Mortgage Leverage and House Prices *

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Abstract

I measure the effect of mortgage leverage restrictions on house prices using a change in the eligibility requirements imposed by Fannie Mae and Freddie Mac. In 1999, Fannie Mae and Freddie Mac's debt-to-income requirements diverged, leading to tighter lending standards in places where local lenders had pre-existing relationships with Freddie Mac. Locations with tighter debt-to-income requirements experience an immediate relative reduction in house prices, showing that changes in lending standards have powerful effects. The effect builds over time, resulting in a smaller house price boom and bust in these locations during the 2000s. I use a simple model to interpret the empirical results and extrapolate to other similar policies, finding that a relaxation of debt-to-income restrictions is important for explaining the 2000s housing boom.

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1. Introduction

A decade after the financial crisis, the question of what caused the 2000s housing boom is still largely unanswered. Some authors suggest that the boom was the result of a decline in lending standards (Mian and Sufi (2009); Mian and Sufi (2017)). But despite a strong empirical link between credit and house prices in general, there is still disagreement about the nature of this initial shock, and indeed whether it occurred at all (Adelino et al. (2016); Foote et al. (2016)). From a theoretical perspective, it is far from obvious that a change in lending standards could have triggered a housing boom of this magnitude. The transmission of lending standards to house prices depends on a variety of factors, including the nature of house price expectations; housing supply; credit supply; and housing market segmentation. While some recent papers suggest that a change in lending standards could not have caused the housing boom (Justiniano et al. (2016); Kaplan et al. (2017)), others claim that lending standards played an important role (Greenwald, 2016). Resolving this question is crucial for understanding whether macroprudential policies implemented in response to the crisis will be effective.

In this paper, I use a natural experiment to show that mortgage debt-to-income (DTI) limits have a large effect on house prices.[1] I find that tightening debt-to-income rules reduces house prices, and that the long-run effect is considerably larger than the short-run effect. I show that the short-run effect is consistent with a simple model of housing demand. When I add adaptive house price expectations into the model I can also replicate the long-run effect measured in the data. If households incorporate the past effect of the policy into their expectations for future house price growth, they adjust their housing demand and this causes the effect to expand over time. Finally, I use the model to show that an expansion of debt-to-income limits in the late 1990s can explain a sizable share of the housing boom.

My identification strategy is based on a change in the debt-to-income limits used by the Government Sponsored Enterprises (GSEs) Fannie Mae and Freddie Mac. In the United States lenders sell mortgages to Fannie and Freddie, and their eligibility requirements strongly influence lending standards.[2] The GSEs use a variety of different criteria to determine whether they are willing to purchase a mortgage. Mortgages that satisfy these

[1] The debt-to-income ratio is defined as the ratio of the borrower's monthly mortgage repayment and other financial obligations to their income. Other financial obligations include child support, alimony, payments on other debts and property tax payments.

[2] The relationship between GSE standards and aggregate standards is also documented by authors looking at the jumbo market (Loutskina and Strahan (2009); Calem et al. (2013); Adelino et al. (2014)).

criteria are referred to as 'conforming'. The most salient criterion is a dollar limit on loan size known as the conforming loan limit, but eligibility criteria go well beyond this and include complex interactions of the debt-to-income ratio, loan-to-income ratio and credit score.

While Fannie Mae and Freddie Mac have used broadly similar rules historically, their criteria have sometimes diverged. When this happens, effective lending standards diverge across locations depending on whether local lenders sell to Fannie Mae or Freddie Mac. In this paper, I describe how debt-to-income requirements imposed by Freddie Mac diverged from those of Fannie Mae during 1999, and were not realigned until several years later. I then show that a price gap emerges between counties that had different debt-to-income limits after 1999 because of pre-existing lender relationships with either Fannie Mae or Freddie Mac.[3]

The tighter debt-to-income requirements imposed by Freddie Mac affected around 5 per cent of borrowers and led to a short-run relative decline in prices of about $1\frac{1}{2}$ per cent when comparing locations where lenders sell to Freddie with those where lenders sell to Fannie. The change also dampened the entire price cycle, with the initial $1\frac{1}{2}$ per cent effect expanding to over 7 per cent in 2005. It is important to remember that house prices were growing rapidly during this period. This means the relative change should be interpreted as areas exposed to Freddie Mac experiencing a smaller boom – not an absolute price decline.

I use a simple model to help understand what is behind the long-run price divergence. Some of the divergence can be explained by changes in the national debt-to-income distribution. The average debt-to-income ratio rose gradually over the course of the boom, meaning that the share of households affected by the policy change increased over time. However, this channel cannot account for most of the long-run effect. In contrast, the effect can be explained if households incorporate recent price growth into their house price expectations.

I discipline the feedback from house prices to expectations using survey evidence on the relationship between expected price growth over the next year and realized price growth over the previous year (Case et al. (2012); Armona et al. (2017)). The idea is that households in areas with more Freddie sellers develop more pessimistic price expectations following the policy change. This reduces their housing demand relative to other areas, ultimately resulting in a smaller housing boom. While this is not the only possible expla-

[3] In Section 3 I show that lenders often have exclusive relationships with either Fannie or Freddie, and that these relationships are very persistent.

nation, it is plausible and consistent with empirical evidence on house price expectations. I also provide evidence to rule out explanations based on other policy differences between Fannie Mae and Freddie Mac.

This paper also has implications for the role the GSEs played in the housing boom. Some authors have suggested that government affordable housing policy started the boom, with private sector players merely perpetuating it (Pinto (2011); Wallison (2015)). This argument is based on the idea that the GSEs purchased a large volume of subprime mortgages in order to promote low-income credit access. While there are now a number of papers credibly refuting a direct link to affordable housing policy (Bolotnyy (2013); Ghent et al. (2015)), my results suggest that the GSEs' underwriting policies did, nonetheless, contribute to the housing boom.

