Moral Hazard versus Liquidity in Household Bankruptcy *

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Abstract

This paper studies the role of moral hazard and liquidity in driving household bankruptcy. First, I estimate that increases in potential debt forgiveness have a positive, but small, effect on filing using a regression kink design. Second, exploiting quasi-experimental variation in mortgage payment reductions, I estimate that filing is five times more responsive to cash-on-hand than relief generosity. Using a sufficient statistic, I show the estimates imply large consumption-smoothing benefits of bankruptcy for the marginal filer. Finally, I conclude 83% of the filing response to dischargeable debt comes from liquidity effects rather than a moral hazard response to financial incentives.

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1 Introduction

Consumer bankruptcy is a major source of insurance in the US. By erasing debt obligations, bankruptcy can help households better smooth their consumption across different states of the world. The scale of bankruptcy rivals that of other explicit forms of social insurance. In a typical year, bankruptcy offers US households $189 billion in debt forgiveness, exceeding transfers from unemployment insurance at its 2010 peak ($139 billion).[1] Every year around one million US households seek debt relief through bankruptcy, with one in ten having filed at some point in their life (Stavins, 2000; Keys, 2018). The implicit insurance bankruptcy provides is potentially welfare-improving to the extent it mitigates incomplete credit and insurance markets. But like other forms of insurance, generous debt relief can also create moral hazard. By distorting borrower incentives to repay, generous bankruptcy could encourage more filing, and in turn discourage lending.

This paper sheds new light on the trade-offs of generous bankruptcy by comparing the filing response to increases in potential debt relief versus cash-on-hand. A marginal dollar of debt forgiveness affects filing through a "moral hazard effect" by distorting the wealth gain from filing.[2] In contrast, cash available both in and out of bankruptcy (instead of only in bankruptcy) affects filing through a "liquidity effect." An increase in cash can deter filing, even when leaving the *wealth gain* from filing unaltered, by alleviating liquidity constraints. Pressure from liquidity constraints may otherwise motivate filing to increase consumption by discharging debt or stopping creditors from garnishing wages.

This paper's main contribution is to estimate and compare the causal effect on filing

[1] Sources: Annual BAPCPA report (Tables 1A and 1D) and Bureau of Economic Analysis (state and federal unemployment transfers).

[2] The filing response to relief generosity is a form of hidden information moral hazard as defined in Hart and Holmström (1987). Hidden information, rather than hidden action, moral hazard arises when random events *after* contracting (such as income shocks) can change the agent's type and, ultimately, their actions.

of increases in debt relief generosity and cash-on-hand. Combining data on millions of US households and two quasi-experimental research designs, I find that the liquidity effect (cash-on-hand) is five times stronger than the moral hazard effect (relief generosity). I estimate a small moral hazard effect: a $1,000 rise in relief generosity increases annual filing by 0.02 percentage points, indicating that a key component of the social cost of generous bankruptcy is small. To further interpret these estimates, I show in a model of household bankruptcy that the ratio of liquidity and moral hazard effects is an informative sufficient statistic. In particular, a higher ratio implies larger consumption-smoothing benefits of bankruptcy but also larger non-monetary or dynamic costs of bankruptcy for the marginal filer. Finally, using a decomposition in the style of Chetty (2008), I conclude that 83% of the filing response to dischargeable debt is due to liquidity effects, rather than a moral hazard response to financial incentives.

The first analysis focuses on moral hazard, estimating the causal effect of the debt relief available in bankruptcy on filing. The amount of debt relief provided by bankruptcy depends on the filer's debts, assets, and states' bankruptcy exemption laws. Prior analyses examine how filing varies with potential debt relief in the cross-section (Fay et al., 2002; Zhang et al., 2015). However, omitted variables that both lower wealth and increase filing could cause such approaches to overstate the effect of generosity on filing. Another approach to estimating the effects of bankruptcy policy uses state-level variation in exemptions.[3] But the general equilibrium effect identified by this approach would understate the *direct* effect of generosity if, for example, indirect channels such as reduced credit supply deter filing.[4]

[3]See for example Mahoney (2015); Auclert et al. (2019); Pattison (2019).

[4]For evidence on the credit supply response to bankruptcy laws see Gropp et al. (1997); Pence (2006); Mitman (2016); Gross et al. (2021); Indarte (2022).

I address these identification challenges using a novel application of a sharp regression kink design (RKD). I exploit a kink in the debt relief households receive in bankruptcy induced by states' homestead exemption laws. The homestead exemption affects relief generosity by capping the amount of home equity filers can retain in bankruptcy. Filers pay creditors the value of home equity in excess of the exemption. The *change* in the relationship between filing and home equity at the exemption limit identifies the causal effect of relief generosity if no other factors affecting filing also *kink* at the limit. Thus the RKD isolates variation in the payoff of bankruptcy while holding fixed other non-kinking factors, such as resources available when *not* filing. Estimation uses a quarterly mortgage-level panel of seven million homeowners from CoreLogic, containing 99 million observations.

To implement the RKD, I develop a new econometric approach that corrects for the effects of classical measurement error. The running variable (home equity) is likely measured with error because we cannot observe home values at every point in time. Classical measurement error poses non-standard challenges in both an RKD and regression discontinuity design (RDD). I adapt the standard nonparametric approach (Card et al., 2015) by assuming the relationships between the outcome and running variable above and below the cutoff are quadratic, rather than approximating them as such.[5]

Under these parametric assumptions, I characterize the bias of an OLS estimator relative to the sharp RK estimand. I show that in both an RKD and RDD, bias arises from measurement error assigning observations to the "wrong" side of the cutoff ("assignment bias"). Further bias arises in an RKD—but not an RDD—from attenuated estimates of the slopes above and below the cutoff. I present a measurement-error corrected estimator,

[5] For an RDD, the corresponding framework assumes a linear relationship.

which I implement in my setting using data on 200,000 home sales under the assumption that the sale price is free of measurement error.

The second analysis focuses on liquidity by estimating the causal effect of mortgage payment reductions on filing. Isolating the liquidity effect is challenging because many shocks to cash-on-hand, such as tax rebates (Gross et al., 2014), are also seizable in bankruptcy.[6] Seizable cash reduces debt relief in bankruptcy, and therefore affects filing through both moral hazard and liquidity effects. In contrast, mortgage payments affect cash-on-hand but generally not the payoff of bankruptcy, making them better-suited to isolate the liquidity effect.[7]

I identify the causal effect of mortgage payment reductions using quasi-experimental variation in the *size* of reductions received by households with adjustable-rate mortgages (ARMs). The interest rate on an ARM periodically resets to a new rate based on the prevailing value of a pre-selected "index rate." In 2008 an unprecedented spread opened between two popular index rates: the one-year Libor and Treasury rates. The spread led otherwise similar mortgages to receive very different payment reductions. At the 218 basis point peak in the Libor-Treasury spread in September 2008, borrowers with a median-sized mortgage paid $4,191 more over the next year if indexed to Libor rather than Treasury.

In order to estimate the impact of a *dollar* change in mortgage payments, versus the direct effect of interest rate changes, I adapt the approach of Gupta (2019). While the difference in rate changes among Libor and Treasury-indexed loans is plausibly exogenous,

[6] Smaller increases in cash may also *increase* filing by helping cover the upfront costs of filing (Gross et al., 2014). Larger shocks like the ones studied here ($2,000 versus $200 per year) may be necessary to identify their effect through alleviating liquidity constraints on consumption.

[7] Bankruptcy is generally used to discharge unsecured debt, it does not erase liens on secured debt (e.g., mortgages).

the dollar change in payments is likely endogenous. Households less prone to default may have an easier time obtaining larger mortgages. To exploit the plausibly exogenous variation, I instrument for the post-reset payment using the value of the loan's index rate (Libor or Treasury) at the time of the reset.

The ideal experiment to isolate the liquidity effect would entail a *one-time* change in cash flows not seizable in bankruptcy, leaving expectations over future cash flows unaffected. The ARM instrumental variables (IV) strategy is an approximation of this ideal in two ways. First, although payment changes last for one year, households may update expectations over future payments in response to a current change. Households may also not "receive" this cash flow increase if they stop making mortgage payments (e.g., delinquency or prepayment). To address the first issue, I combine additional data from CoreLogic on these borrower behaviors to estimate the expected net present value (NPV) of mortgage payments after a reset. I then scale the IV estimate to reflect the effect of a one-time change in the current year's cash flows. Second, while the increase in cash-on-hand from a mortgage payment is most often not seizable in bankruptcy, it may be seizable for a subset of Chapter 13 filers. To address this, I consider an extreme scenario in which mortgage payment reductions are 100% seizable for all households. Using a decomposition similar to that of Chetty (2008), I then back out an estimate for the liquidity effect using the IV estimate. The estimate under this scenario is similar.

I find that filing is five times more responsive to a given one-time change in cash-on-hand than an equivalent reduction in the generosity of bankruptcy. The RKD estimates that a $1,000 decrease in the generosity of bankruptcy reduces the annual filing rate by 0.02 percentage points (a 2.63% change relative to the sample's average rate of 0.71%). The small RKD estimate indicates that moral hazard is not a strong driver of bankruptcy, and that increasing the generosity of bankruptcy weakly incentivizes further filing. The ARM IV estimates a \$1,000 one-time decrease in annual mortgage payments reduces the filing rate by 0.09 percentage points (a 12.59% relative decrease).[8] A stronger response to cash-on-hand is consistent with liquidity constraints and a lack of insurance playing a powerful role in driving household bankruptcy.

Next, I develop of a model of the household bankruptcy decision to interpret the empirical estimates. I first derive a sufficient statistic relating the moral hazard and liquidity effects to the consumption-smoothing benefits of bankruptcy for the marginal filer. Intuitively, when filing is less sensitive to cash available *only* in bankruptcy than cash available *regardless* of filing, households are indicating by revealed preference that they value a marginal dollar less in bankruptcy than outside of bankruptcy. That is, marginal utility is higher for the marginal filer when *not* filing. The larger the ratio of liquidity and moral hazard effects, the larger the expected increase in consumption upon filing. But because a marginal filer is by definition indifferent, a larger consumption gain implies larger non-monetary and/or non-immediate costs of filing. These costs may come from stigma and dynamic costs from credit or labor market exclusion.

Using the model, I then decompose the filing response to variation in payments for debt *dischargeable* in bankruptcy into moral hazard and liquidity effects. Higher payments on dischargeable debt encourage filing through *both* moral hazard (by increasing the payoff from filing) and liquidity (by reducing cash-on-hand). The causal effect of debt payments on default is often labeled moral hazard (e.g., Adams et al., 2009; Karlan and Zinman, 2009; Gupta and Hansman, 2022). But the estimates here challenge this interpretation in the context of bankruptcy. The estimates imply that 83% of the filing response to

[8] This estimate comes from re-weighting the ARM sample to resemble the more representative RKD sample, where weights are constructed as in DiNardo et al. (1996) and Gross et al. (2020).

changes in dischargeable debt payments is due to liquidity, not the moral hazard response to financial incentives.

Related Literature: This paper contributes to several strands of literature. First, it adds to the literature on strategic default. On the theoretical side, it advances this primarily empirical literature by linking the moral hazard and liquidity effects to the insurance value of default and the non-monetary/dynamic costs of bankruptcy. On the empirical side, prior work focuses on delinquency (i.e., missed debt payments) rather than bankruptcy. Notably, mortgage payment reductions induced by ARM resets lead to substantial reductions in delinquency (Di Maggio et al., 2017; Fuster and Willen, 2017; Gupta, 2019). However, there is a lack of consensus on the *relative* strength of strategic versus liquidity default motives. For evidence of strong strategic motive see Mayer et al. (2014), Haughwout et al. (2016), and Dobbie and Song (2020); for a strong liquidity motives see Scharlemann and Shore, 2016, Gerardi et al. (2017), Agarwal et al. (2017),Ganong and Noel (2020), Ganong and Noel (2022), and Agarwal et al. (2022). Results for delinquency do not obviously generalize to bankruptcy. Liquidity may be an important driver of delinquency due to a lack of cash making repayment infeasible. But bankruptcy is not mechanically linked to the feasibility of making payments, whereas as delinquency by definition is. This paper also adds a new focus on the default behavior of a broad population, including those who may not currently be experiencing financial distress but could still increase their wealth through default.[9]

Second, this paper adapts the approach of Chetty (2008) to bankruptcy. Chetty (2008)

[9] Qualifying for debt reductions in HAMP (Ganong and Noel, 2020) or the credit card debt modification program of Dobbie and Song (2020) required delinquency or evidence of financial hardship. Understanding both the broader population and subset of financially distressed borrowers is valuable as debt relief policies differ in whether they are targeted or broadly available.

relates reduced-form estimates of behavioral responses to statistics useful for evaluating the welfare effects of unemployment insurance policy. Relative to prior theoretical work on sufficient statistics for optimal bankruptcy design (Dávila, 2020), this paper innovates by formalizing the relationship between moral hazard and liquidity effects and the insurance value of bankruptcy. With estimates of these two effects, one can infer the insurance value for a marginal filer *and* the consumption-equivalent value of dynamic and non-pecuniary costs of bankruptcy.

Third, this paper estimates behavioral responses that are useful for disciplining structural, general equilibrium models used to study the positive and normative effects of bankruptcy policy.[10] The filing responses to potential debt relief and cash-on-hand are key determinants of the moral hazard and consumption-smoothing trade-offs of generous bankruptcy. These elasticities can help such models quantify the macroeconomic impact of changes to bankruptcy policy (for a recent application studying bankruptcy in general equilibrium, see Auclert and Mitman, 2022).

Lastly, this paper also contributes to the RKD/RDD literature (e.g. Calonico et al., 2014; Card et al., 2015). Identification for a "sharp" or "fuzzy" RKD/RDD (Card et al., 2015) can fail when (1) measurement error is a continuous variable, or (2) the researcher lacks independent measures of the policy and running variables. This second scenario can arise in other settings of interest to researchers. For example, when evaluating the effect of means-tested policies in data where income is mis-measured and eligibility/treatment is not directly observed. An important innovation, relative to prior work on measurement error in RDDs (Pei and Shen, 2017; Davezies and Le Barbanchon, 2017; Dieterle et al.,

[10] Notable works include Dubey et al. (1990, 2005); Zame (1993); Athreya (2002, 2006); Livshits et al. (2007); Chatterjee et al. (2007); Athreya (2008); Livshits et al. (2010); Chatterjee and Gordon (2012); Nakajima and Ríos-Rull (2019); Mitman (2016); Gordon (2017); Auclert et al. (2019).

2020), is that this paper's framework does not require the econometrician observes the true value of the explanatory variable (treatment status in an RDD). Additionally, the framework developed here implies that mis-measured running variables bias RDD and RKD estimates away from the true parameter; it also attenuates the magnitude of RKD estimates.

This paper is organized as follows. Section 2 gives background information on consumer bankruptcy. Section 3 describes the data. Section 4 presents the RKD estimation and econometric results. Section 5 gives the estimation results for the effect of payment reductions. Section 6 develops the model and theoretical results. Section 7 concludes.

2 Background: Consumer Bankruptcy in the US

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.[11] By discharging debt, bankruptcy effectively facilitates a transfer of wealth from creditors to debtors. The option to seek this transfer provides debtors an implicit form of insurance; households can obtain debt relief to smooth consumption in response to events such as job loss and illness. Households primarily file under Chapter 7 or Chapter 13.[12]

States' asset exemption laws determine the generosity of the debt relief filers receive in bankruptcy. To receive a debt discharge in bankruptcy, filers must pay creditors the value of assets in excess of asset-specific exemption limits. Higher exemption limits increase the

[11]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.

[12]Chapter 11 is primarily used by businesses, but it is sometimes also used by households. Chapter 11 filings comprised 0.14% of nonbusiness bankruptcies in 2017 (see Table F-2 of the Administrative Office of the US Courts' *Bankruptcy Filings* report, available at https://perma.cc/WN7F-4CV9.)

generosity of bankruptcy by reducing filers' *seizable assets*, effectively lowering the cost of bankruptcy. Under Chapter 7, a household's net financial benefit of filing equals

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

Filers incur court fees around \$300 and, if they hire a lawyer, legal fees around \$1,000-\$2,000.

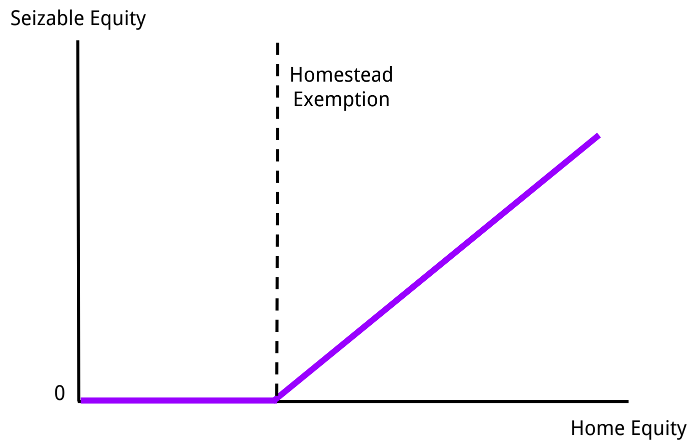
Costs to households in Chapter 13 are closely related to those in Chapter 7. Chapter 7 filers make a one-time payment to creditors while Chapter 13 filers make monthly payments over a three to five year period. Payments under Chapter 13 are sometimes set equal to the filer's disposable income, which is calculated from detailed income and expense reports. The Federal Bankruptcy Code requires that creditors receive at least as much under Chapter 13 as they would have received under Chapter 7. The Chapter 7 financial cost above is therefore a lower bound for the financial cost to households filing for Chapter 13.

The Homestead Exemption: Most of the variation in households' potential debt relief is due to the homestead exemption (Auclert et al., 2019), which protects a filer's home equity. The homestead exemption induces a kink in *seizable equity* as a function of home equity:

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

Below the homestead exemption limit, a marginal dollar of home equity has no effect on seizable equity. But above the exemption, every additional dollar of home equity is another dollar that must be repaid to creditors in bankruptcy. Figure 1 illustrates this kink. The first analysis exploits this kink as a quasi-experimental source of variation in bankruptcy generosity.

Figure 1: The Kink in Seizable Equity



Note. 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 Appendix Figure A.1). In 2017, exemptions ranged from $0 in New Jersey, to $550,000 in Nevada, to an unlimited amount in Texas. Exemption levels change infrequently, occurring two to three times for most states over 2000 to 2017. Cross-state differences in generosity are highly persistent (Hynes et al., 2004) and are well-explained by historical political and economic events (Skeel, 2001).

3 Data

The main dataset used in the empirical analysis is CoreLogic's Loan Level Market Analytics (LLMA) database. The LLMA contains detailed information on mortgage characteristics at origination and monthly loan performance over the life of the loan for a large sample of borrowers. The LLMA tracks households' mortgage balances, mortgage payments (actual and required), and bankruptcy filings at a monthly frequency. It also reports detailed mortgage origination characteristics such as the home value at origination and contract features of ARMs governing interest rate resets. Appendix Table A.1 presents average characteristics for the two samples used in the RKD and ARM IV analysis.

CoreLogic collects this data from 25 of the largest mortgage servicers in the US. The LLMA tracks approximately 5.7 million mortgages each year and on average includes 45% of mortgages originated in the US over the sample period (2000 to 2016). The LLMA contains over a billion month-mortgage observations. The data are also geographically diverse. On average, the LLMA includes 99.97% of population-weighted counties (98.97% by ZIP code) each year.

The average annual filing rate in the LLMA is 0.71%, which is lower than the national average of 1.12%. This is likely because the LLMA only includes households with mortgages, and homeowners are likely in better financial shape than renters and thus less prone to bankruptcy. Homeowners comprise 66.8% of households over 2000 to 2016.[13] These statistics suggest that homeowners account for a substantial share of bankruptcies (42.5%). Additionally, the filing rate in the LLMA sample closely tracks the national filing rate over time (see Appendix Figure A.2).

Measuring Home Equity: The legal standard for home valuation in bankruptcy is fair market value. Acceptable valuation methods range from a formal appraisal to comparisons with current listings of similar, nearby properties (e.g, obtained via Zillow.com). To measure home equity, I subtract the end-of-month mortgage balance from an imputed

[13] U.S. Census Bureau, Homeownership Rate for the United States [RSAHORUSQ156S], retrieved from FRED, Federal Reserve Bank of St. Louis; https://fred.stlouisfed.org/series/RSAHORUSQ156S.

home price. I impute the home price by taking the sale price of the home and projecting it forward over time using monthly changes in a ZIP-level house price index specific to the property's size, similarly to Di Maggio et al. (2017).[14] While this imputation is conceptually similar to how courts value properties in bankruptcy, this measure of home equity is likely subject to measurement error. Additionally, while the LLMA contains information on second mortgages made during origination, it lacks information on subsequent smaller loans (e.g., home equity loans). Measurement error poses unique challenges in a regression kink design, which I address using a new econometric approach outlined in Section 4.2.

Additional Data: Measuring *seizable* home equity requires data on homestead exemptions. I construct a quarterly panel of states' homestead exemption levels by manually collecting this information from the original state statutes.[15] To capture local economic conditions, I use county-level data on unemployment and income as well as ZIP-level data on house price growth, income, and unemployment claims. See Appendices C.3 and C.4 for details on sources and construction. Throughout, I deflate nominal variables using the CPI with a base year of 2010.

4 The Effect of Reduced Bankruptcy Generosity on Filings

4.1 Empirical Strategy: Regression Kink Design (RKD)

To identify how changes in a household's cost of bankruptcy affect their likelihood of filing, I use an RKD. 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 house-

[14]See Appendix C.2 for details.
[15]The exemption dataset is available for download at https://sashaindarte.github.io/public_goods/.

hold's home equity and their state's homestead exemption, which I'll refer to as "equity distance" and denote by $D \in \mathbb{R}$. The goal is to estimate how changes in *seizable* home equity, defined as $S \equiv \max\{D, 0\}$, affect the household's probability of filing ($\mathbb{E}(B)$).

