# Moral Hazard versus Liquidity in Household Bankruptcy *

**Sasha Indarte**†
Duke University

June 8, 2020

**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.

**JEL Classification:** G51, K35

*I am grateful to David Berger, Marty Eichenbaum, Guido Lorenzoni, Paul Mohnen, John Mondragon, and Matt Notowidigdo for their guidance and encouragement while working on this project. For insightful discussions and comments, I thank Kartik Athreya, Scott Baker, Gideon Bornstein, Ivan Canay, Larry Christiano, Anna Cieslak, Anthony DeFusco, Eileen Driscoll, Jan Eberly, Erik Gilje, Isaac Hacamo, Kilian Huber, Stephanie Johnson, Alice Jun, Wenli Li, David Matsa, Brian Melzer, Charlie Nathanson, Matt Rognlie, Nick Roussanov, Paola Sapienza, John Shea, Betsey Stevenson, Max Tabord-Meehan, Fabrice Tourre, Nick Turner, Harald Uhlig, Paul Willen, and Eric Zwick as well as numerous seminar and conference participants. Financial support from the Becker Friedman Institute's Macro Financial Modeling Initiative is gratefully acknowledged. This research was supported in part through the computational resources and staff contributions provided for the Quest high performance computing facility at Northwestern University which is jointly supported by the Office of the Provost, the Office for Research, and Northwestern University Information Technology. Previously circulated under the title *The Impact of Debt Relief Generosity and Liquid Wealth on Household Bankruptcy*. First version: October, 13, 2018.

†Duke University. Email: sasha.indarte@duke.edu

# 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 impact on filing of 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 it leaves the *wealth gain* from filing unaltered, by alleviating liquidity constraints. Pressure from liquidity constraints may otherwise motivate filing to boost 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 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 time stronger than the moral hazard effect (relief generosity). In particular, a $1,000 increase in relief generosity increases annual filing by 0.2 percentage points. The small moral hazard effect implies that a key component of the social cost of generous bankruptcy is small. To interpret these estimates, I develop a model of the household bankruptcy decision. Using a sufficient statistic approach, I show the high ratio of liquidity to moral hazard effects implies large consumption-smoothing benefits of bankruptcy for the marginal filer. However, it implies that non-monetary or dynamic costs of bankruptcy are also large. Finally, using a decomposition similar to that of Chetty (2008) I conclude that 83% of the filing response to dischargeable debt is due to liquidity effects, rather than moral hazard effects of financial incentives.

The first analysis focuses on moral hazard by estimating the causal effect of the debt relief available in bankruptcy on filing. Debt relief in bankruptcy depends on borrower debts,

[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.

assets, and states' bankruptcy exemption laws. Estimating the causal effect of relief generosity is challenging due to unobservable factors affecting both filing and wealth accumulation. The first (and to my knowledge, only other) analysis of this effect examines how filing varies with potential debt relief (Fay, Hurst and White, 2002). Failure to control for factors that both lower wealth and increase filing could cause such an approach to overstate the effect of generosity on filing. A standard 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]

To address this identification challenge I adopt a new approach: 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 sample of seven million homeowners from CoreLogic.

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, Lee, Pei and Weber, 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. This biases estimates towards zero when at least 50% of observations are assigned to the "correct" side, but biases estimates towards the negative of the true effect if more than 50% are incorrectly assigned. 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, 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 pay-

[3] See for example Mahoney (2015); Auclert, Dobbie and Goldsmith-Pinkham (2019); Pattison (2019).

[4] For evidence on the credit supply response to bankruptcy laws see Gropp, Scholz and White (1997); Pence (2006); Mitman (2016); Gross, Kluender, Liu, Notowidigdo and Wang (2019).

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

ment reductions on filing. Isolating the liquidity effect is challenging because many shocks to cash-on-hand, such as tax rebates (Gross, Notowidigdo and Wang, 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] The challenge for causal inference is that households less prone to default may have an easier time obtaining larger mortgages.

To identify the causal effect of mortgage payments, I exploit quasi-experimental variation in the size of payment 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. Adapting the approach of Gupta (2019), I estimate the effect of payment reductions on filing using the value of its index rate (Libor or Treasury) at the time of the reset as an instrument. Estimation uses data from CoreLogic on ARMs originated prior to 2008.

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 in 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.

The main empirical finding is 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

[6]Smaller increases in cash may also *increase* filing by helping cover the upfront costs of filing (Gross, Notowidigdo and Wang, 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, and does not erase liens on secured debt such as mortgages.

RKD estimates that a \$1,000 decrease in the generosity of bankruptcy leads to a 2.63% decrease in annual filings (a 0.02 percentage point drop in the rate). The small RKD estimate indicates that moral hazard is not strong driver of bankruptcy, and that increasing the generosity of bankruptcy weakly incentivizes further filing. The ARM IV estimates a \$1,000 decrease in total mortgage payments over the next year leads to at least a 12.59% decrease in annual filings (a 0.09 percentage point drop).[8] A strong 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. 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, Einav and Levin, 2009; Gupta and Hansman, 2019). But the estimates here challenge this interpretation in the context of bankruptcy. The estimates imply that 83% of the filing response to changes in dischargeable debt payments is due to liquidity, not the moral hazard response to financial incentives.[9]

**Related Literature:** This paper contributes to three strands of literature. First, I add to the literature on strategic default. Prior work focuses on mortgage delinquency, and there is a lack of consensus on the strength of strategic versus liquidity default motives (for evidence of a strong liquidity motives see Scharlemann and Shore, 2016, Gerardi, Herkenhoff, Ohanian and Willen, 2017, and Ganong and Noel, 2020; for strong strategic motives see Mayer, Morrison, Piskorski and Gupta, 2014, Haughwout, Okah and Tracy, 2016, and Dobbie and Song, 2020). Moreover, results for mortgage delinquency do not obviously generalize to bankruptcy. Liquidity may be an important driver of delinquency due to a lack of cash making repayment infeasible. However, bankruptcy is a choice and is not mechanically triggered in the same

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

[9]The decomposition is similar to that of Chetty (2008), which finds that liquidity also accounts for a majority of the unemployment duration response to unemployment benefit levels.

way by a lack of cash. This paper also studies a broader population, beyond households currently experiencing financial distress.[10]

Second, this paper documents new facts on the causes of bankruptcy that are useful for disciplining the general equilibrium models used to study the positive and normative effects of bankruptcy policy (e.g., Dubey, Geanakoplos and Shubik, 1990; Zame, 1993; Athreya, 2002, 2006; Livshits, MacGee and Tertilt, 2007; Chatterjee, Corbae, Nakajima and Ríos-Rull, 2007; Athreya, 2008; Chatterjee and Gordon, 2012; Nakajima and Ríos-Rull, 2019; Mitman, 2016; Gordon, 2017; Auclert, Dobbie and Goldsmith-Pinkham, 2019; Dávila, 2020). The filing response to potential debt relief and cash-on-hand are key determinants of the moral hazard and consumption-smoothing trade-offs of generous bankruptcy. The option to default helps complete markets by introducing contingency into otherwise non-contingent contracts. But "moral hazard also enters the picture...because sellers [of assets] have a choice not to deliver" (Dubey, Geanakoplos and Shubik, 2005). Additionally, the sufficient statistic in this paper builds on that of Dávila (2020) by relating moral hazard and liquidity effects to both the consumption response when filing and the non-monetary and dynamic costs of bankruptcy.

Lastly, this paper also contributes to the RKD/RDD literature (e.g. Calonico, Cattaneo and Titiunik, 2014; Card, Lee, Pei and Weber, 2015). Identification for "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 explanatory 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. The parametric RKD and RDD framework developed here would generally allow future work with mis-measured running variables to conclude that RKD/RDD estimates are at worst attenuated.

This paper is organized as follows. Section 2 gives background information on consumer bankruptcy. Section 3 describes the data. Section 4 presents 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 con-

[10] 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.

[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.

sumption in response to events such as job loss and illness. Households primarily file under Chapter 7 or Chapter 13.[12]

A central determinant of the generosity of debt relief filers receive in bankruptcy are states' asset exemption laws. In order to receive a debt discharge in bankruptcy, filers are required to pay creditors the value of assets in excess of asset-specific exemption limits. Higher exemption limits increase the 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 for bankruptcy 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 are typically $1,000-$2,000.

Costs to households under Chapter 13 are closely related to those in Chapter 7. In Chapter 13, households make monthly payments to creditors over a three to five year period, whereas in Chapter 7 households make a one-time payment. 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, Dobbie and Goldsmith-Pinkham, 2019), which protects a filer's home equity. The homestead exemption induces a kink in *seizable equity* as a function of home equity:

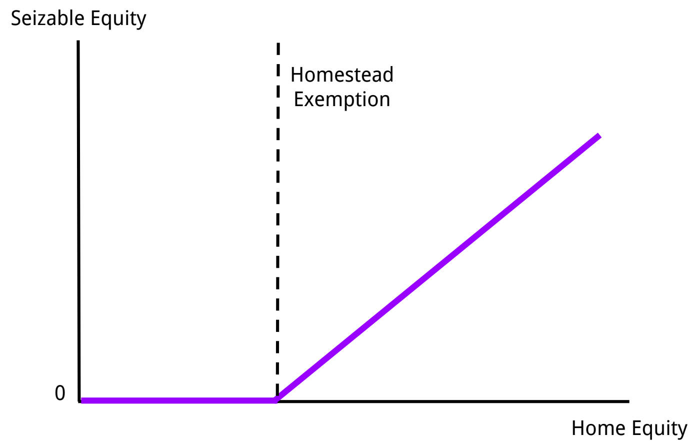
$$\text{Seizable Equity} = \max\{\text{Home Equity} - \text{Homestead Exemption}, 0\}.$$

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

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 2000-2017. Cross-state differences in generosity are highly persistent (Hynes,

[12] Households that own a business sometimes file under Chapter 11. Chapter 11 filings are a small share of non-business filings (0.15% in 2017).