In 1999, the first year for which GSE debt-to-income data are publicly available, both Fannie Mae and Freddie Mac purchased a large volume of loans with a debt-to-income ratio exceeding their historical cutoff of 36 per cent. This expansion of high debt-to-income purchases reflected advances in credit scoring and automated underwriting technology – a movement the GSEs were at the forefront of – and was not necessarily associated with large increase in default risk. These more relaxed standards were only available to lenders using the GSEs' automated underwriting software, meaning that they propagated gradually as software adoption increased over the 1990s.

My results here suggest that this expansion had a large effect on house prices. I use the model to compute the effect of this change and find it can explain up to one third of price growth from 1995 to 2006, depending on the price index used. It is also useful to break this down further, as the story relates primarily to the early stages of the housing boom. While Fannie and Freddie's debt-to-income expansion can explain up to two thirds of price growth between 1995 and 2003, it cannot explain the growth that occurred between 2003 and 2006. In Appendix A I also directly measure the effect of the GSEs' software on house prices using a differences-in-differences approach, and find a response of a similar magnitude.

My paper relates to work in a number of areas. In terms of the empirical analysis, it relates to a policy literature that measures the effect of debt-to-income restrictions on house prices (Igan and Kang (2011); Kuttner and Shim (2016)). The main challenge for researchers in this area is finding variation across otherwise comparable locations that is independent of other policy interventions. These policies are often applied at the national level, and regional policies, where they exist, are usually adjusted in response to local economic conditions. I build on this work by using a new identification strategy and providing evidence in the U.S. context. In my paper, regional variation in leverage policies arises from differential exposure to national changes in GSE policies. This reduces the concern that changes in leverage policies are related to local economic conditions. Given that the response to leverage policies may depend on country-specific factors, for understanding the 2000s housing boom and evaluating U.S. policies it is important to provide empirical evidence specific to the U.S.

There are also several papers providing evidence on other effects of household leverage policies. Evidence from the U.S. suggests that debt-to-income restrictions have limited benefits in terms of reducing individual default risk (DeFusco et al. (2017); Foote et al. (2010)) and reduce credit access for groups falling outside the bounds of the imposed limits (DeFusco et al. (2017); Johnson (2018)).[4] Acharya et al. (2018) look at the effect of a combined loan-to-income and loan-to-value policy on the allocation of mortgage credit, bank risk exposure and house prices in Ireland. Rather than imposing leverage limits at the loan-level, the Irish policy requires that banks keep exposure to certain types of loans below some limit. The loan-to-value restrictions are also considerably more binding than the loan-to-income restrictions in their setting. They find that banks reallocate their lending away from low income borrowers and more exposed locations, and also increase their corporate lending. Banks appear to achieve this reallocation by reducing interest rates to groups less affected by the regulation. They document relatively weaker house price growth in locations with more affected borrowers.

Several recent papers use a quantitative modeling approach to look at the effect of debt-to-income[5] constraints on house prices and mortgage default (Corbae and Quintin (2015); Campbell and Cocco (2015); Greenwald (2016); Kaplan et al. (2017)). There is also a larger body of work focusing on loan-to-value constraints (Stein (1995); Slemrod (1982); Iacoviello (2005); Cocco (2005); Iacoviello and Neri (2010); Kiyotaki et al. (2011); Glaeser et al. (2013); Justiniano et al. (2015); Justiniano et al. (2016); Favilukis et al. (2016)). These models are, however, unable to make conclusive statements about the effect of leverage constraints on house prices because they are sensitive to assumptions about housing market segmentation, the supply of funds, the way house price expectations are formed and the particular way in which households are constrained. One of the main reasons why these papers draw different conclusions relates to their assumptions about the rental market. In these models, leverage policies will have a limited effect on house

[4] In this paper I show that tighter debt-to-income restrictions were associated with substantially lower default rates during the crisis. However, this effect arises primarily through the effect on house prices, and has little to do with loan-level differences in leverage and credit score at origination.

[5] Or loan-to-income, which is closely related.

prices when owner-occupied and rented housing are highly substitutable (Kaplan et al., 2017). The fact that I estimate a large effect in practice suggests that models assuming segmented housing markets are likely to draw more accurate conclusions.

Here I present a static model of housing demand, which uses a realistic joint distribution of income and assets for recent buyers to compute the house price effects of leverage policies. In the model, house prices are endogenous but interest rates are fixed. This is analogous to the empirical setup where I use variation in exposure within U.S. states to identify the effect on county house prices. Another feature of the rational expectations models in this literature is that most of the effect on house prices occurs on impact, which is in contrast to my empirical result. I show that including adaptive expectations can capture both the profile and the magnitude of the empirical effect by generating feedback between lending standards and household beliefs.[6]

The main caveat when comparing my results with these models is that in both the empirical analysis and the model I am measuring a local general equilibrium effect holding the interest rate fixed. Under certain assumptions about the supply of funds a change in leverage constraints raises interest rates and does not generate a large increase in the quantity of credit, directly contradicting the data from the housing boom period (Justiniano et al. (2015); Kaplan et al. (2017)). However, in the context of my paper, institutional features of the U.S. mortgage market mean that changes in leverage policy are likely to have large quantity effects. The mortgage-backed securities (MBS) issued by Fannie and Freddie are guaranteed with respect to default risk, highly-rated, and therefore popular with international investors and institutions who need to hold safe assets. Because they are close substitutes for other assets within this large market, such as government bonds, demand for these securities is likely to be very elastic. Consequently, when the GSEs change their standards, the quantity of credit can increase substantially.[7]

My paper is also connected to work looking at the role of expectations in contributing

[6] Kaplan et al. (2017) emphasize the role of beliefs in contributing to the housing boom and downplay the effect of a change in lending standards. This is a difficult distinction in practice as changes in beliefs could be triggered by changes in lending standards (particularly if changes in lending standards are opaque). Including adaptive expectations is one way to capture this.