The RKD 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, that is,

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

Because the right and left limits of $S'(D_0)$ approach 1 and 0 as $D_0 \to 0$, respectively, the denominator of τ is simply 1. Here, this implies that τ is the change in the slope of the probability of filing with respect to equity distance at the exemption limit.

Identification: An advantage of the RKD is that it requires weaker identifying assumptions compared to approaches assuming exogeneity of the explanatory variable (or instrument). An RKD does *not* require that the running variable is exogenous with respect to the outcome. Rather, the key identifying assumption is that the density of unobserved factors that affect the bankruptcy decision is smooth at the kink (i.e., these factors do not jump/kink at the cutoff). Under this and the technical assumptions of Card et al. (2015), τ nonparametrically identifies the local average response of filing to changes in seizable equity:

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

The RKD addresses two key challenges that make it difficult to identify the causal effect of costly bankruptcy on filing. First, regressing filing on the cost of bankruptcy would likely *overstate* the true causal effect. Omitted variables that increase filing may also lower wealth, and thus the cost of bankruptcy (e.g., unemployment).

The second challenge is to isolate the *direct* effect of generosity from *indirect* channels. One example of an indirect channel is credit supply. In particular, generous bankruptcy can deter lending, which in turn reduces debt and may diminish the incentive to file. The RK estimand isolates the direct effect by differencing out the other channels through which equity distance (D) affects filing (B). As long as omitted factors, such as credit supply, are smooth 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 the change in seizable home equity: $\frac{\partial \mathbb{E}(B|D=\widetilde{D})}{\partial S} S'(\widetilde{D})$.

An important limitation of an RKD is that it identifies a local effect, specifically the average response among households at the cutoff. To mitigate this, I pool the sample across states and time so that it includes many different exemption limits. This means the RKD identifies the average response across a group of households with a variety of levels of home equity.

Internal Validity: I use two standard tests, and two additional tests specific to bankruptcy, to check for possible failures of the smooth density assumption. I first confirm that predicted filing rates based on predetermined covariates exhibit no jump or kink at the exemption limit. This is shown graphically in Appendix Figure D.1, with details on the formal test in Appendix D.1.

The second test focuses on the possibility that agents can manipulate their seizable equity around the cutoff. A potential filer may be temped to try to lower their home equity below their state's exemption limit in order to reduce their cost of bankruptcy. To check for evidence of manipulation, I examine whether 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 to the right. The histogram of equity distance appears to exhibit no such bunching below the exemption limit (see Appendix Figure D.2). The estimated jump is small and statistically insignificant. The estimated change in the density's slope is statistically significant but economically small.[16] This suggests that if there is any bias from a slightly kinked density, it is small. Moreover, if would-be filers manipulated their costs downward this would exaggerate the kink in the filing rate, and at worst overstate the strength of the moral hazard effect.

Households are unlikely to manipulate their seizable home equity for several reasons. First, attempts to manipulate one's cost of bankruptcy are grounds for a judge to dismiss a case. Second, rapidly reducing a home's value is challenging. Stopping upkeep to speed up depreciation of the home would be slow, especially when house prices are generally rising. Third, delinquency is unlikely an attractive option as it can prevent a filer from obtaining an automatic stay in bankruptcy to prevent foreclosure.If the goal of the filer is to remain in their home by getting under the exemption limit, delinquency will in turn make this much harder to achieve. Fourth, a household could also attempt to lower their home equity by taking out an additional loan against their property, but a financially

[16] I estimate a slope change of -0.81% per $1,000 relative to the mean bin size, with a p-value of 0.003.

distressed borrower would have hard time qualifying for such a loan.

The smooth density assumption rules out *deterministic* sorting among households. However, as Card et al. (2015) notes, behavioral models where agents attempt to sort with optimization errors *are* consistent with the smooth density assumption (e.g., Chetty, 2012). In the context of bankruptcy, difficulties agents face in measuring their own home equity can lead to such optimization errors, preventing households from precisely manipulating their home equity. While measurement error on the part of the econometrician creates identification challenges (addressed in the next section), measurement error on the part of households does not generally violate the RKD identifying assumptions.

The third test focuses on a potential concern specific to bankruptcy. Filing may entail fixed costs, for example: the hassle of mandatory debtor education courses or costs associated with selling one's home. The presence of such costs is not a problem per se. But if the likelihood of incurring them is a *kinked* function of equity distance, the RKD may conflate these costs with the effect of seizable equity and overstate the causal effect of seizable equity. This is most naturally a concern regarding home sales. While having nonexempt home equity does *not* require the filer surrender their home, it can be more difficult to avoid selling when nonexempt equity is higher.[17] To evaluate this concern I estimate an RKD for bankruptcy filers where the running variable is equity distance and the outcome is an indicator equal to one if the filer sells their home within 1.5 years after filing. This relationship appears smooth in Appendix Figure D.6. And Appendix Table D.5 reports a small and statistically insignificant estimate.

The fourth test examines the possibility that credit supply is a kinked or discontinuous

[17] Filers with nonexempt home equity can avoid selling their home if they pay in cash the value of the nonexempt portion. This could be done, for example, by liquidating retirement savings, which are generously protected in many states. On average, less than 1% of filers with nonexempt home equity sell their home within 1.5 years of filing for bankruptcy.

function of nonexempt equity. Creditors would likely prefer to restrict credit supply to households with a lower cost of bankruptcy, as they face a greater incentive to file. While debt is likely correlated with equity distance, a *kinked* or discontinuous relationship is unlikely. Many loans are infrequently originated, such as auto and student loans, giving creditors limited ability to fine-tune balances with respect to equity distance after the time of origination. Additionally, the credit bureau data used to underwrite loans such as credit cards does not contain information on home values, making it difficult to precisely estimate home equity. Appendix D.6 uses household-level debt data from the Survey of Income and Program Participation to test for kinks and jumps in borrowing. The results suggest that kinked or discontinuous credit supply is unlikely to be a source of bias.

Estimation: I employ two estimation approaches. The first follows standard practices and nonparametrically estimates the sharp RK parameter $\widehat{\tau}$ using a local quadratic estimator (Card et al., 2015; Gelman and Imbens, 2018). I construct approximation-bias-corrected robust confidence intervals and optimally select the estimation bandwidth using the MSE-minimizing procedures of Calonico et al. (2014). Because the measure of home equity is likely subject to measurement error, I also use a new parametric estimator that corrects for bias due to measurement error.

RKD Sample Restrictions: I restrict the sample to 17 states where filers are *not* allowed to double their homestead exemption when filing jointly.[18] This avoids a source of non-random measurement error, as states that allow doubling have a kink point that depends on marital status (which is unobservable the LLMA data). This sample also has non-zero

[18] These states include Alaska, Arizona, Colorado, Delaware, Idaho, Louisiana, Maryland, Massachusetts, Minnesota, Missouri, Mississippi, North Dakota, Nebraska, Rhode Island, Vermont, Washington, and Wisconsin.

homestead exemptions.[19] This means we can expect households with positive home equity to appear on both sides of the cutoff. I also drop households with negative home equity as these households cannot have positive seizable equity. This improves internal validity by avoiding a population that disappears discontinuously at the cutoff. Appendix C.1 provides more detail on sample restrictions and their impact on sample size.

4.2 A New Approach to Measurement Error in RKDs and RDDs

Measurement error in the running variable (here, equity distance) poses non-standard identification challenges in both an RKD and RDD. This section presents a new approach to characterize and correct for bias due to measurement error. In essence, this approach modifies the framework of Card et al. (2015) by assuming the relationship between the outcome and explanatory variables *is* (piece-wise) quadratic, rather than approximating it as such. For the RKD, bias grows with both the variance of the measurement error *and* the degree to which measurement error causes observations to appear on the "wrong" side of the cutoff. Below I present a characterization of the bias and a bias-correcting estimator for an RKD. A similar result exists for an RDD that assumes a linear rather than quadratic form. The key difference for the RDD is that bias *only* depends on the extent to which measurement error assigns observations to the wrong side of the cutoff. Proofs and further details for both the RKD and RDD are in Appendix B.

The dominant framework used in an RKD/RDD to address measurement error in the running variable and/or a non-deterministic relationship between the explanatory/policy variable (here, seizable equity) is a fuzzy RKD/RDD (Card et al., 2015). When there is measurement error in the running variable, two assumptions are necessary for the fuzzy RKD/RDD estimand to identify a local average response. First, the econometrician must

[19] I drop earlier years for Delaware and Maryland in which they had homestead exemptions of $0.

have observations of the policy variable not directly computed from the running variable. Second, the econometrician must assume that there is a point mass of correctly 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 evaluating the effect of means tested policies in data where income is mis-measured and eligibility/treatment is not directly observed. To my knowledge, this paper is the first to highlight that calculating the policy variable based on a mis-measured running variable violates the identifying assumptions of Card et al. (2015). When this assumption fails, the fuzzy RKD/RDD identifies a parameter proportional to the local average response.[20]

The second assumption of a point mass of correctly measured observations is also strong for many settings. When both assumptions hold, the fuzzy RKD/RDD estimand identifies the local average response conditional on not only being at the cutoff of but also conditional on an observation having zero measurement error. When this second assumption fails, the fuzzy RKD/RDD estimand equals zero and therefore does not identify the local average response.

To overcome these challenges, I propose a new *parametric* approach. This approach assumes that the outcome is a quadratic function of the true values of the running and policy variables and unobserved factors additively affect filing.[21] This assumption yields an alternative characterization of the local average response targeted by the sharp RKD estimand of Card et al. (2015) as a function of parameters. I assume that measurement

[20]Specifically, the estimand would identify the local average response multiplied by the mass of correctly measured observations.

[21]This may be a reasonable approximation for my setting as the plot of the kinked relationship between filings and equity distance appears well-approximated by quadratic functions (see Figure 2).

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.

Next, I define a parametric least-squares estimator for the local average response and characterize its bias relative to the local average response under the assumptions above. The parametric RKD estimator is

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

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 (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). The local average response is

$$\tau = \frac{\beta_1^+ - \beta_1^-}{S'(D)^+ - S'(D)^-}.$$

Under simplifying assumptions,[22] the probability limit of the numerator of the para-

[22] The assumptions are symmetry in the distribution of the running variable as in Griliches and Ringstad (1970) and Pei and Shen (2017) and that the covariance between the true and mis-measured running variable is the same regardless of their signs. I relax these assumptions in Appendix B.

metric RK estimator is

$$\widehat{\beta}_1^+ - \widehat{\beta}_1^- \xrightarrow{p} \underbrace{\left(1 - \frac{\sigma_\mu^2}{\sigma_x^2}\right)}_{\text{attenuation bias}} \underbrace{(1 - \pi^+ - \pi^-)}_{\text{assignment bias}} (\beta_1^+ - \beta_1^-). \tag{6}$$

The above expression characterizes the bias of the estimator relative to the true difference in the linear terms $\beta_1^+ - \beta_1^-$. Here, σ_x^2 is the variance of the mis-measured running variable and σ_μ^2 is the variance of the measurement error. The first term on the right-hand side is attenuation bias, which causes the estimate to shrink in magnitude as the variance of the measurement error (σ_μ^2) grows. I label the second term "assignment bias", as it captures bias due to measurement error assigning observations to the "wrong" side of the kink. In this term, $\pi^+ \in (0, 1)$ denotes the probability that an observation with a positive value for the running variable has a true negative value (and vice versa for π^-). This term biases the estimator towards the opposite sign, and would flip the sign if a majority of observations were assigned to the wrong side (i.e., $\pi^+ + \pi^- > 1$).

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

$$\widehat{\tau}^{PRK-ME} = \frac{\widetilde{\beta}_1^+ - \widetilde{\beta}_1^-}{S'(D)^+ - S'(D)^-} \tag{7}$$

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

where $\widehat{\sigma}_x^2, \widehat{\sigma}_\mu^2, \widehat{\pi^+}$, and $\widehat{\pi^-}$ are the sample variances of the mis-measured running variable and measurement error and the sample averages of how often observations are assigned to the wrong side.[23] I estimate these parameters using a subsample of nearly 200,000 households that are currently selling their home (both filers and non-filers). I treat the observed sale price as the true market value. Comparing this "true" price to the imputed home price, I then estimate the four required parameters needed to correct for measurement error. I assume that these four moments are the same in the subset of homes currently selling as in the full RKD sample. Appendix Figure D.8 suggests that this is a plausible assumption, showing that predicted measurement error, based on a variety of household and local economics characteristics, has a similar distribution among selling and non-selling homes.

4.3 Results: The Effect of Bankruptcy Generosity on Filing

The RKD estimate indicates that higher debt relief generosity in bankruptcy has a positive, but small, effect on bankruptcy filings. Table 1 reports estimates obtained through both standard nonparametric approaches and the new parametric approach correcting for measurement error. The preferred specification (column 2) uses this new approach and linearly controls for home equity. Table 1 scales estimates by the sample's average annual filing rate of 0.71%. The point estimate of column 2 implies that a \$1,000 decrease in seizable equity increases a household's probability of filing by 0.019 percentage points (a 2.63% relative increase over the 0.71% annual rate). The small filing response to relief generosity indicates that moral hazard is not a strong driver of household bankruptcy.

The 95% confidence interval, constructed as in Calonico et al. (2014), rules out an increase greater than 0.023 percentage points (a 3.30% relative change). The Calonico et al. (2014) approach improves upon standard asymptotic inference (i.e., Fan and Gijbels, 1996)

[23] One could do a variant of this approach using split-sample IV (SSIV, Angrist and Krueger, 1992). SSIV could correct for attenuation bias (the σ terms) but not assignment bias. So it would still be necessary to combine a SSIV-based estimate with estimates of $\widehat{\pi^+}$ and $\widehat{\pi^-}$ to recover the parameter of interest.

Table 1: The Effect of Bankruptcy Costs on Filing (RKD Estimates)

	(1)	(2)	(3)	(4)
RK est. $\left(\widehat{\frac{\partial p}{\partial s}}/p\right)$	-1.64***	-2.63***	-2.31***	-2.36***
Std err.	(0.21)	(0.34)	(0.43)	(0.45)
Bandwidth	67.06	67.06	49.42	89.30
Meas. error correction		✓	✓	✓
RKD poly. order	2	2	2	3
Home equity control order	1	1	3	3
LHS Effective Obs.	21,383,503	21,383,503	17,536,206	25,884,133
RHS Effective Obs.	24,637,614	24,637,614	20,013,724	29,029,069

Note. The estimates correspond to the percent change in filing rate, relative to the sample average of 0.71, in response to a $1,000 increase in seizable home equity. Estimation uses a uniform kernel. The value of the parameters used in the measurement-error correction are $\pi^+ = 0.109$, $\pi^- = 0.095$, and $\frac{\sigma_\mu^2}{\sigma_x^2} = 0.22$. Approximation bias-corrected robust standard errors are computed as in Calonico et al. (2014). The bandwidth is optimally chosen for each specification using the MSE-minimizing procedure of Calonico et al. (2014) and is displayed in thousands dollars. Columns 2-4 correct for measurement error. The effective number of observations used to estimate the left and right-hand-side (LHS and RHS) slopes within the bandwidth is displayed at the bottom of the table. For each column, the bandwidth selection step of estimation uses the full sample of 99,233,172 observations. Statistical significance: 0.05*, 0.01**, and 0.01***.

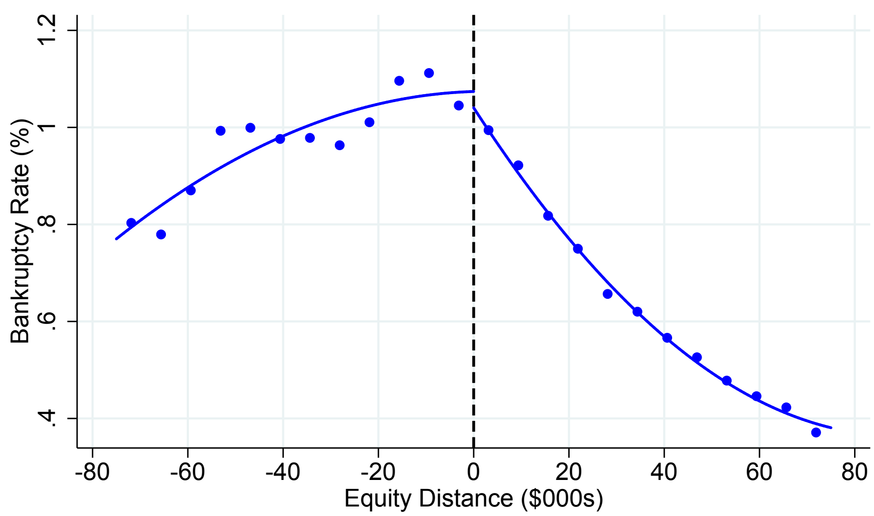
by performing well in terms of correctly distinguishing kinks from (unkinked) nonlinear relationships. An RKD with binary outcome variables does not a priori rule out detecting large effects, as the slope above the exemption limit can be arbitrarily steep.[24] In terms of inference, the large sample size of nearly 100 million observations is an important strength of the empirical setting for precisely estimating the small response to relief generosity.

Figure 2 plots the filing rate against equity distance. Below the exemption limit, filing

[24] A near-infinite response would appear in the RKD figure as a nearly vertical line above the limit.

rises as equity distance approaches zero. But once equity exceeds the exemption limit, and the household has some nonexempt equity, the slope changes and the filing rate begins to fall with equity distance. The *change* in the slope at the exemption limit corresponds to the RKD estimate. A larger moral hazard effect would correspond to a steeper (more negative) slope above the exemption limit. Other factors are likely correlated with both equity distance and bankruptcy, contributing to the upward-sloping relationship below the cutoff. But as long as these factors do not kink at the cutoff, their effect on bankruptcy is differenced out when calculating the change in slope.

Figure 2: The Effect of Seizable Equity on Bankruptcy Filings



Note. The points denote average (annualized) filing rates within equity distance bins. The lines are generated by fitting a quadratic polynomial to the individual observations on each side of the kink. See Appendix Figure A.3 for a version with many more bins.

Two factors likely contribute to the positive slope below the exemption limit. First, households farther below the cutoff may have less unsecured debt, which can diminish the incentive to file. Households with lower equity distance will tend to live in states with more generous bankruptcy exemptions, where they on average face higher borrowing costs and accumulate less unsecured debt (Pence, 2006; White, 2007; Severino and Brown, 2020). Second, as equity distance rises, the option value of bankruptcy diminishes, incentivizing more filing. Because households must wait at least two years (typically seven) before discharging debt in bankruptcy again, a cost of filing today is forgoing the option to file again soon. Far below the cutoff, households can expect to have similarly low seizable equity in the future. But as equity distance rises, they may expect that rising house prices and mortgage amortization will increase their seizable equity.[25]

4.3.1 Robustness and Interpretation

Measurement Error: The preferred specification (column 2) uses the measurement-error corrected estimator and linearly controls for home equity. The analogous nonparametric estimate is given in column 1 of Table 1. The $67,060 bandwidth for this benchmark specification is optimally selected using the procedure in Calonico et al. (2014), and I estimate the preferred specification within the same bandwidth. While both the preferred and benchmark estimates are small, correcting for measurement error increases the magnitude of the point estimate from -1.64 to -2.63.

A different source of measurement error arises from a limitation of the LLMA data. The LLMA data contain first mortgages, but second mortgage balances are not included alongside first mortgage balances. Omitting second mortgages will overstate home equity, effectively shifting this subset of borrowers to the right in Figure 2. However, it is possible to bound the impact of these missing loans on the RKD estimate using the prevalence of second mortgages. On average, 15% of US mortgage borrowers have a second

[25]The effect of equity distance on filing through this option value is differenced out as long as this value is a smooth function of equity distance. Intuitively, this would mean as households move from $1 below to $1 above the exemption limit, their expectations about the future value of bankruptcy evolve smoothly.

mortgage at a given point in time.[26] The second mortgage borrowers that are shifted right will have smooth behavior through the cutoff. At worst, the RKD estimate would be a weighted average of the true filing response and zero (with weights of 85% and 15%, respectively). This would imply a true filing response of -3.10% ($= -2.63/15\%$).

Specification: Estimates are similar across alternative specifications. Increasing the polynomial order of the running variable and home equity control has little effect. Using an Epanechnikov or triangular kernel also yields similar results.[27] The estimate is also similar under alternative bandwidth choices (see Appendix Figure D.3).

Permutation Test: The RKD estimate is also robust to the permutation test of Ganong and Jäger (2018), which is an alternative approach to inference in an RKD. The test repeatedly samples placebo exemption limits from their empirical distribution (e.g., assigning Minnesota's history of limits to Arizona). For each draw, I compute every household's distance to the placebo exemption and re-run the RKD estimation using these placebo measures of equity distance.[28] Intuitively, the test compares the extremeness of the actual RKD estimate to those for the placebo data.

As Ganong and Jäger (2018) note, this permutation test is a useful robustness check as it provides a conservative approach to inference. The test has exact size in finite samples (correctly rejecting the null hypothesis at exactly the desired level, e.g. 5%).However, compared to Calonico et al. (2014), the permutation test of Ganong and Jäger (2018) has

[26] This figure is calculated using borrower-level data from Experian. The Experian data contain 30.8 million observations for a geographically representative sample of 4.9 million mortgage borrowers.

[27] The point estimates under a triangular and Epanechnikov kernel are -2.58 and -2.77, respectively.

[28] In the placebo estimation step, 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).

a more restrictive null hypothesis: that there is no kink (i.e., seizable equity has no effect on filing) *and* the kink points are drawn from a *specific* distribution. To interpret the permutation test as testing for no kink requires assuming we have correctly specified the distribution of counterfactual kinks. Importantly, both methods perform well, in contrast to standard asymptotic inference (Fan and Gijbels, 1996), in distinguishing kinks from nonlinear relationships (Ganong and Jäger, 2018).

