How Did the US Housing Slump Begin?
Role of the 2005 Bankruptcy Reform[1]

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Abstract

Most analyses of the recent Great Recession focus on the consequences of the dramatic slump in housing prices that started in the mid-2000s, without explaining what caused the reversal of housing price growth in the first place. We argue that the passage of the 2005 Bankruptcy Reform Act (BAPCPA) contributed significantly, by generating negative wealth effects for some homeowners, lowering prices of their homes, spreading to other homes via a process of contagion. Evidence consistent with this hypothesis is provided: changes in housing prices and mortgage interest rates at the MSA level following the reform were significantly correlated with BAPCPA-exposure.

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1 Introduction

Housing price growth in the US exhibited a dramatic reversal in the mid-2000s: after growing at an average annual rate of 11% over 2000-05, they fell at an average rate of 10% from mid-2006 till end-2008 (see Figures 1–3 and Mayer et al (2009)). Most analyses of the recent financial crisis in the US have focused on the consequences of this reversal: e.g., on rises in mortgage default rates (Mayer et al (2009)), which in turn were correlated with declines in income and home equity credit at the zip-code level (Mian and Sufi (2009, 2010)), in prices of mortgage-backed securities which subsequently undermined liquidity in the banking system (Diamond and Rajan (2009)). Each of these accounts highlight the powerful ways in which effects of housing price changes can get amplified and propagated throughout the rest of the economy, as hypothesized earlier by Stein (1995) or Kiyotaki and Moore (1997).

For these liquidity spillover effects to materialize, it is necessary to have an initial shock to house price growth. For example, Mian and Sufi (2009) argue that "*house prices, ... are likely jointly driven by unobservable permanent income shocks*" and Diamond and Rajan (2009) after asking "*why did the crisis first manifest itself in the United States?*" explain as follows:

> "Given the proximate causes of high bank holdings of mortgage-backed securities (MBS)...financed with a capital structure heavy on short-term debt, the crisis had a certain inevitability. As housing prices stopped rising, and indeed started falling, mortgage defaults started increasing. MBS fell in value, became more difficult to value, and their prices became more volatile. They became harder to borrow against, even short term. Banks became illiquid, the canonical example being Bear Sterns..." (Diamond and Rajan (2009))

Yet none of these authors explain *why* housing prices 'stopped rising, and indeed started falling', or what the nature of the 'unobservable permanent income shocks' were that may have caused such a dramatic slump in housing prices.

In this paper we argue that the 2005 Bankruptcy Reform Act (Bankruptcy Abuse Prevention and Consumer Protection Act (BAPCPA)) played an important role in triggering the reversal. This Act limited scope for households and small entrepreneurs with high incomes to file for bankruptcy under Chapter 7. Those failing a means test (e.g., those with income above the state median income) would no longer be allowed to file under Chapter 7 and would have to file under Chapter 13 instead. Many states have high exemption limits for homestead and other assets for Chapter 7 filings, with no obligation to repay from future incomes, unlike Chapter 13 which requires significant portions of future incomes to be repaid to creditors. Accordingly most personal bankruptcy filings have traditionally been under Chapter 7. Closing this channel for a significant portion of households accentuated their financial distress; bankruptcy filings fell overall (as well as the proportion of all filings that were Chapter 7 filings) following a sharp spike six months immediately preceding the passage of BAPCPA (see Figure 1). We hypothesize that the resulting negative effect on wealth and liquidity positions of households in financial distress reduced demand for owner-occupied housing, by shrinking their ability to meet current mortgage payments or to make downpayments on new home purchases. This put downward pressure on house prices, setting in motion a process of contagion that eventually lead to sharp declines in housing prices across the board.

Section 2 provides a simple theoretical model with moral hazard in credit markets that illustrates the hypothesis. Penalties for default on unsecured loans are defined by bankruptcy law, while for default on home mortgage loans involve loss of the home. A rise in default penalties on unsecured loans reduces default on such loans and thereby the net wealth of affected borrowers. This reduces demand for housing, inducing a drop in price of houses. In turn this reduces home equity of others with home mortgages that were not directly affected by the bankruptcy reform provisions, raising default rates on their mortgages, causing a further drop in housing prices. A negative multiplier effect on house prices (and a corresponding) positive multiplier effect on home mortgage defaults is thus generated. It is accentuated by a rise in interest rates on new mortgages, owing to higher default rates anticipated by lenders, which further depresses housing demand.

The rest of the paper examines whether empirical evidence concerning housing prices at the MSA-level is consistent with this hypothesis. We measure exposure to BAPCPA by the homestead exemption limit for Chapter 7 filings prior to the passage of the Act, since the benefit lost by borrowers in distress as a result of the Act is the exemption they would have been entitled to had they been been allowed to file under Chapter 7. These exemption limits vary from state to state (see Table 2), and depend mainly on exemption limits prevailing in the early 20th century owing to strong inertia in the political process of reforming these limits (Hynes et al (2004)). We use both the absolute value of these exemption limits as well as relative to local housing values and incomes as alternative measures of BAPCPA exposure.

We measure housing prices in two ways: (i) a regional house price index (OFHEO) based on a weighted repeat transactions methodology similar to that used in the Case-Shiller index, available on a quarterly basis;[4] and (ii) prices of individual houses from the American Housing Survey, available for a longitudinal sample once every two years. Using these, we check the central prediction of

[4] We employ the MSA level data owing to differences in coverage: the Case-Shiller index does not cover 13 states and has incomplete information for another 29 states. See Leventis (2007) for a detailed discussion of the relative coverage of these two respective datasets.

the model: that regions with greater BAPCPA exposure experienced large declines in housing price, after controlling for year, MSA or house-specific dummies. This amounts to a difference-of-differences specification, which washes out the effect of economy-wide macro shocks, as well as cross-sectional variations in fixed regional or house characteristics.

We find a negative effect which is quantitatively (as well as statistically) significant. For the house price index, the following implications emerge. Going from a zero exemption limit state to a $100,000 exemption limit state (the third quartile of the distribution of exemption limits) was associated with a drop in the rate of growth of the regional price index by 1.85% per quarter, compared to an average drop of house price growth of approximately 1% per quarter. At the individual housing unit level, our estimates imply that going from a zero exemption limit state to a $100,000 exemption limit state was associated with a drop in the house price growth by 8% per year, compared to an observed average drop of 9%. Alternatively, a one standard deviation change in the exposure measure was associated with a 35% standard deviation change in rate of growth of the regional housing price index, and 13% standard deviation reduction in the growth rate of prices of individual homes.[5] We also find higher BAPCPA exposures were associated with higher interest rates on home mortgage loans. But the size of the effect is small, due in part to our inability to distinguish new from pre-existing mortgages.

We explore the robustness of these results with respect to controls for alternative explanations for the reversal of housing price growth. For instance, states with high BAPCPA exposure tended to have higher rates of price growth prior to the reform, raising the concern that our measured effects of BAPCPA exposure may be proxying for the size of a pre-2005 housing bubble. Alternatively, high BAPCPA exposure may have been correlated with levels of household indebtedness, which may have interacted with increases in the Fed interest rate in the mid-2000s to reduce house prices. We find that the statistical and quantitative magnitude of the BAPCPA exposure effect remains intact, despite controlling for pre-reform trends as well as average levels of price growth rates, and changes in the Fed interest rate interacted with measures of indebtedness or vulnerability to interest rate increases. It is also robust to controls for land supply elasticity, measures of new housing construction, housing vacancy rates and local unemployment (which happen to be correlated with state exemption limits and proportion of subprime mortgages, two determinants of BAPCPA exposure).

As explained above, as well as in the earlier work of Stein (1995) and Kiyotaki and Moore (1997), contagion effects arise through the price of houses, an asset which has both consumption value as well as used extensively as collateral. Since only those with incomes exceeding the state median income were affected by BAPCPA, we know which homeowners were directly (or initially) impacted. Using

[5] A weaker effect at the individual home level is to be expected, owing to greater volatility of prices at the individual unit level compared with a regional average.

these institutional details, we construct a measure of the extent to which a particular homeowner was directly affected and call this the *personal exposure (PE)* effect. We can also measure how *other* homeowners residing in the same location were affected which we call the *geographic exposure (GE)* effect of the reform.[6] Our results indicate that contagion effects were an important mechanism in how the reform impacted the housing market, and that they were more pronounced for homeowner groups that were financially more fragile (in the sense of having lower incomes, lower home equity, and higher indebtedness).[7]

Our paper is not the first to stress the possible role of the bankruptcy reform in the financial crisis. The first paper to investigate the effect of the BAPCPA reform in light of the financial crisis was Morgan, Iverson and Botsch (2008). Applying a similar identification strategy as employed in this paper to state level data, they find evidence that higher exposure to the BAPCPA reform caused an increase of home foreclosures for subprime borrowers but not for prime borrowers. They also find that delinquency rates on unsecured loans fell in high exemption states, which is consistent with results of our model (assuming the first order effects of the reform dominate). In a subsequent paper, Li, White and Zhu (2010) use a finer micro-level database to investigate similar hypotheses concerning foreclosure as Morgan *et al.* The former find evidence from a large sample of house mortgage data that BAPCPA raised default rates immediately (e.g., three or six months) following the reform compared with (three or six months) before.

These results complement ours, and differ insofar as they concern effects on default rates rather than house price growth rates. Our results help throw light on some puzzling results in their papers, wherein they find significant effects on default rates of subprime mortgages, despite the absence of clear results concerning the extent to which subprime borrowers were directly exposed to the BAPCPA reform. Our results concerning GE effects provides an explanation of this puzzle. Even if subprime borrowers were not personally exposed, the spillover effects from exposure of others in their neighborhoods would cause their housing prices to decline substantially, thus inducing higher defaults. Restricting our sample to subprime borrowers, we find the effects of BAPCPA on their housing prices were comparable to those for prime borrowers. At the same time the impact on mortgage interest rates of subprime borrowers was substantially larger, consistent with the greater financial fragility of this group.

[6]Note that the reform also reflects a second type of spillover effect because it affects all house owners at the same time. This leads to greater correlation of risk after the reform than before the reform and is in the spirit of Shleifer and Vishny (1992) who consider liquidity implications of industry wide shocks of firms' access to credit.

[7]The finding that bankruptcy laws have strong general equilibrium effects that frequently overwhelm personal exposure effects echoes our earlier theoretical analyses (Lilienfeld-Toal and Mookherjee (2008,2010)) and empirical work in the context of India (Lilienfeld-Toal, Mookherjee and Visaria (2009)).

The paper is organized as follows. Section 2 sketches a theoretical model of the main hypothesis, in the context of a simple economy with two dates. Section 3 explains the data and regression specification used. The main empirical results are presented in Section 4, whose robustness is explored in Section 5. Section 6 provides supplementary results concerning the role of contagion and financial fragility. Finally, Section 7 concludes.

2 Model

There are two dates $t = 1, 2$ at which markets open. There are two goods: a divisible consumption good and an indivisible housing good (thought of as home-ownership). We treat home rentals as part of non-housing consumption, with the benefits of home-ownership arising from avoidance of moral hazard problems associated with renting, and favorable tax treatment of home-owners.

