

The Effect of Principal Reduction on Household Distress: Evidence from Mortgage Cramdown*

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Abstract

Mortgage cramdown enabled bankruptcy judges to discharge the underwater portion of a mortgage during Chapter 13 bankruptcy until the Supreme Court disallowed this practice in 1993. We investigate the impact of mortgage cramdown on household distress exploiting the random assignment of cases to judges. We find that a successful bankruptcy filing reduced the three-year foreclosure rate by 27 percentage points in courts that allowed cramdown. These bankruptcy benefits were reduced by more than half after the Supreme Court disallowed cramdown. Our results suggest that large principal reductions considerably reduce homeowners' distress.

Keywords: Debt Relief, Principal Reduction, Debt Overhang, Foreclosures, Consumer Bankruptcy, Housing Policies

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1 Introduction

The United States experienced an unprecedented number of home foreclosures during the Great Recession of 2007–2009. To limit defaults and deadweight losses, the government implemented various policies that reduced monthly payments (i.e., the Home Affordable Modification Program (HAMP)) and facilitated mortgage refinancing (i.e., the Home Affordable Refinancing Program (HARP)). However, various frictions in intermediation brought about only modest success for these initiatives.¹ An alternative policy proposal that was *not* implemented during the Great Recession would have allowed mortgage cramdown by judges as part of the Chapter 13 bankruptcy process. In these restructurings, the underwater portion of the mortgage is treated as unsecured debt and can be discharged during bankruptcy. Our paper investigates this alternative policy by estimating the effects of bankruptcy discharge on borrowers who benefit from mortgage cramdown.

Chapter 13 bankruptcy allows debtors to restructure almost all types of debt, except for loans on the principal place of residence.² However, some bankruptcy courts had been discharging the underwater portion of the mortgage during bankruptcy before the Supreme Court disallowed this practice in 1993. Borrowers who obtain mortgage debt relief due to cramdown have to continue making their monthly mortgage payments following the original terms of the loans. Although cramdown does not affect the short-term liquidity constraints of households, it is beneficial for underwater borrowers because it reduces the debt owed on the property. Mortgage cramdown is thus similar to reducing the maturity of the mortgage rather than reducing the monthly payments. Cramdown thereby diminishes the incentives of homeowners to default on their properties and decreases the likelihood of households being forced to move from their homes.³

¹See, for example, [Agarwal et al. \(2017\)](#) and [Agarwal et al. \(2022\)](#) for a discussion of the effectiveness of HAMP and HARP.

²Other debts that cannot be discharged in bankruptcy are domestic support obligations, criminal penalties, penalties owed to government agencies, taxes, student loans, and fraudulent debts.

³[Ganong and Noel \(2020a\)](#), [Ganong and Noel \(2020b\)](#), and [Indarte \(2021\)](#) discuss whether liquidity considerations or debt overhang lead to default and bankruptcy.

In this paper, we estimate the effect of mortgage cramdown on foreclosures and other economic outcomes. To this end, we estimate the causal effect of the bankruptcy discharge using the judge’s leniency as an instrumental variable for bankruptcy protection, similar to [Dobbie and Song \(2015\)](#) and [Dobbie, Goldsmith-Pinkham, and Yang \(2017\)](#).⁴ As indicated by [Sullivan, Warren, and Westbrook \(1994\)](#), [Norberg and Compo \(2007\)](#), and [Chang and Schoar \(2013\)](#), the variation in leniency across judges is substantial. The identifying assumptions are that judge assignments are associated with bankruptcy protection and that these assignments only impact debtor outcomes through the probabilities of receiving bankruptcy protection.

In addition, we also take advantage of the Supreme Court decision that disallowed cramdown in June 1993. Households who filed before the decision received relief from both their unsecured debt and the underwater portion of their secured debt, whereas those who filed after the decision only had their unsecured debt discharged. Thus, we expect that the change in the discharge effect is due to the lack of mortgage principal reduction. This approach allows us to compare filers who had their bankruptcy case approved to those that had them dismissed within the same office and month. The prediction is that if principal reduction is an important driver of the benefits of receiving bankruptcy protection, the ex-post effects of Chapter 13 should be reduced considerably after the Supreme Court decision in those districts that allowed cramdown.⁵

To estimate the impact of cramdown in Chapter 13 bankruptcy on debtors’ home foreclosures and other economic outcomes, we compile a new data set. First, we collect information from the Public Access to Court Electronic Records (PACER) system, and we complement our data with bankruptcy information from [Gross, Notowidigdo, and Wang \(2014\)](#). Second, we match this data set with two other data sources to study debtors’ mortgage defaults and other post-filing outcomes. Our sample contains bankruptcy records from 36,655 cases. To

⁴A similar empirical strategy has been used to estimate the ex-post impact of the sentence length on earnings ([Kling, 2006](#)), foster care effects ([Doyle, 2007](#)), juvenile incarceration ([Aizer and Doyle, 2015](#)), and corporate bankruptcy ([Chang and Schoar, 2013](#)).

⁵Other papers have used law changes to identify effects of bankruptcy rules, such as the 2005 BAPCA (e.g., [Mahoney, 2015](#); [Gross et al., 2021](#); [Müller, 2022](#)) and the 1993 Supreme Court ruling on *Nobelman v. American Savings Bank* (e.g., [Goodman and Levitin, 2014](#)).

obtain information about the courts that allowed cramdown, we merge our data set with data collected by [Goodman and Levitin \(2014\)](#).

Our main result shows that a successful Chapter 13 bankruptcy filing in a cramdown court (i.e., a bankruptcy discharge) has a large impact on future mortgage distress. We find that a discharge in a cramdown court reduces the three-year foreclosure rate by 27 percentage points before the Supreme Court decision of 1993. The impact of the bankruptcy discharge on foreclosures is reversed by more than half to 10 percentage points after the Supreme Court decision. The benefit of bankruptcy discharge on foreclosures in cramdown courts is even more pronounced if we focus on a narrower window around the Supreme Court decision. We obtain similar results using involuntary sales (i.e., property sales for less than the mortgage amount) instead of foreclosures.

In addition, we find that prior to the Supreme Court decision, the benefits are substantially larger for filers in cramdown districts that exhibited below-median house price appreciation (i.e., areas with an average cumulative depreciation of -10.14% over the prior five years). The benefits drop substantially in these low-appreciation districts following the Supreme Court decision. On the other hand, there is no significant change in foreclosures after the Supreme Court decision for districts with above-median house price appreciation (i.e., areas with an average cumulative five-year appreciation of 16.55%). These results confirm that the cramdown benefit is closely linked to the underwater portion of home mortgages.

Although we find beneficial effects of discharge in cramdown courts across different demographic groups, the benefits tend to be more pronounced for female filers. For example, involuntary sales decline by 42 percentage points for females and by only 21 percentage points for male filers after a discharge in cramdown courts before the Supreme Court decision. These economically large differences may occur because judges require a higher threshold for female filers to qualify for Chapter 13 debt discharge. Indeed, the discharge rate for female filers is 4 percentage points lower than for male filers after controlling for other filer characteristics and court fixed effects. The differential treatment of female filers by judges may be due to

systematic patterns of “in-group” tolerance, as discussed by Egan, Matvos, and Seru (2022) in the context of misconduct in the financial advisory industry. Furthermore, female filers may also be more reluctant to sell their home and move to a new location, as they are more likely the main caregivers of dependent children.

Discharged bankruptcy filers are typically unable to refinance their mortgage debt during the first years after a bankruptcy filing since it requires approval from the bankruptcy judge and since the bankruptcy flag on their credit record makes it difficult to find a new mortgage lender. Consistent with this hypothesis, we find no effects on mortgage refinancing over the first three years after the bankruptcy filing. However, we find that successful bankruptcy filers in cramdown courts significantly increase their propensity to refinance their mortgages after five or more years. These benefits of bankruptcy discharge on mortgage financing in cramdown courts are again significantly reduced after the Supreme Court decision of 1993.

Finally, we combine our samples from cramdown and non-cramdown courts and study whether the effects differ across the two groups. We find that the three-year foreclosure rate prior to the Supreme Court decision declines by 27 percentage points in cramdown courts and by only 3 percentage points in non-cramdown courts. However, after the Supreme Court decision, the foreclosure rates decrease by only 10 percentage points in cramdown and by 2 percentage points in non-cramdown courts. Thus, our results indicate that the cramdown provision is the main driver of the beneficial effects of bankruptcy discharge in cramdown courts. Therefore, given the substantial foreclosure costs, cramdown has significant benefits for homeowners (Diamond, Gureny, and Tan, 2020).

We also provide a partial-equilibrium back-of-the-envelope calculation of how many foreclosures could have been avoided had the United States allowed mortgage cramdown during the 2008–2013 period. Our estimates indicate that around half a million foreclosures could have been avoided with mortgage cramdown.⁶ However, these benefits can potentially be

⁶Luzzetti and Neumuller (2014); Agarwal et al. (2017); Maturana (2017); Piskorski and Seru (2018); Ganong and Noel (2020a); Kaplan, Mitman, and Violante (2020); Abel and Fuster (2021), Gross et al. (2021); Piskorski and Seru (2021); and Agarwal et al. (2022) also study the impact of alternative policies on the financial crisis.

partially offset by higher losses imposed on mortgage lenders and by higher costs imposed on borrowers who do not declare bankruptcy.

Our paper builds on an important literature in household finance that studies the impact of bankruptcy on foreclosures and other economic outcomes. Our paper is most related to [Dobbie and Song \(2015\)](#), who use random judge assignments to study the impact of unsecured debt relief from Chapter 13 protection on subsequent earnings and foreclosures over the period from 1992 to 2005. They document significant economic benefits for households with successful bankruptcy filings. Furthermore, [Dobbie, Goldsmith-Pinkham, and Yang \(2017\)](#) also study the effect of Chapter 13 bankruptcy protection on financial health. Our paper studies the benefits of principal reductions on secured debt over the period from 1989 to 1995. We are the first to provide estimates of the ex-post impact of mortgage cramdown from Chapter 13 on mortgage default and other outcomes using a newly compiled data set. Whereas [Dobbie and Song \(2015\)](#) estimate a large effect of unsecured debt relief through Chapter 13 on avoiding foreclosure, we show that the effect of Chapter 13 in cramdown courts is substantially larger relative to the standard Chapter 13 protection.

By estimating the ex-post effect of principal reduction, our paper also contributes to the literature on cramdown. [White and Zhu \(2010\)](#) examine how filing for bankruptcy helps financially distressed homeowners and present a model that evaluates the effects of introducing a cramdown provision after the 2005 bankruptcy reform. [Goodman and Levitin \(2014\)](#) analyze whether cramdown affects the cost of credit in the mortgage market, whereas [Li, Tewari, and White \(2014\)](#) estimate the effect of cramdown on the supply of mortgages. Interestingly, both of these papers find small effects on the supply side. [Goodman and Levitin \(2014\)](#) find that home loans closed during the time when cramdown was allowed had interest rates 12-16 basis points higher than loans closed in the same state when cramdown was not allowed, which translates to a roughly 1% increase in the monthly payments. Similarly, [Li, Tewari, and White \(2014\)](#) find that the Supreme Court decision led to a short-term reduction of 3% in mortgage interest rates and a short-term increase of 1% in mortgage approval rates,

but only the approval rate effect persists over longer sample periods.

A growing literature studies the mechanisms that explain household default and the effect of various debt-relief interventions. Regarding the drivers of default, there is a longstanding debate on whether mortgage default is triggered by adverse life events (Riddiough, 1991), negative equity (Foster and Van Order, 1984), or both (Foote, Gerardi, and Willen, 2008). Concerning interventions, Ganong and Noel (2020a) document that maturity extensions that increase liquidity have substantial benefits on debtors, while (small) principal reductions have no effect. Dobbie and Song (2020) find that interest write-downs provide positive benefits, whereas immediate payment reductions do not help debtors. Ganong and Noel (2020b) find that negative equity exclusively causes a small fraction of defaults. Using a novel research design, Indarte (2021) finds that bankruptcy filings are five times more responsive to cash-on-hand than relief generosity.⁷ We contribute to this literature by estimating the effect of large principal reductions on mortgage loans and documenting substantial effects, even in the short term, on debtors for this type of intervention. In addition, relative to other papers that study unsecured debt-relief interventions, our setting allows us to examine the mortgage debt overhang motive for defaulting.⁸ Our results do not reject the importance of liquidity constraints as a potential determinant for homeowner default. However, we provide evidence that debt overhang considerations play a role in explaining homeowner default.

The remainder of our paper is structured as follows: We discuss the institutional background in Section 2. Section 3 describes the data sources and summary statistics. Section 4 presents the research design. Section 5 presents our main results, and Section 6 compares outcomes of bankruptcy filings in cramdown and non-cramdown courts. Section 7 discusses the mechanisms that drive our results, the tradeoffs between the costs and benefits of cramdown, and the impact of the cramdown during the Great Recession. Section 8 concludes.

⁷Gross, Notowidigdo, and Wang (2014) show that tax rebates increase short-run bankruptcies.

⁸Elul et al. (2010); Guiso, Sapienza, and Zingales (2013); Mayer, Morrison, Piskorski, and Gupta (2014); Bhutta, Dokko, and Shan (2017); Gerardi, Herkenhoff, Ohanian, and Willen (2018); Abel and Fuster (2021); and Gupta and Hansman (2022) also study the determinants of mortgage default and evaluate debt-relief interventions.

2 Institutional Background

The United States has two personal bankruptcy provisions: Chapter 7 and Chapter 13. Chapter 7 provides unsecured debt relief and protection from wage garnishment in exchange for a debtor's non-exempt assets. In contrast, Chapter 13 bankruptcy filers must forgo part of their future earnings to repay creditors. Therefore, debtors seeking to retain their assets are more likely to file for Chapter 13. Overall, both bankruptcy provisions allow total or partial forgiveness of unsecured obligations, such as credit card debt, unpaid rent, and medical bills.

Based on the 1978 Bankruptcy code, which was amended in 1984, Chapter 13 debtors are required to use all their disposable income to propose a three-year repayment plan (White, 1987).⁹ The main restriction is that the total proposed repayment cannot be less than the value of a debtor's non-exempt assets under Chapter 7. Filers need to submit a bankruptcy petition, a repayment plan, a summary of financial affairs, a copy of tax returns, and the standardized forms of income, expenditures, and assets and liabilities. Subsequently, within each bankruptcy office, filers are typically assigned to judges using a blind rotation system. The debtor meets with a bankruptcy trustee who reviews the claims by creditors and debtors, challenges any aspects of the bankruptcy case if needed, and collects the bankruptcy proceeds during the repayment plan. Finally, the filer appears before the bankruptcy judge at the plan confirmation hearing. This is the only time when the judge and the filer meet. The main role of the assigned judge is to decide all matters connected to a case, in particular, whether to dismiss the filing.

Chapter 13 allows debtors to restructure almost all types of debt, except home mortgage loans. In particular, Section 1322(b)(2) of the Bankruptcy Code provides that a Chapter 13 repayment plan may “modify the rights of holders of secured claims, *other than* a claim secured only by a security interest in real property that is the debtor's principal residence” (11 U.S.C. §1322(b)(2)). Therefore, any mortgage arrears, late fees, and regular installments should be paid off according to their original terms; otherwise, the mortgagee has the right

⁹After the 2005 Bankruptcy Reform, the repayment plans last between three and five years.

to foreclose on the property (Goodman and Levitin, 2014). The rationale behind restricting mortgage restructuring for a principal residence is to encourage the flow of capital into the home lending market (Levitin, 2009).