**Figure 1:** The Kink in Seizable Equity




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

Malani and Posner, 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 (2000-2016). The LLMA data contain 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 2000-2016. 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:** To measure home equity, I subtract the end-of-month mortgage balance from an imputed 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 ZIP-level house price indexes, similarly to Di Maggio, Kermani, Keys, Piskorski, Ramcharan, Seru and Yao (2017).[13] 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 (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. 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 household's home equity and their state's homestead exemption, which I'll refer to as "equity distance" and denote by $D \in \mathbb{R}$. 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

[13] See appendix C.2 for details.

evaluated at the point where equity distance equals $D_0$, i.e.

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

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

**Identification:** Under the assumptions of Card, Lee, Pei and Weber (2015), $\tau$ 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 key identifying assumption is that the density of unobserved factors that affect the bankruptcy decision is smooth at the kink (i.e., no jump and no kink). Importantly, the parameter above is a partial derivative. It reflects purely the *direct* effect of changes in seizable equity on households' filing propensity. Intuitively, the RK estimand isolates this direct effect by differencing out the other channels through which equity distance ($D$) affects filing ($B$). As long as these omitted factors 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: $\frac{\partial\mathbb{E}(B|D=\widetilde{D})}{\partial S}S'(\widetilde{D})$.

The RKD addresses two key challenges that make it difficult to identify the causal effect of costly bankruptcy on filing. First, regressing filing directly on households' net financial benefit of bankruptcy (or its components) would likely *overstate* the deterrence of costly bankruptcy on filing. This is because omitted variables that limit wealth accumulation, and thus reduce the cost of bankruptcy, may independently make bankruptcy more appealing (e.g., income volatility). 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. These are the types of confounding factors differenced out by the RKD.

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. An RKD simply requires that other factors affecting the outcome do not kink at the cutoff. However, 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 to check for a possible failure of the key identifying assumption of no jump or kink in omitted factors. 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.[14] 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 likely 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 would be grounds for a judge to dismiss a case. Second, it would be challenging to reduce one's home value. Reducing 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. This would slowly change home equity as payments towards principal are generally on the order of hundreds of dollars. Moreover, persistent delinquency significantly increases the chances a mortgage lender will begin foreclosing on the property. If the goal of the household is to remain in their home by getting under the exemption limit, delinquency will in turn make this much harder to achieve.[15] Fourth, a household could also attempt to lower their home equity by taking out an additional loan against their property, but a financially distressed borrower would have hard time qualifying for such a loan.

**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 (Fan and Gijbels, 1996; Card, Lee, Pei and Weber, 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, Cattaneo and Titiunik (2014). Because the measure of home equity is likely subject to measurement error, I also use a new

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

[15] An appealing feature of bankruptcy is the "automatic stay" it offers filers, which stops creditors from *initiating* a foreclosure while the household is filing. However, creditors that have already begun the foreclosure process can request (and are often permitted) to continue a foreclosure already in progress (Elias, 2011).

parametric estimator that corrects for bias due to measurement error.

### 4.2 A New Approach to Measurement Error in Regression Kink/Discontinuity Designs

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, Lee, Pei and Weber (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. 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, Lee, Pei and Weber, 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 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, Lee, Pei and Weber (2015). When this assumption fails, the fuzzy RKD/RDD identifies a parameter proportional to the local average response.[16]

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 approach. Integral to this approach is a

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

parametric assumption. Specifically, for the RKD, I assume that the outcome is a quadratic function of the true values of the running and policy variables, and unobserved factors additively affect filing.[17] This assumption gives rise to an alternative characterization of the local average response targeted by the sharp RKD estimand of Card, Lee, Pei and Weber (2015) as a function of parameters. I assume that measurement error is mean-zero and uncorrelated with the running variable and unobserved factors, but I allow for the running variable to be *correlated* with the unobserved factors affecting filing.

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,[18] the probability limit of the numerator of the parametric RK estimator is

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

The above expression characterizes the bias of the estimator relative to the true difference in the linear terms $\beta_1^+ - \beta_1^-$. Here, $\sigma_x^2$ is the variance of the mis-measured running variable and $\sigma_\mu^2$ 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 ($\sigma_\mu^2$) grows. In the second 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

[17] 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).

[18] The assumptions are symmetry in the distribution of the running variable as in Griliches and Ringstad (1970) 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.

vice versa for $\pi^-$). 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., $\pi^+ + \pi^- > 1$).

The difference for the RDD analog is that there is no attenuation bias. However, bias still arises from measurement error assigning observations to the wrong side of the cutoff. In particular

$$\widehat{\tau}^{PRD} \equiv \widehat{\beta}_0^+ - \widehat{\beta}_0^- \xrightarrow{p} (1 - \pi^+ - \pi^-)(\beta_0^+ - \beta_0^-) = (1 - \pi^+ - \pi^-)\tau^{RD}.$$

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:

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

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

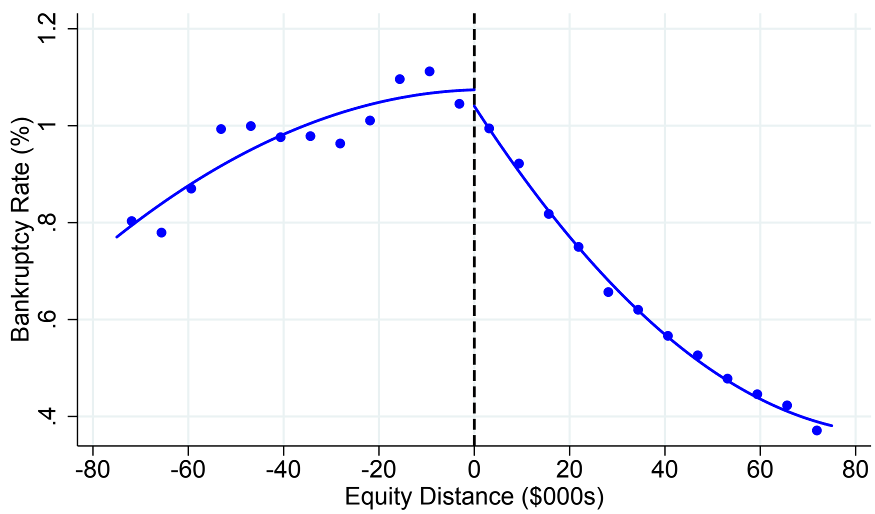
where $\widehat{\sigma}_x^2, \widehat{\sigma}_\mu^2, \widehat{\pi^+}$, and $\widehat{\pi^-}$ are the sample variance of the mis-measured running variable and measurement error and the sample averages of how often observations are assigned to the wrong side. In my application, I use a subsample of households selling their homes in the quarter of interest and treat the observed sale price as the correct home value. Comparing this correct price to the imputed home price, I can then estimate the four required parameters needed to correct for measurement error.

### 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 outlined in section 4.2 correcting for measurement error. The preferred specification (column 2) uses this new approach and linearly controls for home equity. In this column, the point estimate of -2.63 implies that a \$1,000 decrease in seizable equity increases a household's probability of filing by 2.63% relative to the sample's average annual filing rate of 0.71%. The 95% confidence interval rules out an increase greater than 3.30%. The 2.63% estimate corresponds to a 0.019 percentage point change in the level of the filing rate. Filing is not strongly influenced by the generosity of relief borrowers receive in bankruptcy. Moral hazard is a not strong driver of household bankruptcy.

Figure 2 depicts a reduced-form version of the RKD, plotting the filing rate for households against equity distance. Above the exemption limit every additional dollar of home equity is another dollar a filer must pay in bankruptcy, and the filing rate becomes negatively associated with equity distance. Below the exemption limit a marginal dollar of home equity does not affect seizable home equity, and there is a slightly upward-sloping relationship between filing and equity distance.

**Figure 2:** The Effect of Seizable Equity on Bankruptcy Filings




*Notes:* 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.

Two factors likely contribute to the positive slope below the exemption limit. First, households in states with generous exemptions tend to accumulate less unsecured debt and face higher borrowing costs (Pence, 2006; White, 2007; Severino and Brown, 2017). All else equal, having less debt diminishes incentives to file. Second, the payoffs of filing for bankruptcy resemble those of an option where the underlying asset is the potential filer's home equity and the strike price is the exemption limit. As home equity approaches the exemption limit, potential filers may worry that soon their bankruptcy option will no longer be in-the-money and prefer to exercise it today (i.e., file).[19]

The presence of multiple kinks across states and time mitigates two common limitations of an RKD. First, pooling a sample with multiple exemption limits makes it possible to control for home equity while using equity distance as the running variable. Home equity is likely strongly correlated with many factors affecting the bankruptcy decision, and controlling for it may significantly improve statistical power. Second, pooling also means that the RKD identifies a local average response that is a weighted average across multiple exemption limits. By including households at low, medium, and high cutoffs, we can identify a more representative average response.

