,397***
	(105.97)	(110.08)	(126.52)	(133.54)
Stage 1 F-Stat.	20.69	18.50	17.11	15.63
Observations	1,092,072	1,092,072	1,092,072	1,092,072
Time FE	✓	✓	✓	✓
County FE	✓	✓	✓	✓
Loan Age FE		✓	✓	✓
Loan Age x Time FE			✓	✓
County x Time FE				✓

Notes: I scale and normalize the coefficients and standard errors by the annual filing rate in the second stage so that they correspond to the percent change in the filing rate as a result of a $1,000 increase in annual mortgage payments. The units for the first stage coefficient give the dollar change in the mortgage payment following a one percentage point change in the value of the index rate. Standard errors are clustered by county. Each regression includes, as controls, origination characteristics (the ARM's margin, the original payment level, the borrower's FICO score and LTV at origination) and time-varying characteristics (log mortgage balance and log home equity). The estimated coefficients on these controls are omitted for brevity. Statistical significance: 0.05*, 0.01**, 0.001***.

Limitations: This analysis is subject to several limitations. The ideal experiment to isolate the effect of cash-flows on bankruptcy, net of any strategic motives, would randomly vary cash-flows not seizable in bankruptcy. The main limitation is that the mortgage payment reduction may affect filing by changing both current cash-flows and expectations over future mortgage payments. This second effect through expectations is essentially a wealth effect coming from a change in expected permanent income. A related limitation affecting the expected net present value (NPV) of future cash-flows is that households may not "receive" this cash-flow shock if they become delinquent on their mortgage or prepay (following a sale or refinancing).

To address this limitation, in the next section I combine estimates of the expected NPV of a payments and the ARM IV estimate, along with additional assumptions on household behavior, to isolate the filing response to a one-time change in current cash-flows. Deviations from the additional assumptions used in this NPV calculation generally imply the recovered estimate is a lower bound for the true causal effect of changes in current cash-flows on filing.

An additional limitation is that there are cases in which mortgage payment reductions are seizable in bankruptcy. This issue only arises in Chapter 13 bankruptcy cases, which constitutes typically 30% of household bankruptcy filings each year. Statute requires under Chapter 13 that creditors receive the maximum of disposable income over three to five years or current seizable assets.[50] When this payment reduction is seizable, it disincentivizes bankruptcy both through a cash-flow motive by allowing the household increase consumption outside of bankruptcy and by increasing the cost of bankruptcy, as the household must now give up more resources when filing. Intuitively, this should cause the IV estimate here to overstate the pure cash-flow motive.

Section F.2 of the appendix describes the case in which payment reductions are seizable and uses the model of Section 6 to decompose the filing response to a change in *seizable* resources into the responses to changes in non-seizable resources and the generosity of debt relief in bankruptcy. This decomposition is similar to that of Chetty (2008), which splits the effect of increased benefits on unemployment duration into a "moral hazard" and "liquidity" effects. The implications from this decomposition do not alter the main conclusions of a relatively larger response to *non-seizable* cash-flows (see Section F.2 in the appendix for details).

5.3 Interpretation: Cash-Flow versus Wealth Shocks

A reduction in mortgage payments for ARM borrowers may affect filing through both its impact on the current year's payments and expectations over future payments. In this section I combine additional data from CoreLogic on borrower delinquency and prepayment to estimate the expected NPV of these cash-flows under various assumptions over household discount rates and expectations. With this information I can scale the IV estimate to recast it in terms of the filing response given a change in the NPV of cash-flows. Assuming that filing responds the same to a $1 increase cash today as an increase in the future worth $1 in NPV, we can infer the effect on filing of a one-time change in current payments.

Consider a household with a 30-year ARM whose annual mortgage interest payment resets to M_τ in month τ. Suppose also that the household discounts their year $\tau + j$ expected future mortgage payments $\mathbb{E}_\tau(M_{\tau+j})$ at rate $1 + r$. The τ subscript on the expectation indicates that the expectation is conditional on the household's information in month τ. For ease of exposition, assume that the ARM's margin is zero so that the interest payment is entirely determined by the index rate.[51] Let s_t denote the survival rate of the mortgage, i.e. the probability the household does not prepay and exit the mortgage early in period t conditional on remaining in the mortgage in months prior to t. Denote the delinquency rate in month t, conditional on not prepaying in t, by δ_t.

[50] Note also that households with income above their state's median are *only* allowed to file under Chapter 13. Therefore within would-be Chapter 13 filers, this case only occurs for those with income above their state's median but with few enough seizable assets compared to their disposable income so that creditors would receive more from five years of their estimated disposable income (income net of "necessary" expenditures including mortgage payments) than from the value of their seizable assets.

[51] If households understand that the margin component of payments is predetermined, then we could simply relabel the variables here to be the mortgage payment component determined by the index rate.

The (expected) NPV of the interest component of the mortgage payment is

$$M_\tau^{NPV} = \underbrace{s_\tau(1-\delta_\tau)M_\tau}_{\text{current payment}} + \underbrace{\sum_{j=1}^{360-\tau} s_{\tau+j}(1-\delta_{\tau+j})\frac{\mathbb{E}_\tau(M_{\tau+j})]}{1+r}}_{\text{future payments}}.$$

To compute a household's NPV of interest payments, we would also need to know their survival rates, delinquency rates, expected future payments, and delinquency rates. In what follows, I estimate the survival and delinquency rates using the CoreLogic data (which reports these events) and estimate the NPV under various assumptions over discount rates.

In the benchmark estimation, I assume that households believe their mortgage payments are a martingale. By definition, this implies that they expect their future payments to equal their current payment: $\mathbb{E}_\tau(M_{\tau+j}) = M_\tau$ for $j \in 1, ..., 30-\tau$. Second, for now I also assume that households discount their mortgage payments at the average monthly market interest rate, which is consistent with the findings of Busse et al. (2013) for auto loans. The average annual rate on 30-year fixed rate mortgages during the sample period is 4.39%. Under these assumptions, we can rewrite the NPV simply as

$$M_\tau^{NPV} = M_\tau \underbrace{\sum_{j=0}^{30-\tau} \frac{s_{\tau+j}(1-\delta_{\tau+j})}{(1+r)^j}}_{\equiv\theta}. \tag{8}$$

The expected NPV of mortgage payments is proportional to the new payment level, with a constant of proportionality θ.

With this framework, I impute the NPV for each household by estimating $\widehat{\theta}$. For δ_t, I use the average monthly average delinquency in the post-reset years of 1.63% in the CoreLogic sample. I parametrically estimate the survival rates s_t for $t \in \{1, 300\}$ assuming that survival rate follows a Weibull distribution. Combing these estimates using the formula in (8) yields $\theta = 74.68$. This implies that a \$1 increase in the current month's payment implies a present value expected payment increase of \$74.68 over the current and future months. Because the ARM IV estimate is in terms of the annual mortgage payment increase (i.e., $12 \times M_\tau$), the appropriate scaling factor for the IV estimate is $\widehat{\theta}/12 = 6.22$. We can scale the IV estimate by $\widehat{\theta}/12$ to recast its interpretation in terms of the filing response to a change in the NPV of expected mortgage payments. Taking $\widehat{\theta}/12 = 6.22$, the IV estimate implies that a \$1,000 decrease in the expected NPV of mortgage payments lead to a 4.82% decrease in the annual bankruptcy filing rate, a 0.045 percentage point decrease in the probability of filing.

Sensitivity Analysis: How do deviations from the above assumptions affect the implied effect on filings of a \$1,000 increase in the NPV of annual mortgage payments? In general, they imply that the filing response to a \$1,000 increase in the current year's cash-flows will be even higher than the estimated 4.82%. First, if households are more impatient than is implied by the average mortgage payment, then they value future cash-flows less and their expected NPV of mortgages payments will be lower. Table 5 reports the implied effects on the filing rate of a $1,000 increase in the NPV of expected cash-flows. The change in the implied filing rate is not extremely large. Discounting at 10% annually implies a 0.057 percentage point decrease in filings per $1,000 increase in cash-flows.

Table 5: Sensitivity Analysis: Household Discount Rates

Annual Discount Rate:	0.0439	0.05	0.075	0.1
%Δ filing per $1k:	4.82	4.96	5.53	6.12
Δ % filing per $1k:	0.045	0.046	0.051	0.057

Notes: This table uses the IV estimate under the preferred specification from Table 4 and the estimated scaling factor under various assumed discount rates.

Another deviation worth considering is a different process for expectations. Suppose instead households expect interest rates to mean-revert to higher levels in next several years. They would then expect the current reduction in their mortgage payments to be more short-lived, implying a lower NPV of the increase in cash-flows implied by the above assumptions. This reduces the term by which would scale the IV estimate and therefore implies a greater reduction in the filing than 0.045 as the current mortgage payment reduction embodies a smaller *reduction* in the NPV of future payments.

Another important assumption for interpreting the scaled IV estimate as the effect of a change in current cash-flows is that filing responds the same to a $1 increase in current cash as an NPV-equivalent increase in future cash. In reality, credit market frictions such as borrowing constraints likely limit the household's ability to borrow against this future income increase. If cash in the present has additional value due to its liquidity, then bankruptcy filings will be more sensitive to the increase in present cash. The IV estimate would then recover an weighted average response of the stronger effect of current cash and the weaker effect of future cash, biasing the estimate downward relative to the effect of current cash.

Comparing the Strategic and Cash-Flow Motive: The NPV-adjusted estimate is more meaningfully comparable with the RKD estimate of Section 4.4. The RKD estimates imply a $1,000 reduction in the generosity of debt relief households receive in bankruptcy leads to a 1.64-3.42% fall in the bankruptcy rate. The implied effect of a $1000 increase in cash in the same year, cash which is not generally seizable in bankruptcy, leads to at least a 4.82% fall in the filing rate.

To further enhance comparability between these estimates, I re-estimate the RKD and weight observations using the same method in Section 4.4.1 to make the two samples more similar on observables. Table 6 reports the second and first estimation results under this alternative weighting. The RKD sample includes households that are generally less wealthy and more financially distressed than the typical ARM borrower in the year after their reset. Upweighting these households to better reflect the more representative RKD sample yields a much higher estimate, which implies that a $1,000 reduction in the annual mortgage payment leads to 78% decrease in the bankruptcy rate. This corresponds to a 0.56 percentage point decrease in the annual filing rate.

Table 6: IV Estimates of Filing Response to Liquidity (Composition-Adjusted)

	(1)	(2)	(3)	(4)
	2nd stage (outcome = bankruptcy)			
MPay_{ic}	73.58***	68.56***	92.38***	78.45***
	(18.47)	(20.01)	(21.05)	(22.22)
	1st stage (outcome = annual mortgage payment)			
Libor_{ic}	781.19***	749.90***	824.59***	831.41***
	(46.23)	(50.16)	(58.13)	(60.58)
Stage 1 F-Stat.	40.8	31.93	28.74	26.91
Observations	1,059,194	1,059,194	1,059,194	1,059,194
Time FE	✓	✓	✓	✓
County FE	✓	✓	✓	✓
Loan Age FE		✓	✓	✓
Loan Age x Time FE			✓	✓
County x Time FE				✓

Notes: These specifications re-weight the observations in order to match the RKD sample on observables. Standard errors are clustered by county. I scale and normalize the coefficients and standard errors by the annual filing rate in the second stage so that they correspond to the percent change in the filing rate as a result of a $1,000 increase in annual mortgage payments. The units for the first stage coefficient give the dollar change in the mortgage payment following a one percentage point change in the value of the index rate. Each regression includes, as controls, origination characteristics (the ARM's margin, the original payment level, the borrower's FICO score and LTV at origination) and time-varying characteristics (log mortgage balance and log home equity). The estimated coefficients on these controls are omitted for brevity. Statistical significance: 0.05*, 0.01**, 0.001***.

Applying the NPV adjustment, the composition-adjusted IV estimate implies a 12.61% decrease in the annual filing rate following a $1,000 increase in the current year's cash-flows. This is a 0.091 percentage point decrease in the probability of filing for bankruptcy. This estimate is more than three times the size of the 0.025 increase caused by a $1,000 reduction in the current generosity of bankruptcy implied by the RKD estimate. Overall, this suggests that households a much more sensitive to a change in the current year's cash-flows than to a change in the current generosity of the debt relief the household would receive in bankruptcy. This is consistent with a weak relatively strategic motive. The strong response to cash suggests that households lack insurance against shocks to their liquid wealth, and rely on bankruptcy as a source of insurance and not simply as an opportunity to increase their wealth.

5.4 Robustness

Placebo Test: The main threat to identification for the payment reduction analysis is the possibility that some unobservable factor drives both Libor-indexation and bankruptcy filings. To investigate whether such scenarios are likely, I compare filing rates in the year *prior* to the first reset. Using monthly data on these filings, I regress a filing indicator on an indicator for Libor-indexation and include the same controls and fixed effects as in the main analysis (Table 4). Table D.5 in the appendix reports these estimation results. Under the various specifications, the point estimate is small (approximately one tenth the magnitude of the IV estimate) and not statistically significant. This implies Libor-indexation does not predict different filing rates prior to the reset.

Anticipatory Behavior: This robustness analysis sheds further light on the potential importance of wealth effects from expected future changes in mortgage payments. If filing responds to expectations of higher of mortgage payments in the future (which decrease permanent income), then we would expect to see a rise in the current value of the index rate leading to more filing in the present. To separate this wealth effect from the cash-flow effect of payment reductions, I examine how filing in the year *prior* to a reset varies with the current value of an ARM's index rate.

I regress an indicator for filing on the contemporaneous value of the index rate and include the same controls and fixed effects as in the main analysis (Table 4). Table D.6 in the appendix reports the estimation results. The point estimates are small, have the opposite sign (a higher current rate predicts lower bankruptcy rates), and are statistically insignificant. The largest (in magnitude) point estimate implies that a one percent increase in the current index rate, which on average corresponds to a $1,397 increase in annual mortgage payments during a reset, leads to a 5.9% relative decrease in the filing rate (a 0.05 percentage point decrease in the probability of filing). This is much smaller than the 30% relative increase (0.28 percentage point) IV estimate per $1,000 increase in annual mortgage payments (this is the estimate from Table 4, prior to making the NPV or composition adjustments).[52]

One interpretation of this finding is that households only respond to cash-flows and not wealth effects from expected changes in future cash-flows. In this case, the original ARM IV estimate would isolate the effect of changes in the current year's cash-flows. The composition-adjusted estimates from table 6 would then imply that the cash-flow motive is twenty times stronger than the strategic motive, rather than four times stronger.

However, household inattentiveness or unawareness of loan contract terms may make them non-responsive to movements in their index rate. Households may be "rationally inattentive" if acquiring information on contract terms is very costly. Households do not always respond to profitable refinancing opportunities (Andersen, Campbell, Nielsen and Ramadorai, 2015), and those with ARMs underestimate or do not know how much their interest rates could change (Bucks and Pence, 2008). This suggests that inattention or unawareness could explain this paper's finding of a limited response to the current index rate. Inattentive or unaware households may not update expectations over future cash-flows in response to movements in their index rate until their mortgage payment changes. Households might then update expectations over future payments after experiencing their first reset. In this second scenario, the NPV adjustment would be an important step to disentangle the immediate cash-flow and future wealth effects.

[52] The regression also allows for the effect of the current rate to differ in 2007, at which point the Libor-Treasury spread had not yet widened. This means households in 2007 may have been more surprised by their actual payment reduction. The point estimate is larger in magnitude, but remains both negative and statistically insignificant.

6 Strategic and Cash-Flow Motives in a Model of Household Bankruptcy

In this section, I present a model of the household's bankruptcy decision. Using the model, I derive comparative statics that characterize the household's filing response to changes in (1) the generosity of bankruptcy and (2) non-seizable cash-flows. These comparative statics correspond to the RKD and ARM IV estimates, respectively. The model relates the relative strength of these motives to model primitives.[53]

The empirical finding of a relatively weak strategic motive has two key implications. First, marginal utility in bankruptcy is much higher than outside of bankruptcy for the marginal filer. This means that consumption must rise significantly for the marginal filer. Second, other costs of bankruptcy (e.g., stigma or credit market exclusion) must also be large. This second result follows because the marginal filer is by definition indifferent between filing and not filing, and if the consumption gain is large, the other costs must also be large in order for them to be indifferent.

6.1 The Household's Problem

A representative household lives for two periods $t \in \{1, 2\}$. At the beginning of both periods, the household draws a stochastic income shock $y_t \sim F(y_t)$.Each period, they have the option to file for bankruptcy. When their income realization is low, the household will be tempted to file for bankruptcy in order to limit the drop in their consumption.

The household begins period one with debt d_1. If they do not file for bankruptcy in period one, they then choose how much to borrow (d_2), taking the gross interest rate schedule $R_t(d_t)$ as given.[54] If the household files for bankruptcy, they keep e exempt assets and completely discharges their debt d_t.[55] The household also incurs a utility penalty $\sigma > 0$ when filing, which reflects social stigma, a moral aversion to default, or other immediate "hassle" costs of filing for bankruptcy such as time spent on paperwork. A filer in period one also incurs a dynamic cost $\delta > 0$ in the second period; this proxies for costs such credit market exclusion.[56] Regardless of their filing decision, they

[53] There are many close parallels between the model here to that of Chetty (2008). The strategic and cash-flow bankruptcy motives resemble the moral hazard and liquidity responses to increases in unemployment insurance (UI) benefit levels in Chetty (2008). In Chetty (2008), households reduce search effort in response to an increase in UI benefits both because it distorts the payoff of their search effort (moral hazrad) and because it reduces pressure on liquidity-constrained households to quickly find a job. An important difference in our settings is that bankruptcy is governed by a threshold-style rule. This means that the marginal filing responses are informative about the *marginal* filer. In contrast, optimal search effort is determined by equating average utility in and out of employment in Chetty (2008). This implies that the marginal unemployment duration responses in Chetty (2008) are informative about the *average* household.

[54] Similarly to the borrower in Arellano (2008), and Chatterjee et al. (2007).

[55] Here I effectively assume that the household has resources in the non-filing state in excess of the exemption limit. That is, if their income is their only seizable asset, I assume $y_t > e$. This is to simplify the problem and only means that the comparative statics here apply to marginal filers that are above the limit. Households with assets under the exemption do not benefit from an increase.

[56] See the extension of the dynamic model featuring credit market exclusion in Section F.3 of the appendix for an explicit micro-foundation of these types of dynamic costs.

receives payout a from an annuity in each period. The budget constraints are:

$$\begin{aligned} c_1^N &= y_1 + a - R_1(d_1)d_1 + d_2 \qquad\qquad c_t^B = a + e, \quad t = 1,2. \\ c_2^N &= y_2 + a - R_t(d_2)d_2 \end{aligned} \tag{9}$$

The superscripts N and B denote to the non-bankrupt and bankrupt states, respectively.

The household's objective is to choose consumption, whether or not to file for bankruptcy each period, and borrowing in the first period in order to maximize the present value of utility, where utility of consumption each period is a strictly increasing and strictly concave function $u(\cdot)$. The household chooses to file for bankruptcy when the present value of utility in bankruptcy is higher than when not filing, following a threshold rule with respect to income.[57] Specifically, for a given amount of debt d_t, the household files if $y_t < y_t^\star(d_t)$, where $y_t^\star$ is an endogenous threshold that is characterized by the income level such that the household is indifferent between filing and not filing. Note that by choosing their borrowing, the household effectively chooses their default probability in the next period. For brevity, the notation omits the dependence of $y_t^\star$ on d_t.