[7] I describe in Section 2 how both Fannie Mae and Freddie Mac dramatically relaxed their debt-to-income limits during the 1990s. It is also an empirical fact that Fannie and Freddie's MBS issuance increased substantially over the same period while conforming mortgage rates did not increase. Barta et al. (2000) noted that: 'The volume of loans [Fannie Mae and Freddie Mac] handle has tripled over the past decade, and they pay out $460 million in underwriting fees to Wall Street firms each year, up from less than $8 million in 1990. Among investors, they are considered second only to U.S. Treasurys as a safe place to invest money. With U.S. Treasury debt shrinking, the two combined are on a path to become one of the largest issuers of debt in the world, and could soon replace Treasury bonds as the market's benchmark – which would help lower their borrowing costs and enable them to grow even more.'

to the housing boom and bust. One way to account for my empirical results is through feedback from the policy to expectations. In this case changes in expectations are important, but follow as a direct consequence of changes in credit conditions. This seems consistent with Cox and Ludvigson (2018), who consider the role of credit conditions and expectations jointly and find that only changes in credit conditions have substantial explanatory power for house price growth. Depending on the nature of this feedback to expectations, a relaxation of leverage policy may generate a bust as a direct consequence of the preceding boom, without the need for a subsequent policy reversal.[8] This is consistent with my estimates, which show that the policy effect starts to reverse during 2005, though it is hard to draw a clear conclusion in my setting as the policy was also gradually unwound shortly afterwards.

Section 2 provides institutional context for the identification strategy. In Section 3 I describe the data. In Section 4 I describe the policy change and in Section 5 I measure the effect of the change on house prices. In Section 6 I measure the relationship between exposure to the policy change and default rates. Section 7 sets out a model for computing the effect of combined loan-to-value and debt-to-income policies on house prices. This is useful for validating the empirical results, understanding the long-run effect, and extrapolating to other policies.

2. Institutional Background

The Government Sponsored Enterprises, Fannie Mae and Freddie Mac, were established with the goal of providing a liquid secondary market for U.S. residential mortgages.[9] Fannie Mae was created in 1938 and originally used government funds to provide lenders with mortgage financing, thereby supporting public goals with respect to affordable homeownership. After Fannie Mae was privatized in 1968, Congress established Freddie Mac, primarily to provide a competitor. Since the 1980s, both Fannie and Freddie have funded their mortgage purchases mainly by issuing mortgage-backed securities with a default risk guarantee. To limit their exposure to default risk, the GSEs require the loans they purchase to meet a set of eligibility criteria. This is on top of the conforming loan limit, which is a dollar value limit on the size of loans the GSEs are allowed to purchase ($453,100 in 2018). Mortgages that meet these eligibility criteria are referred to as 'conforming' or

[8] When agents make expectational errors, busts can follow directly from booms (see for example Bordalo et al. (2018) and Barberis et al. (2018)).

[9] Other GSEs include Ginnie Mae, Sallie Mae, Farmer Mac and the Federal Home Loan Banks.

'prime' and are generally considered to be low risk.

Historically, the GSEs' criteria took the form of manual underwriting guidelines and included limits on debt-to-income and loan-to-value ratios. But, following the release of their automated underwriting software in the mid 1990s, the GSEs started to base eligibility on more complex rules informed by default-risk analysis. These new algorithms were able to identify high-risk applicants more effectively, and the GSEs started to expand the set of loans they were willing to purchase. In particular, loans underwritten using the GSEs' software were subject to much more relaxed debt-to-income criteria than those outlined in manual underwriting guidelines (Barta et al. (2000); Maselli (1994)). This meant that once lenders had adopted the software debt-to-income limits were dramatically relaxed.[10,11]

Although lenders were initially slow to adopt the software after its release in 1995, usage rose rapidly during the late 1990s refinancing boom and was mostly complete for large lenders by the early 2000s.[12] Both GSEs continued to make changes to their software algorithms over time. I identify these changes using loan-level data provided by the GSEs. To my knowledge many of these changes were not publicized, including the change I use here. The important point for this paper is that Freddie Mac imposed tighter debt-to-income criteria than Fannie Mae for several years between 1999 and the financial crisis. I document this in more detail in Section 4 and Appendix C.

Although lenders can make loans that do not meet the GSEs' criteria, in practice when the GSEs change a particular constraint – for example deciding to purchase loans with higher loan-to-value ratios – the effect is similar to a national change in the constraint. I provide direct empirical evidence of this, but there are also theoretical reasons why it is likely to be true. If an application meets GSE criteria it can generally be quickly approved using the GSEs' automated underwriting software. Importantly, if a loan is eligible for purchase by Fannie Mae or Freddie Mac the originator does not need to hold the loan on

[10] Freddie Mac's software, Loan Prospector, always applied different rules from those set out in Freddie's manual underwriting guide, and incorporated a relaxation of debt-to-income limits from its first release. As discussed in Appendix A, however, a broad-based relaxation of debt-to-income limits did not occur until a little later. Early versions of Fannie Mae's software, Desktop Underwriter, applied the same rules as the manual guide, but by 1997 Desktop Underwriter seems to have been using a similar approach to Loan Prospector. These developments are referred to in Straka (2000) as well as industry publications (Cocheo (1995); McDonald et al. (1997); Maselli (1994); Muolo (1996); American Banker (1997))

[11] For a more detailed summary of these developments see Straka (2000) and Markus et al. (2008).