[19]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.

**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 Obs. | 21,383,503 | 21,383,503 | 17,536,206 | 25,884,133 |
| RHS Obs. | 24,637,614 | 24,637,614 | 20,013,724 | 29,029,069 |

*Notes:* 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 $\frac{\sigma^2_\mu}{\sigma^2_x} = 0.22$. Approximation bias-corrected robust standard errors are computed as in Calonico, Cattaneo and Titiunik (2014). The bandwidth is optimally chosen for each specification using the MSE-minimizing procedure of Calonico, Cattaneo and Titiunik (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 is displayed at the bottom of the table. Statistical significance: 0.05*, 0.01**, and 0.01***.

#### 4.3.1 Robustness and Interpretation

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, Cattaneo and Titiunik (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.

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, instead of a uniform kernel, yields similar results.[20] 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 the empirical distribution of limits (e.g., assigning Minnesota's history of limits to Arizona). For each draw, I compute every household's dis-

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

tance to the placebo exemption and re-run the RKD estimation using these placebo measures of equity distance.[21] Intuitively, the test compares the extremeness of the actual RKD estimate to those computed for the placebo data.

Formally, the permutation test assesses the null hypothesis of no treatment effect (i.e., seizable equity does not affect bankruptcy filings). A key assumption in this test is that the actual exemption limits are drawn from a known distribution of placebo exemption limits. The main advantage of the permutation test is that it has exact size in finite sample. Simulations in Ganong and Jäger (2018) document the downside of the permutation test, which is that it is underpowered relative to inference done with the bias-corrected confidence intervals of Calonico, Cattaneo and Titiunik (2014). However, this means that statistical significance in the permutation test is a more compelling rejection of the null hypothesis. An advantage of both the methods of Ganong and Jäger (2018) and Calonico, Cattaneo and Titiunik (2014) is that, in simulations, they perform better than standard asymptotic inference in distinguishing kinks from highly nonlinear relationships between the outcome and running variable (Ganong and Jäger, 2018).

I run 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 of no effect at the 10% level.

**Ex Ante vs. Ex Post Moral Hazard:** The direct effect of relief generosity on filing is not the only moral hazard response generous bankruptcy may engender. The direct effect estimated by the RKD is a form of *ex post* moral hazard. It describes how a borrower's ex post response to the shocks they face change with their incentives. However, generous bankruptcy may also cause *ex ante* moral hazard such as increased borrowing of dischargeable debt. By identifying the direct effect on filing, the RKD isolates one channel of moral hazard, but a crucial channel. The ex post response to a marginal dollar of debt relief is informative about the *effects* of ex ante behavior that change the decision to file. A small direct effect of potential debt forgiveness means that the moral hazard effect on filing of a marginal dollar of dischargeable debt is small. That is, even if borrowing rises significantly, a small response to debt forgiveness implies a limited effect a higher debt burden on filing through moral hazard.

### 4.3.2 Heterogeneity

Are some households more or less responsive to changes in bankruptcy's generosity? I investigate this by estimating the RKD in subsamples with above and below median covariate

[21] 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, Cattaneo and Titiunik, 2014).

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, 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.[22] Additionally, it differs from prior work finding that households at a low risk of delinquency were most likely to default in response to a change in the generosity of mortgage debt relief they could obtain through delinquency (Mayer, Morrison, Piskorski and Gupta, 2014).

How does the filing response vary with borrower and local economic characteristics? Households with an above-median loan-to-value (LTV) at origination are 71% more sensitive to bankruptcy's generosity. Those with a below-median FICO score at origination are nearly eight times more sensitive to a given change in relief generosity. High LTV and low FICO borrowers are more likely to experience financial distress. Both factors are associated with more costly and scarce future 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 seven times more responsive.

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 insurance (Gross and Notowidigdo, 2011). Additionally, mortgage delinquency tends to fall when UI becomes more generous (Hsu, Matsa and Melzer, 2018).

The model of section 6 gives insights into the mechanisms driving this heterogeneity. The model implies stronger responses come from either (1) being more likely to be near the brink of bankruptcy and/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

[22] 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.

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 Q1 to 2010 Q4), and post-recession (2011 Q1 to 2016 Q1). The point estimate in the recession is more than double the estimates for 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 time larger. The model of section 6 can help rationalize this spike in sensitivity. 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, the dynamic costs of bankruptcy in the present becomes smaller. The model formalizes that these dynamic costs can limit the strength of the moral hazard effect.

### 4.4 Discussion: The Moral Hazard Effect

The RKD estimates a smaller filing response to bankruptcy generosity than prior work. Notably, Fay, Hurst and White (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% decrease in the filing rate in response to a $1,000 decrease in the financial benefit of bankruptcy.[23] One potential reason for the larger estimate is that adverse events like job loss or illness can lower wealth, reducing the cost of bankruptcy, and also independently motivate filing for bankruptcy. The RKD can net out the effect of these confounding factors under the assumption that they are not kinked functions of equity distance.

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.[24] The RKD estimates the moral hazard effect by isolating the direct effect of generosity. Instrumental variables and difference-in-difference approaches can identify the (causal) general equilibrium effect of generosity (e.g., Mahoney, 2015; Auclert, Dobbie and Goldsmith-Pinkham, 2019; Pattison, 2019). Both effects are of independent interest and jointly can inform models assessing the positive and

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

[24]Empirically, generous exemptions are strongly associated with lower unsecured credit and higher interest rates (Gropp, Scholz and White, 1997; Pence, 2006; White, 2007; Severino and Brown, 2017). Looking directly in the cross-section, filing tends to be *higher* where homestead exemptions are lower (Mitman, 2016) – the opposite sign of the RKD estimate.

normative effects of generous bankruptcy.

A third approach uses the 2005 BAPCPA reform as a source of variation in filing costs (Mitman, 2016; Gross, Kluender, Liu, Notowidigdo and Wang, 2019). BAPCPA increased the costs of filing for bankruptcy by raising upfront legal and court fees by $500-600 (Lupica, 2012) and also 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.[25] Variation in a backloaded cost is useful here because it 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.

In sum, using an RKD complements prior work on bankruptcy policy by isolating the moral hazard effect. This paper also compares the size of the moral hazard and liquidity effects. The next section focuses on the estimation of the liquidity effect.

# 5 The Effect of Mortgage Payment Reductions on Bankruptcy Filings

## 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 – bankruptcy is used to discharge unsecured debts – and 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:

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

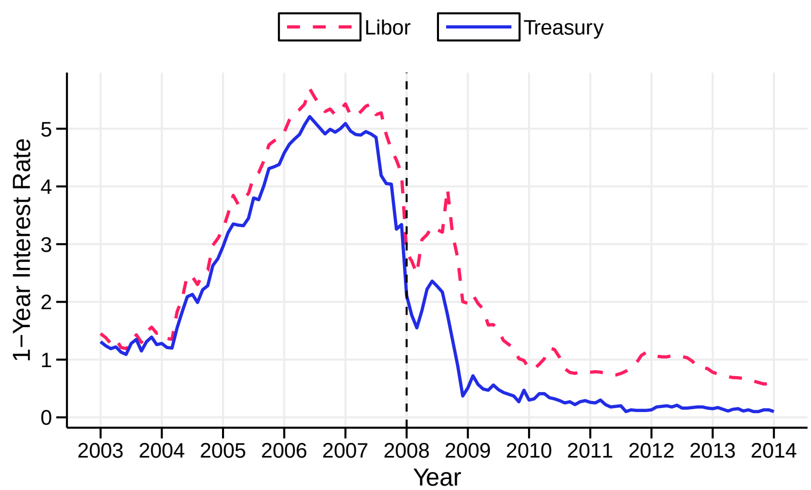
ARMs originated during the sample period typically had a margin of 2-2.5%, and popular

[25] 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, Notowidigdo and Wang, 2014).

choices for index rates were the one-year Libor and Treasury rates.

During 2003-2008, the Libor and Treasury rates had a nearly constant spread of about 25-50 basis points (see Figure 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.[26] The spread in this unusual period resulted in a natural experiment in which otherwise identical mortgages received different-sized payment reductions when resetting. 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.

**Figure 3:** One-Year Libor and Treasury Rates




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

I exploit this natural experiment in an instrumental variables strategy. The parameter of interest is the coefficient $\beta$ 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 explanatory variable of interest is $\text{MPay}_{ic}$, 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 $\beta$, I instrument for $\text{MPay}_{ic}$ using the value of the index rate (Libor or Treasury) at the time of the reset. The first-stage equation is

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

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

where $\text{IndexRate}_{ic}$ 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. This means the IV estimator 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 $t$.[27] Controlling for home equity is important as it can affect filing through seizable equity. Additionally, interest rate reductions can have a small effect on the rate at which the mortgage balance is paid off over the next year.[28] Controlling for home equity helps to separate the direct effect of payment reductions on filing from any indirect effects through changes in seizable home equity and the generosity of the debt relief the household would receive in bankruptcy.

