



# Essays in Consumer and Corporate Finance

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# Essays in Consumer and Corporate Finance

A dissertation presented

by

Paul Goldsmith-Pinkham

to

The Department of Economics

in partial fulfillment of the requirements

for the degree of

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in the subject of

Business Economics

Harvard University

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## **Essays in Consumer and Corporate Finance**

### **Abstract**

The tension between creditors and debtors is an integral component in finance. My dissertation focuses on two important cases where this tension has important economic implications. In my first two chapters, I focus on debtor protections in consumer finance. In chapter one, I examine the institution of consumer bankruptcy and the effect it has on consumers' access to credit and subsequent financial health. In chapter two, I study the effect of debtor protections during the recent recession, and quantify the extent to which these policies can alleviate the decline associated with debt-driven recessions. Finally, in the third chapter, I focus on the governance of firms, specifically examining a new measure capturing the extent to which foreign firms cross-listing in the United States bind to domestic governance rules. In sum, my dissertation chapters provide new perspectives on the interaction between creditors and debtors, and the extent to which policy environments can influence this interaction.

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To my parents.

# Introduction

The tension between creditors and debtors is an integral component in finance. My dissertation focuses on two important cases where this tension has important economic implications. In my first two chapters, I focus on debtor protections in consumer finance. In chapter one, I examine the institution of consumer bankruptcy and the effect it has on consumers' access to credit and subsequent financial health. In chapter two, I study the effect of debtor protections during the recent recession, and quantify the extent to which these policies can alleviate the decline associated with debt-driven recessions. Finally, in the third chapter, I focus on the governance of firms, specifically examining a new measure capturing the extent to which foreign firms cross-listing in the United States bind to domestic governance rules. In sum, my dissertation chapters provide new perspectives on the interaction between creditors and debtors, and the extent to which policy environments can influence this interaction.

In chapter one, "Consumer Bankruptcy and Financial Health," (with Will Dobbie and Crystal Yang) I use random assignment of consumer bankruptcy judges to assess the causal impact of bankruptcy on financial outcomes. Despite the significant levels of consumer bankruptcy in the U.S. economy, little is known of whether bankruptcy protection reduces financial distress. I use a new dataset linking bankruptcy filings to credit bureau records to estimate the impact of Chapter 13 bankruptcy protection on post-filing financial outcomes. We find that Chapter 13 protection is largely successful in alleviating the most direct adverse consequences of excessive debt. Over the first five post-filing years, Chapter 13 protection significantly decreases measures related to adverse financial events and increases individuals'

access to credit, as measured by credit line utilization and credit score. Taken together, the results suggest that for the group most affected by the random assignment, consumer bankruptcy has substantial post-filing benefits.

In chapter two, “Debtor Protections and the Great Recession,” (with Will Dobbie) I use 1.2 million individual credit reports and exogenous variation in state laws to assess the impact of debtor protections on household balance sheets and the macroeconomy during the Great Recession. I study bankruptcy homestead exemptions and non-recourse mortgage protections, which respectively protect homeowners from unsecured and secured creditors. At the individual level, I find that both policies helped homeowners reduce their debt between 2008 and 2010. However, while bankruptcy homestead protections raised regional consumption and employment, non-recourse protections lowered both. These contrasting aggregate results can be explained by non-recourse laws exacerbating house price declines through an increase in foreclosures. I find no similar spillover effects on house prices from bankruptcy homestead exemptions.

In chapter three, “Opting out of good governance,” (with C. Fritz Foley, Jonathan Greenstein and Eric Zwick) I use hand-collected data to show that 80% of cross-listed foreign firms opt out of at least one U.S. exchange governance rule, instead committing to observe the rules of their home country. I show that, relative to firms that comply, firms that opt out have weaker governance practices, as reflected by the composition of their board. I also show that the decision to opt out reflects the relative costs and benefits of doing so, with cross-listed firms opting out more when coming from countries with weak corporate governance rules. However, if firms based in such countries are growing and have a need for external finance, they are more likely to comply. Finally, I estimate that opting out affects the value of cash holdings. For cross-listed firms based in countries with weak governance rules, a dollar of cash held inside the firm is worth \$1.52 if the firm fully complies with U.S. exchange rules but just \$0.32 if it is non-compliant.

## Chapter 1

# Consumer Bankruptcy and Financial Health<sup>1</sup>

America is a nation of debtors. The amount of debt held by the average American consumer increased from \$31,840 to \$45,500 between 2003 and 2013, with more than 14 percent of consumers having at least one debt in collections by 2013 (Federal Reserve Bank of New York 2014). Theoretical work has long suggested that excessive debt and financial distress can distort repayment, consumption, and labor supply decisions (e.g. Myers 1977, Krugman 1988). For example, borrowers with excessive debt have an incentive to avoid repayment through strategies with significant deadweight costs, such as leaving the formal banking system to avoid seizure of assets or leaving the formal labor market to avoid wage garnishment. Consistent with this literature, recent empirical work shows that debt overhang reduces labor supply (Dobbie and Song 2015) and increases mortgage default rates (Melzer 2012).<sup>2</sup> Perhaps the most important program meant to alleviate the adverse consequences of financial distress is the consumer bankruptcy system, the legal process to

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<sup>1</sup>Co-authored with Will Dobbie and Crystal Yang

<sup>2</sup>There is also evidence that financial distress imposes negative externalities on nearby individuals. For example, home foreclosures can reduce nearby home values (e.g. Campbell, Giglio, and Pathak 2011, Mian, Sufi, and Trebbi 2011) and consumer debt overhang can depress regional consumption and employment (e.g. Guerrieri and Lorenzoni 2011, Hall 2011, Midrigan and Philippon 2011, Eggertsson and Krugman 2012, Farhi and Werning 2013, Mian, Rao, and Sufi 2013, Mian and Sufi forthcoming).

resolve unpaid debts in the United States.

The consumer bankruptcy system allows debtors to choose between Chapter 7 bankruptcy, which provides debt relief and protection from wage garnishment in exchange for a debtor's non-exempt assets, and Chapter 13 bankruptcy, which adds the protection of most assets in exchange for a partial repayment of debt. Each year, more than one million Americans file for bankruptcy protection, with nearly one in ten American households having filed for bankruptcy at some point in their lifetimes (Stavins 2000). In terms of financial distress, bankruptcy filers are nearly two and half times as likely to have a delinquent debt before filing compared to the typical credit user, twice as likely to have a debt in collections, and three times as likely to have a lien or repossession. Even after filing, bankruptcy filers are much more likely to experience financial distress than otherwise similar individuals.<sup>3</sup>

Despite over one percent of American households filing for bankruptcy each year, little is known about whether bankruptcy protection reduces or exacerbates financial distress. In theory, bankruptcy protection benefits debtors directly by improving their balance sheets and preventing the seizure of important assets such as a home or car. These direct benefits may in turn indirectly benefit debtors by increasing their credit score or access to credit. Yet, in practice, there is little empirical evidence that bankruptcy protection provides any economically significant benefits to debtors. Cross-sectional comparisons suggest that bankruptcy filers work about the same number of hours and accumulate less wealth than non-filers (Han and Li 2007, 2011), and within-individual comparisons show that filers have less access to credit after receiving bankruptcy protection (Cohen-Cole, Duygan-Bump and Montoriol-Garriga 2013, Jagtiani and Li 2014). However, much of this prior work has been hampered by the lack of a plausible comparison group. Bankruptcy filers likely had worse outcomes even before filing, biasing cross-sectional estimates, and the most commonly reported causes of filing, such as job loss, also impact later outcomes, biasing within-individual estimates.<sup>4</sup>

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<sup>3</sup>See Table 1.3 for details and additional summary statistics.

<sup>4</sup>The most commonly reported causes of bankruptcy are unexpected income or expense shocks. Sullivan, Warren, and Westbrook (2000) find that 67.5 percent of bankruptcy filers report job loss as a factor in filing



This paper uses a new dataset linking bankruptcy filings to credit bureau records to estimate the impact of Chapter 13 bankruptcy protection on post-filing financial outcomes. We estimate the ex-post causal effect of Chapter 13 protection by comparing the outcomes of filers randomly assigned to bankruptcy judges with different propensities to grant bankruptcy protection. The identified parameter measures the treatment effect for filers whose bankruptcy decision is altered by the judge assignment due to disagreement on whether or not they should receive bankruptcy protection (i.e. the marginal recipients of bankruptcy protection). The estimates hold fixed any independent effects of bankruptcy filing, such as having a bankruptcy flag on a credit report (Han, Keys, and Li 2013), and any ex-ante impacts of bankruptcy, such as over-borrowing, moral hazard in the workplace (White 2011), entrepreneurial risk-taking (Fan and White 2003, Armour and Cumming 2008), or the crowding out of formal insurance (Mahoney 2015). Using the same identification strategy, Dobbie and Song (2015) find that Chapter 13 protection increases earnings and decreases mortality risk.<sup>5</sup>

There are three main contributions of this paper relative to Dobbie and Song (2015). First, we estimate the effect of Chapter 13 protection on a broad range of financial outcomes that shed new light on the well-being of debtors. We employ a new dataset constructed for the purposes of this study that links over 175,000 bankruptcy filings to credit bureau records. These data allow us to examine the effects of consumer bankruptcy on post-filing adverse financial events, unsecured debt, secured asset holdings, credit access, and credit score. Moreover, because we observe detailed information on distressed borrowers both before and after bankruptcy, we are able to provide new evidence on the long-term consequences

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for bankruptcy, 22.1 percent report family issues such as divorce, and 19.3 percent report medical expenses, with subsequent work suggesting a somewhat larger role for medical expenses (Domowitz and Sartain 1999, Warren, Sullivan, and Jacoby 2000, Himmelstein et al. 2009). Using data from the PSID, Fay, Hurst, and White (2002) find that households are also more likely to file for bankruptcy protection when there are larger financial benefits to doing so.

<sup>5</sup>Kling (2006) uses a similar empirical strategy to estimate the ex-post impact of sentence length on earnings, and subsequent papers have used similar methodologies to estimate the ex-post effects of foster care (Doyle 2007, 2008), juvenile incarceration (Aizer and Doyle forthcoming), corporate bankruptcy (Chang and Schoar 2008), temporary-help employment (Autor and Houseman 2010), and Disability Insurance (Maestas, Mullen, and Strand 2013, French and Song 2014).

of excessive debt and the extent to which bankruptcy protection mitigates these adverse consequences. Second, we describe the characteristics of filers who are more likely to be affected by judge assignment. We find that young filers are more likely to be affected by a lenient judge assignment, but that there are no differences by baseline credit score or homeownership status. These results provide new evidence on the types of cases for which the instrumental variables estimates are most likely to apply, and the types of filers who are most likely to be affected by changes in bankruptcy laws. Finally, we estimate a variety of non-experimental specifications that allows us to reconcile our estimates with a literature finding negative impacts of bankruptcy protection on post-filing finances (e.g. Han and Li 2007, 2011, Cohen-Cole, Duygan-Bump and Montoriol-Garriga 2013, Jagtiani and Li 2014).

In our empirical analysis, we find that Chapter 13 protection is largely successful in alleviating the most direct adverse consequences of excessive debt. Over the first five post-filing years, Chapter 13 protection decreases an index measuring adverse financial events such as civil judgment and repossession by 0.316 standard deviations, and significantly decreases seven of the eight individual measures of financial strain that compose the index. Chapter 13 protection has little impact on the amount of open unsecured debt, but the amount of debt in collections decreases by \$1,315, a 31.2 percent change from the dismissed filer mean of \$4,217. These results suggest that the marginal recipient of Chapter 13 protection reduces his or her unsecured debt through the bankruptcy system, while the marginal non-recipient is unable to prevent his or her unsecured debts from being sold to a third-party debt collector. Chapter 13 protection also increases the probability that the marginal recipient retains his or her home by 13.2 percentage points, a 36.4 percent increase from the dismissed filer mean of 36.3 percent, but there are no discernible effects on the probability of having a car.

Chapter 13 protection also has important impacts on credit access proxies and credit score, two financial outcomes not directly affected by bankruptcy protection. Over the first five post-filing years, Chapter 13 protection decreases revolving credit utilization by 16.1 percentage points, a 34.5 percent change from the dismissed filer mean, and decreases the

number of non-mortgage inquiries by 0.293, a 18.5 percent change from the dismissed filer mean. Chapter 13 protection increases the marginal recipient's credit score by 14.9 points over the first five post-filing years, a 2.6 percent increase from the dismissed filer mean.

We find suggestive evidence that protection from debt collectors and debt forgiveness are both important mechanisms that help explain our results, although large standard errors make definitive conclusions impossible. To test the importance of protection from debt collectors, we compare treatment effects in states that do and do not allow wage garnishment. Consistent with there being significant costs of not being protected from debt collectors, we find large and statistically significant effects of Chapter 13 protection in states that allow wage garnishment, but small and imprecisely estimated effects in the four states that prohibit wage garnishment where creditors have fewer options to collect unpaid debts from dismissed filers. However, only one of eight differences is statistically significant due to large standard errors. To test the importance of debt forgiveness, we compare treatment effects in states with higher and lower Chapter 7 homestead exemption levels. Since Chapter 13 requires that creditors are repaid at least as much as they would have received in Chapter 7, homeowners that file for Chapter 13 in high exemption states are required to repay creditors less than filers in low exemption states. Consistent with the benefits of Chapter 13 protection increasing in the amount of debt that is forgiven, we find that the effects of Chapter 13 protection are larger for homeowners in states with high Chapter 7 exemption levels compared to homeowners in low Chapter 7 exemption states. However, once again, only two of eight differences are statistically significant due to the imprecision of our estimates.

The results reported in this paper stand in sharp contrast to the prior literature showing few benefits of filing for bankruptcy protection using non-experimental methods (e.g. Han and Li 2007, 2011, Cohen-Cole, Duygan-Bump and Montoriol-Garriga 2013, Jagtiani and Li 2014). Descriptive results show that the outcomes of both dismissed and granted bankruptcy filers deteriorate one to two years before filing. Outcomes for both groups remain depressed after filing, though much more so for dismissed filers. These descriptive trends suggest

that non-experimental estimates are likely to be biased downwards due to selection into bankruptcy filing. Consistent with this scenario, we find that OLS estimates using a non-filer comparison group and within-individual estimates suggest *negative* effects of bankruptcy protection in our data. Conversely, OLS estimates using a dismissed filer comparison group are broadly consistent with our judge IV estimates, suggesting that selection into filing accounts for most of the bias in non-experimental specifications.

The remainder of the paper is structured as follows. Section 1.1 provides a brief overview of the consumer bankruptcy system in the United States. Section 1.2 describes our data and provides summary statistics. Section 1.3 describes our empirical strategy. Section 1.4 estimates the impact of Chapter 13 bankruptcy protection on post-filing financial outcomes. Section 1.5 reconciles our estimates with the prior literature, and Section 1.6 concludes. A data appendix provides additional information on the outcomes used in our analysis.

## **1.1 Chapter 13 Bankruptcy Protection**

### **1.1.1 Overview**

Under Chapter 13 bankruptcy, filers propose a three- to five-year plan to partially repay their unsecured debt in exchange for a discharge of the remaining unsecured debt, a hold on debt collection, and the retention of most assets.<sup>6</sup> Chapter 13 requires filers to use all of their disposable income, defined as their predicted income less predicted expenses, to repay creditors. Creditors must receive at least as much as they would have received if the filer's assets were liquidated under Chapter 7, a requirement known as the "best interest of creditors" test. Chapter 13 filers are also required to fully repay priority claims, such as child support and alimony, unless the claimant agrees to a reduced payment. If a filer wants to keep any collateral securing a claim, he or she must keep up to date on all current payments and include any arrears in the repayment plan. The filer can also choose to give up the collateral and discharge the remaining debt. Thus, Chapter 13 allows filers to avoid

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<sup>6</sup>During our sample period, Chapter 13 filers were able to choose the length of their repayment plan. In our data, granted filers took an average of 3.7 years to complete their plan.

a costly home foreclosure and the repossession of a car by including any arrears in the repayment plan, with the original debt contract reinstated on the completion of the Chapter 13 repayment plan. In a sample of Delaware cases, 71 percent of filers included mortgage arrears in their repayment plans, 41 percent included car loans, and 38 percent included priority debt (White and Zhu 2010). Survey evidence suggests that approximately seventy percent of filers choose Chapter 13 in order to avoid foreclosure (Porter 2011).

Chapter 13 cases begin with the debtor filing the proposed repayment plan, a bankruptcy petition, a statement of financial affairs, a copy of his or her most recent tax return, executory contracts and unexpired leases, and schedules of current income, expenditures, and assets and liabilities. The bankruptcy trustee then holds a meeting with the debtor and any interested creditors in order to resolve problems with the proposed repayment plan.<sup>7</sup> After this meeting, the bankruptcy judge decides whether the repayment plan is feasible and meets the standards for confirmation set forth in the Bankruptcy Code. If the judge confirms the repayment plan, the debtor makes biweekly or monthly payments to the trustee until the plan is complete. The judge may later dismiss or convert the case to Chapter 7 if the filer fails to make any payments, any post-filing domestic support obligations, or any post-filing taxes. If a Chapter 13 filing is dismissed, debtors may refile for either Chapter 7 or Chapter 13 after 180 days.

Debtors also have the option of filing under Chapter 7, which discharges unsecured debts and stops collection efforts in exchange for any non-exempt assets. Chapter 7 bankruptcy does not allow debtors to retain non-exempt assets or collateral securing delinquent debt. Our analysis focuses on the effects of Chapter 13 protection due to limited variation in the treatment of Chapter 7 cases. See Dobbie and Song (2015) for additional details and a discussion of the differences between Chapter 7 and Chapter 13.

We estimate the benefits of Chapter 13 protection, net the costs of repayment, compared

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<sup>7</sup>There is typically one Chapter 13 bankruptcy trustee who works with all judges in an office. If an office has a particularly high Chapter 13 caseload, judges may have their own Chapter 13 trustee. As a result, it is not possible to isolate the independent impact of trustees on the probability of receiving bankruptcy protection using our empirical methodology.

to the best outside option for the marginal dismissed filer. During our sample period, approximately 27 percent of dismissed filers convert or refile for Chapter 7 bankruptcy within one year, with another one percent refiling under Chapter 7 at some point after one year. Conditional on converting or refiling under Chapter 7, 95 percent of dismissed Chapter 13 filers are able to discharge at least some of their debt through Chapter 7. About another 13 percent of dismissed filers refile under Chapter 13 and are dismissed a second time, with about 2.5 percent of dismissed filers refiling under Chapter 13 successfully. The remaining 57 percent of dismissed Chapter 13 filers never file for bankruptcy protection again.<sup>8</sup>

### **1.1.2 Bankruptcy Judges**

Bankruptcy judges are federal judges appointed to 14-year terms by the Court of Appeals in their judicial district. There are a total of 94 federal bankruptcy courts in the United States, including at least one bankruptcy court in each state, the District of Columbia, and Puerto Rico. Each bankruptcy court hears all cases originating from counties in its jurisdiction, and are often further divided into offices that hear all cases originating from a subset of counties in the court's jurisdiction. Bankruptcy judges often hear cases across multiple offices within their court, but only hear cases filed in their bankruptcy court. These cases are typically assigned to judges using a random number generator or a blind rotation system within each office.<sup>9</sup>

The assigned bankruptcy judge decides all matters connected to a case, including whether the repayment plan is feasible and meets the standards for confirmation set forth in the Bankruptcy Code. Common reasons for dismissal include the debtor being able to repay his or her debts without bankruptcy protection, the repayment plan repaying creditors too little, or the repayment plan being infeasible given the debtor's predicted

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<sup>8</sup>Authors' calculations using all available PACER data from 2002 - 2005.

<sup>9</sup>The median court in our sample is divided into three offices, with little systematic pattern to the number of offices in each court. There is considerable variation in the number of bankruptcy judges in each bankruptcy court and office, with courts serving more populous regions tending to have more judges. Of the 205 offices we observe in our data, 110 have only one Chapter 13 judge, 52 have two Chapter 13 judges, 25 have three Chapter 13 judges, and 18 have four or more Chapter 13 judges. See Dobbie and Song (2015) for additional details.

income and expenses (Hynes 2004). In Section 1.3, we discuss how we use systematic differences in the probability that a judge dismisses a filing to estimate the causal impact of bankruptcy protection. The variation in judge behavior that we measure is likely to be driven by differences in how judges interpret the above criteria.

Our empirical strategy also assumes that judges only impact future outcomes through the probability of receiving bankruptcy protection. This exclusion restriction would be violated if judges affect debtor outcomes in other ways, such as by providing financial counseling. The assumption that judges only systematically affect debtor outcomes through bankruptcy is fundamentally untestable, and our estimates should be interpreted with this potential caveat in mind. However, we argue that the exclusion assumption is not unreasonable in our setting. Despite the central role of bankruptcy judges, debtors typically have only limited interaction with the assigned judge. Chapter 13 filers appear before the bankruptcy judge at the plan confirmation hearing, but all other administrative aspects of the bankruptcy process are conducted by the bankruptcy trustee and not the judge. Thus, it seems unlikely that judges would significantly impact debtors other than through the probability of receiving Chapter 13 protection.

### **1.1.3 Potential Benefits of Chapter 13 Protection**

There are at least three reasons that debtors may directly benefit from Chapter 13 bankruptcy protection. First, filing for and obtaining bankruptcy protection puts a hold on current and future debt collection efforts.<sup>10</sup> Bankruptcy protection may therefore decrease the incentive to avoid repayment through strategies with significant deadweight costs, such as leaving the formal banking system to avoid seizure of assets or leaving the formal labor market to avoid wage garnishment.<sup>11</sup> Second, Chapter 13 protection discharges any unsecured

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<sup>10</sup>Dismissed filers receive a temporary stay on collections activity that lasts until the filing is dismissed. Estimates on debt collections activity are therefore likely to be biased downwards, at least in the short run.

<sup>11</sup>Creditors have a number of options to collect unpaid debts if a debtor has not filed for bankruptcy protection or after a case is dismissed, including wage garnishment, collection letters or phone calls, in-person visits at home or work, and seizing of assets through a court order. Debtors can make these collection efforts more difficult by ignoring collection letters and calls, changing their telephone number, or moving without

debts not repaid under the proposed plan, significantly improving a debtor's balance sheet. Third, Chapter 13 bankruptcy allows debtors to restructure secured debts such as a car or mortgage loan. Creditors are allowed to seize assets securing a delinquent loan if a debtor has not filed for bankruptcy protection or after a case has been dismissed, suggesting that Chapter 13 may allow debtors to retain important assets and avoid a potentially costly repossession or foreclosure.

There are also several potential indirect benefits of bankruptcy protection. Most importantly, bankruptcy protection may benefit debtors by increasing their access to credit through an improved balance sheet and fewer adverse collection events reported on a credit record. This may allow debtors to avoid more costly forms of credit, such as pawn or payday loans. Bankruptcy protection may also prevent any sharp drops in consumption that have important long-term consequences, such as becoming sick due to the lack of medical care. Finally, bankruptcy protection may increase economic stability by allowing debtors to avoid foreclosure or eviction.

There are also many reasons to believe that Chapter 13 protection will have little impact on debtors. First, it is possible that the bankruptcy process may exacerbate financial distress by forcing filers to devote all of their disposable income to the repayment plan. It is also possible that debtors are able to avoid most debt collection efforts at a relatively low cost or that collections strategies do not significantly affect most debtors. Finally, bankruptcy protection will have little impact if filers' financial distress stems from broader economic conditions, or immutable individual characteristics such as low human capital.

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leaving a forwarding address. Debtors can also leave the formal banking system to hide their assets from seizure, change jobs to force creditors to reinstate a garnishment order, or work less so that their earnings are not subject to garnishment. See Hynes, Dawsey, and Ausubel (2013) for additional discussion of the debt collection process.



## 1.2 Data

### 1.2.1 Data Sources and Sample Construction

Our empirical analysis uses data from individual bankruptcy filings merged to credit bureau records from TransUnion. The bankruptcy records come from the 72 (out of 94) federal bankruptcy courts that allow full electronic access to their dockets. These data include approximately 75 percent of all filings during our sample period. Each record includes information on the filer's name, address, bankruptcy chapter, filing date, court, office, outcome, and the name of the judge and trustee assigned to the case.

Following Dobbie and Song (2015), we make four restrictions to the bankruptcy data. First, we drop filings from 110 offices that only have a single Chapter 13 bankruptcy judge and filings from counties that assign all cases to a single judge, as in both scenarios there is no variation in judge leniency that allows us to estimate the impact of Chapter 13 protection. Second, we drop office-by-year bins where a retiring judge's cases were reassigned with no documentation as to the original judge. Third, we restrict the sample to debtors who filed for Chapter 13 bankruptcy for the first time between June 2002 and December 2005, ensuring that we obtain at least five years of post-filing outcomes and at least one year of pre-filing baseline outcomes for all debtors. This restriction also ensures that filings occurred before the 2005 Bankruptcy Reform Act came into effect. Finally, we drop office-by-year-by-judge bins with fewer than ten cases where we are unlikely to be able to accurately measure judge leniency. These sample restrictions leave us with 253,863 filings.

We matched these 253,863 filings to credit bureau records from TransUnion using name and address at the time of filing. We were able to successfully match 68.9 percent of our estimation sample to the TransUnion data. Our match rate is similar to Finkelstein et al. (2012), who matched 68.5 percent of Oregon Medicaid applicants to TransUnion data using name, address, and date of birth. The probability of being matched to the credit report data is not significantly related to judge leniency (see Panel F of Table 1.3).

The TransUnion data are available from June 2002 to June 2010. We observe each individual in the credit bureau data annually in June. The TransUnion data are derived

from public records, collections agencies, and trade lines data from lending institutions. The data also include geographic location at the ZIP code-level and age. No other demographic information is available at the individual level. See Avery et al. (2003) and Finkelstein et al. (2012) for additional details on the TransUnion data.

Our estimation sample includes the 253,863 filings matched to at least one post-filing year of credit bureau data. This sample consists of 175,076 filers from 39 offices and 29 bankruptcy courts. The sample includes 348 office-by-year-by-judge observations – the level of variation that drives our empirical design. The number of cases in each office-by-year-by-judge bin ranges from 31 to 2,040, with a median of 799. Table 1.1 provides additional details on each of the offices in our estimation sample.

## **1.2.2 Measures of Financial Outcomes**

We use the linked dataset to estimate the impact of Chapter 13 bankruptcy protection on financial strain, unsecured debt, asset holdings, credit access, and credit score. This section briefly describes how we construct the measures used in our main analysis. The data appendix provides additional details on all of the measures used in our analysis.

Financial strain is measured using indicators for delinquency, creditor charge-offs, collections, bankruptcy, foreclosure, civil judgments, liens, and repossessions within the last 12 months. Delinquency occurs when at least one trade is reported 30 or more days past due, and is our most common measure of financial strain. Credit charge-offs typically occur after 180 days of non-payment on an account, implying that this measure therefore captures a more severe form of non-payment than delinquency. Collections indicate that at least one account has been transferred to a third-party collections agency or is in the process of collection at some point in the last 12 months. Our collections measure does not include debts sent to collection agencies that do not report to credit bureaus, and therefore represents a lower bound on total collections activity. Bankruptcy indicates a new filing in the last 12 months. Foreclosures indicate any foreclosure related action during the last 12 months, including a foreclosure being initiated, a foreclosure being discontinued, and

**Table 1.1:** *Bankruptcy Offices in Chapter 13 IV Sample*

Court	Office	Years	Judges	Discharge	$\sigma_Z$
Northern District of Alabama	Birmingham	2002-2005	3	0.349	0.036
Southern District of Alabama	Mobile	2002-2005	2	0.464	0.006
Southern District of California	San Diego	2002-2005	4	0.472	0.011
Southern District of Florida	Fort Lauderdale	2002-2005	2	0.448	0.006
Southern District of Florida	Miami	2002-2005	2	0.537	0.007
Northern District of Georgia	Atlanta	2004-2005	8	0.322	0.035
Northern District of Georgia	Rome	2004-2005	2	0.414	0.015
District of Idaho	Boise	2002-2005	2	0.548	0.006
Southern District of Indiana	Indianapolis	2002-2005	3	0.529	0.006
Eastern District of Kentucky	Lexington	2002-2005	2	0.556	0.034
District of Massachusetts	Boston	2002-2003	3	0.334	0.036
Eastern District of Michigan	Detroit	2003-2005	3	0.299	0.001
Western District of Michigan	Grand Rapids	2002-2005	3	0.507	0.010
District of Minnesota	Minneapolis	2002-2005	2	0.530	0.001
District of Minnesota	St. Paul	2002-2005	2	0.543	0.044
Eastern District of Missouri	St. Louis	2003-2005	2	0.422	0.019
Western District of Missouri	Kansas City	2002-2005	4	0.505	0.011
Middle District of North Carolina	Durham	2005	2	0.568	0.017
District of New Mexico	Albuquerque	2002-2005	2	0.419	0.022
District of Nevada	Las Vegas	2002-2005	3	0.389	0.011
Southern District of Ohio	Cincinnati	2002-2005	3	0.570	0.025
Southern District of Ohio	Columbus	2002	3	0.600	0.057
Southern District of Ohio	Dayton	2002-2005	3	0.609	0.022
Northern District of Oklahoma	Tulsa	2002-2005	2	0.480	0.011
District of Oregon	Eugene	2002-2005	2	0.600	0.016
District of Oregon	Portland	2002-2005	3	0.551	0.114
District of South Carolina	Columbia	2003-2005	2	0.758	0.021
Eastern District of Tennessee	Chattanooga	2002-2005	2	0.443	0.009
Middle District of Tennessee	Columbia	2002-2005	3	0.469	0.010
Middle District of Tennessee	Cookeville	2002-2005	3	0.476	0.014
Middle District of Tennessee	Nashville	2002-2005	3	0.493	0.015
Western District of Tennessee	Memphis	2002-2005	3	0.270	0.003
Western District of Texas	San Antonio	2002-2005	2	0.443	0.002
District of Utah	Salt Lake City	2003-2005	3	0.348	0.007
Eastern District of Virginia	Alexandria	2002-2005	2	0.567	0.001
Eastern District of Virginia	Newport News	2002-2005	2	0.569	0.038
Eastern District of Virginia	Norfolk	2002-2005	2	0.597	0.001
Western District of Washington	Tacoma	2002-2005	2	0.574	0.004
Eastern District of Wisconsin	Milwaukee	2003-2005	3	0.472	0.010

Notes: This table presents descriptive statistics for the 39 offices in the 29 bankruptcy courts that randomly assign filings to judges in our instrumental variables sample.  $\sigma_Z$  is the standard deviation of leave-one-out measure of judge leniency described in the text.

a foreclosure being redeemed. The foreclosure measure used in this paper is therefore more inclusive than the foreclosure measure used in Dobbie and Song (2015), which only included foreclosure sales and transfers. Civil judgments include all wage garnishment orders, liens against property, and levies on checkings or savings accounts in the last 12 months. Civil judgments are often difficult and costly to obtain, meaning that this measure is likely proxying for particularly large unpaid bills. Liens indicate at least one public records claim on a lien in the past 12 months. Public record liens include federal and state tax liens, hospital liens, and judicial liens. Repossession indicates that a creditor has attempted to take back a secured asset, such as a car or boat, in the last 12 months.

Each financial strain measure is the average of five indicator variables for having experienced the listed event from the filing year to the fifth post-filing year, with two exceptions. We measure both charge-offs and new bankruptcies from the first full post-filing year to fifth year after filing to exclude the mechanical effect of the original Chapter 13 filing on these outcomes in the year of filing. Table 1.2 reports results using the number of adverse events in the first five post-filing years and the cumulative probability of an event occurring at least once in the first five post-filing years for each of the eight adverse financial events in our data.

We also report results using a financial strain index, a summary index of these eight adverse events designed to broadly capture financial distress associated with collections activity. Following Fryer and Katz (2013), for each post-filing year, we first standardize each component in the financial strain index using the mean and standard deviation of the component for the dismissed filer group in the baseline year. Next, we sum the eight components in each year, restandardizing using the mean and standard deviation of the index for the dismissed filer group in the baseline period. To exclude the mechanical effect of filing on charge-offs and new bankruptcies in the year of filing, the financial strain index in the year of filing is composed of the other six measures of adverse financial events. Finally, we average these annual index measures over the first five post-filing years. Because each of the financial strain components represent adverse events that negatively impact access to

**Table 1.2: Results for Alternative Financial Strain Measures**

	Dismissed		
	Mean	2SLS Results	
<i>Panel A: Ever Experienced</i>	(1)	(2)	(3)
Delinquency	0.942 (0.233)	0.002 (0.027)	-0.007 (0.026)
Collection	0.920 (0.271)	-0.147** (0.061)	-0.098 (0.066)
Charge-off <sup>†</sup>	0.581 (0.493)	-0.112* (0.060)	-0.120* (0.065)
New Bankruptcy <sup>†</sup>	0.351 (0.477)	-0.271*** (0.056)	-0.278*** (0.060)
Foreclosure	0.248 (0.432)	-0.111*** (0.031)	-0.062** (0.025)
Judgment	0.252 (0.434)	-0.154*** (0.056)	-0.127** (0.058)
Lien	0.128 (0.334)	-0.116*** (0.026)	-0.111*** (0.028)
Repossession	0.084 (0.278)	-0.071*** (0.027)	-0.074** (0.029)
<i>Panel B: Number of Experiences</i>			
Delinquencies	6.774 (5.434)	1.247* (0.650)	0.719 (0.688)
Collections	6.847 (6.722)	-2.992*** (0.624)	-2.235*** (0.750)
Charge-offs <sup>†</sup>	1.220 (1.562)	-0.364*** (0.110)	-0.395*** (0.130)
New Bankruptcies <sup>†</sup>	0.479 (0.772)	-0.336*** (0.103)	-0.339*** (0.110)
Foreclosures	0.369 (0.767)	-0.158*** (0.047)	-0.077* (0.045)
Judgments	0.384 (0.836)	-0.304*** (0.090)	-0.262*** (0.093)
Liens	0.237 (1.069)	-0.283*** (0.102)	-0.275** (0.111)
Repossessions	0.094 (0.329)	-0.076** (0.034)	-0.078** (0.036)
Controls	-	No	Yes
Observations	97006	175076	175076

Notes: This table reports two-stage least squares results of the impact of Chapter 13 bankruptcy protection for alternative versions of the financial strain variables. All outcomes are annual averages for the year of filing to fifth year post-filing, with the exceptions of outcomes with a † where outcomes are averaged over the first full year after filing to the fifth year post-filing to remove the mechanical effect of the bankruptcy filing. Panel A reports results for indicator variables equal to one if the listed event occurred at least once in the first five post-filing years. Panel B reports results for the number of times the listed event occurred in the first five post-filing years. See Table 3 notes for additional details. \*\*\* = significant at 1 percent level, \*\* = significant at 5 percent level, \* = significant at 10 percent level.

credit, a higher index represents worse outcomes throughout.

Unsecured debt and collections activity are measured using the current balance of open revolving loans, and the amount of debt currently in collections. Revolving loans includes all current bank cards, retail cards, and check credit accounts. Collections debt include all loans that have been transferred to a collection agency or that are in the process of collection. Following the above discussion, our measure of collections debt is likely a lower bound. Our unsecured debt data do not include information on some non-bank and non-retail forms of unsecured credit, such as pawn and payday loans. The data also do not include information on the cost of revolving debt. We are therefore unable to estimate the impact of Chapter 13 on these outcomes.

Retention of secured assets is measured using indicators for having an open mortgage loan within the past 12 months and having an open auto loan within the past six months, and the current balance of all open mortgages and open auto loans. All of the debt balance measures are captured in June of each year. Having an active mortgage or auto loan proxies for ownership of these assets, but is an underestimate of actual ownership as some filers have likely fully paid off their mortgage or auto loans.

We measure credit access using the total utilization on revolving accounts, and the number of non-mortgage inquiries in the last six months. TransUnion does not provide credit line information for each category of non-mortgage debt, so we proxy for credit supply using revolving trades, the largest category of non-mortgage credit among all credit users and our estimation sample. Revolving trades include any bank card accounts, retail accounts, and check credit accounts. Results are qualitatively similar using bank card trades, a subset of revolving trades. Utilization is defined as the current balance divided by the credit limit, where TransUnion measures the credit limit using either the reported credit limit, or the highest amount ever owed on an account if the credit limit is unreported. Avery et al. (2003) discuss this imputation procedure, concluding that the credit limit variable is likely a lower bound. Accordingly, utilization measures likely reflect an upper bound for accounts where the credit limit is imputed. Importantly, estimates using utilization may be

biased if Chapter 13 protection impacts the highest amount ever owed on an account, as credit limits will appear higher for these individuals. Our utilization estimates should be interpreted with this potential measurement bias in mind. Our second measure of credit access is the number of non-mortgage inquiries. Inquiries are made to ensure that an applicant for credit, apartment rental, insurance, or employment meets minimum standards, and is considered a proxy for excess credit demand.

Credit score is measured using an ordinal credit score variable calculated by TransUnion to capture credit risk. The TransUnion credit score variable is used by creditors to determine the price and eligibility for credit, and is similar to the FICO score commonly referenced in the consumer finance literature. Our credit score variable should therefore be interpreted as a summary measure of a debtor's financial risk, and incorporates many of the potential effects on the outcomes discussed above. We report estimates using the scale provided by TransUnion.

### 1.2.3 Descriptive Statistics

Table 1.3 presents summary statistics for our data. Column 1 reports summary statistics for a random sample of the population of credit users in the TransUnion database.<sup>12</sup> Column 2 reports summary statistics for individuals in this random sample that file for bankruptcy protection during our sample period. The TransUnion data does not report chapter of filing, so these individuals include a mix of Chapter 7, Chapter 11, Chapter 12, and Chapter 13 filers. Because very different types of individuals file under various bankruptcy chapters, bankruptcy filers in the national sample are likely to differ in substantial ways from Chapter 13 filers.<sup>13</sup> Columns 3 and 4 report summary statistics for Chapter 13 filers in our estimation

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<sup>12</sup>See Dobbie and Goldsmith-Pinkham (2014) for additional details on the credit user sample. The data contain approximately two percent of all credit users in the United States during this time period.

<sup>13</sup>The TransUnion data do not provide information on the date of bankruptcy filing or the chapter of bankruptcy, but each calendar year pull provides information on the number of bankruptcy filings in the last 12 months. From this bankruptcy filing flag, we define bankruptcy filers as those individuals who filed for bankruptcy for the first time in the last 12 months based on credit report data between 2003 and 2006. Individuals whose bankruptcy flag is turned on in multiple years between 2003 and 2006 are excluded.

sample assigned to judges with below and above median judge leniency as defined in Section 1.3.

Bankruptcy filers are younger and more likely to own a home than the typical credit user in the United States. The typical bankruptcy filer in the national sample is 43.7 years old, compared to 48.5 years old for all credit users. Fifty-two percent of bankruptcy filers own a home. In comparison, 47.0 percent of all credit users own a home. In our estimation sample, 65.5 percent of Chapter 13 filers are homeowners and the average age is 44.8 years old.

Perhaps not surprisingly, bankruptcy filers are far more likely to suffer an adverse financial event than the typical credit user even before filing. In the national sample, 41.3 percent of bankruptcy filers have at least one delinquency before filing, 29.6 percent have at least one debt in collections, 18.8 percent have at least one creditor charge-off, 3.4 percent have at least one civil judgment, 1.0 percent have experienced a foreclosure, 1.1 percent have at least one property lien, and 1.2 percent have at least one repossession. Chapter 13 filers in our estimation sample are even more likely to have suffered an adverse financial event before filing compared to the typical credit user, with 67.7 percent having had a delinquency in the past 12 months, 46.3 percent having a debt in collections, 30.9 percent having a charge-off, 6.3 percent having a judgment, 5.1 percent having a foreclosure, 2.1 percent having a lien, and 2.1 percent having a repossession. In comparison, only 14.8 percent of all credit users have a delinquency in the past 12 months, 13.7 percent have a debt in collections, 6.5 percent have a charge-off, 0.9 percent have a judgment, 0.3 percent have a foreclosure, 0.4 percent have a lien, and 0.3 percent have a repossession.