The period one value functions for the household's problem are

$$\begin{aligned} V_1^N(y_1, d_1) &= \max_{d_2}\ u(c_1^N) + \int_0^{y_2^\star} V_2^B dF(y_2) + \int_{y_2^\star}^{\infty} V_2^N(y_2, d_2) dF(y_2) \\ V_1^B &= u(c_1^B) - \sigma + \int_0^{\infty} \left[V_2^N(y_2, 0) - \delta \right] dF(y_2). \end{aligned}$$

The value function in the terminal period is

$$\begin{aligned} V_2^N(y_2, d_2) &= u(c_2^N) \\ V_2^B &= u(c_2^B) - \sigma \end{aligned}$$

where all of the above problems are subject to the budget constraints in (9).

The first-order condition governing borrowing is

$$u'(c_1^N) q_1 = \int_{y_2^\star}^{\infty} u'(c_2^N) dF(y_2).$$

The threshold governing filing in period one is characterized by an indifference condition:

$$V_1^B = V_1^N(y_1^\star, d_1) \tag{10}$$

The probability that the household files for bankruptcy, denoted p_t, is the probability that their

[57]The threshold characterization of the filing decision follows from the monotonicity in current income of the net payoff to filing. This is a general property of default models in this style (e.g., Arellano, 2008).

income realization is below the threshold:

$$p_t = P[y_t < y_t^\star(d_t)] = F[y_t^\star(d_t)].$$

6.2 Comparative Statics: Strategic and Cash-Flow Motives

This section explores how the probability of filing is affected by changes in exemption generosity e and non-seizable cash-flows a. Increases in generosity distort the household's incentive to repay their debt by increasing the implicit cost of doing so, but do not increase resources outside of bankruptcy.Bankruptcy/default driven by this type of motive, which affects willingness rather than ability to repay, is often referred to as operating along a "strategic" motive. On the other hand, increases in seizable cash-flows affect filing not by distorting the cost of repaying, but by making it easier to afford consumption in all states of the world. Below I show how a strong "cash-flow" motive, meaning that filing is very sensitive to changes in cash-flows, is consistent with a world in which the marginal filer lacks insurance against shocks to their wealth and/or faces borrowing constraints that limit consumption outside of bankruptcy.

The direct effect on the filing probability of a one-time change in the $t = 1$ level of either the exemption e or the non-seizable annuity a is:

$$\frac{\partial p_1}{\partial e_1} = f(y_1^\star)\frac{\partial y_1^\star}{\partial e_1}, \qquad \frac{\partial p_1}{\partial a_1} = f(y_1^\star)\frac{\partial y_1^\star}{\partial a_1}.$$

To derive $\frac{\partial y_1^\star}{\partial e_1}$ and $\frac{\partial y_1^\star}{\partial a_1}$, I implicitly differentiate the indifference condition (10). Substituting these derivatives into the comparative statics for the filing rate yields

$$\frac{\partial p_1}{\partial e_1} = f(y_1^\star)\frac{u'(c_1^B)}{u'(c_1^{N\star})} \geq 0 \tag{11}$$

$$\frac{\partial p_1}{\partial a_1} = f(y_1^\star)\frac{u'(c_1^B) - u'(c_1^{N\star})}{u'(c_1^{N\star})}. \tag{12}$$

Above, $c_1^{N\star}$ denotes consumption when not filing for a households whose income realization is *at the bankruptcy threshold*, i.e. $y_t = y_t^\star$. We obtain such simple expressions because the household chooses borrowing optimally; the partial derivative of either value functions with respect to any of the household's actions is zero by the envelope theorem.[58] Equation (11) reflects the marginal strategic motive and is unambiguously non-negative. In general, the sign of the marginal cash-flow motive, equation (12), is ambiguous. The estimated cash-flow effect is negative in Section 5, which in the model arises when marginal utility is lower in bankruptcy than when out of bankruptcy for the marginal filer.

[58]Formally, we can apply the multivariate implicit function theorem here using the borrowing FOC as the second equation. The fact that debt d_2 has no effect on the indifference condition would lead to any terms associated with d_2 to cancel, yielding the same expression as what we obtain only using the indifference condition.

Implications for the Costs and Benefits of Filing for the Marginal Filer

Taking the ratio of the cash-flow motive to the strategic motive, we have

$$\frac{-\partial p_1/\partial a_1}{\partial p_1/\partial_1 e} = \frac{u'(c_1^{N\star})}{u'(c_1^B)} - 1. \tag{13}$$

A strong cash-flow motive relative to the strategic motive implies that the difference between marginal utility in and out of bankruptcy is large for the marginal filer. If we assume that the marginal utility functions are the same in and out of bankruptcy, this implies that consumption is higher in bankruptcy than out of bankruptcy for the marginal filer:

$$c_1^{N\star} << c_1^B.$$

Is this implied large relative increase in consumption realistic? If the marginal filer would have to drop her consumption very low in order to repay her debt outside of bankruptcy, eliminating her debt service payments could significantly increase her consumption. Additionally, in an extension I instead allow for the marginal filer to be delinquent in the non-filing state, a large consumption gain could arise from bankruptcy if she was having her wages garnished outside of bankruptcy.[59]

However, the large consumption response for the marginal filer also implies that the "other" costs of bankruptcy are substantial. In the model, these costs include stigma σ and the dynamic costs δ. This implication arises because by definition the marginal filer is indifferent between filing and not filing. In order for them to be indifferent when receiving a large increase in consumption upon filing, it must be that the difference between the other terms in the indifference condition (10) are large. We can rewrite the indifference condition more compactly using expectations, where $\mathbb{E}^B$ and $\mathbb{E}^N$ respectively denote the expectation conditional on filing and not filing in period one:

$$u(c_1^B) - \sigma + \mathbb{E}^B \left[V_2(y_2, 0) - \delta\right] = \max_{d_2} u(c_1^{N\star}) + p_2 \mathbb{E}^N V_2^B + (1 - p_2)\mathbb{E}^N V_2^N(y_2, d_2)$$

Thus if $u(c_1^B) >> u(c_1^{N\star})$, then

$$\underbrace{-\sigma}_{\text{utility penalty}} \quad - \underbrace{\left\{\delta + p_2 \mathbb{E}^N[V_2^B] + (1-p_2)\mathbb{E}^N\left[V_2^N(y_2, d_2)\right] - \mathbb{E}^B\left[V_2(y_2, 0)\right]\right\}}_{\text{dynamic cost}} << 0$$

where $\mathbb{E}^N\left[V_2^N(y_2, d_2)\right]$ is evaluated at the optimally chosen d_2. It follows that either the utility penalty, the dynamic costs, or both are large when the cash-flow motive is strong compared to the strategic motive.[60]

Is this second implication of large costs of bankruptcy outside of the immediate financial costs realistic? A large dynamic cost could arise from credit market exclusion. After filing, a

[59] An attractive feature of discharging debt is to eliminate wage garnishments ordered by courts to help creditors recover delinquent debt. See Section F.4 in the appendix for this extension with delinquency.

[60] In the dynamic model, credit market exclusion appears in the difference between expected continuation values.

bankruptcy "flag" remains on the filer's credit report for seven to ten years. It is well-documented that bankruptcy flag removals lead to large increases in credit access.[61] Significant stigma is also realistic as household survey evidence finds 82% of households report it being morally wrong to default when capable of paying (Guiso, Sapienza and Zingales, 2013).

The empirical findings of a cash-flow response more than three times as great as than the strategic response imply both large consumption gains to filers and substantial costs beyond the immediate financial cost of bankruptcy. This is consistent with a world where the marginal filer is very financially distressed and would have extremely low consumption outside of bankruptcy. The possibility of large costs and stigma imply that this large consumption increase comes at steep price for the marginal filer.

Implications for Heterogeneity in Filing Sensitivity

In the heterogeneity analysis of the RKD used to estimate the strategic motive, the filing of households with lower FICO scores at origination, higher origination LTVs and in areas with lower income or higher unemployment was more sensitive to changes in their cost of bankruptcy. The model implies that this greater sensitivity for these more financially distressed households could arise from two sources: more mass at the filing threshold (higher $f(y_1^\star)$) or more sensitive threshold (higher $\frac{\partial p_1}{\partial e}$). Both channels are plausible.

Greater sensitivity could arise if financially distressed households have a distribution shocks to income/wealth with more mass near the filing threshold.If their threshold is relatively low, households with a greater probability of low income realizations may have more mass at the threshold. Intuitively, households more likely to experience negative shocks will tend to be closer to the bankruptcy threshold and therefore more likely to be pushed over their filing threshold.

Another reason a household could be more sensitive to the generosity of bankruptcy is if her threshold is more sensitive (high $\frac{\partial y_1^\star}{\partial e}$). The effect of bankruptcy generosity on the threshold is greater when marginal utility in the filing state is higher, meaning consumption is lower. A household with fewer resources when filing will be more responsive. For example, better insured households, those with higher a, will be less responsive. One reason a household may be better insured is if they are married and could rely on their partner's income when filing. Similarly, a household living in a state with more generous unemployment insurance (not explicitly modeled here), could keep their consumption higher if their income drop was associated with job loss. Additionally, when a household has more of their other assets protected in bankruptcy (e.g., retirement savings accounts) they would also be better insured.[62]

[61] See for example Gross, Notowidigdo and Wang (2018a); Dobbie, Keys and Mahoney (2017); Dobbie, Goldsmith-Pinkham, Mahoney and Song (2019); Herkenhoff, Phillips and Cohen-Cole (2016); Musto (2004).

[62] To see this, suppose we considered only the effect on filing of changes to one of multiple exemption limits. The partial derivatives above would still equal the same marginal utility terms and only our expressions for consumption would differ (i.e., $c_1^B = e_1 + e_2 + \cdots + e_N + a$ with N distinct exemptions).

7 Conclusion

This paper presented new evidence on the causes of household bankruptcy. First, using a regression kink design (RKD) I estimate a small, but positive response of bankruptcy filing to an increase in the generosity of the debt relief households receive in bankruptcy. The RKD exploits the kink induced by homestead exemption laws in household's seizable equity as a function of home equity. This results in a kink in one component of the cost of bankruptcy facing households. The RKD uses a large and representative borrower-level dataset that tracks bankruptcy filing and mortgages debt balances over time; it also includes a rich array of borrower characteristics.

However, home equity must be imputed and is likely subject to measurement error. In a new approach, I address the non-standard challenges that measurement error creates in an RKD by imposing stronger parametric assumptions on the relationship between filings and home equity. These assumptions enable me to characterize the bias induced by measurement error, which is in part due to the standard attenuation bias created by noise in the covariates but also is made worse when measurement error results in a higher fraction of observations being assigned to the "wrong" side of the cutoff. Using a sample of 200,000 home sales, I correct for this bias under the assumption that the sale price is the actual price at which the home would be valued in bankruptcy. The standard non-parametric RK estimator implies a \$1,000 reduction in generosity of bankruptcy leads to a 1.64% decrease in bankruptcy filings (a 0.011% percentage point decrease in the annual probability of filing). After correcting for measurement error the implied effects are 3.42% and 0.025 percentage points (respectively).

Reductions in the generosity of bankruptcy do not directly affect a household's resources outside of bankruptcy, but they do increase the relative payoff of filing. Generous bankruptcy distorts borrower incentive to repaying by altering these relative payoffs and encouraging more filing and discouraging lending. The stronger this strategic filing motive, the more costly generous bankruptcy is. However, generous bankruptcy can potentially be welfare improving if the insurance value it provides offsets these costs. When households lack insurance against cash-flow shocks, bankruptcy potentially has a valuable role to play as a source of insurance.

To shed light on the strength of cash-flow motives, I investigate the effect of mortgage payment reductions on filing. Reductions in mortgage payments approximate shocks to non-seizable cash-flows. To identify the causal effect of payment reductions on filing, I use a large sample of households with adjustable-rate mortgages (ARMs) originated in 2003-2008. When ARMs reset to a new interest rate, the rule governing the new rate is predetermined but depends on the current value of a pre-selected index rate (most often the one-year Libor or Treasury rate). I exploit the unprecedentedly large increase in the spread between Libor and Treasury beginning in 2008 as source of exogenous variation in mortgage payment sizes for resetting ARMs in an instrumental variables strategy. I estimate that a \$1,000 reduction in annual mortgage payments leads to a 30-80% decrease in bankruptcy filings (0.28-0.56 percentage point decrease in the annual filing rate). Adjusting the IV estimate to account for potential effects on expectations over future mortgage payments, I find a \$1,000 increase in the current year's cash-flows implies a 4.82-12.61% decrease in the filing rate (0.045-0.091 percentage point decreases). This suggests households respond much more strongly to changes in the current year's cash-flows than the current generosity of the debt relief the household would receive in bankruptcy.

Lastly, using a model of the household bankruptcy decision, I show the strengths of these motives are informative about the circumstances of the marginal bankruptcy filer. The relatively weak strategic motive implies that consumption gains to filers must be large but that other costs of bankruptcy, such as social stigma or from credit market exclusion, must also be large. My findings are consistent with a lack of insurance against cash-flow shocks driving bankruptcy and imply that increases in the generosity of bankruptcy only weakly incentivize further filing.

References

Adelino, Manuel, Antoinette Schoar, and Felipe Severino, "House Prices, Collateral, and Self-employment," *Journal of Financial Economics*, 2015, *117* (2), 288–306.

Amromin, Gene, Jennifer Huang, Clemens Sialm, and Edward Zhong, "Complex Mortgages," *NBER Working Paper No. 17315*, 2011.

Andersen, Steffen, John Y. Campbell, Kasper Meisner Nielsen, and Tarun Ramadorai, "Inattention and Inertia in Household Finance: Evidence from the Danish Mortgage Market," Technical Report, National Bureau of Economic Research 2015.

Arellano, Cristina, "Default risk and income fluctuations in emerging economies," *American Economic Review*, 2008, *98* (3), 690–712.

Athreya, Kartik B., "Welfare Implications of the Bankruptcy Reform Act of 1999," *Journal of Monetary Economics*, 2002, *49* (8), 1567–1595.

—, "Fresh Start or Head Start? Uniform Bankruptcy Exemptions and Welfare," *Journal of Economic Dynamics and Control*, 2006, *30* (11), 2051–2079.

Auclert, Adrien, Will Dobbie, and Paul Goldsmith-Pinkham, "Macroeconomic Effects of Debt Relief: Consumer Bankruptcy Protections in the Great Recession," 2019.

Berger, David, Nick Turner, and Eric Zwick, "Stimulating Housing Markets," *Journal of Finance (forthcoming)*, 2019.

Bucks, Brian and Karen Pence, "Do Borrowers Know their Mortgage Terms?," *Journal of Urban Economics*, 2008, *64* (2), 218–233.

Bursztyn, Leonardo, Stefano Fiorin, Daniel Gottlieb, and Martin Katz, "Moral Incentives in Credit Card Debt Repayment: Evidence from a Field Experiment," *Journal of Political Economy (forthcoming)*, 2018.

Busse, Meghan R., Christopher R. Knittel, and Florian Zettelmeyer, "Are Consumers Myopic? Evidence from New and Used Car Purchases," *American Economic Review*, 2013, *103* (1), 220–56.

Calonico, Sebastian, Matias D. Cattaneo, and Rocio Titiunik, "Robust Nonparametric Confidence Intervals for Regression-Discontinuity Designs," *Econometrica*, 2014, *82* (6), 2295–2326.

Card, David, David S. Lee, Zhuan Pei, and Andrea Weber, "Inference on Causal Effects in a Generalized Regression Kink Design," *Econometrica*, 2015, *83* (6), 2453–2483.

Chatterjee, Satyajit and Grey Gordon, "Dealing with Consumer Default: Bankruptcy vs Garnishment," *Journal of Monetary Economics*, 2012, *59*, S1–S16.

—, Dean Corbae, Makoto Nakajima, and José-Víctor Ríos-Rull, "A Quantitative Theory of Unsecured Consumer Credit with Risk of Default," *Econometrica*, 2007, *75* (6), 1525–1589.

Chetty, Raj, "Moral Hazard versus Liquidity and Optimal Unemployment Insurance," *Journal of Political Economy*, 2008, *116* (2), 173–234.

Dávila, Eduardo, "Using Elasticities to Derive Optimal Bankruptcy Exemptions," 2016.

Di Maggio, Marco, Amir Kermani, Benjamin J. Keys, Tomasz Piskorski, Rodney Ramcharan, Amit Seru, and Vincent Yao, "Interest Rate Pass-Through: Mortgage Rates, Household Consumption, and Voluntary Deleveraging," *American Economic Review*, 2017, *107* (11), 3550–88.

DiNardo, John, Nicole M. Fortin, and Thomas Lemieux, "Labor Market Institutions and the Distribution of Wages, 1973-1992: A Semiparametric Approach," *Econometrica*, 1996, *64* (5), 1001–1044.

Dobbie, Will and Jae Song, "Targeted Debt Relief and the Origins of Financial Distress: Experimental Evidence from Distressed Credit Card Borrowers," 2018.

— **, Benjamin J. Keys, and Neale Mahoney**, "Credit Market Consequences of Credit Flag Removals," 2017.

— **, Paul Goldsmith-Pinkham, Neale Mahoney, and Jae Song**, "Bad Credit, No Problem? Credit and Labor Market Consequences of Bad Credit Reports," 2019.

Elias, Stephen R., *The New Bankruptcy: Will It Work for You?*, Nolo, 2011.

Fan, Jianqing, "Design-Adaptive Nonparametric Regression," *Journal of the American Statistical Association*, 1992, *87* (420), 998–1004.

— **and Irène Gijbels**, "Local Polynomial Modelling and Its Applications," 1996.

Fay, Scott, Erik Hurst, and Michelle J. White, "The Household Bankruptcy Decision," *American Economic Review*, 2002, *92* (3), 706–718.

Fuster, Andreas and Paul S. Willen, "Payment Size, Negative Equity, and Mortgage Default," *American Economic Journal: Economic Policy*, 2017, *9* (4), 167–91.

Ganong, Peter and Pascal Noel, "Liquidity vs. Wealth in Household Debt Obligations: Evidence from Housing Policy in the Great Recession," 2019.

— **and Simon Jäger**, "A Permutation Test for the Regression Kink Design," *Journal of the American Statistical Association*, 2018, pp. 1–11.

Gerardi, Kris, Kyle Herkenhoff, Lee E. Ohanian, and Paul Willen, "Can't Pay or Won't Pay? Unemployment, Negative Equity, and Strategic Default," *Review of Financial Studies*, 2017, *31* (3), 1098–1131.

Gordon, Grey, "Optimal Bankruptcy Code: A Fresh Start for Some," *Journal of Economic Dynamics and Control*, 2017, *85*, 123–149.

Griliches, Zvi and Vidar Ringstad, "Error-in-the-Variables Bias in Nonlinear Contexts," *Econometrica*, 1970, *38* (2), 368–370.

Gropp, Reint, John Karl Scholz, and Michelle J. White, "Personal Bankruptcy and Credit Supply and Demand," *The Quarterly Journal of Economics*, 1997, *112* (1), 217–251.

Gross, Tal and Matthew J. Notowidigdo, "Health Insurance and the Consumer Bankruptcy Decision: Evidence from Expansions of Medicaid," *Journal of Public Economics*, 2011, *95* (7-8), 767–778.

— **, — , and Jialan Wang**, "Liquidity Constraints and Consumer Bankruptcy: Evidence from Tax Rebates," *Review of Economics and Statistics*, 2014, *96* (3), 431–443.

