Misperceptions and Ordeals in Consumer Bankruptcy*

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Abstract

Chapter 7 bankruptcy offers generous debt relief. The average filer discharges $115,000 of debt, and only 7% of filers forfeit any assets. Even so, most eligible debtors never file. We show that prospective filers systematically underestimate bankruptcy's financial benefits, amplifying the deterrent effects of small filing barriers. We analyze a novel dataset of 29,686 debtors who have completed their bankruptcy paperwork. Only 49% ultimately file, and nonfilers forgo an average of $42,709 of debt relief. We show that one small barrier, the modest $338 filing fee, meaningfully deters filings. In a regression discontinuity at the income threshold for fee waiver eligibility, the fee reduces filings by 11.2 percentage points (22% of the mean). This sensitivity is difficult to explain with rational models, as RD estimates are similar for debtors with varying financial benefits and liquid assets. In a survey of high-debt individuals, we find that misperceptions can explain this pattern. Respondents underestimate bankruptcy's generosity and overestimate its damage to their credit score. Correcting these misperceptions in a randomized information provision experiment causes participants to take action toward filing for bankruptcy. Finally, we show that these bankruptcies create real benefits. Compared to observably similar nonfilers, Chapter 7 filers subsequently have higher credit scores, greater credit access, fewer delinquent debts, and lower mortality rates.

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1 Introduction

Chapter 7 of the US bankruptcy code offers debtors a uniquely generous "fresh start." Chapter 7 filers discharge $115,000 of debt on average, and only 7% are required to surrender personal assets to repay creditors (US Courts, 2023). Despite these generous terms, only 0.1% of US adults file each year.[1] Given that 47% of US adults experience daily stress about their debts,[2] why do so few file for bankruptcy?

In this paper, we provide a novel explanation for this puzzle: debtors substantially underestimate the generosity of Chapter 7 and overestimate the negative credit-score consequences of filing. These misperceptions amplify the barriers embedded in the bankruptcy code that deter filing. As a result, debt relief is misallocated toward individuals who understand the system and can afford its fees, rather than those who would benefit most.

To support this explanation, we combine evidence from two settings: a new dataset of prospective bankruptcy filers and a randomized information provision experiment of high-debt individuals. Our dataset reveals that even highly motivated debtors, who were determined to be good candidates for Chapter 7 and completed *all* necessary filing forms to file, are meaningfully deterred by small ordeals: only 49% eventually file, and the $338 filing fee causally reduces filings by 11.2 percentage points (22% of the baseline filing rate). To understand this extraordinary sensitivity to filing barriers, we field a survey of high-debt individuals. Our survey reveals that debtors' concerns about bankruptcy center on financial consequences: their primary worries are failing to discharge their debts, losing assets, and damaging their future access to credit. We document that all three concerns reflect systematic misperceptions. Respondents are incorrectly pessimistic about the financial consequences experienced by actual Chapter 7 filers. Randomly providing corrective information causes individuals to take real-world actions toward filing for bankruptcy. Thus, misperceptions amplify the impact of filing barriers.

We begin by studying a novel dataset of 29,686 prospective bankruptcy filers from Upsolve, a nonprofit that provides free software to prepare Chapter 7 filings. Upsolve users have discharged

[1] In the 12 months preceding June 2025, 320,007 individuals filed for Chapter 7 bankruptcy. See https://www.uscourts.gov/data-news/data-tables/2025/06/30/bankruptcy-filings/f-2. The U.S. adult population at the same time was roughly 268 million, see https://www.census.gov/quickfacts/fact/table/US/PST040225#PST040225.

[2] See https://www.cbsnews.com/news/nearly-50-of-borrowers-stress-about-debts-daily-what-to-do-about-yours-now/. The *average* household below the poverty line has over $28,000 in dischargeable debt. See Table 4 at https://www.census.gov/data/tables/2022/demo/wealth/wealth-asset-ownership.html.

$1.09 billion of debt since 2018 (Upsolve, 2026) and accounted for 16% of all *pro se* filers since 2021.[3] Relative to existing sources, these data offer three key advantages. First, we observe *complete* Chapter 7 petitions for both 14,593 filers and 15,093 *prospective filers who did not file*, providing a unique view of deterred individuals. Second, Upsolve users are highly likely to benefit from bankruptcy: users are screened for eligibility and fit, so the average user could discharge $46,187 in debt. Third, the questionnaire captures unusually detailed information on debts, assets, income, and expenses. Through a partnership with Upsolve, we augment these data with a survey on users' financial circumstances and reasons for considering bankruptcy.

Why do 51% of Upsolve users never file? We show that even these motivated debtors are deterred by small ordeals. Filers with incomes above 150% of the Federal Poverty Level (FPL) must pay a $338 filing fee, while those below are eligible for a waiver.[4] We exploit this eligibility threshold in an instrumental variables–regression discontinuity (IV-RD) design to identify the causal effect of the fee on filing rates. The fee reduces filings by 11.2 percentage points (22%), with a p-value below 0.001. The specification has a strong first stage: individuals below the cutoff are 88 percentage points less likely to pay the fee. We find no evidence of bunching or differences in filers' characteristics around the threshold, and the estimates are robust to the choice of controls, bandwidth, kernel weights, and functional form. Estimates are larger at narrower bandwidths, providing further support for the exclusion restriction assumption.

Subsample tests do not support two obvious explanations for this result. First, the filing fee could serve as an effective screen by deterring marginal filers with lower financial benefits from filing. This rational explanation would imply smaller RD estimates among individuals with higher financial benefits. We find the opposite: debtors with an above-median financial benefit of filing have RD estimates 33% larger than below-median debtors, though both are significant at the 1% level. Second, individuals deterred by the fee may place an extraordinarily high value on preserving liquidity. While liquidity is evidently an important consideration for prospective filers (Gross et al., 2014; Argyle et al., 2021; Indarte, 2023), it cannot explain our results on its own. The RD estimates are remarkably similar among individuals with high and low liquid assets, and RD estimates for both groups are significant with p-values below 0.001. While this result is particularly surprising, it

[3] A *pro se* filer is one who forgoes hiring a lawyer. Between the third quarter of 2021 and the first quarter of 2026, there were 89,067 *pro se* filings in the Federal Judicial Center database, with 14,519 from Upsolve.

[4] Debtors subject to the fee can elect to pay in installments over 120 days after the filing date (US Courts, 2024a).

likely reflects the ability of filers to pay the fee in installments and the small size of the fee relative to income and the cost of servicing outstanding debt.[5] Taken together, these rational explanations appear insufficient: some additional reluctance toward filing is needed to explain the outsized effect of this small ordeal.

We argue that this reluctance stems from misperceptions about bankruptcy. In the second part of the paper, we explore debtors' concerns and beliefs about filing by partnering with a credit reporting agency to survey 245 high-debt individuals who could benefit from bankruptcy. We first ask participants directly what concerns would deter them from filing. Contrary to conventional wisdom and earlier work (Gross and Souleles, 2002; Athreya, 2004), only 5% of individuals cite stigma as a primary deterrent. Instead, 46% worry that a bankruptcy will either fail to erase debt or require them to surrender assets. Another 29% are primarily concerned that filing will impair their future access to credit.

We show that these stated concerns reflect large misperceptions. Official court records reveal that Chapter 7 filers fare far better than these concerns would suggest: 96% discharge all eligible debt, only 7% surrender assets, and the average filer's credit score increases by 80 points in the year after filing (Jagtiani and Li, 2015). When we elicit participants' beliefs about these outcomes, we confirm that they are systematically too pessimistic. The median respondent underestimates the discharge rate by 46 percentage points, overestimates the rate of asset surrender by 43 percentage points, and expects a 33 point credit score decline rather than an 80 point increase. Thus, respondents' top concerns about filing reflect misperceptions rather than accurate assessments of bankruptcy's financial consequences.

Next, in a preregistered randomized controlled trial (RCT), we provide causal evidence that these misperceptions deter filings. After measuring bankruptcy knowledge, we randomly assign participants to one of four groups. A control group receives a placebo fact about labor markets. A "credit access" group learns the true average impact of filing on credit scores (Jagtiani and Li, 2015). A "net worth" group learns the true share of filers who successfully discharge debts and the share who surrender assets. A "combined" group learns all of the bankruptcy facts mentioned above.

[5] 58% of filers subject to the fee choose the installment option. Waiver-ineligible nonfilers are servicing an average of $72,221 of debt.

We find that correcting misperceptions increases interest in filing. At the end of our survey, treated participants are 14 to 25 percentage points more likely to report willingness to consider bankruptcy in the next year (57% to 102% of the control-group mean). Approximately 13% of treated participants click on a link to bankruptcy information at the end of the survey. Additionally, we use an incentive-compatible mechanism to measure willingness to pay for bankruptcy information: participants enter a lottery and choose whether they prefer a lottery prize of $30 or a prize involving further information about bankruptcy. Our treatment increases willingness to pay for bankruptcy information (i.e., willingness to forgo $30) by 16 percentage points (76%). The net worth treatment is more impactful than the credit access treatment.

A follow-up survey administered two months after the initial survey shows that these effects persist. Treatment persistently reduces misperceptions and increases interest in bankruptcy. Notably, the net worth treatment has a statistically significant effect on real-world action: treated participants are more likely to have formed a plan to file or begun filling out bankruptcy forms.

Finally, we show that debtors deterred from filing forgo real benefits. We match 35,898 Upsolve users to a panel of credit records and compare filers to nonfilers who completed the same filing paperwork, pairing each filer with the nonfiler most similarly likely to file given their pre-filing characteristics and trends. Filers' credit scores subsequently rise and the credit-access costs that debtors fear are short-lived: filers' card holding and credit limits contract sharply in the filing year as accounts are closed, but one year later filers are 14 percentage points *more* likely than nonfilers to hold a credit card, and within two years their credit limits fully recover. Auto loan balances are $1,187 higher three years after filing. Filers have persistently lower delinquent debt and, consistent with the random-judge Chapter 13 estimates of Dobbie and Song (2015), filers also have lower mortality and higher imputed income. Matched filers and nonfilers trend in parallel in the years before filing for nearly every outcome we study, supporting the parallel-trends assumption that underlies the design.

In sum, our results demonstrate that negative misperceptions about bankruptcy amplify the deterrent effect of small ordeals, preventing filings that would have created real benefits. These findings carry direct implications for policy makers. For any fixed amount of debt forgiveness the system offers, misperceptions reduce welfare by distorting the allocation of debt relief, directing it toward those who understand bankruptcy rather than those who would benefit most. Our results suggest that legislative proposals to reduce ordeals, such as the 2022 Consumer Bankruptcy Reform Act, could expand access and improve the targeting of debt relief, with larger effects if paired with clearer communication about the consequences of filing.

Contributions to the Literature. Our results bring new evidence to three distinct literatures.

First, we contribute to the literature on the consumer bankruptcy filing decision. White (1998) estimates that 15% of households would increase their net worth by filing. This finding has motivated a body of work that aims to explain why so few of these households file. Early work cites stigma (Brinig and Buckley, 1998; Gross and Souleles, 2002; Fay et al., 2002; Athreya, 2004) and a desire to retain the option to file later (White, 1998; Zhu et al., 2019) as deterrents. Our survey respondents report worrying less about these concerns than about the financial consequences of bankruptcy. Stigma could nonetheless contribute to misperceptions by causing debtors to underinvest in learning about bankruptcy through, for example, the "ostrich effect" (Olafsson and Pagel, 2025). Other papers in this literature argue that the risk of asset forfeiture, often studied through the lens of asset exemptions (e.g., Gropp et al., 1997; Mahoney, 2015; Severino et al., Forthcoming), discourages filings. Macroeconomic models of consumer bankruptcy focus on the role of future credit access (Athreya, 2002; Livshits et al., 2007; Chatterjee et al., 2007; Athreya et al., 2012), as does the empirical literature on post-bankruptcy borrowing (Musto, 2004; Han and Li, 2011). Our survey confirms that debtors worry about future credit access and asset forfeiture, but shows that they systematically misperceive these financial consequences as they pertain to Chapter 7 filers. Our filing-fee RD results confirm a pivotal role for liquidity constraints (Gross et al., 2014; Argyle et al., 2021; Indarte, 2023), but the deterrent effect of the filing fee persists even for those with above-median liquid assets and large financial benefits of filing, implying that liquidity constraints on their own are insufficient. Our RCT results, which document the presence of large misperceptions and show that correcting them leads to increased interest in filing, can reconcile these complementary stories. Misperceptions also provide a mechanism explaining the effects of lawyer advertising (Lee, Forthcoming) and imply that the "non-monetary and/or non-immediate" costs of filing posited by Indarte (2023) could instead reflect misperceived monetary costs.

We also contribute the new evidence on the Chapter 7 benefits nonfilers forgo. There is good evidence on the effects of Chapter 13, where Dobbie and Song (2015) and Dobbie et al. (2017)

exploit random assignment to judges and show that marginal filers have higher earnings, better credit, and lower mortality relative to nonfilers. It is unclear whether these effects translate to Chapter 7, which offers more generous relief to worse-off filers. There is limited direct evidence on the effects of Chapter 7, which accounts for the majority of filings. Jagtiani and Li (2015) traces filer outcomes in credit report data, and Parra (2022) compares Chapter 7 filers to Chapter 13 filers. Our matched nonfilers, who completed the same filing paperwork as filers, provide a novel counterfactual and generate the most credible evidence on the effects of filing Chapter 7.

Second, we contribute to the broader literature on how program design affects take-up and the targeting of social-insurance policies. Consumer bankruptcy is an unusual but high-stakes form of social insurance, with costs borne by other borrowers rather than taxpayers (Gross et al., 2021) and access restricted by ordeals such as the means test, mandatory education courses, and upfront filing costs. The canonical argument for such ordeals is that they improve targeting by inducing self-selection among potential beneficiaries (Nichols and Zeckhauser, 1982; Kleven and Kopczuk, 2011). Rafkin et al. (Forthcoming) estimate that this self-targeting generates net social benefits across major US transfer programs. Empirical evidence is otherwise mixed. Finkelstein and Notowidigdo (2019) find that marginal SNAP enrollees brought in by informational outreach have smaller benefits than infra-marginal enrollees, and Shepard and Wagner (2025) show that removing ordeals in health insurance brings in higher-cost enrollees. In contrast, information and assistance bring in higher-value participants in college financial aid (Bettinger et al., 2012), the Earned Income Tax Credit (Bhargava and Manoli, 2015; Linos et al., 2022), disability insurance (Deshpande and Li, 2019), and retirement savings (Goda, 2024). The 2005 bankruptcy reform (BAPCPA) increased the costs and ordeals involved in filing in an effort to deter "abusive" filers. Consistent with broader disagreement over the targeting value of bankruptcy ordeals, structural models reach mixed conclusions about the 2005 bankruptcy reform: Chatterjee et al. (2007) find large welfare gains from introducing means testing, Li and Sarte (2006) find that means testing would not improve upon existing bankruptcy provisions, and Mitman (2016) finds that BAPCPA reduced bankruptcy but increased foreclosures when house prices fell and generated only modest welfare gains. Empirically, Gross et al. (2021) find that the reform sharply reduced filings without meaningfully changing the income distribution of filers. We contribute to this literature with causal evidence on a small but representative ordeal. We find that the $338 filing fee substantially reduces filings, with no targeting benefit—if anything, it appears to more strongly deter debtors with the largest financial benefits and fewest assets. Our companion evidence on debtor misperceptions provides the mechanism: when debtors misperceive the private value of filing, even small ordeals are disconnected from their self-targeting function and instead target on knowledge of the policy.

Third, we contribute to a growing literature documenting that consumer financial decisions reflect inaccurate beliefs and non-rational expectations about future outcomes. For example, beliefs about inflation shape spending (D'Acunto et al., 2021, 2022), misperceived interest costs shape borrowing (Stango and Zinman, 2009), beliefs about student loan forgiveness shape repayment (Koustas et al., 2026), and deviations from full-information rational expectations shape consumption decisions (Indarte et al., 2026). While Bernstein et al. (2023) document inaccurate beliefs among small-business owners considering corporate bankruptcy, and Antill and Hunter (2026) show that consumers underestimate corporate-bankruptcy survival rates, no analogous evidence exists for consumer bankruptcy. We show that high-debt consumers are systematically too pessimistic about Chapter 7 outcomes and that correcting their misperceptions shifts behavior toward filing.

2 Setting

U.S. Chapter 7. The U.S. bankruptcy code provides two paths to personal debt relief. Chapter 7, the "fresh start" option and focus of this paper, accounts for 58% of consumer bankruptcies (US Courts, 2023). Under Chapter 7, filers discharge eligible debts, forfeit non-exempt assets to creditors, and retain all of their post-bankruptcy income. By contrast, Chapter 13 filers retain their assets but must pay all their disposable income to creditors under a court-approved repayment plan for up to five years. Only about half successfully complete their repayment plan (Argyle et al., 2025). Eligible debts typically include credit card debt, medical debt, utility bills, auto loans, personal loans, and payday loans. Student loans, recent income taxes, and domestic support obligations are typically non-dischargeable, and secured debts are not discharged if the underlying collateral is retained. Asset exemptions, specified both federally and by state, protect certain assets from forfeiture. The most important are the homestead exemption, which covers the filer's primary residence, the motor vehicle exemption, and the "wildcard" that covers some otherwise non-exempt property.[6]

These provisions make Chapter 7 an unusually generous form of debt relief. Among consumer Chapter 7 filers, 96% obtain a discharge and only 7% surrender any assets to the trustee (Antill, 2024). The average filer discharges $115,000 of debt (US Courts, 2023) and, as a result, sees their credit score rise by 80 points in the year after filing (Jagtiani and Li, 2015).

Access to filing Chapter 7 is governed by the 2005 Bankruptcy Abuse Prevention and Consumer Protection Act (BAPCPA), which was passed to "protect those who legitimately need help" while stopping "those who try to commit fraud" (Bush, 2005). BAPCPA imposed several new ordeals debtors must navigate in order to file. While BAPCPA persistently reduced the filing rate by 50%, it is unclear whether it achieved its aims of improving the targeting of debt relief (Gross et al., 2021).

Ordeals and the Filing Process. In order to file, a prospective filer must first pass the "means test," which prohibits high-income individuals from filing for Chapter 7. Next, the filer must complete the required forms, which include: (1) a voluntary petition which initiates the bankruptcy filing; (2) schedules of assets, liabilities, income, and expenses; (3) a statement of financial affairs which covers any legal proceedings, property transfers, and other relevant financial activities; and (4) a statement of current monthly income used to confirm eligibility under the means test (US Courts, 2024c). Third, the filer must pay the upfront costs of filing: (1) approximately $20 for a mandatory pre-filing credit counseling course,[7] (2) the court fee of $338, and (3) attorney fees of approximately $2,000 if the debtor chooses to hire an attorney (O'Neill, 2023). The Chapter 7 court fee is constant throughout our study period and was last adjusted in December 2020, increasing from $335 to $338 (Rao, 2020). Attorney fees must be paid upfront for Chapter 7 filings, while they can be amortized in the repayment plan for Chapter 13. Most filers work with an attorney, but 6.5% of filers file *pro se* (i.e., on one's own behalf) (Federal Judicial Center, 2024).