There are two types of agents: households and banks. In between the two dates households experience earnings shocks. Households consume at $t = 1$ after markets close, but before they experience the earnings shock. Then $t = 2$ arrives, markets open, followed by consumption.

At $t = 1$ banks extend two kinds of loans to households: secured and unsecured, which are due for repayment at $t = 2$. Secured loans use houses owned by households as collateral. The penalty for defaulting on a secured loan is that the household has to transfer the collateral to the bank. The penalty for defaulting on unsecured loans is defined by bankruptcy law, defined either by Chapter 7 or Chapter 13. For those qualifying to file under Chapter 7, the only penalty is a loss of reputation for the borrower. Under Chapter 13 the penalty is larger, involving a transfer of future earnings in addition to reputation loss. BAPCPA induced the top half of the population in a state to stop having the Chapter 7 option. Hence for a fraction of the population, the penalties for defaulting on unsecured debt went up.

Household i's preferences are given by the following utility function:

$$U_i \equiv u(c_{i1}) + \gamma h_{i1} + \delta[u(c_{i2}) + \gamma h_{i2} - \psi_i d_u] \quad (1)$$

where u is strictly increasing, strictly concave, defined on $c_{it} \geq 0$ which denotes i's consumption at t, $h_{it} \in \{0, 1\}$ denotes home-ownership of i at t, $d_u \in \{0, 1\}$ denotes the decision of the household to default on unsecured debt, ψ_i an associated penalty imposed on the household in terms of loss of future utility owing to loss of reputation or future income, $\delta \in (0, 1)$ is a discount factor, and $\gamma > 0$ is a parameter representing household preferences for home ownership. We are therefore assuming that (i) there is no reputational loss associated with default on a secured loan, and (ii) the penalty for defaulting on unsecured loans can vary from household to household, depending on the Chapter

7 exemption limits prevailing the state the household resides in, as well as future income anticipated by the household.

Household i has endowments $w_{i1} \geq 0, \bar{h}_{i1} \in \{0, 1\}$ of consumption good and housing before markets open at $t = 1$, and anticipates earning $e_i \cdot w_{i1}$, where e_i is a nonnegative random variable representing a shock to endowments, which is realized between $t = 1$ and $t = 2$. There is a future after $t = 2$ but we abstract from it except when referring to reputational costs incurred after $t = 2$. This is to keep the model simple; date 2 can be thought of as representing the entire future following repayment (or lack thereof) of date 1 loans.

Banks have no preferences for consumption smoothing and are risk neutral. They have no intrinsic valuation for home ownership, and value only the consumption good, according to the utility function $c_1 + \delta c_2$, where c_t denotes the net inflow of the consumption good into the bank at date t, which is not subject to any non-negativity constraint. Banks compete with one another in Bertrand fashion, have no limit on lending capacity, and their lending costs are normalized to zero. Competitive equilibrium will ensure they break even in expectation; equilibrium credit contracts will maximize expected utility of each borrower subject to a break-even constraint for banks.[8]

Equilibrium requires the housing market to clear. Let p_t denote the price of homes on the date t market. The per capita supply of homes in the economy is given by $\bar{h}$, the sum of homes owned by households and banks at the beginning of date 1; we abstract from the possibility of new homes constructed between the two dates.

At $t = 1$ there are three markets: for housing, secured and unsecured loans. Households can lend by keeping deposits in banks at the going rate of interest $\frac{1}{\delta} - 1$. They can borrow from banks at $t = 1$. A secured loan requires a home to be owned by the borrower which serves as collateral. The secure loan involves an amount borrowed B_s and a corresponding repayment obligation of R_s. If the borrower fails to repay R_s at $t = 2$, the home is transferred to the bank (who can sell it on the housing market at $t = 2$). An unsecured loan of B_u involves a repayment liability of R_u at $t = 2$. Failure to repay involves the utility loss of ψ_i for household i at $t = 2$, the present value of loss of reputation and future income beyond this date.

At $t = 1$, each household i selects a decision concerning sale or purchase of a home, in combination with a secured and unsecured loan, taking housing price p_1, and interest rates on secured and unsecured loans as given. A secured loan can be obtained conditional on the household owning a home at $t = 1$. It will also decide how much to consume of the consumption good at $t = 1$. The household will make these decision to maximize its expected utility evaluated at $t = 1$. The interest rates on each kind of loan will have to ensure that the expected rate of return to the bank will equal

[8]The equilibrium is independent of the endowments of the banks, which is why they have not been specified.

$\frac{1}{\delta} - 1$, and will therefore incorporate anticipated default risk.

After the household consumes at $t = 1$, it experiences the earnings shock $e_i \cdot w_{i1}$. Then $t = 2$ arrives. It now has an endowment

$$w_{i2} = w_{i1} + B_{iu} + B_{is} - p_1[h_{i1} - \bar{h}_{i1}] - c_{i1} + e_{i1} \cdot w_{i1} \tag{2}$$

of the consumption good, and h_{i1} of the housing good. It decides whether to default on either kind of loan ($d_s \in \{0, 1\}, d_u \in \{0, 1\}$), as well as h_{i2} home-ownership at $t = 2$. There is a market now only for homes, which clears at price p_2.

We focus on an equilibrium with date 2 housing price p_2 and default decisions d_{is}, d_{iu} correctly anticipated at $t = 1$. Default decisions induce a modification of endowments of i from (w_{i2}, h_{i2}) to

$$\hat{w}_{i2} = w_{i2} - (1 - d_u)R_u - (1 - d_s)R_s, \hat{h}_{i2} = h_{i2}(1 - d_s). \tag{3}$$

2.1 Housing Price at $t = 2$

Given default decisions made by each household, there is a Walrasian market for homes at $t = 2$, with endowment distribution given by μ_2 the proportion of households i with homes (with $\hat{h}_{i2} = 1$), distribution of wealth of home-owners given by c.d.f. $F_2(\hat{w}_2; 1)$, and of non-homeowners $F_2(\hat{w}_2; 0)$.

A household with wealth $\hat{w}_2$ will purchase a home at $t = 2$ when the price is p_2, if and only if $\hat{w}_2 > w^H(p_2)$, where $w^H(p)$ solves for w in $u(w) - u(w - p) = \gamma$.

The aggregate demand for homes at $t = 2$ at price p_2 is therefore

$$\mu_2[1 - F_2(w^H(p_2); 1)] + (1 - \mu_2)[1 - F_2(w^H(p_2); 0)]$$

which has to equal the fixed per capita supply $\bar{h}$. Note that secured loan defaults will transfer homes from defaulters to banks, thus keeping the per capita supply unchanged. Defaulters have the option to buy back the home they lost.

It follows that the equilibrium housing price p_2 will depend on the distribution of household wealth. A FOSD (first order stochastic dominance) shift of the wealth distribution will increase the housing price.

So p_2 is a function of the distribution of household wealth $\hat{w}_2$ resulting at $t = 2$ following default decisions.

2.2 Default Decisions

All households take p_2 as given. Consider any given household i and drop the subscript i in what follows. Let its date 2 (pre-default-stage) endowments be denoted by (w_2, h_2) respectively, where w_2

equals $w_{i2} \geq 0$ given by (2) above, and h_2 equals h_{i1}. It faces the problem of selecting d_s, d_u both in $\{0,1\}$ to maximize

$$v(w_2 + (1-d_s)[p_2 h_1 - R_s] - (1-d_u)R_u; p_2) - \psi.d_u \tag{4}$$

where v is the indirect utility function defined by

$$v(w;p) \equiv \max_{h=0,1}[u(w-hp) + \gamma.h] \tag{5}$$

A non-negativity constraint also has to be satisfied:

$$w_2 + (1-d_s)[p_2 h_1 - R_s] - (1-d_u)R_u \geq 0 \tag{6}$$

but it turns out not to bind at the unconstrained optimum of (4).

It is evident that maximization of (4) by a homeowner (with $h_2 = 1$) implies $d_s = 1$ if and only if $p_2 < R_s$, i.e. the owner defaults on its secured loan if his home equity $p_2 - R_s$ is negative.

Moreover, it will default on its unsecured loan if and only if

$$w_2 + (1 - \chi_{\{R_s > p_2\}})[p_2 h_1 - R_s] < w^*(R_u, \psi) \tag{7}$$

where χ_A denotes the indicator function for event A, and $w^*(R_u, \psi)$ denotes the solution for $v(w) - v(w - R_u) = \psi$.

Proposition 1 *An increase in penalty for default on unsecured debt causes a reduction in default on unsecured debt for a subset of households, and a reduction in net wealth for such households (for a given housing price p_2).*

The proof (provided in the Appendix) entails comparative statics of the default decisions for all parameter values, including when the indirect utility function is non-concave. The intuition for this result is that rising default penalties on unsecured default for a subset of households will lead to less unsecured defaults and a reduction in household net wealth for those affected.

The effect on housing prices now follows from the fact that there is a FOSD shift in the distribution over post-default-decision wealths of households.

Proposition 2 *Given arbitrary date-1 credit contracts, an increase in penalty for default on unsecured debt causes a reduction in housing price at date 2.*

The drop in housing prices will have a further feedback on default decisions: there will be more defaults on secured loans (those for which home equity turns negative), and this in turn will give rise to higher defaults on unsecured credit among those who do not default on their secured loans (as the drop in housing prices lowers their home equity, causing the net wealth $w_2 + p_2 - R_s$ of some of them to now fall below $w^*(R_u, \psi)$). If the household in question was not directly subject to the rise in ψ — e.g., those below the state median income, there will be higher defaults on unsecured credit. If the household was directly subject to a rise in ψ, then the net effect on default on unsecured debt is ambiguous: the first-order effect lowers default risk, the second-order effect through the fall in housing prices raises the risk.

If we were to extend the model to include a future loss of reputation following a secured loan default, we would expect an increase in penalties on unsecured loan defaults to have a direct positive effect on secured loan default, even if home equity or home prices are unaffected. For then the affected households would have a lower net wealth owing to their having to pay off their unsecured loans, raising the current attractiveness to the household of a secured loan default. This personal exposure effect has been emphasized by some previous authors. This would reinforce the geographic exposure effect isolated above.

We summarize the predictions on the date 2 equilibrium, of an increase in ψ for a subset of households in the economy:

(1) A fall in housing prices.

(2) A rise in default rates on secured loans as result of (1), even for households that may not be directly affected by the rise in ψ.

(3) A rise in default rates on unsecured loans of those who do not default on their secured loans and those not directly affected by the rise in ψ.

(4) A fall in default rates on unsecured loans of those directly affected by the rise in ψ and those who own no homes or default on their secured loans.

2.3 Effects on Date 1 Equilibrium

The model predicts that interest rates will be nonlinear, since default risks will depend on the amount borrowed. For example, the repayment liability R_s on the secured loan determines the home equity of the homeowner: the higher is R_s, the lower the home equity and hence the higher is the default risk. The same is true of unsecured loans: the greater the repayment liability R_u the higher the risk of default on the unsecured loan. Hence each borrower will be offered a schedule of interest rates corresponding to different amounts borrowed, on each kind of loan, and depending on the borrower's characteristics observed by the bank.