— **, — , and —** , "The Marginal Propensity to Consume Over the Business Cycle," *American Economic Journal: Macroeconomics (forthcoming)*, 2018.

— **, Raymond Kluender, Feng Liu, Matthew J. Notowidigdo, and Jialan Wang**, "The Economic Consequences of Bankruptcy Reform," 2018.

Guiso, Luigi, Paola Sapienza, and Luigi Zingales, "The Determinants of Attitudes toward Strategic Default on Mortgages," *The Journal of Finance*, 2013, *68* (4), 1473–1515.

Gupta, Arpit, "Foreclosure Contagion and the Neighborhood Spillover Effects of Mortgage Defaults," *The Journal of Finance (forthcoming)*, 2018.

Haughwout, Andrew, Ebiere Okah, and Joseph Tracy, "Second Chances: Subprime Mortgage Modification and Redefault," *Journal of Money, Credit and Banking*, 2016, *48* (4), 771–793.

Herkenhoff, Kyle, Gordon Phillips, and Ethan Cohen-Cole, "How Credit Constraints Impacts Job Finding Rates, Sorting & Aggregate Output," 2016.

Hsu, Joanne W., David A. Matsa, and Brian T. Melzer, "Unemployment Insurance as a Housing Market Stabilizer," *American Economic Review*, 2018, *108* (1), 49–81.

Hynes, Richard M., Anup Malani, and Eric. A. Posner, "The Political Economy of Property Exemption Laws," *The Journal of Law & Economics*, 2004, *47* (1), 19–43.

Keys, Benjamin J., "The Credit Market Consequences of Job Displacement," *Review of Economics and Statistics*, 2018, *100* (3), 405–415.

Kroft, Kory and Matthew J. Notowidigdo, "Should Unemployment Insurance Vary With the Unemployment Rate? Theory and Evidence," *Review of Economic Studies*, 2016, *83* (3), 1092–1124.

Livshits, Igor, James MacGee, and Michele Tertilt, "Consumer Bankruptcy: A Fresh Start," *American Economic Review*, 2007, *97* (1), 402–418.

Mahoney, Neale, "Bankruptcy as Implicit Health Insurance," *American Economic Review*, 2015, *105* (2), 710–46.

Mayer, Christopher, Edward Morrison, Tomasz Piskorski, and Arpit Gupta, "Mortgage Modification and Strategic Behavior: Evidence from a legal Settlement with Countrywide," *American Economic Review*, 2014, *104* (9), 2830–2857.

McCrary, Justin, "Manipulation of the Running Variable in the Regression Discontinuity Design: A Density Test," *Journal of Econometrics*, 2008, *142* (2), 698–714.

Mian, Atif and Amir Sufi, "What Explains the 2007–2009 Drop in Employment?," *Econometrica*, 2014, *82* (6), 2197–2223.

Mitman, Kurt, "Macroeconomic Effects of Bankruptcy and Foreclosure Policies," *American Economic Review*, 2016, *106* (8), 2219–55.

Mondragon, John, "Household Credit and Employment in the Great Recession," 2018.

Musto, David K., "What Happens When Information Leaves a Market? Evidence from Postbankruptcy Consumers," *The Journal of Business*, 2004, *77* (4), 725–748.

Nakajima, Makoto and José-Víctor Ríos-Rull, "Credit, Bankruptcy, and Aggregate Fluctuations," 2014.

Pence, Karen M., "Foreclosing on Opportunity: State Laws and Mortgage Credit," *Review of Economics and Statistics*, 2006, *88* (1), 177–182.

Scharlemann, Therese C. and Stephen H. Shore, "The Effect of Negative Equity on Mortgage Default: Evidence from HAMP's Principal Reduction Alternative," *The Review of Financial Studies*, 2016, *29* (10), 2850–2883.

Severino, Felipe and Meta Brown, "Personal Bankruptcy Protection and Household Debt," 2017.

Skeel, David A., "Debt's Dominion: A History of Bankruptcy Law in America," *Princeton University Press*, 2001.

Stavins, Joanna, "Credit Card Borrowing, Delinquency, and Personal Bankruptcy," *New England Economic Review*, 2000, (July), 15–30.

White, Michelle J., "Bankruptcy Reform and Credit Cards," *Journal of Economic Perspectives*, 2007, *21* (4), 175–200.

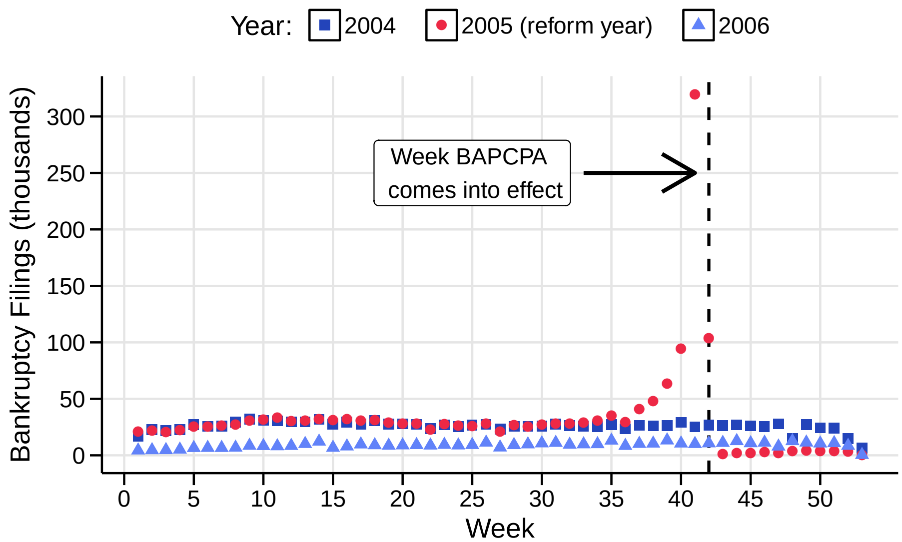
Appendix

Contents

A Figures

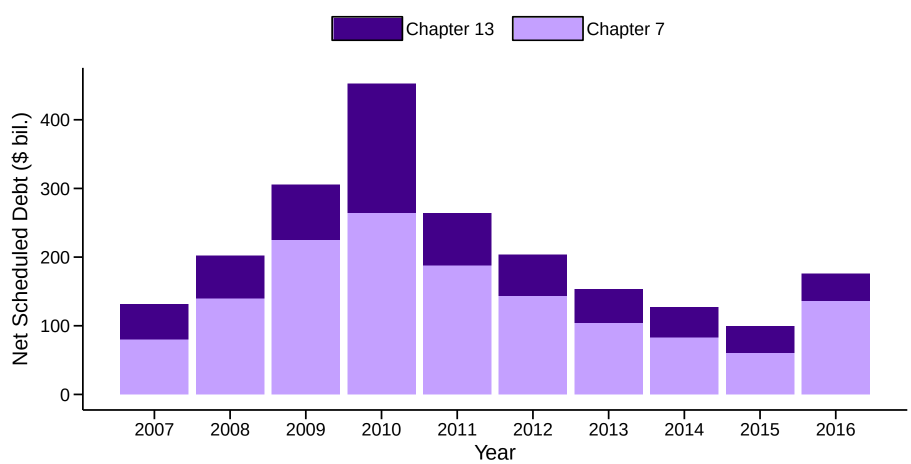
A.1 Aggregate Bankruptcy Statistics

Figure A.1: Weekly Bankruptcy Filings Around the 2005 Bankruptcy Reform (BAPCPA)



Notes: This graph plots weekly filing counts for 2004, 2005, and 2006. In 2005, BAPCPA was enacted and filing spiked significantly in the run-up to the reform. This graph is constructed using the case-level bankruptcy data from Gross, Notowidigdo and Wang (2014).

Figure A.2: Aggregate Dischargeable Debt (Chapters 7 and 13)



Notes: This graph plots the total amount of net scheduled debt among Chapter 7 and Chapter 13 filers. The height corresponds to the total amount under both chapters. Net scheduled debt consists of the total amount of debt that filers are typically able to discharge in bankruptcy. The actual amount may differ if judges invoke discretion to prevent or allow the discharge of particular debts. The data used in the graph are from the annual BAPCPA reports that began in 2007 (tables 1A and 1D). Both original sources are available at http://www.uscourts.gov/statistics-reports/caseload-statistics-data-tables.

Figure A.3: Number of Bankruptcy Filings (Chapters 7 and 13)

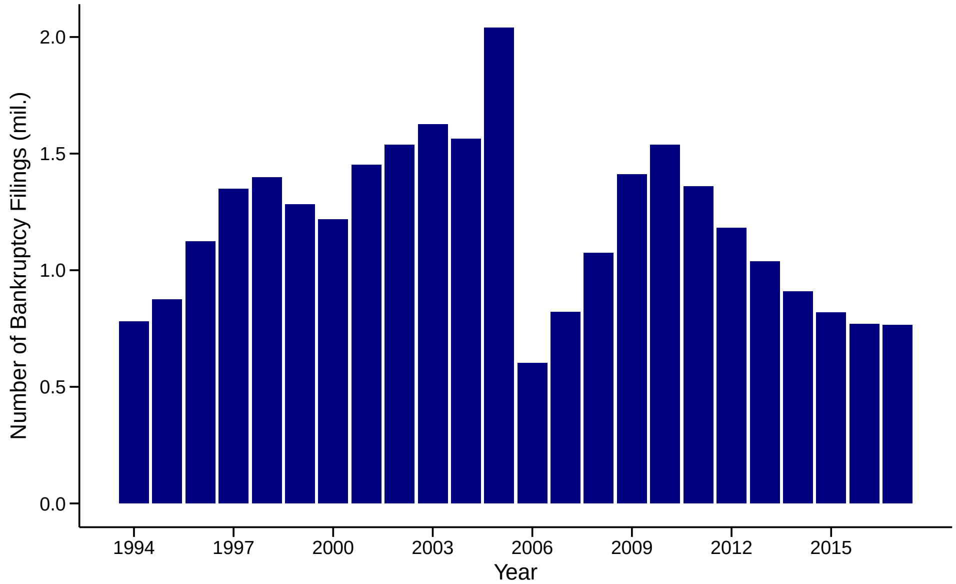
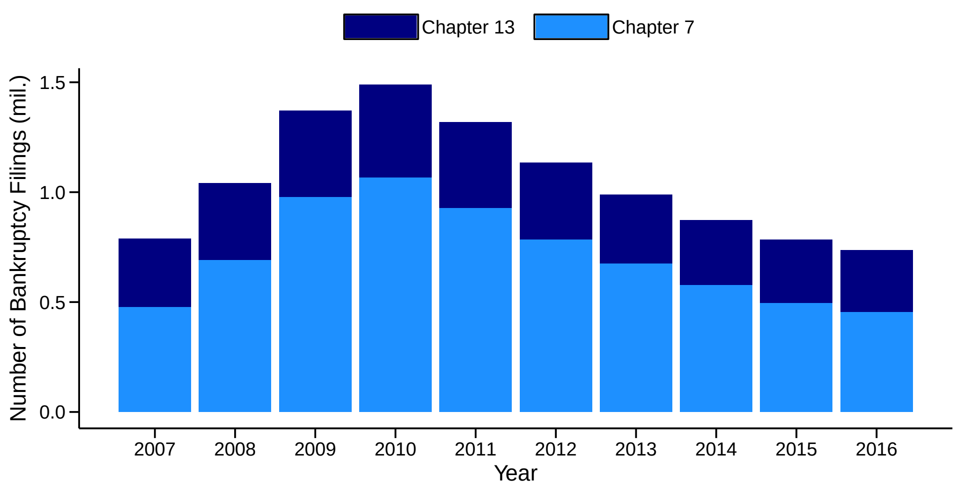


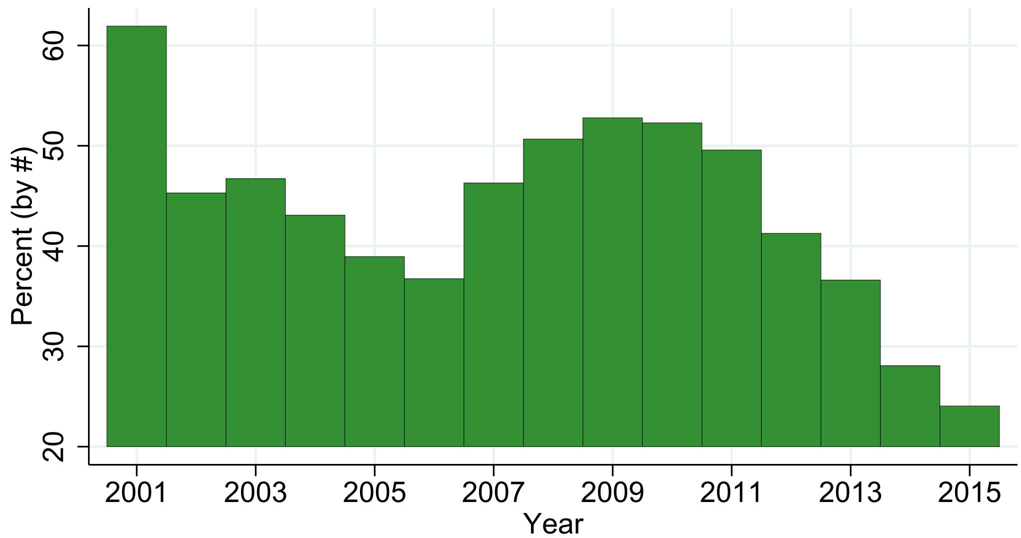
Figure A.4: Number of Bankruptcy Filings (by Chapter)



Notes: These graphs plot the total number of Chapter 7 and 13 bankruptcy filings per year. The data used to construct the top graph is from the American Bankruptcy Institute. The bottom graph obtains the Chapter-specific breakdown from the annual BAPCPA reports that began in 2007 (tables 1A and 1D). Both original sources are available at http://www.uscourts.gov/statistics-reports/caseload-statistics-data-tables.

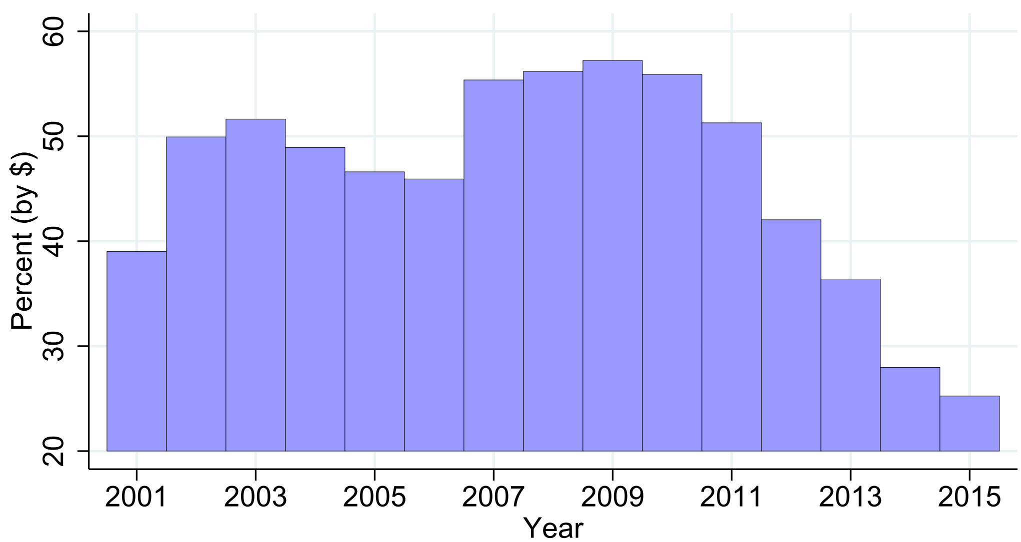
A.2 LLMA Representativeness

Figure A.5: Percent of Mortgage Originations Covered (by number)



Notes: This graph plots the annual number of mortgage originations in the LLMA data against total annual originations reported as part of the Home Mortgage Disclosure Act (HMDA). HMDA contains the near universe of residential mortgage originations in the US.

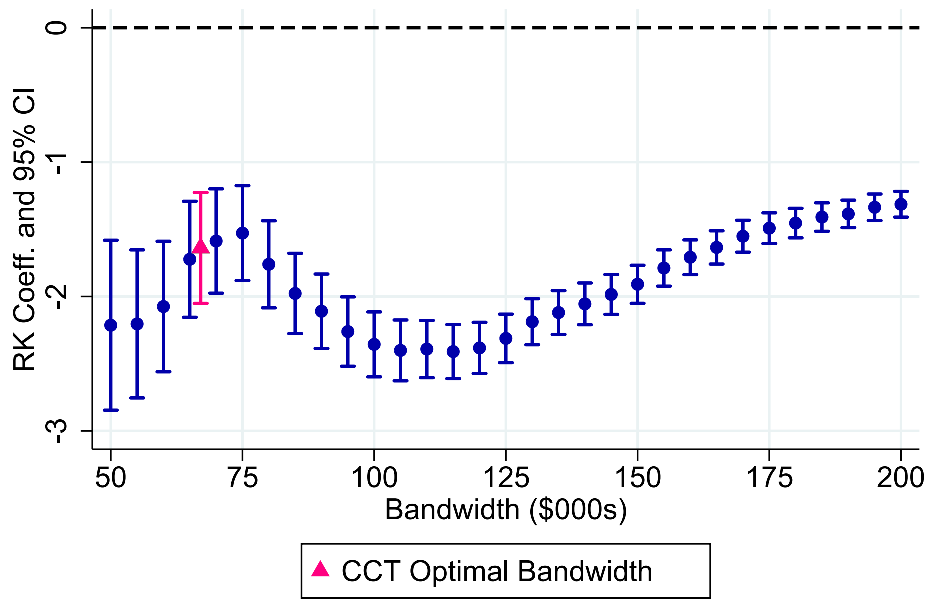
Figure A.6: Percent of Mortgage Originations Covered (by amount)



Notes: This graph plots the annual amount of mortgage originations in the LLMA data against total annual originations reported as part of the Home Mortgage Disclosure Act (HMDA). HMDA contains the near universe of residential mortgage originations in the US.