[12] Small lenders were a little slower to adopt the software, but by 2004 46% of responders to the American Community Banker's Real Estate Lending Survey were using Freddie Mac's software and 32% were using Fannie Mae's. Among community banks surveyed, the share using either Fannie or Freddie's software was 47% for banks with less than $50 million in assets, increasing to 86% for banks with more than $1 billion in assets (Costanzo, 2004).

its balance sheet, making the origination decision arguably independent of lender-specific factors. Even when a lender wishes to retain residential mortgage exposure, it may be optimal to hold mortgage-backed securities issued by Fannie Mae or Freddie Mac rather than whole loans. Not only are these securities more liquid, they also receive favorable treatment under regulatory capital requirements. Overall, GSE eligibility tends to raise approval probability and reduce the interest rate conditional on approval, meaning that changes to GSE criteria have a large, instantaneous effect on mortgage credit access that is highly correlated across lenders and regions.[13]

3. Data

My identification strategy uses the idea that lenders who sell to Freddie Mac will tighten debt-to-income limits when Freddie Mac tightens its criteria. As a result, lending standards will tighten in places exposed to these lenders, relative to places where lenders sell to Fannie Mae. I use an exposure measure based on the share of loans sold to Freddie Mac, and measure the effect on county house prices using a proprietary price index. The main results are also robust to using price indices produced by Zillow and the FHFA.

I compute Freddie Mac exposure using the Home Mortgage Disclosure Act (HMDA) dataset, which provides fairly comprehensive coverage of U.S. mortgage originations.[14] The exposure measure for county c is:

$$\text{Exposure}_{c,1998} = \frac{\#\text{ Loans in county } c \text{ sold to Freddie in 1998}}{\#\text{ Loans in county } c \text{ sold to Freddie or Fannie in 1998}}$$

I exclude lenders originating more than 20000 purchase loans in 1998. This is because in 1999 the GSEs started to negotiate deals with large lenders that resulted in relationship changes and in some cases allowed lenders use their own proprietary underwriting software rather than the GSEs' software. The main result is robust to including all HMDA loans sold to Fannie or Freddie in 1998, though the estimates are less precise.

The identification strategy will be most effective when there is limited substitution from Freddie to Fannie following the policy change. To the extent that this substitution

[13] A number of papers document discontinuities consistent with GSE eligibility affecting mortgage credit terms and availability in a meaningful way (Calem et al. (2013); Adelino et al. (2014); Kaufman (2014); DeFusco and Paciorek (2017)).

[14] Coverage is more limited for very small lenders and rural counties. In my analysis I consider only counties located in a core-based statistical area (metropolitan or micropolitan area).

occurs, the effect on house prices will be lower than what it would have been were the same policy applied nationally. The existence of exclusive, persistent relationships between lenders and GSEs limits the potential for substitution.[15] The HMDA dataset allows me to determine whether a lender has an exclusive relationship with Freddie or Fannie. I define a lender as having an exclusive relationship with Freddie Mac if more than 99 per cent of mortgages it sells to the GSEs are sold to Freddie Mac.

Figure 1(a) shows that in 1998 most lenders selling to at least one GSE sold the vast majority of their conforming loans exclusively to either Fannie or Freddie. Around 38 per cent of lenders sold more than 95 per cent to Freddie Mac and around 45 per cent of lenders sold more than 95 per cent to Fannie Mae. Figure 1(b) shows Kaplan-Meier estimates of the probability that a 1998 exclusive relationship still survives in later years. The estimates suggest that these exclusive relationships are very persistent, and are also broadly similar regardless of whether the 1998 relationship was with Fannie or Freddie.

To address the concern that a selected group of lenders changed GSE relationships in response to the underwriting changes I document, I measure county exposure to Freddie Mac in 1998 before the policy change occurred. Figure 2 shows how the 1998 exposure measure varies across counties.

Next I examine the relationship between the exposure measure and other county characteristics. For counties with above and below median exposure I compare the average value of median household income, population density, FICO score and the Saiz housing supply elasticity.[16] Table 1 shows that there are statistically significant differences between areas with high and low exposure to Freddie Mac, but the main economically meaningful difference is with respect to population density. This difference arises because counties with very high population density tend to have a relatively low share of Freddie Mac sellers. In the empirical analysis I demonstrate that the effect on house prices clearly coincides with the timing of the policy change, and that there is no significant pre-trend. This result holds without additional controls when using state-time fixed effects, but is robust to conditioning on the variables in Table 1.

I also use other mortgage data sources containing variables that are not reported in the public HMDA dataset. Fannie and Freddie's Single Family Loan Performance datasets and Public Use Databases contain additional information about the loans they purchased. I characterize the policy change using the Single Family Loan Performance datasets. These

[15]It is also important to note that Fannie and Freddie's software algorithms were proprietary. Changes were generally not publicly announced and lenders would learn about them gradually through experience using the software.

[16]Saiz (2010)

datasets are useful because they contain information on debt-to-income, loan-to-value and credit score, which are important for determining whether a loan meets eligibility criteria. However, these datasets do not provide a precise measure of the property location, reporting only the state, MSA and three-digit Zip Code.[17]

To provide support for the channel from Freddie Mac exposure to house prices, I also show that areas more exposed to Freddie Mac experienced a decline in high debt-to-income lending coinciding with the policy change. For this I use the CoreLogic Loan-level Market Analytics Database, which has information on debt-to-income and the county where the property is located.