Bankruptcy filers also have significantly higher unsecured debt and collections activity compared to the typical credit user. Bankruptcy filers in the national sample have \$13,083 in revolving debt and \$1,432 of debt in collections. Chapter 13 filers in our estimation sample have \$10,460 in revolving debt and \$2,460 of debt in collections. In comparison, the typical credit user has \$6,011 in revolving debt and \$601 of debt in collections.

Bankruptcy filers are more likely to have an open mortgage than the typical credit user.



**Table 1.3: Descriptive Statistics and Randomization Balance**

	All Credit Users		Judge Sample		
	Full Sample	Bankruptcy Filers	Harsh Judge	Lenient Judge	p-value
	(1)	(2)	(3)	(4)	(5)
<i>Panel A: Judge Leniency</i>					
Judge Leniency	-	-	-0.013	0.012	0.000
<i>Panel B: Baseline Characteristics</i>					
Age	48.549	43.699	44.843	44.863	0.229
Homeowner	0.470	0.520	0.668	0.643	0.175
<i>Panel C: Baseline Financial Events</i>					
Delinquency	0.148	0.413	0.681	0.675	0.962
Collection	0.137	0.296	0.460	0.467	0.897
Charge-off	0.065	0.188	0.308	0.310	0.630
Bankruptcy	0.010	0.007	0.046	0.048	0.318
Judgment	0.009	0.034	0.067	0.060	0.403
Foreclosure	0.003	0.010	0.055	0.048	0.632
Lien	0.004	0.011	0.021	0.021	0.445
Repossession	0.003	0.012	0.022	0.020	0.491
<i>Panel D: Baseline Unsecured Debt and Collections</i>					
Revolving Balance	6.011	13.083	10.939	10.007	0.440
Collection Balance	0.601	1.432	2.421	2.497	0.676
<i>Panel E: Baseline Secured Assets</i>					
Have a Mortgage	0.367	0.434	0.591	0.567	0.274
Mortgage Balance	42.460	39.848	56.804	53.437	0.213
Have an Auto Loan	0.283	0.454	0.479	0.468	0.778
Auto Balance	4.391	7.803	8.359	8.207	0.913
<i>Panel F: Baseline Credit Access</i>					
Revolving Utilization	25.495	61.443	70.869	70.968	0.858
Non-Mortgage Inquiries	0.807	1.841	2.355	2.362	0.186
<i>Panel G: Baseline Credit Score</i>					
Credit Score	739.538	630.096	581.373	580.155	0.730
<i>Panel H: Data Characteristics</i>					
Matched to Credit Report	-	-	0.692	0.687	0.823
Missing Age	0.169	0.062	0.091	0.098	0.751
Missing Baseline Outcomes	-	0.137	0.028	0.029	0.037
Missing Credit Score	0.053	0.014	0.019	0.023	0.043
Observations	3308824	56906	85173	89903	175076

Notes: This table reports summary statistics. The all credit user sample consists of a two percent random sample of credit users in the United States from 2002-2005. Bankruptcy filers consist of individuals who filed for any bankruptcy chapter from 2002-2006. The judge sample consists of Chapter 13 bankruptcy filers originating from offices that randomly assigns filers to judges between 2002-2005 that are linked to credit report data in the year of filing. Column 5 reports p-values calculated from separate regression models of each baseline characteristic on an indicator for being assigned to a judge with above median leniency. Column 5 also controls for office-by-filing-month fixed effects and clusters standard errors at the office level. See the data appendix for details on the data and variable construction.

In the national sample, 43.4 percent of bankruptcy filers have at least one open mortgage, compared to 36.7 percent for all credit users. In our estimation sample, 57.9 percent of Chapter 13 filers have at least one open mortgage. Note that active mortgage rates are generally lower than homeownership rates in both the national sample and estimation sample, suggesting that approximately seven to ten percent of homeowners have already paid off their mortgages.

While bankruptcy filers in the national sample are more likely to have a mortgage, they have mortgage balances that are \$2,612 lower than the typical credit user, while Chapter 13 filers in our estimation sample have mortgage balances that are \$12,615 more than the typical credit user. Home mortgage balances are likely higher among Chapter 13 filers than bankruptcy filers in the national sample because national bankruptcy filers comprise those who file under Chapter 7 as well as Chapter 13, and Chapter 7 filers are less likely to be homeowners.

Bankruptcy filers are also 17.1 percent more likely to have an open auto loan compared to the typical credit user, with Chapter 13 filers in our estimation sample 19.1 percent more likely to have an active auto loan than the typical credit user. Accordingly, bankruptcy filers in the national sample have auto balances that are \$3,412 more than the typical credit user. Chapter 13 filers in our estimation sample have auto balances \$3,892 more than the typical credit user.

Bankruptcy filers in the national sample have higher utilization on revolving accounts and more credit inquiries than the typical credit user, suggesting that bankruptcy filers have excess credit demand conditional on credit supply. Specifically, bankruptcy filers in the national sample have utilization rates that are 35.9 percentage points higher than the average credit user, and also have 1.0 more non-mortgage inquiries in the last six months than the typical credit user. In our estimation sample, Chapter 13 filers have 45.4 percentage points higher utilization on revolving accounts than the typical credit user, and 1.5 more non-mortgage inquiries.

Bankruptcy filers also have lower credit scores than the typical credit user in the United

States. Average pre-filing credit scores are 630.0 for bankruptcy filers in the national sample. In comparison, average credit scores are 739.5 for all credit users. In our estimation sample, the average credit score is 580.7.

### 1.3 Research Design

Consider a model that relates post-filing outcomes such as credit score to the receipt of Chapter 13 bankruptcy protection:

$$y_{it} = \alpha + \beta\mathbf{X}_i + \gamma\text{Bankruptcy}_i + \varepsilon_{it} \quad (1.1)$$

where  $i$  denotes individuals,  $t$  is the year of observation,  $\gamma$  is the causal impact of bankruptcy protection,  $\mathbf{X}_i$  includes controls such as age and lagged outcomes, and  $\varepsilon_{it}$  is noise. Our key empirical problem is that OLS estimates of Equation (1.1) may be biased if bankruptcy protection is correlated with the unobservable determinants of later outcomes, explored further in Section 1.5.

We estimate the impact of Chapter 13 protection on debtors using judge leniency as an instrument for bankruptcy protection. Our empirical strategy exploits the fact that judges are randomly assigned to filings, and that those bankruptcy judges have differing tendencies to grant Chapter 13 protection. In this specification, we interpret any difference in post-filing outcomes as the causal effect of the change in the probability of receiving bankruptcy protection operating through judge assignment. The second stage estimating equation is:

$$y_{it} = \alpha + \alpha_{ot} + \beta\mathbf{X}_i + \gamma\text{Bankruptcy}_i + \varepsilon_{it} \quad (1.2)$$

where  $\alpha_{ot}$  are office-by-filing-month fixed effects and  $\mathbf{X}_i$  includes baseline age bins, home-ownership, financial strain, revolving, mortgage, auto, and collections debt, indicators for open mortgage and open auto loans, revolving utilization, non-mortgage inquiries, and credit score.  $\mathbf{X}_i$  also includes indicators for missing age and baseline characteristics.

The corresponding first stage estimating equation associated with Equation (1.2) is:

$$Bankruptcy_{it} = \alpha + \alpha_{ot} + \beta \mathbf{X}_i + \delta \sigma_j + \varepsilon_{it} \quad (1.3)$$

where  $\sigma_j$  is the systematic component of judge behavior and  $\delta$  represents the impact of judge behavior on the probability of receiving bankruptcy protection. We cluster standard errors at the office level in both the first and second stage regressions to account for any serial correlation across filers at the level of randomization. Results are qualitatively similar if we cluster at the office-by-judge or office-by-filing-month level.

Following the previous literature (e.g. Kling 2006, Chang and Schoar 2008, Doyle 2007, 2008, Aitor and Houseman 2010, French and Song 2014, Aizer and Doyle forthcoming, Maestas, Mullen, and Strand 2013, and Dobbie and Song 2015), we define judge leniency  $Z_{ioj}$  as the leave-one-out fraction of filings granted by judge  $j$  in office  $o$  minus the leave-one-out fraction granted in office  $o$ :

$$Z_{ioj} = \frac{1}{n_{oj} - 1} \left( \sum_{k=1}^{n_{oj}} (B_k) - B_i \right) - \frac{1}{n_o - 1} \left( \sum_{k=1}^{n_o} (B_k) - B_i \right) \quad (1.4)$$

where  $i$  again denotes individuals,  $o$  denotes offices,  $j$  is the assigned judge,  $B_i$  is an indicator for receiving bankruptcy protection,  $n_{oj}$  is the number of cases seen by a judge in office  $o$ , and  $n_o$  is the number of cases seen by an office. We calculate judge leniency using all filings in the full sample of filings, including those not matched to TransUnion credit records.

Our preferred measure of judge leniency uses the final decision on each bankruptcy filing, not whether a plan is initially confirmed or dismissed. We focus on this measure of judge leniency for two reasons. First, the resulting two-stage least squares estimates can be interpreted as the causal effect of receiving bankruptcy protection, which has clearer policy implications than plan confirmation. Second, we do not observe the reason for case dismissal in our data, and are therefore unable to measure plan confirmation directly. In Section 1.4.7, we present estimates that use judge leniency measured over the first 90 days, a proxy for plan confirmation. These results are nearly identical to our preferred estimates discussed below. See Section 1.4.7 for additional details on this alternative measure of judge

leniency and other robustness checks.

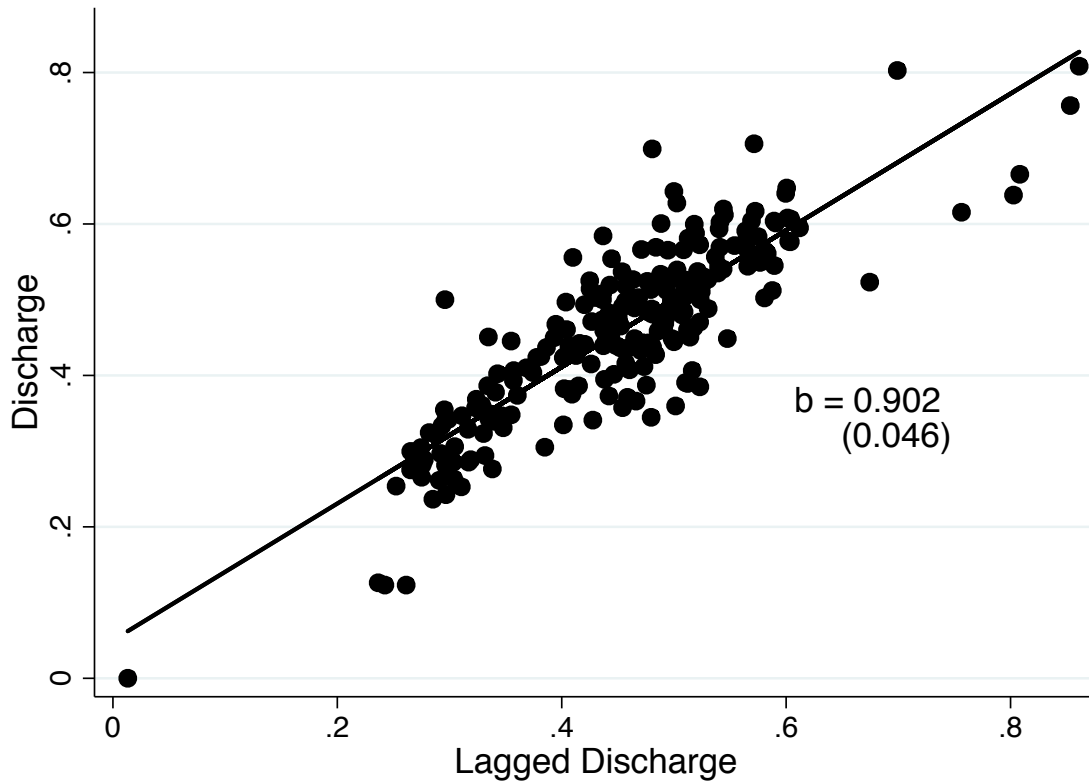
Consistent with Dobbie and Song (2015), we find considerable variation in the treatment of Chapter 13 cases within an office.<sup>14</sup> The standard deviation of  $Z_{ioj}$  is 0.025 for Chapter 13 filers in our sample. There is also significant persistence in our measure of judge behavior. Figure 1.1 plots current and lagged judge discharge rates, with each point representing a separate judge-by-office-by-year observation. Discharge rates are highly correlated across time, with an OLS regression relating each judge-by-office-by-year discharge rate to the lagged discharge rate yielding a coefficient of 0.902. These results suggest that we are capturing systematic differences in judge behavior, not random year to year noise.

Using our measure of judge leniency  $Z_{ioj}$  as an instrument for the receipt of Chapter 13 bankruptcy protection, two-stage least squares estimates from Equation (1.2) measure the local average treatment effect of Chapter 13 protection for filers whose bankruptcy outcomes are altered by judge assignment. Three conditions must hold to interpret these estimates as the local average causal impact of bankruptcy protection: (1) judge assignment is associated with bankruptcy protection, (2) judge assignment only impacts debtor outcomes through the probability of receiving bankruptcy protection, and (3) the impact of judge assignment on the probability of receiving bankruptcy protection is monotonic across filers.

Figure 1.2 tests the first assumption by plotting average discharge against our leave-one-out measure of judge leniency. The estimation sample includes first-time filers between 2002 and 2005 in the 39 offices in the 29 courts that randomly assign Chapter 13 filings to judges. Figure 1.2 is constructed by calculating the mean residuals from a regression of an indicator for receiving Chapter 13 protection on office-by-filing-month fixed effects. For ease of interpretation, we add the mean discharge rate to the mean residual in each judge-by-year bin. The plotted line and corresponding coefficient show the best linear fit estimated on the underlying individual-level data, controlling for office-by-filing-month fixed effects and with standard errors clustered at the office level. Table 1.4 presents analogous individual-level

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<sup>14</sup>See Sullivan, Warren, and Westbrook (1994) and Norberg and Compo (2007) for additional discussion on the variation in bankruptcy judge behavior.



**Figure 1.1:** *Persistence of Judge Leniency Measure*

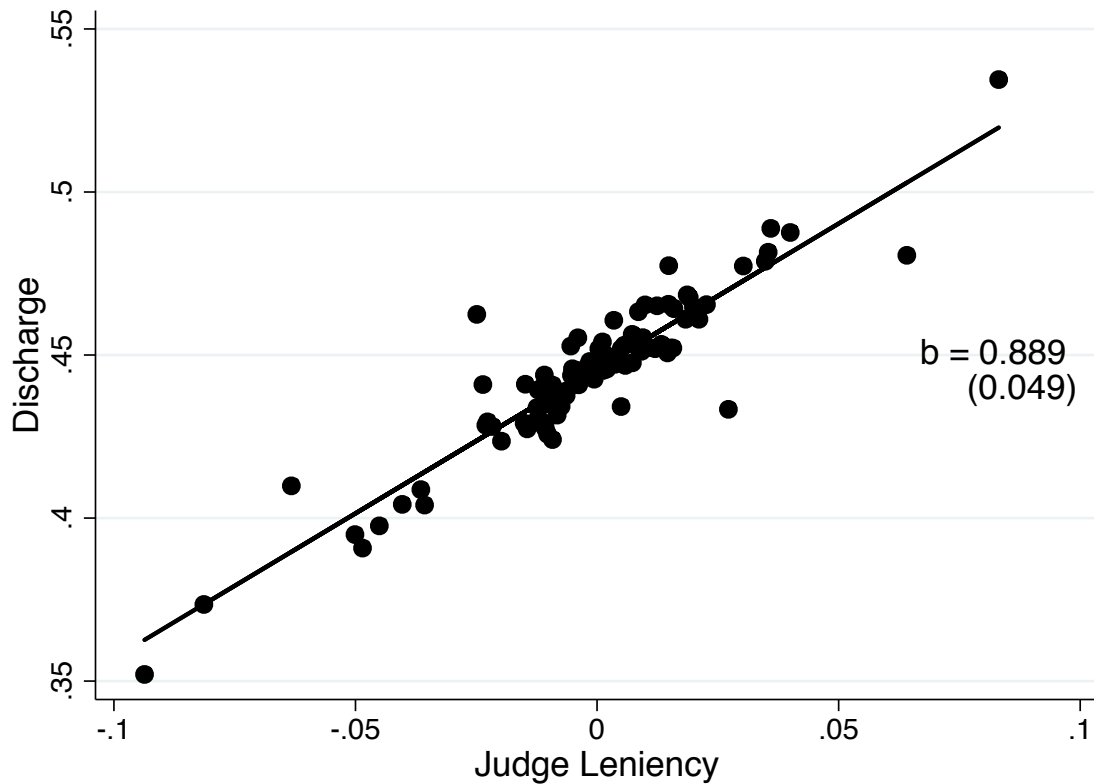
Notes: This figure plots current Chapter 13 discharge vs. lagged discharge for each judge-by-office-by-year. The sample consists of all first-time Chapter 13 filers from 2002-2005 linked to credit report data, for whom we observe credit data in the year of filing. Judge leniency is the leave-one-out mean rate of granting Chapter 13 bankruptcy protection for the assigned judge minus the leave-one-out mean rate of granting bankruptcy protection for the office. Each point in the scatter plot represents a separate judge-by-office-by-year observation. To construct the scatter plot, we regress current discharge rate on lagged discharge rate. The solid line shows the best linear fit estimated on the underlying micro data estimated using OLS. The coefficient shows the estimated slope of the best-fit line, with standard errors clustered at the office by judge level reported in parentheses.

estimates with and without controls.

Figure 1.2 and Table 1.4 indicate that judge leniency is highly predictive of the probability of receiving bankruptcy protection. With no controls, a one percentage point increase in  $Z_{ioj}$  increases the probability that a debtor receives bankruptcy protection by 0.889 percentage points. Controlling for all baseline characteristics in column 6, our measure of judge leniency remains highly predictive of the probability of receiving bankruptcy protection, with a one percentage point increase in  $Z_{ioj}$  increasing the probability that a debtor receives bankruptcy protection by 0.811 percentage points. Thus, a one standard deviation (2.5 percentage point) increase in judge leniency increases the likelihood of receiving bankruptcy protection by about 2.0 percentage points, corresponding to a 4.5 percent change from the mean discharge rate of 44.6 percent.

Consistent with the first stage results in Dobbie and Song (2015), the probability of receiving Chapter 13 protection does not increase one-for-one with our measure of judge leniency, likely because of measurement error that attenuates the effect toward zero. For instance, the accuracy of our leave-one-out measure will be reduced if judge leniency drifts over the course of the year or fluctuates with case characteristics. Nevertheless, our first stage results confirm that our measure of judge leniency is highly predictive of case outcomes.

The coefficients on our baseline controls are of independent interest for understanding the types of individuals more or less likely to receive Chapter 13 protection. The probability of receiving bankruptcy protection is increasing in filer age. Homeowners are also more likely to receive Chapter 13 protection than non-homeowners. The probability of receiving Chapter 13 protection is decreasing in most measures of financial strain and the amount of debt in collections. The probability of receiving bankruptcy protection is also decreasing in mortgage and auto debt, although individuals with open mortgage and auto loans are more likely to receive Chapter 13 protection. Conversely, filers with higher unsecured debt are more likely to receive bankruptcy protection, as are filers with more revolving accounts. Finally, the probability of receiving Chapter 13 is decreasing with the number of credit inquiries in the last six months, and increasing with credit score.



**Figure 1.2:** *Judge Leniency and Bankruptcy Protection*

Notes: This figure plots Chapter 13 discharge vs. our leave-one-out measure of judge leniency. The sample consists of all first-time Chapter 13 filers from 2002-2005 linked to credit report data, for whom we observe credit data in the year of filing. Judge leniency is the leave-one-out mean rate of granting Chapter 13 bankruptcy protection for the assigned judge minus the leave-one-out mean rate of granting bankruptcy protection for the office. To construct the binned scatter plot, we first regress an indicator for discharge on office-by-filing-month fixed effects and calculate residuals. We then take the mean residual in each judge-by-year bin, adding the mean discharge rate to each residual to aid in the interpretation of the plot. The solid line shows the best linear fit estimated on the underlying micro data estimated using OLS. The coefficients show the estimated slope of the best-fit line including office-by-filing-month fixed effects, with standard errors clustered at the office level reported in parentheses.



**Table 1.4: Judge Leniency and Chapter 13 Protection**

	(1)	(2)	(3)	(4)	(5)	(6)
Judge Leniency	0.88872*** (0.04873)	0.83789*** (0.05299)	0.84220*** (0.04777)	0.85585*** (0.05243)	0.85780*** (0.04588)	0.81092*** (0.05186)
Age at Filing		0.00305*** (0.00016)	0.00235*** (0.00018)	0.00424*** (0.00017)	0.00310*** (0.00017)	0.00072*** (0.00018)
Homeowner		0.03749*** (0.01047)	-0.01142 (0.01305)	0.02574*** (0.00512)	0.00205 (0.01222)	-0.00827 (0.00536)
Delinquency		-0.06518*** (0.00908)				0.03066*** (0.00402)
Collection		-0.12082*** (0.00765)				-0.04848*** (0.00428)
Charge-off		0.00413 (0.00320)				0.00117 (0.00267)
Bankruptcy		-0.13191*** (0.01038)				-0.08388*** (0.00809)
Judgment		-0.07933*** (0.01277)				-0.04120*** (0.01031)
Foreclosure		-0.17495*** (0.01508)				-0.12185*** (0.01319)
Lien		-0.10001*** (0.00847)				-0.06406*** (0.00842)
Repossession		-0.02670*** (0.00855)				0.00054 (0.00804)
Revolving Balance			0.00575*** (0.00025)			0.00487*** (0.00021)
Collection Balance			-0.00617*** (0.00051)			-0.00206*** (0.00038)
Have a Mortgage				0.00883 (0.01359)		0.04324*** (0.00972)
Mortgage Balance				-0.00028*** (0.00007)		-0.00039*** (0.00005)
Have an Auto Loan				0.10670*** (0.00488)		0.06466*** (0.00463)
Auto Balance				-0.00065*** (0.00024)		-0.00068*** (0.00022)
Revolving Utilization					-0.00004 (0.00003)	0.00009*** (0.00002)
Non-Mortgage Inquiries					-0.01126*** (0.00081)	-0.00861*** (0.00069)
Credit Score					0.00075*** (0.00008)	0.00142*** (0.00005)
Observations	175076	175076	175076	175076	175076	175076

Notes: This table reports first stage results. The sample consists of Chapter 13 bankruptcy filers originating from offices that randomly assigns filers to judges between 2002-2005 that are linked to credit report data in the year of filing. Judge leniency is the leave-one-out mean rate of granting Chapter 13 bankruptcy protection for the assigned judge minus the leave-one-out mean rate of granting bankruptcy protection for the office. All characteristics are measured one year prior to the bankruptcy filing. All regressions control for office-by-filing-month fixed effects and cluster standard errors at the office level. See the data appendix for details on the data and variable construction. \*\*\* = significant at 1 percent level, \*\* = significant at 5 percent level, \* = significant at 10 percent level.

Following Doyle (2008), we also present first stage results for different subsamples to shed light on the characteristics of filers who are more likely to be affected by judge assignment. In the case of a binary instrument, the relative likelihood that a complier has a given characteristic is equal to the first-stage coefficient for that group divided by the first-stage coefficient for the full sample. Similar logic applies to the case of a continuous instrument. Given that filers likely differ in how much they benefit from Chapter 13 protection, these results provide new evidence on the types of cases for which the instrumental variables estimates are most likely to apply, that is, those filers for whom judges most disagree on whether to grant bankruptcy protection. These results also provide insight into the likely impacts of debt-relief policy interventions that target different types of debtors.

Table 1.5 shows the first-stage estimates for subgroups of interest. We also present the ratio of the subgroup first-stage coefficient to the overall first-stage coefficient from column 6 of Table 1.5. The most striking first-stage result is for filers 25 to 39 years old at the time of filing, who have a first stage coefficient that is 16.5 percent higher than the overall first stage, although the ratio is not significantly different from one due to variability in the data. Conversely, filers who are 60 and up have a first-stage coefficient that is only 52.3 percent of the overall first stage, significantly different from one. First-stage results are not substantially different between filers with below and above median baseline credit scores, and by baseline homeownership status. These results suggest that young filers are most likely to be affected by a lenient judge assignment, indicating greater judicial disagreement over these types of filers.

Our second identifying assumption is that judge assignment only impacts debtor outcomes through the probability of receiving bankruptcy protection. This assumption would be violated if judge leniency is correlated with unobservable determinants of future outcomes. We partially test this assumption by assessing whether observable filer characteristics differ based on whether filers are assigned to a judge with either a high or low propensity to grant Chapter 13 protection. Following Aizer and Doyle (forthcoming), columns 3 and 4 of Table 1.3 present summary statistics separately for filers assigned to judges with above

**Table 1.5: First Stage Results by Filer Characteristics**

	Age at Filing			Baseline Credit Score		Baseline Homeowner	
	25 to 39 (1)	40 to 59 (2)	60 and up (3)	High (4)	Low (5)	Yes (6)	No (7)
Discharge	0.945*** (0.097)	0.854*** (0.059)	0.424*** (0.149)	0.796*** (0.045)	0.815*** (0.069)	0.814*** (0.070)	0.785*** (0.064)
Relative to Overall First Stage	1.165	1.053	0.523	0.982	1.005	1.004	0.968
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	54442	81206	19100	84154	82241	111432	58627

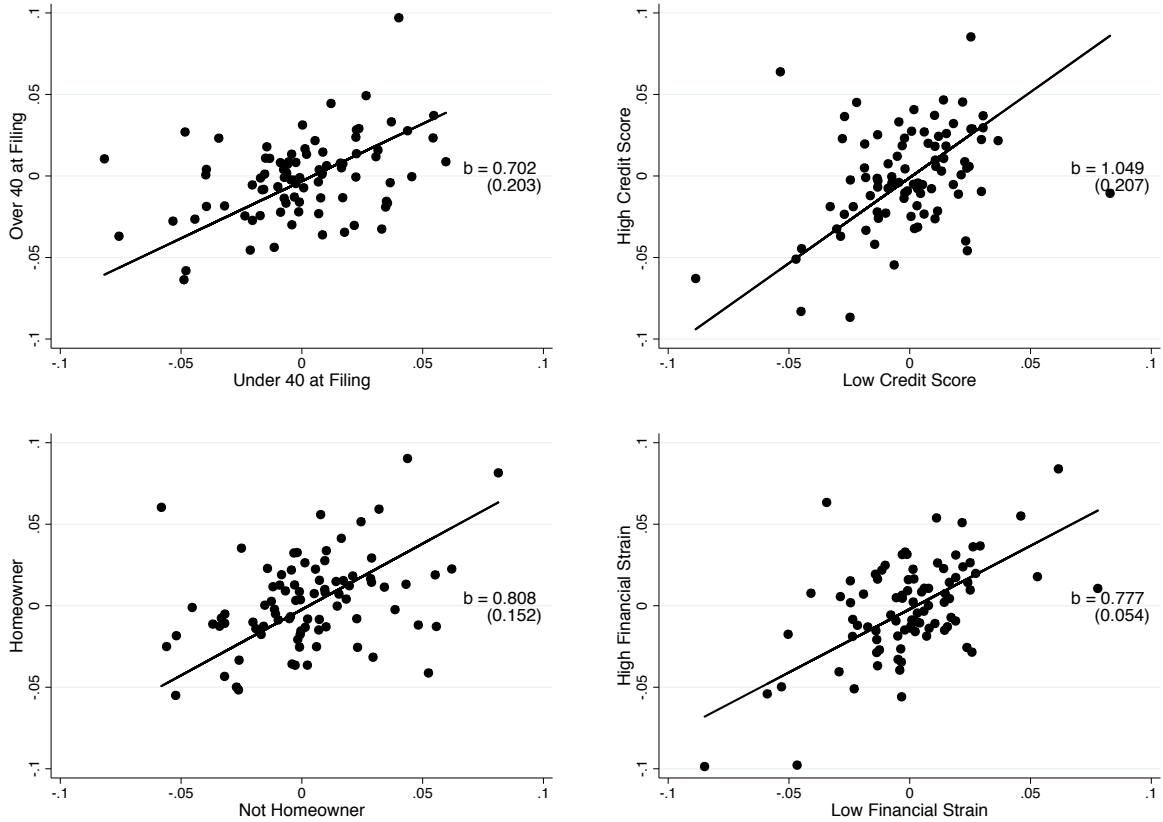
Notes: This table reports first stage and two-stage least squares results by baseline characteristics. The sample consists of Chapter 13 bankruptcy filers originating from offices that randomly assigns filers to judges between 2002-2005 that are linked to credit report data in the year of filing. The post-filing mean for dismissed filers is reported in brackets for each subgroup. We instrument for Chapter 13 protection using the leave-one-out mean rate of granting Chapter 13 bankruptcy protection for the assigned judge minus the leave-one-out mean rate of granting bankruptcy protection for the office. Subgroup instruments are constructed using the matched estimation sample. All regressions control for age at filing and baseline homeownership, credit score, financial strain index, installment balance, revolving balance, collection balance, mortgage balance, and non-mortgage credit access index, office-by-filing-month fixed effects, and cluster standard errors at the office level. See the data appendix for details on the data and variable construction. \*\*\* = significant at 1 percent level, \*\* = significant at 5 percent level, \* = significant at 10 percent level.

and below median leniency. Column 5 reports results from a series of OLS regressions of each observable filer characteristic on an indicator for being assigned to a judge with above median leniency and office-by-filing-month fixed effects with standard errors clustered by office. Consistent with our identifying assumptions, there is only one statistically significant difference in the 26 variables we consider.

Our third identifying assumption is that there is a monotonic impact of judge assignment on the probability of receiving bankruptcy protection. The monotonicity assumption implies that being assigned to a more (less) lenient judge does not decrease (increase) the likelihood of receiving Chapter 13 protection. Following Dobbie and Song (2015), we partially test the validity of the monotonicity assumption by examining how judges treat filings from observably different filers. Any significant differences in the way that judges treat these filings would suggest that the monotonicity assumption is violated. Figure 1.3 plots judge leniency measures calculated separately by age at filing, baseline credit score, baseline home ownership, and baseline financial strain. Each plot reports the coefficient and standard error from an OLS regression relating each measure of judge leniency. Consistent with our monotonicity assumption, we find that judge tendencies are very similar across observably different filers.

In unreported results, we also examine whether measures of judge leniency for subcategories of individuals, such as young versus old filers, are additionally predictive beyond the average leniency for a judge. If the monotonicity assumption holds, these subgroup specific measures of judicial leniency should not be predictive of case outcomes after we condition on average judge leniency. Consistent with our monotonicity assumption, we find that only the average measure of judge leniency is a statistically significant predictor of case outcomes. None of the subgroup specific measures of judicial leniency are statistically significant, and a joint test of significance yields a p-value of 0.776. Using principal component analysis, we also find no evidence of clustering in each judge's subgroup specific leniency measures. These results further suggest that judicial leniency does not vary across different types of filers. None of our results suggest that the monotonicity assumption is likely to be invalid

Figure 1.3: Judge Leniency by Filer Characteristics



Notes: These figures show the correlation between judge leniency for different groups of filers. Age is determined at the time of filing, and credit score and homeownership are determined in the full year prior to filing. The sample consists of all first-time filers between June 2002 and 2005 in the 39 offices that randomly assign filings to judges. Judge leniency is defined as the leave-one-out mean rate of granting bankruptcy protection for the assigned judge minus the leave-one-out mean rate of granting bankruptcy protection for the office. We take the average leniency for each group over all available years of data. Subgroup instruments are constructed using the matched estimation sample. The solid line shows the best linear fit estimated using OLS relating each judge leniency measure.

in our setting.

## **1.4 Results**

We begin by exploring the impact of Chapter 13 protection on financial strain associated with debt collection, the amount of unsecured debt, and retention of assets. We then estimate the indirect effects of Chapter 13 protection on credit access and credit score. We conclude by examining the most likely mechanisms driving our results and testing the robustness of our empirical design.

### **1.4.1 Financial Strain**

Panel A of Table 1.6 reports two-stage least squares estimates for our financial strain index and each individual component of the index. As discussed above, each individual component of the financial strain index is the average of five indicator variables for having experienced the listed event from the filing year to the fifth post-filing year. For all of our financial strain measures, our estimates should be interpreted as the average change in the probability of experiencing an event each year. The financial strain index combines all eight adverse financial events, as described previously. Our estimation sample consists of Chapter 13 bankruptcy filers originating from offices that randomly assign filers to judges between 2002 and 2005 that are linked to the credit report data in the year of filing. Column 1 reports the mean post-filing average probability for each event for dismissed filers. Column 2 presents two-stage least squares estimates using our leave-one-out measure of judge leniency controlling only for office-by-filing-month fixed effects. Column 3 adds controls for baseline age bins, homeownership, financial strain, revolving, mortgage, auto, and collections debt, indicators for open mortgage and open auto loans, revolving utilization, non-mortgage inquiries, and credit score. Details on these measures can be found in the data appendix. We report standard errors clustered at the office level throughout.

**Table 1.6: Chapter 13 Protection and Financial Well-Being**

	Dismissed	2SLS Results	
	Mean	(2)	(3)
<i>Panel A: Adverse Financial Events</i>	(1)	(2)	(3)
Financial Strain Index	-0.068 (0.575)	-0.369*** (0.070)	-0.316*** (0.071)
Delinquency	0.596 (0.292)	0.019 (0.017)	0.013 (0.018)
Collection	0.584 (0.305)	-0.201*** (0.053)	-0.149** (0.061)
Charge-off <sup>†</sup>	0.216 (0.227)	-0.065*** (0.017)	-0.067*** (0.020)
New Bankruptcy <sup>†</sup>	0.109 (0.167)	-0.064*** (0.023)	-0.065*** (0.025)
Foreclosure	0.070 (0.139)	-0.030*** (0.009)	-0.016* (0.008)
Judgment	0.066 (0.128)	-0.039** (0.016)	-0.031* (0.016)
Lien	0.034 (0.099)	-0.034*** (0.012)	-0.034** (0.013)
Repossession	0.019 (0.064)	-0.015*** (0.006)	-0.016** (0.006)
<i>Panel B: Unsecured Debt and Collections</i>			
Revolving Balance	2.563 (6.065)	0.199 (0.720)	-0.920 (0.745)
Collection Balance	4.217 (5.898)	-1.842*** (0.449)	-1.315*** (0.432)
<i>Panel C: Secured Assets</i>			
Have a Mortgage	0.363 (0.369)	0.110** (0.045)	0.132*** (0.021)
Mortgage Balance	26.833 (41.359)	12.196*** (4.132)	14.267*** (4.955)
Have an Auto Loan	0.178 (0.263)	0.046* (0.028)	0.020 (0.032)
Auto Balance	4.067 (5.797)	-0.500 (0.500)	-0.904 (0.581)
<i>Panel D: Credit Access</i>			
Revolving Utilization	46.729 (46.437)	-15.132*** (3.884)	-16.148*** (3.282)
Non-Mortgage Inquiries	1.584 (1.487)	-0.410*** (0.121)	-0.293** (0.118)

Continued on next page

**Table 1.6: Chapter 13 Protection (continued)**

	Dismissed	2SLS Results	
	Mean	(2)	(3)
	(1)		
<i>Panel E: Credit Score</i>			
Credit Score	565.433 (44.543)	28.511*** (4.234)	14.981*** (3.270)
Controls	–	No	Yes
Observations	97006	175076	175076

Notes: This table reports two-stage least squares results of the impact of Chapter 13 bankruptcy protection on post-filing outcomes. The sample consists of Chapter 13 bankruptcy filers originating from offices that randomly assigns filers to judges between 2002-2005 that are linked to credit report data in the year of filing. All outcomes are annual averages for the year of filing to fifth year post-filing, with the exceptions of outcomes with a † where outcomes are averaged over the first full year after filing to the fifth year post-filing to remove the mechanical effect of the bankruptcy filing. Column 1 reports the post-filing mean and standard deviation for dismissed filers. Columns 2-3 instrument for Chapter 13 protection using the leave-one-out mean rate of granting Chapter 13 bankruptcy protection for the assigned judge minus the leave-one-out mean rate of granting bankruptcy protection for the office. All regressions control for office-by-filing-month fixed effects and cluster standard errors at the office level. Column 3 adds controls for baseline age bins, homeownership, credit score, financial strain index, revolving balance, collection balance, mortgage balance, auto balance, indicators for mortgage and auto loans, revolving utilization, and non-mortgage inquiries as controls. The financial strain index contains the non-cumulative probabilities of the following eight components: delinquency, collection, charge-off, bankruptcy, foreclosure, judgment, lien and repossession. For each year post-filing, each component is standardized using the mean and standard deviation for the dismissed filer group in the baseline year. We sum across the eight components to create an index, restandardizing using the mean and standard deviation of the dismissed filer group in the baseline period. The index is then averaged over the five years post-filing. See the data appendix for details on the data and variable construction. \*\*\* = significant at 1 percent level, \*\* = significant at 5 percent level, \* = significant at 10 percent level.

We find that Chapter 13 significantly improves financial well-being by reducing the likelihood of adverse financial events and creditor actions. Over the first five post-filing years, Chapter 13 protection decreases the marginal recipient’s level of financial strain by 0.316 to 0.369 standard deviations. Turning to each individual component, we find that Chapter 13 protection decreases the marginal recipient’s probability of having a debt in collections by 14.9 to 20.1 percentage points, a 25.5 to 34.4 percent decrease from the dismissed filer mean of 58.4 percent. Credit charge-offs decrease by 6.5 to 6.7 percentage points, a 30.0 to 31.0 percent decrease from the dismissed filer mean of 21.6 percent. Subsequent bankruptcy filings (of all chapters) decrease by 6.4 percentage points, a 58.7 percent decrease from the dismissed filer mean of 10.9 percent. Foreclosures decrease by 1.6 to 3.0 percentage points, a 22.9 to 42.8 percent decrease from the dismissed filer mean of 7.0 percent. Creditor



judgments decrease by 3.1 to 3.9 percentage points, a 46.9 to 59.0 percent decrease from the dismissed filer mean of 6.6 percent. Liens decrease by 3.4 percentage points, a 100.0 percent decrease from the dismissed filer mean of 3.4 percent. Repossessions decrease by 1.5 percentage points, a 78.9 percent decrease from the dismissed filer mean of 1.9 percent. Conversely, there is no impact of Chapter 13 protection on delinquency, defined as the probability of any trade being at least thirty days past due.<sup>15</sup>

Table 1.8 presents two-stage least squares results from our preferred specification separately by age, baseline credit score, and baseline homeownership. Chapter 13 reduces financial strain by 0.416 standard deviations for homeowners compared to just 0.165 standard deviations for non-homeowners, though the difference is not statistically significant. There are no economically or statistically significant differences by age or baseline credit score.

Consistent with Dobbie and Song (2015), we find that our results are driven by a deterioration of outcomes among dismissed filers rather than gains among granted filers (see Figure 1.4). Taking repossession as an illustrative example, we see that both dismissed and granted filers are more likely to experience a repossession than non-filers even before filing. In the four years before filing, repossession rates average 0.7 percent and 1.4 percent among granted and dismissed filers, respectively. Repossession rates increase to 1.1 percent for granted filers and 1.6 for dismissed filers in the year before filing, before peaking at 2.4 and 3.4 percent, respectively, in the year of filing. Repossession rates for dismissed filers remain elevated at approximately 1.4 percent throughout our sample period, while the rates for granted filers fall to about 0.5 percent, comparable to non-filers. These results are consistent with bankruptcy protection mitigating the long-term consequences of financial shocks that might otherwise harm debtors, but not conferring benefits in the absence of a financial shock.