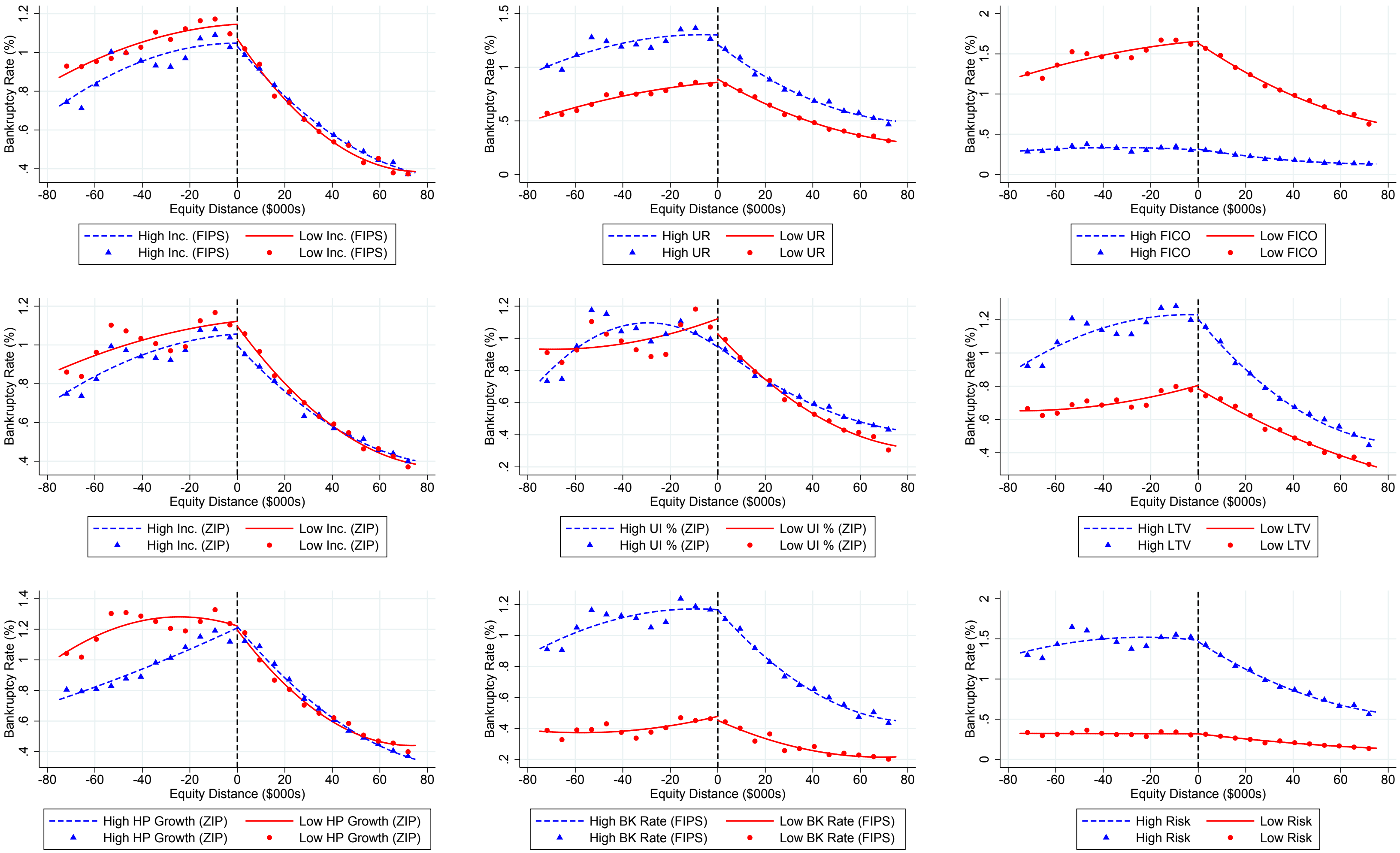
A.3 RKD Figures

Figure A.7: Sensitivity of Filings to Bankruptcy Generosity: Various RKD Bandwidths



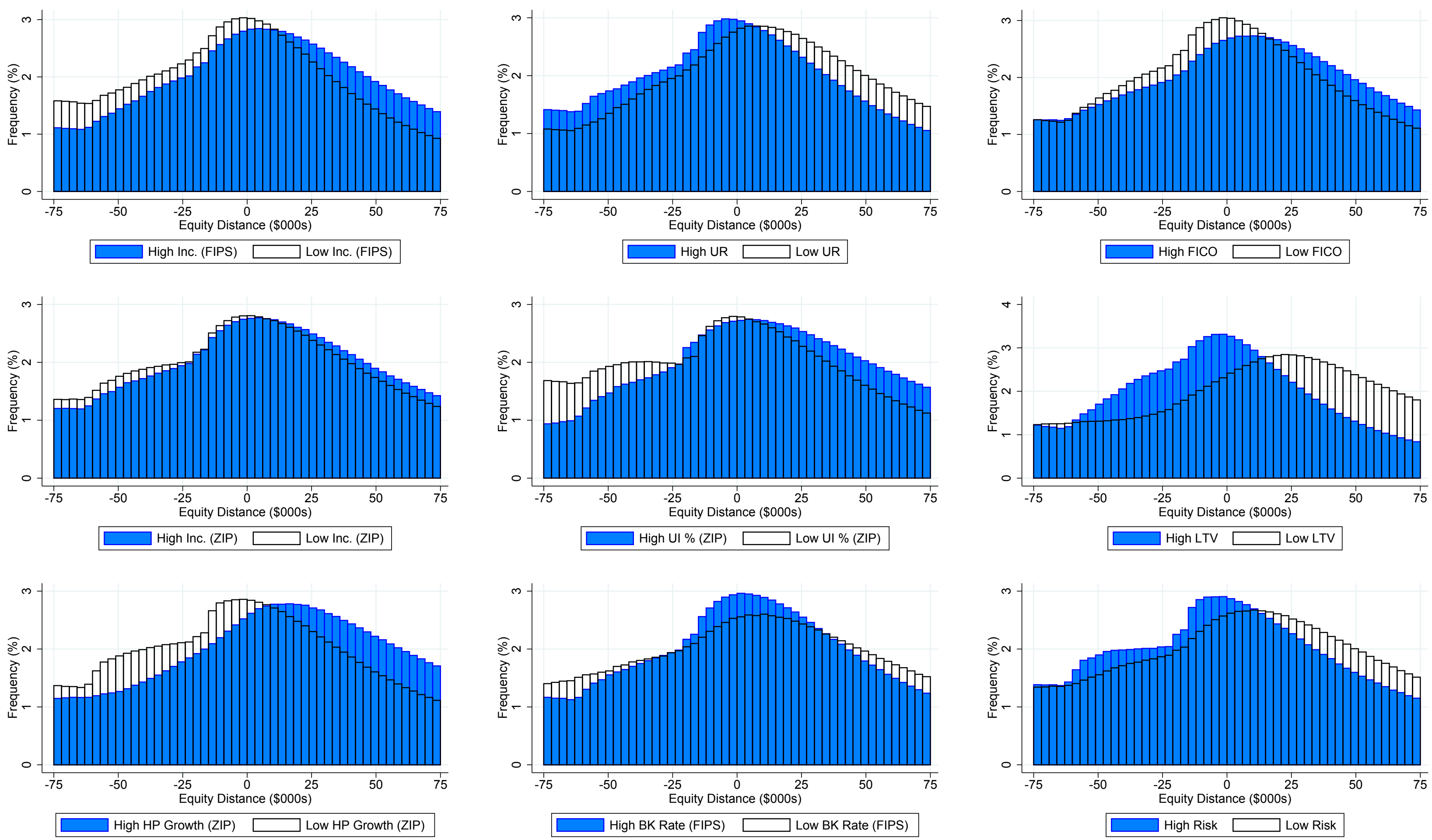
Notes: This graph plots RKD estimates obtained under various bandwidth choices and their 95% confidence intervals. The estimator used is the standard local quadratic RKD estimator and estimates are not corrected here for measurement error. Confidence intervals are computed as in Calonico, Cattaneo and Titiunik (2014).

Figure A.8: Heterogeneity in Filing Rate Kinks



Notes: These figures plot quarterly bankruptcy rates for equally-spaced bins of equity distance. The two lines are quadratic polynomials fit separately to the underlying data above and below the cutoff.

Figure A.9: Distribution of Home Equity for Subgroups



Notes: These figures plot histograms of seizable equity. Each plot contains histograms for two separate subset of the main sample. The subsets partition the data into observations with either high or low value of a specific characteristic.

B Tables

B.1 RKD: Summary Statistics

Table B.1: Summary Statistics (full RKD sample)

	Mean	SD	Min	25^{th}	50^{th}	75^{th}	Max	Obs.
Bankruptcy								
File (%)	0.72	16.84	0	0	0	0	400	99,233,172
Never Filed (%)	99.91	2.95	0	100	100	100	100	99,233,172
Years Since Last Bankruptcy	2.71	4.3	0	0	0	8.25	15.25	248,285
Home Equity								
Home Equity	104.52	90.85	0	42.36	77.61	138.57	546.53	99,233,172
Equity Distance	-47.92	174	-615.57	-112.5	-11.87	47.43	536.72	99,233,172
State's Homestead Exemption	150.29	152.26	8	40	100	150	500	99,233,172
Borrower/Mortgage								
Mortgage Age (months)	40.62	35.59	0	13	30	60	869	99,206,695
Origination LTV	77.72	8.62	60	71.63	79.4	80	95	99,233,172
FICO at Origination	719.16	66.63	300	679	731	772	900	85,536,164
Local Economy								
Unemployment Rate	5.89	2.11	0.9	4.4	5.4	7	29.9	98,946,282
HP Growth	1.90	6.10	-54.78	-1.69	1.69	5.31	51.42	70,249,170
Median Income	85.34	11.27	49.55	77.97	83.82	93.29	130.37	68,630,838
ln(Median Income)	11.35	0.13	10.81	11.26	11.34	11.44	11.78	68,630,838

Notes: This table presents summary statistics for the full sample. The data are quarterly but I annualize the bankruptcy rate. All dollar amounts are reported in thousands of 2010 dollars. The "Years Since Last Bankruptcy" variable is the average across the subset of households that have filed for bankruptcy in the past. The unemployment rate and median income are measured at the county-level. House price growth is measured as 100$\times$ the difference in ZIP-level house prices over the previous year (i.e., from the beginning of $t-4$ to the end of $t-1$). Median income is also adjusted to reflect regional cost of living differences (the details of this adjustment are discussed in the main text).

Table B.2: Summary Statistics (RKD filers)

	Mean	SD	Min	25th	50th	75th	Max	Obs.
Bankruptcy								
File (%)	400	0	400	400	400	400	400	176,384
Never Filed (%)	91.84	27.37	0	100	100	100	100	176,384
Years Since Last Bankruptcy	0	0	0	0	0	0	0	176,384
Home Equity								
Home Equity	59.77	57.07	0	22.72	44.01	77.9	546.25	176,384
Equity Distance	-85.53	158.16	-600.2	-121.21	-32.1	9.66	516.43	176,384
State's Homestead Exemption	144	153.06	8	40	75	150	500	176,384
Borrower/Mortgage								
Mortgage Age (months)	51.82	33.76	0	24	45	75	311	176,384
Origination LTV	80.14	8.3	60	75	80	86.27	95	176,384
FICO at Origination	666.48	68.93	300	628	674	715	900	148,495
Local Economy								
Unemployment Rate	6.64	2.29	1.8	4.9	6.3	8.2	27.2	175,803
HP Growth	0.70	6.40	-46.45	-3.33	0.49	4.64	40.50	92,720
Median Income	83.64	11.20	52.08	76.62	82.51	90.75	130.37	124,529
ln(Median Income)	11.33	0.13	10.86	11.25	11.32	11.42	11.78	124,529

Notes: See notes for Table B.1. This table restricts the sample to filers.

Table B.3: Summary Statistics (RKD non-filers)

	Mean	SD	Min	25th	50th	75th	Max	Obs.
Bankruptcy								
File (%)	0	0	0	0	0	0	0	99,056,788
Never Filed (%)	99.93	2.69	0	100	100	100	100	99,056,788
Years Since Last Bankruptcy	9.37	1.2	8	8.50	9	10	15.25	71,901
Home Equity								
Home Equity	104.6	90.88	0	41.41	77.68	138.68	546.53	99,056,788
Equity Distance	-47.85	174.02	-615.57	-112.48	-11.83	47.52	536.72	99,056,788
State's Homestead Exemption	152.26	152.26	8	40	100	150	500	99,056,788
Borrower/Mortgage								
Mortgage Age (months)	40.6	35.39	0	13	30	59	869	99,030,311
Origination LTV	77.72	8.62	60	71.62	79.4	80	95	99,056,788
FICO at Origination	719.25	66.58	300	679	732	772	900	85,387,669
Local Economy								
Unemployment Rate	5.88	2.11	0.9	4.4	5.4	7	29.9	98,770,479
HP Growth	1.90	6.10	-54.78	-1.69	1.69	5.32	51.42	70,156,450
Median Income	85.34	11.27	49.55	77.97	83.82	93.29	130.37	68,506,309
ln(Median Income)	11.35	0.13	10.81	11.26	11.34	11.44	11.78	68,506,309

Notes: See notes for Table B.1. This table restricts the sample to non-filers.

Table B.4: Summary Statistics (RKD, below state's exemption)

	Mean	SD	Min	25^{th}	50^{th}	75^{th}	Max	Obs.
Bankruptcy								
File (%)	0.88	18.76	0	0	0	0	400	55,009,070
Never Filed (%)	99.92	2.84	0	100	100	100	100	55,009,070
Years Since Last Bankruptcy	2.11	3.93	0	0	0	0	15	157,032
Home Equity								
Home Equity	71.71	64.05	0	28.76	53.4	96.16	546.53	55,009,070
Equity Distance	-156.34	147.97	-615.57	-270.26	-97.47	-35.77	0	55,009,070
State's Homestead Exemption	224.84	165.51	8	100	150	390	500	55,009,070
Borrower/Mortgage								
Mortgage Age (months)	37.74	34.29	0	11	27	56	869	54,991,555
Origination LTV	79	8.72	60	73.62	80	84.7	95	55,009,070
FICO at Origination	716.66	66.6	300	675	727	770	900	47,798,415
Local Economy								
Unemployment Rate	6.09	2.26	0.9	4.4	5.6	7.3	29.9	54,932,294
HP Growth	1.38	6.46	-54.78	-2.53	1.19	5.14	46.35	38,703,662
Median Income	84.01	10.44	49.55	77.22	83.04	91.72	130.37	38,545,223
ln(Median Income)	11.33	0.13	10.81	11.25	11.33	11.43	11.78	38,545,223

Notes: See notes for Table B.1. This table restricts the sample to households below the exemption limit.

Table B.5: Summary Statistics (RKD, above state's exemption)

	Mean	SD	Min	25^{th}	50^{th}	75^{th}	Max	Obs.
Bankruptcy								
File (%)	0.48	14.12	0	0	0	0	400	44,224,102
Never Filed (%)	99.91	3.07	0	100	100	100	100	44,224,102
Years Since Last Bankruptcy	3.74	4.69	0	0	0	8.75	15.25	91,253
Home Equity								
Home Equity	145.34	102.04	9.49	69.42	117.96	192.95	546.53	44,224,102
Equity Distance	86.94	88.82	0	24.29	56.87	118.74	536.72	44,224,102
State's Homestead Exemption	57.56	49.33	8	22.98	40	60	500	44,224,102
Borrower/Mortgage								
Mortgage Age (months)	44.2	36.39	0	16	34	63	359	44,215,140
Origination LTV	76.14	8.25	60	70	77.47	80	95	44,224,102
FICO at Origination	722.33	66.53	300	684	737	775	900	37,737,749
Local Economy								
Unemployment Rate	5.64	1.88	0.9	4.2	5.3	6.6	27.2	44,013,988
HP Growth	2.53	5.57	-53.15	-0.74	2.16	5.52	51.42	31,545,508
Median Income	87.03	-12.04	52.08	78.48	85.14	95.29	130.37	30,085,615
ln(Median Income)	11.36	0.14	10.86	11.27	11.35	11.46	11.78	30,085,615

Notes: See notes for Table B.1. This table restricts the sample to households above the exemption limit.

B.2 RKD: Heterogeneity

Table B.6: Heterogeneity in Filing Sensitivity (RKD Sample Splits)

	(1) Low	(2) High	(3) Low	(4) High
	Income (County)		Income (ZIP)	
RK est. $\left(\widehat{\frac{\partial p}{\partial s}}\right)$	-1.58***	-1.54***	-2.74***	-1.51***
	(0.36)	(0.22)	(0.33)	(0.25)
Obs. (mil.)	11,577,301	36,157,472	16,586,486	20,063,488
	Unemp. Rate (County)		UI claims (ZIP)	
RK est. $\left(\widehat{\frac{\partial p}{\partial s}}\right)$	-1.82***	-1.34***	-3.31***	-0.44+
	(0.22)	(0.26)	(0.36)	(0.23)
Obs. (mil.)	24,898,146	22,740,920	13,865,644	16,341,710
	House Price Bust		Orig. FICO	
RK est. $\left(\widehat{\frac{\partial p}{\partial s}}\right)$	-3.19***	-1.10***	-2.96***	-0.34+
	(0.29)	(0.48)	(0.35)	(0.18)
Obs. (mil.)	18,672,092	18,737,706	19,507,407	18,221,361
	Orig. LTV		Predicted P(file)	
RK est. $\left(\widehat{\frac{\partial p}{\partial s}}\right)$	-1.46***	-2.47***	-0.43*	-2.12***
	(0.23)	(0.26)	(0.17)	(0.43)
Obs. (mil.)	23,135,018	24,751,526	12,843,168	11,846,132

Notes: The estimates correspond to the percent change in filing rate, relative to the sample average of 0.72, in response to a $1,000 increase in seizable home equity. Each estimates comes from an RKD estimated on a subset of the main sample. Columns 1 and 2 (and 3 and 4) partition the sample into groups with below and above average values for the specified characteristics. Each specification is estimated using the same specification choices as the benchmark specification. The Calonico et al. (2014) optimally chosen bandwidths are omitted for brevity. Results are similar when reweighting the observations in order to keep other observables similar across the partitions. The bottom-right variable, the probability of filing, is a predicted filing rate generated from an OLS regression of a filing indicator based on the other characteristics used here to split the sample. Statistical significance: 0.1+, 0.05*, 0.01**, and 0.01***.

Table B.7: Heterogeneity in Filing Sensitivity (OLS-Estimated Interactions)

	(1)	(2)	(3)	(4)	(5)
RK est. ($\widehat{\partial p/\partial s}$)	-1.70***	-1.21***	-1.71***	-0.68**	-1.39***
	(0.16)	(0.21)	(0.26)	(0.24)	(0.30)
$\widehat{\partial p/\partial s}\times$Unemp. % (FIPS)		-0.10***	-0.04*		
		(0.01)	(0.02)		
$\widehat{\partial p/\partial s}\times$UI % (ZIP)				6e-3	0.05***
				(6e-3)	(0.01)
$\widehat{\partial p/\partial s}\times$ln(income) (FIPS)		-0.33**	0.52**		
		(0.12)	(0.17)		
$\widehat{\partial p/\partial s}\times$ln(income) (ZIP)				0.17***	0.20***
				(0.05)	(0.06)
$\widehat{\partial p/\partial s}\times 100\Delta$ ln(HP)		1.45***	2.55***	3.36***	3.28***
		(0.27)	(0.29)	(0.29)	(0.32)
$\widehat{\partial p/\partial s}\times$FICO Score		0.02***	0.01***	0.01***	0.01***
		(5e-4)	(5e-4)	(5e-4)	(6e-4)
$\widehat{\partial p/\partial s}\times$LTV		-0.04***	-0.02***	-0.03***	-0.02***
		(3e-3)	(3e-3)	(3e-3)	(3e-3)
$\widehat{\partial p/\partial s}\times$ Lawyers			-0.01***		-0.01***
			(1e-3)		(1e-3)
Observations (mil.)	46.03	27.44	15.06	19.72	11.79

Notes: The estimates correspond to the percent change in filing rate, relative to the sample average of 0.72, in response to a $1,000 increase in seizable home equity. The table above reports results from estimating the RKD using OLS and interaction terms (i.e., interacting distance from the cutoff with a dummy for an observation being on the right versus left). OLS is equivalent to the nonparametric analog that uses a uniform kernel (as in the preferred specification), but without the bias correction and associated confidence intervals of Calonico et al. (2014). The specifications here are quadratic in equity distance, linearly control for home equity, and are estimated within the same bandwidth as the preferred specification ($67k). I interact the main term of interest (corresponding to the RKD estimate) with additional covariates. The unemployment rate is measured at the county-quarter level. The UI % is the fraction of households in a ZIP Code that received unemployment insurance benefits in the past year. I use annual measures of log median income at both the county and ZIP-level. Log house price growth is measured over the previous year (i.e., period $t-4$ to t) within each ZIP. The FICO score and LTV (loan-to-value ratio) are measured at the household-level at origination. The last covariates is the number of lawyers per 10,000, measured using annual county-level data from the ACS. Statistical significance: 0.1^{+}, 0.05*, 0.01**, and 0.01***.