Although mortgage modification is typically prohibited in Chapter 13 bankruptcy, some bankruptcy courts allowed principal mortgage reduction, a practice known as mortgage cramdown, between 1981 and 1993. These decisions were not built on policy analysis; instead, they were based on statutory interpretations (Levitin, 2009). In particular, these courts understood cramdown as a simple reclassification of the loan in bankruptcy. The main argument was that the prohibitive clause (the “other than”) of §1322(b)(2) was limited to the value of the collateral, whereas any negative equity could be classified as unsecured debt and, thus, was dischargeable through bankruptcy. Thus, the mortgage loans were “bifurcated” into a secured component, corresponding to the value of the property, and an unsecured component, corresponding to the underwater portion of the loan, that could be discharged.¹⁰

Mullaugh (1994) states that courts that approved cramdown under Chapter 13 plans based their decisions on four factors. First and most importantly, the courts felt that the language led to the conclusion that bifurcation was permissible. Second, the rules of statutory construction also led to the same interpretation as the plain language. Third, at the time, there were Congressional initiatives to balance the interests of lenders and debtors. Lastly, allowing the debtor to “strip down” an undersecured mortgage was in line with the bankruptcy goal of a “fresh start” for filers.

In practice, a Chapter 13 filer had to propose a repayment plan with the remaining secured claim. Under this plan, the filer had to pay the arrearages in full within a reasonable time and continue to make the monthly payments following the original terms of the loan until the principal had been paid in an amount equal to the value of the property. In courts that allowed cramdown, any negative equity portion of the mortgage was automatically forgiven

¹⁰11 U.S.C. §506(a) reads, in pertinent part: “An allowed claim of a creditor secured by a lien on property in which the estate has an interest [...] is a secured claim to the extent of the value of such creditor’s interest [...] and is an unsecured claim to the extent that the value of such creditor’s interest [...] is less than the amount of such allowed claim.”

if the Chapter 13 filing was discharged.¹¹

On June 1st 1993, the Supreme Court prohibited cramdown after ruling on the issue in *Nobelman v. American Savings Bank* (508 U.S. 324 [1993]), a decision that banned cramdown in all states. The Supreme Court’s rationale was to provide special protection to home mortgage lenders to enable them to offer lower interest rates and thus promote homeownership.¹²

Prior to the Supreme Court decision on *Nobelman v. American Savings Bank* in 1993, four circuit courts allowed cramdown (i.e., the 2nd, 3rd, 9th, and 10th Circuit Courts).¹³ However, the 5th Circuit Court did not approve the practice after hearing the *Nobelman* case in August 1992. Prior to *Nobelman*, no circuit court had explicitly barred cramdown in Chapter 13, and several bankruptcy courts in other circuits had approved Chapter 13 reorganization plans that included principal write-down (Mullaugh, 1994).

It is important to note that a judge’s decision in a bankruptcy case can change the law. Although any decision affects only the district (i.e., all the courts within a district) in which the judge is presiding, a fact that generates differences in law across districts within a circuit court (Li, Tewari, and White, 2014). However, law differences within a circuit are resolved by the circuit court deciding an appeal from a district decision, leading to a law change in the entire circuit. In particular, half of the courts in our final sample allowed cramdown after a circuit ruling. The most important ruling was by the 9th Circuit Court ruling in October 1989, which affected most of them. Finally, between 1981 and 1993, 38 courts in 29 states allowed mortgage principal reduction; 32 of them permitted cramdown after 1988.

¹¹An example is the Wilsons’ bankruptcy filing. In 1983, the Wilsons purchased their home. By 1988, the balance of the mortgage was \$38,176.75, while the market value of the Wilson’s home had depreciated to \$22,000. Wilsons’ Chapter 13 plan bifurcated the mortgage debt under section 506(a), thus limiting the secured claim to \$22,000 (*Wilson v. Commonwealth Mortgage Corp.*, 895 F.2d 123, 128 (3d Cir. 1990)).

¹²*Nobelman v. American Savings Bank*, 508 U.S. 324 (1993).

¹³There are 11 circuit courts in the United States, and each circuit court covers two to nine states. Each state is divided into one to four bankruptcy court regions.

3 Data Sources and Summary Statistics

To estimate the impact of cramdown in Chapter 13 bankruptcy on debtors' home foreclosures and other outcomes, we assemble several data sets obtained from different sources.

3.1 Data Sources

We compile a new data set from the PACER system. We received fee exemptions to the docket records of 59 bankruptcy district courts in 36 states. In addition, we complement our data with bankruptcy data from [Gross, Notowidigdo, and Wang \(2014\)](#). Thus, our sample contains bankruptcy records from 84 (out of 94) courts. This data set contains individual filer information, such as the debtor's name, address, and last four digits of their social security number, and case information, such as the chapter filed, filing date, court, office, outcome (i.e., discharged or dismissed), and judge and trustee names.

To obtain information about the courts that allowed cramdown, we merge our data set with data collected by [Goodman and Levitin \(2014\)](#). This data set comprises all judicial rulings on the permissibility of cramdown from federal bankruptcy, district, and circuit courts between October 1979 and June 1993. With this information, we classify courts into two groups: cramdown and non-cramdown courts.

Our final sample comprises Chapter 13 bankruptcy filers in those district courts in which bankruptcy cases are randomly assigned to judges and the Chapter 13 filers for whom we obtain real estate data. To this end, we follow [Dobbie and Song \(2015\)](#) and impose several data filters. First, we drop offices with a single Chapter 13 bankruptcy judge and exclude filings in courts that assign all cases to a single judge, since in these two situations, there is no randomization of the bankruptcy cases. Second, we exclude office-by-year bins in which a retiring judge's cases have been reassigned with no documentation about the original judge. Third, we drop office-by-year-by-judge bins with fewer than ten cases. Fourth, we restrict our cramdown-period analysis of first-time filers to the period 1989 to May 1993, the period in which most of the cramdown rulings occurred. Using the same filters and offices, we extend

our sample to study the post-cramdown period from June 1993 to December 1995. The post-cramdown sample comprises only offices included in the cramdown-period and maintains the same cramdown and non-cramdown court classification. Finally, we restrict our sample to Chapter 13 filers that we matched to real estate data purchased from Attom using the address data and the last name provided in the bankruptcy dockets.

These filters leave us with 36,655 Chapter 13 filers in 28 offices, 22 bankruptcy courts, and 18 states. In particular, 23,240 cases were filed between 1989 and 1995 in 18 offices in 12 states that allowed cramdown in the pre-period (i.e., cramdown courts), whereas 13,415 cases correspond to courts that did not allow cramdown in the pre-period (i.e., non-cramdown courts). Panel A of Figure 1 depicts the district court locations in our sample, whereas Panel B shows the ones in our sample that allowed cramdown. The final sample includes 375 office-year-judge observations. The number of cases in each office-year-judge bin ranges from 10 to 599, with an average of 206 cases.

For outcome variables, we rely on two proprietary sources. The first includes deeds and foreclosure data from Attom Data Solutions (former RealtyTrac), one of the leading foreclosure listing data providers. Attom collects five types of filings from legal documents submitted by lenders during their foreclosure process. The first two filings, a notice of default (NOD) and a lis pendens (LIS), or written notice of a lawsuit, are submitted before a foreclosure auction. Two of the subsequent filings, a notice of a trustee sale (NTS) and a notice of a foreclosure sale (NFS), are directly associated with a foreclosure auction. Attom also collects information on whether the foreclosed home is purchased by the lender at auction or is real estate owned (REO). From the counties in which Attom has coverage starting in 1989, we successfully match 69% of the cases using only address data from the bankruptcy dockets and 31% using the filer's address and last name. This approach ensures that the debtor was the effective owner at the time of the filing. Prior work has used Attom to obtain foreclosure data (e.g., Mian, Sufi, and Trebbi, 2015; Currie and Tekin, 2015).

The second source of outcome and covariate variables is LexisNexis (LN) Public Records.

LN aggregates data on over 500 million U.S. individuals, both alive and deceased, from a variety of sources, such as birth records, property tax assessment records, criminal records, and voting records. LN provides a panel data set of records for U.S. individuals over time. Specifically, we obtain data about gender, age, history of addresses, and real property records. We successfully match 99% of filers using the names and social security numbers provided in the bankruptcy dockets.¹⁴

3.2 Summary Statistics

Table 1 presents descriptive statistics for all first-time Chapter 13 bankruptcy filers between 1989 and 1995.¹⁵

We have 36,655 cases in our sample from 1989 to 1995. Around 61% of cases are discharged. Less than one-quarter of filers are female. The average filer is 42 years old at the time of filing. More than 85% of filers own a single-family home. In addition, only 1% of the filers have criminal filings.

Additional filer characteristics are measured at the ZIP code level: The average filer in our sample lives in a neighborhood where around 38% are white, 9% are single, 7% have college degrees, and the median income is close to \$29,000 (in 1980s dollars). Finally, other local covariates are measured at the district or state level and are from the earliest year available. In our sample, the average Chapter 7 share is 57.4%, the unemployment rate is 5.8%, and the percentage of the population participating in the Supplemental Nutrition Assistance Program (SNAP) is 4.4%. The average GDP growth at the state level is 2.1%.

¹⁴Prior work has used LexisNexis to obtain personal data on executives (Cronqvist, Makhija, and Yonker, 2012; Yermack, 2014), fund managers (Pool, Stoffman, and Yonker, 2012; Chuprinin and Sosyura, 2018), and financial journalists (Ahern and Sosyura, 2015).

¹⁵Table IA.1 reports separate statistics for the cramdown and non-cramdown samples.

4 Research Design

Following [Dobbie and Song \(2015\)](#), we estimate the effect of receiving Chapter 13 bankruptcy protection in courts that offered principal reduction. We exploit the fact that some U.S. bankruptcy courts use a blind rotation system to assign cases to judges within offices. Although there are standard principles by which judges evaluate and dismiss bankruptcy cases, the variation in the interpretation of these principles across judges is substantial ([Sullivan, Warren, and Westbrook, 1994](#); [Norberg and Compo, 2007](#); [Chang and Schoar, 2013](#)).

Ordinary least squares specifications of bankruptcy post-filing outcomes on whether the debtor receives Chapter 13 bankruptcy protection in courts that allowed cramdown is problematic for several reasons. For example, an omitted variable bias may occur because of unobserved characteristics that can affect post-filing outcomes. To overcome such identification problems, our research design uses the random assignment of judges. In particular, we exploit the differences in judge discharge rates (i.e., leniency) as an instrument for receiving Chapter 13 bankruptcy protection, through a two-stage least squares approach.

Following the literature, we estimate judge styles every year by omitting their own observations to address the own-observation bias problem.¹⁶ We define the judge’s leniency of filing i as the leave-one-out fraction of filings granted by judge j in court c and year t :

$$Leniency_{icjt} = \frac{1}{n_{cjt} - 1} \left(\sum_{k=1}^{n_{cjt}} D_k - D_i \right) - \frac{1}{n_{ct} - 1} \left(\sum_{k=1}^{n_{ct}} D_k - D_i \right), \quad (1)$$

where D_i is an indicator variable for discharge, n_{ct} is the number of cases in court c in year t , and n_{cjt} is the number of cases by judge j in year t . We estimate the judge’s leniency for each year during the period from 1989 to 1995.

Subsequently, we estimate the causal effect of having a Chapter 13 case discharged through a two-stage least squares regression using the judge’s leniency as an instrumental variable

¹⁶See, for example, [Kling \(2006\)](#); [Doyle \(2007\)](#); [David and Houseman \(2010\)](#); [Chang and Schoar \(2013\)](#); [Dahl, Kostøl, and Mogstad \(2014\)](#); [French and Song \(2014\)](#); [Aizer and Doyle \(2015\)](#); [Maestas, Mullen, and Strand \(2013\)](#); [Dobbie and Song \(2015\)](#); [Dobbie, Goldsmith-Pinkham, and Yang \(2017\)](#); and [Cheng, Severino, and Townsend \(2021\)](#).

for bankruptcy protection. Specifically, we focus this analysis only on offices that allowed cramdown during the cramdown period. Therefore, the second-stage estimating equation is

$$y_i = \alpha_{ot} + \beta D_i + \gamma' X_i + \varepsilon_i, \quad (2)$$

where y is consumer i 's outcome in the period of interest (e.g., foreclosure within five years after filing), D is an indicator of whether the Chapter 13 case was discharged, α_{ot} are the office-by-month-of-filing fixed effects, and X is a vector of pre-treatment covariates. β is the local average treatment effect (LATE) of receiving discharge in Chapter 13 for filers whose outcomes are altered by judge assignment (Imbens and Angrist, 1994). The first-stage estimating equation is

$$D_i = \alpha_{ot} + \theta \text{Leniency}_{icjt} + \gamma' X_i + \varepsilon_i, \quad (3)$$

where *Leniency* is the systematic component of judge behavior. To account for any serial correlation across filers at the level of randomization in both the first and second stages, we cluster standard errors at the office-by-judge level.

The three main identifying assumptions are (a) judge assignments are associated with bankruptcy protection, (b) judge assignments only impact debtor outcomes through the probabilities of receiving bankruptcy protection, and (c) the monotonicity assumption.

The exclusion restriction implies that the judge's leniency is unrelated to unobservable determinants of future outcomes. To partially test for this assumption, we estimate in Table 2 whether any pre-treatment individual- or local-level characteristics, such as age, gender, criminal filings, single-family property, education, and income, can explain judge leniency. We control for office-by-month-of-filing fixed effects and cluster standard errors at the office-judge level. None of our covariates are significantly related to leniency, neither using individual-level nor local-level characteristics, and joint F -tests have p -values of 0.4769 and 0.2641, respectively.¹⁷ We discuss the relevance of judge assignments in more detail in Section 5.1.

¹⁷In Section 5.2, we show that adding controls to the regressions does not significantly change our estimates, consistent with the hypothesis that cases are randomly assigned (Dahl, Kostøl, and Mogstad, 2014).

The exclusion restriction is violated if the judge’s leniency affects debtor outcomes through variables other than bankruptcy protection. For example, more lenient judges could also be more likely to provide financial counseling to debtors. Since judges typically only interact with debtors at the confirmation hearing, it seems unlikely that judges would influence debtors other than through their ruling on the bankruptcy filing.

Finally, to identify a local average treatment effect, we must assume monotonicity. In our setting, the monotonicity assumption is that the probability of debt being discharged in Chapter 13 does not decrease when assigned to a relatively more lenient judge.¹⁸ A partial test of the monotonicity assumption is that the first-stage estimates should be non-negative for different subsamples (Dahl, Kostøl, and Mogstad, 2014). Section 5.1 provides evidence of a positive first stage based on filer characteristics.

The local average treatment effect β in Equation (2) captures the total impact of Chapter 13 discharge on debtor’s future outcomes. Thus, it includes the direct effect of cramdown as well as the indirect effect of other debt discharge.

To investigate the direct effect of cramdown, we take advantage of the Supreme Court decision that disallowed cramdown in June 1993. The motivation is that those who filed before June 1st, 1993, received both unsecured and secured debt relief, whereas those who filed after June 1st only had their unsecured debt discharged. Therefore, we expect that the change in the discharge effect on individual outcomes in the post-cramdown period is due to the lack of mortgage principal reduction. Consequently, we study whether the discharge effect changed after the Supreme Court decision for the offices that allowed cramdown:

$$y_i = \alpha_{ot} + \beta D_i + \delta D_i \times Post_t + \gamma' X_i + \varepsilon_i, \quad (4)$$

where $Post_t$ is an indicator variable for whether the bankruptcy case was filed after the Supreme Court decision. We instrument for $D_i \times Post_t$ using the interaction between judge

¹⁸Without the monotonicity assumption, the IV estimator will deliver a weighted average of the marginal treatment effects (Angrist, Imbens, and Rubin, 1996).

leniency and the $Post_t$ indicator. The coefficient of interest δ captures the change in the discharge effect after the Supreme Court decision in courts that initially permitted cramdown. The uninteracted $Post_t$ variable is not included in the equation since it is subsumed by the time fixed effects.

One important advantage of this approach is that it allows us to make within-office comparisons. The prediction is that if principal reduction is an important driver of the benefits of receiving bankruptcy, we should expect δ to have the opposite sign as β , and the higher the relevance of principal reduction, the smaller the difference between δ and β .

