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Essays in Financial Intermediation and Household Finance

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Essays in Financial Intermediation and Household Finance

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Dedicated to my family Carlos, Yolanda, Leonardo, Nicolas and Jacelly for their amazing support.

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Essays in Financial Intermediation and Household Finance

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This dissertation consists of two papers. The papers examine the U.S. credit markets, first through an examination of the consumer bankruptcy system, and second through the study of the impact of deposit shocks on credit supply. The common thread throughout this dissertation is a focus on the causes and consequences of financial intermediation in the U.S. This dissertation is characterized by the use of new datasets and cross-sectional identification techniques.

In the first paper, I study the effect of debt relief provided by the personal bankruptcy system on debtor's ex-post economic behavior. This paper exploits, through a regression discontinuity design, the income thresholds that prevent some households from filing for Chapter 7 protection introduced by the 2005 Bankruptcy Abuse Prevention and Consumer Protection Act (BAPCPA). I find that Chapter 7 bankruptcy protection increases the probability that the filer creates a new business by 10.30 percentage points, become a first time homeowner by 14.9 percentage points and obtain secured lending by 10.2 percentage points. In addition, Chapter 7 reduces

the probability of home foreclosure by 49.7 percentage points and default on tax and non-tax obligations by 39.1 percentage points, relative to filers that did not have access to Chapter 7. In addition, I find that the improvement of debtor's balance sheet following Chapter 7 is responsible for most of the findings. These results provide direct evidence that BAPCPA generated negative consequences on those debtors for whom access to Chapter 7 was restricted.

The second paper explores the impact of deposit shocks on bank's balance sheet. The empirical strategy exploits as a natural experiment the lottery jackpot winners of Powerball and Mega Millions. Using hand-collected data, I find that the banks that receive the jackpot winner shock experience a large increase in deposits and total lending. The estimate of the elasticity of small business lending with respect to deposits is 0.876. Consistent with frictions that originate from adverse selection, the set of small and medium-sized banks and those with the most illiquid balance sheets significantly increase loan origination after the winners' shocks.

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Chapter 1

How Does Consumer Bankruptcy Protection Impact Household Outcomes?

The Great Recession was preceded by a substantial accumulation of household debt and followed by a collapse in household net worth. Policymakers and academics have proposed debt forgiveness plans, such as mortgage write-downs, to improve household balance sheets and thus reduce the economic distortions associated with household indebtedness (e.g., Posner and Zingales, 2009; Mian and Sufi, 2015). While it is difficult to directly assess the potential benefits of these proposals, this paper instead indirectly examines this question by evaluating an existing debt forgiveness program. In particular, this paper studies how debt relief provided by Chapter 7 bankruptcy protection, which is a program that allows households to eliminate part of their outstanding debt obligations, influences the household's subsequent real investment choices and financial performance.

The U.S. personal bankruptcy code includes two alternative provisions, Chapter 7 and Chapter 13. Chapter 7 enables debtors to eliminate most of their unsecured debt obligations, but requires them to sell their assets above exemption limits. Chapter 13 allows debtors to keep most of their assets, but their debt obligations are only partially extinguished. In general, Chapter 7 is the more attractive alternative, i.e., most debtors with a choice prefer Chapter 7. However, not all insolvent debtors can qualify for it.[1] To qualify for Chapter 7, the debtor must have an income below certain thresholds, described in more detail below. Despite the fact that each year more than $100 billion in debt relief is granted through the consumer bankruptcy system and that around 12 percent of American households have at a certain point filed for bankruptcy (Mann et al., 2012), limited evidence regarding the subsequent effects of Chapter 7 on debtor outcomes exists.[2]

Identifying the impact of Chapter 7 protection is challenging due to selection and endogeneity concerns. For example, the estimates could be biased because Chapter 7 protection can be potentially correlated with unobserved variables that affect debtor subsequent outcomes, such as job loss (e.g., Keys, 2010) or health shocks (e.g., Himmelstein et al., 2005; Gross and Notowidigdo, 2011; Ramsey et al., 2013). To address these challenges, the empirical strategy exploits, in a regression discontinuity (RD) design, the income thresholds that limit access to Chapter 7.

My analysis is performed on a unique dataset of more than 40,000 bankruptcy cases filed between 2006 and 2009 from 65 (out of 90) district courts in 45 states (see Figure 1). The data is hand-collected from filers' bankruptcy forms and matched with two other datasets allowing the study of debtors' post-filing outcomes. This new dataset contains filer's characteristics such as income, liabilities, etc. which are exploited by the RD approach. Access to this dataset is crucial since the available public-use household survey data (e.g., Panel Survey of Income Dynamics (PSID))

[1] For example, in 2005 Chapter 7 bankruptcies represented 80% of all bankruptcy filings.
[2] See Figure B.1 for the value of the debt relief.

contains no information on the determinants of Chapter 7 eligibility (e.g., disposable income), making it impossible to detect a discontinuity in the data.

A key aspect of my novel research design is to take advantage of income thresholds that prevent some households from filing for Chapter 7 protection, specifically, the means test, imposed by the 2005 Bankruptcy Abuse Prevention and Consumer Protection Act (BAPCPA). In detail, debtors with average gross incomes above the state median need to determine their disposable income and compare it against two thresholds to establish their eligibility for bankruptcy protection under Chapter 7.[3] Under the identifying assumptions that debtors cannot precisely manipulate their incomes and that they do not systematically opt out of filing if they are ineligible for Chapter 7, the income thresholds create quasi-random assignment of filers into receiving Chapter 7 around the thresholds.[4] Thus, using these features of the bankruptcy code together with detailed data on individual filers, I employ a (fuzzy) RD design to estimate first, the causal effect of receiving Chapter 7 protection on subsequent financial performance and investment behavior and second, the marginal effect of debt relief on post-filing outcomes.

I find that Chapter 7 protection helps debtors avoid subsequent financial distress. In particular, over the first six post-filing years, the marginal recipient of

[3]The disposable income is determined by deducting from the debtor's average gross monthly income certain predetermined allowances for housing costs, transport costs, and personal expenses, which are formulated periodically by the IRS.

[4]Those filers that do not receive Chapter 7 are filers who are dismissed from Chapter 7 or would have been dismissed because they do not qualify for Chapter 7 protection (and whose assets are protected by asset exemptions). However, the estimates are robust to include in the control group i) all debtors who did not qualify for Chapter 7, or ii) Chapter 13 filers who also qualified for Chapter 7.

Chapter 7 protection reduces the probability of home foreclosure by 45.2 percentage points.[5] Chapter 7 also decreases the probability of default on tax and non-tax obligations, measured by being subject to a judgment lien, by 39.1 percentage points.[6] It also increases subsequent real investment. The marginal recipient of Chapter 7 is (10.3 percentage points) more likely to create a new business, to become a first time homeowner (14.9 percentage points), and to obtain future secured lending (10.2 percentage points). Finally, in terms of the marginal effect of debt relief, I find that one standard deviation increase in debt relief provided by Chapter 7 leads to an increase in the probability of business creation by 10.79 percentage points and a decrease in the probability of home foreclosure by 51.74 percentage points.

Subsequently, I explore two potential channels for these findings. The first channel is the debtors' improved balance sheets stemming from the discharge of their unsecured debt obligations. The second channel arises from protection against non-judicial collection efforts, such as collection letters, phone calls, and visits at home or work.[7] My evidence suggests that the first channel is responsible for most of the results.[8]

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[5] 86% reduction relative to the control group filers mean. Dobbie and Song (2015) find that Chapter 13 protection reduces the probability of home foreclosure by 127% relative to their control group mean.

[6] Judgment liens are court rulings that provide a creditor the right to take possession of a debtor's real property if the debtor fails to fulfill contractual obligations.

[7] To test for the second potential mechanism I exploit variations in anti-harassment statutes across different states. I find that no distinction can be found in the effects of Chapter 7 on debtors living in states with consumer protection laws that provide the right of action against harassment from abusive creditors compared to those in states without such protection.

[8] This is consistent with models of debt overhang (Myers, 1977; Krugman, 1988) and models of net worth and investment (e.g., Bernanke and Gertler, 1989) that suggest that debt relief can raise the probability of attracting new lending and value-increasing investment.

I also examine key threats to the RD approach. Filers could try to manipulate their incomes, or debtors may opt out of filing if they are ineligible for Chapter 7. Using a rich set of robustness checks on the density of debtors and on filers' characteristics, I find no evidence of heaping at the various thresholds and of discontinuity of observable characteristics at the cutoffs. In addition, I find no evidence of reduction in labor supply (e.g., job tenure, other incomes, etc.) or other possible strategic behaviors from bankruptcy filers (e.g., expenses, household size, joint filing for married couples, etc.). Finally, the reported estimates are robust to a wide variety of specifications (e.g., variety of window bandwidths, functional forms and to the possibility of heaping, etc.).

A caveat to the analysis is that because the empirical strategy is a fuzzy RD, the identified parameter measures the treatment effect for the marginal recipients of Chapter 7 bankruptcy protection at the income discontinuities. To address this issue, I estimate the marginal threshold treatment effect (MTTE) which approximates the impact on treatment effects of marginal change in the threshold ignoring general equilibrium considerations (Dong and Lewbel, 2012).[9] The MTTE estimates suggest that the local average treatment effect for the different post-filing outcomes would slightly decrease if each of the regulatory thresholds were marginally increased. Thus, the effect of Chapter 7 protection would still be large.[10]

[9]Knowledge of these magnitudes may be of interest for policy makers in assessing the likely impacts of changing the bankruptcy eligibility requirements.

[10]It is also important for the external validity of the estimates to examine the characteristics of the compliers. I find that married filers over 40 and unmarried debtors under 40 are more likely to be among the compliers. In addition, these findings provide evidence on the types of filers who are more likely affected by BAPCPA eligibility requirements.

A quantitative assessment of the post-filing effects of Chapter 7 is of interest for several reasons. First, a discussion of the welfare implications requires the quantification of the ex post effects (and ex ante effects) of the personal bankruptcy protection (Livshits, 2015). Thus, any analysis would be incomplete without quantifying the effect this debt relief program has on households. Second, the identified parameters are of interest for policymakers, especially after BAPCA, when the qualifications requirements for Chapter 7 protection became more restricted. Third, the prior literature shows mixed results regarding the benefits of bankruptcy protection (e.g., Han and Li, 2007).[11] For example, on the one hand Porter and Thorne (2006) find, using survey data, that in the first year after a bankruptcy, 25% of debtors struggle to pay routine bills, and 33% are in a financial situation similar to or worse than their situation before bankruptcy. In the same spirit, Kanz (2015) uses quasi-experimental data from India's largest household-level debt relief program (similar to Chapter 7) and finds that debt forgiveness, even though it has a positive impact on households' balance sheets, does not increase investment. On the other hand, other, Dobbie and Song (2015) and Dobbie et al. (2015) find positive effects on Chapter 13 marginal recipients. However, these estimates are for filers before BAPCA, and they may differ from Chapter 7, since the methods for partial repayment of their debt varies across provisions, and this difference could affect debtor's ex post incentives.

This paper is related to a number of strands of literature. By analyzing the effect of Chapter 7 on foreclosure and financial outcomes, it complements recent work

[11] One explanation for the lack of consistent results in prior studies is the shortage of a suitable control group (Dobbie and Song, 2015).

by Dobbie and Song (2015) and Dobbie et al. (2015). Both employ differences in judge leniency as an instrumental variable for bankruptcy protection to identify the impact of Chapter 13 on subsequent earnings, foreclosure, and credit scores of the marginal recipient of protection. However, their judge assignment instrument does not allow them to estimate the effect of Chapter 7 protection. Another key difference is that the present paper, using detailed data from bankruptcy petitions, estimates the marginal effect of debt relief on debtors' outcomes.

Related literature examines the effects of the BAPCPA. Li et al. (2011) estimate the impact of the 2005 bankruptcy reform on mortgage default and foreclosure. In contrast, the present paper uses the income thresholds introduced by the reform to identify the effect of Chapter 7 on debtors' post-filing outcomes.

Finally, by analyzing the impact of debt relief provided by the consumer bankruptcy system on foreclosure, this paper is related to the literature that studies government interventions in mortgage markets such as loan renegotiation (e.g., Agarwal et al. 2011; Piskorski et al. 2010), mortgage modification (e.g., Agarwal et al. 2013; Mayer et al. 2014) and refinancing (e.g., Agarwal et al. 2015) programs.

The paper is organized as follows. Section 2 provides a brief summary of the institutional details of the U.S. personal bankruptcy system, and it discusses the potential benefits of bankruptcy protection. Section 3 describes the data sources and introduces the research design. Section 4 documents the effect of Chapter 7 protection along with internal validity checks, and it discusses alternative explanations for the findings. Section 5 compares the estimates with the findings of prior literature and their external validity, and Section 6 concludes the paper.

1.1 Personal Bankruptcy System

1.1.1 Institutional Background

There are two personal bankruptcy provisions in the United States, Chapter 7 and Chapter 13. Under Chapter 7 bankruptcy, filers have the ability to protect future wages due to the "fresh start" provision.[12] Chapter 7 provides debt relief and protection from wage garnishment in exchange for a debtor's non-exempt assets. This is one reason most filings file under Chapter 7. Under this provision, bankruptcy filers are not allowed to re-file another Chapter 7 case for the next six years (increased to eight by the 2005 Act), and must have a bankruptcy flag on their credit report for 10 years after filing. The key feature of Chapter 7 bankruptcy protection is to provide debtors a financial fresh start through debt discharge. The primary justification for the discharge policy is to preserve human capital by maintaining incentives for work (White, 2009).[13]

In contrast, Chapter 13 bankruptcy filers have to forgo a fraction of earnings in order to repay creditors. Thus, Chapter 13 filers receive protection of most of

[12]The U.S. Supreme Court provided the justification for the fresh start: "from the viewpoint of the wage earner, there is little difference between not earning at all and earning wholly for a creditor." Local Loan Co. v. Hunt, 202 U.S. 234 (1934). However, "this argument has never been carefully analyzed" (White, 2005). The fresh start's effects on incentives to work are non-trivial because there are two competing effects—the substitution effect (no tax on future earnings) and the wealth effect (debtors no longer need to work to service their debt).

[13]While a debtor is in bankruptcy, a judge stops all collection efforts (foreclosure, repossession of other assets, civil suits, garnishment of wages, and dunning) while the court determines which debts are discharged and which debts the borrower must repay by reselling assets or by pledging future income.

their assets in exchange for a partial repayment of debt. Debtors propose their own repayment plans lasting from three to five years (post-2005 they must use all of their law-defined disposable income to pay off debts), with the restriction that the total proposed repayment cannot be lower than the value of their non-exempt assets under Chapter 7. A Chapter 13 bankruptcy flag stays on the credit record for 10 years after filing (Nosal et al., 2014).

2005 Bankruptcy Reform

The number of personal bankruptcy filings in the US rose 5-fold between 1980 and 2005. This dramatic increase led congress to pass the Bankruptcy Abuse Prevention and Consumer Protection Act (BAPCPA) in 2005.

BAPCPA caused two major changes. The first was the adoption of a means test which requires some bankruptcy filers to use some of their future earnings to repay debt. The second major change under the 2005 bankruptcy reform was to raise the cost of filing for bankruptcy by imposing a number of new requirements on both debtors and bankruptcy lawyers.

The new requirements increased debtors' costs of filing for Chapter 7 from a median level of $700 to $1,100 and for Chapter 13, from a median level of $2,000 to $3,000 (Jones, 2008). By making bankruptcy more difficult and costly, the reform caused the number of filings to plummet from around 1.5 million per year in 2004 to only 600,000 in 2006. In addition, the proportion of bankruptcy filings under Chapter 13 rose from 20 percent in 2005 to around 40 percent in 2006 and 2007.

Moreover, consumers must now, as a prelude to access, fulfill a number of new requirements, including enrollment in a pre-petition credit counseling course within the 180-day period prior to filing for bankruptcy and compliance with the mandate to produce a dramatically increased number of personal and financial documents and historical records before filing.[14]

Means Test

As mentioned above, one of the major changes that came with the BAPCPA of 2005 was the introduction of the means test that forces some debtors to file under Chapter 13 and to use their future income to repay part of their unsecured debt. Filers must first calculate their average gross monthly income (AGMI) and compare their AGMI to the appropriate median income figure. The bankruptcy law defines AGMI as the average monthly gross income received during the six-month period that ends on the last day of the month preceding the filing date, whether or not the income is taxable.[15]

After computing the AGMI, households need to convert it to a yearly income figure and compare it with the median family income of their states, adjusted for family size. The census bureau periodically publishes family median income figures

[14]Debtors are now required to submit copies of their past tax returns and copies of all pay stubs for income received during the prior 60-days and take a debt management course before they receive a debt discharge. Now, bankruptcy lawyers must certify the accuracy of all information that debtors provide on their bankruptcy forms, and they can be found liable if debtors provide false information.

[15]AGMI includes income from all sources except: i) payments received under the Social Security Act (including Social Security Retirement, SSI, SSDI, TANF), ii) payments to victims of war crimes, and iii) payments to victims of international or domestic terrorism.

for all 50 states for different household sizes. If the AGMI is lower than the state's median income for a similar household's size, filers automatically qualify for Chapter 7, and they are not subject to the means test in a Chapter 7 bankruptcy filing.[16] However, if their AGMI exceeds the state's median income for a similar household size, filers are required to take the means test in order to see if they qualify for Chapter 7. The means test measures certain expenses and deductions against AGMI to see whether households have any income they can spare to pay debt. Thus, if a debtor fails to pass the means test, meaning there is enough disposable income to propose a repayment plan under Chapter 13, the Chapter 7 case will be dismissed on the basis of "abusing" the bankruptcy law. In other words, the means test determines whether a "presumption of abuse" arises, that is, whether filing a Chapter 7 bankruptcy would be presumed to be an abuse of the bankruptcy laws (Elias and Bayer, 2013).

The means test itself is contained in Official Forms 22A and 22C (see Table B.1 and B.2). To complete the test, filers must use certain predetermined allowances for housing costs, transportation costs, and personal expenses, which are formulated periodically by the IRS and vary according to state, region, and household size.[17] If the debtor's AGMI is above the state's median income, Chapter 13 filers also have to complete the statement of current monthly income using the same IRS standards to compute their disposable income to propose a five-year plan.[18]

[16]If the filer decides to file for Chapter 13 and her AGMI is lower than the state's median income, she may propose a plan that is based on her actual expenses and lasts for only three years.

[17]In addition to the predetermined allowances, filers can also deduct from their AGMI: their mortgage and car loan payments, one-sixtieth of arrears they owe on a secured debt, and one-sixtieth of your priority debts.

[18]The bankruptcy forms are the same for Chapter 7 and 13.

Filers with a monthly disposable income less than $109.58 after all the deductions are entitled to file for Chapter 7.[19] On the other hand, if the filer's income is over $182.50 monthly, abuse is presumed, and the case will be dismissed.[20] Filers with a monthly disposable income above the latter cutoff should convert the case to Chapter 13 in order to have access to bankruptcy protection. If the disposable income is between the two thresholds, filers need to compute their unsecured (non-priority) debt to test whether the amount of disposable income over five years pays at least 25 percent of their unsecured, non-priority debt.[21] If filers fail this test, abuse is presumed and their cases will be dismissed as well.

Even if a filer passes the means test, the bankruptcy law allows the trustee to challenge his Chapter 7 bankruptcy case on the basis of "abuse under all the circumstances." For instance, a filer may have been unemployed during that six-month look-back period, but when he files for bankruptcy, he has just found a new job, which leaves him with a disposable income higher than $182.50 per month. Even though he passed the means test, his actual income when compared to his actual expenses leaves him enough disposable income every month that would fund a Chapter 13 repayment plan.

Another doctrine that may affect Chapter 7 eligibility is what's commonly called "bad faith." Under this doctrine, a judge can dismiss a bankruptcy filing if he or she believes the case was filed for reasons other than to get a fresh start, or

[19] Before 2007, this cutoff was $100 per month.

[20] Before 2007, this cutoff was $166.67 per month.

[21] The unsecured non-priority debt can be found in the Statistical Summary of Certain Liabilities form.

if the filer engaged in pre-bankruptcy behavior that is inconsistent with the need for a bankruptcy debt discharge. For example, the court can dismiss a case because the debtor purchased an expensive good shortly before filing in order for the high monthly payments on that good to allow him to pass the means test.

To ensure that all the documents filed by debtors are accurate, the bankruptcy trustee assumes legal control of all property and debts as of the date of filing. The bankruptcy trustee's primary duties are: 1) to see that nonexempt property is seized and sold for the benefit of unsecured creditors, 2) to make sure that the paperwork submitted is accurate and complete, and 3) to administer the case for the court. The trustee is required, under the supervision of the U.S. trustee, to assess all bankruptcy papers for accuracy and for signs of possible fraud or abuse of the bankruptcy system.

Random audits are also performed to verify the accuracy of filers' submitted documents. One out of every 250 bankruptcy cases is to be audited under the new bankruptcy rules. In addition, the bankruptcy trustee has an active role in those cases where bankruptcy papers or any testimony at the creditors' meeting might indicate that the filer's AGMI is more than the median income for their state, the filer earns enough actual income to support a Chapter 13 plan, and the filer has apparently engaged in illegal actions that warrant investigative follow-up (such as perjury).

1.1.2 Personal Bankruptcy Protection: Possible Benefits

Chapter 7 protection provides discharge of the debtor's unsecured debt thus improving the debtor's balance sheet. The standard model of debt overhang (Myers,

1977) suggests that excessive leverage deters new productive investment, especially if the new investment is financed through junior claims to the current debt.[22] This is because with risky debt, part of the increase in value generated by the new investment goes to the existing creditors and is therefore unavailable to repay those who financed the investment.[23] Thus, a large debt burden can lead to underinvestment.[24]

Furthermore, previous debt obligations reduce the net worth which can decrease the probability of new financing. Bernanke and Gertler (1989) showed how shocks to the net worth of borrowers reduce their ability to borrow.[25], [26] Asymmetry of information between the borrower and the lender in the context of Townsend's (1979) costly-state-verification model, in which investors can only verify the cash flows by paying fixed auditing costs, means that lenders require borrowers to have equity in the project which can generate deadweight losses (expected agency costs).[27] Thus, changes in the net worth of borrowers (e.g., unsecured debt relief from bankruptcy) can affect their overall capacity to borrow.[28] A negative shock to the net worth

[22]In the context of international finance, Krugman (1988) suggests that the debtor's incentives may be distorted by the presence of a debt overhang and that the distortion will be reduced if creditors provide debt forgiveness.

[23]In addition, the new investment cannot be financed because renegotiation with previous creditors is not feasible.

[24]Melzer (2010) examines how mortgage debt overhang affects homeowners financial decisions. He shows that negative equity homeowners reduced significantly on home improvements and mortgage principal payments.

[25]See also Kiyotaki et al. (1997).

[26]Moral hazard can also lead to credit rationing if prior claims on assets decreases the net worth below the level required to satisfy the lenders individual rationality constraint (Tirole, 2006).

[27]The lower the net worth of the borrower, the more he must borrow, and the greater the likelihood that auditing costs will be incurred. Therefore, less net worth leads to greater deadweight costs and lower investment.

[28]Negative shocks on debtors' net worth can also affect consumption. See Eggertsson and Krugman (2012).

reduces overall investment in the economy even if there are still plenty of value-increasing projects available as before.

Thus, excessive household indebtedness can distort economic decisions (i.e., investment and labor supply decisions). Chapter 7 bankruptcy protection tries to eliminate these distortions by reducing debt burdens and improving the balance sheet, which could increase investment and raise the likelihood of attracting new lending.

Another benefit is that bankruptcy protection stops non-judicial collection efforts such as collection letters or phone calls and visits at home or work. Debtors without bankruptcy protection could ignore collection letters and calls, change their telephone number, or move without leaving a forwarding address. However, a borrower in default without bankruptcy protection under the federal Fair Debt Collection Practices Act (FDCPA) can request that a debt collector stop non-judicial collection efforts entirely. Even if the debtor moves without leaving an address, it would not prevent the collector from trying to collect. Collectors that do not have a consumer's address can legally contact the debtor's employers or friends to inquire about his address. In contrast, if the collector does have the debtor's address, then contacting employers or friends is illegal. Thus, there is no evidence that debtors without bankruptcy protection move more often or change their phones to avoid collectors.

Finally, the FDCPA does not apply to original creditors, who are under other types of regulation. The Federal Trade Commission can use administrative actions against creditors for overly aggressive debt collection, and states have their own statutes specifically regulating non-judicial debt collection (Dawsey et al., 2013).

1.2 Data Collection and Research Design

1.2.1 Data Collection

Households declaring bankruptcy must reveal several financial and demographic details to the court at the time of filing. Such documents are available through the Public Access to Court Electronic Records (PACER) system. In order to be granted a fee waiver from PACER, I applied to 89 out of 90 bankruptcy district courts (Puerto Rico was not considered). I received fee exemptions to the records of 65 bankruptcy district courts in 45 states. Those courts are evenly spread throughout the U.S. (see Figure 1.1).

I constructed a random sample of around 45,000 filings from 2006 to 2009. I downloaded 1,257,785 pages of PDFs from the legal documents of those filing, hand-collected and parsed the documents, and then cleaned them into usable data.[29] From the bankruptcy forms, I recorded characteristics of households such as: gross income, disposable income, household size, expenses, address, employment status, employment tenure, assets, liabilities, among others. This is a novel dataset, which has not been collected in such richness and magnitude before.[30]

[29] I collected 44,862 cases (and over 51,000 documents) given the cases restriction from the courts' exception fee.

[30] To my knowledge, only Agarwal et al. (2010) and Gross et al. (2014) have collected data from electronics filing documents. However, in the first case the sample size was 3,000, and in the second case the samples size was 6,487 cases filed in 2001 and 2008 from 10 district courts. In addition, both papers did not collect the main data used in this paper (e.g., 22A and 22C forms).

To ensure that I have at least five years of post-filing outcomes for all debtors, I have restricted the sample to first-time filers between 2006 and 2009.[31] I randomly selected an equal number of cases per year. From the 44,862 cases randomly selected, some filers had failed to submit some of the required documents (e.g., form 22A or 22C), and those restrictions left me with 38,856 filings in 65 bankruptcy district courts, that were split into 67% Chapter 7 and 33% Chapter 13 filings.

For the set of debtors' outcome variables, I have used data from two purchased proprietary sources. The first includes foreclosure data from RealtyTrac, which collects data from legal documents submitted by lenders during their foreclosure process. There are five types of filings collected by RealtyTrac. The first two are filings that are done before a foreclosure auction: a notice of default (NOD) and a *lis pendens* (LIS), or written notice of a lawsuit. Two of the filings are directly associated with a foreclosure auction: a notice of a trustee sale (NTS) and a notice of a foreclosure sale (NFS). RealtyTrac also collects information on whether the foreclosed home is purchased by the lender at auction or is real-estate owned (REO). I have been able to successfully match 48.62% of the filers using the real estate and addresses data provided in the bankruptcy forms.[32]

The second source for outcome variables is the LexisNexis Public Records. LexisNexis provides a panel data set of records for individuals over time. Specifically, I have obtained data indicating gender, race, address, judgment lien, real property

[31] I select 2006 as my starting year, since the BAPCPA reform took place in 2005, and my empirical strategy relies on the means test adopted after October 2005.

[32] From Schedule A and Voluntary Petition of the bankruptcy forms.

records, bankruptcy information, personal business and criminal filings data. I have been able to successfully match 99.15% of filers using their names and SSNs provided in the bankruptcy forms.[33]

Sample Description

Table 1.1 reports the summary statistics for all first-time filers between 2006 and 2009. I divide filers' characteristics into three groups: general characteristics, assets, and liabilities. All monetary values are expressed in (year) 2000 dollars.

The data show that the average debtor has a household size of around three family members. In terms of marital status, 49.4% of filers are married. In addition, not all married debtors file for bankruptcy jointly, though 34.5% of the filers do file jointly.[34] Filers earn an average of $35,954 per year. Relative to gender, 67.4% of debtors (as the main filer) are male.[35] Around 15% of filers have criminal records (e.g., arrest records, court conviction records, traffic violations) and 7% have their own business. Over the same period, debtors have had $105,272 in real property on average. Finally, the typical bankruptcy filer carries around $175,943 in debts.

[33] From the Voluntary Petition form.

[34] Married couples are allowed to file bankruptcy together with one petition. Filing jointly means that the combined property and debts are all part of the same bankruptcy filing.

[35] For the sub-sample of filers for which I found race data, 78% are white.

1.2.2 Research Design

I recover estimates of Chapter 7 bankruptcy protection using a novel empirical strategy based on a RD design that compares outcomes for filers with incomes just below the incomes cutoffs to qualify for Chapter 7 protection to outcomes for filers with incomes just above the cutoffs.[36] The idea behind the RD approach is that if access to Chapter 7 protection changes discontinuously at the income thresholds, then the causal impact of this access can be identified. Intuitively, suppose that filers with incomes close to the cutoffs on either side are comparable in terms of the observable and unobservable (to the econometrician) determinants of debtors' outcomes (e.g., foreclosure), but that those just below the cutoff are more likely to receive Chapter 7 protection. Under this assumption, filers with incomes just above the cutoff will provide an adequate control group for debtors just below, and any difference in their outcomes can be attributed to access to Chapter 7.

In fuzzy regression discontinuity (FRD) designs, threshold-crossing causes a discontinuous jump in the probability of treatment, but this jump is not from one to zero (i.e., treatment is not a deterministic function of the running variable).[37] Because filers whose access to Chapter 7 responds to threshold-crossing may differ from other debtors with similar incomes the estimates I have obtained should be in-

[36] The point estimates could be biased if OLS is used to estimate the effect of Chapter 7 because Chapter 7 bankruptcy protection might be correlated with unobservable variables that affect debtor's ex-post outcomes such as job loss (e.g., Keys, 2010) or health shocks (e.g., Gross and Notowidigdo 2011; Himmelstein et al. 2005; Ramsey et al. 2013).

[37] The empirical strategy is a fuzzy RD approach due to imperfect compliance. For example, there are special circumstances (e.g., serious medical condition or an order to active duty in the Armed Forces) in which the judge could grant Chapter 7 protection to a debtor who fail the means test.

terpreted as a local average treatment effect (LATE) for filers at the margin of access (i.e., group of compliers with the income eligibility thresholds from the bankruptcy law as Angrist et al. (1996)).

The adoption of the means test provides three different cutoffs. The first one determines the automatic qualification for Chapter 7 (see Figure 1.2). Thus, filers with AGMI below the state median income do not have to take the means test for Chapter 7 protection. I call this cutoff 1 (C_1). In addition, those filers with AGMIs above the state median income but with disposable income lower than \$109.58 monthly can also file for Chapter 7. This is called cutoff 2 (C_2). Finally, debtors with AGMIs above the state median income and disposable income lower than \$182.5 monthly, and whose amount of disposable income does not pay at least 25% of their (non-priority) unsecured debt, can also file for Chapter 7. I refer to this as cutoff 3 (C_3).[38]

Because the thresholds are public data, debtors probably know them in advance, so this feature of the setting could lead to two phenomena.[39] First, debtors could manipulate the different running variables, or they may opt out of filing if they are ineligible for Chapter 7 protection. However, I conduct a range of tests that show no evidence of manipulation or selective filing at the different thresholds. Second,

[38]An alternative strategy is to consider the cases between the second and the third cutoff and try to create another discontinuity that exploits the constraint that limit access to Chapter 7 to those debtors whose disposable income pay at least 25 percent of their (non-priority) unsecured debt. However, because this constraint only affects debtors between C_2 and C_3 (e.g., it is not binding for those debtors with disposable income lower than \$109.58 or greater than \$182.5), it only uses around 40 percent of the debtors relative to the pooled specification explained below.

[39]The data for the state median income (first cutoff) comes from the Census Bureau, and it is updated quarterly.

those debtors that would fail the means test would probably file directly for Chapter 13. Recall that the introduction of the means test was to restrict access to Chapter 7. Otherwise, if the debtor still files for Chapter 7, the trustee will file a motion for the case to be dismissed (which will be subsequently approved). Since the bankruptcy forms are the same for Chapter 7 and 13, the setting provides the data to determine those Chapter 13 cases that did not qualify for Chapter 7 (i.e., would have failed the means test).[40]

As a support for the second point, from the sample's distribution, the number of Chapter 7 filers drops 16 times above the disposable income threshold of \$182.50 (C_3), and the number of Chapter 7 cases dismissed above this threshold increases 5 times. Thus, the empirical strategy uses those filers close to the thresholds who did not qualify for Chapter 7 and who are also non-homeowners or whose home equity was protected by the homestead exemption as part of the control group.[41] The assumption is that these filers make a reasonable control group.[42] I below show as support for this assumption that there is no evidence of difference in a set of pre-treatment covariates between debtors who were close to the threshold and filed

[40]If the debtor's AGMI is above the state's median income, Chapter 13 filers also have to complete the statement of current monthly income to compute the debtor's disposable income.

[41]Filers that would have to give up their home in Chapter 7 (because their home equity is higher than their homestead exemption) might be inclined to file for Chapter 13 regardless of whether they qualify for Chapter 7. This is because Chapter 13 is most often used as a home saving procedure (White and Zhu, 2008). However, the estimates are similar if all those filers who did not qualify for Chapter 7 are included in the control group. In addition, the estimates are qualitatively similar if the control group includes debtors that file for Chapter 13 even though qualifies for Chapter 7.

[42]If the value of debtor's home is covered by homestead exemption, Chapter 7 is the best option, since by getting rid of most of other debts, maintaining the mortgage is more bearable for debtors (Caher and Caher, 2011).

for Chapter 13 not qualifying for Chapter 7 and two other groups: Chapter 7 filers, and Chapter 7 filers whose cases were dismissed.[43]

Finally, the control group comprises: 1) debtors who filed for Chapter 7 and whose cases were dismissed, 2) filers who filed directly for Chapter 13 and did not qualify for Chapter 7 and whose assets were protected, and 3) debtors who filed first for Chapter 7 then converted their cases to Chapter 13 after having their cases dismissed.[44] Thus, like Dobbie and Song (2015), I estimate the impact of receiving Chapter 7 protection relative to both no bankruptcy protection and protection via Chapter 13.

I estimate specifications of the following form. Let y_{it} be debtor's ex-post outcome (e.g., foreclosure) for individual i in period t. Let B_{it} be an indicator variable for Chapter 7 protection (i.e., 1 if the Chapter 7 case is discharged), $\tilde{R}_{it}$ is the running variable and represents the distance between the debtor's (gross or disposable) income and the respective cutoff faced, and $f()$ is a smooth function. The parameter of interest is τ which is the local average treatment effect for each regression.[45] Neither covariates nor any fixed effects are needed for identification. I include a set of covariates (e.g., age at filing, marital status, etc.) to increase the precision of the point estimates. The estimating equation is then:

[43] See Table 1.3.

[44] In order to be discharged from debt under Chapter 13, debtors have to complete their repayment plans which may last from three to five years depending on their disposable incomes. In the sample, 49% of debtors successfully completed their repayment plans and had their remaining unsecured debt discharged (i.e., received Chapter 13 bankruptcy protection).

[45] The identified parameter measures the treatment effect for filers who receive Chapter 7 protection if and only if their (gross or disposable) income is below their respective cutoff (i.e., subpopulation of compliers).

$$y_{it} = \alpha + \tau B_{it} + f(\tilde{R}_{it}) + \varepsilon_{it} \tag{1.1}$$

There are two ways to estimate the parameter τ in an RD design. First, one can impose a specific parametric function for $f()$, using all the available income data to estimate the above equation via ordinary least squares (typically referred to as the global polynomial approach). Alternatively, one can specify $f()$ to be a linear function of the running variable and estimate the equation over a narrower range of data, using a local linear regression. Following Hahn et al. (2001), Porter (2003), Imbens and Lemieux (2008) and Gelman and Imbens (2014), in this paper the preferred specification is drawn from local linear regressions within an specific bandwidth on either side of the cutoff suggested by the procedure in Imbens and Kalyanaraman (2011). The estimator of the impact of Chapter 7 protection is constructed using kernel-based local linear regression on either side of the threshold (i.e., equal weight for all observations in the estimation sample). This estimator in the RD literature is non-parametrically identifiable under mild continuity conditions, and such regression estimators are particularly well-suited for inference in the RD approach because of their good properties at the boundary of the support of the regression function (Calonico et al., 2014). In addition, heteroskedastic adjusted errors are used in all regressions.[46] Finally, the interpretation of τ as an effect for compliers requires the monotonicity condition where there are no individuals who received Chapter 7 protections if and only if their income is above the respective cutoff (Angrist et al.,

[46]Since the running variables in the setting are continuous (Imbens and Lemieux, 2008; Lee and Card, 2008).

1996), this prerequisite seems plausible in the present setting.[47]

I instrument for B_{it} with Z_{it}, which is an indicator variable if the debtor's gross income (or disposable income) is below the specific threshold. Recall that BAPCPA of 2005 generated three cutoffs that determine access to Chapter 7.[48] In addition, I take advantage of the richness of the present setting and use all three cutoffs to identify the causal effect of interest. Thus, an additional benefit of the setting is that it allows the use of the three thresholds to estimate the heterogeneity of the treatment effect along different income levels.[49]

For this empirical strategy to produce consistent estimates, it requires several identifying assumptions to hold. The threshold-crossing variable (Z_{it}) must be conditionally uncorrelated with unobservable outcomes determinants (ε_{it}) when incomes are close to the cutoffs. In this case, this assumption will hold if debtors do not attempt to manipulate their gross income (or disposable income) or if manipulation is imprecise, and debtors do not systematically opt out of fling if they are ineligible for Chapter 7. How reasonable are these assumptions? In section 4.1 I discuss how plausible are these assumptions in this setting.

The first cutoff (C_1) allows to file for Chapter 7 automatically if the filer's

[47]Because those individuals would have also received Chapter 7 if they are below their respective cutoff.

[48]The IV exclusion restriction also has to hold. This is especially a concern for the first cutoff if threshold- crossing affects eligibility with other programs that may also depend on the debtor's income relative to the state median income. However, for programs like Medicaid, the eligibility depends on the federal poverty level. Thus, exclusion restriction plausibly holds in this setting including the first cutoff.

[49]The average gross income of households around cutoffs C_2 and C_3 is \$57,203 while around C_1, it is \$41,980.