I run the permutation test using 1,000 random draws of exemption regimes. Appendix Figure D.4 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 at the 10% level.

Ex Ante vs. Ex Post Moral Hazard: The RKD identifies an *ex post* moral hazard response in that it reflects the impact of potential debt relief on filing for a given amount of debt. However, generous debt relief may also engender *ex ante* moral hazard behaviors, such as increased borrowing. Such behavior is also relevant for evaluating bankruptcy policy. Prior research estimates *negative* (Pence, 2006) or small effects of exemptions on household borrowing (Severino and Brown, 2020).[29] Additionally, delaying personal bankruptcy leads to more debt at the time of filing (Argyle et al., 2021). A key reason to focus on ex post moral hazard is that it is this particular behavioral response that appears in the sufficient statistic derived in Section 6.

Dissecting the relationship between interest rates and consumer default, Karlan and Zinman (2009) finds stronger evidence of ex post "moral hazard" compared to ex ante

[29] Severino and Brown (2020) finds that a 1% increase in asset exemption growth (approximately $469 in 2010 dollars) leads to a 0.018% rise in credit card debt.

adverse selection. Their notion of ex post moral hazard corresponds to the causal effect of debt payment size on default. In contrast, the moral hazard effect estimated here corresponds to the effect of the *payoff of default* on the decision to default. Larger debt payments can causally increase default through both a moral hazard and liquidity effect, by both raising the payoff of default and reducing cash-on-hand when not defaulting.[30] This distinction is important for policy. Karlan and Zinman (2009) posits that a strong effect of interest rates on default suggests costlier default is welfare-improving. This follows if the causal effect is mainly due to the moral hazard effect. But if it is mainly due to liquidity effects, it instead points to a high insurance value of bankruptcy, which can justify reducing default costs.

4.3.2 Heterogeneity

When and where are households most sensitive to changes in bankrupty's generosity? I first investigate this by estimating the RKD in subsamples with above and below median covariate values (Appendix Table D.3) and also by using interaction terms in an OLS estimation (Appendix Table D.4). I find households facing greater economic and financial distress (e.g., with higher leverage or in lower income ZIP codes), and at a higher risk of bankruptcy overall, are more sensitive to debt relief generosity. This runs counter to the narrative that drove the 2005 bankruptcy reform (BAPCPA) of widespread "abuse" of bankruptcy by economically secure households capable of repaying their debts.[31] Additionally, it differs from prior work finding that households at a low risk of delinquency

[30] Appendix E.4 formalizes this decomposition for the model presented in Section 6.

[31] For example, Senator Chuck Grassley, the sponsor of the reform, argued "Most people think it should be more difficult for people to file for bankruptcy. Americans have had enough; they are tired of paying for high rollers who game the current system and its loopholes to get out of paying their fair share." Source: https://www.grassley.senate.gov/news/news-releases/opening-statement-sen-chuck-grassley-bankruptcy-reform-hearing.

were most likely to default in response to a change in the generosity of mortgage debt relief they could obtain through delinquency (Mayer et al., 2014). These heterogeneity results are described in detail in Appendix D.4.

I also investigate how the moral hazard effect varies with the size of the homestead exemption. Appendix Figure D.5 modifies Figure 2 to plot households in high and low exemption states separately.[32] The strongest response comes from households facing low exemptions (-3.52 versus -0.60). And seizable home equity only has a statistically significant effect for households in low-exemption states. Recall that an RKD identifies the average response at the exemption limit, which means that the low-exemption households will have less home equity. The stronger moral hazard effect among households with lower housing wealth fits the general pattern documented here: weaker financial and economic conditions predict a stronger moral hazard effect.

The model of Section 6 gives insights into the mechanisms driving this heterogeneity. Stronger filing responses come from either (1) more filers being near the brink of bankruptcy or (2) the rule governing the bankruptcy decision being more sensitive. The first channel can help account for these patterns of heterogeneity to the extent that greater vulnerability to financial and economic distress tends to bring a household closer to the threshold of filing. When other forms of insurance, such as UI, are more limited, households can face more extreme low-consumption states of the world. This can make costly bankruptcy a relatively weaker deterrent to filing. Appendix E.5 formalizes these arguments in the context of the model.

Filing sensitivity also varies over time. Appendix Table D.2 estimates the RKD separately in three post-BAPCPA periods: pre-recession (2006 Q1 to 2007 Q4), recession (2008

[32] I classify households as belonging to a high exemption state if their homestead exemption at that time exceeds the median homestead exemption (in real 2010 dollars).

Q1 to 2010 Q4), and post-recession (2011 Q1 to 2016 Q1). In the recession, the estimate is more than doubles (compared to the pre and post-recession periods). This stronger sensitivity during the recession is consistent with the cross-sectional heterogeneity results above that find a stronger response among people facing worse economic conditions.

Splitting the sample into pre (2000 Q1 to 2005 Q2) and post-BAPCPA (2006 Q1 to 2016 Q1), the sensitivity to bankruptcy's generosity is unchanged. The point estimates are similar and not statistically different. However, in the period during which BAPCPA was anticipated but not yet implemented (2005 Q3 to Q4), the filing response to bankruptcy's generosity was nearly five times larger, suggesting that moral hazard was heightened during this period. The model of section 6 can help rationalize this spike in moral hazard. BAPCPA significantly reduced the generosity of bankruptcy for both high and low-income households through a means test and greater upfront filing fees (respectively). In this intermediate time period, the option value of waiting to file significantly eroded. When this value falls, this future cost of filing in the present becomes smaller.

4.4 Discussion: The Moral Hazard Effect

The RKD estimates a smaller filing response to bankruptcy generosity than prior work. Notably, Fay et al. (2002) investigates the relationship between filing and the financial benefit of bankruptcy using detailed household balance sheet data in the PSID. Comparing our estimates, the RKD implies a smaller effect: a 2.63% versus 5.03% relative decrease in the filing rate in response to a $1,000 decrease in the financial benefit of bankruptcy.[33] One potential reason for the larger estimate in Fay et al. (2002) is that adverse events like job loss or illness can lower wealth, reducing the cost of bankruptcy, and also indepen-

[33]To make our estimates comparable, I use the CPI to inflation-adjust the estimate of 7.0% reported in Table 5 of Fay et al. (2002) from 1996 to 2010 dollars to match the units of the RKD.

dently motivate filing for bankruptcy.

A second approach exploits the plausible exogeneity of homestead exemption laws. Exemption laws can affect filing both directly through seizable home equity and indirectly through general equilibrium channels such as credit supply.[34] The RKD estimates the moral hazard effect by isolating the direct effect of generosity. In contrast, instrumental variables and difference-in-difference approaches can identify the (causal) general equilibrium effect of generosity (e.g., Mahoney, 2015; Auclert et al., 2019; Pattison, 2019). Both effects are of independent interest and jointly can inform models assessing the positive and normative effects of generous bankruptcy.

A third approach uses the 2005 BAPCPA reform as a source of variation in filing costs (Mitman, 2016; Gross et al., 2021). BAPCPA increased the cost of filing for bankruptcy by raising *upfront* legal and court fees by $500-600 (Lupica, 2012) and by barring high-income households from filing under the weakly cheaper Chapter 7. In contrast, the RKD exploits variation in seizable home equity, which is a *backloaded* cost. Liquidity constraints could heighten sensitivity to upfront costs relative to backloaded ones.[35] Variation in backloaded costs directly affects consumption in the post-bankruptcy state of the world, which enables the sufficient statistic of Section 6 to relate to consumption pre vs. post bankruptcy. Additionally, a large-scale overhaul like BAPCPA may have been more salient to households than variation in exemptions and home equity. The RKD estimate may be attenuated relative to a fully-known/understood policy change.

[34] Empirically, generous exemptions are strongly associated with lower unsecured credit and higher interest rates (Gropp et al., 1997; Pence, 2006; White, 2007; Severino and Brown, 2020). Looking directly in the cross-section, filing tends to be *higher* where homestead exemptions are lower (Mitman, 2016; Indarte, 2022)—the opposite sign of the RKD estimate.

[35] Upfront costs can be such a strong barrier to filing that increases in seizable cash on hand (tax rebates) can *increase* the likelihood of filing for some households (Gross et al., 2014).

5 The Effect of Mortgage Payment Reductions on Bankruptcy Filings

This section examines the effect of mortgage payment reductions on household bankruptcy filings to quantify the strength of the liquidity effect. I exploit rules governing interest rate resets for adjustable-rate mortgages (ARMs) in an instrumental variables (IV) strategy as a source of exogenous variation in the size of payment reductions. In Section 5.3, I address challenges in comparing the IV estimate to the RKD estimate. These arise from differences in the sample populations, scenarios in which mortgage payments can affect the payoff of bankruptcy for a subset of filers, and the fact that one-time changes in payments may affect expectations over future payments.

5.1 Empirical Strategy: Instrumenting for Payments with ARM Index Rates

Isolating the liquidity effect requires exogenous variation in non-seizable cash flows. Changes in non-seizable cash flows affect borrower resources available both in and out of bankruptcy, but because they are not seizable in bankruptcy they do not distort the financial payoff of filing. This analysis focuses on mortgage payments because mortgage debt is generally not discharged in bankruptcy, therefore changes in mortgage payments are not generally seizable in bankruptcy. A challenge for causal inference is that creditors may be more willing to give lower-risk households larger mortgages. If households less prone to default tend to have larger mortgage payments, then OLS estimates could understate the strength of the liquidity effect. To address this, I exploit a natural experiment in which borrowers received different-sized mortgage payment reductions as a result of a plausibly exogenous mortgage contract feature.

The contract feature I exploit is the "index rate" of an adjustable rate mortgage (ARM). ARMs feature a fixed interest rate for an initial period, typically five years, and then begin to reset periodically to a new rate. During the floating rate period, the interest rate generally resets either every six or twelve months. The new "reset rate" is the sum of a "margin" (selected at origination) and the current value of a pre-selected index rate:

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

ARMs originated during the sample period typically had a margin of 2-2.5%, and popular choices for index rates were the one-year Libor and Treasury rates.

During 2003 to 2008, the Libor and Treasury rates had a nearly constant spread of about 25-50 basis points (see Figure 3). But in 2008, a large spread opened up between the two rates. As US monetary easing drove the Treasury rate near zero, Libor fell less due to distress in interbank lending markets.[36] The widened spread in this unusual period created a natural experiment in which otherwise identical mortgages received different-sized payment reductions. While *both* types of ARMs received reductions, the larger fall in the Treasury rate led to larger reductions for Treasury-indexed ARMs. At the spread's 218 basis point peak in September 2008, a household with a median-sized ARM would have paid $4,191 more over the next year if indexed to Libor rather than Treasury.

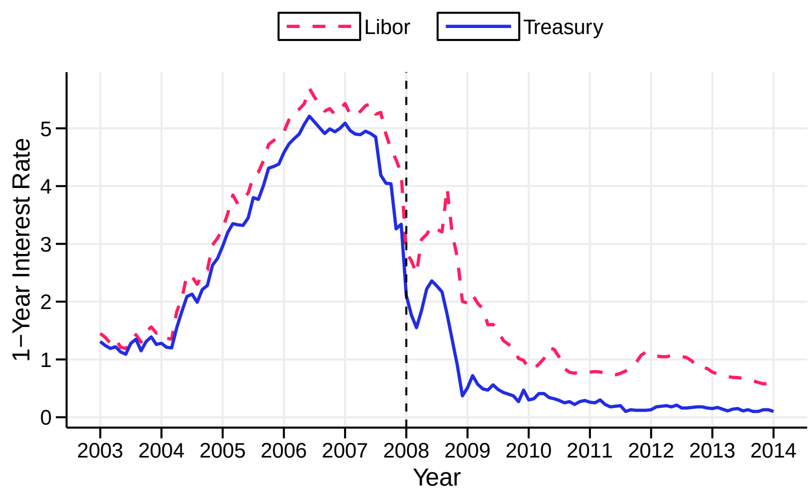
I exploit this natural experiment in an instrumental variables strategy. The parameter of interest is the coefficient β in the second stage equation:

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

where $\text{Bankruptcy}_{ict} = 1$ if household i in county c files for bankruptcy in month t. The

[36] The Libor rate is calculated from daily self-reports from the largest global banks of their expected borrowing costs on the interbank market.

Figure 3: One-Year Libor and Treasury Rates



Note. This graph plots one-year Libor and Treasury rates at a monthly frequency.

explanatory variable of interest is MPay_{ic}, which is the component of the new mortgage payment determined by the index rate (as opposed to the component due to the margin). The baseline specification includes county and 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

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

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.

Time fixed effects help absorb macro-level factors that influence filing. The IV estimator therefore identifies the effect of payments on filing from variation in the index rates *within* a given time period. The county fixed effect accounts for persistent differences in filing rates across locations. The controls include origination characteristics: the margin on the ARM, original payment level, FICO score, and loan-to-value (LTV) ratio. The time-varying controls are the log mortgage balance and log home equity in time t.[37] Controlling for home equity is important as it can affect filing through seizable equity. Additionally, rate resets can have a small effect on the rate at which the mortgage balance is paid off over the next year.Controlling for home equity helps to separate the direct effect of payment reductions on filing from indirect effects from changes in seizable home equity and the generosity of the debt relief the household would receive in bankruptcy.

Identification: The key identifying assumption is an exclusion restriction: the index rate only affects filing through the household's mortgage payment. What drives variation in the index rate choice? Mortgage lenders have persistent relationships with mortgage-backed securities (MBS) investors who purchase their mortgages, and purchasers differ in the denomination of their cost of funds (Libor or Treasury). Investors may prefer to purchase MBS with a payment structure matching that of their cost of funds. Gupta (2019) shows that lender identity explains over 50% of the variation in index rate choice.

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 to 2008 did not anticipate the upcoming widening spread and would not have a had a strong reason to prefer Libor indexation. The main differences between Libor and Treasury ARMs originated in this period was that Libor ARMs tend to have a larger mortgage balances. This makes it im-

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

portant to control for the balance in the IV estimation. Other mortgage characteristics and local economic conditions (county unemployment, income, etc.) are similar for Libor and Treasury-indexed mortgages.[38]

Additionally, I conduct a placebo test that compares filing rates in the year *prior* to the first reset for Libor versus Treasury-indexed households. The estimated difference in filing rates is small (approximately one tenth the magnitude of the corresponding IV estimate) and its 95% confidence interval includes zero.[39] This is evidence against selection of bankruptcy-prone households into Libor-indexed ARMs.

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. Similarly to Gupta (2019), the IV approach here uses *within-period* variation in the size of rate changes. While the mortgage rate change induced by resets is plausibly exogenous, the change in the *dollar amount* of the payments may be endogenous. Higher income and wealth households tend to have larger mortgages and, for a given mortgage rate change, will experience larger payment changes. Households with better access to larger mortgages may be less default prone. If so, OLS estimates using payment changes during resets would tend to understate the causal effect of payment reductions. The IV strategy addresses this problem by instrumenting for the size of the payment change using the mortgage's index rate.

Sample: This analysis uses a subsample from the LLMA of ARMs originated during 2003 to 2008. I truncate the sample to these years so that the mortgages were *originated*

[38]See Appendix Table A.1 for summary statistics and Appendix Table D.8 for a regression of an indicator for Libor indexation on mortgage and regional characteristics. The latter jointly tests for differences in characteristics.

[39]Appendix Table D.7 reports estimation results for the placebo test.

prior to the widening of the Libor-Treasury spread but *reset after*. Additionally, ARMs resetting after 2008 almost universally received rate decreases due to the low rate environment. 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 because increases create an incentive to refinance, which can introduce selection bias.

To keep timing consistent, I restrict the sample to ARMs indexed to a one-year interest rate (either Libor or Treasury) and with an annual reset frequency. I further restrict the monthly sample to the year of the first reset (so each mortgage has twelve observations). I also limit the sample to non-delinquent mortgages so that a reduction in payments results in an increase in cash-on-hand. In total, the subsample has 1.1 million monthly observations and includes 51,164 Libor-indexed ARMs and 45,186 Treasury-indexed ARMs. Appendix Table A.1 presents summary statistics for these mortgages and Appendix Table D.8 formally tests for observables that predict Libor indexation.

5.2 Results: The Effect of Payment Reductions on Filings

Table 2 reports the IV estimation results.[40] The estimate under the preferred specification (column 4) implies that a $1,000 decrease in annual mortgage payments leads to a statistically significant 0.21 percentage point drop in the annual bankruptcy filing rate (a 30% overall drop relative to the 0.71% average filing rate). The preferred specification includes loan age-time fixed effects, which means the regression implicitly compares mortgages originated and resetting at the same time. Intuitively, these mortgages were originated in a similar lending environment and are resetting in a similar macroeconomic climate, but reset to different payment levels due to the divergence in the Libor-Treasury spread over the five or more years since they were originated. The preferred specification

[40] Appendix Table A.2 reports full results, including coefficients on control variables.

also adds a county-time fixed effect to account for local economic developments affecting the bankruptcy decision.

The first-stage estimate in column 4 implies that a one percentage point decrease in the index rate on average leads to a \$1,397 decrease in annual mortgage payments. The first-stage F-statistics for the excluded instrument are consistently over 100, alleviating weak instrument concerns. Analogous OLS estimates are ten times smaller than the IV estimates (Appendix Table D.10). This is consistent with a negative OLS bias stemming from households who are less prone to filing obtaining larger mortgages.

This empirical setting departs from the experimental ideal in one key way that affects the interpretation of the IV estimates in Table 2. The ideal experiment randomly assigns households a *one-time* payment reduction without affecting the expected value of future payments. Although the reset rate applies for exactly one year, this one-time change may affect expectations over future mortgage payments. To address this, I next combine estimates of the expected NPV of mortgage payments and the IV estimate to isolate the filing response to a one-time change in cash flows. This yields an estimate of the liquidity effect that we can compare against the RKD estimate of the moral hazard effect.

5.3 Comparing Moral Hazard and Liquidity Effects

This next part presents two adjustments to the IV estimate of the liquidity effect that make it more comparable to the RKD estimate of the moral hazard effect. The first adjustment scales the IV estimate to reflect the filing response to a change in the net present value (NPV) of mortgage payments, making the estimate units more comparable. The second adjustment addresses the possibility of selection on the types of borrowers in the ARM sample. After making both adjustments, I estimate that the liquidity effect is nearly five times as strong as the moral hazard effect.

Table 2: IV Estimates of Filing Response to Liquidity

	(1)	(2)	(3)	(4)
	Panel A: Second Stage (outcome = Bankruptcy$_{ict}$)			
MPay$_{ic}$	30.72***	27.49***	33.49***	29.98***
	(7.35)	(7.64)	(8.46)	(8.48)
	Panel B: First Stage (outcome = MPay$_{ic}$)			
Index Rate$_{ict}$	1,275***	1,253***	1,384***	1,397***
	(105.92)	(110.02)	(126.31)	(130.01)
Stage 1 F-Stat.	144.99	129.62	120.14	115.40
Observations	1,092,072	1,092,072	1,092,072	1,092,072
Time FE	✓	✓	✓	✓
County FE	✓	✓	✓	✓
Loan Age FE		✓	✓	✓
Loan Age × Time FE			✓	✓
County × Time FE				✓

Note. 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). Coefficients on controls are omitted. Statistical significance: 0.05*, 0.01**, and 0.01***.

Accounting for Expectations of Future Payments: If a one-time reduction in required annual mortgage payments lowers expected future mortgage payments, the IV estimate may overstate the *direct* effect of a one-time payment reduction. On the one hand, the estimated *total* effect is policy-relevant; for example, it could help quantify the impact on bankruptcy of policies that encourage wider usage of ARMs to improve the pass-through of monetary policy (e.g., Eberly and Krishnamurthy, 2014; Piskorski and Seru, 2018). On the other hand, because the RKD estimate of the moral hazard effect reflects the effect of a one-time change in a state variable, it is ideal to estimate the liquidity effect as the response to a one-time change in cash flows to make the units more comparable.

To isolate the effect of a *one-time* change in mortgage payments, I first estimate the expected NPV of payments as a function of the current payment. This makes it possible to map the initial IV estimate to the response to a one-time change in the NPV of payments. If households face liquidity constraints, they may be more responsive to cash in the present than an NPV-equivalent amount in the future. Therefore, the response to a \$1 change in NPV is likely a lower bound for the response to a one-time \$1 change in cash-on-hand.

Consider the problem of estimating the expected NPV of mortgage payments for a household with a 30-year ARM whose annual mortgage payment resets to M_τ in month τ. Suppose the household discounts their month $\tau + j$ expected future mortgage payments $\mathbb{E}_\tau(M_{\tau+j})$ at rate r. Let s_t denote the survival rate of the mortgage, that is, the probability that the household does not prepay and exit the mortgage early in period t, conditional on remaining in the mortgage up to month t. Denote the monthly delinquency rate by δ. The expected NPV of payments is

$$M_\tau^{NPV} = \underbrace{s_\tau(1-\delta)M_\tau}_{\text{current payment}} + \underbrace{\sum_{j=1}^{360-\tau} s_{\tau+j}(1-\delta)\frac{\mathbb{E}_\tau(M_{\tau+j})}{(1+r)^j}}_{\text{future payments}}. \tag{8}$$

To estimate the relationship between the NPV and current payment, I first estimate the survival and delinquency rates from the LLMA data. Because most observations are censored by year nine, I assume the survival rates follow a Weibull distribution and parametrically estimate rates over the life of the mortgage.[41] The average monthly delinquency

[41] I estimate scale and shape parameters $\alpha = 5.9\text{e-}4$ and $\gamma = 1.59$, respectively, for the Weibull survival

rate is 1.67%. I assume households discount their mortgage payments at the average market interest rate, which is consistent with the findings of Busse et al. (2013) for auto loans. Specifically, I assume households discount future payments at an annual rate of 4.39%, which is the average annual rate on 30-year fixed rate mortgages during the sample period.[42] Finally, I assume expectations over future mortgage payments are Martingale, which implies $\mathbb{E}_\tau(M_{\tau+j}) = M_\tau$ for all periods j.[43] When expectations are Martingale, the NPV is linear in the current payment:

$$M_\tau^{NPV} = M_\tau \underbrace{\sum_{j=0}^{30-\tau} \frac{s_{\tau+j}(1-\delta)}{(1+r)^j}}_{\equiv 12\times\theta}. \tag{9}$$

Estimating the mapping between current payments and the NPV of payments entails estimating the annual scaling factor θ. The chosen discount rate and estimated survival and delinquency rates yield an estimate of 6.23 for θ. In the ARM sample, the average monthly delinquency rate is 1.63% and the median mortgage duration is seven years beyond the time of reset. Scaling the original estimate of a 30% decrease in the probability of filing, following a $1,000 reduction in annual mortgage payments, implies a 4.82% decrease in response to a one-time change in payments.