It is evident that any parametric change that induces a fall in equilibrium house prices at $t = 2$

will raise default risks on the secured loan, as well as lower the value placed on the collateral recovered by banks in the event of a default. For both reasons, the interest rate charged by banks for any amount borrowed using the house as collateral, will go up.

Note that if we were to allow more dates after $t = 2$ with a credit market opening at $t = 2$ for loans to be repaid at $t = 3$, the higher interest rate charged on secured loans would likely reduce home loans taken, which would further depress housing prices at $t = 2$. So we would obtain a greater decline in p_2. This would intensify the 'contagion', causing in turn more defaults on housing loans.

3 Data and Regression Specification

3.1 Data

We use two main sets of data for house prices, a house price index (HPI) and the American Housing Survey (AHS). For HPI, we use the OFHEO index.[9] This is a repeat transaction index and uses both sales data and appraisals to determine the house price in a certain region. It uses the same weighted repeat sales methodology as in the Case-Shiller index. The appraisals come from conventional conforming mortgage transactions from Freddie Mac and Fannie Mae. The AHS data comes from a national longitudinal survey at the housing unit level administered once every two years to current inhabitants. We restrict the analysis to owner occupied units as those contain information on house values.[10] In addition to this, we use information from the 2000 Census and the American Community Survey. Finally, information about homestead exemption limits is taken from Morgan et al. (2008).

Both HPI and AHS data are panel data and differ along the following dimensions. The HPI house price index reports house price information at the regional (MSA) level for each quarter, while the AHS reports prices at the housing unit level once every two years. Both AHS and HPI are restricted to MSAs.[11] One advantage of the HPI index is that it is reported every quarter while the AHS is only reported every odd year. On the other hand, the AHS has information on the value of the house in question as well as distinct mortgage information (in particular the interest rate on the mortgage)

[9] This can be downloaded from the http://www.fhfa.gov/Default.aspx?Page=87. We use the series for the Metropolitan Statistical Areas and Divisions from 1995-2008.

[10] The American Housing Survey (AHS) is conducted by the Bureau of the Census for the Department of Housing and Urban Development (HUD). The data can be downloaded from http://www.census.gov/hhes/www/housing/ahs/nationaldata.html.

[11] Only a subset of housing units within the AHS are actually located within an MSA. All other observations do not have any regional information and we are unable to determine the homestead exemption limit in question.

and income information of the current home owner.

The main disadvantage of the HPI data is that it is based on actual transactions. While the methodology is designed to keep the average value of housing fixed, it is difficult to maintain this goal and a bias may occur if certain houses — for example low quality houses in subprime areas— were sold more often after the reform. This type of bias is not an issue with the AHS data as the same house is traced over time and initial differences in housing quality is absorbed in the household fixed effect. However, one disadvantage of the AHS is that the data item house value is the (subjective) answer to the question concerning the current market value of the house. For all these reasons, we employ both datasets in our analysis.

3.2 Specification

Predictions on our endogenous variables of interest — house price growth, as well as interest rates — all depend on *BAPCPA exposure* of the region in question. We measure BAPCPA exposure in a variety of ways. The first and most straightforward definition makes use of the homestead exemption limits of the region in question where a higher homestead exemption is equivalent to higher exposure of the region. This follows the logic that losing the option to file under Chapter 7 has a higher impact in states with a high homestead exemption limit. After all, home owners can only keep their home under Chapter 7 if the value of their home equity is below the homestead exemption limit.

We run a panel regression of home price growth at the regional level on pre vs post-BAPCPA, interacted with BAPCPA exposure of the region, after controlling for region and year dummies using HPI data. For the AHS data, this specification is used with home price growth at the house level. The only difference is that fixed effects in the AHS data are at the household level while they are at the regional level in the AHS data. Since pre-reform homestead exemption limits vary across states, this double-difference specification allows us to filter out nationwide year specific macro shocks.

Unfortunately, using the size of the homestead exemption limits as a measure of *BAPCPA exposure* is not straightforward, as some states have unlimited homestead exemptions, while others have finite limits. We deal with this in two ways. In the first, we put in a dummy for states with unlimited homestead exemption and measure homestead exemptions as a continuous variable for all remaining states. The resulting specification is

$$y = \alpha_0 + \alpha_1 \times post \times exposure + \alpha_2 \times post \times unlimited + \beta X + \epsilon$$

where *post* is a dummy for post-reform periods, *exposure* is the size of the homestead exemption limit (we measure this in logs and relative to the median income of the region) and our dependent variables y are house price growth and interest rate on the mortgage. Additional controls are denoted by X and in particular always include time fixed effects and fixed effects at the lowest regional unit available (i.e. region fixed effect in the HPI regressions and household fixed effects in the AHS regressions.[12]

In the second option, *BAPCPA exposure* is $AVEX \equiv min\{\texttt{median house price, homestead exemption limit}\}$ where we use the median house price of the region and the homestead exemption limit at the state level. The median house price is taken from the 2005 American Community Survey for the HPI data and from the 2005 American Housing Survey for our AHS data. The logic behind this is that defaulting borrowers have to sell their home if their home equity exceeds the homestead exemption and the house value is an upper bound of the home equity. This gives rise to the following specification:

$$y = \alpha_0 + \alpha_1 \times post \times exposure + \beta X + \epsilon$$

where the main difference is that *exposure* is now $AVEX$ measured either in logs or relative to the median income. The advantage of this specification is that a separate dummy for states with unlimited exemptions is no longer required.

4 Empirical Results

4.1 Descriptive Statistics

Table 1 presents summary statistics from each dataset. Panel A shows a mean quarterly growth rate of the HPI index of 1.35% before the reform, compared with 0.37% after the reform, amounting to a drop in the annual growth rate of approximately 4%. Panel B shows a mean growth rate (over a two year period) of 15.7% before the reform, compared with -2.8% after, which corresponds to a drop in the annual growth rate of approximately 9%. Average interest rates dropped from 6.8% to 5.8%. $AVEX$, the measure of BAPCPA exposure described above was approximately equal to median income at the MSA level in the HPI data, while averaging about 20% of incomes of homeowners in the AHS data.

Table 2 shows how exemption limits varied across states in 2005. At one extreme, some states (New Jersey, Pennsylvania, Maryland) allowed no exemptions, and others (Alabama, Georgia, Illinois, Kentucky, North Carolina, South Carolina, Ohio, Tennessee and Virginia) had low exemptions below ten thousand dollars. Some (Arizona, Massachussetts, Minnesota, Montana, New Hampshire)

[12]For the AHS, instead of using household fixed effects, we could also use housing unit fixed effects and the results are unaffected by that choice.

had exemptions at or above a hunder thousand dollars, and some (Arkansas, Florida, Iowa, Kansas, Oklahoma, South Dakota and Texas) had unlimited exemptions. Table 3 shows the median exemption limit in either dataset was forty thousand dollars. As a number of scholars have found (to be elaborated further below), this wide variation reflects historical circumstances of these respective states from the 19th and early 20th century, rather than current economic circumstances such as population density, poverty rates, occupational and demographic structure. This is the basis of our strategy of identifying the effect of the bankruptcy reform by comparing changes in housing price growth rates before and after the reform across states with varying exemption limits.

4.2 Graphical Illustration

We start with a visual depiction of the effect of the reform. To begin with, it is apparent from Figure 1 that the reform had a strong effect on the filing for Chapter 7 and a much weaker effect on filings under Chapter 13. Borrowers with adverse shocks were eager to file under Chapter 7 while it was still possible which caused a sharp increase of Chapter 7 filings just prior to the reform.

Figure 2 plots housing price growth over time. There is a clear change of house price growth regime just around the time of the reform, where an upward trend visible for three or four years prior to 2005 is interrupted and commences a clear downward trend. It is possible, of course, that this reversal may have been caused by other macroeconomic factors occurring around 2005. So we examine whether house price growth dropped more for states with higher exemption limits. Figure 3 plots average growth separately for states with high and low exemption limits. We use states above/below the median of exemption limits (above/below $40,000) to form the two classes. Figure 3 highlights that both high and low exemption states experienced a change in the growth rates right around the time of the reform. A graphical example in the spirit of our identification strategy is presented in Figure 4, which shows a sharp drop in the difference in house price growth between high and low exemption limit states.

Figures 5 and 6 subsequently plot vectors of coefficient estimates $\hat{\alpha}_1$ and $\hat{\alpha}_2$ with the corresponding 90% confidence interval for specifications

$$y = \alpha_0 + \alpha_1 \times yearD \times exposure + \alpha_2 \times yearD \times unlimited + \beta X + \epsilon$$

where $yearD$ is a vector of year dummies from 1996-2008. This implies that the confidence interval is measured relative to 1995. For every year after the reform, the point estimate of the slope with respect to exposure is negative and statistically significant. This is clearly not due to a pre-existing trend, as point estimates were consistently positive before the reform. We now proceed to regression results which allow us to examine robustness of these results with respect to inclusion of various controls.

4.3 Regression results: House price growth

Our main results are reported in Table 4, using fixed effects at the MSA level (in the HPI data) and at the housing unit level (in the AHS data), and time dummies (quarterly for HPI, year for AHS). Coefficient estimates using the HPI data are reported on the left hand side in columns 1-4, and those using the AHS data are shown in the last four columns. The columns differ by the precise exposure variable used: within each set the first two use the exemption limit for states with finite limits (either measured in logs in column (1) or (5) or relative to the median income of the region in column ((2) and (6)), and a dummy for those with unlimited limits. The latter two columns use the AVEX measure, defined above as the minimum of the exemption limit and median home price either in logs or relative to the median income of the region.

The coefficient estimates are significant and negative. The HPI results pertain to quarterly growth rates, while the AHS results pertain to growth spanning a two year period which is consistent with the view that higher exposure to the BAPCPA reform leads to a stronger price decline after the reform. The exposure slope effect in the HPI regressions vary between 0.3–0.4% per quarter, and in the AHS data between 4 and 8% every two years, which are roughly consistent with one another, but with the AHS results showing a stronger effect. The more conservative HPI results imply that a one standard deviation increase in the exemption limit by one standard deviation was associated with a 1/4 standard deviation decline in the growth rate, compared to an observed decline of approximately 1/2 standard deviation. The coefficient in column (1), row (1) implies that the decline in quarterly growth rate in a state with the median exemption limit state was 1% larger than a zero exemption limit state. This is a quantitatively large magnitude, compared for instance with an average post reform growth rate for zero exemption limit states of .8% on a quarterly basis.

Note, however, that the coefficients for the unlimited exemption limits are insignificant and have a positive sign in column 2. In the case of the AHS data, the effect is negative, though insignificant in column 6. The unlimited exemption limits are unusual in some respects, e.g., they all tend to be land rich and less urbanized. We shall examine how these results are modified with inclusion of further controls.