AGMI is lower than the state median income. I define $\tilde{R}_{1isjt} = (I_{it} - C_{1sjt})$ and $Z_{1isjt} = 1[\tilde{R}_{1ijst} \leq 0]$, where I_{it} is the AGMI for household i and C_{1sjt} is the state median income in state s, adjusted by household size j in period t. Because each state has different median income levels which also vary by household size and time, I use a pooled specification across state cutoffs.[50] The first stage estimating equation associated with C_1 is:

$$B_{isjt} = \gamma_0 + \gamma_1 Z_{1isjt} + \gamma_2 \tilde{R}_{1isjt} + \gamma_3 Z_{1isjt} \tilde{R}_{1isjt} + \nu_{isjt} \tag{1.2}$$

Similarly for C_2, I define $\tilde{R}_{2it} = (DI_{it} - C_{2t})$ and $Z_{2it} = 1[\tilde{R}_{2it} \leq 0]$, where DI_{it} is the monthly disposable income for filer i and C_{2t} equals \$109.58 per month if $t \geq 2007$ and \$100 per month if $t = 2006$. The first stage estimating equation associated with C_2 is:

$$B_{it} = \delta_0 + \delta_1 Z_{2it} + \delta_2 \tilde{R}_{2it} + \delta_3 Z_{2it} \tilde{R}_{2it} + \epsilon_{it} \tag{1.3}$$

Finally for C_3, I define $\tilde{R}_{3it} = (DI_{it} - C_{3t})$ and $Z_{3it} = 1[\tilde{R}_{3it} \leq 0]$, where C_{3t} equals \$182.50 per month if $t \geq 2007$ and \$166.67 per month if $t = 2006$. The first stage estimating equation associated with C_3 is:

$$B_{it} = \lambda_0 + \lambda_1 Z_{3it} + \lambda_2 \tilde{R}_{3it} + \lambda_3 Z_{3it} \tilde{R}_{3it} + u_{it} \tag{1.4}$$

[50] The data contain thousands of cutoffs for C_1. For the sake of statistical power, I focus on regressions which pool data across cutoffs relying on the fact that $(I_{it} - C_{1sjt})$ measures the distance between each debtor's AGMI and their respective state cutoff.

While I present the results for the second and third cutoffs individually, the preferred specification pools both cutoffs to gain statistical power.[51] Pooling requires the treatment intensity to be of comparable magnitude in order to interpret the size of estimated impacts. Since the second and the third cutoffs are relatively close, the difference in debt relief is small and not statistically different ($12,430 and $13,651 for the second and third cutoffs, respectively). In addition, with similar treatment intensity, it seems reasonable to expect similar treatment effects for the second and third cutoff, which I find in Table B.4.

However, treatment effects need not be the same across cutoffs. If treatment effects are heterogeneous, the pooled approach identifies the weighted average across cutoffs of the local average treatment effects (Cattaneo et al., 2015; Litschig and Morrison, 2013).[52]

For the pooled analysis, I need to make observations comparable in terms of the distance from their respective cutoff.[53] To this end, I partition the disposable income support into two segments, above and below the following segment variable,

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[51]Since the running variable in both thresholds are the distance between the debtor's disposable income and the respective disposable income threshold. In addition, treatment effects need not be the same across cutoffs. If treatment effects are heterogeneous, the pooled approach identifies the treatment effect average across cutoffs.

[52]The ability to combine different local effects to estimate an average effect depends on how these treatment effects are (Bertanha, 2015). For example, Pop-Eleches and Urquiola (2013) study the effects of assignment of students to more or less elite high schools based on test scores where every town has its own admission cutoffs scores. Thus, different school qualities expose students to different treatment doses across cutoffs. By comparing students with test scores just below the cutoff to students with scores just above the cutoff, RD design allows identification of the impact of school quality on the average academic achievement of those students with test scores equal to certain cutoff (see Hastings et al. (2013) for a different example).

[53]For similar applications, see for example Anderson and Magruder (2012) and Litschig and Morrison (2013).

let seg_t equals \$133.33 if $t = 2006$ and seg_t equals \$146.04 if $t \geq 2007$. The running variable for this analysis is:

$$R_{it} = \begin{cases} DI_{it} - C_{2t} & if\ DI_{it} \leq seg_t \\ DI_{it} - C_{3t} & if\ DI_{it} > seg_t \end{cases}$$

The estimating equation for the reduced form (or intention-to-treat) in this case is:

$$y_{it} = \rho 1[R_{it} \leq 0]1_p + [\alpha_{10}R_{it} + \alpha_{11}R_{it}1[R_{it} \leq 0]]\,1_1 + [\alpha_{20}R_{it} + \alpha_{21}R_{it}1[R_{it} \leq 0]]\,1_2 + \sum_{j=1}^{2}\beta_j 1_j + \xi_{it} \tag{1.5}$$

where $1_1 = 1[DI_{it} \leq seg_t]$, $1_2 = 1[DI_{it} > seg_t]$ and $1_p = 1_1 + 1_2$. Equation (5) imposes a common effect ρ. As mentioned, when estimating the above equations, I restrict my sample to filers with AGMIs or disposable incomes (whichever applies depending on the cutoff selected) within a relatively narrow window around the cutoff value. The goal of this restriction is to avoid identifying local effects caused by variation far from the cutoff value (Imbens and Lemieux, 2008).[54] As is standard in the RD literature, I present results for a variety of window bandwidths, including the optimal bandwidth, and functional forms.

[54]For each separated threshold, I restrict the bandwidth to be the same above and below the cutoff. However, to increase power for the pooled sample I do not restrict the bandwidth to be the same above and below the cutoff.

1.3 Results

1.3.1 Internal Validity Checks

A standard concern with any RD design is the ability for individuals to precisely control the assignment variables (i.e., the average gross monthly income received during the six-month period prior to the filing or the debtor's disposable income). However, under BAPCPA, debtors that want to behave strategic face a complicated planning system that involves their wealth, income, expenditures, and debt.[55]

A first concern is misreporting by debtors. However, the bankruptcy law has several mechanisms to avoid misreporting. First, the trustee seeks to dismiss (or convert) Chapter 7 bankruptcy filings on the grounds of presumed abuse, thus debtors are now required to submit copies of their past tax returns and pay stubs, which are carefully reviewed to avoid any misrepresentation, along with a statement of the average monthly gross income over the previous six months. Second, attorneys must investigate their clients' bankruptcy petitions and certify that the petitions do not constitute an abuse (attorneys may be sanctioned if they file petitions that are dismissed because of abuse). Third, because of filing fees and waiting periods, debtors cannot file for bankruptcy more than once each six months. Fourth, cases are selected

[55]Debtors that are allowed to file under Chapter 7 could have the incentive to shift wealth from nonexempt categories to exempt categories in order to reduce their obligation to repay. However, BAPCPA eliminated many of the asset-sheltering strategies. For example, if the debtor shelters financial assets by using them to pay for home improvements, the increase in the value of their homes is not exempt under the homestead exemption unless the improvements were made more than two and half years prior to filing. In addition, if debtors convert nonexempt assets into home equity by paying down their mortgages, the additional home equity is not exempt unless the conversion occurred more than ten years prior to filing.

for random audits (one out of every 250 bankruptcy cases). Finally, filers must swear under penalty of perjury that they have been truthful on their bankruptcy forms. The most likely consequence for failing to be scrupulously honest is a dismissal of the bankruptcy case, but the filer could be also prosecuted for perjury if it is evident that he deliberately lied.

Debtors can try to reduce their incomes by reducing their labor supply enough to pass the means tests to file under Chapter 7. However, it is not clear that debtor can precisely control their incomes due to optimization frictions, such as search costs and hours constraints set by their firms, which might lead debtors to not adjust their labor supply. Another potential strategy is that debtors could also avoid taking the means test by increasing their family size because the median state income levels are higher for larger families. Though, some courts count only dependents as part of the household. Below I test for both potential strategies.

Another concern is that debtors delay filing to precisely manipulate their income. Debtors in general file for bankruptcy because they have fallen behind on their payments, so they are likely subject to wage garnishment, notice of foreclosure, intensive phone calls, dunning letters, and a variety of other judicial and non-judicial debt collection techniques in an effort to induce debtors to pay. These mechanisms could reduce the possibility that individuals can delay filing for bankruptcy in order to perfectly manipulate their income. Moreover, another problem for filers is that the exact locations of the state median income cutoffs changes every quarter (see footnote 38). However, debtors could potentially time better for filing in states in which wage garnishment is banned for most debts (i.e., Texas, Pennsylvania, North

Carolina, and South Carolina). I test for this last concern below.

There could also be the concern that debtors move to state with more generous exemptions (or higher state median income). However, after 2005 there are several resident requirements that filers have to meet before claiming a state's exemptions. The debtor has to make his current state his home for at least two years to use that state's exemptions. In addition, if the debtor has lived for more than 91 days but less than two years, he has to file in the state and use the exemptions of the state where he lived, for the better part of the 180-day period immediately, prior to the two-year period preceding his filing.

Relative to the means test, since most of the consumption allowances are determined by the IRS, then debtors cannot pass the means test (or reduce their repayment obligations) by increasing expenditures. However, BAPCPA allows some additional expense deductions that are based on actual consumption, so debtors could potentially pass the means test (or reduce their obligation to repay) by increasing expenditures in these categories. Below I test for whether there are differences in additional expenses at the cutoffs, which is what we should expect if debtors are strategically increasing their expenses.

In general, BAPCPA made planning for bankruptcy more complicated and costly. In addition, debtors that want to behave strategically must plan far in advance rather than wait until just before filing. Thus, since planning for bankruptcy is more costly, fewer debtors will behave opportunistically (White, 2007).

Another concern for the empirical strategy is that debtors may opt out of filing for bankruptcy. This can be an option for very low income debtors and for those who do not qualify for Chapter 7 (i.e., would have failed the means tests). Debtors who file for bankruptcy protection are usually in default, so they could face judicial and non-judicial collection practices. Thus, it is unclear whether a debtor in default could opt out of filing for bankruptcy. If the debtor does not expect to be productive in the near future then he might choose not to file for bankruptcy protection. However, this hypothesis can be a concern in the case of very low income filers who do not earn enough, for creditors to choose to institute wage garnishment. In addition, because the empirical strategy uses for identification those debtors around (and above) the state median income, it is plausible to assume that they have enough income to trigger collection if they fall in default. I evaluate this hypothesis relative to high income debtors who may opt out of filing by testing whether the density of debtors is a continuous function of the Chapter 7 eligibility cutoffs, especially at pooled cutoff, and by examining the continuity of observable filer characteristics at the cutoffs. Table 1.2, as I describe below, shows that there is no difference in pretreatment covariates for these thresholds. Finally, under this hypothesis, the point estimates would be downward biased because only debtors who are ineligible and expect to benefit the most from bankruptcy protection would file.

Since extensive manipulation of AGMI (or disposable income) would bias the estimates, I check for any evidence of sorting, notably discontinuous income distributions. In addressing these concerns, I consider two tests that are standard in the regression discontinuity literature. The main test looks for discontinuities in the density of AGMI and the disposable income at each cutoff point (McCrary, 2008).

The argument is that if some filers manipulate their AGMIs by perfectly timing the bankruptcy filing date to fall below the state median income threshold, the density of the filer distributions will be significantly higher just below the cutoff than just above. Figure 1.3 shows the density of the two running variables for the three cutoffs for the random sample of filers between 2006-2009. The McCrary (2008) test shows no significant break in the AGMI or disposable income densities with (absolute value) test statistics equal to 0.967 and 1.177 respectively, which are statistically not significantly different from zero at any conventional level.[56]

To further test the density distribution, following Zimmerman (2014), I provide another informative visual test for income manipulation for the first cutoff. In absence of manipulation, the test should show a relative continuity in the ratios of the conditional densities to the unconditional density (i.e., $\frac{f(\tilde{R}|x)}{f(\tilde{R})}$). Assume that observable and unobservable outcome determinants (x, ε) have some continuous unconditional joint distribution $h(x, \varepsilon)$. A sufficient condition for unbiased RD estimation is that the conditional joint distribution $h(x, \varepsilon|\tilde{R})$ be continuous in $\tilde{R}$. Using Bayes's rule,

$$h(x, \varepsilon|\tilde{R}) = h(x, \varepsilon)\frac{f(\tilde{R}|x, \varepsilon)}{f(\tilde{R})}$$

[56]The setting and the data also enable me to: i) test whether debtors are timing by testing for potential manipulation in states that ban wage garnishment, since filers could delay filing for bankruptcy, and ii) by excluding these states, estimate the potential bias that manipulation of the running variables could generate. In untabulated results, I find that in those states in which wage garnishment is banned for most debts (i.e., Texas, Pennsylvania, North Carolina, and South Carolina), only in Texas and within Texas only one district courts (out of three) the McCrary test rejects the null hypothesis of no manipulation at 10% only in the first cutoff. In addition, Table B.6 show that the point estimates remain unchanged when all four states are excluded.

Therefore, $h(x, \varepsilon|\tilde{R})$ is continuous if the ratio of the conditional to unconditional densities is continuous. This test is considered more direct than the McCrary, which is based on $f(\tilde{R})$, since it focuses specifically on the object that determines the continuity of debtor outcome determinants in income. The intuition is that if the discontinuity in the income distribution is due to a process that is plausibly exogenous to the determination of the treatment, any jumps in the conditional distributions should be matched by discontinuous jumps in the unconditional distribution. The ratio of the two densities should be continuous even if each individual density is not. Figure B.2 presents the density ratios described in the above equation for three different pretreatment covariates: household size, age at filing, and marital status. Each point represents the ratio of the proportion of observations in the sample of filers with the stated characteristic to the proportion of all observations within each bin. Consistent with a valid RD, each density ratio is continuous around the cutoff value.

The continuity of the density ratios is closely related to the second standard test of RD validity, which is to test for the balance of observable covariates across the threshold. This second main test estimates equations (2)-(5) for a host of pretreatment covariates. This test has become standard in the RD literature as an alternative and is often the preferred approach for testing the validity of the RD design (Lee and Lemieux, 2010). Table 1.2 reports the point estimates of the effects of threshold crossing on baseline characteristics. Each column presents the local linear regression estimates. To alleviate any concerns over bandwidth, I present the baseline characteristics over varying bandwidths. There is no statistical evidence of discontinuities in the pretreatment covariates, out of the 54 hypothesis tests in Table 1.3 for all there thresholds none reject the null. In particular, for variables in which there could also be strategic behavior such as household size or joint filing, there are no significant differences for each threshold (nor for the pooled threshold). A visual representation for the first and pooled cutoffs is provided in Figure 1.4 and 1.5 (see Figure B.3 and B.4 for the second and third cutoffs, respectively). Additionally, in Table B.3, I also test for discontinuities in the pooled cutoffs in expenses allowed under IRS, additional expenses and deduction for debt payments in the means test, and I find no evidence of discontinuities.

One may think that some filers have incentives to decrease labor supply as a mechanism to reduce their income and fall below the thresholds. To understand the potential for identification problems caused by manipulation, consider a simple labor supply model. Debtors strive to maximize the present discounted value of utility from income. Each debtor may choose to work full-time, part-time, or not at all. Debtors are eligible for Chapter 7 bankruptcy protection if their AGMIs are lower than the state median income or if they pass a means test based on their disposable income. If the program did not exist, debtors would supply full labor. However, the existence of those thresholds raise the possibility that debtors can manipulate the running variable, withholding labor supply in order to meet the means test and gain access to Chapter 7 protection.

For highly compensated debtors with AGMIs (or disposable income) beyond the respective thresholds, reducing labor supply is never worth it, because even with part-time work, the debtor could not satisfy the means test. Resigning to their current job may also be costly since firms may deny future employment upon learning the applicant has filed for bankruptcy.[57] Similarly for poorly paid debtors with AGMIs or disposable incomes below the respective threshold, the model predicts no manipulation, but for a different reason: such a debtor has access to Chapter 7, even if working full-time. However, those debtors with AGMIs or disposable income very close to the cutoff may indeed find it worthwhile to reduce labor supply, because they would otherwise fail the means test. These debtors would reduce their labor supply in response to the bankruptcy protection requirements if the utility of receiving protection under Chapter 7 (instead of the alternative Chapter 13) was higher than the cost of reducing their labor supply.

To further test this hypothesis, I hand-collect data from pay stubs for each bankruptcy case around the thresholds to compare the income volatility for those filers below and above each cutoff. Table 1.2 shows that the null hypothesis that income volatility is equal among those debtors cannot be rejected. In the same spirit, using hand-collected data from the bankruptcy documents (i.e., Schedule I), I also test if the job-tenure differs between those debtors above and below the cutoffs.[58] The rationale behind this test is that if individuals are manipulating the running variable through labor supply, then the tenure for those below the cutoff should be significantly different from those above the cutoff. Table 1.2 reports that there are no significant differences between those debtors above and below the thresholds.

[57] Federal, state, and local governmental units cannot legally discriminate against filers simply because they have filed for bankruptcy. However, the rules are more lax when it comes to private entities and businesses (Elias and Bayer, 2013).

[58] Among the information bankruptcy filers should submit in Schedule I are their occupation, name of employer, and tenure of the main job.

In addition, another way to decrease labor supply is to give up second sources of income. I test whether there are significant differences in other incomes between those above and below the cutoff. Table 1.2 reports that there are no significant differences between those debtors with other incomes above and below the thresholds. Finally, I test for differences in pre-treatment covariates between the filers close to the thresholds who did not qualify for Chapter 7 and two other groups: the Chapter 7 filers and the Chapter 7 cases that were dismissed. Table 1.3 reports the estimates showing that there is no evidence of differences in observable characteristics.[59]

Overall, these findings reject the hypothesis of strategic threshold crossing in favor of a non-strategic sorting hypothesis. I have shown that the baseline characteristics are smooth around all thresholds. Indeed, if debtors were strategically manipulating results, then this phenomenon should occur at cutoffs. I find no evidence of significant discontinuities at any cutoffs for the baseline covariates.

1.3.2 Access to Chapter 7 Bankruptcy Protection

Table 1.4 and Figure 1.6 present first stage results. The econometric specifications differ only in terms of bandwidth. Panel A shows results for the first cutoff. Panels B, C and D present estimates for the remaining thresholds and the pooled cutoff respectively. Figure 1.6 shows that the probability of receiving Chapter 7 protection changes discontinuously not only when filers have higher AGMIs than the state median income, but especially when the disposable income is higher than C_2 and C_3. Having a AGMI just higher than the median income reduces the probability

[59]I find similar results for second and third cutoff separately.

of Chapter 7 by around 9 percentage points. This small drop is explained by the extensive amount of debtors just above the first threshold who receive Chapter 7 since they pass the means test. This can reduce the power for this cutoff because most filers around it either below or above are receiving Chapter 7 protection.

In contrast, the probability of being granted access to bankruptcy protection drops around 25 percentage points when the disposable income is slightly higher than C_2, and 55 percentage points when it is above C_3. These results are expected since for those filers with disposable incomes above the third threshold, "abuse" is automatically assumed and the case is dismissed.[60] Finally, Table 1.4 shows that the point estimates with other bandwidths, functional forms and the inclusion of pre-treatment covariates are qualitatively similar. It is therefore safe to conclude that the IV estimates do not suffer from the problem of weak instruments.

1.3.3 Impact on Debtor Outcomes

This section discusses the impact of Chapter 7 bankruptcy protection on post-filing households' investment behavior and their financial health. In terms of investment decisions, debtor outcomes include the business creation and the buying of real estate properties (particularly if filers become new homeowners). Related to financial distress, the outcomes are foreclosure (for homeowners at the time of the filing), judgment liens, and bankruptcy refiling. Finally, I also estimate the effect of Chapter 7 on debtor mortality.[61]

[60] See footnote 37.

[61] See Variable Definitions in the appendix for more details.

Since filers who are granted Chapter 7 bankruptcy protection have their unsecured debt discharged around three months after filing, it is of interest to study the dynamics of the ex-post effects not only in the long term but also in the short term. Thus, I define short-term as three years post-filing and long-term as six years post-filing.

Figure 1.7 and 1.8 show the estimates of the intention-to-treat or reduced form estimates(i.e., outcome variables on threshold crossing indicator) for each threshold.

Household Investment Behavior

Table 1.5 reports the fuzzy RD estimates of the impact of Chapter 7 on household outcome for the first and pooled cutoffs. In addition, I present the point estimates for different bandwidths, linear and quadratic forms and with the inclusion of pre-treatment covariates. Table B.4 shows that the estimates are similar for the second and third cutoff separately.

Business creation outcome is an indicator for a filer registering a business on or before the indicated year (after filing for bankruptcy).[62] Receiving Chapter 7 protection leads to an economic and significant increase in the likelihood of starting a business within 6-years post-filing by around 23 and 17 percentage points for the marginal recipients of Chapter 7 in the first and pooled cutoffs, respectively.

To study the effect across the filers' characteristics, Table B.5 reports the estimates by marital status, age at filing, and household size for both thresholds.

[62] This can be a proxy of self-employment.

The effect is larger for single filers with household size below 3, who are employed homeowners with a job tenure of 2 to 7 at the time of filing. In addition, Table 1.6 Panel A shows that the estimates are qualitatively similar after adjusting business creation for firm survival. This last finding is suggestive evidence that Chapter 7 leads to productive investment.[63]

One of the economic justifications for having a personal bankruptcy procedure is that it encourages entrepreneurial behavior ex-ante. Starting a business is risky and risk-averse individuals are more likely to do so if bankruptcy softens the consequences of failure by discharging the entrepreneur's debts in those states where the business does not succeed. However, interestingly these estimates show that Chapter 7 has positive ex-post effect on entrepreneurial behavior.

One concern is that those new businesses that Table 1.5 documents are from entrepreneurs with previous entrepreneurial experience.[64] However, a partial test for this concern is that there are no pretreatment differences across the thresholds in business ownership. Additionally, Table 1.6 Panel B reports the estimates for those filers who were not business owners at the time of filing. I find that Chapter 7 increases the probability of becoming a new business owner by 15 and 12 percentage points for the marginal recipient in the first and the pooled cutoffs, respectively.

Table 1.7 shows the estimates separately by whether or not the debtor has positive home equity (at the time of filing). Interestingly, the effect of Chapter 7 on

[63] Business licenses must be renewed each year, and a fictitious business name statement expires five years from the date it is filed.

[64] In addition, if more than 50% of debtor's debt is from business debts, then he does not have to take the means test.

starting a business is relatively higher when the household has positive home equity. A possible explanation for this finding is that debtors could use their positive home equity to obtain financing to fund their new businesses.

Bankruptcy could negatively affect the ability to obtain credit. Filers receive a bankruptcy flag in their credit report that remains up to 10 years after filing. Moreover, even debtors who file for bankruptcy and have their cases dismissed receive a flag for the same period. In addition, bankruptcy can also affect the ability to be hired by private employers because the bankruptcy code permits private employers to conduct credit checks on job applicants.[65] One may think that Chapter 7 marginal recipients are more prone to start businesses, relative to those whose cases are dismissed, because they have a "bankruptcy stigma" that does not allow them to find a job. However, because filers in both the treatment and control group have a bankruptcy flag (regardless of they were dismissed or discharged) the results in terms of business creation are not due to the bankruptcy flag.

An important investment decision for households is to acquiring real estate properties. Real assets account for the most important portion (70%) of household wealth, with little variation across wealth levels (Guiso and Sodini, 2012). Thus, studying home ownership is of interest. Table 1.5 reports that Chapter 7 protec-

[65]Section 525 of the Bankruptcy code contains two subsections. Subsection (a) states that government employers may not deny employment to, terminate the employment of, or discriminate with respect to employment against a person who has filed bankruptcy solely because of that filing. Subsection (b) provides that no private employer "may terminate the employment of, or discriminate with respect to employment against" individuals for declaring bankruptcy. However, section (b) relative to private entities is very salient since it does not mention denial of employment in its list of prohibited discriminatory actions.

tion increases the probability of acquiring a property by 20.7 percentage points for marginal recipients in the pooled threshold. The effect is similar but imprecise for debtors in the first cutoff. In terms of new homeowners, Chapter 7 also has positive effects. Table B.9 shows that marginal recipients of Chapter 7 are 15 percentage points more likely to become new homeowners relative to filers in the control group.

Overall, the estimates show that Chapter 7 has real effects in terms of business formation and home ownership.

Household Financial Performance

In the case of foreclosure, Chapter 7 could help homeowners save their homes because discharge of unsecured debt loosens their budget constraints and increases their ability to pay their mortgages. In addition, filing under Chapter 7 stops mortgage lenders from foreclosing for a few months, so homeowners who have fallen behind on their mortgage payments get additional time to repay their arrears (Li et al., 2011). Not only academics but also practitioners have long recognized how filing Chapter 7 and discharging unsecured debts can help avert foreclosure. Many debtors file bankruptcy precisely so that they can pay their mortgage by discharging other debts (Berkowitz and Hynes, 1999).

Table 1.5 reports estimates of the effect of Chapter 7 on foreclosure (conditional on being matched to a home).[66] For filers below the state median income

[66]Home foreclosure is an indicator for a debtor's home receiving a notice of default, receiving a notice of transfer or sale, or having been transferred to a REO or a guarantor on or before the indicated year (after filing) similarly as Dobbie and Song (2015).

threshold (first cutoff), Chapter 7 reduces the likelihood of facing home foreclosure, but the estimates are imprecise. The marginal recipient of bankruptcy protection in the pooled threshold is 60 percentage points less likely to be involved in a foreclosure event relative to filers in the control group (similar results are found for each separate cutoff). The effect is persistent through six years after filing.[67] In terms of the impact of Chapter 7 by filers characteristics, as shown in Table B.5 panel B, the effect is larger for filers who are married, older than 40, with a family size greater than two.[68]

In addition, it is of interest to study the foreclosure outcomes depending on the debtor's home equity. It could be the case that even if filers receive Chapter 7 (and their unsecured debt is discharged), that they may choose to reallocate resources to pay (or continuing paying) their mortgages only if he has positive home equity (i.e., no underwater mortgages). Table 1.7 shows the estimates separately by whether the debtor has positive home equity (at the time of the filing). Interestingly, the effect of Chapter 7 on foreclosure is concentrated on filers with positive home equity. Thus, after receiving debt relief, which increases debtors' ability to pay their mortgages, filers on average decide to repay when they are not underwater.

As other measures of the post-filing financial distress, I employ a judgment lien indicator function and an ex-post bankruptcy dummy (for any chapter). The judgment lien variable includes tax liens and non-tax liens that may come from past

[67]In untabulated results the estimates are qualitatively similar by dismissing the first year after filing.

[68]In untabulated results, I also find that the impact of Chapter 7 on foreclosure is larger in recourse states and in states with homestead exemptions higher than the median.

due medical bills and rent eviction, among others.[69] Table 1.5 reports the estimates of the effect of Chapter 7. Being granted Chapter 7 bankruptcy protection leads to substantial reduction in the probability of being subject to a judgment lien by 60 percentage points for filers in the first cutoff and around 41 percentage points for debtors in the pooled cutoff. Thus, Chapter 7 protection helps to avoid debtor default on contractual obligations (e.g., taxes), and the effect is persistent through time. Table B.5 presents the results by debtor characteristics, Chapter 7 effect leads to a greater decline in judgment liens for debtors who are married, older than 40, and with household sizes greater than two.

Since one of the objectives of the fresh start is to avoid bankruptcy refiling, this is seen as a failure of the bankruptcy process, and it is thus interesting to study if Chapter 7 helps debtors' avoid subsequent refiling for bankruptcy.[70] Chapter 7 leads to a reduction of 67 percentage points for a second bankruptcy on or before 2015 for those filers in the first cutoff. However, there is a negative but imprecise effect of debt relief on future bankruptcy for debtors with positive disposable incomes in the pooled cutoff.

Overall, these findings show that Chapter 7 does lead to an improvement in the debtor's ex-post financial health, which is one of the main goals of the Bankruptcy Law.

[69] A tax lien may be imposed for delinquent taxes owed on real property or due to failure to pay income (or other) taxes

[70] Debtors can refile for Chapter 7 after 8 years. While, to receive a discharge on a subsequent Chapter 13, the petitioner must wait 4 years from the date of filing the first Chapter 7.

Miscellaneous Outcome

Using public records data from LexisNexis, I next look at the impact of Chapter 7 on debtors' mortality. I find no evidence of effect of Chapter 7 on mortality both in the short term and long run for those debtors in the first cutoff. Mortality is reduced by 8.50 percentage points in the short run for those debtors' in the pooled cutoff. However, the effects of Chapter 7 largely disappear in the long run. These findings contrast with Dobbie and Song (2015) who find that Chapter 13 leads to a reduction of 1.3 percentage points in mortality.

1.3.4 Additional Robustness Tests

Heaping will only bias RD estimates to the extent that it creates imbalances in outcome determinants across the thresholds. Standard tests show no evidence of this. However, Barreca et al. (2011) argue that if heaping is associated with determinants of the outcome variable, it can create biases even when the RD passes standard balance tests. To address the concern, I follow two approaches recommended in Barreca et al. (2011). The first is to estimate a "donut" RD that drops observations precisely at the cutoff value and just below each cutoff. The second approach is to control flexibly for heterogeneity related to the possibility of heaping by allowing for separate intercepts and trends for the observations just below each threshold. Table 1.12 presents results obtained by implementing these modifications in the main specification. The estimates are robust to both approaches.

Furthermore, it is also of interest to study if there are cohort effects by estimating the post-filing outcomes for debtors that filed in 2006-07 and 2008-09. I find, as shown in Table B.7, that the estimates of Chapter 7 protection on financial distress and real investment behavior are similar in both cohorts and not statistically different.

Finally, I test whether there are discontinuities in debtors' outcomes at other places away from the thresholds. Finding discontinuities at pseudo-thresholds where eligibility does not change would raise the concern that the findings are due to misspecified nonlinearities in the relationship between the running variable and the outcome (Imbens and Lemieux, 2008). I look for discontinuities at pseudo-thresholds close to the regulatory cutoffs: \$-1,000 and \$+1,000 above and below in the first cutoff and \$-100 and \$+100 above and below in the pooled cutoff, respectively. Table B.8 reports the lack of evidence of discontinuities at these thresholds.

1.3.5 Potential Mechanisms

In this section, I explore two potential mechanisms that may explain the results.

Improvement of the Debtor's Balance Sheet

First, Chapter 7 protection leads to a discharge of the debtor's unsecured debt improving their balance sheet. Debt-overhang (Myers, 1977; Krugman, 1988), and net worth effects and investment models (e.g., Bernanke and Gertler, 1989; Kiyotaki et al., 1997) suggest that debt relief can raise the probability of attracting new lending and value-increasing investment.

Since distortions should be diminishing for debtors who receive relatively higher debt forgiveness (i.e., had previously higher relative leverage), I estimate the effective debt relief received by filers on the basis of the bankruptcy data. Debtors who receive Chapter 7, obtain unsecured debt relief net of non-exempt assets. I estimate the non-exempt assets using debtors' home equity and their state homestead exemption. The debt relief in the case of those debtors who filed for Chapter 13 and had their cases discharged (either because they were Chapter 7 filers, whose cases were dismissed and converted to Chapter 13 or who did not qualify for Chapter 7 at all) is their unsecured debt net of their repayment plan. The five-year repayment plan is their monthly disposable income, as established by the means tests. If the debtor is a homeowner, the repayment plan is the larger of either their disposable income for the next five years or their entire home equity minus their homestead exemption. Finally, dismissed filers do not receive debt forgiveness. Due to outlying observations, the debt relief variable is Winsorized at the 5th and 95th percentiles.[71] Table 8 reports the estimates.[72] I find that one standard deviation increase in debt relief through Chapter 7 leads to an increase in the probability of business creation by 10.79 percentage points for filers in the first cutoff and 12.48 percentage points for filers in the pooled cutoff. It also increases the probability of acquiring a new property by 18.72 percentage points. Debt relief also has substantial effects in terms of foreclosure. One standard deviation increase in debt relief decreases the probabil-

[71] I find similar results if I use the log of the (raw) debt relief.

[72] In this case, pooling requires the treatment intensity to be of comparable magnitude in order to interpret the size of estimated impacts (see footnote 50). Since the cutoffs are relative close, the difference in debt relief is small ($12,430 and $13,651 for the second and third cutoffs, respectively). In fact the difference is not statistically significant in untabulated results.

ity of home foreclosure by 51.74 percentage points. It also reduces the probability of being subject to liens by 38.74 percentage points.[73]

In addition, debt-overhang models predict that debt relief can improve debtors' incentives because the returns of investment/effort are captured mainly by the debtors themselves rather that the lenders. Thus, by discharging unsecured debt, Chapter 7 can preserve debtors' incentives by protecting their wages from garnishment. Thus, as a test for the improvement of incentives from bankruptcy protection, I exploit the across-state variation in wage garnishment. Table B.10 reports the estimates for those states that ban wage garnishment or at least preserve 90% of the debtor's wages (low wage garnishment), and those states that do allow wage garnishment (high wage garnishment). I find large and significant effects on judgment liens in the set of states that allow wage garnishment and positive point estimates in the business creation measure.

Furthermore, for homeowners with positive home equity the homestead exceptions directly impact the net benefit of Chapter 7. The benefits are lower when the homestead exceptions are relative less generous. Using the across-state variations in homestead exemptions, I find that in those states with homestead exemptions above the median filers are on average more likely to start a business and also avoid financial distress (see Table 9).

However, even though the debtors in the treatment group received unsecured debt relief, homeowners can still face debt-overhang problems from their mortgages.

[73]Figure 9 shows the first-stage estimation.

An additional test of the importance of debt-overhang on household behavior is to estimate the effect of Chapter 7 on foreclosures based on debtors' home equity. Table 7 reports the estimates of Chapter 7 on whether or not the debtor is underwater (at the time of the filing). The effects of Chapter 7 on foreclosure are stronger for both thresholds for homeowners with positive home equity. These findings are consistent with the prediction of standard debt-overhang models (Myers, 1977; Krugman, 1988).[74] Finally, Table 10 reports the effects of Chapter 7 on Uniform Commercial Code (UCC) secured loans.[75] I find that Chapter 7 protection increases the probability of secured lending for marginal recipients, especially those filers with positive home equity.

Non-Judicial Collection Protection

Other mechanism that can help explain the estimates is that bankruptcy protection stops non-judicial collection efforts, such as collection letters, phone calls, and visits at home or work. To test for it, I exploit the across-state variation in anti-harassment statutes that tries to protect borrowers against aggressive collection techniques.[76]

Under this mechanism, the effect of Chapter 7 protection should be higher in

[74] These results provide support for the mortgage cram-down proposal (e.g., Mian and Sufi (2015)). Even with unsecured debt relief, households have no incentive to save their house if they are underwater.

[75] UCC is a state-level filing registry that records loans secured by fixed assets.

[76] In addition, in this mechanism debtors in the control group that do not receive bankruptcy protection could be more prone to move or change their phone number. Using public records data, I estimate the impact of Chapter 7 on the number of times post-filing that debtors move and the number of phones. Table B.11 shows that there are no significant effects.

those states that do not regulate non-judicial debt collection. This is because, even though the treatment group receives Chapter 7 and cannot face any harassment by creditors, but debtors in the control group that are dismissed can be subject to non-judicial collection efforts unless they reside in states with anti-harassment statutes. Table 1.11 reports the findings and overall shows no clear evidence of a difference in Chapter 7's effects on debtors in states with or without statutes that provide the right of action against a harassing or abusive creditor.

It is also of interest to explore the effect of Chapter 7 on non-economic outcomes. Using public records data, I estimate the impact of Chapter 7 on criminal records, if debtors are in the same zip-code and marital status (i.e., divorced). Table B.12 shows no significant effects. These findings do not support the nonjudicial collection protection channel, since under this mechanism, we should expect that debtors in the control group, who do not receive bankruptcy protection, could be more prone to move or change their phone number to avoid collection efforts from their creditors.

Overall, these results suggest that improving the debtor's balance sheet is the main driver for the estimates of Chapter 7 bankruptcy protection.

1.4 Discussion

1.4.1 Comparison to Other Studies

The results show that receiving Chapter 7 bankruptcy protection has economically and statistically significant effects on real investment decisions and ex-post financial performance. However, the prior literature has found mix results for the benefits of bankruptcy protection (e.g., Han and Li, 2007). One explanation for the lack of consistent results in prior studies is the shortage of a suitable control group (Dobbie and Song, 2015).

In terms of post-filing financial well-being, Porter and Thorne (2006) find using survey data that in the first year post-bankruptcy, 25% of debtors struggle to pay routine bills, and 33% are in a financial situation similar or worse than before bankruptcy. On the other hand, Dobbie and Song (2015) and Dobbie et al. (2015) find positive effects on Chapter 13 marginal recipients. They find that Chapter 13 protection reduced by 127% the probability of being involved in home foreclosure and by 100% the probability of receiving liens over the first five post-filing years relative to their control group. I find that debtors 86% less likely to be involved in home foreclosure within six years post-filing, relative to the control group filers mean. In addition, the Chapter 7 marginal recipient has 124% lower probability (pooled cutoff) for receiving a judgment lien. One explanation for this difference is that through Chapter 13, filers should use part of their budget to repay unsecured debt, which leaves less resources available to serve current debt and to pay routine bills, which makes them more vulnerable relative to Chapter 7 recipients.

One may think that debt relief should alleviate debt-overhang problems and have positive effects on productive investment. Kanz (2015) uses quasi-experimental data from India's largest household-level debt relief program and finds that debt forgiveness, even though it has a positive impact on a household's balance sheets, does not affect investment. Specifically, the investment expenditures of households receiving full debt relief is around 20 percentage points lower relative to households receiving partial or no debt relief. My results show exactly the opposite, that debt relief through Chapter 7 protection has real effects. One explanation for the difference in findings is that contrary to India's program, Chapter 7 has not only significant impact on household balance sheets but it also relaxes liquidity constraints (e.g., through secure lending) sufficient enough to encourage new investment.

Finally, a related literature examines the effect of debt relief on access to credit. Some studies find that households have less access to credit after receiving debt relief through bankruptcy protection (Cohen-Cole et al., 2013; Han and Li, 2011), presumably because lenders perceive these borrowers as having observably higher default risk. However, consistent with my results showing that Chapter 7 recipients have access to secured lending such as Mortgages and UCC loans, Dobbie et al. (2015) also find that Chapter 13 recipients have significantly more access to mortgages; however they do not find significant results for unsecured debt.

Other Debt Relief Programs and Foreclosure Agarwal et al. (2013) examine the effects of the 2009 Home Affordable Modification Program (HAMP) that provided servicers with financial incentives to renegotiate mortgages. They find that renegotiations resulted in a moderate decline in foreclosures, and the program reached around one-third of the targeted indebted households.

Furthermore, Mayer et al. (2014) studies the potential costs of debt relief initiatives, in this case, a mortgage modification program. They find that the delinquency rate increased after settlement against Countrywide Financial Corporation, which agreed to offer modifications to seriously delinquent borrowers.

Finally, Agarwal et al. (2015) study the how the Home Affordable Refinancing Program (HARP) affected household outcomes (e.g., consumption and foreclosure). HARP allowed borrowers to refinance their mortgages by extending explicit federal credit guarantees to lenders. In addition, the program aimed to provide economic stimulus in order to potentially lower the likelihood of delinquencies and subsequent foreclosures. They find that regions more exposed to the program experienced a relative decline in foreclosure rates.

1.4.2 External Validity of the Results

Marginal Threshold Treatment Effect (MTTE)

FRD models identify the local average treatment effect (LATE) at one point (Hahn et al., 2001). Thus, the external validity of the estimates is a concern, unless we assume homogenous effects. It is useful to know whether the effects documented in Table 1.5 for the marginal recipient of Chapter 7 bankruptcy protection are similar at points other than the specific thresholds. For example, if the effects were substantially different at only slightly different values of the cutoffs, then the external validity of the estimate should be a concern. On the other hand, if marginal changes in the thresholds do not significantly affect the identified LATE, then it would be plausible to extrapolate the results (Dong and Lewbel, 2012).

To investigate how robust the results are as we move away from the cutoff, I estimate the marginal threshold treatment effect (MTTE), which is the change in the treatment effect that would result from a marginal change in the threshold.