An alternative approach is a design akin to a triple-difference specification to study whether the Supreme Court decision had a differential impact on debtors from courts that initially allowed cramdown relative to those that did not allow cramdown:

$$y_i = \alpha_{ot} + \beta D_i + \eta D_i \times Cramdown_i + \delta D_i \times Post_t + \lambda D_i \times Cramdown_i \times Post_t + \gamma' X_i + \varepsilon_i. \quad (5)$$

The variable $Cramdown_i$ captures whether a bankruptcy case occurred in a court that allowed cramdown before the Supreme Court decision. We instrument for $D_i \times Cramdown_i \times Post_t$ using the interaction between judge leniency, the $Cramdown_i$ indicator, and the $Post_t$ indicator. Importantly, we use the same set of offices that allowed cramdown as in Equations (2) and (4). Two coefficients are of particular interest: η captures the difference in the discharge effect between cramdown and non-cramdown courts before the Supreme Court decision and λ captures the change in the discharge effect between cramdown and non-cramdown courts following the Supreme Court decision. The office \times month fixed effects subsume the omitted direct and interaction effects. In this case, the prediction is that if principal write-down is an important driver for the post-filing benefits of Chapter 13 in cramdown courts, we expect λ to have the reverse sign as η , and, similarly as before, the sum of both coefficients should be lower the greater the importance of mortgage cramdown.

5 Bankruptcy Protection in Cramdown Courts

We discuss in this section the effects of Chapter 13 bankruptcy discharges in cramdown courts. Households that desire to keep their homes typically prefer to file for Chapter 13 instead of Chapter 7 bankruptcy. In addition, mortgage cramdown can be beneficial for homeowners because it reduces the debt burden if their mortgage debt exceeds the fair value of the home. However, homeowners will still need to make regular mortgage payments in the same amount as the mortgage's original terms to avoid foreclosure. Furthermore, refinancing is typically not possible during the first years after filing for Chapter 13 bankruptcy as the filer needs a judge's approval to refinance and as the credit score deteriorates after a bankruptcy filing.

5.1 First Stage

Figure 2 illustrates the relevance of the judge's leniency for bankruptcy discharges, aggregated at the judge-year level. The figure shows the relation between the residualized bankruptcy discharge rate (i.e., ε_i from Equation (3) averaged for each judge-year level) against the leave-one-out measure of the judge's leniency (i.e., $Leniency_{icjt}$ from Equation (1) averaged for each judge-year level). The regression Equation (3) includes office-by-month fixed effects. By purging month and office effects, this procedure isolates the variation among judges in the same office at the same time. Panel A depicts the first-stage relation without demographic controls, and Panel B shows the relation with controls for age, gender, property type, criminal filings, the percentages of white, single, and college-educated inhabitants; and the ZIP-code-level logarithm of the median income. We observe a strong positive relationship between the judge's leniency and residualized bankruptcy discharge, both with and without control variables.

Interestingly, Figure 2 shows that the judge's leniency has a very broad range from around -0.3 to 0.6. Thus, the discharge rate for the most lenient judges is around 60 percentage points higher than that for the other judges in the same office at the same time, and the discharge rate for the strictest judges is around 30 percentage points lower than that of their peer judges.

This substantial range in leniency demonstrates important differences in the interpretation of the standard principles of bankruptcy across judges. We also find significant persistence in judge leniency. Figure 3 depicts current and lagged judge discharge rates, with each point representing a separate judge-by-year observation. We find that the discharge rate is significantly persistent between 1989 and 1995.

Table 3 presents the linear-probability relation (Equation (2)) between bankruptcy discharge and the judge’s leniency using disaggregated individual-level data. The standard errors are clustered at the office-by-judge level.

Column (1) presents the estimates without any controls besides office-by-month fixed effects. We find that a one-percentage-point increase in the judge’s leniency in other filings increases the probability that a debtor receives Chapter 13 protection by 0.862 percentage points. The estimates are similar when we include controls at the individual level (Column (2)) and at the ZIP code level (Column (3)). The leave-one-out measure of leniency, filer characteristics, and office-by-month fixed effects explain only around 21% of the variation in discharge rates. Thus, a substantial proportion of the variation cannot be explained by observable variables.

Interestingly, Chapter 13 filings by female debtors are less likely to be discharged than those of male debtors. The discharge rate for female filers is around 4 percentage points lower than the rate for male filers after controlling for various neighborhood characteristics. Furthermore, filings by debtors living in ZIP codes with a larger fraction of white inhabitants are more likely to be successful. These differences could be due to unobserved characteristics but also due to discrimination.¹⁹ Judges may impose higher qualification thresholds on individuals who represent minority groups in bankruptcy filings.²⁰

¹⁹Agarwal, Chomsisengphet, McMenemy, and Skiba (2010) use information on name and ZIP code to calculate a Bayesian-likelihood estimate of individual debtors’ race. Controlling for debtor demographics and financial situation, their results show that white judges are 21% more likely to dismiss the Chapter 13 petition of an African American debtor relative to a white debtor.

²⁰In untabulated results, we hand-collect data on the gender of the judges in our sample. We find that, on average, the gender of the judge does not impact the probability of receiving Chapter 13 protection. In addition, we find no significant evidence of an interaction effect on the judge’s gender and the filer’s gender.

To test for monotonicity we study the characteristics of compliers, following Angrist and Pischke (2008). Table IA.2 in the Internet Appendix reports the distribution of filers in cramdown courts from 1989 to 1995 by gender and age group. Column (1) presents the proportion of the sample within each gender and age group. Column (2) shows the first-stage estimates for each gender and age group, corresponding to Table 6. Consistent with the monotonicity assumption, we find positive first-stage estimates for the different subgroups. Column (3) reports the distribution of compliers by gender and age. The last column is the ratio of the likelihood of a filer belonging to a particular gender and age group in the compliers relative to the probability in the sample. This column shows that women over 40 are slightly underrepresented in the compliers groups in cramdown courts.

Altogether, Figure 2 and Table 3 confirm that judges' leniency is highly predictive of receiving bankruptcy protection. Thus, the leniency satisfies the relevance criterion and the monotonicity assumption.

5.2 Discharge Effect on Distress Events in Cramdown Courts

To study whether a Chapter 13 discharge in cramdown courts reduces future foreclosures, we estimate the two-stage least squares specification, summarized in Equations (2) and (4).

5.2.1 Discharge Effect Before Supreme Court Decision

Table 4 reports the estimates of the impact of Chapter 13 bankruptcy protection in courts that allowed mortgage cramdown on homeowners' distress events.

We use two different distress events: foreclosures (Panel A) and involuntary sales (Panel B). Home foreclosure is an indicator for a filer's home receiving a notice of default, transfer or sale, or having been transferred to a real estate owned after the bankruptcy filing. Involuntary sales capture cases where homeowners sell their properties for less than the amount due on the mortgage after the bankruptcy filing.²¹ The controls include gender, age, property type,

²¹In our sample, 69% of involuntary sales are associated with foreclosure events.

criminal filings, the percentage of the ZIP code’s population that is white, single, and college educated, and the median log income in the ZIP code. All regressions include office-by-month-of-filing fixed effects, and the standard errors are clustered at the office-by-judge level.

The first column in each panel reports the results for Equation (2) for the period between 1989 and May 1993. Column (1) in Panel A of Table 4 shows that a discharge in cramdown courts reduces foreclosures by 26.3 percentage points after three years.²² The estimate is highly statistically and economically significant and very similar if we exclude the demographic control variables (see Table IA.3 in the Internet Appendix). Furthermore, the results are also economically significant, as the average three-year foreclosure rate in the sample of Chapter 13 dismissed filers is 23.8 percentage points.²³

Panel A of Figure 4 depicts the yearly changes in home foreclosure rates after discharge in cramdown courts. The economic magnitude of the discharge effect is relatively small in the first year but grows substantially over the next two years. The discharge effect stabilizes after four years.²⁴

Column (1) in Panel B of Table 4 and Panel B of Figure 4 report similar effects for involuntary sales. For example, bankruptcy discharge decreases the probability of an involuntary sale over the first three years by 24.4 percentage points, relative to the control group mean of 18 percentage points.

5.2.2 Discharge Effect Around Supreme Court Decision

The last three columns of Table 4 report the results for Equation (4) including the bankruptcy filings after the Supreme Court decision of June 1993. We find that the discharge benefit decreases substantially after cramdown was disallowed. In particular, for the period from

²²Figure IA.1 in the Internet Appendix plots average home foreclosure rates for discharged and dismissed Chapter 13 cases. At three years post-filing, foreclosure rates for discharged homeowners are 2.5 percent, relative to around 23 percent for dismissed filers.

²³Because most foreclosures result in a change of address, we use the dismissed filer mean for reference, as we only observe an individual address at the time of filing.

²⁴In panel A of Figure IA.2 and Table IA.4 in the Internet Appendix, we find similar results if we use a distress measure that includes other events, such as real-estate-owned (REO) liquidation, besides the foreclosure events.

1989 to 1995, a discharge reduces foreclosures by 26.8 percentage points before the Supreme Court decision and by just 10.2 (i.e., $-0.268+0.166$) percentage points after the decision. The difference in the discharge effect is economically and statistically significant. The results over the narrower windows around the Supreme Court decision further confirm that there are smaller discharge benefits on foreclosures after cramdown was disallowed: 10.2 and 3.3 percentage points for the 1991-1995 and 1992-1994 windows, respectively.

Panel B of Table 4 reports similar results for involuntary sales. Column (2) shows that following May 1993, the Chapter 13 effect was just 2.1 percentage points (i.e., $-0.236+0.215$). Columns (3) and (4) show similarly small benefits of being discharged over narrower windows.

Overall, these results suggest that principal reduction plays an important role in the documented effects in Column (1) of Table 4 and Figure 4.

5.2.3 House Price Appreciation

To further understand the mechanism behind the documented effects of discharge in cramdown courts, we estimate whether the effect differs between homeowners in areas with high and low house price appreciation. Bankruptcy filers are more likely to have negative home equity in districts with worse house price performance and these debtors benefit more from mortgage cramdown. To perform this test, we construct house price indices at the district court level using data from the Federal Housing Finance Agency (FHFA) at the ZIP code level. First, we create population-weighted house price indices at the county level. Subsequently, using the county-district court crosswalk by Hansen, Chen, and Davis (2015), we build population-weighted house price indices at the district court level. Using these indices, we estimate each district's previous five-year house price appreciation and classify them annually (i.e., using a rolling window classification) into two groups: below- and above-median appreciation. The median cumulative five-year appreciation is 1.97%, and the average appreciation for those in the below-median group is -10.14%.²⁵ Thus, our sample period provides

²⁵The average cumulative five-year appreciation for those in the above-median group is 16.55%.

substantial geographic variation in house price appreciation.

Table 5 reports the estimates. Column (1) of Panel A shows that during the cramdown period, bankruptcy discharge decreases the probability of foreclosures by 7.1 percentage points for filers who reside in district courts with house appreciation above the median. However, filers who are more likely to be underwater (i.e., districts with house appreciation below the median) are 30.7 (i.e., $-0.236-0.071$) percentage points less likely to be foreclosed.

Columns (2)-(4) present the estimates for Equation (4) around the Supreme Court decision. For the post-cramdown period, we continue to build the above and below the median district court classification as we described earlier.²⁶ Column (2) shows that after the Supreme Court decision, bankruptcy discharge decreases the probability of foreclosures by 10.3 percentage points (i.e., $-0.0724-0.234-0.0277+0.231$) for filers who reside in district courts with house appreciation below the median, a 23.1 percentage point reduction of the discharge benefits once cramdown was disallowed. Columns (3) and (4) use narrower windows and confirm a substantial drop in benefits after the Supreme Court decision for borrowers living in areas with distressed housing markets. Panel B shows similar findings for involuntary sales.

5.2.4 Filer Characteristics

The benefit of mortgage discharge in cramdown courts may differ among households with varying characteristics. Gender might matter because judges may treat female filers differently than male filers, as documented in Table 3. Female filers might also be more reluctant to move, as they are more likely to be the main caregivers of dependent children. Similarly, foreclosures may be more disruptive for younger debtors with school-aged children. Finally, foreclosures could be more costly for individuals living in single-family homes, since they are more likely to customize their homes according to their own preferences.

Table 6 shows that the discharge benefits on foreclosures and involuntary sales in cramdown courts are substantial across all the different demographic groups. Furthermore, the

²⁶Table IA.5 in the Internet Appendix shows that the results are similar if we keep the below and above median classification of May 1993 for the period following the Supreme Court decision.

benefits of bankruptcy discharge are reduced for all groups after the Supreme Court decision. In addition, the benefits of discharge in the pre-period and the reduction in the benefits in the post-period are typically more pronounced for female, younger, and single-family borrowers, although these interaction coefficients are often not statistically significant due to limited power in our sample. For the analysis comparing female and male borrowers, we find statistically significant interaction effects for female borrowers using involuntary sales, as shown in Panel B. Bankruptcy discharge in cramdown courts reduces involuntary sales by 21 percentage points for men and by 42 percentage points for women prior to the Supreme Court decision. In contrast, after the Supreme Court disallowed cramdown, bankruptcy discharge reduces involuntary sales by only 3.4 percentage points for men (i.e., $-0.21+0.176$) and by 1.6 percentage points for women (i.e., $-0.21-0.21+0.176+0.26$). The estimates indicate that women benefit more from cramdown and have been hurt more when cramdown was disallowed.²⁷

Columns (3) and (4) in Table 6 show the differences in the discharge impact between filers below and above 40 years. Young filers are more likely to have dependent children which increases the costs of moving. We find that young filers benefit from a larger reduction of involuntary sales after a mortgage discharge during the cramdown period (Panel B). However, the benefit of mortgage discharge is not significantly larger for young filers using foreclosures as a measure of distress (Panel A).

Finally, owners of single-family homes exhibit larger point estimates for the benefits of mortgage discharge in the pre-period for both distress proxies, although the differences are not statistically significant.

²⁷A potential concern with this result is how we define gender for joint filers. We classify the filer's gender based on the first filer in the bankruptcy docket. To mitigate this concern, we separately estimate the effects of receiving Chapter 13 protection for female, male, and joint filers. In untabulated results, we find a similar difference in post-filing outcomes between female and male filers as in Table 6. We also document a difference between female and joint filers.

5.2.5 Heterogeneity

The magnitude of the discharge effects could also depend on a state’s recourse status, homestead exemption generosity, or wage garnishment rates. In this section, we study the heterogeneity of the discharge effect of Chapter 13 in cramdown courts on foreclosures across these dimensions.

The first two columns of Table 7 show the differential effect of receiving bankruptcy protection on foreclosures based on whether the filer resides in a recourse state. Column (1) shows that the difference between recourse and non-recourse states during the cramdown period is not statistically significant. Moreover, when we expand the sample to include the post-cramdown period (1989-1995), the coefficient of the triple interaction is also statistically insignificant.

We compute the homestead exemption median for the states in our sample, using information on these exemptions from [Hynes, Malani, and Posner \(2004\)](#). In Column (3), we find that the estimates are statistically indistinguishable between states with different exemption generosityes during the cramdown period. In Column (4), we also find no differences if we include the post-cramdown period.

Lastly, we evaluate whether the documented effects depend on differences in garnishment rates. To this end, we collect wage garnishment information for each state in our sample. In states that follow the federal guidelines, creditors are allowed to garnish each additional dollar of disposable earnings between 30 and 40 times the minimum wage (i.e., subsistence allowance), but only 25 cents of every extra dollar beyond that. However, there is also across-state variation in garnishment rates. For instance, some states raise the threshold of wages that are protected or reduce the percentage of wages that can be garnished, and in a few cases, wage garnishment is banned. Therefore, for each state, we compute the statutory maximum amount per month that can be garnished for individuals whose incomes are just above the subsistence amount per month (i.e., $4.33 \times \text{minimum wage} \times \text{subsistence}$

allowance).²⁸ Then, we classify each state into two groups: states with garnishment rates above the median and those below the median. Column (5) shows that during the cramdown period, the difference between states with wage garnishment above or below the median is not statistically significant. Column (6) presents consistent results during the post-cramdown period.²⁹

5.3 Additional Benefits

The debt relief provided by successful Chapter 13 filings in cramdown courts may have additional benefits for debtors. For example, mortgage discharge can stabilize the living arrangements and circumstances of households.