5 Robustness checks

5.1 Pre-Housing Bubble, Fed Interest Rates, and Land Supply Elasticity

Several competing explanations for the reversal of housing price growth have been forwarded in the literature. One argument is based on a pre-2005 housing bubble, which burst around 2005 for a variety of reasons, such as an increase in the Federal Reserve Board's interest rate in the mid-2000s (Rajan (2010, Ch. 5)). Figure 3 shows that housing price growth prior to 2005 was larger in states with high exemption limits. States with a larger bubble would be expected to experience a larger decline in growth rates once the bubble burst. Hence our estimated effects may simply be proxying for the size of the pre-2005 bubble. Other concerns with the identification strategy may arise with regard to correlates of state exemption limits — e.g., states with unlimited exemptions were frontier states in the 19th century that are land-rich, have lower population density and involve a different kind of real estate market compared with other states today.

Table 5 shows how our results are affected by various controls for other determinants of house price movements. Column 1 includes as control an interaction between the post-reform-time dummy with the elasticity of land supply for each region as provided by Saiz (2010). The point estimates for the limited exemption limit states in row (1) is slightly reduced while the point estimate for the unlimited exemption limit dummy in row (2) is unaffected. However, the precision for estimating the effects is increased; the effect on the unlimited exemption limit states is now significant at the 10% level for the HPI data and at the 1% level for AHS data. The estimated effect of housing supply after the reform is what one may expect: house prices fell by less in states with a higher supply elasticity.

The remaining columns control for various measures of the size of the pre-reform bubble. Column 2 includes an interaction of the post-reform-time dummy with the pre-reform time trend in the growth rate. While we do see a significant effect of the pre-trend as expected, the statistical significance of the slope with respect to BAPCPA exposure is hardly altered. The quantitative magnitude is smaller by one-fifth in the case of the HPI-based regressions, and by less than a tenth in the AHS regression.

The next column in Panel A uses instead of the pre-reform trend rate a different measure of the bubble: the 2004 imputed-to-real rent ratio reported in Himmelberg, Mayer, and Sinai (2005). Roughly speaking, the imputed rent is the rent predicted by a simple equilibrium model of the housing market, taking as given house prices and home-ownership costs. The relation of this to actual rents is a measure of whether house prices are too high. The subsequent column (4) replaces this with another measure reported by Himmelberg et al. (2005): the price-to-rent ratio prevailing in 2004, while column 5 uses the price-to-rent ratio taken from the 2000 Census and measured at the MSA level wherever available and at the state level otherwise. These controls do not alter the statistical significance of the estimated coefficients, while raising the magnitude of the effect for unlimited exemption states considerably. Panel B shows the AHS regression results to also be robust with respect to these controls: column 3 uses the average price-to-rent ratio calculated using the 2005 AHS survey while column 4 uses an indebtness measure which is average ratio of housing costs (the main component being mortgage payment) over income.

In almost all our bubble control specifications, the pre-existing bubble measure is highly significant and has a sign that is expected. To the extent that our control measures proxy for higher (or too high) price levels before the reform, it is a robust finding that higher price growth before the reform was associated with a higher drop in house price growth after the reform. It is interesting to compare the quantitative effects of the various controls with those of BAPCPA exposure. For panel A dealing with the HPI data, a one standard deviation change in each of the controls would imply the following changes in the growth rate: 0.5 for the supply elasticity, -0.6 for the pre-trend (column 2), -1.1 for the imputed to real rent ratio (column 3) as well as for the 2004 price to rent ratio (column 4), and -0.6 for the Census based price-rent ratio (column 5). This compares to an effect of -.7 for a one standard deviation increase of the BAPCPA exposure measure, from column 1 of Table 4, or about -0.5 from this Table. Hence the effect of various bubble measures are comparable to the effect of the BAPCPA reform, and at a maximum twice as important in predicting the price growth reversal. A similar calculation for panel B predicts effects of a one standard deviation change in BAPCPA exposure to be -15.6, larger than any of the controls: 5.7 for the supply elasticity (column 1), -13.3 for the pre-trend (column 2), -13 for the price-rent ratio (column 3) and -2.8 for indebtedness (column 4).

A related argument for the reversal of the price growth centers on the dual role of the US Federal Reserve, in both fueling the housing bubble and in its subsequent burst. Between July 2000 and December 2003, the Fed reduced the "Effective Federal Funds Rate" (which we hereafter refer to as the Fed interest rate) from 6.54% to a low of 0.98%.[13] This arguably fueled demand for housing and a surge in house prices followed. From December 2003 to July 2007 the Fed interest rate rose from 0.98% to 5.26%. This would have imparted a negative shock to housing demand, inducing a reduction in house price. Finally, starting in July 2007 a series of Fed interest rate cuts followed so that by the end of 2008 it was essentially down to zero. Since interest rates are fundamental to the housing market, we have to worry about the possibility that our estimated effect of BAPCPA reform

[13] The "Effective Federal Funds Rate" and other interest rate time-series can be downloaded at http://research.stlouisfed.org/fred2/categories/118.

could be proxying for the effects of the rise in the Fed interest rate during the previous year. The effect of the latter *per se* is of course captured by the time dummies, but we need to check whether across-state-heterogeneity in the effects of the rise in interest rates may be correlated with BAPCPA exposure.

The effect of a rise in interest rates on demand for housing is likely to be greater in states with high initial debt and mortgage levels. Consider a household with an adjustable-rate mortgage. Any increase in the interest rate will make the mortgage and thus owning a house more expensive. The effect of the interest rate increase will be bigger for higher mortgage amounts. If the average level of mortgages is correlated with exemption limits, this will have biased our results concerning effects of the latter. To examine robustness of our results with respect to this concern, we control for the fraction of ownership costs over income in the HPI data, and the ratio of mortgage payments to homeowner income in the AHS data. We also use house-price-to-income ratios as another control, since higher house prices translate into higher mortgages and thus a greater exposure to interest rate increases for newly acquired houses. We measure these variables in the year 2000, and interact them with changes in the Fed interest rate, which serves as a control for our earlier estimates of the BAPCPA reform exposure. The results are presented in Table 6.

Panel A shows the results for HPI data. Columns 1 and 5 report estimation results for our standard specification where we add as a control an interaction between the Fed interest rate and a measure of exposure to the interest rate increase. In column 1, the exposure measure is ownership costs over income and in column 5 it is the price-to-income ratio. Both coefficients turn out to be highly significant, providing support to the arguments described above. But our estimates of the BAPCPA exposure effect remain virtually unaffected. The same is true in columns 2 and 6 which replaces the Fed interest rate by a dummy which takes the value one when the interest rate is above 3% and zero otherwise, which is interacted with the same controls as in columns 1 and 5 respectively. Columns 3 and 7 uses dummies for periods of rising and falling interest rates. The dummy variable $D00-03$ is one for quarter 2 in 2000 up to the third quarter 2003, the variable $D03-07$ is one from quarter 4 in 2003 up to quarter 3 in 2007 and $D07-08$ is one starting in quarter 4 2007 until the end of our sample.

As expected, house price growth was higher in times of interest rate reductions in areas with higher indebtedness (when $D00-03 = 1$). Somewhat surprising, house prices in areas with high initial indebtedness continued to outgrow areas with low initial indebtedness even in times of increasing interest rates (when $D03-07 = 1$). It is only from 2007 onwards (when $D07-08 = 1$) that highly indebted neighborhoods experienced lower growth rates. Hence the evidence is consistent with the view that falling interest rates in the early 2000s fueled the bubble. But it is less clear that rising interest rates caused the bubble to burst, since housing price growth continued to be higher in areas with greater exposure to interest rate increases throughout 2003-07 when the Fed was raising its rate.

The economic importance of the effect of the BAPCPA reform is comparable to the effects of changes in the Fed interest rate. For example, a one standard deviation increase in the exposure to the Fed interest rate increase ($int * control$ or $(int > 3) * control$) leads to the following change in the quarterly growth rate: -1.2 for column (1), -1.4 for column (2), -.32 for column (4), and -.38 for column (5). This compares to an implied change of the quarterly house price growth rate due to a one standard deviation increase in exposure to BAPCPA of about -.8 in these regressions.

Nevertheless, it is evident that the results concerning effects of BAPCPA exposure remain unaffected, except for a decline in the estimated slope effect down to about 0.3 from 0.4 in earlier tables. The same turns out to be true for the AHS regressions shown in Panel B. In this case, however, the effects of Fed interest rate changes turn out insignificant by and large.

In summary, our results concerning effects of the BAPCPA reform are robust with respect to controls for pre-reform bubble and Fed interest rate changes. While the latter factors are likely to have played some role in reversing housing price growth, our estimates of the BAPCPA reform played a separate and independent role which was statistically and quantitatively significant.

5.2 Exemption Limit Determinants

Exemption limits are set at a state level and as a result, pre-reform levels of exemption limits are endogenous to the political process. Consistent with existing literature, we argue that it is difficult to see particular patterns in the way exemption limits vary across states, except that they are mainly determined by historical levels. For instance, Hynes et al. (2004) try to explain variation in state exemption limits with doctors, lawyers, farmers, banks, income, and transfers (all per capita), the divorce rate, cost of living, and population density. They find that the only robust predictor of exemption limits are historic values of exemption limits.

In the same spirit, Table 7 shows a cross-state regression for the log of the 2005 exemption limit (with unlimited exemption states assigned an exemption of $1,000,000) on various state characteristics such as per capita number of movers, population density, occupational and demographic structure, poverty, race, families, besides measures of new construction, housing values, costs, housing age and vacancies. The first column shows that the best predictor of exemption limits are historic (1996) values of exemption limits. Controlling for these, only the percent of vacant houses are significant at the 5% level whereas density and unemployment are significant at the 10% level. The subsequent columns drop the historic exemption limits and only two or three variables remain significant but even these are not robust to variations in specification. Only two of the controls are significant in at least two of the specifications.

Nevertheless, this table is helpful insofar as it allows us to understand which of these covariates are correlated with exemption limits, so we can examine robustness of our results concerning the role of BAPCPA exposure when we control for these. Results of this step are given in Table 8. The point estimate for the exposure effect for states with finite exemption limits is reduced by 30-40%, but remains statistically and economically significant. At the same time, the effect on the states with unlimited exemption limits is now statistically significant in both samples, while the point estimates are unaffected. Hence we do not find any evidence that the BAPCPA exposure effects were proxying for observed state-level determinants of exemption limits.

6 Contagion and Financial Fragility

Our model described geographic exposure effects of the reform and the feedback effects via a liquidity spiral. The AHS data allows us to differentiate these from personal exposure effects of the reform. This is useful in exploring how the initial direct effects of the reform may have propagated through the rest of the economy, affecting other homeowners who were not personally impacted by the bankruptcy reform.