[6] Appendix Table A.1 lists the set of federal exemptions. We focus on non-homeowning debtors, for whom Chapter 7 is extremely likely to be the best option, in both the Upsolve data and survey experiment.

[7] BAPCPA requires filers to complete a pre-filing credit counseling course and a post-filing debtor education course. These courses cost approximately $15 to $25 each and are typically offered online through private providers. Federal law requires course providers to offer fee waivers or reductions for filers with incomes below 150% of the FPL. These course fee waivers introduce an additional discontinuity in upfront filing costs at 150% of the FPL; however, we do not observe whether filers apply for or receive these waivers.

The Filing Fee Waiver. Filers with incomes below 150% of the Federal Poverty Level (FPL) ($23,940 for a single household in 2026) are eligible to apply for a court fee waiver (US Courts, 2024b). Filers ineligible for the fee waiver may request to pay the $338 fee in up to four installments. The last installment must be paid within 120 days of the filing date (US Courts, 2024a).

3 Ordeals: Evidence from a Bankruptcy Filing Tool

3.1 Upsolve

We partner with Upsolve, a 501(c)(3) nonprofit organization that provides free online resources to help debtors in the United States understand their debt relief options, dispute or negotiate their debt, or file for personal bankruptcy. Most users arrive at Upsolve's website through web searches that lead them to their online resources, which include attorney-written articles on debt- and bankruptcy-related topics such as asset exemptions, debt collection practices, and wage garnishment.

Upsolve provides a "TurboTax for bankruptcy" tool that helps qualified users prepare their Chapter 7 bankruptcy filing forms without incurring the cost of an attorney. Access to the application is limited to debtors with comparatively simple bankruptcy cases, excluding debtors who own homes, plan to file jointly with a spouse, or are involved in personal injury lawsuits. The tool also does not serve users with high incomes that are likely to fail the means test. Upsolve instead connects these users with a bankruptcy attorney in their area. The application auto-populates users' Chapter 7 paperwork using their responses to a detailed questionnaire that covers sources of income, secured and unsecured debts, real and personal assets, expenses, and household characteristics.[8] At the end of the questionnaire, the application automatically assesses users' eligibility for the fee waiver and helps users complete the filing fee waiver application form. Upsolve users have discharged $1.09 billion of debt since 2018 (Upsolve, 2026).

Upsolve offers a valuable setting to study the bankruptcy filing decision. We observe all components of 29,686 users' filing forms, including asset and liability-level information. We also observe their demographics and motivations for filing, as measured in an intake survey. While all users

[8]To assist users with filling out the questionnaire, Upsolve pulls their most recent credit report and pre-populates some fields.

in our sample completed their filing paperwork, only 14,593 (49%) ultimately filed, allowing us to compare filers and nonfilers. We observe the same detailed information for both sets of users and can thus explore a large set of observable factors that influence the filing decision. Further, 10,526 users (35%) have incomes within 49.2 percentage points (the MSE-optimal bandwidth in our preferred specification) of the fee waiver eligibility threshold (150% FPL), which provides ample power for our regression discontinuity (RD) design. Upsolve filers comprise 16% of *pro se* Chapter 7 filers over our sample period (see Appendix Table A.2 and Appendix Figure A.1).

3.2 Data

We obtained deidentified data on 29,686 users who completed the bankruptcy filing forms using Upsolve's application between September 2021 and March 2026. We observe each field required to complete the forms including income, expenses, account-level assets and debts, and household structure. We also observe responses to an intake survey that asks users about: (i) their motivations for considering bankruptcy, (ii) the other actions they have taken to improve their financial situation, (iii) whether they would file in the absence of Upsolve, and (iv) basic demographic questions.[9] Upsolve tracks whether users applied for a fee waiver, whether they ultimately filed for bankruptcy, and whether filers received a discharge.[10]

Table 1 presents Upsolve user characteristics. The average Upsolve user is 42 years old, female (62.4%), and unmarried (78.2%). Nearly half (45.7%) have dependents and 30.1% receive some form of government benefits. Upsolve users are more likely to be Black (30.4%) than the national population. Filers tend to be older and are less likely to be married or have dependents. They are also more likely to receive government benefits. As shown in Appendix Table A.3, the most commonly cited reasons for considering bankruptcy are falling behind on bills (64.1%), irresponsible spending (37.4%), job loss (36.9%), and medical bills (24.1%).

Appendix Table A.2 compares the Upsolve sample to all Chapter 7 and Chapter 13 personal bankruptcy filers in the Federal Judicial Center (FJC) Integrated Database, which records all bankruptcy filings in the United States, during our sample period. Upsolve users have significantly

[9] In an effort to simplify their questionnaire, Upsolve stopped administering this survey in July 2025.

[10] We do not observe whether users ultimately received a filing fee waiver, or whether they applied for or received fee waivers for the pre-filing credit counseling or post-filing debtor education courses. Some providers automatically waive these fees if the US Bankruptcy Court waived the debtor's filing fee, while others require eligible debtors to complete a form requesting a fee waiver.

lower monthly income (\$2,085 vs. \$3,841) and assets (\$12,653 vs. \$91,168) on average than Chapter 7 filers overall (see Appendix Figure A.2 for a comparison of the income distributions). Upsolve users have less debt overall (\$75,512 vs. \$132,589) than typical Chapter 7 filers, but this difference is driven entirely by secured debt such as mortgages. Upsolve users have as much unsecured non-priority debt, which is the most easily dischargeable debt in bankruptcy, as typical Chapter 7 filers (\$63,026 vs. \$68,836). Compared to typical Chapter 7 filers, Upsolve filers are much more likely to apply for a fee waiver (57.6% of Upsolve filers applied for a waiver, while only 4.1% of all Chapter 7 filers received a waiver).

Prospective filers face extreme financial hardship. The average Upsolve user has \$75,512 in total debt, of which \$46,187 is likely dischargeable through bankruptcy. In other words, by filing for bankruptcy, the average Upsolve user could discharge an amount of debt equal to over two years of their income. Appendix Tables A.4 and A.5 provide more detailed breakdowns of Upsolve users' assets and liabilities, including an overview of available exemptions and debt dischargeability.

3.3 Empirical Framework

We use an RD design to estimate the causal effect of the \$338 filing fee on the decision to file for bankruptcy. Our design exploits the threshold for fee waiver eligibility—income equal to 150% of the FPL. At this threshold, we estimate the discontinuity in applications for the fee waiver (and thus exposure to the filing fee). The running variable (income as a percentage of the FPL) is recorded when Upsolve evaluates the prospective filer's eligibility for the fee waiver, and users are notified of their eligibility after completing the questionnaire. Applying for the fee waiver is optional, but Upsolve automatically screens users for eligibility and 95% of eligible users complete the paperwork to apply. For a small number of users, eligibility can change (e.g., if they wait to file and their income shifts above or below the threshold). To account for this imperfect compliance, we use a "fuzzy RD design": we instrument for fee waiver applications using an indicator for whether the user's reported income falls below 150% of the FPL.

We estimate a two-stage least squares regression with the following first and second stage equations:

$$W_i = \alpha_0 + \alpha_1 E_i + \alpha_2(FPL_i - 150) + \alpha_3 E_i \times (FPL_i - 150) + \alpha_4 X_i + \gamma_s + \delta_t + \varepsilon_i \tag{1}$$

$$F_i = \beta_0 + \beta_1 \hat{W}_i + \beta_2(FPL_i - 150) + \beta_3 E_i \times (FPL_i - 150) + \beta_4 X_i + \gamma_s + \delta_t + \varepsilon_i. \tag{2}$$

In these equations, i indexes users, FPL_i is income as a percentage of FPL, E_i is an indicator for waiver eligibility (determined by $\mathbf{1}[FPL_i < 150]$), and W_i is an indicator for fee waiver application. Our outcome variable is F_i, an indicator for filing for bankruptcy. The running variable, $(FPL_i - 150)$, represents the percentage-point distance from the 150% eligibility threshold. The vector X_i contains controls for user characteristics. We include year-month fixed effects (δ_t) and state fixed effects (γ_s). Our coefficient of interest is β_1, the effect of receiving the fee waiver ($\hat{W}_i$) on filing (F_i).

Our baseline specification assumes linear relationships above and below the threshold and uses uniform kernel weights. We control for the number of days the user spent on the questionnaire, the log of each category of debt, and the log of each category of assets. We use the mean squared error (MSE) optimal bandwidth, based on the algorithm from Calonico et al. (2020), which we calculate as 49.2 percentage points (100.8-199.2% FPL) under this specification and set of controls.

Our identifying assumption is that, absent the discontinuity in fee waiver eligibility, the filing rate and any relevant factors for the filing decision would trend smoothly through the discontinuity. We show three tests that support this assumption. First, we present the McCrary (2008) and Cattaneo et al. (2020) tests for manipulation of the running variable, which could suggest selection into eligibility for the fee waiver. We find no evidence of bunching around the threshold, which is visually evident in Figure 1 and confirmed by the tests presented in Appendix Figure A.3. We also assess whether covariates trend smoothly through the discontinuity by estimating equations (1) and (2) with each of the covariates listed above as the dependent variable. These results are shown in Appendix Table A.6. None of the covariates have statistically significant discontinuities at the threshold.

In Table 2 we sequentially add the controls for state and year-month fixed effects, debt, and assets, controlling for questionnaire completion time in all specifications (our baseline specification in column (4) includes all controls). In Appendix Table A.7, we also show robustness to alternate RD specifications. To evaluate the bias-variance tradeoff, we vary the bandwidth from 10 to 150 percentage points and apply triangular kernel weights, which are inversely proportional to the distance from the threshold. This bandwidth range includes the mean squared error (MSE) optimal bandwidth, based on the algorithm from Calonico et al. (2020), which we calculate as 49.2 and 34.1 percentage points when using uniform and triangular kernels, respectively. The results are remarkably stable and stronger for narrower bandwidths, providing further evidence that the exclusion restriction holds.

3.4 Results

Effects of the Filing Fee. Figure 1 presents the first- and second-stage estimates from our baseline RD specification. Panel A presents the distribution of income, with the colors of the bars indicating who opted to apply for a fee waiver (red), apply to pay the fee in installments (blue), or pay the fee in full (green). Panel B plots the first-stage effect of income on application for the filing fee waiver, as estimated in equation (1). Users just below the threshold are 88.4 percentage points more likely to apply for the fee waiver than those just above the threshold. The majority of waiver-ineligible filers within the MSE-optimal bandwidth apply to pay the filing fee in installments rather than upfront (65%), which should partially remediate the liquidity barrier to filing for those above the threshold (as in Kluender, 2024). As a result, we will understate the impacts of the $338 filing fee on the filing rate relative to a world in which the fee must be paid in full by everyone who does not receive a waiver.

Panel C of Figure 1 presents the second-stage estimates of the fee waiver on bankruptcy filing, as estimated in equation (2). We observe a positive relationship between income and filing both above and below the fee waiver eligibility threshold, and a clear discontinuity in filing at threshold. The $338 filing fee significantly reduces the likelihood of filing by 11.2 percentage points (22%, p-value $<$ 0.001), revealing an extraordinary degree of sensitivity to the financial barriers prospective filers face in order to obtain debt relief. Nonfilers above the eligibility threshold within the bandwidth forgo $43,190 of debt relief on average. This sensitivity would be difficult to generate in any theoretical model that does not include extreme binding liquidity constraints, perceived non-financial costs, or misperceptions of the financial consequences of filing.

Our results are broadly robust to alternative specifications. Table 2 presents how our results change when we incrementally add state and year-month fixed effects and controls for debt and assets (our baseline specification in column (4) includes the full set of controls). The RD estimate remains significant at the 1% level, varying from 10.9 to 11.2 percentage points across specifications. In Appendix Figure A.4, we vary the bandwidth from 10 to 150 percentage points and test triangular kernel weights. Under both kernels, the RD estimate holds steady between 8 and 12 percentage points until the bandwidth falls below 20 percentage points, after which the small sample size introduces noise. The 95% confidence interval around the estimate predictably widens with narrower bandwidths. The RD estimate remains significant at the 5% level up to a 15 percentage-point bandwidth in the uniform kernel specification and up to a 20 percentage-point bandwidth in the triangular kernel specification. Our estimate remains robust at the MSE-optimal bandwidth under both kernels.[11]

Who Does the Filing Fee Prevent from Filing? The filing fee causally reduces the filing rate by 11.2 percentage points, despite amounting to less than 1% of the average potential debt discharge in our sample. The filing fee is thus a meaningful barrier to bankruptcy access for many potential filers. We now analyze whether this barrier improves the targeting efficiency of bankruptcy. In the targeting literature, the efficiency of a barrier is typically assessed by characterizing the compliers who are deterred by the barrier (Kleven and Kopczuk, 2011). We implement this standard approach by estimating the characteristics of the compliers who file if and only if they receive a fee waiver.

The magnitudes of our RD treatment effect estimates (β_1 in equation (2)) capture the extent to which the filing fee deters filings. If the treatment effect is larger for some populations, that suggests that fee waivers are a more significant deterrent for that population. Accordingly, if the treatment effect is larger in subsamples of debtors who the bankruptcy code is designed to help (and smaller in subsamples of debtors who may be more opportunistic), that suggests that the filing fee might be increasing Type I errors (i.e., screening out debtors the bankruptcy code is designed to help). We implement subsample RDs for a given binary variable X by estimating our main RD specification separately for the subsample in which $X = 1$ and the subsample in which $X = 0$.

[11] Detailed regression output and RD plots are shown for selected bandwidths (149, 100, 75, and 50 percentage-point and MSE-optimal) in Appendix Table A.7 and Appendix Figure A.5.

We control for only state and year-month fixed effects and questionnaire completion time, aligning with the specification in column (1) of Table 2.

Table 3 presents the subsample RD estimates (corresponding figures can be found in Appendix Figure A.6). Filing fees are a larger deterrent for debtors with more to gain from bankruptcy: the RD estimate is 3.3 percentage points larger for those with an above-median financial benefit of filing and 7.3 percentage points larger for those with above-median total debt. Prospective filers with these characteristics are exactly the population that the "fresh start" of Chapter 7 is designed to serve. This pattern is echoed in the cross-sectional relationship between the financial benefit of filing and the filing decision. A 10% increase in the financial benefit is associated with only a 0.9-percentage-point higher probability of filing, two orders of magnitude smaller per dollar than the response to the filing fee (see Appendix Figure A.7).

In contrast, the filing fee deters debtors with high and low liquid assets at similar rates: the RD estimate is 10.7 percentage points for debtors with below-median liquid assets and 11.7 percentage points for those with above-median liquid assets. This is a particularly striking result given that liquidity is evidently an important consideration for prospective filers (Gross et al., 2014; Indarte, 2023). That liquidity is insufficient on its own to explain the result likely reflects the ability of filers to pay the fee in installments and the small size of the fee relative to income and the cost of servicing outstanding debt.

Taken together, these results imply that the filing fee has poor targeting properties. The filing fee does not marginally deter less-deserving filers and, if anything, seems to deter the most vulnerable debtors with the most to gain from filing. The standard explanations in the long literature on the consumer bankruptcy filing decision also have difficulty explaining why such a large share of Upsolve users, who have already been screened as good fits for Chapter 7, fail to follow through on filing *after* completing all of the requisite filing paperwork. Asset forfeiture, often studied through the lens of variation in state exemptions (e.g., Gropp et al., 1997; Mahoney, 2015; Severino et al., Forthcoming), is unlikely to bind given Upsolve users' minimal non-exempt assets. Future credit access, the central financial cost of filing in macroeconomic models of consumer bankruptcy (e.g., Livshits et al., 2007; Chatterjee et al., 2007) and a major focus of the empirical literature on post-bankruptcy borrowing (e.g., Musto, 2004; Han and Li, 2011), is in our heavily indebted sample more likely to be a benefit of filing than a cost. For example, Jagtiani and Li (2015) document an average 80-point increase in credit scores in the year after discharge. Retaining the option to file later (White, 1998; Zhu et al., 2019) could cause filers to delay, but they would need to repeat the costly process of compiling the paperwork they already decided was worth completing today. Finally, stigma is frequently invoked (e.g., Brinig and Buckley, 1998; Gross and Souleles, 2002; Fay et al., 2002; Athreya, 2004), but the debtors have already confronted the idea of filing.

In the next part of the paper, we ask debtors directly about their concerns and show that they systematically misperceive the financial consequences of Chapter 7 filing. These misperceptions can amplify the deterrent effect of small ordeals like the filing fee, and generate misallocation of debt relief.

4 Misperceptions: Evidence from a Randomized Controlled Trial

We conduct a randomized controlled trial (RCT) through a survey experiment to establish the importance of bankruptcy misperceptions. The trial was preregistered and IRB-approved.[12]

4.1 Experiment Details

We partner with a major credit reporting bureau to obtain email addresses for individuals who are likely to benefit from bankruptcy. The credit bureau used a proprietary model to identify individuals who were likely to be good candidates for Chapter 7 and provided a sample of the top one million prospects in their databases with available email addresses. We conducted the baseline survey from March 2025 to November 2025, sending survey invitations via email to the full sampling frame provided by the credit bureau. We imposed additional sample restrictions to ensure participants met our criteria, specifically we required that: (1) they do not own real estate, eliminating the risk of losing a home in bankruptcy; (2) they have never previously filed for bankruptcy, ensuring they are eligible to file; (3) they expect that they will never fully repay their debts; and (4) they have at least $10,000 in total debt. Individuals must also pass rigorous attention checks, confirming that they are carefully reading survey questions. We obtain complete survey responses from 245 individuals meeting these criteria.[13]

[12] See `https://www.socialscienceregistry.org/trials/15378`.

[13] We sent survey invitations to all one million email addresses in our sampling frame. 24.7% of recipients opened the email—implying it survived any filters—and 2,832 individuals started the survey. Our survey start rates are similar to Koustas et al. (2026) who had 4,847 unique respondents to 1,300,000 emails using a similar survey approach. After

After consenting to participate and passing attention checks, participants provide information about their debts and assets, including: (1) income; (2) debts outstanding by category (auto loans, medical debt, credit card debt, student loans, payday loans, or other debts); and (3) the value of any vehicles they own.

Participants then indicate how likely they would be to consider bankruptcy on a numeric scale from 1 (zero chance) to 7 (extremely likely). We then provide participants with a list of potential concerns about filing for bankruptcy and ask them to identify their top concern.