Accounting for Sample Differences: The final step to make the IV estimate comparable to the RKD estimate is to address potential differences in the samples used. The RKD sample is much larger (100 million versus 1 million), broader, and spans 2000 to

function $S(t) = \exp(-\alpha t^\gamma)$.

[42] 4.39% is the average rate over 2008 to 2016, calculated using weekly data furnished by Freddie Mac, accessed via FRED (under the series name "MORTGAGE30US").

[43] This assumption is also conservative if households expected low rates to be temporary. The approach here would overstate the impact on expected payments reductions if low rates were not expected to persist.

2016. Households with ARMs tend to buy more expensive homes and use more leverage compared to the RKD sample. The ARM sample is also concentrated in the financial crisis (2008 to 2016), which could feature a higher-than-average liquidity effect. However, the ARM sample also restricts to non-delinquent households, which could select on less-liquidity-constrained households with weaker liquidity effects.

To obtain a more representative estimate of the average liquidity effect, I redo the IV estimation using the procedure of DiNardo et al. (1996). This entails a weighted IV estimation, where the weights are chosen to match the ARM sample to the RKD sample along a vector of observables. To construct the weights, I pool the ARM and RKD samples and estimate a probit regression, where the outcome is an indicator for appearing in the RKD sample. The explanatory variables are county-level median income and unemployment rates, origination FICO scores and LTV ratios, and annual ZIP-level house price growth. The probit regression yields a *predicted* probability $\widehat{p}_i$ of appearing in the RKD sample for each observation i. The weights used in the ARM IV estimation are

$$w_i = \frac{\widehat{p}_i}{1-\widehat{p}_i} \times \left[\frac{\sum_j^N \mathbf{1}(j \text{ is in RKD sample})/N}{1-\sum_j^N \mathbf{1}(j \text{ is in RKD sample})/N}\right].$$

Comparing Moral Hazard and Liquidity Effects: Table 3 gives IV estimates incorporating both the DFL and NPV adjustments. When using the preferred specification and both adjustments, the estimate implies that a \$1,000 one-time reduction in annual mortgage payments lowers the probability of filing by 0.09 percentage points (a 12.59% change relative to the average annual filing rate of 0.71%). This estimate of the liquidity effect is five times larger than the RKD estimate of the moral hazard effect (12.59 vs. 2.63).

The estimates indicate that the decision to file for bankruptcy is more sensitive to

Table 3: IV Estimates of Filing Response to Liquidity (NPV and Composition Adjusted)

	(1)	(2)	(3)	(4)
	Panel A: Second Stage (outcome = Bankruptcy$_{ict}$)			
MPay$_{ic}$	11.81***	11.01***	14.83***	12.59***
	(2.96)	(3.22)	(3.37)	(3.47)
	Panel B: First Stage (outcome = MPay$_{ic}$)			
IndexRate$_{ic}$	4,866.52***	4,671.64***	5,136.90***	5,179.40***
	(287.82)	(312.31)	(361.54)	(367.44)
Stage 1 F-Stat.	285.89	223.76	201.88	198.69
Observations	1,059,194	1,059,194	1,059,194	1,059,194
Time FE	✓	✓	✓	✓
County FE	✓	✓	✓	✓
Loan Age FE		✓	✓	✓
Loan Age × Time FE			✓	✓
County × Time FE				✓

Note. These specifications use both the NPV and DFL-adjustments described in the text. 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 NPV of mortgage payments 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). Appendix Table A.2 reports estimates for the control variables too. Statistical significance: 0.05*, 0.01**, and 0.01***.

changes in cash-on-hand (holding constant the wealth gain from filing) compared to changes in the wealth gain from filing (holding constant cash-on-hand). The strong liquidity effect is consistent with households lacking insurance against shocks to their liquid wealth and relying on bankruptcy as a form of insurance, and not simply as a means to increase their wealth. Together, these estimates suggest that one cost of generous debt relief in bankruptcy (ex post moral hazard) is relatively small, and there may be important insurance benefits to filers. This suggests generous bankruptcy may be a useful tool in mitigating market incompleteness.

Robustness: An alternative approach to the DFL adjustment for achieving internal validity is running the RKD and ARM estimation on the exact same subsample. The overlapping sample contains 86,256 monthly observations (33,585 quarterly) for 7,809 households.[44] In this subsample, I estimate that the liquidity effect is 1.3-8.2 times stronger than the moral hazard effect. The point estimates here imply a $1,000 increase in non-seizable cash reduces filings by 14.86-92.42% (versus 12.59-78% for the full ARM-IV sample). An equivalent increase in relief generosity reduces filing by 11.28% (versus 2.63% for the full RKD sample). However, estimates on this smaller subsample are much less precise and are not statistically different from each other at the 5% level.

If filing does not respond to future changes in mortgage payments, the DFL-adjusted estimate of a 78% decrease would capture the response to one-time $1,000 reduction in annual mortgage payments. A null effect of future changes could arise from either inattention, myopia, or liquidity constraints.[45] To test for sensitivity to future expected payments, I examine the filing response to movements in the index rate in the twelve months *prior* to the reset and find they do not predict filing (Appendix Table D.9). However, loan contract terms (or their effects) could become more salient after a payment reset. While suggestive, this evidence cannot definitively rule out that future payments affect filing.

[44] This sample is much smaller because this entails restricting observations to those with ARMs, in the 14 states with no exemption doubling for couples, and non-delinquent households. See Appendix Tables D.11 for estimation results.

[45] Consistent with liquidity constraints, Ganong and Noel (2020) find that mortgage default and consumption, among financially distressed households, are significantly more responsive changes in short-term mortgage payments compared to reductions in the NPV of mortgage payments. Consistent with inattention/myopia, households do not always respond to profitable refinancing opportunities (Andersen et al., 2020; Keys et al., 2016), and those with ARMs often underestimate or do not know how much their interest rates could change (Bucks and Pence, 2008).

In this sense, NPV-adjusting the estimate is conservative.

How does Chapter choice impact identification of the liquidity effect? In Chapter 13 cases, which comprise 31% of consumer bankruptcies, filers pay the maximum of seizable assets or disposable income.[46] Court guidelines for calculating disposable income subtract "essential" expenses such as mortgage payments. Thus, when disposable income is sufficiently high, a reduction in mortgage payments is not "received" in bankruptcy because it increases disposable income, which is paid to creditors. Effectively, a filer in this scenario has the additional cost of forgoing the mortgage payment reduction when filing. And in this scenario the payment reduction deters bankruptcy by both increasing cash-on-hand (liquidity) and decreasing the generosity of bankruptcy (moral hazard).[47] For these filers, the IV estimate would equal the sum of the moral hazard and liquidity effects. This would also be the case if a non-filer would *switch* to becoming delinquent in bankruptcy, as they would no longer "receive" the payment reduction in bankruptcy if they are not paying their mortgage.[48]

These scenarios likely have a limited quantitative impact. To bound their impact, consider the extreme case where *all* households only receive the payment reduction outside of bankruptcy. The true liquidity effect would be a 9.96% (12.59% - 2.63%) reduction in filings per $1,000 change in cash. This lower bound implies that the liquidity effect is at least 3.8 times larger than the moral hazard effect. If this scenario applied to 31% of filers (the fraction that file for Chapter 13), the IV and RKD estimates would imply a liquidity

[46]This figure is the average Chapter 13 share of Chapter 7 and 13 cases over 2000 to 2016, calculated from the *State Filing Trends* reported by the American Bankruptcy Institute (https://www.abi.org/newsroom/bankruptcy-statistics).

[47]Appendix E.4 formalizes this claim by using the model of Section 6 to decompose the filing to a change in seizable resources into the sum of moral hazard and liquidity effects.

[48]This second scenario may be unlikely as bankruptcy appears to be more of a substitute rather than a complement to mortgage delinquency (Mitman, 2016). By erasing unsecured debt obligations, bankruptcy can make it easier for households to keep up with mortgage payments and avoid delinquency.

effect of 11.77% filings per $1,000 (4.5 times the moral hazard effect).

5.4 Discussion: Moral Hazard, Liquidity, and Default

Estimating the liquidity effect for bankruptcy adds to our broader understanding of the importance of liquidity in driving household default. Prior work finds mortgage payment reductions significantly reduce mortgage delinquency and foreclosures (Di Maggio et al., 2017; Fuster and Willen, 2017; Gupta, 2019; Ganong and Noel, 2020). These other forms of default may be sensitive to liquidity as a result of cash flow shocks making it infeasible to repay debt. Bankruptcy, in contrast, is not automatically triggered by missing debt payments and is rarely creditor-initiated. It is therefore not obvious that we should have expected a large filing response given the large delinquency/foreclosure response to liquidity shocks. The strong filing response is consistent with the marginal filer anticipating a large consumption gain from filing. The consumption-smoothing benefits of default, and not only feasibility of repaying, are important drivers of household default.

Moreover, evidence of a stronger liquidity effect implies that moral hazard plays a relatively minor role in driving the relationship between debt payments and default on the *dischargeable* debt. Higher payments incentivize default on dischargeable debt by both (1) reducing cash-on-hand, making it harder to smooth consumption (liquidity) and (2) by increasing the payoff of default, reducing the incentive to repay (moral hazard). The relationship between debt payment size and delinquency is often interpreted as pure moral hazard (e.g., Adams et al., 2009; Karlan and Zinman, 2009; Gupta and Hansman, 2022). The estimated moral hazard and liquidity effects imply that at least 83% of the causal effect on filing of dischargeable debt payments is due to liquidity.[49] This distinction is

[49] The implied response to a $1,000 rise in seizable resources (i.e., cash available only outside of bankruptcy) is a 12.59% + 2.63% = 15.22% rise in filings. 83% of this response comes from the liquidity effect (12.59%).

important because the default response to high debt payments may be mainly efficient in that it helps households smooth consumption and is not primarily a result of distorted incentives to repay.

6 Moral Hazard and Liquidity in a Model of Household Bankruptcy

This section analyzes a model of the household bankruptcy decision. I derive comparative statics that characterize the filing response to changes in (1) the generosity of bankruptcy and (2) non-seizable cash flows (corresponding to the RKD and ARM IV estimates, respectively). Using a sufficient statistics approach, I show that the weaker moral hazard effect has two implications. First, marginal utility *in* bankruptcy is much lower than *outside* of bankruptcy for the marginal filer. This means that the marginal filer anticipates a large increase in consumption upon filing. We can also interpret this as households tolerating very low consumption before being willing to file for bankruptcy. Second, households perceive non-monetary or dynamic costs, such as stigma or credit market exclusion, as 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 Baseline Model

A representative household lives for two periods $t \in \{1,2\}$. At the beginning of each period, they draw a stochastic income shock $y_t \sim F(y_t)$. Markets are incomplete, and the household can only borrow using debt d_t at interest rate $R_t(d_t)$. Every period, they have the option to file for bankruptcy.

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. If the household files, they keep e exempt assets and completely discharge their debt d_t.[50] The household incurs a utility penalty $\sigma > 0$ when filing, which reflects social stigma: a moral aversion to default. Regardless of their filing decision, they receive payout a from an annuity each period. The budget constraints are:

$$\begin{aligned} c_1^N &= y_1 + a - R_1(d_1)d_1 + d_2 \qquad\qquad c_t^B = a + e, \quad t = 1, 2. \\ c_2^N &= y_2 + a - R_2(d_2)d_2 \end{aligned} \tag{10}$$

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

The household chooses consumption, whether to file for bankruptcy, and (in the first period) borrowing, in order to maximize the present value of utility. Utility each period is a strictly increasing and strictly concave function $u(\cdot)$ of consumption. The household chooses to file for bankruptcy when the expected present value of utility in bankruptcy is higher than when not filing, following a threshold rule with respect to income.[51] 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 income threshold such that the household is indifferent between filing and not filing. For simplicity, the notation omits the dependence of $y_t^\star$ on d_t.

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

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

[50] I assume the support of y_t is bounded below by e, so that $y_t > e$.

[51] 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 Arellano (2008).

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 (10).

The first-order condition governing borrowing is

$$u'(c_1^N) = R_2(d_2) \int_{y_2^\star}^{\infty} u'(c_2^N) dF(y_2).$$

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

$$V_1^B = V_1^N(y_1^\star, d_1) \tag{11}$$

The probability that the household files for bankruptcy in period t, denoted p_t, is the probability that their 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: Moral Hazard and Liquidity Effects

Changes in the exemption level e affect filing p through a moral hazard effect while changes in non-seizable annuity a affect filing p through a liquidity effect. The direct effect of a higher exemption e is to raise wealth available in bankruptcy. This corresponds to the negative of the RKD estimate. The ARM IV identifies the direct effect of a marginal change in wealth available both in and out of bankruptcy, corresponding to variation in the annuity a.

The direct effects on the filing probability of a change in the $t = 1$ level of the exemption e or the non-seizable annuity a are:

$$\frac{\partial p_1}{\partial e_1} = f(y_1^\star)\frac{\partial y_1^\star}{\partial e_1} = f(y_1^\star)\frac{u'(c_1^B)}{u'(c_1^{N\star})} \tag{12}$$

$$\frac{\partial p_1}{\partial a_1} = f(y_1^\star)\frac{\partial y_1^\star}{\partial a_1} = f(y_1^\star)\frac{u'(c_1^B) - u'(c_1^{N\star})}{u'(c_1^{N\star})}. \tag{13}$$

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 (11). Above, $c_1^{N\star}$ denotes consumption when not filing and income is *at the bankruptcy threshold* (i.e., $y_t = y_t^\star$). Equation (12) is the moral hazard effect. For a strictly increasing utility function, the moral hazard effect is positive: filing increases when the payoff of bankruptcy rises. Equation (13) is the liquidity effect. In general, its sign is ambiguous, but the ARM IV estimate implies it is negative: filing falls when cash-on-hand rises.

If households faced *perfect* credit and insurance markets, they could smooth their consumption perfectly and marginal utility would not differ across the bankrupt and non-bankrupt states. The moral hazard effect would simply equal $f(y_1^\star)$ and the liquidity effect would equal zero. Filing would still respond to debt relief because it distorts incentives to repay. With perfect markets, bankruptcy provides no insurance value and the option to file is socially inefficient if there is any moral hazard effect. The strong empirical response to cash-on-hand implies markets are far from perfect and suggests bankruptcy can provide welfare-enhancing insurance.

Sufficient Statistic: The ratio of the liquidity and moral hazard effects equals

$$\frac{-\partial p_1/\partial a_1}{\partial p_1/\partial e_1} = \frac{u'(c_1^{N\star})}{u'(c_1^B)} - 1. \tag{14}$$

Taking their ratio eliminates the $f(y_1^\star)$ term and isolates terms related to marginal utility.[52] The term on the right is the relative difference in marginal utility when filing versus not filing (for the marginal filer). When the liquidity effect is negative (i.e., $\partial p_1/\partial a_1 < 0$), as estimated in Section 5, the left term is positive. Therefore, when the liquidity effect is stronger than the moral hazard effect ($-\partial p_1/\partial a_1 > \partial p_1/\partial e_1$), marginal utility is higher outside of bankruptcy than in bankruptcy. Intuitively, by comparing these filing responses, we can infer from revealed preference that a marginal dollar is more valuable outside of bankruptcy than in bankruptcy.

Implications for Consumption: When the liquidity effect is larger than the moral hazard effect, the marginal filer anticipates a larger increase in consumption upon filing. This follows from marginal utility being higher when not filing. For CRRA utility with a relative risk-aversion parameter (γ) of two, the estimated effects imply the marginal filer's consumption is 58.4% lower when *not* filing.[53] Chetty and Szeidl (2007) and Chetty (2008) suggest that a larger γ, such as five, may be appropriate if consumption commitments limit households' ability to adjust some consumption goods. For $\gamma = 5$, the estimates imply flexible consumption is 29.6% lower when *not* filing.

The relative consumption gain of infra-marginal agents would likely be even larger as their wealth outside of bankruptcy, by definition, is even lower than the marginal filer's.

[52] Directly estimating $f(y_1^\star)$ would be difficult, as it depends on the filing *threshold*.

[53] Because the sufficient statistic compares marginal utility across states of the world, as opposed to across time, risk aversion is an appropriate parameter to use in this exercise.

Such large increases in consumption suggest bankruptcy could play a useful role as an automatic stabilizer and enhance macroeconomic stability. Consistent with this, Auclert et al. (2019) estimates that access to bankruptcy boosted employment by nearly 2% in the Great Recession. With such large potential benefits, how to best design household debt relief remains an important question for future research. Regions with less access to debt relief during the Great Recession experienced *persistently* weaker economic conditions (Piskorski and Seru, 2021). In contrast, the CARES Act provided swift and substantial debt relief in response to the COVID-19 recession, significantly limiting household financial distress (Cherry et al., 2021). Recently, Auclert and Mitman (2022) argues that *countercyclical* bankruptcy generosity could have superior welfare benefits, by limiting moral hazard during booms.

Implications for Non-Monetary and Dynamic Costs of Bankruptcy: A stronger liquidity effect also implies that the marginal filer perceives costs outside of the immediate monetary cost of bankruptcy as large. These include non-monetary costs such as stigma or dynamic costs such as credit or labor market exclusion. Because the marginal filer is by definition indifferent between filing and not filing, if they anticipate a large consumption gain, this benefit must be offset by a large cost in order to maintain indifference. Let $\mathbb{E}^B$ and $\mathbb{E}^N$ denote the expectation conditional on filing and not filing in period one, respectively. The indifference condition is:

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

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

$$\underbrace{-\sigma}_{\text{utility penalty}} \quad - \underbrace{\left\{ p_2 \mathbb{E}^N(V_2^B) + (1-p_2)\mathbb{E}^N \left[V_2^N(y_2, d_2) \right] - \mathbb{E}^B \left[V_2^N(y_2, 0) \right] \right\}}_{\text{dynamic cost}} << 0 \qquad (15)$$

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 liquidity effect is much stronger than the moral hazard effect.

Intuitively, households' tolerance of a larger drop in consumption before being willing to file for bankruptcy is consistent with perceptions of costly default. Waiting until circumstances become sufficiently bad indicates that, by revealed preference, households have a strong aversion to bankruptcy. This aversion can help explain the "double-trigger" theory of default, which posits that both a large payoff and adverse events reducing liquidity are necessary to induce default (Foote et al., 2010; Elul et al., 2010; Foote and Willen, 2018; Ganong and Noel, 2022). Additionally, it can help explain why fifteen times as many households would financially benefit from bankruptcy than the number that actually file (White, 1998). Sizable stigma or dynamic costs could account for reluctance to default even when it would significantly increase wealth.

Dynamic costs could arise from credit and/or labor market exclusion. Credit reports for US filers contain a "bankruptcy flag" for seven to ten years after filing, which creditors/employers may view as a negative signal about the individual's type. Empirically, the removal of flags is associated with increased credit access (Musto, 2004; Dobbie et al., 2017; Herkenhoff et al., 2022; Gross et al., 2020). Bos et al. (2018) finds positive effects of flag removal on employment and earnings in Sweden, while Dobbie et al. (2020) estimates a precise null effect among US households.

Although bankruptcy flags may limit credit access, at the margin it is not obvious if a financially distressed household would have fared much better without filing. In fact, among delinquent households, those that file see better credit market access in the years following their bankruptcy (Albanesi and Nosal, 2022). Additionally, quasi-experimental evidence from the random assignment of judges finds that filing leads to improved earnings outcomes among those seeking bankruptcy protection (Dobbie and Song, 2015). This suggests that *at the margin* dynamic costs from credit and labor market exclusion are not likely a major deterrent to filing.

Three plausible sources of non-monetary costs are stigma, costly access to liquidity, and information/attention frictions. Upfront legal and court fees, typically on the order of $1,000, can be a significant barrier to filing (Gross et al., 2014). Acquiring sufficient funds may entail non-monetary costs such as forgoing leisure time or services from a liquidated asset such as a car. Additionally, households may have to exert costly effort to learn about bankruptcy rules, identify dischargeable debts, and value their assets.

Evidence on delinquency suggests that a moral aversion (stigma) may be an important deterrent to default. In a large survey, Guiso et al. (2013) finds that 82% of US households agree with the statement that it is morally wrong to default on mortgage debt when you are capable of paying. Additionally, randomized field experiments find that moralizing language motivates delinquent borrowers to begin repaying (Bursztyn et al., 2019) and that anticipated disclosure of default to peers deters delinquency (Diep-Nguyen and Dang, 2022). However, evidence of peer effects for bankruptcy suggest stigma may diminish if filing becomes more common (Kleiner et al., 2021).