6.1 Geographic Exposure vs. Personal Exposure

For each homeowner in the AHS sample, we calculate personal exposure as follows. We first calculate home equity of the homeowner.[14] Next, we use the home equity to calculate the *relevant lost home equity*. Since households with an income below the state median income do not forfeit the option of filing under chapter 7, their relevant lost home equity is set to zero. For agents with an income above the state median income the relevant lost home equity is equal to their home equity. The extent to which agents are affected by the reform is then a combination of the relevant lost home equity and the homestead exemption limit. The *personal exposure* (PE) for the homeowner is then calculated as $min\{\texttt{relevant lost homeequity, exemption limit }\}$.

[14]Unfortunately, the AHS does not report the outstanding amount of the mortgage. We use an indirect way to calculate the amount of the mortgage outstanding. We have information on the monthly amount paid for the mortgage which includes payments on both interest and principal. In calculating the amount of the mortgage outstanding, we assume that the interest rate will be fixed for the entire period of the mortgage and that the home owner repays 1% of the mortgage.

The *geographic exposure* (GE) measure is defined at the regional level, equal to the average PE of all homeowners in the region. Our definition of region corresponds to each relevant housing market. For each MSA, we consider the following three local markets: "Central city of MSA", "Inside MSA, but not in central city - urban", "Inside MSA, but not in central city - rural". In other words, a region corresponds to a MSA-local market combination.

Using these definitions, we arrive at the specification

$$y = \alpha_0 + \alpha_1 \times post \times log(GE) + \alpha_2 \times post \times log(PE) + \beta X + \epsilon.$$

Additional controls include time and household fixed effects. Including year dummies does not suffice to capture macro shocks that affect different households or regions of certain characteristics differently. We want to understand how exposure to the reform affects individuals of comparable characteristics differently. Since, for example, the PE effect is $min\{$`relevant lost homeequity, exemption limit` $\}$, we include year dummies interacted with `relevant lost homeequity`. This filters out macro shocks that affect individuals with low relevant lost home equity differently from individuals with high relevant lost home equity. Similarly, regions with low average relevant lost home equity (which will be regions with particularly many poor households) may be affected by macro shocks differently than regions with few poor individuals. Hence, we also include year dummies interacted with the average relevant lost home equity of the region. Then the coefficients of the PE and GE effects capture the effect of the reform over and above macro shocks that affect different groups of individuals differently.

Table 9 displays the results for the specification which splits up the PE and GE effects. Only the GE effect generates statistically significant results of the predicted sign. The PE effects are either insignificant or of the wrong sign. The GE effect is comparable in economic magnitude to the implied effects of our main specification. For example, comparing a zero exemption limit state with an implied zero GE effect with a state of an average GE effect has the following implication. Annual house price growth will be decreased by approximately 22% over a two year period compared to a sample mean of two year period house price growth of 9.95%.[15] The spillover effects thus dominate individual circumstances of homeowners.

6.2 Mortgage Interest rates

Our model also predicted that BAPCPA would raise home mortgage rates, which would further amplify the depressionary impact on housing prices. We examine the evidence for this now. In the

[15]This is of a similar order of magnitude as using the log of 2005 exemption limits as our exposure measure as in our main specification.

AHS data, we observe the interest rate paid on the first mortgage, which we therefore focus on in our analysis. The effects of BAPCPA exposure on the interest rate paid on the first mortgage is described in Table 10. Interest rates effects of the reform are economically relatively small. Comparing a zero exemption limit state with an unlimited exemption limit state in column 1 tells us that the change in the interest rate is greater in the latter by .18 percentage points, as compared with a mean interest rate of 6%. Similarly, in column 1 a one standard deviation increase in log(ex.) leads to a 3% standard deviation increase in the interest rate after the reform.

While the economic significance appears to be limited, the effect is measured with great precision which results in a statistically significant effect. Most likely, a large fraction of the standard deviation of interest rates is filtered out in our specification for two reasons. First, most of the standard deviation in interest rates is in the cross section and this effect is absorbed using housing unit fixed effects. Furthermore, mortgage contracts are long term contracts (with the option to leave for the borrower). Hence, if market rates for new mortgages increase, most borrowers will keep their old mortgage and thus not face a change in the interest rate. Only for those borrowers who had a short term mortgage (either due to bad luck since the mortgage expired or due to teaser rates with initially low mortgage rates and then higher rates) were affected. This interpretation is consistent with results in Table 11 below, where we report estimates using a restricted sample of one possible definition of subprime borrowers. It is known that subprime borrowers use teaser rates more often and therefore, the effect can be expected to be more immediate for these borrowers. It turns out that the implied effect on interest rates is about 5 times higher for subprime borrowers as compared to our overall estimate. Unfortunately, we do not have a direct measure of teaser rates or expiration dates to be able to compare the interest rate effects on new mortgages.

With regard to PE vs. GE exposure, we again find that GE exposure is the driving force behind our results. The economic effect of GE exposure is comparable. As above, we can compare a zero exemption limit state with an implied zero GE effect with a state of an average GE effect. Interest rate increases were approximately .5% higher for the former, compared to a sample mean of 6%.

6.3 Impacts on Subprime and Other Financially Fragile Homeowner Groups

It could be argued that our results concerning the GE effects are unsurprising, given that housing prices are subject to large geographic spillovers. Anyone who decides to acquire a home in a given neighborhood will compare prices across different homes in that neighborhood; hence any shock which depresses the prices of some homes will have a ripple effect on prices of other homes in the neighborhood. Hence the evidence concerning the significance of the GE effects does not necessarily provide any support for our model, which stresses the role of financial fragility as a mechanism of contagion. For this we need evidence of spillover effects that vary with financial fragility of concerned homeowners. We now examine how the impacts of BAPCPA varied across subgroups in the population varying with respect to financial fragility.

One special group of homeowners that have attracted a lot of attention recently are those with subprime mortgages. Unfortunately, the AHS does not report the FICO score for a household, so we cannot identify subprime borrowers directly. We therefore use the data to predict whether a given homeowner has a subprime mortgage. We do so by relying on two data items: the mortgage interest rate and the debt-to-income ratio.[16] We now deem a household to be subprime if either the debt-to-income or the interest rate is in the highest decile as of 2005. Restricting the sample to subprime households so defined, we end up with a substantially smaller sample size.[17]

Table 11 reports results for subprime borrowers, as defined above. There are two interesting patterns. First, the effect of BAPCPA exposure on house price growth does not differ between subprime and prime borrowers. The slope with respect to BAPCPA exposure is almost identical; only the effect for the unlimited exemption limit states are stronger for the subprime households. One potential explanation for this is that subprime and prime borrowers tend to reside in similar neighborhoods, and are exposed to the same geographic effects.

When it comes to mortgage interest rates, the economic impact of the results differ quite substantially: subprime interest rates are affected five times more than are prime interest rates. As interest rates capture anticipated default rates, this makes sense. As subprime borrowers have a much smaller home equity to begin with, a decrease in house prices will have a sharper impact on default behavior (concerning the mortgage) as for households with higher home equity. It is also quite remarkable that the statistical significance is only slightly decreased even though the sample size shrank to less than 10% of the full sample.

Table 12 presents results based on the AHS data for the extreme decile of homes organized by a number of alternative measures of financial fragility: the lowest house values (based on the presumption that homeowners of the lowest incomes will be living in homes of the lowest value, and low income homeowners are more financially fragile owing to relative lack of collateralizable wealth), highest indebtedness measured by the ratio of mortgage payments to income, the highest ratio of

[16] AHS data does not directly report the debt-to-income ratio but allows us to generate the debt-to-income ratio as it contains information on income and interest cum principal payments.

[17] Due to higher attrition of subprime borrowers, we have less than 10% of observations as compared to our main specification.

loan to house value, and the lowest home equity (all of which relate to the ability and motivation of the homeowner to making mortgage payments in order to hold on to the house). Panel A presents results for our main specification concerning the BAPCPA effect on housing price growth, while Panel B presents corresponding PE and GE effects, for these extreme deciles, and contrasts them with the corresponding results for the overall population that have been shown in previous table (shown here in the last column of each panel). The GE variable is defined in the same way as in table 9, using all households within the same geographic area. BAPCPA effects as well as the GE effects turn out to be the largest for houses of the lowest value decile, which are least likely to be occupied by homeowners directly affected by the BAPCPA reform (i.e., those above the state median income). The results are at least as twice as strong for the most indebted homeowner groups, compared with the overall population. They are also significantly stronger for those with the least home equity. Hence the evidence does show larger effects for homeowner groups that are the most fragile from a financial point of view.

6.4 Additional Robustness Checks

As a final robustness check, Table 13 adds time varying controls such as household income, regional income, unemployment, and crime to our main specification, in the AHS regressions. It is evident that the main results are virtually unchanged.

7 Concluding Comments

The results presented are consistent with the view that the 2005 Bankruptcy Reform Act triggered declines in house price growth rates, in the manner consistent with the hypothesis that direct exposure of a segment of households in financial distress caused house prices to decline, thereby lowering home equity of neighboring households and triggering off a downward spiral. Subprime borrowers may have been less directly affected, but would have been hit by the ripple effects from declining house prices, thus inducing comparable effects on subprime defaults as on prime defaults. Our results were robust to controls for common macro effects of changes in Fed interest rates, as well as for measures of a pre-2005 housing bubble, other correlates of bubbles, state exemption limits, or relevant characteristics of local housing markets such as housing supply elasticities, vacancy rates or rates of construction of new homes.

The nature of the datasets restricted the empirical work in a variety of ways. We did not have access to mortgage default rates, so could not measure effects of house prices on default rates. We also did not have direct measures of subprime mortgage status, lacking access to FICO scores. It would be interesting to explore other datasets that may shed additional light on the role of the bankruptcy reform in precipitating the housing crisis. To the extent that the evidence is supportive of the hypothesis, it adds one more dimension to our understanding of the US financial crisis and the need for regulatory reform.

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Appendix: Proofs

It helps to consider first a simpler case where the indirect utility function is concave in wealth.

Lemma 3 *Suppose ψ does* **not** *lie in the interval $[\underline{\psi}, \bar{\psi}]$ where $\underline{\psi} \equiv u(w^H(p_2) - u(w^H(p_2) - R_u)$ and $\bar{\psi} \equiv u(w^H(p) + R_u - p) - u(w^H(p) - p)$. Then the household's optimal default decisions are as follows:*

(I) If $p_2 < R_s$ and $w_2 < w^*(R_u, \psi)$ then $d_s = d_u = 1$.

(II) If $p_2 < R_s$ and $w_2 > w^*(R_u, \psi)$ then $d_s = 1, d_u = 0$.

(III) If $p_2 > R_s$ and $w_2 < w^*(R_u, \psi) - (p_2 + R_s)h_1$ then $d_s = 0, d_u = 1$.

(IV) If $p_2 > R_s$ and $w_2 > w^*(R_u, \psi) - (p_2 + R_s)h_1$ then $d_s = d_u = 0$.

Under the conditions of this Proposition, it is evident that an increase in penalties ψ for default on unsecured loans causes a decrease in the threshold $w^*(R_u, \psi)$. This induces less default in unsecured loans, and a decrease in post-default wealth of the household, if w_2 falls between the old and new values of w^* in the case where $p_2 < R_s$, or if w_2 falls between the old and new values of $w^* - p_2 + R_s$ when $p_2 > R_s$. For all other households there is no change in the default decision or resulting wealth.

