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

Ulf von Lilienfeld-Toal[2] and **Dilip Mookherjee**[3]

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Abstract

Most analyses of the recent financial crisis in the US focus on the consequences of the dramatic slump in housing prices that started in the mid-2000s, which led to rising mortgage defaults, shrinking home equity credit and liquidity in the banking system. Yet these accounts do not explain what caused the reversal of housing price growth in the first place. This paper argues that the passage of the 2005 Bankruptcy Reform Act (BAPCPA) contributed significantly to the reversal. The reform generated negative wealth effects for a category of homeowners, lowering prices of their homes, which spread via a process of contagion to the prices of other homes. 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. The results are robust to controls for the size of the pre-2005 price growth, local unemployment rates, rates of new home construction and home vacancies, apart from MSA, house and year dummies.

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[2]Department of Finance, Stockholm School of Economics; Ulf.vonLilienfeld-Toal@hhs.se

[3]Department of Economics, Boston University; dilipm@bu.edu

1 Introduction

Housing price growth in the US exhibited a dramatic reversal in the mid-2000s: following an average annual growth 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 well known Case-Shiller index, available on a quarterly basis; and (ii) prices of individual houses from the American Housing Survey, available for a longitudinal sample once every two years. Using either of these, we check if the change in growth rates in prices in the MSA following the passage of BAPCPA is related to BAPCPA exposure, 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: a one standard deviation change in the exposure measure results in a 42% standard deviation change in rate of growth of the regional housing price index. This means, for instance, that 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 price growth by 0.6% per quarter, compared to an average drop in growth rate of 1.5%. The effect on changes in price growth at the individual housing unit level was also statistically significant and negative, with a magnitude about three-quarters of the measured impact on the regional index magnitude.[4]

The evidence also shows changes in interest rates on mortgage loans and in access to home equity credit were significantly related to BAPCPA exposure of the region. We find a corresponding significant positive effect on the interest rate. But the size of the effect is small, due in part to our inability to distinguish new from pre-existing mortgages. The effect on home equity credit access (measured by home equity credit lines as provided in the AHS) is negative but measured less precisely: the significance of this effect depends on the precise exposure measure used.

These results are robust in various directions, such as controls for pre-reform trends as well as average levels of price growth rates. States with high BAPCPA exposure tended to have higher rates of price growth prior to the reform, so this alleviates the concern that our measured effects of BAPCPA exposure may be proxying for the size of a pre-2005 housing bubble. It is also robust to inclusion of controls for 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 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.[5] Our results indicate that contagion effects were an important mechanism in

[4]This is what one would expect from greater volatility of prices at the individual unit level compared with a regional average.

[5]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.

how the reform impacted the housing market.[6]

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 has been Morgan, Iverson and Botsch (2008). Applying a similar identification strategy as employed in this paper to state level data, they find convincing 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, Iverson and Botsch (2008). Li, White and Zhu (2010) also find convincing 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. When Li, White and Zhu distinguish effects on subprime and prime mortgages they find significant effects on both categories: they estimate default rates rose by 14% and 23% respectively in these two categories as a result of the reform. This is in contrast to Morgan, Iverson and Botsch (2008) who do not find a significant effect on prime borrowers.

Since Li, White and Zhu (2010) use mortgage-level data while Morgan, Iverson and Botsch (2008) use state level data, the former paper can address more detailed questions, in particular concerning personal exposure. Li, White and Zhu find that the evidence concerning personal exposure effects of the reform on subprime defaults is mixed, thus raising the question why the BAPCPA effect on subprime defaults was so pronounced. 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.

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 we use, and regression

[6]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)).

specification. The main empirical results concerning effects on housing price growth, interest rate and home equity credit are presented in Section 4. Section 5 provides supplementary results concerning personal exposure versus geographic exposure effects, effects on subprime borrowers, and results for nonparametric specifications of the main regressions. Finally, Section 6 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 $t = 1$ immediately after markets close, but before they experience the earnings shock. They consume again at $t = 2$ after markets close.

Banks extend two kinds of loans to households: secured and unsecured, at $t = 1$, 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:

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

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. Note that we are 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, thus 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 $e_i.w_{i1}$, where e_i is a nonnegative random variable, between $t = 1$ and $t = 2$.

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 lending costs are normalized to zero. Competitive equilibrium will ensure they break even in expectation, and credit contracts will maximize expected utility of each borrower subject to a break-even constraint for banks. For this reason we do not need to specify the endowments of banks.

Equilibrium will require 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, with no scope for construction of new homes between the two dates.

At $t = 1$ there are three markets: 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 $\frac{1}{\delta} - 1$, and will therefore incorporate anticipated default risk.

After the household consumes at $t = 1$, it experiences the earnings shock $e_i.w_{i1}$. Then $t = 2$

arrives. It now has an endowment

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

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 cdf $F_2(\hat{w}_2; 1)$, 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 (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 $v(w) - v(w - R_u) = \psi$.

In the following we consider first a simpler case where the indirect utility function is concave in wealth.

Proposition 1 *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.

This result turns out to hold more generally, even when the indirect utility function is not concave. We note this below.

Proposition 2 *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 leftward shift in the distribution over post-default-decision wealths of households.

Proposition 3 *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).

HPI We use the OFHEO index which can be downloaded from the http://www.fhfa.gov/Default.aspx?Page=87. More precisely, we use the series for the Metropolitan Statistical Areas and Divisions through 2010Q1 which is published every quarter. In our analysis we use data from 1995 onwards. The OFHEO house price index is a repeat transaction index and to determine the house price in a certain region, both sales data and appraisals are used. This house price index uses a weighted repeat sales methodology used in the Case-Shiller index. The appraisals come from conventional conforming mortgage transactions from Freddie Mac and Fannie Mae.

AHS The American Housing Survey is a panel survey which follows the same set of housing units over time. We use the national survey which is carried out every second year. In the survey, current inhabitants are asked a series of questions and we restrict the analysis to owner occupied units as those have information on house values. 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.

In addition to these data we use information from the 2000 Census; , the 2005 MSA median house values is taken from the 2005 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 only reports house price information at an aggregate level, namely the regional (MSA) house price index. In contrast to this, the AHS American Housing Survey reports house prices at the housing unit level. Both AHS and HPI are restricted to MSAs. [7] One advantage of the HPI index is that it is reported every quarter while the AHS is only reported every odd year. The AHS has information on the value of the house in question as well as distinct mortgage information (in particular the current interest rate on the mortgage) and income information of the house owner. The AHS also reports home equity line of credit.

3.2 Specification

Predictions on our endogenous variables of interest -house price growth, interest rates, and home equity credit line- all depend on *BAPCPA exposure* of the region in question. We measure *BAPCPA exposure* in a variety of ways. The first 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. Since pre-reform homestead exemption limits vary across states, this main specification allows us to filter out nationwide year specific macro shocks by employing a difference-in-difference specification. This difference-in-difference specification runs a panel regression of home price growth

[7] 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.

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.

Unfortunately, using the size of the homestead exemption limits as a measure of *BAPCPA exposure* is not as straightforward as it may initially seem. First, some states have unlimited homestead exemptions, raising the issue of how to specify the limit for such states. We make use of the following two options.

1. 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 relevant specification is then

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

   where *exposure* is the size of the homestead exemption limit (we measure this in logs, absolute values, relative to the median house value, and relative to the median income of the region) and our dependent variables y are house price growth, interest rate on the mortgage, and home equity line of credit. 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[8]

2. In the second option, *BAPCPA exposure* is $min\{\texttt{median house price, homestead exemption limit}\}$ where we use the median house price of the region and the homestead exemption limit at the state level. The 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:

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

where the main difference is that *exposure* is now $min\{\texttt{median house price, homestead exemption limit}\}$ which is again either measure in logs, in absolute values, relative to the median house price, and relative to the median income.

Personal vs. Geographic exposure effects

[8]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.

The second feature of the reform is helpful to further refine the specification, and separate effects of personal exposure from geographic spillover effects, resulting from exposure of others residing in the same neighborhood. The reform only directly affects the 'rich', i.e. those with above the state median income. An additional refinement of *BAPCPA exposure* looks at personal exposure of individuals -which we call the *Personal Exposure (PE) effect*. We can calculate this for each homeowner in the AHS sample. The average of the *PE effect* within a region is defined as the *Geographic Exposure (GE) effect* for that region.

More specifically:

PE effect In a first step, the exposure of every individual is calculated. To do so, we first calculate the home equity of every household.[9] 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 PE effect is then calculated as $min\{\texttt{relevant lost homeequity, exemption limit}\}$.

The GE effect is defined to be the average PE effect in the region. Since house markets are segmented to a certain degree, we take the average of the PE effects of each individual over the 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". Every MSA-local market combination then forms a relevant housing market.

Using these definitions, we arrive at the specification

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

are displayed.