4. The Policy Change

In this section I use loan level data to document the nature and timing of Freddie Mac's change in debt-to-income rules. To my knowledge, this paper is the first to point out this policy change and to use it to identify the effects of underwriting standards. The policy change was not publicized in any way; instead, it only becomes apparent by using the data to back out the underwriting standards that were applied.[18]

Applying this reverse engineering approach to data on the GSEs' mortgage purchases, I show that eligibility criteria imposed by Fannie Mae and Freddie Mac diverged after June 1999, with Freddie Mac becoming relatively less likely to buy mortgages with a debt-to-income ratio exceeding 50 per cent. This relative contraction occurred following a period in which both GSEs had dramatically expanded their high debt-to-income purchases. Historically, both had been willing to purchase loans with a debt-to-income ratio of up to 36 per cent, but by 1999 over one third of purchase loans to owner-occupiers in the GSEs' Single Family Loan Performance datasets had debt-to-income ratios above this cutoff.[19]

[17] There are over 900 three-digit Zip Codes in the U.S. corresponding to areas served by a single postal facility. Three digit Zip Codes often cover multiple counties.

[18] Although not publicly announced, lenders noticed a divergence in the algorithms. When asked about 1999 industry developments in June 2000, the President of InterFirst (a division of ABN AMRO) noted that [Freddie and Fannie's automated underwriting engines were] 'not quite as parallel as they were in the past' and that 'consistency between the engines sometimes is hard to manage; that's a problem.' (LaMalfa, 2000)

[19] While both Fannie and Freddie had been expanding their criteria since the 1990s, by 2002 Freddie was using more conservative language regarding these developments. A 2002 *Mortgage Banking* article quoted a Freddie representative, saying: 'Freddie Mac "worries quite a lot" about credit risk' and that 'Freddie Mac's vision is "not to turn the subprime market into an extension of its prime business, but rather to keep it a distinct area."' In the same article, a Fannie Mae representative stated 'Quite frankly, [automated underwriting] has erased the bright line between the conforming and subprime markets. Now it is more a continuum.' (Morse, 2002)

For this reason, the debt-to-income distribution prior to the policy change looks relatively unconstrained.

The difference in policies can be seen clearly from looking at the debt-to-income distributions of Freddie and Fannie's mortgage purchases. Figure 3(a) shows that Freddie and Fannie had similar debt-to-income distributions prior to the policy change. However, for mortgages originated in 2000 or 2001, Freddie's distribution shows a sharp drop in the mass above 50 per cent (Figure 3(b)). Because I track the price response over several years, it is also important to understand later differences in Freddie and Fannie's rules. Figures 3(c) and 3(d) show that Freddie's rules continue to differ from Fannie's right up until the financial crisis; however, the percentage difference in the share of purchases with a debt-to-income ratio above 50 per cent declines.

In Appendix C, I show that this reflects a partial unwinding of Freddie's 1999 policy change. Also in Appendix C I show that Fannie and Freddie's policies did not diverge substantially along other dimensions. Together, these two facts imply that the increase in the price response over time is unlikely to be caused by further divergence in policies between Fannie and Freddie after 1999. If anything, it may be attenuated by the partial reversal of the 1999 policy change.

I document the timing of the policy change more precisely by plotting the share of high debt-to-income purchases for Freddie relative to Fannie conditional on location. Specifically I plot estimates of β_t from:

$$\text{High DTI}_i = \gamma_{s,t} + \beta_t \text{Freddie}_i + \epsilon_i$$

where loan i is originated in month t in state s, High DTI$_i$ is an indicator equal to one for loans with a DTI greater than 50 per cent, $\gamma_{s,t}$ is a state by month fixed effect and Freddie$_i$ is an indicator equal to one for loans sold to Freddie Mac and zero for loans sold to Fannie Mae. This means that I am comparing Fannie and Freddie's purchases of loans originated in the same month, for the purchase of properties located in the same state. Figure 4(a) plots the estimates of β_t, showing that Freddie Mac's high debt-to-income purchases start to diverge from Fannie Mae's after mid 1999.

Next I look at Freddie and Fannie's purchases separately to confirm that the previous result reflects a contraction by Freddie, rather than an expansion by Fannie. Because I am now simply comparing high debt-to-income purchases at different points in time, it is important to adjust for movements in the interest rate, which can have a substantial effect on the debt-to-income distribution. In this case, there was a large increase in interest rates during 1999 which raised debt-to-income ratios substantially. I construct the following adjusted debt-to-income ratio, which holds average interest rates fixed at August 1999 levels.

$$\text{High } \widetilde{\text{DTI}} = \frac{f(r_{\text{Aug 1999}})}{f(r)} DTI$$

where $f(r)$ is the 30 year fixed mortgage payment on \$1 of debt.[20] Separately for Freddie and Fannie's purchases, I plot estimates of β_t from:

$$\text{High } \widetilde{\text{DTI}}_i = \gamma_s + \beta_t + \epsilon_i$$

where High $\widetilde{\text{DTI}}$ is an indicator equal to one for loans with an adjusted DTI greater than 50 per cent. Figures 4(b) and 4(c) clearly show that Freddie tightened its debt-to-income policy while Fannie's policy remained unchanged. Figure 4(b) also provides a more precise indication of the policy timing and motivates my choice of June 1999 as the base period for monthly regressions, and 1998 as the base period for annual regressions. The sharper change when using the adjusted level, rather than the difference between Fannie and Freddie, is partly a reflection of the movement in interest rates. As interest rates increase, the debt-to-income distribution moves to the right and the percentage point difference in high debt-to-income purchases continues to expand gradually following the policy change (this increase in rates was short-lived, however, and was fully reversed by 2001). Furthermore, Fannie's dataset contains a reduced number of loans prior to the fourth quarter of 1999, so comparisons during this earlier period are less precise. Figure 5(a) extends the sample period, showing that the percentage point difference between Fannie and Freddie's high debt-to-income purchases remains substantial up until 2010, after which neither Fannie nor Freddie purchased loans with a debt-to-income ratio above 50 per cent.