Proof of Lemma 3: Clearly $v(w; p)$ equals $u(w)$ if $w < w^H(p)$ and $u(w - p) + \gamma$ otherwise. Then $v(w; p) - v(w - R_u; p)$ is decreasing in w always when $p < R_u$. In that case $d_u = 1$ if and only if w falls below a threshold $w^*(R_u, \psi)$ defined by solution to w in $v(w; p) - v(w - R_u; p) = \psi$.

Now consider the case where $p > R_u$. It is evident that $w^H(p) > p$ always. Then for $w < w^H(p)$, it is the case that $v(w;p) = u(w), v(w-R_u;p) = u(w-R_u)$ so $v(w;p)-v(w-R_u;p) = u(w)-u(w-R_u)$ which is decreasing in w.

Next, over the range w between $w^H(p)$ and $R_u + w^H(p)$, we have $v(w;p) = u(w-p)+\gamma, v(w-R_u;p) = u(w)$, and so $v(w;p) - v(w-R_u;p) = u(w-p) - u(w) + \gamma$ which is locally increasing in w.

Finally over the range $w > R_u + w^H(p)$, $v(w;p) = u(w-p)+\gamma, v(w-R_u;p) = u(w-R_u-p)+\gamma$ and $v(w;p) - v(w-R_u;p) = u(w-p) - u(w-R_U-p)$ which is locally decreasing in w.

It follows that $v(w;p) - v(w-R_u;p)$ attains a local minimum of $\underline{\psi}$ at $w = w^H(p)$ and a local maximum of $\bar{\psi}$ at $w = w^H(p) + R_u$.

Lemma 3 now follows as a consequence.

Proof of Proposition 1:

If the conditions of Lemma 3 do not apply, there exist thresholds $w_1(R_u, \psi)$ below $w^H(p)$, $w_2(R_u, \psi)$ between $w^H(p)$ and $R_u + w^H(p)$, and $w_3(R_u, \psi)$ above $R_u + w^H(p)$ all of which satisfy $v(w;p) - v(w-R_u;p) = \psi$. Then $d_u = 1$ if and only if w falls below $w_1(R_u;p)$ or in between $w_2(R_u;\psi)$ and $w_3(R_u, \psi)$. Noting that w_1 and w_3 are both decreasing in ψ and w_2 is increasing in ψ, the comparative static result of Proposition 1 follows. *QED*

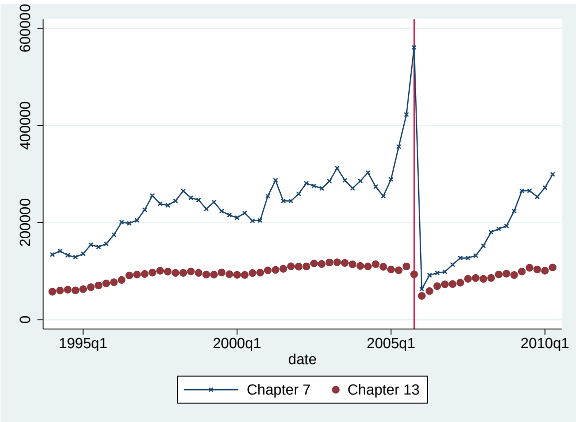

Figure 1: Quarterly filings for bankruptcy using chapter 7 and chapter 13.

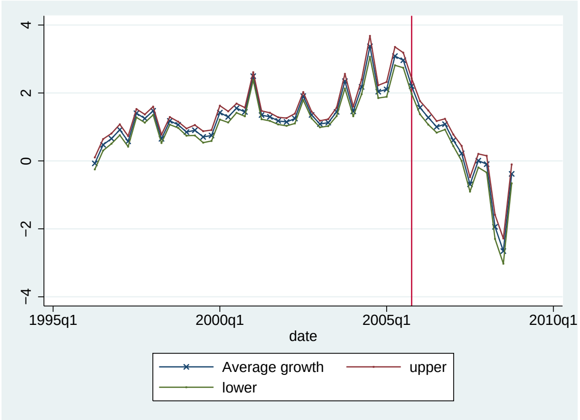

Figure 2: Average growth rates of house prices over time.

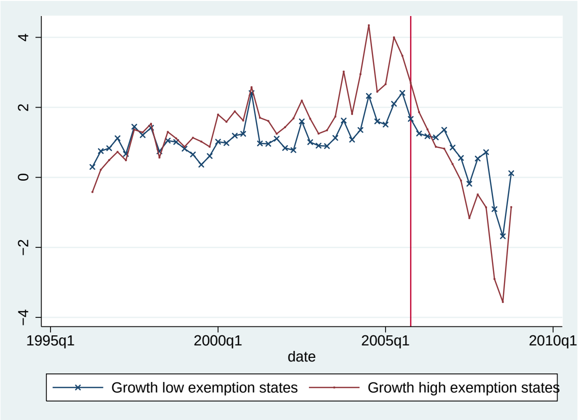

Figure 3: Average growth rates of house prices for high exemption limit states (exemption limit above median) and low exemption limit states (exemption limit below median).

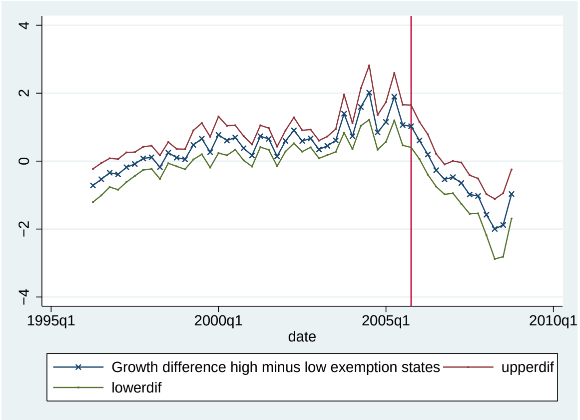

Figure 4: Difference in growth rates of house prices between high and low exemption limit states (growth rate high exemption limit states minus growth rate low exemption limit states).

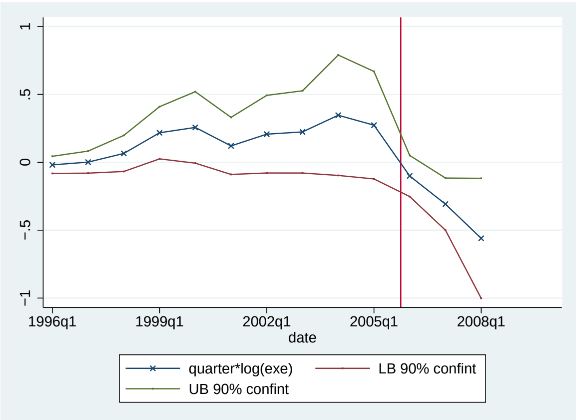

Figure 5: Interaction term of year dummies with log exemption limits in regressions for housing price growth rates.

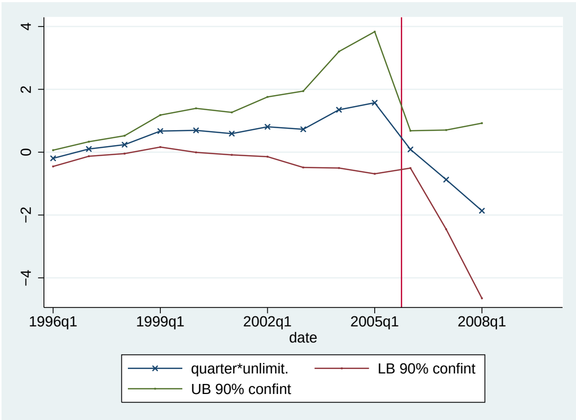

Figure 6: Interaction term of year dummies with unlimited exemption limits in regressions for housing price growth rates.

Table 1: Summary Statistics for AHS and HPI sample.

Panel A: House Price Index (HPI).

	All		Post reform		Pre reform	
	mean/sd	min/max	mean/sd	min/max	mean/sd	min/max
growth	1.10/1.98	-20.7/13.6	0.37/2.67	-20.7/9.9	1.35/1.62	-7.68/13.6
index	145.2/43.7	84.9/364.2	/	/	/	/
year	2001.5/4.03	1995/2008	2006.7/1.03	2005/2008	1999.8/3.04	1995/2005
log(ex.)	/	/	2.41/1.71	0/6	0/0	0/0
ex./inc.	/	/	0.61/1.19	0/8.3	0/0	0/0
log(AVEX)	/	/	3.29/1.40	0/6.2	0/0	0/0
AVEX/inc.	/	/	0.97/1.16	0/6.6	0/0	0/0
Observations	18901		4732		14169	

Panel B: American Housing survey.

	All		Post reform		Pre reform	
	mean/sd	min/max	mean/sd	min/max	mean/sd	min/max
growth	9.95/113.8	-1359.2/1442.0	-2.82/123.7	-1359.2/1442.0	15.7/108.4	-1343.1/1424.8
intrate	6.49/1.64	1/20	5.75/1.41	1/20	6.78/1.63	1/20
peeffect	6733.1/43288.7	0/1540794	24036.1/79207.9	0/1540794	0/0	0/0
geeffect	8022.8/24070.8	0/295937	28102.0/38279.4	0/295937	0/0	0/0
year	2003.1/3.95	1997/2009	2008.1/0.99	2007/2009	2001.1/2.73	1997/2005
log(ex.)	/	/	2.29/1.87	0/6.2	0/0	0/0
ex./inc.	/	/	0.90/1.90	0/15.6	0/0	0/0
log(AVEX)	/	/	3.02/1.79	0/6.2	0/0	0/0
AVEX/inc.	/	/	0.20/0.73	-2.46/26.6	0/0	0/0
Observations	90806		25397		65409	

Standard deviation in brackets. Growth rate is the quarterly growth rate, log(ex.) is the log of the homestead exemption limit (in 1000$) measured in 2005. The variable *inc.* is the median income from the census, and $AVEX_i$ is measured as $\min\{exemption_i, \texttt{median house price}_i\}$ where `median house price` (measured in 1000$) and *inc.* are taken from the 2000 decenial census for HPI data and from the 2005 AHS survey for AHS data. The time period for Panel A is one quarter while for Panel B it is two years.

Table 2: Statelist and exemption limits.

WEST		NORTHEAST	
Alaska	54	Connecticut	75
Arizona	150	Maine	35
California	50	Massachusetts	500
Colorado	45	NewHampshire	100
Hawaii	20	NewJersey	0
Idaho	50	NewYork	50
Montana	100	Pennsylvania	0
Nevada	350	Vermont	75
NewMexico	30		
Oregon	25	SOUTH	
Utah	20	Alabama	5
Washington	40	Arkansas	unlimited
Wyoming	10	Delaware	50
MIDWEST		Florida	unlimited
Illinois	7.5	Georgia	10
Indiana	15	Kentucky	5
Iowa	unlimited	Louisiana	25
Kansas	unlimited	Maryland	0
Michigan	30	Mississippi	75
Minnesota	200	NorthCarolina	10
Missouri	15	Oklahoma	unlimited
Nebraska	12.5	SouthCarolina	5
NorthDakota	80	Tennessee	5
Ohio	5	Texas	unlimited
SouthDakota	unlimited	Virginia	5
Wisconsin	40	WestVirginia	25

This table reports the homestead exemption limits measured in thousand dollars in the year 2005.