In the next section of the survey, we ask participants questions to test their understanding of bankruptcy, instructing them to provide their best guesses without searching the internet. We begin with one question unrelated to bankruptcy: what percentage of working-age adults are currently working or looking for work?[14] We then provide a brief summary of Chapter 7 bankruptcy before asking four knowledge questions related to bankruptcy: (1) do you think you would have to surrender any assets if you filed for Chapter 7 bankruptcy? (2) what fraction of Chapter 7 filers surrender any assets? (3) in what fraction of cases does the filer erase all of their dischargeable debt (medical debt, credit card debt, or payday loans)? and (4) what is the average filer's credit score one year after filing relative to their credit score at filing?

Participants are then randomly assigned to one of four groups: (1) a control group, which learns the labor-market participation rate; (2) a "credit" treatment group, which learns that the average filer's credit score increases by 80 points after filing (Jagtiani and Li, 2015); (3) a "net worth" treatment group, which learns that 7% of filers surrender any assets and 96% of filers receive a debt discharge; and (4) a "both" treatment group, which learns all of the bankruptcy facts mentioned above. Participants must successfully reiterate the facts provided to complete the survey.

We then measure three outcomes. First, we measure "stated interest" in bankruptcy by repeating the earlier question asking how likely the participant would be to consider bankruptcy on a scale from 1 to 7. Second, we elicit the participant's willingness to pay for bankruptcy information. We ask whether they prefer (1) additional information about bankruptcy or (2) a $30 prize.[15] To

imposing our additional sample restrictions and rigorous attention checks, we have 245 qualified survey responses.

[14] In the information-provision portion of the survey, the control group learns the correct answer to this question. This ensures that any differences between the control and treatment groups reflect the topic of the information provided — whether it pertains to bankruptcy — rather than the mere act of receiving a statistic.

[15] The additional information consists of a link to the Upsolve website providing facts about bankruptcy in their state (e.g., asset exemptions).

incentivize honest reporting, we inform participants that they have a chance of receiving their preferred choice. Third, at the end of the survey, all treatment-group participants receive a link to the Upsolve website; our third outcome is an indicator equal to one if they click the link. The survey concludes with questions about demographics and prior experience with bankruptcy.

Roughly two months after completing the initial survey, participants receive an invitation to complete a follow-up survey. Those who pass attention checks are asked the same four bankruptcy knowledge questions to test retention of the facts provided. Finally, we ask participants whether they have taken any of the following actions since the initial survey: (1) spoken with a bankruptcy attorney; (2) made a plan to file for bankruptcy within the next year; or (3) begun entering information into Upsolve to prepare bankruptcy paperwork. We define "Demonstrated Interest" as an indicator equal to one if the participant reports having taken any of these actions. We obtain complete follow-up survey responses for 142 of the 245 participants who completed the initial survey.

4.2 Experiment Results

4.2.1 Concerns About Filing

Table 4 summarizes participants' concerns about filing for bankruptcy, including the fraction who identify each concern and the fraction who rank it as their top concern. The most common primary concern, cited by 37% of participants, is that bankruptcy will not work and they will be left with remaining debt. Another 29% are primarily concerned that filing will prevent future access to credit. Roughly 14% cite cost as their primary concern, and 10% are primarily concerned about surrendering assets.

Notably, several factors that the literature emphasizes as barriers to filing generate little concern among respondents. Only 5% are primarily concerned about the social stigma of others finding out about their filing. Only 3% cite the inability to file again in the future as their top concern.

4.2.2 Bankruptcy Misperceptions

Table 4 shows that many high-debt individuals are concerned that bankruptcy might not work, could lead to asset forfeiture, and could limit future credit access. How warranted are these concerns? We answer this question by examining participants' reported beliefs about the bankruptcy outcomes related to each concern.

Figure 2 Panel A shows the distribution of responses regarding the probability of surrendering assets in Chapter 7. The x-axis reports participants' stated probability and the y-axis reports the frequency of each response. The vertical line marks the correct answer of 7%. The vast majority of participants (96%) overestimate this probability. The median participant overestimates the likelihood of asset surrender by 43 percentage points.

Figure 2 Panel B shows the distribution of responses regarding the probability of successfully discharging debt. The axes mirror Panel A, and the vertical line marks the correct answer of 96%. Nearly all participants (98%) underestimate this probability. The median participant underestimates the likelihood of a full debt discharge by 46 percentage points.

Figure 2 Panel C shows the distribution of responses regarding the average change in credit score in the year after filing. The axes mirror Panel A, and the vertical line marks the correct answer—an 80-point increase. The majority of participants (73%) are incorrectly pessimistic about bankruptcy's impact on credit scores. The median participant believes that the average filer's credit score *drops* by 33 points, whereas Jagtiani and Li (2015) find that it *increases* by 80 points.

Taken together, participants' top concerns about filing (asset forfeiture, failed debt discharge, and damaged credit) are rooted in large and systematic misperceptions. In each case, participants dramatically overestimate the likelihood of adverse outcomes. These patterns suggest that misinformation, rather than the reality of bankruptcy, is a primary barrier to filing.

4.2.3 The Causal Effects of Correcting Bankruptcy Misperceptions

Next, we estimate the effects of our randomized information treatments using participant-level regressions. The outcome variables are measures of interest in bankruptcy. The key independent variables are indicators for the three treatment groups. We omit the indicator for the control group. Control variables include: (1) baseline beliefs regarding credit score changes, asset forfeiture rates, and discharge rates; (2) stated interest in bankruptcy at the *start* of the survey, measured on a scale from 1 to 7; (3) age-bin fixed effects; (4) education-bin fixed effects; (5) an indicator for whether the participant lacks liquidity to cover an unexpected $400 expense; and (6) total debt across all categories. We use robust standard errors.

Our first outcome is an indicator equal to one if the participant expresses interest in bankruptcy at the *end* of the survey. Following our preregistration, we define this as a binary variable equal to one if the participant selects a value of 5 (somewhat likely) or higher when asked how likely they are to consider bankruptcy in the next year. Column (1) of Table 6 presents the results. The net worth treatment, which corrects beliefs about asset forfeiture and debt discharge rates, increases interest in filing by 20.9 percentage points (86% of the control group mean). The credit treatment, which corrects beliefs about post-filing credit scores, increases interest by 13.8 percentage points (57%). The combined treatment, which provides both sets of facts, increases interest by 24.6 percentage points (102%).

Our second outcome variable is an indicator equal to one if the participant prefers additional bankruptcy information over a $30 prize. Column (2) of Table 6 shows that the net worth treatment increases the fraction of participants willing to forgo $30 for bankruptcy information by 16.1 percentage points (76%). The other treatments have smaller, statistically insignificant effects.

Our third outcome is an indicator equal to one if the participant clicks the link to the Upsolve website at the end of the survey. Because the link is provided only to treatment-group participants, this outcome is mechanically zero for the control group and should be interpreted accordingly. Nonetheless, it is informative to examine how often treated individuals seek out further information about bankruptcy. Column (3) of Table 6 shows that 13% of participants in the net worth and combined treatment groups click the link, compared to 10% in the credit treatment group.

Across all three outcomes, correcting bankruptcy misperceptions increases both stated and demonstrated interest in filing. The consistently larger effects of the net worth treatment suggest that concerns about asset forfeiture and debt discharge weigh more heavily on individuals' decisions than concerns about credit access.

4.2.4 Follow-Up Survey Outcomes

We next examine the effects of our information treatments on follow-up survey responses. We obtain complete follow-up responses for 142 of the 245 participants who completed the initial survey.

The follow-up survey includes no new information treatment. Instead, we examine how treatment in the initial survey affects bankruptcy knowledge two months later. The regression specifications are identical to those in columns (1) to (3), with the exception of the outcome variable and the change in sample. In column (4) of Table 6, the outcome is the participant's reported probability of successfully discharging debt. We find that the net worth treatment increases the perceived discharge rate by 13.8 percentage points. The combined treatment has a similar effect. The credit treatment has no effect, as expected: participants who were not told the discharge rate hold beliefs similar to the control group.

Column (5) shows a parallel result for beliefs about credit-score impacts. The credit and combined treatment groups are more optimistic about post-filing credit scores. The net worth treatment group, which did not receive the credit fact, holds beliefs similar to the control group. Column (6) shows that the net worth and combined treatment groups are slightly more optimistic about asset preservation, consistent with the fact they received in the initial survey, though this effect is not statistically significant given the small sample.

Treated participants are also more likely to take action toward filing. Column (7) uses an indicator equal to one if the participant has spoken with a bankruptcy attorney, formed a plan to file, or begun the Upsolve process since the initial survey. The net worth treatment increases the fraction taking such actions by 13 percentage points, significant at the 10% level. The other treatments have smaller, positive, statistically insignificant effects.

In summary, the follow-up survey provides promising evidence that our information treatments persistently correct misperceptions and increase interest in filing. We intend to eventually link this survey data with credit report data to assess the extent to which our treatments drive actual bankruptcy filings.

4.2.5 Discussion

Two aspects of our results warrant further discussion. First, our bankruptcy knowledge questions ask about *average* outcomes among *filers* which allows us to benchmark responses against objective facts. An individual considering bankruptcy cares primarily about their *own* outcome as a *prospective filer*. Our results therefore document misperceptions about aggregate outcomes rather than individual-specific ones. Nevertheless, our findings are difficult to reconcile with rational beliefs. If individuals held accurate beliefs about their own outcomes, learning aggregate facts should not affect their interest in filing as we document. Our results imply that treated individuals update their beliefs about their own potential outcomes, suggesting they misperceive those outcomes even though we do not personally tailor the information provision to each participant.

Second, while the net worth treatment consistently increases interest across all outcomes, other patterns are less uniform. The credit treatment increases click-through rates but has no effect on willingness to pay or demonstrated action. The combined treatment has a strong effect on stated interest and click-through rates but no effect on willingness to pay or action. Two forces likely explain this. The willingness-to-pay and action outcomes require a substantially higher level of commitment than stated interest or click-through. Thus, our net worth treatment is the only one strong enough to shift costly actions. Additionally, the weaker performance of the combined treatment relative to the net worth treatment alone is consistent with Haaland et al. (2023), who note that simultaneous provision of multiple pieces of information may reduce attention to any single piece and weaken treatment effects.

4.3 Field Evidence from Upsolve: Connecting Ordeals and Misperceptions

Our evidence on ordeals and misperceptions comes from two different populations. While we selected the survey sample to resemble prospective filers, they do not perfectly match the Upsolve users who have additionally demonstrated an intent to file. However, after sharing the results of the survey experiment, Upsolve made changes to their messaging based on the evidence we generated.

Upsolve changed the messaging on its homepage in November 2025 to emphasize that bankruptcy "erases" or "eliminates" debt, language that had been present on its pages but secondary to explanations of how its free tool works. The share of users who clicked through to the screener increased by 41% (from 9% on average between June 2025 and November 2025 to 13% between November 2025 and June 2026). These changes are were not randomized and reflect time-series variation, and so should be taken as suggestive. We are currently working with Upsolve on exploring opportunities to randomize and refine the information provided to prospective filers, which may provide an opportunity to replicate the survey experiment in the field.

5 The Forgone Benefits of Filing Chapter 7

Our results show that high-debt individuals underestimate the benefits of Chapter 7, amplifying the deterrent effects of small ordeals and preventing filings. These results matter only if the deterred debtors forgo real benefits. In this section, we estimate the effects of filing Chapter 7 by linking

Upsolve filers and nonfilers to a panel of credit report data.

Although Chapter 7 accounts for 58% of consumer bankruptcies, we know remarkably little about its effects on filers' financial health.[16] Credible identifying variation to estimate the effects has been easier to find for Chapter 13, where Dobbie and Song (2015) and Dobbie et al. (2017) use a random-judge IV design and find that Chapter 13 protection increases annual earnings by $5,562, increases credit scores by 15 points, and reduces five-year mortality by 1.2 percentage points. No comparable discretion exists in Chapter 7, where 96% of filers receive a discharge. Chapter 13 estimates may not generalize to Chapter 7 given the differences between Chapter 13's "repayment plan" protection and Chapter 7's "fresh start" relief. The two chapters also serve different debtors: the means test limits access to Chapter 7 to lower income debtors, while debtors with assets above the exemption thresholds are likely to select into Chapter 13. It is unclear whether we should expect the benefits of Chapter 7 to be larger, due to the immediate debt relief and high failure rates of Chapter 13 repayment plans (Argyle et al., 2025), or smaller, because Chapter 7 filers are poorer and have fewer assets from which to build post-filing.

The Upsolve data make it feasible to estimate the effects of Chapter 7 by providing a sample of filers and marginally deterred nonfilers. We can compare two debtors who both sought out Upsolve, were identified as a good candidate for Chapter 7, and completed the same paperwork as filers, demonstrating the similar intent to file. We can further match filers on an extremely detailed set of financial characteristics based on both the Upsolve data and credit report records to estimate a staggered difference-in-differences design.[17] We provide details below.

5.1 Credit Report Data

We match 35,898 Upsolve users to credit records from 2017 through 2025 provided by a credit reporting bureau. We match 97% of users who completed the questionnaire before the credit data

[16] Jagtiani and Li (2015) descriptively track bankruptcy filers in credit report data but it is infeasible to identify comparable nonfilers (especially intent to file) using credit report data alone. Parra (2022) compares Chapter 7 filers to Chapter 13 filers around the means test's income thresholds, where both the timing of filing and disposable income are potentially manipulable by the filer through labor supply and the expenses included in the disposable income test.

[17] In principle, the fee-waiver discontinuity from Section 3.3 could serve as an instrument for filing. In practice, we are underpowered to use this variation to estimate the third-stage effects of filing on other outcomes. We nevertheless report the results in Appendix Table A.8. While we find suggestive evidence of post-filing improvements, the 2SLS estimates are imprecisely estimated, with large standard errors.

we acquired the credit report data in August 2025.[18] For each user, we observe their credit record annually, measured each September, which allows us to observe each user's credit variables both before and after their questionnaire date. The records include credit score (VantageScore), credit limits, utilization, balances, delinquencies, collections, and public records, along with tradeline-level detail on account open dates, balances, and payments. We also examine two additional outcomes: the bureau's income insight score, a model-based estimate of the user's income, and an indicator for whether the bureau has recorded the user as deceased. The income insight score should be interpreted with caution given the formula is proprietary and we cannot rule out that its inputs may be mechanically affected by filing.

5.2 Empirical Framework

We estimate the effects of filing in a difference-in-differences (DiD) design. Let T_i denote the year in which Upsolve user i started their Upsolve questionnaire. Our first difference compares users before and after their questionnaire. Our second difference compares users who file for bankruptcy through Upsolve to users who never file (19,156 file through Upsolve in their questionnaire year T_i).[19]

Specifically, we estimate the following dynamic DiD specification by OLS:

$$Y_{ikt} = \alpha_i + \lambda_t + \sum_{k \neq -1} \delta_k \mathbf{1}\{t - T_i = k\} + \sum_{k \neq -1} \beta_k \left(\mathbf{1}\{t - T_i = k\} \times F_i\right) + \varepsilon_{ikt}. \tag{3}$$

In this equation, i indexes Upsolve users, F_i is an indicator for filing through Upsolve, t denotes calendar years corresponding to credit bureau observation dates, and k denotes *event time*: the year relative to user i's questionnaire start date T_i. The credit bureau outcome Y_{ikt} varies across specifications. The calendar-year fixed effects λ_t control for macroeconomic conditions that would otherwise correlate with both filing and outcomes. We include individual fixed effects α_i and event-time fixed effects δ_k, with δ_{-1} omitted to avoid multicollinearity. We limit the sample to event

[18] The sample includes 14,084 users who completed their questionnaire prior to September 2021 who were not included in the RD analysis, when Upsolve collected but did not store the running variable required for the RD.

[19] The median Upsolve user who completes the questionnaire does so in four days, and the median Upsolve filer files 21 days after questionnaire completion. In the credit report data, 1,840 Upsolve nonfilers (11%) eventually filed for bankruptcy outside of Upsolve at a later date. We drop their post-filing observations so that they contribute observations only while untreated.

years $k = -3, -2, ..., 2, 3$ because few users are observed outside this window.

The coefficients β_k measure differences in outcomes between filers and nonfilers at each event time, relative to the omitted year $k = -1$. Our identifying assumption is that, absent filing, filers' outcomes would have evolved in parallel with nonfilers'. To improve the comparability of filers and nonfilers, we pair each filer with the nonfiler who was most similarly likely to file, as predicted by their pre-questionnaire finances and the trajectory of those finances over the two preceding years. Specifically, we estimate the probability of filing via a logistic regression with state fixed effects, questionnaire start year fixed effects, and baseline (relative year $k = -1$) values of total debt, debt in collections, credit score, estimated income, and an indicator for having an auto loan. To capture pre-existing financial trajectories, we also include changes in credit score and debt in collections between relative years $k = -3$ and $k = -1$. We then match each filer to the nonfiler with the closest estimated propensity score, subject to a caliper of 0.05. After limiting to this matched sample of users in event times $k \in [-3, 3]$, our DiD sample includes 17,153 users. As a robustness check, we also present an alternative specification using coarsened exact matching (CEM) on the same covariates.[20]

Because the questionnaire year T_i varies across users, this staggered specification is subject to the critique in Callaway and Sant'Anna (2021) and Sun and Abraham (2021).[21] The concern is muted in our setting because the comparison group consists of matched users who never file, rather than not-yet-treated filers. Nevertheless, in Appendix Table A.10 we show that our estimates are qualitatively similar under the Sun and Abraham (2021) estimator designed to address this concern.

Finally, to assess the average treatment effect over post-filing event times $k \in \{1, 2, 3\}$, we estimate the following pooled DiD by OLS:

$$Y_{it} = \alpha_i + \lambda_t + \beta \left(Post_{it} \times F_i \right) + \varepsilon_{it}, \tag{4}$$

where Y_{it} is a credit bureau outcome measured in calendar year t and $Post_{it}$ is an indicator for years

[20]To implement CEM, we partition users by state, questionnaire start year, the baseline auto loan indicator, and quartiles of the continuous matching variables, and match filers to nonfilers within the same coarsened strata. The resulting matched sample is weighted using CEM weights to balance observed pre-period characteristics between the two groups.

[21]Because λ_t is estimated off calendar-year variation pooled across cohorts with different T_i, it implicitly compares already-treated users to not-yet-treated users. Since β_k is identified net of λ_t, this contamination flows directly into the β_k estimates.

after Upsolve user i completes their questionnaire: we set $Post_{it} = 1$ in event years 1 through 3 and $Post_{it} = 0$ in event years -3 through -1. We omit relative year 0 from this pooled specification because it mixes the pre- and post-questionnaire periods, depending on when in the year the individual joined Upsolve. The coefficient β measures the difference in credit bureau outcomes between filers and nonfilers following the questionnaire start date, net of individual fixed effects and aggregate time effects.

5.3 The Effects of Filing Chapter 7

Table 7 presents estimates from the propensity-score matched staggered DiD specification. Each row displays estimates for a different outcome variable Y. In each row, column (1) reports the pooled difference-in-differences estimate β from (4). Columns (2) through (7) report the event-study coefficients β_k from equation (3), with each column corresponding to a distinct event time k. Figure 3 plots the event-study coefficients β_k for a subset of the outcomes shown in Table 7.