We conclude this section by comparing the magnitude of our two-stage least squares

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<sup>15</sup>Panel A of Table 1.7 presents additional financial strain results. We find that Chapter 13 protection decreases both the number of paid and unpaid collections, but has no impact on the number of medical collections. The decrease in judgments is due to a decrease in unpaid judgments.

**Table 1.7: Results for Additional Outcomes**

	Dismissed		
	Mean	2SLS Results	
	(1)	(2)	(3)
<i>Panel A: Adverse Financial Events</i>			
Number of Paid Collections	0.744 (1.266)	-0.336*** (0.057)	-0.224*** (0.051)
Number of Unpaid Collections	4.251 (4.374)	-1.899*** (0.475)	-1.296** (0.528)
Number of Medical Collections	0.513 (0.942)	-0.077 (0.090)	-0.004 (0.101)
Number of Paid Judgments	0.087 (0.338)	-0.051** (0.023)	-0.032 (0.025)
Number of Unpaid Judgments	0.560 (0.977)	-0.339*** (0.107)	-0.238** (0.102)
<i>Panel B: Student Debt</i>			
Any Active Student Debt	0.167 (0.343)	0.003 (0.057)	0.026 (0.050)
Any Deferred Student Debt	0.038 (0.154)	-0.015 (0.027)	-0.010 (0.026)
<i>Panel C: Home Transitions</i>			
Living in Same Residence	0.496 (0.500)	0.270*** (0.049)	0.250*** (0.054)
Moved to Rental	0.429 (0.495)	-0.267*** (0.065)	-0.250*** (0.059)
Move to Home	0.075 (0.263)	-0.003 (0.040)	-0.001 (0.040)
<i>Panel D: Revolving Trades</i>			
Number of Open Revolving Trades	0.766 (1.312)	0.576*** (0.144)	0.321** (0.130)
Credit Limit Revolving Trades	6.083 (12.691)	3.362*** (1.256)	-0.599 (0.801)
Controls	-	No	Yes
Observations	97006	175076	175076

Notes: This table reports two-stage least squares results of the impact of Chapter 13 bankruptcy protection on additional outcomes available in the credit bureau data. The sample consists of Chapter 13 bankruptcy filers originating from offices that randomly assigns filers to judges between 2002-2005 that are linked to credit report data in the year of filing. All outcomes are measured over the first five post-filing years. Column 1 reports the post-filing mean and standard deviation for dismissed filers. Columns 2-3 instrument for Chapter 13 protection using the leave-one-out mean rate of granting Chapter 13 bankruptcy protection for the assigned judge minus the leave-one-out mean rate of granting bankruptcy protection for the office. All regressions control for office-by-filing-month fixed effects and cluster standard errors at the office level. Column 3 adds controls for baseline age bins, homeownership, credit score, financial strain index, revolving balance, collection balance, mortgage balance, auto balance, indicators for mortgage and auto loans, revolving utilization, and non-mortgage inquiries as controls. \*\*\* = significant at 1 percent level, \*\* = significant at 5 percent level, \* = significant at 10 percent level.

**Table 1.8: Chapter 13 Results by Filer Characteristics**

	Age at Filing		60 and up (3)	Baseline Credit Score		Baseline Homeowner	
	25 to 39 (1)	40 to 59 (2)		High (4)	Low (5)	Yes (6)	No (7)
Financial Strain	-0.402*** (0.085)	-0.302*** (0.070)	-0.388 (0.405)	-0.327*** (0.065)	-0.344*** (0.071)	-0.416*** (0.047)	-0.165* (0.091)
Revolving Balance	[0.040]	[-0.041]	[-0.248]	[-0.132]	[0.012]	[0.010]	[-0.184]
	0.059	-1.304	1.168	-1.249	-0.011	-0.817	-0.586
	(0.700)	(1.130)	(1.985)	(0.996)	(0.774)	(0.747)	(0.918)
	[2.171]	[2.961]	[4.282]	[3.161]	[2.226]	[3.061]	[1.676]
Collection Balance	-1.719**	-1.269***	-1.528	-2.210***	-0.384	-1.531**	-1.011**
	(0.747)	(0.410)	(1.589)	(0.433)	(1.089)	(0.641)	(0.474)
	[4.737]	[4.160]	[2.950]	[3.354]	[4.737]	[3.935]	[4.661]
Mortgage Balance	11.449	18.233***	26.818***	15.583***	14.912*	20.372**	4.530
	(7.240)	(4.750)	(10.474)	(4.614)	(8.725)	(9.536)	(6.037)
	[25.937]	[31.594]	[28.102]	[29.721]	[26.420]	[39.126]	[6.404]
Auto Balance	-1.570**	-0.743	1.846	-0.698	-0.908*	-0.889	-0.923
	(0.780)	(0.790)	(1.729)	(0.837)	(0.478)	(0.592)	(1.023)
	[4.797]	[4.188]	[3.129]	[4.508]	[3.920]	[4.224]	[3.810]
Revolving Utilization	-21.246***	-13.434***	-7.594	-15.683***	-14.441**	-16.708***	-14.152***
	(6.534)	(3.011)	(9.937)	(5.045)	(5.652)	(2.577)	(6.707)
	[49.679]	[48.149]	[45.564]	[41.543]	[52.114]	[46.686]	[47.368]
Non-Mortgage Inquiries	-0.280	-0.340***	0.256	-0.440*	-0.193	-0.440**	-0.069
	(0.210)	(0.078)	(0.621)	(0.227)	(0.389)	(0.189)	(0.156)
	[1.843]	[1.552]	[1.221]	[1.422]	[1.714]	[1.595]	[1.548]
Credit Score	22.280***	9.952**	32.090***	18.579***	10.864	18.369***	7.573
	(5.913)	(4.211)	(11.259)	(5.718)	(7.662)	(2.576)	(7.158)
	[56.870]	[569.943]	[596.253]	[585.202]	[553.228]	[570.837]	[556.678]
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	54442	81206	19100	84154	82241	111432	58627

Notes: This table reports first stage and two-stage least squares results by baseline characteristics. The sample consists of Chapter 13 bankruptcy filers originating from offices that randomly assigns filers to judges between 2002-2005 that are linked to credit report data in the year of filing. The post-filing mean for dismissed filers is reported in brackets for each subgroup. We instrument for Chapter 13 protection using the leave-one-out mean rate of granting Chapter 13 bankruptcy protection for the assigned judge minus the leave-one-out mean rate of granting bankruptcy protection for the office. Subgroup instruments are constructed using the matched estimation sample. All regressions control for baseline age bins, homeownership, credit score, financial strain index, revolving balance, collection balance, mortgage balance, auto balance, indicators for mortgage and auto loans, revolving utilization, non-mortgage inquiries, office-by-filing-month fixed effects, and cluster standard errors at the office level. See the data appendix for details on the data and variable construction. \*\*\* = significant at 1 percent level, \*\* = significant at 5 percent level, \* = significant at 10 percent level.

estimates to the deterioration of outcomes for dismissed filers. This calculation provides a back-of-the-envelope approximation of the extent to which Chapter 13 protection mitigates the adverse consequences of financial distress. Specifically, we calculate the change in outcomes for dismissed filers by subtracting the average post-filing outcomes for the first five post-filing years from the pre-filing outcomes in the third to fourth years pre-filing. This calculation implies that Chapter 13 protection mitigates approximately 91.1 percent of the deterioration in the financial strain index. For our other financial strain measures, Chapter 13 protection mitigates approximately 28.6 percent of the deterioration in the probability of having a foreclosure and approximately 100 percent of the potential deterioration in the probability of having collections debt, a charge-off, a new bankruptcy, a judgment, a lien, and a repossession.

#### **1.4.2 Unsecured Debt and Collections**

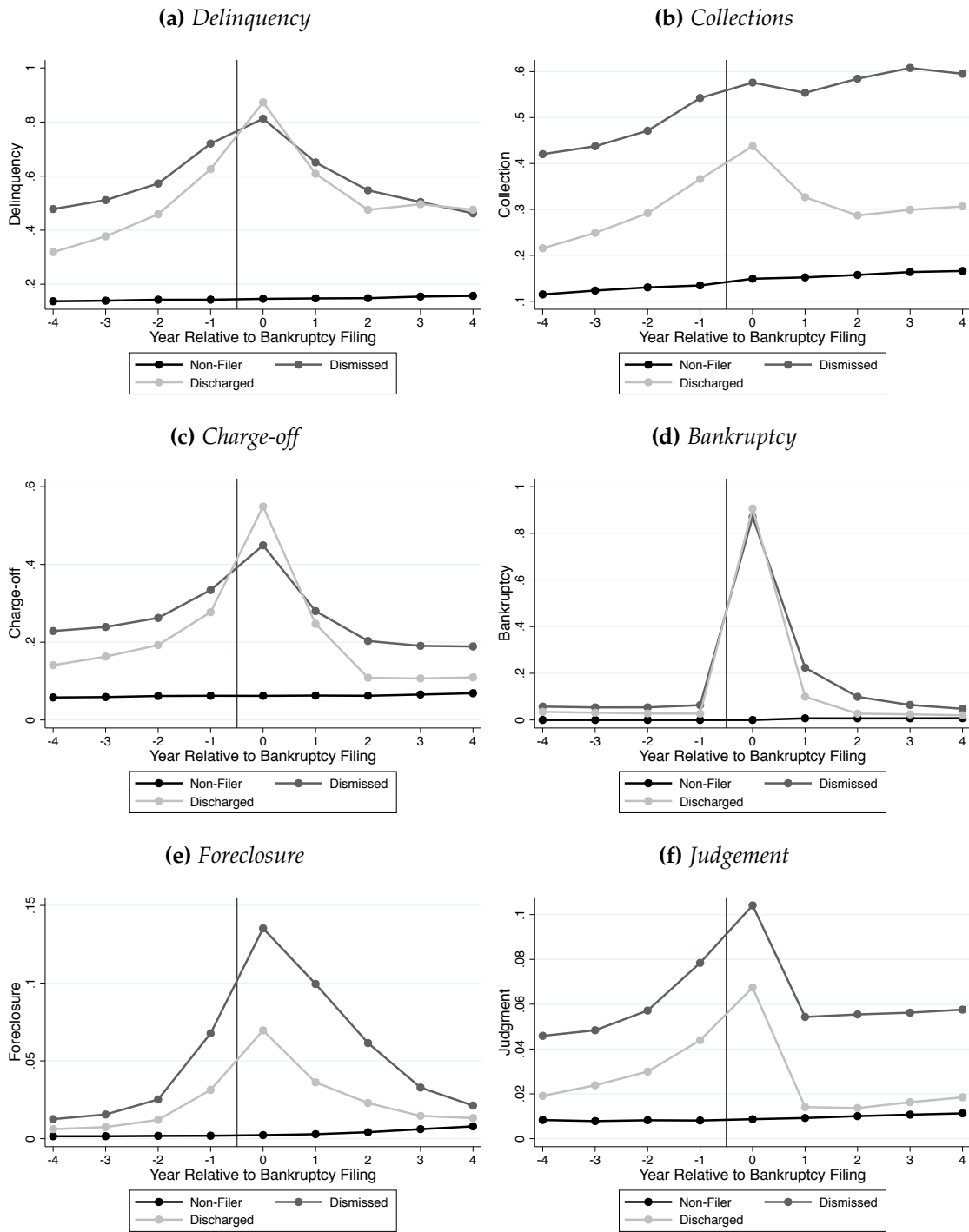
Panel B of Table 1.6 reports two-stage least squares estimates on open unsecured debt and collections debt. Each dependent variable is the average amount of debt reported in each category. We report results using the year of filing to the fifth post-filing year.

There is little impact of Chapter 13 protection on open unsecured debt. Point estimates are small and not statistically different from zero. However, the marginal recipient of Chapter 13 has \$1,315 to \$1,842 less debt in collections, a 31.2 to 43.6 percent decrease from the dismissed filer mean of \$4,217. The impact of Chapter 13 on collections debt is higher for filers with high baseline credit scores, but does not vary by age or homeownership.<sup>16</sup>

Comparison of means shows that open unsecured debt falls for both granted and dismissed filers post-bankruptcy. Collections debt increases for both groups before filing, but falls to pre-filing levels for granted filers only in the post-filing years. Taken together with our above results, these trends suggest that the marginal recipient of Chapter 13 protection reduces his or her unsecured debt through the bankruptcy system, while the

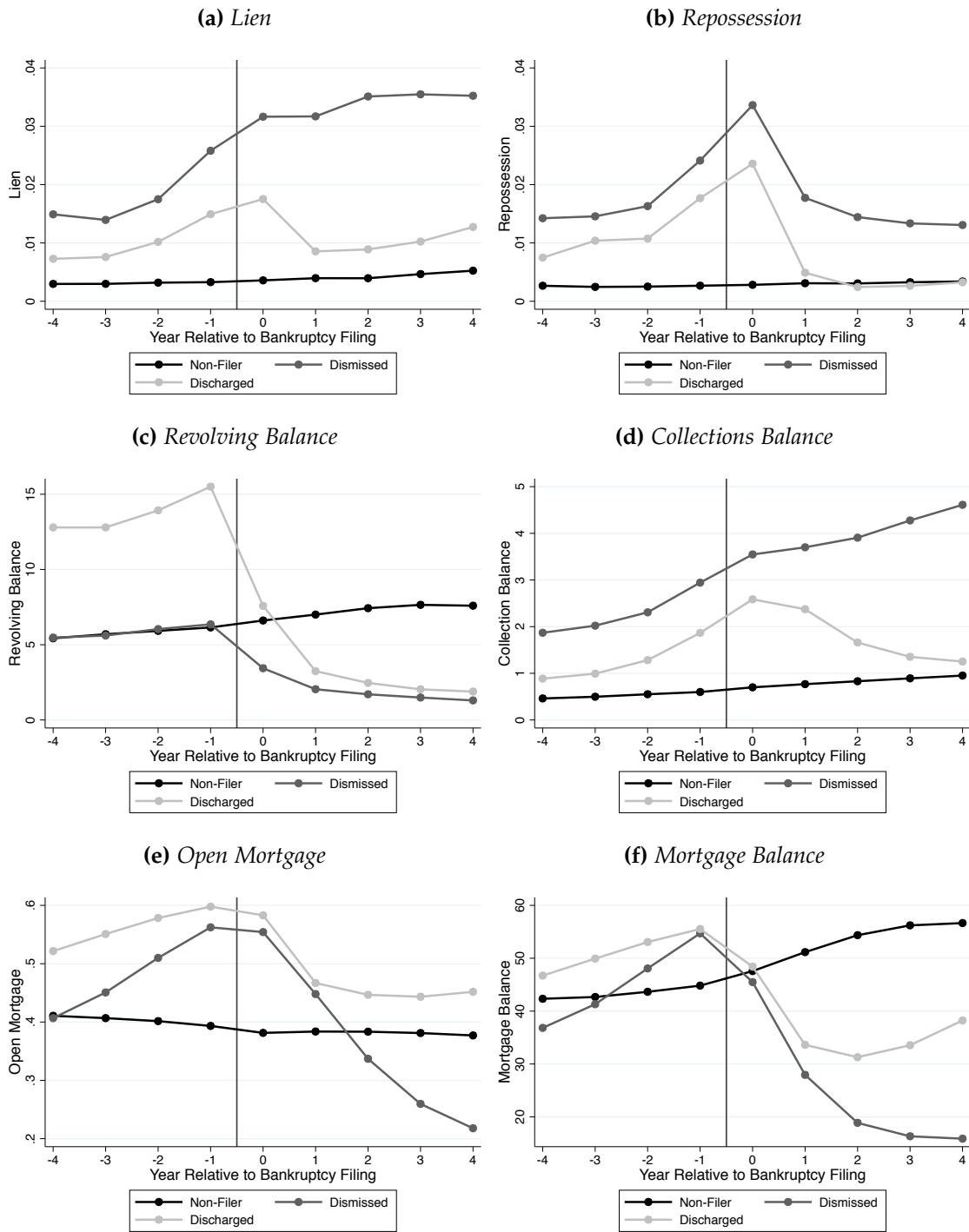
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<sup>16</sup>Panel B of Table 1.7 presents results for student debt, a form of unsecured debt that is not discharged under Chapter 13 protection. We find no impact of Chapter 13 on active or deferred student debt.



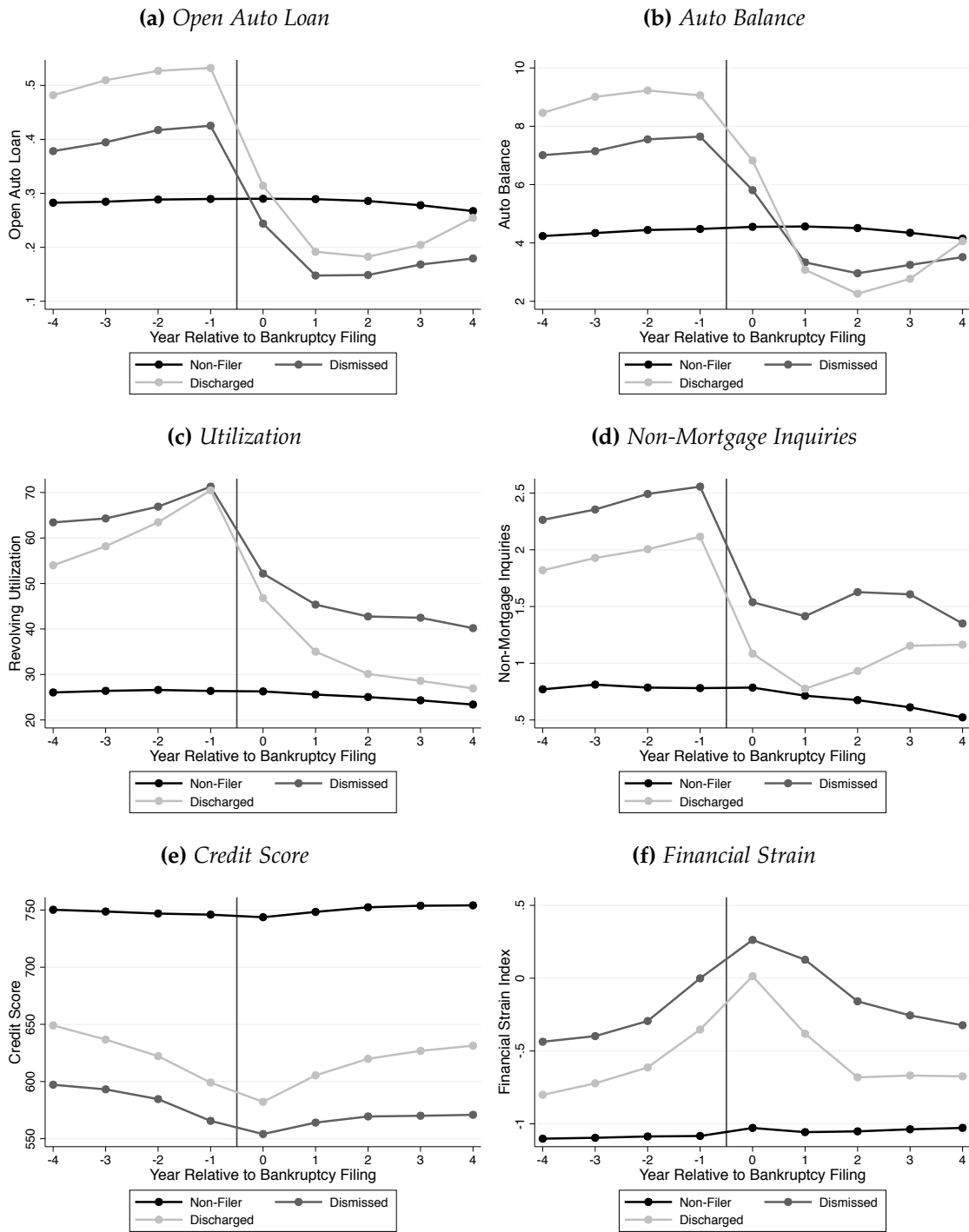
**Figure 1.4: Trends by Filing Status (1)**

Notes: These figures show the coefficients on year relative to filing dummies interacted with filer status: non-filer, dismissed filer, and discharged filer. Raw data figures include no controls.



**Figure 1.5: Trends by Filing Status (2)**

Notes: These figures show the coefficients on year relative to filing dummies interacted with filer status: non-filer, dismissed filer, and discharged filer. Raw data figures include no controls.



**Figure 1.6: Trends by Filing Status (3)**

Notes: These figures show the coefficients on year relative to filing dummies interacted with filer status: non-filer, dismissed filer, and discharged filer. Raw data figures include no controls.

marginal non-recipient is unable to prevent his or her unsecured debts from being sold to a third-party debt collector. Using these trend results, we estimate that Chapter 13 protection mitigates approximately 63.7 percent of the potential increase in collections debt.

### **1.4.3 Secured Assets**

Panel C of Table 1.6 reports two-stage least squares estimates for the probability of having an open mortgage, the average amount of mortgage debt, the probability of having an open auto loan, and the amount of auto debt. We report results using the year of filing to the fifth post-filing year.

We find that Chapter 13 protection significantly increases the probability of having a mortgage by 11.0 to 13.2 percentage points, a 30.3 to 36.4 percent increase from the dismissed filer mean of 36.3 percent. Chapter 13 protection also increases the marginal recipient's mortgage debt by \$12,196 to \$14,267, a 45.4 to 53.2 percent increase from the dismissed filer mean of \$26,833. Unsurprisingly, the impact of Chapter 13 on both homeownership and mortgage debt is higher for baseline homeowners. Effects are also larger for filers who are 60 or older at the time of filing.

The increase in mortgage debt may be the result of dismissed filers decreasing their mortgage debt by downsizing or becoming renters, or by granted filers increasing their mortgage debt by buying a new home. Panel C of Table 1.7 sheds light on this issue by estimating the impact of Chapter 13 protection on the probability of living in the same residence, the probability of moving to a rental, and the probability of moving to a home. Each dependent variable is measured in the fifth post-filing year, with identical results for earlier years. Chapter 13 protection increases the probability of staying in the same residence by 25.0 to 27.0 percentage points and decreases the probability of moving to a rental by 25.0 to 26.7 percentage points. These results are consistent with Chapter 13 decreasing the probability that filers sell or lose their homes.

Comparison of means provides additional evidence on this issue. The probability of having a mortgage increases in the years before filing for both granted and dismissed filers,



peaking the year before filing at 59.8 percent and 56.2 percent, respectively. For granted filers, the probability of having a mortgage falls modestly after filing to around 45 percent. For dismissed filers, the probability of having a mortgage falls all the way to 21.8 percent by the fifth year after filing. These trend results suggest that Chapter 13 protection alleviates more than 100 percent of the potential fall in homeownership rates.

We also explore the impact of Chapter 13 protection on whether a debtor has an open auto loan and average auto debt. Because at least some car owners do not have an open car loan, our measures of car ownership are lower bounds. Chapter 13 protection does not have a statistically significant effect on the probability of having a car loan or on auto debt. In Figure 1.6, we find that the probability of having a car loan falls in the year of filing for both granted and dismissed filers, suggesting that most debtors give up their cars. These results suggest that the retention of a home may be a more important priority for Chapter 13 filers. This interpretation is consistent with survey results showing that over seventy percent of dismissed filers choose to file under Chapter 13 bankruptcy to avoid foreclosure (Porter 2011).

#### **1.4.4 Credit Access**

Panel D of Table 1.6 reports two-stage least squares estimates for the total utilization on revolving accounts, and the number of non-mortgage inquiries. Each dependent variable is a five-year average from the year of filing to the fifth post-filing year. As discussed previously, while these outcomes have a less clear economic interpretation than our other outcomes, they are still suggestive of potential economic benefits to bankruptcy protection.

We find that Chapter 13 protection decreases revolving credit utilization, as measured by the balance to credit limit ratio. These revolving trades include bank credit cards, retail credit cards and check credit accounts. Revolving accounts are the most common type of credit accounts, representing 63 percent of all credit accounts, and about 71 percent of all open accounts (Avery et al. 2003). Utilization decreases by 15.1 to 16.1 percentage points, a 32.3 to 34.5 percent decrease from the dismissed filer mean of 46.7 percent. Chapter 13

protection also decreases the number of non-mortgage inquiries in the past six months by 0.293 to 0.410, a 18.5 to 25.9 percent decrease from the dismissed filer mean of 1.584. The impact of Chapter 13 on credit utilization is larger among younger filers, and the impact on non-mortgage inquiries is larger among baseline homeowners. Overall, these results suggest that Chapter 13 protection increases credit access.<sup>17</sup>

However, a comparison of means shows that utilization rates and non-mortgage inquiries fall (i.e. improves) for both granted and dismissed filers after filing, with larger falls for granted filers. For dismissed filers, average revolving utilization decreases by 19.3 percent and average non-mortgage inquiries decrease by 0.8 from the pre- to post-filing periods. These results are consistent with the pre-filing levels of credit usage being unsustainable for all filers. This suggests that our two-stage least squares estimates may be more correctly interpreted as Chapter 13 protection decreasing unmet credit demand, rather than increasing credit supply. Following our earlier back-of-the-envelope calculations, our estimates suggest that Chapter 13 protection further augments the fall in utilization by an additional 78.2 percent, and further augments the fall in the number of inquiries by an additional 36.5 percent.

#### **1.4.5 Credit Score**

Panel E of Table 1.6 reports two-stage least squares estimates for credit score, an aggregate measure of financial health or credit risk. The dependent variable is the mean credit score from the year of filing to the fifth post-filing year.

There is a large and precisely estimated impact of Chapter 13 bankruptcy protection on post-filing credit score. The two-stage least squares results with no controls suggest that Chapter 13 protection increases the marginal recipient's post-filing credit score by 28.5 points, a 5.0 percent increase from the dismissed filer mean of 565.4 points. With controls,

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<sup>17</sup>The increase in credit access is most likely the results of improved financial health documented above, as opposed to any limits on future bankruptcy filing. While discharged Chapter 13 filers have to wait six years before filing under Chapter 7, filers are still in the process of making plan payments during three to five years post-filing.

the estimated impact is 14.9 points, a 2.6 percent increase from the dismissed filer mean. The smaller point estimate when baseline controls are included is likely due to baseline credit scores being modestly different for filers assigned to more and less lenient judges in our matched sample. Estimates are larger for homeowners and filers with higher baseline credit scores.

Consistent with our earlier results, a comparison of means shows that credit scores for both granted and dismissed bankruptcy filers deteriorate several years before filing. In the years prior to filing for bankruptcy, Chapter 13 filers experience a 40 to 50 point decline in credit score. Credit scores increase slightly for both dismissed and granted filers after filing, with granted filers experiencing a quicker score increase. Following our above calculations, we find that Chapter 13 protection mitigates about 50.8 percent of the potential deterioration in credit scores.

#### **1.4.6 Potential Mechanisms**

In this section, we explore two potential mechanisms that might explain our findings: (1) protection from debt collectors and (2) debt forgiveness.<sup>18</sup>

We test the importance of the Chapter 13 provision that puts a hold on debt collection efforts using across-state variation in state garnishment laws. In the four states that do not allow wage garnishment – Florida, Pennsylvania, South Carolina, and Texas – creditors have fewer options to collect unpaid debts from dismissed filers. Treatment effects in states that allow wage garnishment include the effect of the hold on debt collection, debt forgiveness, and asset retention. Treatment effects in states that do not allow wage garnishment only include the effect of debt forgiveness and asset retention. If the two sets of estimates are

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<sup>18</sup>The retention of assets is a third potential mechanism that we are unable to fully test. One partial test of this hypothesis is to compare treatment effects for baseline homeowners to baseline renters. In Table 1.8, we find positive benefits of Chapter 13 protection for both homeowners and non-homeowners, but results are somewhat larger for homeowners. These results suggest that retention of important assets, such as a home, is a modest but important mechanism explaining our results. An alternative test of this mechanism is to compare treatment effects for homeowners and non-homeowners in states with and without judicial foreclosure. In states without judicial foreclosure, creditors can initiate foreclosure proceedings more easily. It is plausible that Chapter 13 protection may have a larger impact for homeowners in these states if the retention of the home is an important driver of our results.

different, this implies that the hold on debt collection is empirically important.<sup>19</sup> Table 1.9 presents two-stage least squares results for filers in states that do and do not allow wage garnishment. Consistent with our hypothesis, we find large and statistically significant effects of Chapter 13 protection in states that allow wage garnishment, and small and imprecisely estimated effects in the four states that prohibit wage garnishment. However, only one of the eight differences is statistically significant due to the imprecision of the point estimates in states that do not allow wage garnishment. These results are therefore consistent with there being significant costs of not being protected from debt collectors that may help explain the deterioration of outcomes among dismissed filers, but are far from conclusive evidence.

Next, we test the importance of debt forgiveness using across-state variation in Chapter 7 homestead exemption levels. Recall that the Chapter 13 repayment plan must pay unsecured creditors at least as much as they would receive under Chapter 7. Moreover, the amount that unsecured creditors receive under Chapter 7 depends on state home exemption levels. If debt forgiveness is empirically important, the effect of Chapter 13 protection should therefore be larger in states that have high exemptions where filers are able to discharge a larger fraction of their debt. Table 1.9 presents two-stage least squares results for baseline homeowners and renters in states with above and below median home exemption levels. The results are broadly consistent with the idea that benefits of Chapter 13 protection are larger when more debt is forgiven. However, once again, only two of the eight differences are statistically significant due to large standard errors. The effect of Chapter 13 protection on financial strain is 0.175 standard deviations larger for homeowners in high exemption states compared to homeowners in low exemption states (p-value=0.116), and the effect on mortgage balance is \$27,709 larger (p-value=0.008). For renters, we find that the effect of Chapter 13 protection on auto balance is larger in high exemption states because states

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<sup>19</sup>It is plausible that the decision to file for Chapter 13 is influenced by state wage garnishment laws. If the effects of Chapter 13 protection are different for these filers, our estimates will also incorporate this heterogeneity of treatment effects. The same logic applies to our Chapter 7 exemption results in Table 1.10.

**Table 1.9: Chapter 13 Results by State Wage Garnishment Laws**

	Garnishment Allowed		
	Yes	No	p-value
	(1)	(2)	(3)
Financial Strain	-0.319*** (0.070) [-0.058]	-0.132 (0.242) [-0.167]	0.449
Revolving Balance	-1.061 (0.654) [2.477]	6.606 (5.119) [3.407]	0.132
Collection Balance	-1.285*** (0.441) [4.190]	-2.888 (1.913) [4.482]	0.407
Mortgage Balance	13.985*** (5.190) [26.517]	29.399 (29.864) [29.940]	0.606
Auto Balance	-1.017* (0.532) [4.051]	5.158 (5.366) [4.223]	0.245
Revolving Utilization	-16.619*** (3.477) [46.375]	9.060 (27.639) [50.211]	0.349
Non-Mortgage Inquiries	-0.319*** (0.120) [1.584]	1.086 (0.818) [1.584]	0.084
Credit Score	15.243*** (3.180) [564.281]	2.614 (27.332) [576.741]	0.643
Controls	Yes	Yes	-
Observations	154611	20465	-

Notes: This table reports two-stage least squares results of the impact of Chapter 13 bankruptcy protection for states that do and do not allow wage garnishment. The sample consists of Chapter 13 bankruptcy filers originating from offices that randomly assigns filers to judges between 2002-2005 that are linked to credit report data in the year of filing. The post-filing mean for dismissed filers is reported in brackets for each subgroup. We instrument for Chapter 13 protection using the leave-one-out mean rate of granting Chapter 13 bankruptcy protection for the assigned judge minus the leave-one-out mean rate of granting bankruptcy protection for the office. Subgroup instruments are constructed using the matched estimation sample. All regressions control for baseline age bins, homeownership, credit score, financial strain index, revolving balance, collection balance, mortgage balance, auto balance, indicators for mortgage and auto loans, revolving utilization, non-mortgage inquiries, office-by-filing-month fixed effects, and cluster standard errors at the office level. See the data appendix for details on the data and variable construction. \*\*\* = significant at 1 percent level, \*\* = significant at 5 percent level, \* = significant at 10 percent level.

with high home exemptions also have high auto exemptions.<sup>20</sup> These results suggest that debt forgiveness also plays an important role in explaining our findings, though again the evidence is far from conclusive.

#### 1.4.7 Additional Robustness Tests

Table 1.11 explores the robustness of our main results to alternative measures of judge leniency. Column 1 replicates our preferred estimates from Table 1.6 using the leave-one-out measure of judge leniency as an instrument for Chapter 13 protection. Column 2 uses a leave-month-out version of judge leniency as an instrument for Chapter 13 protection that purges any remaining correlation between a filer's outcomes and our instrument introduced by the estimation of the office-by-filing-month fixed effects in our first and second stage regressions. Column 3 uses a leave-one-out measure of judge leniency calculated using decisions after only 90 days to proxy for the initial judicial decision to confirm or dismiss a filing.<sup>21</sup> Column 4 uses a randomly selected subset of 25 percent of filers to calculate a leave-month-out measure of judge leniency that is used as an instrument in the mutually exclusive subset of filers. We also test the robustness of our results using judge fixed effects directly rather than our reduced form measure of judge leniency. Columns 5 through 7 present results that use judge fixed effects as instruments for bankruptcy protection estimated using two-stage least squares, LIML, and jackknife IV, respectively. Results across all specifications are nearly identical to our preferred specifications. None of the estimates suggest that our preferred estimates are invalid.

Table 1.12 examines the persistence of our findings using outcomes for an unbalanced panel of filers in the sixth through eighth post-filing years. In the sixth through eighth

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<sup>20</sup>The correlation between a state's homestead exemptions level and auto exemptions level is 0.43.

<sup>21</sup>We calculate judge leniency using decisions after 90 days because the bulk of dismissals occur within 90 days of filing. Under the Bankruptcy Code, the Chapter 13 trustee must hold a meeting of creditors between 21 and 50 days after the debtor files for bankruptcy. A judge is required to hold a confirmation hearing for the proposed repayment plan no later than 45 days after the meeting of the creditors. Our results are qualitatively similar using decisions after 60 and 120 days post-filing.

**Table 1.10: Chapter 13 Results by State Homestead Exemption Laws**

	Homeowners			Renters		
	High	Low	p-value	High	Low	p-value
	Home Ex.	Home Ex.		Home Ex.	Home Ex.	
(1)	(2)	(3)	(4)	(5)	(6)	
Financial Strain	-0.454*** (0.025) [0.032]	-0.279** (0.114) [-0.022]	0.116	-0.180** (0.090) [-0.141]	-0.159 (0.182) [-0.214]	0.912
Revolving Balance	-1.149** (0.538) [3.333]	0.452 (1.782) [2.677]	0.373	-0.880 (1.066) [2.010]	-0.101 (1.105) [1.435]	0.592
Collection Balance	-1.670*** (0.602) [4.089]	-0.930 (1.898) [3.718]	0.692	-1.019* (0.616) [4.699]	-1.135 (0.793) [4.634]	0.897
Mortgage Balance	26.390*** (7.832) [42.984]	-1.319 (7.902) [33.692]	0.008	5.566 (6.412) [8.444]	7.000 (5.018) [4.934]	0.853
Auto Balance	-0.916 (0.708) [4.327]	-0.594 (0.797) [4.080]	0.755	-1.548** (0.736) [4.449]	0.697 (0.993) [3.350]	0.046
Revolving Utilization	-17.234*** (1.712) [46.774]	-16.214* (9.174) [46.561]	0.910	-9.741* (5.558) [47.841]	-26.188*** (9.145) [47.027]	0.100
Non-Mortgage Inquiries	-0.501** (0.244) [1.637]	-0.194 (0.328) [1.537]	0.435	-0.169 (0.108) [1.596]	0.142 (0.267) [1.514]	0.215
Credit Score	18.776*** (2.658) [572.428]	16.311*** (5.572) [568.595]	0.676	9.496* (5.056) [561.523]	2.743 (15.766) [553.179]	0.650
Controls	Yes	Yes	-	Yes	Yes	-
Observations	61334	50098	-	25900	32727	-

Notes: This table reports two-stage least squares results of the impact of Chapter 13 bankruptcy protection for states with above median and below median homestead exemption amounts, separately by homeownership status. The sample consists of Chapter 13 bankruptcy filers originating from offices that randomly assigns filers to judges between 2002-2005 that are linked to credit report data in the year of filing. The post-filing mean for dismissed filers is reported in brackets for each subgroup. We instrument for Chapter 13 protection using the leave-one-out mean rate of granting Chapter 13 bankruptcy protection for the assigned judge minus the leave-one-out mean rate of granting bankruptcy protection for the office. Subgroup instruments are constructed using the matched estimation sample. All regressions control for baseline age bins, homeownership, credit score, financial strain index, revolving balance, collection balance, mortgage balance, auto balance, indicators for mortgage and auto loans, revolving utilization, non-mortgage inquiries, office-by-filing-month fixed effects, and cluster standard errors at the office level. See the data appendix for details on the data and variable construction. \*\*\* = significant at 1 percent level, \*\* = significant at 5 percent level, \* = significant at 10 percent level.

**Table 1.11: Robustness of Main Results**

	Judge Leniency			Judge Fixed Effects			
	Own-Out (1)	Month-Out (2)	90 Days (3)	Split Sample (4)	2SLS (5)	LIML (6)	Jackknife (7)
Financial Strain	-0.316*** (0.071)	-0.305*** (0.072)	-0.371*** (0.091)	-0.253*** (0.094)	-0.340*** (0.065)	-0.341*** (0.080)	-0.340*** (0.090)
Revolving Balance	-0.920 (0.745)	-0.914 (0.735)	-1.018 (0.642)	-0.938 (1.071)	-0.938 (0.616)	-0.965 (0.706)	-0.938 (1.002)
Collection Balance	-1.315*** (0.432)	-1.349*** (0.400)	-1.071 (1.157)	-1.809*** (0.576)	-1.385*** (0.392)	-1.349*** (0.475)	-1.385*** (0.528)
Mortgage Balance	14.267*** (4.955)	13.922*** (4.922)	25.538*** (8.713)	10.703* (6.640)	12.728** (5.503)	13.937** (6.245)	12.730 (9.714)
Auto Balance	-0.904 (0.581)	-0.790 (0.563)	-1.615 (1.189)	-1.172 (0.846)	-1.015* (0.529)	-1.128* (0.605)	-1.015 (0.648)
Revolving Utilization	-16.148*** (3.282)	-15.555*** (3.080)	-18.225*** (4.290)	-16.119*** (4.454)	-15.023*** (2.811)	-16.452*** (3.658)	-15.020*** (3.408)
Non-Mortgage Inquiries	-0.293** (0.118)	-0.291** (0.113)	-0.392 (0.298)	-0.217 (0.199)	-0.351*** (0.115)	-0.356** (0.140)	-0.351** (0.163)
Credit Score	14.981*** (3.270)	16.048*** (3.173)	16.252** (6.574)	15.047** (7.329)	16.426*** (3.407)	15.056*** (3.975)	16.430*** (3.785)
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	175076	175076	175076	131416	175076	175076	175076

Notes: This table reports robustness checks for our main results. The sample consists of Chapter 13 bankruptcy filers originating from offices that randomly assigns filers to judges between 2002-2005 that are linked to credit report data in the year of filing. Column 1 replicates our preferred estimates from Table 3 using leave-one-out judge leniency as an instrument for Chapter 13 protection. Column 2 uses a leave-month-out measure of judge leniency where we calculate judge leniency only using cases in all other months as an instrument for Chapter 13 protection. Column 3 uses a leave-one-out measure of judge leniency measured using the case decision after the first 90 post-filing days as an instrument. Column 4 uses a randomly selected subset of 25 percent of filers to calculate a leave-month-out measure of judge leniency that is used as an instrument in the mutually exclusive subset of filers. Columns 5-7 present results that use judge fixed effects as instruments for bankruptcy protection estimated using two-stage least squares, LIML, and jackknife IV. All regressions control for baseline age bins, homeownership, credit score, financial strain index, revolving balance, collection balance, mortgage balance, auto balance, indicators for mortgage and auto loans, revolving utilization, non-mortgage inquiries, office-by-filing-month fixed effects, and cluster standard errors at the office level. See the data appendix for additional details on the data and variable construction. \*\*\* = significant at 1 percent level, \*\* = significant at 5 percent level, \* = significant at 10 percent level.



post-filing years, Chapter 13 protection reduces the marginal recipient's financial strain by 0.127 standard deviations, reduces the amount of debt in collections by \$1,931, increases the probability of being a homeowner by 26.0 percentage points, and increases credit score by 28.8 points. While there is no change in the probability of having an open auto loan in the first five post-filing years (see Table 1.6), Chapter 13 protection increases the probability of having an auto loan by 12.6 percentage points by the sixth to eighth post-filing years, suggesting that dismissed filers are more likely to lose their cars several years after filing.