Table B.8: Time Variation in Filing Sensitivity to Bankruptcy Cost

	(1) **Pre-Rec.**	(2) **Recession**	(3) **Post-Rec.**	(4) **Pre-Reform**	(5) **Rush to File**	(6) **Post-Reform**
			Panel A: Unweighted			
RKD Est.	-1.23***	-2.73***	-1.36***	-1.57***	-7.58***	-1.43***
Std. Err.	(0.28)	(0.46)	(0.28)	(0.37)	(1.55)	(0.20)
Bandwidth	72.64	56.76	70.07	86.86	66.30	77.13
Obs. (mil.)	11.12	1.64	34.51	8.63	9.12	18.71
			Panel B: Weighted for Constant Composition			
RKD Est.	-1.85***	-2.38***	-1.64***	-3.11***	-12.71***	-2.16***
Std. Err.	(0.39)	(0.47)	(0.37)	(0.55)	(2.02)	(0.25)
Bandwidth	76.61	55.13	66.04	78.99	78.22	72.89
Obs. (mil.)	7.83	1.30	29.63	6.78	9.13	16.44

Notes: The estimates correspond to the percent change in filing rate, relative to the sample average of 0.72, in response to a $1,000 increase in seizable home equity. Each column of each panel is result of estimating the RKD on different sample periods. The pre-recession period is defined as 2006 Q1 to 2007 Q4, the recession era is 2008 Q1 to 2010 Q4, and the post-recession period is 2011 Q1 to 2016 Q1. The pre-reform era is 2000 Q1 to 2005 Q2, the rush to file era includes 2005 Q3 and Q4, and the post-reform era includes 2006 Q1 to 2016 Q1. All specification choices match those of the baseline specification. Statistical significance: 0.1⁺, 0.05*, 0.01**, and 0.01***.

B.3 ARM IV: Summary Statistics

Table B.9: Summary Statistics: ARMs (Origination Characteristics)

	Mean	SD	Min.	25th	50th	75th	Max.	N
				Panel A: Libor				
Orig. Bal.	289.2	200.56	38.3	148.97	227.36	370.51	3,957.65	51,164
FICO	727.4	47.87	350	695	731	765	839	48,237
Orig. LTV	74.45	12.55	6.32	70	79.93	80	146.7	51,164
ARM Term	65.59	11.23	60	60	60	60	120	51,164
ARM Margin	2.22	0.32	-2.75	2.25	2.25	2.25	3.75	51,164
Subprime	0.07							51,164
Low/No-Doc.	56.26							47,316
Conforming	75.68							51,164
IO	42.38							50,423
Own. Occ.	82.16							50,423
				Panel B: Treasury				
Orig. Bal.	254.23	192.4	40.5	135.06	199.49	301.36	2,992	45,186
FICO	727.2	50.4	360	694	734	767	875	43,044
Orig. LTV	71.24	15.58	5.58	64.66	77.08	80	172	45,186
ARM Term	65.73	10.53	60	60	60	60	120	45,186
ARM Margin	2.55	0.49	0	2.6	2.75	2.75	5	45,186
Subprime	0.11							45,186
Low/No-Doc.	68.28							38,699
Conforming	83.81							45,186
IO	13.84							45,184
Own. Occ.	85.93							45,184

Table B.10: Summary Statistics: ARMs (Reset Characteristics)

	Mean	SD	Min.	25th	50th	75th	Max.	N
				Panel A: Libor				
Init. Rate	5.71	0.76	3.38	5.12	5.75	6.25	9	51,164
New Rate	3.4	0.48	0.88	3	3.25	3.5	8	51,164
Init. Pay.	1,522.07	1,079.53	113.92	781.77	1,182.42	1,924.36	14,643	51,164
New Pay.	1,081.23	780.29	87.09	561.6	863.26	1,348.60	11,788	51,164
Net New Rate	1.18	0.59	0	0.75	1	1.25	8	51,164
Net New Pay.	363.84	305.16	0	171.22	276.61	453.13	5,755	51,164
Balance	268.57	188.23	37.61	135.64	209.54	346.1	1,220.06	51,164
Home Eq.	50.91	230.62	-933.24	-38.97	10.93	75.39	8,827.05	51,164
BK Rate	1.04							51,164
				Panel B: Treasury				
Init. Rate	5.03	0.62	3.25	4.62	5	5.38	8.88	45,186
New Rate	3.4	0.47	1.75	3.12	3.25	3.5	6.75	45,186
Init. Pay.	1,340.42	984.69	120.96	721.73	1,056.14	1,583.87	16,063.21	45,186
New Pay.	1,127.90	861.15	113.38	590.06	876.96	1,334.40	6,966.25	45,186
Net New Rate	0.84	0.69	0	0.38	0.62	1.12	5.75	45,186
Net New Pay.	254.2	256.78	0	94.74	175.99	324.7	6,966.25	45,186
Balance	227.79	175.4	37.57	118.17	176.15	268.55	1,220	45,186
Home Eq.	148.37	296.92	-598.25	24.3	72.17	166.33	9,448	45,186
BK Rate	0.80							45,186

C Data

C.1 CoreLogic LLMA Data

The LLMA database tracks a large number of mortgages at a monthly frequency. Each mortgage can be thought of as a household, though in a principle a household could reappear in the sample under a different loan ID number if they obtain a new mortgage. Households leave the sample upon paying off their mortgage, typically when refinancing or selling their home. A large fraction of total originations appear in the dataset and many household bankruptcies are captured too. The full data contain about 10% of all household bankruptcies in the US.

C.1.1 Subset Used for Bankruptcy Generosity Analysis

To obtain the main sample used in the analysis of bankruptcy generosity, I make a number of restrictions on the full sample. Table C.1 below gives counts of observations, households (HHs), and bankruptcies after making various restrictions.

Table C.1: Number of Observations After Applying Filters

# HH-Time Obs.	# HHs	# Bankrtupcies	Avg # HH per year	Avg # BK per year
		No Filter		
4,558,381,215	102,379,198	3,550,304	5,687,733	197,239
		+ Keeping States of Interest		
883,223,242	21,393,099	592,000	1,188,506	32,889
		+ Known ZIP, Current Mortgage Balance, and Sale Price		
877,358,345	21,177,265	586,703	1,176,515	32,595
		+ Owner Occupied		
776,503,203	18,803,566	530,294	1,044,643	29,461
		+ Collapsing to Quarterly		
259,968,136	18,308,067	528,929	1,017,115	29,350
		+ Sample Households and Drop Outliers		
99,233,172	7,815,751	176,384	459,750	10,376

Reasons for Sample Restrictions

I first restrict to states that do not permit doubling. Because I cannot identify with certainty if households are married and therefore eligible to double their homestead exemption, this restriction removes any uncertainty about what is the relevant exemption limit facing the household. These states include Alaska, Arizona, Colorado, Idaho, Louisiana, Massachusetts, Minnesota, Missouri, Mississippi, North Dakota, Nebraska, Rhode Island, Vermont, and Washington. I use data from Wisconsin through 2008 Q4. Wisconsin allows households to file using federal exemptions; and beginning in 2009 the federal homestead exemption was more generous for married households in Wisconsin. In 2010, Wisconsin also began to allow married households to double their homestead exemption. I also use data on Delaware and Maryland beginning in 2005 Q1 and 2010 Q4 (respectively). I do not use earlier years for Delaware and Maryland because their homestead exemption limits were $0, which means there were no positive equity households under the exemption limit in those years.

I then drop observations missing crucial information needed to measure home equity. I also drop observations for investment properties. This is because only home equity in owner-occupied properties is eligible for protection under homestead exemptions. To get to the final line, I randomly sample 15 million households in order to reduce the computational burden and drop those with negative home equity. I drop household with negative home equity because the fraction of negative equity households likely drop discontinuously at the cutoff as no negative equity household could have positive seizable equity. Histogram plots of equity distance bins suggest this is the case (available upon request). This discontinuity could potentially bias the regression kink design. So, in order to preserve internal validity, I drop these households. I also eliminate outliers with unusually large amounts of home equity.

C.1.2 Subset Used for Liquidity Analysis

The analysis of the effect of liquidity shocks on filing also employs the LLMA data but uses a different subset and makes use of additional variables. This entails restricting the sample to households with adjustable-rate mortgages (ARMs) and the use of the rich information on the ARM contract details.

Key variables are:

ARM Index ID: This identifier specifies the rate to which the ARM is indexed during its resets. I restrict the sample to ARMs indexed by the one-year Treasury rate or one-year Libor rate (which comprise the bulk of the loans).

ARM Margin: The variable captures the margin used during the reset, specified at origination. Recall that the new rate computed during the reset equals the sum of the margin and the indexing rate.

ARM Term: This is the number of months until the first reset after origination. The most common term is 60 months. Other common terms are 84 and 120 months. In the CoreLogic sample, a large group of observations have values just below these common terms (i.e., 59, 83, and 119). This appears to be a quirk of how loan ages were recorded in CoreLogic and then used to compute the ARM term. I therefore round up the term by 1 month in this case as these appear to be miscodings

Other variables used as controls or in the robustness analysis:

IO Flag: This flag indicates whether or not an ARM features an interest-only (IO) period. During the IO period, the borrower only pays interest on the mortgage and does not make any payments towards principal.

IO Term: This is the number of months that the IO-period lasts. After this term is reached, the household begins making payments toward the mortgage's principal.

Owner-Occupied Flag: This flag identifies mortgages originated to purchase the primary home in which the borrower resides. When equal to zero, it indicates that the mortgage is either for a second home or an investment property. This distinction is important in the context of bankruptcy as equity in homes other than the primary residence is not protected in consumer bankruptcy. Additionally, this characteristic is also relevant as research on mortgage delinquency suggests investor-owners may much more willing to default strategically (Amromin, Huang, Sialm and Zhong, 2011).

C.2 Measuring Home Equity

The LLMA data report the original balance on the mortgage and its loan-to-value (LTV) ratio at origination; taking their ratio gives the value of the home at origination. When the LTV ratio is not reported, I instead use the appraised value of the home which is also sometimes reported. I prefer to use the LTV whenever possible because the appraised value is rounded to the nearest thousandth whereas the LTV is reported with greater precision.

I impute the value of homes over time using ZIP-level monthly Zillow Home Value Indexes. To minimize measurement error, I use Zillow's indexes for one, two, three, four, and five-plus bedroom homes. Because the actual bedroom count is not available for most of the observations, I assign a bedroom count based on the proximity of the actual sale price to different index levels. That is, if a home sold for $100,000 and in that month and ZIP code one-bedrooms were selling for $95,000 and two-bedrooms sold for $ 120,000 on average, I classify the home as a one-bedroom for the purpose of updating the home value over time.

The relevant home equity measure is how a household's homes would be valued in bankruptcy. In practice, bankruptcy courts value a filer's home based on the recent transaction prices of nearby homes. However, I cannot verify how individual courts define the geography of a "nearby" home, how "recent" is defined, nor what house price data they use. While my method for imputing a home's value is closely aligned conceptually with how courts actually value homes in bankruptcy cases, this procedure is still subject to measurement error.

A second source of error is that I cannot consistently observe additional loans collateralized by the same property (e.g., second mortgages). This means in some cases equity could be overstated. But this error is likely to be small and rare as these additional types of loans are fairly uncommon at a given point in time and typically make up a small portion of households' total mortgage debt.

C.3 Homestead Exemption Data

To identify the relevant statutes specifying homestead exemption, I used Elias (2011). From the laws I collected information on homestead exemption levels, any rules governing the updating the homestead exemption, whether or not married joint filers could double, and whether or not the filer(s) could use federal exemptions.

Some states specify that homestead exemptions are to be updated at a given frequency (typically every three years) and that the new level will be based on inflation since the last update. Variations on this policy include Minnesota, where exemptions are updated once CPI growth exceeds 10% since the last update. Since these laws specify the rule, an initial level, but not always the actual changes, I used state government announcements of exemption level changes to fill in the exemption levels for the years since the adoption of rules-based updating. For Michigan, I could not locate such announcements for several years and interpolated the updates using the stated rule and the CPI.

A subset of states allow households to choose between their own exemptions and a set of federal exemptions. To identify the relevant kink facing a household, I track whether or not the federal homestead exemption level is more or less generous. This is imperfect as a household with significant retirement wealth but little housing wealth may prefer their state's exemption if protection for retirement savings is much more generous. Even if their state's homestead exemption was less generous than the federal exemption, the relevant homestead exemption limit would be the federal one. This is unlikely to be a major problem as most states that are more generous in terms of the homestead exemption are more generous in terms of other exemptions, compared to the federal exemptions. But ideally I would have sufficient balance sheet information to always know which kink is relevant to each household, but unfortunately this is not available in the LLMA data. A household may also be forced to use the federal exemptions if they do not meet residency requirements to use their state's exemptions.

C.4 Other Data

County-Level Economic Data

I obtain county-level unemployment rate data and median income data using GeoFRED. The unemployment data are originally from the monthly non-seasonally adjusted series produced by the Bureau of Labor Statistics (BLS). I aggregate the panel to a quarterly level by using the end-of-quarter rate. The original source for the annual median income is the Small Area Income and Poverty Estimates from the US Census Bureau.

IRS SOI Data

The IRS SOI data are aggregated to the ZIP-code level from the universe of US tax returns. Beginning in 2005, the data contain the number of tax filers that claimed unemployment benefits at some point during a given tax year. Median income, however, is not directly reported. Beginning in 2005, the data began to report the fraction of households in five to six income bins and the average income within each bin. I impute median income as a weighted average of the bins surrounding the 50^{th} percentile, where values are upweighted based on the relative proximity of the bin's percentile to 50%.

CPI Data

I obtain quarterly CPI data from the BLS. Throughout I adjust nominal quantities to be in terms of 2010 dollars.

D Robustness

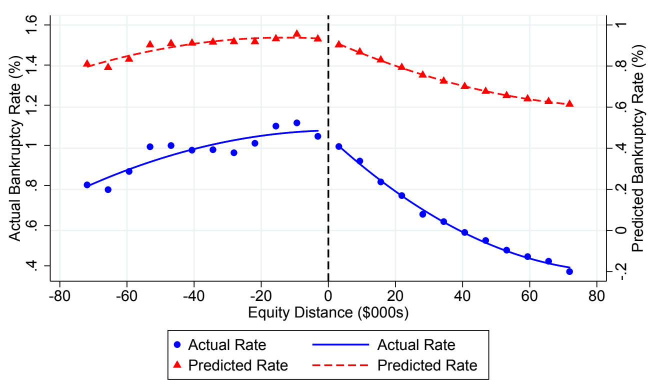
D.1 RKD: Testing for Smoothness in Predetermined Covariates

Table D.1: Estimates of Jumps and Kinks Associated with Predetermined Covariates

	(1)	(2)	(3)	(4)	(5)	(6)
Cov. used:	All	FICO	Orig. LTV	Orig. Date FE	Δ ln(HP)	County $\times$ Time FE
	Panel B: Level Change Estimation Results (RDD)					
Estimate	-0.15	-0.01	-0.10	-2e-3	0.03	-0.06
Std. Err.	(0.32)	(0.14)	(0.05)	(0.09)	(0.04)	(0.14)
Bandwidth	5.08	8.20	3.97	4.88	4.4	5.92
Obs.	2,713,153	6,534,186	3,735,073	4,587,112	2,703,685	5,558,075
	Panel B: Slope Change Estimation Results (RKD)					
Estimate	-0.04	0.05	0.02	-4e-3	0.02	0.16***
Std. Err.	(0.04)	(0.04)	(0.03)	(0.04)	(0.01)	(0.02)
Bandwidth	25.24	16.01	8.46	10.92	14.06	21.63
Obs.	12,543,506	12,392,443	7,899,000	10,121,395	8,408,310	19,082,722

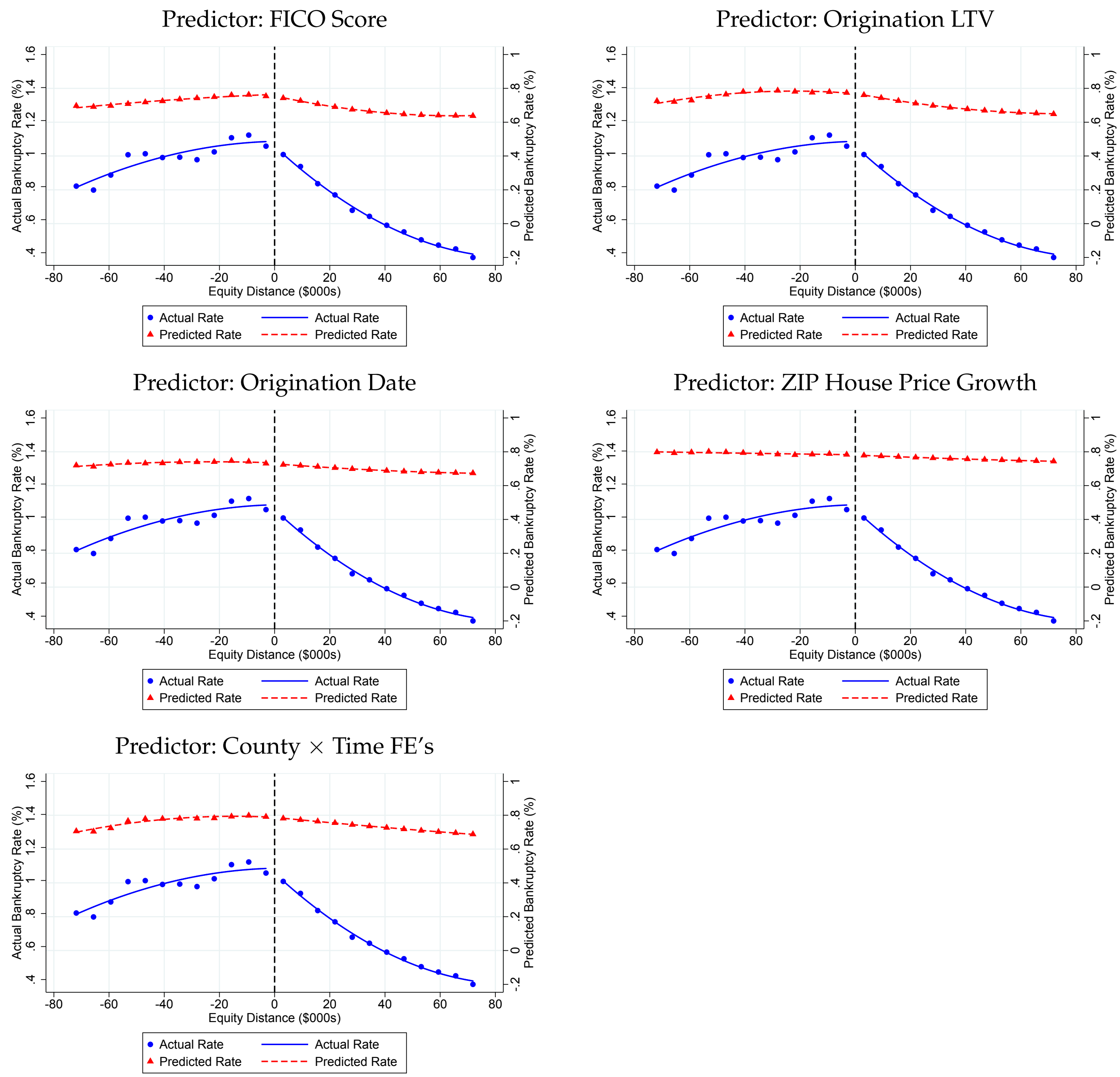
Notes: This table presents results from tests for jumps and kinks in predetermined covariates. The column indicates the covariate or group of covariates. For each test column, I compute a predicted bankruptcy rate for each household using a linear probability model, where the predictors are the covariates noted in the top part of the table. I then use this fitted variable as the outcome variable in an RDD (panel A) and RKD (panel B). The RDD and RKD test for a jump and kink in the predicted probability of filing with respect to a household's actual equity distance. I make the same estimation choices for the RDD and RKD as in the main analysis (uniform kernel, bandwidth selection and confidence intervals à la Calonico et al. (2014), a linear control for home equity, and a quadratic RKD and linear RDD). Column 1 presents the main result which jointly tests for kinks associated with the covariates. The covariates used are the FICO score at origination, the origination LTV of the household's mortgage, dummies for the date of origination, ZIP-level house price growth over the previous year (i.e., from the beginning of $t-4$ to the end of $t-1$), and county-time fixed effects.