Extensions: The sufficient statistic in equation (14) and the implications for consumption and non-monetary/dynamic costs are robust to several model extensions. Similarly to Chetty (2008), allowing for additional agent choices (e.g., labor supply or housing) and constraints (e.g., borrowing constraints) does not change the results under the assumption that a marginal change in the exemption e_1 or non-seizable endowment a_1 do not cause constraints to start/stop binding. An interesting example of such robust extensions are models with consumption commitments (Chetty and Szeidl, 2007).[54]

Allowing the model to have an arbitrary time horizon also leaves the results unaffected (see Appendix E.1). Dynamic costs from credit market exclusion after bankruptcy (with stochastic re-entry) appear in the dynamic cost term in equation (15) (see Appendix E.2). Introducing delinquency as an alternative to bankruptcy/repaying leaves the results unaltered in the sense that sufficient statistic still reflects differences in marginal utility when filing versus not filing (see Appendix E.3). However, "not filing" could now correspond delinquency instead of repaying debt. A stronger liquidity effect would then imply that the marginal delinquent filer still experiences a consumption gain from filing and perceives bankruptcy as costly. Additionally, Indarte (2022) shows that it is straightforward to adapt the framework here to characterize the moral hazard and liquidity effects for other forms of default such as *delinquency*.

[54]Models with "consumption commitments" feature choice sets with both a flexible good and a good with adjustment costs (e.g., food versus housing). In both the model of this paper or an extended version with consumption commitments, the comparative statics and sufficient statistic in Equations (12), (13), and (14) are in terms of the marginal utility of wealth (i.e., $u'(c) = \lambda$). However, in the consumption commitments model, although the marginal utility of wealth equals the marginal utility of the flexible good, it does not equal the marginal utility of the inflexible good if the household opts to not adjust it. Consumption commitments effectively create greater risk aversion within the non-adjustment region. If the marginal filer is a non-adjuster, the greater curvature of her value function with respect to wealth could be part of the reason why marginal utility is so different in versus out of bankruptcy. The insurance value of bankruptcy may be high because potential filers are constrained by consumption commitments.

Connection to Chetty (2008): Comparing the empirical results for bankruptcy here to those for UI in Chetty (2008), we both find liquidity to be a central driver in households' use of insurance. The causal effect of both UI benefits and debt payments on unemployment duration and default (respectively) has historically been labeled moral hazard. Chetty (2008) challenges this interpretation by estimating that liquidity effects account for 60% of the response to UI benefits. Similarly, the bankruptcy filing response to dischargeable debt payments can operate through both moral hazard and liquidity effects. The empirical estimates imply that 83% of the filing response to a marginal change in dischargeable debt payments is due to a liquidity effect. Together these findings suggest that liquidity, rather than moral hazard, is a more powerful driver of the utilization of insurance among households. Appendix E.6 presents a more detailed comparison of the theoretical results here with both Chetty (2008) and Dávila (2020).

7 Conclusion

This paper uses data on millions of mortgage borrowers and two quasi-experimental research designs to quantify the importance of moral hazard versus liquidity in driving household bankruptcy. Using an RKD and a kink in the cost of bankruptcy arising from asset exemption laws, I estimate a positive, but small, effect debt relief generosity on the probability of filing. Exploiting plausibly exogenous variation in mortgage payment reductions in an IV strategy, I estimate that the liquidity effect is five times stronger than the moral hazard effect.

This paper develops a new approach to correct for the effects of measurement error in both an RKD and RDD. In essence, this framework assumes that the relationship between the outcome and policy variables is quadratic (linear, for an RDD), rather than approximating it as such. Under these parametric assumptions, standard RKD and RDD estimators are biased towards zero when at least 50% of observations appear on the "correct" side of the cutoff. If more than 50% are on the "wrong" side due to measurement error, the estimators are biased towards the negative of the true causal effect. One useful direction for future research to build on this parametric approach would be to explore its implications for optimal bandwidth selection. There may be a justification to use a larger bandwidth to have more observations assigned to the "correct" side.

The weak moral hazard effect implies that increasing the generosity of bankruptcy only weakly incentivizes further filing, indicating that one of the key components of the cost of generous bankruptcy is small. The strong liquidity effect indicates that a lack of liquidity is a powerful driver of bankruptcy. In other words, households file for bankruptcy not because of they can get, but because of what they don't have.

Through a sufficient statistic, I show that when the liquidity effect is much stronger than the moral hazard effect, the insurance value of bankruptcy is large for the marginal filer. A stronger response to liquidity indicates by revealed preference that marginal utility is higher when *not* filing, for the marginal filer. This means the marginal filer anticipates a large gain in consumption—or, will tolerate a large drop in consumption before becoming willing to file. However, the stronger liquidity effect also implies that the marginal filer perceives non-monetary and/or dynamic costs of bankruptcy, such as stigma or future credit market exclusion, as large.

In terms of social welfare, the estimates point towards lower costs and higher benefits of generous bankruptcy. To further evaluate the positive and normative effects of changes to bankruptcy policy, a useful next step would be to use these estimates to discipline a structural model. The estimates quantify important channels shaping the key trade-offs of generous debt relief. Additionally, the strong implied consumption response for the marginal filer suggests bankruptcy may play a useful role as an automatic stabilizer. The estimates here, and related work on stigma, suggest that stigma may be an important force shaping the decision to default. Future work on stigma and bankruptcy, in particular investigations of whether it changes with bankruptcy policy, would be valuable and could also inform further structural work.

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Internet Appendix: for "Moral Hazard versus Liquidity in Household Bankruptcy"*

Sasha Indarte

Contents

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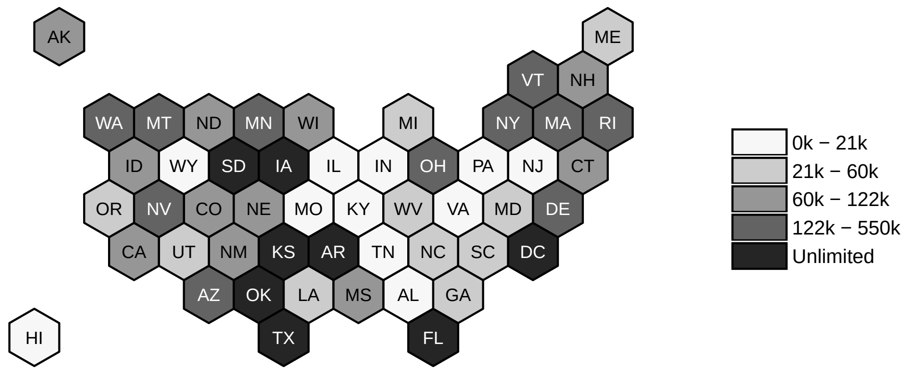
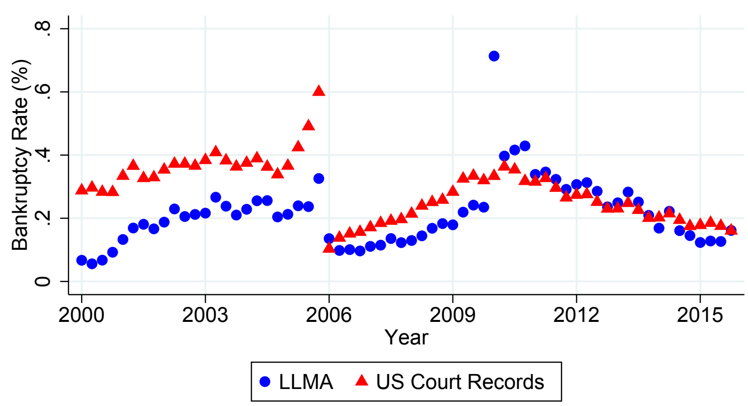
A Additional Figures and Tables

Figure A.1: Homestead Exemptions By State (2017)



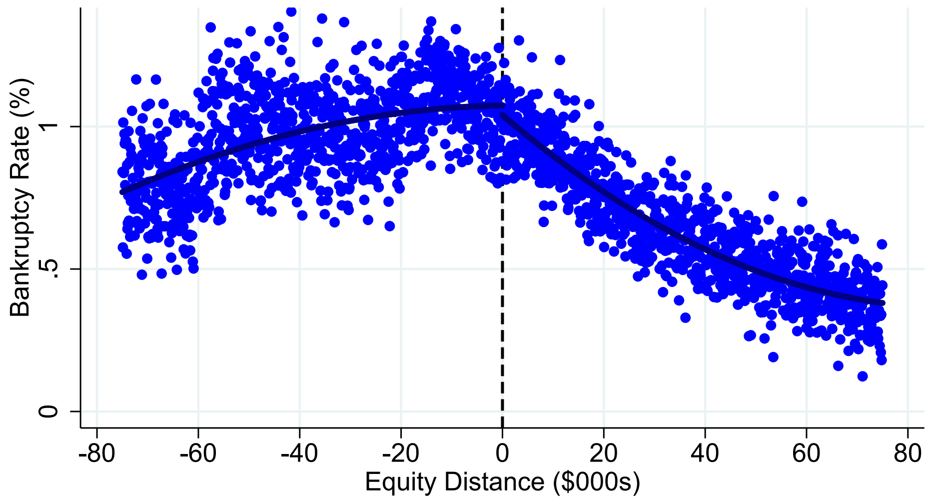
Note. This figure color-codes states based on their 2017 homestead exemption limits. For the states that allow doubling, the exemption level for single households is used.

Figure A.2: Quarterly Household Bankruptcy Rates in the LLMA Data vs. the National Rate



Note. 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.

Figure A.3: Bankruptcy Rates versus Seizable Home Equity (Additional Bins)



Note. The points denote average (annualized) filing rates within equity distance bins. The lines are generated by fitting a quadratic polynomial to the individual observations on each side of the kink.

Table A.1: Summary Statistics: Sample Averages

	Sample					
	RKD (all)	ARM (all)	RKD (under)	RKD (over)	ARM (Lib.)	ARM (Tre.)
Panel A: Borrower						
Home Value	276.80	346.29	245.53	315.70	323.52	371.91
Mortgage Bal.	172.28	248.40	173.82	170.36	267.94	226.42
Home Equity	104.52	97.89	71.71	145.34	55.58	145.48
Orig. Balance	204.47	274.65	104.21	128.34	291.04	256.21
Orig. LTV	77.72	72.88	79.00	76.14	74.43	71.13
Obs. (Mil.)	99.23	1.09	55.01	44.22	0.58	0.51
Orig. FICO	719.16	727.33	716.66	722.33	727.37	727.29
Obs. (Mil.)	85.54	1.09	47.80	37.74	0.58	0.51
Panel B: Bankruptcy						
Filing Rate	0.71	0.93	0.88	0.50	1.06	0.79
Equity Distance	-47.92	22.63	-156.34	86.94	-16.27	63.84
Homestead Ex.	152.44	80.64	228.05	58.40	73.56	88.14
Obs. (Mil.)	99.23	1.09	55.01	44.22	0.58	0.51
Panel C: Local Economy						
UR %	5.89	9.18	6.09	5.64	9.04	9.34
Obs. (Mil.)	98.95	1.09	54.93	44.01	0.58	0.51
Med. Inc	59.29	84.42	58.51	60.27	83.10	85.91
Obs. (Mil.)	99.23	1.07	55.01	44.22	0.57	0.50
HP Growth	1.83	-1.74	0.24	3.63	-1.35	-2.16
Obs. (Mil.)	70.32	0.74	37.40	32.92	0.38	0.36
Panel D: ARM Variables						
Libor Index. (%)	–	52.94	–	–	100.00	0.00
ARM Margin (%)	–	2.38	–	–	2.22	2.55
Old Payment	–	1,445.01	–	–	1,530.35	1,349.03
New Payment	–	1,111.64	–	–	1,088.44	1,137.75
Obs. (Mil.)		1.09			0.58	0.51

Note. This table presents summary statistics for both the RKD and ARM IV subsamples. The first two columns display averages for the RKD and ARM subsamples. The next two columns give averages for the RKD sample under and above the homestead exemption limit. The last two columns give averages for the ARM sample indexed Libor and Treasury. Nominal variables are all inflation-adjusted to be in terms of 2010 dollars and (except for old and new mortgage payments) given in terms of thousands of dollars. "Orig" denotes mortgage characteristics at the time of origination. The RKD sample restricts to states where married couples cannot double their homestead exemption while the ARM sample does not. The homestead exemption calculated for the ARM sample is the amount for a single filer and omits states with unlimited homestead exemptions. The subsample of the ARM sample where the homestead exemption is not unlimited contains 933,852 observations. Median income and unemployment rate data is measured at the county-level. House price growth is given at an annual rate and measured at the ZIP-level. The "old" and "new" payments are the monthly mortgage payment in the year prior and following the reset (respectively). Appendix Table D.8 jointly tests for differences in characteristics for Treasury and Libor-indexed ARMs.

Table A.2: ARM IV Estimation

	(1)	(2)	(3)	(4)
	Panel A: Second Stage (outcome = Bankruptcy$_{ict}$)			
MPay$_{ic}$	30.72***	27.49***	33.49***	29.98***
	(7.35)	(7.64)	(8.46)	(8.48)
Margin$_{ic}$	110.31***	99.44***	118.57***	107.10***
	(25.76)	(27.22)	(29.78)	(30.91)
Old Pay$_{ic}$	-6.46***	-5.70***	-6.78***	-6.10***
	(1.32)	(1.34)	(1.45)	(1.44)
Orig. FICO$_{ic}$	-0.75***	-0.75***	-0.75***	-0.77***
	(0.08)	(0.08)	(0.08)	(0.08)
Orig. LTV$_{ic}$	0.51*	0.59*	0.55*	0.69**
	(0.23)	(0.23)	(0.24)	(0.26)
ln(Home Eq.)$_{ict}$	-2.09***	-1.63**	-1.70**	-1.48*
	(0.53)	(0.54)	(0.56)	(0.61)
ln(Bal.)$_{ict}$	-12.51	-16.88	-20.35	-21.54
	(12.72)	(13.32)	(13.81)	(14.87)
	Panel B: First Stage (outcome = MPay$_{ic}$)			
IndexRate$_{ic}$	1,275.34***	1,252.55***	1,384.46***	1,396.60***
	(105.92)	(110.02)	(126.31)	(130.01)
Margin$_{ic}$	-3,120.45***	-3,139.87***	-3,220.32***	-3,280.06***
	(82.11)	(82.78)	(81.55)	(83.20)
Old Pay$_{ic}$	168.55***	166.09***	164.86***	163.95***
	(4.06)	(4.12)	(4.17)	(4.25)
Orig. FICO$_{ic}$	-0.98***	-0.85***	-0.78***	-0.74***
	(0.12)	(0.11)	(0.11)	(0.11)
Orig. LTV$_{ic}$	3.22***	1.56*	0.51	1.60*
	(0.58)	(0.65)	(0.67)	(0.68)
ln(Home Eq.)$_{ict}$	19.61***	9.07***	7.80***	8.90***
	(1.40)	(1.04)	(0.94)	(1.00)
ln(Bal.)$_{ict}$	612.38***	741.81***	766.12***	797.70***
	(59.64)	(61.71)	(62.47)	(64.61)
Stage 1 F-Stat.	144.99	129.62	120.14	115.40
Observations	1,092,072	1,092,072	1,092,072	1,092,072
Time FE	✓	✓	✓	✓
County FE	✓	✓	✓	✓
Loan Age FE		✓	✓	✓
Loan Age × Time FE			✓	✓
County × Time FE				✓

Note. This table reports results for the baseline ARM IV estimation. 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 NPV of mortgage payments 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). Statistical significance: 0.05*, 0.01**, and 0.01***.

Table A.3: ARM IV Estimates Under Various Adjustments

	(1)	(2)	(3)	(4)
	Panel A: 2^{nd} and 1^{st} Stage Estimates (Baseline)			
MPay_{ic}	30.72***	27.49***	33.49***	29.98***
	(7.35)	(7.64)	(8.46)	(8.48)
IndexRate_{ic}	1,275.34***	1,252.55***	1,384.46***	1,396.60***
	(105.92)	(110.02)	(126.31)	(130.01)
	Panel B: 2^{nd} and 1^{st} Stage Estimates (NPV-Adj.)			
MPay_{ic}	4.93***	4.41***	5.38***	4.81***
	(1.18)	(1.23)	(1.36)	(1.36)
IndexRate_{ic}	7,944.91***	7,802.94***	8,624.73***	8,700.35***
	(659.82)	(685.38)	(786.87)	(809.89)
Stage 1 F-Stat.	20.71	18.52	17.16	16.49
Observations	1,092,072	1,092,072	1,092,072	1,092,072
	Panel C: 2^{nd} and 1^{st} Stage Estimates (DFL-Adj.)			
MPay_{ic}	73.58***	68.56***	92.38***	78.45***
	(18.46)	(20.09)	(21.01)	(21.64)
IndexRate_{ic}	781.19***	749.90***	824.59***	831.41***
	(46.20)	(50.13)	(58.04)	(58.98)
	Panel D: 2^{nd} and 1^{st} Stage Estimates (DFL and NPV-Adj.)			
MPay_{ic}	11.81***	11.01***	14.83***	12.59***
	(2.96)	(3.22)	(3.37)	(3.47)
IndexRate_{ic}	4,866.52***	4,671.64***	5,136.90***	5,179.40***
	(287.82)	(312.31)	(361.54)	(367.44)
Stage 1 F-Stat.	285.89	223.76	201.88	198.69
Observations	1,059,194	1,059,194	1,059,194	1,059,194
Time FE	✓	✓	✓	✓
County FE	✓	✓	✓	✓
Loan Age FE		✓	✓	✓
Loan Age × Time FE			✓	✓
County × Time FE				✓

Note. This table reports second and first stage estimation results under the baseline specification and the baseline augmented with either the DFL, NPV, or both adjustments. 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 mortgage payments 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). Statistical significance: 0.05*, 0.01**, and 0.01***.

B RKD/RDD Measurement Error

This section presents the identification results for the new econometric approach used in the estimation. The first subsection gives results for an RKD, the second gives them for an RDD.

B.1 Measurement Error in an RKD

I first present the parametric assumptions on the relationship between the outcome and variables of interest. I then characterize the local average response (LAR), which is the same object identified by the sharp RK estimand of Card et al. (2015), as a function of parameters. Next, I show that, without measurement error, we can consistently estimate the parameters characterizing the LAR. I then introduce classical measurement error to the running variable and characterize the bias created due to measurement error. Finally, I define a measurement-error corrected estimator.

B.1.1 Setting and Identification of the Local Average Response (RKD)

The outcome y is a quadratic function of the running variable x and the explanatory/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. Specifically:

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

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 $\gamma^+ \neq \gamma^-$. Throughout, I continue to assume $x \in [-h, h]$, 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 $\mathbb{E}(x\varepsilon) \neq 0$).

Remark 1. *The parameter β_1^s identifies the LAR. To see this, take the conditional expectation of the partial derivative of y with respect to s and evaluate it at $x = 0$:*

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

Next, we can write β_1^s as a function of more easily estimable parameters. To do so, first rewrite the outcome:

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

Defintion 1. *Define the parametric RK estimand as*

$$\tau^{PRK} = \frac{\beta_1^+ - \beta_1^-}{\gamma^+ - \gamma^-}. \tag{16}$$

Remark 2. *The parametric RK estimand τ^{PRK} identifies the LAR:*

$$\tau^{PRK} = \frac{\beta_1^+ - \beta_1^-}{\gamma^+ - \gamma^-} = \beta_1^s = \mathbb{E}\left(\frac{\partial y}{\partial s}\bigg| x = 0\right).$$

Note that this is the same local average response identified in the sharp RK framework of Card et al. (2015). Parametric assumptions allow us to write this response in terms of parameters.

B.1.2 RKD Estimation and Consistency: Without Measurement Error

Here I introduce a least-squares estimator for the local average response. I also show that it is consistent for the parametric RK estimand in the absence of measurement error. The next part then characterizes this estimator's bias in the presence of measurement error in the running variable and presents a corrected estimator that eliminates this bias.

Notation: Let $\beta^+ = (\beta_0^+, \beta_1^+, \beta_2^+)'$ and $\beta^- = (\beta_0^-, \beta_1^-, \beta_2^-)'$. 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, for example, $\mathbf{x} = (1, x, x^2)'$ for a particular x. Let $\mathbb{E}^+$ denote the expectation conditional on $x \geq 0$. We can similarly define $\mathbf{X}^-$, Y^-, N^-, and $\mathbb{E}^-$.

The parametric RK estimator is defined formally below.

Defintion 2. *Define the parametric RKD estimator to be:*

$$\widehat{\tau}^{PRK} = \frac{\widehat{\beta}^+ - \widehat{\beta}^-}{\gamma^+ - \gamma^-}$$

where γ^+ and γ^- are known parameters and

$$\widehat{\beta}^+ = \left(\mathbf{X}^{+\prime}\mathbf{X}^+\right)^{-1}\mathbf{X}^{+\prime}Y^+ \quad \textit{(above-the-cutoff estimator)}$$

$$\widehat{\beta}^- = \left(\mathbf{X}^{-\prime}\mathbf{X}^-\right)^{-1}\mathbf{X}^{-\prime}Y^- \quad \textit{(below-the-cutoff estimator).}$$

Proposition 1 (Consistency of the Parametric RK Estimator without Measurement Error)**.** *In the absence of measurement error, if the omitted variables bias is the same above and below the cutoff, that is:*

$$\left[\mathbb{E}^+\left(\mathbf{x}\mathbf{x}'\right)\right]^{-1}\mathbb{E}^+\left(\mathbf{x}\varepsilon\right) = \left[\mathbb{E}^-\left(\mathbf{x}\mathbf{x}'\right)\right]^{-1}\mathbb{E}^-\left(\mathbf{x}\varepsilon\right),$$

then the parametric RK estimator is consistent for the parametric RK estimand:

$$\widehat{\tau}^{PRK} \xrightarrow{p} \tau^{PRK} \quad as \quad N^+, N^- \to \infty.$$

Proof.