We assess the plausibility of the conditional parallel trends assumption using a Wald test of the joint significance of the pre-treatment event-study coefficients $\{\beta_{-3}, \beta_{-2}\}$. The resulting p-values are reported in the final column of Table 7. All but one of the tests fail to reject the null hypothesis that the pre-treatment coefficients are jointly equal to zero, suggesting little evidence of differential pre-treatment trends after matching. The exception is the credit score, where the pre-period coefficients are jointly significant, driven by a 3.6-point difference two years before the questionnaire. This gap is small relative to the post-filing estimates of 18 to 25 points but suggests some small difference in in credit score trajectories remain after the matching procedure. Consistent with the recent difference-in-differences literature (e.g., Roth, 2022), we interpret these diagnostics as evidence supporting the plausibility of the parallel trends assumption rather than definitive proof, since pre-trend tests may have limited power and cannot rule out post-treatment deviations from parallel trends.

Filing for Chapter 7 leads to broad and persistent improvements in financial health relative to comparable nonfilers. Credit scores rise by 22 points in the pooled specification and remain 18 to 25 points higher throughout the three post-filing years (Table 7 Panel A). While credit lines are mechanically closed after filing, access to credit rebounds quickly for filers. In the filing year, filers are 10.2 percentage points less likely to hold a credit card and their credit card limits fall by \$3,287 as accounts are closed in bankruptcy; one year later, filers are 14.3 percentage points *more* likely than matched nonfilers to hold a card, and limits recover to parity by the second year. Auto loans repeat the pattern: holdings fall 12.6 percentage points in the filing year, return to parity within two years, and auto loan balances finish $1,200 to $1,500 higher, consistent with access to larger loans (Panels B and C). Filing thus appears to accelerate re-entry into mainstream credit markets, with the quantity of available credit recovering more gradually.

Filers also exhibit a sharp reduction in financial distress. Total debt past due falls by roughly $4,800 in the pooled specification and the reduction persists through all three post-filing years, with no evidence of a differential pre-trend (Panel D). Balances on credit cards and student loans fall by $929 and $2,174, respectively (Panel B). Consistent with Dobbie et al. (2017), estimated income rises modestly after filing, though as noted above the income insight score should be interpreted with caution. Finally, consistent with the mortality decline that Dobbie and Song (2015) find for Chapter 13 protection, filers have a marginally significant 0.2 percentage-point (51%) lower mortality rate than matched nonfilers (p-value of 0.057).

Our findings are stable across matching algorithms and estimators. The coarsened exact matching specification, which matches a larger sample of 21,208 users, produces similar and more precisely estimated effects, though several additional outcomes exhibit differential pre-trends (Appendix Table A.9). The Sun and Abraham (2021) interaction-weighted estimates show the same qualitative patterns, with less stable pre-period coefficients (Appendix Table A.10). Across specifications, credit scores improve and measures of financial distress decline sharply and persistently after filing.

6 Conclusion

We provide new evidence on why so few US households take advantage of Chapter 7 bankruptcy. In a new dataset of 29,686 prospective filers who have completed the forms to file, only 49% ultimately do. One representative ordeal, the $338 court filing fee, causally reduces filings by 11.2 percentage points (22% of the mean). This effect persists even when focusing on the those with the highest debt and liquid assets, and is therefore difficult to explain with conventional rational explanations or liquidity constraints alone. In a survey of individuals who would be good candidates for filing, we show that they systematically misperceive Chapter 7's financial consequences. They overestimate the probability of asset forfeiture, underestimate the discharge rate, and expect a post-filing credit score 113 points below what filers actually realize. Correcting these misperceptions in a preregistered randomized controlled trial causally increases interest in filing, willingness to pay for bankruptcy information, and, two months later, real-world action toward filing.

Our results also bring a new perspective on the targeting properties of bankruptcy ordeals. When debtors misperceive the value of filing, even small ordeals fail to screen on need and instead screen on understanding of the policy, generating not just lower take-up but potentially regressive misallocation of debt relief toward better-informed filers. The balance between access and abuse prevention has long been contested in U.S. bankruptcy policy, with the 1978 Bankruptcy Reform Act liberalizing access, the 2005 Bankruptcy Abuse Prevention and Consumer Protection Act reversing direction through a means test and heightened filing requirements, and current proposals before Congress proposing to reduce filing fees, simplify the process, and expand dischargeable debt to include student loans.[22] Our results suggest that reducing filing fees could meaningfully expand access for marginal filers, with the response mediated by debtors' beliefs about the financial consequences of filing.

The same logic extends to other high-stakes settings where the canonical targeting framework (Nichols and Zeckhauser, 1982; Kleven and Kopczuk, 2011) assumes accurate perception of benefits, including health insurance, disability benefits, and retirement saving. Understanding what prospective participants believe about these policies is an important direction for future work.

[22] See the Consumer Bankruptcy Reform Act of 2022 (S. 4980, 117th Congress, https://www.congress.gov/bill/117th-congress/senate-bill/4980), the Student Loan Bankruptcy Improvement Act of 2025 (H.R. 4444, 119th Congress, https://www.congress.gov/bill/119th-congress/house-bill/4444), and the Private Student Loan Bankruptcy Fairness Act of 2025 (H.R. 423, 119th Congress, https://www.congress.gov/bill/119th-congress/house-bill/423).

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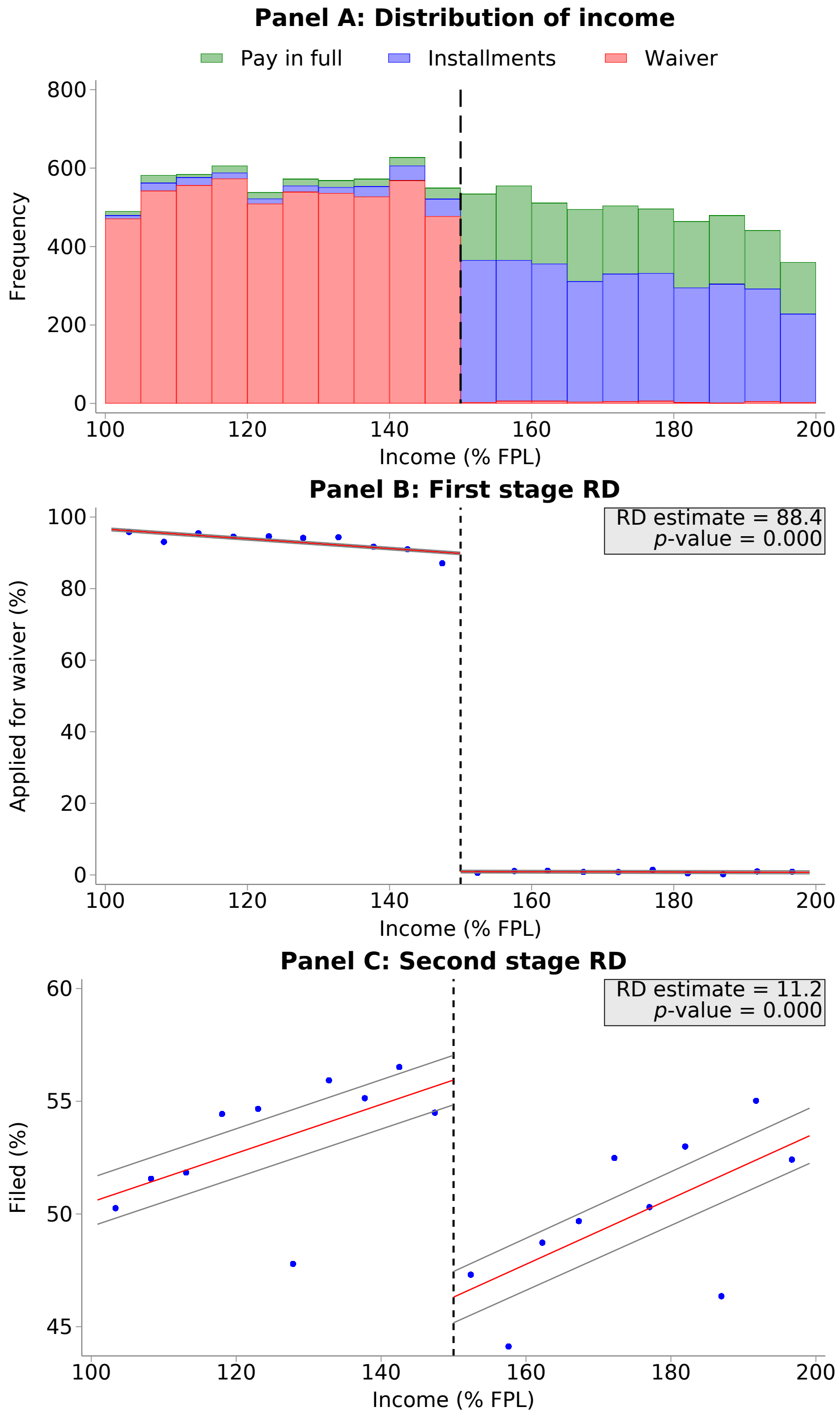
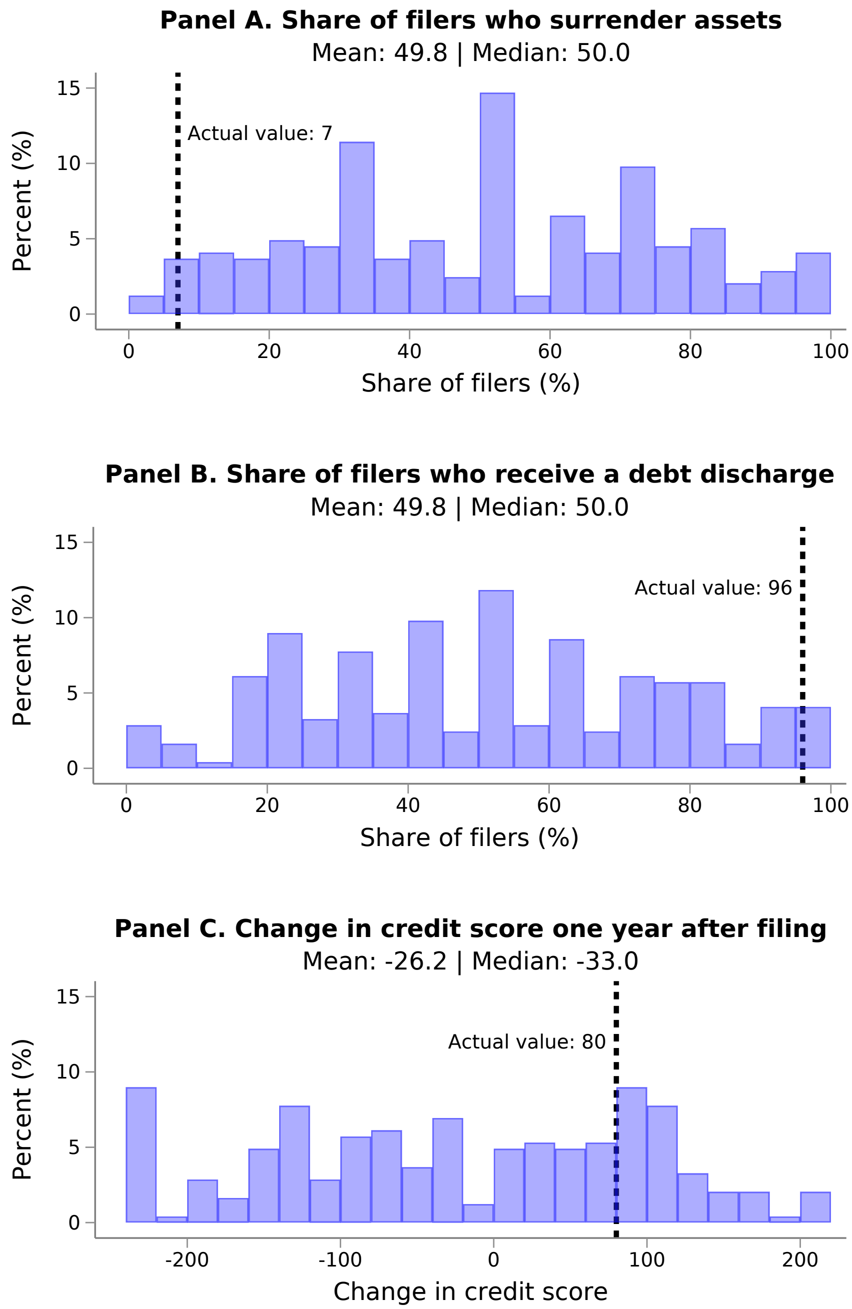
Figure 1: Distribution of Applicant Income and Regression Discontinuity Estimation



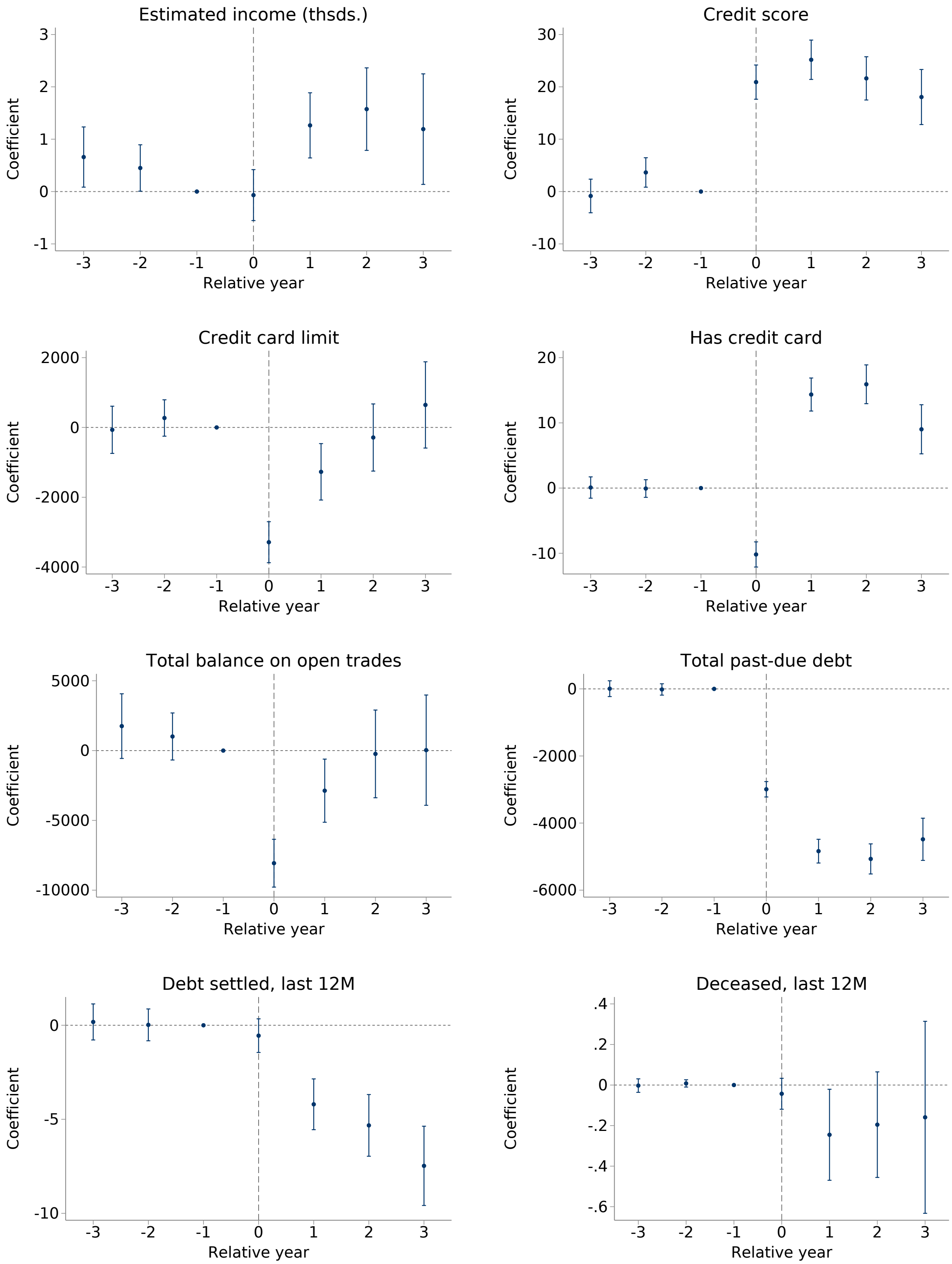
Notes: Panel A presents the distribution of income as a percent of the FPL within the MSE-optimal bandwidth (49.2 percentage points), highlighting users who applied for a fee waiver, applied to pay the fee in installments, or opted to pay the fee in full. Panel B shows the first-stage effect of fee-waiver eligibility (income below 150% FPL) on fee waiver application, and Panel C shows the second-stage effect on filing for bankruptcy. We control for questionnaire completion time, state and year-month fixed effects, debt, and assets. The RD estimate and corresponding p-value is included in the top right of panel C. Dots show the mean waiver application and filing rates for 20 quantile income bins. Solid red lines are fitted values from first-order polynomials, and gray lines represent 95% confidence intervals.

Figure 2: Beliefs About the Effects of Bankruptcy



Notes: This figure provides histograms of respondents' reported beliefs regarding three facts about bankruptcy: (1) the share of filers who surrender assets, (2) the share of filers who obtain a debt discharge, and (3) the average change in credit score one year after filing. Blue bars indicate the frequency (on the y axis) with which participants report each value on the x axis. The vertical black lines mark the correct answers (7%, 96%, and an 80-point increase).

Figure 3: Effects of Filing on Credit Outcomes



Notes: This figure plots estimates from the matched staggered difference-in-differences model outlined in Section 5. Error bars denote 95% confidence intervals based on standard errors clustered at the individual level.

Table 1: Upsolve Summary Statistics

	Full sample (1)	Filers (2)	Non-filers (3)
Panel A: Demographics and household characteristics			
Age (years)	41.6	42.6	40.5
Female (%)	62.4	62.4	62.4
Black (%)	30.4	31.3	29.4
Hispanic or Latino (%)	13.1	12.8	13.5
White (%)	57.8	57.1	58.6
Has dependents (%)	45.7	44.0	47.4
Married (%)	21.8	19.6	24.2
Government benefits (%)	30.1	31.4	28.9
Monthly household income ($)	2,085	2,132	2,040
Panel B: Household assets			
Total assets ($)	12,653	13,436	11,895
Liquid financial assets ($)	480	518	443
Other financial assets ($)	3,908	4,176	3,648
Vehicle assets ($)	6,754	7,173	6,349
Other personal assets ($)	1,357	1,427	1,290
Panel C: Household debt			
Total debt ($)	75,512	80,835	70,364
Unsecured debt ($)	66,183	71,969	60,588
Dischargeable debt ($)	46,187	49,783	42,709
Auto debt ($)	13,805	13,440	14,158
Credit card debt ($)	16,388	19,272	13,599
Debt in collections ($)	5,716	5,877	5,561
Medical debt ($)	1,591	1,808	1,381
Payday loans ($)	151	179	124
Priority claims ($)	2,911	3,128	2,702
Student debt ($)	22,531	24,226	20,892
Other debt ($)	10,371	11,007	9,756
N	29,685	14,593	15,092

Notes: Columns (1) to (3) present summary statistics for the full Upsolve sample, those who file for bankruptcy, and those who do not file. Some users did not answer demographic questions and Upsolve stopped collecting several of these variables in July 2025, so the sample sizes are smaller for age (11,573), gender (19,107), race (15,685), and marital status (24,986). The variables in Panels B and C are winsorized at the 99^{th} percentile conditional on being non-zero.