First, we study whether the filer has at least one change of the principal address during the three years after filing. Second, we analyze if, after filing for bankruptcy, the individual moves to a ZIP code with a median income higher than the ZIP code of residence at the time of filing. Third, we explore if the filer is involved in a property sale different from a home short sale in the first three years after filing. Finally, we study if the filer has been charged with a misdemeanor or a felony during the three years after filing. Table 8 reports the effects of discharge on these other outcomes around the Supreme Court decision. The empirical specification is given in Equation (4), where we use the described outcome variables.

We find that households with discharged debt are significantly less likely to move to a new address during the three years after the filing in cramdown courts before the Supreme Court decision. However, there is no significant differential effect after the Supreme Court ruling.

Although borrowers of discharged debt are less likely to move, they are more likely to move to ZIP codes with higher median income levels during the cramdown period. We do

²⁸A factor of 4.33 is used to convert weekly to monthly figures as in [Argyle, Iverson, Nadauld, and Palmer \(2021\)](#).

²⁹In Table IA.6 in the Internet Appendix, we also study the heterogeneity of the discharge effect of Chapter 13 in cramdown courts on foreclosures in recession periods relative to non-recession periods from 1989 to 1993 and find that the difference is not statistically significant.

not find a statistically significant differential effect after cramdown was disallowed by the Supreme Court. However, this coefficient post-period is similar in absolute magnitude to the discharge coefficient for the pre-period. Thus, the overall impact of moving to a better ZIP code is nonexistent for the period without cramdown.

Discharged bankruptcy filers are 8.9 percentage points less likely to voluntarily sell their homes over the subsequent three years (excluding short sells) during the cramdown period. However, the effect substantially decreases to 1.7 percentage points (i.e., $-0.0892+0.0721$) once cramdown is banned.

Finally, debt discharge in cramdown courts also has broader benefits for households during the cramdown period, as the rate of criminal filings decreases over the first three post-filing years. However, we do not find a significant differential effect after the Supreme Court decision.

Refinancing a mortgage is difficult during the first years after a bankruptcy filing. It requires approval from the bankruptcy judge, and mortgage lenders are unwilling to lend to borrowers with a recent bankruptcy flag on their credit records. Consistent with this, in Column (1) of Table 9, we find no significant increase in refinancing in the first three post-filing years. However, as Columns (2)-(4) show, we find a significant increase in refinancing activity after five years. Homeowners start to refinance their mortgage debt as they build up home equity and as their credit scores improve. In addition, after five years, filers have completed their repayment plan. In addition, we find that the effects on refinancing after five years are substantially lower following the Supreme Court decision. Thus, cramdown does not only reduce the debt overhang of bankruptcy filers, but it also reduces their liquidity constraints over the medium- to long-term as they are able to refinance their mortgages.

6 Comparison between Cramdown and Non-Cramdown Courts

Our previous results show that debt discharge in cramdown courts has a large impact on the economic circumstances of households. In this section, we combine our samples from cramdown and non-cramdown courts and study whether the effects differ across the two samples. We again take advantage of the Supreme Court decision of 1993 to help identify the causal impact of cramdown.

6.1 Baseline Results

We exploit the across-district variation in cramdown status and the removal of principal reduction by the Supreme Court in June 1993 to estimate the impact of Chapter 13 bankruptcy protection in cramdown courts relative to non-cramdown courts in the specification summarized in Equation (5).

Figure 5 provides the dynamics of the impact of debt discharge on the two distress proxies between cramdown and non-cramdown courts before the Supreme Court decision. Debt discharge is substantially more beneficial in cramdown courts because of the reduction of debt for underwater homeowners. The first column of Table 10 confirms that the impact of debt discharge is substantially more pronounced in cramdown courts. For example, the three-year foreclosure rate declines by more than 26.7 percentage points for cramdown courts and by only 3.4 percentage points for non-cramdown courts before the Supreme Court decision. The difference in the two discharge effects is highly statistically significant. It is reassuring that the cramdown effect of 23.3 percentage points estimated by comparing cramdown and non-cramdown courts before June 1993 (Column (1) of Table 10) is similar to the cramdown effect of between 16.6 and 25.5 percentage points estimated by comparing cramdown courts before and after the Supreme Court decisions (Columns (2)–(4) of Table 4).

We also study whether the discharge effects across the two types of courts change after the

Supreme Court decision. The results are summarized in the last three columns of Table 10.³⁰ The triple-interaction coefficient is significantly positive, indicating that the discharge effect is significantly attenuated for bankruptcy filings in cramdown courts after the Supreme Court decision. For example, a discharge in a cramdown court reduces home foreclosures by 26.6 percentage points (i.e., $-0.0344-0.232$) before June 1993 and by just 10.2 percentage points (i.e., $-0.0344-0.232+0.0186+0.146$) after May 1993. The total discharge effect on foreclosure in cramdown courts after the Supreme Court decision is even smaller if we focus on the narrower window between 1992 and 1994, as summarized in the last column of the table.³¹ Panel B in Table 10 and Figure 5 report very consistent results using involuntary sales instead of home foreclosures.

6.2 Robustness Tests

The propensity to file for Chapter 13 may differ across cramdown and non-cramdown courts. For example, more households may file for Chapter 13 bankruptcy in cramdown courts because of the potential discharge of the underwater portion of the mortgage. These additional filers therefore may be less distressed. Furthermore, trustees and judges in cramdown courts may follow different debt restructuring strategies. Judges may be more prone to approve a restructuring plan in cramdown courts since these plans are more likely to succeed if a write-down of mortgage debt is possible.

The characteristics of bankruptcy filers could change after the Supreme Court decision. To test this hypothesis, we exploit district variation in cramdown status in a difference-in-differences approach on observable characteristics at the individual level at the time of the filing, such as age, gender, criminal records, and single-family residence. We also expand our analysis to variables at the local level, such as the chapter filing composition, changes in the house price index, unemployment rate, percentage of the population that apply to the SNAP

³⁰By studying the discharge effects of Chapter 13 bankruptcy around the Supreme Court decision we alleviate concerns regarding cross-state differences in bankruptcy filing rates (Lefgren and McIntyre, 2009).

³¹Table IA.7 in the Internet Appendix reports that the estimates are similar if we exclude covariates.

program, and GDP growth.³² Table 11 shows no evidence of changes in observable covariates at the individual (Panel A) or local level (Panel B).³³

Moreover, to investigate whether the mortgage cramdown feature significantly affected the propensity to file for Chapter 13 bankruptcy, we study the number of these filings around the Supreme Court decision of June 1993 that prohibited mortgage cramdown in all states. Figure 6 shows no evidence of bunching in the number of filings around the 1993 Supreme Court decision. This result is also consistent with the small effect of cramdown on credit supply and cost of credit documented by Goodman and Levitin (2014) and Li, Tewari, and White (2014).

We also restrict our analysis to district courts that adopted cramdown after a circuit court (i.e., higher level courts) decided an appeal from a district decision. In our analysis, two circuit-level decisions introduced cramdown to district courts that did not allow it before. First, we focus on the cramdown adoption from the Ninth Circuit following the decision on *Houglund v. Lomas & Nettleton Co.* In this case, cramdown was permitted in the District of Oregon since 1988, when the judge in charge of Houglund’s bankruptcy case allowed principal mortgage reduction. However, this decision was appealed and brought to the Ninth Circuit Court, which upheld the cramdown adoption. Thus, we focus on the five courts (excluding Oregon) that introduced cramdown in our sample after this ruling. Second, we also focus on the 10th Circuit ruling that allowed cramdown on the districts that belong to this circuit, particularly the District of New Mexico.

Thus, we restrict our analysis to courts that became cramdown courts following a high circuit ruling (i.e., District of Alaska, District of Arizona, Northern District of California, Southern District of California, District of Idaho, District of New Mexico, and District of

³²As our treatment, in this case, is at the district level, we cluster the standard errors at the district level (Abadie, Athey, Imbens, and Wooldridge, 2017). Since we have 22 districts in our sample, we follow MacKinnon and Webb (2017) and apply the correction for the number of clusters using ordinary wild bootstrap.

³³We also test in a difference-in-differences approach whether the discharge rate differs before and after the Supreme Court decision in cramdown and non-cramdown courts. We find no statistically significant differences with a p -value equal to 0.2513.

Nevada). Table IA.8 in the Internet Appendix presents the foreclosures and involuntary sales estimates, which are consistent with the findings in Table 10.

Finally, the effects documented in Table 10 could be explained by cross-state differences. We focus on states with cramdown courts and those with both cramdown and non-cramdown courts (i.e., we exclude states with only non-cramdown courts). The estimates on this subsample, reported in Table IA.9 in the Internet Appendix, are similar to those in Table 10. These findings support that cross-state differences do not explain the documented effects.

7 Discussion

In this section, we discuss the mechanisms for our results, the costs and benefits of cramdown, and the impact of the cramdown proposal during the Great Recession.

7.1 Mechanisms

In the previous sections, we document substantial benefits for homeowners of successful Chapter 13 filings in cramdown courts and that a sizable fraction of these benefits disappeared after cramdown was disallowed. In addition, the comparison of the ex-post effects of successful filers in courts with and without cramdown provides evidence that the principal reduction through personal bankruptcy offers substantial benefits for homeowners.

Two potential explanations for the large economic magnitude of the effects merit discussion. The first, which we call the liquidity hypothesis, is that the reduction in short-term payments helps debtors by alleviating liquidity constraints. The second is that the reduction in long-term obligations helps debtors by reducing their debt relative to the value of their homes. We call this second explanation “the debt overhang hypothesis” (Ganong and Noel, 2020a; Dobbie and Song, 2020; Ganong and Noel, 2020b; Indarte, 2021).

Two features in the setting shed light on the mechanism. First, in courts that allowed cramdown, the negative equity portion of the mortgage is classified as unsecured debt and

thus is dischargeable. Second, in cramdown, the loan will not be modified. That means debtors must continue making monthly payments following the original loan terms until the principal payments total the present value of the collateral. Thus, cramdown primarily provides debt relief in the form of the second hypothesis, and it seems that the debt overhang channel plays an important role in explaining our results.

There are two challenges to this interpretation. First, debtors could have also received some unsecured debt relief, in addition to the principal reduction in Chapter 13. The analysis around the Supreme Court decision within cramdown courts isolates the effect of principal reduction. In addition, the comparison of cramdown in Chapter 13 to standard Chapter 13 (as shown in Figure 5), where we find that the impact of cramdown is substantially larger on foreclosures, alleviates this concern.³⁴

The second challenge is that debtors could have received unsecured or secured credit following Chapter 13 bankruptcy. However, in the case of unsecured debt, after filing for Chapter 13, lenders can cancel the debtor's account. Also, the trustee may confiscate the filer's credit cards since the trustee must supervise the debtor's finances for three to five years (Elias, 2013). In addition, Dobbie, Goldsmith-Pinkham, and Yang (2017) do not find an effect on unsecured credit following Chapter 13. In the case of secured lending, it could be that debtors refinanced their mortgages following cramdown. However, as shown in Table 9, refinancing is typically unavailable in the first years following a Chapter 13 filing, and it is difficult in practice. For example, the debtor typically needs to have a good credit score, which is unlikely after receiving a bankruptcy flag, and sufficient home equity to be approved for a mortgage. Furthermore, the filer requires the judge's approval. Thus, it seems unlikely that debtors receive substantial access to external financing, at least during the first years following Chapter 13. However, over the longer term, cramdown can relax liquidity constraints as households are able to refinance their mortgages.

³⁴Another challenge is that successful Chapter 13 cases in cramdown courts could also have done lien stripping of second and third mortgages. However, this procedure can also be done in standard Chapter 13 cases, and thus, comparing the discharge effect of Chapter 13 in cramdown versus non-cramdown lessens this concern.

A caveat of our findings is that our setting does not allow us to study the role of liquidity constraints in foreclosure since we estimate the impact of principal reductions. Consequently, our results do not reject the importance of liquidity constraints as a potential determinant for homeowner default.

Finally, one question that this discussion raises is why our estimates of the principal reduction in Chapter 13 are substantially larger relative to the documented effect of principal reduction policies implemented during the Great Recession. One explanation is that the principal reduction programs employed during the financial crisis (e.g., HAMP) were relatively small and thus, homeowners were probably still underwater following these loan modifications. In contrast, homeowners in cramdown courts had their whole negative equity portion forgiven in Chapter 13 bankruptcy and therefore had greater incentives to avoid default.

7.2 Trade-offs

The assessment of consumer bankruptcy laws typically involves the evaluation of two effects. The first effect is that bankruptcy provides partial insurance by helping households smooth consumption when hit by a particularly adverse event (e.g., health shocks). The second effect is the greater borrowing costs due to a higher risk of default, which reduces households' ability to smooth expected changes in earnings (e.g., [Athreya, 2002](#); [Li and Sarte, 2006](#); [Livshits et al., 2007](#); [Chatterjee and Gordon, 2012](#); [Gross et al., 2021](#)).

Our paper focuses on the first effect of changes in the generosity of the personal bankruptcy code: The ex-post benefits of cramdown in Chapter 13. We document substantial ex-post effects for marginal filers on household distress. Given the high costs of foreclosure ([Diamond, Guren, and Tan, 2020](#)), particularly for marginal debtors who are most responsive to foreclosure mitigation policies, and the negative spillovers of foreclosure, cramdown has significant ex-post benefits for homeowners.³⁵

³⁵Mian, Sufi, and Trebbi (2015), Adelino, Schoar, and Severino (2017), Amromin, Huang, Sialm, and Zhong (2018), and Guren and McQuade (2020) find that foreclosures have large market-level effects, while Immergluck and Smith (2006), Campbell, Giglio, and Pathak (2011), Harding, Rosenblatt, and Yao (2009), Anenberg and Kung (2014), Gerardi, Rosenblatt, Willen, and Yao (2015), and Gupta (2019) provide evidence

However, cramdown may be costly for borrowers ex-ante or could hurt other market participants ex-post. In particular, the introduction of cramdown could lead to higher borrowing costs. [Goodman and Levitin \(2014\)](#) and [Li, Tewari, and White \(2014\)](#) find relatively small effects of cramdown on interest rates and other mortgage terms. The former finds that when cramdown was allowed, home loans had interest rates that were 16 basis points higher, and the latter finds that when the Supreme Court abolished mortgage cramdown, this led to a short-term reduction of 3% in mortgage interest rates and an increase of 1% in mortgage approval rates. Furthermore, [Levitin \(2009\)](#) argues that the market is unlikely to price against bankruptcy modifications since lenders face smaller losses from bankruptcy modifications than from foreclosures.

[Mayer, Morrison, and Piskorski \(2009\)](#) discuss some risks of introducing cramdown legislation during the Great Recession. They state that cramdown could generate a massive number of bankruptcy filings and impose large burdens on bankruptcy judges and trustees. As mentioned, [Figure 6](#) shows no significant bunching in the bankruptcy filings in cramdown courts in June 1993. Furthermore, bankruptcy filings in cramdown courts did not significantly decrease after the Supreme Court decision, as Chapter 13 bankruptcy became less beneficial. This result is consistent with [Indarte \(2021\)](#) who finds only a small incidence of strategic bankruptcy filings. In addition, during our sample period, bankruptcy was substantially less popular, which alleviates concerns about strategic behavior.