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

Similarly, we have:

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

If $\left[\mathbb{E}^+\left(\mathbf{x}\mathbf{x}'\right)\right]^{-1}\mathbb{E}^+\left(\mathbf{x}\varepsilon\right) = \left[\mathbb{E}^-\left(\mathbf{x}\mathbf{x}'\right)\right]^{-1}\mathbb{E}^-\left(\mathbf{x}\varepsilon\right)$, then

$$\widehat{\tau}^{PRK} = \frac{\widehat{\beta}_1^+ - \widehat{\beta}_1^-}{\gamma^+ - \gamma^-} \xrightarrow{p} \frac{\beta_1^+ - \beta_1^-}{\gamma^+ - \gamma^-} = \beta_1^s = \tau^{PRK}.$$

□

B.1.3 RKD Estimation 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 continuous, linear, kinked 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 y and $x = x_\star + \mu$, where μ is zero mean noise (measurement error) and $\mathbb{E}(x_\star \mu) = \mathbb{E}(\varepsilon\mu) = 0$. That is, we have classical measurement error in the running variable. The following proposition characterizes the bias induced by measurement error in the parametric RK estimator. Without loss of generality, suppose that for both above and below-the-cutoff estimators we demean the outcome y and the mis-measured running variable x. Then the vector of regressors is now simply $\mathbf{x} = (x, x^2)'$, with $\mathbf{x}_\star$ defined similarly.

Proposition 2 (Bias in the Parametric RK Estimator with Measurement Error)**.** *If the omitted variables bias is the same above and below the cutoff, that is:*

$$\left[\mathbb{E}^+\left(\mathbf{x}\mathbf{x}'\right)\right]^{-1}\mathbb{E}^+\left(\mathbf{x}\varepsilon\right) = \left[\mathbb{E}^-\left(\mathbf{x}\mathbf{x}'\right)\right]^{-1}\mathbb{E}^-\left(\mathbf{x}\varepsilon\right) \equiv \Omega,$$

and the variance and covariance matrices are the same above and below the cutoff:

$$\begin{aligned} \mathbb{E}^+(\mathbf{x}\mathbf{x}') &= \mathbb{E}^-(\mathbf{x}\mathbf{x}') \equiv \Sigma \\ \mathbb{E}^{++}(\mathbf{x}\mathbf{x}_\star') = \mathbb{E}^{+-}(\mathbf{x}\mathbf{x}_\star') &= \mathbb{E}^{-+}(\mathbf{x}\mathbf{x}_\star') = \mathbb{E}^{--}(\mathbf{x}\mathbf{x}_\star') \equiv \Sigma_\star \end{aligned}$$

then, as $N^+, N^- \to \infty$,

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

where

$$\begin{aligned} \pi^+ &= P(x_\star < 0 | x \geq 0) \\ \pi^- &= P(x_\star \geq 0 | x < 0). \end{aligned}$$

Proof. First, note that the above-the-cutoff estimator is now no longer consistent for β^+ plus a term due to omitted variables bias. Specifically,

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

As before, $\mathbb{E}^+$ denotes the expectation conditional on the $x \geq 0$, but note now that this condition does not imply that the true value is positive (i.e., $x_\star \geq 0$). When plugging in for y^+ we must now consider the case when the signs of x and the true value $x_\star$ differ. Let $\mathbb{E}^{++}$ denote the expectation conditional on both $x \geq 0$ and $x_\star \geq 0$, and let $\mathbb{E}^{+-}$ denote the expectation conditional on $x \geq 0$ and $x_\star < 0$. Using the law of total probability, we can rewrite the expression above as:

$$\begin{aligned}\widehat{\beta}^+ &\xrightarrow{p} \left[\mathbb{E}^+(\mathbf{x}\mathbf{x}')\right]^{-1}\left[\mathbb{E}^{++}(\mathbf{x}\mathbf{x}'_\star)(1-\pi^+)\beta^+ + \mathbb{E}^{+-}(\mathbf{x}\mathbf{x}'_\star)\pi^+\beta^-\right] + \left[\mathbb{E}^+(\mathbf{x}\mathbf{x}')\right]^{-1}\mathbb{E}^+(\mathbf{x}\varepsilon)\\ &= \Sigma^{-1}\Sigma_\star\left[(1-\pi^+)\beta^+ + \pi^+\beta^-\right] + \Omega\end{aligned}$$

Similarly, we can obtain:

$$\widehat{\beta}^- \xrightarrow{p} \Sigma^{-1}\Sigma_\star\left[(1-\pi^-)\beta^- + \pi^-\beta^+\right] + \Omega.$$

Subtracting the below-the-cutoff estimator from the above-the-cutoff estimator gives

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

□

Remark 3. *Measurement error biases the estimate both through attenuation bias and due to assigning observations to the "wrong" side of the cutoff. Attenuation bias arises through the $\Sigma^{-1}\Sigma_\star$ term. Assignment to the wrong side of the cutoff biases the estimate towards $-\tau^{PRK}$. That is, as $\pi^+, \pi^- \to 1$, the probability limit of $\widehat{\tau}^{PRK}$ becomes closer to $-\tau^{PRK}$. Intuitively, the above-the-cutoff estimator is a linear combination of the mis-measured slopes above and below the cutoff. When more weight is put on the "wrong" slope, our estimate of β_1^+ gets closer to β_1^-.*

With information on the nature of the measurement error, it is possible to implement an estimator that corrects for its bias. The following estimator corrects for bias due to measurement error.

Defintion 3. *The parametric RK estimator corrected for measurement error is*

$$\widehat{\tau}^{PRK-ME} = (1,0)\left[\widehat{\Sigma}^{-1}\widehat{\Sigma_\star}\right]^{-1}(1-\widehat{\pi^-}-\widehat{\pi^+})^{-1}(\widehat{\beta}^+ - \widehat{\beta}^-)$$

where $\widehat{\Sigma}$ is the sample estimate of $\mathbb{E}(\mathbf{x}\mathbf{x}')$, $\widehat{\Sigma_\star}$ is an estimate of $\mathbb{E}(\mathbf{x}\mathbf{x}')$, $\widehat{\pi^+}$ is an estimate of $P(x_\star < 0 | x \geq 0)$, and $\widehat{\pi^-}$ is an estimate of $P(x_\star \geq 0 | x < 0)$. Note that we pre-multiply by (1, 0) to select first row of the other term (which identifies $\beta_1^+ - \beta_1^-$).

Remark 4. *Under simplifying assumptions, we can obtain a simpler expression for the parametric RKD estimator corrected for measurement error. Similarly to Griliches and Ringstad (1970); Pei and Shen (2017), assume that the distribution of measurement error μ is symmetric and the distributions of $x_\star$ above and below the cutoff are symmetric. This implies $\mathbb{E}(\mu^3) = \mathbb{E}^+(x^3) =$*

$\mathbb{E}^-(x^3) = 0$. *Then, the probability limit of the parametric RKD estimator, as $N^+, N^- \to \infty$, is*

$$\widehat{\tau}^{PRK} \xrightarrow{p} \left(1 - \frac{\sigma_\mu^2}{\sigma_x^2}\right)(1 - \pi^- - \pi^+)\tau^{PRK}$$

where $\sigma_\mu^2 = \mathbb{E}(\mu^2)$ and $\sigma^2 = \mathbb{E}(x^2)$. The parametric RK estimator corrected for measurement error is now simply

$$\widehat{\tau}^{PRK-ME} = \left(1 - \frac{\widehat{\sigma_\mu^2}}{\widehat{\sigma_x^2}}\right)^{-1}(1 - \widehat{\pi^-} - \widehat{\pi^+})^{-1}\widehat{\tau}^{PRK}.$$

B.2 Measurement Error in an RDD

We can obtain analogous results for a linear RDD estimator. For completeness, the results for an RDD are presented below. One key difference arises for an RD versus an RK. The parametric RD estimator does not exhibit attenuation bias, but it is biased towards the negative of the true treatment effect as a result of measurement error assigning observations to the "wrong" side of the cutoff. A similar correction for measurement error can be implemented for an RD that requires less information on the nature of the measurement error that what is needed for an RK.

B.2.1 Setting and Identification of the Local Average Treatment Effect (RDD)

The outcome y is a linear function of the running variable x and a treatment indicator $T \in \{0, 1\}$ for $x \in [-h, h]$, where h is a given positive constant. Unobserved factors that also affect filing do so additively through ε. Specifically:

$$y = \beta_0 + \beta_1^x x + \beta_1^T T + \varepsilon.$$

Treatment occurs when the running variable crosses zero, that is,

$$T = \mathbf{1}[x \geq 0].$$

I assume that $\mathbb{E}(\varepsilon) = 0$, but allow for $\mathbb{E}(x\varepsilon) \neq 0$.

Remark 5. *The parameter β_1^T identifies the local average treatment effect (LATE):*

$$\mathbb{E}(y|T = 1, x = 0) - \mathbb{E}(y|T = 0, x = 0) = \beta_1^T.$$

Note that this is the same treatment effect identified in a standard sharp nonparametric RDD (e.g., as in Calonico et al., 2014). The key difference is that the parametric assumptions about how the outcome y depends on the running variable x and treatment T imply that the effect of interest equals a model parameter.

Next, we can write β_1^T as a function of more easily estimable parameters. To do so, first rewrite the outcome:

$$y = \begin{cases} \underbrace{\beta_0 + \beta_1^T}_{\equiv \beta_0^+} + \beta_1^x x + \varepsilon & x \geq 0 \\ \underbrace{\beta_0}_{\equiv \beta_0^-} + \beta_1^x x \varepsilon & x < 0 \end{cases}.$$

Defintion 4. *Define the parametric RD estimand as*

$$\tau^{PRD} = \beta_0^+ - \beta_0^-. \tag{17}$$

Remark 6. *The parametric RD estimand τ^{PRK} identifies the LATE:*

$$\tau^{PRD} = \beta_0^+ - \beta_0^- = \beta_1^T = \mathbb{E}(y|T = 1, x = 0) - \mathbb{E}(y|T = 0, x = 0).$$

B.2.2 RDD Estimation and Consistency: Without Measurement Error

Here I introduce a least-squares estimator for the LATE. I show that it is consistent for the parametric RD estimand in the absence of measurement error. The next part then characterizes this estimator's bias in the presence of measurement error in the running variable and presents a corrected estimator that eliminates this bias.

Notation: Let $\beta^+ = (\beta_0^+, \beta_1^x)'$ and $\beta^- = (\beta_0^-, \beta_1^x)'$. Let $\mathbf{X}^+$ denote the $(N^+ \times 2)$ matrix whose first column is a vector of ones and the second is the vector of x's such that $x \geq 0$. Let Y^+ denote the $(N^+ \times 1)$ of corresponding y values. Below I'll use lower case letters to denote individual observations, for example, $\mathbf{x} = (1, x)'$ for a particular x. Let $\mathbb{E}^+$ denote the expectation conditional on $x \geq 0$. We can similarly define $\mathbf{X}^-$, Y^-, N^-, and $\mathbb{E}^-$.

The parametric RD estimator is defined formally below.

Defintion 5. *Define the parametric RD estimator to be:*

$$\widehat{\tau}^{PRD} = \widehat{\beta}^+ - \widehat{\beta}^-$$

where

$$\widehat{\beta}^+ = \left(\mathbf{X}^{+\prime}\mathbf{X}^+\right)^{-1}\mathbf{X}^{+\prime}Y^+ \quad \textit{(above-the-cutoff estimator)}$$

$$\widehat{\beta}^{-} = \left(\mathbf{X}^{-\prime}\mathbf{X}^{-}\right)^{-1}\mathbf{X}^{-\prime}Y^{-} \quad \textit{(below-the-cutoff estimator).}$$

Proposition 3 (Consistency of the Parametric RD Estimator without Measurement Error)**.** *In the absence of measurement error, if the omitted variables bias is the same above and below the cutoff, that is:*

$$\left[\mathbb{E}^{+}\left(\mathbf{x}\mathbf{x}'\right)\right]^{-1}\mathbb{E}^{+}\left(\mathbf{x}\varepsilon\right) = \left[\mathbb{E}^{-}\left(\mathbf{x}\mathbf{x}'\right)\right]^{-1}\mathbb{E}^{-}\left(\mathbf{x}\varepsilon\right),$$

then the parametric RD estimator is consistent for the parametric RD estimand:

$$\widehat{\tau}^{PRD} \xrightarrow{p} \tau^{PRD} \quad \textit{as} \quad N^{+}, N^{-} \to \infty.$$

Proof.

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

Similarly, we have:

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

If $\left[\mathbb{E}^{+}\left(\mathbf{x}\mathbf{x}'\right)\right]^{-1}\mathbb{E}^{+}\left(\mathbf{x}\varepsilon\right) = \left[\mathbb{E}^{-}\left(\mathbf{x}\mathbf{x}'\right)\right]^{-1}\mathbb{E}^{-}\left(\mathbf{x}\varepsilon\right)$, then

$$\widehat{\tau}^{PRD} = \widehat{\beta}_0^{+} - \widehat{\beta}_0^{-} = \tau^{PRD}.$$

□

B.2.3 RDD Estimation and Consistency: With Measurement Error

Now suppose the true relationship is

$$y = \beta_0 + \beta_1^{x} x_\star + \beta_1^{T} T + \varepsilon$$

and

$$T_\star = \mathbf{1}[x_\star \geq 0].$$

Now, we only observe y and $x = x_\star + \mu$, where μ is zero mean noise (measurement error) and $\mathbb{E}(x_\star\mu) = \mathbb{E}(\varepsilon\mu) = 0$. That is, we have measurement error in the running variable. The following proposition characterizes the bias induced by measurement error in the parametric RD estimator. Let $\mathbf{x}_\star = (1, x_\star)'$. Without loss of generality, suppose we demean the mis-measured running variable x for both the above and below-the-cutoff estimators.

Proposition 4 (Bias in the Parametric RD Estimator with Measurement Error)**.** *If the omitted variables bias is the same above and below the cutoff, that is:*

$$\left[\mathbb{E}^{+}\left(\mathbf{x}\mathbf{x}'\right)\right]^{-1}\mathbb{E}^{+}\left(\mathbf{x}\varepsilon\right)=\left[\mathbb{E}^{-}\left(\mathbf{x}\mathbf{x}'\right)\right]^{-1}\mathbb{E}^{-}\left(\mathbf{x}\varepsilon\right)\equiv\Omega,$$

and the variance and covariance matrices are the same above and below the cutoff:

$$\begin{aligned}\mathbb{E}^{+}(\mathbf{x}\mathbf{x}')&=\mathbb{E}^{-}(\mathbf{x}\mathbf{x}')\equiv\Sigma\\ \mathbb{E}^{++}(\mathbf{x}\mathbf{x}'_{\star})&=\mathbb{E}^{+-}(\mathbf{x}\mathbf{x}'_{\star})=\mathbb{E}^{-+}(\mathbf{x}\mathbf{x}'_{\star})=\mathbb{E}^{--}(\mathbf{x}\mathbf{x}'_{\star})\equiv\Sigma_{\star}\end{aligned}$$

then, as $N^{+}, N^{-}\to\infty$,

$$\widehat{\tau}^{PRD}\xrightarrow{p}(1-\pi^{-}-\pi^{+})\tau^{PRD}$$

where

$$\begin{aligned}\pi^{+}&=P(x_{\star}<0|x\geq 0)\\ \pi^{-}&=P(x_{\star}\geq 0|x<0).\end{aligned}$$

Proof. First, note that the above-the-cutoff estimator is now no longer consistent for β^{+} plus a term due to omitted variables bias. Specifically,

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

As before, $\mathbb{E}^{+}$ denotes the expectation conditional on the $x\geq 0$, but note now that this condition does not imply that the true value is positive (i.e., $x_{\star}\geq 0$). When plugging in for y^{+} we must now consider the case when the signs of x and the true value $x_{\star}$ differ. Let $\mathbb{E}^{++}$ denote the expectation conditional on both $x\geq 0$ and $x_{\star}\geq 0$, and let $\mathbb{E}^{+-}$ denote the expectation conditional on $x\geq 0$ and $x_{\star}<0$. Using the law of total probability, we can rewrite the expression above as:

$$\begin{aligned}\widehat{\beta}^{+}&\xrightarrow{p}\left[\mathbb{E}^{+}(\mathbf{x}\mathbf{x}')\right]^{-1}\left[\mathbb{E}^{++}(\mathbf{x}\mathbf{x}'_{\star})(1-\pi^{+})\beta^{+}+\mathbb{E}^{+-}(\mathbf{x}\mathbf{x}'_{\star})\pi^{+}\beta^{-}\right]+\left[\mathbb{E}^{+}(\mathbf{x}\mathbf{x}')\right]^{-1}\mathbb{E}^{+}(\mathbf{x}\varepsilon)\\ &=\Sigma^{-1}\Sigma_{\star}\left[(1-\pi^{+})\beta^{+}+\pi^{+}\beta^{-}\right]+\Omega\end{aligned}$$

Similarly, we can obtain:

$$\widehat{\beta}^{-}\xrightarrow{p}\Sigma^{-1}\Sigma_{\star}\left[(1-\pi^{-})\beta^{-}+\pi^{-}\beta^{+}\right]+\Omega.$$

Subtracting the below-the-cutoff estimator from the above-the-cutoff estimator gives

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

The variance-covariance term on the intercept is simply equal to one. Therefore

$$\widehat{\beta}_0^{+} - \widehat{\beta}_0^{-} \xrightarrow{p} (1 - \pi^{+} - \pi^{-})(\beta_0^{+} - \beta_0^{-})$$

and

$$\widehat{\tau}^{PRD} \xrightarrow{p} (1 - \pi^{-} - \pi^{+})\tau^{PRD}.$$

$\square$

Remark 7. *In contrast to the parametric RK estimator, measurement error biases the parametric RD estimator through only one channel. While attenuation bias does not occur for the estimate of the intercept, bias still arises as a result of measurement error assigning observations to the wrong side of the cutoff. The bias* $[1 - \pi^{+} - \pi^{-}] \in [-1, 1]$ *could, at worst, cause the estimator to identify* $-\tau^{PRD}$ *(flipping the sign). The more observations are assigned to the wrong side, the more severe the measurement error. Intuitively, the estimate of the intercept in each region is a mixture of the true intercepts above and below the cutoff as a result of measurement error causing some observations to end up on the wrong side.*

With information on the nature of the measurement error, it is possible to implement an estimator that corrects for its bias. The following estimator corrects for bias due to measurement error.

Defintion 6. *The parametric RD estimator corrected for measurement error is*

$$\widehat{\tau}^{PRD-ME} = (1 - \widehat{\pi^{-}} - \widehat{\pi^{+}})^{-1}\widehat{\tau}^{PRD}$$

where $\widehat{\pi^{+}}$ *is an estimate of* $P(x_{\star} < 0 | x \geq 0)$*, and* $\widehat{\pi^{-}}$ *is an estimate of* $P(x_{\star} \geq 0 | x < 0)$*.*

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 data used in the analyses draw on both the original LLMA database and CoreLogic's "Supplemental Loan Analytics" module. Both are purchased from CoreLogic.

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. The starting point is the full sample spanning January 2000 to March 2016.

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, Delaware, Idaho, Louisiana, Maryland, Massachusetts, Minnesota, Missouri, Mississippi, North Dakota, Nebraska, Rhode Island, Vermont, Washington, and Wisconsin. All of these states also have non-zero homestead exemptions, which means there will be household positive home equity on both sides of the cutoff.

I use data from Wisconsin up to 2008 Q4 because of subsequent changes to its bankruptcy laws. In the following quarter, Wisconsin began allowing filers to use federal exemptions. And within a year, Wisconsin also started 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) because in prior years their homestead exemption limits were \$0. This meant 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. I drop household with negative home equity because the fraction of negative equity households appears to drop discontinuously at the cutoff; no negative equity household could have

Table C.1: Number of Observations After Applying Filters

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

Note. This table summarizes the cumulative effect of data filters used to obtain the sample for analysis on sample size. The first column gives the total number of observations, the second the number of unique households (i.e., mortgages), the third the total number of bankruptcies, and the fourth the average number of bankruptcies per year.

positive seizable equity. 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.2 Measuring Home Equity

The LLMA data report the mortgage balance and loan-to-value (LTV) ratio at origination. Using these variables I calculate the sale price. 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.

C.3 Homestead Exemption Data

To identify the relevant statutes specifying homestead exemptions, I used Elias (2011). From the statutes, I collected information on homestead exemption levels, 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 and 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 states with more generous homestead exemptions tend to have more exemptions in general. So a more generous state homestead exemption would likely be a good indicator that state exemptions would be preferable. Additionally, Auclert et al. (2019) finds that most of the variation in households' potential debt relief is due to the homestead exemption.

The homestead exemption data I assembled is available for download at https://sashaindarte.github.io/public_goods/.

C.4 Other Data

County-Level Economic Data

I obtain county-level **unemployment** rate data and median **income** data using GeoFRED. The unemployment data come 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 returns 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%.

Zillow's ZHVI Data

I obtain monthly ZIP-level **house price** data from Zillow. When controlling for house prices in estimations, I use the Zillow Home Value Index (ZHVI) for all homes. To impute home values, I use Zillow's ZHVIs for one, two, three, four, and five or more bedroom homes. For each post-sale time period, I multiply the initial sale price by the growth rate of the corresponding ZHVI over the same period, using the ZHVI that matches that particular home's bedroom count.