Table 2: Estimated Effect of the Fee Waiver on the Decision to File

	(1)	(2)	(3)	(4)
Panel A: Reduced form (Filed $\sim$ Below 150% FPL)				
Below 150% FPL (E_i)	9.8	9.7	9.9	9.9
	(1.9)	(1.9)	(1.9)	(1.9)
	[0.000]	[0.000]	[0.000]	[0.000]
Panel B: First stage (Waiver $\sim$ Below 150% FPL)				
Below 150% FPL (E_i)	88.9	88.5	88.5	88.4
	(0.8)	(0.8)	(0.8)	(0.8)
	[0.000]	[0.000]	[0.000]	[0.000]
Panel C: Second stage (Filed $\sim$ Waiver)				
Waiver ($\hat{W}_i$)	11.0	10.9	11.1	11.2
	(2.2)	(2.2)	(2.1)	(2.1)
	[0.000]	[0.000]	[0.000]	[0.000]
R^2	0.003	0.034	0.065	0.073
Observations				
N	10,527	10,527	10,526	10,526
Controls				
Questionnaire duration	Yes	Yes	Yes	Yes
Year-month FE	No	Yes	Yes	Yes
State FE	No	Yes	Yes	Yes
Debt	No	No	Yes	Yes
Assets	No	No	No	Yes

Notes: Table presents linear RD estimates from the Upsolve fuzzy RD model outlined in equations (1) and (2), using the MSE-optimal bandwidth (49.2 percentage points). Columns incrementally control for questionnaire completion time, state and year-month fixed effects, debt, and assets (these controls are outlined in Section 3.3). For each specification we present first-stage (Panel B) and second-stage (Panel C) estimates, as well as reduced-form estimates from the OLS regression of filing on fee waiver eligibility (Panel A). Robust standard errors and p-values are reported in parentheses and square brackets, respectively, below the point estimates. At the bottom of Panel C we present the R^2 from the second-stage model.

Table 3: Heterogeneity in the Effect of the Fee Waiver

	X=1			X=0			Difference	
	β_1	p	N	β_1	p	N	Diff.	p
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: Demographics								
Has dependents	13.40	0.000	5,639	8.44	0.008	4,888	4.96	0.251
	(2.93)			(3.18)			(4.32)	
Panel B: Household assets and debts								
Assets below median ($8K)	11.58	0.000	5,263	9.87	0.001	5,264	1.71	0.693
	(3.12)			(2.99)			(4.32)	
Liquid assets below median ($189)	10.70	0.000	5,261	11.69	0.000	5,266	-0.99	0.818
	(3.02)			(3.07)			(4.30)	
Owns vehicle	12.87	0.000	7,391	6.60	0.102	3,136	6.26	0.190
	(2.55)			(4.04)			(4.78)	
Debt above median ($55K)	14.64	0.000	5,263	7.39	0.017	5,263	7.25	0.091
	(2.96)			(3.11)			(4.29)	
Credit card debt above median ($9K)	13.16	0.000	5,263	8.79	0.004	5,263	4.36	0.309
	(2.97)			(3.09)			(4.28)	
Has medical debt	11.33	0.002	3,370	10.60	0.000	7,156	0.73	0.874
	(3.72)			(2.65)			(4.57)	
Has student debt	12.01	0.000	5,400	9.59	0.002	5,126	2.42	0.575
	(3.00)			(3.10)			(4.32)	
Panel C: Financial benefit from filing								
Fin. benefit above median ($34K)	13.08	0.000	5,263	9.83	0.001	5,264	3.25	0.450
	(3.02)			(3.06)			(4.30)	

Notes: Columns (1) and (4) show the linear RD estimates for sub-groups based on binary characteristics defined prior to treatment, controlling for year-month fixed effects, state fixed effects, and questionnaire completion time. Standard errors are reported below in parentheses, p-values are shown in columns (2) and (5), and sample sizes are shown in columns (3) and (6). Column (7) presents the difference in estimates, and column (8) presents the associated p-value. Restricts to users within the MSE-optimal bandwidth (49.2 percentage points).

Table 4: Concerns With Filing for Bankruptcy

	Concern (%) (1)	Top Concern (%) (2)
It might not work	53.9	36.6
I might lose access to credit	51.0	28.8
It would be too expensive	38.8	14.4
I might have to surrender property	24.5	9.8
People might find out that I filed	24.9	5.2
It might prevent me from filing in the future	11.8	2.6
It might take a long time	30.6	2.6
N	245	153

Notes: Table presents survey respondents' concerns with filing for bankruptcy. Column (1) lists the share of respondents who selected each concern, and column (2) lists the share of respondents who selected each concern as their top concern. The bottom row provides the number of nonmissing observations.

Table 5: Survey Summary Statistics and Balance

	Means					Wald test
	Full sample	Net worth treatment	Credit treatment	Both treatment	Control	p
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Demographics						
Age (years)	37.4	37.0	35.1	40.2	36.8	0.06
Female (%)	75.9	79.3	76.4	74.2	74.2	0.90
White (%)	59.6	69.0	60.0	57.6	53.0	0.31
Married (%)	27.3	31.0	23.6	30.3	24.2	0.71
Household size	2.6	2.8	2.4	2.7	2.7	0.65
College degree (%)	68.2	70.7	65.5	68.2	68.2	0.95
Full-time employee (%)	58.8	58.6	52.7	62.1	60.6	0.75
Panel B: Household finances						
Annual income ($)	73,531	72,457	69,273	70,871	80,682	0.63
Owns vehicle (%)	71.8	75.9	69.1	66.7	75.8	0.57
Vehicle value ($)	12,817	12,143	9,816	14,976	13,790	0.23
Total debt ($)	52,454	55,956	52,805	47,555	53,983	0.38
Panel C: Financial distress						
Wages garnished (%)	24.5	20.7	27.3	25.8	24.2	0.86
Considered filing (%)	40.8	51.7	38.2	39.4	34.8	0.27
Can't pay $200 expense (%)	44.1	51.7	38.2	43.9	42.4	0.53
N	245	58	55	66	66	

Notes: Table presents summary statistics for the full survey sample and for each treatment group. Column (6) presents the p-value from the Wald test for treatment group balance.

Table 6: Effects of Information Provision on Interest in Bankruptcy

	Baseline outcomes			Follow-up outcomes			
	Interest in filing (1)	WTP for info (2)	Clicked link (3)	Discharge belief (4)	Credit access belief (5)	Surrender belief (6)	Bankruptcy action (7)
Net worth	20.9	16.1	13.2	13.8	25.7	-4.5	12.8
	(6.8)	(7.3)	(4.9)	(6.0)	(24.4)	(5.9)	(6.7)
	[0.002]	[0.028]	[0.007]	[0.023]	[0.294]	[0.446]	[0.060]
Credit access	13.8	0.4	10.1	-0.0	104.1	1.8	2.7
	(6.8)	(7.9)	(4.1)	(5.2)	(24.2)	(5.6)	(6.8)
	[0.043]	[0.963]	[0.016]	[0.993]	[0.000]	[0.753]	[0.689]
Net worth + credit access	24.6	-1.7	13.0	13.2	71.1	-8.8	4.2
	(6.0)	(7.1)	(4.5)	(5.5)	(24.1)	(6.3)	(6.4)
	[0.000]	[0.812]	[0.005]	[0.019]	[0.004]	[0.164]	[0.509]
N	245	245	245	142	142	142	142
Control mean	24.2	21.2	0.0	58.8	-14.6	45.0	5.0

Notes: Table presents the effects of the information provision on interest in bankruptcy, actions taken to file, and bankruptcy knowledge. See the main text for the control variables and dependent variable definitions. Robust standard errors and p-values are reported in parentheses and square brackets, respectively, below the point estimates.

Table 7: Effects of Filing on Credit Outcomes

	Pooled	Event time (years rel. to interview)						Wald test
	β	-3	-2	0	1	2	3	p
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A. Income and credit access								
Estimated income (thsds.)	1.0	0.7	0.5	-0.1	1.3	1.6	1.2	0.062
	(0.3)	(0.3)	(0.2)	(0.2)	(0.3)	(0.4)	(0.5)	
	[0.002]	[0.025]	[0.047]	[0.784]	[0.000]	[0.000]	[0.027]	
Credit score	21.8	-0.8	3.6	20.9	25.2	21.6	18.1	0.000
	(1.8)	(1.6)	(1.4)	(1.7)	(1.9)	(2.1)	(2.7)	
	[0.000]	[0.607]	[0.011]	[0.000]	[0.000]	[0.000]	[0.000]	
Credit card limit ($)	-765	-68	271	-3,289	-1,272	-288	645	0.123
	(505)	(345)	(266)	(299)	(412)	(491)	(630)	
	[0.130]	[0.844]	[0.309]	[0.000]	[0.002]	[0.557]	[0.306]	
Panel B. Balances ($)								
Total	-2,089	1,753	1,010	-8,067	-2,872	-237	31	0.316
	(1,295)	(1,181)	(860)	(874)	(1,154)	(1,603)	(2,015)	
	[0.107]	[0.138]	[0.240]	[0.000]	[0.013]	[0.882]	[0.988]	
Auto loans	876	368	-118	-1,849	263	1,481	1,189	0.155
	(331)	(303)	(231)	(257)	(339)	(393)	(500)	
	[0.008]	[0.224]	[0.609]	[0.000]	[0.437]	[0.000]	[0.017]	
Credit cards	-929	-38	-136	-2,695	-1,235	-748	-533	0.801
	(274)	(249)	(237)	(235)	(270)	(314)	(372)	
	[0.001]	[0.879]	[0.568]	[0.000]	[0.000]	[0.017]	[0.152]	
Student loans	-2,171	478	1,122	-3,007	-1,696	-2,309	-2,116	0.038
	(756)	(568)	(486)	(496)	(733)	(960)	(1,363)	
	[0.004]	[0.399]	[0.021]	[0.000]	[0.021]	[0.016]	[0.121]	
Panel C. Has tradeline types (%)								
Auto loans	-2.5	0.7	0.1	-12.6	-5.9	-0.5	2.4	0.700
	(1.1)	(0.9)	(0.7)	(0.8)	(1.1)	(1.4)	(1.9)	
	[0.026]	[0.466]	[0.903]	[0.000]	[0.000]	[0.713]	[0.194]	
Credit cards	14.5	0.1	-0.1	-10.2	14.3	15.9	9.0	0.972
	(1.1)	(0.8)	(0.7)	(1.0)	(1.3)	(1.5)	(1.9)	
	[0.000]	[0.920]	[0.918]	[0.000]	[0.000]	[0.000]	[0.000]	
Student loans	-4.3	1.0	0.8	-7.7	-3.4	-4.5	-5.6	0.383
	(0.9)	(0.8)	(0.7)	(0.7)	(1.0)	(1.3)	(1.7)	
	[0.000]	[0.190]	[0.226]	[0.000]	[0.001]	[0.001]	[0.001]	
Panel D. Deliquency and collections								
Total debt past due ($)	-4,812	6	-17	-2,992	-4,837	-5,068	-4,483	0.947
	(194)	(120)	(86)	(117)	(180)	(229)	(321)	
	[0.000]	[0.961]	[0.841]	[0.000]	[0.000]	[0.000]	[0.000]	
Trades 30+ days past due (#)	-0.2	0.2	0.1	1.0	-0.3	0.0	0.1	0.072
	(0.1)	(0.1)	(0.1)	(0.1)	(0.1)	(0.1)	(0.1)	
	[0.007]	[0.045]	[0.455]	[0.000]	[0.007]	[0.847]	[0.465]	
Panel E. Events in last 12 months (%)								
Debt settlement	-5.11	0.18	0.02	-0.55	-4.20	-5.32	-7.47	0.917
	(0.63)	(0.49)	(0.43)	(0.46)	(0.69)	(0.84)	(1.07)	
	[0.000]	[0.717]	[0.961]	[0.226]	[0.000]	[0.000]	[0.000]	
Fin. counseling	-0.13	0.08	0.02	-0.37	-0.31	0.15	0.02	0.886
	(0.21)	(0.17)	(0.20)	(0.24)	(0.27)	(0.29)	(0.26)	
	[0.533]	[0.643]	[0.921]	[0.121]	[0.241]	[0.607]	[0.948]	
Deceased	-0.22	-0.00	0.01	-0.04	-0.25	-0.20	-0.16	0.517
	(0.12)	(0.02)	(0.01)	(0.04)	(0.11)	(0.13)	(0.24)	
	[0.057]	[0.870]	[0.383]	[0.268]	[0.032]	[0.141]	[0.510]	
N (users)	17,153							
N (user-year observations)	73,628							

Notes: Table reports estimates from the staggered difference-in-differences model outlined in Section 5. Filers and non-filers are matched with one-to-one propensity score matching. Column (1) reports pooled difference-in-differences estimates comparing all post-interview observations (years 1 to 3) to the pre-interview period (years -3 to -2, -1 omitted). Columns (2)–(7) report the event-study coefficients by relative year. Column (8) reports p-values from the Wald test for the joint significance of pre-treatment coefficients. Robust standard errors clustered at the individual level are reported in parentheses, and p-values are reported in square brackets. The final row indicates the number of users across all event years.

A Regression Discontinuity Specification and Robustness

This section investigates potential threats to our primary empirical specification and assesses robustness to alternate specifications. As described in Section 3.3, we use a regression discontinuity design to estimate the causal effect of the $338 filing fee on the decision to file for bankruptcy. Our running variable is income as a percent of the federal poverty line and the discontinuity is at 150% FPL, the threshold below which individuals are eligible for the fee waiver.

Figure A.8 shows the distribution of our running variable for the full sample, excluding a handful of positive outliers. The color of the stacked bars indicates who applied for a fee waiver, applied to pay the fee in installments, or opted to pay the fee in one lump-sum payment. While there is a strong discontinuity in the probability of waiver application around the threshold, it does not jump from zero to one, motivating our use of a fuzzy RD design. This imperfect compliance is unlikely to be driven by lack of knowledge about the fee waiver, because Upsolve automatically calculates eligibility and encourages eligible users to apply. However, users may wait to file after completing their questionnaire and retroactively update their income if it changes, potentially shifting eligibility for the waiver.

Equations (A.1) and (A.2) outline the first- and second-stage equations of our fuzzy RD model (equivalent to equations (1) and (2) in the main text). FPL_i denotes the running variable, income as a percent of the FPL. E_i denotes eligibility for the fee waiver, determined by $FPL_i - 150 < 0$. W_i is an indicator for fee waiver application, and F_i is an indicator for filing for bankruptcy. γ_s and δ_t are state and year-month fixed effects, respectively. X_i controls for days spent on the questionnaire.

$$W_i = \alpha_0 + \alpha_1 E_i + \alpha_2(FPL_i - 150) + \alpha_3 E_i \times (FPL_i - 150) + \alpha_4 X_i + \gamma_s + \delta_t + \varepsilon_i \qquad \text{(A.1)}$$

$$F_i = \beta_0 + \beta_1 \hat{W}_i + \beta_2(FPL_i - 150) + \beta_3 E_i \times (FPL_i - 150) + \beta_4 X_i + \gamma_s + \delta_t + \varepsilon_i \qquad \text{(A.2)}$$

In our primary specification, we use the MSE-optimal bandwidth, as calculated using the algorithm from Calonico et al. (2020), which we calculate to be 49.2 percentage points. We use a uniform kernel, which applies equal weight to all observations within the bandwidth. We assume the relationship between income (% FPL) and filing has a linear functional form, and we allow the slope of this relationship to vary above and below the threshold. Under these assumptions, we estimate the fee waiver increases the probability of filing by 11.2 percentage points (22%, $p < 0.01$). Section A.1 tests robustness of our main estimate to varying each of these assumptions, and Section A.2 presents a series of tests to support our identification assumption.

A.1 Robustness to Bandwidth, Kernel, and Functional Form

Bandwidth: When setting the RD bandwidth, we face a trade-off between maximizing power and minimizing bias from observations further from the cutoff. Panel A of Figure A.4 illustrates how our RD estimate varies with bandwidths ranging from 10 to 150 percentage points, and Panel C shows how our sample size is affected. Table A.7 and Figure A.5 show regression output and RD plots for a handful of these bandwidths, including the mean squared error (MSE) optimal bandwidth calculated using the algorithm from Calonico et al. (2020). Our sample size drops off steadily with narrower bandwidths. Our RD estimate holds steady between 8 and 12 percentage points until the bandwidth falls below 20 percentage points, after which the estimate fluctuates given the smaller sample size. As expected, the 95% confidence interval widens with narrower bandwidths, and the RD estimate loses significance at the 5% level once the bandwidth falls below

15 percentage points. This stability in the magnitude and significance of the RD estimate gives us confidence in our estimated effect.

Kernel Weights: Another way of managing the bias-variance trade-off is to reweight observations based on their distance from the cutoff. Our baseline specification uses a uniform kernel (also called a rectangular kernel), which applies equal weight to all observations within the bandwidth. We test robustness to using a triangular kernel, which weights observations inversely proportionately from their distance from the cutoff. Compared to a uniform kernel, a triangular kernel reduces bias from observations further from the cutoff but reduces power. We select the uniform kernel in our baseline to preserve power. Panel B of Figure A.4 shows that our RD estimate is broadly robust to applying triangular kernel weights (see also Table A.7 and Figure A.5). As with the uniform kernel, the RD estimate holds steady near 11 percentage points until the bandwidth falls below 20 percentage points. The RD estimate loses significance at the 5% level once the bandwidth narrows to 20 percentage points, a bit earlier than with the uniform kernel. Again, the RD estimate remains stable in magnitude and significance.

A.2 Examining Our Identification Assumption

Our main identification assumption is that within the MSE-optimal bandwidth, users just above and below the 150% threshold are similar in all observed and unobserved characteristics, ensuring that a discontinuous jump in filing behavior around the threshold is caused by the fee waiver rather than by other factors. If individuals manipulate their income to affect their fee waiver eligibility, this assumption will be violated. We present three pieces of evidence that suggest our identification assumption holds.