Additional controls again include time and household fixed effects. Importantly, only including year fixed effects 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. For example, the PE effect is $min\{\texttt{relevant lost homeequity, exemption limit}\}$. Therefore, we include year dummies in-

[9] 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.

teracted with `relevant lost homeequity` . This filters out macro shocks that affect individuals with low relevant lost home equity differently than 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.

4 Empirical Results

4.1 Graphical illustration

Due to the time dimension of the HPI data, it is possible to start with a visual analysis 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 the house price growth over time. There is a clear change of house price growth regime around the time of the reform. Figure 3 plots average growth rates over time for states with high and states with 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.

It is difficult to tell from these figures whether the drop in the house price growth was caused by the reform or by some other macroeconomic factor that happens to coincide with the reform. So we examine whether house price growth dropped more for states with higher exemption limits. A graphical example in the spirit of our identification strategy is presented in Figure 4. There the difference in house price growth between high and low exemption limit states is plotted. High exemption limit states are those with exemption limits above the median, i.e. $40,000 and low exemption limit states are those with exemption limits above $40,000. Figure 4 shows that there has been a sharp drop in the growth rate difference of high and low exemption limit states. After the reform, the house price growth of high exemption limit states dropped more strongly than low exemption limit states. In what follows, we will analyze the differential effect of low and high exemption limit states more rigorously in a regression framework.

4.2 Regression results

Our main results are reported in table 4 and table 5 Panel A-C. We discuss the effect of the reform in the order of the dependent variables.

House price growth

HPI data We are able to analyze the effect of the BAPCPA reform on house price growth using our two samples. Coefficient estimates using the HPI data are reported in 4. We use two variants of our main specification including a dummy for unlimited exemption limits where appropriate.

The coefficient estimates are generally negative which is consistent with the view that higher exposure to the BAPCPA reform as measured by the different proxies as set out in the upper row of table 4 leads to a decrease in the growth rates. Most of the estimates are quantitatively and statistically significant. The coefficient in column (1), row (1) implies that an increase in exemption limits by one standard deviation leads to a decrease in the growth rate of 1/3 of the standard deviation of growth rates (measured over the entire time period). Note that the difference in growth rates before and after the reform is approximately .7 standard deviations of the growth rate (measured over the entire time period). The coefficient in column (2), row (1) implies that going from a zero exemption limit to the median exemption limit of $40,000 will cause an (additional) decrease in the growth rate of approximately .2% or approximately 20% of the average growth rate over the entire time period (the mean of the growth rate is .92 as can be inferred from table 1 Panel A. Going from an exemption limit of zero to $100,000 (the 75 th percentile) implies an further reduction of the growth rate of approximately .42, approximately half of the full sample mean growth rate. The result in column (3) imply that a one deviation change in the exposure measure leads to a 5% deviation change of the growth rate. Column (4) implies that a one deviation change in the exemption limit leads to a 20% deviation change in the growth rate. The implied effects for the AVEX measures of exemption limits are comparable. For example column (5) implies that a one deviation change in the exposure measure leads to a 42% standard deviation change of the price growth.

While these effects are generally economically and statistically meaningful, there are two exceptions. On the one hand, the coefficients for the unlimited exemption limits are generally insignificant and have a positive sign in two cases. On the other hand, the coefficient in columns (3) and (7) are insignificant and in both cases, the exemption limit is weighted by the median house value in 2005. The effect on the unlimited states may be confounded by some underlying features of the unlimited exemption limits states. As unlimited exemption states tend to be land rich, it may be the muting effect of rural areas which suppresses some of the effect. This interpretation is supported by the results found in table 8 Panel A where we control for the influence of variables that have predict exemption limits and in which case the unlimited dummies turn out be significant.

AHS Data

Estimates of the effect of BAPCPA exposure on house price growth are displayed in table 5 Panel A and are comparable with respect to their statistical significance. To understand the economic significance, note that the growth rate of the HPI data is on a quarter to quarter definition while the growth rate for the AHS data is over a 2 year period. The annualized mean growth rates in the HPI and AHS data are comparable and in the range of 4-5% on an annual basis (tables 1 Panel A and 1 Panel B). It turns out that on comparable measures, the economic significance of the AHS results are much larger. The coefficient on the limited exemption limits in column (1) on an annualized basis is approximately -4 for the AHS data and -1.5 for the HPI data. Similarly, the dummy for the effects of an unlimited exemption limit implies an effect of -11.5 for annualized growth rates in the AHS while it is an effect of -4 for annualized growth rates in the HPI data.

To further understand the economic significance, note that the implied effect of the reform for a state with a zero exemption limit is zero (and zero exemption limit states effectively serve as our control group). On the other hand, states have on average a value of $log(ex.) = 2.37$ which implies a growth rate decrease of approximately -19.74. This is more than twice the average sample growth rate of 9.95 (table 1 Panel B). A similar magnitude can be found in the estimations in column (5). Column (1) also implies that going from a zero exemption limit state to a state with unlimited exemption limits reduces the growth rate by a factor exceeding the average growth rate of that time period. A one standard deviation increase in log(ex.) implies a 15% standard deviation increase in the growth rate. This seemingly smaller magnitude in the AHS data is due to the fact that the standard deviation of the growth rate in the AHS data is much higher. AHS growth rate is measured at the individual housing unit while the HPI data is measured at the regional level and a lot of noise is already filtered out.

The coefficient estimates for the unlimited exemption limits using AHS data are economically more meaningful and statistically significant at the 10% level in the first column. At the same time, the effects of exposure normalized by the MSA median income measured in 2005 is still weak but now significant at the 10% level in column (3).

It is reassuring that results concerning house price growth are comparable using both the HPI and the AHS data as both data sets have different disadvantages. 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- where 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.

Interest rates The effects of BAPCPA exposure on the interest rate paid on the first mortgage is described in table 5 Panel B. 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 interest rate will increase by .18% (an increase of .18 percentage points). This compares to 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 of all, most of the standard deviation in interest rates is in the cross section and this effect is absorbed using the 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 will 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 10 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.

Home Equity Lines of Credit The results on home equity lines of credit are displayed in table 5 Panel C. Comparable to the interest rate effects, these results are economically less important than the house price growth effects. For example, the statistically significant effect in column (2) implies that an increase of the state exemption limit by $100,000 leads to a decrease of .1 of log of home equity line of credit. This compares to the mean of log of home equity line of credit of 10. In contrast to all other specifications using AHS data, the effects in columns (1) and (5) are not significant. Also, effects on unlimited exemptions are insignificant. This time, it turns out that effects for exposure measures normalized by median income or median house value (at the MSA level) are statistically more significant. Overall, the evidence on home equity credit line is more mixed as compared to the effects on house price growth rates and interest rates. While we do find significant effects in some specifications and for some measures of exposure, there is no statistically significant effect for others.

As was already pointed out earlier, we see two major concerns. On the one hand, a pre-existing housing bubble with a contemporaneous burst of the bubble may be interfering with our estimates. On the other hand, we may have an endogeneity problem with respect to the pre-reform exemption limits. We will discuss the robustness checks concerning these two objections against our identification strategy now.

4.3 Identification issues

There are two main issues with our identification. On the one hand, there may have been a housing bubble prior to the reform and the prick of the bubble afterwards which may bias our estimations. On the other hand, we may have an endogeneity problem with respect to pre-reform homestead exemption limits. We discuss these two objections against our identification strategy in this section.

4.4 Pre-trend controls

Consider first the possibility of a pre-existing housing bubble. This would be particularly worrisome if the bubble existed prior to the time of the reform and pricked approximately at the time of the reform for reasons unrelated to the reform. Furthermore, the graphical illustration in figure 3 shows that even low exemption limit states were facing a drop in the growth rate after the reform.

One way to address this problem would be to control for factors that favor or strengthen a bubble. Unfortunately, there are several aspects that may be favoring bubbles. Furthermore, the existence and the pricing in times of a bubble -by its very nature- is not related to fundamentals and hence difficult to control for. As we do not and cannot control for all potential causes of a bubble, we follow a more direct road. A defining feature of a bubble is that prices are too high. If the bubble grew in the years prior to the reform, a higher increase in the growth rate over time of house prices prior to the reform will then capture the existence and severity of a bubble. As prices go up when a bubble is build, they must go down when the bubble burst. Hence, a higher increase of prices during the building of a bubble will lead to a stronger decrease of prices when the bubble burst. Consequently, we estimate time trends in the average growth rates before the reform as a measure of the price increase when the bubble started. This predicted time trend of the growth rate is then interacted with a post dummy (which is one after the reform) and this interaction term is included in our regression. The results for the HPI data are reported in table 6 Panel A.