Intuitively, one can think of the marginal threshold treatment effect (MTTE) as the derivative of the average treatment effect for the compliers ($D(x)$) when the running variable (X) equals the cutoff value (c), $D'(c)$ as the coefficient of the interaction term between the treatment T and $X - c$ in a local linear regression of Y on a constant, T , $X - c$ and $(X - c)T$.

In parametric models $D(x)$ is identified both at $x = c$ and for values $x \neq c$, permitting identification of $D'(x)$ only because the functional form allows us to evaluate counterfactual objects. For example, in a sharp design with the expectation assumed to be quadratic, $Y = a + bX + dX^2 + \beta T + \gamma XT + \delta X^2 T + e$, and $E(e \mid X = x) = 0$. Then, in this case $D(x) = \beta + \gamma x + \delta x^2$, so $D'(x) = \gamma + 2\delta x$. Therefore, the treatment effect derivative is given by $\gamma + 2\delta x$ and is thereby identified for all x in an interval.

In addition, MTTE is nonparametrically identified. Previous papers have shown (Hahn et al., 2001) that RD only needs continuity, not differentiability of $E(Y \mid X = x)$ for identification. However, nonparametric estimators of $E(Y \mid X = x)$ typically used in applied work assume differentiability (e.g., local linear regression). Dong and Lewbel (2012) exploit, and assume, this differentiability to nonparametrically identify the MTTE.

There are three main assumptions to identify the MTTE. First, for each individual, the outcome variable, the running variable, and the endogenous variable are observed. Second, smoothness of the conditional means of potential outcomes and probabilities of selection into each type of individuals (i.e., compliers, always takers and never takers) is required . Thus, the mean outcome just below or above the cutoff is a weighted average of the mean outcomes for each type of individual, weighted by the probabilities of each type (Dong and Lewbel, 2012).[77] The last assumption requires local policy invariance, which is a *ceteris paribus* assumption like the one often used in partial equilibrium analysis. Under the local policy invariance assumption, the MTTE equals the derivative of the treatment effect with respect to the running variable at the cutoff (also referred to as TED). Policy invariance implies that the treatment effect as a function of the running variable does not change when the policy threshold changes infinitesimally.

A sufficient condition for local policy invariance is if the treatment effect for current compliers would not change if the thresholds used for determining treatment were increased from c to c_{new}, which would lead to more compliers.[78] This assumption holds if having more debtors qualifying for Chapter 7 does not affect the propensity of current compliers to pay their bills (i.e., avoid lien), acquire new real properties, or start businesses or foreclosures. There is one caveat in terms of foreclosure. If the marginal increase in the cutoff allows an individual who lives close to the original complier to have access to Chapter 7, then any peer effects that induce changes in foreclosure decision and affects their house prices would lead to such a violation. It seems unlikely that the magnitude of these effects could be large enough to cause more than a very small difference between the TED and the MTTE; thus the local policy invariance assumption is plausible in this setting.

[77]Intuitively, when the conditional means for each type and the related probabilities are all smooth at the cutoff, the mean outcome difference at the cutoff then just equals the mean change in outcomes for compliers.

[78]It does not place any restriction on how the treatment effect depends on the running variable.

The estimation results are reported in Table 1.13. The first column report the estimated effect of Chapter 7 on household outcomes at the pre-determined regulatory threshold as in Table 1.5. The second column present the TED (or MTTE if local policy invariance holds) and the new treatment effect, if the regulatory threshold were marginally increased by 1 percent (i.e., \$41 in the first cutoff and \$1.40 in the pooled cutoff), which means that more debtors would qualify for Chapter 7. Standard errors for the estimated TED (MTTE) and the new treatment effect are calculated using the Delta Method.

The estimated TEDs (or MTTEs) for debtors' outcomes imply that the impact of Chapter 7 on debtors would be lower if the eligibility thresholds were marginally increased. Thus, the treatment effect estimates holds also among filers with slightly higher (disposable or gross) income. In addition, if the regulatory thresholds are marginal increased, the effect of Chapter 7 would still large. Finally, knowledge of these magnitudes may be of interest for policy makers for assessing the likely impacts of changing the bankruptcy eligibility requirements.

Characteristics of Compliers

As previous mentioned, the FRD strategy identifies the effect of Chapter 7 protection for the complier group at the cutoff: filers who receive Chapter 7 protection if and only if their (gross or disposable) income is below specific cutoffs. Examining certain characteristics of the complier group is also important for the external validity of the findings.

The proportion of compliers in a given marital status–age group are calculated as the ratio of the first stage for that subgroup to the overall first stage, multiplied by the proportion of the full sample in the marital status–age group (Angrist and Pischke, 2008). Column 1 in Table B.13 reports the proportion of the sample in each marital status–age group, and column 2 shows the first stage estimates for different marital status–age groups. Column 3 reports the distribution of the compliers by marital status-age, whereas column 4 shows the relative probability of a complier's belonging to a particular marital status–age group compared to the full sample.

In the pooled cutoff, although 20.5% of the total filers are married with ages less than or equal to 40 (at filing), only 4.2% of the compliers are debtors in this marital status-age group. In addition, while 34.3% of the full sample are filers married with ages greater than 40, 44.8% of the compliers are debtors in this marital status-age group. Table B.13 also shows that unmarried filers under 40 are more likely to be among the compliers. These results also provide evidence on the types of filers who are more likely affected by BAPCPA eligibility requirements for Chapter 7 protection. Finally, in the first cutoff, unmarried filers over 40 are overrepresented in the compliers subpopulation, while unmarried filers under 40 are underrepresented among the compliers.

1.5 Conclusion

In this paper, I estimate the impact of Chapter 7 bankruptcy protection on debtors' post-filing financial distress and investment behavior. RD estimates show that Chapter 7 lowers the probability of financial distress by reducing the likelihood of post-filing foreclosures, judgment liens, and subsequent bankruptcy. In addition, in terms of household's investment decisions, marginal recipients generally are more likely (after receiving Chapter 7 protection) to start businesses, obtain secured lending, and become first time homeowners. Finally, after a rich variety of tests I find no evidence of discontinuities in the pretreatment covariates, manipulation of gross income or disposable income, reduction in labor supply, or other strategic behaviors (e.g., expenses, household size, etc.) from bankruptcy filers.

I also explore the potential mechanisms that may explain the results. Taking advantage of the data, I estimate the impact of debt relief provided by Chapter 7 on debtors' outcomes, which is a critical parameter in consumer credit markets and for policy makers. I find that one standard deviation increase in debt relief from Chapter 7 leads to an increase in the probability of business creation by 10.79 percentage points and a decrease in the probability of home foreclosure by 51.74 percentage points. The findings are consistent with models of debt overhang (Myers, 1977; Krugman, 1988) and models of net worth and investment (e.g., Bernanke and Gertler, 1989).

These results provide direct evidence that the BAPCPA generated negative consequences on those debtors for whom access to Chapter 7 was restricted. Moreover, in the wake of the Great Recession, household indebtedness has increased continually;[79] bankruptcy filing has been reduced due to BAPCPA's increased barriers to filing, in particular the increase in filing and legal fees. This last feature of the new law can negatively affect liquidity-constrained households (Gross et al., 2014);

[79] See "A fresh start" published in The Economist on March 14, 2015.

and, given the estimates of this paper, such debtors are worse off.

Finally, any quantitative valuation of the U.S. consumer bankruptcy system typically involves the assessment of two opposing effects. First, in incomplete markets, bankruptcy enables consumption smoothing across states by discharging some debt when debtors' ability to repay turns out to be low. Second, bankruptcy reduces debtors' ability to smooth consumption over time by making credit more costly (Athreya, 2002; Livshits et al., 2007). However, it would be interesting to incorporate, in these general equilibrium models of the credit market, the first-order relationships of bankruptcy, ex-post investment behavior, and financial distress that this study estimates.

Tables and Figures

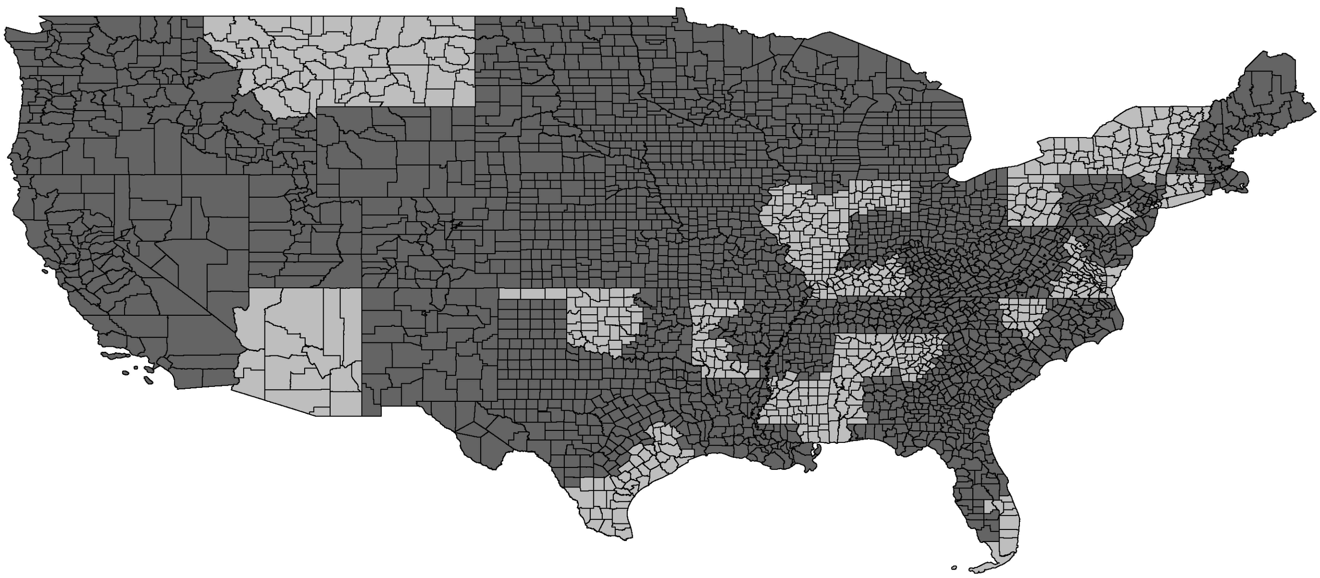

Figure 1.1 Bankruptcy Districts in Sample

The 65 bankruptcy district courts shaded in dark gray, plus Alaska, are those included in the sample.

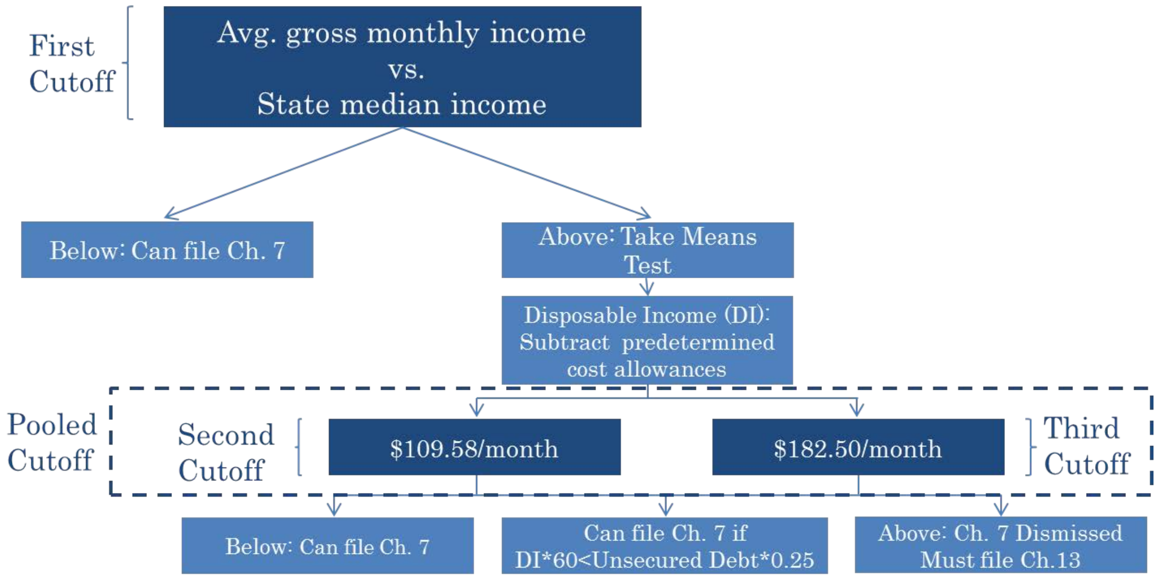

Figure 1.2 The Bankruptcy Means Test

The diagram describes the eligibility for Chapter 7 bankruptcy protection based on the current bankruptcy law.

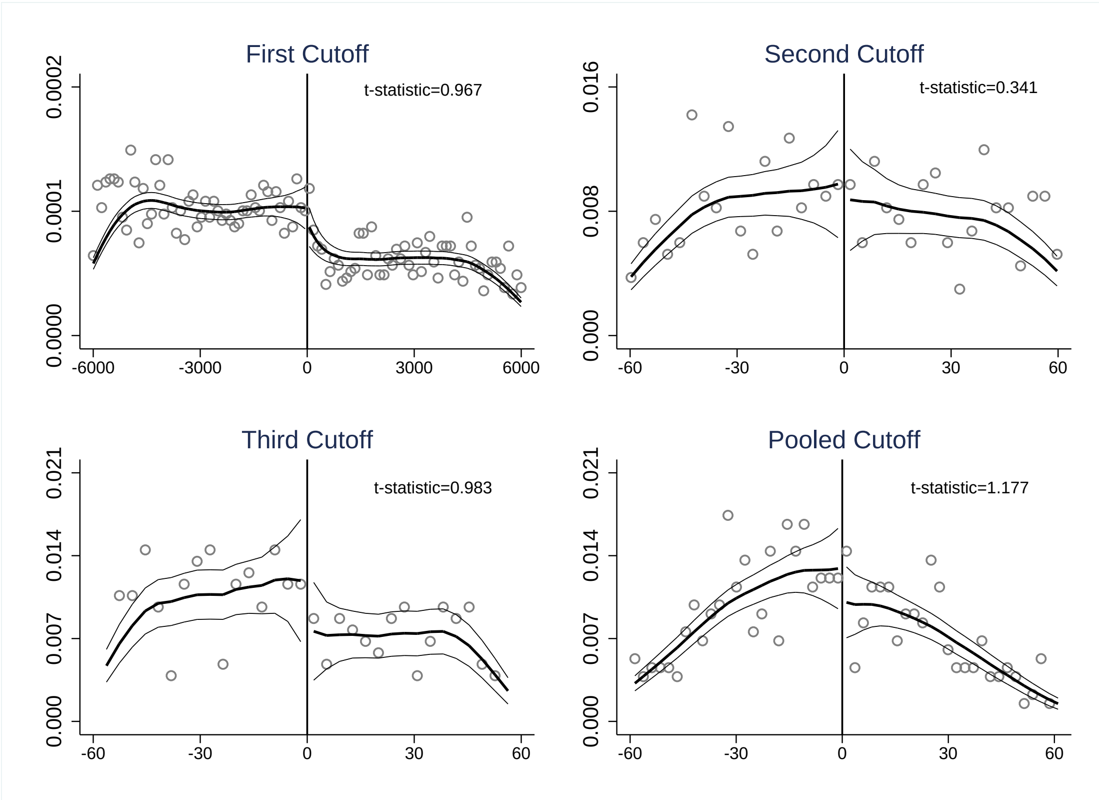

Figure 1.3 Density of the Running Variables

The McCrary density test fails to reject the null hypothesis of no discontinuity in the density at conventional levels of significance for the four cutoffs. The x-axis presents the running variable measured in US Dollars. The y-axis corresponds to the density of filers. The solid vertical line represents the respective cutoffs. The pooled cutoff comprises the second and third cutoff. The figure shows the histogram, estimated density, and 95% confidence intervals generated using the code provided by J. McCrary on his website and based on McCrary (2008).

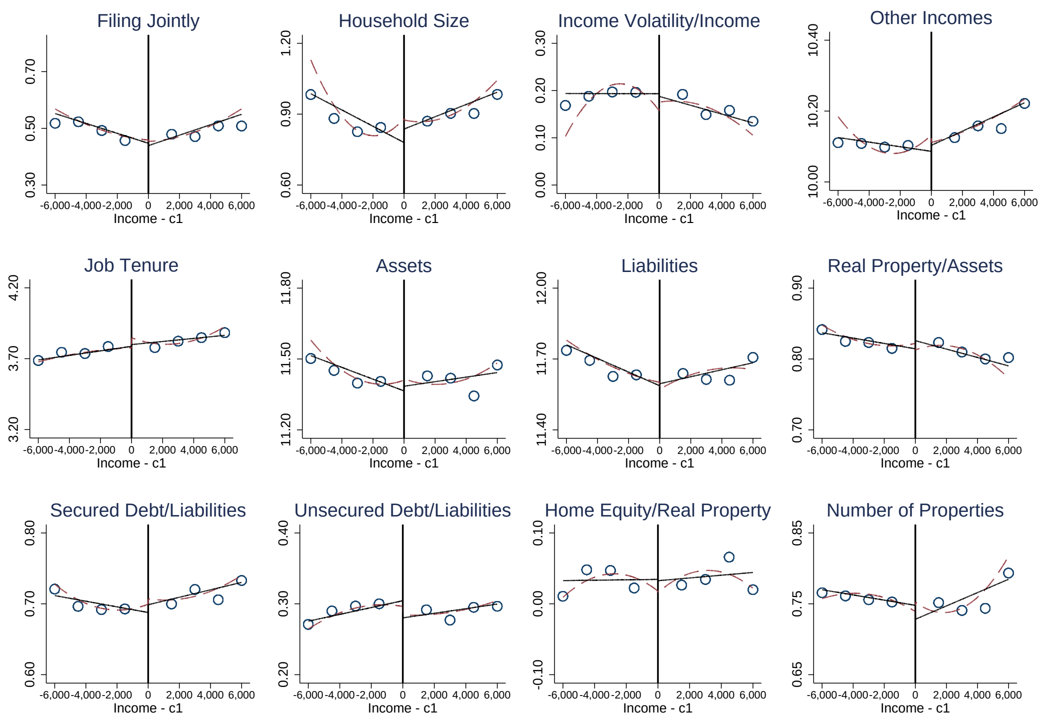

Figure 1.4 Test for Smoothness of Baseline Characteristics around the First Cutoff

The figure describes means of pretreatment covariates by distance relative to the first cutoff in order to test for covariate balance around the threshold. In the first cutoff, the running variable is the difference between the Average Gross Monthly Income (AGMI) and the state median income based on household size. Household size corresponds to the log of all the people who occupy a housing unit as their usual place of residence and are dependent on the debtor for tax purposes. Debtor income volatility is the standard deviation of the debtor's income over the last six months before filing relative to the income. Other Income is the log of the gross income other than wages. Job tenure is the log of the debtor's tenure in years at the filing date. Assets and Liabilities correspond to the log of total assets and total liabilities at the filing date. Real Property/Assets is real property to total assets. Secured Debt/Liabilities comprises total debt backed by collateral relative to total debt. Unsecured Debt/Liabilities is unsecured claims to liabilities. Home equity/ Real property is the difference between the property's market value and the outstanding balance of all liens on the property relative to the total real estate assets. Number of properties is the log of the number of real properties held by the debtor at the date of filing. Solid lines are nonparametric fits from a local linear regression, and dashed lines are quadratic fits that use rectangular kernels. The bin width is \$1,500. All specifications allow for differential slopes on each side of the cutoff.

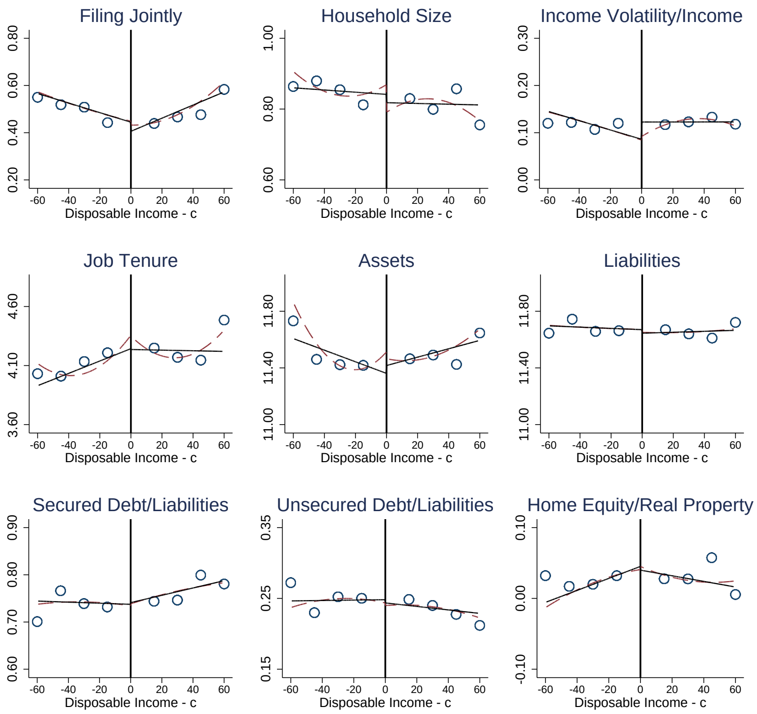

Figure 1.5 Test for Smoothness of Characteristics around the Pooled Cutoff

The figure describes means of pretreatment covariates by distance relative to the pooled cutoff to test for covariates balance around the threshold. The pooled cutoff combines the second and third cutoffs, as Figure 7 describes. In the pooled cutoff the running variable is the difference between monthly disposable income and the respective threshold that debtor faces. The pooled specifications include thresholds indicator. Household size corresponds to the log of all the people who occupy a housing unit as their usual place of residence and are dependent on the debtor for tax purposes. Debtor income volatility is the standard deviation of the debtor's income over the last six months before filing relative to the income. Other Income is the log of the gross income other than wages. Job tenure is the log of the debtor's tenure in years at the filing date. Assets and Liabilities correspond to the log of total assets and total liabilities at the filing date. Real Property/Assets is real property to total assets. Secured Debt/Liabilities comprises total debt backed by collateral relative to total debt. Unsecured Debt/Liabilities is unsecured claims to liabilities. Home equity/ Real property is the difference between the property's market value and the outstanding balance of all liens on the property relative to the total real estate assets. Number of properties is the log of the number of real properties held by the debtor at the date of filing. Solid lines are nonparametric fits from a local linear regression, and dashed lines are quadratic fits that use rectangular kernels. The bin width is $15. All specifications allow for differential slopes on each side of the cutoff.

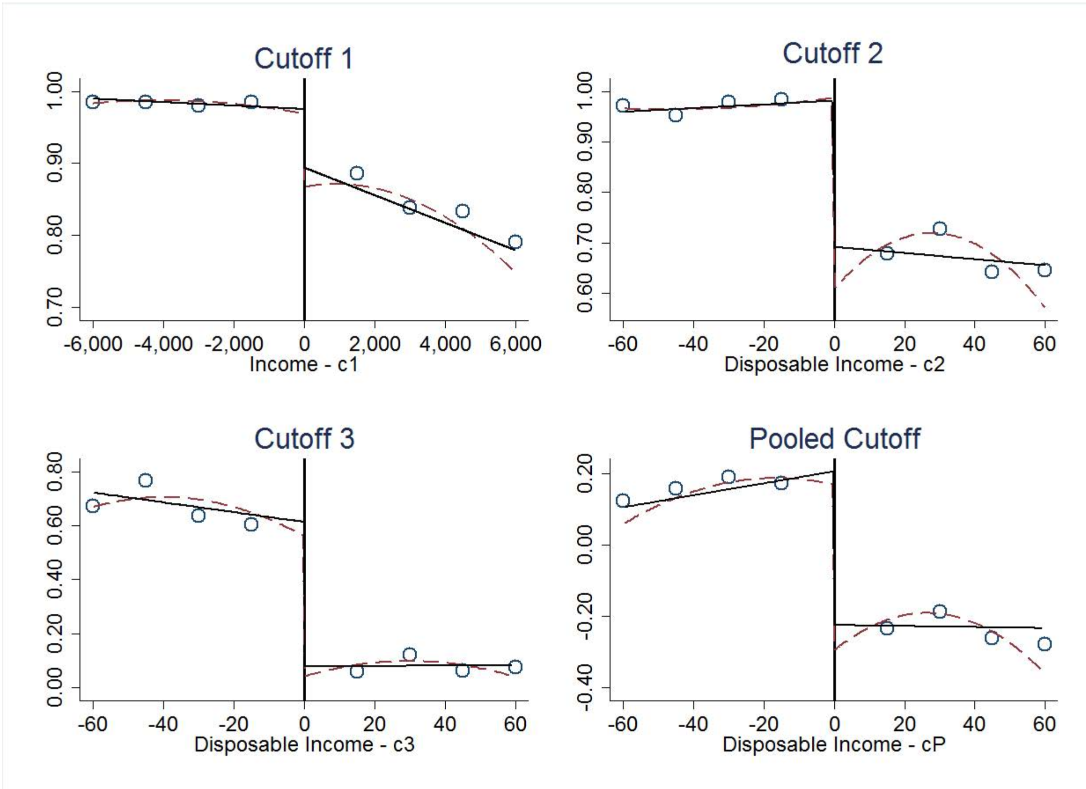

Figure 1.6 Access to Chapter 7

The figure illustrates the first stage for the probability receiving of Chapter 7 protection, by plotting the distribution of filers and the running variables around the cutoff. The x-axis presents the running variable in a bandwidth of \$6,000 for the first cutoff and \$60 for the other cutoffs. The y-axis corresponds to the probability of receiving Chapter 7 bankruptcy protection. In the first cutoff, the running variable is the difference between the Average Gross Monthly Income (AGMI) and the state median income based on household size. In the second cutoff, the running variable is the difference between monthly disposable income and \$100 (before 2007 and \$109.58 after 2007). In the third cutoff, the running variable is the difference between monthly disposable income and \$166.67 (before 2007 and \$182.50 after 2007). The pooled cutoff combines the second and third cutoffs. The pooled specifications include thresholds indicator. Solid lines are nonparametric fits from a local linear regression, and dashed lines are quadratic fits that use rectangular kernels. All specifications allow for differential slopes on each side of the cutoffs.

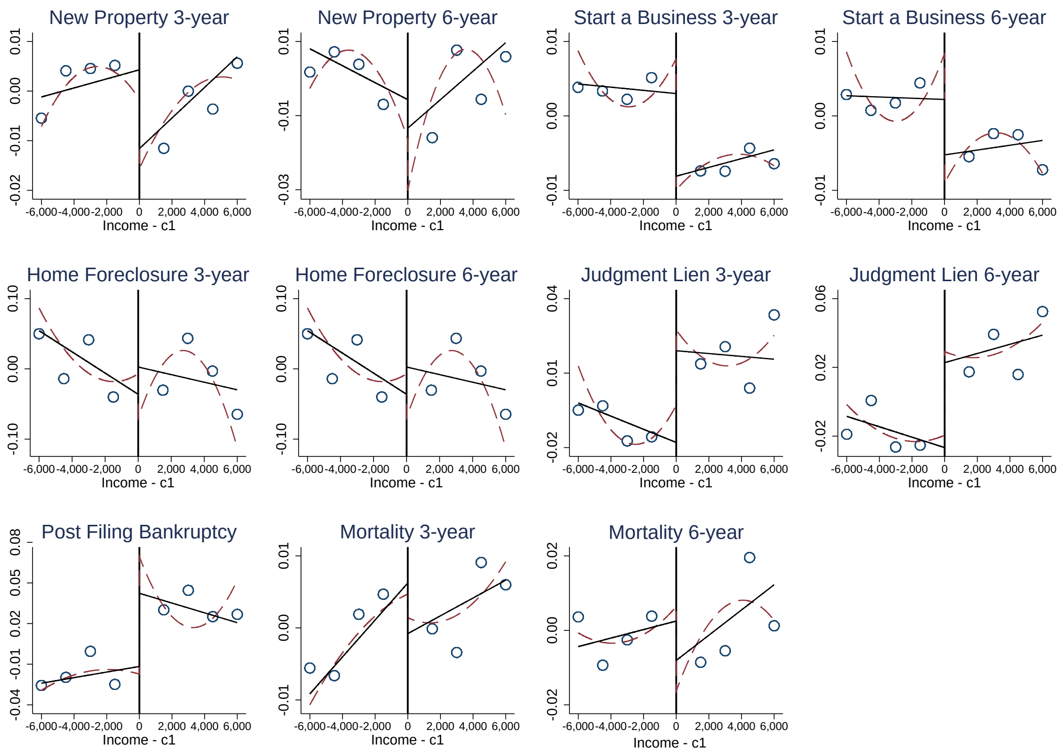

Figure 1.7 Impact on Debtors' Post-Filing Outcomes for the First Cutoff

The figure describes the intention to treat (or reduced form) of the first cutoff on debtors' post-filing outcomes. The running variable is the difference between the Average Gross Monthly Income (AGMI) and the state median income based on household size. Debtors' outcome variables are measured three years and six years post-filing. Home foreclosure is an indicator for a filer's home receiving a notice of default, receiving a notice of transfer or sale, or having been transferred to an REO or a guarantor on or before the indicated year. New Property comprises the acquisition of a new real property by the filer. Judgment Lien is an official claim that gives a creditor the right to take possession of a debtor's real property if the debtor fails to fulfill his or her contractual obligations. It includes tax liens, hospital liens, and judicial liens. Start a Business is an indicator variable for any business registered in public records by the debtor post-filing. Solid lines are nonparametric fits from a local linear regression, and dashed lines are quadratic fits that use rectangular kernels. The bin width is $1,500. All specifications allow for differential slopes on either side of the cutoff.

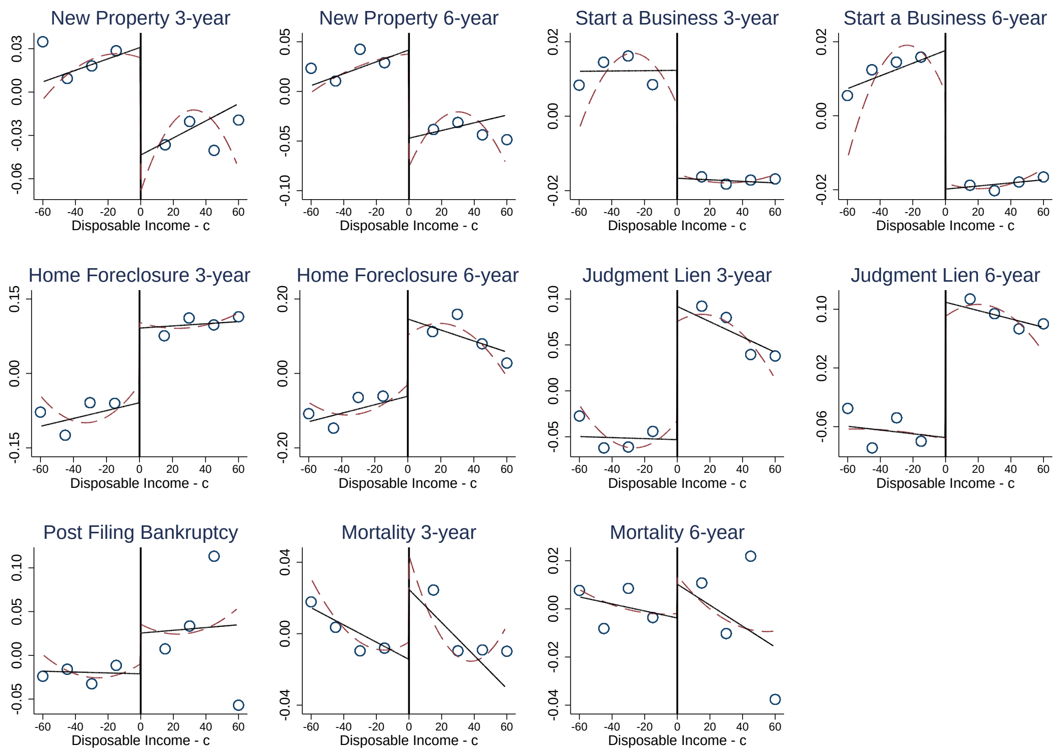

Figure 1.8 Impact on Debtors' Post-Filing Outcomes for the Pooled Cutoff

The figure describes the intention to treat (or reduced form) of the pooled cutoff on debtors' post-filing outcomes. The pooled cutoff combines the second and third cutoffs, as Figure 7 describes. In the pooled cutoff the running variable is the difference between monthly disposable income and the respective threshold that debtor faces. The pooled specifications include thresholds indicator. Debtors' outcome variables are measured three years and six years post-filing. Home foreclosure is an indicator for a filer's home receiving a notice of default, receiving a notice of transfer or sale, or having been transferred to an REO or a guarantor on or before the indicated year. New Property comprises the acquisition of a new real property by the filer. Judgment Lien is an official claim that gives a creditor the right to take possession of a debtor's real property if the debtor fails to fulfill his or her contractual obligations. It includes tax liens, hospital liens, and judicial liens. Start a Business is an indicator variable for any business registered in public records by the debtor post-filing. Solid lines are nonparametric fits from a local linear regression, and dashed lines are quadratic fits that use rectangular kernels. The bin width is \$15. All specifications allow for differential slopes on each side of the cutoff.

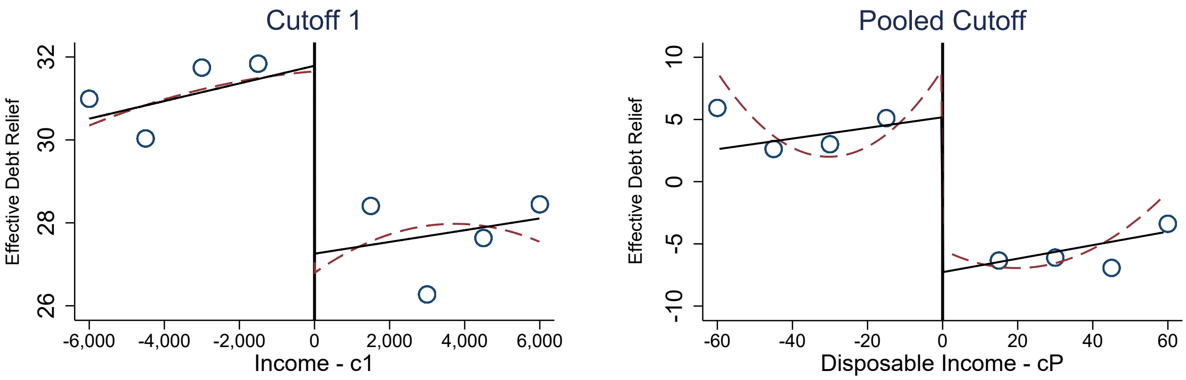

Figure 1.9 Debt Relief at the Cutoffs

lThe figure illustrates the debt relief provided by Chapter 7, by plotting the distribution of filers and the running variables around the cutoff. The x-axis presents the running variable in a bandwidth of \$6,000 for the first cutoff and \$60 for the other cutoffs. The y-axis corresponds to the effective debt relief received through Chapter 7 bankruptcy protection. Debtors who receive Chapter 7, obtain unsecured debt relief net of non-exempt assets, thus I estimate the non-exempt assets using debtors' home equity and their state homestead exemption. The debt relief in the case of those debtors who filed for Chapter 13 and had their cases discharged (either because they were Chapter 7 filers, whose cases were dismissed and converted to Chapter 13 or who did not qualify for Chapter 7 at all) is their unsecured debt net of their repayment plan. The five-year repayment plan is their monthly disposable income, as established by the means tests. Dismissed filers do not receive debt forgiveness. In the first cutoff, the running variable is the difference between the Average Gross Monthly Income (AGMI) and the state median income based on household size. The pooled cutoff combines the second and third cutoffs, as Figure 7 describes. The pooled specifications include thresholds indicator. Solid lines are nonparametric fits from a local linear regression, and dashed lines are quadratic fits that use rectangular kernels. All specifications allow for differential slopes on each side of the cutoffs.

Table 1.1 Summary Statistics

This table reports summary statistics. The full sample consists of a random sample of first-time filers from 65 bankruptcy courts between 2006 and 2009. The RD sample comprises those cases around the thresholds. The data comes from legal bankruptcy documents submitted by filers through PACER and Lexis-Nexis public records. Household size, marital status, filing jointly and gross annual income come from Forms 22A and 22C. Assets and liabilities of individual debtors come from the Summary of Schedules. Data on age at filing, gender, race, criminal background (e.g., arrest records, court conviction records, traffic violations) and business owners comes from Lexis-Nexis public records. All monetary values are expressed in year 2000 U.S. dollars divided by 1,000.

	Full Sample				RD Sample				
	Mean	Median	Chapter 7	Chapter 13	Mean	Median	Chapter 7	Chapter 13	p-value
General Debtors Characteristics									
% Marital status (Married)	49.41		45.10	56.08	49.76		49.69	50.35	0.798
% Filing jointly	34.56		33.03	36.93	38.24		38.42	36.64	0.487
% Gender (Male)	67.45		66.53	77.31	64.42		64.30	65.60	0.772
% Race (White)	78.04		79.76	78.26	77.01		79.05	70.60	0.146
% Criminal background	15.87		15.94	13.75	15.95		15.85	17.18	0.185
% Business owners	6.92		7.16	4.33	6.99		7.15	5.62	0.220
Household size	2.57	2.00	2.42	2.97	2.38	2.00	2.37	2.41	0.568
Age at filing	44.01	43.00	43.68	45.82	43.87	43.00	43.71	44.26	0.210
Gross Annual Income	35.95	31.68	31.59	42.71	44.05	42.69	43.89	45.98	0.227
Liabilities-to-income-ratio	4.89	3.73	5.66	3.99	4.08	3.26	4.11	3.83	0.235
Assets of individual debtors									
Total Assets	129.63	87.46	116.14	141.43	141.41	106.59	140.49	149.67	0.206
Real Property	105.27	69.26	92.81	113.34	114.54	85.56	113.57	123.25	0.289
Liabilities of individual debtors									
Liabilities	175.94	118.42	178.94	170.56	180.00	139.44	180.53	175.73	0.768
Secured Debt	112.40	82.26	102.81	129.64	116.99	95.52	116.94	117.56	0.952
Unsecured Debt	60.03	33.73	71.24	40.04	59.06	41.78	59.68	53.45	0.443
Number of Cases			38,855				4,536		

Table 1.2 Test of Discontinuities in Pretreatment Covariates

This table reports the estimates of the test for the balance of observable covariates across the threshold. In the first cutoff, the running variable is the difference between the Average Gross Monthly Income (AGMI) and the state median income based on household size. The pooled cutoff combines the second and third cutoffs, as Figure 7 describes. In the pooled cutoff, the running variable is the difference between monthly disposable income and the respective threshold the debtor faces. Table entries are local linear regression estimates with a rectangular kernel of discontinuities in pretreatment covariates around the different cutoffs provided by law and described in Figure 7. Neighborhood is the distance from the respective cutoffs (bandwidth). Each cell represents a separate regression with baseline covariates as the dependent variable and the threshold crossing variable. Heteroskedasticity-robust standard errors in parentheses. ***, **, and * indicate the p-values of 1%, 5%, and 10%, respectively.