## 1.5 Reconciling Estimates with Prior Literature

Our results from Section 1.4 show that Chapter 13 protection has an economically and statistically significant impact on the marginal recipient's financial health. These results stand in sharp contrast to the prior literature showing few benefits of filing for bankruptcy protection using non-experimental methods (e.g. Han and Li 2007, 2011, Cohen-Cole, Duygan-Bump and Montoriol-Garriga 2013, Jagtiani and Li 2014). In this section, we explore the extent to which these contrasting results can be explained by bias in the non-experimental specifications used in this literature.<sup>22</sup>

We begin by revisiting the descriptive results comparing the means of granted and dismissed filers before and after filing. Outcomes for both dismissed and granted filers are worse than non-filers even before filing. Moreover, the outcomes of both dismissed and granted bankruptcy filers deteriorate one to two years before filing, and remain depressed after filing. For example, consider our credit score results discussed above. Four years

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<sup>22</sup>There are at least two other reasons why our estimates would deviate from the prior literature. First, the prior literature has largely relied on either small samples of survey responses (e.g. Han and Li 2007, 2011), or administrative credit bureau data over a limited time span (e.g. Cohen-Cole, Duygan-Bump and Montoriol-Garriga 2013). Our analysis uses administrative credit data linked to bankruptcy filings that cover at least five post-filing years for all filers. It is possible that the benefits of bankruptcy protection are only detectable with the larger sample size and a longer time horizon afforded by our data. Consistent with this explanation, we find that our results are more modest one year after filing compared to subsequent post-filing years. Second, many analyses have considered the effects of filing for Chapter 7 and Chapter 13 bankruptcy together (e.g. Han and Li 2011, Cohen-Cole, Duygan-Bump and Montoriol-Garriga 2013). Dobbie and Song (2015) find that the effects of Chapter 7 protection on labor supply and mortality are smaller than the effects of Chapter 13.

**Table 1.12:** Results in the Sixth to Eighth Post-Filing Years

	Dismissed		
	Mean	2SLS Results	
	(1)	(2)	(3)
Financial Strain Index	-0.416 (0.777)	-0.216*** (0.055)	-0.127** (0.054)
Revolving Balance	1.014 (3.878)	0.697* (0.360)	0.094 (0.348)
Collection Balance	4.666 (7.376)	-2.534*** (0.563)	-1.931*** (0.599)
Have a Mortgage	0.184 (0.371)	0.259*** (0.032)	0.260*** (0.028)
Mortgage Balance	15.861 (48.046)	34.967*** (11.495)	36.780*** (13.654)
Have an Auto Loan	0.190 (0.367)	0.127*** (0.041)	0.126*** (0.045)
Auto Balance	3.518 (6.972)	0.641 (0.655)	0.486 (0.700)
Revolving Utilization	34.362 (53.460)	-8.942 (8.636)	-8.601 (9.178)
Non-Mortgage Inquiries	0.974 (1.375)	-0.107 (0.184)	-0.037 (0.196)
Credit Score	576.720 (57.229)	45.603*** (7.761)	28.792*** (6.609)
Controls	-	No	Yes
Observations	83792	151655	151655

Notes: This table reports two-stage least squares results of the impact of Chapter 13 bankruptcy protection on post-filing outcomes in the sixth through eighth post-filing years. The sample consists of Chapter 13 bankruptcy filers originating from offices that randomly assigns filers to judges between 2002-2005 that are linked to credit report data in the year of filing and at least one observation in the sixth through eighth post-filing years. Column 1 reports the post-filing mean and standard deviation for dismissed filers. Columns 2-3 instrument for Chapter 13 protection using the leave-one-out mean rate of granting Chapter 13 bankruptcy protection for the assigned judge minus the leave-one-out mean rate of granting bankruptcy protection for the office. All regressions control for office-by-filing-month fixed effects and cluster standard errors at the office level. Column 3 adds controls for baseline age bins, homeownership, credit score, financial strain index, revolving balance, collection balance, mortgage balance, auto balance, indicators for mortgage and auto loans, revolving utilization, and non-mortgage inquiries as controls. See the data appendix for details on the data and variable construction. \*\*\* = significant at 1 percent level, \*\* = significant at 5 percent level, \* = significant at 10 percent level.

before filing, credit scores for both granted and dismissed filers are over 100 points lower than non-filers. In the years prior to filing for bankruptcy, both groups of filers experience a 40 to 50 point decline in credit score. Credit scores increase slightly for both dismissed and granted filers after filing, with granted filers experiencing a quicker score increase.

These descriptive results suggest that non-experimental estimates are likely to be biased downwards for at least two reasons. First, there are important differences between filers and non-filers that may not be fully accounted for by the controls that are typically available in survey data. Second, there is significant selection into filing even conditional on pre-filing characteristics. Our descriptive data suggests that bankruptcy filings are often the result of adverse shocks that have independent effects on outcomes even after filing. In this scenario, both OLS estimates with a non-filing comparison group and within-individual estimates will be downward biased. The magnitude of the bias is likely to be decreasing in the number of available baseline controls that can account for pre-filing differences, and the number of available baseline observations before the pre-filing deterioration of outcomes.<sup>23</sup>

Table 1.13 presents formal OLS and within-individual estimates that test these predictions. Columns 1 and 2 of Table 1.13 present OLS results comparing discharged Chapter 13 filers to non-filers living in the same zip code. We exclude dismissed Chapter 13 filers from this comparison to focus on the effects of receiving Chapter 13 protection.<sup>24</sup> Column 1 only controls for zip code-by-filing year fixed effects. Column 2 adds our standard set of baseline controls to account for observable differences between discharged filers and non-filers. Standard errors are clustered at the zip code level. We find that discharged Chapter 13 filers have lower credit scores and higher financial strain compared to non-filers. We also find that Chapter 13 filers have lower revolving, mortgage, and auto balances compared to non-filers.

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<sup>23</sup>The pre-filing fall in outcomes is similar to the drop in earnings among participants in job training programs. See Ashenfelter (1978), Ashenfelter and Card (1985), and Heckman and Hotz (1989) for additional discussion.

<sup>24</sup>To remain consistent with the panel format of our judge sample, we create pseudo filing events for non-filers in the national sample. We randomly assign all credit users in the national sample one of four pseudo filing years between 2003 and 2006. We then exclude individuals who filed for bankruptcy in any of the years prior to this pseudo filing date. Remaining individuals comprise the non-filer comparison group.

Collections balances, revolving utilization, and non-mortgage inquiries are higher among Chapter 13 recipients in our specification without controls, but lower when we include our baseline controls. These results are broadly consistent with those reported in Han and Li (2011), who use data from the Survey of Consumer Finances to show that bankruptcy filers have less wealth compared to non-filers.

Column 3 presents within-individual estimates comparing the pre- and post-filing outcomes of discharged Chapter 13 filers. We use three years of baseline data to estimate the individual fixed effects, and cluster standard errors at the individual level. We find that these within-individual estimates yield incorrectly signed point estimates for financial strain, mortgage balance, and credit scores. As discussed above, this finding is likely due to adverse shocks that independently affect post-filing outcomes. These results are also consistent with Cohen-Cole, Duygan-Bump, and Montoriol-Garriga (2013), who find that filers have less access to credit after filing using credit bureau data.<sup>25</sup>

Columns 4 through 6 present results comparing dismissed and discharged Chapter 13 filers. This approach is in the spirit of Bound's (1989) analysis of accepted and rejected Disability Insurance applicants, and more recent work estimating the effects of job loss on subsequent outcomes (Jacobson, Lalonde, and Sullivan 1993, von Wachter, Song, and Manchester 2009, Sullivan and von Wachter 2009). With the exception of Dobbie and Song (2015), the previous bankruptcy literature has not used dismissed filers as a comparison group. Following our earlier results with a non-filer comparison group, column 4 presents results with zip code-by-filing year fixed effects, column 5 adds our standard set of baseline controls, and column 6 adds individual fixed effects. We cluster standard errors at the office level for columns 4 and 5, and at the individual level for column 6. In contrast to the non-experimental estimates discussed above, the non-experimental estimates with a dismissed filer comparison group are broadly consistent with our judge IV estimates. We

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<sup>25</sup>Specifications that use only one year of pre-filing data to estimate the individual fixed effects yield point estimates that are correctly signed for most outcomes. This surprising result is due to the individual fixed effects being measured in a year where outcomes are most depressed. As a result, mean reversion in the outcomes of granted filers generates upwards bias.

**Table 1.13: Non-Experimental Estimates with Different Comparison Groups**

	All Credit Users		Ind. FE		Dismissed Filers	
	(1)	(2)	(3)	(4)	(5)	(6)
Financial Strain	0.505*** (0.004)	0.101*** (0.003)	0.039*** (0.004)	-0.407*** (0.023)	-0.337*** (0.014)	-0.098*** (0.006)
Revolving Balance	-3.094*** (0.053)	-10.178*** (0.100)	-11.555*** (0.099)	1.085*** (0.122)	-0.771*** (0.075)	-7.508*** (0.112)
Collection Balance	0.934*** (0.021)	-0.907*** (0.018)	0.010 (0.023)	-2.229*** (0.092)	-1.576*** (0.049)	-1.336*** (0.037)
Mortgage Balance	-8.011*** (0.374)	-34.320*** (0.377)	-17.744*** (0.293)	10.549*** (0.810)	7.584*** (0.893)	7.410*** (0.404)
Auto Balance	-0.651*** (0.031)	-3.914*** (0.033)	-5.530*** (0.054)	-0.078 (0.089)	-0.362*** (0.065)	-1.829*** (0.070)
Revolving Utilization	6.813*** (0.167)	-21.376*** (0.169)	-35.834*** (0.285)	-11.388*** (0.790)	-8.758*** (0.630)	-10.166*** (0.448)
Non-Mortgage Inquiries	0.336*** (0.007)	-0.332*** (0.006)	-1.096*** (0.010)	-0.484*** (0.028)	-0.322*** (0.022)	-0.042*** (0.015)
Credit Score	-118.878*** (0.866)	-21.275*** (0.342)	-2.973*** (0.308)	44.848*** (1.606)	24.414*** (0.668)	12.743*** (0.392)
Baseline Controls	No	Yes	No	No	Yes	No
Individual Fixed Effects	No	No	Yes	No	No	Yes
Observations	1721287	1721287	253014	175076	175076	563636

Notes: This table reports non-experimental estimates of the impact of Chapter 13 bankruptcy protection on post-filing outcomes. Columns 1-2 report OLS estimates comparing discharged filers in our estimation sample to all non-filers in the full sample of all credit users. Column 3 reports within-individual estimates comparing the pre- and post-filing outcomes of discharged filers using three years of baseline data. Columns 4-5 report OLS estimates comparing discharged filers to dismissed filers in our estimation sample. Column 7 reports within-individual estimates comparing discharged filers to dismissed filers in our estimation sample using three years of baseline data. See the text for additional details on each specification, and the data appendix for details on the data and variable construction. \*\*\* = significant at 1 percent level, \*\* = significant at 5 percent level, \* = significant at 10 percent level.

find that Chapter 13 filers granted bankruptcy protection have lower financial strain than dismissed filers, and have less collections debt, higher mortgage balances, more credit access, and higher credit scores.

In sum, the results from Table 1.13 are consistent with both OLS estimates using a non-filer comparison group and within-individual estimates being biased against finding any benefits of bankruptcy protection. Using these non-experimental approaches, we find qualitatively similar results to the prior literature (Han and Li 2007, 2011, Cohen-Cole, Duygan-Bump, and Montoriol-Garriga 2013). In contrast, estimates using a dismissed filer comparison group are broadly consistent with our judge IV estimates, suggesting that selection into filing accounts for most of the bias in non-experimental specifications.

## **1.6 Conclusion**

In this paper, we exploit the random assignment of bankruptcy filers to judges to estimate the impact of Chapter 13 bankruptcy protection on post-filing financial outcomes. We find that Chapter 13 protection reduces financial strain, increases the probability of being a homeowner, and reduces the amount of debt in collection. Chapter 13 protection also increases credit scores and credit access proxies. The effects of Chapter 13 protection are largest in states with more creditor-friendly laws and states with higher Chapter 7 exemption levels, suggesting that protection from debt collectors and debt forgiveness are key drivers of our results.

Our results complement earlier work by Dobbie and Song (2015) showing that Chapter 13 protection increases earnings and reduces mortality risk among marginal recipients. The results in this paper suggest that Chapter 13 protection also has important impacts on financial well-being and economic stability. Importantly, and contrary to much of the prior literature, we find that Chapter 13 decreases the likelihood of adverse financial events, allows debtors to retain important assets such as a home, and increases both credit access measures and credit scores. These results suggest that the benefits of bankruptcy protection are much broader than previously realized.

Both papers also find that the benefits of bankruptcy protection are driven by a deterioration of outcomes among dismissed filers, rather than gains among granted filers. These results provide new evidence on the mechanisms through which excessive debt and financial distress distort borrower behavior. In particular, our results suggest that both excessive debt and the debt collection process have significant long-term consequences, and that bankruptcy protection can ameliorate many of these adverse consequences.

The findings from this paper and Dobbie and Song (2015) will also help inform ongoing efforts to evaluate the welfare impact of the consumer bankruptcy system. These evaluations typically use quantitative models to weigh the trade-off between the ex-post consumption smoothing benefits provided by bankruptcy protection estimated in this paper, with the ex-ante increased borrowing costs suggested by economic theory (e.g. Athreya 2002, Li and Sarte 2006, Livshits, MacGee, and Tertilt 2007, Chatterjee and Gordon 2012). An important limitation of this literature has been the lack of empirical evidence on the magnitude of the benefits provided by bankruptcy protection for the marginal recipient. Our results suggest that the ex-post benefits of consumer bankruptcy on important outcomes, such as credit access and debt repayment, are significantly larger than previously assumed by this literature. Moreover, we find that consumer bankruptcy also impacts a number of outcomes previously assumed to be fixed, such as asset holdings and labor supply. We therefore view the incorporation of our empirical estimates into a general equilibrium model of the credit market as an important area for future research.

## Chapter 2

# Debtor Protections and the Great Recession<sup>1</sup>

### 2.1 Introduction

Debtor protections are a common feature of consumer credit markets. A typical justification for these protections is that they insure borrowers against negative idiosyncratic shocks. However, this insurance comes at the cost of more expensive borrowing ex ante and as a result, a less efficient allocation of capital. In this paper, we explore another way in which debtor protections affect economic efficiency, building on the literature examining the adverse consequences of corporate and household debt when there are negative aggregate shocks. These adverse consequences include fire sales of collateral (Shleifer and Vishny 1992) and depressed economic activity (Fisher 1933, Eggertson and Krugman 2012) when firms and consumers deleverage in response to the negative shock. We show empirically that debtor protections can mitigate these adverse consequences, but they can also exacerbate them.

The recent recession provides an ideal environment to assess the impact of debtor protections following an aggregate shock. House prices declined dramatically just prior to

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<sup>1</sup>Co-authored with Will Dobbie



the downturn, significantly decreasing household wealth. There is evidence that households in the regions hit hardest by this shock reduced their consumption more, leading to lower local employment in non-tradable sectors (Mian and Sufi 2010, 2011, 2014, Mian, Rao, and Sufi 2013). In theory, debtor protections can mitigate the fall in consumption and employment following this kind of house price shock by helping households delever without cutting consumption. However, it is also possible that debtor protections can exacerbate the adverse consequences of a house price collapse by encouraging mortgage defaults and foreclosures.

In this paper, we use state variation in non-recourse mortgages and bankruptcy homestead exemptions to estimate the effect of debtor protections on household balance sheets and the regional economy during the Great Recession. Both policies protect homeowners. Non-recourse mortgages allow debtors to default on underwater mortgages without any liability for the remaining mortgage balance, while bankruptcy homestead exemptions protect debtors' home equity from non-mortgage creditors in bankruptcy. Each policy protects different subsets of homeowners, with non-recourse mortgages protecting households with negative equity and bankruptcy homestead exemptions covering homeowners with positive equity. Theoretically, these debtor protections can provide protection even when households do not formally declare bankruptcy or go into foreclosure, by either improving the threat point for debtors during renegotiations or dissuading creditors from pursuing costly collection efforts (Dawsey and Ausubel 2009, Mahoney 2015).

We study non-recourse and bankruptcy homestead protections to contrast the effect of protection from secured versus unsecured creditors. Because the effect of default on secured loans can have a significant impact on the value of collateral, the distinction between secured and unsecured creditors may be important when designing debtor protections. If a debtor defaults on a mortgage, the forced sale of the house through foreclosure can depress the value of homes nearby (Campbell, Giglio and Pathak 2011, Mian, Sufi and Trebbi 2011). In contrast, unsecured loans like credit cards have no collateral and defaulting on them is not likely to cause any fire sales. Crucially, sizable fractions of the homeownership

population were eligible for each of these protections. Estimates of the fraction of underwater homeowners in 2010 range from 20 to 30 percent, and roughly 35 percent of positive equity homeowners were completely protected by bankruptcy homestead exemptions.

The key difficulty in estimating the effects of these debtor protections is the potential endogeneity of protection status. To begin, it could be the case that states with default-prone residents also have more lenient debtor protection laws. Our identification assumes that the distribution of debtor protections across states is exogenous. We assume that there is no systematic difference between states that is both correlated with our outcomes of interest and debtor protection laws. We support this assumption in four main ways. First, we argue that the historical origins of the debtor protection laws supports the idea that they were not put in place in response to recent aggregate shocks. Second, we run a battery of correlation tests with possible confounding characteristics and find no significant confounders. Third, we verify the robustness of our results by controlling for additional characteristics and omitting certain states. Finally, we perform several placebo tests that support our interpretation of the results. See Section 2.5.3 for further details.

Another threat to identification is that individuals may respond to the protections by manipulating their home equity to ensure protection. Specifically, homeowners may borrow less against their home equity and more from unsecured creditors in states with stronger home equity protections. To address this concern, we construct an instrument for each protection that isolates state differences in the generosity of the laws, and strips out any potential manipulation of home equity. We create our measure of protections using the simulated instruments technique introduced in Currie and Gruber (1996) and extended in Mahoney (2015). For a given state, we estimate the extent to which individuals of similar age and credit quality in other states are protected under that state's bankruptcy and non-recourse laws. By leaving out the individuals in our national sample who are from the state of interest, our instrument avoids the local endogenous response to state laws. Furthermore, we control non-parametrically for age and credit score to account for the direct effects of these characteristics.

Our analysis proceeds in two steps. We first analyze the effect of debtor protections on household balance sheets. We then turn to the regional economic consequences. Using data from 1.2 million individual-level credit reports, we find that both bankruptcy homestead exemptions and non-recourse protections reduced homeowners' debt from 2007 to 2010. Compared to individuals without non-recourse protection, underwater homeowners with non-recourse protection were 15.5 percentage points more likely to default on their mortgages, 9.4 percentage points more likely to experience foreclosures, and had their mortgage balances fall by 39,000 dollars more from 2008 to 2010. Similarly, homeowners whose home equity was fully protected under bankruptcy homestead exemptions were 2.5 percentage points more likely to default on non-mortgage debt, 3.3 percentage points more likely to have a non-mortgage debt charge-off and had credit card debt drop an average of 387 dollars. Moreover, we find no significant effect of bankruptcy homestead protections on mortgage debt, and no significant effect of non-recourse protections on non-mortgage debt. Our interpretation of these results is that homeowners used both protections to reduce their debt balances through defaults.

We next turn to the regional economic consequences. While both protections led to higher defaults and lower debt during the crisis, the regional economic effects of these two protections were very different. For bankruptcy homestead protections, increasing the fraction of protected homeowners raised county-level employment and state-level non-durable consumption growth from 2008 to 2011. A one standard deviation (roughly 30 percentage points) increase in the fraction of protected individuals increased non-tradable employment growth by 1.17 percentage points in a county, and had no statistically significant effect on employment in the tradable sector. We find that a one standard deviation increase in the fraction of individuals in a state with fully protected home equity is associated with a 1.44 percentage point increase in non-durable consumption growth. These results are consistent with the idea that changes in local consumption drove differences in non-tradable employment.

In contrast, we find that non-recourse laws are associated with lower county-level

employment and state-level consumption growth from 2008 to 2011. A one standard deviation increase (roughly ten percent) in the fraction of underwater individuals in a county with no liability in a foreclosure is associated with 1.37 percentage points lower employment growth. Moreover, a one standard deviation increase in the fraction of individuals in a state with no liability in foreclosure decreases non-durable goods consumption by 0.88 percentage points. What drives these differences between non-recourse and homestead protection? We find evidence that areas with higher non-recourse protections had a larger decline in regional house prices from 2007 to 2011, consistent with foreclosure sales depressing local house prices. A ten percentage point increase in the fraction of underwater homeowners with non-recourse protections at the zip code level is associated with a 4.69 percentage point decline in house prices from 2007 to 2011. We find no significant effect of bankruptcy homestead protections on house prices over this period. This evidence is consistent with housing wealth declines leading to a fall in consumption and employment, as areas with non-recourse protections suffered a larger fall in house prices and higher foreclosures.

Taken together, our findings suggest that while some forms of debtor protections can mitigate the adverse consequences of debt after a negative aggregate shock, others can exacerbate the effects. Protections from both secured and unsecured creditors appear to increase the probability of default and decrease debt loads after an aggregate shock. However, while greater protection from unsecured creditors increases local consumption and employment, protection from secured creditors can amplify the effects of a negative wealth shock by further depressing housing wealth. These negative spillovers appear to outweigh the benefits for the protected debtors. This suggests careful attention should be paid to the effects of debtor protections on the asset market for secured collateral. In contrast, homestead protections avoided this negative effect on surrounding homeowners and demonstrated the consumption and employment benefits of debt relief policies.

Our results provide new evidence that at least some forms of debtor protections can improve economic efficiency following a negative aggregate shock. However, an important caveat is that we do not account for the ex-ante effect of debtor protections in the run-up to

the crisis. It is possible that the high levels of borrowing observed before the recession were partially the result of the debtor protections examined in this paper. We are also not able to estimate the impact of non-recourse mortgages and bankruptcy homestead protections for individuals experiencing an idiosyncratic shock. As a result, we are not able to conduct a full welfare analysis of these debtor protection policies. Combining our estimates of the ex-post impact of debtor protections when there is a negative aggregate shock with a better understanding of these other potential effects of debtor protections remains an important area for future work.

This paper is related to an important literature showing how household balance sheet distress can amplify an economic downturn. Recent theoretical work suggests that demand shocks driven by household debt can affect the real economy due to nominal or labor market rigidities (e.g. Guerrieri and Lorenzoni 2011, Hall 2011, Midrigan and Philippon 2011, Eggertson and Krugman 2012, Farhi and Werning 2013). Empirically, Mian and Sufi (2010, 2011) and Mian, Rao, and Sufi (2013) find evidence suggesting that indebted households delevered by cutting consumption following the fall in house prices. Mian and Sufi (2014) also find that regional house price shocks lower employment in non-tradable sectors of the economy, and Mian, Sufi, and Trebbi (2014) use variation in state foreclosure laws to show that foreclosures led to a large decline in house prices, residential investment, and consumer demand from 2007 to 2009. Finally, Chodorow-Reich (2014) shows that lender balance sheet health also had an economically and statistically significant impact on employment at small and medium size firms during the financial crisis.

Our paper is also related to a large literature estimating the effect of debtor protections on financial markets. Pence (2006) finds that mortgage origination amounts are three to seven percent smaller in states with more debtor friendly foreclosure laws. Ghent and Kudlyak (2011) find that borrowers are more likely to default in non-recourse states, but find no effect of non-recourse laws on mortgage interest rates. Gropp et al. (1997) and Lin and White (2001) examine the cross-sectional relationship between bankruptcy laws and borrowing costs, while Severino, Brown, and Coates (2014) use within-state variation in bankruptcy law

to show that that an increase in Chapter 7 exemptions levels increases unsecured borrowing. Kuchler and Stroebe (2009) and Li et al. (2011) examine how bankruptcy laws affect mortgage default and foreclosure rates. Finally, Davila (2014) presents an analytic solution to the optimal bankruptcy asset exemption levels as a function of different elasticities, but focuses on the case of strictly idiosyncratic risk. Finally, Athreya (2002), Li and Sarte (2006), Livshits, MacGee, and Tertilt (2007), Chatterjee and Gordon (2012), and Mitman (2014) measure the welfare consequences of consumer bankruptcy laws using quantitative models of the credit market.

The remainder of the paper is structured as follows. Section 2.2 provides a brief overview of the relevant debtor protections and outlines our conceptual framework. Section 2.3 formalizes our testable empirical hypotheses. Section 2.4 describes our data and provides summary statistics. Section 2.5 details our empirical strategy. Section 2.6 presents estimates of the impact of debtor protections on household balance sheets, employment, consumption, and house prices. Section 2.7 concludes.

## **2.2 Background and Conceptual Framework**

We now describe the non-recourse mortgage and bankruptcy homestead protections in more detail. We focus on non-recourse and bankruptcy homestead protections for three reasons. First, they were pre-existing, broadly applicable and not designed in response to the recessions. Second, they protected the homeowner subpopulation most affected by the collapse in house prices from 2006 to 2010. Finally, they allow us to contrast the effects of policies targeted at similar individuals but with protections from different creditors.

Non-recourse mortgage laws protect homeowners with home prices that are less than the remaining balance on their mortgage, also known as “underwater” homeowners. Typically, underwater homeowners cannot simply sell their home and make the creditor whole. For example, if the mortgage balance is 100,000 dollars and the house is only worth 80,000, the debtor still owes 20,000 dollars to the mortgagor after the sale. In states with recourse mortgage policy, the creditor can sue a debtor for the remaining balance on the mortgage.

Conversely, debtors can walk away from the remaining 20,000 dollars with no additional liability in non-recourse states. Thus, non-recourse laws allow homeowners with negative equity to default on their mortgage with no additional liability, essentially forcing a transfer from their creditors.

Bankruptcy homestead laws instead protect homeowners with positive home equity.<sup>2</sup> Individuals granted bankruptcy are required to partially repay their creditors through the sale of their assets. However, the bankruptcy system allows certain assets to be protected from creditors, including home equity up to a state-specified amount. Moreover, there is significant variation in these state home equity protections. For example, consider a homeowner in 2007 with 100,000 dollars of home equity. If he files for bankruptcy in Massachusetts, which has a homestead exemption of 500,000 dollars, this home equity would be completely protected in bankruptcy from creditors attempting to recover on outstanding balances, such as credit card debt. Alternatively, if he filed for bankruptcy in Louisiana where the exemption is 25,000 dollars, the remaining 75,000 dollars of equity would be unprotected and seizable by creditors.

As our above discussion makes clear, both non-recourse mortgage laws and bankruptcy homestead exemption laws protect debtors by not allowing creditors to pursue assets after default. Instead, creditors are forced to write-off the debt, thereby transferring wealth from creditors to debtors. The benefits of these transfers can be substantial. In the case of non-recourse mortgages, the policy removes a large debt on the balance sheet of those with significantly underwater homes. For those homeowners with positive equity, bankruptcy exemptions allow homeowners to readjust their unsecured debt balances, which can be many thousands of dollars.

Both non-recourse and homestead exemption laws also increase the threat point for

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<sup>2</sup>The U.S. bankruptcy system allows debtors to choose between Chapter 7 bankruptcy that provides debt relief and protection from wage garnishment in exchange for a debtor's non-exempt assets, and Chapter 13 bankruptcy that adds the protection of most assets in exchange for a partial repayment of debt. Homestead exemptions only directly apply to Chapter 7 filers, which make up approximately 75 percent of all bankruptcy filings. Homestead exemptions also indirectly apply to Chapter 13 filers, as the amount that these filers are required to repay is linked to the amount they would have given up under Chapter 7. Throughout the paper, we use "bankruptcy" to refer to Chapter 7 bankruptcy protection.

consumers against creditors. The effect of this increased bargaining power can be realized in several ways. Consumers may default as we have described above, knowing that they are protected from creditors seeking the remaining debt balance. Alternatively, debtors may renegotiate with their creditors using this threat as a negotiating tool. For example, Mahoney (2015) uses variation in the amount of protected assets in bankruptcy to identify the effect of bankruptcy exemptions as a form of a health insurance. He finds that hospitals will renegotiate their bills down to the amount available to the hospitals in the case of bankruptcy. However, lenders may be hesitant to engage in renegotiation. There has been concern in housing markets that renegotiation may create a form of adverse selection as those individuals who would not otherwise default would threaten default in order to reduce their mortgage debts. Moreover, the costs of renegotiation may be relatively high, especially in unsecured credit card markets. As a result, creditors may choose to write off the debts after a default if the debtors are protected by these policies. Both scenarios should lead to a lower outstanding debt balance for the debtor.

However, our above discussion also makes clear that non-recourse and bankruptcy homestead protections differ in at least two important ways. First, non-recourse and bankruptcy exemption laws protect different types of assets for different populations of homeowners. Non-recourse protects the non-housing assets of homeowners whose home equity is completely eliminated. Conversely, homestead exemptions protect the housing wealth of homeowners with positive equity.

A second important distinction between non-recourse and homestead protections is the effect on housing. Since mortgages are collateralized, a default on mortgages leads to a very different outcome in house prices than a default on unsecured debt. With non-recourse mortgages, debt relief occurs when the asset, the house, is relinquished to the creditor. Typically this will happen in the form of a foreclosure, and the house will be auctioned. Campbell, Giglio and Pathak (2011) and Mian, Sufi and Trebbi (2011) show that these foreclosures can have a significant negative effect on surrounding home prices. Consequentially, forced sales have the adverse effect of lowering other homeowners' house



prices, and potentially encouraging more mortgage defaults. In contrast, the bankruptcy homestead exemptions are unlikely to have this kind of spillover effect. The transfer from creditors to debtors will typically not entail any forced sale of collateral, and consequentially not generate any externalities in the housing market.

## 2.3 Hypothesis Development

### 2.3.1 Household Debt Hypotheses

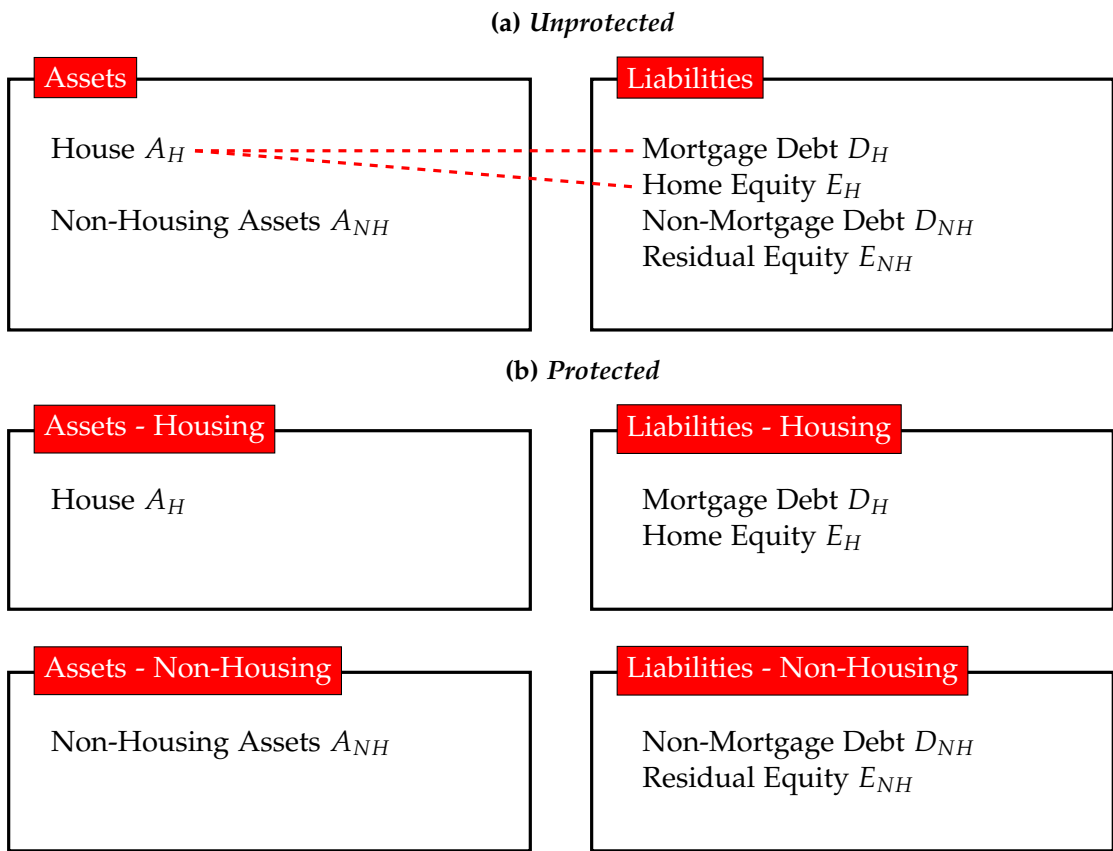
In this section, we formalize the testable implications from our conceptual framework using simple balance sheet terms. Let  $A_H$  be a homeowner's house price value and  $D_H$  be the home's mortgage debt. Let  $A_{NH}$  be non-housing assets and let  $D_{NH}$  be non-housing debt. Thus, the homeowner's assets are  $A_H$  and  $A_{NH}$ , and liabilities are  $D_H$  and  $D_{NH}$ . Figure 2.1a lays out these terms in a simple balance sheet framework without any form of protection.

For homeowners with negative home equity, or  $E_H = A_H - D_H < 0$ , non-recourse mortgages give homeowners the ability to default on their mortgages without the mortgage lender having any legal ability to recover the remaining balance on  $D_H$ . This turns the mortgage into a limited liability contract, since for all values of  $A_H$  greater than  $D_H$ , the homeowner has claim on the residual equity, but for values  $A_H - D_H < 0$ , the homeowner owes nothing. This implies that in states with non-recourse, homeowners with negative equity should be more likely to default on their mortgages than homeowners in states with recourse.<sup>3</sup>

In contrast, when the debtor has positive home equity, he does not need to default on his mortgage debt and can instead sell the asset. However, the debtor may still choose to default on non-mortgage (unsecured) debt,  $D_{NH}$ . Let  $E_S^*$  be the home equity protection in an individual's state, such that if a debtor defaults on non-mortgage debt, a creditor

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<sup>3</sup>The effects of limited liability are slightly richer than this, as non-recourse laws turn the mortgage into an option on house prices. Depending on the expectation of house price changes, at small negative values of home equity the option value in the mortgage may encourage less default. On average, however, the limited liability should encourage a broader default behavior as home equity values become more negative. See Deng et al. (2000).



**Figure 2.1:** *Stylized Homeowner Balance Sheet*

Notes: Figure 2.1a illustrates a simple balance sheet for a homeowner with no debtor protections. Mortgage Debt  $D_H$  is secured by the house,  $A_H$ , which non-mortgage debt is unsecured. In Figure 2.1b, the same balance sheet is presented with protections. With either non-recourse protection in the case of negative equity, or complete homestead protection in the case of positive equity and  $E_H < E_S^*$ , homeowner balance sheet can be treated as two separate balance sheets, with no cross-collateralization.

may seize up to  $\max\{E_H - E_S^*, 0\}$  of home equity. For every dollar of home equity that is seizable by the non-mortgage creditor, the value of defaulting on non-mortgage debt decreases. Therefore, a debtor with fully protected home equity should be more likely to default on non-mortgage debt than a debtor with unprotected home equity.

With the presence of protections, the standard balance sheet from Figure 2.1a separates into two different balance sheets, similar to Figure 2.1b. Since debtors cannot claim other assets, this lack of cross-collateralization makes the decision to default a function of the value of default within a particular balance sheet. This leads to two predictions regarding default probabilities: first, when  $A_H < D_H$ , those with non-recourse protections have higher mortgage default and foreclosure probabilities than recourse states. Second, if  $A_H - D_H > 0$ , those with  $E_H < E_S^*$  have a higher probability to default on non-mortgage debt than those with  $E_H > E_S^*$ .

### 2.3.2 Predictions for Macroeconomic Outcomes

The debt relief provided by these protection policies involves a transfer from creditors to debtors. In a zero-frictions model, the transfers from creditors to debtors should not have any macroeconomic benefits, as the benefit to the debtors should be offset by the costs borne by the creditors. However, a prominent feature of the recession was the significant debt burden that consumers carried into the downturn. Given a readjustment of income prospects and these debts, consumers without a default option would be forced to pay down their debts and reduce their consumption. If the marginal propensity to consume is higher for these indebted individuals receiving relief, transfers from creditors to debtors may stimulate consumer demand and potentially alleviate an aggregate demand shortfall.

Additionally, if changes in local consumption demand affect local non-tradable employment, debt relief may stimulate non-tradable employment as well. In the spirit of Mian and Sufi (2014), this mechanism works through wage rigidities in the local labor markets. As housing wealth falls and consumption declines in an area, demand for both tradables and non-tradables falls. However, while consumption of both tradables and non-tradables will

fall, only non-tradables employment should fall significantly, as tradable employment is cushioned by other markets to sell in. To the extent that these debt protection policies can alleviate the local consumption demand shortfall, the protections should cause an increase in non-tradable employment, but have no effect on employment in the tradable sector.

There are two potential negative effects that could counter the positive consumption benefits of deleveraging. The first is that default losses may cause creditors to tighten their lending to debtors. As a result, consumption may become more expensive and fall. This should be particularly true for consumption of goods that are typically funded using credit, such as automobile loans. Second, as discussed in the introduction, the foreclosure on housing collateral may lead to significant adverse effects on the housing market. As described in Campbell, Giglio and Pathak (2011), the illiquid and heterogeneous nature of housing markets makes the forced sale of a foreclosed home likely to either create “an imbalance of demand and supply in an illiquid housing market” or directly impact surrounding housing values through degradations or vandalism. Hence, the effect of debtor protections on secured debt may have a negative effect on surrounding house prices as foreclosed homes depress local housing values.

This reduction in local house prices can be important for regional consumption. As outlined in Mian, Rao and Sufi (2013), the fall in house prices lead to a substantial decline in housing wealth and consumption. Hence, the spillovers from foreclosed homes could have a substantial externality on local consumption as foreclosed homes depressed prices.

This leads to two macroeconomic predictions. First, debtor protections on unsecured debt should lead to increased consumption in goods that does not require financing, and has an ambiguous effect on consumption that requires financing. Moreover, this should lead to a higher level of employment in non-tradable sectors, and no effect in tradable sectors. Second, debtor protections on secured debt has an ambiguous effect on consumption, with the reduction in debt potentially increasing consumption, but the fall in house prices depressing regional consumption. This will be reflected in non-tradable employment as well, but not tradable employment.

## 2.4 Data

Our empirical analysis uses information from individual-level credit reports and aggregate employment, consumption, and house price data. This section details each data source and presents summary statistics for our analysis sample.

### 2.4.1 Individual Credit Reports

Information on household balance sheets come from TransUnion, one of the major consumer credit bureaus in the United States. The TransUnion data include information on account details for the near-universe of revolving credit accounts, mortgages, and installment loans, as well as demographic information, including zip code, age and credit score. These data are derived from public records, collections agencies, and trade lines data from lending institutions.