**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. In fact, 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 2003-2008 did not anticipate the upcoming widening spread and would not have a had a strong reason to prefer Libor indexation. Looking for systematic difference in Libor versus Treasury loans originated in this time, the main difference is that Libor loans tended to have a larger mortgage balances. This makes it important to control for the balance in the IV estimation. But other mortgage characteristics and local economic conditions (county unemployment, income, etc.) are similar for Libor and Treasury-indexed mortgages.[29]

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%

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

[28] For the many mortgages in the sample that are "interest-only" during the first reset, this is not an issue. But for households making both principal and interest payments, a rate decrease will slightly increase the rate of amortization early on in the life of a mortgage.

[29] See Appendix Table A.1 for summary statistics and Appendix Table D.7 for a regression of an indicator for Libor indexation on mortgage and regional characteristics.

confidence interval includes zero.[30] This evidence is 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, Kermani, Keys, Piskorski, Ramcharan, Seru and Yao (2017), which study the effects of ARM resets on mortgage delinquency and consumption in event-study and difference-in-difference frameworks. In contrast, the IV approach here uses *within*-period variation in payment reduction sizes instead of variation due to the timing of resets. In essence, I adapt the approach of Gupta (2019), which also uses ARM index rate choices as a source of variation in interest rate reductions, to address an additional identification challenge. Namely, while the mortgage rate change induced by resets is plausibly exogenous, the change in the *dollar amount* of the required payment may be endogenous. Loan-to-value and payment-to-income restrictions on mortgages can limit access to large mortgages for low-wealth/income households. All else equal, households with larger mortgages will tend to get larger payment reductions; and households with better access to larger mortgages may be less default-prone. OLS estimation using payment changes during resets may therefore tend to understate the causal effect of payment reductions.

**Sample:** This analysis uses a subsample from the LLMA of ARMs originated during 2003-2008. I truncate the sample to these years so that the mortgages were *originated prior* to the widening of the Libor-Treasury spread but *reset after*. Additionally, ARMs resetting post-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.[31]

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.

### 5.2 Results: The Effect of Payment Reductions on Filings

Table 2 reports the IV estimation results.[32] The estimate under the preferred specification (column 4) implies that a $1,000 decrease in annual mortgage payments leads to a statistically

[30] Appendix Table D.6 reports estimation results for the placebo test.

[31] As discussed in Fuster and Willen (2017), financially constrained households (e.g., unemployed or with negative equity) will have a harder time qualifying for a refinance. A rate increase may "worsen" the pool of non-refinancing borrowers in terms of financial health. Using rate increases could induce selection bias that leads the IV estimate to overstate the causal effect of high rates/payments on default.

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

significant 30% drop in the annual bankruptcy filing rate. This corresponds to a 0.21 percentage point decrease in the probability of filing over the next year relative to the sample's average annual 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 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 above ten, rejecting that the instrument is weak. Analogous OLS estimates are ten times smaller than the IV estimates (Appendix Table D.9). This is a consistent with a negative bias stemming from households who are less prone to filing obtaining larger mortgages.

**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. | 20.71 | 18.52 | 17.16 | 16.49 |
| 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 | | | | ✓ |

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

The first and second stage estimates are robust to a number of changes in the econometric specification. This includes using the log mortgage payment component, using a binary indicator for Libor indexation, and collapsing the data to a longitudinal panel (i.e. one, rather than twelve, observations per mortgage).[33] Using the entire new mortgage payment, as opposed to only the component determined by the index rate, delivers similar point estimates, but the first stage is weaker.

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.

**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. This total effect may be of interest for assessing the impact on bankruptcy of policies encouraging wider usage of ARMs (e.g., Eberly and Krishnamurthy, 2014). But 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 $M_\tau$ in month $\tau$. 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, i.e. the probability the household does

[33] Tables for the additional results described here are available by request.

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 $\delta$. The expected NPV of payments is

$$M_\tau^{NPV} = \underbrace{s_\tau(1-\delta)M_\tau}_{\text{current payment}} + \underbrace{\sum_{j=1}^{360-\tau} s_{\tau+j}(1-\delta)\frac{\mathbb{E}_\tau(M_{\tau+j})}{(1+r)^j}}_{\text{future payments}}. \tag{7}$$

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.[34] The average monthly delinquency 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, Knittel and Zettelmeyer (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. Finally, I assume expectations over future mortgage payments are Martingale, which implies $\mathbb{E}_\tau(M_{\tau+j}) = M_\tau$ for all periods $j$.[35] When expectations are Martingale, the NPV is linear in the current payment:

$$M_\tau^{NPV} = M_\tau \underbrace{\sum_{j=0}^{30-\tau} \frac{s_{\tau+j}(1-\delta)}{(1+r)^j}}_{\equiv 12\times\theta}. \tag{8}$$

Estimating the mapping between current payments and the NPV of payments entails estimating the annual scaling factor $\theta$. Using the chosen discount rate and estimated survival and delinquency rates yields an estimate of 6.23 for $\theta$. 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 2000-2016. 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 (2008-2016), which could feature a higher-than-average liquidity effect. However, the ARM sample also restricts

[34] I estimate scale and shape parameters $\alpha = 5.9\text{e-}4$ and $\gamma = 1.59$,respectively, for the Weibull survival function $S(t) = \exp(-\alpha t^\gamma)$.

[35] 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.

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, Fortin and Lemieux (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 sample composition 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 for bankruptcy by 12.59%. This estimate of the liquidity effect is five times larger than the RKD estimate of the moral hazard effect (12.59 vs. 2.63). This relative change corresponds to a 0.09 percentage point decrease in the annual probability of filing compared to the RKD sample average of 0.71%.

The estimates indicate that the decision to file for bankruptcy is more sensitive to 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.[36] In this subsample, I estimate that the liquidity effect is 1.3-8.2 times stronger than the moral hazard effect. The point estimates imply a $1,000 increase in non-seizable cash reduces filing by 14.86-92.42% while an equivalent increase in relief generosity reduces filing by 11.28%.

[36]This sample is much smaller mainly because this entails restricting the sample to those with ARMs, in the 14 states with no exemption doubling for couples, and non-delinquent households. See Appendix Tables D.10 for estimation results.

**Table 3:** IV Estimates of Filing Response to Liquidity (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. | 40.84 | 31.97 | 28.84 | 28.38 |
| 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 | | | | ✓ |

*Notes:* 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***.

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.[37] 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.8). 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 do affect filing. In this sense, NPV-adjusting the estimate is conservative.

Appendix E.4 addresses a scenario that some households may face in which mortgage payments affect filing through both a moral hazard and liquidity effect. In Chapter 13 bankrupt-

[37]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, Campbell, Meisner-Nielsen and Ramadorai, 2020; Keys, Pope and Pope, 2016), and those with ARMs often underestimate or do not know how much their interest rates could change (Bucks and Pence, 2008).

cies, which comprise typically 30% of filings, statute requires creditors receive the maximum of disposable income over three to five years or the value of seizable assets. Disposable income is calculated net of expenses such as mortgage payments. When disposable income is sufficiently high, a fall in mortgage payments can reduce the net financial benefit of bankruptcy by increasing disposable income. A payment reduction in this scenario would change *seizable* resources, affecting filing through both moral hazard and liquidity. This would lead the IV estimate to overstate the liquidity effect. Appendix E.4 uses the model of Section 6 to decompose the filing response to a change in *seizable* resources into the sum of moral hazard and liquidity effects. The decomposition implies that the true liquidity effect must be at least as large as the IV estimate minus the moral hazard effect. Therefore, the liquidity effect is still stronger than the moral hazard effect (i.e., 12.59% - 2.63% = 9.96% $\geq$ 2.63%).

### 5.4 Discussion: Moral Hazard, Liquidity, and Default

The estimate of the liquidity effect for bankruptcy adds to our broader understanding of the importance of liquidity in driving household default. Prior work finds that mortgage payment reductions significantly reduce mortgage delinquency and foreclosures (Fuster and Willen, 2017; Di Maggio, Kermani, Keys, Piskorski, Ramcharan, Seru and Yao, 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. This suggests 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 can motivate default on dischargeable debt by both reducing cash-on-hand and making it harder to smooth consumption (liquidity), and by increasing the payoff of delinquency and reducing the incentive to repay (moral hazard). Often, the relationship between debt payment size and delinquency is interpreted as pure moral hazard, a substitution effect whereby higher payments reduce the incentive to repay (e.g., Adams, Einav and Levin, 2009; Gupta and Hansman, 2019). 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.[38] This distinction is 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.

[38] 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%).

## 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. These comparative statics correspond 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 $d_t$.[39] 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 receives payout $a$ from an annuity each period. The budget constraints are:

$$\begin{aligned} c_1^N &= y_1 + a - R_1(d_1)d_1 + d_2 \qquad\qquad c_t^B = a + e, \quad t = 1, 2. \\ c_2^N &= y_2 + a - R_2(d_2)d_2 \end{aligned} \tag{9}$$

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

The household chooses consumption, whether to file for bankruptcy each period, and borrowing in the first period in order to maximize the present value of utility, where 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.[40] 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

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

[40] 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).