Figure D.1: Actual vs. Predicted Bankruptcy Filing Rates Over Equity Distance



Notes: The points are mean quarterly filing rates for equity distance bins. The lines are generated by fitting a quadratic polynomial to the individual observations on each side of the kink. The difference in the slopes evaluated at the kink corresponds to an RKD estimate. The difference in levels corresponds to an RDD estimate. The "predicted rate" values are obtained for each observations as the fitted value from a regression projecting actual bankruptcy outcomes on covariates described in the text. The actual bankruptcy rate exhibits a sharp kink while the predicted values do not. Statistical tests reject that there is a jump or a kink in the predicted filing rate at the cutoff.

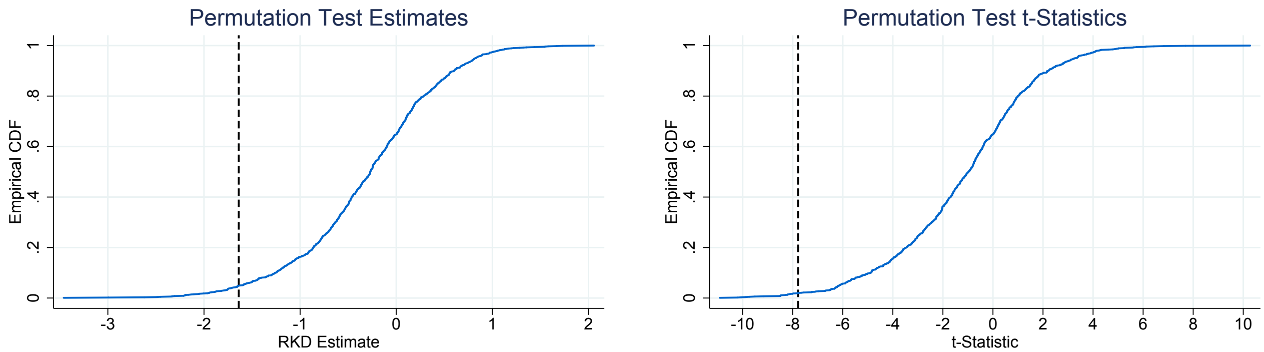
Figure D.2: Actual vs. Predicted Bankruptcy Filing Rates



Notes: These figures plot the actual and predicted mean quarterly filing rates for equity distance bins and polynomial estimates of the rates for both sides of the cutoff. Each graph reproduces the same plot for the actual filing rate (the solid blue line and circles). The red dashed line and points are for the predicted filing rate one obtains from fitting a linear probability model to a specified covariate. The actual bankruptcy rate exhibits a sharp kink while the predicted values do not. Results from formally testing for jumps and kinks are reported in Table D.1.

D.2 RKD Permutation Test

Figure D.3: Distribution of RKD Statistics From Permutation Test



Notes: These graphs display the distribution of the 1,000 coefficients and t-statistics generated in the permutation test. The dashed line marks the actual coefficient and t-statistic obtained in the main analysis.

D.3 RKD: Smoothness in the Probability of Sale

A potential identification concern is that there might be a kink in the probability that households sell their home as a function of their equity distance. If there were such a kink, the kink in filing rates at the cutoff could be due to households reacting to the increased probability of having to sell their home. There are large fixed costs to selling a home and households may also incur psychological costs of losing a home to which they have become attached.

While households are still responding to a kink in the cost, in this situation we cannot interpret the RKD estimator as the direct effect of seizable equity on filing. Rather, it would be the direct effect *plus* the indirect effect through the probability that households would have to sell their homes. Because the RKD estimator still differences out GE effects, we can still interpret the estimator as the behavioral response of filing to an increased cost of bankruptcy, but where \$1 increase in seizable equity is also affecting costs through the probability of having to sell one's home. While this estimator still captures an interesting economic response, failure to isolate the direct effect makes this object not perfectly match the partial derivative discussed in the conceptual framework.

To assess whether or not this caveat is likely relevant, I merge in sale data from CoreLogic's Supplementary Loan Analytics database to the LLMA data, which contains information on home sales after the mortgage is originated. I test for this kink formally within the subset of households that file for bankruptcy. To do so, I estimate an RKD for the subset of filing households where the outcome is now the probability that a filer sells their home. Visually, we can see in Figure D.4 below that, although the probability of selling increases with seizable equity above the kink and is flat below, the lines look smooth and continuous near the cutoff.

I estimate the RKD using the same specification choices as the benchmark. Table D.2 reports results from this estimation. The parameter estimate implies a 0.82% increase in the probability of selling, conditional on filing, per \$1000 increase in seizable equity.

Should we have expected such a jump or kink to exist? It is likely that this probability would vary smoothly with equity distance as households are able to use other resources to pay for their seizable equity in bankruptcy. For example, if a household has \$10 in seizable equity, they likely can

Figure D.4: Distribution of RKD Statistics From Permutation Test

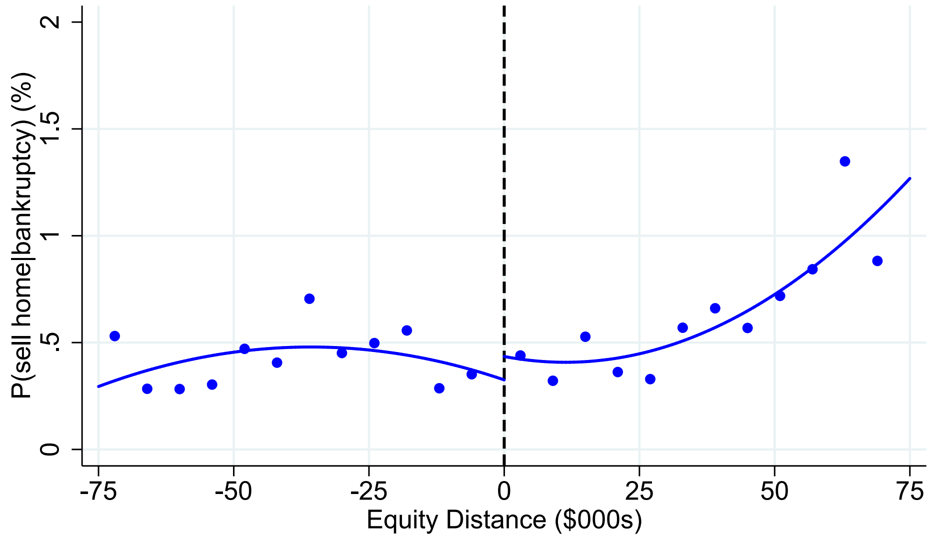


Table D.2: Testing for a Kink in the Probability of Sale Conditional on Filing

Estimate	0.82
Std. Err.	(12.851)
Bandwidth	64.06
Obs.	72,672

Notes: This entails using a uniform kernel, a quadratic polynomial in equity distance, and bias correction and confidence interval construction as in Calonico, Cattaneo and Titiunik (2014). I scale the coefficients so that the units corresponds to the percent change in the quarterly probability of selling per $1000 increase in seizable equity. Statistical significance: 0.1^{+}, 0.05^{*}, 0.01^{**}, and 0.01^{***}.

liquidate other protected assets (e.g., cash in savings accounts or retirement accounts).[63] Consider two households close to the cutoff. One has $0 in seizable home equity and is just below the cutoff. The other has $10 in seizable in equity. Both are unlikely to sell their home in bankruptcy, as the household with $10 above the cutoff likely can liquidate other assets to afford this $10 payment to creditors. As seizable equity increases, we can expect it to become more likely that households will not have the resources to prevent their home from being sold.

D.4 RKD in Subset with Known Sale Prices

To assess how measurement error affect the RKD, I estimate the RKD within a subset of the data in which measurement error is much less likely to arise. This subset is the set of households selling their home in the quarter of interest. Relative to the main subsample used in the RKD, I simply

[63]Households are not allowed to reallocate wealth across assets before bankruptcy in order to minimize payoffs. If a judge suspects this is the case, they have discretion to dismiss the petition to file. But after the amount that must be repaid to creditors is calculated by the court, households have wide latitude to determine which assets they will liquidate to meet this obligation.

Table D.3: RKD Using Actual Sale Price

Estimate	-2.18
	(2.00)
Bandwidth	186.88
Obs.	200,988

Notes: This entails using a uniform kernel, a quadratic polynomial in equity distance, and bias correction and confidence interval construction as in Calonico, Cattaneo and Titiunik (2014). I scale the coefficients so that the units corresponds to the percent change in the quarterly probability of selling per $1000 increase in seizable equity. Statistical significance: 0.1+, 0.05*, 0.01**, and 0.01***.

drop observations for quarters during which the household does not sell their home. For sellers, we can see the actual sale price of the home.

This exercise is not perfect as there could still be two sources of measurement error. The first is any omitted borrowing against the home, this could lead to overstating the household's equity. The second is that the actual sale price could differ from the expected sale price, which is the actual covariate on which households base their filing decision.

I mitigate the first concern by further restricting the subset to households for whom it is known whether or not they had a piggyback mortgage at origination. However, there could be future home equity loans still missing from the computation of the household's home equity. The second concern is likely to be minor since during the bankruptcy process, tools such as appraisals are often used, which should give the households a reasonably accurate forecast of the price at which their home will sell.

Table D.3 below reports the estimation results. The point estimate of -2.18 is very close to the benchmark estimate of -2.91. However, estimation is much less precise when only using this small subset of households that sold their house in the quarter under consideration.

D.5 ARM IV: Testing for Characteristics Correlated with Libor Indexation

Table D.4: Testing for Predictors of Libor Indexation

	(1)	(2)	(3)	(4)	(5)
Margin$_{ic}$	-35.40***	-36.01***	-38.79***	-39.88***	-44.88***
	(1.71)	(1.74)	(2.02)	(2.20)	(3.30)
Old Pay.$_{ic}$	0.04	0.05	0.08	0.04	0.03
	(0.05)	(0.05)	(0.05)	(0.04)	(0.06)
Orig. FICO$_{ic}$	0.001	-0.01**	-0.01**	-0.01	-0.01
	(4e-3)	(4e-3)	(4e-3)	(4e-3)	(0.01)
Orig. LTV$_{ic}$	0.27***	0.22***	0.20***	0.19***	0.21***
	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)
ln(Orig. Bal.)$_{ic}$	0.03*	0.01	0.002	0.02*	0.02
	(0.01)	(0.01)	(0.01)	(0.01)	(0.02)
UR$_{ic}$ %	-0.29	-0.33	-0.46	-0.57	
	(0.41)	(0.36)	(0.38)	(0.40)	
ln(Med. Inc.)$_{ic}$	0.18	0.23	0.24	0.31	
	(0.22)	(0.19)	(0.18)	(0.19)	
Δ ln(HP)$_{ic}$	-4e-4	3e-3	3e-3	0.01	3e-3
	(4e-3)	(4e-3)	(4e-3)	(5e-3)	(0.01)
Observations	61,482	61,482	61,482	61,482	61,482
Time FE	✓	✓	✓	✓	✓
County FE	✓	✓	✓	✓	✓
Loan Age FE		✓	✓	✓	✓
Loan Age x Time FE			✓	✓	✓
ZIP FE				✓	✓
County x Time FE					✓

Notes: This table reports OLS estimates from regressing an indicator for Libor indexation on borrower's mortgage-level and regional characteristics. I flatten the data to include one entry per mortgage. The regional characteristics are the values at the time of the reset. I scale the coefficients so that they correspond to the percentage change in the probability of being indexed to Libor. For example the FICO estimate in column 5 implies a -0.01 percentage point decrease in the likelihood of being Libor given a one point increase in the FICO score at origination; the estimate on log median income implies a 0.0031 percentage point increase in the probability of being indexed to Libor given a 1 log point increase in median income. House price growth is measured over the past year at the ZIP code level. Statistical significance: 0.1^{+}, 0.05*, 0.01**, and 0.01***.

D.6 ARM IV: Placebo Test

Table D.5: Testing if Libor-Indexation Predicts Higher Bankruptcy Prior to Resets

	(1)	(2)	(3)	(4)
Libor_{ic}	3.93	0.25	-0.53	3.70
	(10.93)	(10.68)	(10.83)	(11.34)
Margin_{ic}	-8.06	-8.78	-11.48	-2.51
	(11.34)	(11.37)	(11.60)	(11.01)
Old Pay_{ic}	-2.13**	-1.95**	-1.94**	-1.91*
	(0.75)	(0.75)	(0.74)	(0.78)
Orig. FICO_{ic}	-0.78***	-0.78***	-0.77***	-0.80***
	(0.10)	(0.10)	(0.10)	(0.11)
Orig. LTV_{ic}	0.81**	0.86**	0.92***	0.96***
	(0.26)	(0.27)	(0.27)	(0.28)
$\ln(\text{Home Eq.})_{ict}$	-1.07	-0.65	-0.75	-0.56
	(0.63)	(0.62)	(0.64)	(0.76)
$\ln(\text{Bal.})_{ict}$	15.68	9.99	10.03	8.01
	(16.25)	(16.30)	(16.34)	(17.36)
Obs.	1,094,998	1,094,998	1,094,998	1,094,998
Time FE	✓	✓	✓	✓
County FE	✓	✓	✓	✓
Loan Age FE		✓	✓	✓
Loan Age x Time FE			✓	✓
County x Time FE				✓

Notes: The sample used in these regressions is monthly data on bankruptcy filings in the year *prior* to an ARM reset. All regressions include the same household-level controls as in the main IV specification (Table 4). Standard errors are clustered by county. I scale coefficient and standard errors on the Libor indicator so that it corresponds to the difference in the filing rate (relative to the sample mean), which makes it easier to compare to the IV estimate (whose units are the relative change in the filing rate per $1,000). Statistical significance: 0.05*, 0.01**, 0.001***.

D.7 ARM IV: Testing for Anticipatory Behavior

Table D.6: Pre-Reset Bankruptcy Filings versus the Current Index Rate Value

	(1)	(2)	(3)	(4)
IndexRate_{ict}	-0.04	-3.90	-5.90	-1.53
	(11.67)	(11.56)	(11.68)	(12.44)
IndexRate_{ict} x 2007	-20.38	-19.69	-14.81	-11.56
	(12.01)	(12.24)	(12.05)	(14.37)
Margin_{ic}	-11.58	-12.21	-14.92	-5.74
	(11.26)	(11.31)	(11.53)	(11.06)
Old Pay_{ic}	-2.13**	-1.95**	-1.94**	-1.91*
	(0.75)	(0.75)	(0.74)	(0.78)
Orig. FICO_{ic}	-0.78***	-0.78***	-0.77***	-0.80***
	(0.10)	(0.10)	(0.10)	(0.11)
Orig. LTV_{ic}	0.83**	0.88***	0.94***	0.97***
	(0.26)	(0.26)	(0.27)	(0.28)
ln(Home Eq.)$_{ict}$	-1.09	-0.65	-0.76	-0.59
	(0.63)	(0.61)	(0.63)	(0.76)
ln(Bal.)$_{ict}$	15.78	9.87	10.04	8.15
	(16.24)	(16.29)	(16.34)	(17.36)
Obs.	1,094,998	1,094,998	1,094,998	1,094,998
Time FE	✓	✓	✓	✓
County FE	✓	✓	✓	✓
Loan Age FE		✓	✓	✓
Loan Age x Time FE			✓	✓
County x Time FE				✓

Notes: The sample used in these regressions is monthly data on bankruptcy filings in the year *prior* to an ARM reset. All regressions include the same household-level controls as in the main IV specification (Table 4). Standard errors are clustered by county. I scale coefficient and standard errors on the IndexRate_{ict} covariates so that the coefficient corresponds to the relative (percent) change in the filing rate per 1% increase in the index rate. The second line interacts the index rate with an indicator for whether or not the current year is 2007. Statistical significance: 0.05*, 0.01**, 0.001***.

E RKD Measurement Error

E.1 Consistency Result for Parametric Framework

I characterize the effect of measurement error in setting where I assume the outcome of interest, y, is a quadratic function of the running variable x and the policy variable s within a given bandwidth ($x \in [-h, h]$). Unobserved additive factors can also influence the outcome and be correlated with x. This particular representation is a special case of the more general relationship allowed for in Card et al. (2015).

I proceed by first showing that model parameters identify the local average response identified by the RK estimand of Card et al. (2015). I then propose an OLS-based estimator and show it is consistent for the local average response in the absence of measurement error. [64] With measurement error, the estimator is biased in the opposite direction of the sign of the local average response. I propose an alternative estimator that uses information on a subset of data with both correctly and mis-measured observations of the running variable to consistently estimate the local average response. I do not characterize the standard errors associated with the measurement-error-adjusted estimator. In practice, a researcher can bootstrap the estimation to obtain a confidence interval to conduct inference.[65] A useful pursuit for future research would be to pursue a similar result with weaker parametric assumptions, closer to those of Card et al. (2015) and Fan and Gijbels (1996).

E.1.1 Setting and Identification of the Local Average Response

The outcome y is a quadratic function of the running variable x and the policy variable s for $x \in [-h, h]$, where h is a given positive constant. Unobserved factors that also affect filing do so additively through ε. The parameter of interest is β_1^s:

$$y = \beta_0 + \beta_1^x x + \beta_2^x x^2 + \beta_1^s s + \beta_2^s s^2 + \varepsilon.$$

The policy variable s is a continuous, linear, kinked function of x in this region:

$$s = S(x) \equiv \begin{cases} \gamma^+ x & : x \in [0, h] \\ \gamma^- x & : x \in [-h, 0) \end{cases}$$

where $\gamma^+ \neq \gamma^-$. Below I assume $x \in [-h, h]$ throughout, but will suppress explicitly conditioning on this event to minimize notation. I assume $\mathbb{E}(\varepsilon) = 0$ and I do not rule out that x is correlated with ε (i.e., allowing $\mathbb{E}(x\varepsilon) \neq 0$). We can see that β_1^s identifies the local average response by taking the conditional expectation of partial derivative of y with respect to x and evaluating it at $x = 0$:

$$\mathbb{E}\left(\frac{\partial y}{\partial s}\bigg| X = 0\right) = \beta_1^s + \beta_2^s 2 \underbrace{\mathbb{E}(s|X = 0)}_{=0} = \beta_1^s.$$

Define the parametric RK estimand

$$\tau^{PRK} = \frac{\lim_{x_0 \to 0+} \frac{dE(Y|X=x)}{dx}\Big|_{x_0=0} - \lim_{x_0 \to 0-} \frac{dE(Y|X=x)}{dx}\Big|_{x_0=0}}{\gamma^+ - \gamma^-} \tag{14}$$

[64] For ease of exposition I use OLS, but one could also use weighted least squares with weighting to match the weights implied by a chosen kernel for a non-parametric estimator.