The trade line data make up the vast majority of the TransUnion records. These data include nearly all credit provided by banks, finance companies, credit unions, and other institutions. Each record includes the account opening date, outstanding balances, credit limit, and payment history for revolving credit, mortgages, and installment loans. These trade lines data are considered a near comprehensive set of information on the credit available to the general population. However, these data do not include any information on the approximately 22 million adults (nine percent of adults) in the United States without credit files, or information on non-traditional forms of credit such as payday lending, pawn shops, and borrowing from relatives. As a result, the data are likely to be less representative on the behaviors and outcomes of very poor populations.

We construct several measures of default and deleveraging using the TransUnion data. For mortgage debt, we construct an indicator measure of default that is equal to one if a line of mortgage credit is sixty days or more delinquent in the past year in either 2008, 2009 or 2010. Our measure of foreclosure is an indicator that is equal to one if there is a foreclosure in the past year in either 2008, 2009, or 2010. Our change in mortgage debt is the total change between 2008 and 2010 of both mortgage and home equity debt. For

non-mortgage debt, we construct an indicator measure of default that is equal to one if a line of non-mortgage credit is sixty days or more delinquent in the past year in either 2008, 2009, or 2010. We measure charge-offs in a similar fashion with an indicator variable.<sup>4</sup> Finally, we examine change in credit card debt as the total change in bank card debt between 2008 and 2010.

The TransUnion data also contain an ordinal credit score calculated by TransUnion to measure credit risk. This measure is similar to the FICO score commonly referenced in the consumer finance literature. Finally, the data include geographic location at the zip code level and age. No other demographic information is available at the individual level. See Avery et al. (2003) and Finkelstein et al. (2012) for additional details on the TransUnion data.

Our sample of homeowners is drawn from a broader random sample of TransUnion credit reports. Our initial credit report sample consists of a random sample of four million credit reports. These data are an approximately two percent random sample of the population of credit users in the TransUnion database. The full random sample samples four million individuals from the TransUnion database in 2010 and pulls their full credit records annually using TransUnion's matched records across time. Our credit report data is pulled in June of each year.<sup>5</sup>

This sample is restricted to 2007 homeowners credit report data over the period of 2007 to 2010, located in zip codes with house price data. We define homeownership using the presence of mortgage or home equity line on an individual's credit report. In our data, approximately 46 percent of individuals are marked as homeowners in 2007, which compares to a 68 percent homeownership rate in the U.S. Census for the same time period.

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<sup>4</sup>Charge-offs indicate that the creditor does not expect to collect the balance and chooses write the debt off as a loss or sell at a discount to a credit collection agency.

<sup>5</sup>While TransUnion database is linked over time, the database is not perfectly matched across time periods. For our sample of four million individuals in 2010, we have 3,550,696 individuals in 2007. The reasons for this are twofold. First, there were new individuals with credit reports between 2007 and 2010. Second, TransUnion does not have a perfect match across time periods in its sample. Individuals are not dropped from TransUnion's database if they stop using credit, so there should not be any panel attrition concerns.

This difference is not surprising, as recent data released by Zillow estimates that almost 29 percent of homeowners in 2014 had no mortgage. While we cannot adjust for joint homeownership, we note that the gap between 46 percent and 68 percent is likely covered by the combination of homeowners without mortgages and joint homeownership.<sup>6</sup> Of this 46 percent, roughly 74 percent of homeowners have the necessary zip code house price data, leaving us with approximately 1.2 million homeowners.

#### **2.4.2 County Employment Records**

County by industry employment and payroll data are from the County Business Patterns (CBP) data set published by the U.S. Census Bureau. CBP data are recorded in March each year. The data contain the number of employees and total payroll bill within a county for every four-digit industry. Following Mian and Sufi (2014), we define each four-digit industry as tradable or non-tradable. An industry is defined as a tradable sector if it has imports plus exports equal to at least 10,000 dollars per worker, or if total exports plus imports for the industry exceeds 500 million dollars. Non-tradable industries are defined as the retail sector and restaurants. See Appendix Table 1 of Mian and Sufi (2014) for a complete list of all NAICS four-digit industry codes in each category.

#### **2.4.3 State Consumption Data**

State consumption expenditures come from the Personal Consumption Expenditures by State dataset published by the U.S. Bureau of Economic Analysis (BEA). Data from the Economic Census and other sources are used to create an initial set of annual nominal expenditure estimates for 77 detailed spending categories. These initial estimates are then balanced across states to match BEA's national consumption expenditure totals in each category.<sup>7</sup> The data are then aggregated to the 16 expenditure categories that correspond

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<sup>6</sup>The Census measure captures the fraction of housing stock that is owner-occupied, which would undercount the number of individuals with a mortgage.

<sup>7</sup>State-level Economic Census receipts are used for approximately 60 percent of the data in Economic Census years. For other years, where state-level Economic Census receipts are not available, annual data from

to the national expenditure categories published by BEA. There are eight categories of goods, seven categories of services, and the net expenditures of nonprofit institutions serving households. The consumption levels are reported in current dollars, and reflect variation in both prices and quantities. See the BEA website for additional information on the construction of the data.

#### **2.4.4 House Prices**

Information on house prices at the zip code-by-year level are from Zillow.com, an online real estate site. See Guerrieri et al. (2010) for a description of the differences and similarities between Fiserv Case Shiller Weiss and the Zillow.com data.

#### **2.4.5 Summary Statistics**

Table 2.1 presents summary statistics for our sample. Individual Data reports the summary statistics for our individual-level analysis. Column 1 reports the mean, column 2 reports standard deviation and column 3 reports the number of observations available for the variable. There are high levels of financial distress in our sample period. Between 2008 and 2010, 14.5 percent of our sample were 60 days or more delinquent on a mortgage debt, 4.2 percent experienced a home foreclosure, 14.4 were 60 days or more delinquent on a non-mortgage debt, and 13.5 experienced a non-mortgage credit line charged off. Credit card debt for the average homeowner fell by 1,346 dollars between 2008 and 2010.

County Data reports the summary statistics for our county-level employment regressions. For both sub-categories of employment we examine, employment fell significantly. Tradable employment collapsed from 2008 to 2011, falling roughly 10.3 percent. By comparison, non-tradable employment growth was negative, but smaller in magnitude, falling about

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the Quarterly Census of Employment and Wages are used to interpolate and extrapolate expenditures. In these cases, the wages are those of the workers employed in the establishments within the state providing the goods and services to consumers. For the remaining approximately 40 percent of the data, other annual state-level data sources are used to estimate expenditures. These include the subcategories within housing and utilities and health care, as well as education services, food furnished to employees, railway transportation, air transportation, and net foreign travel.



4.46 percent.

State Data reports the summary statistics for our state-level consumption regressions. Non-durable goods consumption grew by 8.75 percent between 2008 and 2011, and retail and restaurant consumption grew by 6.98 percent over this period.

Finally, Zip Code Data reports the summary statistics from the zip code level house price regression. Between 2007 and 2011, house prices fell almost 19 percent.

**Table 2.1: Summary Statistics**

	Mean	Std. Dev.	Observations
	(1)	(2)	(3)
<u>Individual Data:</u>			
Mortgage Default	0.145	0.352	1,244,136
Foreclosure	0.042	0.200	1,244,136
Mortgage Debt Change (\$000)	-13.628	130.773	1,244,136
Non-Mortgage Default	0.144	0.351	1,244,136
Non-Foreclosure Charge-off	0.135	0.342	1,244,136
Credit Card Debt Change (\$000)	-1.346	10.071	1,244,136
Non-Recourse Protected	0.068	0.251	1,244,136
Homestead Protected	0.504	0.500	1,244,136
Non-Recourse Protected Inst.	0.029	0.058	1,238,855
Homestead Protected Inst.	0.634	0.249	1,238,855
<u>State Data:</u>			
Non-Durable Consumption Growth (2008-2011)	8.756	3.660	49
Retail and Restaurant Cons. Growth (2008-2011)	6.980	3.584	49
Non-Recourse Protected	0.046	0.097	49
Homestead Protected	0.512	0.288	49
Non-Recourse Protected Inst.	0.023	0.046	49
Homestead Protected Inst.	0.635	0.183	49
<u>County Data:</u>			
Tradable Employment Growth (2008-2011)	-10.328	31.897	1,327
Non-Tradable Employment Growth (2008-2011)	-4.466	10.836	1,330
Non-Recourse Protected	0.041	0.104	1,326
Homestead Protected	0.510	0.316	1,326
Non-Recourse Protected Inst.	0.020	0.043	1,330
Homestead Protected Inst.	0.617	0.187	1,330
<u>Zip Code Data:</u>			
House Price Growth (2007-2011)	-18.895	16.729	11,782
Non-Recourse Protected	0.055	0.122	11,688
Homestead Protected	0.498	0.313	11,688
Non-Recourse Protected Inst.	0.025	0.047	11,689

Continued on next page

**Table 2.1: Summary Statistics (continued)**

	Mean	Std. Dev.	Observations
	(1)	(2)	(3)
Homestead Protected Inst.	0.635	0.186	11,689

Notes: This table reports summary statistics at the four levels of aggregation used in the analysis. Individual Data lists the summary statistics for variables used in the individual-level regressions. Mortgage Default is an indicator for having a mortgage being 60 days or more delinquent reported sometime between 2008 and 2010. Foreclosure is an indicator for having a foreclosure reported sometime between 2008 and 2010. Mortgage Debt Change is the thousand dollar change in home equity and mortgage lines of credit between 2008 and 2010. Non-Mortgage Default is an indicator for having a non-mortgage line of credit 60 days or more delinquent reported sometime between 2008 and 2010. Non-Foreclosure Charge-offs is an indicator for having a non-foreclosure charge-off of a credit line reported between 2008 and 2010. Credit Card Debt Change is the dollar change, in thousands, of the balance of credit card debt between 2008 and 2010. Non-Recourse Protected is an indicator for having negative equity and living in 2007 in a state that is non-recourse. Homestead Protected is an indicator for having home equity less than the bankruptcy homestead protection for the individual's state of residence in 2007. Non-Recourse Protected Inst. and Homestead Protected Inst. are the two simulated instruments described in Section 2.5. State Data lists the summary statistics for the outcome measures used in the consumption regressions. Non-Durable Consumption Growth is the percentage change in Non-Durable Goods from 2008 to 2011. Retail and Restaurant Cons. Growth is the percentage change in the total of retail and restaurant consumption from 2008 to 2011. These averages are weighted by population. Non-Recourse Protected is the fraction of individuals with negative equity if the state is non-recourse. Homestead Protected is the fraction of individuals with home equity less than the state's bankruptcy homestead exemption. Both measures are as of 2007. Non-Recourse Protected Inst. and Homestead Protected Inst. are the two simulated instruments described in Section 2.5. County Data lists the summary statistics for variables used in the county-level regressions. Tradable Employment Growth is the percentage change in employment from 2008 to 2011 in industries marked as tradable by Mian and Sufi (2014). Non-Tradable Employment Growth is the percentage change in employment from 2008 to 2011 in industries marked as non-tradable by Mian and Sufi (2014). Zip Code Data lists the summary statistics for variables used in the zip code-level regressions. House Price Growth is the percentage change in median house prices at the zip code, as measured by the Zillow house price index.

## 2.5 Research Design

We begin this section by outlining our strategy for estimating the impact of debtor protections on individual-level outcomes. We then extend our approach to outcomes measured at the zip code, county, and state levels. Finally, we present a series of specification checks to partially test our identifying assumptions.

### 2.5.1 Empirical Specification for Individual-Level Outcomes

We measure non-recourse and homestead protections using individual-level indicators variables defined as follows:

$$\text{NonRecourseProtected}_{is} = \mathbb{1}(E_{H,i} < 0) \times \text{NonRecourse}_s$$

$$\text{HomesteadProtected}_{is} = \mathbb{1}(E_{H,i} < H_s^*),$$

where  $\text{NonRecourseProtected}_{is}$  is an indicator variable for whether an individual  $i$  has negative equity and lives in a state  $s$  with non-recourse mortgages, and  $\text{HomesteadProtected}_{is}$  is an indicator variable for whether an individual's home equity is completely protected by the state's bankruptcy homestead exemption amounts.

Now, consider the empirical model that relates the individual outcomes such as default to these two measures of protection:

$$\text{Default}_{is} = \alpha + \beta \text{NonRecourseProtected}_{is} + \gamma \text{HomesteadProtected}_{is} + \epsilon_{is} \quad (2.1)$$

where  $i$  denotes individuals,  $s$  denotes the state of residence, and  $\epsilon_{is}$  is noise. Estimating equation (2.1) directly using OLS may lead to biased estimates of debtor protections for at least three reasons. First, equation (2.1) does not control for an individual's home equity, which is likely to be correlated with both the default decision and our measures of debtor protection. This would be easy to address by controlling for home equity in a sufficiently flexible way. A second, and more important concern, is that state debtor protection laws may influence an individual's choice of home equity in such a way that creates a correlation between home equity and future outcomes. For example, more strategic households may keep their home equity just below the amount protected under a state's laws, and these more strategic households may also be more likely to default following an aggregate shock. In this scenario, OLS estimates of equation (2.1) would be positively biased. Conversely, it is possible that more risk averse households make keep their home equity fully protected, creating a negative bias in OLS estimates of equation (2.1). A final concern is that we are likely to measure home equity with error, potentially biasing our estimates of equation (2.1).

To address these two problems, we generate a pair of exogenous instruments that exploit the variation in the laws across states. Intuitively, the simplest instrument would be to use whether a state is non-recourse and the level of bankruptcy homestead exemption as our instruments. However, there are two simple extensions we can make. First, we recognize that these laws can have very different effects for different demographic groups. For example, a young subprime borrower is much more likely to have a highly levered

mortgage that has become underwater due to the house price shock. As a result, we can interact these debtor protection laws with pre-crisis demographic characteristics to exploit this differential response to protections. To avoid having this instrument picking up effects due to different demographic effects, such as subprime borrowers defaulting more, we can control directly for the demographic effects and exploit exclusively the interaction between the laws and individuals' demographics. Second, while we want to avoid using the individuals' home equity values due to endogeneity concerns, we can use the national distribution of home equity for each demographic group to identify the benefit of each states' law on a particular demographic.

Formally, we instrument for debtor protections using a version of the simulated instrument approach developed by Currie and Gruber (1996) and extended by Mahoney (2015). Our simulated instrument isolates variation in state bankruptcy homestead exemptions and non-recourse protections that is purged of variation due to the characteristics of each state's residents. To construct each instrument, we first divide the sample into  $g = 1, \dots, G$  demographic groups based on the full interaction of four-year age bins and 50 point baseline credit score bins.<sup>8</sup> We divide the sample using a baseline year of 2007, and define the level of protection for group  $g$  in state  $s$  as the fraction of group  $g$  from the national sample (excluding own state) that would be protected if they were subject to the state laws in  $s$ . Formally, we define the bankruptcy homestead protection instrument as:

$$\widehat{\text{HomesteadProtected}}_{gs} = |I_{g,-s}|^{-1} \sum_{j \in I_{g,-s}} \text{HomesteadProtected}_{js} \quad \text{for } g = 1, \dots, G$$

where  $I_{g,-s}$  is the entire set of individuals in group  $g$  in all states excluding state  $s$ , and  $\text{HomesteadProtected}_{js}$  is an indicator for individual  $j$  having all home equity protected by bankruptcy homestead exemptions if they were subject to the state laws in  $s$ . Following the same notation, the corresponding measure for non-recourse protections is:

$$\widehat{\text{NonRecourseProtected}}_{gs} = |I_{g,-s}|^{-1} \sum_{j \in I_{g,-s}} \text{NonRecourseProtected}_{js} \quad \text{for } g = 1, \dots, G$$

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<sup>8</sup>Our age bucket cuts are from 16 to 80 in four year intervals, as well as a category for missing age.

where  $\text{NonRecourseProtected}_{js}$  is an indicator for individual  $j$  with negative equity and the state  $s$  being non-recourse. These measures of debtor protection differ for each state by demographic group. We control for demographic group effects with fixed effects,  $X_g$ , in all specifications to partial out cross-group variation in protection levels.

Using our estimates of  $\widehat{\text{HomesteadProtected}}_{gs}$  and  $\widehat{\text{NonRecourseProtected}}_{gs}$  as instruments, we estimate the following two-stage least squares equation:

$$\text{Default}_{igs} = \beta_{2.3}X_g + \gamma_{2.3}\widehat{\text{HomesteadProtected}}_{igs} \quad (2.2)$$

$$+ \eta_{2.3}\widehat{\text{NonRecourseProtected}}_{igs} + \varepsilon_{igs} \quad (2.3)$$

$$\widehat{\text{HomesteadProtected}}_{igs} = \beta_{2.5}X_g + \gamma_{2.5}\widehat{\text{HomesteadProtected}}_{gs} \quad (2.4)$$

$$+ \eta_{2.5}\widehat{\text{NonRecourseProtected}}_{gs} + u_{1igs} \quad (2.5)$$

$$\widehat{\text{NonRecourseProtected}}_{igs} = \beta_{2.7}X_g + \gamma_{2.7}\widehat{\text{HomesteadProtected}}_{gs} \quad (2.6)$$

$$+ \eta_{2.7}\widehat{\text{NonRecourseProtected}}_{gs} + u_{2igs} \quad (2.7)$$

where  $X_g$  includes the demographic group fixed effects. We cluster standard errors at the state level throughout to account for both unobserved random shocks at the state level, as well as the within-state correlation in bankruptcy homestead and non-recourse laws. Note that this estimation procedure addresses our two concerns regarding the non-recourse and bankruptcy homestead exemptions, namely that there will be unobservables driving both protections and the outcomes. As described by Currie and Gruber (1996), this instrument gives a convenient parameterization of the protection from the law. States with high protections will give the national sample a large amount of protection compared to a state with low protections, and this will be independent of any local individual's tendency to manipulate their balance sheet. This identification strategy rests on conditional exogeneity of the laws, which we will discuss in Section 2.5.3.

## 2.5.2 Empirical Specification for Regional Outcomes

We estimate equation (2.3) at the individual level for the debtor outcomes, such as mortgage default, foreclosure, non-mortgage default and credit card borrowing. Information on house price growth, consumption and employment is only available at the zip code, state

and county level, respectively. In order to estimate the effect of the population in a location being more or less protected, we aggregate our individual protection measures and create aggregated versions of our instrument. We first residualize our demographic-level instruments by running the following regressions:

$$\widehat{\text{HomesteadProtected}}_{igs} = \tau_{2.8}X_{ig} + \varepsilon_{igs} \quad (2.8)$$

$$\widehat{\text{NonRecourseProtected}}_{igs} = \tau_{2.9}X_{ig} + \varepsilon_{igs} \quad (2.9)$$

and defining

$$\begin{aligned} \widehat{\text{HomesteadProtected}}_{igs}^{\perp} &= \widehat{\text{HomesteadProtected}}_{igs} - \hat{\tau}_{2.8}X_{ig} \\ \widehat{\text{NonRecourseProtected}}_{igs}^{\perp} &= \widehat{\text{NonRecourseProtected}}_{igs} - \hat{\tau}_{2.9}X_{ig}. \end{aligned}$$

Residualized of demographic characteristics in  $X_{gs}$ , we then average both these measures as well as the individual outcomes across the relevant geographic level to estimate the average measure of bankruptcy and foreclosure protection in each location. We denote the average of the individual outcomes as  $\overline{\text{NonRecourseProtected}}_{ls}$  and  $\overline{\text{HomesteadProtected}}_{ls}$ , and the average of the instruments as  $\widehat{\text{HomesteadProtected}}_{ls}^{\perp}$  and  $\widehat{\text{NonRecourseProtected}}_{ls}^{\perp}$ .

Then, at the aggregate location level, we estimate the effect of these measures of debtor protections using the following two-stage least squares specification:

$$y_{ls} = \alpha_{2.10} + \gamma_{2.10}\overline{\text{HomesteadProtected}}_{ls} + \eta_{2.10}\overline{\text{NonRecourseProtected}}_{ls} + \varepsilon_{ls} \quad (2.10)$$

$$\overline{\text{HomesteadProtected}}_{ls} = \alpha_{2.11} + \gamma_{2.11}\widehat{\text{HomesteadProtected}}_{ls}^{\perp} + \eta_{2.11}\widehat{\text{NonRecourseProtected}}_{ls}^{\perp} + u_{1ls} \quad (2.11)$$

$$\overline{\text{NonRecourseProtected}}_{ls} = \alpha_{2.12} + \gamma_{2.12}\widehat{\text{HomesteadProtected}}_{ls}^{\perp} + \eta_{2.12}\widehat{\text{NonRecourseProtected}}_{ls}^{\perp} + u_{2ls} \quad (2.12)$$

where subscript  $l$  denotes either zip code, county or state and  $s$  denotes the state.

### 2.5.3 Specification Checks

Our empirical analysis rests on the assumption that the variation in state debtor protection laws captured by our simulated instruments is not related to other factors that might independently impact household balance sheets, such as human capital, local credit markets, and consumer preferences. We provide four forms of evidence to support this view. First, we outline the legislative origins of the laws and argue that their legislative history makes them uncorrelated with the current recession. Second, we regress our measures of protection on state demographic and policy measures to partially test for possible confounders. Third, we estimate our main results including controls for possible confounders and excluding potentially anomalous states such as Florida and the “sand states” of California, Nevada, New Mexico and Arizona. Fourth and finally, we conduct several placebo tests to see if our simulated instruments are correlated with unrelated outcomes.

We begin by considering the legislative origins of state bankruptcy and foreclosure laws, which we argue lend credibility to the exogeneity of bankruptcy exemption levels. Homestead exemption levels emerged over the second half of the nineteenth century as a result of idiosyncratic state circumstances that are plausibly unrelated to current state characteristics (Goodman 1993). Although most changes to the homestead exemptions have been to correct for inflation (Skeel 2001), one potential concern is that there may be contemporaneous reasons why some states have higher homestead protections than others. To address this concern, we follow Mahoney (2015) and present robustness tests of our results using inflation-adjusted 1920 bankruptcy homestead exemptions to calculate the simulated instrument described in Section 2.5. Panel A in Table 2.2 replicates our main results using historical 1920 bankruptcy homestead exemptions from Mahoney (2015) and finds broadly consistent results. Further evidence comes from Mahoney (2015), who finds a nearly one-to-one relationship between a simulated instrument created using current homestead exemptions and one created using inflation adjusted 1920 homestead exemptions. For our main results, we use current measure of protections for two reasons. The first is that historical exemptions are not available for several of the states in our sample, and

are missing disproportionately from non-recourse states. Second, the measurement error introduced from the historical bankruptcy exemption measure is substantial. Both of these issues limit our ability to analyze both laws concurrently and weaken the strength of the overall analysis.

**Table 2.2:** *Assessing Robustness of Results to Alternative Instruments*

	Mortgage Default (1)	Non-Mortgage. Default (2)	Non-Tradable Employment (3)	Non-Durable Consumption (4)	House Prices (5)
<i>Panel A: Historical Bankruptcy Homestead Exemptions</i>					
Non-Recourse Protected	0.231*** (0.034)	0.042* (0.024)	-14.804*** (4.149)	-10.339*** (3.114)	-69.160*** (6.574)
Homestead Protected	0.040 (0.068)	0.057** (0.024)	4.415 (2.997)	6.750* (3.354)	-5.334 (17.882)
<i>Panel B: Age-Only Instrument</i>					
Non-Recourse Protected	0.062 (0.069)	-0.064 (0.042)	-13.574*** (3.595)	-8.873*** (2.953)	-46.486*** (15.549)
Homestead Protected	0.040 (0.028)	0.039** (0.016)	3.885** (1.906)	4.913*** (1.814)	-12.615 (13.918)
<i>Panel C: Cross-State Instrument</i>					
Non-Recourse Protected	0.057 (0.063)	-0.075 (0.040)	-13.655*** (3.632)	-8.899*** (2.948)	-46.695*** (15.749)
Homestead Protected	0.041 (0.026)	0.038** (0.015)	3.942** (1.903)	4.965*** (1.819)	-12.600 (14.059)

Notes: This table presents the main results from Tables 2.4-2.7 using alternative instruments for non-recourse and bankruptcy homestead protection. The dependent variable for column 1 is Mortgage Default as reported in column 1 of Table 2.4. The dependent variable for column 2 is Non-Mortgage Default as reported in column 4 of Table 2.4. The dependent variable for column 3 is Non-Tradable Employment Growth as reported in column 1 of Table 2.5. The dependent variable for column 4 is Non-Durable Consumption Growth as reported in column 1 of Table 2.6. The dependent variable for column 5 is House Price Growth as reported in column 1 of Table 2.7. Panel A uses the inflation-adjusted 1920 Homestead amounts from Mahoney (forthcoming) as the bankruptcy homestead exemption amount. Panel B uses only age demographics in the estimation of the simulated instrument. Panel C does not use any demographics in the estimation of the simulated instrument to isolate the cross-state variation in the laws. See the notes for Tables 2.4-2.7 for additional details. \*\*\* = significant at 1 percent level, \*\* = significant at 5 percent level, \* = significant at 10 percent level.

Ghent (2012) similarly finds that there is tremendous path-dependence in state non-recourse and foreclosure laws. Non-recourse laws, which we focus on, were set during the Great Depression, and much of the driving factor during this period for these laws was the foreclosure rate on farms. Ghent (2012) finds that "...there is no evidence that the foreclosure rate on urban mortgages affected the likelihood that a state would enact a



sweeping anti-deficiency statute.” These non-recourse mortgage laws have significantly not changed since the Great Depression. Similarly, most foreclosure procedures were set “very early in state histories, typically before the U.S. Civil War.” The existing variation in state foreclosure laws is therefore “the result of path-dependent quirks in the wording of various proposed statutes and decisions of individual judges,” with “[no] clear economic reasons for why states adopted different procedures for the remedies they offer lenders.”

Our second piece of evidence in support of our identifying assumption comes from regressions our debtor protection measures on state demographic and policy measures. We report these results in Table 2.3. These tests fall into three categories. We first test the hypothesis that areas with higher levels of protections are more likely to be debtor friendly or liberal in their policies more generally. We find no significant effect of either protection on Democrat vote share in 2006. Next, we examine the demographic breakdown by age and education, and find a small correlation between age and non-recourse protections, but no effect of education or any effect of bankruptcy homestead protections. Finally, we examine the effects of the protections on economic growth, as measured by income. We find no evidence of correlation in the pre-boom period between income levels and income growth from 1998 to 2002.

To further test the robustness of our results, we also estimate our main regional results including controls for possible confounders and excluding potentially anomalous states such as Florida and the “sand states” of California, Nevada, New Mexico and Arizona. These results are presented in Section 2.6. The point estimates on non-recourse and bankruptcy homestead protections remain economically and statistically significant in all specifications, and none of the results suggest that our identifying assumption is violated.

Our final piece of evidence in support of our identifying assumption comes from several placebo tests to see if our simulated instruments are correlated with other unrelated outcomes. If there were an underlying force driving stronger or weaker debtor protections, we would expect that these forces influence both protections equally. To the extent that these laws directly affect their corresponding debt (i.e. non-recourse affecting mortgage defaults,

**Table 2.3: Debtor Protections and State Characteristics**

	Non-Recourse Protections	Homestead Protections
Democrat Vote Share	0.402 (0.279)	-0.038 (0.128)
Max. UI Benefits (\$000)	4.288 (12.141)	0.668 (7.382)
Income Ch. (98-02)	0.329 (0.306)	-0.018 (0.040)
Income (2002)	21.529 (23.222)	-0.507 (7.181)
House Price Growth (98-02)	0.247 (0.179)	1.674 (1.066)
College Educ Share	-0.105 (0.130)	-0.055 (0.052)
Share Under Age 44	0.288** (0.135)	-0.020 (0.068)
Observations		49

Notes: This table reports OLS estimates of state characteristics on non-recourse and bankruptcy homestead exemption laws. Democratic Vote Share is measured in the Federal 2006 Elections at the state-level. College Educated Fraction is the fraction of individuals with a college or graduate degree in 2006. Fraction Under 44 is the population fraction under the age of 44. Income (2002) is the income level in 2002. Income Ch. (1998-2002) is the percentage change in income from 1998 to 2002. House Price Growth (98-02) is the percentage change in house prices from 1998 to 2002. Each row reports estimates from a single regression on our non-recourse and bankruptcy homestead protection measures. The Non-Recourse Protected measure is the fraction of individuals in an age-credit score bin who have no liability in the event of a foreclosure residualized in age-credit score regression at the individual level. The Homestead Protected measure is the fraction of individuals in an age-credit score bin who have their home equity completely protected by bankruptcy homestead exemptions, residualized in age-credit score regression at the individual level. We then take the state-level weighted average of each instrument. All regressions are weighted by state population with robust standard errors. \*\*\* = significant at 1 percent level, \*\* = significant at 5 percent level, \* = significant at 10 percent level.

and exemptions affecting non-mortgage defaults), and we estimate both simultaneously in our empirical specification, we should not expect that they each affect the laws independently. These tests allow for a tight placebo test of the direct mechanism we expect. Any plausible unobservables that we would expect to drive defaults would be economic factors correlated with financial distress, which should affect both mortgage and non-mortgage default. These results are again presented in Section 2.6, with none of the estimates suggesting that our identifying assumption is violated.

## 2.6 Results

We begin by looking at the effect of bankruptcy homestead exemptions and non-recourse protections on mortgage default and non-mortgage defaults. For both policies, we find strong responses consistent with consumers defaulting in response to the protections. Moreover, the default behavior is only correlated with the appropriate protection. We next examine the effect of these protections on economic outcomes at the regional level. We find that bankruptcy homestead protections had significant positive effects on non-tradable employment and non-durable consumption, and non-recourse protections had the opposite effect. We finally show that house prices were strongly negatively correlated with non-recourse protections and not homestead protections.

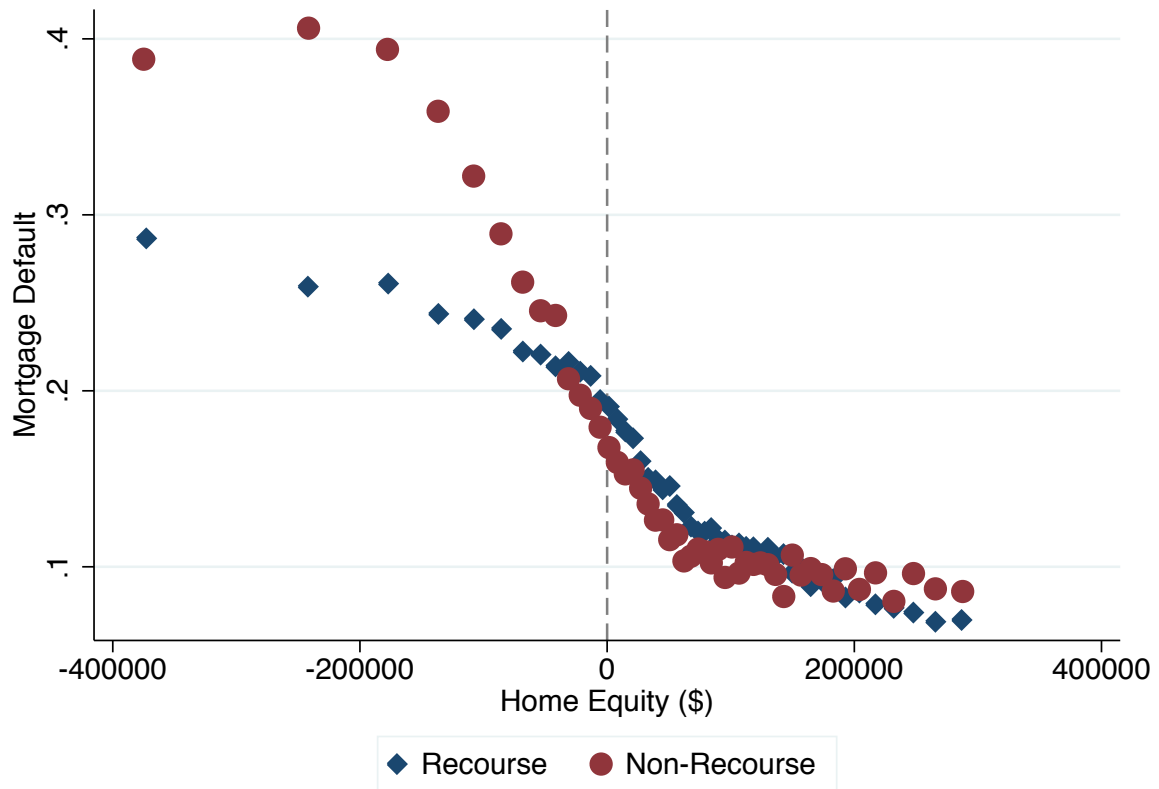
### 2.6.1 Individual Defaults and Debt

Figures 2.2 and 2.3 outline the main default effects that we find. In both figures, the x-axis denotes home equity values projected forward into 2010.<sup>9</sup> In Figure 2.2, the y-axis represents mortgage defaults, as defined as having a mortgage loan 60 days or more delinquent between 2008 and 2010. We note that mortgage defaults are significantly higher in non-recourse states, specifically for those home owners who have negative home equity. Figure 2.3 presents positive equity home owners with non-mortgage defaults on the x-axis, where non-mortgage default is defined as having a non-mortgage loan 60 days or more delinquent between 2008 and 2010. In both cases, we see that protected individuals default more than unprotected.

Table 2.4 presents the instrumental variable estimates from Equation 2.3, which regress the individual-level outcomes on whether a homeowner is protected from recourse, and whether a homeowner's home equity is protected by homestead exemptions. The first three columns represent outcomes associated with mortgage credit, where default is defined as

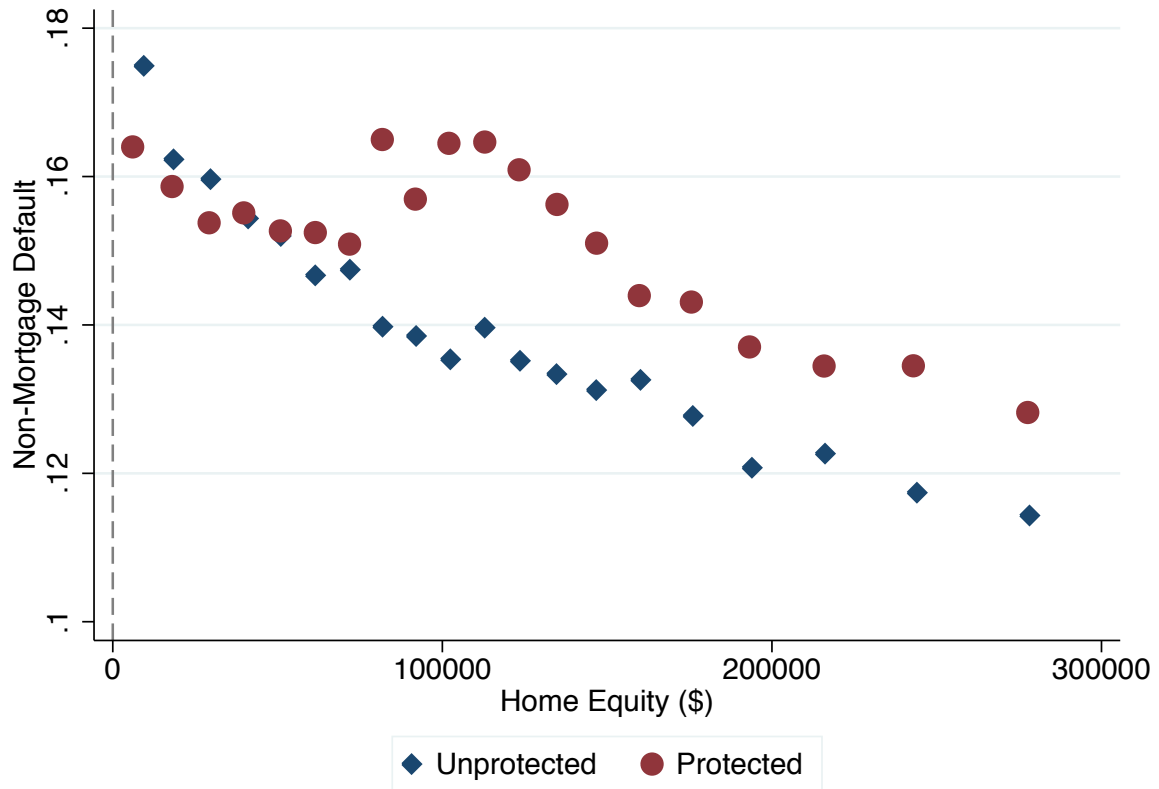
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<sup>9</sup>We project home equity values by taking homeowners' mortgage debt and zip code location as of 2007, and estimate their projected home equity in 2010 as the house price value in 2010 less the homeowner's mortgage debt in 2007.



**Figure 2.2:** *Non-Recourse Protections and Mortgage Defaults*

Notes: This figure plots mortgage defaults against home equity values for 2007 homeowners. Mortgage default is defined as having a mortgage 60 days or more delinquent sometime between 2008 and 2010. Home Equity is defined as the 2010 house price in the 2007 zip code of residence, and subtracting the outstanding mortgage and home equity balances from 2007. Non-Recourse and Recourse are defined by state of residence in 2007. This sample restricts to home equity values between -500,000 and 500,000 dollars. Each dot represents two percent of the the subsample.



**Figure 2.3:** *Bankruptcy Exemption Protections and Non-Mortgage Defaults*

Notes: This figure plots non-mortgage defaults against home equity values for 2007 homeowners. Non-mortgage default is defined as having a non-mortgage line of credit 60 days or more delinquent sometime between 2008 and 2010. Home Equity is defined as the 2010 house price in the 2007 zip code of residence, and subtracting the outstanding mortgage and home equity balances from 2007. Protected is defined by whether home equity values are less than the 2007 state of residence’s homestead exemption value. This sample restricts to home equity values between 0 and 300,000 dollars. Each dot represents five percent of the the subsample.

**Table 2.4:** *Debtor Protections and Individual Debt Reductions*

	Mortgage Credit			Non-Mortgage Credit		
	Default (1)	Foreclosure (2)	$\Delta$ (\$000) (3)	Default (4)	Charge-offs (5)	$\Delta$ (\$000) (6)
Non-Recourse Protected	0.155** (0.063)	0.094** (0.038)	-39.786** (18.760)	0.010 (0.032)	0.048 (0.044)	-0.684 (0.689)
Homestead Protected	0.027 (0.023)	0.022 (0.017)	-3.892 (3.945)	0.025*** (0.007)	0.033*** (0.010)	-0.387*** (0.129)
Dep. Var. Mean	0.146	0.042	-13.657	0.144	0.135	-1.348
Clusters	49	49	49	49	49	49
Observations	1,238,855	1,238,855	1,238,855	1,238,855	1,238,855	1,238,855

Notes: This table reports two-stage least squares estimates of the effect of non-recourse and bankruptcy homestead exemption protections on individual-level outcomes. The dependent variable in column 1 is an indicator for having a mortgage 60 or more days delinquent reported on credit reports between 2008 to 2010. The dependent variable in column 2 is an indicator for having a foreclosure reported on credit reports between 2008 to 2010. The dependent variable in column 3 is the dollar change (in thousands) of mortgage debt from 2008 to 2010. The dependent variable in column 4 is an indicator for having a non-mortgage line of credit 60 or more days delinquent reported on credit reports between 2008 to 2010. The dependent variable in column 5 is the total number of non-mortgage charge-offs reported on credit reports between 2008 to 2010. The dependent variable in column 6 is the dollar change (in thousands) of credit card debt from 2008 to 2010. Homeownership status is defined as the presence of a mortgage or home equity trade line of credit on an individual's credit report. Non-Recourse Protected is an indicator for whether an individual is protected from recourse in the event of foreclosure. Homestead Protected is an indicator for whether an individual's home equity is completely protected by bankruptcy homestead exemptions. All regressions are estimated using 2007 homeowners from a random sample of individual-level credit reports. All regressions also control for age-credit score demographic bucket fixed effects and cluster standard errors at the state level. \*\*\* = significant at 1 percent level, \*\* = significant at 5 percent level, \* = significant at 10 percent level.

having a mortgage loan that has been delinquent by 60 days or more reported between 2008 and 2010, foreclosure is defined as having a foreclosure reported between 2008 and 2010, and  $\Delta$  (\$000) is the change in total mortgage and home equity debt between 2008 and 2010. The next three columns represent outcomes with non-mortgage credit. Non-mortgage default is defined as having a non-mortgage line of credit that has been delinquent by 60 days or more reported between 2008 and 2010, charge-offs is defined as having a non-mortgage line of credit charge-off reported between 2008 and 2010, and  $\Delta$  (\$000) is the change in total mortgage and home equity debt between 2008 and 2010. All specifications are estimated at the individual-level using our estimation sample of credit reports described in Section 2.4. All specifications control for age-baseline credit score fixed effects and cluster standard errors at the state level.