Manipulation Tests: First, we test for manipulation of the running variable at the cutoff using the tests proposed by McCrary (2008) and Cattaneo et al. (2020). These tests estimate density functions above and below the cutoff and examine whether there is a significant jump in density at the cutoff. As shown in Figure A.3, the p-values are 0.790 and 0.982, respectively, indicating there is no significant discontinuity. These results suggest that individuals do not strategically manipulate their reported income to qualify for the fee waiver.

Covariate Balance: Second, we check for covariate balance by examining discontinuities in covariates at the cutoff. For each covariate, we estimate a linear RD with the covariate as the outcome variable, income (% FPL) as the running variable, and no fixed effects or control variables. Following our baseline model, we use the MSE-optimal bandwidth (49.2 percentage points) and a uniform kernel. The results are shown in Table A.6. Among the 14 covariates, none are significant at the 5% level.

Table A.1: Bankruptcy Exemptions

Exemptions	Coverage	Federal Limit		
		April 1, 2019	April 1, 2022	April 1, 2025
Homestead	Equity in one's primary residence	\$25,150	\$27,900	\$31,575
Vehicle	One motor vehicle	\$4,000	\$4,450	\$5,025
Personal assets	Furniture, appliances, clothing, etc.	\$13,400	\$14,875	\$16,850
Jewelry	Wedding rings, watches, family heirlooms, etc.	\$1,700	\$1,875	\$2,125
Tools of the trade	Equipment, books, instruments, etc.	\$2,525	\$2,800	\$3,175
Life insurance	Unmatured policy with cash value	\$13,400	\$14,875	\$16,850
Personal injury claims	Compensation for personal bodily injury	\$25,150	\$27,900	\$31,575
Wildcard	Remaining nonexempt assets	\$1,325	\$1,475	\$1,675
Unused homestead	Remaining nonexempt assets (only available (if homestead exemption was not fully used)	Up to \$13,950	Up to \$12,575	Up to \$15,800

Notes: Table presents the asset exemptions provided by the federal bankruptcy code and the dollar limits effective as of 2019, 2022, and 2025.

Table A.2: Summary Statistics of Upsolve Users vs. All Chapter 7 Filers

	Upsolve			FJC	
	All (1)	Non-Filers (2)	Filers (3)	Ch. 7 (4)	Ch. 13 (5)
Panel A: Filing characteristics (%)					
Chapter 7	.	.	99.9	100.0	0.0
Applied for waiver	57.6	58.2	56.9	.	.
Received fee waiver	.	.	.	4.1	0.0
Pro se	.	.	98.7	7.3	7.9
Panel B: Filing outcomes (%)					
Discharged	.	.	92.2	95.2	7.4
Dismissed	.	.	5.7	3.3	92.0
Panel C: Monthly income ($)					
Monthly household income	2,085	2,040	2,132	3,841	5,315
Panel D: Assets ($)					
Total assets	12,653	11,895	13,436	91,168	176,615
Panel E: Debts ($)					
Total debt	75,512	70,364	80,835	132,589	185,538
Secured debt	8,712	8,885	8,534	62,482	133,539
Unsecured priority debt	2,911	2,702	3,128	2,470	4,655
Unsecured non-priority debt	63,026	57,571	68,667	68,836	60,925
Panel F: Region (%)					
Northeast	14.3	13.8	14.8	11.3	10.2
Midwest	22.7	23.6	21.7	26.9	22.2
South	37.2	38.7	35.6	36.1	55.8
West	25.9	23.9	27.9	25.1	10.0
N	29,685	15,092	14,593	1,218,855	778,138

Notes: Table compares Upsolve filers and nonfilers to individuals in the Federal Judicial Center (FJC) Integrated Database who filed for personal bankruptcy (Federal Judicial Center, 2024). The FJC sample is limited to individuals with consumer debt who filed new Chapter 7 or 13 bankruptcy cases between September 1, 2021 and March 31, 2026. Income, assets, and debts are deflated to August 2024 prices and winsorized at the 99^{th} percentile. For the Upsolve sample, monthly income, assets, and debt are as of the Upsolve interview date. For the FJC sample, data are as of the closing date if the bankruptcy case is closed, and they are as of March 31, 2026 if the bankruptcy case is still pending.

Table A.3: Upsolve Users' Reasons for Considering Bankruptcy

	Reason (%)			Top reason (%)		
	Full sample (1)	Filers (2)	Non-filers (3)	Full sample (4)	Filers (5)	Non-filers (6)
Behind on bills	64.1	62.9	65.2	20.8	20.4	21.4
Lost job	37.0	37.1	36.7	17.6	18.2	16.8
Spent irresponsibly	37.4	36.7	38.2	12.4	11.8	13.1
Sick or disabled	22.4	21.9	22.9	9.9	9.9	10.0
Wages garnished	12.8	12.7	12.9	8.0	8.1	7.8
Hours or pay cut	20.1	20.0	20.2	5.7	6.1	5.2
Separation or divorce	11.3	10.7	11.8	4.5	4.5	4.5
Family lost income	11.5	11.4	11.7	3.0	3.2	2.6
Medical bills	24.1	23.1	25.3	2.7	2.4	3.1
Eviction	6.0	4.9	7.3	1.7	1.2	2.1
Foreclosure	0.3	0.2	0.5	0.1	0.0	0.2
Other	36.2	36.8	35.5	13.6	14.0	13.1

Notes: Table presents Upsolve users' reasons for considering bankruptcy. Columns (1) to (3) show the share of all users, filers, and nonfilers that selected each reason, and columns (4) to (6) show the share that selected each reason as the most important. Includes all users through April 10, 2025, after which Upsolve stopped collecting this data.

Table A.4: Composition of Assets and Exemption Status

	Applicable exemption	Distribution of non-zero assets ($)				
		N	Mean	P25	P50	P75
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Vehicles						
Vehicles	Motor vehicle	19,432	10,318	2,936	6,677	14,886
Total		**19,432**	**10,318**	**2,936**	**6,677**	**14,886**
Panel B: Other personal assets						
Household staples	Household goods	28,336	1,068	467	878	1,469
Jewelry	Jewelry	14,597	235	48	101	225
Tools	Tools of the trade	1,072	1,770	260	679	1,757
High-value items (e.g., trailers, firearms)	Wildcard	3,547	793	155	289	533
Other household items	Wildcard	8,046	314	72	193	484
Total		**28,437**	**1,417**	**528**	**1,060**	**1,847**
Panel C: Liquid financial assets						
Liquid financial assets	Wildcard	29,676	480	39	163	525
Total		**29,676**	**480**	**39**	**163**	**525**
Panel D: Other financial assets						
Domestic support	Domestic support	1,530	17,601	2,931	9,637	23,631
Government benefits	Public benefits	91	10,242	893	2,586	9,000
Retirement accounts	Retirement accounts	8,166	8,304	471	2,107	8,178
Financial claims	Wildcard	11,938	1,426	610	1,250	1,810
Intangible assets	Wildcard	361	2,371	5	97	459
Tax assets	Wildcard	1,771	2,430	494	1,446	3,740
Other financial assets	Wildcard	16,112	124	1	4	39
Total		**23,117**	**5,018**	**136**	**1,253**	**3,562**
N		29,685				

Notes: Table shows the number of Upsolve users with each category (in bold) and sub-category (not in bold) of assets, along with the distribution of asset values among asset holders. The "Covered by Exemptions" columns indicate whether (1) the asset sub-category is covered by the federal exemptions and (2) the asset sub-category is covered by an exemption in at least one state. Asset categories (e.g., other personal assets) and sub-categories (e.g., clothes) are winsorized at the 99$^{\text{th}}$ percentile conditional on being positive.

Table A.5: Composition of Debt and Dischargeability

		Distribution of non-zero debt ($)				
	Discharged	N	Mean	P25	P50	P75
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Auto debt						
Secured auto debt	If collateral is surrendered	12,029	19,276	10,084	16,927	25,183
Unsecured auto debt	✓	9,880	17,956	8,321	14,121	23,448
Total		**18,643**	**21,981**	**10,488**	**18,093**	**28,680**
Panel B: Credit card debt						
Secured credit cards	If collateral is surrendered	1,317	2,368	291	600	1,626
Unsecured credit cards	✓	27,735	17,420	3,258	11,369	24,225
Total		**27,887**	**17,445**	**3,272**	**11,354**	**24,262**
Panel C: Debt in collections						
Secured debt in collections	If collateral is surrendered	27	18,207	544	1,455	9,559
Debt in collections	✓	19,359	8,751	1,791	5,098	11,467
Total		**19,362**	**8,764**	**1,790**	**5,098**	**11,480**
Panel D: Medical debt						
Medical debt	✓	8,762	5,391	608	1,756	4,878
Total		**8,762**	**5,391**	**608**	**1,756**	**4,878**
Panel E: Payday loans						
Payday loans	✓	1,848	2,429	493	1,142	2,828
Total		**1,848**	**2,429**	**493**	**1,142**	**2,828**
Panel F: Priority claims						
Unsecured priority debt		6,874	12,573	1,088	3,677	11,138
Total		**6,874**	**12,573**	**1,088**	**3,677**	**11,138**
Panel G: Student debt						
Student debt		15,462	43,258	10,708	28,046	57,769
Total		**15,462**	**43,258**	**10,708**	**28,046**	**57,769**
Panel J: Other debt						
Utilities debt	✓	888	1,352	335	857	1,707
Other secured debt	If collateral is surrendered	997	37,393	1,090	3,852	17,150
Other unsecured non-priority debt	✓	21,412	12,793	2,304	6,943	15,256
Total		**21,661**	**14,118**	**2,365**	**7,083**	**15,575**
N		29,685				

Notes: Table shows the number of Upsolve users with each category (in bold) and sub-category (not in bold) of debt, along with the distribution of balances among debt holders. The "Discharged" column indicates our assumption for whether each sub-category of debt is discharged in bankruptcy. Debt categories (e.g., auto debt) and sub-categories (e.g., secured auto debt) are winsorized at the 99$^{\text{th}}$ percentile conditional on being non-zero.

Table A.6: Test for Covariate Balance

	Means			Linear RD	
	Full sample (1)	101-150% FPL (2)	150-199% FPL (3)	Estimate (4)	p-value (5)
Panel A: Log assets					
Vehicle assets	6.1	5.9	6.3	0.1	0.659
Liquid financial assets	5.1	4.9	5.2	-0.1	0.056
Other personal assets	6.7	6.6	6.7	0.0	0.734
Other financial assets	5.3	5.1	5.5	0.2	0.220
Panel B: Log debt					
Auto debt	6.3	6.3	6.4	0.2	0.418
Debt in collections	5.5	5.6	5.5	0.1	0.647
Credit card debt	8.4	8.3	8.6	-0.1	0.464
Medical debt	2.4	2.3	2.5	0.1	0.619
Payday loans	0.5	0.5	0.6	-0.0	0.561
Priority claims	1.8	1.7	1.9	0.1	0.439
Student debt	5.2	5.1	5.3	0.1	0.801
Other debt	6.4	6.3	6.5	-0.1	0.728
Panel C: Time to complete questionnaire					
Days to finish questionnaire	45.6	46.1	45.0	2.6	0.645
N	10,527	5,437	5,090		

Notes: Table presents summary statistics for the full Upsolve sample and users within the MSE-optimal bandwidth (49.2 percentage points). The two rightmost columns report coefficients on an indicator for fee-waiver eligibility (income below 150% FPL) and p-values from our baseline RD specification, using the MSE-optimal bandwidth and excluding the fixed effects and controls.

Table A.7: Robustness of RD Estimation to Kernel Weights and Selected Bandwidths

	Bandwidth selection				
	149pp	100pp	75pp	50pp	MSE Optimal
	(1)	(2)	(3)	(4)	(5)
Panel A: Uniform kernel					
Coefficient on Below 150% FPL (E_i)	91.4	90.3	89.2	88.3	88.4
(Outcome = Waiver)	(0.4)	(0.5)	(0.6)	(0.8)	(0.8)
	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]
Coefficient on Waiver ($\hat{W}_i$)	7.1	8.9	9.3	11.1	11.2
(Outcome = Filed)	(1.3)	(1.5)	(1.7)	(2.1)	(2.1)
	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]
R^2	0.066	0.068	0.066	0.073	0.073
N	23,207	18,804	15,365	10,714	10,526
Panel B: Triangular kernel					
Coefficient on Below 150% FPL (E_i)	90.3	89.1	88.4	87.3	87.2
(Outcome = Waiver)	(0.5)	(0.6)	(0.7)	(0.9)	(0.9)
	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]
Coefficient on Waiver ($\hat{W}_i$)	8.5	9.6	10.2	11.5	11.5
(Outcome = Filed)	(1.4)	(1.6)	(1.9)	(2.3)	(2.3)
	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]
R^2	0.067	0.069	0.072	0.079	0.080
N	23,207	18,802	15,365	10,706	10,526

Notes: Table presents RD estimates from the Upsolve fuzzy RD model outlined in equations (1) and (2), using different bandwidths around the 150% FPL threshold and different kernel weights. The bandwidth is indicated in the column header. Panel A uses uniform weights and Panel B uses triangular weights. Both panels control for questionnaire completion time, state and year-month fixed effects, debt, and assets (outlined in Section 3.3). The coefficients on "Below 150% FPL" are first-stage estimates, and the coefficients on "Waived" are second-stage estimates. Robust standard errors and p-values are reported in parentheses and square brackets, respectively, below the point estimates. The R^2 for the second-stage model and sample size are shown at the bottom of each column.

Table A.8: RD Event Study Estimates

	Event time (years rel. to interview)		
	1	2	3
	(1)	(2)	(3)
Panel A. Income and credit access			
Estimated income	-6.7	15.1	-11.2
	(11.4)	(23.3)	(23.1)
	[0.558]	[0.518]	[0.628]
Credit score	-58.7	23.2	76.2
	(65.2)	(108.4)	(125.5)
	[0.368]	[0.830]	[0.543]
Credit card limit ($)	2,590	12,772	32,441
	(5,964)	(12,375)	(28,032)
	[0.664]	[0.302]	[0.247]
Panel B. Balances ($)			
Total	-1,966	-22,705	-65,202
	(33,091)	(50,588)	(88,982)
	[0.953]	[0.654]	[0.464]
Auto loans	-4,815	4,556	25,119
	(9,508)	(15,588)	(27,623)
	[0.613]	[0.770]	[0.363]
Credit cards	-939	-525	12,832
	(2,729)	(4,858)	(11,378)
	[0.731]	[0.914]	[0.259]
Student loans	33,195	-17,565	-59,455
	(27,588)	(36,719)	(60,493)
	[0.229]	[0.632]	[0.326]
Panel C. Has tradeline types (%)			
Auto loans	-34.1	34.5	128.7
	(40.8)	(66.7)	(126.7)
	[0.404]	[0.605]	[0.310]
Credit cards	64.0	145.8	135.2
	(47.9)	(109.5)	(125.6)
	[0.182]	[0.183]	[0.282]
Student loans	-1.9	-77.6	-67.6
	(36.5)	(73.2)	(78.1)
	[0.957]	[0.289]	[0.386]
Panel D. Deliquency and collections			
Total debt past due ($)	-8,186	-24,411	-13,613
	(5,084)	(16,339)	(11,047)
	[0.107]	[0.135]	[0.218]
Trades 30+ days past due (#)	2	4	2
	(3)	(5)	(4)
	[0.474]	[0.420]	[0.631]
Panel E. Events in last 12 months (%)			
Debt settlement	-9.7	40.3	-11.2
	(19.9)	(59.5)	(28.7)
	[0.626]	[0.498]	[0.696]
Fin. counseling	4.7	11.7	3.2
	(6.4)	(18.2)	(9.5)
	[0.462]	[0.521]	[0.734]
Deceased	-6.9	-18.1	21.9
	(4.8)	(20.8)	(17.2)
	[0.151]	[0.384]	[0.202]
N	7,863		

Notes: Table reports estimates from the fuzzy regression discontinuity model described in Section 3.3, using filing as the first-stage outcome and credit report attributes as the second-stage outcomes. The running variable is household income as a percentage of the FPL, with eligibility for the fee waiver at 150% serving as an instrument for Chapter 7 filing. Each event-study coefficient is estimated separately by relative year. Robust standard errors clustered at the individual level are reported in parentheses, and p-values are reported in square brackets.