The effect of adding the pre-trend control is that the statistical significance of BAPCPA exposure is hardly altered. At the same time, the economic significance is slightly reduced as compared to the main specification reported in table 4. Interestingly, the pre-trend control is highly significant. It implies that indeed those regions with a high time trend of the growth rates in the years 2001-2005 witnessed a high drop in the growth rates after the reform. Overall, our main results are robust and seem not to be driven by bubble effects alone.[10]

Table 6 Panel B displays the results with similar pre-trend controls using AHS data. Here, the pre-trend is estimated at the relevant housing market (as explained in the discussion of GE effects each MSA consists of three relevant housing markets). The impact of adding pre-trend controls on the AHS house price growth results are comparable to those with the HPI data. The effects on house price growth remain statistically and economically almost unchanged.

Tables 6 Panel C and table 6 Panel D report results where the pre-trend in house price growth is interacted with a post dummy and added as a control in the main interest rate and home equity line of credit regressions. When it comes to interest rate results, adding pre-trend controls leaves the magnitude of the estimated effects virtually unchanged but helps to reduce standard errors.

Overall, we view the results on pre-trend controls as encouraging. In particular our results for our main variable of interest, the house price growth remain economically and statistically significant and the effect of BAPCPA exposure goes over and above the plain effects of a housing bubble.

4.5 Exemption limit determinants

If one accepts the view that the change in growth rates was caused by the reform, then our specification will underestimate the impact of the reform as the effect on the low exemption limit states will be captured by the time fixed effects. On the other hand, it may be that the change in the growth rate was due to some other macro economic effect which happened around the time of the reform -a reasonable candidate being the existence and burst of a housing bubble. This causes problems for our identification if the degree of the shock to house price growth (not due to the reform) is correlated with factors which are in turn correlated with exemption limits -an endogeneity problem. We address these worries as follows.

In a first step, we will argue that -in line with the literature- it is difficult to see particular patterns in the way exemption limits vary across states. This is also done in a formal regression analysis of exemption limits as the dependent variable. Next, we control for those variables that might happen to be significantly correlated with exemption limits, in the main regression by interacting them with a post-BAPCPA time dummy. It turns out that our main results are unaffected by these additional

[10]The regressions are robust with respect to several non-linear specifications of the pre-trend controls not shown here to conserve space.

controls.

Consider first table 2 which displays a list of states with their respective exemption limits, ordered by region. In every region, there is considerable variation of exemption limits. One notable pattern is the fact that unlimited exemption limits are only present in the South and the Midwest. Unlimited exemption limits seem to be particularly likely in land rich states. Similarly in Table 3 Panel B it is evident that there is substantial variation in exemption limits. In particular, 6.5% of the HPI sample has zero exemption limits and approximately 20% unlimited. It is remarkable though that finite exemption limits above $50,000 are less frequent than below $50,000.

To further counter worries of endogeneity problems of the reform, we examine how other state attribute are correlated with exemption limits. The results are displayed in Table 7. It reports estimates from a cross section of states where the log of the 2005 exemption limit is explained by a variety of endogenous variables. In this regression, the value of exemption limits of unlimited states is set to $1,000,000. The first column in table 7 shows that the best predictor of exemption limits are historic values of exemption limits. This is in line with the findings in the literature. For example, Hynes et al. (2004) try to explain 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 our regression which includes historic values of exemption limits as controls we see relatively few significant explanatory variables. Only the percent of vacant houses are significant at the 5% level whereas density and unemployment are significant at the 10% level. In particular, it is difficult to detect a clear pattern in the regressions, only two of the controls are significant in at least two of the specifications.

This table is helpful as it allows us to understand which of these covariates may be correlated with exemption limits. In a next step, we take all variables significant in any one of the specifications at least once at the 10% level. These latter variables are then interacted with a post-BAPCPA time dummy and used as additional controls in our main specification. Results of this step are given in table 8 Panel A and table 8 Panel B to table 8 Panel D.

It turns out that the results on house price growth and interest rates are unaffected by this robustness check. Table 8 Panel A reveals that the magnitude of the effects of BAPCPA exposure for the limited exemption limits are somewhat reduced and comparable to the effects in the specification with pre-trend controls. In contrast to this, the economic magnitude of the effect for unlimited exemption limits are increased and all are now of the expected sign. When it comes to statistical significance, no clear pattern of change can be seen from comparing the main regressions with results displayed in table 8 Panel A. The unlimited dummy in column (1) and the coefficient in column

(7) now become significant at the 5% and 10% significance level while the coefficient in column (4) is now only significant at the 10% level. As the economic magnitude is reduced, this indicates that controlling for determinants of exemption limits allows to substantially reduce the standard errors.

Overall, our analysis of exemption limits did not reveal any clear pattern as to what is correlated with exemption limits. Furthermore, the robustness checks concerning pre-trends of growth rates and exemption limit determinants indicate that the results of our main specification in tables 4 and tables 5 Panel A to 5 Panel C are not driven by a pre-existing housing bubble, or pre-reform variations in exemption limits proxying for other observable state characteristics.

5 Additional Aspects

The measures of BAPCPA exposure we have employed so far have been fairly straightforward as they only depended on the pre-reform homestead exemption limits. For the case of the HPI data, they are also the only measure we can construct with the data we have available. These straightforward and easy to interpret measures have one particular disadvantage. They lump together geographic exposure and personal exposure effects. One distinctive feature of our model are the geographic exposure effects of the reform and the feedback effects via a liquidity spiral. The differentiation of personal and geographic exposure effects of the reform also distinguishes our argument of how the BAPCPA reform stood at the beginning of the subprime crisis from the argument discussed in Li, White, and Zhu (2009) and Morgan, Iverson, and Botsch (2008).

We next discuss the GE effect results and followed by a focus on subprime borrowers as they have been at the core of the housing crisis.

5.1 Geographic Exposure Effects vs. Personal Exposure Effects

Table 9 displays the results for regression specifications which split up the PE and GE effects. It turns out that 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 are 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 post mean effect has the following implications. House price growth will be decreased by approximately 10 as compared to a sample mean of house price growth of 9.95. Interest rates will be approximately .5 higher, compared to a sample mean of 6. The effect on home equity credit line is again insignificant.

Concerning house prices, the spillover effects thus appear to dominate individual circumstances of homeowners. The results also show that personal exposure plays a minor role in the determination of the mortgage interest rate. This is consistent with the evidence found in Li, White, and Zhu (2010) who find mixed evidence of the role of personal exposure for subprime borrowers. Within the category of subprime borrowers, effects on housing prices or default rates would not be expected to differ significantly between those that were and were not personally exposed. On the other hand, comparing between subprime and prime borrowers, we would expect a stronger effect of a given housing price decline in the neighborhood on default rates (and hence on mortgage interest rates) for the former group. We check this prediction next.

5.2 Impact on Subprime Borrowers

Unfortunately, the AHS does not report the FICO score for a household. Therefore, we have to construct a dummy variable for subprime borrowers using information from the data. We do so by relying on two data items. The interest rate and the debt-to-income ratio (AHS data does not directly report the debt-to-income ratio but allows us to generate the debt-to-income ratio as we have information on income and interest cum principal payments). 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, we end up with a substantially smaller sample size.[11]

Table 10 reports results for subprime borrowers. There are two interesting patterns. First, the effect of BAPCPA exposure on house price growth does not seem to differ between subprime and prime borrowers. The coefficient on log(ex.) are almost identical and 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 so are exposed to the same geographic effects.

When it comes to 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.

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

5.3 Additional Robustness Checks

We end with two additional robustness checks. On the one hand, we consider non-parametric versions of our regressions to see whether our results are driven by the assumption of a particular functional form of the effect. Next, we add several time varying covariates in our main regressions using the AHS sample.

The results are robust to a non-parametric analysis of the effects. Tables 11 Panel A to 11 Panel D report coefficient estimates in which states are lumped into groups of exemption limits. The definition of groups become apparent in the very first column. We then run our main specification with time and regional (household) fixed effects and add interaction terms of group dummies and post reform dummies. Due to collinearity, one of the latter interaction terms have to be dropped and we drop the interaction term with the blank field on the diagonal. The main lesson from these regressions is that house price growth is more negatively affected by higher exemption limits. Particularly strong results occur for state the exemption limits of $30,000, $30,000, and $75,000 - $500,000. Negative signs on the lower triangular of the table indicate that higher exemption limits lead to higher drops in the house price growth after the reform. Each coefficient corresponds to the test whether the effect is significantly different from the (omitted) base effect on the coefficient which is zero.

To go further down the non-parametric group, we take the regression of column (1) of each table and make a rank correlation between the rank of the exemption limit and the rank of the estimated coefficient (the coefficient on the diagonal is then set to zero). This non-parametric test reported in table 12 also indicates that higher exemption limits lead to lower post reform growth and higher post reform interest rates.

Table 13 adds additional time varying controls like household income, regional income, unemployment, or crime to our main specifications. The results are unchanged.