	First cutoff		Second cutoff		Third cutoff	
Running variable	AGMI		Disposable Income		Disposable Income	
Neighborhood	5,000	6,000	50	60	50	60
Household size	-0.030	-0.004	0.131	0.162	-0.051	-0.117
	(0.046)	(0.042)	(0.130)	(0.102)	(0.159)	(0.127)
Married	0.043	0.044	0.081	0.063	-0.187	-0.013
	(0.039)	(0.036)	(0.137)	(0.089)	(0.139)	(0.111)
Filing jointly	-0.003	-0.002	0.081	0.139	-0.050	-0.100
	(0.037)	(0.034)	(0.116)	(0.092)	(0.136)	(0.109)
Ln Assets	-0.080	-0.030	0.366	0.273	-0.059	-0.047
	(0.131)	(0.122)	(0.407)	(0.318)	(0.347)	(0.310)
Ln Liabilities	0.015	0.007	0.144	0.173	0.031	0.042
	(0.110)	(0.102)	(0.272)	(0.240)	(0.316)	(0.272)
Ln Job tenure	-0.095	-0.089	-0.059	-0.037	-0.067	-0.068
	(0.116)	(0.110)	(0.347)	(0.299)	(0.363)	(0.284)
Age at filing	0.424	0.539	-3.776	-3.458	-0.813	-1.655
	(0.876)	(0.806)	(2.669)	(2.600)	(2.955)	(2.370)
Male	0.080	0.069	-0.177	-0.118	-0.356	-0.353
	(0.060)	(0.055)	(0.171)	(0.148)	(0.354)	(0.334)
White	-0.025	-0.017	-0.054	-0.085	0.070	0.102
	(0.083)	(0.069)	(0.263)	(0.229)	(0.287)	(0.260)
Criminal background	0.011	0.012	-0.144	-0.044	0.065	0.068
	(0.029)	(0.026)	(0.092)	(0.069)	(0.097)	(0.080)
Business owners	-0.009	-0.011	-0.050	-0.031	0.054	0.038
	(0.024)	(0.018)	(0.040)	(0.034)	(0.059)	(0.049)
Income Volatility/Income	0.123	0.132	0.054	0.048	0.172	0.267
	(0.123)	(0.121)	(0.055)	(0.058)	(0.237)	(0.277)
Real Properties/Assets	-0.022	-0.029	-0.016	-0.033	-0.081	-0.042
	(0.018)	(0.021)	(0.047)	(0.036)	(0.049)	(0.038)
Secured Debt/Liabilities	-0.021	-0.021	-0.012	-0.012	-0.034	-0.046
	(0.025)	(0.023)	(0.052)	(0.042)	(0.057)	(0.049)
Unsecured Debt/Liabilities	0.022	0.023	0.021	0.010	0.047	0.057
	(0.024)	(0.023)	(0.051)	(0.033)	(0.057)	(0.049)
Home Equity/Real Properties	0.007	0.012	-0.066	-0.023	-0.100	-0.063
	(0.034)	(0.032)	(0.064)	(0.052)	(0.069)	(0.055)
Number of Properties	0.039	0.036	-0.044	-0.055	-0.235	-0.182
	(0.056)	(0.052)	(0.136)	(0.119)	(0.184)	(0.220)

Table 1.3 Test of Discontinuities in Covariates for Filers who do not Qualify for Chapter 7

Panel A tests for differences between those debtors who file for Chapter 13 protection but do not qualify for Chapter 7 against those who file for Chapter 7. Panel B tests for differences between those debtors who file for Chapter 13 protection but do not qualify for Chapter 7 against those who file for Chapter 7 and are dismissed. In the first cutoff, the running variable is the difference between the Average Gross Monthly Income (AGMI) and the state median income based on household size. The pooled cutoff combines the second and third cutoffs, as Figure 7 describes. In the pooled cutoff, the running variable is the difference between monthly disposable income and the respective threshold that the debtor faces. The pooled specifications include thresholds indicator. Table entries are local linear regression estimates with a rectangular kernel of discontinuities in pretreatment covariates using the first and the pooled cutoffs. Each cell represents a separate regression with baseline covariates as the dependent variable and an indicator variable for filers do not qualify for Chapter 7. Neighborhood is the distance from the respective cutoffs. Heteroskedasticity-robust standard errors in parentheses. ***, **, and * indicate p-values of 1%, 5%, and 10%, respectively.

Panel A	First cutoff		Pooled cutoff	
Running variable	AGMI		Disposable Income	
Neighborhood	5,000	6,000	50	60
Household size	0.158	0.100	0.065	0.092
	(0.141)	(0.136)	(0.113)	(0.109)
Married	-0.127	-0.104	-0.175	-0.147
	(0.124)	(0.119)	(0.113)	(0.106)
Filing jointly	-0.119	-0.116	0.084	0.039
	(0.117)	(0.113)	(0.105)	(0.094)
Ln Assets	-0.038	-0.037	0.028	0.017
	(0.143)	(0.140)	(0.105)	(0.099)
Ln Liabilities	-0.178	-0.156	-0.184	-0.137
	(0.195)	(0.187)	(0.219)	(0.111)
Ln Job tenure	-0.014	-0.048	0.077	0.038
	(0.154)	(0.150)	(0.062)	(0.046)
Age at filing	0.157	0.188	-1.361	-1.795
	(0.251)	(0.247)	(2.445)	(2.145)
Male	0.079	0.045	-0.028	-0.061
	(0.170)	(0.169)	(0.147)	(0.124)
White	-0.035	-0.064	0.078	0.042
	(0.074)	(0.072)	(0.190)	(0.095)
Criminal background	-0.091	-0.071	0.113	0.158
	(0.079)	(0.076)	(0.083)	(0.079)
Business owners	0.046	0.055	0.086	0.072
	(0.081)	(0.074)	(0.058)	(0.053)
Income Volatility/Income	0.033	0.040	0.051	0.058
	(0.149)	(0.159)	(0.069)	(0.073)
Real Properties/Assets	0.011	0.021	0.046	0.031
	(0.036)	(0.037)	(0.047)	(0.026)
Secured Debt/Liabilities	0.054	0.057	0.086	0.076
	(0.060)	(0.057)	(0.077)	(0.065)
Unsecured Debt/Liabilities	-0.068	-0.069	-0.091	-0.080
	(0.061)	(0.057)	(0.086)	(0.085)
Home Equity/Real Properties	0.107	0.103	0.064	0.081
	(0.073)	(0.071)	(0.059)	(0.075)
Number of Properties	-0.029	-0.036	0.072	0.062
	(0.034)	(0.036)	(0.052)	(0.041)

Table 1.3 continued

Panel B	First cutoff		Pooled cutoff	
Running variable	AGMI		Disposable Income	
Neighborhood	5,000	6,000	50	60
Household size	-0.014	-0.019	-0.020	-0.016
	(0.245)	(0.233)	(0.082)	(0.071)
Married	-0.240	-0.264	-0.294	-0.237
	(0.216)	(0.206)	(0.273)	(0.258)
Filing jointly	-0.129	-0.103	-0.180	-0.164
	(0.113)	(0.112)	(0.180)	(0.173)
Ln Assets	-0.179	-0.252	-0.225	-0.197
	(0.190)	(0.185)	(0.215)	(0.205)
Ln Liabilities	-0.113	-0.135	-0.128	-0.153
	(0.179)	(0.171)	(0.176)	(0.183)
Ln Job tenure	-0.541	-0.561	0.720	0.752
	(0.439)	(0.457)	(0.776)	(0.824)
Age at filing	-2.385	-3.452	3.101	2.942
	(5.683)	(5.659)	(2.994)	(2.817)
Male	0.073	0.081	-0.091	-0.036
	(0.328)	(0.321)	(0.209)	(0.147)
White	-0.163	-0.178	-0.073	-0.109
	(0.172)	(0.155)	(0.096)	(0.101)
Criminal background	-0.109	-0.102	0.175	0.127
	(0.199)	(0.198)	(0.181)	(0.196)
Business owners	-0.021	-0.022	0.007	0.005
	(0.108)	(0.107)	(0.007)	(0.008)
Income Volatility/Income	-0.103	-0.108	0.080	0.101
	(0.261)	(0.270)	(0.070)	(0.124)
Real Properties/Assets	0.084	0.055	0.077	0.072
	(0.192)	(0.190)	(0.048)	(0.046)
Secured Debt/Liabilities	0.158	0.140	0.016	0.020
	(0.233)	(0.231)	(0.061)	(0.054)
Unsecured Debt/Liabilities	-0.146	-0.130	-0.028	-0.031
	(0.173)	(0.171)	(0.059)	(0.042)
Home Equity/Real Properties	0.101	0.090	0.093	0.084
	(0.097)	(0.094)	(0.101)	(0.092)
Number of Properties	-0.137	-0.150	0.102	0.107
	(0.139)	(0.139)	(0.111)	(0.104)

Table 1.4 Access to Chapter 7

This table presents the first stage estimates of the respective threshold crossing indicator (e.g., below the first cutoff) on Chapter 7 protection. In the first cutoff, the running variable is the difference between the Average Gross Monthly Income (AGMI) and the state median income based on household size. The pooled cutoff combines the second and third cutoffs, as Figure 7 describes. In the pooled cutoff, the running variable is the difference between monthly disposable income and the respective threshold the debtor faces. The pooled specifications include thresholds indicator. Table entries are local linear regression with a rectangular kernel. Each cell represents a separate regression as the dependent variable (Chapter 7 protection indicator) and the threshold crossing variable. Covariates include age at filing, household size and marital status. Neighborhood is the distance from the respective cutoffs. Heteroskedasticity-robust standard errors in parentheses. ***, **, and * indicate p-values of 1%, 5%, and 10%, respectively.

Panel A			First cutoff		
Running variable			AGMI		
Neighborhood	5,000	5,000	6,000	6,000	6,000
Chapter 7	0.093***	0.097***	0.082***	0.085***	0.108***
	(0.022)	(0.022)	(0.021)	(0.021)	(0.031)
Specification	Linear	Linear	Linear	Linear	Quadratic
Covariates and Year FE	N	Y	N	Y	Y

Panel B			Second Cutoff		
Running variable			Disposable Income		
Neighborhood	50	50	60	60	60
Chapter 7	0.315***	0.305***	0.266***	0.260***	0.338***
	(0.080)	(0.081)	(0.067)	(0.068)	(0.095)
Specification	Linear	Linear	Linear	Linear	Quadratic
Covariates and Year FE	N	Y	N	Y	Y

Panel C			Third Cutoff		
Running variable			Disposable Income		
Neighborhood	50	50	50	60	60
Chapter 7	0.554***	0.548***	0.556***	0.549***	0.515***
	(0.081)	(0.081)	(0.079)	(0.078)	(0.109)
Specification	Linear	Linear	Linear	Linear	Quadratic
Covariates and Year FE	N	Y	N	Y	Y

Panel D			Pooled Cutoff		
Running variable			Disposable Income		
Neighborhood	50	50	60	60	60
Chapter 7	0.446***	0.443***	0.444***	0.438***	0.450***
	(0.066)	(0.067)	(0.064)	(0.064)	(0.086)
Specification	Linear	Linear	Linear	Linear	Quadratic
Covariates and Year FE	N	Y	N	Y	Y

Table 1.5 Chapter 7 and Debtors' Post-Filing Outcomes

This table reports the fuzzy RD estimates of Chapter 7 bankruptcy protection on post-filing investment decisions, financial distress events and miscellaneous outcomes. In the first cutoff the running variable is the difference between the Average Gross Monthly Income (AGMI) and the state median income based on household size. The pooled cutoff combines the second and third cutoffs, as Figure 7 describes. In the pooled cutoff the running variable is the difference between monthly disposable income and the respective threshold that debtor faces. The pooled specifications include thresholds indicator. Local linear regression estimates with rectangular kernel. Each cell represents a separate regression with debtor's ex-post outcome as the dependent variable and the indicator variable of Chapter 7 protection. Home foreclosure is an indicator for a filer's home receiving a notice of default, receiving a notice of transfer or sale, or having been transferred to an REO or a guarantor on or before the indicated year. New Property comprises the acquisition of a new real property by the filer. Judgment Lien is an indicator variable if debtor receives at least one lien. It includes tax liens, hospital liens, and judicial liens. Start a Business is an indicator variable for any business registered in public records by the debtor post-filing. Covariates include age at filing, household size and marital status. Neighborhood is the distance from respective cutoff. Heteroskedasticity-robust standard errors in parentheses. ***, **, and * indicate p-values of 1%, 5%, and 10%, respectively.

	First cutoff				
Running variable	AGMI				
Neighborhood	5,000	5,000	6,000	6,000	6,000
Investment decisions					
New real property (3-year)	0.266	0.256	0.249	0.241	0.265
	(0.217)	(0.211)	(0.234)	(0.228)	(0.237)
New real property (6-year)	0.157	0.148	0.156	0.150	0.148
	(0.247)	(0.241)	(0.269)	(0.262)	(0.274)
Start a Business (3-year)	0.210**	0.210**	0.233**	0.233**	0.241**
	(0.096)	(0.094)	(0.113)	(0.112)	(0.121)
Start a Business (6-year)	0.188*	0.192*	0.194**	0.216**	0.211*
	(0.110)	(0.103)	(0.102)	(0.109)	(0.117)
Financial Distress Events					
Home foreclosure (3-year)	-0.288	-0.298	-0.370	-0.357	-0.238
	(0.412)	(0.407)	(0.377)	(0.375)	(0.463)
Home foreclosure (6-year)	-0.538	-0.553	-0.597	-0.589	-0.434
	(0.445)	(0.440)	(0.409)	(0.408)	(0.471)
Judgment Lien (3-year)	-0.589**	-0.618**	-0.586**	-0.616**	-0.634**
	(0.299)	(0.297)	(0.286)	(0.300)	(0.305)
Judgment Lien (6-year)	-0.713**	-0.696**	-0.680**	-0.665**	-0.674**
	(0.342)	(0.334)	(0.343)	(0.334)	(0.316)
Future Bankruptcy	-0.664***	-0.687***	-0.675***	-0.692***	-0.804***
	(0.229)	(0.221)	(0.212)	(0.209)	(0.296)
Miscellaneous Outcome					
Mortality (3 year)	0.051	0.050	0.057	0.055	0.033
	(0.067)	(0.064)	(0.070)	(0.068)	(0.078)
Mortality (6-year)	0.119	0.127	0.123	0.111	0.136
	(0.156)	(0.124)	(0.166)	(0.124)	(0.164)
Specification	Linear	Linear	Linear	Linear	Quadratic
Covariates and Year FE	N	Y	N	Y	Y

Table 1.5 continued

Running variable	Pooled cutoff Disposable Income				
Neighborhood	50	50	60	60	60
Investment decisions					
New real property (3-year)	0.239***	0.225***	0.213**	0.202**	0.206**
	(0.089)	(0.085)	(0.087)	(0.083)	(0.099)
New real property (6-year)	0.229**	0.219**	0.212**	0.207**	0.201**
	(0.109)	(0.104)	(0.106)	(0.102)	(0.094)
Start a Business (3-year)	0.076*	0.078*	0.083**	0.082**	0.066**
	(0.043)	(0.044)	(0.040)	(0.042)	(0.033)
Start a Business (6-year)	0.145*	0.169*	0.152**	0.167**	0.096**
	(0.082)	(0.088)	(0.077)	(0.085)	(0.041)
Financial Distress Events					
Home foreclosure (3-year)	-0.617**	-0.639**	-0.617**	-0.605**	-0.452**
	(0.315)	(0.321)	(0.309)	(0.303)	(0.220)
Home foreclosure (6-year)	-0.640**	-0.658**	-0.658**	-0.646**	-0.497**
	(0.323)	(0.328)	(0.317)	(0.311)	(0.241)
Judgment Lien (3-year)	-0.353**	-0.377**	-0.391***	-0.410***	-0.414**
	(0.150)	(0.161)	(0.147)	(0.158)	(0.207)
Judgment Lien (6-year)	-0.485***	-0.508***	-0.527***	-0.540***	-0.498**
	(0.171)	(0.184)	(0.168)	(0.182)	(0.231)
Future Bankruptcy	-0.131	-0.145	-0.114	-0.131	-0.085
	(0.122)	(0.117)	(0.122)	(0.117)	(0.144)
Miscellaneous Outcome					
Mortality (3 year)	-0.105*	-0.104*	-0.086*	-0.085*	-0.093
	(0.058)	(0.057)	(0.050)	(0.049)	(0.057)
Mortality (6-year)	-0.020	-0.012	-0.024	-0.017	-0.010
	(0.070)	(0.070)	(0.060)	(0.060)	(0.070)
Specification	Linear	Linear	Linear	Linear	Quadratic
Covariates and Year FE	N	Y	N	Y	Y

Table 1.6 Business Creation adjusted for Firm Survival and New Business Owners

This table reports the fuzzy RD estimates of Chapter 7 bankruptcy protection on different sub-samples for starting a business. In the first cutoff the running variable is the difference between the Average Gross Monthly Income (AGMI) and the state median income based on household size. The pooled cutoff combines the second and third cutoffs, as Figure 7 describes. In the pooled cutoff the running variable is the difference between monthly disposable income and the respective threshold that debtor faces. The pooled specifications include thresholds indicator. Local linear regression estimates with a rectangular kernel. Each cell represents a separate regression with debtor's ex-post outcome as the dependent variable and the indicator variable of Chapter 7 protection. Panel A includes only those firms that were created post-filing and remain active in 2015. Panel B comprises only those firms created by a filer who did not have a business registered before filing for bankruptcy. Covariates include age at filing, household size and marital status. Neighborhood is the distance from respective cutoff. Heteroskedasticity-robust standard errors in parentheses. ***, ** and * indicate p-values of 1%, 5%, and 10%, respectively.

Running variable	First Cutoff AGMI				Pooled Disposable Income			
Neighborhood	5,000	5,000	6,000	6,000	50	50	60	60
Panel A: Adjusting for Firm Survival								
Start a Business (3-year)	0.210**	0.211**	0.187**	0.188**	0.025**	0.026**	0.030**	0.031**
	(0.088)	(0.087)	(0.079)	(0.078)	(0.012)	(0.013)	(0.015)	(0.015)
Start a Business (6-year)	0.192**	0.212**	0.162*	0.184**	0.076**	0.096**	0.087**	0.103**
	(0.097)	(0.097)	(0.087)	(0.088)	(0.033)	(0.046)	(0.041)	(0.045)
Panel B: New Business Owners								
Start a Business (3-year)	0.141**	0.137**	0.139**	0.136**	0.071**	0.078**	0.073**	0.080**
	(0.070)	(0.068)	(0.069)	(0.067)	(0.036)	(0.038)	(0.035)	(0.038)
Start a Business (6-year)	0.149**	0.146**	0.148**	0.145**	0.101*	0.121*	0.107*	0.121*
	(0.072)	(0.071)	(0.072)	(0.071)	(0.058)	(0.072)	(0.065)	(0.070)
Specification	Linear	Linear	Linear	Linear	Linear	Linear	Linear	Linear
Covariates and Year FE	N	Y	N	Y	N	Y	N	Y

Table 1.7 Home Equity and Debtors' Post-Filing Outcomes

This table presents the fuzzy RD estimates of Chapter 7 bankruptcy protection by home equity. In the first cutoff the running variable is the difference between the Average Gross Monthly Income (AGMI) and the state median income based on household size. The pooled cutoff combines the second and third cutoffs, as Figure 7 describes. In the pooled cutoff the running variable is the difference between monthly disposable income and the respective threshold that debtor faces. The pooled specifications include thresholds indicator. Local linear regression estimates with a rectangular kernel. Each cell represents a separate regression with debtor's ex-post outcome as the dependent variable and the indicator variable of Chapter 7 protection. Home foreclosure is an indicator for a filer's home receiving a notice of default, receiving a notice of transfer or sale, or having been transferred to an REO or a guarantor on or before the indicated year. New Property comprises the acquisition of a new real property by the filer. Judgment Lien is an indicator variable if debtor receives at least one lien. It includes tax liens, hospital liens, and judicial liens. Start a Business is an indicator variable for any business registered in public records by the debtor post-filing. Covariates include age at filing, household size and marital status. Neighborhood is the distance from respective cutoff. Heteroskedasticity-robust standard errors in parentheses. ***, **, and * indicate p-values of 1%, 5%, and 10%, respectively.

	First cutoff			Pooled cutoff		
Home Equity	Negative	Positive		Negative	Positive	
Neighborhood / p-value	6,000	6,000	p-value	60	60	p-value
Investment Decisions						
New real property (3-year)	0.284	0.303	0.332	0.157*	0.228**	0.130
	(0.220)	(0.221)		(0.089)	(0.100)	
New real property (6-year)	0.156	0.181	0.287	0.138	0.282*	0.044
	(0.259)	(0.261)		(0.138)	(0.153)	
Start a Business (3-year)	0.143*	0.181*	0.083	0.047*	0.110**	0.010
	(0.087)	(0.099)		(0.026)	(0.055)	
Start a Business (6-year)	0.108	0.145*	0.104	0.025	0.141**	0.059
	(0.083)	(0.075)		(0.030)	(0.070)	
Financial Distress Events						
Home foreclosure (3-year)	-0.381	-0.628*	0.000	-0.479	-0.659*	0.074
	(0.373)	(0.373)		(0.351)	(0.349)	
Home foreclosure (6-year)	-0.523	-0.772*	0.000	-0.609*	-0.831**	0.028
	(0.398)	(0.399)		(0.370)	(0.370)	
Judgment Lien (3-year)	-0.535	-0.585*	0.027	-0.513***	-0.561***	0.443
	(0.330)	(0.334)		(0.196)	(0.198)	
Judgment Lien (6-year)	-0.647*	-0.688*	0.105	-0.663***	-0.671***	0.919
	(0.378)	(0.383)		(0.219)	(0.227)	
Future Bankruptcy	-0.531**	-0.541**	0.481	-0.040	-0.115	0.088
	(0.223)	(0.225)		(0.144)	(0.140)	
Covariates and Year FE	Y	Y		Y	Y	

Table 1.8 Impact of Debt relief on Debtors' Post-Filing Outcomes

This table reports the fuzzy RD estimates of debt relief through Chapter 7 bankruptcy protection on post-filing investment decisions, and financial distress events. In the first cutoff the running variable is the difference between the Average Gross Monthly Income (AGMI) and the state median income based on household size. The pooled cutoff combines the second and third cutoffs, as Figure 7 describes. In the pooled cutoff the running variable is the difference between monthly disposable income and the respective threshold that debtor faces. The pooled specifications include thresholds indicator. Debt relief is expressed in 1981 dollars divided by 1,000 and corresponds to the total amount of debt discharged. Due to outlying observations, the debt relief variable is Winsorized at the 5th and 95th percentiles. Local linear regression estimates with a rectangular kernel. Each cell represents a separate regression with debtor's ex-post outcome as the dependent variable and debt relief. Home foreclosure is an indicator for a filer's home receiving a notice of default, receiving a notice of transfer or sale, or having been transferred to an REO or a guarantor on or before the indicated year. New Property comprises the acquisition of a new real property by the filer. Judgment Lien is an indicator variable if debtor receives at least one lien. It includes tax liens, hospital liens, and judicial liens. Start a Business is an indicator variable for any business registered in public records by the debtor post-filing. Covariates include age at filing, household size and marital status. Neighborhood is the distance from respective cutoff. Heteroskedasticity-robust standard errors in parentheses. ***, **, and * indicate p-values of 1%, 5%, and 10%, respectively.

	First cutoff		Pooled Cutoff	
Running Variable	AGMI		Disposable Income	
Neighborhood	5,000	6,000	50	60
Investment decisions				
New real property (3-year)	0.0025	0.0021	0.0068**	0.0071**
	(0.0043)	(0.0044)	(0.0034)	(0.0033)
New real property (6-year)	0.0020	0.0021	0.0072**	0.0072**
	(0.0053)	(0.0055)	(0.0037)	(0.0035)
Start a Business (3-year)	0.0049**	0.0043**	0.0030**	0.0033**
	(0.0024)	(0.0021)	(0.0014)	(0.0014)
Start a Business (6-year)	0.0047**	0.0036**	0.0046**	0.0048**
	(0.0020)	(0.0017)	(0.0019)	(0.0017)
Financial Distress Events				
Home foreclosure (3-year)	-0.0032	-0.0030	-0.0228**	-0.0258**
	(0.0065)	(0.0061)	(0.0110)	(0.0126)
Home foreclosure (6-year)	-0.0053	-0.0050	-0.0178**	-0.0199**
	(0.0074)	(0.0068)	(0.0093)	(0.0101)
Judgment Lien (3-year)	-0.0143**	-0.0132**	-0.0121**	-0.0124**
	(0.0062)	(0.0061)	(0.006)	(0.006)
Judgment Lien (6-year)	-0.0158**	-0.0156**	-0.0156**	-0.0149**
	(0.0068)	(0.0063)	(0.0070)	(0.0067)
Future Bankruptcy	-0.0147**	-0.0149**	-0.0051	-0.0047
	(0.0074)	(0.0076)	(0.0062)	(0.0077)
Covariates and Year FE	Y	Y	Y	Y

Table 1.9 Homestead Exemption and Debtors' Post-Filing Outcomes

This table reports the Fuzzy RD estimates of access of Chapter 7 Bankruptcy protection for states with above median and below median homestead exemption, conditional on having positive home equity. Local linear regression estimates with rectangular kernel. Each cell represents a separate regression with debtor's ex-post outcome as the dependent variable and the indicator variable of access to Chapter 7. Controls include pretreatment covariates include age at filing, household's size and marital status. Heteroskedasticity-robust standard errors in parentheses. ***, ** and * indicate p-values of 1%, 5%, and 10%, respectively.

	First cutoff			Pooled cutoff		
Homestead Exemption	Below Median	Above Median		Below Median	Above Median	
Neighborhood	6,000	6,000	p-value	60	60	p-value
Investment decisions						
New real property (3-year)	0.090	0.227	0.268	0.130	0.108	0.638
	(0.666)	(0.267)		(0.172)	(0.136)	
New real property (6-year)	0.031	0.278	0.319	0.315	0.186	0.217
	(0.530)	(0.303)		(0.233)	(0.168)	
Start a Business (3-year)	0.170	0.225**	0.087	0.065	0.202*	0.070
	(0.184)	(0.115)		(0.057)	(0.108)	
Start a Business (6-year)	0.233	0.228*	0.124	0.087	0.245**	0.002
	(0.204)	(0.132)		(0.060)	(0.116)	
Financial Distress Events						
Home foreclosure (3-year)	-0.371	-0.774**	0.071	-0.346	-0.748**	0.057
	(0.731)	(0.356)		(0.674)	(0.291)	
Home foreclosure (6-year)	-0.131	-0.895**	0.068	-0.348	-0.839***	0.043
	(0.828)	(0.408)		(0.701)	(0.276)	
Judgment Lien (3-year)	0.167	-0.896**	0.049	-0.446	-0.501**	0.069
	(0.355)	(0.386)		(0.416)	(0.223)	
Judgment Lien (6-year)	-0.279	-0.888**	0.033	-0.687	-0.490*	0.095
	(0.299)	(0.413)		(0.576)	(0.257)	
Future Bankruptcy	-0.478	-0.644**	0.086	-0.077	-0.446**	0.067
	(0.480)	(0.326)		(0.288)	(0.213)	
Covariates and Year FE	Y	Y		Y	Y	

Table 1.10 Chapter 7 and Secured Lending

This table presents the fuzzy RD estimates of Chapter 7 Bankruptcy protection on secured lending. In the first cutoff the running variable is the difference between the Average Gross Monthly Income (AGMI) and the state median income based on household size. The pooled cutoff combines the second and third cutoffs, as Figure 7 describes. In the pooled cutoff the running variable is the difference between monthly disposable income and the respective threshold that debtor faces. The pooled specifications include thresholds indicator. UCC loans are loans with collateral in which a UCC-1 form was filed. Mortgage corresponds to loans for the acquisition of real estate properties. Local linear regression estimates with a rectangular kernel. Each cell represents a separate regression with debtor's ex-post outcome as the dependent variable and the indicator variable of Chapter 7 protection. Covariates include age at filing, household size and marital status. Heteroskedasticity-robust standard errors in parentheses. ***, **, and * indicate p-values of 1%, 5%, and 10%, respectively.

Running Variable	First cutoff AGMI	Pooled cutoff Disposable Income
Neighborhood	6,000	60
UCC loans (3-year)	0.074	0.085**
	(0.114)	(0.041)
UCC loans (6-year)	0.094	0.102**
	(0.142)	(0.045)
UCC loans / Home equity (3-year)	0.201**	0.089**
	(0.095)	(0.040)
UCC loans / Home equity (6-year)	0.179**	0.188**
	(0.101)	(0.100)
Mortgage (3-year)	0.028	0.123**
	(0.148)	(0.061)
Mortgage (6-year)	0.121	0.165**
	(0.224)	(0.076)
Covariates and Year FE	Y	Y

Table 1.11 Non-Judicial Debt Collection and Debtors' Post-Filing Outcomes

This table presents the fuzzy RD estimates of Chapter 7 Bankruptcy protection by level of Non-Judicial Debt Collection laws. States with anti-harassment laws are those which do not allow non-judicial debt collection. Local linear regression estimates a with rectangular kernel. Each cell represents a separate regression with debtor's ex-post outcome as the dependent variable and the indicator variable of Chapter 7 protection. Home foreclosure is an indicator for a filer's home receiving a notice of default, receiving a notice of transfer or sale, or having been transferred to an REO or a guarantor on or before the indicated year. New Property comprises the acquisition of a new real property by the filer. Judgment Lien is an indicator variable if debtor receives at least one lien. It includes tax liens, hospital liens, and judicial liens. Start a Business is an indicator variable for any business registered in public records by the debtor post-filing. Covariates include age at filing, household size and marital status. Heteroskedasticity-robust standard errors in parentheses. ***, ** and * indicate p-values of 1%, 5%, and 10%, respectively.

	First cutoff			Pooled cutoff		
Non-Judicial Debt Collection allowed	No	Yes		No	Yes	
Neighborhood / p-value	6,000	6,000	p-value	60	60	p-value
Investment Decisions						
New real property (3-year)	0.199	0.194	0.613	0.245**	0.210**	0.480
	(0.187)	(0.187)		(0.104)	(0.095)	
New real property (6-year)	0.095	0.083	0.364	0.278**	0.193*	0.120
	(0.218)	(0.217)		(0.124)	(0.116)	
Start a Business (3-year)	0.223**	0.211**	0.066	0.102**	0.052*	0.038
	(0.093)	(0.089)		(0.046)	(0.029)	
Start a Business (6-year)	0.179*	0.167*	0.121	0.137*	0.101*	0.123
	(0.103)	(0.101)		(0.075)	(0.057)	
Financial Distress Events						
Home foreclosure (3-year)	-0.142	-0.213	0.057	-0.531	-0.634**	0.122
	(0.346)	(0.342)		(0.326)	(0.298)	
Home foreclosure (6-year)	-0.377	-0.413	0.366	-0.527	-0.694**	0.365
	(0.373)	(0.370)		(0.328)	(0.310)	
Judgment Lien (3-year)	-0.543**	-0.526*	0.251	-0.392**	-0.375**	0.166
	(0.268)	(0.264)		(0.166)	(0.148)	
Judgment Lien (6-year)	-0.626**	-0.594**	0.128	-0.568***	-0.494***	0.656
	(0.302)	(0.300)		(0.190)	(0.172)	
Future Bankruptcy	-0.701***	-0.690***	0.538	-0.106	-0.166	0.070
	(0.211)	(0.207)		(0.124)	(0.110)	
Covariates and Year FE	Y	Y		Y	Y	

Table 1.12 Robustness of Core Results to the Possibility of Heaping

This table reports the fuzzy RD estimates of Chapter 7 bankruptcy protection on post-filing investment decisions and financial distress as robustness to the possibility of heaping, following Barreca et al. (2011). "Drop Cutoff Heap" drops observations \$500 below the first cutoff and \$5 below the pooled cutoff ("donut" RD). "Trends in Heaps" controls for a dummy equal to one for observations \$500 below the first cutoff and \$5 below the pooled cutoff and an interaction between those dummies and distance from the cutoff and also the interaction with distance from the cutoff threshold crossing variable. In the first cutoff the running variable is the difference between the Average Gross Monthly Income (AGMI) and the state median income based on household size. The pooled cutoff combines the second and third cutoffs, as Figure 7 describes. In the pooled cutoff the running variable is the difference between monthly disposable income and the respective threshold that debtor faces. Local linear regression estimates with rectangular kernel. Each cell represents a separate regression with debtor's ex-post outcome as the dependent variable and the indicator variable of Chapter 7 protection. Home foreclosure is an indicator for a filer's home receiving a notice of default, receiving a notice of transfer or sale, or having been transferred to an REO or a guarantor on or before the indicated year. New Property comprises the acquisition of a new real property by the filer. Judgment Lien is an indicator variable if debtor receives at least one lien. It includes tax liens, hospital liens, and judicial liens. Start a Business is an indicator variable for any business registered in public records by the debtor post-filing. Covariates include age at filing, household size and marital status. Neighborhood is the distance from respective cutoff. Heteroskedasticity-robust standard errors in parentheses. ***, ** and * indicate p-values of 1%, 5%, and 10%, respectively.

	First cutoff AGMI				Pooled cutoff Disposable Income			
	Drop Cutoff Heap		Trends in Heap		Drop Cutoff Heap		Trends in Heap	
Neighborhood	6,000		6,000		60		60	
Investment decisions								
New real property (3-year)	0.336	0.304	0.290	0.304	0.231**	0.220**	0.231**	0.226**
	(0.241)	(0.231)	(0.221)	(0.231)	(0.105)	(0.101)	(0.105)	(0.101)
New real property (6-year)	0.269	0.222	0.211	0.224	0.185**	0.178**	0.190**	0.179**
	(0.273)	(0.263)	(0.253)	(0.263)	(0.083)	(0.076)	(0.092)	(0.081)
Start a Business (3-year)	0.193**	0.191**	0.209**	0.211**	0.079**	0.087**	0.084**	0.089**
	(0.088)	(0.087)	(0.092)	(0.091)	(0.040)	(0.044)	(0.040)	(0.043)
Start a Business (6-year)	0.191**	0.209**	0.194*	0.211**	0.105**	0.114**	0.092**	0.105**
	(0.097)	(0.095)	(0.107)	(0.106)	(0.045)	(0.041)	(0.039)	(0.040)
Financial Distress Events								
Home foreclosure (3-year)	-0.418	-0.405	-0.426	-0.404	-0.602**	-0.589**	-0.611**	-0.597**
	(0.363)	(0.339)	(0.364)	(0.339)	(0.307)	(0.302)	(0.305)	(0.300)
Home foreclosure (6-year)	-0.668*	-0.635*	-0.656*	-0.637*	-0.682**	-0.669**	-0.694**	-0.685**
	(0.392)	(0.369)	(0.384)	(0.370)	(0.342)	(0.337)	(0.352)	(0.336)
Judgment Lien (3-year)	-0.534**	-0.554**	-0.534*	-0.523*	-0.505**	-0.505**	-0.517**	-0.516**
	(0.276)	(0.278)	(0.287)	(0.285)	(0.225)	(0.232)	(0.226)	(0.229)
Judgment Lien (6-year)	-0.651**	-0.637**	-0.651**	-0.636**	-0.577**	-0.592**	-0.569**	-0.574**
	(0.326)	(0.314)	(0.327)	(0.324)	(0.254)	(0.265)	(0.247)	(0.250)
Future Bankruptcy	-0.658***	-0.663***	-0.641***	-0.635***	-0.086	-0.085	-0.078	-0.089
	(0.206)	(0.200)	(0.212)	(0.210)	(0.129)	(0.129)	(0.133)	(0.128)
Specification	Linear	Linear	Linear	Linear	Linear	Linear	Linear	Linear
Covariates and Year FE	N	Y	N	Y	N	Y	N	Y

Table 1.13 Change in Outcomes Resulting from a Marginal Increase in Thresholds

This table reports the fuzzy RD estimates of Chapter 7 bankruptcy protection on post-filing investment decisions and financial distress events if thresholds were increased 1% (i.e., increased access to Chapter 7), following Dong and Lewbel (2012). The MTTE is the change in the RD treatment effect resulting from a marginal change in the RD threshold. For the first cutoff, 1% increase in the gross monthly income is $41, and for the pooled cutoff, 1% increase in the monthly disposable income is $1.40. Treatment effect - new refers to the RD treatment effect if the threshold were marginally increased by 1%. Local linear regression estimates with a rectangular kernel. Each cell represents a separate regression with debtor's ex-post outcome as the dependent variable and the indicator variable of Chapter 7 protection. Home foreclosure is an indicator for a filer's home receiving a notice of default, receiving a notice of transfer or sale, or having been transferred to an REO or a guarantor on or before the indicated year. New Property comprises the acquisition of a new real property by the filer. Judgment Lien is an indicator variable if debtor receives at least one lien. It includes tax liens, hospital liens, and judicial liens. Start a Business is an indicator variable for any business registered in public records by the debtor post-filing. Covariates include age at filing, household size and marital status. Neighborhood is the distance from respective cutoff. Heteroskedasticity-robust standard errors in parentheses for the treatment effect. Standard errors for the estimated MTTE and the new treatment effect are calculated using the Delta method. ***, **, and * indicate p-values of 1%, 5%, and 10%, respectively.

	First cutoff			Pooled cutoff		
	Treatment	MTTE	Treatment New	Treatment	MTTE	Treatment New
Neighborhood		6,000			60	
Investment Decisions						
New real property (3-year)	0.241	-0.0031	0.238	0.202**	-0.0078	0.194**
	(0.228)	(0.0040)	(0.228)	(0.083)	(0.0083)	(0.083)
New real property (6-year)	0.150	-0.0043	0.146	0.207**	-0.0119	0.195**
	(0.262)	(0.0046)	(0.262)	(0.102)	(0.0103)	(0.102)
Start a Business (3-year)	0.233**	-0.0029	0.230**	0.082**	0.0040	0.078*
	(0.112)	(0.0025)	(0.112)	(0.042)	(0.0035)	(0.042)
Start a Business (6-year)	0.216**	-0.0014	0.215**	0.167**	-0.0095	0.157*
	(0.109)	(0.0019)	(0.109)	(0.085)	(0.0068)	(0.085)
Financial Distress Events						
Home foreclosure (3-year)	-0.357	0.0010	-0.356	-0.605**	0.0090	-0.596*
	(0.375)	(0.0046)	(0.375)	(0.303)	(0.0256)	(0.304)
Home foreclosure (6-year)	-0.589	0.0014	-0.588	-0.646**	0.0149	-0.631**
	(0.408)	(0.0052)	(0.408)	(0.311)	(0.0266)	(0.312)
Judgment Lien (3-year)	-0.616**	0.0067	-0.609**	-0.410***	0.0084	-0.401***
	(0.300)	(0.0058)	(0.300)	(0.158)	(0.0124)	(0.158)
Judgment Lien (6-year)	-0.665**	0.0054	-0.659**	-0.540***	0.0141	-0.529***
	(0.334)	(0.0063)	(0.334)	(0.182)	(0.0110)	(0.183)
Future Bankruptcy	-0.692***	0.0070	-0.685***	-0.131	-0.0076	-0.123
	(0.209)	(0.0045)	(0.209)	(0.117)	(0.0119)	(0.118)

Chapter 2

Deposit Shocks and Credit Supply: Evidence from U.S. Lottery Winners

Do shocks in the supply of deposits affect loan origination? The answer would be no, if banks operated in the Miller-Modigliani frictionless world. Bank lending would not be constrained by the availability of deposits. Instead, they could simply issue debt, or equity, to offset a loss of deposits.[1] However, the slow recovery of the economy from the 2008-2009 financial crisis (despite extensive policy intervention), combined with the decline in bank lending, has received considerable attention from policymakers (e.g., Bernanke, 2008) emphasizing the need to quantify the impact of shocks to providers of capital. In particular, the change in bank's loan origination to changes in bank's liquidity which is a critical parameter for policy.