Furthermore, [Mayer, Morrison, and Piskorski \(2009\)](#) discuss that cramdown mortgage modifications would be more aggressive than other types of modifications and would impose excessive losses on lenders. The question of whether lenders benefit from cramdown depends on the magnitude of the underwater portion of the debt and on the preferences of households to remain in their homes. If the cost due to negative equity is less than the private benefits, borrowers would not walk away and default, but rather continue making their mortgage payments even if the value of their home is less than what they owe. In this case, cramdown

of localized foreclosure spillovers.

would impose a loss on the lenders equal to the amount that is being written off. If the underwater portion of the mortgage is relatively large, households would likely decide to default on their mortgage if no cramdown is offered. The losses to the lenders and to the mortgage investors would in this case likely be higher than the underwater portion of the loan due to various frictions of the foreclosure process.³⁶

7.3 Cramdown Proposal during the Financial Crisis

The United States experienced an unprecedented number of home foreclosures during the Great Recession of 2007–2009. The government implemented various policies to reduce defaults and the deadweight losses of foreclosures (e.g., HAMP, HARP). However, [Agarwal et al. \(2017\)](#) and [Agarwal et al. \(2022\)](#) show that various frictions hampered the impact of these policies. One alternative proposal in 2008 was to allow mortgage cramdown by judges through Chapter 13 bankruptcy. The proposal passed the House of Representatives but failed in the Senate. In this section, we provide a back-of-the-envelope calculation of the potential benefits that cramdown would have had during the financial crisis.

To estimate the number of foreclosures avoided, we extrapolate our estimates in a partial equilibrium approach. We make several assumptions: First, we assume similar effects across groups (i.e., compliers vs. non-compliers). Second, we assume that the effect of cramdown would have been the same during the Great Recession as during our sample period. Third, the number of total filers is the same with and without cramdown. Fourth and finally, we do not consider general equilibrium effects (e.g., credit supply, interest rate, home values, spillover effects of foreclosures).

We start with 7.6 million total filings of Chapters 7 and 13 bankruptcies between 2008 and 2013.³⁷ The homeownership rate for bankruptcy filers during this period equaled, on average, 61.55%, resulting in around 4.68 million filings by homeowners, which would be expected to

³⁶For example, [Campbell, Giglio, and Pathak \(2011\)](#) show that houses sold after foreclosure, or close in time to the death or bankruptcy of a seller, suffer discounts of around 27 percent of the value of a home.

³⁷In this period, 69.82% of the cases were Chapter 7, and 30.18% were Chapter 13.

file for Chapter 13.³⁸ During this period, 49% of Chapter 13 filings were discharged (Parra, 2020), resulting in 2.29 million successful bankruptcy filings. Table 10 Column (1) documents that cramdown reduced five-year foreclosure rates by around 23.3 percentage points relative to non-cramdown. Using this estimate, we find a reduction of around half a million foreclosures over the five years after the bankruptcy.

Our estimates could underestimate the effect of cramdown during the Great Recession since the decline in house prices was greater during this time period.³⁹ Furthermore, the proportion of households that filed for Chapter 13 bankruptcy could have increased with a more generous bankruptcy procedure. This analysis could also overestimate the effect of cramdown since we identify the causal impact of cramdown for the compliers. However, our estimates for the compliers are relevant for policy since reforms aimed at introducing cramdown will likely have the largest effect on debtors on the margin of program participation.⁴⁰

8 Conclusions

We document that mortgage cramdown has significant ex-post benefits for bankruptcy filers. We find that in cramdown courts, a successful Chapter 13 filing reduces the three-year foreclosure rate by 27 percentage points before 1993. The impact of the bankruptcy discharge on foreclosures is reversed by more than half to around 10 percentage points after the Supreme Court disallowed mortgage principal reduction in 1993. We also find that cramdown explains the vast majority of the reduction in foreclosure rates and involuntary sales for Chapter 13 filers. Our results indicate that cramdown is effective in reducing home foreclosures and enhancing the living standards of bankruptcy filers. Our results suggest that debt overhang considerations play an important role in explaining homeowner default.

³⁸The homeownership rate in this period was 56.63% for Chapter 7 and 72.93% for Chapter 13 (Parra, 2020).

³⁹Although the average drop in home prices was not as pronounced during our sample period as during the Great Recession, there were markets in our sample, such as Massachusetts, where house prices declined by 23% between 1988 and 1993 (Foote, Gerardi, and Willen, 2008).

⁴⁰Cramdown potentially would have pushed more banks to modify mortgages, and thus our partial equilibrium analysis could underestimate the overall reduction in foreclosures by cramdown.

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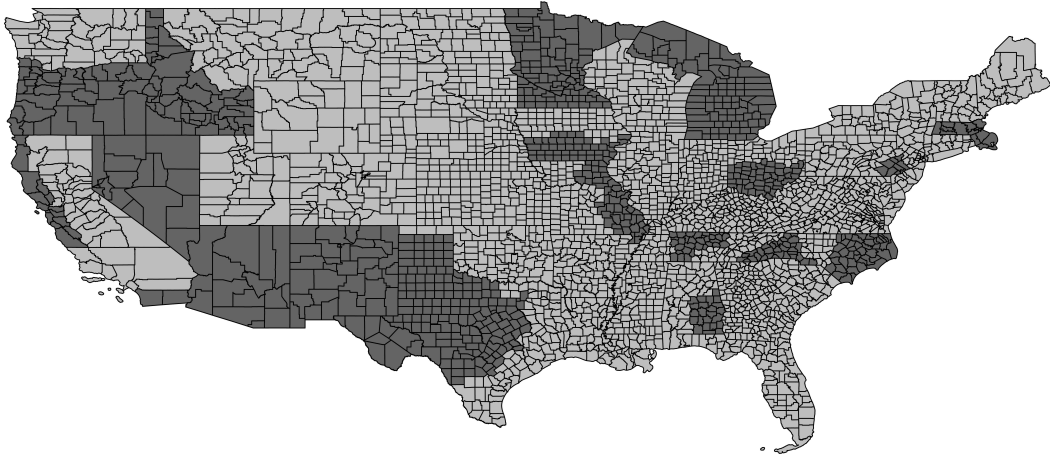
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Panel A. District courts in sample



Panel B. District courts in sample that allowed cramdown

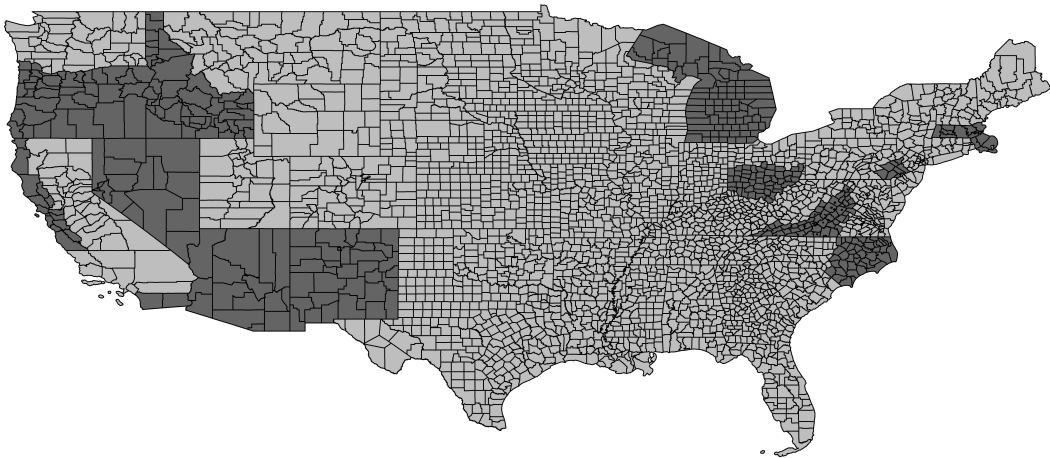
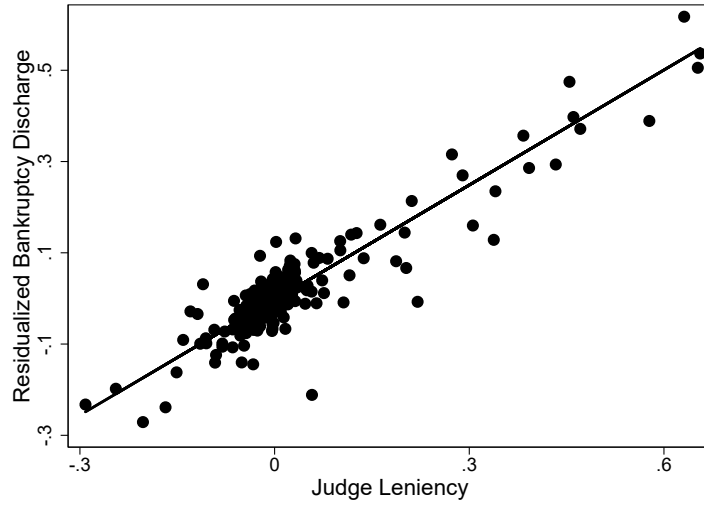


Figure 1. Bankruptcy district courts that allowed cramdown

These panels depict all courts in our sample and those that allowed mortgage principal reduction. The dark gray shading in Panel A represents all the district courts that randomly assigned cases in our sample, whereas in Panel B, the dark gray shading represents the courts in our sample that allowed mortgage cramdown between 1989 and May 1993.

Panel A. First stage without controls



Panel B. First stage with controls

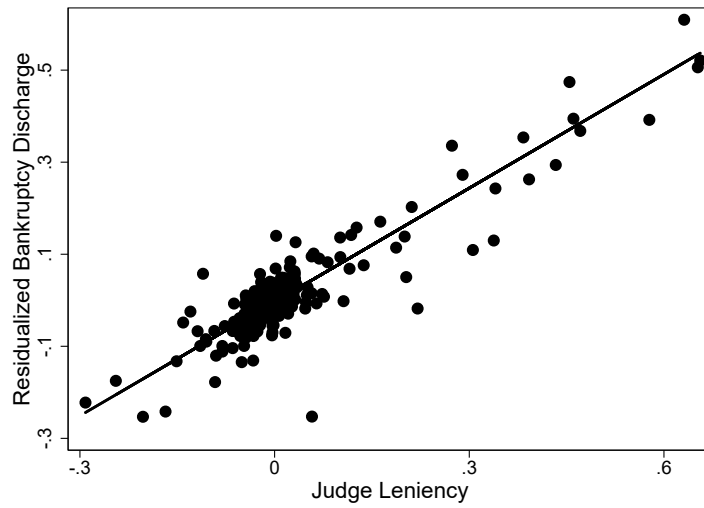


Figure 2. First stage: Relation between judge leniency and bankruptcy discharge
These panels plot Chapter 13 discharge against the leave-one-out measure of judge leniency in cramdown courts. The sample consists of all first-time Chapter 13 filers between 1989 and 1995. To construct the binned scatter plot in Panel A, we regress an indicator variable for Chapter 13 discharge on office-by-month-of-filing fixed effects and calculate residuals. Subsequently, we calculate the mean residual in each judge-by-year bin. In Panel B, we follow the same procedure but include pre-treatment covariates at the filer and ZIP code levels.

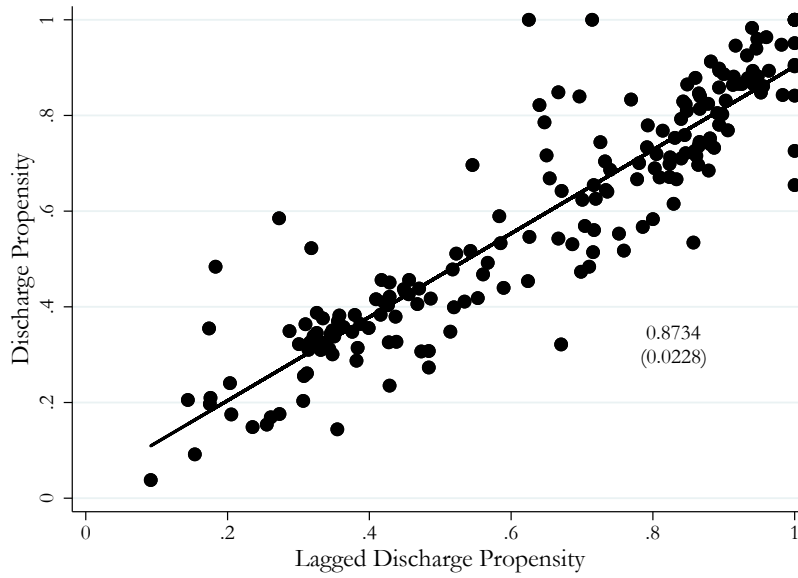


Figure 3. Persistence of Chapter 13 judge discharge rates

This figure shows the correlation between judge-specific Chapter 13 discharge rates in the current and lagged years. The sample consists of all first-time Chapter 13 filers between 1989 and 1995 in the offices that randomly assigned filings to judges and that allowed mortgage cramdown.

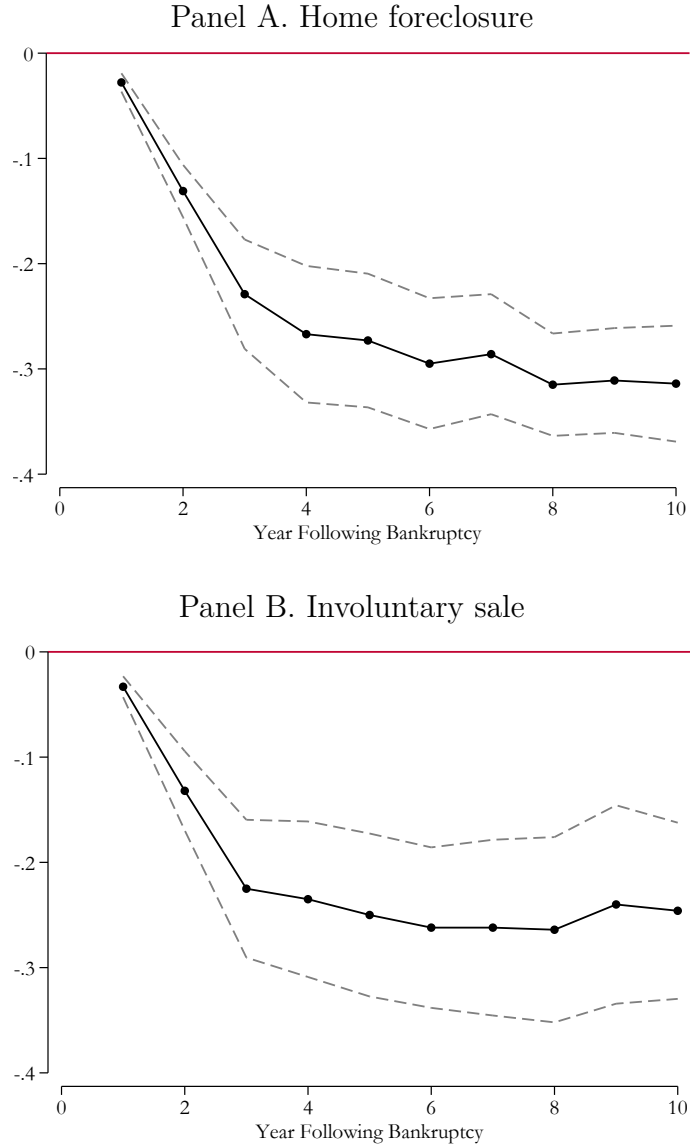


Figure 4. Home foreclosures and involuntary sales in cramdown courts
 These panels plot two-stage least squares estimates of the impact of Chapter 13 bankruptcy protection on home foreclosures and involuntary sales in courts that allowed mortgage cramdown between 1989 and May 1993. Home foreclosure is an indicator for a filer's home receiving a notice of default, transfer or sale or having been transferred to a real estate owned in or before the indicated year. Involuntary sale is an indicator for when a home short sale is registered in or before the indicated year. The dashed lines represent 95% confidence intervals from standard errors clustered at the office-by-judge level.