In table 14 we also cluster along a variety of other dimensions (smaller geographic units like MSA or household, or year) and clustering at the state level usually leads to the highest standard errors. Hence, all our results are robust to other levels of clustering.

6 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 methodology controlled for common macro effects of changes in interest rates, as well as for measures of a pre-2005 housing bubble, or other correlates of bubbles or proportion of subprime borrowers such as rates of construction of new homes, housing vacancy rates and unemployment rates.

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.

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Appendix

Proof of Propositions 1 and 2: 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 $w = w^H(p)+R_u$.

Hence if the conditions of Proposition 1 apply, the default decision d_u follows a simple cutoff rule given by that Proposition.

If the conditions do not apply, then 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 2 follows. *QED*

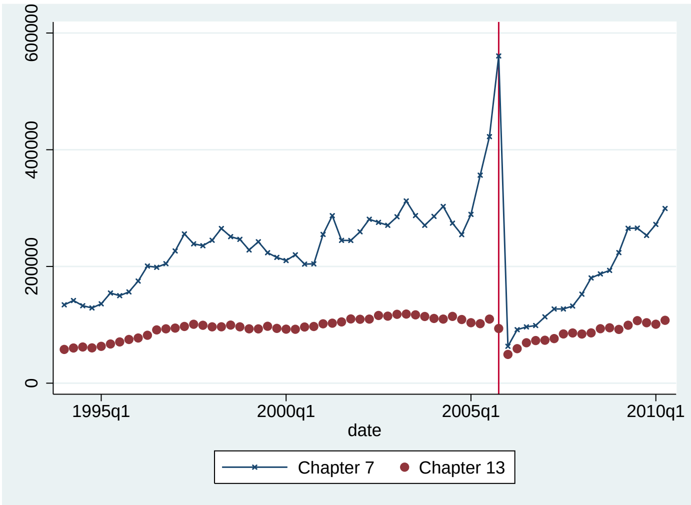

Figure 1: Quarterly filings for bankruptcy using chapter 7 and chapter 13.

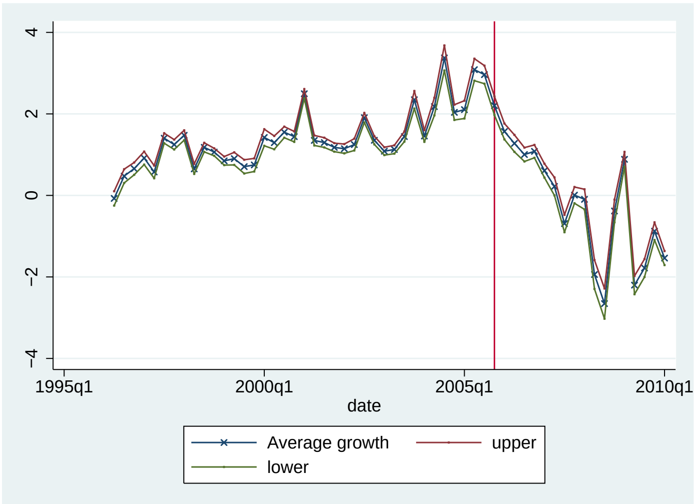

Figure 2: Average growth rates over time.

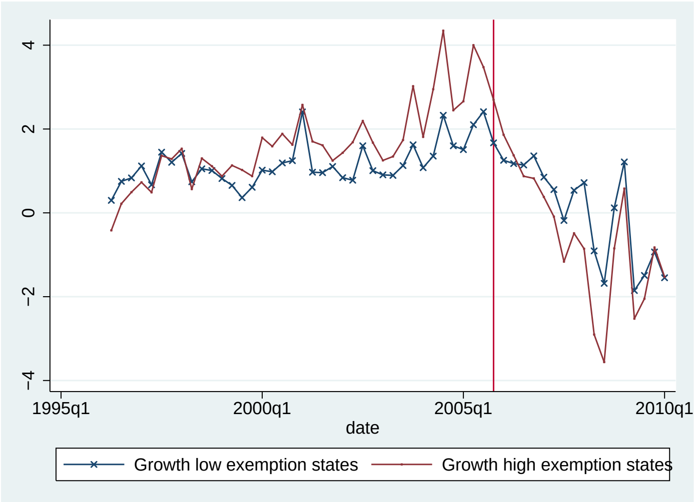

Figure 3: Average growth rates for high exemption limit states (exemption limit above median) and low exemption limit states (exemption limit below median).

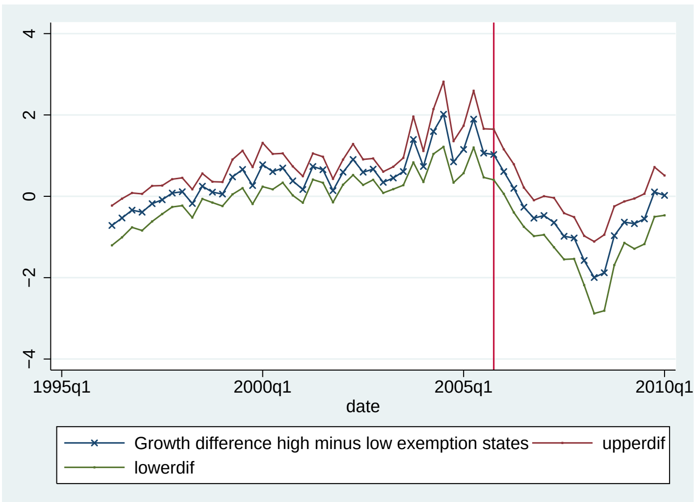

Figure 4: Difference in growth rates between high and low exemption limit states (growth rate high exemption limit states minus growth rate low exemption limit states).

Table 1 Panel A: Summary Statistics House Price Index (HPI) sample.

	All		Post reform		Pre reform	
	mean/sd	min/max	mean/sd	min/max	mean/sd	min/max
growth	0.92	-20.7/13.6	-0.018	-20.7/9.9	1.35	-7.68/13.6
	(2.09)		(2.63)		(1.62)	
index	148.2	84.9/364.2		/		/
	(43.8)					
year	2002.1	1995/2010	2007.4	2005/2010	1999.8	1995/2005
	(4.40)		(1.42)		(3.04)	
log(ex.)		/	2.41	0/6		/
			(1.71)			
ex./value		/	0.19	0/2.9		/
			(0.32)			
ex./inc.		/	0.00061	0/0.008		/
			(0.0012)			
log(AVEX)		/	3.29	0/6.2		/
			(1.40)			
AVEX/value		/	0.37	0/1		/
			(0.38)			
AVEX/inc.		/	0.00097	0/0.007		/
			(0.0012)			
Observations	20593		6422		14171	

Standard deviation in brackets.

This table provides summary statistics for information used in regression using the house price index. Growth rate is the quarterly growth rate, log(ex.) is the log of the homestead exemption limit for the state in question measured in 2005 and the homestead exemption is measured in thousand dollars. The variable called value is the median house value of the MSA taken from the 2000 decenial census measured in thousand dollars, the variable inc. is the median income in dollars from the census, and $AVEX_i$ is measured as $\min\{exemption_i, \texttt{median house price}_i\}$.

Table 1 Panel B: Summary Statistics American Housing survey.

	All		Post reform		Pre reform	
	mean/sd	min/max	mean/sd	min/max	mean/sd	min/max
growth	9.95	-1359.2/1442.0	-2.82	-1359.2/1442.0	15.7	-1343.1/1424.8
	(113.8)		(123.7)		(108.4)	
intrate	6.49	1/20	5.75	1/20	6.78	1/20
	(1.64)		(1.41)		(1.63)	
loghecr1	10.7	0.69/13.4	10.9	0.69/13.4	10.5	0.69/13.4
	(1.33)		(1.25)		(1.34)	
peeffect	7092.1	0/1540794	24342.9	0/1540794	0	0/0
	(43988.7)		(78879.4)		(0)	
geeffect	8347.3	0/295936.5	28153.4	0/295936.5	0	0/0
	(23961.0)		(37132.6)		(0)	
year	2003.0	1997/2009	2008.1	2007/2009	2001.1	1997/2005
	(3.94)		(0.99)		(2.76)	
log(ex.)		/	2.37	0/6.2		/
			(1.86)			
ex./value		/	0.00016	0/0.003		/
			(0.00032)			
ex./inc.		/	0.00095	0/0.02		/
			(0.0019)			
log(AVEX)		/	3.10	0/6.2		/
			(1.74)			
AVEX/value		/	0.29	0/1		/
			(0.38)			
AVEX/inc.		/	0.20	-2.46/26.6		/
			(0.72)			
Observations	197962		50743		144237	

Standard deviation in brackets.