CPI Data

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

D Empirical Analysis: Robustness and Additional Results

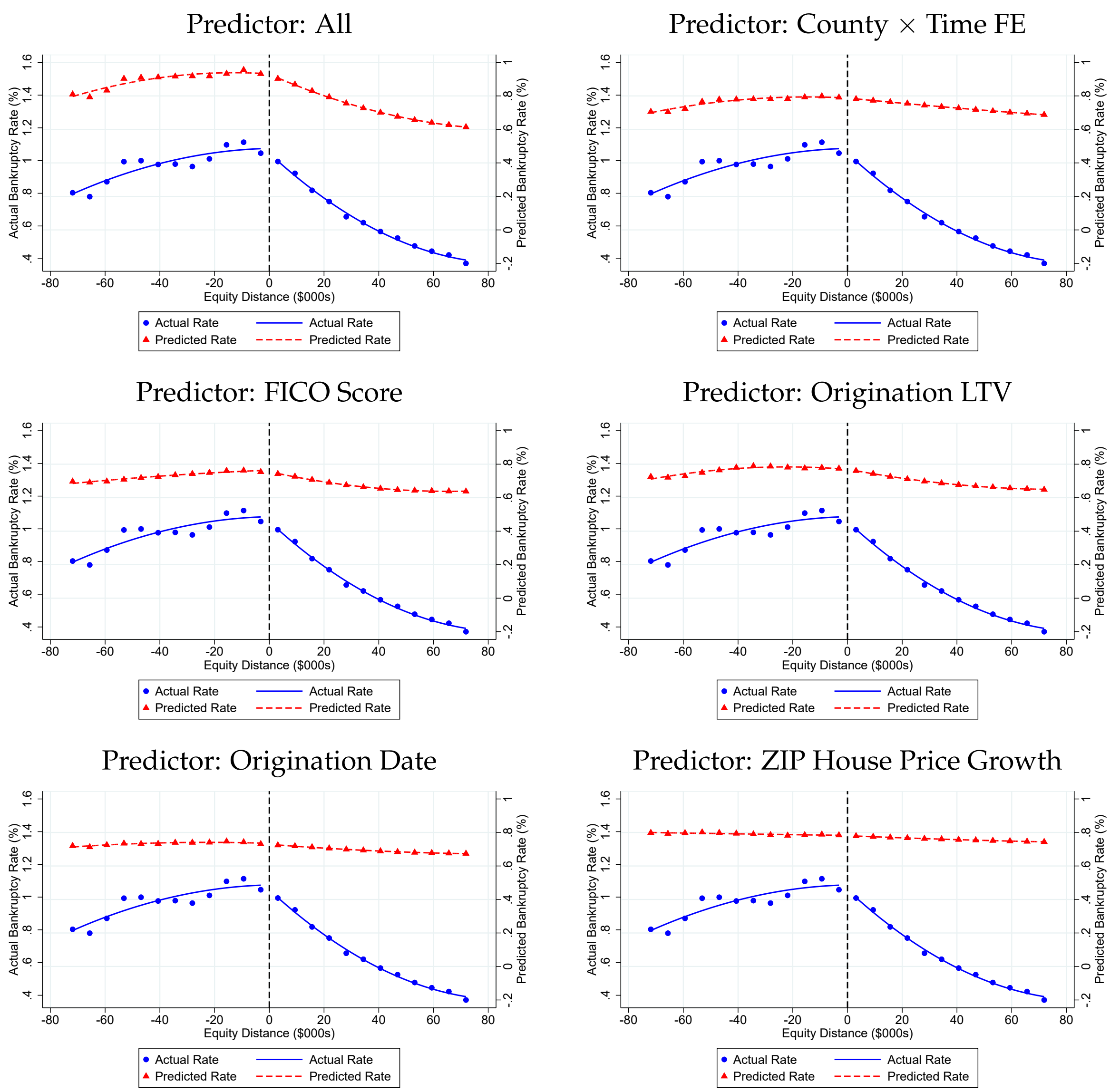
D.1 RKD: Testing for Failure of Smooth Density Assumption

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)	FIPS $\times$ Time FE
	Panel A: Level Change Estimation Results (RDD)					
Estimate	-0.16	-0.02	-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	4.44	5.91
Obs.	2,712,805	6,538,311	3,734,972	4,589,884	2,704,073	5,555,473
	Panel B: Slope Change Estimation Results (RKD)					
Estimate	-0.04	0.05	0.02	-5e-3	0.02	0.16***
Std. Err.	(0.04)	(0.04)	(0.03)	(0.04)	(0.01)	(0.02)
Bandwidth	25.23	16.01	8.46	10.92	14.06	21.63
Obs.	12,538,659	6,538,311	7,900,691	10,121,944	8,406,731	19,081,604

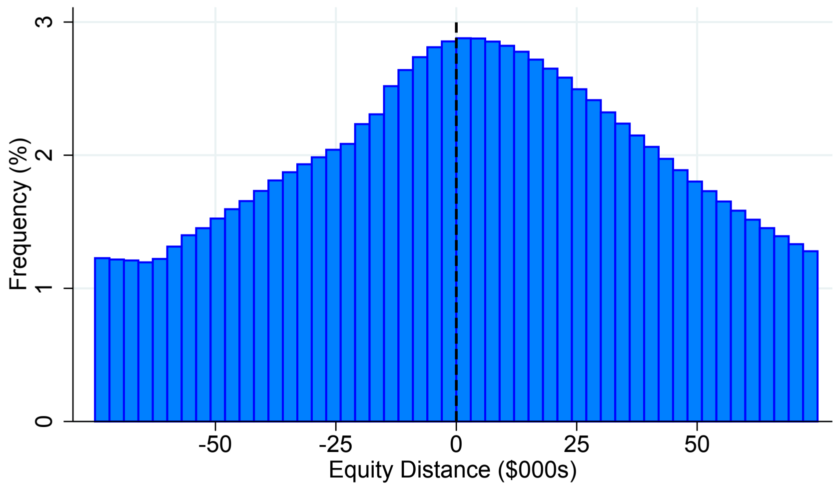
Note. This table gives results from tests for jumps and kinks in predetermined covariates. The column indicates the covariate or group of covariates. For each test column, I compute a predicted bankruptcy rate for each household using a linear probability model, where the predictors are the covariates 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). 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. Statistical significance: 0.05*, 0.01**, and 0.01***.

Figure D.1: Actual vs. Predicted Bankruptcy Filing Rates



Note. 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(s). 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 Appendix Table D.1.

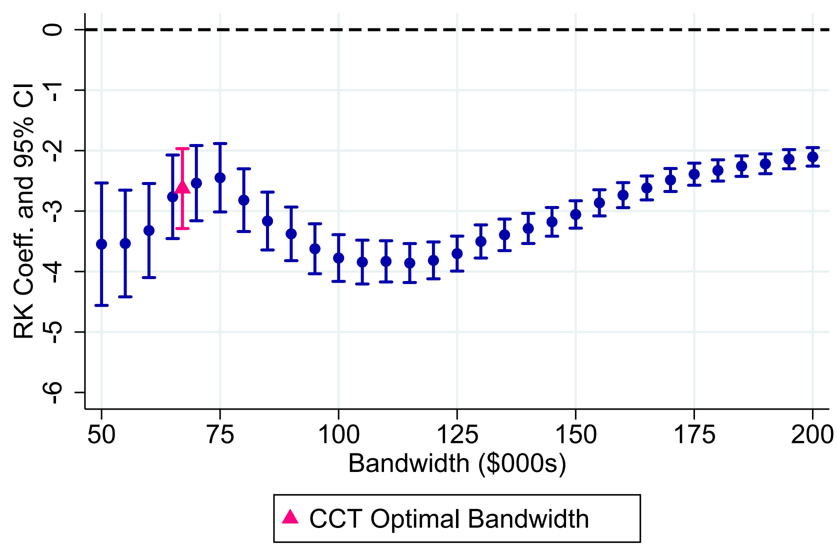
Figure D.2: Empirical Distribution of Equity Distance



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

D.2 RKD: Alternative Bandwidths

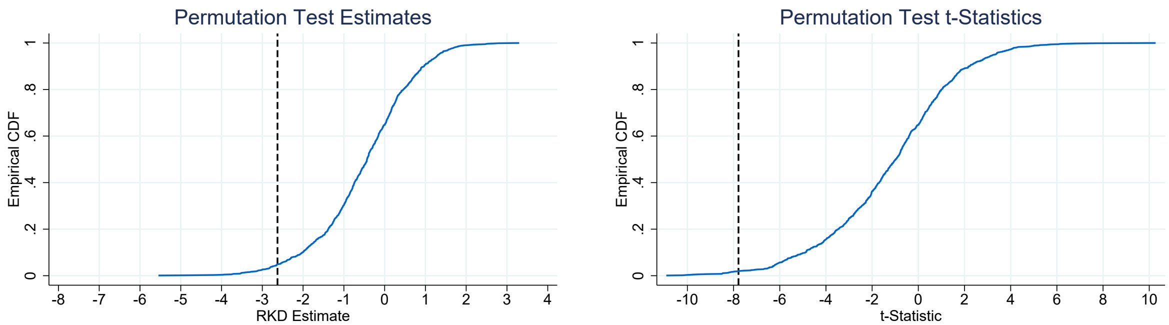
Figure D.3: RKD Estimate under Alternative Bandwidths



Note. This graph plots RKD estimates obtained under various bandwidth choices and their 95% confidence intervals. Each estimate corrects for measurement error using the procedure described in section 4.2. RKD Confidence intervals are computed as in Calonico et al. (2014).

D.3 RKD: Permutation Test

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



Note. 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.4 RKD: Heterogeneity

How does the filing response vary with local economic and borrower characteristics? Households with an above-median loan-to-value (LTV) at origination are 71% more sensitive to bankruptcy's generosity. Those with below-median FICO scores at origination are at least three times more sensitive to a given change in relief generosity. Both factors are associated with financial distress and limited credit access. At the county level, median income and the unemployment rate are not associated with different sensitivity. But at the ZIP level, households in ZIP codes with below-median income respond on average 85% more to a given change in bankruptcy's generosity. Borrowers in ZIP codes with *below*-median unemployment insurance (UI) claim rates are at least three times more responsive.[1]

The result for UI may seem at odds with the others if we interpret the UI claim rate as a proxy for unemployment. However, UI claim rates may be positively associated with overall UI generosity (in terms of eligibility, duration, and benefit levels). When households are better insured, the consumption-smoothing benefits of bankruptcy diminish, and a given change in generosity can be a weaker incentive to file. In fact, bankruptcy appears to be a substitute with other forms of insurance like health insurance (Mahoney, 2015), and filing rates are lower where households have more access to public health in-

[1] The filing responses of above-median FICO and UI claim rate households are not statistically significant. The reported ratios come from comparing the 95% confidence intervals of the estimates.

surance (Gross and Notowidigdo, 2011). Additionally, mortgage delinquency tends to fall when UI becomes more generous (Hsu et al., 2018).

Table D.2: 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**
RKD Est.	-1.97***	-4.31***	-2.11***	-2.51***	-11.82***	-2.30***
Std. Err.	(0.45)	(0.73)	(0.44)	(0.60)	(2.48)	(0.33)
Bandwidth	86.92	66.54	76.79	72.97	56.40	70.05
LHS Obs.	3,581,783	4,902,345	8,883,014	4,867,345	627,684	16,522,282
RHS Obs.	5,055,409	4,240,244	9,771,956	6,275,235	1,007,332	17,981,518

Note. The estimates correspond to the percent change in filing rate, relative to the sample average of 0.71, in response to a $1,000 increase in seizable home equity. Each column is the 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: 0.05*, 0.01**, and 0.01***.

Table D.3: Heterogeneity in Filing Sensitivity (RKD Sample Splits)

	(1) Low	(2) High	(3) Low	(4) High
	Income (County)		Income (ZIP)	
RK est.	-2.53***	-2.45***	-4.42***	-2.38***
	(0.58)	(0.35)	(0.52)	(0.40)
Bandwidth	69.74	71.25	61.01	76.33
Obs.	11,546,502	36,091,315	16,622,096	20,245,302
	Unemp. Rate (County)		UI claims (ZIP)	
RK est.	-2.89***	-2.17***	-5.29***	-0.71
	(0.35)	(0.43)	(0.57)	(0.37)
Bandwidth	68.16	75.66	58.73	95.39
Obs.	24,996,058	22,697,022	13,871,261	16,345,721
	Orig. LTV		Orig. FICO	
RK est.	-2.32***	-3.97***	-4.74***	-0.57
	(0.37)	(0.42)	(0.56)	(0.29)
Bandwidth	77.07	64.06	63.43	62.38
Obs.	23,028,090	24,739,988	19,577,574	18,160,114
	Bankruptcy Rate (County)		Predicted P(file)	
RK est.	-1.86***	-2.81***	-0.69*	-3.37***
	(0.35)	(0.43)	(0.28)	(0.70)
Bandwidth	75.75	61.46	75.33	63.20
Obs.	13,308,153	31,932,873	12,855,683	11,807,497

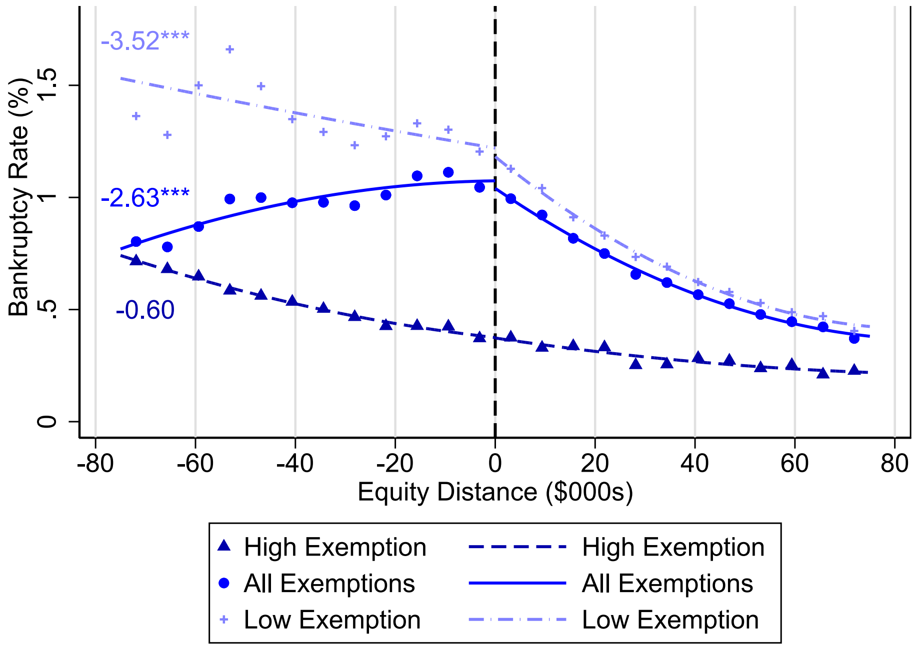
Note. The estimates correspond to the percent change in filing rate, relative to the sample average of 0.71, in response to a $1,000 increase in seizable home equity. Each estimates comes from an RKD estimated on a subset of the main sample. Columns 1 and 2 (and 3 and 4) partition the sample into groups 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: 0.05*, 0.01**, and 0.01***.

Table D.4: Heterogeneity in Filing Sensitivity (OLS-Estimated Interactions)

	(1)	(2)	(3)	(4)
$\widehat{\tau}$	-2.73***	-1.95***	-1.09**	-1.08***
	(0.25)	(0.34)	(0.38)	(0.33)
Unemp. Rate (%, FIPS) $\times\widehat{\tau}$		-0.35***		
		(0.04)		
UI Claims (%, ZIP) $\times\widehat{\tau}$			0.02	
			(0.04)	
ln(income) (FIPS) $\times\widehat{\tau}$		-0.11**		
		(0.04)		
ln(income) (ZIP)$\times\widehat{\tau}$			0.15***	
			(0.04)	
Δ ln(HP) $\times\widehat{\tau}$		0.20***	0.45***	
		(0.04)	(0.04)	
FICO $\times\widehat{\tau}$		1.50***	1.39***	
		(0.05)	(0.05)	
LTV $\times\widehat{\tau}$		-0.53***	-0.38***	
		(0.04)	(0.04)	
$\widehat{\text{P(file)}} \times \widehat{\tau}$				-1.44***
				(0.04)
Observations	46,023,153	27,443,032	19,713,992	24,087,047

Note. The estimates correspond to the percent change in filing rate, relative to the sample average of 0.71, 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, are estimated within the same bandwidth as the preferred specification ($67,070), and correct for measurement error in the running variable. 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 Claim % 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 quarterly on a year-over-year basis (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 $\widehat{\text{P(file)}}$ variable is a predicted filing rate based on the county and loan-level data. Interacted covariates are demeaned and divided by their standard deviation prior to the regression. Statistical significance: 0.05*, 0.01**, and 0.01***.

Figure D.5: RKD Estimate in High and Low Exemption States



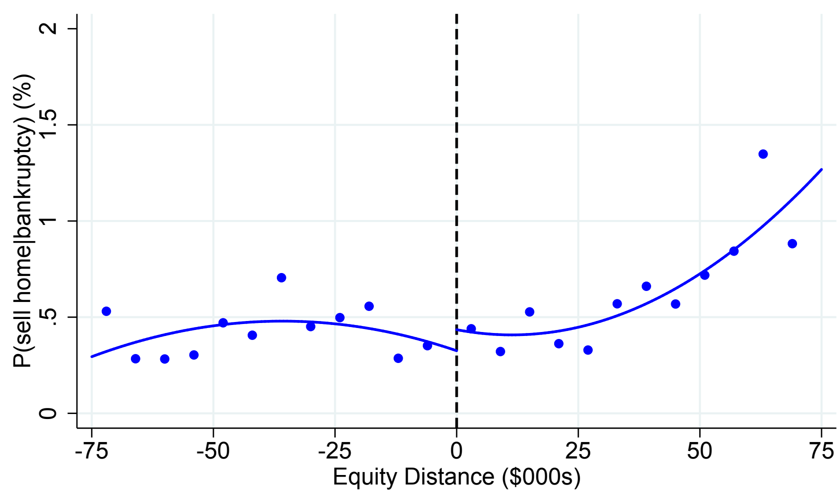
Note. This graph compares the relationship between filing and equity distance in high and low exemption states. I classify observations as belonging to a high exemption state if the homestead exemption they currently face is above the sample median (in terms of real 2010 dollars). The points denote average (annualized) filing rates within equity distance bins. The lines are generated by fitting a quadratic polynomial to the individual observations on each side of the kink. For comparison, I also reproduce the graphs for the full sample (the solid line and circular points), corresponding to Figure 2. The numbers correspond to the RKD estimates associated with each population. The RKD is estimated using the same specification as column 2 of Table 1. For specification details, see that table's note.

D.5 RKD: Smoothness in the Probability of Sale

A potential identification concern is that the probability that a household would have to sell their home in bankruptcy is a kinked function of equity distance—kinking at the exemption limit. This means the kink in the filing rate could be due to not only the kink in seizable home equity, but also a kink in costs associated with selling the home. The RKD estimate would then overstate the direct effect of seizable home equity on filing, conflating it with an indirect effect through the cost of selling a home.

To assess this threat to identification, I estimate the causal effect of seizable equity on the probability of selling their home for bankruptcy filers. Visually the relationship appears to be smooth (see Figure D.6 below). Table D.5 reports results testing for a kink using an RKD, which yield a small and statistically insignificant effect of seizable equity on the probability of sale. The magnitude of the point estimate corresponds to a 0.03% increase in the probability of sale for a $1,000 increase in seizable home equity.

Figure D.6: Probability of Sale versus Equity Distance



Note. This graph plots binned averages of the probability of a home sale for various levels of equity distance. The sample consists of households within 1.5 years of having filed for bankruptcy. The lines are quadratic polynomials fitted to the full subsample.

D.6 RKD: Smoothness in Credit Use

The analysis below addresses the possibility that credit supply is a kinked or discontinuous function of equity distance. Because the LLMA data do not contain additional measures of credit use outside of mortgages, here I instead use data from the public Survey of Income and Program Participation (SIPP) over 2013 to 2019.

Table D.5: Testing for a Kink in the Probability of Sale Conditional on Filing

Estimate	0.03
Std. Err.	(0.05)
Bandwidth	64.06
LHS Obs.	41,197
RHS Obs.	31,475

Note. Estimation uses a uniform kernel, quadratic polynomial in equity distance, and linearly controls for home equity. I correct for approximation bias and construct confidence interval construction as in Calonico et al. (2014). I scale the coefficient and standard errors to be in terms of percentage points. Statistical significance: 0.05*, 0.01**, and 0.01***.

The SIPP is a monthly person-level panel. I first collapse the data to the household level. Because debt-related variables are only measured at an annual frequency, I effectively have an annual household-level panel with 18,691 observations. To measure home equity, I use the household's self-reported measure of home equity.

Appendix Table D.6 tests for both a kink and a discontinuity for credit card, auto, and student loans. Figure D.7 plots the SIPP debt measures against equity distance. I examine both indicators for whether the household has each kind of debt and measures of the amount of each kind. The specifications mirror those used for the main RKD results in Table 1. Generally, I find small and statistically insignificant point estimates. The only statistically significant variable is an indicator for the presence of auto loan debt in the RKD. The point estimate implies a $1,000 increase in the cost of bankruptcy *reduces* the likelihood of having auto debt by 1% (a 0.40 percentage point decrease). This estimate is economically small and is the opposite sign of what we would expect if credit supply increased with a kink as nonexempt equity rose. And note that the estimate for the amount of auto debt implies a statistically insignificant 0.04% ($2.78) increase per $1,000 in nonexempt equity.