[65] I do not prove consistency of the bootstrap in this setting.

The following proposition shows when the difference in the limits of the total derivative of the conditional expectation at the cutoff $x = 0$, divided by the change in slope of the rule related x and s, identifies β_1^s.

Proposition E.1: Identification of the Local Average Response

Given a continuous, linear, kinked rule $S(x)$, if the derivative of the conditional expectation of the unobserved factors ε is continuous at $x = 0$:

$$\lim_{x_0 \to 0+} \left. \frac{d\mathbb{E}(\varepsilon|X = x)}{dx} \right|_{x_0=0} = \lim_{x_0 \to 0-} \left. \frac{d\mathbb{E}(\varepsilon|X = x)}{dx} \right|_{x_0=0} \equiv \Omega$$

then the parametric RK estimand identifies the local average response β_1^s:

$$\tau^{PRK} = \beta_1^s.$$

Proof. First, rewrite the outcome y as

$$y = \begin{cases} \beta_0 + \underbrace{\left(\beta_1^x + \beta_1^s \gamma^+\right)}_{\equiv \beta_1^+} x + \underbrace{\left(\beta_2^x + \beta_2^s \left(\gamma^+\right)^2\right)}_{\equiv \beta_2^+} x^2 + \varepsilon & x \geq 0 \\ \beta_0 + \underbrace{\left(\beta_1^x + \beta_1^s \gamma^-\right)}_{\equiv \beta_1^-} x + \underbrace{\left(\beta_2^x + \beta_2^s \left(\gamma^-\right)^2\right)}_{\equiv \beta_2^-} x^2 + \varepsilon & x < 0 \end{cases}.$$

Taking the total derivative of the expectation of y conditional on $X = x_0$ as x_0 approaches zero from above and below yields:

$$\lim_{x_0 \to 0+} \left. \frac{dE(Y|X = x)}{dx} \right|_{x_0=0} = \beta_1^x + \beta_1^s \gamma^+ + \Omega$$
$$\lim_{x_0 \to 0-} \left. \frac{dE(Y|X = x)}{dx} \right|_{x_0=0} = \beta_1^x + \beta_1^s \gamma^- + \Omega.$$

Taking the difference of these limits (the numerator of τ^{PRK}) gives

$$\lim_{x_0 \to 0+} \left. \frac{dE(Y|X = x)}{dx} \right|_{x_0=0} - \lim_{x_0 \to 0-} \left. \frac{dE(Y|X = x)}{dx} \right|_{x_0=0} = \beta_1^s(\gamma^+ - \gamma^-).$$

And dividing the above by $(\gamma^+ - \gamma^-)$ gives β_1^s. $\square$

Using the notation from the proof, we can write

$$\beta_1^s = \frac{\beta_1^+ - \beta_1^-}{\gamma^+ - \gamma^-}$$

The next setting considers estimation of $\beta_1^+ - \beta_1^-$. For what follows, let $\beta^+ = (\beta_0^+, \beta_1^+, \beta_2^+)'$ and $\beta^- = (\beta_0^-, \beta_1^-, \beta_2^-)'$.

E.1.2 Estimator and Consistency with No Measurement Error

Suppose we use OLS to estimate the positive and negative portions of the equations.[66] Let $\mathbf{X}^+$ denote the $(N^+ \times 3)$ matrix whose first column is a vector of ones, the second is the vector of x's such that $x \geq 0$ and third contains the square of these x's. Let Y^+ denote the $(N^+ \times 1)$ of corresponding y values. Below I'll use lower case letters to denote individual observations, e.g., $\mathbf{x}^+ = (1, x, x^2)'$ for a particular x. We can similarly define $\mathbf{X}^-$, $\mathbf{x}^-$, Y^-, and N^-. Let $\mathbb{E}^+$ denote the expectation conditional on $x \geq 0$. Consider the following OLS estimator for $\widehat{\beta}^+$:

$$
\begin{aligned}
\widehat{\beta}^+ &= \left(\mathbf{X}^{+\prime}\mathbf{X}^+\right)^{-1}\mathbf{X}^{+\prime}Y^+ \\
&= \left[\sum_{i=1}^{N^+}(\mathbf{x}_i^+\mathbf{x}_i^{+\prime})/N^+\right]^{-1}\left[\sum_{i=1}^{N^+}(\mathbf{x}_i^+ y_i)/N^+\right] \\
&\xrightarrow{p} \left[\mathbb{E}^+\left(\mathbf{x}^+\mathbf{x}^{+\prime}\right)\right]^{-1}\mathbb{E}^+\left(\mathbf{x}^+ y^+\right) \\
&= \left[\mathbb{E}^+\left(\mathbf{x}^+\mathbf{x}^{+\prime}\right)\right]^{-1}\mathbb{E}^+\left(\mathbf{x}^+\mathbf{x}^{+\prime}\beta^+\right) + \left[\mathbb{E}^+\left(\mathbf{x}^+\mathbf{x}^{+\prime}\right)\right]^{-1}\mathbb{E}^+\left(\mathbf{x}^+\varepsilon^+\right) \\
&= \beta^+ + \left[\mathbb{E}^+\left(\mathbf{x}^+\mathbf{x}^{+\prime}\right)\right]^{-1}\mathbb{E}^+\left(\mathbf{x}^+\varepsilon^+\right).
\end{aligned}
$$

For an analogous $\widehat{\beta}^-$ we can obtain

$$\widehat{\beta}^- \xrightarrow{p} \beta^- + \left[\mathbb{E}^-\left(\mathbf{x}^-\mathbf{x}^{-\prime}\right)\right]^{-1}\mathbb{E}^-\left(\mathbf{x}^-\varepsilon^-\right).$$

If $\mathbb{E}^+\left(\mathbf{x}^+\varepsilon^-\right) = \mathbb{E}^-\left(\mathbf{x}^-\varepsilon^-\right)$, then the omitted variables bias is the same above and below $x = 0$ and

$$\widehat{\beta}^+ - \widehat{\beta}^- \xrightarrow{p} \beta^+ - \beta^- = \beta_1^s(\gamma^+ - \gamma^-).$$

E.1.3 Estimator and Consistency *with* Measurement Error

Now suppose the true relationship is

$$y = \beta_0 + \beta_1^x x^\star + \beta_2^x x^{\star 2} + \beta_1^s s^\star + \beta_2^s s^{\star 2} + \varepsilon$$

and $s^\star$ is still a kinked linear function of $x^\star$:

$$s^\star = \begin{cases} \gamma^+ x^\star & : x^\star \geq 0 \\ \gamma^- x^\star & : x^\star < 0 \end{cases}.$$

But we only observe $x = x^\star + \mu$ where μ is zero mean noise (measurement error) and $\mathbb{E}(x^\star\mu) = \mathbb{E}(\varepsilon\mu) = 0$. Now our estimator will be biased. To see this, first note that the probability limit of the above-the-cutoff estimator is

$$\widehat{\beta}^+ \xrightarrow{p} \left[\mathbb{E}^+\left(\mathbf{x}^+\mathbf{x}^{+\prime}\right)\right]^{-1}\mathbb{E}^+\left(\mathbf{x}^+ y^+\right)$$

where here the superscript corresponds to observations with $x \geq 0$, which does not guarantee $x^\star \geq 0$. The expectation $\mathbb{E}^+$ conditions on the mis-measured running variable being positive

[66] In practice, you can jointly estimate them by adding an interaction for an indicator for whether or not $x \geq 0$, but I avoid writing it this way to minimize notation.

$(x \geq 0)$ too. Plugging in for y^+ we must now consider the case when the signs of x and the true value $x^\star$ differ:

$$\begin{aligned}
\widehat{\beta}^+ &\xrightarrow{p} \left[\mathbb{E}^+\left(\mathbf{x}^+\mathbf{x}^{+\prime}\right)\right]\mathbb{E}^+\left\{\mathbf{x}^+\mathbf{x}^{\star+\prime}\left[\mathbf{1}(x^\star \geq 0)\beta^+ + \mathbf{1}(x^\star < 0)\beta^-\right]\right\} + \underbrace{\left[\mathbb{E}^+\left(\mathbf{x}^+\mathbf{x}^{+\prime}\right)\right]^{-1}\mathbb{E}^+\left(\mathbf{x}^+\varepsilon^+\right)}_{\Omega^+} \\
&= \left[\mathbb{E}^+\left(\mathbf{x}^+\mathbf{x}^{+\prime}\right)\right]^{-1}\left\{\mathbb{E}^+\left[\mathbf{x}^+\mathbf{x}^{\star+\prime}\mathbf{1}(x^\star \geq 0)\right]\beta^+ + \mathbb{E}^+\left[\mathbf{x}^+\mathbf{x}^{\star+\prime}\mathbf{1}(x^\star < 0)\right]\beta^-\right\} + \Omega^+.
\end{aligned}$$

Note also that we can write

$$\begin{aligned}
\mathbb{E}^+\left[\mathbf{x}^+\mathbf{x}^{\star+\prime}\mathbf{1}(x^\star \geq 0)\right] &= (1-\pi^+)\mathbb{E}^+\left[\mathbf{x}^+\mathbf{x}^{\star+\prime}|(x^\star \geq 0)\right] \equiv (1-\pi^+)\mathbb{E}^{++}\left(\mathbf{x}^+\mathbf{x}^{\star+}\right) \\
\mathbb{E}^+\left[\mathbf{x}^+\mathbf{x}^{\star+\prime}\mathbf{1}(x^\star < 0)\right] &= \pi^+\mathbb{E}^+\left[\mathbf{x}^+\mathbf{x}^{\star+\prime}|(x^\star < 0)\right] \equiv (1-\pi^+)\mathbb{E}^{+-}\left(\mathbf{x}^+\mathbf{x}^{\star+}\right)
\end{aligned}$$

where $\pi^+ = P(x^\star < 0|x \geq 0)$, the probability that the true $x^\star$ is negative given that the observed and mis-measured x is positive. Intuitively, this is the probability that the mis-measured x was assigned to the wrong side in terms of its corresponding $x^\star$.

To further simplify notation, let $\Sigma_+ = \mathbb{E}^+\left(\mathbf{x}^+\mathbf{x}^{+\prime}\right)$, $\Sigma_{++} = \mathbb{E}^{++}\left(\mathbf{x}^+\mathbf{x}^{\star+}\right)$, $\Sigma_{+-} = \mathbb{E}^{+-}\left(\mathbf{x}^+\mathbf{x}^{\star+}\right)$. We can rewrite the probability limit of the estimator as

$$\widehat{\beta}^+ \xrightarrow{p} \Sigma_+^{-1}\Sigma_{++}(1-\pi^+)\beta^+ + \Sigma_+^{-1}\Sigma_{+-}\pi^+\beta^- + \Omega^+.$$

The first term is proportional to the coefficient above $x = 0$ and the probability that the true $x^\star$ also has a positive sign. The second is proportional to the coefficient below $x = 0$ and the probability that the true $x^\star$ actually has a negative sign. The last term is the omitted variables bias. We can similarly obtain

$$\widehat{\beta}^- \xrightarrow{p} \Sigma_-^{-1}\Sigma_{--}(1-\pi^-)\beta^- + \Sigma_-^{-1}\Sigma_{-+}\pi^-\beta^+ + \Omega^-$$

where $\pi^- = P(x^\star \geq 0|x < 0)$ and the Σ covariance matrices are defined similarly. Supposing that $\Omega^+ = \Omega^-$, the probability limit of the difference between these estimates is now

$$\widehat{\beta}^+ - \widehat{\beta}^- \xrightarrow{p} \Sigma_+^{-1}\Sigma_{++}(1-\pi^+)\beta^+ + \Sigma_+^{-1}\Sigma_{+-}\pi^+\beta^- - \Sigma_-^{-1}\Sigma_{--}(1-\pi^-)\beta^- - \Sigma_-^{-1}\Sigma_{-+}\pi^-\beta^+. \tag{15}$$

Proposition E.2: Consistency for Quadratic RKD with Measurement Error

If $\Sigma_+^{-1} = \Sigma_-^{-1} \equiv \Sigma$ *and* $\mathbb{E}(\mathbf{x}\mathbf{x}^{\star\prime}|x \geq 0) = \mathbb{E}(\mathbf{x}\mathbf{x}^{\star\prime}|x < 0)$ *then*

$$\widehat{\beta}^+ - \widehat{\beta}^- \xrightarrow{p} \Sigma^{-1}\widetilde{\Sigma}_\star(\beta^+ - \beta^-) \tag{16}$$

where

$$\widetilde{\Sigma}_\star \equiv \Sigma_{++}(1-\pi^+) - \Sigma_{-+}\pi^-.$$

Proof. First, we can use the law of total probability to rewrite

$$\begin{aligned}
\mathbb{E}(\mathbf{x}\mathbf{x}^{\star\prime}|x \geq 0) &= \mathbb{E}(\mathbf{x}\mathbf{x}^{\star\prime}|x^\star \geq 0, x \geq 0)P(x^\star \geq 0|x \geq 0) + \mathbb{E}(\mathbf{x}\mathbf{x}^{\star\prime}|x^\star < 0, x \geq 0)P(x^\star < 0|x \geq 0) \\
&= \Sigma_{++}(1-\pi^+) + \Sigma_{+-}\pi^+
\end{aligned}$$

And similarly:

$$\mathbb{E}(\mathbf{x}\mathbf{x}^{\star\prime}|x < 0) = \Sigma_{--}(1-\pi^-) + \Sigma_{-+}\pi^-.$$

Then

$$\Sigma_{++}(1-\pi^{+}) + \Sigma_{+-}\pi^{+} = \Sigma_{--}(1-\pi^{-}) + \Sigma_{-+}\pi^{-}$$
$$\Sigma_{++}(1-\pi^{+}) - \Sigma_{-+}\pi^{-} = \Sigma_{--}(1-\pi^{-}) - \Sigma_{+-}\pi^{+} \equiv \widetilde{\Sigma}_{\star}$$

Grouping by β^{+} and β^{-}, we then have

$$\begin{aligned}
\widehat{\beta}^{+} - \widehat{\beta}^{-} \xrightarrow{p} & \Sigma_{+}^{-1}\Sigma_{++}(1-\pi^{+})\beta^{+} + \Sigma_{+}^{-1}\Sigma_{+-}\pi^{+}\beta^{-} - \Sigma_{-}^{-1}\Sigma_{--}(1-\pi^{-})\beta^{-} - \Sigma_{-}^{-1}\Sigma_{-+}\pi^{-}\beta^{+} \\
= & \left[\Sigma_{+}^{-1}\Sigma_{++}(1-\pi^{+}) - \Sigma_{-}^{-1}\Sigma_{-+}\pi^{-}\right]\beta^{+} - \left[\Sigma_{-}^{-1}\Sigma_{--}(1-\pi^{-}) - \Sigma_{+}^{-1}\Sigma_{+-}\pi^{+}\right]\beta^{-} \\
= & \Sigma^{-1}\widetilde{\Sigma}_{\star}\beta^{+} - \Sigma^{-1}\widetilde{\Sigma}_{\star}\beta^{-} \\
= & \Sigma^{-1}\widetilde{\Sigma}_{\star}(\beta^{+} - \beta^{-}).
\end{aligned}$$

□

Additionally, if we are willing to assume also that$\Sigma_{++} = \Sigma_{+-} = \Sigma_{--} = \Sigma_{-+} \equiv \Sigma_{\star}$, the probability limit simplifies to

$$\widehat{\beta}^{+} - \widehat{\beta}^{-} \xrightarrow{p} \Sigma^{-1}\Sigma_{\star}(1-\pi^{+}-\pi^{-})(\beta^{+}-\beta^{-}). \tag{17}$$

The first scaling factor, $\Sigma^{-1}\Sigma_{\star}$ resembles the familiar attenuation bias induced by measurement error in linear models. The second term, $(1-\pi^{+}-\pi^{-}) \in [-1,1]$, reflects bias due to assigning observations to the incorrect side. Note that if we assign more observations to the wrong side than the right side $(1-\pi^{+}-\pi^{-} < 0)$, the estimator and true difference would have different signs. In general, this bias from incorrect assignment will bias results in the opposite direction from the true difference in slopes.

F Bankruptcy Model

F.1 Dynamic Model

The Household's Dynamic Problem: Here I present a dynamic version of the model from Section 6. We will now consider a representative household that lives for T periods indexed by $t = 1, 2, ..., T$. Each period they have the option to file for bankruptcy. When filing, their consumption is

$$c_t = a_t + e_t.$$

When not filing, consumption is

$$c_t = \begin{cases} y_t + a_t - R_t(d_t)d_t + d_{t+1} & : t < T \\ y_t + a_t - R_t(d_t)d_t & : t = T \end{cases}.$$

The t subscripts on the exemption level e_t and non-seizable assets a_t allow these objects to take on different, but deterministic values.

The household's value functions in periods $t < T$ are

$$\begin{aligned}
V_t^N(y_t, d_t) &= \max_{d_{t+1}} \; u(c_t^N) + \int_0^{y_{t+1}^{\star}} V_{t+1}^B dF(y_{t+1}) + \int_{y_{t+1}^{\star}}^{\infty} V_{t+1}^N(y_{t+1}, d_{t+1}) dF(y_{t+1}) \\
V_t^B &= u(c_t^B) - \sigma + \int_0^{\infty} V_{t+1}^N(y_{t+1}, 0) dF(y_{t+1}).
\end{aligned}$$

Note that here I now abstract from modeling the dynamic cost as as a utility penalty δ. The value functions in the terminal period are

$$V_T^N(y_T, d_T) = u(c_T^N)$$
$$V_T^B = u(c_T^B) - \sigma.$$

The FOC governing borrowing each period is

$$u'(c_t^N) = R_{t+1} \int_{y_{t+1}^\star}^{\infty} u'(c_{t+1}^N) dF(y_{t+1}).$$

The period t bankruptcy threshold $y_t^\star$ is characterized by the indifference condition

$$V_t^B = V_t^N(y_t^\star, d_t). \tag{18}$$

Comparative Statics: Here I examine the effects on the initial ($t = 1$) probability of filing of both a one-time and permanent shocks. First, consider a marginal change in either the initial exemption level e_1 or initial non-seizable cash-flows a_1. Implicitly differentiating the indifference condition in equation (18), we get

$$\frac{\partial p_1}{\partial e_1} = f(y_1^\star)\frac{\partial y_1^\star}{\partial e_1}, \qquad \frac{\partial p_1}{\partial a_1} = f(y_1^\star)\frac{\partial y_1^\star}{\partial a_1}.$$

These equations are unchanged relative to the static version of the model. Taking the partial derivative of $y_1^\star$ also yields the same equations as before:

$$\frac{\partial p_1}{\partial e_1} = f(y_1^\star)\frac{u'(c_1^B)}{u'(c_1^{N\star})} \geq 0$$
$$\frac{\partial p_1}{\partial a_1} = f(y_1^\star)\frac{u'(c_1^B) - u'(c_1^{N\star})}{u'(c_1^{N\star})}.$$

Why are these equations unchanged? The initial and future borrowing choices $d_2, ..., d_T$ are chosen optimally, so marginal changes in borrowing have no effect on welfare. Recall also that $c_1^{N\star}$ is consumption in the non-filing state for the *marginal* filer.