This table provides summary statistics for information used in regression using the American Housing Survey (AHS). Growth rate is the two year growth rate, log(ex.) is the log of the homestead exemption limit for the state in question measured in 2005 and the homestead exemption is measured in thousand dollars. The variable called value is the median house value of the MSA measured in thousand dollars calculated within the AHS in 2005, the variable inc. is the median income in dollars calculated using the 2005 AHS data and $AVEX_i$ is measured as $\min\{exemption_i, \texttt{median house price}_i\}$.

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 2005 homestead exemption limits measured in thousand dollars.

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

	(1)		
	Exemption limits (homestead)		
	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	
Observations	20593		

Homestead exemptions limits are from 2005 and measured in thousand dollars.

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

	(1)		
	Exemption limits (homestead)		
	b	pct	cumpct
0	29586	14.95	14.95
5	13283	6.71	21.66
7.5	16659	8.42	30.07
10	4788	2.42	32.49
15	4804	2.43	34.92
20	2191	1.11	36.02
25	2975	1.50	37.53
30	12716	6.42	43.95
40	6050	3.06	47.00
45	1631	0.82	47.83
50	56867	28.73	76.56
75	1982	1.00	77.56
150	4571	2.31	79.87
200	3768	1.90	81.77
350	1335	0.67	82.44
500	4933	2.49	84.93
unlimited	29823	15.07	100.00
Total	197962	100.00	
Observations	197962		

Homestead exemptions limits are from 2005 and measured in thousand dollars.

Table 4: Effect of BAPCPA reform on house price growth using HPI data. The dependent variable is growth in all regressions.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	log(ex.)	ex.	ex./value	ex./inc.	log(AVEX)	AVEX	AVEX/value	AVEX/inc.
expos.*post	-0.389**	-0.00491**	-0.341	-324.2**	-0.307**	-0.00652***	-0.126	-394.7***
	(-2.46)	(-2.45)	(-0.88)	(-2.53)	(-2.11)	(-3.07)	(-0.14)	(-2.75)
unlimit.*post	-1.063	-0.125	0.146	0.0128				
	(-1.17)	(-0.14)	(0.17)	(0.02)				
TimeDummy	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
r2	0.343	0.339	0.319	0.333	0.337	0.339	0.322	0.336
N	20591	20591	19150	20103	20164	20164	19577	19981

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

This table reports coefficient estimates of specifications that use various measures of exposure to the BAPCPA reform. The dependent variable is the logarithmic growth of the house price index $log(hpi_t/hpi_{t-1})$. The first to fourth columns report results from a specification $growth = \alpha_0 + \alpha_1 \times post \times exposure + \alpha_2 \times post \times unlimited + X + \epsilon$ where exposure is the measure denoted in the first line of the table. Unlimited is a dummy variable which takes the value one if the measure of BAPCPA exposure is infinite, i.e. if the pre-reform homestead exemption is unlimited. On the right hand side of the table, all measures of exposure are limited and the unlimited dummy is dropped. Additional controls include time and region fixed effects and are denoted by X. The error term is ϵ.

The measures of exposure to the reform are presented in two groups. The **LHS** of the table uses the 2005 pre-reform homestead exemption limit of the state in question. The **RHS** of the table relates the pre-reform exemption limit to the median house price of the region in question. More specifically, the average exposure of the region i called $AVEX_i$ is measured as $\min\{exemption_i, \texttt{median house price}_i\}$. The measure of exposure is then entered in logs (in column 1 and 5), in absolute values (in column 2 and 6), relative to the median house price of the region (in column 3 and 7), or relative to the median pre-reform income of the region (in column 4 and 8).

Table 5 Panel A: Effect of BAPCPA reform house price growth using AHS data. The dependent variable is growth in all regressions

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	log(ex.)	ex.	ex./value	ex./inc.	log(AVEX)	AVEX	AVEX/val.	AVEX/inc.
expos.*post	-8.330**	-0.0995***	-18487.6*	-4357.4***	-6.666***	-0.104***	-7.586	-6.833***
	(-2.58)	(-4.39)	(-1.90)	(-4.73)	(-2.91)	(-3.17)	(-0.49)	(-3.14)
unlimit.*post	-23.52*	-4.442	-2.859	-4.322				
	(-1.98)	(-0.31)	(-0.18)	(-0.30)				
TimeDummy	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
r2	0.234	0.233	0.232	0.233	0.233	0.233	0.232	0.115
N	58836	58836	58836	58836	58836	58836	58836	45216

t statistics in parentheses. SE clustering the state level. Household fixed effects included.* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 5 Panel B: Effect of BAPCPA reform on the interest using AHS data. The dependent variable is the interest rate in all regressions.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	log(ex.)	ex.	ex./value	ex./inc.	log(AVEX)	AVEX	AVEX/val.	AVEX/inc.
expos.*post	0.0275**	0.000450**	162.6***	24.61***	0.0288**	0.000431	0.147**	-0.0235
	(2.17)	(2.41)	(3.51)	(3.07)	(2.50)	(1.59)	(2.37)	(-1.29)
unlimit.*post	0.180**	0.122*	0.132*	0.128*				
	(2.47)	(1.85)	(1.98)	(1.94)				
TimeDummy	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
r2	0.665	0.665	0.665	0.665	0.664	0.664	0.665	0.594
N	52129	52129	52129	52129	52129	52129	52129	34092

t statistics in parentheses. SE clustering the state level. Household fixed effects included.* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 5 Panel C: Effect of BAPCPA reform on the log of home equity line of credit using AHS data. The dependent variable is the log of home equity line of credit in all regressions.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	log(ex.)	ex.	ex./value	ex./inc.	log(AVEX)	AVEX	AVEX/val.	AVEX/inc.
expos.*post	-0.0459	-0.00108***	-295.8***	-50.17***	-0.0351	-0.000956	-0.204	-0.0933**
	(-1.07)	(-4.68)	(-3.82)	(-4.53)	(-0.83)	(-1.25)	(-0.81)	(-2.47)
unlimit.*post	-0.0562	0.0178	0.0193	0.0191				
	(-0.26)	(0.10)	(0.11)	(0.11)				
TimeDummy	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
r2	0.733	0.733	0.733	0.733	0.732	0.733	0.732	0.698
N	7988	7988	7988	7988	7988	7988	7988	5939

t statistics in parentheses. SE clustering the state level. Household fixed effects included.* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

This table reports coefficient estimates of specifications that use various measures of exposure to the BAPCPA reform using AHS data. The first to fourth column report results from a specification $y = \alpha_0 + \alpha_1 \times post \times exposure + \alpha_2 \times post \times unlimited + X + \epsilon$ where exposure is the measure denoted in the first line of the table. Unlimited is a dummy variable which takes the value one if the measure of BAPCPA exposure is infinite, i.e. if the pre-reform homestead exemption is unlimited. On the right hand side of the table, all measures of exposure are limited and the unlimited dummy is dropped. Additional controls include time and housing unit fixed effects and are denoted by X. The error term is ϵ. The dependent variables are growth $growth = log(value_t/value_{t-1})$, interest rate, and the log of home equity credit line (loghecr1) and are presented in the different panels of the table.

The measures of exposure to the reform are presented in two groups. The **LHS** of the table uses the 2005 pre-reform homestead exemption limit of the state in question. This is measured in logs (in column 1), in absolute terms (in column 2), relative to the median house price of the region (in column 3), or relative to the median pre-reform income of the region (in column 4). The **RHS** of the table relates the pre-reform exemption limit to the median house price of the region in question. More specifically, the average exposure AVE of the region is measured as $\min\{exemption, \texttt{median house price}\}$ where the median house price is constructed using the AHS data. AVE is again measure in logs, in absolute value, relative to the median house price value, and relative to the median income of the region in question.