This paper estimates how banks respond to deposit shocks and the kinds of bank attributes that may enhance the impact of deposits flows. Since small-sized businesses do not have ready alternatives to banks for their financing needs (Bernanke, 1983), banks play a crucial role in the functioning of the economy.[2]

[1]However, for example, Stein (1998) shows that if banks face adverse selection problems, then shocks that compromise the ability to raise deposits will lead to declines in lending since banks will face difficulty replacing deposits with other forms of financing (e.g., commercial paper).

[2]If firms have costless access to external capital markets, then their functioning should be insensitive to the shocks experienced by their capital providers. However, frictions, i.e., adverse selection

Therefore, quantifying the extent to which bank lending reacts to deposits shocks is a first-order question, and to my knowledge we have no estimates for the U.S.[3] Furthermore, understanding which banks are the most affected can lead to more targeted and more effective implementation of policy interventions.

Since the literature suggests that many banks rely heavily on deposit financing, and local deposit supply impacts local lending, the ideal experiment in this case would randomly assign deposits across banks in different locations. A close variation of such an experiment is possible by examining U.S. lottery jackpot winners of the Powerball and Mega Millions lotteries. Both are jointly shared jackpot games offered in 43 states as of June, 2013 (see Figure 2.1).

The paper's research design relies on the fact that the occurrence of a jackpot winner in a specific locality and at a specific time is random, conditional on the sales of lottery tickets. Since each lottery ticket has the same chance of winning as any other, the probability of selling a winning ticket is a linear function of lottery sales for that particular game. An interesting feature of the setting is that the amount won is also random, conditional on sales.[4] This allows me to test whether there is a positive causal effect of the amount received (less income tax withholdings) on the outcome variable (e.g., deposits). This quasi-experimental design allows me to use a difference-in-difference strategy to estimate the treatment effect at the local level.

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and moral hazard, can lead to financially-constrained firms.

[3]For example, previous papers outside the U.S. have documented that a 1 percent change in bank liquidity leads to 0.60 change in loan origination in Pakistan (Khwaja and Mian, 2008) and 0.745 in Argentina (Paravisini, 2008).

[4]The mean jackpot prize, after tax withholdings and in 2013 dollars, is $46 million.

The setting also provides falsification tests (e.g., prizes that were unclaimed) that allows direct testing for the identification condition; in the absence of the winner's shock, the average change in the outcome variable for the treatment group does not differ relative to the control group.

Furthermore, using branch office deposits data and the estimated amounts received by each jackpot winner, it is possible to determine the branches, and thus the banks, that potentially received the prizes. This last characteristic of the quasi-experimental design allows me to estimate the effect of the shock at the bank level, and whether the effect is persistent, since I also have estimates of the dates on which the winners received (and deposited) their prizes. Thus, the empirical strategy compares (small business) loan origination for banks in the treatment group to banks in the control group, while controlling for any observable and unobservable time-varying effects at the Core Base Statistical Area (CBSA) level (e.g., demand-side effects on the lending behavior of all banks within a given CBSA-year).

This paper proceeds as follows: First, I estimate the causal effect of the jackpot winners' shocks at the local-level (i.e., CBSA) on deposits and small business lending. I next estimate the winners' shock impact at the bank-level (both at the intensive and extensive margins). Subsequently, I examine the heterogeneity in the response to the exposure of treatment with respect to bank attributes.

There are four primary findings. First, the jackpot winners' shocks lead to a significant increase in deposits (4.05% yearly change) and an increase in small business lending at the CBSA level (4.28% yearly change). The shock's effect on small business lending is greater in those CBSAs that have high levels of local bank concentration (6.79%).

Second, banks in the treatment group experience a significant increase in deposits and total lending (1.98% and 2.38% average quarterly change after one standard deviation increase) and the shock induces an increase in small-business lending at the bank level (5.02% after one standard deviation change) controlling for demand conditions in the local markets. Surprisingly, banks on average increase their loan origination the same quarter of the winners shock, but the shock's effect is not persistent. The estimate of the elasticity of total small business lending with respect to deposits is around 0.876 to 0.934, using the winner's shock as an instrument for deposits.[5] Third, there is no evidence that banks in the treatment group experience a relative worsening in their loan portfolio in terms of nonperforming loans or a decrease of interest revenues. Additionally, the winner's shock has no significant effect at the extensive margin.

These results are robust to: (i) different specifications of the treatment variable, (ii) multiple falsification tests provided by the setting (e.g., winners that reside in states other than where the winning tickets were sold), (iii) the possibility of pre-existing trends in the data, (iv) alternative geographical units of analysis, (v) different control groups, (vi) control for local demand-side effects, among others.

[5]The exclusion restriction for the winner shock as an instrument could be violated if it impacts small-business lending through channels other than deposits (e.g., local demand). In this case, the exclusion assumption seems plausible since the CBSA-by-year fixed effects control for any unobservable time-varying effect at the local level (including demand-side effects). However, the identified parameter measures the treatment effect for the subpopulation of compliers whose deposits are altered due to the winner's shock. To examine the external validity of the point estimates, I study the characteristics of the complier group.

The findings suggest that a certain set of banks are financially constrained before experiencing the jackpot winner shock (i.e., small-and medium-sized banks and those with the most illiquid balance sheets). This is consistent with frictions that originate from adverse selection.[6]

This study is related to strands of literature in banking especially on the lending channel that emphasizes the role of financially-constrained banks in amplifying the real effects of aggregate shocks (Bernanke and Blinder, 1992; Kashyap et al., 1993; Kashyap and Stein, 1995, 2000; Houston et al., 1997). This paper is also related to literature on the economics of banking regulation (Kroszner and Strahan, 1999; Berger and Hannan, 1998; Barth et al., 2004). It is also related to the literature on relationship banking. This literature suggests that relationships generate value, since banks obtain soft information about borrowers to help in their credit decisions (Berger and Udell, 1995; Petersen and Rajan, 2002; Berger et al., 2005; Agarwal and Hauswald, 2010). Finally, this paper is also related to more recent literature examining the causal link between shocks to the liability side of banks' balance sheets and lending to firms (Khwaja and Mian, 2008; Paravisini, 2008; Gilje, 2011; Schnabl, 2012; Jimenez et al., 2012; Gilje et al., 2013).

This paper also complements the literature on lending channel in two ways. First, the institutional features of the research design, with shocks spread all over the

[6]Additional to the findings relative to nonperforming loans and decrease of interest revenues, the extensive margin results of no significant effect of the winner's shock on the loan acceptance rates can be interpreted as support for the costly external finance model since the probability of granting loans is not affected, so the banks generally extend credit to borrowers with whom they presumably have had prior relationships with.

U.S., allow studying the deposits shocks effect on banks' balance sheet (e.g., securities holdings, lending, etc.). In addition, it allows isolation of supply shocks from local demand conditions, and estimating the elasticity of (small business) lending with respect to deposits.

The paper is organized as follows: Section 2 provides some background on U.S. lotteries while Section 3 provides details on the data sources. Section 4 explains the research design and presents the findings. Finally, section 5 concludes the paper.

2.1 Institutional Background

There is no national lottery in the U.S. The introduction of government-sponsored lotteries began in Puerto Rico in 1934, followed by New Hampshire in 1964. Currently, lotteries are established in 44 states, the District of Columbia, and Puerto Rico. Powerball and Mega Millions are two U.S. jointly shared jackpot games offered in 44 and 43 states, respectively. The six remaining non-participating states do not operate state lotteries by law.[7] Figure 2.1 shows the U.S. states that offered both Mega Millions and Powerball as June 2013.

Powerball is a shared jackpot game. It is coordinated by the Multi-State Lottery Association (MUSL), a non-profit organization formed by an agreement among

[7] On October 13, 2009, the Powerball and the Mega Millions consortium signed an agreement to allow U.S. lotteries to sell both games, no longer requiring exclusivity. The expansion occurred on January 31, 2010, as 10 Mega Millions members began selling Powerball tickets for their first drawing on February 3. Simultaneously, 23 Powerball members began offering Mega Millions tickets for their first drawing on February 2. Subsequently, during 2010, Arizona, Colorado, Maine, Montana, Nebraska, Oregon and South Dakota started offering Mega Millions. Finally, Louisiana joined Mega Millions in 2011. Alabama, Alaska, Hawaii, Mississippi, Nevada and Utah do not have state lotteries.

various U.S. state lotteries. Powerball's current minimum advertised jackpot is $40 million (in the form of an annuity). There is no maximum jackpot for the Powerball. The jackpot increases when no top-prize (i.e., jackpot) ticket is sold. In Powerball, winning numbers are drawn as follows: a drawing machine randomly draws five white balls from 59 white balls loaded into the machine, while another drawing machine randomly draws one red ball out 35 red balls loaded into the machine. The jackpot is won by matching all five white balls in any order and the red "Powerball." The odds of winning the jackpot are 1 in 175,223,510.[8]

Mega Millions, which is sold in 43 states, has a minimum jackpot of $15 million. In Mega Millions, five white balls are drawn randomly from a drawing machine loaded with 75 white balls numbered one to 75, and one gold "Mega Ball" is draw randomly from a machine loaded with 15 "gold" balls numbered 1 to 15. Players can win the jackpot by matching all six winning numbers in a drawing. The current odds of winning the jackpot are 1 in 258,890,850.[9]

The jackpot winner can choose between the annuity or the cash option. The annuity option is paid in 30 graduated installments over 29 years. The cash option is a lump-sum payment which is the approximate present value of the installments. If a player chooses the cash option, then the lottery will pay the entire cash amount to the winner less income tax withholding amounts required by federal and state

[8]Currently, each ticket costs $2, or $3 with the Power Play option. Prior to January 15, 2012, the games cost $1 each, or $2 with the Power Play option. Power Play is a special feature that allows a winner to increase the original prize amount.

[9]Each ticket costs $1 per play.

laws.[10] The winner has between 90 days to one year to claim the prize depending on the state lottery. After that period, the prize becomes unclaimed.

To claim the jackpot, the player must go to the lottery headquarters in his or her state to verify that the ticket is actually a winning ticket. For jackpot winners, there is normally a 15-day waiting period before a prize can be paid.[11] This waiting period allows all participating states to balance their sales and prize amounts and arrange their funds to pay the prize. However, this waiting period depends on the individual state lotteries. For example, California requires a waiting period of six to eight weeks after the jackpot winner submits the claim. I gathered data on the various waiting periods from conversations with representatives of more than half of the U.S. state lotteries.[12] After submitting a valid claim, the lottery pays the winner. If the winner chooses the lump-sum payment, she receives the prize minus withheld taxes. The way lotteries pay winners varies from state to state. Based on conversations with lottery representatives, around more than half of state lotteries offer wire transfer to remit prize money to winners. In addition, some also offer to pay by check, which in most states, is mailed to the winner. The lotteries representatives' prior is that the winners deposit winnings in their respective cities and in their existing bank accounts.[13] Finally, according to lottery representatives, winners usually buy their

[10]If the winner has a debt owed to the state, the winner will receive the prize minus income tax withholding and the amount owed to the state.

[11]The date when the winner claims the prize can be extracted from the dates of the press releases.

[12]In some cases if the winner claims the prize after two weeks, he or she can receive the jackpot in his or her bank account the following day. I contacted all the state lotteries and in those states from which I did not receive an answer, I assumed a 15 day wait period depending on the date the winner claimed the prize and the date of the game (all which can be found in the press releases).

[13]For example, one state lottery claims the following in their Winner's Handbook relative to what

tickets close to where they live or, in some cases, where they work.[14] However, it is not necessary to be a state resident to play a state lottery. One can be only visiting the state and still play. Fortunately, the data about the winner's state of residence is usually available in the press releases from the lotteries.

All state lotteries except for five have laws that require them to release the winner's name, his or her city of residence, the name and location of the retailer who sold the winning ticket, the game, the drawing date, and the amount won, upon request.[15] Sometimes there are multiple winners in different states and in those cases the different winners share the prize equally. These features of the U.S. lotteries allow me to compile a data set from different sources (including hand-collected data) of all jackpot winners for the period from 2002 to 2013 for Mega Millions and for the period from 2003 to 2013 for Powerball.[16] The data set includes whether the prize was claimed, whether the winner chose the cash option or the annuity option, the date of the game, the date when the prize was claimed, the approximate date the winner received the prize, the name and city of residence, the zip code of the retailer, and federal and state tax rates, among other information.

Table 2.1 shows a summary of the statistics related to the jackpot winners

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to do with their winnings: "Your current bank or credit union is a good place to start".

[14]For instance, the Powerball website (http://www.powerball.com) states the following: "The vast majority of winning tickets are purchased by someone who is close to the lottery terminal where it was purchased".

[15]Delaware, Kansas, Maryland, and Ohio allow the winner to remain anonymous (i.e., not required to release the name). However, these states do reveal the name and location of the retailer who sold the winning ticket; game, date, and the prize amount.

[16]The reason for the starting date in the case of Mega Millions is that in May 2002 the current game name and format (game matrix and prize amounts) were introduced. In the case of Powerball, 2003 is the earliest year in which I could gather all the data for jackpot winners.

over the period from 2002 to 2013 for Mega Millions (MM) and 2003 to 2013 for Powerball (PB) (up to June 2013).[17] The 284 jackpot winners are almost evenly split between MM (139) and PB (145).[18] These winners are located across 41 states, from the 43 states that offered the games. PB has jackpot winners in 38 states, and MM has winners in 16. In addition, the winners are spread across 142 CBSAs.[19]

Figure 2.2 is a map of the U.S. with the shading of different counties reflecting the counties in which there was a jackpot winner over the period of the data set. Of the 284 jackpot winners, 263 (92.6%) choose the cash option, and the remaining 21 (7.4%) consists either of unclaimed jackpots or winners who chose the annuity option. Most of the winners (255, or 89.8%) bought the winning ticket in their state of residence. The mean jackpot prize in 2013 dollars, after tax withholdings, is $46.09 million. The mean prize is very similar between the two games: $46.51 million for MM and $45.73 million for PB (See Table B.14). Finally, the winners, in the full years in the sample (2003-2012), are also evenly distributed over this period, with 26.7 jackpot winners per year between both games (See Table B.14).

[17] The reason the data set compiled ends in June 2013 is because the Summary of Deposits from the Federal Deposit Insurance Corporation (FDIC) ends in June 2013.

[18] There were 286 jackpot winners over this period. However, for one winner, I do not have the amount received and in the other case, I do not have the location of the retailer. Thus, I am left with 284 jackpot winners.

[19] Core Based Statistical Areas (CBSAs) consist of the county or counties associated with at least one core urbanized area of at least 10,000 population. Metropolitan Statistical Areas are CBSAs associated with at least one urbanized area that has a population of at least 50,000. Micropolitan Statistical Areas are CBSAs associated with at least one urban cluster that has a population of at least 10,000 but less than 50,000.

2.2 Data Collection

The U.S. lotteries jackpot winners' data set was hand-collected. It is derived from different public sources and complemented with data from discussions with U.S. lotteries representatives.[20] To study the causal effect of jackpot winners as a shock in the supply of deposits, I first estimate the effects on the deposits at the CBSA level. The data come from the Summary of Deposits (SOD), which is the annual survey of branch office deposits for all FDIC-insured institutions. SOD provides the branch office deposits as of June 30 of every year.[21] To estimate the deposits at the CBSA level, I sum all the branch deposits in each CBSA-year. I use the data from 1999 to 2013.

The lending data come from the Community Reinvestment Act (CRA) disclosure and from aggregate reports from the Federal Financial Institutions Examination Council (FFIEC). The CRA requires that banks above a certain asset threshold report small business lending each year and by Census tract. The asset threshold was $1.186 billion in 2013 and is adjusted with CPI.[22] CRA disclosure reports provide data by bank, county, CBSA and year. And, the aggregate report offers total lending data. The CRA provides two types small lending data: i) the total dollar amount of small business loan origination, defined as loans under $1 million, and ii) the dollar

[20] I contacted the 43 U.S. state lotteries that offer both Powerball and Mega Millions, and other industry representatives (e.g., North American Association of State & Provincial Lotteries (NASPL)). I received answers from 23 state lotteries.

[21] The setting allows estimating the date in which the winner received (and deposited) his or her prize. Unfortunately, the SOD data is only available at the year level. However, the Call Report, financial data set at the bank level, is available at the quarterly frequency.

[22] Previous to 2005, the asset threshold was $250 million.

amount of small business loan origination to businesses with $1 million or less in annual gross revenue. I use the data from 1999 to 2012.

To complement the CRA data, I use the Report of Condition and Income, Call Report. There are two advantages of this data set: i) It includes data on all banks regulated by the Federal Reserve System, the Federal Deposit Insurance Corporation, and the Comptroller of the Currency. ii) The data are available at the quarterly frequency. However, the Call Report is only available nationally at the bank level. I use the data from 1999 to 2013. More details about this data set are available in the Appendix. Finally, in some estimating equations, I also include CBSA characteristic controls derived from Census data.

2.3 Conceptual Framework

In a setting with informational asymmetries and agency problems, bank external financing is costly. Banks are unable to raise unlimited amounts of financing at the market rate, because issuing debt either could be a bad signal of the quality of banks' assets or it could increase the incentives of self-interested managers to engage in opportunistic behavior (Stiglitz and Weiss, 1981; Myers and Majluf, 1984; Jensen, 1986).

For example, Stein (1998) shows the consequence of banks' adverse selection problems on lending. In his setting, banks raise funds from individuals and then lend these funds to borrowers. Depending on the type of liability issued by the bank, this may create an adverse-selection problem (since investors are not well informed about the bank's value) that constrains the bank's ability to make positive net-present-value loans. In particular, no adverse-selection problem would arise if the bank could fund all its needs with insured deposits. However, when the availability of insured deposits is constrained, the bank must rely on other sources of financing (e.g., commercial paper), in which case adverse selection plays a role since investors are exposed to default risk and lending behavior can be distorted. Thus, shocks that compromise the ability to raise deposits can lead to declines in lending, and they have subsequent effects on the investment of bank-dependent firms (see also Bernanke and Blinder, 1988).[23]

This is the central idea of the bank lending channel of monetary transmission, in which central bank open-market operations have independent consequences for the credit supply. Thus, when the central bank withdraws reserves from the banking system, this compromises banks' ability to raise money with reservable sources of financing, such as insured deposits.

[23]Paravisini (2005) considers an adaptation of Froot et al. (1993), Kaplan and Zingales (1997) and Stein (2003) reduced form two-period models to the case of financial firms. The intuition is that in a frictionless world, profit-maximizing banks can raise funds in the capital market at a constant cost, r_m, and lend until the marginal return on loans is equal to the marginal cost of finance. If marginal loan profitability is decreasing, lending beyond this point yields a return lower than r_m. Thus, for each extra dollar of subsidized financing (i.e., insured deposits) at a rate $r < r_m$, banks may use it to repurchase a dollar of debt and earn $r_m - r_s$, or issue an extra dollar in new loans, which would yield a return below $r_m - r_s$. Thus, in a frictionless world, an extra dollar of insured deposits will increase the inframarginal profits of the bank, but will not affect lending as long as banks hold some financing at the market rate. However, bank external financing is costly in a setting with informational asymmetries and agency problems. Therefore, in this scenario, the marginal cost of external financing is increasing in the amount of externally raised finance. Banks will lend until the marginal cost of finance is equal to the marginal return on loans, but now each extra dollar of insured deposits will shift out the marginal cost of external finance. Thus, an increase in available subsidized finance leads to an expansion in lending.

2.4 Research Design and Results

2.4.1 Local-Level Analysis: Deposits

This paper's research design is based on the observation that having a jackpot winner in a specific local area and time is a completely random shock conditional on the sales of lottery tickets. From the previous section, and based on state lotteries representatives' prior, I test the hypothesis that, on average, lottery winners deposit their prizes in their respective CBSA. This hypothesis is tested directly in the first stage of estimation using the SOD data.[24] Specially, I estimate the following specification:

$$log(deposits)_{it} = \alpha_i + \alpha_t + \beta\, 1(winner)_{it} + \gamma' X_{it-1} + \varepsilon_{it} \tag{2.1}$$

where i indexes CBSA, t indexes the year, and *deposit* denotes deposits. The expression $1(winner)$ is an indicator function equal to 1 in those CBSA-years with jackpot winners who choose the cash option and reside in the state where the winning ticket was sold, and 0 otherwise. The variables α_i and α_t are CBSA and year-fixed effects, and X is a vector of the CBSA's demographic characteristics.[25] Since each lottery ticket has the same chance of winning, the probability of selling a winning ticket is a linear function of lottery sales. However, I do not have data on lottery sales at the

[24] The local-level results shown in the paper are at the CBSA level, but the results are qualitatively similar at the county level.

[25] I include the following (lag) CBSA controls: race composition (% white), sex composition (% male), age composition (% over age 45), and income per capita. In addition, since some CBSA are in multiples states, I also include state fixed effects (FE). However, the results are almost identical without state FE.

CBSA level. To proxy for CBSA's sales, I use the lag of the CBSA characteristics.[26] Thus, conditional on the proxies for sales, each CBSA has the same chance of selling a winning lottery number, $E[\varepsilon_{it}/1(winner)_{it} = 1\,, X_{it-1}] = E[\varepsilon_{it}/1(winner)_{it} = 0\,, X_{it-1}]$, and the parameters of (1) are unbiased and consistently estimated. The parameter of interest is β, and the variation used for identification is the average change in deposits for CBSAs with jackpot winner at year t with respect to the average change in deposits for CBSAs without winners at time t. The control groups are CBSAs from the 43 states with state lotteries (i.e., the possible treated group). Thus, this is a difference-in-difference estimator. In the different specifications in the paper, the point estimates reflect the shock's effect only during the year that it occurred. Finally, I report the standard errors clustered at the CBSA level to account for serial correlation, and this is robust to heteroskedasticity.

An interesting feature of the setting is that the prize amount is randomly assigned, conditional on lottery sales.[27] This allows one to test whether there is a positive causal effect on the possible amount received and the outcome variable. Thus, I create different variables to estimate the intensity of treatment (i.e., the prize won effect).[28] I use $log(1 + prize)$ as the treatment variable, where prize is the amount won after withheld federal and state taxes in 1999 dollars. Also, I create the following variable: $prize_{it}/deposits_{ipre}$, where $prize_{it}$ is the amount won in CBSA i

[26]The point estimates after just controlling for the lag of population do not vary when other (lag) demographics controls are introduced.

[27]In those cases in which there are multiple winners in the same CBSA/year, I add up the prizes received.

[28]Similar to the winner dummy, all the treatment variables are not zero only in those cases in which the winner chooses the cash options and resides in the state in which the winning ticket was sold.

and year t and 0 otherwise, and $deposits_{ipre}$ are the deposits in CBSA i and the year before treatment.

The quasi-experimental setting allows for different falsification tests. I create an indicator variable (Non-cash Winner), which is equal to 1 if there is a jackpot winner but the prize was unclaimed or the winner choose the annuity option, and equal to 0 otherwise. Also, I generate a dummy variable equal that is equal to 1 (Winner out-of-state) if the winner lives in a state other than where the winning ticket was sold, and equal to 0 otherwise. Finally, I create an indicator variable that combines the variables non-cash winners and out-of-state winners (non-cash and winner out-of-state).

Results

The bottom panel of Table 2.1 shows the summary statistics on CBSA characteristics and deposit growth depending whether the CBSA had a jackpot winner.The third column reports the p-values on the t-test for difference. Not surprisingly, CBSAs that had winners have higher population (p-value is 0.00). There are also significant differences in other demographic characteristics. Consequently, the regression specifications include this set of CBSA characteristics. Most importantly, there is no statistical difference in deposit growth between both groups (p-value is 0.952) from 1994 to 2001, which supports the fact that the jackpot winner's shock is randomly assigned.

Table 2.2, Panel A, reports the parameters of interest from the regression specification (1). Also, recall that (lag) CBSAs demographic controls are included, and the control group consists of CBSAs from the 43 states that have state lotteries. Column (1) shows the point estimates that imply that those CBSAs with jackpot winners experience an increase, on average, of 3.13% in deposits in the year of the shock.[29] Columns (2) and (3) report the estimates for the different treatment variables. For example, Column (2) reports the average increase in deposits is 4.05% from the winners' shock.[30] To see to what extent the reduced form estimates are sensitive to the inclusion of time-varying observable factors, Table B.15 reports the point estimates after controlling only for (lag) CBSA population. The estimates are very similar to Table 2.2, in which all (lag) demographic controls are introduced.

Robustness Check

To test the identification condition, Table 2.2 Panel C, Column 1 reports the point estimate of the indicator variable that combines the variables non-cash winners and out-of-state winners (non-cash and winner out-of-state). There is no significant change in deposit (0.80%) in those cases in which the prizes were unclaimed, or the winner was from a different state, or the winners chose the annuity option. Table B.16, Columns (1)-(2), shows the separate estimates of Non-cash Winner and Winner out-of-state dummy variables. Column 1 shows that in those cases in which the prize was unclaimed or the winner choose the annuity option (Non-cash Winner), there is no significant change in deposits (-0.62%). Column (2) reports a similar estimate of change in deposits (-0.88%) for those cases in which the winner's state of residence

[29] In untabulated results, the point estimates weighted by the CBSA's 2001 log(population) are similar.

[30] $exp\,[0.00382 * ln(1 + 32,964.8)] - 1$. From Table 2.1, we have data that the average jackpot winner received $ 32.9 million.

is different from where the winning ticket was sold (Winner out-of-state). These findings support the identification condition that, in the absence of the winner's shock, the average changes in deposits for the treatment group do not differ with the control group.

To further examine the identification condition, Column (2) in Table 2.2, Panel C, presents the specification (1) augmented with leads. I add dummy variables for one, two, and three years before the shock, and the winner indicator (year 0). The coefficients on the winner leads are all insignificant at conventional test levels; and in the year of the shock, deposits increase an average of 3.27%. Thus, there is no evidence of pre-existing trends in the data.

In Table B.17, Columns (1) and (2) report the point estimates of the different treatment variables when I split the sample into two subsamples based on the (lag) population size: equal or below 500,000 and above 500,000. As expected, for all the treatment variables, the winners' shock does not have a significant effect on deposits for CBSAs with a population above 500,000.[31] For example, Column (1) in Table B.18 reports that for those CBSAs with a population higher than 500,000, the winners' shock does not affect the deposits (-0.0066).

Finally, Table 2.2, Panel C, Columns (3) and (4) report the results using different control groups as a robustness check. In Column (3), I restrict the sample to states with jackpot winners, and the sample consists of states with at least one jackpot winner. The point estimate for the average change in deposits (3.29%) is similar

[31] The results are similar for different population cut-offs (e.g., above or below 200,000).

to Column (1) in Panel A. Finally, Column (4) reports the estimates, excluding from the sample those states with more than eight jackpot winners, showing the average change in deposits is 3.92% in the year when there is a jackpot winner. Thus, the results are not driven by the CBSAs in those states with relatively more winners.[32]

Overall, these estimates provide evidence of a positive causal effect of the jackpot winner's shock on deposits, and support the hypothesis that lottery winners, on average, deposit their winnings in their respective CBSAs.

2.4.2 Local-Level Analysis: Small Business Lending

Having established a strong relationship between the jackpot winner's shock and deposits, I turn to examining the effect of these shocks on small business loan origination. To examine the effects of jackpot winners on bank lending at the CBSA level, I use CRA Aggregate data to estimate the same specification as (1), but the outcome variable is the log of small business loan origination in CBSA i and year t.[33] The identifying assumption in this case is that the treatment variable (the jackpot winner) is a supply shock, rather than a reflection of demand conditions, in the areas where the banks have operations (i.e., the liquidity shocks do not affect credit demand in the area). This assumption is plausible, at least over short periods, since I estimate the winner's effect at the CBSA level in the calendar year of the shock.[34][35]

[32] Those states with more than 8 jackpot winners for the period from 2002 to 2013 are CA, GA, IN, MI, NJ, NY, OH and PA.

[33] Unfortunately, the SOD data set does not have lending data.

[34] Unless, the jackpot winner spends a substantial part of their wealth in the same year to have an effect on the local demand conditions of the entire CBSA.

[35] Anecdotal evidence shows that Andrew Jackson "Jack" Whittaker, Jr, who won the Powerball Jackpot in 2002, is the most renowned case of financial troubles for a jackpot winner. After winning

Recall that the estimates in this paper report the shock's effect only on the year during which it occurred. The tests in the robustness check section will examine the identifying assumption.

Since the interest of this paper is in the bank lending channel, the origination of small business loans is of great importance, especially since these loans are harder to securitize. In addition, the relationship banking literature acknowledges that banks have an important role in mitigating frictions (i.e., asymmetric information) especially for small firms. Thus, small firms are relatively more bank-dependent businesses. For example, previous papers have found evidence that banks obtain soft information about firms to help their credit decisions (e.g., Agarwal and Hauswald, 2010).

Results

Table 2.2, Panel A, Columns (4)-(10) report the estimates of equation (1) in which the outcome variables are either small business loan originations with $1 million or less in revenue or total small business loan originations (loans under $1 million). Columns (1) to (3) show the point estimates for the different treatment variables. All the estimates confirm the positive casual effect of the jackpot winner's shock on small business loan origination. Column (4) in Table 2.2 Panel A reports a significant economic increase (4.31%) in loans to businesses with revenues lower than $1 million for those treated CBSA, a similar result (3.71%) is shown in Column

the lottery, he had several personal tragedies and legal issues throughout the years. However, even in this extreme case, there is no evidence the winner spent most of his fortune the same year of winning.

(8) for total small business loan originations. Column (9) reports that the average increase in small business lending is 4.28% from the treatment shock.

We should expect the jackpot winner shock to have a greater influence on small business lending in those CBSAs in which smaller banks are the dominant players. The reason is that smaller banks are relatively more constrained since their marginal source of funds is deposits. To obtain a proxy variable for which CBSA small banks prevailed, I use the SOD data to construct a measure of the ratio of the number of branches in each CBSA-year for which the banks have assets lower than or equal to $2 billion (in 1999 dollars) relative to the total of all branches in each CBSA. Then, I create quintiles from the yearly distribution of the ratio of branches. Finally, I create an indicator variable (small bank) that takes the value of 1 if the CBSA is in the 5th quintile of the yearly distribution, and takes 0 otherwise. The variable of interest in this case is the interaction between the winner indicator and the small bank dummy. Table 2.2, Panel B, Columns (1) and (3) show the estimates of the interaction, CBSA with a winner shock and in which the small banks predominated, are economically and statically . For example, the loans to businesses with revenues lower than $1 million increase by 13.1% (Table 2.2 Panel C, Column 1) in those CBSA with a jackpot winner and with small banks as the dominant players. Table B.18 shows that the results are robust to using a different threshold for assets ($1 billion). To exclude the possibility of pre-trends, Table B.18 reports the point estimates of the interaction variables three years before the shock for both asset thresholds. These point estimates are all negative and insignificant.

Robustness Check

To examine the identification condition, Table 2.2, Panel C, Columns (5) and (9); and Table B.19, Columns (3)-(6) report the different placebo tests that the quasi-experimental setting provides. All the estimates of the Non-cash Winner, Winner out-of-state, and Non-cash and winner out-of-state variables are insignificant. These findings support the identification condition that, in the absence of treatment, the average changes in small business lending do not differ between the treatment and the control group.

Table 2.2, Panel C, Columns (6) and (10) report the estimates of lead variables for 1-3 years before the shock and the winner indicator (year 0). In both cases, the coefficients of the lead, or pretreatment, variables are insignificant (which suggests that there are no pre-existing trends in the data); while the estimates of the winner variable (year 0) are large, in economic terms, and highly significant. Columns (3) and (4) of Table 2.2, Panel C, show that the findings are economically significant for different control groups (treated states and those states with fewer than eight winners). Finally, Table B.20, Columns (3)-(6) report the estimates of the different treatment when I split the sample according to the size of the population. Similar to the case of deposits, the results are as expected (i.e., no significant changes in those CBSAs with population above 500,000).

To test the identifying assumption that the winner's shock is a supply shock, since states collect taxes from the winners, the results could be driven by an increase in demand due to an increase in public state spending. Fortunately, not all states have individual income taxes. Thus, to test for the identifying assumption, I generate a dummy variable equal to 1 for those states with individual state-level income taxes.

Under the identifying assumption, there should be no difference in the causal effect of the winners shock on those states that collect taxes relative to those state that do not. The variable of interest in this test is the interaction between the winner indicator and the state taxes dummy. Table 2.2, Panel B, Columns (2) and (4) show that the interaction term in both cases is negative and is not significant. These results rule out the hypothesis that the increase in loan origination is due to an increase in demand from state spending coming the taxes collected on winners.

To further examine the identifying assumption, I conduct the following investigation. Since I have data on the address of the retailer that sold the winning ticket, I can assume (as mentioned from my conversations with lottery representatives) that the winners reside, on average, in the retailer's county. Given that some CBSAs are compose of several counties. Thus, I can include CBSA-year fixed effects to control for any observable and unobservable time-varying effect at the CBSA level (such as local demand conditions). The set of fixed effects implies that the regressions are identified through variations between the treatment group (counties with a lottery winner) and the control group (counties without winners) within a given CBSA in a given year. Under the identifying assumption, there should be a significant difference in deposits between the counties with a jackpot winner, relative to counties with no winner, within a given CBSA in a given year; but there should not be a difference between small business lending growth between with counties (i.e., in the credit supply hypothesis, we should expect an increase in the entire CBSA). The hypothesis is that the local bank that receives the winner's shock increases lending across the entire CBSA and not just near the winner's location (i.e., the winner's county). The specification is

$$log(small\,lending)_{ijt} = \alpha_{jt} + \beta\, prize_{it}/deposits_{ipre} + \gamma' X_{it-1} + \varepsilon_{ijt}$$

where the outcome variable is small business lending in county i in CBSA j in year t, α_{jt} is a vector of CBSA-by-year fixed effects, X is a vector of county demographic characteristics similar to (1), and $prize_{it}/deposits_{ipre}$; where $prize_{it}$ is prize received by the winner in county i in year t and 0 otherwise, and $deposits_{ipre}$ are the deposits in county i in the year prior to treatment. The standard errors are clustered at the county level to account for serial correlation.

Table B.21 reports the point estimates.[36] Column (1) shows that, on average, those counties with winners increase their deposits by 2.12% (after a one standard deviation increase), relative to those counties without winners, within a given CBSA in a given year. Column (2) shows the coefficients of the pre-treatment variables (three years before the shock). The estimate is negative and insignificant, which rejects the hypothesis of pre-trends in the data. Most importantly, Columns (3) and (5) show that, on average, within a given CBSA in a given year, the small-business growth for those counties with winners is not significantly different with respect to counties with winners. These findings support the identifying assumption that the winner's shock is a supply shock.

As a final test on the identifying assumption, local demand may increase from the winner's shock because the firm's net worth (i.e., firm's collateral) may

[36]I find similar results for the different treatment variables.

be increasing due to possibly changing housing prices. To rule out this hypothesis, I interact the different treatment variables with an indicator of whether the MSA is above the median relative to the housing elasticity measure established by Saiz (2010).[37] Under the identifying assumption, there should be no difference in the effect of the winner's shock on those MSAs above the median and below the median elasticity. Table B.22 reports the estimates of the interaction of the different treatment variables and the indicators of whether the MSA is above the median of the Saiz (2010) housing elasticity measure. The point estimates of the interaction are all insignificant at conventional test levels, which supports the identifying assumption.

In general, the results in Table 2.2 show a positive causal effect of the jackpot winner shock on small-business lending. Additionally, these findings also indicate that the local supply of deposits matter for small business loan origination in the U.S., even after the developments in the financial sector such as state banking deregulation (Kroszner and Strahan, 1999; Johnson and Rice, 2008), increase in securitization (Loutskina and Strahan, 2009), and the development of impersonal means of information transmission (Petersen and Rajan, 2002).

Local-Level *Elasticity of Total Small Business Lending*

Assumptions about the appropriate general equilibrium are required to further show the overall effect of the jackpot winner shocks on local lending. With this caveat

[37] Saiz (2010) housing supply elasticity measure includes a geographic and regulatory component that is meant to capture the relative ease with which the housing stock in an area can adjust to a positive shift in the demand for housing. Areas where is it relatively easy to build tend to see more construction, and smaller house price increases, when demand for housing increases, whereas low elasticity areas tend to see higher prices and lower levels of new construction. Finally, the Saiz (2010) measure only includes data about the MSA.

in mind, I estimate a 2-stage least squares model (2SLS) in which total small business loan origination is the dependent variable, and the independent variable is the sum of the contemporaneous deposits at the CBSA level.[38] The empirical specification is (1), which includes the CBSA and year fixed-effects and the set of (lag) CBSA characteristic controls. Finally, the instrument is $log(1 + prize)$. The models are estimated on data from 1999 through 2012, since this is the last period available from the CRA aggregate.

Table 2.3 reports the estimates. Column (1) shows that the first-stage F-statistic is 13.41, which is above the conventional threshold of 10 for weak instruments (e.g., Stock et al. (2012)). The argument for the exclusion restriction is that an increase in small business lending through the winner's shock only occurs when there is growth in deposits as shown in Columns (1), (5), and (9) of Table 2.2 C (and Table B.17 Columns (1)-(6)). Thus, the instrument ($log(1 + prize)$) only affects small business lending through its effect on deposits. Table 2.3, Columns (2) and (3) report the OLS and reduced form regressions. Since this is a single-instrument estimation, 2SLS equals indirect least squares (i.e., the ratio of reduced form to first-stage coefficients on the instrument). Therefore, as Column (4) shows, the 2SLS estimate is 0.49 (=0.0036/0.0074), which is the elasticity of total small-business lending with respect to deposits. The magnitude of the elasticity of total small-business lending with respect to deposits is underestimated by a factor of 1.6, which is not properly accounted for using the 2SLS model. Finally, the average jackpot

[38]The deposits at the CBSA level are only for those banks that meet the CRA asset size threshold to be subject to data reporting requirements.

winner received $32.9 million (in 1999 dollars) and, from the estimates shown in Columns (1) and (4) of Table 2.3, the average total small-business lending increases by 3.78%.[39] This estimate is large in economic terms and is highly significant.

2.4.3 Bank-Level Analysis

Since the SOD provides deposit data for each branch, and I estimate the amount received by each jackpot winner; then, on principle, I can identify the possible branch, and thus the bank, that received the prize. The assumptions of the individual detection algorithm are as follows: The winner deposits in her respective CBSA, and in those branches that are closest in driving distance to where she bought the ticket. The deposit findings in Table 2.2 support the first assumption, and the second assumption is plausible based on discussions with state lottery representatives. More details about the algorithm are available in the Appendix.