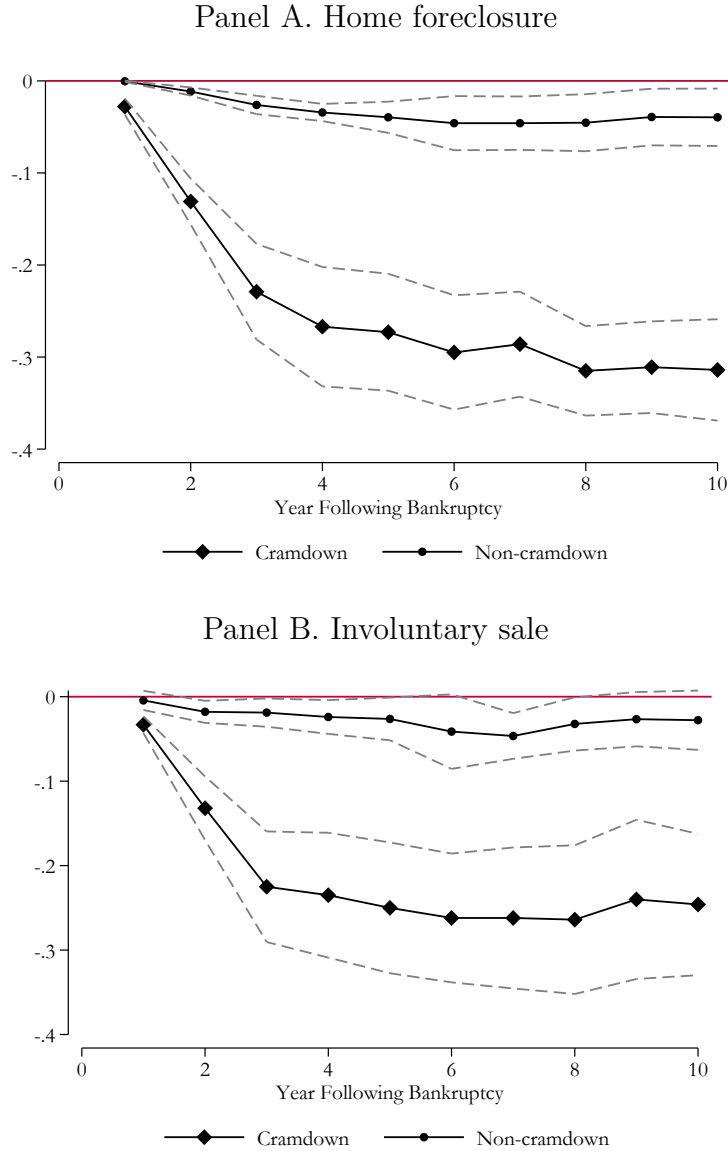


Figure 5. Cramdown versus non-cramdown courts

These panels plot the two-stage least squares estimates of the impact of Chapter 13 bankruptcy protection on home foreclosures and involuntary sales in cramdown and non-cramdown courts between 1989 and May 1993. Home foreclosure is an indicator for a filer's home receiving a notice of default, transfer or sale, or having been transferred to a real estate owned in or before the indicated year. Involuntary sale is an indicator for when a home short sale is registered in or before the indicated year. The dashed lines represent 95% confidence intervals from standard errors clustered at the office-by-judge level.

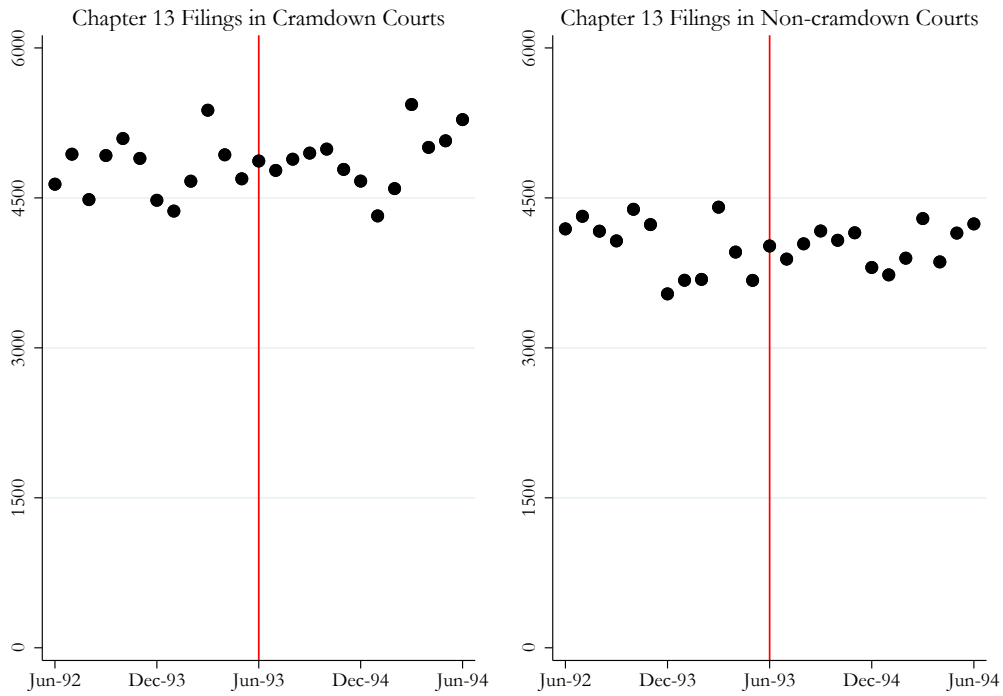


Figure 6. Time series of Chapter 13 bankruptcy filings around the Supreme Court decision in June 1993

These figures depict the monthly number of Chapter 13 bankruptcy filings before and after the Supreme Court disallowed cramdown in June 1993. The sample consists of all Chapter 13 bankruptcy filings from June 1992 through June 1994 for which we have docket data from PACER. Each dot in the panels represents the total count of Chapter 13 filings for that month.

Table 1. Summary statistics

This table reports summary statistics for the sample consisting of first-time Chapter 13 filers in the offices that randomly assign filings to judges between 1989 and 1995. Discharge is an indicator for receiving Chapter 13 bankruptcy protection. Age data come from the Lexis-Nexis (LN) public records and correspond to the individual's age at the time of filing. Gender and criminal filings data are from LN. Single-family property data are from Attom. ZIP-code-level pre-treatment characteristics are from the 1980 Centennial Census. Other local covariates are from the earliest year available. *Unemployment rate* data come from the Bureau of Labor Statistics and correspond to the unemployment rate at the district level in 1990. *SNAP share* data from Food and Nutrition Service and correspond to the percentage of the population participating in the Supplemental Nutrition Assistance Program (SNAP) at the district level in 1989. *GDP growth* data come from the Bureau of Economic Analysis and correspond to the state GDP growth in 1989.

	Mean (1)	SD (2)	25th (3)	50th (4)	75th (5)
Female	24.13%	42.76%	0	0	0
Age	42.16	11.01	34	41	49
Single-family	85.62%	35.08%	1	1	1
Criminal filings	1.10%	10.41%	0	0	0
Case discharge	60.80%	48.82%	0	1	1
Number of cases			36,655		
<i>Pre-treatment local covariates</i>					
Percentage white (ZIP code)	38.14%	35.33%	0%	31%	71%
Percentage single (ZIP code)	9.39%	9.36%	0%	7%	17%
Percentage college degree (ZIP code)	6.97%	8.37%	0%	4%	10%
Median income (ZIP code)	28,726	19,127	12,870	29,413	42,872
<i>Other local covariates</i>					
Chapter 7 share (district)	57.40%	25.70%	38.64%	61.54%	75.49%
Unemployment rate (district)	5.77%	1.16%	4.90%	5.54%	6.86%
SNAP share (district)	4.43%	2.12%	2.17%	4.06%	6.11%
GDP growth (state)	2.10%	2.08%	0.85%	1.85%	2.91%

Table 2. Randomization test

This table presents reduced-form estimates testing the random assignment of filings to judges in cramdown courts. The sample consists of all first-time Chapter 13 filers in the district courts that randomly assigned cases between 1989 and 1995. Cramdown courts are courts that allowed mortgage principal reduction between 1989 and May 1993. The estimates are from an OLS regression of judge leniency on the variables listed and office-by-month-of-filing fixed effects. Judge leniency, measured between 1989 and 1995, is 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 court in the same filing year. The p -value reported is for an F -test of the joint significance of the variables. Standard errors are clustered at the office-by-judge level. $*p < 0.1$; $**p < 0.05$; $***p < 0.01$.

	Leniency	
	(1)	(2)
Female	-0.00136 (0.00163)	-0.00127 (0.00165)
Age	8.23e-05 (7.79e-05)	8.86e-05 (7.74e-05)
Criminal filings	-0.00424 (0.00301)	-0.00416 (0.00302)
Single-family	-0.00108 (0.00333)	-0.00116 (0.00334)
Percentage white (ZIP code)		-0.00977 (0.00934)
Percentage single (ZIP code)		-0.0773 (0.0705)
Percentage college degree (ZIP code)		0.0554 (0.0823)
ln (Median income) (ZIP code)		0.000719 (0.000759)
Joint F-test	0.4769	0.2641
Office x Month FE	Y	Y
Observations	23,240	23,240
R -squared	0.047	0.048

Table 3. First stage

This table reports the first-stage estimates in cramdown courts. The sample consists of all first-time Chapter 13 filers in the district courts that randomly assigned cases between 1989 and 1995. Cramdown courts are courts that allowed mortgage principal reduction between 1989 and May 1993. Judge leniency, measured between 1989 and 1995, is 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 court in the same filing year. All regressions include office-by-month-of-filing fixed effects. Standard errors are clustered at the office-by-judge level. $*p < 0.1$; $**p < 0.05$; $***p < 0.01$.

	Discharge		
	(1)	(2)	(3)
Leniency	0.862*** (0.0292)	0.861*** (0.0294)	0.859*** (0.0294)
Female		-0.0393*** (0.00781)	-0.0381*** (0.00763)
Age		0.00105*** (0.000354)	0.00106*** (0.000352)
Single-family		-0.0122 (0.0125)	-0.0129 (0.0126)
Criminal filings		-0.0648 (0.0431)	-0.0646 (0.0431)
Percentage white (ZIP code)			0.0609** (0.0292)
Percentage single (ZIP code)			-0.348*** (0.122)
Percentage college degree (ZIP code)			-0.0191 (0.117)
ln (Median income) (ZIP code)			0.00267 (0.00263)
Office x Month FE	Y	Y	Y
Observations	23,240	23,240	23,240
<i>R</i> -squared	0.211	0.212	0.213

Table 4. The effect of Chapter 13 in cramdown courts on homeowners' distress events
This table reports two-stage least squares estimates of the impact of Chapter 13 bankruptcy protection on homeowners' distress events in courts that allowed mortgage cramdown. The sample consists of all first-time Chapter 13 filers in the district courts that randomly assigned cases. Cramdown courts are courts that allowed mortgage principal reduction between 1989 and May 1993. Column (1) reports estimates for the three-year post-filing outcomes for the period between 1989 and May 1993. Columns (2) to (4) present the estimates for the three-year post-filing outcomes for alternative sample periods. *Post* is an indicator variable for filings after June 1st, 1993. *Foreclosure* is an indicator variable for a filer's home receiving a notice of default, transfer or sale, or having been transferred to a real estate owned in or before three years post-filing. *Involuntary sale* is an indicator variable for when a homeowner sells their property for less than the amount due on the mortgage in or before three years post-filing. Controls include age, gender, single-property, and criminal filings indicators, and ZIP-code-level controls are the percentage of white population, percentage of single individuals, percentage of individuals with a college degree, and median log income. All regressions include office-by-month-of-filing fixed effects. Standard errors are clustered at the office-by-judge level. * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

Panel A				
		Foreclosure		
	(1)	(2)	(3)	(4)
Discharge	-0.263*** (0.0324)	-0.268*** (0.0314)	-0.328*** (0.0422)	-0.288*** (0.0777)
Discharge x post		0.166*** (0.0378)	0.226*** (0.0479)	0.255*** (0.0869)
Sample period	1989–May 1993	1989–1995	1991–1995	1992–1994
Control group mean	0.238	0.169	0.169	0.167
Controls	Y	Y	Y	Y
Office x Month FE	Y	Y	Y	Y
Observations	6,100	23,240	22,232	12,389
<i>R</i> -squared	0.090	0.065	0.060	0.047
Panel B				
		Involuntary sale		
	(1)	(2)	(3)	(4)
Discharge	-0.244*** (0.0386)	-0.236*** (0.0362)	-0.252*** (0.0346)	-0.158*** (0.0469)
Discharge x post		0.215*** (0.0369)	0.232*** (0.0346)	0.149*** (0.0462)
Sample period	1989–May 1993	1989–1995	1991–1995	1992–1994
Control group mean	0.181	0.082	0.075	0.074
Controls	Y	Y	Y	Y
Office x Month FE	Y	Y	Y	Y
Observations	6,100	23,240	22,232	12,389
<i>R</i> -squared	0.048	0.028	0.019	0.032

Table 5. Differential effect of Chapter 13 in cramdown courts based on house price appreciation

This table reports two-stage least squares estimates of the differential effect based on house price appreciation of the impact of Chapter 13 bankruptcy protection on homeowners' distress events in courts that allowed mortgage cramdown. Sample periods, outcome variables, and controls are analogous to those in Table 4. We construct population-weighted house price indexes at the district court level. Then, we estimate each bankruptcy district's previous five-year returns and classify filers annually into two groups: below- and above-median appreciation. *Below* is an indicator variable for a case being filed in a district court with below median home price appreciation. Standard errors are clustered at the office-by-judge level. * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

Panel A				
	Foreclosure			
	(1)	(2)	(3)	(4)
Discharge	-0.0710** (0.0329)	-0.0724** (0.0325)	-0.0923* (0.0461)	-0.0821* (0.0445)
Discharge x below	-0.236*** (0.0388)	-0.234*** (0.0385)	-0.292*** (0.0577)	-0.326*** (0.104)
Discharge x post		-0.0277 (0.0335)	-0.00785 (0.0448)	-0.0205 (0.0490)
Discharge x below x post		0.231*** (0.0460)	0.289*** (0.0630)	0.425*** (0.115)
Sample period	1989–May 1993	1989–1995	1991–1995	1992–1994
Controls	Y	Y	Y	Y
Office x Month FE	Y	Y	Y	Y
Observations	6,100	23,240	22,232	12,389
<i>R</i> -squared	0.103	0.070	0.066	0.044
Panel B				
	Involuntary sale			
	(1)	(2)	(3)	(4)
Discharge	-0.0182 (0.0124)	-0.0188 (0.0128)	-0.0161 (0.0169)	-0.0187 (0.0193)
Discharge x below	-0.260*** (0.0232)	-0.259*** (0.0239)	-0.291*** (0.0199)	-0.215*** (0.0471)
Discharge x post		-0.0228 (0.0190)	-0.0254 (0.0218)	-0.00454 (0.0257)
Discharge x below x post		0.287*** (0.0301)	0.319*** (0.0269)	0.234*** (0.0494)
Sample period	1989–May 1993	1989–1995	1991–1995	1992–1994
Controls	Y	Y	Y	Y
Office x Month FE	Y	Y	Y	Y
Observations	6,100	23,240	22,232	12,389
<i>R</i> -squared	0.068	0.035	0.026	0.041

Table 6. The effect of Chapter 13 in cramdown courts by filer characteristics

This table reports two-stage least squares estimates of the impact of Chapter 13 bankruptcy protection on homeowners' distress events in courts that allowed mortgage cramdown by filer characteristics. The table reports estimates for the three-year post-filing outcomes with controls. *Characteristic* refers to *Female* for Columns (1) and (2), to *Age<40* for Columns (3) and (4), and to *Single-family* for Columns (5) and (6). *Female*, *Age<40*, and *Single-family* are included in the respective columns, but estimates are not reported for expositional purposes. The sample period in Columns (1), (3), and (5) is from 1989 to May 1993. The period in Columns (2), (4), and (6) is from 1989 to 1995. Outcome variables and controls are analogous to those in Table 4. All regressions include office-by-month-of-filing fixed effects. Standard errors are clustered at the office-by-judge level. * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