Table 3 Panel A: Distribution of exemption limits in the House Price index (HPI) sample.

	(1)		
	Exemption limits (homestead) from 2005		
	b	pct	cumpct
0	1339	6.50	6.50
5	2806	13.63	20.13
7.5	549	2.67	22.79
10	1625	7.89	30.69
12.5	61	0.30	30.98
15	976	4.74	35.72
20	305	1.48	37.20
25	915	4.44	41.65
30	1159	5.63	47.27
35	183	0.89	48.16
40	1342	6.52	54.68
45	427	2.07	56.75
50	2745	13.33	70.08
54	122	0.59	70.67
75	549	2.67	73.34
80	61	0.30	73.64
100	305	1.48	75.12
150	305	1.48	76.60
200	183	0.89	77.49
350	183	0.89	78.38
500	427	2.07	80.45
unlimited	4026	19.55	100.00
Total	20593	100.00	

Homestead exemptions limits measured 1000$.

Table 3 Panel B: Distribution of exemption limits in the AHS sample.

	(1)		
	Exemption limits (homestead) from 2005		
	b	pct	cumpct
0	14940	16.45	16.45
5	6423	7.07	23.53
7.5	8305	9.15	32.67
10	1968	2.17	34.84
12.5			
15	2216	2.44	37.28
20	1078	1.19	38.47
25	1261	1.39	39.86
30	7144	7.87	47.72
35			
40	2710	2.98	50.71
45	703	0.77	51.48
50	22349	24.61	76.09
54			
75	846	0.93	77.02
80			
100			
150	2356	2.59	79.62
200	1946	2.14	81.76
350	595	0.66	82.42
500	2203	2.43	84.84
unlimited	13763	15.16	100.00
Total	90806	100.00	

Homestead exemptions limits measured 1000$.

Table 4: Effect of BAPCPA reform on house price growth: Main results.

Data-source:	HPI House price index				AHS American Housing Survey			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	growth	growth	growth	growth	growth	growth	growth	growth
expos.*post	-0.406**	-0.329***	-0.321**	-0.398***	-8.330**	-4.357***	-6.666***	-6.833***
	(-2.32)	(-3.00)	(-2.14)	(-2.85)	(-2.58)	(-4.73)	(-2.91)	(-3.14)
unlimit.*post	-1.125	0.00373			-23.52*	-4.322		
	(-1.25)	(0.00)			(-1.98)	(-0.30)		
expos.	log(ex.)	ex./inc.	log(avex)	avex/inc.	log(ex.)	ex./inc.	log(avex)	avex/inc.
FE	msa	msa	msa	msa	hh	hh	hh	hh
TimeFE	quart.	quart.	quart.	quart.	year	year	year	year
r2	0.285	0.274	0.277	0.276	0.234	0.233	0.233	0.115
N	18901	18453	18509	18341	58836	58836	58836	45216

t stats in parentheses. SE clustered at state level. Unit and time fixed effects included.* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$
This table reports coefficient estimates of specifications $y = \alpha_0 + \alpha_1 \times post \times exposure + \alpha_2 \times post \times unlimited + \beta \times X + \epsilon$ or $y = \alpha_0 + \alpha_1 \times post \times exposure + \beta \times X + \epsilon$ that use various measures of exposure to the BAPCPA reform using our two datasources. Exposure is the measure *expos.* denoted in the first line below the main table. The dependent variable is house price growth (logs). The exposure measure is either the exemption limit (column 1,2,5,6) or $avex = \min\{exemption, \texttt{median house price}\}$ (column 3,4,7,8). These exposure measures are used in logs $log(1 + exposure)$ or weighted by the median regional income. Median regional income and median house price are from the 2000 Census for the hpi data and from the 2005 ahs survey for the ahs data. *Unlimited* is a dummy which is one if the homestead exemption limit is unlimited and zero otherwise. *Unlimited* is dropped whenever avex is used as an exposure measure. Additional controls X are time fixed effects and MSA level (resp. household) fixed effects for HPI data (AHS data).

Table 5: Robustness checks pre-existing bubble.

	Panel A: HPI House price index				
	(1)	(2)	(3)	(4)	(5)
	growth	growth	growth	growth	growth
log(ex.)*post	-0.367**	-0.309**	-0.462***	-0.305***	-0.295**
	(-2.42)	(-2.55)	(-3.77)	(-3.00)	(-2.42)
unlimit.*post	-1.186*	-0.722	-1.484***	-1.007***	-1.639*
	(-1.88)	(-1.42)	(-4.96)	(-6.10)	(-1.95)
post*control	0.377***	-5.561**	-9.852***	-6.571***	-0.0146***
	(2.88)	(-2.60)	(-6.80)	(-11.71)	(-3.14)
contr.	elast	pretrend	imprent	pricerent04	pricerent
FE	msa	msa	msa	msa	msa
TimeFE	quart.	quart.	quart.	quart.	quart.
r2	0.325	0.303	0.575	0.564	0.298
N	14164	18901	1736	1792	18901

	Panel B: AHS American Housing survey			
	(1)	(2)	(3)	(4)
	growth	growth	growth	growth
log(ex.)*post	-6.667***	-7.701**	-6.271***	-8.247***
	(-3.37)	(-2.42)	(-3.07)	(-2.96)
unlimit.*post	-26.27***	-11.90	-25.49***	-23.51*
	(-3.91)	(-1.40)	(-3.30)	(-2.01)
post*control	6.116*	-1.712***	-0.0858***	-10.72
	(1.73)	(-5.50)	(-6.15)	(-0.14)
contr.	elast	pretrend	pricerent	indebt
FE	hh	hh	hh	hh
TimeFE	year	year	year	year
r2	0.217	0.224	0.236	0.234
N	21307	53342	58829	58829

t statistics in parentheses with state level clustering. MSA level fixed effects included in Panel A. Household fixed effects included in Panel B. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. This table reports coefficient estimates of $growth = \alpha_0 + \alpha_1 \times post \times exposure + \alpha_2 \times post \times unlimited + post*control + \beta \times X + \epsilon$ using HPI and AHS data. The following $control$ variables are used in $post*control$ as denoted in the first line below the main table of each panel: housing supply elasticity (Saiz 2010) in column (1), trend controls in column (2) where pretrend is the linear time trend of house price growth in a region (HPI) or relevant market (AHS) between 2001 and 2005. AHS allows to identify three local markets "Central city of MSA", "Inside MSA, but not in central city - urban", "Inside MSA, but not in central city - rural". Every MSA-local market combination is a relevant house market. Columns (3) and (4) in Panel A use bubble measures as taken from Himmelberg, Mayer, and Sinai (2005), namely imputed-to-actual-rent ratio, and price-to-rent ratio. Columns (5) uses price-to-rent ratio taken from the 2000 Census and measured at the MSA level if available and at the state level otherwise. Columns (3)-(4) of Panel B use price-to-rent ratio and indebtness constructed using the 2005 AHS census. Price-to-rent ratio is calculated by taking ratios of the average price, and average rent for each relevant market. *indebt* is the homeowner's ratio of mortgage payment over income, averaged at the relevant market.

Table 6: Robustness Checks: Fed Interest Rate Increase.

	Panel A: HPI House price index					
	(1)	(2)	(3)	(4)	(5)	(6)
	growth	growth	growth	growth	growth	growth
log(ex.)*post	-0.408**	-0.394**	-0.296***	-0.408**	-0.398**	-0.292**
	(-2.32)	(-2.32)	(-2.80)	(-2.32)	(-2.30)	(-2.33)
unlimit.*post	-1.126	-1.114	-1.025	-1.124	-1.123	-1.112
	(-1.25)	(-1.27)	(-1.43)	(-1.25)	(-1.26)	(-1.40)
int*control	-0.0440***			-0.0609***		
	(-6.79)			(-3.56)		
(int>3)*control		-0.187***			-0.280***	
		(-6.78)			(-4.78)	
control*D00-03			0.217***			0.328***
			(5.88)			(6.62)
control*D03-07			0.360***			0.522***
			(7.56)			(3.04)
control*D07-08			-0.605***			-1.298***
			(-3.40)			(-6.23)
contr.	costs	costs	costs	pinc	pinc	pinc
FE	msa	msa	msa	msa	msa	msa
TimeFE	quart.	quart.	quart.	quart.	quart.	quart.
r2	0.291	0.292	0.365	0.287	0.288	0.330
N	18901	18901	18901	18901	18901	18901

t statistics in parentheses with state level clustering. Region fixed effects included. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 6: Robustness Checks: Fed Interest Rate Increase.

	Panel B: AHS American Housing survey					
	(1)	(2)	(3)	(4)	(5)	(6)
	growth	growth	growth	growth	growth	growth
log(ex.)*post	-8.324**	-8.326**	-8.246***	-8.272**	-8.173**	-7.219***
	(-2.58)	(-2.62)	(-2.96)	(-2.59)	(-2.66)	(-3.17)
unlimit.*post	-23.55*	-23.52*	-23.47*	-23.44*	-23.60*	-27.51***
	(-1.99)	(-1.99)	(-2.00)	(-1.98)	(-2.03)	(-2.78)
int*control	4.960			3.40e-09		
	(0.84)			(1.15)		
(int>3)*control		2.894			2.61e-08	
		(0.05)			(1.62)	
control*D00-03			8.882			-1.55e-08
			(0.23)			(-1.47)
control*D03-07			-34.28			9.29e-09
			(-0.43)			(0.47)
control*D07-08			-18.43			-7.02e-08***
			(-0.26)			(-6.44)
contr.	costs	costs	costs	pinc	pinc	pinc
FE	hh	hh	hh	hh	hh	hh
TimeFE	year	year	year	year	year	year
r2	0.234	0.233	0.234	0.234	0.234	0.235
N	58829	58829	58829	58829	58829	58829

t statistics in parentheses with state level clustering. Household fixed effects included. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$
This table reports coefficient estimates similar to the specifications used in table 5. However, different robustness controls are used which try to control for effects of varying interest rates. Column (1) and (4) add as additional control an interaction term $interestrate * control$ where $control$ is either $costs$ or $pinc$ where costs=mortgage payment-to income and pinc=price-to-income (AHS data) or costs=housing costs-to-income and pinc=price-to-income (HPI-data). For the HPI data, this information is from the 2000 Census (at the MSA level when available and at the state level otherwise) while the information is taken from the 2005 AHS for the AHS data. Column (2) and (5) add instead an interaction term $D3 * control$ where $D3$ is a dummy which is one if the interest rate is above 3%. Finally, the time dummies used in column (3) and (6) are one in times of increasing ($D00 - 03$ and $D07 - 08$) interest rates. See the text for further explanation.

Table 7: Predicting exemption limits using US census data.