One concern with this reverse engineering approach is that the Single Family Loan Performance datasets do not contain the universe of loans purchased by Fannie and Freddie. The datasets include information on standard mortgage loans purchased by the two institutions since 1999, but do not contain mortgages with non-standard characteristics such as interest-only repayments, or mortgages purchased under special programs. This

[20] Assuming that other financial obligations are zero, the debt-to-income ratio can be adjusted for changes in the mortgage rate, r, in the following way. From $DTI = f(r)\frac{\text{Loan}}{\text{Income}}$ and $\widetilde{DTI} = f(r_{\text{Aug 1999}})\frac{\text{Loan}}{\text{Income}}$, it follows that $\widetilde{DTI} = \frac{f(r_{\text{Aug 1999}})}{f(r)} DTI$. In practice this is not exact because there are other financial obligations in the numerator; however, the adjustment should still broadly capture movements in the debt-to-income distribution which are driven by changes in interest rates.

leaves open the possibility that the changes I identify reflect selection into the dataset. Two points are important here. Firstly, Freddie's dataset provides high coverage of its single family 30 year fixed rate mortgage purchases. For the years prior to 2004 over 90 per cent of these loans are included.[21] Secondly, it is possible to quantify the overall rate of coverage using the GSE Public Use Database, which is more comprehensive but unfortunately does not contain information on key variables important for backing out policy changes. I calculate coverage of around 60 per cent for both Fannie and Freddie prior to 2002 when it declines to 30-40 per cent. After 2002 Freddie's coverage is usually at least 10 percentage points higher than Fannie's.

Because of these concerns, I also use HMDA to validate my conclusions about differences in Fannie and Freddie's debt-to-income policy. While HMDA provides a more comprehensive picture, it does not include the debt-to-income ratio used by the GSEs to assess eligibility, instead reporting the initial loan amount and income. The debt-to-income ratio is defined as the ratio of the monthly mortgage payment, as well as other financial obligations, to gross monthly income. To calculate the debt-to-income ratio given initial loan size and income I would therefore need to know both the household's mortgage interest rate and their other financial obligations, which are not reported. Nonetheless, loan-to-income and debt-to-income are still fairly closely related.[22]

Figure 5(b), constructed using HMDA, shows that the share of loans purchased by Fannie and Freddie with a loan-to-income ratio above 4 displays a similar profile to the high debt-to-income share plotted in Figure 5(a). Because HMDA is available back to 1991, it also allows me to show that Fannie and Freddie's criteria were similar for several years prior to the 1999 change I document. Both Figures 5(a) and 5(b) support the idea that Fannie and Freddie maintained different standards with respect to debt-to-income between 2000 and 2010.

Although Freddie Mac reduced its purchases of loans with a debt-to-income ratio above 50 per cent, it did not eliminate them entirely. This suggests that only some borrowers were affected by the change. In Appendix C I back out this affected group and analyze the policy more detail. The important thing to note is that whether a borrower is

[21]Single Family Loan-Level Dataset Frequently Asked Questions (FAQs), p.3. Fannie Mae does not report similar statistics to my knowledge.

[22]In the absence of other financial obligations, loan-to-income (LTI) and debt-to-income (DTI) are related in the following way:

$$DTI = f(\text{interest rate})LTI$$

where f is the function that coverts the interest rate to the monthly mortgage repayment per \$1 of loan principal.

allowed a high debt-to-income ratio depends on their credit score and loan-to-value ratio. This is illustrated in Figure 6, which shows how the maximum loan-to-value ratio that can be combined with a debt-to-income ratio above 50 per cent varies by credit score. I incorporate this dependence on the loan-to-value ratio into the modeling exercise in Section 7.[23]

In Appendix C I also document how the policy changes over the longer term, and show that other differences in policy between Fannie and Freddie were minor over the period I look it. Establishing that tighter debt-to-income rules were the main difference between Fannie and Freddie prior to 2008 is very important for interpreting the price results. This analysis is also directly relevant for the model in Section 7, as there I incorporate not only the 1999 policy change but also subsequent changes when computing the long-run effect.

When interpreting the results it is useful to have a sense for the share of borrowers affected by the change. Figures 4(a) and 4(b) show that the share of Freddie Mac's purchases with a debt-to-income ratio above 50 per cent fell by around 5 percentage points. I also calculate the share of purchases prior to the change where the borrower falls in the affected group backed out in Appendix C, and has a debt-to-income ratio above 50 per cent. These loans are around 5 per cent of all pre-policy purchases, consistent with the first measure. Overall, around 5 per cent of borrowers were affected in some way, which could mean applying to additional lenders, taking out a smaller loan, or not taking out a loan at all.

5. Effect of DTI Rules on House Prices

Tighter debt-to-income policy reduces the maximum amount of mortgage debt a household can have. This means that some households may not be able to pay as much for a house as they would have under a more relaxed policy. How this transmits to house prices depends on many factors, including the share of households affected by the policy, the substitutability of owned and rented housing and the housing supply elasticity. In this Section I measure the effect of tighter debt-to-income policy on house prices. Firstly, I show that after Freddie tightened its standards, borrowers whose loans were sold to Freddie spent relatively less when buying a house. This is informative about the chan-

[23]In principle, it is important to incorporate the dependence on the loan-to-value ratio as it is a variable that the borrower can potentially adjust. One way to think about the policy is that it gives the borrower a choice between a high LTV loan with a low DTI, or a low LTV loan with a high DTI. In practice, however, it makes sense to think about the policy as a DTI tightening because for most borrowers it does not make sense to switch to the low LTV high DTI option.

nel, but cannot be translated into an equilibrium price effect, partly because of possible substitution from Freddie to Fannie. I then measure the equilibrium price effect by comparing counties with different exposure to lenders who sell to Freddie. In all specifications I use a policy implementation date of June 1999, informed by the analysis in Section 4. I also use within state variation in Freddie Mac exposure. When looking at county level outcomes I condition on county population density from the 2000 census. The regressions are unweighted.