Table 6 Panel A: Effect of BAPCPA reform on house price growth using House Price index data. Robustness check concerning linear pre-trends. The dependent variable is house price growth in all regressions.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	log(ex.)	ex.	ex./value	ex./inc.	log(AVEX)	AVEX	AVEX/value	AVEX/inc.
expos.*post	-0.283***	-0.00393***	-0.0724	-214.2***	-0.211**	-0.00480***	0.149	-235.3*
	(-2.73)	(-6.61)	(-0.20)	(-4.94)	(-2.25)	(-4.09)	(0.29)	(-1.94)
unlimit.*post	-0.636	0.0360	0.226	0.134				
	(-1.35)	(0.07)	(0.46)	(0.28)				
afterplainpretrend	-6.298***	-6.591***	-6.944***	-6.243***	-6.123***	-6.018***	-7.185***	-6.000**
	(-3.75)	(-3.53)	(-3.32)	(-3.16)	(-3.28)	(-2.87)	(-3.75)	(-2.64)
TimeDummy	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
r2	0.366	0.365	0.347	0.356	0.358	0.360	0.353	0.355
N	20591	20591	19150	20103	20164	20164	19577	19981

t statistics in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 6 Panel B: Effect of BAPCPA reform on growth using AHS data: pre-trend controls.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	log(ex.)	ex.	ex./value	ex./inc.	log(AVEX)	AVEX	AVEX/val.	AVEX/inc.
expos.*post	-8.220**	-0.104***	-18465.9*	-4311.6***	-5.655**	-0.0836*	-1.169	-6.130**
	(-2.68)	(-4.92)	(-1.79)	(-4.26)	(-2.20)	(-1.78)	(-0.08)	(-2.55)
unlimit.*post	-13.96*	4.930	6.603	5.200				
	(-1.72)	(0.49)	(0.57)	(0.51)				
afterplainpretrend	-1.686***	-1.805***	-1.767***	-1.764***	-1.531***	-1.604***	-1.667***	-1.710***
	(-5.24)	(-4.91)	(-4.50)	(-4.67)	(-4.34)	(-4.04)	(-3.83)	(-4.03)
TimeDummy	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
r2	0.256	0.256	0.255	0.255	0.255	0.255	0.254	0.132
N	50962	50962	50962	50962	50962	50962	50962	39940

t statistics in parentheses. SE clustering the state level. Household fixed effects included.* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 6 Panel C: Effect of BAPCPA reform on the interest rate (intw) using AHS data: pre-trend controls.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	log(ex.)	ex.	ex./value	ex./inc.	log(AVEX)	AVEX	AVEX/val.	AVEX/inc.
expos.*post	0.0282**	0.000447***	160.4***	24.64***	0.0304***	0.000434	0.153**	-0.0287*
	(2.50)	(2.81)	(3.77)	(3.49)	(2.79)	(1.59)	(2.61)	(-1.73)
unlimit.*post	0.194***	0.132**	0.142**	0.139**				
	(2.85)	(2.06)	(2.25)	(2.20)				
afterplainpretrend	-0.00215	-0.00161	-0.00167	-0.00179	-0.00192	-0.00149	-0.00189	-0.00104
	(-0.53)	(-0.42)	(-0.45)	(-0.47)	(-0.46)	(-0.37)	(-0.50)	(-0.28)
TimeDummy	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
r2	0.657	0.657	0.657	0.657	0.657	0.657	0.657	0.590
N	44931	44931	44931	44931	44931	44931	44931	30159

t statistics in parentheses. SE clustering the state level. Household fixed effects included.* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 6 Panel D: Effect of BAPCPA reform on the log of home equity line of credit using AHS data: pre-trend controls.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	log(ex.)	ex.	ex./value	ex./inc.	log(AVEX)	AVEX	AVEX/val.	AVEX/inc.
expos.*post	-0.0359	-0.000761**	-213.8**	-34.50**	-0.0380	-0.000984***	-0.243**	-0.127**
	(-1.07)	(-2.62)	(-2.72)	(-2.53)	(-1.24)	(-3.00)	(-2.13)	(-2.15)
unlimit.*post	-0.242	-0.174	-0.177	-0.175				
	(-1.45)	(-1.18)	(-1.22)	(-1.19)				
afterplainpretrend	0.00219	0.00144	0.00170	0.00185	0.00193	0.00155	0.00212	0.00280
	(0.26)	(0.17)	(0.21)	(0.23)	(0.24)	(0.19)	(0.27)	(0.39)
TimeDummy	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
r2	0.733	0.733	0.733	0.733	0.733	0.734	0.733	0.704
N	7034	7034	7034	7034	7034	7034	7034	5339

t statistics in parentheses. SE clustering the state level. Household fixed effects included.* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

This table reports coefficient estimates of our main specification with an additional pre-trend of house price growth control. The linear pre-trend is obtained by taking the time trend of house price growth in a region (HPI) or relevant market (AHS) between 2001 and 2005. 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.

Panel A uses HPI data and has the growth rate of house prices as the dependent variable, Panel B-D use AHS data and the dependent variables are house price growth, interest rate, and the log of home equity line of credit, respectively.

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 Panel A: Robustness HPI growth concerning exemption limit determinants. Dependent variable is growth in all regressions.

	(1) log(ex.)	(2) ex.	(3) ex./value	(4) ex./inc.	(5) log(AVEX)	(6) AVEX	(7) AVEX/value	(8) AVEX/inc.
expos.*post	-0.236**	-0.00331*	-0.188	-239.0*	-0.257**	-0.00611***	-0.631*	-423.6***
	(-2.36)	(-1.70)	(-0.58)	(-1.68)	(-2.66)	(-2.94)	(-1.84)	(-3.41)
unlimit.*post	-1.077**	-0.581	-0.529	-0.564				
	(-2.33)	(-1.58)	(-1.46)	(-1.58)				
TimeDummy	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
post*exemcontr	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
r2	0.380	0.380	0.363	0.373	0.376	0.379	0.370	0.378
N	20591	20591	19150	20103	20164	20164	19577	19981

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

Table 8 Panel B: Effect of BAPCPA reform on house price growth using AHS data: controls for exemption limit determinants.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	log(ex.)	ex.	ex./value	ex./inc.	log(AVEX)	AVEX	AVEX/val.	AVEX/inc.
expos.*post	-5.372**	-0.105***	-22934.4***	-4777.3***	-4.884**	-0.118***	-20.26**	-5.116***
	(-2.15)	(-4.01)	(-4.28)	(-4.32)	(-2.38)	(-3.68)	(-2.26)	(-2.89)
unlimit.*post	-21.77**	-10.93	-14.20	-11.85				
	(-2.05)	(-1.19)	(-1.59)	(-1.32)				
TimeDummy	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
post*exemcontr	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
r2	0.236	0.236	0.236	0.236	0.236	0.236	0.236	0.121
N	58836	58836	58836	58836	58836	58836	58836	45216

t statistics in parentheses. SE clustering the state level. Household fixed effects included. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 8 Panel C: Effect of BAPCPA reform on the interest rate using AHS data: controls for exemption limit determinants.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	log(ex.)	ex.	ex./value	ex./inc.	log(AVEX)	AVEX	AVEX/val.	AVEX/inc.
expos.*post	0.0680***	0.000940***	278.8***	46.90***	0.0264*	0.000309	0.0839	-0.0350*
	(4.18)	(4.18)	(4.49)	(4.71)	(1.82)	(1.05)	(0.96)	(-1.70)
unlimit.*post	0.0247	-0.101	-0.0758	-0.0970				
	(0.29)	(-1.05)	(-0.82)	(-1.02)				
TimeDummy	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
post*exemcontr	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
r2	0.665	0.665	0.665	0.665	0.665	0.665	0.665	0.595
N	52129	52129	52129	52129	52129	52129	52129	34092

t statistics in parentheses. SE clustering the state level. Household fixed effects included. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 8 Panel D: Effect of BAPCPA reform on the log of home equity line of credit using AHS data: controls for exemption limit determinants.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	barlogexh	barexempth	relexeh	relincexeh	logmedianpeh	medianpeh	medianperelh	perelinch
expos.*post	-0.0484	-0.000914	-108.1	-36.10	-0.0484	-0.000935	-0.162	-0.0766
	(-0.98)	(-1.38)	(-0.57)	(-1.16)	(-1.24)	(-1.36)	(-0.81)	(-1.10)
unlimit.*post	-0.284	-0.224	-0.234	-0.227				
	(-0.98)	(-0.81)	(-0.81)	(-0.82)				
TimeDummy	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
post*exemcontr	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
r2	0.733	0.734	0.733	0.733	0.733	0.733	0.733	0.699
N	7988	7988	7988	7988	7988	7988	7988	5939

t statistics in parentheses. SE clustering the 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 and table 5 Panel A-C. Additional controls include a set of interaction terms where a post reform dummy is interacted with the set of control variables from table 7 which are significant at the 10% level at least in one specification.

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

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	growth	growth	growth	intrate	intrate	intrate	loghecr1	loghecr1	loghecr1
log(PE effect)*post	0.389		1.381*	-0.00292		-0.00505	-0.0167		-0.00649
	(0.57)		(1.95)	(-0.60)		(-1.10)	(-0.75)		(-0.18)
log(GE effect)*post		-2.179***	-2.640***		0.0112**	0.0137***		-0.0124	-0.00903
		(-3.16)	(-2.99)		(2.39)	(3.24)		(-0.81)	(-0.28)
TimeDummy	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
r2	0.146	0.169	0.149	0.598	0.601	0.596	0.701	0.709	0.707
N	48653	44958	44061	35206	33215	32340	6040	5735	5673

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 loose 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 the BAPCPA reform on high interest rate high debt-to-income 'subprime' borrowers.