Panel A						
Characteristic	Foreclosure					
	Female		Age<40		Single-family	
	(1)	(2)	(3)	(4)	(5)	(6)
Discharge	-0.244*** (0.0302)	-0.242*** (0.0303)	-0.264*** (0.0451)	-0.262*** (0.0449)	-0.183*** (0.0636)	-0.181*** (0.0630)
Discharge x characteristic	-0.116 (0.130)	-0.117 (0.129)	9.80e-05 (0.0604)	-0.00166 (0.0600)	-0.0929 (0.0852)	-0.0932 (0.0831)
Discharge x post		0.120*** (0.0374)		0.151*** (0.0535)		0.137* (0.0695)
Discharge x characteristic x post		0.199 (0.142)		0.0299 (0.0720)		0.0196 (0.0953)
Controls	Y	Y	Y	Y	Y	Y
Office x Month FE	Y	Y	Y	Y	Y	Y
Observations	6,100	23,240	6,100	23,240	5,641	21,077
R-squared	0.084	0.062	0.090	0.065	0.091	0.069
Panel B						
Characteristic	Involuntary sale					
	Female		Age<40		Single-family	
	(1)	(2)	(3)	(4)	(5)	(6)
Discharge	-0.212*** (0.0352)	-0.210*** (0.0358)	-0.199*** (0.0373)	-0.197*** (0.0372)	-0.149** (0.0720)	-0.146** (0.0686)
Discharge x characteristic	-0.209** (0.0974)	-0.210** (0.0971)	-0.0883* (0.0496)	-0.0902* (0.0489)	-0.109 (0.107)	-0.111 (0.103)
Discharge x post		0.176*** (0.0369)		0.193*** (0.0397)		0.156** (0.0658)
Discharge x characteristic x post		0.260** (0.103)		0.0535 (0.0555)		0.0738 (0.0997)
Controls	Y	Y	Y	Y	Y	Y
Office x Month FE	Y	Y	Y	Y	Y	Y
Observations	6,100	23,240	6,100	23,240	5,641	21,077
R-squared	0.021	0.013	0.046	0.025	0.046	0.028

Table 7. The effect of Chapter 13 on foreclosure based on states' recourse status, homestead exemption generosity, and wage garnishment rates

This table reports two-stage least squares estimates of the impact of Chapter 13 bankruptcy protection on foreclosures in courts that allowed mortgage cramdown based on a state's recourse status, homestead exemption generosity, and wage garnishment rates. The sample consists of all first-time Chapter 13 filers in district courts that randomly assigned cases between 1989 and 1995. *Recourse* is an indicator for a case being filed in a recourse state. The *above-median homestead exemption* indicator variable is computed using 1996 homestead exemptions. The *above-median wage garnishment* indicator variable is calculated using the statutory maximum amount per month that can be garnished for individuals whose incomes are just above the subsistence amount. The sample period in Columns (1), (3), and (5) is from 1989 to May 1993. The period in Columns (2), (4), and (6) is from 1989 to 1995. Controls are analogous to those in Table 4. All regressions include office-by-month-of-filing fixed effects. Standard errors are clustered at office-by-judge level. * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

	Foreclosure					
	Recourse state		Above-median homestead exemption		Above-median wage garnishment	
	(1)	(2)	(3)	(4)	(5)	(6)
Discharge	-0.286*** (0.0253)	-0.286*** (0.0248)	-0.263*** (0.0323)	-0.262*** (0.0319)	-0.192** (0.0825)	-0.189** (0.0817)
Discharge x characteristic	0.0966 (0.0999)	0.0964 (0.101)	-0.0854 (0.174)	-0.0888 (0.174)	-0.0925 (0.0849)	-0.0943 (0.0847)
Discharge x post		0.0803 (0.0843)		0.162*** (0.0394)		0.0732 (0.0932)
Discharge x characteristic x post		0.0477 (0.130)		-0.209 (0.145)		0.128 (0.0970)
Controls	Y	Y	Y	Y	Y	Y
Office x Month FE	Y	Y	Y	Y	Y	Y
Observations	6,100	23,240	6,100	23,240	6,100	23,240
R-squared	0.094	0.077	0.089	0.067	0.102	0.073

Table 8. The effect of Chapter 13 in cramdown courts on other outcomes

This table reports two-stage least squares estimates of the impact of Chapter 13 bankruptcy protection on other filers' outcomes in courts that allowed mortgage cramdown. The sample consists of all first-time Chapter 13 filers in district courts that randomly assigned cases between 1989 and 1995. Cramdown courts are courts that allowed mortgage principal reduction between 1989 and May 1993. *Post* is an indicator variable for filings from June 1st, 1993, onward. *New address* is an indicator variable for the filer having at least one change of the principal address in or before three-year post-filing. *Moving to better ZIP* is an indicator variable for the filer moving for the first time post-filing to a ZIP code with a median income higher than the ZIP code at the time of bankruptcy filing. *Voluntary sale* is an indicator variable for a property sale different from a home short sale in or before three-year post-filing. *Criminal filings* is an indicator variable for the filer being charged with a misdemeanor or a felony in or before three-year post-filing. Controls are analogous to those in Table 4. All regressions include office-by-month-of-filing fixed effects. Standard errors are clustered at the office-by-judge level. * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

	New address (1)	Moving to better zip (2)	Voluntary sale (3)	Criminal filings (4)
Discharge	-0.203*** (0.0476)	0.0550** (0.0261)	-0.0892*** (0.0332)	-0.00943* (0.00510)
Discharge x post	-0.0433 (0.0578)	-0.0490 (0.0428)	0.0721* (0.0364)	0.00264 (0.00618)
Sample period		1989–1995		
Controls	Y	Y	Y	Y
Office x Month FE	Y	Y	Y	Y
Observations	23,240	23,240	23,240	23,240
<i>R</i> -squared	0.023	0.002	0.005	0.005

Table 9. The effect of Chapter 13 in cramdown courts on mortgage refinancing
This table reports two-stage least squares estimates of the impact of Chapter 13 bankruptcy protection on mortgage refinancing in courts that allowed mortgage cramdown. The sample consists of all first-time Chapter 13 filers between 1989 and 1995 in district courts that randomly assigned cases between 1989 and 1995. Cramdown courts are courts that allowed mortgage principal reduction between 1989 and May 1993. *Post* is an indicator variable for filings from June 1st, 1993, onward. *Refinancing* is an indicator variable for the filer refinancing a mortgage in or before the indicated year. Controls are analogous to those in Table 4. All regressions include office-by-month-of-filing fixed effects. Standard errors are clustered at the office-by-judge level. * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

	Refinancing			
	Year 3 (1)	Year 5 (2)	Year 7 (3)	Year 10 (4)
Discharge	0.00263 (0.0162)	0.0713*** (0.0181)	0.207*** (0.0295)	0.323*** (0.0305)
Discharge x post	-0.0410 (0.0263)	-0.0868* (0.0484)	-0.104* (0.0597)	-0.132** (0.0626)
Sample period		1989–1995		
Controls	Y	Y	Y	Y
Office x Month FE	Y	Y	Y	Y
Observations	23,240	23,240	23,240	23,240
<i>R</i> -squared	0.001	0.003	0.033	0.052

Table 10. Cramdown and non-cramdown in Chapter 13 bankruptcy

This table reports two-stage least squares estimates of the impact of Chapter 13 bankruptcy protection on homeowners' distress events in courts that did and did not allow cramdown. The sample consists of all first-time Chapter 13 filers in the district courts that randomly assigned cases between 1989 and 1995. *Cramdown* is an indicator variable for a case being filed in a court that allowed cramdown between 1989 and May 1993. Sample periods, outcome variables, and controls are analogous to those in Table 4. All regressions include office-by-month-of-filing fixed effects. Standard errors are clustered at the office-by-judge level. * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

Panel A				
	Foreclosure			
	(1)	(2)	(3)	(4)
Discharge	-0.0343*** (0.00476)	-0.0344*** (0.00465)	-0.0409*** (0.00294)	-0.0758*** (0.00891)
Discharge x cramdown	-0.233*** (0.0330)	-0.232*** (0.0324)	-0.286*** (0.0431)	-0.207*** (0.0785)
Discharge x post		0.0186** (0.00788)	0.0251*** (0.00606)	0.0607*** (0.00617)
Discharge x cramdown x post		0.146*** (0.0390)	0.199*** (0.0489)	0.189** (0.0874)
Sample period	1989–May 1993	1989–1995	1991–1995	1992–1994
Controls	Y	Y	Y	Y
Office x Month FE	Y	Y	Y	Y
Observations	11,333	36,655	34,643	20,358
<i>R</i> -squared	0.084	0.061	0.057	0.041
Panel B				
	Involuntary sale			
	(1)	(2)	(3)	(4)
Discharge	-0.0240** (0.0102)	-0.0239** (0.0102)	-0.0280*** (0.00247)	-0.0516*** (0.00663)
Discharge x cramdown	-0.211*** (0.0366)	-0.210*** (0.0362)	-0.223*** (0.0354)	-0.102** (0.0468)
Discharge x post		0.00555 (0.0141)	0.00955 (0.00759)	0.0167 (0.0179)
Discharge x cramdown x post		0.209*** (0.0380)	0.221*** (0.0362)	0.128** (0.0490)
Sample period	1989–May 1993	1989–1995	1991–1995	1992–1994
Controls	Y	Y	Y	Y
Office x Month FE	Y	Y	Y	Y
Observations	11,333	36,655	34,643	20,358
<i>R</i> -squared	0.044	0.024	0.016	0.023

Table 11. Covariates around the Supreme Court decision

This table reports the difference-in-differences estimates of the effect of cramdown on filers' and districts' observable covariates. *Cramdown* is an indicator for a case being filed in a court that allowed cramdown between 1989 and May 1993. *Post* is an indicator variable for filings from June 1st, 1993, onward. Panel A presents outcomes at the filer level. Columns (1) to (4) in Panel B present outcomes at the district (or state) level. GDP growth is measured at the state level. Outcomes variables in Panel B are calculated annually, except for *Chapter 7 share*, which is monthly. *Unemployment rate* data come from the Bureau of Labor Statistics and correspond to the unemployment rate at the district level. *SNAP share* data from Food and Nutrition Service and correspond to the percentage of the population participating in the Supplemental Nutrition Assistance Program (SNAP) at the district level. *GDP growth* data come from the Bureau of Economic Analysis and correspond to the state GDP growth. Sample periods and Averages are between 1989 and 1995 (excluding 1993), except for *Unemployment rate*, measured between 1990 and 1995. All regressions include district and month (or year) fixed effects. The table reports ordinary wild bootstrap *p*-values in brackets.

Panel A				
	Age	Female	Criminal filings	Single-family
	(1)	(2)	(3)	(4)
Cramdown x post	1.113 [0.2312]	0.0110 [0.5846]	-0.00287 [0.2643]	-0.0264 [0.7307]
Average	42.159	0.241	0.011	0.856
District FE	Y	Y	Y	Y
Month FE	Y	Y	Y	Y
Observations	36,658	36,658	36,658	36,658
<i>R</i> -squared	0.041	0.018	0.119	0.081

Panel B					
	Chapter 7 share	House price change	Unemployment rate	SNAP share	GDP growth
	(1)	(2)	(3)	(4)	(5)
Cramdown x post	0.0439 [0.5185]	-0.022 [0.124]	0.00304 [0.3403]	0.00126 [0.7618]	0.00304 [0.6246]
Average	0.564	0.036	0.063	0.049	0.026
District or State FE	Y	Y	Y	Y	Y
Month or Year FE	Y	Y	Y	Y	Y
Observations	1,794	132	110	118	108
<i>R</i> -squared	0.587	0.333	0.834	0.938	0.570

Internet Appendix

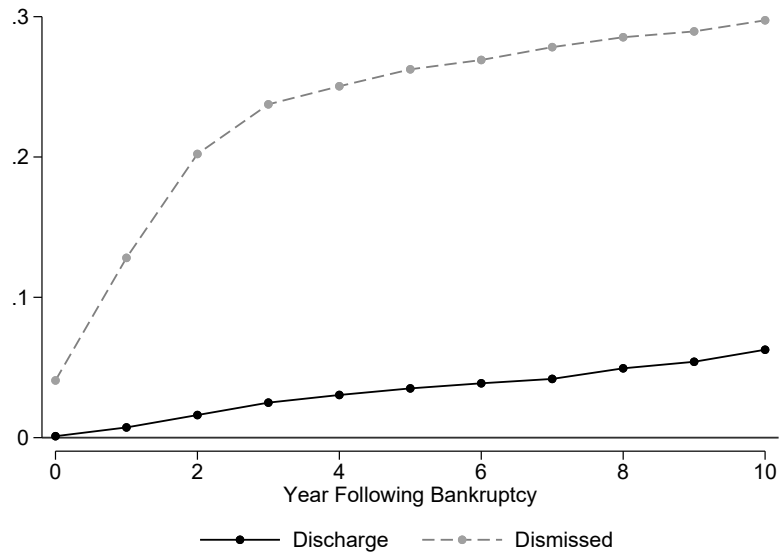
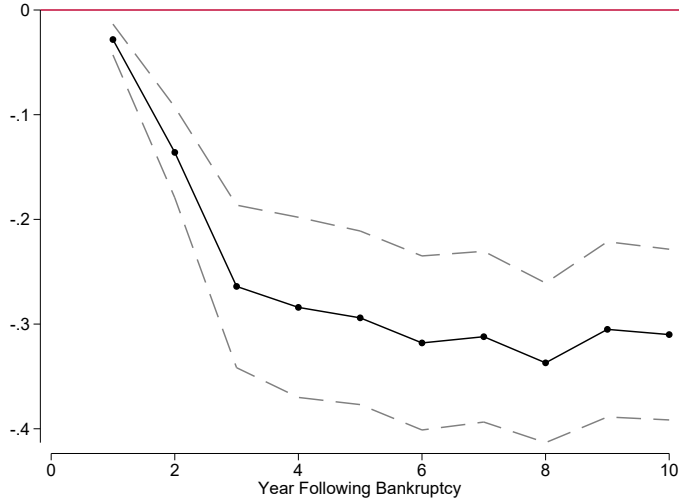


Figure IA.1. Home foreclosure of discharged and dismissed filers in cramdown courts
These figure plots the average home foreclosure rate for granted and dismissed Chapter 13 bankruptcy filers in courts in which cramdown was allowed between 1989 and May 1993. Home foreclosure is an indicator for a filer's home receiving a notice of default, transfer or sale, or having been transferred to a real estate owned in or before the indicated year.

Panel A. Cramdown courts



Panel B. Non-cramdown courts

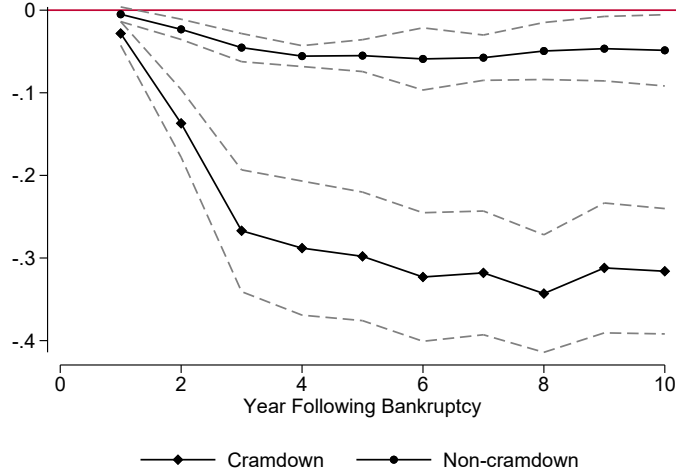


Figure IA.2. Any distress event in cramdown and non-cramdown courts
 These figures plot two-stage least squares estimates of the impact of Chapter 13 bankruptcy protection on any distress event in cramdown and non-cramdown courts between 1989 and May 1993. Any distress event is similar to the foreclosure indicator described in the Figure 4 variable plus other events, such as real-estate-owned liquidation (REO) in or before the indicated year. Panel A depicts the estimates for any distress events in courts that allowed cramdown in Chapter 13, whereas Panel B presents the estimates for non-cramdown courts. The dashed lines represent 95% confidence intervals from standard errors clustered at the office-by-judge level.