The identifying assumption for the bank-level analysis is that the winner's shock is exogenous to the bank, conditional on bank characteristics. The identifying assumption is supported by the fact that it is exogenous that the bank has a branch in the winner's CBSA and potentially a prior relationship with the winner, as banks do not open branches in an attempt to systematically predict lottery jackpot winners. In addition, the winner is an outsider and plausibly has as much information about the bank as the econometrician.[40]

[39] $[exp\,[0.0074 * ln(1 + 32,964.8)] - 1] * 0.49$.

[40] The point estimates using the identified banks that received the winners' shock, from the individual detection algorithm, will have attenuation bias. Thus, in this case the estimates will underestimate the effect of treatment, making it more difficult to find results and can be considered a lower bound of the true causal effect.

In the bank-level analysis, using data from the Call Report, the empirical specification is

$$log(outcome)_{it} = \alpha_i + \alpha_t + \beta\, prize_{it}/deposits_{ipre} + \gamma' X_{it-1} + \varepsilon_{it} \qquad (2.2)$$

where the dependent variables are deposit (or loans) for bank i at quarter t. The preferred specification for the treatment variable to account for the intensity of treatment, given the heterogeneity in size in the banking industry, is $prize_{it}/deposits_{ipre}$; where $prize_{it}$ is prize received by the bank i in quarter t and 0 otherwise, and $deposits_{ipre}$ are the deposits in bank i at the quarter prior to treatment. Finally, α_i and α_t are bank and quarter fixed effects, and X is a vector of bank characteristics in quarter $t-1$. An interesting feature of the Call Report is that the data is quarterly, and thus it allows one to use the data on the estimated date in which the winner received (and deposited) her prize. Specification (2) is also a difference-in-difference estimator. I report the standard errors clustered at the bank level to account for serial correlation and robust to heteroskedasticity.

The CRA disclosure data allow for studying the small-business lending of banks in the treatment group to that of other banks in the control group and in the different CBSAs in which they originated loans. Thus, the empirical specification is

$$log(small\,lending)_{ijt} = \alpha_{jt} + \beta\, prize_{it}/deposits_{ipre} + \gamma' X_{it-1} + \rho\, \psi_{ijt-1} + \varepsilon_{ijt} \quad (2.3)$$

where the outcome variable is small business lending for bank i in CBSA j in year t, α_{jt} is a vector of CBSA-by-year fixed effects, X is a vector of bank characteristics similar to (2), and ψ is the number of branches for bank i in CBSA j and year $t-1$. $prize_{it}/deposits_{ipre}$; where $prize_{it}$ is prize received by the bank i in year t and 0 otherwise, and $deposits_{ipre}$ are the deposits in bank i in the year prior to treatment.[41] I control for the number of branches owned by each bank in every CBSA, since the higher the number of branches the bank has, the higher the probability of being treated (i.e., the higher the probability of receiving the deposit from the jackpot winner). Thus, conditional on banks' characteristics and the number of branches, treatment is exogenous. The standard errors are clustered at the bank level to account for serial correlation. Finally, the battery of fixed effects implies that the regressions are identified through variation between treatment group and control group banks within a given CBSA in a given year. The fixed effects control for any observable and unobservable time-varying effect at the CBSA level, including demand-side effects that affect the lending behavior of all banks within a given CBSA-year.

Results

Table 2.4 reports the results of the individual detection algorithm. From 2002 through June 30, 2013, there were 191 non-group winners who chose the cash option and resided in the state where the winning ticket was sold. And among these, the algorithm matched 134 winners.[42] Of the winners matched, 71 were single matched.

[41] The banks in the treatment group are the ones identified in the detection algorithm.

[42] The reason to focus on non-group prizes is that in some instances the prize is not divided equally between the members of the group (e.g., pool of workers that buy tickets together), and in some cases the data about how the groups share their price is not available in the press releases. Therefore, to reduce the possibility of mistakes, I focus on non-group winners.

The mean driving distance in minutes from the retail location that sold the winning ticket to the branch is 16.23 minutes. In addition, Table 2.4 shows that the branches in the treatment group had lower deposit growth from 1994 to 2001 (p-value is 0.00). Finally, Table 2.4 reports the bank-level attributes regarding whether the bank was treated or not. There are differences in size, profitability, and equity/assets ratio between both groups. Consequently, I include these characteristics as controls in (2) and (3).

Deposits and Total Lending

Similar to the local-level section, the analysis starts with the estimates of the winners' shock effect on deposits. Panel A of Table 2.5 reports the parameters of interest from (2). Column (1) reports that a one standard deviation increase in the the intensity of treatment variable (*prize/deposits*) leads to an increase of 1.30% in the deposits (=0.0814*0.1598) in the quarter of the shock (0 m, 3 m). Column (2) reports the effect of treatment in the year of treatment (0 m, 12 m). There was a 1.98% average quarterly change after an one standard deviation increase.

Column (3) reports the result with the lead variable of *prize/deposits* that captures the effect on deposits before the winners' shock (-12m, 0m), and Table 2.5 Panel C reports the estimate of (2), augmented with lead variables. In both cases, the results are insignificant. These findings suggest that there are no pre-existing trends in the data.

I perform the following additional check: Since there is heterogeneity in the treatment group in terms of asset sizes, and I do not expect the winners' shocks to have any significant effect on the large banks. Panel B shows the point estimates if I split the sample into two subsamples (based on the (lag) bank's asset size), those in the bottom 99% of the asset size distribution and those in the top 1% of the distribution.[43] The results are shown in Panel B of Table 2.4, Columns (2) and (3). As expected, the effect of the shock is concentrated among those banks in the bottom 99% of the asset distribution. In addition, the estimates of (2), augmented with lead variables, show no evidence of pre-existing trends. Overall, the estimates provide evidence that the jackpot winners' shocks have an economic and significant increase on deposits in the treatment group. In addition, the results show that the largest set of banks is not affected, as expected, by winners' shocks.

Additionally, the results in Table 2.5 (e.g., Columns (1)-(3) of Panel B) show that the individual detection algorithm matched those banks that received the jackpot winning reasonably well, since the point estimates reflect the effect on the exact quarter in which I estimated the winner claimed and deposited her prize, and because there are no pre-treatment quarterly trends in the results (recall that the SOD data used to match the treated banks is annual data).

Panel A of Table 2.5, Columns (4)-(6) report the estimate of (2), in which the outcome variable is total loans. Since the winners' shock effects can range within the year of treatment, I estimate the effect for the year of treatment. Columns (4) and (5) report the estimate in the quarter of the shock (0m, 3m) and in the year

[43] In untabulated results, the findings are similar for other size distribution cut-offs (90% and 95% of the asset distribution).

of treatment (0m, 12m). In the first case, lending increases, on average, by 2.46% in the same quarter of the shock (=0.154*0.1598) and a 2.38% average quarterly increase (in the year of the shock) after a one standard deviation increase. Column (6) and Table 2.5 Panel C, Column (4) show that the pre-treatment coefficients are insignificant, rejecting the hypothesis of pre-existing trends.

Panel B of Table 2.5 reports the estimates for two subsamples based on the (lag) asset size. Column (6) shows that there is no effect for the larger banks in the treatment group. Columns (4) and (5) report the point estimates for each quarter after treatment or treatment of lag variables ((0m, 3m), (3m, 6m), (6m, 9m), and (9m, 12m)) and lead variables. Interestingly, Columns (4) and (5) show that there is a credit supply effect only in the first three quarters of treatment. And, in the fourth quarter, the shock's effect disappears. Columns (4)-(6) exclude the hypothesis of pre-trends in the data. Finally, Table B.23 shows that the winner's shock does not have an effect on bank's securities investment.

Table B.24 reports the point estimates for which the dependent variable is total small business loans. Column (1) shows the coefficient of the treatment variable $prize/deposits$ in the year of treatment (0m, 12m). Relative to the estimates for total loans, the average effect is larger for total small business loans (a 3.87% quarterly change, on average, after a one standard deviation increase). A possible explanation for this result is that these types of loans are harder to securitize. Thus, if the bank has marginal lending opportunities that are profitable, the shock induces a higher increase in this loan category. Finally, Column (2) shows that the results are not driven by pre-trends in the data.

In summary, the results in this section show a positive causal effect of the jackpot winners' shock on deposits and lending at the bank level. In addition, as expected, there is no treatment effect on the larger banks, and the shock's effect on lending for the set of non-large banks is not persistent. Also, as expected, the estimated effect is larger on small-business lending.

It is also of interest to study the effect of potential deposit shocks, others than those from lottery winners. To this end, I modify the matching algorithm used to detect lottery winners, and I perform the following procedure: First, I implement the analysis by focusing on CBSAs / year without jackpot winners to avoid identifying deposit shocks from lottery winners. Second, I exploit the branch deposit data from the SOD, which provides data for each branch from 1994, to estimate the fitted value in deposits for each branch in year t. Third, I estimate the difference between the realized deposits at year t and the predicted deposits for each branch to estimate the potential deposit shock that the branch received. Fourth, I identify branches that experienced a change in deposits (from the third step) at year t greater than \$ 10 millions (to potentially detect economic significant shocks). Fifth, for those matched branches from the last step, I focus on those for which the growth in deposits in year t was the maximum growth they experienced since 1994. The idea is to focus on those branches that experienced significant changes in their deposits to detect possible shocks. Finally, those banks identified by the procedure are matched with the Call Report to estimate the effect at the bank level (since the SOD does not provide lending data).

Table B.24 Panel A, reports the estimates. Column (1) reports that a one standard deviation increase in the intensity of the treatment variable (deposit change / total deposit) leads to an increase of 6.4% in the deposits in the year of treatment (0y, 1y). Similarly, Column (3) reports that a one standard deviation increase in treatment leads to an increase of 4.02% in total lending. To test whether there are pre-existing trends in the data, Columns (2) and (4) report the results, including the lead variable that captures the effect on deposits before the potential shock (-1y, 0y). In both cases, the results are significant. These findings do not support the hypothesis that there are no pre-existing trends in the data.

One potential problem with the previous analysis is that the estimated effects could be due to local demand, instead of a credit supply shock. To potentially isolate a supply shock, I focus on identifying and estimating the effect in which only a few branches experienced a significant increase in deposits within a CBSA / year. To identify these branches, I only include in the treatment group cases in which there are at most three branches in a CBSA / year that underwent a deposit shock (I find similar estimates when focusing on cases in which only one branch experienced a shock).

Table B.24 Panel B, shows the findings. In this case, a one standard deviation increase in the intensity of the treatment variable leads to an increase in deposits and total lending in 4.89% and 3.54% in the year of treatment, respectively. Columns (2) and (4) show that the pretreatment coefficients are significant, which does not support the hypothesis of the lack of pre-existing trends in the treatment group relative to the control group. In particular, the results of Column (4) can also be consistent with a demand hypothesis: For example, banks in the treatment group face significant demand for loans in year $t-1$ and thus issue more loans in year t, which translates to an increase in deposits.

Loan Origination and Local Demand Conditions: Intensive and Extensive Margin

In order to further test that the findings are not driven by demand-side effects, I estimate the effect of the winners' shock on small-business lending at the bank level in the different CBSAs in which the banks originated loans. This allows one to control for any unobservable time-varying effect at the CBSA level.[44] Table 2.6 reports the parameters of interest from the regression specification (3). Columns (1) and (3) show the coefficients of $prize/deposits$ to account for the intensity of treatment. A one standard deviation change increases small-business loan originations to businesses with revenues less than $1 million by 5.89% (=.00946*6.223) in the year of the shock, on average; and total small-business loan originations by 5.02% (=.00946*5.304). Columns (2) and (4) report the coefficients of the pre-treatment variables; in both cases the results are insignificant which rejects the hypothesis of pre-trends in the data.

To study the extensive margin, I estimate a linear probability model (LPM) as (3) in which the outcome variable is a loan indicator equal to 1 if the loan was granted and 0 otherwise. I estimate the specifications using OLS despite the binary type of the dependent variable, since using a nonlinear model (e.g., probit) would

[44] The estimates are qualitatively similar at the county level.

lead to an incidental parameters problem because the large number of fixed effects in (3). The control variables and the set of fixed effects are the same as before. I also include borrower control characteristics. Table 2.6, Column (5) reports the estimate. The winners' shock does not affect the probability of lending. Since the winner's shock induces an increase in lending to small businesses but not an increase in the probability of lending, one interpretation of the findings is that, on average, the increase in lending is to the existing borrowers and not to the new borrowers.

Thus, these results confirm that (a) the jackpot winners' shocks have a large and significant economic effect on lending in the treated banks across the different CBSAs in which they have locations, and (b) these results are not driven by demand conditions in the local markets.[45]

Finally, it is of interest to estimate the elasticity of total small-business lending with respect to deposits at the bank level. I estimate a 2SLS model in which the outcome variable is total small business lending for bank i in CBSA j in year t, and the regressor of interest is total deposits for bank i in CBSA j in year t (from the SOD). The problem for inference in this case is that OLS estimates may be biased if deposits are correlated with the unobservable determinants of small-business lending. For example, variation in deposits is potentially correlated with demand for credit (e.g., Jayaratne andMorgan 2000; Paravisini 2008). I identify the causal impact of deposits on small-business lending by using the intensity of the treatment variable

[45] In untabulated results, the estimates of the treatment variable ($prize/deposits$) when I split the sample to the CBSAs with a jackpot winner, and those without one, are economic and highly significant in both cases. In addition, there is no evidence of pre-trends in the data in both sub-samples.

(*prize/deposits*) as instrument.

In order to interpret the 2SLS estimate as the causal impact of deposits on small-business lending, the necessary condition is that the winners' shocks only impact lending through the deposits. Thus, the exclusion restriction could be violated if the winners' shocks impact small-business lending through channels (e.g., local demand) other than deposits.[46] However, I argue that this exclusion assumption is reasonable in this setting. Recall that the fixed effects (i.e., CBSA-by-year) in this case control for any unobservable time-varying effect at the local level, including demand-side effects. Table 2.7 Panel A reports the first-stage, reduced-form OLS and 2SLS estimates. Column (1) shows that the first stage F-statistic is 10.6. Column (4) reports the 2SLS point estimate of 0.934 (=5.036/5.394) for the sensitivity of small-business lending deposits. In addition, including bank fixed effects produces similar estimates. Table B.25 reports the results. In this case, the elasticity of total small-business lending with respect to deposits is 0.872.

There is an caveat to the IV analysis: Following Imbens and Angrist (1994), I interpret the 2SLS estimate as the local average treatment effect (LATE) of deposits on small-business lending for the subpopulation of compliers whose deposits are altered due to the winner's shock. However, it is possible that the effect of deposits is different for banks that are not the marginal recipient. To explore the external validity of the estimates, I study the characteristics of the complier group. I estimate the first stage, using Call report data, for different bank groups according to their

[46] In addition to the testable assumption that winner's shock is associated with bank's deposits.

attributes. Table B.25 Panel B reports the estimates. Column (1) reports the distribution of the population of commercial banks by the regulatory capital measure tier 1 risk-based capital ratio and balance sheet liquidity. Column (2) reports the distribution of compliers by tier1-liquidity groups, calculated as the ratio of the first stage for that subgroup to the overall first stage, multiplied by the proportion of the population in the group (i.e., the proportion of the treated who are compliers) (Angrist and Pischke, 2008). There are almost no compliers in the bottom of the tier 1 distribution. What is interesting is that, for the banks not in the bottom of the tier 1 distribution, the compliers are evenly split between those below and those above the median in terms of balance-sheet liquidity. Column 3 displays the relative likelihood of a bank belonging to a particular group, among the complier group, compared to the population at large. We see that those banks that are not in the bottom of the tier 1 distribution are almost equally represented among the compliers compared to the population at large.

Costly External Financing or Agency Problems?

The findings show that the jackpot winner's shocks positively increase loan origination for the set of smaller banks. However, what is the precise mechanism that drives the results? There are two hypotheses that can explain the findings: (a) underinvestment and (b) free cash flow. The underinvestment hypothesis (i.e., the costly external financing hypothesis) states that banks that are financially constrained due to adverse-selection problems can have profitable marginal investment opportunities that are not exploited (Myers and Majluf, 1984; Stein, 1998). On the other hand, the agency problems hypothesis (i.e., the free cash flow hypothesis) asserts that financial frictions can constrain empire-building managers from overinvesting, which could negatively impact the bank's credit risk (Jensen, 1986; Stulz, 1990; Hart and Moore, 1995; Paravisini, 2008).[47] Using Call Report data, I estimate whether there is an increase in the ratio of nonperforming loans to total loans, and a decrease in the ratio interest revenues to total loans within the year and within two, three, and four years of treatment. The empirical specification is (2) where the outcome variables are ratios of nonperforming loans to total loans, and interest revenues to total loans.

Table 2.8 reports the estimates. Columns (1) through (4) report the point estimates in the case of nonperforming loans, and columns (5) to (8) report the point estimates for interest revenues. For both variables, there are no significant effects in the year of treatment, and within two, three, and four years of the shock. There could be concerns related to the power of these tests. However, recall that the sample period includes the Great Recession, in which the default rates increased significantly; and, most importantly, the test uses the same data set in which I could reject the null hypothesis of no effect on deposits and loans (e.g., Table 2.5).

In addition, the extensive margin findings in Table 2.6 can be interpreted as support for the costly external finance model, since the probability of granting loans was not affected. This implies that the banks were extending credit, on average, to borrowers with whom they presumably had a prior relationship (and have therefore

[47] Stulz (1990) and Hart and Moore (1995) models predict ex-post over-investment in those states in which the available funds relative to investment opportunities are higher than expected.

developed soft information about them). These results can be interpreted as support for the underinvestment hypothesis related to costly-external finance models, and thus support for the conclusion that frictions prevent small banks from taking advantage of profitable lending opportunities. As consequence, these results are not consistent with the Modigliani–Miller proposition for banks.

2.4.4 Bank Attributes and Credit Supply

Having established a positive impact of the winner's shock on a bank's credit supply, controlling for local demand conditions, I turn to examining the heterogeneity in the response to the exposure of treatment.Following Stein (1998) cross-sectional predictions related to the extent of information asymmetry problems that banks face, I use bank size as proxy for the extent of information asymmetry that banks face when they attempt to raise financing. Small banks presumably face a relatively more serious problem of adverse selection, and thus rely more on deposits (e.g., greater asymmetric information about the value of the loan portfolio). I split the sample according to the (lag) bank's asset size. The results in Table 2.5 Panel B Columns (5)-(6) and Table 2.9 Panel A Columns (1)-(4) and Table B.23 show that the set of non-large banks (those in the 99% of the asset distribution) increase lending, on average, after the winner's shock. In addition, I find no evidence of an effect on banks between the 76th and 98th percentile. These findings are consistent with Stein (1998), since only the small banks significantly respond to the winner's shock. These results are also consistent with Kashyap and Stein (1995), Kashyap and Stein (2000) and Campello (2002), who indicate that large banks can offset Fed policies.

The reason is that large banks can undo Fed policies on the margin due to relatively greater access to nondeposits financing.

Following the previous findings of no significant treatment effect on deposits (Table 2.5 Panel B Columns (2)-(3)) and lending for the set of large banks (Table 2.5 Panel B Columns (5)-(6) and Table 2.9 Panel A Columns (1)-(4)), the subsequent tests will focus on the set of non-large banks. Next, I test whether the lending behavior depends on how liquid (or illiquid) is the LHS of the bank's balance sheet. To this end, I split the sample in those banks above and below the median in the ratio of cash and securities relative to assets. Table 2.9 Panel A Columns (5)-(8) report the point estimates. The results show that the shock's effect on total loans according to how illiquid the balance sheets are similar to both groups of banks. However, for small business lending the shock's effect is greater for those banks with the relative illiquid balance sheet. This last finding is consistent also with Stein (1998), since the most illiquid banks could face greater adverse selection problems because their balance sheet could be more difficult to valuate. In addition, the banks with the most illiquid balance sheets cannot so easy sell their illiquid assets to fund illiquid loans.

Subsequently, for the set of non-large banks, I study the effect of the winners' shocks to the bank's liability side on credit supply depending on the bank's capital. In the presence of banks' capital regulation, the binding capital constraint can limit the bank's credit supply response to a shock on deposits. To this end, I focus on two of the regulatory capital measures: Tier 1 risk-based capital ratio and leverage ratio.[48] I split the sample on the (lag) bank's capital, those banks in the bottom 10% of the distribution, and those above the 10th percentile.[49] Table 2.9 Panel B reports the point estimates. The estimates show that only those banks above the 10th percentile of the capital ratio distribution for both regulatory measures significantly increase their lending after the winner's shock.

Finally, I test whether lending behavior depends on the number of branches, the number of CBSAs, and the number of states in which the bank has operations (i.e., at least one branch). To this end, I split the sample in those banks above and below the median in the number of branches, the number of CBSAs, and the number of states. Table B.24 reports the estimates. The results show that the winner's shock increases total lending for banks below the median in both variables. However, I find no evidence of an effect in the case of small business lending. Thus, the evidence is mixed in this case.

2.5 Conclusion

In this paper, I estimate the impact of shocks in the supply of deposits on loan origination by exploiting the U.S. jackpot lottery winner along with hand-collected data. The identification strategy utilizes the fact that the occurrence of a jackpot winner, and the amount won, in a specific local area and at a specific time is randomly assigned, conditional on lottery ticket sales. Additionally, the identification condition at the local level (i.e., in the absence of a winner shock, the changes in

[48] The other regulatory capital measure is total risk-based capital ratio.

[49] The findings are similar using, instead of 10% cut-off, 5% or 20% cut-offs.

the outcome variable do not differ between the treatment and control group) is directly testable from features of the settings (e.g., unclaimed prizes). Furthermore, it is possible to determine the receiving branch, and thus the bank, that received the prize. This allows one to study the shock's effect at the bank level, controlling for any unobservable time-varying effects at the CBSA-level (e.g., local demand conditions within a given CBSA-year).

The analysis finds that the jackpot winner shock leads to a significant increase in deposits and an increase in small-business lending at the CBSA-level (i.e., 4.05% and 4.28%, respectively). At the bank level, banks in the treatment group experience a large growth in deposits, total lending, and small business lending. The estimate of the elasticity of total small business lending with respect to deposits is 0.934. In addition, the winners' shock effects on the set of non-large banks is not persistent, and after controlling for demand conditions in the local markets, banks in the treatment group increase on average their small business loan origination (5.02% after a one-standard-deviation increase). There is no evidence that the treated banks experience a relative increase in nonperforming loans and a decrease of interest revenues. Finally, the winners' shock does not affect the probability of lending. Thus, since the shock leads to great lending origination to small businesses, but not an increase in probability of lending, an interpretation of the findings is that on average the increase in lending is to current clients and not new clients.

The findings at the bank-level suggest that a set of banks (i.e., small and medium-sized banks and those with the most illiquid balance sheet) were financially constrained before experiencing the winners' shock, and thus are not consistent with the Modigliani–Miller proposition for banks.

Tables and Figures

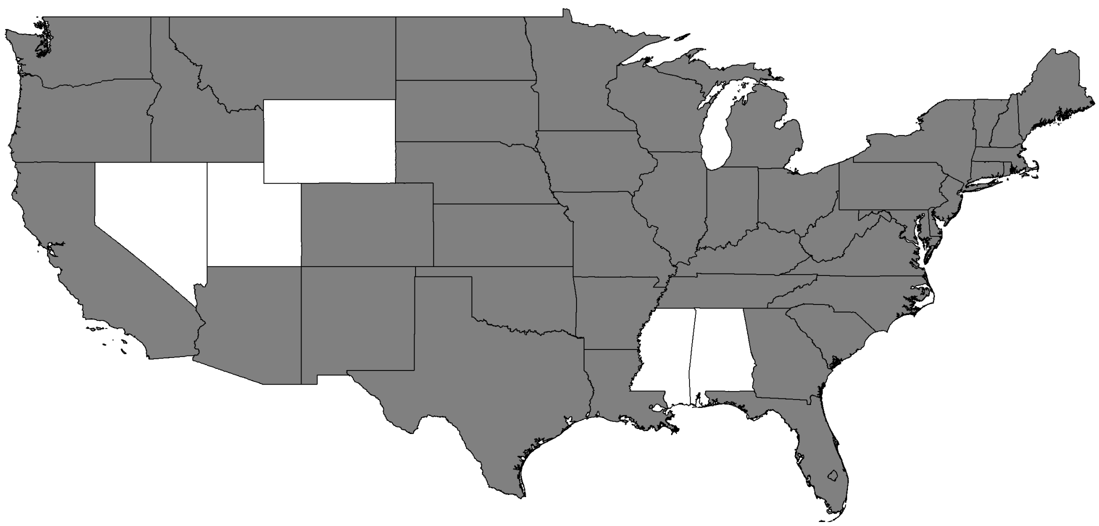

Figure 2.1 States offering Mega Million and Powerball as of June 2013

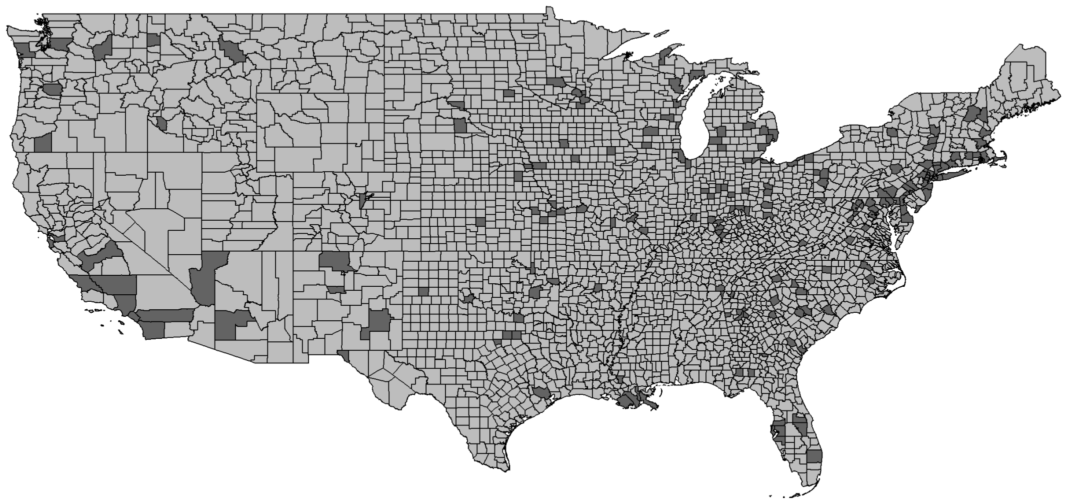

Figure 2.2 Mega Millions and Powerball Jackpot Winners by county, 2002-2013

Table 2.1 Jackpot Winners and CBSA characteristics

The U.S. lottery jackpot winners dataset comes from different public sources. Deposit data comes from the Summary of Deposits (SOD). Population data is from the U.S. Census. Standard deviations in brackets.

Number of states where both lotteries are played (June 2013)		43
	Mega Millions	43
	Powerball	43
Number of jackpot winners		284
	Mega Millions	139
	Powerball	145
Number of different states with winners		41
	Mega Millions	16
	Powerball	38
Number of different CBSA with winners		142
	Mega Millions	65
	Powerball	95
Type of prize		
	Cash	263
	Non-cash (annuity or unclaimed)	21
Winners' state of residence		
	Same state	255
	Different state	29
Prize amounts (after-taxes in 2013 dollars)		
	Mean	$46,094,764
	25th percentile	$17,093,562
	75th percentile	$66,603,208

CBSA characteristics			
	Winner	Non-winner	p-value on t-test for difference
Ln (population in 2001)	13.247	11.239	0.000
	[1.037]	[0.961]	
% white in 2001	0.785	0.802	0.037
	[0.144]	[0.184]	
% male in 2001	0.489	0.493	0.000
	[0.009]	[0.016]	
% over age 45 in 2001	0.346	0.358	0.000
	[0.046]	[0.053]	
Deposit growth (%) 1994-2001	0.021	0.021	0.952
	[0.073]	[0.311]	

Table 2.2 Effect of Jackpot Winners' shock at the CBSA-level

Data are from the SOD, 1999-2013, and Federal Financial Institutions Examination Council (FFIEC), 1999-2012. An observation is a CBSA by year cell. The three dependent variables include the log deposits at the CBSA level, the log of the total small business loan originations defined as loans under \$1 million, and the log small business loans originated with gross annual revenues < 1 million at the CBSA level. Winner is an indicator equal to one in those CBSA/years with Jackpot winners, who chose the cash option and who reside in the state where the winning ticket was bought, and equal to zero otherwise. The variable, Log Prize, is the amount won after federal and state taxes in 1999 dollars. Prize/deposits equals the ratio of the amount won in CBSA i and year t over deposits in CBSA i and the year before treatment. Small bank equals one if the CBSA is in the 5th quintile of the yearly distribution of the ratio of the number of branches from banks with assets lower or equal to \$1 billion (in 1999 dollars) to the total number of branches in each CBSA, and zero otherwise. State taxes is a dummy variable equal to one for those states with individual income state taxes. Winner (t+1) is a lead variable equal to one in the year before the shock in those CBSAs with a winner at t, and zero otherwise. Winner (t+2), Winner (t+3), and Winner (t+4) are defined analogously. All specifications include lag CBSA characteristics (log (population), % white, % male, % over age 45, income per-capita), CSBA, and year fixed effects (FE). Robust standard errors in parentheses, clustered at the CBSA level. ***, **, and * indicate the p-values of 1%, 5%, and 10%, respectively.

Panel A	Log Deposits			Log Small Business Loans Originated with Gross Annual Revenues < 1			Log Total Amount of Small Business Loans		
	(1)	(2)	(3)	(4)	(5)	(6)	(8)	(9)	(10)
Winner	0.0313***			0.0431**			0.0371**		
	(0.0119)			(0.0185)			(0.0148)		
Log Prize		0.0038***			0.0045**			0.0040***	
		(0.0012)			(0.0018)			(0.0015)	
Prize / Deposit			0.828**			1.890**			1.304*
			(0.413)			(0.930)			(0.789)
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
CBSA FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Additional controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	11,802	12,588	11,802	10,916	10,912	11,420	10,916	10,912	11,420

Table 2.2 continued

Panel B	Log Small Business Loans Originated with Gross Annual Revenues < 1		Log Total Amount of Small Business Loans	
	(1)	(2)	(3)	(4)
Winner	0.0417**	0.0758*	0.0365**	0.0454
	(0.0186)	(0.0397)	(0.0150)	(0.0477)
Winner x State taxes		-0.0353		-0.00740
		(0.0444)		(0.0501)
Small Bank	-0.0363		-0.0272	
	(0.0292)		(0.0235)	
Winner x Small Bank	0.131***		0.0679***	
	(0.0252)		(0.0204)	
Year FE	Yes	Yes	Yes	Yes
CBSA FE	Yes	Yes	Yes	Yes
Additional controls	Yes	Yes	Yes	Yes
Observations	10,912	10,964	10,912	10,964

Table 2.2 continued

Panel C	Log Deposits			Log Small Business Loans Originated with Gross Annual Revenues < 1			Log Total Amount of Small Business Loans		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Winner (-3y, -2y)		-0.0157			-0.00424			0.00563	
		(0.0131)			(0.0243)			(0.0190)	
Winner (-2y, -1y)		0.0126			0.0273			0.0269	
		(0.00943)			(0.0254)			(0.0190)	
Winner (-1y, 0y)		0.0142			0.00855			0.00633	
		(0.0115)			(0.0235)			(0.0173)	
Winner (0y, 1y)		0.0327**	0.0329***		0.0460**	0.0428**		0.0411**	0.0384***
		(0.0144)	(0.0120)		(0.0219)	(0.0183)		(0.0174)	(0.0148)
Non-cash and winner out-of-state	-0.00803			-0.0105			-0.0115		
	(0.0238)			(0.0268)			(0.0229)		
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
CBSA FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Additional controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Sample	Possible treated		Treated states	Possible treated		Treated states	Possible treated		Treated states
Observations	11,802	11,802	10,421	11,420	10,916	10,176	11,420	10,916	10,176

Table 2.3 2SLS of Small Business Loan Originations and Deposit Supply

Data are from the FFIEC and SOD, 1999-2012. An observation is a CBSA by year cell. The dependent variable equals the log of small business loan originations to businesses with $1 million in annual gross revenue or less at the CBSA level. Log Deposits are the log deposits at the CBSA level for those banks that meet the CRA asset size threshold to be subject to data reporting requirements. Log Prize is the amount won after federal and state taxes in 1999 dollars. All specifications include lag CBSA characteristics (log (population), % white, % male, % over age 45, income per-capita), and CSBA and year FE. Robust standard errors in parentheses, clustered at the CBSA level. ***, **, and * indicate p-values of 1%, 5%, and 10%, respectively.

	First Stage	OLS	RF	2SLS
	Log Deposits	Log Total Amount of Small Business Loans		
	(1)	(2)	(3)	(4)
Log Deposits		0.309***		0.490***
		(0.0226)		(0.188)
Log Prize	0.00741***		0.00364**	
	(0.00202)		(0.00146)	
First Stage F-stat	13.41			
Year FE	Yes	Yes	Yes	Yes
CBSA FE	Yes	Yes	Yes	Yes
Additional controls	Yes	Yes	Yes	Yes
Observations	10,776	10,776	10,776	10,776

Table 2.4 Individual Detection Algorithm Summary Statistics

The U.S. lottery jackpot winners dataset comes from different public sources. Branch characteristics data comes from the SOD. Bank characteristics data comes from the Call Report. Standard deviations in brackets.

Number of non-group jackpot winners	191		
Winners matched	134		

Winners matched		Number of branches	
Single branch matched	71	71	
Multiple branches matched (up to three)	63	146	
Total	134	217	
Driving distance from retailer to branch (minutes)			
Mean	16.23		
25th percentile	10.92		
75th percentile	25.98		

Branch characteristics	Winner	Non-winner	p-value on t-test for difference
Deposit growth (%) 1994-2001	0.05	0.12	0.000
	[.163]	[.269]	

Banks characteristics	Winner	Non-winner	p-value on t-test for difference
Ln(Assets)	14.11	11.12	0.000
	[2.360]	[1.300]	
ROA	0.0047	0.0050	0.003
	[.008]	[.009]	
Equity/Assets	0.100	0.110	0.000
	[.040]	[.050]	

Table 2.5 Effect of Winners' Shock on Deposits and Loans at the Bank-level

Data are from the Call Report, 1999-2013. An observation is a bank by quarter cell. The dependent variable equals the Log Deposits at the bank level, and Log Total Loans. Prize/deposits (0m, 3m) equals the ratio, on the quarter of treatment, of the prize deposit in bank i in quarter t to deposits in banks i and the quarter before treatment. Prize/deposits (0m, 12m) is the same ratio but takes the value of the ratio in the 12 months after treatment for the banks in the treatment group (0 m, 12 m), and zero otherwise. Prize/deposits (-12m, 0m) is the same ratio but takes the value of the ratio in the 12 months before the shock. Prize/deposits (-3m, 0m) is the same ratio but takes the value of the ratio in the three months before the shock. Prize/deposits (-6m, -3m) is the same ratio but takes the value of the ratio in the three-six months before the shock. Prize/deposits (3m, 6m) is the same ratio but takes the value of the ratio in the three-six months after the shock. Bank controls include the lag of log (assets), ROA, and Equity/Assets. All specifications include bank and quarter FE. Robust standard errors in parentheses, clustered at the bank level. ***, **, and * indicate p-values of 1%, 5%, and 10%, respectively.

Panel A	Log Total Deposits			Log Total Loans		
	(1)	(2)	(3)	(4)	(5)	(6)
Prize / Deposit (0m, 3m)	0.0814*			0.154*		
	(0.0434)			(0.0813)		
Prize / Deposit (0m, 12m)		0.124***			0.149***	
		(0.0209)			(0.0326)	
Prize / Deposit (-12m, 0m)			-0.0236			-0.464
			(0.0676)			(0.308)
Quarter FE	Yes	Yes	Yes	Yes	Yes	Yes
Bank FE	Yes	Yes	Yes	Yes	Yes	Yes
Additional controls	Yes	Yes	Yes	Yes	Yes	Yes
Observations	451,142	448,025	451,142	451,190	448,068	451,190

Panel B	Log Total Deposits			Log Total Loans		
	(1)	(2)	(3)	(4)	(5)	(6)
Prize / Deposit (-6m, -3m)	-0.0625	-0.0566	-4.345	-0.748	-0.749	8.052
	(0.0748)	(0.0736)	(22.81)	(0.498)	(0.498)	(7.403)
Prize / Deposit (-3m, 0m)	0.113	0.126	-5.055	-0.259	-0.258	21.25
	(0.0870)	(0.0900)	(32.97)	(0.163)	(0.164)	(16.47)
Prize / Deposit (0m, 3m)	0.0808*	0.0912**	-7.320	0.158*	0.159*	11.48
	(0.0424)	(0.0429)	(18.18)	(0.0834)	(0.0837)	(10.20)
Prize / Deposit (3m, 6m)				0.179***	0.179***	2.116
				(0.0514)	(0.0515)	(6.712)
Prize / Deposit (6m, 9m)				0.191***	0.191***	14.10
				(0.0459)	(0.0459)	(9.292)
Prize / Deposit (9m, 12m)				0.0311	0.0308	3.349
				(0.0593)	(0.0594)	(13.10)
Quarter FE	Yes	Yes	Yes	Yes	Yes	Yes
Bank FE	Yes	Yes	Yes	Yes	Yes	Yes
Additional controls	Yes	Yes	Yes	Yes	Yes	Yes
Sample (Size)	All	Bottom 99%	Top 1%	All	Bottom 99%	Top 1%
Observations	436,078	433,027	3,051	435,027	431,984	3,043

Table 2.6 Effect of Winners' shock on the Intensive and Extensive Margin

Data are from the FFIEC and SOD, 1999-2012. An observation is a bank by year cell. The dependent variables are log of the total small business loan origination, defined as loans under $1 million, log of small business loan originations to businesses with $1 million in annual gross revenue or less at the bank level, and an indicator variable equal to one if a loan is originated (extensive margin). Prize/deposits equals the ratio, on the year of treatment, of the prize deposit in bank i in year t to deposits in banks i and year before treatment. Prize/deposit is the same ratio but takes the value of the ratio three years before the shock for those banks treated at t, and zero otherwise. Bank controls include the lag of ln(assets), ROA, and Equity/Assets, and the number of bank branches i in CBSA j in year t-1. All specifications include CBSA by year FE. Robust standard errors in parentheses, clustered at the bank level. ***, **, and * indicate p-values of 1%, 5%, and 10%, respectively.