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

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

The value function in the terminal period is

$$V_2^N(y_2, d_2) = u(c_2^N)$$
$$V_2^B = u(c_2^B) - \sigma$$

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

The first-order condition governing borrowing is

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

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

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

The probability that the household files for bankruptcy 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

The moral hazard and liquidity effects, estimated in the empirical analyses, correspond to comparative statics within the model. Specifically, 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. Next, I derive these comparative statistics and show that their ratio is a sufficient statistic for the change in marginal utility upon filing for the marginal filer. This sufficient statistic implies that, when the liquidity effect is stronger than the moral hazard effect, the marginal filer anticipates both a large consumption gain from filing but also large costs outside of the immediate monetary cost.

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

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

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

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

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

Above, $c_1^{N\star}$ denotes consumption when not filing and income is *at the bankruptcy threshold*, i.e. $y_t = y_t^\star$. The expressions are simple because the household chooses borrowing optimally.[41] Equation (11) is the moral hazard effect, i.e. the filing response to a marginal change in the wealth gain from filing. For a strictly increasing utility function, the moral hazard effect is positive: filing increases when the net gain from bankruptcy rises. The liquidity effect corresponds to equation (12), which is the filing response to a marginal change wealth available both in and out of bankruptcy. The sign of the liquidity effect is theoretically ambiguous, but the estimates of Section 5 imply it is negative: filing decreases when cash-on-hand increases.

If households faced *perfect* credit and insurance markets, changes to the exemption $e$ would still affect filing, but changes in the non-seizable annuity $a$ would have no effect. If households could perfectly smooth their consumption, marginal utility would be the same whether or not the household files. The moral hazard effect would simply equal $f(y_1^\star)$ and the liquidity effect would equal zero. Intuitively, filing still responds to debt relief because it distorts incentives to repay. A dollar increase in bankruptcy generosity is another dollar forgone when *not* filing for bankruptcy. With perfect markets, bankruptcy provides no insurance value and the option to file is socially inefficient if filing reduces lending. 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

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

The term on the right is the relative change 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

[41] Applying the envelope theorem, the partial derivative of either value function with respect to borrowing is zero. The comparative statics here also follow from applying the multivariate implicit function theorem, using the borrowing first-order condition as the second equation. But due to the envelope theorem, one can simply implicitly differentiate the indifference condition.

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. Under CRRA preferences with a risk-aversion parameter of two, the estimated effects imply that consumption is 41.6% lower when *not* filing, for the marginal filer. Additionally, the relative consumption gain of infra-marginal agents would likely be even larger as their income was even lower than the marginal filer's. Such a large increase in consumption suggests bankruptcy could play a useful role as an automatic stabilizer and enhance macroeconomic stability. Consistent with this, (Auclert, Dobbie and Goldsmith-Pinkham, 2019) estimates that access to bankruptcy boosted employment by nearly 2% in the Great Recession.

**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

$$\underbrace{-\sigma}_{\text{utility penalty}} - \underbrace{\left\{p_2\mathbb{E}^N(V_2^B) + (1-p_2)\mathbb{E}^N\left[V_2^N(y_2, d_2)\right] - \mathbb{E}^B\left[V_2^N(y_2, 0)\right]\right\}}_{\text{dynamic cost}} << 0 \tag{14}$$

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 signals by revealed preference that household 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, Gerardi, Goette and Willen, 2010; Elul, Souleles, Chomsisengphet, Glennon and Hunt, 2010; Foote and Willen, 2018). Additionally, it can help explain why fifteen times as many households would financial 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, Keys and Mahoney, 2017; Gross, Notowidigdo and Wang, 2020; Herkenhoff, Phillips and Cohen-Cole, 2019). Bos, Breza and Liberman (2018) finds positive effects of flag removal on employment and earnings in Sweden, while Dobbie, Goldsmith-Pinkham, Mahoney and Song (2019) 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, 2020). 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.

Evidence on delinquency suggests that a moral aversion (stigma) may be an important deterrent to default. In a large survey, Guiso, Sapienza and Zingales (2013) find 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, Fiorin, Gottlieb and Katz, 2019) and that anticipated disclosure of default to peers deters delinquency (Diep-Nguyen and Dang, 2019). However, evidence of peer effects for bankruptcy suggest stigma may diminish if filing becomes more common (Kleiner, Stoffman and Yonker, 2019).

**Extensions:** The sufficient statistic in equation (13) and the implications for consumption and non-monetary/dynamic costs of bankruptcy are robust to several model extensions. Similarly to Chetty (2008), allowing for additional agent choices (e.g., labor supply) and constraints (e.g., borrowing constraints) does not change the results as long as a marginal change in the exemption $e_1$ or non-seizable endowment $a_1$ do not cause constraints to start/stop binding. 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 (14) (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.

### 6.3 Discussion

Comparing the model and sufficient statistic here to related work on unemployment insurance (UI) illuminates similarities and differences between bankruptcy and UI. Notably, Chetty (2008) decomposes the response of job-finding rates to changes in UI benefits into a moral hazard and liquidity effect. Benefits can inefficiently deter job search through moral hazard by distorting the payoff from finding a job. Benefits can also efficiently deter job search by alleviating pressure from liquidity constraints while unemployed.

The causal effect of both benefit and debt payment levels 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 (see Appendix E.4 for the decomposition). 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.

The sufficient statistic here differs from the that of Chetty (2008) in one important dimension. When the outcome of interest is *binary* and follows a threshold-style rule, as is standard in models of default, the equilibrium response is driven by the *marginal* agent's discrete jump from no default to default. For bankruptcy, the ratio of liquidity and moral hazard effects therefore reflects differences in marginal utility for this marginal agent. In contrast, *each* unemployed agent in Chetty (2008) chooses their *intensity* of search effort in order to equate the marginal benefits and costs of search. The ratio of liquidity and moral hazard effects therefore reflects *average* expected differences in marginal utility. This is why Chetty (2008) obtains a sufficient statistic for the insurance value of UI whereas with bankruptcy the analogous sufficient statistic is informative about the value of bankruptcy for the marginal filer. Contrasting these sufficient statistics for bankruptcy and UI highlights that the nature of an agent's decision(s) matters for whose preferences are revealed by estimates of moral hazard and liquidity effects.

## 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 payments reductions in an IV strategy, I estimate that the liquidity effect is five times stronger than the moral hazard effect. The

RKD quantifies the moral hazard effect by isolating variation in the payoff from filing while holding constant cash-on-hand. The IV quantifies the liquidity effect using variation in cash available both in and out of bankruptcy, holding fixed the payoff from filing.

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. It would be useful for future research to build on this parametric framework. One especially useful direction would be to explore optimal bandwidth selection in this framework, as 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 moral hazard is not a strong driver of household bankruptcy. Increasing the generosity of bankruptcy weakly incentivizes further filing, implying 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. When the liquidity effect is much stronger than the moral hazard effect, this implies a key component of the benefit of generous bankruptcy – its ability to smooth consumption – is large. This follows from a sufficient statistic. 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. Together, these suggest significant scope for generous bankruptcy to improve welfare. 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. More empirical work on stigma in the context of bankruptcy would be valuable. Such work could inform structural analyses that allow for stigma to change when bankruptcy becomes more common and/or more generous.

## References


**Adams, William, Liran Einav, and Jonathan Levin**, "Liquidity Constraints and Imperfect Information in Subprime Lending," *American Economic Review*, 2009, *99* (1), 49–84.

**Albanesi, Stefania and Jaromir Nosal**, "Insolvency After the 2005 Bankruptcy Reform," 2020.

**Andersen, Steffen, John Y. Campbell, Kasper Meisner-Nielsen, and Tarun Ramadorai**, "Sources of Inaction in Household Finance: Evidence from the Danish Mortgage Market," *American Economic Review (forthcoming)*, 2020.

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

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

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

_ , "Default, Insurance, and Debt over the Life-Cycle," *Journal of Monetary Economics*, 2008, *55* (4), 752–774.

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

**Bos, Marieke, Emily Breza, and Andres Liberman**, "The Labor Market Effects of Credit Market Information," *The Review of Financial Studies*, 2018, *31* (6), 2005–2037.

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

**Bursztyn, Leonardo, Stefano Fiorin, Daniel Gottlieb, and Martin Katz**, "Moral Incentives in Credit Card Debt Repayment: Evidence from a Field Experiment," *Journal of Political Economy*, 2019, *127* (4), 1641–1683.

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

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

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

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

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

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

**Dávila, Eduardo**, "Using Elasticities to Derive Optimal Bankruptcy Exemptions," *Review of Economic Studies*, 2020, *87* (2), 870–913.

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

**Diep-Nguyen, Ha and Huong Dang**, "Social Collateral," 2019.

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

**Dobbie, Will and Jae Song**, "Debt Relief and Debtor Outcomes: Measuring the Effects of Consumer Bankruptcy Protection," *American Economic Review*, 2015, *105* (3), 1272–1311.

_ **and** _ , "Targeted Debt Relief and the Origins of Financial Distress: Experimental Evidence from Distressed Credit Card Borrowers," *American Economic Review*, 2020, *110* (4), 984–1018.