Table IA.1. Cramdown and non-cramdown samples

This table reports summary statistics for the cramdown and non-cramdown samples. The sample consists of first-time Chapter 13 filers between 1989 and 1995 in the offices that randomly assign filings to judges. The cramdown sample comprises the district courts that allowed mortgage cramdown in Chapter 13 between 1989 and May 1993. The non-cramdown sample includes all the courts that did not allow cramdown in the same period. *Discharge* is an indicator for receiving Chapter 13 bankruptcy protection. Age data come from the Lexis-Nexis public records and correspond to the individual's age at the time of filing. Gender and criminal filings data are from Lexis-Nexis. Single-family property data are from Attom. ZIP-code-level pre-treatment characteristics are from the 1980 Centennial Census. Other local covariates are from the earliest year available. *Unemployment rate* data come from the Bureau of Labor Statistics and correspond to the unemployment rate at the district level in 1990. *SNAP share* data from Food and Nutrition Service and correspond to the percentage of the population participating in the Supplemental Nutrition Assistance Program (SNAP) at the district level in 1989. *GDP growth* data come from the Bureau of Economic Analysis and correspond to the state GDP growth in 1989.

	Cramdown		Non-cramdown	
	Mean	SD	Mean	SD
	(1)	(2)	(3)	(4)
Female	22.68%	41.84%	26.66%	44.25%
Age	43.08	10.93	40.58	10.97
Single-family	83.25%	37.34%	89.73%	30.35%
Criminal filings	0.87%	9.27%	1.49%	12.13%
Case discharge	58.18%	49.32%	65.26%	47.62%
Number of cases	23,240		13,415	
<i>Pre-treatment local covariates</i>				
Percentage white (ZIP code)	37.74%	35.75%	38.82%	34.59%
Percentage single (ZIP code)	9.63%	9.69%	9.00%	8.75%
Percentage college degree (ZIP code)	7.39%	8.90%	6.28%	7.33%
Median income (ZIP code)	22,788.48	21,791.70	20,947.76	18,603.09
<i>Other local covariates</i>				
Chapter 7 share (district)	56.30%	19.62%	57.86%	27.85%
Unemployment rate (district)	5.92%	1.20%	5.52%	1.37%
SNAP share (district)	4.43%	2.12%	3.35%	1.40%
GDP growth (state)	2.51%	2.27%	1.30%	1.68%

Table IA.2. Complier characteristics in cramdown courts

This table presents the characteristics of compliers for filers in courts that allowed cramdown. The sample consists of all first-time Chapter 13 filers between 1989 and 1995 in the district courts that randomly assigned cases. Cramdown courts are courts that allowed mortgage principal reduction between 1989 and May 1993. Column (1) reports the distribution of the sample by gender and age, $P(X = x)$. Column (2) shows the first-stage estimates for each gender and age group. Column (3) reports the distribution of compliers by gender and age, $P(X = x|I_1 > I_0)$, calculated as the first-stage estimate for the gender minus age group times the sample share, divided by the overall first-stage estimate. Column (4) shows the relative likelihood of a filer belonging to a particular gender and age group, in the complier group compared with the sample.

	$P(X=x)$ (1)	First Stage (2)	$P(X=x / I_1 > I_0)$ (3)	$P(X=x / I_1 > I_0) / P(X=x)$ (4)
Men				
23-40	0.327	0.872	0.331	1.012
40 and up	0.447	0.850	0.441	0.986
Women				
23-40	0.080	0.911	0.085	1.057
40 and up	0.146	0.807	0.136	0.936
Overall	1.000	0.862	1.000	1.000

Table IA.3. The effect of Chapter 13 in cramdown courts on homeowners' distress events when pre-treatment covariates are not included

This table reports two-stage least squares estimates of the impact of Chapter 13 bankruptcy protection on homeowners' distress events in courts that allowed mortgage cramdown. Outcome variables and sample periods are analogous to those in Table 4. All regressions include office-by-month-of-filing fixed effects. Standard errors are clustered at the office-by-judge level. * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

Panel A				
		Foreclosure		
	(1)	(2)	(3)	(4)
Discharge	-0.256*** (0.0349)	-0.264*** (0.0343)	-0.323*** (0.0446)	-0.274*** (0.0792)
Discharge x post		0.161*** (0.0399)	0.221*** (0.0497)	0.241*** (0.0880)
Sample period	1989–May 1993	1989–1995	1991–1995	1992–1994
Controls	N	N	N	N
Office x Month FE	Y	Y	Y	Y
Observations	6,100	23,240	22,232	12,389
<i>R</i> -squared	0.088	0.065	0.060	0.047

Panel B				
		Involuntary sale		
	(1)	(2)	(3)	(4)
Discharge	-0.236*** (0.0419)	-0.232*** (0.0390)	-0.247*** (0.0370)	-0.147*** (0.0467)
Discharge x post		0.211*** (0.0396)	0.227*** (0.0371)	0.138*** (0.0460)
Sample period	1989–May 1993	1989–1995	1991–1995	1992–1994
Controls	N	N	N	N
Office x Month FE	Y	Y	Y	Y
Observations	6,100	23,240	22,232	12,389
<i>R</i> -squared	0.047	0.028	0.019	0.031

Table IA.4. The effect of Chapter 13 in cramdown courts on homeowners' any distress events
This table reports two-stage least squares estimates of the impact of Chapter 13 bankruptcy protection on homeowners' any distress events in courts that allowed mortgage cramdown. *Any distress event* is similar to the *Foreclosure* indicator described in Table 4 plus other events, such as real-estate-owned liquidation (REO) in or before the indicated year. Sample periods, outcome variables, and controls are analogous to those in Tables 4 and 10. All regressions include office-by-month-of-filing fixed effects. Standard errors are clustered at the office-by-judge level. * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

Panel A				
	Any distress event			
	(1)	(2)	(3)	(4)
Discharge	-0.291*** (0.0408)	-0.290*** (0.0396)	-0.319*** (0.0389)	-0.205*** (0.0511)
Discharge x post		0.173*** (0.0444)	0.202*** (0.0446)	0.158** (0.0665)
Sample period	1989–May 1993	1989–1995	1991–1995	1992–1994
Controls	Y	Y	Y	Y
Office x Month FE	Y	Y	Y	Y
Observations	6,100	23,240	22,232	12,389
R-squared	0.105	0.071	0.070	0.059
Panel B				
	Any distress event			
	(1)	(2)	(3)	(4)
Discharge	-0.0238** (0.0101)	-0.0239** (0.0100)	-0.0281*** (0.00263)	-0.0514*** (0.00700)
Discharge x cramdown	-0.265*** (0.0427)	-0.264*** (0.0419)	-0.289*** (0.0404)	-0.147*** (0.0525)
Discharge x post		-0.00135 (0.0146)	0.00272 (0.00951)	0.0164 (0.0179)
Discharge x cramdown x post		0.173*** (0.0475)	0.197*** (0.0466)	0.135* (0.0692)
Sample period	1989–May 1993	1989–1995	1991–1995	1992–1994
Controls	Y	Y	Y	Y
Office x Month FE	Y	Y	Y	Y
Observations	11,333	36,655	34,643	20,358
R-squared	0.097	0.065	0.064	0.052

Table IA.5. Differential effect of Chapter 13 in cramdown courts based on house price appreciation without a rolling window classification after June 1993

This table reports two-stage least squares estimates of the differential effect based on house price appreciation analogous to Table 5 with an alternative house price appreciation classification methodology. We construct population-weighted house price indexes at the district court level. Between 1989 and May 1993, we estimate each bankruptcy district's previous five-year returns and classify filers annually into two groups: below and above the median appreciation. For the period after June 1993, we preserve the same classification as in May 1993. *Below* is an indicator for a case being filed in a district court with below median house price appreciation. Standard errors are clustered at the office-by-judge level. * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

Panel A				
	Foreclosure			
	(1)	(2)	(3)	(4)
Discharge	-0.0710** (0.0329)	-0.0726** (0.0325)	-0.0925** (0.0461)	-0.0746 (0.0457)
Discharge x below	-0.236*** (0.0388)	-0.233*** (0.0385)	-0.292*** (0.0577)	-0.338*** (0.110)
Discharge x post		-0.0387 (0.0360)	-0.0188 (0.0464)	0.0522 (0.0489)
Discharge x below x post		0.270*** (0.0465)	0.327*** (0.0629)	0.278* (0.164)
Sample period	1989–May 1993	1989–1995	1991–1995	1992–1994
Controls	Y	Y	Y	Y
Office x Month FE	Y	Y	Y	Y
Observations	6,100	23,240	22,232	12,389
<i>R</i> -squared	0.103	0.068	0.064	0.057
Panel B				
	Involuntary sale			
	(1)	(2)	(3)	(4)
Discharge	-0.0182 (0.0124)	-0.0185 (0.0127)	-0.0159 (0.0168)	-0.0186 (0.0193)
Discharge x below	-0.260*** (0.0232)	-0.259*** (0.0239)	-0.291*** (0.0199)	-0.215*** (0.0471)
Discharge x post		0.00666 (0.0136)	0.00393 (0.0174)	0.00919 (0.0214)
Discharge x below x post		0.224*** (0.0269)	0.256*** (0.0282)	0.212*** (0.0617)
Sample period	1989–May 1993	1989–1995	1991–1995	1992–1994
Controls	Y	Y	Y	Y
Office x Month FE	Y	Y	Y	Y
Observations	6,100	23,240	22,232	12,389
<i>R</i> -squared	0.068	0.042	0.033	0.044

Table IA.6. The effect of Chapter 13 on foreclosure during recession

This table reports two-stage least squares estimates of the differential effect during the 1991 recession of the impact of Chapter 13 bankruptcy protection in courts that allowed mortgage cramdown on foreclosures. The sample consists of all first-time Chapter 13 filers between 1989 and May 1993 in district courts that randomly assigned cases. *Recession* is an indicator variable for a case being filed between July 1990 and March 1991, as classified by the NBER. Outcome variables and controls are analogous to those in Table 4. All regressions include office-by-month-of-filing fixed effects. Standard errors are clustered at office-by-judge level. $*p < 0.1$; $**p < 0.05$; $***p < 0.01$.

	Foreclosure (1)
Discharge	-0.262*** (0.0403)
Discharge x recession	-0.0245 (0.0722)
Sample period	1989–May 1993
Controls	Y
Office x Month FE	Y
Observations	6,100

Table IA.7. Cramdown and non-cramdown in Chapter 13 bankruptcy events when pre-treatment covariates are not included

This table reports two-stage least squares estimates of the impact of Chapter 13 bankruptcy protection on homeowners' distress events for courts that did and did not allow cramdown. Outcome variables and sample periods are analogous to those in Table 10. All regressions include office-by-month-of-filing fixed effects. Standard errors are clustered at the office-by-judge level. * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

Panel A				
	Foreclosure			
	(1)	(2)	(3)	(4)
Discharge	-0.0339*** (0.00497)	-0.0339*** (0.00493)	-0.0399*** (0.00253)	-0.0754*** (0.00819)
Discharge x cramdown	-0.230*** (0.0341)	-0.230*** (0.0339)	-0.283*** (0.0446)	-0.199** (0.0794)
Discharge x post		0.0183** (0.00800)	0.0243*** (0.00595)	0.0612*** (0.00603)
Discharge x cramdown x post		0.143*** (0.0401)	0.197*** (0.0500)	0.179** (0.0879)
Sample period	1989–May 1993	1989–1995	1991–1995	1992–1994
Controls	N	N	N	N
Office x Month FE	Y	Y	Y	Y
Observations	11,333	36,655	34,643	20,358
R -squared	0.084	0.061	0.057	0.041
Panel B				
	Involuntary sale			
	(1)	(2)	(3)	(4)
Discharge	-0.0234** (0.0104)	-0.0234** (0.0103)	-0.0271*** (0.00210)	-0.0513*** (0.00619)
Discharge x cramdown	-0.208*** (0.0378)	-0.208*** (0.0375)	-0.220*** (0.0370)	-0.0960** (0.0470)
Discharge x post		0.00521 (0.0142)	0.00888 (0.00751)	0.0169 (0.0177)
Discharge x cramdown x post		0.206*** (0.0392)	0.218*** (0.0378)	0.121** (0.0492)
Sample period	1989–May 1993	1989–1995	1991–1995	1992–1994
Controls	N	N	N	N
Office x Month FE	Y	Y	Y	Y
Observations	11,333	36,655	34,643	20,358
R -squared	0.044	0.019	0.016	0.023

Table IA.8. The effect of Chapter 13 bankruptcy protection in district courts that allowed cramdown following a circuit level decision

This table reports two-stage least squares estimates of the impact of Chapter 13 bankruptcy protection on homeowners' distress events in the sub-sample of courts that allowed mortgage cramdown following a circuit court decision. The sample consists of all first-time Chapter 13 filers in this sub-sample of district courts that randomly assigned cases. Outcome and control variables are analogous to those in Tables 4 and 10. All regressions include office-by-month-of-filing fixed effects. Standard errors are clustered at the office-by-judge level. * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

	Foreclosure		Involuntary sale	
	(1)	(2)	(3)	(4)
Discharge	-0.360*** (0.0398)	-0.0391*** (0.00279)	-0.295*** (0.0204)	-0.0266*** (0.00222)
Discharge x post	0.181** (0.0782)	0.0235*** (0.00594)	0.272*** (0.0207)	0.00895 (0.00763)
Discharge x cramdown		-0.321*** (0.0397)		-0.268*** (0.0204)
Discharge x cramdown x post		0.156** (0.0778)		0.261*** (0.0220)
Controls	Y	Y	Y	Y
Office x Month FE	Y	Y	Y	Y
Observations	12,056	25,471	12,056	25,471
<i>R</i> -squared	0.091	0.084	0.018	0.014

Table IA.9. The effect of Chapter 13 bankruptcy protection on states with cramdown courts and with both cramdown and non-cramdown courts

This table reports two-stage least squares estimates of the impact of Chapter 13 bankruptcy protection on homeowners' distress events within states with both cramdown and non-cramdown courts. The sample consists of all first-time Chapter 13 filers between 1989 and 1995 in district courts that randomly assigned cases. Only states that have cramdown courts or that have both cramdown and non-cramdown courts are included. Sample periods, outcome variables, and controls are analogous to those in Table 10. All regressions include office-by-month-of-filing fixed effects. Standard errors are clustered at the office-by-judge level. * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

Panel A			
	Foreclosure		
	(1)	(2)	(3)
Discharge	-0.0362*** (0.00532)	-0.0439*** (0.00180)	-0.0818*** (0.00217)
Discharge x cramdown	-0.232*** (0.0320)	-0.285*** (0.0423)	-0.193** (0.0793)
Discharge x post	0.0144** (0.00545)	0.0219*** (0.00122)	0.0484*** (0.00151)
Discharge x cramdown x post	0.151*** (0.0384)	0.204*** (0.0481)	0.192** (0.0880)
Sample period	1989–1995	1991–1995	1992–1994
Controls	Y	Y	Y
Office x Month FE	Y	Y	Y
Observations	25,084	23,395	13,029
<i>R</i> -squared	0.064	0.060	0.046
Panel B			
	Involuntary sale		
	(1)	(2)	(3)
Discharge	-0.0452** (0.0185)	-0.0403*** (0.00140)	-0.0574*** (0.00337)
Discharge x cramdown	-0.191*** (0.0353)	-0.205*** (0.0360)	-0.0993** (0.0463)
Discharge x post	0.0361* (0.0186)	0.0313*** (0.00104)	-0.0206*** (0.00578)
Discharge x cramdown x post	0.181*** (0.0359)	0.195*** (0.0354)	0.168*** (0.0464)
Sample period	1989–1995	1991–1995	1992–1994
Controls	Y	Y	Y
Office x Month FE	Y	Y	Y
Observations	25,084	23,395	13,029
<i>R</i> -squared	0.028	0.020	0.031