We first focus on mortgage credit outcomes. Column 1 reports results for mortgage

default. There is a large and precisely estimated impact of non-recourse protection on mortgage default rates. Non-recourse protection increases mortgage default probabilities by 15.5 percentage points, doubling the average rate of default of 14.6 percent among homeowners, and is statistically significant at the five percent level. In column 2, foreclosure rates are also significantly higher, with non-recourse protections increasing foreclosures by 9.4 percent. Lastly, in column 3, non-recourse protections caused significant declines in outstanding mortgage debt, with mortgage and home equity debt totals declining by roughly 39,700 dollars from 2008 to 2010. In all three columns, bankruptcy homestead protections have no statistically significant effects on the propensity to default or decrease mortgage credit.

Next, we turn to non-mortgage credit outcomes. Column 4 reports the effect on non-mortgage defaults. There is a 2.6 percentage point increase in the probability of a non-mortgage default for individuals whose home equity is completely protected, compared to an average default rate over this period of 14.4 percent, almost a 20 percent increase in the probability of default. In column 5, we see that bankruptcy homestead protection increased the probability of having a charged-off line of credit by 3.3 percentage points. Lastly, in column 6, we see that homestead protections were associated with a decline of 387 dollars, compared to an average change over this period of 1,348 dollars. All three results are significant at the one percent level. In addition, we find no significant effect of non-recourse protection on any of the non-mortgage credit outcomes.

## **2.6.2 County Employment**

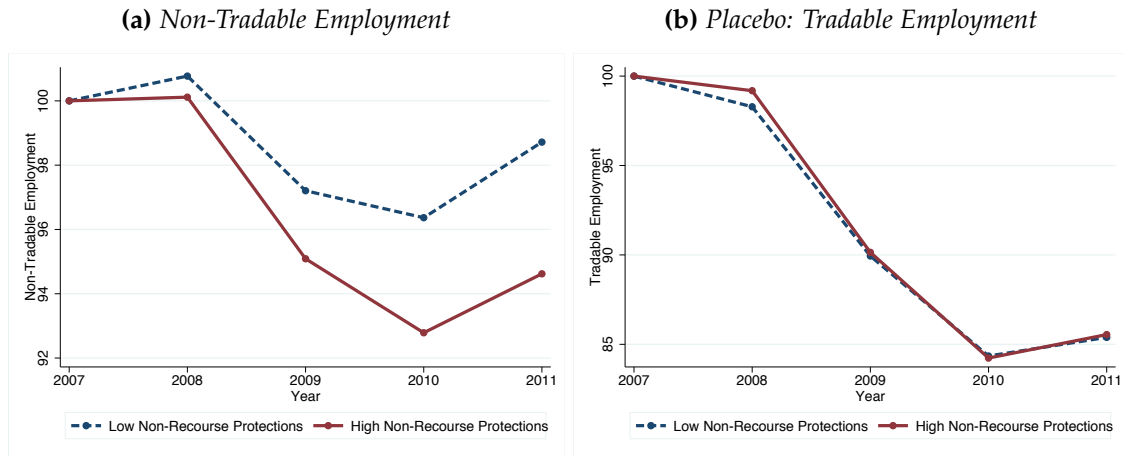
We next examine the effects of the protections on employment. To directly show the effect of local demand, we examine county employment growth in non-tradable and tradable sectors, as defined by Mian and Sufi (2014). These categories roughly divide themselves into retail and restaurants as the non-tradable sectors, and industries like manufacturing in tradables. This split allows us to examine industries whose demand should be completely driven by local consumption versus those whose demand should be driven by global markets. If

debtor protections alleviate debt and hence raise consumption, this should translate into higher employment in non-tradables, but no notable effects in tradables. See Mian and Sufi (2014) for more details on the distinction between tradable and non-tradable employment.

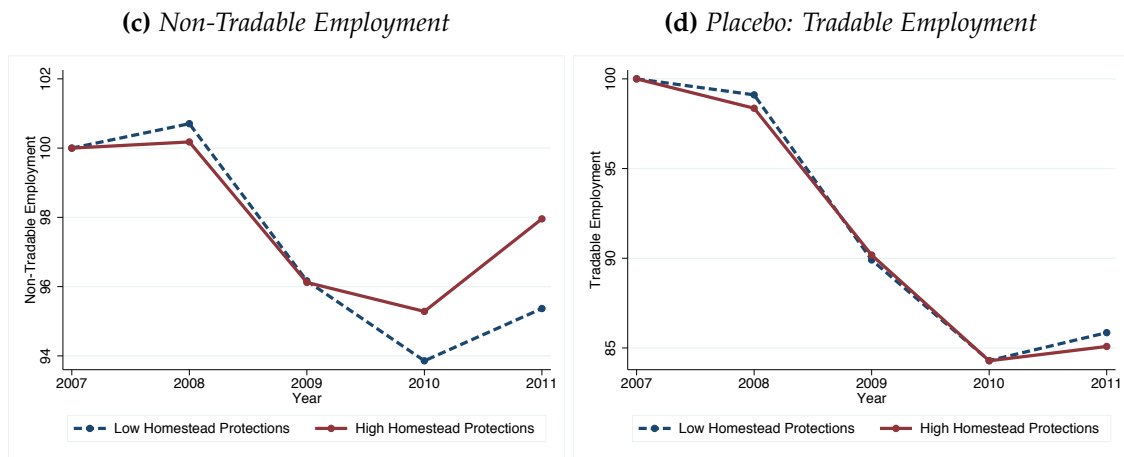
Figures 2.4 and 2.5 show the main effect of bankruptcy exemptions and non-recourse protections on employment. In all figures, employment is normalized to 100 in 2007, and each protection is split into population-weighted bins by the median protection. In each graph, the effect of the other protection is controlled for by first residualizing using a regression of year dummies interacted with the protection measure, and then taking the residuals and adding back in the year means. As we noted previously, with labor market rigidities, non-tradable sectors should be affected by local demand shocks. Hence, we should expect that local debtor protections affect employment in non-tradable industries, but not employment in tradable industries.

Figure 2.4 demonstrates a strong negative effect of non-recourse protection on non-tradable employment, with no effect on tradable employment. In comparison, in Figure 2.5, non-tradable employment is positively affected by bankruptcy homestead protections, with no effects found in tradable employment. It is worth noting that the effect is immediate and sharp for non-recourse protection, while the effect is greater during the recovery period for bankruptcy homestead protections. Table 2.5 presents the instrumental variables regression analysis of Figures 2.4 and 2.5. This table reports the results of the protections on growth rates between 2008 and 2011 for both non-tradable and tradable employment. Column 1 and 4 show the main results from Figures 2.4 and 2.5. In column 1, we see a substantial negative effect on non-tradable employment growth from non-recourse protections and a positive significant effect of bankruptcy homestead protections. A one standard deviation increase (roughly ten percent) in the fraction of individuals benefiting from non-recourse protection leads to a 1.37 percent point decline in non-tradable employment growth. A one standard deviation increase (roughly 30 percentage points) in the fraction protected by homestead protections leads to a 1.17 percentage point increase in non-tradable employment growth from 2008 to 2011. In contrast, we see no significant effect on tradable employment growth





**Figure 2.4:** *Non-Recourse Protection and Employment Growth*



**Figure 2.5:** *Homestead Protection and Employment Growth*

Notes: These figures plot county employment by year for areas with high and low non-recourse and homestead protections. Figure 2.4 splits by the median population weighted value of the non-recourse protection instrument described in the text. Employment is residualized by regressing on year dummies interacted with the county homestead protection instrument. Figure 2.5 splits by the median population weighted value of the homestead protection instrument described in the text. Employment is residualized by regressing on year dummies interacted with the county non-recourse instrument. Non-tradable and tradable sectors are defined following Mian and Sufi (2014) in both graphs. See the text for additional details.

in column 4.

**Table 2.5: Debtor Protections and County Employment Growth**

	Non-Tradable Sector (2008-2011)			Tradable Sector (2008-2011)		
	(1)	(2)	(3)	(4)	(5)	(6)
Non-Recourse Protected	-13.733*** (3.651)	-14.281*** (5.159)	-17.442*** (5.039)	4.057 (6.856)	5.298 (5.979)	19.833 (12.173)
Homestead Protected	3.907** (1.922)	3.709* (2.061)	6.872*** (1.073)	1.524 (2.554)	1.792 (2.276)	3.497 (2.279)
Mean Dep. Var.	-3.729	-3.729	-2.973	-13.271	-13.271	-12.784
Observations	1,323	1,323	1,188	1,323	1,323	1,188
Clusters	49	49	44	49	49	44

Notes: This table reports two-stage least squares estimates of the effect of bankruptcy homestead and non-recourse protections on county employment growth. The dependent variable in each column is the percentage growth in employment from 2008 to 2011 for the indicated industry. In columns 1 and 4, we estimate the growth without any additional controls. In columns 2 and 5, we control for MSA supply elasticity following Saiz (2008), and use a dummy to control for missing supply elasticity measures. In columns 3 and 6, we exclude Florida and the “sand states” of California, Nevada, New Mexico and Arizona. Tradable and Non-Tradable industries are defined following Mian and Sufi (2014). The Non-Recourse Protected measure is the fraction of individuals in an age-credit score bin who have no liability in the event of a foreclosure residualized in age-credit score regression at the individual level. The Homestead Protected measure is the fraction of individuals in an age-credit score bin who have their home equity completely protected by bankruptcy homestead exemptions, residualized in age-credit score regression at the individual level. We then take the county-level weighted average of each instrument. All regressions are estimated using a county-level dataset of NAICS four-digit industry-level employment and are weighted by county population. Standard errors are clustered at the state-level. \*\*\* = significant at 1 percent level, \*\* = significant at 5 percent level, \* = significant at 10 percent level.

The remaining columns in Table 2.5 perform additional robustness tests to confirm our results. In columns 2 and 5, we control for MSA supply elasticity from Saiz (2008), using a dummy to control for counties where the measure is missing. In columns 3 and 6, we exclude California, Nevada, New Mexico, Arizona, and Florida. We find no significant difference in our effects.

These results are consistent with increases in local consumption due to debtor protections. By identifying a channel through non-tradables and not tradables, this underlines the mechanism that only local demand has been affected. While this does not directly demonstrate changes in consumption due to these policies, it provides strong evidence that there are both positive and negative effects from debtor protections policies.

**Table 2.6:** *Debtor Protections and State Consumption Growth*

	Non-Durable Goods (2008-2011)			Retail and Rest. Sector (2008-2011)		
	(1)	(2)	(3)	(4)	(5)	(6)
Non-Recourse Protected	-8.851*** (2.953)	-6.790* (3.601)	-2.026 (5.784)	-12.229*** (4.283)	-10.346* (5.423)	-3.849 (7.184)
Homestead Protected	4.826*** (1.796)	4.849*** (1.370)	6.364*** (1.752)	4.317* (2.233)	4.307** (1.835)	5.967** (2.230)
Mean Dep. Var.	8.722	8.722	9.292	6.967	6.967	7.788
Observations	49	49	44	49	49	44

Notes: This table reports two-stage least squares estimates of the effect of bankruptcy homestead and non-recourse protections on state consumption growth. The dependent variable in each column is the percentage growth in consumption from 2008 to 2011 in the indicated category. In columns 1 and 4, we estimate consumption growth without any additional controls. In columns 2 and 5, we control for average MSA supply elasticity from Saiz (2008), and control for the fraction of the state that does not have a measure for supply elasticity. In columns 3 and 6, we exclude Florida and the “sand states” of California, Nevada, New Mexico and Arizona. The Non-Recourse Protected measure is the fraction of individuals in an age-credit score bin who have no liability in the event of a foreclosure residualized in age-credit score regression at the individual level. The Homestead Protected measure is the fraction of individuals in an age-credit score bin who have their home equity completely protected by bankruptcy homestead exemptions, residualized in age-credit score regression at the individual level. We then take the state-level weighted average of each instrument. The regressions are estimated using a state-level dataset of BEA consumption figures and are weighted by state population. Standard errors are robust. \*\*\* = significant at 1 percent level, \*\* = significant at 5 percent level, \* = significant at 10 percent level.

### 2.6.3 State Consumption

To provide additional support for the changes in local consumption hypothesis, we present evidence using state-level consumption data. While this data is significantly more aggregated than the county-level employment data, it lets us directly examine the consumption mechanism. Table 2.6 presents instrumental variable estimates of the impact of bankruptcy exemption and non-recourse protections on state consumption growth. Columns 1 to 3 report the results on non-durable goods consumption, while columns 4 to 6 report the results on Retail and Restaurant consumption. All specifications are estimated at the state-code level weighted by the number of credit reports observed in each state.

We find evidence consistent with declines in consumption in areas with higher fractions of individuals with non-recourse protections. A ten percent increase in the fraction of non-recourse homeowners leads to a decline of -0.88 percentage points in non-durable goods consumption from 2008 to 2011, and a 1.22 percentage point decline in retail and restaurant consumption growth. In contrast, a one standard deviation in homestead protection leads

to a 1.34 percentage point increase in non-durable goods consumption growth and a 1.2 percentage point increase in retail and restaurant consumption. This suggests that the local consumption channel is driving the employment in non-tradables.

Similar to Table 2.5, the remaining columns in Table 2.6 perform additional robustness tests to confirm our results. In columns 2 and 5, we control for MSA supply elasticity from Saiz (2008), controlling for the fraction of the state where the measure is missing. In columns 3 and 6, we exclude California, Nevada, New Mexico, Arizona, and Florida from our regression. We find no significant difference in our effects in columns 2 and 5, while in column 3 and 6 much of the main effect in non-recourse protections is too noisy to precisely measure.

These results are broadly consistent with the debtor protections leading to higher consumption. Similar to the employment results, we find that bankruptcy homestead exemptions lead to higher consumption growth, while non-recourse protections decrease consumption. We interpret this evidence as supporting our county-level hypothesis that employment in non-tradables is influenced by local consumption due to debtor protections.

#### **2.6.4 House Price Growth**

One possible explanation for the negative effects of non-recourse protections on consumption and employment is that the forced sales from foreclosures driven by non-recourse protections lead to declines in local housing prices. This drop in housing wealth could cause a significant decline in consumption and employment as well, following the work of Mian, Rao and Sufi (2013) and Mian and Sufi (2014). We test this mechanism by examining the effect of debtor protections on house prices growth. Table 2.7 presents instrumental variable estimates of the impact of non-recourse and bankruptcy home equity protections on house price growth from 2007 to 2011. Column 1 present results for the full sample of zip codes. Column 2 to 4 present results for those zip codes where the Saiz (2008) measure is available. All specifications are estimated at the zip code level weighted by the number of credit reports observed in each zip code. Standard errors are clustered at the state level.

**Table 2.7: Debtor Protections and House Price Growth**

	House Price Growth (2007-2011)			
	(1)	(2)	(3)	(4)
Non-Recourse Protected	-46.996*** (15.243)	-54.347*** (11.118)		-36.311** (13.607)
Homestead Protected	-12.308 (13.610)	-10.101 (14.050)		-8.190 (10.348)
Supply Inelasticity			-8.078*** (1.881)	-6.812** (2.575)
Mean Dep. Var.	-22.190	-23.635	-23.635	-23.635
Observations	11,688	6,728	6,728	6,728
Clusters	49	39	39	39

Notes: This table reports two-stage least squares estimates of the effect of bankruptcy homestead and non-recourse protections on house price growth at the zip code level. The dependent variable in each column is the percentage growth in house prices, from 2007 to 2011. In column 1, we estimate the effect of Non-Recourse Protected and Homestead Protected without any additional controls. In column 2, we limit the sample to zip codes with an MSA supply elasticity measure from Saiz (2008). In column 3, we regress on MSA supply elasticity from Saiz (2008), multiplied by negative one and standardized to mean zero with standard deviation one. In column 4, we control for all three measures. The Non-Recourse Protected measure is the fraction of individuals in an age-credit score bin who have no liability in the event of a foreclosure residualized in age-credit score regression at the individual level. The Homestead Protected measure is the fraction of individuals in an age-credit score bin who have their home equity completely protected by bankruptcy homestead exemptions, residualized in age-credit score regression at the individual level. We then take the zip code-level weighted average of each instrument. The regressions are estimated using a zip code-level dataset of house price data from Zillow and are weighted by the zip code population. Standard errors are clustered at the state-level. \*\*\* = significant at 1 percent level, \*\* = significant at 5 percent level, \* = significant at 10 percent level.

There is an economically and statistically significant impact of foreclosure non-recourse protection on house price growth during the financial crisis. Using the estimates from column 1, we see that a ten percentage point increase in the fraction of individuals with no liability in a foreclosure decreased house prices by 4.7 percentage points, compared to an overall decline of 22 percent during this period. These results are consistent with foreclosure protections increasing the number of foreclosures in a zip code, which in turn decreases area house prices. This collateral channel had a tremendous impact on house prices, and likely exacerbated an already significant downturn. Moreover, the impact of homestead protections on house prices is statistically insignificant.

In order to control for areas with a possible “boom-bust” cycle, in columns 2 to 4 we compare the effects of the protections with the Saiz (2008) supply elasticity measure. Several authors have used this measure successfully to predict the house price run-up and decline observed during the housing bubble. We note that this result holds in our data, even when controlling for our two measures of debtor protections. We note that in column 4, a one standard deviation increase in supply inelasticity is associated with a 6.8 percentage point decline in house price growth, compared to a one standard deviation effect of non-recourse protections of -3.63 percentage points.

## **2.7 Conclusion**

We estimate the impact of state bankruptcy homestead exemption and non-recourse mortgage laws on household balance sheets, consumption, and employment during the Great Recession. We find that both bankruptcy homestead protections and non-recourse protections reduced homeowners’ debt from 2007 to 2010. Higher levels of bankruptcy protection are also associated with increased county employment growth from 2008 to 2010 in non-tradable sectors and increased state non-durable consumption growth over the same time period. In contrast, higher levels of non-recourse protection are associated with lower county employment growth and lower state consumption growth from 2008 to 2010. We find evidence consistent with these contrasting results being due to non-recourse protec-

tions increasing foreclosures and decreasing area house prices, with no similar effects from bankruptcy homestead protections.

Our results provide new evidence on the effects of debtor protections during a financial crisis. These findings are particularly important given the recent debate on the use of debt relief and debt modification programs to stimulate the economy during the Great Recession. Our findings are consistent with the view that debtor protections allow financially distressed households to relax their debt constraints following an aggregate financial shock, and therefore have the potential to mitigate the kind of fall in household consumption and regional employment observed during the recent downturn. However, our results are also consistent debtor protections having the potential to exacerbate economic distress when they encourage collateral fire sales.

The main limitation of our analysis is that we do not account for the impact of debtor protections on ex-ante borrowing costs or behavior. There may also be important ex-post impacts of debtor protections on outcomes such as interest rates or labor market outcomes that we are unable to measure with our data. It is therefore not possible to derive the optimal level or structure of debtor protections using the estimates from our analysis. These issues remain an important area for future research.

## Chapter 3

# Opting Out of Good Governance<sup>1</sup>

### 3.1 Introduction

Corporate governance mechanisms provide tools for suppliers of capital to control managers. However, investor powers and protections vary widely across countries. In some jurisdictions, corporate governance practices are weak, and insiders can enjoy private benefits at the expense of external capital providers. As a result, outsiders discount financial claims on firms and make it costly for firms to raise funds to pursue growth opportunities.

Firms that conduct business within a single country are typically tied to the corporate governance practices established by their country's legal and regulatory environment. However, firms that cross-list shares on foreign exchanges expose themselves to alternative legal and regulatory environments. This fact forms the basis of one prominent hypothesis for why firms cross-list: the legal bonding hypothesis. According to this hypothesis, managers from countries with weak corporate governance can bond themselves from extracting private benefits at the expense of capital providers by cross-listing into a legal and regulatory environment offering greater investor protections. For firms that have a cross-listing in the U.S., some of these protections are a consequence of needing to comply with SEC regulations concerning disclosures and corporate actions. Others are exchange-specific, and each of the

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<sup>1</sup>Co-authored with Fritz Foley, Jonathan Greenstein and Eric Zwick



major U.S. exchanges has detailed listing requirements.

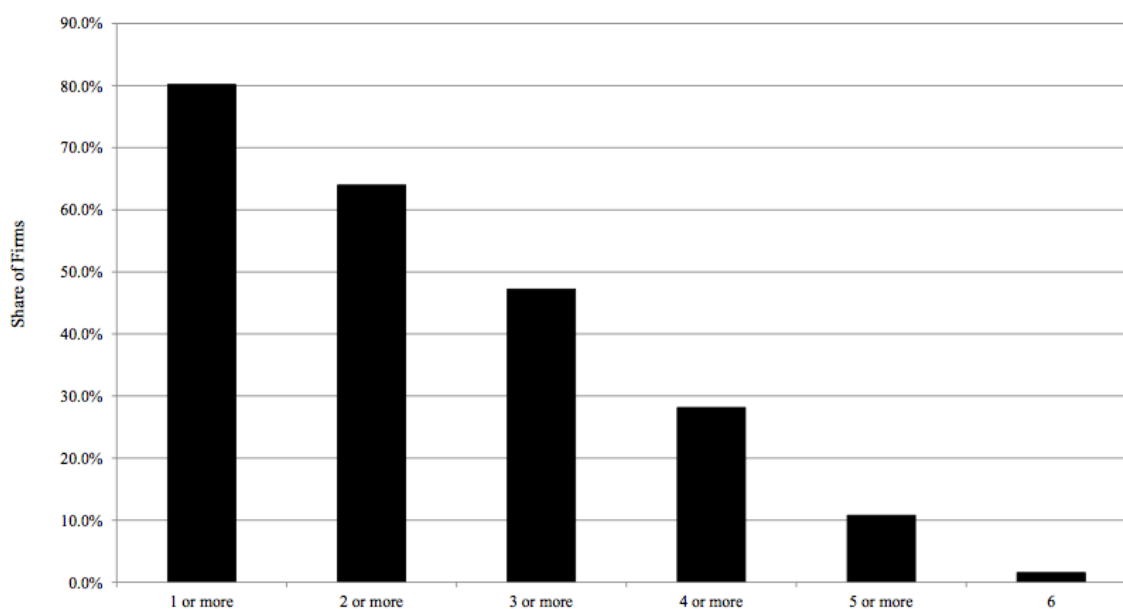
This paper documents the extent to which cross-listed firms choose to opt out of exchange-specific governance regulations, analyzes what drives the choice to opt out, and explores the consequences of this choice. Prior work recognizes that exchanges do not require firms to comply with these regulations, offering firms the option to comply with home country rules instead.<sup>2</sup> Exchange rules refer explicitly to governance structures that are relevant to the bonding hypothesis, but data limitations have prevented extensive formal analysis of firm compliance.<sup>3</sup> Historically compliance choices were not well publicized. But this changed in September 2008, when the SEC deemed the compliance choices important for investors and mandated that foreign firms listed on U.S. exchanges disclose opt out choices in a more consolidated and concise form in their Form 20-F annual filings. Organizing the exchange-specific governance rules into six categories relating to board requirements, auditing, stock issuance, and business practices, this paper presents and analyzes the opt out choices disclosed in the Form 20-F filings immediately after the SEC rule change.

Four main findings emerge. First, opting out is very common. Figure 3.1 displays the share of firms that opt out of different numbers of exchange governance requirements. 80.2% of cross-listed firms opt out of at least one category of requirements. A large fraction of firms opt out of many types as well; 47.2% of firms opt out of three or more categories of requirements. Second, opting out of exchange governance requirements is correlated with weaker governance practices. While many governance practices are hard to observe directly, it is possible to measure features of a firm's board of directors. Analysis of the board composition of cross-listed firms reveals that firms opting out of board independence rules, board committee rules, and audit committee rules have significantly fewer independent

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<sup>2</sup>Coffee (2002) notes: "Although U.S. exchanges do impose significant corporate governance requirements on domestic firms that regulate board structure and protect shareholder voting rights, they have largely waived these substantive corporate governance requirements in the case of foreign issuers."

<sup>3</sup>For example, an important set of these rules covers board structure and independence, which have been found to affect firm values and performance. Adams, Hermalin, and Weisbach (2010) survey this literature and Dahya, Dimitrov, and McConnell (2008) provide evidence that board structure affects valuations within the sample of cross-listing firms.



**Figure 3.1:** *Share of Firms Opting Out of Different Number of Requirements*

Notes: This figure displays the share of firms that opt out of different numbers of exchange governance requirements. Each bar represents the share when the number of categories of opt outs correspond to the values displayed on the x-axis.

board members.

Third, the decision to opt out of exchange governance requirements seems to reflect the incentives created by insiders' ability to consume private benefits when governance remains weak and by managers' desire to raise capital when growth opportunities are attractive. Managers of firms based in countries where corporate governance is weak typically give up larger private benefits by complying fully with U.S. exchange requirements. Consistent with this notion, tests reveal that firms are more likely to opt out of U.S. exchange requirements if they are based in civil law countries and countries with lower measures of the Anti-Self-Dealing Index created by Djankov, LaPorta, Lopez-de-Silanes, and Shleifer (2008). However, managers of firms based in countries with weak governance appear to be more willing to comply with U.S. exchange requirements if they need capital to fund growth of their firm. In particular, firms based in countries with weak corporate governance are less likely to opt out if they are small, are experiencing higher levels of growth in property, plant, and

equipment, or are engaging in equity issuances.

The fourth main finding is that opting out has value consequences. Simple analysis of the relationship between opting out and Tobin's  $q$  is confounded by unobservable determinants of the value of cross-listed firms, such as the extent of growth opportunities in different countries. However, using methods developed by Faulkender and Wang (2006), Dittmar and Mahrt-Smith (2007), and Frésard and Salva (2010) enables a direct test of the governance mechanism by studying the value of cash inside the cross-listed firms. For cross-listed firms based in civil law countries that are fully compliant with U.S. exchange governance requirements, a dollar inside the firm is worth \$1.52. However, if such a firm opts out of all six types of requirements, a dollar inside the firm is worth only \$0.32.

These findings have implications for several strands of the finance literature. Most directly, the findings contribute to the active debate on the determinants of cross-listing. Karolyi (1998, 2006, and 2012) surveys this literature and discusses the motives for and effects of cross-listing. In characterizing the incentives to cross-list, earlier work emphasized a variety of considerations, including market risk exposures and liquidity. Stulz (1999) and Coffee (1999) raise the possibility that corporate governance issues are central to explaining cross-listing and that managers decide to list on U.S. exchanges in order to commit to abide by certain legal and regulatory practices, thereby limiting opportunities for insiders to expropriate capital providers. These themes have received significant empirical support.<sup>4</sup> Although this literature has pointed out differences between exchange-listed and non-exchange listed ADRs, it has not provided detailed analysis of the extent to which managers actually opt out of U.S. exchange requirements when they cross-list.<sup>5</sup> More generally, few studies have been able to observe the governance choices of cross-listed firms, which offer significant insight into the motives for cross-listing and also into the channels through which

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<sup>4</sup>See, for example, Reese and Weisbach (2002), Doidge (2004), Doidge, Karolyi, and Stulz (2004), and Doidge, Karolyi, Lins, Miller, and Stulz (2009).

<sup>5</sup>Siegel (2005) also questions the legal bonding hypothesis, argues for a nuanced view of what complying with U.S. securities laws entails, and stresses the importance of reputational bonding as a mechanism for committing to lawfulness, disclosure, and good governance.

the cross-listing decision influences corporate governance and valuation.<sup>6</sup>

In the literature on cross-listings, the insights in Doidge, Karolyi, Lins, Miller, and Stulz (2009) (DKLMS) and Doidge, Karolyi, and Stulz (2004) (DKS) are particularly relevant to the study of opting out. DKLMS present evidence that when managers have the opportunity to enjoy private benefits, they are less likely to cross-list their firms on U.S. exchanges. DKLMS also find that managers of firms with better investment opportunities are more likely to cross-list because, by reducing the extraction of private benefits, cross-listing lowers the cost of raising external capital. As this paper shows, these same considerations motivating the decision to cross-list help explain the decision to opt out of exchange requirements. Managers of firms that are likely to have the opportunity to enjoy private benefits are less likely to comply with exchange requirements, but, if such managers are in charge of firms with attractive growth opportunities, compliance levels are relatively higher. DKS illustrate that cross-listing is associated with a larger valuation gap for firms based in countries with weak corporate governance practices than it is for firms based in countries with strong corporate governance practices. Similarly, analysis of the extent to which firms opt out of exchange requirements illustrates that opting out has value consequences. Thus, this paper's results suggest refinements to, rather than a rethinking of, the legal bonding hypothesis used to explain cross-listing.

More generally, this paper adds to work on how better governance practices facilitate the process of raising capital from external sources. The modern formulation of agency costs in Jensen and Meckling (1976) illustrates that insiders who want to raise capital must give up larger claims on their firms if they are expected to divert more resources to their own private benefit. A considerable body of work shows that country-level differences in governance practices affect the ease with which firms can raise external finance.<sup>7</sup> By linking the extent of exchange rule compliance to capital raising activity, this paper demonstrates

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<sup>6</sup>One exception is Hope, Kang, and Zang (2007), who investigate cross-listing firms' choice of disclosure regime.

<sup>7</sup>See, for example, LaPorta, Lopez-de-Silanes, Shleifer, and Vishny (1997); Rajan and Zingales (1998); Beck, Demirguc-Kunt, and Maksimovic (2005); and Djankov, LaPorta, Lopez-de-Silanes, and Shleifer (2008).

that governance considerations matter when firms are seeking to raise funds from external sources.<sup>8</sup>

Finally, in illustrating how the value of cash inside of firms varies with the extent to which firms opt out of exchange requirements, this paper advances the idea that corporate governance affects valuations. Recent work on this topic, such as Gompers, Ishii, and Metrick (2002), Durnev and Kim (2005), Pinkowitz, Stulz, and Williamson (2006), and Bebchuk, Cohen, and Farrell (2009), indicates that financial claims are valued more dearly in the market when corporate governance practices are stronger. As Karolyi (2012) notes, the cross-listing literature has struggled to reach consensus about the sign and persistence of valuation effects and their relation to legal bonding. Because many studies either compare cross-listed firms to domestic counterparts or to U.S. firms, their designs may suffer from omitted variable bias. In contrast, this paper documents evidence supporting the cost-benefit framework of bonding using variation within the sample of firms cross-listed on U.S. exchanges and controlling for home country effects. The rest of this paper is organized as follows. Section 3.2 describes the governance requirements imposed by U.S. exchanges and documents the extent to which cross-listed firms opt out of these requirements. Section 3.3 discusses whether opting out is associated with material differences in the governance practices of cross-listed firms. Section 3.4 provides analysis of potential motivations managers of cross-listed firms might have for opting out, and Section 3.5 considers if opting out has value consequences. Section 3.6 concludes.

### **3.2 Exchange Governance Requirements and Foreign Firm Opt Outs**

When issuing securities in the U.S., foreign private issuers trigger the Securities Act of 1933, the Exchange Act of 1934, and the Sarbanes-Oxley Act of 2002, and thereby become exposed

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<sup>8</sup>Reese and Weisbach (2002) and Gozzi, Levine, and Schmukler (2008) study capital raising activity post-cross-listing and how this varies by home country regime but do not explore the relationship between capital raising and firm governance choices.

to potential SEC and private enforcement action. These laws and mandated accounting standards remain largely silent on firm governance practices. The major exchanges, namely the NYSE, NASDAQ and AMEX, impose additional governance requirements on listed firms. However, the exchanges make an exception for foreign cross-listing firms, which are excluded from mandatory compliance with many of these rules.<sup>9</sup> Instead, U.S. stock exchanges permit listed foreign firms to follow their home country governance practices, provided firms disclose how these practices differ from those stated in the exchange requirements.

Prior to 2008, firms that opted out of exchange governance requirements had discretion in how they presented this information, often placing it on the company website or in annual report footnotes. It was not uncommon for investors to be led through a series of cross-references among different disclosure documents and websites in order to compile the firm's governance details.

In September 2008, seeking to standardize these disclosures and to facilitate investors' ability to monitor foreign firms' corporate governance practices, the SEC amended its rules to require foreign firms listed on U.S. stock exchanges to file annual governance disclosures on Form 20-F under a new section, "Item 16G – Corporate Governance." This rule went into effect for fiscal years ending on or after December 15, 2008. This change raised the potential cost of not disclosing deviations from exchange governance requirements because it added to the risk of stock exchange penalties the additional liability arising from material misstatements or omissions in an annual SEC filing.

This paper presents data of firms' governance exemptions that were made available by the SEC's rule change and hand collected from the first Item 16G of Form 20-F filings. The dataset covers 519 firms listed on the NYSE, NASDAQ, and AMEX markets through Level II ADRs, Level III ADRs, or direct listings, based on the SEC's official list of "Foreign companies registered and reporting with the U.S. SEC; December 31, 2008" (SEC, 2009). The

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<sup>9</sup>See, for example, Exchange Act Release No. 24,634, 52 Fed. Reg. 24230 (June 23, 1987) ("Order Approving Proposed Rule Changes by the American Stock Exchange, Inc. and the New York Stock Exchange Inc. to Amend the Exchanges' Listing Standards for Foreign Companies").

other forms of cross-listing, namely unregistered transactions and Level I ADRs, maintain the Exchange Act’s Rule 12g3-2(b) exemption from registration and are not exchange-listed so they are not subject to exchange governance requirements.<sup>10</sup>

Each of the exchanges has a listing standards manual that details the corporate governance requirements that firms must follow unless they opt out. Table 3.1 provides general governance rules and then describes each of the provisions imposed by the NYSE, NASDAQ, and AMEX. There are 12 provisions for the NYSE and 20 for NASDAQ and AMEX. The provisions of different exchanges follow the same basic framework. In Table 3.1, provisions are grouped in the categories of board independence requirements, board committee requirements, audit committee requirements, general corporate practices, shareholder approval requirements for stock issuance, and good governance practices. These categories generally reflect the manner in which the provisions are presented in the exchanges’ listing manuals and by firms in their Item 16G disclosures.

**Table 3.1:** *Description of the U.S. Stock Exchanges’ Governance Requirements*

Governance Requirement	Description
<b>A. NYSE</b>	
<i>General Rules</i>	Listed firms that are foreign private issuers, as such term is defined in Rule 3b-4 under the Exchange Act, are permitted to follow home country practice in lieu of the provisions of Section 303A of the NYSE Listed Company Manual. Section 303A.11 requires the disclosure of differences between the corporate governance rules contained in Section 303A which reflect the practices required of domestic firms and a particular foreign firm’s practices. Deviations from exchange requirements related to provisions for shareholder approval for issuance of securities (Rule 312.03) and solicitation of proxies (Rule 402.00,402.04) need not be disclosed in Item 16G so these are not captured in the analysis.
<b>Board Independence Requirements</b>	
<i>Continued on next page</i>	

<sup>10</sup>U.S. securities laws apply to all foreign private issuers—a term that covers non-U.S. issuers, excluding foreign governments—entering U.S. capital markets to raise capital or to enhance the liquidity of their shares. However, the regulations imposed on foreign firms are intended to be proportional to the perceived investor risk posed by firms’ modes of accessing the U.S. markets. Unregistered transactions, such as those occurring offshore, or through Level I ADR programs which represent shares that are traded only over-the-counter in the U.S., through private placements, or through Rule 144A resales to qualified institutional buyers, do not require filing a registration statement with the SEC and meeting specific disclosure and financial statement requirements, nor will these transactions typically be subject to the Sarbanes-Oxley Act (Cohen et al., 2009).

**Table 3.1:** *Description of the U.S. Stock Exchanges' Governance Requirements (continued)*

Governance Requirement	Description
Majority Board Independence	Rule 303A.01 requires that a majority of the directors of a listed firm be independent. Independence is determined in accordance with the criteria in Rule 303A.02. To be considered independent, the Board must determine that the member has no material relationship with the firm. The NYSE also has identified certain specific relationships with the firm that preclude an individual from being considered independent until 3 years after the specified relationship has ended. The bright line test items include: being an employee of the firm, receiving direct compensation of \$120,000 in any year, or being an executive officer or having a family member be an executive officer of an entity that receives the greater of \$1 million or 2% of its consolidated gross revenues from the firm.
Executive Sessions of Non-Management Directors	Rule 303A.03 requires that non-management directors meet at regularly scheduled sessions without management. This NYSE requirement allows directors to be included in such sessions who are not considered independent for reasons other than being an officer of the firm. In the event that executive sessions include non-independent members, the NYSE recommends that there be one meeting per year where only independent members meet in executive session.
<b>Board Committee Requirements</b>	
Establish Nominating Committee or Nomination Decisions by Independent Directors	Rule 303A.04 requires that listed firms have a nominating committee composed entirely of independent directors or that the directors be nominated by the independent board members. The committee is required to have a written charter that describes the committee's purpose and responsibilities and also requires the committee to conduct an annual performance appraisal. The committee's responsibilities include identifying and selecting the director nominees unless the firm is legally bound by law or contract to have others nominate certain directors.
Establish Compensation Committee or Compensation Decisions by Independent Directors	Rule 303A.05 requires that listed firms have a compensation committee composed entirely of independent directors. The committee is required to have a written charter which describes the committee's purpose and responsibilities and also requires the committee to conduct an annual performance appraisal. Among the committee's responsibilities are to set the CEO's compensation and to make recommendations to the board for the compensation of non-CEO executive officers.
<b>Audit Committee Requirements</b>	
Audit Committee: Minimum of Three Members	Rule 303A.07(a) requires that the audit committee of listed firms have at least three members and each member must have knowledge in finance. At least one of its members must have experience in accounting or financial matters. This rule also limits the number of audit committees on which a member can serve to three unless the board determines that simultaneous service on multiple boards does not interfere with the member's duties to this committee.