	(1) growth	(2) intrate	(3) loghecr1	(4) growth	(5) intrate	(6) loghecr1
log(ex.)*post	-8.148*	0.123**	-0.144			
	(-1.71)	(2.25)	(-1.29)			
unlimited*post	-30.67*	0.280	-1.413			
	(-1.78)	(1.28)	(-1.49)			
log(ge)*post				-2.745***	0.0189*	-0.0605
				(-2.97)	(1.71)	(-0.72)
log(pe)*post				2.092	0.0152	-0.00456
				(0.87)	(0.67)	(-0.05)
r2	0.203	0.545	0.782	0.162	0.536	0.789
N	4393	4247	416	3742	3580	355

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 11 Panel A: Effect of BAPCPA reform on house price growth using the house price index data. The dependent variable is growth in all regressions.

	(1) growth	(2) growth	(3) growth	(4) growth	(5) growth	(6) growth	(7) growth	(8) growth	(9) growth
(Exemp==0)*post		-0.289	-0.282	-0.375	1.029	-0.0119	1.891	1.173*	0.274
		(-0.58)	(-0.57)	(-0.68)	(1.58)	(-0.02)	(1.65)	(1.87)	(0.28)
(Exemp==05&7.5)*post	0.289		0.00682	-0.0856	1.318***	0.277	2.180**	1.462***	0.564
	(0.58)		(0.03)	(-0.27)	(2.80)	(1.09)	(2.07)	(3.34)	(0.65)
(Exemp==10&15)*post	0.282	-0.00682		-0.0924	1.311***	0.270	2.173**	1.455***	0.557
	(0.57)	(-0.03)		(-0.31)	(2.85)	(1.16)	(2.07)	(3.41)	(0.65)
(Exemp==20&25)*post	0.375	0.0856	0.0924		1.404**	0.363	2.265**	1.548***	0.649
	(0.68)	(0.27)	(0.31)		(2.69)	(1.07)	(2.11)	(3.14)	(0.73)
(Exemp==30)*post	-1.029	-1.318***	-1.311***	-1.404**		-1.041**	0.862	0.144	-0.755
	(-1.58)	(-2.80)	(-2.85)	(-2.69)		(-2.14)	(0.76)	(0.24)	(-0.79)
(Exemp==40&45)*post	0.0119	-0.277	-0.270	-0.363	1.041**		1.903*	1.185**	0.286
	(0.02)	(-1.09)	(-1.16)	(-1.07)	(2.14)		(1.80)	(2.61)	(0.33)
(Exemp==50)*post	-1.891	-2.180**	-2.173**	-2.265**	-0.862	-1.903*		-0.718	-1.616
	(-1.65)	(-2.07)	(-2.07)	(-2.11)	(-0.76)	(-1.80)		(-0.64)	(-1.20)
(Exemp==75-500)*post	-1.173*	-1.462***	-1.455***	-1.548***	-0.144	-1.185**	0.718		-0.898
	(-1.87)	(-3.34)	(-3.41)	(-3.14)	(-0.24)	(-2.61)	(0.64)		(-0.96)
(Exemp==unlimited)*post	-0.274	-0.564	-0.557	-0.649	0.755	-0.286	1.616	0.898	
	(-0.28)	(-0.65)	(-0.65)	(-0.73)	(0.79)	(-0.33)	(1.20)	(0.96)	
TimeDummy	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	19859	19859	19859	19859	19859	19859	19859	19859	19859

t statistics in parentheses using state level clustering. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 11 Panel B: Effect of BAPCPA reform on house price growth. The dependent variable is growth in all regressions.

	(1) growth	(2) growth	(3) growth	(4) growth	(5) growth	(6) growth	(7) growth	(8) growth	(9) growth
(Exemp==0)*post		-0.316	8.277	-5.545	22.39***	11.32**	37.53*	29.49***	16.71
		(-0.02)	(1.61)	(-1.08)	(3.70)	(2.39)	(1.92)	(2.80)	(1.64)
(Exemp==05&7.5)*post	0.316		8.594	-5.229	22.71	11.64	37.85	29.81*	17.02
	(0.02)		(0.65)	(-0.40)	(1.65)	(0.89)	(1.64)	(1.85)	(1.06)
(Exemp==10&15)*post	-8.277	-8.594		-13.82**	14.12**	3.045	29.26	21.21*	8.431
	(-1.61)	(-0.65)		(-2.58)	(2.18)	(0.60)	(1.51)	(1.97)	(0.78)
(Exemp==20&25)*post	5.545	5.229	13.82**		27.94***	16.87***	43.08**	35.04***	22.25**
	(1.08)	(0.40)	(2.58)		(4.34)	(3.32)	(2.22)	(3.26)	(2.04)
(Exemp==30)*post	-22.39***	-22.71	-14.12**	-27.94***		-11.07*	15.14	7.097	-5.686
	(-3.70)	(-1.65)	(-2.18)	(-4.34)		(-1.81)	(0.74)	(0.64)	(-0.53)
(Exemp==40&45)*post	-11.32**	-11.64	-3.045	-16.87***	11.07*		26.21	18.17*	5.386
	(-2.39)	(-0.89)	(-0.60)	(-3.32)	(1.81)		(1.36)	(1.71)	(0.51)
(Exemp==50)*post	-37.53*	-37.85	-29.26	-43.08**	-15.14	-26.21		-8.042	-20.83
	(-1.92)	(-1.64)	(-1.51)	(-2.22)	(-0.74)	(-1.36)		(-0.38)	(-0.97)
(Exemp==75-500)*post	-29.49***	-29.81*	-21.21*	-35.04***	-7.097	-18.17*	8.042		-12.78
	(-2.80)	(-1.85)	(-1.97)	(-3.26)	(-0.64)	(-1.71)	(0.38)		(-0.89)
(Exemp==unlimited)*post	-16.71	-17.02	-8.431	-22.25**	5.686	-5.386	20.83	12.78	
	(-1.64)	(-1.06)	(-0.78)	(-2.04)	(0.53)	(-0.51)	(0.97)	(0.89)	
TimeDummy	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	58836	58836	58836	58836	58836	58836	58836	58836	58836

The data source is the American Housing survey. All regressions include household fixed effects.
Standard errors are clustered at the state level. t statistics in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 11 Panel C: Effect of BAPCPA reform on the interest rate. The dependent variable is the interest rate in all regressions.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	intrate	intrate	intrate	intrate	intrate	intrate	intrate	intrate	intrate
(Exemp==0)*post		-0.0300	-0.196	-0.102	-0.0992**	0.0166	-0.0660	-0.205**	-0.171**
		(-0.40)	(-1.42)	(-0.47)	(-2.26)	(0.34)	(-1.14)	(-2.67)	(-2.42)
(Exemp==05&7.5)*post	0.0300		-0.166	-0.0722	-0.0691	0.0466	-0.0359	-0.175*	-0.141
	(0.40)		(-1.13)	(-0.32)	(-1.03)	(0.68)	(-0.47)	(-1.92)	(-1.64)
(Exemp==10&15)*post	0.196	0.166		0.0933	0.0964	0.212	0.130	-0.00921	0.0249
	(1.42)	(1.13)		(0.37)	(0.72)	(1.58)	(0.94)	(-0.06)	(0.17)
(Exemp==20&25)*post	0.102	0.0722	-0.0933		0.00307	0.119	0.0363	-0.103	-0.0684
	(0.47)	(0.32)	(-0.37)		(0.01)	(0.55)	(0.17)	(-0.46)	(-0.31)
(Exemp==30)*post	0.0992**	0.0691	-0.0964	-0.00307		0.116***	0.0332	-0.106	-0.0715
	(2.26)	(1.03)	(-0.72)	(-0.01)		(3.54)	(0.71)	(-1.55)	(-1.19)
(Exemp==40&45)*post	-0.0166	-0.0466	-0.212	-0.119	-0.116***		-0.0825	-0.221***	-0.187***
	(-0.34)	(-0.68)	(-1.58)	(-0.55)	(-3.54)		(-1.57)	(-3.10)	(-2.90)
(Exemp==50)*post	0.0660	0.0359	-0.130	-0.0363	-0.0332	0.0825		-0.139*	-0.105
	(1.14)	(0.47)	(-0.94)	(-0.17)	(-0.71)	(1.57)		(-1.73)	(-1.40)
(Exemp==75-500)*post	0.205**	0.175*	0.00921	0.103	0.106	0.221***	0.139*		0.0341
	(2.67)	(1.92)	(0.06)	(0.46)	(1.55)	(3.10)	(1.73)		(0.39)
(Exemp==unlimited)*post	0.171**	0.141	-0.0249	0.0684	0.0715	0.187***	0.105	-0.0341	
	(2.42)	(1.64)	(-0.17)	(0.31)	(1.19)	(2.90)	(1.40)	(-0.39)	
TimeDummy	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	52129	52129	52129	52129	52129	52129	52129	52129	52129

The data source is the American Housing survey. All regressions include household fixed effects.
Standard errors are clustered at the state level. t statistics in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 11 Panel D: Effect of BAPCPA reform on the log of home equity line of credit. The dependent variable is the log of home equity line of credit in all regressions.