Table A.9: Effects of Filing on Credit Outcomes (Coarsened Exact Matching)

	Pooled	Event time (years rel. to interview)						Wald test
	β	-3	-2	0	1	2	3	p
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A. Income and credit access								
Estimated income (thsds.)	0.4	0.5	0.5	-0.6	0.7	0.9	0.3	0.004
	(0.2)	(0.2)	(0.2)	(0.2)	(0.2)	(0.3)	(0.4)	
	[0.083]	[0.012]	[0.001]	[0.001]	[0.003]	[0.002]	[0.432]	
Credit score	15.7	9.5	7.8	18.0	23.2	20.1	19.4	0.000
	(1.3)	(1.3)	(1.1)	(1.2)	(1.4)	(1.6)	(2.2)	
	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	
Credit card limit ($)	-2,368	318	726	-4,435	-2,546	-1,717	-1,195	0.000
	(328)	(233)	(174)	(213)	(289)	(331)	(424)	
	[0.000]	[0.171]	[0.000]	[0.000]	[0.000]	[0.000]	[0.005]	
Panel B. Balances ($)								
Total	-4,800	598	1,139	-10,430	-5,300	-3,777	-4,560	0.148
	(935)	(825)	(598)	(562)	(833)	(1,116)	(1,616)	
	[0.000]	[0.469]	[0.057]	[0.000]	[0.000]	[0.001]	[0.005]	
Auto loans	400	358	170	-2,292	-59	1,066	681	0.264
	(244)	(220)	(169)	(183)	(251)	(305)	(399)	
	[0.100]	[0.103]	[0.316]	[0.000]	[0.815]	[0.000]	[0.088]	
Credit cards	-1,684	-536	-105	-3,377	-2,152	-1,697	-1,568	0.001
	(187)	(171)	(151)	(165)	(194)	(214)	(260)	
	[0.000]	[0.002]	[0.488]	[0.000]	[0.000]	[0.000]	[0.000]	
Student loans	-2,321	562	898	-3,095	-1,611	-2,576	-2,848	0.040
	(578)	(411)	(358)	(378)	(580)	(758)	(1,007)	
	[0.000]	[0.171]	[0.012]	[0.000]	[0.005]	[0.001]	[0.005]	
Panel C. Has tradeline types (%)								
Auto loans	-2.7	1.2	0.6	-12.7	-5.3	-0.3	1.3	0.276
	(0.9)	(0.7)	(0.6)	(0.7)	(0.9)	(1.1)	(1.5)	
	[0.002]	[0.109]	[0.281]	[0.000]	[0.000]	[0.767]	[0.389]	
Credit cards	12.0	0.2	0.7	-12.3	12.7	13.2	6.0	0.339
	(0.9)	(0.7)	(0.6)	(0.8)	(1.0)	(1.2)	(1.5)	
	[0.000]	[0.783]	[0.198]	[0.000]	[0.000]	[0.000]	[0.000]	
Student loans	-4.0	0.6	0.3	-7.7	-3.7	-4.3	-5.3	0.645
	(0.8)	(0.6)	(0.5)	(0.6)	(0.8)	(1.1)	(1.3)	
	[0.000]	[0.349]	[0.526]	[0.000]	[0.000]	[0.000]	[0.000]	
Panel D. Deliquency and collections								
Total debt past due ($)	-4,372	-33	-87	-2,786	-4,529	-4,706	-3,827	0.247
	(135)	(77)	(61)	(87)	(125)	(167)	(215)	
	[0.000]	[0.667]	[0.155]	[0.000]	[0.000]	[0.000]	[0.000]	
Trades 30+ days past due (#)	-0.1	-0.2	-0.1	1.2	-0.3	-0.1	0.1	0.019
	(0.1)	(0.1)	(0.1)	(0.1)	(0.1)	(0.1)	(0.1)	
	[0.203]	[0.007]	[0.011]	[0.000]	[0.001]	[0.472]	[0.614]	
Panel E. Events in last 12 months (%)								
Debt settlement	-4.50	0.74	0.51	-0.34	-3.21	-4.60	-6.51	0.119
	(0.44)	(0.36)	(0.33)	(0.36)	(0.49)	(0.60)	(0.83)	
	[0.000]	[0.044]	[0.128]	[0.342]	[0.000]	[0.000]	[0.000]	
Fin. counseling	-0.42	-0.01	-0.10	-0.33	-0.52	-0.42	-0.30	0.617
	(0.14)	(0.14)	(0.13)	(0.16)	(0.18)	(0.21)	(0.25)	
	[0.002]	[0.970]	[0.412]	[0.033]	[0.003]	[0.043]	[0.227]	
Deceased	-0.28	-0.01	0.00	-0.04	-0.20	-0.37	-0.39	0.689
	(0.10)	(0.01)	(0.00)	(0.03)	(0.08)	(0.15)	(0.25)	
	[0.006]	[0.480]	[0.713]	[0.189]	[0.013]	[0.011]	[0.124]	
N (users)	21,208							
N (user-year observations)	90,476							

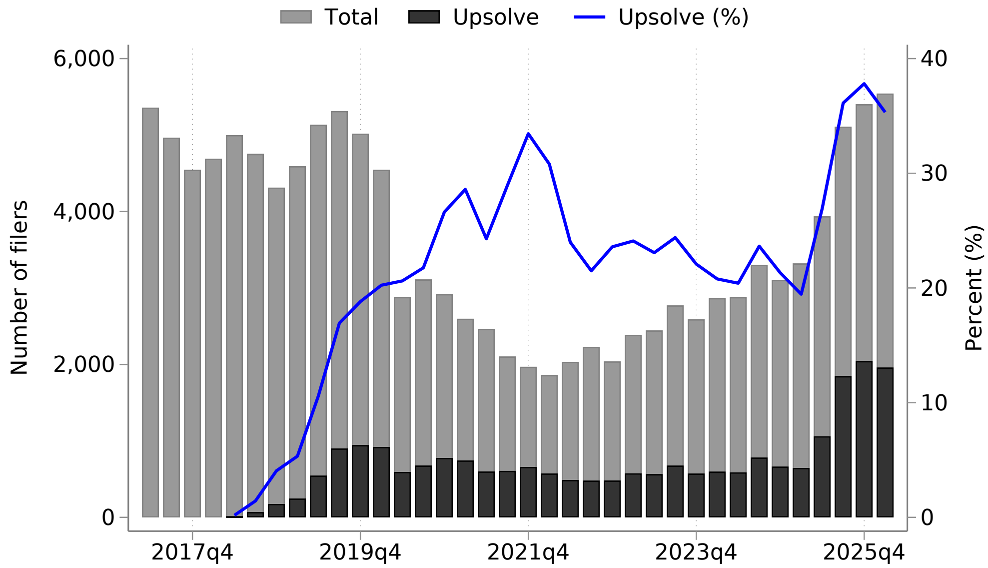
Notes: Table reports estimates from the matched staggered difference-in-differences model outlined in Section 5. Filers and non-filers are matched with one-to-one coarsened exact matching. Column (1) reports pooled difference-in-differences estimates comparing all post-interview observations (years 1 to 3) to the pre-interview period (years -3 to -2, -1 omitted). Columns (2)–(7) report the event-study coefficients by relative year. Column (8) reports p-values from the Wald test for the joint significance of pre-treatment coefficients. Robust standard errors clustered at the individual level are reported in parentheses, and p-values are reported in square brackets.

Table A.10: Effects of Filing on Credit Outcomes (Sun and Abraham, 2021)

	Event time (years rel. to interview)					
	-3	-2	0	1	2	3
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A. Income and credit access						
Estimated income (thsds.)	-1.2	0.1	-2.5	-1.2	1.0	2.9
	(0.2)	(0.1)	(0.2)	(0.2)	(0.3)	(0.4)
	[0.000]	[0.364]	[0.000]	[0.000]	[0.001]	[0.000]
Credit score	20.3	17.6	1.1	39.9	58.4	73.1
	(1.2)	(0.8)	(0.9)	(1.3)	(1.8)	(2.4)
	[0.000]	[0.000]	[0.236]	[0.000]	[0.000]	[0.000]
Credit card limit ($)	-2,356	418	-6,936	-4,515	-187	4,241
	(356)	(171)	(187)	(285)	(389)	(532)
	[0.000]	[0.014]	[0.000]	[0.000]	[0.631]	[0.000]
Panel B. Balances ($)						
Total	-4,685	-446	-15,697	-11,543	-3,436	4,243
	(860)	(549)	(531)	(865)	(1,230)	(1,708)
	[0.000]	[0.417]	[0.000]	[0.000]	[0.005]	[0.013]
Auto loans	-1,230	-260	-4,100	-1,755	1,078	3,008
	(219)	(138)	(151)	(236)	(318)	(415)
	[0.000]	[0.060]	[0.000]	[0.000]	[0.001]	[0.000]
Credit cards	-3,232	-1,118	-5,322	-4,903	-2,677	-401
	(200)	(131)	(133)	(183)	(242)	(328)
	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.223]
Student loans	-268	531	-3,111	-2,718	-3,105	-3,757
	(446)	(336)	(343)	(581)	(802)	(1,096)
	[0.549]	[0.114]	[0.000]	[0.000]	[0.000]	[0.001]
Panel C. Has tradeline types (%)						
Auto loans	-4.9	-1.4	-19.8	-12.1	-2.0	6.4
	(0.7)	(0.4)	(0.5)	(0.8)	(1.1)	(1.5)
	[0.000]	[0.001]	[0.000]	[0.000]	[0.060]	[0.000]
Credit cards	-4.9	-0.4	-21.2	4.2	18.1	25.6
	(0.7)	(0.5)	(0.6)	(0.8)	(1.1)	(1.5)
	[0.000]	[0.353]	[0.000]	[0.000]	[0.000]	[0.000]
Student loans	1.0	1.0	-7.4	-4.7	-4.2	-5.1
	(0.6)	(0.4)	(0.5)	(0.7)	(0.9)	(1.3)
	[0.098]	[0.016]	[0.000]	[0.000]	[0.000]	[0.000]
Panel D. Deliquency and collections						
Total debt past due ($)	131	-112	-2,593	-4,368	-5,169	-5,765
	(120)	(62)	(78)	(118)	(170)	(236)
	[0.276]	[0.072]	[0.000]	[0.000]	[0.000]	[0.000]
Trades 30+ days past due (#)	-0.6	-0.5	2.4	-0.8	-1.8	-1.9
	(0.1)	(0.0)	(0.1)	(0.1)	(0.1)	(0.1)
	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]
Panel E. Events in last 12 months (%)						
Debt settlement	1.04	0.24	-1.20	-3.23	-4.60	-5.87
	(0.37)	(0.27)	(0.29)	(0.41)	(0.57)	(0.75)
	[0.005]	[0.373]	[0.000]	[0.000]	[0.000]	[0.000]
Fin. counseling	-0.04	-0.09	-0.04	-0.52	-0.21	-0.02
	(0.13)	(0.10)	(0.11)	(0.15)	(0.21)	(0.25)
	[0.792]	[0.373]	[0.716]	[0.000]	[0.303]	[0.939]
Deceased	0.13	0.07	-0.09	-0.13	-0.06	0.16
	(0.05)	(0.02)	(0.03)	(0.05)	(0.09)	(0.17)
	[0.016]	[0.001]	[0.004]	[0.016]	[0.496]	[0.371]
N (users)	17,153					
N (user-year observations)	73,628					

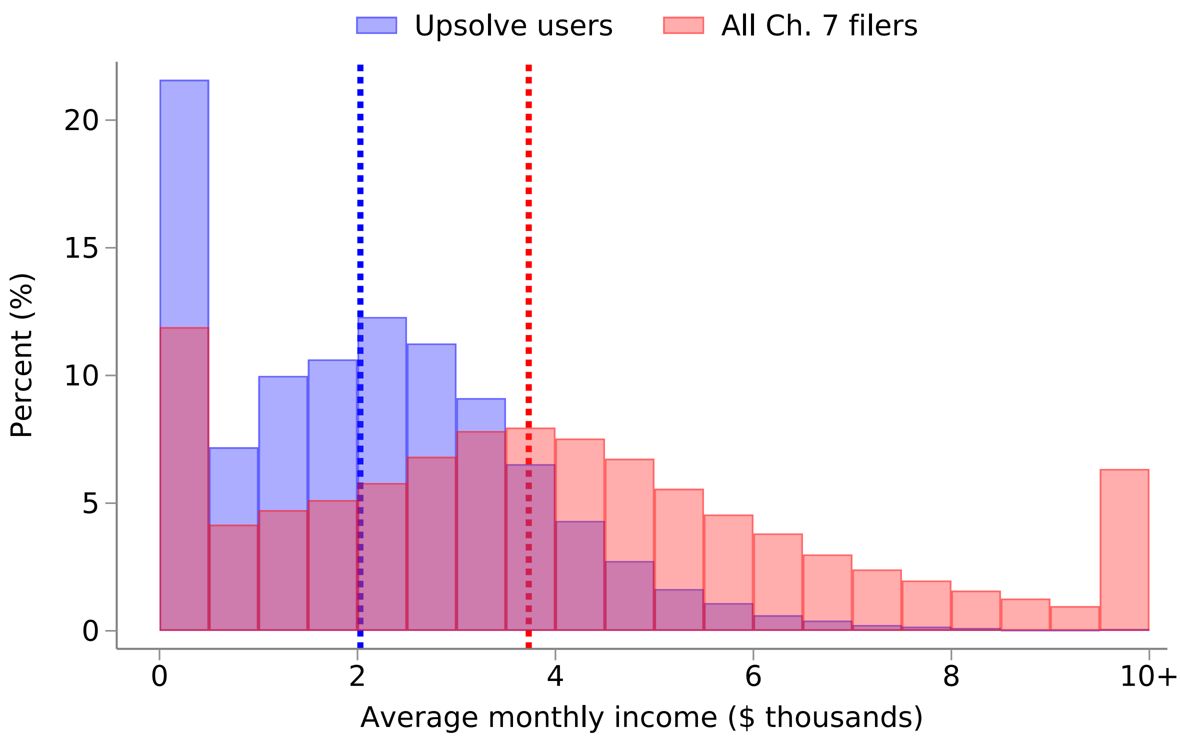
Notes: Table reports estimates from the Sun and Abraham (2021) specification of the staggered difference-in-differences model outlined in Section 5. Filers and non-filers are matched with one-to-one propensity score matching. Robust standard errors clustered at the individual level are reported in parentheses, and p-values are reported in square brackets.

Figure A.1: Upsolve-Eligible Chapter 7 Filings Over Time



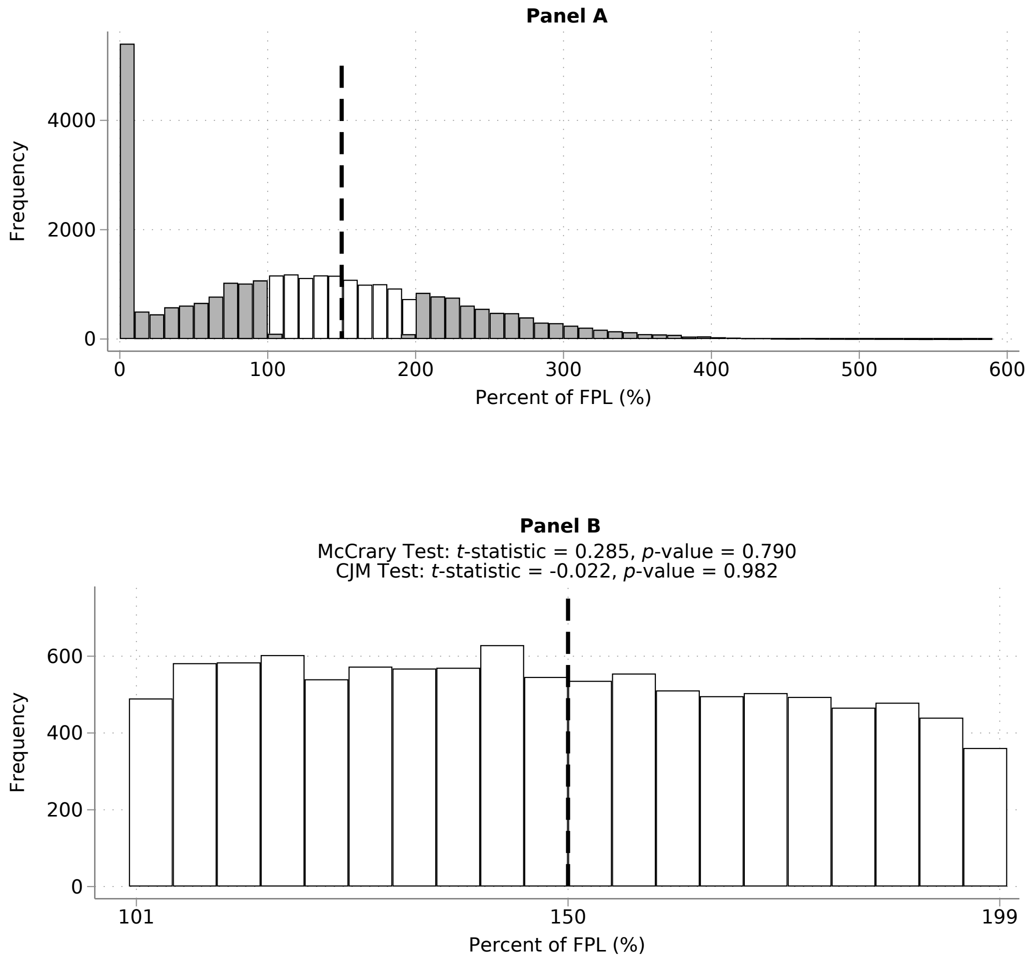
Notes: Figure shows the number of Upsolve bankruptcy filings and Upsolve-eligible filings in the Federal Judicial Center (FJC) database between 2017 Q2 and 2026 Q1 (Federal Judicial Center, 2024). The first recorded Upsolve filing is in 2018 Q2. We define Upsolve-eligible filings as non-joint Chapter 7 cases filed pro se by non-homeowners. The blue line presents the share of Upsolve-eligible filings attributed to Upsolve users, calculated by dividing the number of Upsolve filings by the number of Upsolve-eligible FJC filings.

Figure A.2: Distribution of Income for Upsolve Users vs. All Chapter 7 Filers



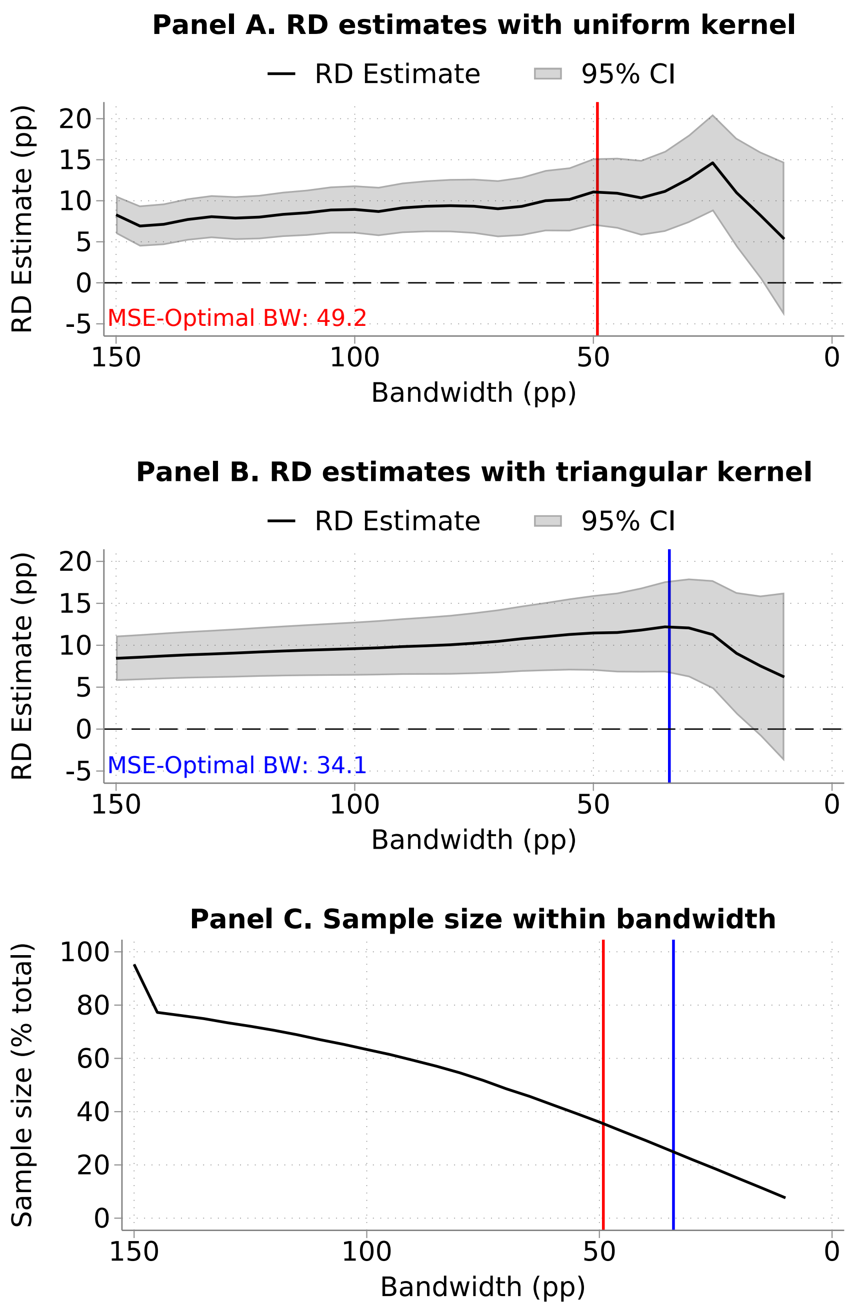
Notes: Figure shows the distribution of average monthly income for Upsolve users (filers and nonfilers) and all Chapter 7 filers in the Federal Judicial Center (FJC) database between September 2021 and March 2026. Income is deflated to August 2024 prices and is winsorized at $0 and $10,000. The bars of the histogram are $500 in width, and the blue and red dashed lines represent the median income for the Upsolve and FJC samples, respectively.