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**Table 3.1:** Description of the U.S. Stock Exchanges' Governance Requirements (continued)

Governance Requirement	Description
Audit Committee: Independent Members	Rule: 303A.07(b) requires that in addition to the independence requirements set forth in Rule 10A-3 under the Exchange Act that prohibit a director from receiving compensation from the firm or being an affiliated person of the firm or any of its subsidiaries, each audit committee member must satisfy the specific bright line test requirements for independence set in 303A.02 that are described above.
Audit Committee: Charter	Rule 303A.07(c) requires that the audit committee have a written charter that addresses the duties and responsibilities of the audit committee which must include those set out in Rule 10A-3 of the Exchange Act and under Section 303A.07(c). The Exchange Act principally requires the committee to be responsible for the appointment, compensation, retention, and oversight of the outside auditors and have such auditors report directly to the committee. Section 303A.07(c) requires the committee to conduct a performance appraisal of the committee, to obtain and review the outside auditors report of the firm's internal controls, to discuss policies regarding risk and risk management, to review the firm's financial statements and disclosures, and to set hiring policies involving employees of the outside auditor.
Audit Committee: Internal Audit Function	Rule 303A.07(d) requires that each listed firm have an internal audit department to provide management and the audit committee with ongoing assessments of the firm's risk management processes and system of internal control.
<b>Shareholder Approval Requirements for Stock Issuances</b>	
Shareholder Approval to Establish or Amend Equity Compensation Plan	Rule 303A.08 requires that shareholders must be given the opportunity to vote on all equity compensation plans and material revisions thereto.
<b>General Good Governance Practices</b>	
Corporate Governance Guidelines	Rule 303A.09 requires that listed firms must adopt and disclose their corporate governance guidelines. The board of directors should evaluate the performance of its functions and its committees at least once a year.
Code of Business Conduct and Ethics	Rule 303A.10 requires that listed firms must adopt and disclose a Code of Business Conduct and Ethics for directors and employees and promptly disclose any waivers of the code for directors or executive officers. The code of ethics must deal with conflicts of interest, corporate opportunities, compliance with laws and regulations, and confidentiality.
Corporate Governance Website Portal	Rule 303A.14 requires that listed firms have and maintain a publicly accessible website.
<b>B. NASDAQ</b>	

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**Table 3.1:** *Description of the U.S. Stock Exchanges' Governance Requirements (continued)*

Governance Requirement	Description
<i>General Rules</i>	The 5600 series rules of the NASDAQ Manual comprise the NASDAQ's Corporate Governance Requirements. The requirements under this section are more comprehensive than the 303A corporate governance requirements contained in the NYSE Listed Company Manual. Rule 5615 provides that a foreign firm may follow its home country practice in lieu of the requirements of Rule 5600, provided that the foreign private issuer discloses in its annual reports filed with the SEC each requirement of the 5600 series rules that it does not follow and describes the home country practice followed instead.
<b>Board Independence Requirements</b>	
Majority Board Independence	Rule 5605(b)(1) requires that a majority of the Board of Directors be independent directors as defined in Rule 5605(a)(2). To be independent, the individual cannot be an executive officer or employee of the firm, or an individual that has a relationship with the firm that the board determines would interfere with the individual exercising independent judgment. This definition differs from the NYSE definition in that the NASDAQ permits a relationship so long as the board determines that that it will not interfere with the individual's independent judgment. The NASDAQ also lists certain specific relationships with the firm that would disqualify an individual from being considered independent for a period of 3 years after the specified relationship ends. The bright line test indicate that a relationship exists if an individual is an employee of the firm, if he or a family member receives direct compensation of \$120,000 or more in any 12 month period, if the individual is an executive officer of the firm, or if the individual or a family member is an executive officer of an entity that receives the greater of \$200,000 or 5% of its consolidated gross revenues from the firm in a given year.
Executive Sessions of Independent Directors	Rule 5605(b)(2) requires that the independent directors must have regularly scheduled meetings at which only they are present. There should be a minimum of two executive sessions each year.
<b>Board Committee Requirements</b>	
Establish Compensation Committee or Compensation Decisions by Independent Directors	Rule 5605(d)(1) requires that the compensation of the CEO and other executive officers be determined, or recommended to the Board of Directors, either by a majority of the independent directors or by a compensation committee comprised solely of independent directors. The CEO may not be present during voting or deliberations. Unlike the NYSE rule, no charter is required for the compensation committee.
Establish Nominating Committee or Nominating Decisions by Independent Directors	Rule 5605(e)(1) requires that director nominees must be selected or recommended for selection by the board of directors, either by a majority of the independent directors or by a nominations committee comprised solely of independent directors, in accordance with the nominations process set forth in a formal written charter or board resolution.
Adopt Charter or Board Resolution Governing Nominating Committee	Rule 5605(e)(2) requires that the board must adopt a charter or resolution describing the committee's responsibilities, including the nomination process.

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**Table 3.1:** *Description of the U.S. Stock Exchanges' Governance Requirements (continued)*

Governance Requirement	Description
<b>Audit Committee Requirements</b>	
Audit Committee Charter	Rule 5605(c)(1) requires that each firm adopt a formal written audit committee charter that explains the duties and responsibilities of the audit committee which, at a minimum, must include those set out in Rule 10A-3 of the Exchange Act. The purpose of the committee is to oversee the firm's financial reporting process and oversee auditor independence.
Audit Committee Composition Members Independent	Rule 5605(c)(2)(A)(i) requires that the firm must have an audit committee of at least three members who are independent as defined under Rule 5605(a)(2), meet the independence criteria set forth in Rule 10A-3(b)(1) under the Exchange Act and satisfy certain other criteria. The independence requirements set forth in Rule 10A-3 under the Exchange Act prohibit a director from receiving compensation for anything other than board and committee service from the firm or being an affiliated person of the firm or any of its subsidiaries. The SEC provides limited exemptions from the audit committee requirements for firms from certain countries where country laws require a statutory auditor or board of auditors.
Audit Committee No Participation in Preparing Financial Statements	Rule 5605(c)(2)(A)(iii) requires that each member must certify that they have not participated in the preparation of the firm's financial statements at any time during the last three years.
Audit Committee Member Financial Literacy	Rule 5605(c)(2)(A)(iv) requires that each member must be able to read and understand financial statements and at least one member of the audit committee must have past employment in finance or accounting.
<b>General Corporate Practices</b>	
Hold Annual Shareholder Meeting	Rule 5620(a) requires that each firm with listed common stock must hold an annual meeting of shareholders no later than one year after the end of the firm's fiscal-year end.
Solicit Proxies and Provide Proxy Statement for Shareholder Meetings	Rule 5620(b) requires that each firm must solicit proxies and provide proxy statements for all meetings of shareholders and provide copies of such proxy solicitation to the NASDAQ.
Distribution of Annual Reports	Rule 5250(d)(1) requires that firms must make their annual report available to shareholders containing audited financial statements within a reasonable period after it is filed with the SEC.
33.33% Quorum	Rule 5620(c) requires that each firm must provide for a quorum for any meeting of its shareholders. The quorum may not be less than 33.33% of the outstanding shares of the firm's voting common stock.
<b>Shareholder Approval Requirements for Stock Issuances</b>	
Shareholder Approval to Establish or Amend Equity Compensation Plan	Rule 5635(c) requires shareholder approval when an equity compensation plan is established or materially amended.

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**Table 3.1:** *Description of the U.S. Stock Exchanges' Governance Requirements (continued)*

Governance Requirement	Description
Shareholder Approval for Change of Control	Rule 5635(b) requires a shareholder vote before the firm's common stock is issued if such issuance will result in a change of control of the firm.
Shareholder Approval for Acquisition of Stock or Assets of Related Party or Involving a 20% Private Issuance	Rule 5635(a) requires a shareholder vote before the firm's stock is issued in connection with certain acquisitions of stock of another firm where a related party is involved or where the stock to be issued exceeds 20% of the firm's stock.
Shareholder Approval for 20% Private Issuance at Below Market Value	Rule 5365(d)(1-2) requires a shareholder vote if 20% or more of the firm's stock is issued privately at a price below the current market value of the stock.
<b>General Good Governance Practices</b>	
Direct Registration Program	Rule 5255a requires that securities listed on the NASDAQ must be eligible for a direct registration program operated by a clearing agency. Direct registration allows the shareholder to be registered directly with the transfer agent without the need of a physical certificate to provide evidence of ownership.
Code of Conduct	Rule 5610 requires that listed firms must adopt a code of conduct applicable to all directors, officers, and employees.
Conflicts of Interest	Rule 5630(a) provides that the audit committee or another independent body of the board of directors of each firm must conduct appropriate review and oversight of all related party transactions for potential conflict of interest situations on an ongoing basis.
<b>C. AMEX Requirements</b>	
<i>General Rules</i>	AMEX Rule 110 provides that a foreign private issuer may follow its home country practice in lieu of the requirements of Part 8 of the AMEX Company Guide, provided that the foreign private issuer discloses in its annual reports filed with the SEC or on its website each requirement of Part 8, as well as certain specified provisions outside of Part 8, that it does not follow and describes the home country practice followed instead.
<b>Board Independence Requirements</b>	
Majority Board Independence	Rule 802(a) requires that a majority of the Board of Directors be independent directors as defined in Rule 803(A)(2). To be independent, the individual cannot be an officer or employee of the firm, or an individual that has a relationship with the firm that the board determines would interfere with the individual exercising independent judgment. This differs from the NYSE rule in that it allows a relationship to exist so long as the board determines that it will not interfere with the individual carrying out the required duties.

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**Table 3.1:** *Description of the U.S. Stock Exchanges' Governance Requirements (continued)*

Governance Requirement	Description
Executive Sessions of Independent Directors	Rule 802 (c) requires that the independent directors must have regularly scheduled meetings at which only they are present. There should be a minimum of one executive sessions each year.
<b>Board Committee Requirements</b>	
Establish Compensation Committee or Compensation Decisions by Independent Directors	Rule 805(a) requires that compensation of the CEO and other executive officers must be determined, or recommended to the Board of Directors, either by a majority of the independent directors or by a compensation committee comprised solely of independent directors. The CEO may not be present during voting or deliberations. No charter is required. Under the AMEX rules a compensation is not required if the independent directors approve CEO compensation.
Establish Nominating Committee or Nominating Decisions by Independent Directors	Rule 804(a) requires that director nominees must be selected, or recommended for selection by the board of directors, either by a majority of the independent directors or by a nominations committee comprised solely of independent directors, in accordance with the nominations process set forth in a formal written charter or board resolution. A nomination committee is not necessary if the independent directors nominate the directors.
Adopt Charter or Board Resolution Governing Nominating Committee	Rule 804(c) requires that the board adopt a charter or resolution describing the committee's responsibilities, including the nomination process.
<b>Audit Committee Requirements</b>	
Audit Committee Charter	Rule 803(B)(1) requires that each firm adopt a formal written audit committee charter that explains the duties and responsibilities of the audit committee which, at a minimum, must include those set out in Rule 10A-3 of the Exchange Act.
Audit Committee Composition Members Independent	Rule 803(B)(2)(a)(i) requires that each firm must have an audit committee of at least three members who are independent as defined under Rule 803(A)(2), meet the independence criteria set forth in Rule 10A-3(b)(1) under the Exchange Act, and satisfy certain other criteria.
Audit Committee No Participation in Preparing Financial Statements	Rule 803(B)(2)(a)(ii) requires that each member must certify that they have not participated in the preparation of the firm's financial statements at any time during the last three years.
Audit Committee Member Financial Literacy	Rule 803(B)(2)(a)(iii) requires that each member must be able to read and understand financial statements and that at least one member of the audit committee must have past employment in finance or accounting.
<b>General Corporate Practices</b>	
Hold Annual Shareholder Meeting	Rule 704 requires that each firm hold an annual meeting of shareholders no later than one year after the end of the firm's fiscal-year end.

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**Table 3.1:** *Description of the U.S. Stock Exchanges' Governance Requirements (continued)*

Governance Requirement	Description
Solicit Proxies and Provide Proxy Statement for Shareholder Meetings	Rule 705 requires that each firm must solicit proxies and provide proxy statements for all meetings of shareholders and provide copies of such proxy solicitation to the AMEX.
Distribution of Annual Reports	Rule 610 requires that each firm must make their annual report available to shareholders containing audited financial statements within a reasonable period after it is filed with the SEC.
33.33% Quorum	Rule 123 requires that each firm must have a quorum for any meeting of its shareholders. The quorum may not be less than 33.33% of the outstanding shares of the firm's voting common stock.
<b>Shareholder Approval Requirements for Stock Issuances</b>	
Establish or Amend Equity Compensation Plan	Rule 711 requires shareholder approval when an equity compensation plan is established or materially amended.
Change of Control	Rule 713(b) requires a shareholder vote before the firm's common stock is issued if such issuance will result in a change of control of the firm.
Acquisition of Stock or Assets of Related Party or Involving a 20% Private Issuance	Rule 712(b) requires a shareholder vote before the firm's stock is issued in connection with certain acquisitions of stock of another firm where a related party is involved or where the stock to be issued exceeds 20% of the firm's stock.
20% Private Issuance at Below Market Value	Rule 713(a)(ii) requires a shareholder vote if 20% or more of the firm's stock is issued privately at a price below the current market value of the stock.
<b>General Good Governance Practices</b>	
Direct Registration Program	Rule 135 requires that securities listed on AMEX must be eligible for a direct registration program operated by a clearing agency. Direct registration allows the shareholder to be registered directly with the transfer agent without the need of a physical certificate to provide evidence of ownership.
Code of Conduct	Rule 807 requires that each firm must adopt a code of conduct applicable to all directors, officers, and employees.
Conflicts of Interest	Rule 120 requires that each firm conduct appropriate review and oversight of all related party transactions for potential conflict of interest situations on an ongoing basis by the firm's audit committee or another independent body of the board of directors.

Notes: This table provides a description of each of the governance requirements of the NYSE, NASDAQ, and AMEX that foreign cross-listed firms might opt out of. For each exchange, the individual requirements are grouped into the larger categories used in the paper: Board Independence, Board Committee, Audit Committee, General Corporate Practices, Shareholder Approval for Stock Issuances, and General Good Governance Practices.

Board independence requirements mandate that a majority of board directors be "inde-

pendent,” based on several bright line tests including current employment, remuneration, and family connections to current employees. These provisions also require that independent directors have regular meetings that exclude inside directors. Board committee requirements state that executive compensation and nominations for new directors must be determined by a committee consisting of a majority of independent directors. Audit committee requirements mandate the existence of a chartered audit committee consisting of independent directors and restrict the ability of these directors to participate in the preparation of the firm’s financial statements. Shareholder approval requirements for stock issuance stipulate that shareholders be allowed to vote on new equity compensation plans, as well as the issuance of additional company stock. The general corporate practices and good governance practices categories include rules about, for example, soliciting shareholder proxies, distributing annual reports, reviewing big transactions for conflicts of interest, and establishing a posted code of conduct. Table 3.1 contains additional details about each of these categories.

Provisions tend to be very similar across exchanges, although there are a few differences. For example, all three exchanges require a majority of directors be independent and that there be executive sessions of non-management directors, but there is variation in exactly how independence is determined and who can and cannot participate in executive sessions. The most notable difference between the requirements of different exchanges, as analyzed in this paper, is that the NYSE’s corporate governance standards section does not have provisions categorized as general corporate practices.

The SEC rule requires that a firm note significant differences between its practices and the governance requirements of its exchange in Item 16G. An exemption is coded anytime a requirement is reviewed in an Item 16G disclosure, and a foreign firm’s practices are not consistent with the practices that would be followed by a compliant U.S. firm’s practice. A firm that opts out of at least one of the specific provisions within a category is coded as opting out of that category. Because the SEC rules only require the disclosure of exemptions, the absence of disclosures on any governance requirement is assumed to indicate compliance.

Exemptions are measured in the first year they are reported under the requirements of the September 2008 SEC rule change; thus, these are captured for the first fiscal year of firms that ends on or after December 15, 2008.

Firms' governance exemptions appear to remain very stable following these initial filings. In order to make a change to governance practices, managers typically must win any approvals required by their firm's bylaws or corporate charter. They must also promptly notify the exchange where the firm's shares are listed and file forms noting changes with the SEC. Failing to report exemptions in an accurate and timely manner leaves firms liable to legal action. A review of Form 6-K filings as well as a hand comparison of a sample of Form 20-F filings covering the years 2008-2011 revealed few minor changes to governance practices. Therefore, firms do not appear to temporarily mislead investors by claiming to act in accordance with certain required governance practices and then changing those required practices.

To illustrate the collection procedure, consider the case of Advanced Semiconductor Engineering, Inc., a Taiwan-based company cross-listed on the NYSE. In Item 16G of its Form 20-F filing for the fiscal year ending December 31, 2008, the company lists its opt outs from NYSE governance rules. For example, the NYSE requires that a listed firm have a nominating committee and a compensation committee, each composed entirely of independent directors and governed by a written charter that provides for certain responsibilities of the committee set out in the NYSE listing standards. The company writes, "We do not have a nominating/corporate governance committee. The ROC Company Law does not require companies incorporated in the ROC to have a nominating/corporate governance committee." With regards to a compensation committee, the company states, "We do not have a compensation committee. Under the ROC Company Law, companies incorporated in the ROC are not required to have a compensation committee." In the previous year's annual filing, prior to the change in SEC disclosure rule, there is no mention of the opt outs from exchange requirements, indicating that the SEC rule change affected company disclosures.



In another example, the Brazilian company CPFL Energia S.A.'s Form 20-F for the fiscal year ending December 31, 2008 notes, "The non-management directors of CPFL do not meet at regularly scheduled executive sessions without management"—a would-be violation of the NYSE requirement regarding executive sessions of the board. As with Advanced Semiconductor, the Form 20-F from the prior year does not contain a section indicating opt outs.

The resulting dataset of measured opt outs provides a striking picture of the extent of compliance with exchange governance requirements. 80.2% of firms opt out of at least one category of provisions. Table 3.2 displays the extent to which firms from different countries opt out of provisions and the extent to which firms opt out of different categories of provisions. The sample includes cross-listed firms with headquarters based in 45 countries.<sup>11</sup> There is considerable variation in the extent to which firms opt out of different categories of governance requirements, as indicated in the last row of the table that presents the share of firms opting out of each provision. Opting out appears to be common regarding board and audit committee matters. 51.1% of firms opt out of board independence requirements, 54.7% opt out of board committee requirements, and 40.7% opt out of audit committee requirements. 61.2% of firms opt out of general corporate practices, 31.2% opt out of shareholder approval requirements for stock issuance, and 27.4% opt out of general good governance practice requirements.<sup>12</sup> It is noteworthy that different cross-listed firms from the same country exhibit distinct opt out disclosures; this implies that firms may not simply opt out because home country requirements prevent a firm from adopting the requirements of a U.S. exchange.

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<sup>11</sup>For eight of the firms in the sample, the headquarters are located in the U.S. Of these firms, six are incorporated in Canada, one in the U.K., and one in the British Virgin Islands. We include these firms in our analysis but the results are robust to excluding them.

<sup>12</sup>As explained above, NYSE firms are not subject to general corporate practice requirements and therefore cannot have more than 5 total opt outs.

**Table 3.2: Summary Statistics of Opt Outs by Country**

Country	Number of Firms	Any Provisions	Board Independence	Board Committee fee	Audit Committee fee	General Corporate Practices	Shareholder	
							Approval Requirements for Stock Issuance	Good Governance Practices
Argentina	14	93%	93%	93%	86%	100%	0%	64%
Australia	11	73%	27%	55%	27%	71%	27%	18%
Bahamas, The	1	100%	100%	100%	100%	100%	0%	100%
Belgium	1	0%	0%	0%	0%	n.a.	0%	0%
Bermuda	16	69%	56%	63%	38%	75%	19%	38%
Brazil	29	97%	93%	93%	86%	100%	24%	66%
Canada	39	56%	13%	8%	3%	52%	26%	0%
Cayman Islands	1	0%	0%	0%	0%	0%	0%	0%
Chile	13	100%	100%	100%	85%	n.a.	31%	77%
China	79	59%	32%	30%	18%	36%	24%	14%
Colombia	2	100%	100%	100%	100%	n.a.	50%	50%
Denmark	2	100%	50%	50%	50%	100%	50%	100%
Finland	1	100%	0%	0%	0%	n.a.	100%	0%
France	10	100%	60%	60%	80%	100%	10%	50%
Germany	10	100%	90%	90%	90%	100%	30%	20%
Greece	23	83%	52%	39%	9%	83%	39%	30%
Hong Kong, China	17	76%	71%	65%	24%	64%	29%	24%
Hungary	1	100%	100%	100%	0%	n.a.	0%	0%
India	13	85%	69%	54%	54%	100%	8%	54%
Indonesia	2	100%	100%	50%	100%	n.a.	0%	50%
Ireland	11	64%	18%	27%	27%	50%	18%	27%
Israel	66	82%	35%	39%	12%	63%	45%	11%
Italy	5	100%	80%	80%	100%	100%	20%	20%
Japan	21	100%	95%	90%	95%	100%	71%	48%
Korea, Rep.	11	91%	36%	64%	55%	67%	27%	55%
Luxembourg	4	75%	50%	25%	50%	100%	0%	50%
Mexico	19	100%	95%	95%	79%	100%	42%	32%
Netherlands	15	87%	40%	53%	60%	80%	53%	7%
New Zealand	1	100%	0%	100%	0%	n.a.	0%	0%
Norway	1	100%	100%	100%	100%	n.a.	0%	0%

*Continued on next page*

**Table 3.2: Summary Statistics of Opt Outs by Country (continued)**

Country	Number of Firms	Any Provisions	Board Independence	Board Committee	Audit Committee	General Corporate Practices	Shareholder Approval Requirements for Stock Issuance	Good Governance Practices
Panama	2	100%	50%	100%	0%	n.a.	50%	0%
Papua New Guinea	1	100%	100%	0%	0%	100%	0%	0%
Peru	1	100%	100%	100%	100%	n.a.	0%	100%
Philippines	1	100%	100%	100%	100%	n.a.	0%	0%
Portugal	1	100%	100%	100%	100%	n.a.	0%	100%
Russian Federation	5	100%	80%	100%	60%	n.a.	80%	60%
Singapore	1	100%	100%	100%	100%	n.a.	0%	0%
South Africa	6	83%	50%	50%	33%	100%	17%	0%
Spain	5	100%	80%	80%	80%	100%	20%	40%
Sweden	1	100%	100%	100%	100%	100%	0%	0%
Switzerland	7	100%	14%	14%	86%	n.a.	71%	0%
Taiwan, China	10	90%	70%	80%	60%	60%	50%	40%
Turkey	1	100%	100%	100%	100%	n.a.	0%	100%
United Kingdom	30	87%	27%	77%	23%	67%	27%	23%
United States	8	25%	0%	0%	0%	25%	25%	0%
All Countries	519	80.2%	51.1%	54.7%	40.7%	61.2%	31.2%	27.4%

Notes: This table provides basic statistics on opting out. It indicates the number of firms that are cross-listed on a U.S. exchange but headquartered in each of the countries in the first column. It also provides the share of firms from each of those countries that opt out of U.S. exchange governance requirements that are associated with the categories displayed in the top row. The last row of the table provides information for the whole sample. General Corporate Practices is only relevant to firms listed on NASDAQ and AMEX, so it is computed only for these firms. The notation "n.a." indicates that the percentage is not available because all firms for that country are listed on the NYSE, and the NYSE does not have requirements characterized as General Corporate Practices.

Table 3.3 provides pairwise correlations indicating the extent to which firms that opt out of one category of requirement are likely to opt out of another category. These correlations are all positive, and 13 of the 15 correlations are also statistically distinguishable from zero. Thus, a firm that opts out of one category of governance requirement typically opts out of others as well. As one might expect, the correlation between opting out of board independence requirements and board committee requirements is very high; its value is 0.6426.

**Table 3.3:** *Correlation of Opt Outs*

	Board Independence	Board Committee	Audit Committee	General Corporate Practices	Shareholder Approval Requirements for Stock Issuance	General Good Governance Practices
Board Independence	1.0000					
Board Committees	0.6426***	1.0000				
Audit Committee Requirements	0.5592***	0.5322***	1.0000			
General Corporate Practices	0.4205***	0.3936***	0.3426***	1.0000		
Shareholder Approval Requirements for Stock Issuance	0.1105**	0.0531	0.0689	0.1382***	1.0000	
General Good Governance Practices	0.4452***	0.402***	0.4423***	0.2658***	0.1089**	1.0000

Notes: This table displays the correlation matrix for dummy variables indicating whether a cross-listed firm has opted out of a particular category of U.S. exchange governance requirement. There are six dummies, one for each of the categories of requirements listed in the first row and first column. \*\*\*, \*\*, and \* denote significance at the 1, 5, and 10 percent levels, respectively.

The main variable used to measure opt outs in the analysis below is the total number of opt outs. As indicated in Table 3.4, which presents descriptive statistics, firms have an average of 2.3 opt outs and a median of 2.0. Thus, the median cross-listed firm opts out of 2 of the 6 categories of governance requirements. These basic patterns in the extent to which firms opt out of exchange governance requirements and the positive correlation among types of opt outs suggest that opting out significantly reduces the impact of U.S. exchange requirements on the governance of cross-listed firms. The next section considers this possibility.

**Table 3.4: Summary Statistics**

Variable	Mean	SD	Median
<b>Table 3.5</b>			
Fraction of Independent Directors	0.4293	0.2206	0.4615
Number of Opt Outs	2.3179	1.6803	2.0000
Opt Out of Board Independence Requirements	0.5106	0.5004	1.0000
Opt Out of Board Committee Requirements	0.5472	0.4982	1.0000
Opt Out of Audit Committee Requirements	0.4066	0.4917	0.0000
Opt Out of General Corporate Practices	0.2678	0.4433	0.0000
Opt Out of Shareholder Approval Requirements for Stock Issuance	0.3121	0.4638	0.0000
Opt Out of General Good Governance Practices	0.2736	0.4462	0.0000
Board Size	9.9271	4.8657	9.0000
Log Assets	7.1807	2.8473	7.0242
Leverage	0.3196	0.2775	0.2960
Return on Assets	0.0078	0.1754	0.0354
<b>Table 3.6</b>			
Average Number of Opt Outs	2.6039	1.1790	2.6154
Average of Opt Out Dummy	0.8608	0.2450	1.0000
Average Fraction of Opt Outs	0.4360	0.3205	0.4000
Civil Law Dummy	0.6667	0.4767	1.0000
Anti-Self-Dealing Index	0.5029	0.2374	0.4600
Stock Market Turnover	0.8456	0.6275	0.8100
Log GDP Per Capita	10.0167	0.8518	10.3402
<b>Table 3.7</b>			
Number of Opt Outs	2.3179	1.6803	2.0000
Net PP&E Growth	0.3521	0.6186	0.1668
Equity Issuance	0.2025	0.3546	0.0390
Log Assets	7.1807	2.8473	7.0242
Leverage	0.3196	0.2775	0.2960
Return on Assets	0.0078	0.1754	0.0354
Industry q	1.6281	0.4188	1.5390
<b>Table 3.8</b>			

*Continued on next page*

**Table 3.4: Summary Statistics (continued)**

Variable	Mean	SD	Median
Annualized Excess Returns	0.0738	0.6243	-0.0306
Change in Cash Holdings/ME	0.0224	0.1916	0.0049
Number of Opt Outs X Change in Cash Holdings/ME	0.0583	0.5221	0.0000
Number of Opt Outs	2.2923	1.6633	2.0000
Change in Earnings/ME	0.0259	0.2108	0.0072
Change in Net Assets/ME	0.1141	0.5004	0.0498
Change in R&D/ME	0.0004	0.0162	0.0000
Change in Interest Expenses/ME	0.0017	0.0217	0.0000
Change in Dividends/ME	0.0013	0.0203	0.0000
Lagged Cash/ME	0.2978	0.4501	0.1592
Debt/Market Value	0.2902	0.2683	0.2569
New Finance/ME	0.0497	0.1940	0.0023
Lagged Cash/ME X Change in Cash Holdings/ME	0.0019	0.2555	0.0002
Leverage X Change in Cash Holdings/ME	0.0097	0.0923	0.0000

Notes: This table summarizes the variables used in the regressions in Tables 3.5-3.8. For Table 3.5, Fraction of Independent Directors is the average fraction of directors who are classified as independent in the BoardEx database. Number of Opt Outs is the number of governance categories the firm opts out of. The other opt out variables are dummy variables that are equal to one for firms that opt out of distinct categories of requirements that appear in Table 3.1. Opt Out of General Corporate Practices is equal to zero for all NYSE-listed firms, because the NYSE does not have any such requirements. Board Size is the number of directors on the company's board. Leverage is total debt, defined as the sum of short term and long term debt, divided by total debt plus book equity. Return on Assets is net income divided by total assets. All time-varying covariates are averages of the corresponding variables taken over the five years from 2004 to 2008. For Table 3.6, Average Number of Opt Outs is the number of governance categories the firm opts out of, averaged by country. A firm's country is measured as the reported location of their headquarters. Average of Opt Out Dummy is the country average of a dummy equal to one if a firm opts out of any of the governance requirements. Average Fraction of Opt Outs is the country average of the fraction of categories of requirements that a firm opts out of; there are six categories for NASDAQ and AMEX listed firms and five for NYSE listed firms. The Civil Law Dummy is a dummy equal to one for firms with headquarters in a country with a civil law legal origin. The Anti-Self-Dealing Index is drawn from Djankov, La Porta, Lopez-de-Silanes, and Shleifer (2008), and higher values of this variable indicate that a country imposes stronger controls on self dealing. Stock Market Turnover measures the total value of stocks traded as a fraction of average market capitalization. For Table 3.7, Number of Opt Outs is the number of governance categories the firm opts out of. Net PP&E Growth is the annual first difference in net property, plant, and equipment scaled by lagged property, plant, and equipment. Equity Issuance is the change in common equity plus the change in deferred tax assets minus the change in retained earnings, scaled by lagged assets. Leverage is total debt, defined as the sum of short term and long term debt, divided by total debt plus book equity. Return on Assets is net income divided by total assets. Industry q is calculated by first, for each firm, computing the ratio of the book value of total assets less the book value of equity plus the market value of equity less the book value of deferred taxes to the book value of total assets, and then taking the median value of this ratio for each 2-digit SIC code. All time-varying characteristics are averages of firm variables over the five years from 2004-2008. For Table 3.8, Annualized Excess Returns is the annualized excess return of the firm relative to the Fama and French (1993) 25 size and book-to-market portfolios. Cash includes cash and marketable securities. Many variables are scaled by the market value of equity (ME). Number of Opt Outs is the number of governance categories that firms opt out of. Earnings is earnings before extraordinary items plus interest, deferred taxes, and investment tax credits. Net assets is the value of assets net of cash, and R&D is the value of R&D expenses. Interest expenses include total interest and related expenses. Dividends include common dividends paid, and lagged cash is the lagged value of cash. Debt/Market Value is the ratio of the sum of long term and short term debt to the sum of the long term debt, short term debt, and the market value of equity. New Finance is the sum of net equity issues and net debt issues.

### 3.3 Opting Out and Corporate Governance

Because many features of a firm's management practices are difficult to observe, it is challenging to pinpoint whether opting out of exchange requirements is truly associated with weaker corporate governance. Fortunately, it is possible to measure the share of a firm's directors who are independent. A number of studies have shown that firms with more independent directors tend to have better financial performance and more professional board committees.<sup>13</sup> Hence, the correlation between the extent to which a firm opts out of exchange requirements and the fraction of the directors of that firm who are independent directors would be an indicator that opt outs are associated with weaker governance. Regressions of this fraction on measures of opt outs reveal this correlation. In such regressions, prior work points out the importance of controlling for the size of the board as well as the size, leverage, and profitability of the firm.

#### 3.3.1 Data

The data for these tests come from a few sources. Information on board independence is from BoardEx, a database containing information on firm leadership and boards for global firms. These data track the individual directors of firms in each year and provide information indicating the extent to which directors also hold management roles. Directors are classified as independent if their role indicates that they are not insiders.<sup>14</sup> The Fraction of Independent Directors is computed for each firm in each year by dividing the number of independent board directors by the total number of board members. Measures of the independence of directors and of board size are merged with the data on cross-listed firms that trade on U.S. exchanges using a name-matching routine. 439 firms are successfully matched.

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<sup>13</sup>See Hermalin and Weisbach (2003) and Adams, Hermalin, and Weisbach (2010) for surveys of this literature. The latter survey emphasizes that causal links from board independence to firm outcomes have not been easy to show.

<sup>14</sup>Specifically, "Independent Director," "Independent NED," "Independent Board Member," and "Independent Outside Director" are mapped to the independent indicator.

Information on the characteristics of firms is drawn from Compustat. The log of assets is used as a measure of firm size. Leverage is the ratio of total debt to the sum of total debt and book equity, and profitability is measured as the ratio of net income to assets, or return on assets. The measures of board size, board independence, and firm characteristics are time varying, and in order to reduce the impact of any unusual values in a particular year, average values of these variables taken using data from 2004 to 2008 are used in the specifications. The specifications also include fixed effects for the country of a firm's headquarters as well as fixed effects for the exchange a firm is listed on.

### **3.3.2 Results**

Analysis of the relationship between opting out and board director independence appears in Table 3.5. The -0.0330 coefficient on Number of Opt Outs in column 1 indicates that the average share of independent directors is 3.3 percentage points lower for each additional exchange requirement that a firm opts out of. This is a roughly seven percent decrease in the average number of independent board members. The specification in the second column includes controls for the size of the board, the log of firm assets, firm leverage, and the firm's return on assets. The coefficient on the number of opt outs remains negative and significant in this specification, and its magnitude is similar. In addition, smaller firms and firms with larger boards tend to have a lower share of independent directors.

Only three of the categories of exchange governance requirements described in Table 3.1 relate directly to the independence of directors, namely those related to board independence requirements, board committee requirements, and audit committee requirements. The specifications in columns 3 and 4 of Table 3.5 provide a test of whether the measures of opting out of these particular requirements identify the extent to which firms have independent directors. In column 3, the coefficients on dummies for firms that opt out of board independence requirements, board committee requirements, and audit committee requirements are each negative and significant. However, the coefficients on the dummies that are equal to one for the other categories of opt outs are each statistically insignificant



**Table 3.5: Independent Directors and Opting Out**

Dependent Variable:	Fraction of Independent Directors			
	(1)	(2)	(3)	(4)
Number of Opt Outs	-0.0330*** (0.0066)	-0.0336*** (0.0063)		
Opt Out of Board Independence Requirements			-0.0562*** (0.0203)	-0.0460** (0.0207)
Opt Out of Board Committee Requirements			-0.0388* (0.0222)	-0.0456** (0.0194)
Opt Out of Audit Committee Requirements			-0.0872*** (0.0263)	-0.0910*** (0.0250)
Opt Out of General Corporate Practices			0.0179 (0.0364)	0.0090 (0.0351)
Opt Out of Shareholder Approval Requirements for Issuance			-0.0166 (0.0160)	-0.0088 (0.0155)
Opt Out of General Good Governance Practices			0.0049 (0.0236)	-0.0048 (0.0222)
Board Size		-0.0114*** (0.0023)		-0.0110*** (0.0023)
Log Assets		0.0146* (0.0073)		0.0163** (0.0070)
Leverage		0.0492* (0.0275)		0.0430 (0.0312)
Return on Assets		0.0743 (0.0484)		0.0790 (0.0537)
Country Fixed Effects?	Yes	Yes	Yes	Yes
Exchange Fixed Effects?	Yes	Yes	Yes	Yes
No. of Obs.	439	438	439	438
R-Squared	0.4831	0.5156	0.4988	0.5305

Notes: The dependent variable is the average fraction of directors who are classified as independent in the BoardEx database. Number of Opt Outs is the number of governance categories the firm opts out of. The other opt out variables are dummy variables that are equal to one for firms that opt out of distinct categories of requirements that appear in Table 3.1. Opt Out of General Corporate Practices is equal to zero for all NYSE-listed firms, because the NYSE does not have any such requirements. Board size is the number of directors on the company's board. Leverage is total debt, defined as the sum of short term and long term debt, divided by total debt plus book equity. Return on Assets is net income divided by total assets. The dependent variable and the time-varying covariates are averages of the corresponding variables taken over the five years from 2004 to 2008. Each specification is an OLS specification that includes country fixed effects as well as fixed effects for the exchange the firm is listed on. Heteroskedasticity-consistent standard errors that correct for clustering at the country level appear in parentheses. \*\*\*, \*\*, and \* denote significance at the 1, 5, and 10 percent levels, respectively.

and small in magnitude.<sup>15</sup> Similar results appear in column 4, which presents a specification that includes additional controls. Thus, the measures of the extent to which firms opt out of exchange governance requirements appear to be meaningfully related to governance practices. Firms that opt out of U.S. exchange governance requirements seem to follow weaker governance practices.

### **3.4 Opting Out and the Costs and Benefits of Complying**

Given that such a large fraction of cross-listed firms opt out of U.S. exchange requirements and that opting out appears to be associated with materially distinctive corporate governance choices, it is natural to ask what motivates firms to opt out. Opting out could reflect some of the same costs and benefits that have been used to explain which firms decide to cross-list in the existing literature. Given the findings in recent work, notably Doidge, Karolyi, Lins, Miller, and Stulz (2009), two types of considerations seem especially salient. The first relates to the private benefits managers enjoy. Evidence suggests that these benefits are larger in countries where corporate governance is weak.<sup>16</sup> Thus, managers of firms based in countries where corporate governance is weak might be reluctant to comply fully with U.S. exchange requirements, while the costs of complying for managers of firms based in countries with strong governance practices might be smaller. Alternatively, the benefits of complying with U.S. exchange governance requirements may be larger for firms whose home country requirements are weaker, implying that firms from such countries would be less likely to opt out. These hypotheses can be tested in country-level analysis of what types of environments are home to firms that opt out of U.S. exchange governance requirements.

The second type of consideration relates to a firm's need for capital. For firms that are growing and have a need for external finance, complying fully with U.S. exchange governance requirements might increase access to capital. When a firm is not bound to

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<sup>15</sup>As the NYSE does not have requirements characterized as General Corporate Practice requirements, the dummy for opt outs of such requirements is set equal to zero for NYSE-listed firms.

<sup>16</sup>See, for example, Dyck and Zingales (2004).

strong corporate governance practices, investors should anticipate potential agency problems and be willing to pay less for an ownership stake. Thus, firms based in countries with weak governance rules that are growing quickly and have a need to raise capital are likely to benefit from committing to stringent governance requirements. Firm-level analysis of the relationship between opting out and measures of growth for firms based in countries with strong and weak governance sheds light on these ideas.

### 3.4.1 Data

In order to conduct country-level analysis of the correlation between the extent to which a cross-listed firm opts out of U.S. exchange requirements and the governance practices in a firm's home country, firms are assigned a home country on the basis of the location of the firm's headquarters.<sup>17</sup> Tests consider two measures of the extent to which the home country legal and regulatory environment permit managers to consume private benefits. The first is a dummy that is equal to one for firms based in civil law countries and zero for firms based in common law countries. These legal origins are drawn from Djankov et al. (2008) and the CIA World Factbook. Common law tends to promote market outcomes by protecting private property and contract rights while civil law is more concerned with market failures and supports state-desired allocations in ways that can attenuate the strength of private contracts. La Porta, Lopez-de-Silanes, and Shleifer (2008) reviews research on legal origins, and La Porta, Lopez-de-Silanes, Shleifer, and Vishny (1998) and Djankov, La Porta, Lopez-de-Silanes, and Shleifer (2003) show that common law countries offer stronger protections to holders of financial claims that are more efficiently enforced.

The second measure of corporate governance in a country is the Anti-Self-Dealing Index created by Djankov, La Porta, Lopez-de-Silanes, and Shleifer (2008). This index measures the legal protection of minority shareholders against expropriation by corporate insiders, and it has been shown to predict a variety of stock market outcomes. The country-level tests

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<sup>17</sup>An alternative choice of country would be the firm's country of incorporation. However, because the headquarter country will likely have more sizeable assets than the incorporating country when these are different, the headquarter country is a better measure when considering potential legal actions.

also include controls for market liquidity and GDP per capita. Stock market turnover is defined as the ratio of the value of total shares traded to the average market capitalization, and it is taken from the World Bank Financial Structure Database. GDP per capita is drawn from the Penn World Tables. Each of the independent variables in the country-level analysis is measured using data from the year 2008.

Data for the firm-level analysis are drawn from Compustat. Measures of net property, plant, and equipment (PP&E) growth, and equity issuance are used as proxies for the extent to which a firm is growing and has a need for external finance. Net PP&E growth is computed as the annual percentage change in net PP&E. Equity issuance is the change in common equity plus the change in deferred tax assets minus the change in retained earnings, scaled by lagged assets, following the approach in Baker, Stein, and Wurgler (2003). The specifications used in the firm-level analysis also include country fixed effects, exchange fixed effects, and several additional controls. Firm size is measured using the log of assets; leverage is measured as the ratio of total debt to the sum of total debt and the book value of equity; and profitability is measured as the return on assets or the ratio of net income to assets. The specifications also control for industry  $q$ , which is calculated by first, for each firm, computing the ratio of the book value of total assets less the book value of equity plus the market value of equity less the book value of deferred taxes to the book value of total assets. Then the median value of this ratio is calculated for each 2-digit SIC code. In order to reduce the impact of any unusual values in a particular year, each of the right hand side variables in the firm-level analysis is measured as an average of annual values covering the 2004 to 2008 period. To reduce the influence of outliers, the net PP&E growth and equity issuance variables are censored at the 1% and 99% level. Summary statistics for these variables appear in Table 3.2.