	(1) loghecr1	(2) loghecr1	(3) loghecr1	(4) loghecr1	(5) loghecr1	(6) loghecr1	(7) loghecr1	(8) loghecr1	(9) loghecr1
(Exemp==0)*post		0.0438	-0.00727	0.104	0.411***	0.158	-0.00348	0.318	0.000330
		(0.23)	(-0.02)	(0.45)	(2.94)	(0.88)	(-0.02)	(1.47)	(0.00)
(Exemp==05&7.5)*post	-0.0438		-0.0511	0.0599	0.367***	0.114	-0.0473	0.274	-0.0435
	(-0.23)		(-0.15)	(0.26)	(2.91)	(0.69)	(-0.37)	(1.32)	(-0.21)
(Exemp==10&15)*post	0.00727	0.0511		0.111	0.418	0.165	0.00379	0.325	0.00760
	(0.02)	(0.15)		(0.31)	(1.35)	(0.50)	(0.01)	(0.93)	(0.02)
(Exemp==20&25)*post	-0.104	-0.0599	-0.111		0.307	0.0538	-0.107	0.214	-0.103
	(-0.45)	(-0.26)	(-0.31)		(1.63)	(0.25)	(-0.57)	(0.86)	(-0.42)
(Exemp==30)*post	-0.411***	-0.367***	-0.418	-0.307		-0.254**	-0.415***	-0.0935	-0.411**
	(-2.94)	(-2.91)	(-1.35)	(-1.63)		(-2.32)	(-25.77)	(-0.57)	(-2.46)
(Exemp==40&45)*post	-0.158	-0.114	-0.165	-0.0538	0.254**		-0.161	0.160	-0.157
	(-0.88)	(-0.69)	(-0.50)	(-0.25)	(2.32)		(-1.48)	(0.82)	(-0.80)
(Exemp==50)*post	0.00348	0.0473	-0.00379	0.107	0.415***	0.161		0.321*	0.00381
	(0.02)	(0.37)	(-0.01)	(0.57)	(25.77)	(1.48)		(1.97)	(0.02)
(Exemp==75-500)*post	-0.318	-0.274	-0.325	-0.214	0.0935	-0.160	-0.321*		-0.317
	(-1.47)	(-1.32)	(-0.93)	(-0.86)	(0.57)	(-0.82)	(-1.97)		(-1.37)
(Exemp==unlimited)*post	-0.000330	0.0435	-0.00760	0.103	0.411**	0.157	-0.00381	0.317	
	(-0.00)	(0.21)	(-0.02)	(0.42)	(2.46)	(0.80)	(-0.02)	(1.37)	
TimeDummy	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	7988	7988	7988	7988	7988	7988	7988	7988	7988

The data source is the American Housing survey. All regressions include household fixed effects.
Standard errors are clustered at the state level. t statistics in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$
This table reports coefficients of non-parametric regressions $growth = \alpha_0 + \alpha_1 \times post \times ExClass + X + \epsilon$ where $ExClass$ is a set of dummy variables for different levels of the homestead exemption limit as measured in 2005. In this regression, we construct different classes of exemption limits as indicated in the first column of the table. For the HPI regressions in Panel A, we employ region fixed effects, for the AHS data (Panel B-D), we use household fixed effects. One of the interaction terms of the post dummy with one of the ExClass dummies needs to be dropped due to perfect collinearity. We run the regressions and drop the interaction term on the diagonal. Hence, the first column reports all regressions relative to the effect of being in the time period after the reform for the zero exemption limit. The second column reports the results relative to a homestead exemption limit of $5,000 etc. A more negative effect for higher exemption limits on growth rates would imply negative signs of the coefficients in the South-West corner of the table (and correspondingly a positive sign in the North-East part of the table).

Table 12: Non-parametric test

	(1) growthHPI	(2) growthAHS	(3) int. rate	(4) log(hecr)
Panel A: Combined classes.				
Number of obs	9	9	9	9
Spearman's rho	-0.7000	-0.7333	0.4167	-0.2667
$Prob > \|t\|$	0.0358**	0.0246**	0.2646	0.4879
Panel B: Each exemption limit forms a class.				
Number of obs	17	17	17	17
Spearman's rho	-0.7549	-0.6324	0.4632	-0.3848
$Prob > \|t\|$	0.0005***	0.0065***	0.0611*	0.1272

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$ This table presents non-parametric tests of the relation between homestead exemption limits and house price growth. We use the following two step procedure. In a first step, a similar regression as in table 11 Panel A is estimated: $growth = \alpha_0 + \alpha_1 \times post \times ExClass + X + \epsilon$ where $ExClass$ is a set of dummy variables for different levels of the homestead exemption limit as measured in 2005. In Panel A, we use the same set of classes as in table 11 Panel A, in Panel B, each exemption limit forms a distinct class. The second step results are reported here: the Spearman's rank correlation between the exemption limit and the estimated post effect implied by the regression results of the first step.

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

	(1) growth	(2) intrate	(3) loghecr1	(4) growth	(5) intrate	(6) loghecr1
log(ex.)	-8.556**	0.0269**	-0.0467			
	(-2.66)	(2.07)	(-1.16)			
unlimit.*post	-24.36**	0.180**	-0.0479			
	(-2.06)	(2.44)	(-0.23)			
log(GE effect)*post				-2.630***	0.0129***	-0.00732
				(-3.12)	(2.89)	(-0.23)
log(PE effect)*post				1.025	-0.00316	-0.00747
				(1.38)	(-0.75)	(-0.19)
work	0.444	-0.0370	-0.141	-2.264	-0.0494	-0.113
	(0.11)	(-1.00)	(-0.93)	(-0.75)	(-1.35)	(-0.75)
crime	1.382	0.0212	-0.0462	1.322	0.0133	-0.0422
	(0.70)	(0.81)	(-0.71)	(0.74)	(0.46)	(-0.73)
income	0.0000627***	-0.000000241*	0.000000199	0.0000661***	-0.000000252*	0.000000366
	(4.90)	(-1.87)	(0.55)	(4.88)	(-1.89)	(1.30)
average work	-0.339	0.0546	0.176	4.767	0.134	0.461
	(-0.03)	(0.38)	(0.39)	(0.36)	(0.95)	(1.25)
areainc	-0.0000315	-0.000000322	0.00000292	-0.0000984	-0.000000107	0.00000182
	(-0.23)	(-0.34)	(1.21)	(-0.79)	(-0.11)	(0.71)
average crime	-9.260	-0.256	-0.483	-9.424	-0.449**	-0.0945
	(-0.37)	(-1.15)	(-0.54)	(-0.38)	(-2.12)	(-0.14)
r2	0.228	0.665	0.733	0.147	0.597	0.707
N	55741	52129	7988	42443	32340	5673

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 house market. All additional controls are contemporaneous.

Table 14: Robustness: Clustering.

	(1)	(2)	(3)	(4)	(5)	(6)
	growth	growth	intrate	intrate	loghecr1	loghecr1
log(pe)*post	1.381*		-0.00505		-0.00649	
	(1.95)		(-1.10)		(-0.18)	
log(ge)*post	-2.640***		0.0137***		-0.00903	
	(-2.99)		(3.24)		(-0.28)	
log(ex.)*post		-8.330**		0.0275**		-0.0459
		(-2.58)		(2.17)		(-1.07)
unlimit*post		-23.52*		0.180**		-0.0562
		(-1.98)		(2.47)		(-0.26)
Standard error for log(pe)*post clustered at						
State	0.708		0.00459		0.0367	
SMSA	0.679		0.00590		0.0246	
Year	0.992		0.00585		0.0126	
Household	0.483		0.00620		0.0215	
Standard error for log(ge)*post clustered at						
State	0.882		0.00424		0.0320	
SMSA	0.648		0.00498		0.0289	
Year	0.872		0.00280		0.0150	
Household	0.461		0.00629		0.0229	
Standard error for log(ex.)*post clustered at						
State		3.227		0.0127		0.0427
SMSA		2.047		0.0130		0.0548
Year		0.917		0.0138		0.0204
Household		0.997		0.0133		0.0453
Standard error for unlimit*post clustered at						
State		11.86		0.0728		0.214
SMSA		7.586		0.0731		0.292
Year		7.396		0.0640		0.122
Household		4.034		0.0757		0.317
Fixed Effects	household	household	household	household	household	household
r2	0.149	0.234	0.596	0.665	0.707	0.733
N	44061	58836	32340	52129	5673	7988

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

This table reports coefficient estimates of the main specification and the upper part replicates the main specification. In the lower half of the table, standard errors with clustering at various levels are reported.