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

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

**Dubey, Pradeep, John Geanakoplos, and Martin Shubik**, "Default and Efficiency in a General Equilibrium Model with Incomplete Markets," *Cowles Foundation Discussion Paper No. 773*, 1990.

_ **,** _ **, and** _ , "Default and Punishment in General Equilibrium," *Econometrica*, 2005, *73* (1), 1–37.

**Eberly, Janice and Arvind Krishnamurthy**, "Efficient Credit Policies in a Housing Debt Crisis," *Brookings Papers on Economic Activity*, 2014, *2014* (2), 73–136.

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

**Elul, Ronel, Nicholas S. Souleles, Souphala Chomsisengphet, Dennis Glennon, and Robert Hunt**, "What 'Triggers' Mortgage Default," *American Economic Review: Papers & Proceedings*, 2010, *100* (2), 490–494.

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

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

**Foote, Christopher and Paul Willen**, "Mortgage-Default Research and the Recent Foreclosure Crisis," *Annual Review of Financial Economics*, 2018, *10*, 59–100.

_ **, Kristopher Gerardi, Lorenz Goette, and Paul Willen**, "Reducing Foreclosures: No Easy Answers," *NBER Macroeconomics Annual*, 2010, *24* (1), 89–138.

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

**Ganong, Peter and Pascal Noel**, "Liquidity vs. Wealth in Household Debt Obligations: Evidence from Housing Policy in the Great Recession," 2020.

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

**Gelman, Andrew and Guido Imbens**, "Why High-Order Polynomials Should Not Be Used in Regression Discontinuity Designs," *Journal of Business & Economic Statistics*, 2018, pp. 1–10.

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

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

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

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

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

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

**_ , _ , and _** , "The Marginal Propensity to Consume Over the Business Cycle," *American Economic Journal: Macroeconomics*, 2020, *12* (2), 351–384.

**_ , Raymond Kluender, Feng Liu, Matthew J. Notowidigdo, and Jialan Wang**, "The Economic Consequences of Bankruptcy Reform," 2019.

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

**Gupta, Arpit**, "Foreclosure Contagion and the Neighborhood Spillover Effects of Mortgage Defaults," *The Journal of Finance*, 2019, *74* (5), 2249–2301.

**_ and Christopher Hansman**, "Selection, Leverage, and Default in the Mortgage Market," 2019.

**Hart, Oliver D and Bengt Holmström**, "The Theory of Contracts, in (Bewley, T. eds.) Advances in Economic Theory: Fifth World Congress," 1987.

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

**Herkenhoff, Kyle, Gordon Phillips, and Ethan Cohen-Cole**, "How Credit Constraints Impact Job Finding Rates, Sorting & Aggregate Output," 2019.

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

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

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

**_ , Devin G. Pope, and Jaren C. Pope**, "Failure to Refinance," *Journal of Financial Economics*, 2016, *122*, 482–499.

**Kleiner, Kristoph, Noah Stoffman, and Scott E. Yonker**, "Friends with Bankruptcy Protection," *Journal of Financial Economics (forthcoming)*, 2019.

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

**Lupica, Lois R.**, "The Consumer Bankruptcy Fee Study," *American Bankruptcy Institute Law Review*, 2012, *20*, 17.

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

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

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

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

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

**Nakajima, Makoto and José-Víctor Ríos-Rull**, "Credit, Bankruptcy, and Aggregate Fluctuations," 2019.

**Pattison, Nathaniel**, "Consumption Smoothing and Debtor Protections," 2019.

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

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

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

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

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

**White, Michelle J.**, "Why Don't More Households File for Bankruptcy?," *Journal of Law, Economics, & Organization*, 1998, *14* (2), 205–231.

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

**Zame, William R.**, "Efficiency and the Role of Default When Security Markets are Incomplete," *American Economic Review*, 1993, *83* (5), 1142–1164.

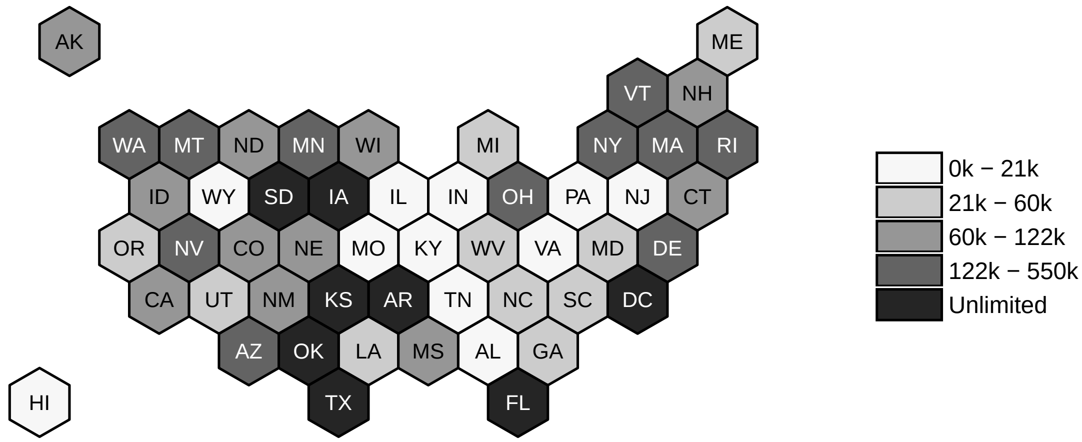
# Moral Hazard Versus Liquidity in Household Bankruptcy Appendix (for online publication)

**Sasha Indarte**

## Contents

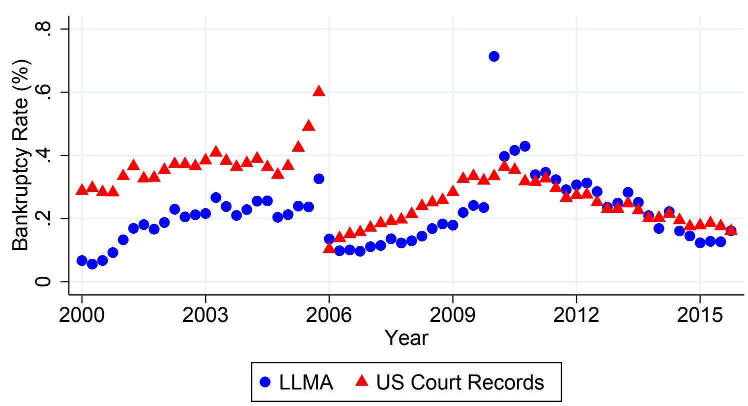
## A Additional Figures and Tables

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




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

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




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

**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 |

*Notes:* 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).

**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. | 20.71 | 18.52 | 17.16 | 16.49 |
| 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 | | | | ✓ |

*Notes:* 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)* | | | |
| $\text{MPay}_{ic}$ | 30.72*** | 27.49*** | 33.49*** | 29.98*** |
| | (7.35) | (7.64) | (8.46) | (8.48) |
| $\text{IndexRate}_{ic}$ | 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.)* | | | |
| $\text{MPay}_{ic}$ | 4.93*** | 4.41*** | 5.38*** | 4.81*** |
| | (1.18) | (1.23) | (1.36) | (1.36) |
| $\text{IndexRate}_{ic}$ | 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.)* | | | |
| $\text{MPay}_{ic}$ | 73.58*** | 68.56*** | 92.38*** | 78.45*** |
| | (18.46) | (20.09) | (21.01) | (21.64) |
| $\text{IndexRate}_{ic}$ | 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.)* | | | |
| $\text{MPay}_{ic}$ | 11.81*** | 11.01*** | 14.83*** | 12.59*** |
| | (2.96) | (3.22) | (3.37) | (3.47) |
| $\text{IndexRate}_{ic}$ | 4,866.52*** | 4,671.64*** | 5,136.90*** | 5,179.40*** |
| | (287.82) | (312.31) | (361.54) | (367.44) |
| Stage 1 F-Stat. | 40.84 | 31.97 | 28.84 | 28.38 |
| 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 | | | | ✓ |

*Notes:* 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, Lee, Pei and Weber (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 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 $\varepsilon$. 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 $\varepsilon$ (i.e., allowing $\mathbb{E}(x\varepsilon) \neq 0$).

**Remark 1.** *The parameter $\beta_1^s$ 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 $\beta_1^s$ 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*

$$\tau^{PRK} = \frac{\beta_1^+ - \beta_1^-}{\gamma^+ - \gamma^-}. \tag{15}$$

**Remark 2.** *The parametric RK estimand $\tau^{PRK}$ 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, Lee, Pei and Weber (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, e.g., $\mathbf{x} = (1, x, x^2)'$ 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 $\gamma^+$ and $\gamma^-$ 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, i.e.:*

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

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

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

*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

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

□

#### 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 $\mu$ 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, i.e.:*

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

*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*

$$\pi^+ = P(x_\star < 0 | x \geq 0)$$
$$\pi^- = P(x_\star \geq 0 | x < 0).$$

*Proof.* First, note that the above-the-cutoff estimator is now no longer consistent for $\beta^+$ 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 $\beta_1^+$ gets closer to $\beta_1^-$.*

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), assume that the distribution of measurement error $\mu$ 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 $\varepsilon$. Specifically:

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

Treatment occurs when the running variable crosses zero, i.e.