	Log Small Business Loans with Annual Revenues < 1		Log Total Amount of Small Business Loans		Y=1 if loan originated
	(1)	(2)	(3)	(4)	(5)
Prize / Deposit (0y, 1y)	6.223**		5.304**		0.0287
	(2.552)		(2.264)		(0.195)
Prize / Deposit (-3y, -2y)		1.432		0.663	
		(2.073)		(2.145)	
CBSA x Year FE	Yes	Yes	Yes	Yes	Yes
Additional controls	Yes	Yes	Yes	Yes	Yes
Observations	73,946	73,946	75,503	75,503	10,124,017

Table 2.7 2SLS of the Relationship Between Loans and Deposit at the Bank-level

Data are from the FFIEC and SOD, 1999-2012. An observation is a bank by year cell. The dependent variable is the log of the total small business loan origination. Prize/deposits equals the ratio, on the year of treatment, of the prize deposit in bank i in year t to deposits in banks i and year before treatment. Bank controls include the lag of ln(assets), ROA, and Equity/Assets, and the number of branches bank i in CBSA j in year $t-1$. All specifications include CBSA by year FE. Robust standard errors in parentheses, clustered at the bank level. ***, **, and * indicate p-values of 1%, 5%, and 10%, respectively. Panel B presents characteristics of compliers, following Angrist and Pischke (2008). Column 1 Panel B reports the distribution of the population of commercial banks by tier 1 risk-based capital ratio and balance sheet liquidity, $P(X=x)$. Column 2 Panel B reports the distribution of compliers by tier1-liquidity groups, calculated as the ratio of the first stage for that subgroup to the overall first stage, multiplied by the proportion of the population in the group, $P(X=x \mid I_1 > I_0)$. Column 3 Panel B displays the relative likelihood of a bank belonging to a particular group, in the complier group compared to the population at large.

Panel A	First Stage	OLS	RF	2SLS
	Log Deposits	Log Total Amount of Small Business Loans		
	(1)	(2)	(3)	(4)
Log Deposits		0.362***		0.934**
		(0.0358)		(0.418)
Prize / Deposit	5.394***		5.036**	
	(1.657)		(2.177)	
First Stage F-stat	10.60			
CBSA x Year FE	Yes	Yes	Yes	Yes
Additional controls	Yes	Yes	Yes	Yes
Observations	75,486	75,486	75,486	75,486

Panel B	Compliance for different bank groups		
	$P(X=x)$	$P(X=x \mid I_1 > I_0)$	$\frac{P(X=x \mid I_1>I_0)}{P(X=x)}$
	(1)	(2)	(3)
Tier 1: Bottom 5%			
Liquidity: Below 50%	0.024	0.010	0.390
Liquidity: Above 50%	0.024	0.015	0.612
Tier 1: Top 95%			
Liquidity: Below 50%	0.451	0.495	1.098
Liquidity: Above 50%	0.501	0.481	0.961

Table 2.8 Effect of Jackpot Winners' Shock on Loan Performance

Data are from the Call Report, 1999-2013. An observation is a bank by quarter cell. The dependent variables are the ratio of nonperforming loans (past due 90+ days plus nonaccrual) to total loans at the bank level, along with interest and fee income from loans to total loans at the bank level. Prize/deposits (0m, 12m) equals the ratio, on the year of treatment, of the prize deposit in bank i in year t to deposits in banks i and year t-1 (quarter before treatment). Prize/deposits (0m, 24m) and Prize/deposits (0m, 36m) are defined analogously. All specifications include bank and quarter FE. Robust standard errors in parentheses, clustered at the bank level. ***, **, and * indicate p-values of 1%, 5%, and 10%, respectively.

	Nonperforming Loans to Total Loans				Interest Revenues to Total Loans			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Prize / Deposit (0m, 12m)	-0.00493				-0.000707			
	(0.00651)				(0.000996)			
Prize / Deposit (0m, 24m)		-0.0121				-9.97e-05		
		(0.0118)				(0.000655)		
Prize / Deposit (0m, 36m)			-0.0111				0.000744	
			(0.0135)				(0.000794)	
Prize / Deposit (0m, 48m)				-0.0105				0.000694
				(0.0202)				(0.000772)
Quarter FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Bank FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Additional controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	448,060	448,021	438,646	438,607	413,547	413,508	373,556	373,517

Table 2.9 Bank-Attributes and Credit Supply

Data are from the Call Report, 1999-2013. An observation is a bank by quarter cell. The dependent variable equals the Log Deposits at the banks level, and Log total small business loans. Prize/deposits equals the ratio, on the year of treatment (0m, 12m), of the prize deposit in bank i in quarter t to and deposits in banks i and quarter before treatment. Columns (1) and (3) in panel A are those banks in the 99% of the quarterly asset distribution. Columns (5) and (7) in Panel A are those banks below the median of the ratio (Cash+Assets)/Total Assets distribution, Bank controls include the lag of log(assets), ROA, and Equity/Assets. All specifications include bank and quarter FE. Robust standard errors in parentheses, clustered at the bank level. ***, **, and * indicate p-values of 1%, 5%, and 10%, respectively..

Panel A	Size				Balance Sheet Liquidity			
	Log Total Loans		Log Total Small Business Loans		Log Total Loans		Log Total Small Business Loans	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Prize / Deposit	0.151***	6.278	0.229*	9.956	0.100**	0.139***	0.285*	0.0592
	(0.0312)	(5.748)	(0.132)	(14.32)	(0.0445)	(0.0225)	(0.148)	(0.119)
Percentile	Bottom 99%	Top 1%	Bottom 99%	Top 1%	Below 50%	Above 50%	Below 50%	Above 50%
Quarter FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Bank FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Additional controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	431,984	3,043	424,564	2,665	215,502	219,525	211,324	215,905

Panel B	Tier 1 Risk-Based Capital Ratio				Leverage Capital Ratio			
	Log Total Loans		Log Total Small Business Loans		Log Total Loans		Log Total Small Business Loans	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Prize / Deposit	0.0759	0.166***	0.0440	0.233***	0.241	0.182***	0.441	0.282**
	(0.183)	(0.0280)	(0.429)	(0.0858)	(0.191)	(0.0312)	(0.427)	(0.110)
Percentile	Bottom 10%	Top 90%	Bottom 10%	Top 90%	Bottom 10%	Top 90%	Bottom 10%	Top 90%
Quarter FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Bank FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Additional controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	36,896	337,128	36,768	330,237	36,943	337,081	36,491	330,514

Appendices

Appendix A

Variable Definitions

All variables are measured three years and six years post-filing.

New Real Property: It is an indicator variable equal to one if there is a registry that records the individual acquires a real estate property on or before the indicated year post-filing. New real property data is obtained from public records (LexisNexis).

Start a Business: It is an indicator variable that takes a value equal to one if there is a registry that records a business creation (i.e., fictitious business (DBA), business license, limited liability corporations) on or before the indicated year post-filing. Business creation data is obtained from public records (LexisNexis).

UCC Liens: It is an indicator variable that takes a value equal to one if there is a registry that records an UCC loans secured by fixed assets on or before the indicated year post-filing. UCC loans are loans with collateral in which a UCC-1 form was filed. This data is obtained from public records (LexisNexis).

Home Foreclosure: It is an indicator for a filer's home receiving a notice of default, receiving a notice of transfer or sale, or having been transferred to an REO or a guarantor on or before the indicated year post-filing. Foreclosure ranges from an actual sale or transfer of the home, to merely a notice that foreclosure was initiated. Foreclosure data is obtained from RealtyTrac.

Judgment Lien: It is an indicator variable that takes a value equal to one if there is a registry that records a civil or tax judgment suits on or before the indicated year post-filing. This data is obtained from public records (LexisNexis).

Future Bankruptcy: It is an indicator variable that takes a value equal to one if a debtor refiles for bankruptcy either for Chapter 7 or 13 on or before the indicated year post-filing. Chapter 7 filers can refile for bankruptcy after 8 years. While, to receive a discharge on a subsequent Chapter 13, the petitioner must wait 4 years from the date of filing the first Chapter 7. Future Bankruptcy data is obtained from public records (LexisNexis).

Criminal Filings: It is an indicator variable that takes a value equal to one if a debtor has arrest records, court conviction records or traffic violations on or before the indicated year post-filing. This data is obtained from public records (LexisNexis).

Same Zip-code: It is an indicator variable that takes a value equal to one if a debtor live in the same zip-code recorded in the bankruptcy forms on or before the indicated year post-filing. This data is obtained from public records (LexisNexis).

Divorce: It is an indicator variable that takes a value equal to one if a debtor files for divorce on or before the indicated year post-filing, or if does not live with his/her spouse anymore. This data is obtained from public records (LexisNexis).

Appendix B

Supplementary Tables and Figures

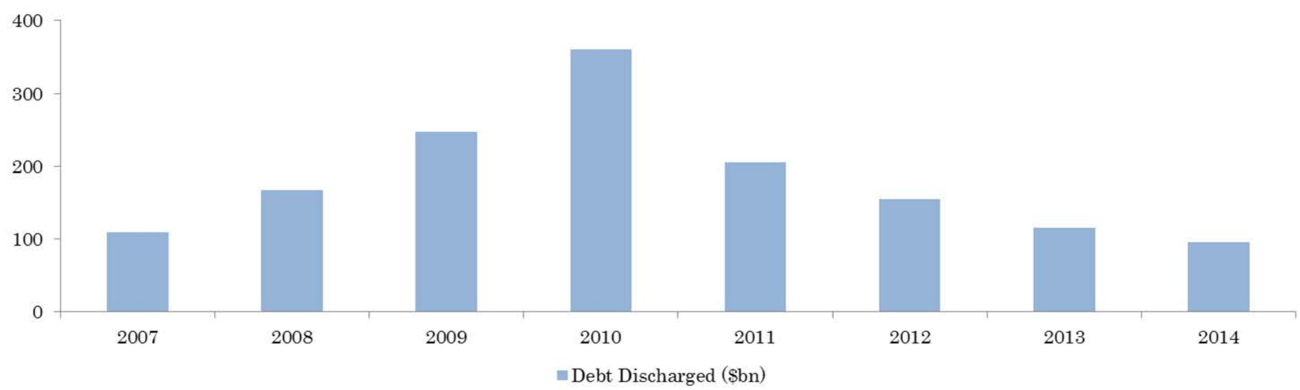

Figure B.1 Debt Relief Provided by Consumer Bankruptcy

This figure plots the yearly debt relief in billions of dollars through the consumer bankruptcy system in year 2000 dollars from 2007 through 2014 extracted from the Statistics Division of the Administrative Office of the United States Courts.

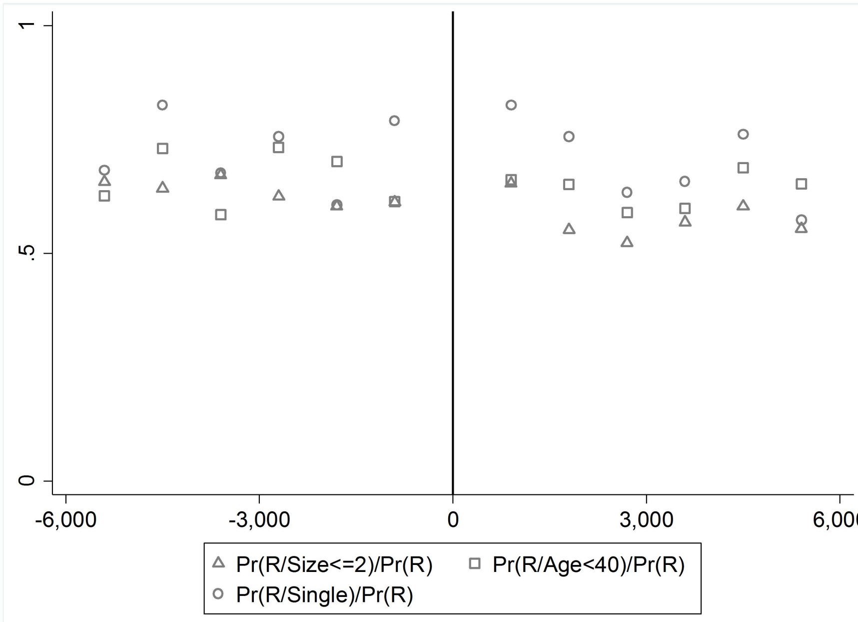

Figure B.2 Test of Continuity of Ratios of Conditional to Unconditional Densities

Ratios of conditional to unconditional densities, following Zimmerman (2014), of filers by distance relative to the Average Gross Monthly Income (AGMI) and the state median income for three different conditioning pre-treatment characteristics: household size, marital status, and age.

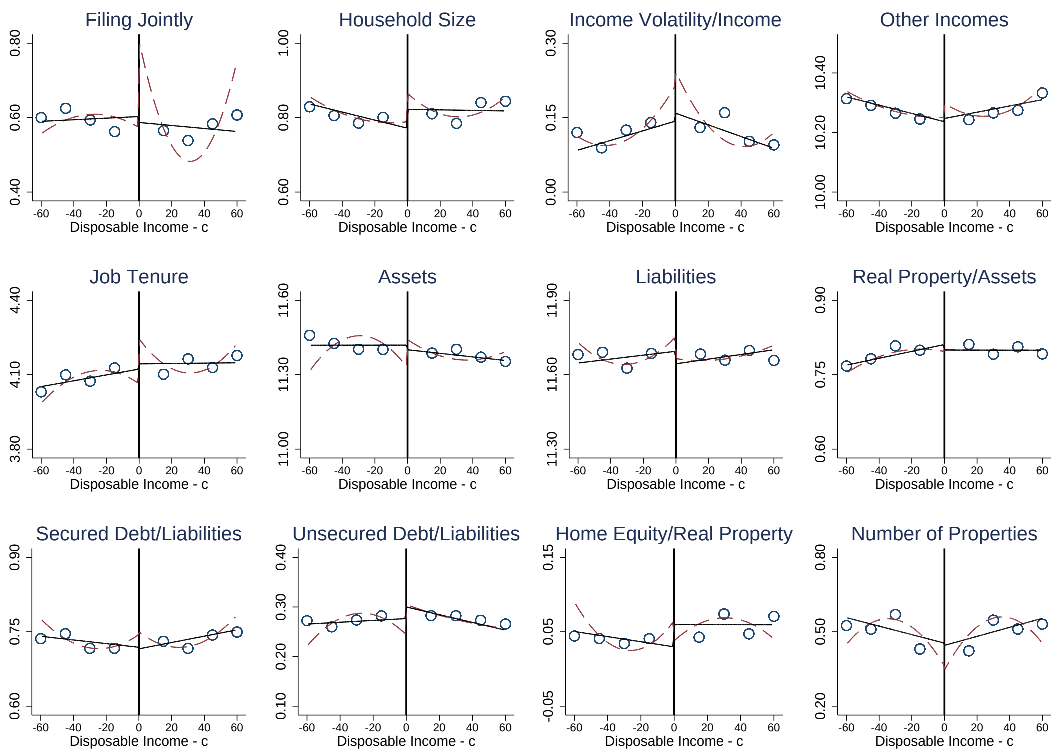

Figure B.3 Test for Smoothness of Characteristics around the Second Cutoff

The figure describes means of pretreatment covariates by distance relative to the second cutoff to test for covariates balance around the threshold. Household size corresponds to the log of all the people who occupy a housing unit as their usual place of residence and are dependent on the debtor for tax purposes. Debtor income volatility is the standard deviation of the debtor's income over the last six months before filing relative to the income. Other Income is the log of the gross income other than wages. Job tenure is the log of the debtor's tenure in years at the filing date. Assets and Liabilities correspond to the log of total assets and total liabilities at the filing date. Real Property/Assets is real property to total assets. Secured Debt/Liabilities comprises total debt backed by collateral relative to total debt. Unsecured Debt/Liabilities is unsecured claims to liabilities. Home equity/ Real property is the difference between the property's market value and the outstanding balance of all liens on the property relative to the total real estate assets. Number of properties is the log of the number of real properties held by the debtor at the date of filing. Solid lines are nonparametric fits from a local linear regression, and dashed lines are quadratic fits that use rectangular kernels. The bin width is $15. All specifications allow for differential slopes on each side of the cutoff.

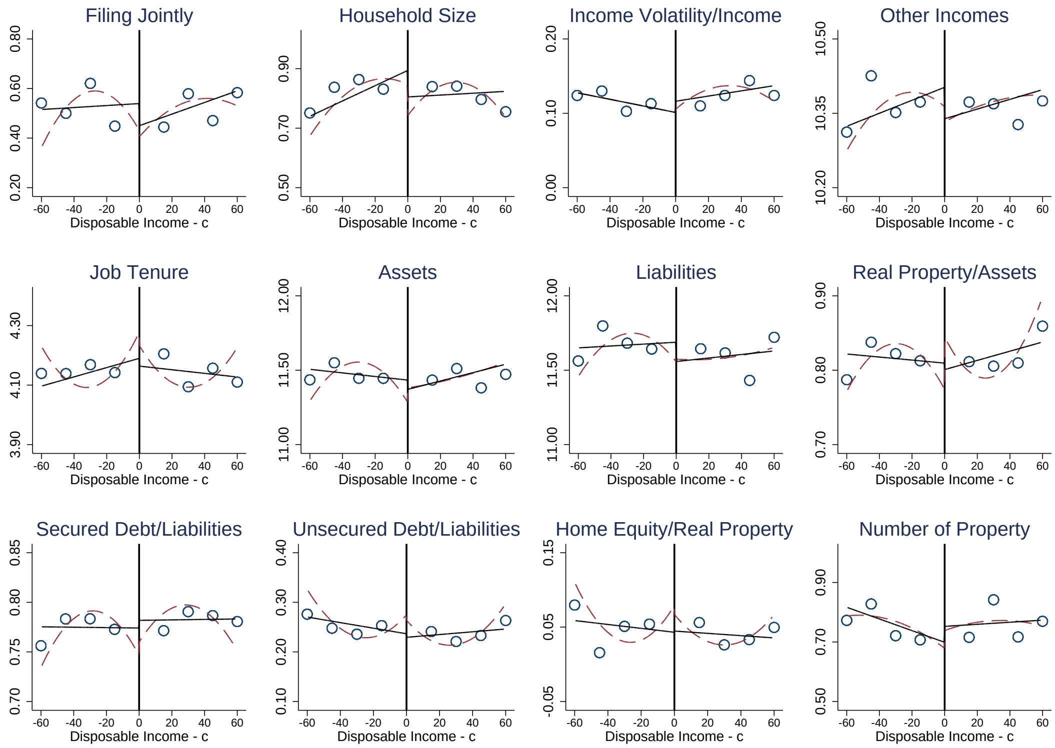

Figure B.4 Test for Smoothness of Characteristics around the Third Cutoff

The figure describes means of pretreatment covariates by distance relative to the third cutoff to test for covariates balance around the threshold. Household size corresponds to the log of all the people who occupy a housing unit as their usual place of residence and are dependent on the debtor for tax purposes. Debtor income volatility is the standard deviation of the debtor's income over the last six months before filing relative to the income. Other Income is the log of the gross income other than wages. Job tenure is the log of the debtor's tenure in years at the filing date. Assets and Liabilities correspond to the log of total assets and total liabilities at the filing date. Real Property/Assets is real property to total assets. Secured Debt/Liabilities comprises total debt backed by collateral relative to total debt. Unsecured Debt/Liabilities is unsecured claims to liabilities. Home equity/ Real property is the difference between the property's market value and the outstanding balance of all liens on the property relative to the total real estate assets. Number of properties is the log of the number of real properties held by the debtor at the date of filing. Solid lines are nonparametric fits from a local linear regression, and dashed lines are quadratic fits that use rectangular kernels. The bin width is $15. All specifications allow for differential slopes on each side of the cutoff.

Appendix Table B.1 Form 22A Means Test Calculation for Chapter 7 Debtors

First page of Form 22A. This form is required for Chapter 7 filers and provides the means test calculation submitted by debtors through PACER.

Form B22A (Chapter 7) (10/05)

In re ______________________
Debtor(s)

Case Number: ______________________
(If known)

According to the calculations required by this statement:
☐ **The presumption arises.**
☐ **The presumption does not arise.**
(Check the box as directed in Parts I, III, and VI of this statement.)

STATEMENT OF CURRENT MONTHLY INCOME AND MEANS TEST CALCULATION

FOR USE IN CHAPTER 7 ONLY

In addition to Schedules I and J, this statement must be completed by every individual Chapter 7 debtor, whether or not filing jointly, whose debts are primarily consumer debts. Joint debtors may complete one statement only.

	Part I. EXCLUSION FOR DISABLED VETERANS
1	If you are a disabled veteran described in the Veteran's Declaration in this Part I, (1) check that box at the beginning of the Veteran's Declaration, (2) check the box for "The presumption does not arise" at the top of this statement, and (3) complete the verification in Part VIII. Do not complete any of the remaining parts of this statement. ☐ **Veteran's Declaration.** By checking this box, I declare under penalty of perjury that I am a disabled veteran (as defined in 38 U.S.C. § 3741(1)) whose indebtedness occurred primarily during a period in which I was on active duty (as defined in 10 U.S.C. § 101(d)(1)) or while I was performing a homeland defense activity (as defined in 32 U.S.C. §901(1)).

	Part II. CALCULATION OF MONTHLY INCOME FOR § 707(b)(7) EXCLUSION		
2	**Marital/filing status.** Check the box that applies and complete the balance of this part of this statement as directed. a. ■ Unmarried. **Complete only Column A ("Debtor's Income") for Lines 3-11.** b. ☐ Married, not filing jointly, with declaration of separate households. By checking this box, debtor declares under penalty of perjury: "My spouse and I are legally separated under applicable non-bankruptcy law or my spouse and I are living apart other than for the purpose of evading the requirements of § 707(b)(2)(A) of the Bankruptcy Code." **Complete only column A ("Debtor's Income") for Lines 3-11.** c. ☐ Married, not filing jointly, without the declaration of separate households set out in Line 2.b above. **Complete both Column A ("Debtor's Income") and Column B ("Spouse's Income") for Lines 3-11.** d. ☐ Married, filing jointly. **Complete both Column A ("Debtor's Income") and Column B ("Spouse's Income") for Lines 3-11.**		
	All figures must reflect average monthly income for the six calendar months prior to filing the bankruptcy case, ending on the last day of the month before the filing. If you received different amounts of income during these six months, you must total the amounts received during the six months, divide this total by six, and enter the result on the appropriate line.	**Column A** **Debtor's Income**	**Column B** **Spouse's Income**
3	Gross wages, salary, tips, bonuses, overtime, commissions.	$	
4	Income from the operation of a business, profession or farm. Subtract Line b from Line a and enter the difference on Line 4. Do not enter a number less than zero. **Do not include any part of the business expenses entered on Line b as a deduction in Part V.**		
	a. Gross receipts — Debtor: $ — Spouse:		
	b. Ordinary and necessary business expenses — Debtor: $ — Spouse:		
	c. Business income — Subtract Line b from Line a	$	
5	Rents and other real property income. Subtract Line b from Line a and enter the difference on Line 5. Do not enter a number less than zero. **Do not include any part of the operating expenses entered on Line b as a deduction in Part V.**		
	a. Gross receipts — Debtor: $ — Spouse:		
	b. Ordinary and necessary operating expenses — Debtor: $ — Spouse:		
	c. Rental income — Subtract Line b from Line a	$	
6	Interest, dividends, and royalties.	$	
7	Pension and retirement income.	$	
8	Regular contributions to the household expenses of the debtor or the debtor's dependents, including child or spousal support. Do not include contributions from the debtor's spouse if Column B is completed.	$	

Appendix Table B.2 Form 22C Means Test Calculation for Chapter 13 Debtors

First page of Form 22C. This form is required for Chapter 13 filers and provides the means test calculation since it determines the payment plan. The form is submitted through PACER.

5/11/09 3:07PM

B22C (Official Form 22C) (Chapter 13) (01/08)

In re ______________________
Debtor(s)

Case Number: ______________________
(If known)

According to the calculations required by this statement:
☐ **The applicable commitment period is 3 years.**
■ **The applicable commitment period is 5 years.**
■ **Disposable income is determined under § 1325(b)(3).**
☐ **Disposable income is not determined under § 1325(b)(3).**
(Check the boxes as directed in Lines 17 and 23 of this statement.)

CHAPTER 13 STATEMENT OF CURRENT MONTHLY INCOME AND CALCULATION OF COMMITMENT PERIOD AND DISPOSABLE INCOME

In addition to Schedules I and J, this statement must be completed by every individual chapter 13 debtor, whether or not filing jointly. Joint debtors may complete one statement only.

Part I. REPORT OF INCOME			
1	**Marital/filing status.** Check the box that applies and complete the balance of this part of this statement as directed. a. ■ Unmarried. **Complete only Column A ("Debtor's Income") for Lines 2-10.** b. ☐ Married. **Complete both Column A ("Debtor's Income") and Column B ("Spouse's Income") for Lines 2-10.**		
	All figures must reflect average monthly income received from all sources, derived during the six calendar months prior to filing the bankruptcy case, ending on the last day of the month before the filing. If the amount of monthly income varied during the six months, you must divide the six-month total by six, and enter the result on the appropriate line.	**Column A** **Debtor's Income**	**Column B** **Spouse's Income**
2	**Gross wages, salary, tips, bonuses, overtime, commissions.**	$	
3	**Income from the operation of a business, profession, or farm.** Subtract Line b from Line a and enter the difference in the appropriate column(s) of Line 3. If you operate more than one business, profession or farm, enter aggregate numbers and provide details on an attachment. Do not enter a number less than zero. **Do not include any part of the business expenses entered on Line b as a deduction in Part IV.** Debtor / Spouse a. Gross receipts: $ / b. Ordinary and necessary business expenses: $ / c. Business income: Subtract Line b from Line a	$	
4	**Rents and other real property income.** Subtract Line b from Line a and enter the difference in the appropriate column(s) of Line 4. Do not enter a number less than zero. **Do not include any part of the operating expenses entered on Line b as a deduction in Part IV.** Debtor / Spouse a. Gross receipts: $ 0.00 / $ b. Ordinary and necessary operating expenses: $ 0.00 / $ c. Rent and other real property income: Subtract Line b from Line a	$	
5	**Interest, dividends, and royalties.**	$	
6	**Pension and retirement income.**	$	
7	**Any amounts paid by another person or entity, on a regular basis, for the household expenses of the debtor or the debtor's dependents, including child support paid for that purpose.** Do not include alimony or separate maintenance payments or amounts paid by the debtor's spouse.	$	
8	**Unemployment compensation.** Enter the amount in the appropriate column(s) of Line 8. However, if you contend that unemployment compensation received by you or your spouse was a benefit under the Social Security Act, do not list the amount of such compensation in Column A or B, but instead state the amount in the space below: Unemployment compensation claimed to be a benefit under the Social Security Act / Debtor $ / Spouse $	$	

Appendix Table B.3 Test of Discontinuities in Pretreatment Deductions and Expenses

This table reports the estimates of the test for the balance of deductions and expenses allowed by the IRS across the threshold. Total expenses comprise standard predetermied expenses allowed under IRS such as: food, personal care, transportation, housing, health care among others. Deductions for debt comprise future payments on secured claims. Additional Expenses comprise other necessary expenses not included by the IRS and must be actual, reasonable, necessary and documented. The pooled cutoff combines the second and third cutoffs, as Figure 7 describes. In the pooled cutoff, the running variable is the difference between monthly disposable income and the respective threshold the debtor faces. Table entries are local linear regression estimates with a rectangular kernel of discontinuities in pretreatment covariates around the different cutoffs provided by law. Neighborhood is the distance from the respective cutoffs (bandwidth). Each cell represents a separate regression with baseline covariates as the dependent variable and the threshold crossing variable. Heteroskedasticity-robust standard errors in parentheses. ***, **, and * indicate the p-values of 1%, 5%, and 10%, respectively.

	Pooled cutoff	
Running variable	Disposable Income	
Neighborhood	50	60
Ln Total Deductions	0.043	0.044
	(0.049)	(0.047)
Ln Total Expenses	0.086	0.071
	(0.070)	(0.069)
Ln Deductions for Debt	-0.056	-0.061
	(0.051)	(0.049)
Ln Additional Expenses	0.021	0.017
	(0.035)	(0.028)

Appendix Table B.4 Chapter 7 and Debtors' Post-Filing Outcomes Cutoff 2 and 3

This table reports the fuzzy RD estimates of Chapter 7 bankruptcy protection on post-filing investment decisions, financial distress events and miscellaneous outcomes. In the second and third cutoff the running variable is the difference between monthly disposable income and the respective threshold that debtor faces, as Figure 7 describes. Local linear regression estimates with a rectangular kernel. Each cell represents a separate regression with debtor's ex-post outcome as the dependent variable and the indicator variable of Chapter 7 protection. Home foreclosure is an indicator for a filer's home receiving a notice of default, receiving a notice of transfer or sale, or having been transferred to an REO or a guarantor on or before the indicated year. New Property comprises the acquisition of a new real property by the filer. Judgment Lien is an indicator variable if debtor receives at least one lien. It includes tax liens, hospital liens, and judicial liens. Start a Business is an indicator variable for any business registered in public records by the debtor post-filing. Covariates include age at filing, household size and marital status. Neighborhood is the distance from respective cutoff. Heteroskedasticity-robust standard errors in parentheses. ***, **, and * indicate p-values of 1%, 5%, and 10%, respectively.

	Second cutoff				
Running variable	Disposable Income				
Neighborhood	50	50	60	60	60
Investment decisions					
New real property (3-year)	0.342*	0.338*	0.337*	0.340*	0.327*
	(0.187)	(0.187)	(0.198)	(0.198)	(0.180)
New real property (6-year)	0.431*	0.452*	0.385*	0.400*	0.449*
	(0.239)	(0.242)	(0.222)	(0.239)	(0.271)
Start a Business (3-year)	0.182*	0.195*	0.180*	0.192*	0.238*
	(0.088)	(0.104)	(0.085)	(0.100)	(0.123)
Start a Business (6-year)	0.116*	0.125*	0.123*	0.131*	0.101*
	(0.067)	(0.073)	(0.072)	(0.069)	(0.057)
Financial Distress Events					
Home foreclosure (3-year)	-0.563*	-0.581*	-0.633**	-0.649**	-0.471*
	(0.312)	(0.309)	(0.307)	(0.301)	(0.261)
Home foreclosure (6-year)	-0.663**	-0.679**	-0.684**	-0.699**	-0.510*
	(0.301)	(0.297)	(0.322)	(0.319)	(0.270)
Judgment Lien (3-year)	-0.568**	-0.585**	-0.553**	-0.570**	-0.526*
	(0.274)	(0.276)	(0.264)	(0.270)	(0.306)
Judgment Lien (6-year)	-0.502**	-0.527**	-0.519**	-0.530**	-0.461*
	(0.230)	(0.211)	(0.209)	(0.210)	(0.243)
Future Bankruptcy	-0.208	-0.211	-0.176	-0.167	-0.186
	(0.133)	(0.137)	(0.128)	(0.132)	(0.126)
Miscellaneous Outcome					
Mortality (3 year)	-0.088	-0.079	-0.086	-0.079	-0.060
	(0.071)	(0.064)	(0.071)	(0.067)	(0.087)
Mortality (6-year)	-0.084	-0.073	-0.018	-0.027	-0.035
	(0.073)	(0.068)	(0.076)	(0.066)	(0.094)
Specification	Linear	Linear	Linear	Linear	Quadratic
Covariates and Year FE	N	Y	N	Y	Y

Table B.4 continued

Running variable		Third cutoff Disposable Income			
Neighborhood	50	50	60	60	60
Investment decisions					
New real property (3-year)	0.108**	0.101**	0.149**	0.152**	0.131*
	(0.051)	(0.046)	(0.073)	(0.075)	(0.075)
New real property (6-year)	0.225**	0.224**	0.247**	0.253**	0.209**
	(0.103)	(0.105)	(0.097)	(0.100)	(0.099)
Start a Business (3-year)	0.110*	0.131*	0.094**	0.118**	0.108*
	(0.062)	(0.072)	(0.044)	(0.056)	(0.060)
Start a Business (6-year)	0.128*	0.154*	0.113**	0.141**	0.090*
	(0.077)	(0.089)	(0.049)	(0.065)	(0.051)
Financial Distress Events					
Home foreclosure (3-year)	-0.498*	-0.524*	-0.504*	-0.512*	-0.442*
	(0.292)	(0.293)	(0.304)	(0.300)	(0.245)
Home foreclosure (6-year)	-0.583**	-0.594**	-0.663**	-0.668**	-0.497**
	(0.286)	(0.288)	(0.307)	(0.306)	(0.233)
Judgment Lien (3-year)	-0.301*	-0.306*	-0.333**	-0.343**	-0.301**
	(0.170)	(0.172)	(0.163)	(0.165)	(0.146)
Judgment Lien (6-year)	-0.508***	-0.514***	-0.533***	-0.545***	-0.541***
	(0.191)	(0.194)	(0.185)	(0.187)	(0.212)
Future Bankruptcy	-0.102	-0.118	-0.077	-0.092	-0.026
	(0.170)	(0.163)	(0.172)	(0.166)	(0.194)
Miscellaneous Outcome					
Mortality (3 year)	-0.106	-0.106	-0.099	-0.099	-0.121
	(0.076)	(0.074)	(0.071)	(0.069)	(0.082)
Mortality (6-year)	-0.018	-0.018	-0.029	-0.028	-0.017
	(0.091)	(0.090)	(0.085)	(0.084)	(0.097)
Specification	Linear	Linear	Linear	Linear	Quadratic
Covariates and Year FE	N	Y	N	Y	Y

Appendix Table B.5 Impact by Debtor Characteristic

This table reports the fuzzy RD estimates of Chapter 7 bankruptcy protection by baseline characteristics: marital status, age and household size. Local linear regression estimates with rectangular kernel. Each cell represents a separate regression with debtor's ex-post outcome as the dependent variable and the indicator variable of Chapter 7 protection. Home foreclosure is an indicator for a filer's home receiving a notice of default, receiving a notice of transfer or sale, or having been transferred to an REO or a guarantor on or before the indicated year. New Property comprises the acquisition of a new real property by the filer. Judgment Lien is an indicator variable if debtor receives at least one lien. It includes tax liens, hospital liens, and judicial liens. Start a Business is an indicator variable for any business registered in public records by the debtor post-filing. Heteroskedasticity-robust standard errors in parentheses.***, **, and * indicate p-values of 1%, 5%, and 10%, respectively.

Panel A	First cutoff					
Debtor's Characteristics	Married	Single	Age<=40	Age>40	Size<=2	Size>2
Neighborhood	6,000					
Investment decisions						
New real property (3-year)	-0.088	0.387	-0.303	0.333*	0.325	0.056
	(0.579)	(0.252)	(0.687)	(0.201)	(0.316)	(0.300)
New real property (6-year)	-0.816	0.526*	-0.017	0.126	0.522	-0.356
	(0.859)	(0.297)	(0.741)	(0.229)	(0.395)	(0.377)
Start a Business (3-year)	0.149	0.261**	0.542*	0.077	0.244*	0.093
	(0.157)	(0.113)	(0.324)	(0.104)	(0.147)	(0.101)
Start a Business (6-year)	0.093	0.243*	0.552*	-0.002	0.309*	0.058
	(0.172)	(0.124)	(0.330)	(0.117)	(0.166)	(0.113)
Financial Distress Events						
Home foreclosure (3-year)	-0.083	-0.221	0.644	-0.553	-0.092	-0.406
	(0.989)	(0.367)	(0.954)	(0.362)	(0.518)	(0.507)
Home foreclosure (6-year)	-0.500	-0.226	0.650	-0.812*	-0.148	-0.720
	(1.074)	(0.408)	(0.989)	(0.415)	(0.575)	(0.546)
Judgment liens (3-year)	-0.886*	-0.252	-0.773	-0.649**	-0.965**	-0.091
	(0.512)	(0.286)	(0.809)	(0.330)	(0.491)	(0.324)
Judgment liens (6-year)	-0.923	-0.413	-0.847	-0.831**	-0.835*	-0.260
	(0.603)	(0.329)	(0.914)	(0.372)	(0.503)	(0.349)
Future Bankruptcy	-0.790**	-0.273	-0.844	-0.824***	-0.972**	-0.577*
	(0.322)	(0.203)	(0.573)	(0.292)	(0.417)	(0.342)
Miscellaneous Outcome						
Mortality (3 year)	0.601	-0.086	0.499	-0.039	-0.022	0.260
	(0.442)	(0.115)	(0.361)	(0.137)	(0.179)	(0.163)
Mortality (6-year)	0.515	-0.034	0.432	-0.008	0.009	0.241
	(0.426)	(0.134)	(0.347)	(0.157)	(0.208)	(0.173)
Covariates and Year FE	N	N	N	N	N	N

Table B.5 continued

Panel B			Pooled cutoff			
Debtor's Characteristics	Married	Single	Age<=40	Age>40	Size<=2	Size>2
Neighborhood			60			
Investment decisions						
New real property (3-year)	0.158	0.259**	0.553**	0.077	0.114	0.340*
	(0.113)	(0.124)	(0.267)	(0.089)	(0.101)	(0.183)
New real property (6-year)	0.239*	0.125	0.744**	0.023	0.051	0.549**
	(0.142)	(0.162)	(0.312)	(0.116)	(0.112)	(0.235)
Start a Business (3-year)	0.028	0.182*	0.043	0.059*	0.135**	0.028
	(0.077)	(0.105)	(0.045)	(0.032)	(0.063)	(0.029)
Start a Business (6-year)	0.028	0.216*	0.072	0.101*	0.153**	-0.023
	(0.077)	(0.113)	(0.056)	(0.055)	(0.066)	(0.114)
Financial Distress Events						
Home foreclosure (3-year)	-0.702**	-0.602	-0.463	-0.658**	-0.266	-0.837*
	(0.287)	(0.491)	(0.811)	(0.289)	(0.277)	(0.507)
Home foreclosure (6-year)	-0.568*	-0.797	-0.689	-0.698**	-0.397	-0.782
	(0.317)	(0.657)	(0.923)	(0.299)	(0.363)	(0.599)
Judgment Lien (3-year)	-0.243*	-0.087	0.292	-0.571***	-0.353**	-0.344
	(0.128)	(0.082)	(0.314)	(0.180)	(0.143)	(0.322)
Judgment Lien (6-year)	-0.504**	-0.317	0.181	-0.718***	-0.385**	-0.575*
	(0.241)	(0.258)	(0.362)	(0.203)	(0.169)	(0.349)
Future Bankruptcy	-0.103	0.107	0.189	-0.118	-0.175	0.0745
	(0.179)	(0.181)	(0.252)	(0.131)	(0.139)	(0.217)
Miscellaneous Outcomes						
Mortality (3 year)	-0.095	-0.075	-0.005	-0.102*	-0.071	-0.090
	(0.073)	(0.071)	(0.009)	(0.060)	(0.053)	(0.103)
Mortality (6-year)	-0.025	-0.083	0.046	-0.029	-0.026	-0.001
	(0.085)	(0.073)	(0.069)	(0.075)	(0.066)	(0.121)
Covariates and Year FE	N	N	N	N	N	N

Appendix Table B.6 Chapter 7 and Outcomes Excluding States that Ban Wage Garnishment

This table reports the fuzzy RD estimates of Chapter 7 bankruptcy protection on post-filing investment decisions and financial distress events excluding those filers in Texas, Pennsylvania, South Carolina, and North Carolina where wage garnishment is banned. In the first cutoff the running variable is the difference between the Average Gross Monthly Income (AGMI) and the state median income based on household size. The pooled cutoff combines the second and third cutoffs, as Figure 7 describes. The pooled specifications include thresholds indicator. In the pooled cutoff the running variable is the difference between monthly disposable income and the respective threshold that debtor faces. Local linear regression estimates with rectangular kernel. Each cell represents a separate regression with debtor's ex-post outcome as the dependent variable and the indicator variable of Chapter 7 protection. Home foreclosure is an indicator for a filer's home receiving a notice of default, receiving a notice of transfer or sale, or having been transferred to an REO or a guarantor on or before the indicated year. New Property comprises the acquisition of a new real property by the filer. Judgment Lien is an indicator variable if debtor receives at least one lien. It includes tax liens, hospital liens, and judicial liens. Start a Business is an indicator variable for any business registered in public records by the debtor post-filing. Covariates include age at filing, household size and marital status. Neighborhood is the distance from respective cutoff. Heteroskedasticity-robust standard errors in parentheses. ***, ** and * indicate p-values of 1%, 5%, and 10%, respectively.