### **3.4.2 Results**

Table 3.6 presents the results of country-level analysis of the relationship between opting out and the home country characteristics of cross-listed firms. The dependent variable in

the first two columns is the average of the number of governance categories opted out of by firms that are headquartered in a particular country. The coefficient on the Civil Law Dummy in column 1 is 0.8768, and it is positive and statistically significant, indicating that firms based in civil law countries are more likely to opt out of U.S. exchange governance requirements. In column 2, the coefficient on the Anti-Self-Dealing Index is negative and significant, indicating that firms from countries where regulations limit self-dealing are less likely to opt out. Each of the specifications in Table 3.6 includes controls for stock market turnover and the log of GDP per capita, so the results on the impact of governance practices in a firm's home country do not merely reflect market liquidity or wealth.

**Table 3.6: Country Characteristics Associated with Opting Out**

Dependent Variable:	Average Number of Opt Outs (1)	(2)	Average of Opt Out Dummy (3)	(4)	Average Frac. of Opt Outs (5)	(6)
Civil Law Dummy	0.8768** (0.3404)		0.1281* (0.0732)		0.1933*** (0.0631)	
Anti-Self-Dealing Index		-1.3589** (0.6074)		-0.2433** (0.1113)		-0.2691** (0.1199)
Stock Market Turnover	-0.2213 (0.2990)	0.0327 (0.2813)	0.0064 (0.0580)	0.0251 (0.0629)	-0.0374 (0.0541)	0.0041 (0.0550)
Log GDP Per Capita	-0.1703 (0.1841)	-0.3420* (0.1806)	-0.0573 (0.0382)	-0.0591 (0.0409)	-0.0412 (0.0359)	-0.0767** (0.0365)
Constant	3.9504** (1.6970)	6.6969*** (1.7526)	1.3617*** (0.3570)	1.5712*** (0.3706)	0.8209** (0.3370)	1.4057*** (0.3553)
No. of Obs.	43	40	43	40	43	40
R-Squared	0.1783	0.1641	0.1478	0.1275	0.2291	0.1848

Notes: The dependent variable in the first two columns is the number of governance categories the firm opts out of, averaged by country. A firm's home country is measured as the reported location of its headquarters. The dependent variable in columns 3 and 4 is the country average of a dummy equal to one if a firm opts out of any of the governance requirements. The dependent variable in columns 5 and 6 is the country average of the fraction of categories of requirements that a firm opts out of; there are six categories for NASDAQ and AMEX listed firms and five for NYSE listed firms. The Civil Law Dummy is a dummy equal to one for firms with headquarters in a country with a civil law legal origin. The Anti-Self-Dealing Index is drawn from Djankov, La Porta, Lopez-de-Silanes, and Shleifer (2008), and higher values of this variable indicate that a country imposes stronger controls on self dealing. Stock Market Turnover measures the total value of stocks traded as a fraction of average market capitalization. The specifications are OLS specifications, and heteroskedasticity consistent standard errors appear in parentheses. \*\*\*, \*\*, and \* denote significance at the 1, 5, and 10 percent levels, respectively.

The dependent variable in specifications 3 and 4 is the country average of a dummy equal to one for firms that opt out of any U.S. exchange governance requirement. As such, these columns analyze cross-country variation in the share of firms that opt out of any governance requirement. The results in these columns are similar to those in the first two columns. The share of firms from civil law countries and countries with weak regulations limiting self-dealing are more likely to opt out of the governance requirements of U.S. exchanges. The last two columns present results of tests using a third measure of opting out, namely the average fraction of opt outs. This measure addresses the issue that the NYSE does not have requirements characterized as General Corporate Practice requirements by scaling the number of opt outs for each firm by the number of categories of requirements imposed by the exchange the firm is listed on. The results in these columns are similar to those in the previous ones.

Overall, the country-level analysis in Table 3.6 suggests that cross-listed firms are more likely to comply with U.S. exchange governance requirements when they already comply with stringent governance requirements in their home country and are unlikely to be able to consume private benefits. The costs of compliance with U.S. exchange regulations appear to be more likely to dominate the benefits for firms from countries with weak as opposed to strong corporate governance regulations.

The tests presented in Table 3.7 examine whether firms with higher growth and external financing needs adhere to exchange requirements. The dependent variable in each specification is the number of categories of U.S. exchange governance requirements a firm opts out of. The specifications estimate the relationship between the number of opt outs and various firm characteristics. All regressions include fixed effects for the exchange the firm is listed on as well as country fixed effects. For the sample of firms based in common law countries, the coefficient on PP&E growth is negative but it is insignificant in explaining the number of opt outs, as indicated in the first column. However, this coefficient is negative and significant in the second column, implying that when firms are based in countries with weak corporate governance regulations, they opt out of fewer requirements if they

are experiencing higher levels of growth. While there are differences in the significance of the coefficients on PP&E Growth for the sample of firms in common law countries and the sample of firms in civil law countries, an F-test reveals it is not possible to conclude that the coefficient on net PP&E growth in column 1 is statistically different from the coefficient on this variable in column 2.



**Table 3.7: Firm Characteristics Associated With Opting Out**

Dependent Variable: Countries in Sample:	Number of Opt Outs							
	Common Law (1)	Civil Law (2)	Common Law (3)	Civil Law (4)	Common Law (5)	Civil Law (6)	Common Law (7)	Civil Law (8)
Net PP&E Growth	-0.0980 (0.2332)	-0.2882*** (0.0859)	-0.0866 (0.2059)	-0.2252** (0.0988)	0.1543 (0.1814)	-0.9760*** (0.2042)	0.3963 (0.3482)	-0.7247*** (0.2050)
Equity Issuance							0.0553 (0.1268)	0.2036*** (0.0600)
Log Assets			0.0363 (0.1276)	0.2083*** (0.0711)			0.5134 (0.9084)	-0.6444 (0.4898)
Leverage			0.3756 (0.9397)	-0.5853 (0.5129)			0.0244 (1.0777)	-0.9638 (0.8534)
Return on Assets			-0.1387 (0.8739)	-0.5399 (0.7463)			-0.3066 (0.3582)	-0.0936 (0.2348)
Industry q			-0.3165 (0.3319)	-0.1672 (0.2307)			Yes Yes	Yes Yes
Country Fixed Effects?	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Exchange Fixed Effects?	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
No. of Obs.	219	292	219	292	220	278	220	278
R-Squared	0.1662	0.4085	0.1799	0.4484	0.1722	0.4242	0.1918	0.4592

Notes: The dependent variable is the number of governance categories the firm opts out of. Net PP&E Growth is the annual first difference in net property, plant, and equipment scaled by lagged property, plant, and equipment. Equity Issuance is the change in common equity plus the change in deferred tax assets minus the change in retained earnings, scaled by lagged assets. Leverage is total debt, defined as the sum of short term and long term debt, divided by total debt plus book equity. Return on Assets is net income divided by total assets. Industry q is calculated by first, for each firm, computing the ratio of the book value of total assets less the book value of equity plus the market value of equity less the book value of deferred taxes to the book value of total assets, and then taking the median value of this ratio for each 2-digit SIC code. All control variables are averages of firm variables over the five years from 2004 to 2008. Each specification is an OLS specification with country fixed effects, as well as fixed effects for the exchange the firm is listed on. Standard errors clustered at the country level appear in parentheses. \*\*\*, \*\*, and \* denote significance at the 1, 5, and 10 percent levels, respectively.

The specifications in columns 3 and 4 include controls for firm size, firm leverage, firm profitability, and a measure of  $q$  for the firm's industry. Once again, the coefficient on net PP&E growth is negative in both specifications, but it is only significant in column 4, which presents results for the sample of firms based in civil law countries.

The coefficients on Log Assets in these specifications are also noteworthy. These coefficients are positive in both specifications, but only the one in column 4 is significant. Thus, in the sample of firms that are based in civil law countries and cross-list on a U.S. exchange, smaller firms are less likely to opt out of exchange corporate governance requirements. Although coefficients on Industry  $q$  are negative in both specifications—suggesting the firms in industries with better investment opportunities are less likely to opt out—these coefficients are not statistically significant.

Columns 5-8 repeat this analysis but instead of exploring the relationship between net PP&E growth and the number of opt outs for firms based in different kinds of countries, the specifications explore the relationship between equity issuance and the number of opt outs. The results in these columns are similar to those in the first four columns. Firms that engage in more equity issuance and that are based in civil law countries opt out of fewer governance requirements than do other firms. It is noteworthy that the coefficients on Equity Issuance are statistically different from each other across both the two samples of firms in columns 5 and 6 and columns 7 and 8.

Taken together, the results in Tables 3.6 and 3.7 provide evidence that while on average cross-listed firms from countries with weak corporate governance practices are more likely to opt out of U.S. exchange governance requirements, if firms from such countries are small, growing and need external finance, they are more likely to comply. Thus, the costs of complying with U.S. exchange governance rules appear, on average, to outweigh the benefits for cross-listed based in countries with weak governance regulations, but the need to fund growth provides especially strong incentives to comply for such firms.

### 3.5 Opting Out and Valuations

Opting out of governance requirements could affect the market value of firms. If governance requirements limit the ability of corporate insiders to make choices that generate private benefits at the expense of capital providers, valuations of firms that abide by requirements should be higher than those of firms that do not. Empirically analyzing the impact of opting out on broad measures of firm value such as Tobin's  $q$ , however, is challenging. Many determinants of value, including the attractiveness of growth opportunities, are difficult to measure in a cross-country setting. Simple tests of whether Tobin's  $q$  varies with the extent to which firms abide by U.S. exchange corporate governance requirements do not yield significant results.

A more revealing approach to exploring the consequences of opting out on firm value focuses on the value of cash holdings, and it is based on the work of Faulkender and Wang (2006), Dittmar and Mahrt-Smith (2007), and Frésard and Salva (2010). These papers develop a method that uses stock market returns to estimate the impact of changes in cash holdings on changes in firm value for different types of firms. Dittmar and Mahrt-Smith (2007) find that the value of cash is lower in poorly governed firms, and their approach can be used to assess if opting out of U.S. exchange governance requirements appears to reduce the value the market assigns to cash held inside of cross-listed firms from countries with weak governance regulations. The motivation for this hypothesis is that cash reserves can be easily accessed by managers, and managers have considerable discretion in how cash reserves are used. If managers are not constrained by corporate governance rules and regulations, they might have greater latitude to use cash in ways that generate private benefits at the expense of shareholder value. While shareholders of a cross-listed firm from a country with strong governance regulations are protected whether or not the firm opts out of U.S. exchange governance requirements, these U.S. exchange governance requirements might play a more significant role in protecting shareholders of firms from countries with weak governance regulations.

In order to consider how the relationship between changes in cash holdings and changes

in firm value varies across different kinds of cross-listed firms, it is informative to regress the annualized excess stock market returns of a firm on changes in cash holdings, changes in cash holdings interacted with a measure of the extent to which cross-listed firms opt out of U.S. exchange requirements, and a set of controls. Given that the consequences of opting out of U.S. exchange requirements are likely to be larger for firms based in countries with weak corporate governance regulations, it is also informative to separately conduct analysis of the subsample of firms based in common law countries and the subsample of firms based in civil law countries. In these specifications, controls for changes in firms' profitability, financial policy, and investment capture idiosyncratic firm characteristics that may be correlated with both firm cash holdings and returns. A more detailed discussion of this approach appears in Faulkender and Wang (2006) and Dittmar and Mahrt-Smith (2007). Frésard and Salva (2010) use this framework to illustrate that investors place a higher value on the cash held by foreign firms that are cross listed in the U.S.

### **3.5.1 Data**

Following the methodology used in prior work, annualized excess stock market returns are calculated using CRSP data. The returns of the 25 reference portfolios come from Kenneth R. French's website.<sup>18</sup> Excess returns are calculated on a monthly basis and annualized for the regressions. Data used to compute the control variables are drawn from Compustat, and these controls include the change in earnings, the change in net assets, the change in R&D expenditures, the change in interest expenses, the change in dividends, beginning of period cash, the value of total debt, and new finance, which is the sum of new equity issues and new debt issues. Each of these is scaled by the market value of equity. Following Faulkender and Wang (2006), the outcome and control variables are censored at the 1% and 99% level. The data used in analyzing the value of cash holdings cover 2000 through 2011. Each specification includes exchange fixed effects.

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<sup>18</sup>[http://mba.tuck.dartmouth.edu/pages/faculty/ken.french/data\\_library.html](http://mba.tuck.dartmouth.edu/pages/faculty/ken.french/data_library.html).

### 3.5.2 Results

The first column in Panel A of Table 3.8 displays results for the full sample. The -0.0927 coefficient on the interaction of the change in cash holdings and the number of opt outs indicates that opting out of an additional U.S. exchange governance requirement category reduces the value of cash by almost \$0.10, but this coefficient is not statistically significant. The specification that is presented in column 2 includes country fixed effects, and it yields similar estimates.

The next four columns present results for the subsamples of firms based in common and civil law countries. Opting out of U.S. exchange governance requirements should have larger effects if governance practices in a firm's home country are weaker. Consistent with this hypothesis, the coefficient on the interaction of the change in cash holdings and the number of opt outs is negative and significant in columns 5 and 6, but it is insignificant in columns 3 and 4. For firms based in civil law countries, opting out of a requirement reduces the value of each dollar of cash by about \$0.20. Opting out of requirements does not appear to have a statistically significant effect on the value of cash for firms based in common law countries.

**Table 3.8: Opting Out and the Value of Cash**

Countries:	All		Common Law			Civil Law
	(1)	(2)	(3)	(4)	(5)	(6)
<b>Panel A: Annualized Excess Returns</b>						
Change in Cash Holdings/ME	1.6489*** (0.2216)	1.6878*** (0.2274)	1.8365*** (0.3396)	1.8587*** (0.3455)	1.6403*** (0.3087)	1.6502*** (0.3112)
Number of Opt Outs X Change in Cash Holdings/ME	-0.0927 (0.0739)	-0.0945 (0.0736)	0.0543 (0.0947)	0.0515 (0.0939)	-0.1955** (0.0909)	-0.1997** (0.0913)
Number of Opt Outs	0.0058 (0.0062)	0.0056 (0.0073)	0.0091 (0.0092)	0.0145 (0.0092)	0.0007 (0.0086)	-0.0074 (0.0106)
Change in Earnings/ME	0.5006*** (0.0942)	0.4976*** (0.0942)	0.4679*** (0.1331)	0.4601*** (0.1347)	0.4558*** (0.1198)	0.4660*** (0.1164)

*Continued on next page*

**Table 3.8: Opting Out and the Value of Cash (continued)**

Countries:	All		Common Law			Civil Law
	(1)	(2)	(3)	(4)	(5)	(6)
Change in Net Assets/ME	0.1592*** (0.0407)	0.1776*** (0.0404)	0.1914** (0.0786)	0.2135** (0.0826)	0.1532*** (0.0474)	0.1651*** (0.0443)
Change in R&D/ME	0.0857 (0.7674)	0.2540 (0.7733)	0.4435 (0.9091)	0.6163 (0.9051)	0.2410 (1.6119)	0.3232 (1.6542)
Change in Interest Expenses/ME	-2.3311*** (0.6745)	-2.2656*** (0.6663)	-4.2232*** (0.9908)	-4.1335*** (1.0000)	-1.3657* (0.7715)	-1.3077* (0.7481)
Change in Dividends/ME	2.1220*** (0.5176)	2.2446*** (0.5230)	2.7898*** (0.7375)	2.8959*** (0.7541)	1.5429** (0.6849)	1.6995** (0.6981)
Lagged Cash/ME	0.1488*** (0.0522)	0.1691*** (0.0547)	0.2633*** (0.0740)	0.2995*** (0.0774)	0.0352 (0.0553)	0.0506 (0.0587)
Debt/Market Value	-0.1663*** (0.0472)	-0.1579*** (0.0488)	-0.0776 (0.0730)	-0.0722 (0.0774)	-0.2054*** (0.0623)	-0.2092*** (0.0625)
New Finance/ME	-0.1450 (0.0949)	-0.1545 (0.0996)	-0.1095 (0.1446)	-0.1415 (0.1531)	-0.1957 (0.1249)	-0.1745 (0.1295)
Lagged Cash/ME X Change in Cash Holdings/ME	-0.6474*** (0.1187)	-0.6712*** (0.1221)	-1.1488*** (0.3055)	-1.1451*** (0.3123)	-0.4436*** (0.1142)	-0.4458*** (0.1167)
Leverage X Change in Cash Holdings/ME	0.0654 (0.3776)	-0.0021 (0.3797)	0.0407 (0.5776)	0.0662 (0.5748)	0.1040 (0.4854)	0.0119 (0.4865)
Country Fixed Effects?	No	Yes	No	Yes	No	Yes
Exchange Fixed Effects?	Yes	Yes	Yes	Yes	Yes	Yes
Observations	2,370	2,370	1,180	1,180	1,190	1,190
R-squared	0.1603	0.1771	0.1901	0.1977	0.1560	0.1861

Notes: The dependent variable is the annualized excess return of the firm relative to the Fama and French (1993) 25 size and book-to-market portfolios. Cash includes cash and marketable securities. Many variables are scaled by the market value of equity (ME). Number of Opt Outs is the number of governance categories that firms opt out of. Earnings is earnings before extraordinary items plus interest, deferred taxes, and investment tax credits. Net assets is the value of assets net of cash, and R&D is the value of R&D expenses. Interest expenses include total interest and related expenses. Dividends include common dividends paid, and lagged cash is the lagged value of cash. Debt/Market Value is the ratio of the sum of long term and short term debt to the sum of the long term debt, short term debt, and the market value of equity. New Finance is the sum of net equity issues and net debt issues. The specifications are OLS specifications, and the specification in the even numbered columns include country fixed effects. All specifications include fixed effects for the exchange the firm is listed on. Heteroskedasticity-consistent standard errors that correct for clustering at the firm level appear in parentheses. \*\*\*, \*\*, and \* denote significance at the 1, 5, and 10 percent levels, respectively.

**Panel B: Value of Cash Estimates**

*Continued on next page*

**Table 3.8: Opting Out and the Value of Cash (continued)**

Countries:	All		Common Law			Civil Law
	(1)	(2)	(3)	(4)	(5)	(6)
Mean Lagged Cash	0.2950	0.2950	0.2938	0.2938	0.2962	0.2962
Mean Leverage	0.2906	0.2906	0.2336	0.2336	0.3442	0.3442
Mean Number of Opt Outs	2.9195	2.9195	2.2168	2.2168	3.5810	3.5810
Marginal Value of \$1, Average Number of Opt Outs	1.21	1.21	1.61	1.65	0.84	0.81
Marginal Value of \$1, Number of Opt Outs=0	1.48	1.49	1.49	1.54	1.54	1.52
Marginal Value of \$1, Number of Opt Outs=6	0.92	0.92	1.82	1.85	0.37	0.32

Notes: This panel displays mean values of lagged cash, leverage, and the number of opt outs for different samples. It also provides estimates of the marginal value of a dollar for firms based in different legal environments that are implied by the regression results in Panel A.

Panel B of Table 3.8 provides estimates of the marginal value of \$1 in cash that are based on the coefficients in Panel A. For the average firm in the full sample, a dollar inside the firm is worth about \$1.21. This estimate is computed using mean values of leverage, lagged cash, and the number of opt outs for firms based in all countries in the sample. For the full sample, on the basis of specifications with country fixed effects, the implied value of a dollar to firms that do not opt out of any U.S. exchange governance requirements is \$1.49, and the implied value of a dollar to firms that opt out of all six types of U.S. exchange governance requirements is \$0.92. The differences in the sign and magnitude of the coefficients on the interaction of the change in cash holdings and the number of opt outs across firms in common and civil law countries imply large disparities in the effects of opting out on the value of cash. For firms based in common law countries that do not opt out of any U.S. exchange governance requirements, a dollar is worth \$1.54, and it is worth \$1.85 for firms from those countries that opt out of all six types of requirements; the difference between these does not have the expected sign, but it is not statistically significant. However, for firms based in civil law countries that do not opt out of any U.S. exchange governance requirements, a dollar is worth \$1.52, and it is only \$0.32 for firms from those countries

that opt out of all six types of requirements. The difference between these two values is statistically different from zero.

The coefficients on the controls in Table 3.8 are similar to those obtained in prior work. In both Faulkender and Wang (2006) and Dittmar and Mahrt-Smith (2007), increases in earnings, dividends, and assets tend to be associated with larger increases in value. Changes in interest expenses, higher levels of debt, lower levels of lagged cash, and new debt and equity issues tend to be associated with decreases in value. Changes in R&D expenditures are insignificant in explaining changes in value, as they were in Dittmar and Marht-Smith (2007). Although the interactions of the change in cash holdings with lagged cash and with leverage are insignificant in Dittmar and Mahrt-Smith (2007), these interactions also have negative and significant coefficients in Faulkender and Wang (2006).

### **3.6 Conclusion**

Foreign firms' ability to opt out of U.S. exchange governance requirements and follow their home country rules provides a window into central questions in corporate governance. As a result of recently enacted SEC disclosure rules, foreign firms listed on U.S. exchanges now must articulate more clearly the extent to which they comply with exchange requirements. Studying the extent to which cross-listed firms opt out provides insight about the costs and benefits of complying with stringent governance rules. Such investigation also sheds light on the effect of governance requirements on valuation.

Analysis of which firms opt out of U.S. exchange requirements and of the consequences of opting out reveals four main findings. First, opting out is quite common. 80.2% of cross-listed firms opt out of at least one U.S. exchange corporate governance requirement. Although prior literature has noted the governance differences between cross-listings that do and do not trade on a U.S. exchange, there is considerable heterogeneity in the extent to which listed foreign firms comply with the governance requirements of exchanges. Second, firms that opt out appear to adopt weaker governance practices. More specifically, firms that opt out of board requirements have fewer independent directors.



Third, the decision to opt out appears to reflect the relative costs and benefits of this governance choice. The costs of complying are likely to be higher for insiders who might enjoy certain private benefits when following weak governance practices allowed in their home country. The benefits of complying are likely to be higher for firms that are attempting to raise capital and grow. Consistent with this tradeoff, the data show that firms based in countries with weak corporate governance are less likely to comply and those that are based in such countries and are expanding and issuing equity are more likely to comply. Finally, the results indicate that opting out of U.S. exchange requirements has consequences for how the market values cash holdings. For firms from countries with weak governance requirements, cash within the firm is worth significantly less if the firm opts out of more U.S. exchange requirements, consistent with the theory that the market views these firms as more weakly governed and vulnerable to expropriation.

While making a causal statement is difficult due to endogeneity concerns, the evidence indicates that compliance facilitates the ability of firms to raise external finance to pursue growth. Foreign firms bond themselves to the more stringent corporate governance requirements of U.S. exchanges in a manner that has meaningful effects on their access to capital and their market valuation. However, the high share of foreign firms that opt out, especially foreign firms from countries with weak governance regulations, suggests that the costs of complying with strict governance requirements are too high for many insiders. This implies a limit to the extent cross-listed firms can effectively borrow the U.S. governance environment. Strong institutions abroad do not appear to easily substitute for the institutional environment in a firm's home country.

These results suggest refinements to the legal bonding hypothesis used to explain cross-listing. While the data indicate that few cross-listed firms completely bond themselves to U.S. law and exchange rules, the cost-benefit framework proposed in the existing literature explains the extent of compliance among firms that do cross-list. An important question for future research is why firms choose to cross-list in the U.S. but opt out of its exchanges' rules.

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# Appendix A

## Appendix to Chapter 1

### A.1 Data Dictionary

#### A.1.1 Judge Leniency

*Judge Leniency* - We calculate judge leniency as the leave-one-out mean rate of granting Chapter 13 bankruptcy protection for the assigned judge minus the leave-one-out mean rate of granting bankruptcy protection for the office.

#### A.1.2 Characteristics

*Homeowner* - Homeownership is based on a home flag calculated by TransUnion. The home flag is set to “Y” if there is any home equity or mortgage trade on file. This measure may overestimate actual homeownership because it does not require a non-zero balance on home equity or mortgage trades. Alternatively, this measure may underestimate actual homeownership if TransUnion does not observe the original mortgage or equity trade.

#### A.1.3 Adverse Financial Events

*Delinquency* - We measure post-filing delinquencies based on the number of trades currently 30+ days past due within the past 12 months, provided by TransUnion.

Delinquency probabilities are non-cumulative, measured as the probability of at least one delinquency in the prior 12 months, averaged over the first five post-filing years.

*Collection* - We measure post-filing collections based on the number of collection trades in the past 12 months, calculated by TransUnion. Collection account records consist of credit accounts and records of unpaid bills that have been transferred to a collection agency or in the process of collection. Generally, accounts sent to collection are listed on a debtor's credit report for seven years. Collection trades are trades either with KOB (Kind of Business) = Collection, MOP (Manner of Payment) = 9B (Collection), or remark/dispute flags such as "Collection account cancelled by creditor," "Placed for collection," and "Collection account." Collection probabilities are non-cumulative, measured as the probability of at least one collection in the prior 12 months, averaged over the first five post-filing years.

*Charge-off* - We measure post-filing charge-offs based on the number of charge-offs within the past 12 months, calculated by TransUnion. A charge-off occurs when a creditor declares a debt unlikely to be paid. An account is usually charged off after 180 days of non-payment, but the creditor can continue to attempt to collect on the debt. The charge-off record generally appears on a credit report for up to seven years. Charge-off information is obtained from trades with remark/dispute codes such as "Bad Debt: Collection Suit," "Claim/PMT Against Guarantor," "Early Termination w/Deficiency," "Skip out of Account," or MOP = 09 (Charged off to bad debt), or MOP = 9P (Paying or paid account with MOP 09). Charge-off probabilities are non-cumulative, and can be thought of as the probability of at least one charge-off in the prior 12 months, averaged over the second to fifth post-filing years.

*Bankruptcy* - We measure post-filing bankruptcies based on the number of bankruptcies within the past 12 months, calculated by TransUnion. Bankruptcies can occur under Chapter 7, Chapter 11, Chapter 12, or Chapter 13. Bankruptcy probabilities are non-cumulative, measured as the probability of at least one bankruptcy in the prior 12

months, averaged over the second to fifth post-filing years.

*Foreclosure* - We measure post-filing foreclosures based on the number of foreclosures within the past 12 months, calculated by TransUnion. A foreclosure is a process in which a bank or mortgage company takes possession of a mortgaged property because the mortgagor has failed to keep up with mortgage payments. Foreclosure information is obtained from public records, and trades with remark/dispute codes that signal foreclosure. In the TransUnion data, foreclosure is defined more expansively than an actual sale or deed transfer. Foreclosure ranges from an actual sale or transfer of the home, to merely a notice that foreclosure was commenced. For instance, the foreclosure flag is turned on for any of the following reasons: foreclosure initiated, foreclosure started, foreclosure discontinued, and foreclosure redeemed. Post-filing foreclosure probabilities are non-cumulative, and can be thought of as the probability of at least one foreclosure in the prior 12 months, averaged over the first five post-filing years.

*Judgment* - We measure post-filing judgments based on the number of civil judgment suits within the past 12 months, calculated by TransUnion. Judgment probabilities are non-cumulative, measured as the probability of at least one judgment in the prior 12 months, averaged over the first five post-filing years.

*Lien* - We measure post-filing liens based on the number of lien public records within the past 12 months, calculated by TransUnion. A lien is an official claim against property or funds for payment of a debt owed. Public record liens include federal and state tax liens, hospital liens, and judicial liens. Lien probabilities are non-cumulative, measured as the probability of at least one lien in the prior 12 months, averaged over the first five post-filing years.

*Repossession* - We measure post-filing repossessions based on the number of repossessions within the past 12 months, calculated by TransUnion. A repossession occurs when a lender takes back an asset, such as an automobile. Repossessions can be

voluntary or involuntary. Late payments leading up to repossession are damaging to a debtor's credit score, and the mark of a repossession appears on credit reports. In the TransUnion data, repossession information is obtained from trades with remark/dispute codes such as "Paid Repossession," "Repossession," "Repossession, redeemed," "Paid by dealer," "Paid from collateral," or MOP (Manner of Payment) = 08 (Repossession). As with foreclosure, TransUnion defines repossessions expansively, including redeemed repossessions where the debtor makes full payment on the loan and takes back the asset. Post-filing repossession probabilities are non-cumulative, and can be thought of as the probability of at least one repossession in the prior 12 months, averaged over the first five post-filing years.

*Financial Strain Index* - The index contains the non-cumulative probabilities of the following eight components: delinquency, collection, charge-off, bankruptcy, foreclosure, judgment, lien and repossession, as defined above. Following Fryer and Katz (2013), for each post-filing year, each component is standardized using the mean and standard deviation for the dismissed filer group in the baseline year. We sum across the eight components to create a yearly index, restandardizing using the mean and standard deviation of the dismissed filer group in the baseline year. The index in the year of filing includes six components, excluding charge-offs and bankruptcies. We then average the yearly index across the first five post-filing years. Because each of the financial strains represent adverse events that negatively impact access to credit, a higher index represents worse outcomes.

#### **A.1.4 Unsecured Debt and Collections Activity**

*Revolving Balance* - Total balance of revolving trades with current balance greater than zero verified within 6 months calculated by TransUnion. Revolving trades include bank card accounts, retail accounts, and check credit accounts. Retail trade accounts include clothing, department stores, grocery, home furnishings, jewelry, computer, camera, and sporting goods stores. According to Avery et al. (2003), revolving trade

balances (dollar-weighted) represent 11 percent of all open account balances.

*Collection Balance* - Aggregate current balance of all collections on file calculated by TransUnion. There are two important shortcomings of the collections data. First, there is incomplete coverage of unpaid bills, with larger entities, such as hospitals and utility companies, more likely to send debts to collection agencies. Second, collection records will not include debts that parties collect themselves and debts sent to collection agencies that do not report to credit bureaus.

#### **A.1.5 Retaining Secured Assets**

*Have a Mortgage* - We measure the probability of having an open mortgage based on the number of open mortgage trades verified in the past 12 months calculated by TransUnion. Mortgage trades are loans such as conventional real estate mortgages, FHA loans, real estate loans, second mortgages, and VA loans.

*Mortgage Balance* - Total balance of all mortgage trades verified in the past 12 months calculated by TransUnion. According to Avery et al. (2003), mortgage balances (dollar-weighted) represent 67 percent of all open account balances.

*Have an Auto Loan* - We measure the probability of having an open auto loan based on the number of open auto loans verified in the past six months calculated by TransUnion. Auto loans typically involve fixed monthly payments that fully amortize the total amount borrowed over the term of the loan, often secured (Avery et al. 2003).

*Auto Balance* - Total balance of open auto trades verified in the past 12 months calculated by TransUnion.

#### **A.1.6 Credit Access**

*Revolving Utilization* - Total outstanding revolving trade balance divided by revolving trade credit limit verified in the past 12 months calculated by TransUnion, expressed in

percentages. Because total credit limit is likely understates actual credit limits (Avery et al. 2003), the credit utilization rate likely overstates actual credit utilization.

*Non-Mortgage Inquiries* - Number of non-mortgage inquiries within the past 6 months calculated by TransUnion. Inquiries are made to ensure that an applicant for credit, apartment rental, insurance, or employment meets minimum standards. When a creditor or lender checks a debtor's credit in connection with an application, a "hard inquiry" is tagged on a credit report. A hard inquiry remains on a credit report for up to two years and may lower a debtor's credit score. When a creditor reviews the credit report of an existing customer, or when a debtor checks his own credit, a "soft inquiry" typically shows up on your credit report. Soft inquiries generally do not lower credit scores or appear to businesses checking a debtor's credit.

#### **A.1.7 Credit Score**

*Credit score* - This measure is an ordinal credit score calculated by TransUnion to measure credit risk. This measure is similar to the FICO score commonly referenced in the consumer finance literature.

#### **A.1.8 Data Characteristics**

*Matched to Credit Report* - Indicator for whether the 253,863 bankruptcy filings sent to TransUnion were matched to credit report data from the baseline filing year.

*Missing Age* - Indicator for whether age at filing is missing.

*Missing Baseline Outcomes* - Indicator for whether baseline credit report outcomes are missing.

#### **A.1.9 Housing Transitions**

*Living in Same Residence* - This measure is calculated based on the number of months at the current address calculated by TransUnion. We define a consumer as being in the

same residence five years after filing if the difference between the number of months at the current address in year 5 and year 0 is at least 48 months.

*Moved to Rental* - We define this measure as individuals who have zero mortgage trades in year 5, coupled with a move between years 0 and 5 (such that they are no longer in the same residence by year 5).

*Moved to Home* - We define this measure as individuals who have non-zero mortgage trades in year 5, coupled with a move between years 0 and 5 (such that they are no longer in the same residence by year 5).



## **Appendix B**

# **Appendix to Chapter 2**

### **B.1 First Stage Results**

**Table B.1: First Stage Results**

	Non-Recourse Instrument	Homestead Instrument	F-Test
	(1)	(2)	(3)
<u>Individual Regressions:</u>			
Non-Recourse Protected	2.177*** (0.117)	0.015 (0.011)	177.181 0.000
Homestead Protected	-0.112 (0.253)	1.272*** (0.042)	597.001 0.000
<u>County Regressions:</u>			
Non-Recourse Protected	2.302*** (0.120)	0.008 (0.011)	376.55 0.000
Homestead Protected	-0.213 (0.381)	1.428*** (0.052)	882.34 0.000
<u>State Regressions:</u>			
Non-Recourse Protected	2.339*** (0.126)	0.007 (0.011)	346.12 0.000
Homestead Protected	-0.161 (0.362)	1.417*** (0.043)	1203.96 0.000
<u>Zip Code Regressions:</u>			
Non-Recourse Protected	2.354*** (0.133)	0.014 (0.014)	316.44 0.000
Homestead Protected	-0.176 (0.363)	1.414*** (0.052)	856.14 0.000

Notes: This table reports representative first stage results for the two-stage least squares results reported in Tables 2-5. Individual Regressions correspond to the individual-level balance sheet results reported in Table 2. County Regressions correspond to the county-level employment results reported in Table 3. State Regressions correspond to the state-level consumption results reported in Table 4. Zip Code Regressions correspond to the zip code-level house price results reported in Table 5. See the notes for Tables 2-5 for additional details. \*\*\* = significant at 1 percent level, \*\* = significant at 5 percent level, \* = significant at 10 percent level.

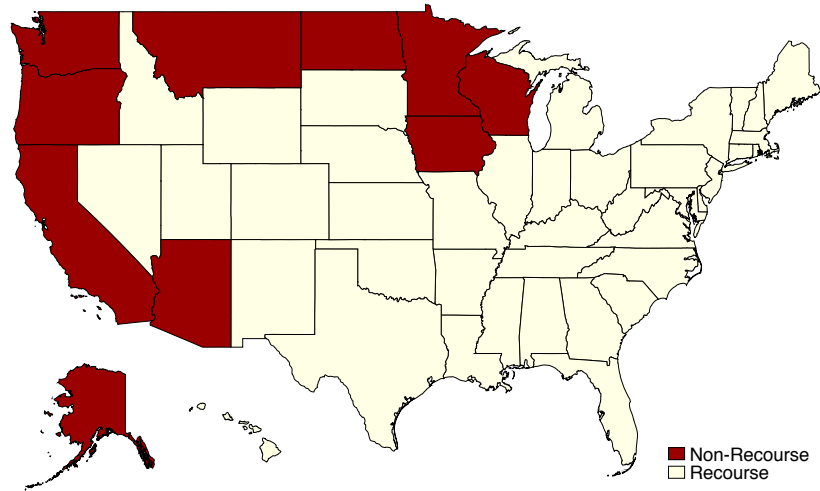
## B.2 Debtor Protection Laws by State

### B.3 Protections Laws

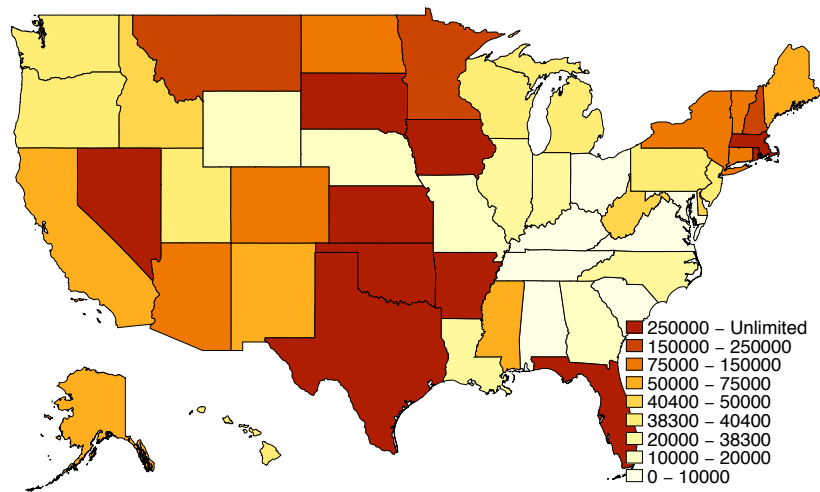
**Table B.2:** *State Non-Recourse and Bankruptcy Exemption Laws*

State	Non-Recourse	Homestead Exemption	State	Non-Recourse	Homestead Exemption
AK	Non-Recourse	67,500	MT	Non-Recourse	200,000
AL	Recourse	10,000	NC	Recourse	37,000
AR	Recourse	Unlimited	ND	Non-Recourse	80,000
AZ	Non-Recourse	150,000	NE	Recourse	12,500
CA	Non-Recourse	75,000	NH	Recourse	200,000
CO	Recourse	90,000	NJ	Recourse	40,400
CT	Recourse	150,000	NM	Recourse	60,000
DE	Recourse	50,000	NV	Recourse	350,000
FL	Recourse	Unlimited	NY	Recourse	100,000
GA	Recourse	20,000	OH	Recourse	10,000
HI	Recourse	40,400	OK	Recourse	Unlimited
IA	Non-Recourse	Unlimited	OR	Non-Recourse	39,600
ID	Recourse	50,000	PA	Recourse	40,400
IL	Recourse	30,000	RI	Recourse	300,000
IN	Recourse	30,000	SC	Recourse	10,000
KS	Recourse	Unlimited	SD	Recourse	Unlimited
KY	Recourse	10,000	TN	Recourse	7,500
LA	Recourse	25,000	TX	Recourse	Unlimited
MA	Recourse	500,000	UT	Recourse	40,000
MD	Recourse	0	VA	Recourse	10,000
ME	Recourse	70,000	VT	Recourse	150,000
MI	Recourse	40,400	WA	Non-Recourse	40,400
MN	Non-Recourse	200,000	WI	Non-Recourse	40,400
MO	Recourse	15,000	WV	Recourse	50,000
MS	Recourse	75,000	WY	Recourse	20,000

Notes: This table lists the state laws for non-recourse mortgages and bankruptcy home equity exemptions. Homestead exemption amount is as of 2007. If Federal bankruptcy exemptions are allowed, the greater of the state and federal exemption amount is used. Source for Recourse Laws: <http://www.alllaw.com/articles/nolo/foreclosure/anti-deficiency-laws.html>, and Ghent and Kudylak (2011). Source for Bankruptcy Exemption Laws: hand-collected by the authors from NOLO and state legal documents.



(a) Non-Recourse Protections



(b) Bankruptcy Homestead Exemptions

**Figure B.1: Distribution of Protections by State**

Notes: These figures display 2007 non-recourse laws and 2007 bankruptcy homestead exemptions by state.