	(1) log(ex.)	(2) log(ex.)	(3) log(AVEX)	(4) unlimited
log(ex96.)	0.374***			
	(4.23)			
movers	3.532	5.856	-5.160	-472.4
	(1.13)	(1.35)	(-1.66)	(.)
density	5963.3*	-421.5	-3670.8	-220549.7
	(1.84)	(-0.10)	(-0.68)	(.)
urban/total	-1.058	-1.911	2.965	72.20
	(-0.39)	(-0.53)	(0.84)	(.)
unempl	58.24*	81.95**	81.66**	1868.9
	(2.02)	(2.48)	(2.70)	(.)
constrind	40.79	30.37	-47.17	-4962.5
	(1.02)	(0.55)	(-1.07)	(.)
tradeind	-31.70	-26.36	41.18*	1556.2
	(-1.14)	(-0.72)	(1.72)	(.)
financeind	35.30	15.28	18.82	1104.9
	(1.28)	(0.81)	(0.74)	(.)
poverty	-13.26	-62.29	-171.4*	-5093.2
	(-0.24)	(-0.69)	(-1.80)	(.)
race	0.853	2.119	-1.492	-34.72
	(0.42)	(0.71)	(-0.60)	(.)
family	-16.44	-32.79*	-10.96	1605.0
	(-1.13)	(-1.71)	(-0.68)	(.)
newcons	33.13	55.11	79.40*	1466.7
	(0.93)	(1.04)	(2.02)	(.)
log house value	2.602	5.732**	0.791	-186.9
	(1.42)	(2.59)	(0.35)	(.)
housing costs/income	-0.109	-0.187	-0.356	-4.513
	(-0.42)	(-0.53)	(-0.92)	(-0.47)
% vacant houses	0.0849***	0.0594	0.0813*	-1.497***
	(2.79)	(1.34)	(1.94)	(-7.61)
median house age	0.0248	0.0477	-0.0247	-3.289***
	(0.32)	(0.40)	(-0.31)	(-33.89)
Observations	49	49	49	49

Robust standard errors. t statistics in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

This table reports coefficient estimates of regressions that predicts the pre-reform homestead exemption limits. In column (1) and (2) the dependent variable is the log of exemption limits and we set the level of the exemption limit to $1,000,000 for unlimited exemption limit states. AXEX is the minimum of the exemption limit and the median house price while unlimited is a dummy variable which is one for states with unlimited exemption limits and zero otherwise. All explanatory variables except historic exemption limits are taken from the 2000 census. Historic exemption limits are taken from Hynes et al. (2004). The variable movers is the fraction of individuals not born in the state over total population in the states. Total population over size of the state, urban/total is the fraction of the population living in urban areas over toal population, unemployment is the unemployment rate. Then, constrind, tradind and financeind, is the fraction of the active population working in the construction industry, in trade, and in the financial industry respectively. Poverty is the number of poor over total population, race the number of whites over total population, and family the number of individuals living in a family over total population. Newcons is the number of housing units constructed in the last 8 months over total number of housing units. Log house value is the log of the median house price. Housing costs/income is the median of housing costs (including in particular mortgage payments) over income.

Table 8: Effect of BAPCPA reform on house price growth: controls for exemption limit determinants.

Data-source:	HPI House price index	AHS American Housing Survey
	(1)	(2)
	growth	growth
expos.*post	-0.263**	-5.372**
	(-2.508)	(-2.152)
unlimit.*post	-1.294**	-21.77**
	(-2.606)	(-2.050)
post*exe.det.	Yes	Yes
expos.	log(ex.)	log(ex.)
FE	msa	hh
TimeFE	quart.	year
r2	0.324	0.236
N	18901	58836

t statistics in parentheses. SE clustering the state level. Unit fixed effects included.* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$ This table reports coefficient estimates of the same specifications as in table 4. Additional controls include a set of interaction terms where a post reform dummy is interacted with the set of control variables which are significantly correlated at the 10% level at least in one specification of our regressions on determinants of exemption limits.

Table 9: GE vs. PE effects of the BAPCPA reform using AHS data.

	(1)	(2)	(3)
	growth	growth	growth
log(PE effect)	0.389		1.384*
	(0.57)		(1.95)
log(GE effect)		-2.196***	-2.655***
		(-3.15)	(-2.98)
TimeDummy	Yes	Yes	Yes
r2	0.146	0.169	0.149
N	48653	44958	44061

t stats in parentheses. SE clustered at state level. Household fixed effects included.* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$ This table reports coefficient estimates of specifications that separates GE and PE effects of the BAPCPA reform using AHS data. Results from a specification $y = \alpha_0 + \alpha_1 \times post \times log(GE) + \alpha_2 \times post \times log(PE) + X + \epsilon$ are displayed. The PE effect=min{relevant lost homeequity, exemption limit }.

Relevant lost homeequity (in case of bankruptcy) is zero for agents below the state median income. Agents below the state median income do not lose any of the home equity they can keep as they still have the option to declare bankruptcy under chapter 7.

The GE effect is the mean PE effect of the relevant house market. For each MSA, we consider the following three local markets "Central city of MSA", "Inside MSA, but not in central city - urban", "Inside MSA, but not in central city - rural". Every MSA-local market combination then forms a relevant house market. All variables used to construct the controls are measured in 2005 prior to the reform.

Additional controls include time and household fixed effects and year fixed effects interacted with individual relevant lost homeequity and the average relevant lost homeequity of the relevant house market.

Table 10: Effect of BAPCPA reform on the mortgage interest rate.

	(1) intrate	(2) intrate	(3) intrate	(4) intrate	(5) intrate
log(ex.)*post	0.0275**		0.0309	0.0286**	0.0309**
	(2.17)		(1.12)	(2.43)	(2.07)
barunlimited	0.180**		0.128	0.179**	0.175**
	(2.47)		(1.30)	(2.52)	(2.60)
log(GE effect)		0.0138***			
		(3.26)			
log(PE effect)		-0.00508			
		(-1.10)			
post*control			0.0366	-0.00179	-0.000147
			(1.10)	(-0.49)	(-0.89)
contr.			elast	pretrend	pricerent
FE	hh	hh	hh	hh	hh
TimeFE	year	year	year	year	year
r2	0.665	0.596	0.646	0.644	0.651
N	52129	32340	16458	44733	46158

t stats in parentheses. SE clustered at state level. Household fixed effects included. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

This table reports coefficient estimates of the same specifications as in table 4, 5 and 9 using the mortgage interest rate as a dependent variable.

Table 11: Effect of the BAPCPA reform on high interest rate high debt-to-income 'subprime' borrowers.

	(1) growth	(2) intrate	(3) growth	(4) intrate	(5) loghecr1
barlogex	-12.71***	0.0567**			
	(-3.19)	(2.44)			
barunlimited	-36.67***	0.307**			
	(-2.83)	(2.62)			
logge			-3.322***	0.0203**	-0.00728
			(-3.61)	(2.16)	(-0.39)
logpe			1.076	0.0164	-0.0256
			(0.96)	(1.34)	(-1.14)
r2	0.184	0.572	0.143	0.547	0.845
N	12924	13258	10750	10844	1202

t stats in parentheses. SE clustered at state level. Household fixed effects included. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

This table reports coefficient estimates of the main specification and the differentiation of personal and geographic exposure effects for a subsample of 'subprime' households. Households are said to be subprime, if their debt-to-income ratio belongs to the highest 10% in 2005 or their interest rate belongs to the highest 10% in 2005.

Table 12: Fragility effects of the BAPCPA reform using AHS data.

	Panel A: Exposure to BAPCPA reform				
	(1)	(2)	(3)	(4)	(5)
	growth	growth	growth	growth	growth
barlogex	-24.46*	-16.31*	-9.723**	-13.70**	-8.330**
	(-1.74)	(-1.91)	(-2.08)	(-2.40)	(-2.58)
barunlimited	-102.0**	-62.43**	-20.40*	-61.68***	-23.52*
	(-2.45)	(-2.43)	(-1.71)	(-3.28)	(-1.98)
TimeDummy	Yes	Yes	Yes	Yes	Yes
fragility	valuextile	indebtdec	loanvaluextile	HEXT	all
r2	0.255	0.194	0.247	0.156	0.234
N	5261	3665	3564	5613	58836
	Panel B: GE effect of BAPCPA reform				
	(1)	(2)	(3)	(4)	(5)
	growth	growth	growth	growth	growth
log(GE effect)	-9.486**	-6.522***	-3.621*	-5.931**	-2.655***
	(-2.32)	(-3.21)	(-1.91)	(-2.36)	(-2.98)
log(PE effect)	1.312	2.033**	2.191	0.493	1.384*
	(0.44)	(2.38)	(0.85)	(0.27)	(1.95)
TimeDummy	Yes	Yes	Yes	Yes	Yes
fragility	valuextile	indebtdec	loanvaluextile	HEXT	all
r2	0.225	0.182	0.187	0.130	0.149
N	4431	3157	3058	4359	44061

This table reports coefficient estimates of the main specification and the differentiation of personal and geographic exposure effects for subsamples of financially fragile households. Our measures of fragility are all from 2005, where deciles are calculated at the relevant market. In column (1), we only keep the 10% cheapest houses in each relevant market. In column (2), we only keep the 10% most highly indebted households in each market where the measure for indebtness is mortgage payment over income. In column (3), we only keep the 10% households with the highest loan to value ratios. Column (4) only considers households with the 10% lowest home equity. Column (5) repeats the estimation results for our full sample for convenience. The GE and PE effects are measured using all households in a relevant market, exactly as in table 9.

Table 13: Effect of the BAPCPA reform with additional controls.

	(1) growth	(2) growth
log(ex.)*post	-8.559**	
	(-2.67)	
unlimit.*post	-24.40**	
	(-2.06)	
log(GE effect)		-2.659***
		(-3.14)
log(PE effect)		1.029
		(1.38)
work	0.488	-2.202
	(0.13)	(-0.73)
crime	1.378	1.322
	(0.69)	(0.74)
income	0.0000633***	0.0000667***
	(5.04)	(5.06)
average work	-3.572	1.099
	(-0.29)	(0.07)
areainc	-0.0000757	-0.000162
	(-0.48)	(-1.11)
average crime	-8.838	-9.023
	(-0.35)	(-0.36)
r2	0.228	0.147
N	55741	42443

t stats in parentheses. SE clustered at state level. Household fixed effects included. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

This table reports coefficient estimates of our main specifications with added controls. Controls all come from the AHS. The variable *work* is a dummy which is one if the householder works and zero otherwise. The variable "crime" is a dummy one if crime is perceived to be a problem in the neighborhood. The variable "average crime" is the average of the variable crime in the relevant region/houseclass in question. The variable income is the household income. The variables "average work" "average income" and "average crime" are the average values of the variables "work", "income", and "crime" in the relevant region/houseclass in question. (In each MSA, there are two regions, inner and outer city. In each region, there are ten different houseclasses, based on house values deciles). Relevant region/houseclass is constructed in 2005 while all additional controls are contemporaneous.