5.1 Research Design

5.1.1 Loan level

Before estimating the effect of debt-to-income restrictions on aggregate house prices I first document a reduction in the house price paid by borrowers whose loans were purchased by Freddie Mac. This helps link the price response directly to the policy change. Specifically, I estimate:

$$
\begin{aligned}
\mathrm{Y}_i = \gamma_s + \gamma_s \times \mathrm{Post}_t + \beta_1 \text{Freddie Mac}_i + \beta_2 \text{Freddie Mac}_i \times \mathrm{Post}_t \\
+ \alpha_1 \mathrm{Controls}_i + \alpha_2 \mathrm{Controls}_i \times \mathrm{Post}_t + \epsilon_{i,t}
\end{aligned}
$$

I use data on loans for home purchase and exclude loans to property investors. The outcomes I consider are the log house price and an indicator equal to one if the debt-to-income ratio exceeds 50 per cent. I include state by Post_t fixed effects. I also condition on a first-time buyer indicator and borrower credit score. Standard errors are clustered by state.

The coefficient of interest, β_2, is interpreted as the difference in outcomes between loans sold to Freddie and loans sold to Fannie in the same state six months after the policy change, relative to the month before the policy change. That is, the post indicator is equal to one if the loan was originated in December 1999 and zero if it was originated in June 1999. This matches the timing used when measuring the county price effect below.

While this loan-level approach helps to establish whether the underwriting change had a direct effect on prices, it does not have a clear implication for the size of the equilibrium price effect. It is unclear how applicants sort across GSEs following the policy change. If a lender actively sells to both Fannie Mae and Freddie Mac, it may start to increase its share of high debt-to-income loans sold to Fannie Mae after the change. Similarly an applicant may choose to go to a different lender. This sorting would lead to the loan-level difference being larger than the equilibrium effect. The price difference could also reflect borrowers substituting to smaller or lower quality properties.

Finally, as I show later, locations where Freddie Mac has a stronger presence experienced a decline in equilibrium prices over the same period, not only within states, but also within core-based statistical areas. Given the lack of precise geographic information in the loan-level dataset it is not possible to abstract from this effect, meaning that the difference in price paid will partly reflect differences in equilibrium prices. We should therefore expect to see a difference in price paid that is considerably larger than the estimated equilibrium price effect, and in Section 5.2 below I show that this is indeed the case. To measure the equilibrium price effect I instead look at how prices diverged across counties with different pre-existing exposure to Freddie Mac.

5.1.2 County level

I estimate the effect of tighter debt-to-income rules on house prices by comparing locations with different pre-existing GSE relationships. I construct an exposure measure based on Freddie Mac's 1998 county market share. The idea is that borrowers applying to lenders who sell to Freddie Mac would face Freddie Mac's tighter rules following the policy change.

Before estimating the effect on prices I first verify that counties more exposed to Freddie Mac experience a relative decline in high debt-to-income lending coinciding with the policy. I estimate:

$$\Delta \log(\text{High DTI}_c) = \gamma_s + \beta \text{Exposure}_{c,1998} + \alpha \text{Controls}_{c,1998} + \epsilon_c$$

where High DTI_c is the share of mortgages originated in county c with a debt-to-income ratio above 50 per cent. The coefficient of interest is β, which is interpreted as the difference in high debt-to-income loan share growth when moving from a location where no lender sells to Freddie, to a location where all lenders sell only to Freddie. I measure the change over three different periods. The short-run effect is measured by comparing 1998 and 2000, and the long-run effect is measured by comparing 1998 and 2005. The pre-period is from 1997 to 1998.[24] I use only counties with non-missing house price data located in a core-based statistical area (metropolitan or micropolitan area). I include state fixed effects and cluster by core-based statistical area. After establishing that a

[24]The dataset has very few observations with non-missing debt-to-income information prior to 1997.

higher Freddie share is associated with a decline in high debt-to-income lending, I look at the effect on house prices using an analogous specification:

$$\Delta \log(\text{Price}_c) = \gamma_s + \beta \text{Exposure}_{c,1998} + \alpha \text{Controls}_{c,1998} + \epsilon_c$$

where the short-run effect is measured over 6 months from June 1999 to December 1999 and the long-run effect is measured over six years from June 1999 to June 2005. It is important to note that with this specification, borrowers switching from Freddie to Fannie will lead to the policy effect being understated, rather than overstated as in the loan-level analysis. This is because when borrowers switch, the connection between Freddie's market share pre-policy, and lending standards post-policy, is weakened. There are good reasons why this substitution is likely to be limited, however. Firstly, as described in Section 3, many lenders have exclusive relationships with either Fannie or Freddie which are very persistent. This restricts the potential for substitution within lender. Secondly, there is evidence that mortgage applicants do not tend to engage in active search and often apply to only one lender.[25]

I make two main claims when interpreting the results. Firstly, I claim that locations with closer ties to Freddie Mac in 1998 experienced different house price outcomes starting in 1999 because of these ties. Secondly, I claim that differences in house prices arise because Freddie Mac imposed more restrictive eligibility criteria with respect to high debt-to-income mortgages. I take a number of steps to address threats to these claims. Firstly, county ties to Freddie Mac are correlated with variables which could be associated with the size of the housing boom. I address this concern by demonstrating that my exposure measure is only associated with house price growth after Fannie and Freddie's criteria diverge (both with and without controls).[26] To verify that the price response precisely matches the timing of the policy change, I plot the response by month using the following specification:

$$\log(\text{Price}_{c,t}) = \gamma_c + \gamma_{s,t} + \beta_t \text{Exposure}_{c,1998} + \alpha_t \text{Controls}_{c,1998} + \epsilon_{c,t} \tag{1}$$

The coefficients β_t are interpreted as the effect of the policy on the total price change since the base period, which is June 1999.