Now suppose that $e_t = e$ and $a_t = a$ for all t. Consider a marginal (permanent) change in e or a. The effect on period one filing is

$$\frac{\partial p_1}{\partial e} = f(y_1^\star)\frac{\partial y_1^\star}{\partial a}, \qquad \frac{\partial p_1}{\partial a} = f(y_1^\star)\frac{\partial y_1^\star}{\partial a}.$$

where

$$\frac{\partial y_1^\star}{\partial e} \equiv \sum_{t=1}^{T} \frac{\partial y_1^\star}{\partial e_t}, \qquad \frac{\partial y_1^\star}{\partial a} \equiv \sum_{t=1}^{T} \frac{\partial y_1^\star}{\partial a_t}.$$

Implicitly differentiating the indifference condition yields

$$\frac{\partial y_1^\star}{\partial e_1} = \frac{u'(c_1^B)}{u'(c_1^{N\star})}$$
$$\frac{\partial y_1^\star}{\partial a_1} = \frac{u'(c_1^B) - u'(c_1^{N\star})}{u'(c_1^{N\star})}$$

and

$$\frac{\partial y_1^\star}{\partial e_t} = \frac{u'(c^B)(p_t^B - p_t^N)}{u'(c_1^{N\star})}$$
$$\frac{\partial y_1^\star}{\partial a_t} = \frac{u'(c^B)(p_t^B - p_t^N) + (1-p_t^B)\mathbb{E}^B\left[u'(c_t^{N\star})\right] - (1-p_t^N)\mathbb{E}^N\left[u'(c_t^{N\star})\right]}{u'(c_1^{N\star})}$$

where $p_t^B = p(B_t = 1|B_1 = 1)$, $p_t^N = p(B_t = 1|B_1 = 0)$, and B_t denotes the event of filings for bankruptcy in period t. Changes in the period one exemption level only affects the difference in the value functions by increasing current consumption in bankruptcy. Similarly, changes in period one non-seizable assets only affect the difference in the values functions by increasing current consumption in and out of bankruptcy.

The effect of changes to the exemption in future periods t on the period one probability of filing for bankruptcy depends on the household's likelihood of filing for bankruptcy in period t. An increase in future exemption levels makes households less likely to file in the present if their probability of filing is in period t is lower if they file for bankruptcy in the present as opposed to in the future ($p_t^B < p_t^N$).

How does an increase in non-seizable cash-flows in a future period t affect period one filing? The filing response depends on the difference between average marginal utility in period t when the household files versus does not file for bankruptcy in period one. If the marginal filer is able to accumulate more wealth after filing for bankruptcy, then we may expect their marginal utility to be lower on average in the future. This would make increases in future non-seizable cash-flows have a negative effect on filing in the present.

Implications for the Marginal Filer: The two key takeaways from Section 6 are unchanged when we consider one-time changes in either the initial exemption e_1 or initial non-seizable cash-flows a_1. A one-time changes in the current exemption is the relevant comparative static to compare with the RKD. The RKD measures the filing response over the current period to marginal changes in the current amount of resources the household would have in bankruptcy. The ARM IV estimate likely embodies a response to a change in current cash-flows and expectations over future cash-flows. But after accounting for the this second wealth effect by estimating the expected NPV of cash-flows, the estimated effect of changes in the current year's cash-flows corresponds to a one-time change in current cash-flows in the model.

We still obtain the prediction that when the response to cash-flows is much stronger than the response to generosity, it implies that consumption must rise significantly in bankruptcy:

$$\frac{-\partial p_1/\partial a_1}{\partial p_1/\partial_1 e} = \frac{u'(c_1^{N\star})}{u'(c_1^B)} - 1. \tag{19}$$

and

$$c_1^{N\star} << c_1^B.$$

Additionally, a relatively strong response to cash-flows also still implies that either the dynamic costs of bankruptcy, stigma, or both must be large. This once again follows from the logic that the marginal filer is indifferent. If the consumption gain from filing is large, in order to be indifferent, the other costs of bankruptcy facing the marginal filer must also be large. If $u(c_1^B) >> u(c_1^{N\star})$, then

$$\underbrace{-\sigma}_{\text{utility penalty}} - \underbrace{\left\{p_2\mathbb{E}^N[V_2^B] + (1-p_2)\mathbb{E}^N\left[V_2^N(y_2, d_2)\right] - \mathbb{E}^B\left[V_2(y_2, 0)\right]\right\}}_{\text{dynamic cost}} << 0.$$

F.2 Decomposing Filing Response to *Seizable* Cash-Flow Shocks

How would a shock to *seizable* cash-flows affect filing? Suppose now that the household receives an endowment w outside of bankruptcy, but that these resources are are seizable in bankruptcy. The effect of a one-time change in period one's w on filing in period one is

$$\frac{\partial p_1}{\partial w_1} = f(y_1^\star)\frac{\partial y_1^\star}{\partial w_1}$$

where

$$\frac{\partial y_1^\star}{\partial w_1} = \frac{-u'(c_1^{N\star})}{u'(c_1^{N\star})} = -1.$$

An increase in w_1 affects the decision to file both through strategic and cash-flow motives. A rise in seizable resources increases the implicit cost of bankruptcy (the household must now give up more resources when filing) and also has more resources to increase consumption outside of bankruptcy. Formally, we can decompose the filing response to a change in w_1 into strategic and cash-flow effects.

$$\begin{aligned} \frac{\partial y_1^\star}{\partial w_1} &= \frac{\partial y_1^\star}{\partial a_1} - \frac{\partial y_1^\star}{\partial e_1} = \frac{u'(c_1^B) - u'(c_1^{N\star})}{u'(c_1^{N\star})} - \frac{u'(c_1^B)}{u'(c_1^{N\star})} \\ \Rightarrow \frac{\partial p_1^\star}{\partial w_1} &= \frac{\partial p_1^\star}{\partial a_1} - \frac{\partial p_1^\star}{\partial e_1} \end{aligned}$$

This parallels the decomposition in Chetty (2008) of the unemployment duration response to changes in the benefit level into moral hazard (strategic) and liquidity (cash-flow) effects. We can similarly decompose the response to a permanent change:

$$\frac{\partial p_1^\star}{\partial w} = \frac{\partial p_1^\star}{\partial a} - \frac{\partial p_1^\star}{\partial e}.$$

This decomposition is useful for considering deviations from the assumptions used to interpret the ARM IV regressions. Section 5.2 describes scenarios in which the payment reduction is potentially seizable. If the payment reduction is seizable with some probability $q \in [0,1]$, then the ARM IV estimate for the response to one year's change in payments would identify a mixture of the responses to w_1 and a_1: $q\frac{\partial p_1}{\partial w_1} + (1-q)\frac{\partial p_1}{\partial a_1}$. Under the extreme assumption that the payment reduction is always seizable, the ARM IV estimate would identify the response to marginal changes in w_1: $\frac{\partial p_1}{\partial w_1}$. This implies that the true response to non-seizable cash-flows is *at least* as large as the difference (in magnitudes) between the IV estimate (scaled to reflect only changes in the current year's mortgage payments) and the RKD estimate of the response to generosity.

To see this more clearly, note that the RKD and IV estimates correspond to

$$\begin{aligned} \tau^{RKD} &= -\frac{\partial p_1}{\partial e_1} < 0 \\ \beta^{IV} &= -q\frac{\partial p_1}{\partial w_1} - (1-q)\frac{\partial p_1}{\partial a_1}. \end{aligned}$$

Note that the RKD has the opposite sign as it estimates the response to an *increase* in seizable equity, which corresponds to a *reduction* in the amount of resources the household keeps in bankruptcy. Similarly, the ARM IV estimates the response to *higher* mortgage payments, which corresponds a *reduction* in cash-flows. Next, we can use the decomposition of the response to marginal changes in w_1 to bound $\frac{\partial p_1}{\partial a_1}$:

$$
\begin{aligned}
-\beta^{IV} &= q\left(\frac{\partial p_1}{\partial a_1} - \frac{\partial p_1}{\partial e_1}\right) + (1-q)\frac{\partial p_1}{\partial a_1} \\
&= q\left(-\frac{\partial p_1}{\partial e_1}\right) + \frac{\partial p_1}{\partial a_1} \\
&= q\tau^{RKD} + \frac{\partial p_1}{\partial a_1}.
\end{aligned}
$$

Therefore:

$$
-\frac{\partial p_1}{\partial a_1} = \beta^{IV} + q\tau^{RKD} \geq \beta^{IV} + \tau^{RKD}.
$$

Given the measurement-error corrected RKD estimate of -3.42 and the present-value and composition-adjusted IV estimate of 12.61 (where the units are the percent changes in filings given a \$1,000 change in generosity and annual mortgage payments, respectively). The lower bound above implies that the response to a \$1,000 increase in non-seizable cash-flows is at least a 9.19% decrease in the filing rate (12.61-3.42). This is approximately a 0.07 percentage point decrease in the annual filing rate of 0.72%, which is still much larger than the 0.02 percentage point fall in response to an equivalent increase in seizable equity. Therefore, even under the most extreme case in which mortgage payments are always seizable, the estimates still imply that the cash-flow motive is much larger than the strategic motive.

More generally, if the household only receives the payment reduction $q^B \in [0,1]$ percent of the time in bankruptcy and $q^N \in [0,1]$ when not in bankruptcy, the expression above still serves as a lower bound on the strength of the cash-flow motive.

To see this, note that this means that ARM IV estimate is a mixture of the responses to cash in bankruptcy and out of bankruptcy, specifically:

$$
\beta^{IV} = -q^B \frac{\partial p_1}{\partial e_1} - q^N \frac{\partial p_1}{\partial w_1}.
$$

Using the decomposition from earlier in this section, we can rewrite this as

$$
\beta^{IV} = -(q^B - q^N)\frac{\partial p_1}{\partial e_1} - q^N \frac{\partial p_1}{\partial a_1}.
$$

Using $\frac{\partial p_1}{\partial ea_1} = \tau^{RKD}$, isolating the cash-flow motive, we get

$$
-\frac{\partial p_1}{\partial a_1} = \frac{\beta^{IV}}{q^N} - \frac{q^B - q^N}{q^N}\tau^{RKD}.
$$

Because $\tau^{RKD} < 0$, the implied cash-flow motive is smallest for $q^N = 1$ and $q^B = 0$, i.e. the payment reduction is only and always received outside of bankruptcy. As before, this means the cash-flow motive is at least the sum of the IV and RKD estimates:

$$
-\frac{\partial p_1}{\partial a_1} \geq \beta^{IV} + \tau^{RKD}.
$$

F.3 Credit Market Exclusion

Now suppose filers are excluded from credit markets immediately after filing for bankruptcy and only re-enter credit markets stochastically with probability $\varrho \in (0,1)$ in future periods. Three value functions characterize this household's problem:

$$V_t^N(y_t, d_t) = \max_{d_{t+1}} u(c_t^N) + \int_0^{y_{t+1}^\star} V_{t+1}^B dF(y_{t+1}) + \int_{y_{t+1}^\star}^\infty V_{t+1}^N(y_{t+1}, d_{t+1}) dF(y_{t+1})$$

$$V_t^B = u(c_t^B) - \sigma + \varrho \int_0^\infty V_{t+1}^N(y_{t+1}, 0) dF(y_{t+1}) + (1-\varrho) \int_0^\infty V_{t+1}^A(y_{t+1}) dF(y_{t+1})$$

$$V_t^A(y_t) = u(y_t + a_t) + \varrho \int_0^\infty V_{t+1}^N(y_{t+1}, 0) dF(y_{t+1}) + (1-\varrho) \int_0^\infty V_{t+1}^A(y_{t+1}) dF(y_{t+1})$$

where the third value function V_t^A corresponds to beginning period t in financial autarky. Filing is still governed by the same indifference condition.[67]

The comparative statics for one-time changes in e_1 and a_1 are little-changed by incorporating credit market exclusion. The only difference is that the filing rate p_1 is now conditional on not currently being in autarky.[68] The formulas for the comparative statics for permanent changes are also unchanged, but the interpretation is slightly different. As with one-time shocks, the comparative statics apply to the filing rate for households not initially in autarky. Additionally, the expectations $\mathbb{E}^B$ and $\mathbb{E}^N$ in the formula for the direct effect of changes in e_t and a_t on the period one filing threshold $y_1^\star$ are taken over states in which the household is either in autarky or simply not filing. Note also that the decomposition of appendix F.2 still applies for a seizable cash-flow shock that is always available outside of bankruptcy (including autarky).

F.4 Delinquency

This section extends the baseline dynamic model from section F.1 to allow for delinquency. Households choose to either repay, go delinquent or file for bankruptcy. Let superscripts R, D, and B, denote variables when the borrower chooses to repay, go delinquent, and go bankrupt (respectively). When delinquent, a percent $\gamma \in [0,1]$ of the household's wages are garnished. A delinquent household's debt evolves according to $d_{t+1} = (d_t - \gamma y_t) R_t(d_t)$. Intuitively, this law of motion means that interest accumulates on outstanding debt balances and debt is partially paid down through garnishment. Consumption under each choice is

$$c_t^R = \begin{cases} y_t + a_t - R_t(d_t) + d_{t+1} & : t < T \\ y_t + a_t - R_t(d_t) & : t = T \end{cases}$$

$$c_t^D = (1-\gamma) y_t + a_t, \quad \forall t$$

$$c_t^B = a_t + e_t, \quad \forall t.$$

[67] Allowing households in autarky to file in the model would have no effect on the rule governing filing. This is because households in autarky would not have an incentive to file for bankruptcy as they have no debt to discharge. Additionally, losing the option to file for bankruptcy for many years resembles the reality that households are ineligible to receive another discharge in bankruptcy for several years. After filing for Chapter 7, households cannot receive another discharge under Chapter 7 for eight years (or under Chapter 13 for four years). If they filed for Chapter 13, they cannot receive another discharge under Chapter 7 for six years (or under Chapter 13 for two years).

[68] We could instead derive comparative statics for the probability of filing in an economy consisting of a unit mass of representative households. In this case, we would apply the law of total probability and scale the comparative statics by the steady-state mass of households not in autarky. This would not change any of the implications derived by taking the ratio of the cash-flow and strategic responses as this fraction would simply cancel out.

Note that the endowment a_t is not subject to garnishment. This makes shocks better resemble mortgage payment reductions as in practice these reductions are not garnished. The three value functions associated with each of these choices are

$$V_t^R(y_t, d_t) = \max_{d_{t+1}} \quad u(c_t^R) + \mathbb{E}^R \left[V_{t+1}^R(y_{t+1}, d_{t+1}) \right] + \mathbb{E}^D \left[V_{t+1}^D(y_{t+1}, d_{t+1}) | \right] + \mathbb{E}^B \left[V_{t+1}^B \right]$$

$$V_t^D(y_t, d_t) = u(c_t^D) + \mathbb{E}^R \left\{ V_{t+1}^R[y_{t+1}, (d_t - \gamma y_t) R_t(d_t)] \right\} + \mathbb{E}^D \left\{ V_{t+1}^D[y_{t+1}, (d_t - \gamma y_t) R_t(d_t)] \right\} + \mathbb{E}^B \left[V_{t+1}^B \right]$$

$$V_t^B = u(c_t^B) - \sigma + \mathbb{E}^R \left[V_{t+1}^R(y_{t+1}, 0) \right].$$

For a given income y_t and initial debt d_t, the household files if and only if

$$V_t^B > \max \left\{ V_t^R(y_t, d_t), V_t^D(y_t, d_t) \right\}.$$

To characterize the comparative statics, we must now consider two cases. The first is when the household prefers repaying over delinquency: $V_t^R(y_{t,}^{\star R} d_t) \geq V_t^D(y_t^{\star R}, d_t)$. Assuming that the houeshold repays when indifferent between repaying and going delinquent and that utility $u(\cdot)$ is a strictly increasing function, the unique income threshold at which the household files for bankruptcy, $y_t^{\star R}$, is characterized by

$$V_t^B = V_t^R(y_t^{\star R}, d_t).$$

This yields the same expression for the partial derivatives in the baseline model.

The second case to consider is when $V_t^R(y_t^{\star D}, d_t) < V_t^D(y_t^{\star D}, d_t)$. The difference in value functions $V_t^B - V_t^D(y_t, d_t)$ is strictly decreasing in y_t (assuming $u(\cdot)$ is strictly increasing), which means that the filing decision is once again characterized by a unique threshold $y_t^{\star D}$. The household files when income falls below the threshold $y_t^{\star D}$. The threshold in the second case is characterized by

$$V_t^B = V_t^D(y_t^{\star D}, d_t).$$

Implicitly differentiating the above equation for $t = 1$ yields the following comparative statics:

$$\frac{\partial p_1}{\partial e_1} = f(y_1^{D\star}) \frac{u'(c_1^B)}{(1-\gamma) u'(c_1^{\star D})}$$

$$\frac{\partial p_1}{\partial a_1} = f(y_1^{D\star}) \frac{u'(c_1^B) - u'(c_1^{\star D})}{(1-\gamma) u'(c_1^{\star D})}$$

where $c_1^{\star D}$ is consumption for the marginal filer when delinquent. This tells us that both a strong strategic or cash-flow motive could arise from a high garnishment rate (γ). Taking the ratio of the effects yields essentially the same result as before:

$$\frac{\partial p_1 / \partial a_1}{\partial p_1 / \partial e_1} = \frac{u'(c_1^B) - u'(c_1^{\star D})}{u'(c_1^{\star D})}.$$

As before, the ratio of responses equals the relative difference in marginal utlity in and out of bankruptcy. But in this second case, now "out of bankruptcy" refers to the state of the world in which the borrower is delinquent. When the cash-flow motive is much stronger than the strategic motive, the mains results that (1) consumption is much higher in bankruptcy than out of bankruptcy and (2) other costs of bankruptcy are large for the marginal filer (e.g., stigma or dynamic costs), for the marginal filer.

Consumption We obtain the same prediction about consumption being much higher bankruptcy versus out of bankruptcy for the marginal filer. This means

$$e_1 + a_1 >> (1-\gamma)y_1^{\star D} + a_1.$$

Recall that we are considering a case where $y_1 \geq e_1$ (the homestead exemption is binding). This then implies

$$\begin{aligned} y_1^{\star D} \geq e_1 &>> (1-\gamma)y_1^{\star D} \\ 1 &>> (1-\gamma). \end{aligned}$$

In this model, the finding of a relatively stronger cash-flow motive also implies that the marginal filer is facing wage garnishment if they would otherwise be delinquent. However, in reality many delinquent households make partial debt payments. Consumption could also be higher in bankruptcy if filing leads to lower debt in bankruptcy than in delinquency.

Heterogeneity In the presence of heterogeneity in initial debt levels, the change in estimated filing probabilities corresponds to the average change across debt levels. The consumption and bankruptcy cost predictions would then describe the average marginal filers (where weights in the average correspond to the proportion with a particular debt level).

With this type of heterogeneity, it's possible that some households are in case one and some are in case two. This still implies on average consumption is higher in bankruptcy than out. But it wouldn't require this to be true for all marginal filers. All or some marginal filers choosing between delinquency or bankruptcy may not have their wages garnished and actually experience a fall in consumption when filing. This would imply that the increase in consumption when filing, for those choosing between repaying versus filing, is even larger.