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

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

**Remark 5.** *The parameter $\beta_1^T$ 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, Cattaneo and Titiunik, 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 $\beta_1^T$ 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*

$$\tau^{PRD} = \beta_0^+ - \beta_0^-. \tag{16}$$

**Remark 6.** *The parametric RD estimand $\tau^{PRK}$ 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, e.g., $\mathbf{x} = (1, x)'$ 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, i.e.:*

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

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

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

*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 $\mu$ is zero mean noise (measurement error) and $\mathbb{E}(x_{\star}\mu)=\mathbb{E}(\varepsilon\mu)=0$. I.e., 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 mismeasured 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, i.e.:*

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

*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 $\beta^{+}$ 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

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

□

**Remark 7.** *In contrast to the parametric RK estimator, measurement error biases the parametric RD estimator through 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.

### C.1.1 Subset Used for Bankruptcy Generosity Analysis

To obtain the main sample used in the analysis of bankruptcy generosity, I make a number of restrictions on the full sample. Table C.1 below gives counts of observations, households (HHs), and bankruptcies after making various restrictions. The starting point is the full sample spanning January 2000 to March 2016.

**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 |

*Notes:* 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.

**Reasons for Sample Restrictions**

I first restrict to states that do not permit doubling. Because I cannot identify with certainty if households are married and therefore eligible to double their homestead exemption, this restriction removes any uncertainty about what is the relevant exemption limit facing the household. These states include Alaska, Arizona, Colorado, Idaho, Louisiana, Massachusetts, Minnesota, Missouri, Mississippi, North Dakota, Nebraska, Rhode Island, Vermont, and Washington.

I use data from Wisconsin up to 2008 Q4 because in the following quarter Wisconsin allowed filers to use federal exemptions. The federal homestead exemption was more generous for married filers in Wisconsin. Wisconsin allows households to file using federal exemptions; and beginning in 2009 the federal homestead .In 2010, Wisconsin also began to allow married households to double their homestead exemption. I also use data on Delaware and Maryland beginning in 2005 Q1 and 2010 Q4 (respectively) 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 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.

## C.4 Other Data

### County-Level Economic Data

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

### IRS SOI Data

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

### CPI Data

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

# D Empirical Analysis: Robustness and Additional Results

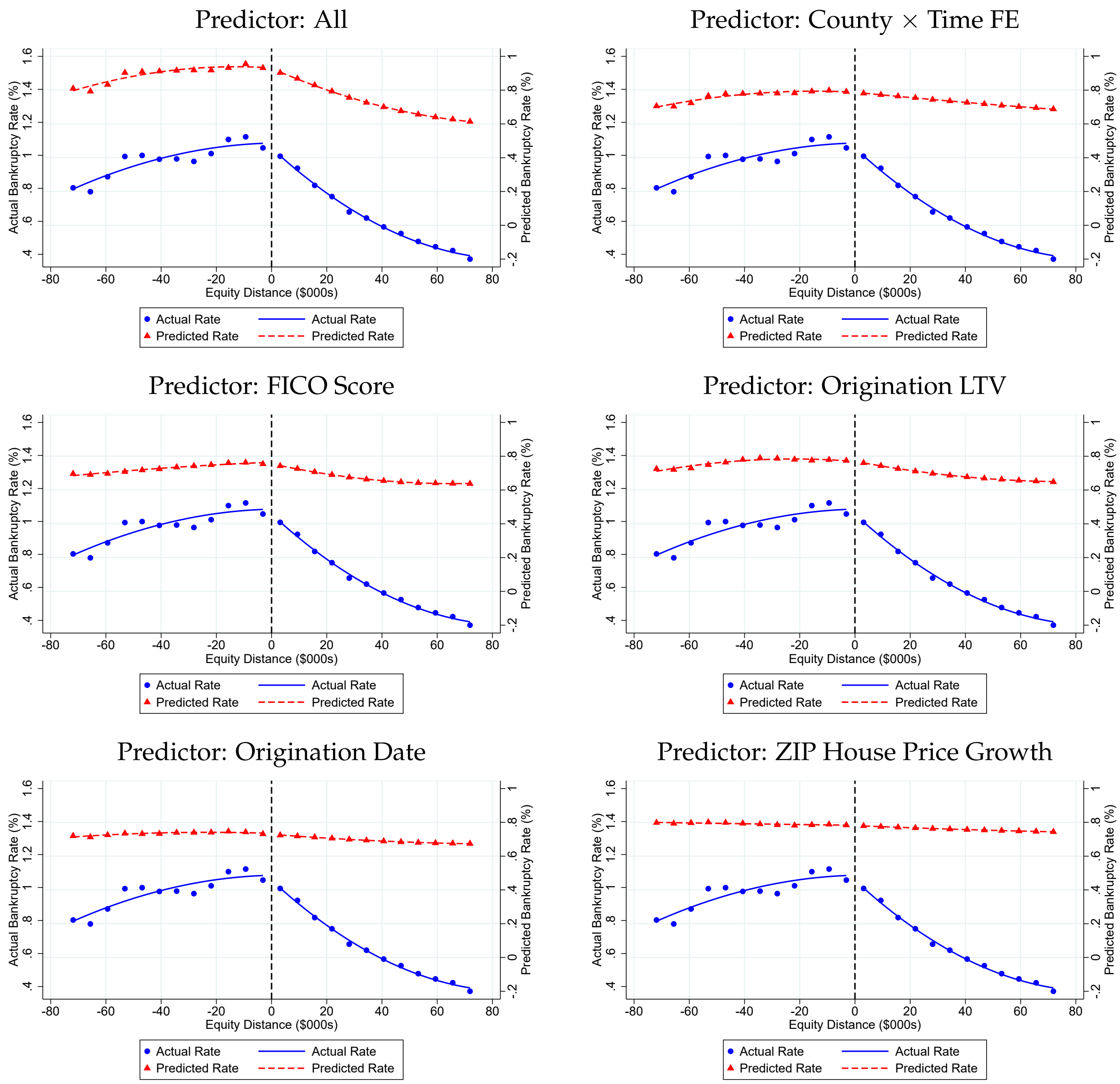
## D.1 RKD: Testing for Failure of Smooth Density Assumption

**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 | $\Delta$ ln(HP) | FIPS $\times$ Time FE |
| | *Panel B: 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 |

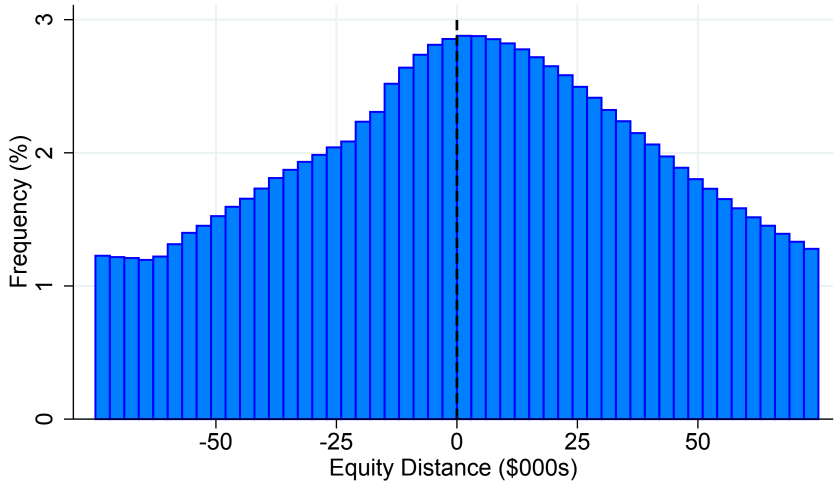
*Notes:* This table gives results from tests for jumps and kinks in predetermined covariates. The column indicates the covariate or group of covariates. For each test column, I compute a predicted bankruptcy rate for each household using a linear probability model, where the predictors are the covariates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




*Notes:* These figures plot the actual and predicted mean quarterly filing rates for equity distance bins and polynomial estimates of the rates for both sides of the cutoff. Each graph reproduces the same plot for the actual filing rate (the solid blue line and circles). The red dashed line and points are for the predicted filing rate one obtains from fitting a linear probability model to a specified covariate(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.