	First cutoff				Pooled cutoff			
Running variable	AGMI				Disposable Income			
Neighborhood	5,000	5,000	6,000	6,000	50	50	60	60
Investment decisions								
New real property (3-year)	0.192	0.187	0.168	0.161	0.279***	0.260***	0.233**	0.224***
	(0.233)	(0.229)	(0.247)	(0.244)	(0.097)	(0.089)	(0.093)	(0.086)
New real property (6-year)	0.109	0.099	0.114	0.101	0.278**	0.247**	0.245**	0.225**
	(0.265)	(0.262)	(0.283)	(0.280)	(0.116)	(0.107)	(0.111)	(0.103)
Start a Business (3-year)	0.193**	0.200**	0.207*	0.217**	0.077**	0.080**	0.080**	0.084**
	(0.092)	(0.093)	(0.106)	(0.109)	(0.039)	(0.040)	(0.039)	(0.040)
Start a Business (6-year)	0.175*	0.205*	0.193**	0.223**	0.144*	0.163**	0.148**	0.165**
	(0.097)	(0.112)	(0.101)	(0.113)	(0.076)	(0.082)	(0.075)	(0.082)
Financial Distress Events								
Home foreclosure (3-year)	-0.367	-0.380	-0.322	-0.319	-0.497*	-0.485**	-0.536*	-0.510**
	(0.525)	(0.522)	(0.443)	(0.445)	(0.261)	(0.236)	(0.282)	(0.244)
Home foreclosure (6-year)	-0.756	-0.770	-0.644	-0.646	-0.564*	-0.582**	-0.605**	-0.613**
	(0.579)	(0.579)	(0.481)	(0.485)	(0.289)	(0.277)	(0.309)	(0.304)
Judgment Lien (3-year)	-0.664**	-0.678**	-0.649**	-0.657**	-0.395***	-0.437***	-0.369**	-0.407**
	(0.331)	(0.333)	(0.324)	(0.331)	(0.152)	(0.166)	(0.151)	(0.165)
Judgment Lien (6-year)	-0.749**	-0.728**	-0.728**	-0.693**	-0.562***	-0.555***	-0.551***	-0.545***
	(0.384)	(0.362)	(0.380)	(0.343)	(0.203)	(0.190)	(0.201)	(0.189)
Future Bankruptcy	-0.689***	-0.712***	-0.680***	-0.701***	-0.169	-0.168	-0.172	-0.172
	(0.223)	(0.215)	(0.223)	(0.215)	(0.130)	(0.131)	(0.127)	(0.127)
Specification	Linear	Linear	Linear	Linear	Linear	Linear	Linear	Linear
Covariates and Year FE	N	Y	N	Y	N	Y	N	Y

Appendix Table B.7 Outcomes for Those Who Filed Before and During the Great Recession

This table reports the fuzzy RD estimates of Chapter 7 bankruptcy protection on post-filing investment decisions and financial distress events by cohort. The first cohort comprises the sub-sample of debtors who filed for bankruptcy before the financial crisis (2006-2007). The second cohort comprises debtors who filed during the financial crisis (2008-2009). In the first cutoff the running variable is the difference between the Average Gross Monthly Income (AGMI) and the state median income based on household size. The pooled cutoff combines the second and third cutoffs, as Figure 7 describes. In the pooled cutoff the running variable is the difference between monthly disposable income and the respective threshold that debtor faces. The pooled specifications include thresholds indicator. Local linear regression estimates with a rectangular kernel. Each cell represents a separate regression with debtor's ex-post outcome by cohort as the dependent variable and the indicator variable of Chapter 7 protection. Home foreclosure is an indicator for a filer's home receiving a notice of default, receiving a notice of transfer or sale, or having been transferred to an REO or a guarantor on or before the indicated year. New Property comprises the acquisition of a new real property by the filer. Judgment Lien is an indicator variable if debtor receives at least one lien. It includes tax liens, hospital liens, and judicial liens. Start a Business is an indicator variable for any business registered in public records by the debtor post-filing. Covariates include age at filing, household size and marital status. Neighborhood is the distance from respective cutoff. Heteroskedasticity-robust standard errors in parentheses. ***, ** and * indicate p-values of 1%, 5%, and 10%, respectively.

	First cutoff			Pooled cutoff		
Running variable	AGMI			Disposable Income		
Neighborhood / p-value	6,000		p-value	60		p-value
Investment decisions						
New real property (6-year)	0.098	0.233	0.558	0.228**	0.198**	0.521
	(0.196)	(0.370)		(0.110)	(0.100)	
Start a Business (6-year)	0.196**	0.256*	0.589	0.212*	0.144*	0.225
	(0.099)	(0.158)		(0.110)	(0.075)	
Financial Distress Events						
Home foreclosure (6-year)	-0.318	-0.545	0.617	-0.685*	-0.615*	0.850
	(0.336)	(0.593)		(0.391)	(0.338)	
Judgment Lien (6-year)	-0.666**	-0.660**	0.740	-0.567**	-0.470**	0.630
	(0.335)	(0.331)		(0.232)	(0.194)	
Future Bankruptcy	-0.650***	-0.809**	0.463	-0.126	-0.159	0.794
	(0.188)	(0.318)		(0.153)	(0.117)	
Cohort	2006-2007	2008-2009		2006-2007	2008-2009	
Specification	Linear	Linear		Linear	Linear	
Covariates and Year FE	Y	Y		Y	Y	

Appendix Table B.8 Robustness Test for Discontinuities at Pseudo-Thresholds

The table reports results of the Regression Discontinuity design described in Table 5 at Pseudo-Thresholds. In the first cutoff the pseudo-thresholds are located at \$-1,000 and \$+1,000 away from the real eligibility threshold. In the pooled cutoff the pseudo-thresholds are located at \$-100 and \$+100 away from the real eligibility threshold. Local linear regression estimates with rectangular kernel. Each cell represents a separate regression with debtor's ex-post outcome as the dependent variable and the indicator variable of Chapter 7 protection. Home foreclosure is an indicator for a filer's home receiving a notice of default, receiving a notice of transfer or sale, or having been transferred to an REO or a guarantor on or before the indicated year. New Property comprises the acquisition of a new real property by the filer. Judgment Lien is an indicator variable if debtor receives at least one lien. It includes tax liens, hospital liens, and judicial liens. Start a Business is an indicator variable for any business registered in public records by the debtor post-filing. Covariates include age at filing, household size and marital status. Neighborhood is the distance from respective cutoff. Heteroskedasticity-robust standard errors in parentheses. ***, **, and * indicate p-values of 1%, 5%, and 10%, respectively.

Running variable	First cutoff AGMI		Pooled Cutoff Disposable Income	
Pseudo-Thresholds at	-1,000	+1,000	-100	+100
Investment decisions				
New real property (3-year)	-0.105	-0.208	0.019	0.110
	(0.562)	(0.803)	(0.919)	(0.472)
New real property (6-year)	-0.187	-0.046	0.207	0.190
	(0.734)	(0.917)	(0.645)	(0.660)
Start a Business (3-year)	0.173	0.089	0.028	0.022
	(0.206)	(0.335)	(0.076)	(0.085)
Start a Business (6-year)	0.042	-0.113	0.028	0.055
	(0.232)	(0.381)	(0.080)	(0.162)
Financial Distress Events				
Home foreclosure (3-year)	-0.115	-0.096	-0.152	-0.169
	(0.658)	(0.165)	(0.473)	(0.227)
Home foreclosure (6-year)	-0.151	-0.028	-0.299	-0.036
	(0.788)	(0.176)	(0.662)	(0.603)
Judgment Lien (3-year)	0.002	-0.415	0.273	-0.304
	(0.605)	(0.934)	(0.610)	(0.539)
Judgment Lien (6-year)	-0.059	-0.014	0.295	-0.125
	(0.680)	(0.318)	(0.420)	(0.163)
Future Bankruptcy	0.013	-0.027	0.775	-0.400
	(0.476)	(0.738)	(0.724)	(0.359)
Miscellaneous Outcome				
Mortality (3 year)	0.162	0.007	-0.039	0.035
	(0.147)	(0.237)	(0.059)	(0.451)
Mortality (6-year)	0.099	-0.175	-0.013	0.002
	(0.255)	(0.446)	(0.061)	(0.435)
Specification	Linear			
Covariates and Year FE	Y			

Appendix Table B.9 Chapter 7 and New Homeowners

This table reports the fuzzy RD estimates of Chapter 7 bankruptcy protection on new homeowners. In the first cutoff the running variable is the difference between the Average Gross Monthly Income (AGMI) and the state median income based on household size. The pooled cutoff combines the second and third cutoffs, as Figure 7 describes. In the pooled cutoff the running variable is the difference between monthly disposable income and the respective threshold that debtor faces. The pooled specifications include thresholds indicator. Local linear regression estimates a with rectangular kernel. Each cell represents a separate regression with debtor's ex-post outcome as the dependent variable and the indicator variable of Chapter 7 protection. Covariates include age at filing, household size and marital status. Neighborhood is the distance from respective cutoff. Heteroskedasticity-robust standard errors in parentheses. ***, ** and * indicate p-values of 1%, 5%, and 10%, respectively.

	First Cutoff				Pooled			
Running variable	AGMI				Disposable Income			
Neighborhood	5,000	5,000	6,000	6,000	50	50	60	60
New Homeowners (3-year)	-0.118	-0.132	-0.106	-0.119	0.123**	0.122**	0.112**	0.111**
	(0.159)	(0.154)	(0.159)	(0.154)	(0.056)	(0.055)	(0.053)	(0.052)
New Homeowners (6-year)	-0.068	-0.082	-0.058	-0.072	0.161**	0.162**	0.148**	0.149**
	(0.176)	(0.170)	(0.174)	(0.169)	(0.063)	(0.063)	(0.060)	(0.060)
Specification	Linear	Linear	Linear	Linear	Linear	Linear	Linear	Linear
Covariates and Year FE	N	Y	N	Y	N	Y	N	Y

Appendix Table B.10 Wage Garnishment Regulations and Debtors' Post-Filing Outcomes

This table presents the fuzzy RD estimates of Chapter 7 bankruptcy protection by level of wage garnishment. States with low wage garnishment are those which ban wage garnishment or preserve at least 90 percent of debtors' wages. In the first cutoff the running variable is the difference between the Average Gross Monthly Income (AGMI) and the state median income based on household size. The pooled cutoff combines the second and third cutoffs, as Figure 7 describes. In the pooled cutoff the running variable is the difference between monthly disposable income and the respective threshold that debtor faces. The pooled specifications include thresholds indicator. Local linear regression estimates with rectangular kernel. Each cell represents a separate regression with debtor's ex-post outcome as the dependent variable and the indicator variable of Chapter 7 protection. Home foreclosure is an indicator for a filer's home receiving a notice of default, receiving a notice of transfer or sale, or having been transferred to a REO or a guarantor on or before the indicated year. New property is an indicator variable which takes a value equals one if the filer acquires a new real property. Judgment Lien is an indicator variable if debtor receives at least one lien. It includes tax liens, hospital liens, and judicial liens. Start a Business is an indicator if the filer registers a fictitious business. Covariates include age at filing, household size and marital status. Neighborhood is the distance from respective cutoff. Heteroskedasticity-robust standard errors in parentheses. ***, ** and * indicate p-values of 1%, 5%, and 10%, respectively.

	First cutoff		Pooled cutoff	
Level of wage garnishment	Low	High	Low	High
Neighborhood	6,000	6,000	60	60
Investment Decisions				
New real property (3-year)	0.058	0.062	0.173	0.253**
	(0.487)	(0.259)	(0.147)	(0.123)
New real property (6-year)	0.101	0.200	0.174	0.205*
	(0.610)	(0.313)	(0.179)	(0.120)
Start a Business (3-year)	0.192	0.298**	0.021	0.085*
	(0.168)	(0.137)	(0.047)	(0.048)
Start a Business (6-year)	0.280	0.244*	0.044	0.211*
	(0.206)	(0.148)	(0.050)	(0.123)
Financial Distress Events				
Home foreclosure (3-year)	-0.179	-0.333	-0.180	-0.429***
	(0.696)	(0.559)	(0.315)	(0.166)
Home foreclosure (6-year)	-0.120	-0.613	-0.246	-0.567**
	(0.739)	(0.621)	(0.344)	(0.260)
Judgment Lien (3-year)	-0.119	-0.849**	-0.261	-0.423**
	(0.604)	(0.404)	(0.256)	(0.180)
Judgment Lien (6-year)	-0.599	-0.834*	-0.185	-0.586***
	(0.702)	(0.441)	(0.276)	(0.209)
Future Bankruptcy	-0.472	-0.770***	-0.049	-0.157
	(0.308)	(0.280)	(0.208)	(0.128)
Covariates and Year FE	Y	Y	Y	Y

Appendix Table B.11 Chapter 7 and Harassment

This table presents the fuzzy RD estimates of Chapter 7 bankruptcy protection and debtors post-filing outcomes. Local linear regression estimates with a rectangular kernel. Each cell represents a separate regression with debtor's ex-post outcome as the dependent variable and the indicator variable of Chapter 7 protection. Covariates include age at filing, household size and marital status. Heteroskedasticity-robust standard errors in parentheses. ***, ** and * indicate p-values of 1%, 5%, and 10%, respectively.

Running Variable	First cutoff AGMI	Pooled cutoff Disposable Income
Neighborhood	6,000	60
Total phone numbers	-1.112	-0.760
	(1.141)	(1.066)
Total number of addresses	-1.652	-0.654
	(1.595)	(0.732)
Covariates and Year FE	Y	Y

Appendix Table B.12 Chapter 7 and Other Miscellaneous Outcomes

This table presents the fuzzy RD estimates of Chapter 7 bankruptcy protection and debtors post-filing miscellaneous outcomes. Local linear regression estimates with a rectangular kernel. Each cell represents a separate regression with debtor's ex-post outcome as the dependent variable and the indicator variable of Chapter 7 protection. Covariates include age at filing, household size and marital status. Heteroskedasticity-robust standard errors in parentheses. ***, ** and * indicate p-values of 1%, 5%, and 10%, respectively.

Running Variable	First cutoff AGMI	Pooled cutoff Disposable Income
Neighborhood	6,000	60
Criminal Records	0.155	-0.134
	(0.225)	(0.121)
Same Zip-Code	0.065	0.195
	(0.408)	(0.198)
Divorced	0.028	0.158
	(0.085)	(0.127)
Covariates and Year FE	Y	Y

Appendix Table B.13 Compliance by Marital Status and Age

This table presents characteristics of compliers, following Angrist and Pischke (2008). Column 1 reports the distribution of the full sample by marital status and age, $P(X = x)$. Column 2 shows the first-stage estimates for each marital status and age group. Column 3 reports the distribution of compliers by marital status and age, $P(X = x \mid I_1 > I_0)$, calculated as (first-stage estimate for the marital status–age group divided by the overall first-stage estimate. Column 4 shows the relative likelihood of a filer belonging to a particular marital status–age group, in the complier group compared to the full sample.

Panel A: First cutoff	$P(X = x)$	First Stage	$P(X = x \mid I_1 > I_0)$	$\frac{P(X=x \mid I_1>I_0)}{P(X=x)}$
Married				
Age at Filing <=40	0.214	0.017	0.047	0.219
Age at Filing >40	0.271	0.079	0.272	1.003
Not Married				
Age at Filing <=40	0.210	0.078	0.207	0.986
Age at Filing >40	0.304	0.123	0.474	1.557

Panel B: Pooled cutoff	$P(X = x)$	First Stage	$P(X = x \mid I_1 > I_0)$	$\frac{P(X=x \mid I_1>I_0)}{P(X=x)}$
Married				
Age at Filing <=40	0.205	0.082	0.042	0.207
Age at Filing >40	0.343	0.518	0.448	1.308
Not Married				
Age at Filing <=40	0.142	0.508	0.182	1.283
Age at Filing >40	0.311	0.417	0.327	1.053

Appendix Table B.14 Jackpot Winners Characteristics

The U.S. lotteries jackpot winners dataset comes from different public sources. Prize amounts are after-taxes and in 2013 Dollars.

Mega Millions	Mean	\$46,513,192
	25th percentile	\$17,604,672
	75th percentile	\$67,939,096
Powerball	Mean	\$45,730,912
	25th percentile	\$14,554,040
	75th percentile	\$66,335,532
Year of winning		
	2002	7
	2003	28
	2004	21
	2005	23
	2006	22
	2007	32
	2008	22
	2009	29
	2010	30
	2011	29
	2012	31
	2013	10

Appendix Table B.15 Effect of Jackpot Winners' Shock on Deposits at the CBSA level

Data are from the SOD, 1999-2013. An observation is a CBSA by year cell. The dependent variable equals the Log Deposits at the CBSA level. Winner is an indicator equal to one in those CBSA/year with Jackpot winners, that chooses the cash option and resides in the state where the ticket winning was bought, and zero otherwise. Log Prize is the amount won after withheld federal and state taxes in 1999 dollars. Prize/deposits equals the ratio of the amount won in CBSA i and year t, and deposits in CBSA i and year before treatment. Non-cash Winner equal to 1 if there is a jackpot winner but the prize was unclaimed or the winner choose the annuity option and zero otherwise. Winner out-of-state equal one if the winner lives in a state different where the winning tickets was sold and zero otherwise. Winner (t+1) is a lead variable equal to 1 a year before the shock, in those CBSA with a winner at t, and zero otherwise. All specifications include lag of log(population), CSBA and year FE. Robust standard errors in parentheses, clustered at the CBSA level. ***, **, and * indicate p-values of 1%, 5%, and 10%, respectively.

Panel A	Log Deposits		
	(1)	(2)	(3)
Winner	0.0315***		
	(0.0117)		
Log Prize		0.00384***	
		(0.414)	
Prize / Deposit			0.844**
			(0.413)
Year FE	Yes	Yes	Yes
CBSA FE	Yes	Yes	Yes
Additional controls	Yes	Yes	Yes
Observations	11,802	12,588	11,802

Appendix Table B.16 Effect of Jackpot Winners' Shock at the CBSA-level

Data are from the SOD, 1999-2013, and FFIEC, 1999-2012. An observation is a CBSA by year cell. The dependent variable equals the Log Deposits at the CBSA level, log of the total small business loan originations, defined as loans under $1 million, and log small business loans originated with gross annual revenues < 1 million at the CBSA level. Non-cash Winner equal to 1 if there is a jackpot winner but the prize was unclaimed or the winner choose the annuity option and zero otherwise. Winner out-of-state equal one if the winner lives in a state different where the winning tickets was sold and zero otherwise. All specifications include lag CBSA characteristics (log(population), % white, % male, % over age 45, income per-capita), CSBA and year FE. Robust standard errors in parentheses, clustered at the CBSA level. ***, **, and * indicate p-values of 1%, 5%, and 10%, respectively.

	Log Deposits		Log Small Business Loans Originated with Gross Annual Revenues < 1		Log Total Amount of Small Business Loans	
	(1)	(2)	(3)	(4)	(5)	(6)
Non-cash Winner	-0.0062		-0.0143		0.0268	
	(0.0369)		(0.0460)		(0.0484)	
Winner out-of-state		-0.00884		-0.00568		-0.0270
		(0.0304)		(0.0321)		(0.0204)
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
CBSA FE	Yes	Yes	Yes	Yes	Yes	Yes
Additional controls	Yes	Yes	Yes	Yes	Yes	Yes
Observations	11,802	11,802	10,916	10,912	10,916	10,912

Appendix Table B.17 Effect of Winners' Shock at the CBSA-level sorted by population

Data are from the SOD, 1999-2013. An observation is a CBSA by year cell. The dependent variable equals the Log Deposits at the CBSA level., log of the total small business loan origination, defined as loans under $1 million, log of small business loan originations to businesses with $1 million in annual gross revenue or less at the bank level Winner is an indicator equal to one in those CBSA/year with Jackpot winners, that chooses the cash option and resides in the state where the ticket winning was bought, and zero otherwise. Log Prize is the amount won after withheld federal and state taxes in 1999 dollars. Prize/deposits equals the ratio of the amount won in CBSA i and year t, and deposits in CBSA i and year before treatment. To save space, each cell represents the coefficient of interest of a different regression. All specifications include lag CBSA characteristics (log(population), % white, % male, % over age 45, income per-capita), CSBA and year FE. Robust standard errors in parentheses, clustered at the CBSA level. ***, **, and * indicate p-values of 1%, 5%, and 10%, respectively.

	Log Deposits		Log Small Business Loans with Annual Revenues $<$ 1		Log Total Amount of Small Business Loans	
	(1)	(2)	(3)	(4)	(5)	(6)
Winner	0.0205*	-0.0066	0.0534**	-0.0162	0.0387*	0.0011
	(0.0107)	(0.0144)	(0.0270)	(0.0198)	(0.0225)	(0.0153)
Log Prize	0.00197*	-0.0005	0.00507*	-0.00206	0.0034*	-0.00015
	(0.0011)	(0.0014)	(0.0027)	(0.0020)	(0.0020)	(0.0015)
Prize / Deposit	0.585*	1.119	1.699*	-4.049	0.991	-1.422
	(0.3520)	(2.950)	(0.896)	(6.400)	(0.777)	(6.219)
Sample	Pop $<$ 500	Pop $>$ 500	Pop $<$ 500	Pop $>$ 500	Pop $<$ 500	Pop $>$ 500
Year FE				Yes		
CBSA FE				Yes		
Additional controls				Yes		

Appendix Table B.18 Effect of Winners' shock on Small Business Loan Originations

Data are from the FFIEC, 1999-2012. An observation is a CBSA by year cell. The dependent variable equals the log of the total small business loan originations, defined as loans under $1 million, and log small business loans originated with gross annual revenues < 1 million at the CBSA level. Winner is an indicator equal to one in those CBSA/year with Jackpot winners, that chooses the cash option and resides in the state where the ticket winning was bought, and zero otherwise. Small bank equals one if the CBSA is in the 5th quintile of the yearly distribution of the ratio of the number of branches from banks with assets lower or equal to $1 billion (in 1999 dollars) to the total number of branches in each CBSA, and zero otherwise. All specifications include lag CBSA characteristics (log(population), % white, % male, % over age 45), CSBA and year FE. Robust standard errors in parentheses, clustered at the CBSA level. ***, **, and * indicate p-values of 1%, 5%, and 10%, respectively.

	Log Small Business Loans Originated with Gross Annual Revenues < 1		Log Total Loan Amount of Small Business Loans	
	(1)	(2)	(3)	(4)
Winner	0.0417**	0.0460**	0.0365**	0.0382**
	(0.0186)	(0.0187)	(0.0150)	(0.0151)
Small bank ($2 Billion)	-0.0363		-0.0272	
	(0.0292)		(0.0235)	
Winner x Small bank ($2 Billion)	0.131***		0.0679***	
	(0.0252)		(0.0204)	
Small bank ($1 Billion)		-0.131***		-0.112***
		(0.0310)		(0.0256)
Winner x Small bank ($1 Billion)		0.154***		0.0905***
		(0.0230)		(0.0188)
Year FE	Yes	Yes	Yes	Yes
CBSA FE	Yes	Yes	Yes	Yes
Additional controls	Yes	Yes	Yes	Yes
Observations	10,912	10,912	10,912	10,912

Appendix Table B.19 Effect of Winners' Shock on Loan Originations at the CBSA-level

Data are from the FFIEC, 1999-2012. An observation is a CBSA by year cell. The dependent variable equals the log of the total small business loan originations, defined as loans under $1 million, and log small business loans originated with gross annual revenues $<$ 1 million at the CBSA level. Winner is an indicator equal to one in those CBSA/year with Jackpot winners, that chooses the cash option and resides in the state where the ticket winning was bought, and zero otherwise. Small bank equals one if the CBSA is in the 5th quintile of the yearly distribution of the ratio of the number of branches from banks with assets lower or equal to $1 billion (in 1999 dollars) to the total number of branches in each CBSA, and zero otherwise. Winner (t+3) is a lead variable equal to 1 a three years before the shock, in those CBSA with a winner at t, and zero otherwise. All specifications include lag CBSA characteristics (log(population), % white, % male, % over age 45, income per-capita), CSBA and year FE. Robust standard errors in parentheses, clustered at the CBSA level. ***, **, and * indicate p-values of 1%, 5%, and 10%, respectively.

	Log Small Business Loans Originated with Gross Annual Revenues < 1		Log Total Amount of Small Business Loans	
	(1)	(2)	(3)	(4)
Winner (-3y, -2y)	-0.00621	-0.00845	-0.00178	-0.00231
	(0.0207)	(0.0205)	(0.0168)	(0.0167)
Small bank (2000) (-3y, -2y)	-0.0385		-0.0302	
	(0.0290)		(0.0231)	
Winner x Small bank (2000) (-3y, -2y)	-0.391		-0.112	
	(0.371)		(0.288)	
Small bank (1000) (-3y, -2y)		-0.130***		-0.112***
		(0.0309)		(0.0256)
Winner x Small bank (1000) (-3y, -2y)		-0.245		-0.0531
		(0.421)		(0.282)
Year FE	Yes	Yes	Yes	Yes
CBSA FE	Yes	Yes	Yes	Yes
Additional controls	Yes	Yes	Yes	Yes
Observations	10,912	10,912	10,912	10,912

Appendix Table B.20 Effect of Winners' Shock on Outcome Variables at the County-Level

The dependent variable equals the Log Deposits at the county level, and log of the total small business loan originations, defined as loans under $1 million, and log small business loans originated with gross annual revenues $<$ 1 million at the county level. Prize/deposits (0m, 3m) equals the ratio, on the quarter of treatment, of the prize deposit in bank i in year t to and deposits in banks i and quarter before treatment. Prize/deposits (0y, 1y) is the same ratio but takes the value of the ratio in the year after treatment for the banks in the treatment group (0y, 1y), and zero otherwise. Prize/deposits($t+3$) (-3y, 2y) is the same ratio but takes the value of the ratio in the 3 years to 2 years before the shock. All specifications include lag county characteristics (log(population), % white, % male, % over age 45, income per-capita), CSBA x year FE. Robust standard errors in parentheses, clustered at the county level. ***, **, and * indicate p-values of 1%, 5%, and 10%, respectively..

	△ Log Deposits		Log Small Business Loans Originated with Gross Annual Revenues $<$ 1		Log Total Amount of Small Business Loans	
	(1)	(2)	(3)	(4)	(5)	(6)
Prize/Deposit (0y, 1y)	0.324**		0.429		0.463	
	(0.159)		(1.512)		(1.498)	
Prize/Deposit (-3y, -2y)		-0.0811		0.199		0.481
		(0.129)		(1.443)		(1.326)
CBSA-Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Additional controls	Yes	Yes	Yes	Yes	Yes	Yes
Observations	16,981	16,979	14,334	12,282	14,336	12,284

Appendix Table B.21 Collateral Channel and Credit Supply

Data are from the FFIEC, 1999-2012. An observation is a MSA by year cell. The dependent variable equals the log of the total small business loan originations, defined as loans under $1 million, and log small business loans originated with gross annual revenues < 1 million at the MSA level. Winner is an indicator equal to one in those MSA/year with Jackpot winners, that chooses the cash option and resides in the state where the ticket winning was bought, and zero otherwise. Winner*Saiz Elasticity is the interaction variable between winner indicator and housing supply elasticity using data from Saiz (2010). All specifications include lag MSA characteristics (log(population), % white, % male, % over age 45, income per-capita), CSBA and year FE. Robust standard errors in parentheses, clustered at the MSA level. ***, **, and * indicate p-values of 1%, 5%, and 10%, respectively.

	Log Small Business Loans Originated with Gross Annual Revenues < 1			Log Total Amount of Small Business Loans		
	(1)	(2)	(3)	(4)	(5)	(6)
Winner	0.0385			0.0317		
	(0.0266)			(0.0200)		
Winner*Saiz Elasticity	-0.0383			-0.0394		
	(0.0368)			(0.0283)		
Log Prize		0.00392			0.00323*	
		(0.00259)			(0.00193)	
Log Prize*Saiz Elasticity		-0.00362			-0.00381	
		(0.00354)			(0.00270)	
Prize / Deposit			2.177**			-0.731
			(1.001)			(1.162)
Prize / Deposit*Saiz Elasticity			-1.989			-1.682
			(3.006)			(2.381)
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
CBSA FE	Yes	Yes	Yes	Yes	Yes	Yes
Additional controls	Yes	Yes	Yes	Yes	Yes	Yes
Observations	3,625	3,709	3,709	3,625	3,709	3,709

Appendix Table B.22 Bank Size and Credit Supply

Data are from the Call Report, 1999-2013. An observation is a bank by quarter cell. The dependent variable equals the Log Deposits at the banks level, and Log total small business loans. Prize/deposits equals the ratio, on the year of treatment (0m, 12m), of the prize deposit in bank i in quarter t to and deposits in banks i and quarter before treatment. Bank controls include the lag of log(assets), ROA, and Equity/Assets. All specifications include bank and quarter FE. Robust standard errors in parentheses, clustered at the bank level. ***, **, and * indicate p-values of 1%, 5%, and 10%, respectively..

Panel A	Size			
	Log Total Loans			
	(1)	(2)	(3)	(4)
Prize / Deposit	0.190***	-0.0678	0.273	6.333
	(0.0301)	(0.283)	(0.463)	(5.732)
Percentile	Below and equal 75th	Between 71th and 95th	Between 96th and 99th	Above 99th
Quarter FE	Yes	Yes	Yes	Yes
Bank FE	Yes	Yes	Yes	Yes
Additional controls	Yes	Yes	Yes	Yes
Observations	333,550	84,743	14,777	3,051

Panel B	Size			
	Log Total Small Business Loans			
	(1)	(2)	(3)	(4)
Prize / Deposit	0.307***	0.0804	0.827	9.956
	(0.112)	(0.276)	(1.089)	(14.32)
Percentile	Below and equal 75th	Between 71th and 95th	Between 96th and 99th	Above 99th
Quarter FE	Yes	Yes	Yes	Yes
Bank FE	Yes	Yes	Yes	Yes
Additional controls	Yes	Yes	Yes	Yes
Observations	327,949	82,589	14,026	2,665

Appendix Table B.23 Other attributes and Credit Supply

Data are from the Call Report, 1999-2013. An observation is a bank by quarter cell. The dependent variable equals the Log Deposits at the banks level, and Log total small business loans. Prize/deposits equals the ratio, on the year of treatment (0m, 12m), of the prize deposit in bank i in quarter t to and deposits in banks i and quarter before treatment. Bank controls include the lag of log(assets), ROA, and Equity/Assets. All specifications include bank and quarter FE. Robust standard errors in parentheses, clustered at the bank level. ***, **, and * indicate p-values of 1%, 5%, and 10%, respectively.

Panel A	Log Total Loans					
	(1)	(2)	(3)	(4)	(5)	(6)
Prize / Deposit	0.138***	-0.138	0.121***	0.106	0.148***	0.0162
	(0.0307)	(0.379)	(0.0362)	(0.315)	(0.0321)	(0.278)
Attribute	Number of Branches		Number of CBSAs		Number of States	
Percentile	Below Median	Above Median	Below Median	Above Median	Below Median	Above Median
Quarter FE	Yes	Yes	Yes	Yes	Yes	Yes
Bank FE	Yes	Yes	Yes	Yes	Yes	Yes
Additional controls	Yes	Yes	Yes	Yes	Yes	Yes
Observations	251,467	194,617	343,050	103,034	427,229	23,961

Panel B	Log Total Small Business Loans					
	(1)	(2)	(3)	(4)	(5)	(6)
Prize / Deposit	0.180	0.191	0.180	0.697	0.246*	0.369
	(0.140)	(0.387)	(0.151)	(0.591)	(0.134)	(0.485)
Attribute	Number of Branches		Number of CBSAs		Number of States	
Percentile	Below Median	Above Median	Below Median	Above Median	Below Median	Above Median
Quarter FE	Yes	Yes	Yes	Yes	Yes	Yes
Bank FE	Yes	Yes	Yes	Yes	Yes	Yes
Additional controls	Yes	Yes	Yes	Yes	Yes	Yes
Observations	244,684	192,081	335,363	101,402	418,093	23,603

Appendix Table B.24 Effect of Jackpot Winners' Shock on Total Securities at Bank-level

Data are from the Call Report, 1999-2013. An observation is a bank by quarter cell. The dependent variable equals the Log Total Securities. Prize/deposits (0m, 3m) equals the ratio, on the quarter of treatment, of the prize deposit in bank i in quarter t to and deposits in banks i and quarter before treatment. Prize/deposits (0m, 12m) equals the ratio, on the year of treatment, of the prize deposit in bank i in quarter t to and deposits in banks i and quarter before treatment. Bank controls include the lag of log(assets), ROA, and Equity/Assets. All specifications include bank and quarter FE. Robust standard errors in parentheses, clustered at the bank level. ***, ** and * indicate p-values of 1%, 5%, and 10%, respectively.

	Log Total Securities	
	(1)	(2)
Prize / Deposit (0m, 3m)	-0.00313	
	(0.256)	
Prize / Deposit (0m, 12m)		0.279
		(0.297)
Quarter FE	Yes	Yes
Bank FE	Yes	Yes
Additional controls	Yes	Yes
Observations	443,545	441,049

Appendix Table B.25 Effect of Winners' Shock on Small Business Loans at the Bank-level

Data are from the Call Report, 1999-2013. An observation is a bank by quarter cell. The dependent variable equals the log of total small business loans at the bank level. Prize/deposits (0 m, 12 m) equals the ratio of the prize deposit in bank i in quarter t and deposits in banks i and quarter before treatment in the 12 months after treatment for the treated banks (0 m, 12 m), and zero otherwise. Prize/deposits(-12 m, 0 m) is a lead variable equal to the previous ratio in the 12 months before the shock (-12 m, 0 m), for the banks in the treatment group, and zero otherwise. Bank controls include the lag of ln(assets), ROA, and Equity/Assets. All specifications include bank and quarter FE. Robust standard errors in parentheses, clustered at the bank level. ***, ** and * indicate p-values of 1%, 5%, and 10%, respectively.

	Log Total Small Business Loans	
	(1)	(2)
Prize / Deposit (0m, 12m)	0.242*	
	(0.133)	
Prize / Deposit (-12m, 0m)		-0.160
		(0.282)
Quarter FE	Yes	Yes
Bank FE	Yes	Yes
Additional controls	Yes	Yes
Observations	438,922	441,696

Appendix Table B.26 2SLS of the Smal Business Loans and Deposit Supply at the Bank-level

Data are from the FFIEC and SOD, 1999-2012. An observation is a bank by year cell. The dependent variable is the log of the total small business loan origination. Prize/deposits equals the ratio, on the year of treatment, of the prize deposit in bank i in year t to deposits in banks i and year before treatment. Bank controls include the lag of ln(assets), ROA, and Equity/Assets, and the number of branches bank i in CBSA j in year $t-1$. All specifications include CBSA by year FE and bank FE. Robust standard errors in parentheses, clustered at the bank level. ***, **, and * indicate p-values of 1%, 5%, and 10%, respectively.

	First Stage	OLS	RF	2SLS
	Log Deposits	Log Total Amount of Small Business Loans		
	(1)	(2)	(3)	(4)
Log Deposits		0.301***		0.872**
		(0.0300)		(0.405)
Prize / Deposit	3.471***		3.027**	
	(1.070)		(1.194)	
First Stage F-stat	10.52			
CBSA x Year FE	Yes	Yes	Yes	Yes
Bank FE	Yes	Yes	Yes	Yes
Additional controls	Yes	Yes	Yes	Yes
Observations	75,486	75,486	75,486	75,486

Appendix C

Call Report

I compile a data set with quarterly balance and income statement information for all reporting banks over the period 1999 through 2013. I exclude all the bank-quarters with missing information on total assets, total loans, or liquid funds. I exclude the acquiring bank data from the quarters before and after a merger using bank mergers data from the Federal Reserve Bank of Chicago. To make sure that outliers are not driving the results, I eliminate all bank-quarters with asset growth over the last quarter in excess of 60%, those with total loan growth exceeding 150%, and those with total loans-to-asset ratios below 10%. In the regression analysis of Small Business (SB) lending, I omit all banks that have less than 5% of their loan portfolio in SB to avoid distortions from banks that do only negligible amounts of SB lending.

To construct the variables, I follow the "Notes on forming consistent time series" from the Federal Reserve Bank of Chicago:

Total Assets: is item RCFD2170.

Total Securities: are items RCFD 1754 and RCFD 1773.

Total Loans and Leases: is item RCFD1400.

Small business lending: are items RCFD1766, RCFD1590 and RCON1480. [1]

Total Deposits: is item RCFD2200.

Nonperforming Total Loans: are items RCFD1403 and RCFD1407.

Interest and Fee Income from Loans: is item RIAD4010.

[1] Item RCFD1600 (Commercial and Industrial Loans) is no longer reported after 2000, thus I used RCFD1766 (Commercial and Industrial Loans - Other).

Appendix D

Individual Detection Algorithm

Given that the SOD provides deposit data for each branch, I estimate the amount received by each jackpot winner. Therefore, on principle, I can identify the possible branch and bank that have received the prize. The assumptions of the procedure are that 1) the winner places deposits in his or her respective CBSA, and 2) the winner deposits in those branches that are closest in driving distance to where the ticket was bought.[1] The results in Table 2.2 support the first assumption, and the second assumption is plausible from conversations with state lottery representatives. In each CBSA/year where there was a non-group winner, i.e., prizes that were claimed by a single person, I do the following procedure.[2] First, I estimate the fitted value in deposit year t for each branch.[3] Then, I estimate the difference between the realized deposits at year t and the predicted deposit for each branch. Subsequently, I create an interval of the prize claimed in the CBSA to estimate the possible change in deposits for that branch. Then, I check for each branch in the CBSA that experienced a change in deposits in the interval of the prize. Next, for those match branches from the last step, I focus on those for which the growth in deposits in year t was

[1] Only the city of residence for each winner is available, not their specific address. However, usually the winner buys their tickets close to their place of residence.

[2] See footnote 38.

[3] Since the SOD has data since 1994, I can estimate the fitted value for each branch.

their maximum experienced since 1994. The idea is to focus on those branches that experienced a shock in their deposits. Finally, since I have data on the location of each deposit branch along with the zip codes of the retailers who sold the winning tickets, I am able to estimate the driving distance in time from the lottery ticket outlet to the bank of deposit. I focus on those branches that are close in driving distance within the same CBSA (up to a maximum of 45 min). In those cases, in which there are multiple branches that could have received the prize, I select the three closest branches to the address of the retailer.

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