Figure A.3: McCrary and CJM Tests



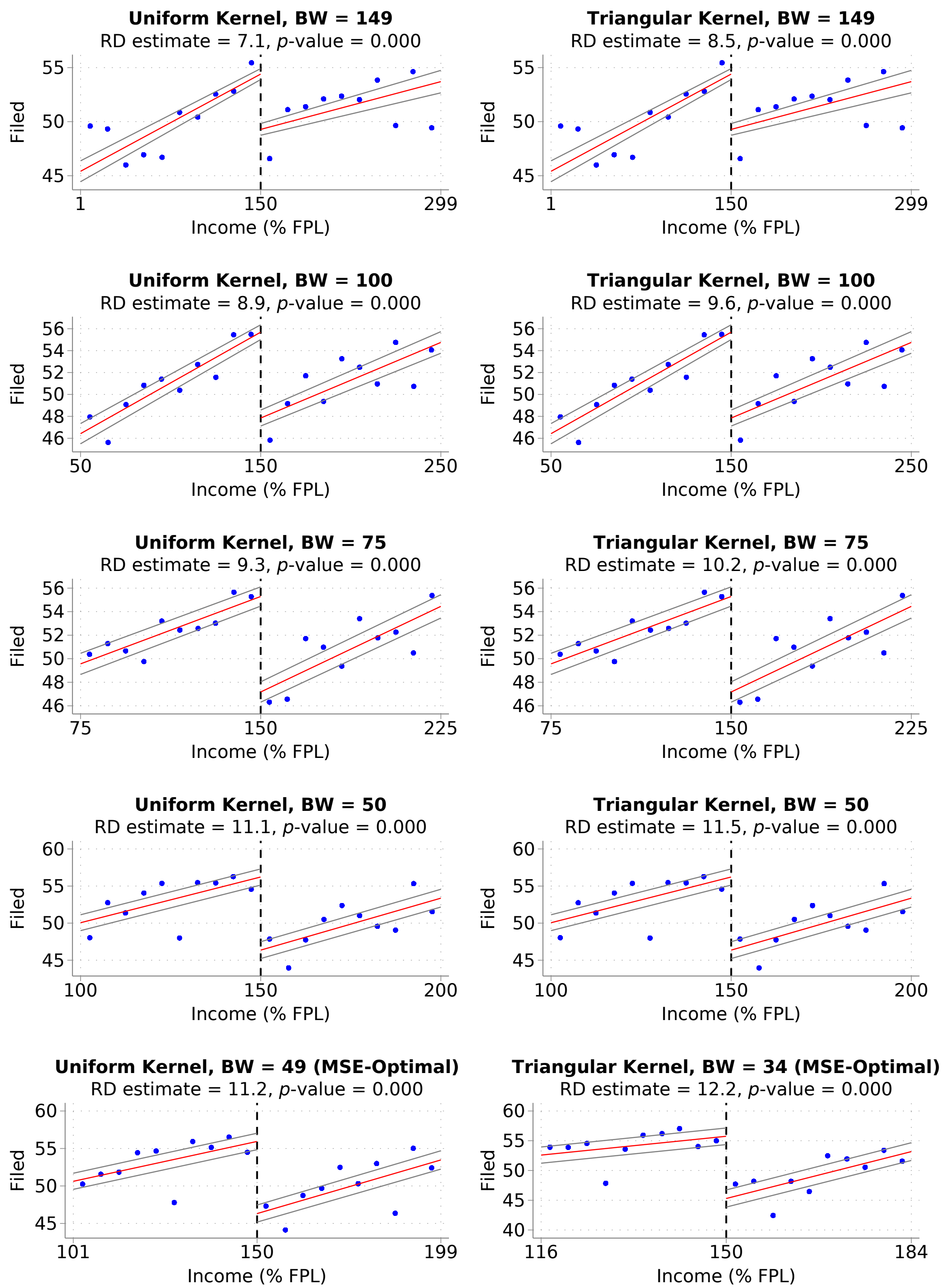
Notes: Panel A shows the distribution of applicant income, with the gray bars representing applicants outside of the MSE-optimal bandwidth (49.2 percentage points). Panel B shows the distribution of applicant income within this bandwidth and reports the t-statistics and p-values from the manipulation tests proposed by McCrary (2008) and Cattaneo et al. (2020).

Figure A.4: Robustness of RD Estimation to Bandwidth Selection and Kernel Weighting



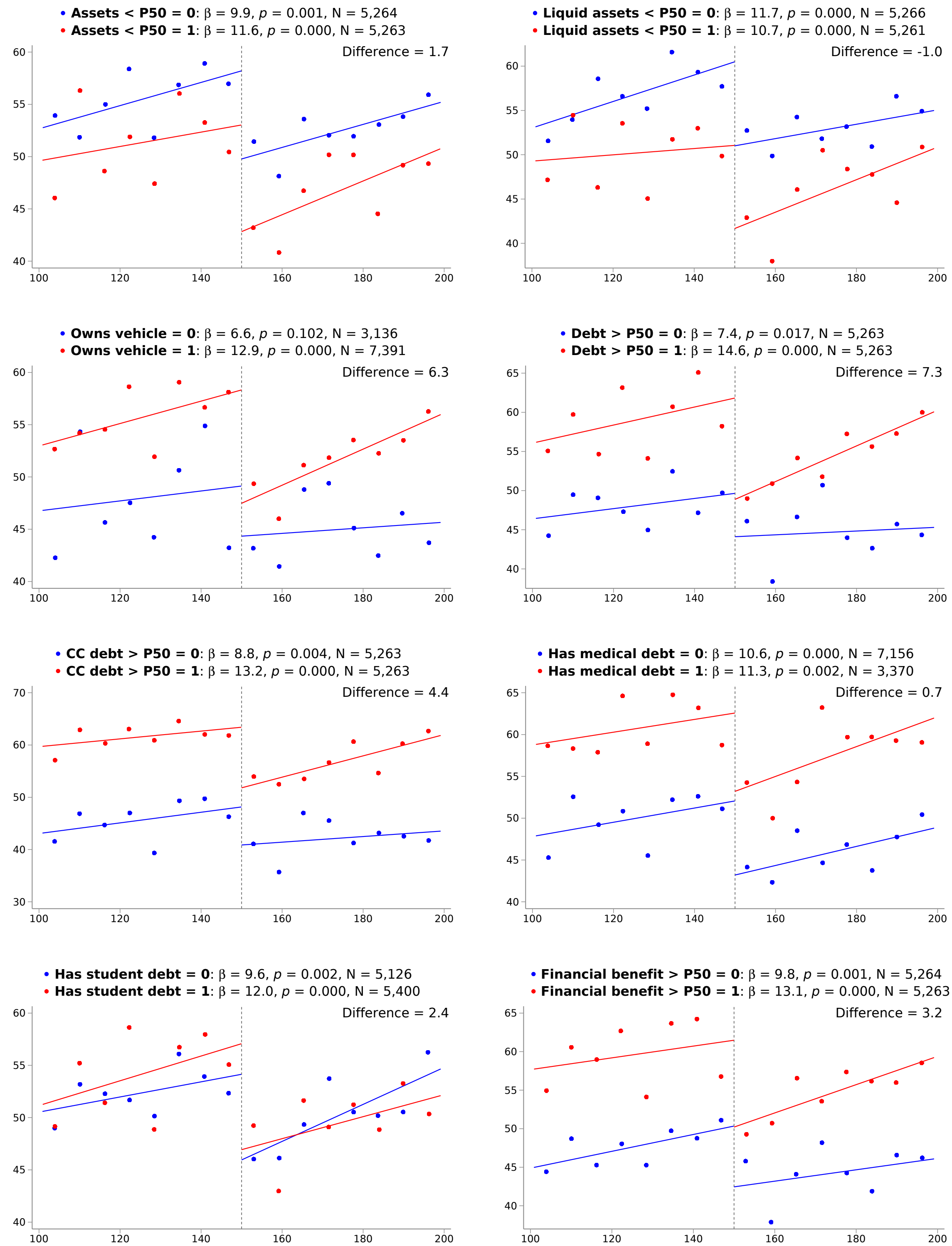
Notes: Figure shows the estimated effect of the fee waiver on filing, as estimated in the fuzzy RD model from equations (1) and (2), under different bandwidths and kernel weights. We vary the bandwidth from 10 to 150 percentage points. Panel A uses uniform kernel weights and Panel B uses triangular kernel weights. In these panels, the black line represents the RD estimate, the grey bands represent a 95% confidence interval, and the red and blue lines represent the MSE-optimal bandwidth based on the algorithm from Calonico et al. (2020). Panel C shows the share of the full sample within each bandwidth.

Figure A.5: Robustness of RD Estimation to Bandwidth Selection and Kernel Weighting



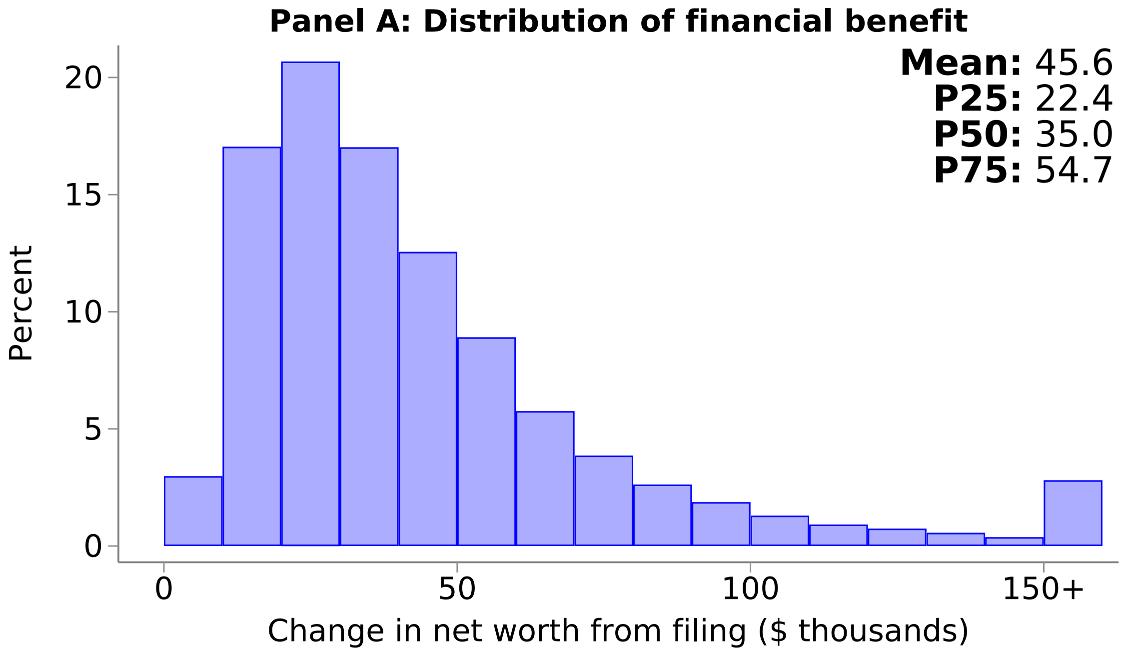
Notes: Figure shows the second-stage effect on filing for bankruptcy under different bandwidths and kernel weights. The bandwidth and kernel weighting approach is specified in the top of each plot. The bottom two plots show the MSE-optimal bandwidths (49.2 and 34.1 percentage points for the uniform and triangular specifications, respectively). Each RD plot controls for state and year-month fixed effects, questionnaire completion time, debt, and assets (outlined in Section 3.3). The RD estimate and corresponding p-value is included in the top right of each plot. Dots show the mean application and filing rates for 20 quantile income bins. Solid lines are fitted values from first-order polynomials, and gray lines represent 95% confidence intervals.

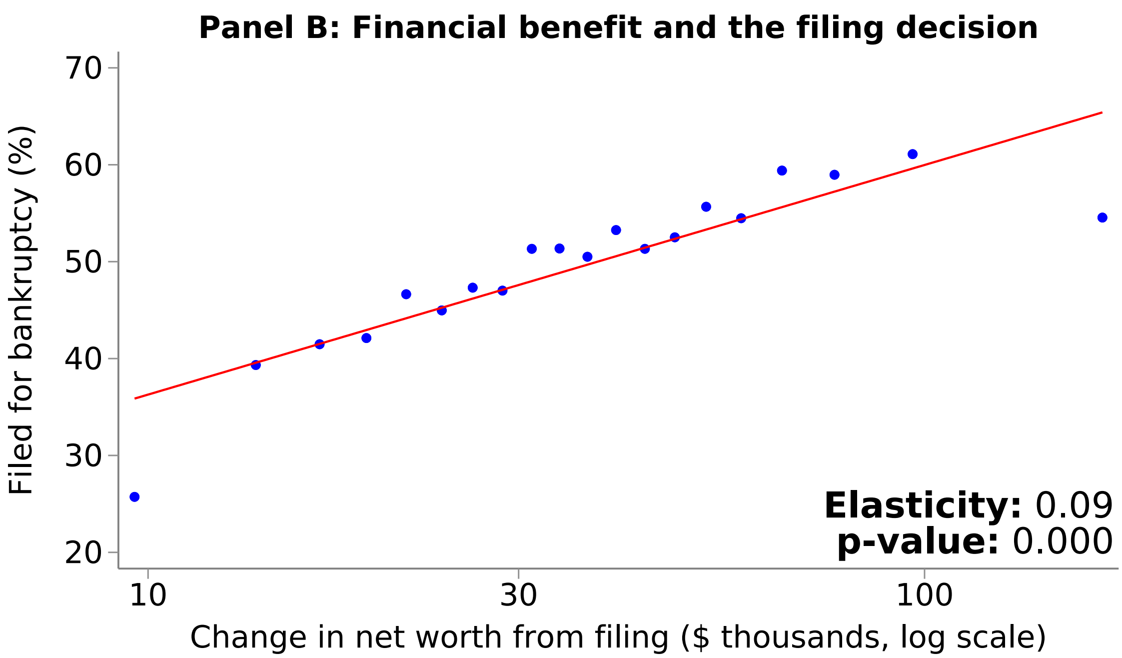
Figure A.6: Heterogeneous Treatment Effects



Notes: Figure plots the fuzzy RD model outlined in equations (1) and (2) for sub-groups based on binary characteristics defined prior to treatment. Uses the MSE-optimal bandwidth (49.2 percentage points), includes state and year-month fixed effects, and controls for questionnaire completion time. The RD estimates and corresponding p-values are presented in the legend next to each group label, and the difference in RD estimates is shown in the top right of the plot. Dots show the mean filing rate for 20 quantile income bins. Solid lines are fitted values from the first-order polynomials with a uniform kernel.

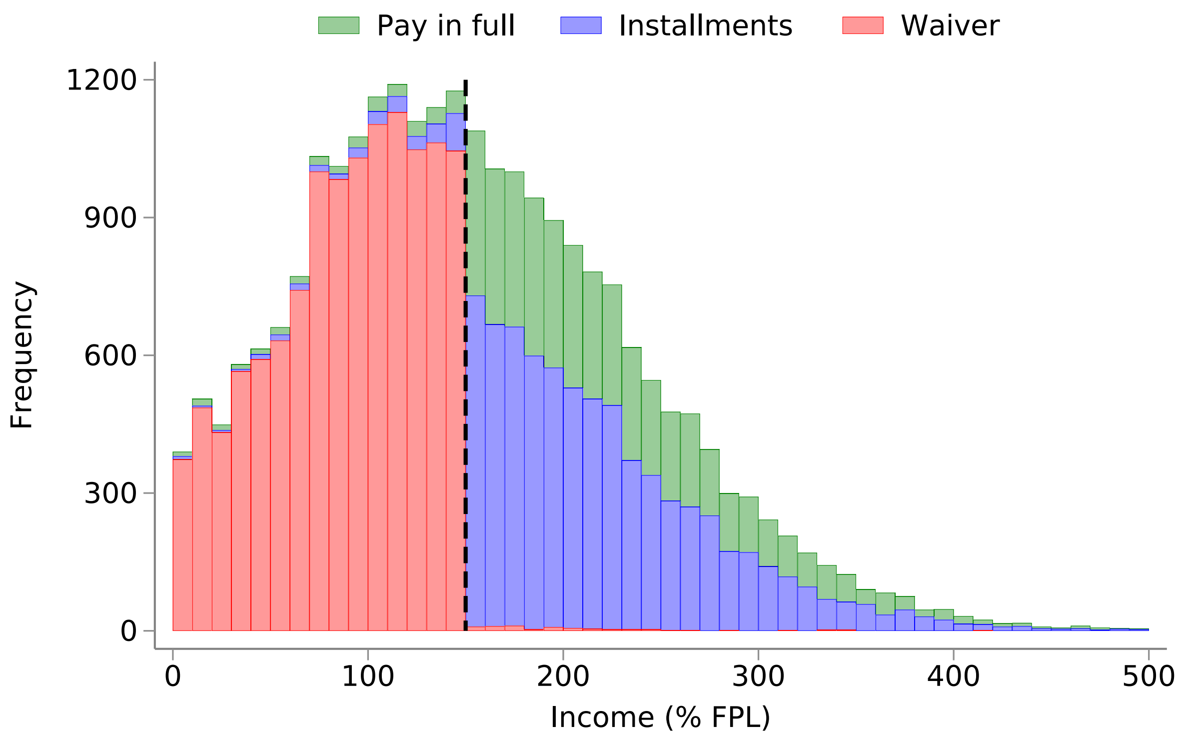
Figure A.7: Upsolve Financial Benefit from Filing and the Filing Decision





Notes: Figure shows the distribution of Upsolve users' financial benefit from filing (Panel A) and the relationship between the financial benefit (on a log scale) and the likelihood of filing (Panel B). The financial benefit from filing is the change in net worth one would experience if they filed for Chapter 7 bankruptcy. The financial benefit is deflated to August 2024 prices and winsorized at the 99^{th} percentile. In Panel B, the binned scatterplot includes 20 quartile bins, and the red line represents a fitted line from the regression of the filing decision on log financial benefit. The estimated elasticity and p-value from this regression are in the bottom right of the plot.

Figure A.8: Distribution of Applicant Income



Notes: Figure presents the distribution of income as a percent of the FPL, highlighting users who applied for a fee waiver, applied to pay the fee in installments, or opted to pay the fee in full. Excludes 21 users with income above 500% of the FPL.