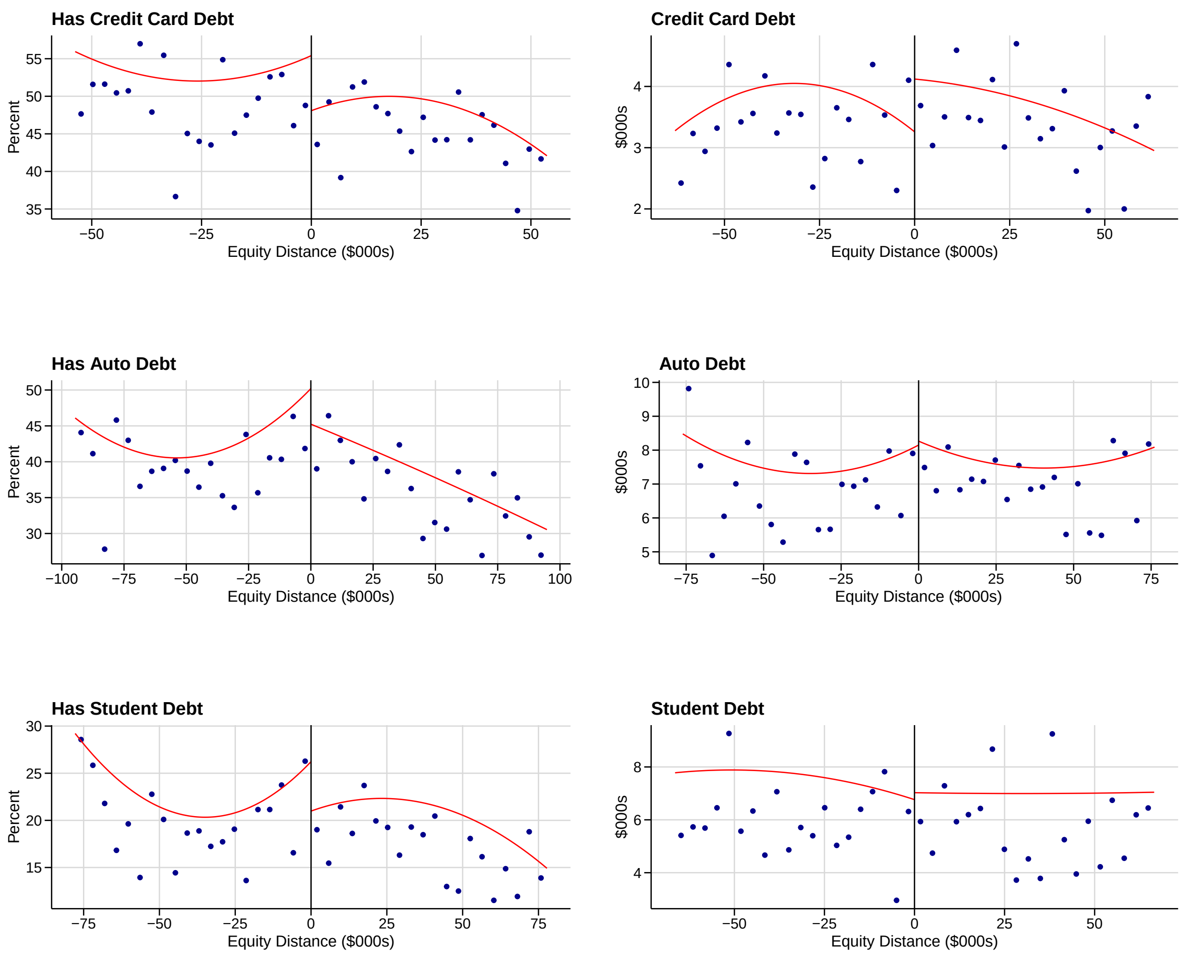
Overall, the results suggest credit supply kinking or jumping at the homestead exemption is unlikely to bias the main RKD estimate. One reason this is likely the case is that auto and student loans are accessed at a relatively low frequency. After origination, these balances mainly evolve based on households' speed of paying them down. And although credit card lenders can change credit limits over time, it would be challenging to index them to a borrower's equity distance. Credit card underwriting is highly automated and relies heavily on credit scores and other credit bureau data. Credit reports do not contain home values or other variables that make it possible to infer an individual's home equity (e.g., it does not contain the origination LTV).

Table D.6: Testing for Kinks and Discontinuities in Credit Use

	1[Has Debt]		Amount	
	RKD	RDD	RKD	RDD
Panel A: Credit Card	(1)	(2)	(3)	(4)
RK est. $\left(\widehat{\frac{\partial Y}{\partial s}}/Y\right)$	-0.22	-5.80	-0.47	14.02
Std. err.	(0.34)	(6.87)	(0.77)	(15.38)
Bandwidth	51.35	53.81	50.34	63.03
LHS Effective Obs.	3,241	3,369	3,168	3,807
RHS Effective Obs.	3,398	3,503	3,336	3,955
Panel B: Auto Loans				
RK est. $\left(\widehat{\frac{\partial Y}{\partial s}}/Y\right)$	-1.05***	2.19	0.04	9.49
Std. err.	(0.26)	(6.85)	(0.42)	(11.45)
Bandwidth	60.42	94.79	66.87	76.35
LHS Effective Obs.	3,672	4,851	4,021	4,282
RHS Effective Obs.	3,863	5,295	4,178	4,568
Panel C: Student Loans				
RK est. $\left(\widehat{\frac{\partial Y}{\partial s}}/Y\right)$	-0.36	15.40	0.79	13.32
Std. err.	(0.47)	(13.44)	(1.18)	(28.04)
Bandwidth	71.44	77.82	63.97	66.60
LHS Effective Obs.	4,176	4,326	3,846	3,985
RHS Effective Obs.	4,412	4,630	3,998	4,134

Note. Columns 1 and 2 report RKD and RDD estimates where the outcome variable is a binary indicator for the presence of given kind of debt (credit card, auto, or student loans). Columns 3 and 4 report RKD and RDD results for the amount of each kind of debt. 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). I also restrict the sample to households with positive home equity as in the main RKD analysis. I use the household-specific weights in the SIPP to yield population-representative estimates. In the notation above, Y denotes the outcome variable. Note that the outcome is divided by its mean, and it is multiplied by 100 (so that a coefficient of one corresponds to a 1% increase relative to the mean). Statistical significance: 0.05*, 0.01**, and 0.01***.

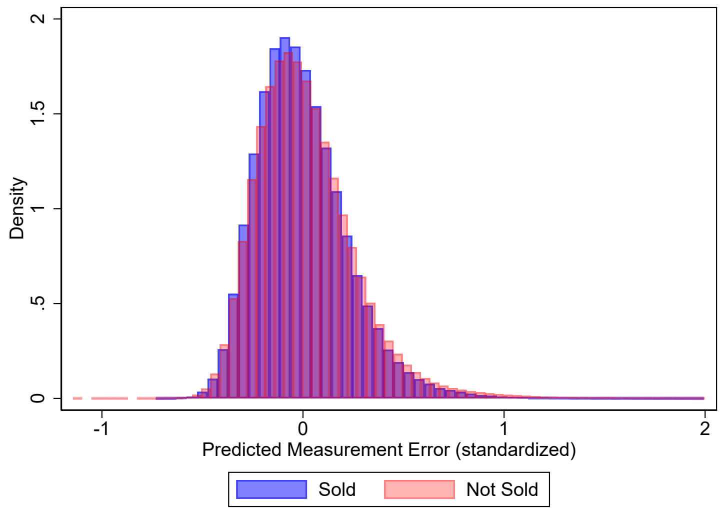
Figure D.7: Credit Use versus Equity Distance



Note. The figures plot indicators for whether a household has each kind of debt (left) and how much (right) against their equity distance. Credit amounts are given in thousands of 2010 dollars. I restrict the sample to households with positive home equity as in the main RKD analysis. The data used here are from the SIPP, spanning 2013 to 2019. Results from formally testing for jumps and kinks are reported in Appendix Table D.6.

D.7 RKD: Predicted Measurement Error

Figure D.8: Distribution of Predicted Measurement Error by Sale Status



Note. Predicted measurement error comes from fitting a linear model to a set of mortgage, household, and local economic characteristics. I regress measurement error on annual ZIP-level house price growth, the county-level bankruptcy rate, the county-level unemployment rate, home equity (including its square and cube), current mortgage balance, the origination balance, the FICO score at origination, time since bought, and equity distance. Estimation uses the subsample of sold homes. I then generate a predicted measurement error for the full RKD sample. Prior to the regression I "standardize" measurement error by demeaning it and dividing it by its standard deviation.

D.8 ARM IV: Placebo Test

Table D.7: Placebo Test—Does Filing Vary with Index Rate Choice Prior to Reset?

	(1)	(2)	(3)	(4)
Libor_{ic}	3.93	0.25	-0.53	3.70
	(10.93)	(10.67)	(10.81)	(11.04)
Margin_{ic}	-8.06	-8.78	-11.48	-2.51
	(11.33)	(11.37)	(11.58)	(10.72)
Old Pay_{ic}	-2.13**	-1.95**	-1.94**	-1.91*
	(0.75)	(0.74)	(0.74)	(0.76)
Orig. FICO_{ic}	-0.78***	-0.78***	-0.77***	-0.80***
	(0.10)	(0.10)	(0.10)	(0.10)
Orig. LTV_{ic}	0.81**	0.86**	0.92***	0.96***
	(0.26)	(0.26)	(0.27)	(0.27)
$\ln(\text{Home Eq.})_{ict}$	-1.07	-0.65	-0.75	-0.56
	(0.63)	(0.62)	(0.63)	(0.74)
$\ln(\text{Bal.})_{ict}$	15.68	9.99	10.03	8.01
	(16.24)	(16.29)	(16.31)	(16.90)
Observations	1,094,998	1,094,998	1,094,998	1,094,998
Time FE	✓	✓	✓	✓
County FE	✓	✓	✓	✓
Loan Age FE		✓	✓	✓
Loan Age × Time FE			✓	✓
County × Time FE				✓

Note. 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 2). 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**, and 0.01***.

D.9 ARM IV: Testing for Predictors of Libor Indexation

Table D.8: Testing for Predictors of Libor Indexation

	(1)	(2)	(3)	(4)
Margin_{ic}	-35.78***	-36.43***	-39.30***	-43.49***
	(1.72)	(1.76)	(2.04)	(2.50)
Old Pay_{ic}	0.04	0.04	0.08	0.07
	(0.05)	(0.05)	(0.05)	(0.06)
$\text{Origination FICO}_{ic}$	0.001	-0.01**	-0.01**	-0.01
	(0.004)	(0.004)	(0.004)	(0.01)
$\text{Origination LTV}_{ic}$	0.27***	0.22***	0.20***	0.21***
	(0.02)	(0.02)	(0.02)	(0.02)
$\ln(\text{Origination Bal.})_{ic}$	2.91*	0.56	0.25	0.02
	(1.37)	(1.26)	(1.25)	(1.54)
UR_{ic} %	-0.42	-0.45	-0.59	
	(0.43)	(0.37)	(0.39)	
$\ln(\text{Median Inc.})_{ic}$	-4.79	-1.85	-1.72	
	(10.27)	(8.87)	(8.34)	
$\Delta \ln(\text{House Prices})_{ic}$	-0.0001	0.003	0.003	0.002
	(0.004)	(0.004)	(0.004)	(0.004)
Observations	61,482	61,482	61,482	61,482
Time FE	✓	✓	✓	✓
County FE	✓	✓	✓	✓
Loan Age FE		✓	✓	✓
Loan Age × Time FE			✓	✓
County × Time FE				✓

Note. 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 4 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 coefficient on logged origination balances implies that a 1 log point increase in origination balances predicts a 0.02% increase in the likelihood of being indexed to Libor. House price growth is measured over the past year at the ZIP code level. Statistical significance: 0.05*, 0.01**, and 0.01***.

D.10 ARM IV: Testing for Anticipatory Behavior

Table D.9: Anticipatory Behavior—Does Filing Vary with Index Rate Prior to Reset?

	(1)	(2)	(3)	(4)
IndexRate_{ic}	-9.04	-12.72	-12.5	-6.61
	-8.11	-7.94	-8.07	-8.15
$\text{IndexRate}_{ic} \times 2007_{ic}$	-43.3	-37.75	-30.06	-28.07
	-23.16	-23.1	-19.5	-27.11
Margin_{ic}	(13.12)	(13.65)	(16.06)	(6.60)
	(11.42)	(11.48)	(11.62)	(10.91)
Old Pay_{ic}	-2.13**	-1.95**	-1.94**	-1.90*
	(0.75)	(0.75)	(0.74)	(0.76)
Orig. FICO_{ic}	-0.78***	-0.78***	-0.77***	-0.80***
	(0.10)	(0.10)	(0.10)	(0.10)
Orig. LTV_{ic}	0.83**	0.88***	0.94***	0.97***
	(0.26)	(0.26)	(0.27)	(0.27)
$\ln(\text{Home Eq.})_{ict}$	(1.16)	(0.70)	(0.79)	(0.61)
	(0.63)	(0.61)	(0.63)	(0.74)
$\ln(\text{Bal.})_{ict}$	16.27	10.15	10.23	8.26
	(16.28)	(16.31)	(16.34)	(16.92)
Obs.	1,094,998	1,094,998	1,094,998	1,094,998
Time FE	✓	✓	✓	✓
County FE	✓	✓	✓	✓
Loan Age FE		✓	✓	✓
Loan Age × Time FE			✓	✓
County × Time FE				✓

Note. 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 2). 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 (at which point the Libor-Treasury spread had not yet widened). Households in 2007 may have been more surprised by their actual payment reduction. The point estimate is larger in magnitude for 2007, but remains both negative and statistically insignificant. Statistical significance: 0.05*, 0.01**, and 0.01***.

D.11 ARM IV: OLS Version

Table D.10: OLS Estimation

	(1)	(2)	(3)	(4)
MPay_{ic}	3.03	2.49	2.46	2.35
	(1.87)	(1.91)	(1.95)	(2.17)
Margin_{ic}	13.40	11.63	6.06	4.90
	(10.90)	(11.06)	(11.66)	(13.04)
Old Pay_{ic}	-1.81***	-1.55**	-1.64**	-1.56**
	(0.51)	(0.51)	(0.52)	(0.54)
Orig. FICO_{ic}	-0.78***	-0.77***	-0.77***	-0.79***
	(0.08)	(0.08)	(0.08)	(0.08)
Orig. LTV_{ic}	0.62**	0.64**	0.60*	0.75**
	(0.23)	(0.23)	(0.24)	(0.26)
$\ln(\text{Home Eq.})_{ict}$	-1.75**	-1.55**	-1.64**	-1.40*
	(0.54)	(0.54)	(0.56)	(0.60)
$\ln(\text{Bal.})_{ict}$	6.03	2.45	4.25	1.34
	(11.57)	(11.70)	(11.62)	(12.37)
Observations	1,092,072	1,092,072	1,092,072	1,092,072
Time FE	✓	✓	✓	✓
County FE	✓	✓	✓	✓
Loan Age FE		✓	✓	✓
Loan Age × Time FE			✓	✓
County × Time FE				✓

Note. This table estimates the second stage equation using OLS. The sample is the same as the one used in the main results (Table 2). Standard errors are clustered by county. I scale the coefficients so that they correspond to the change in the filing rate, relative to the average rate in terms of percentage points, in response to a $1,000 annual change in mortgage payments. Statistical significance: 0.05*, 0.01**, and 0.01***.

D.12 RKD and ARM IV Estimates on Overlapping Subsample

Table D.11: RKD and ARM IV Estimates on Overlapping Subsample

	RKD Estimate	ARM IV Estimate	
		Baseline	NPV-Adjusted
Estimate	-11.28	92.42	14.86
Std. Err.	(8.37)	(62.46)	(10.04)
Bandwidth	89.85	–	–
LHS Obs.	10,744	–	–
RHS Obs.	8,482	–	–
IndexRate_{ic}	–	956.01	5,946.38
Std. Err.	–	(148.82)	(925.66)
Stage 1 F-Stat.	–	41.27	41.27
Observations	–	86,256	86,256

Note. The RKD estimate uses the preferred specification, which uses a quadratic estimator and linearly controls for home equity. The ARM IV estimates are from the preferred specification for that analysis, which includes time, county, and loan age fixed effects as well as loan age and county-specific time trends. The ARM IV estimates here also control for the same controls as in Appendix Table A.2. Standard errors for the RKD estimates are calculated using the method of Calonico et al. (2014), for the ARM IV estimates they are clustered by county. Coefficients are scaled so that their units correspond to the percentage change in the annual filing rate given a $1,000 change in relief generosity (the RKD) or mortgage payments (the IV). Statistical significance: 0.05*, 0.01**, and 0.01***.

E Bankruptcy Model: Extensions and Additional Results

E.1 Extension: 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

$$\begin{aligned} V_T^N(y_T, d_T) &= u(c_T^N) \\ V_T^B &= u(c_T^B) - \sigma. \end{aligned}$$

The first-order condition 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 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:

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

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

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

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

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

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 filing 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. Note that $c_t^B = c^B = a + e$ when $a_t = a$ and $e_t = e$.

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 ($t = 1$) if filing in the present reduces their probability of filing 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

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

E.2 Extension: 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:

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

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.[2]

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

[2]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).

conditional on not currently being in autarky.[3] 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 E.4 still applies for a seizable cash-flow shock that is always available outside of bankruptcy (including autarky).

E.3 Extension: Delinquency

This section extends the baseline dynamic model from section E.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

$$
\begin{aligned}
c_t^R &= \begin{cases} y_t + a_t - R_t(d_t) + d_{t+1} & : t < T \\ y_t + a_t - R_t(d_t) & : t = T \end{cases} \\
c_t^D &= (1-\gamma) y_t + a_t, \quad \forall t \\
c_t^B &= a_t + e_t, \quad \forall t.
\end{aligned}
$$

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

$$
\begin{aligned}
V_t^R(y_t, d_t) &= \max_{d_{t+1}} \quad u(c_t^R) + \mathbb{E}^R \left[V_{t+1}^R(y_{t+1}, d_{t+1}) \right] + \mathbb{E}^D \left[V_{t+1}^D(y_{t+1}, d_{t+1}) | \right] + \mathbb{E}^B \left[V_{t+1}^B \right] \\
V_t^D(y_t, d_t) &= u(c_t^D) + \mathbb{E}^R \left\{ V_{t+1}^R[y_{t+1}, (d_t - \gamma y_t) R_t(d_t)] \right\} + \mathbb{E}^D \left\{ V_{t+1}^D[y_{t+1}, (d_t - \gamma y_t) R_t(d_t)] \right\} \\
&\quad + \mathbb{E}^B \left[V_{t+1}^B \right] \\
V_t^B &= u(c_t^B) - \sigma + \mathbb{E}^R \left[V_{t+1}^R(y_{t+1}, 0) \right].
\end{aligned}
$$

[3] 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 liquidity and moral hazard effects, as this fraction would simply cancel out.

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 household 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 moral hazard or liquidity effect 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 utility. But, in this second case, the "non-bankrupt" state is delinquency. When the liquidity effect is much stronger than the moral hazard effect, the mains results persistent: consumption is much higher in bankruptcy than out of bankruptcy and 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 in 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 stronger liquidity effect 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, some households may be in case one and others in case two. The stronger liquidity effect still implies on average consumption is higher in bankruptcy than out. But this isn't necessarily 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.

E.4 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. Note that w can also represent changes in required payments on dischargeable debt. An increase in dischargeable debt corresponds to a decrease in seizable cash flows. Higher payments decrease resources available outside of bankruptcy while also increasing the payoff from filing.

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 moral hazard and liquidity effects. 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.

We can decompose the filing response to w_1 into moral hazard and liquidity 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 and liquidity 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 5.3 describes scenarios where the payment reduction is potentially seizable. If the payment reduction is seizable with some probability $q \in [0,1]$, then the ARM IV estimate 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

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

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 estimates of the moral hazard and liquidity effects (-2.63 and 12.59, respectively), the lower bound above implies that the response to a $1,000 increase in non-seizable cash flows is at least a 9.96% decrease in the filing rate (12.59-2.63). This is approximately a 0.07 percentage point decrease in the annual filing rate of 0.71%, which is four times the estimated effect of an equivalent change in seizable equity. Therefore, even under the most extreme case in which mortgage payments are always seizable, the estimates still imply that the liquidity effect is much larger than the moral hazard effect.

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 liquidity effect.

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 liquidity effect, 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 liquidity effect is smallest for $q^N = 1$ and $q^B = 0$. That is, the payment reduction is only and always received outside of bankruptcy. As before, this means the liquidity effect is at least the sum of the IV and RKD estimates:

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

E.5 Implications of Heterogeneity in Filing Sensitivity

In the heterogeneity analysis of the RKD, 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 a more sensitive threshold (higher $\frac{\partial p_1}{\partial e}$). Both channels are plausible.

Financially distressed households may have more mass at the filing threshold if they have a different distribution of income shocks. If the threshold is relatively low, households with a greater probability of low income realizations can be more likely to end up at the threshold. Intuitively, households more likely to experience negative shocks will tend to be closer to the bankruptcy threshold and therefore more are 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 despite lower income due to 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.

E.6 Comparison with Chetty (2008) and Dávila (2020)

The results above make two theoretical innovations relative to prior work on sufficient statistics and insurance. First, I show that the ratio of liquidity and moral hazard effects identify a different object when studying a binary decision governed by a threshold rule (such as default), in contrast to continuous decisions like job search intensity as in Chetty (2008). Second, I show that we can use this ratio to learn about the costs and benefits of this decision for the marginal agent.

When the outcome of interest is binary and follows a *threshold* rule, as is standard in models of default, the equilibrium response is due to the *marginal* agent's discrete jump from no default to default. For bankruptcy, the ratio of liquidity and moral hazard effects is a sufficient statistic for the differences in marginal utility in versus out of bankruptcy for this *marginal* agent. By contrast, the equilibrium response of unemployment duration to changes in unemployment insurance (UI) benefits is due to *each* unemployed agent adjusting their search intensity in Chetty (2008). Because of this, the ratio of liquidity and moral hazard effects for UI identifies the *average* difference in marginal utility for all employed versus unemployed workers, which corresponds to the average insurance value of UI. This contrast highlights that the nature of an agent's decision matters for whose preferences are revealed by estimates of moral hazard and liquidity effects.

What can we learn from liquidity and moral hazard effects when their ratio identifies the change in marginal utility of the marginal agent? We cannot proceed as in Chetty (2008) to calculate the welfare gain from changing the generosity of insurance. Doing so requires an estimate of the average insurance value (i.e. the difference in average marginal utilities between filers and non-filers). In particular, theoretical work by Dávila (2020) shows that the average insurance value of bankruptcy and elasticity of interest rates to exemption limits are sufficient statistics for the welfare gain of increased exemptions. For bankruptcy, the ratio of liquidity and moral hazard effects instead sheds light on both the costs and benefits of bankruptcy facing the marginal filer. Specifically, the stronger liquidity effect implies that the marginal filer anticipates: (1) a larger rise in consumption upon filing and (2) larger non-monetary and/or dynamic costs of bankruptcy.