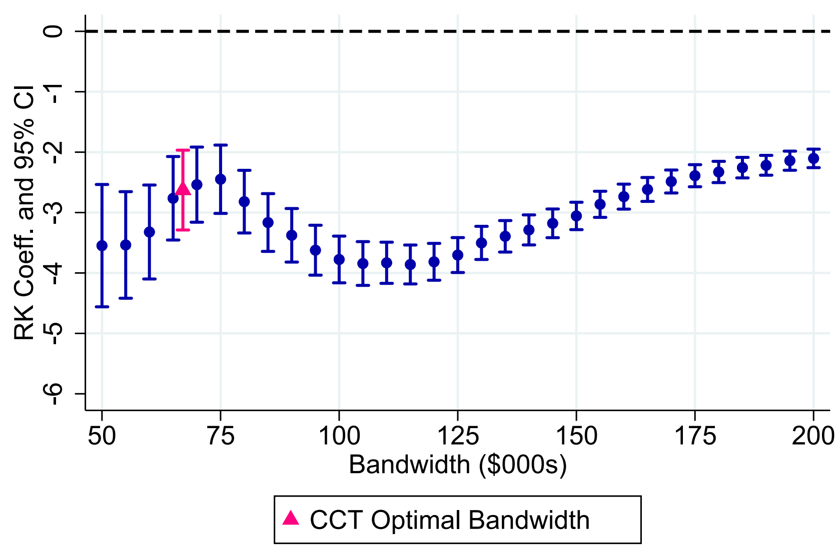
**Figure D.2:** Empirical Distribution of Equity Distance




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

## D.2 RKD: Alternative Bandwidths

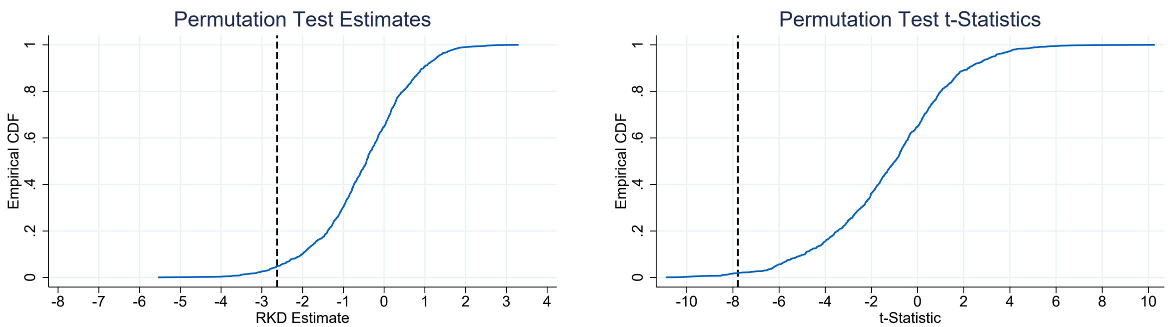
**Figure D.3:** RKD Estimate under Alternative Bandwidths




*Notes:* 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, Cattaneo and Titiunik (2014).

## D.3 RKD: Permutation Test

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




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

## D.4 RKD: Heterogeneity

**Table D.2:** Time Variation in Filing Sensitivity to Bankruptcy Cost

| | (1)<br>**Pre-Rec.** | (2)<br>**Recession** | (3)<br>**Post-Rec.** | (4)<br>**Pre-Reform** | (5)<br>**Rush to File** | (6)<br>**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 |

*Notes:* 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 |

*Notes:* 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, Cattaneo and Titiunik (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) | |
| $\Delta$ 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 |

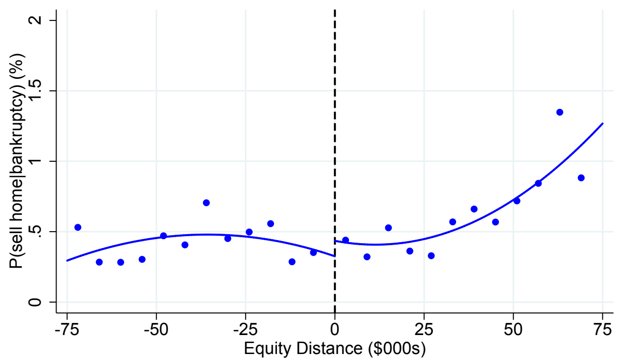
*Notes:* The estimates correspond to the percent change in filing rate, relative to the sample average of 0.71, in response to a $1,000 increase in seizable home equity. The table above reports results from estimating the RKD using OLS and interaction terms (i.e., interacting distance from the cutoff 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, Cattaneo and Titiunik (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***.

## 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.5 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.

**Figure D.5:** Probability of Sale versus Equity Distance




*Notes:* This graph plots binned averages of the quarterly 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.

**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 |

*Notes:* 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, Cattaneo and Titiunik (2014). I scale the coefficient and standard errors to be in terms of annual percentage points. Statistical significance: 0.05*, 0.01**, and 0.01***.

## D.6 ARM IV: Placebo Test

**Table D.6:** Placebo Test – Does Filing Vary with Index Rate Choice Prior to Reset?

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| $\text{Libor}_{ic}$ | 3.93 | 0.25 | -0.53 | 3.70 |
| | (10.93) | (10.67) | (10.81) | (11.04) |
| $\text{Margin}_{ic}$ | -8.06 | -8.78 | -11.48 | -2.51 |
| | (11.33) | (11.37) | (11.58) | (10.72) |
| $\text{Old Pay}_{ic}$ | -2.13** | -1.95** | -1.94** | -1.91* |
| | (0.75) | (0.74) | (0.74) | (0.76) |
| $\text{Orig. FICO}_{ic}$ | -0.78*** | -0.78*** | -0.77*** | -0.80*** |
| | (0.10) | (0.10) | (0.10) | (0.10) |
| $\text{Orig. LTV}_{ic}$ | 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 | | | | ✓ |

*Notes:* The sample used in these regressions is monthly data on bankruptcy filings in the year *prior* to an ARM reset. All regressions include the same household-level controls as in the main IV specification (Table 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.7 ARM IV: Testing for Predictors of Libor Indexation

**Table D.7:** Testing for Predictors of Libor Indexation

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| $\text{Margin}_{ic}$ | -35.78*** | -36.43*** | -39.30*** | -43.49*** |
| | (1.72) | (1.76) | (2.04) | (2.50) |
| $\text{Old Pay}_{ic}$ | 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) |
| $\text{UR}_{ic}$ % | -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 $\times$ Time FE | | | ✓ | ✓ |
| County $\times$ Time FE | | | | ✓ |

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

## D.8 ARM IV: Testing for Anticipatory Behavior

**Table D.8:** Anticipatory Behavior – Does Filing Vary with Index Rate Prior to Reset?

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| $\text{IndexRate}_{ic}$ | -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 |
| $\text{Margin}_{ic}$ | (13.12) | (13.65) | (16.06) | (6.60) |
| | (11.42) | (11.48) | (11.62) | (10.91) |
| Old $\text{Pay}_{ic}$ | -2.13** | -1.95** | -1.94** | -1.90* |
| | (0.75) | (0.75) | (0.74) | (0.76) |
| Orig. $\text{FICO}_{ic}$ | -0.78*** | -0.78*** | -0.77*** | -0.80*** |
| | (0.10) | (0.10) | (0.10) | (0.10) |
| Orig. $\text{LTV}_{ic}$ | 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 | | | | ✓ |

*Notes:* The sample used in these regressions is monthly data on bankruptcy filings in the year *prior* to an ARM reset. All regressions include the same household-level controls as in the main IV specification (Table 2). Standard errors are clustered by county. I scale coefficient and standard errors on the $\text{IndexRate}_{ict}$ 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.9 ARM IV: OLS Version

**Table D.9:** OLS Estimation

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| $\text{MPay}_{ic}$ | 3.03 | 2.49 | 2.46 | 2.35 |
| | (1.87) | (1.91) | (1.95) | (2.17) |
| $\text{Margin}_{ic}$ | 13.40 | 11.63 | 6.06 | 4.90 |
| | (10.90) | (11.06) | (11.66) | (13.04) |
| $\text{Old Pay}_{ic}$ | -1.81*** | -1.55** | -1.64** | -1.56** |
| | (0.51) | (0.51) | (0.52) | (0.54) |
| $\text{Orig. FICO}_{ic}$ | -0.78*** | -0.77*** | -0.77*** | -0.79*** |
| | (0.08) | (0.08) | (0.08) | (0.08) |
| $\text{Orig. LTV}_{ic}$ | 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 | | | | ✓ |

*Notes:* 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.10 RKD and ARM IV Estimates on Overlapping Subsample

**Table D.10:** 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 | – | – |
| $\text{IndexRate}_{ic}$ | – | 956.01 | 5,946.38 |
| Std. Err. | – | (148.82) | (925.66) |
| Stage 1 F-Stat. | – | 5.90 | 5.90 |
| Observations | – | 86,256 | 86,256 |

*Notes:* 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 (point estimates and standard errors available by request). Standard errors for the RKD estimates are calculated using the method of Calonico, Cattaneo and Titiunik (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

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

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

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

The 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

$$V_t^B = V_t^N(y_t^\star, d_t). \tag{17}$$

**Comparative Statics:** Here I examine the effects on the initial ($t = 1$) probability of filing of both a one-time and permanent shocks. First, consider a marginal change in either the initial exemption level $e_1$ or initial non-seizable cash flows $a_1$. Implicitly differentiating the indifference condition in equation (17), 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

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

where

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

Implicitly differentiating the indifference condition yields

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

and

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

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

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

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

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

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

$$\frac{-\partial p_1/\partial a_1}{\partial p_1/\partial_1 e} = \frac{u'(c_1^{N\star})}{u'(c_1^B)} - 1. \tag{18}$$

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.[42]

The comparative statics for one-time changes in $e_1$ and $a_1$ are little-changed by incorporating credit market exclusion. The only difference is that the filing rate $p_1$ is now conditional on not currently being in autarky.[43] 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

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

[42] 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).

[43] 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 ($\gamma$). 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

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

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

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

Therefore:

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

Given the 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 $q^B = 0$, i.e. 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 for 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.