One potential concern is that the change in debt-to-income is simply a consequence

[25] According to the National Survey of Mortgage Borrowers, 77 percent of borrowers in 2013 applied to only one lender (http://files.consumerfinance.gov/f/201501 cfpb consumers-mortgage-shopping-experience.pdf).

[26] 27

of differential house price growth across areas with different exposure to Freddie Mac. This is addressed by the fact that the timing of the policy shown on Figure 4(b) is quite sharp and clearly predates the price response. Furthermore, as described in Section 4, the change specifically affects loans with a debt-to-income ratio above 50 per cent, so the movements in the debt-to-income distribution cannot be easily attributed to changes in average loan characteristics, for example due to house price movements.

Another concern is that if Fannie and Freddie behaved differently along other dimensions, the price response could partly reflect these other policies. In Appendix C I provide evidence that the 1999 debt-to-income change was by far the most substantial divergence in criteria between Fannie and Freddie during the period I consider.

5.2 Results

I first look at the effect of the policy on price paid at the loan level. The estimates in Table 2 show that the policy is associated with a reduction in the price borrowers pay for a house of around 6 per cent. I also look at the effect in the pre-period; however, the dataset only goes back to the start of 1999, and there is a reduced number of Fannie Mae loans at the start of the sample, making the estimates imprecise. Table 2 shows that in the three months prior to June 1999, price paid if anything grew by more for loans sold to Freddie Mac, though the difference is not significant.

Before presenting the county house price results, I first verify that the share of high debt-to-income loans declined in more exposed counties following the policy change. The estimates in the first row and first column of Table 3 are similar to the loan-level estimate in the second column of Table 2, which directly compares loans purchased by Fannie and Freddie within the same state. That is, the relationship between the county high debt-to-income share and Freddie's market share is similar to the direct difference between Freddie's high debt-to-income share and Fannie's high debt-to-income share. This is what we would expect if there is limited substitution. That is, if Freddie tightening its rules had little real impact because of substitution, we would see a weak relationship between overall county debt-to-income statistics and exposure to Freddie sellers. The smaller long-run difference in Table 3 is also consistent with the loan-level data, given the partial reversal of the policy documented in Section 4 and Appendix C. It could also reflect an increase in substitution over time, which if anything would attenuate the long-run effect.

Next I look at the effect on house prices. The first row of Table 4 summarizes the main results. Moving from a Freddie share of zero to one is associated with relative decline in house prices of $1\frac{1}{2}$ per cent in the 6 months following the policy change. Column 4 shows that the policy effect measured over the 6 years to 2005 is substantially larger at over 7 per cent. This difference between the short-run and long-run effects is quite striking and I provide some possible explanations for it below. Table 4 shows that effects of a similar magnitude are obtained using Zillow house price data.

Next I plot the house price response by month and show that it lines up precisely with the timing of the policy change. Figure 7(a) plots the estimates of β_t from Equation 1 and a 95 per cent confidence interval for months close to the policy change. Figure 7(c) illustrates how the effect evolves over the longer term. The coefficient on $\text{Exposure}_{c,1998}$ expands over the course of the housing boom and contracts in the bust.

Table 5 shows that the results are robust to using variation within the same core-based statistical area. Table 6 shows the estimates from a specification where the exposure measure is interacted with housing supply elasticity. The policy change tends to have a stronger effect in locations with more inelastic housing supply.[28]

When constructing the exposure measure, I exclude lenders who originated more than 20000 home purchase loans in 1998. The reason why I exclude very large institutions in the main analysis is that in 1999 Fannie and Freddie renegotiated relationships with several large lenders and in some cases allowed them to use their own software to underwrite loans sold to Fannie or Freddie. This means that for very large lenders, 1998 GSE relationships are not as informative about the underwriting standards used later on. Table 7 shows the results when including all HMDA loans. Both short-run and long-run effects are similar in terms of magnitude, but the long-run estimates have much larger standard errors.

I also check whether the house price effect is monotonic in the exposure measure. Figure 8 plots the average short-run and long-run price changes within each quartile of the exposure measure, conditional on population density. The results are qualitatively similar regardless of the fixed effects used and show the effects are broadly monotonic.

5.3 Interpreting the long run effect

The fact that the effect of the policy continues to build over several years is quite surprising and calls for an explanation. There are two main ways to interpret the long-run price difference as a direct effect of the initial policy change. The first is that as households move closer to the 50 per cent debt-to-income limit over the course of the boom, this widens the price gap between Fannie and Freddie areas. That is, a larger proportion

[28]It helps to remember that the policy effect implies slower growth, not an absolute price decline.

of households are affected by a given difference in debt-to-income limits as the average debt-to-income ratio rises. The second interpretation relates to price momentum, possibly reflecting households incorporating the past effect of the policy into their expectations. In the first case we expect leverage to keep diverging for Fannie and Freddie's purchases in the same location. In the second case, all borrowers in the area are affected regardless of whether their loans are sold to Fannie Mae or Freddie Mac.

As I discuss in Section 7, a simple model suggests that the first explanation cannot account for the long-run effect, and is in line with Figure 5, which shows that Fannie and Freddie's purchases of high leverage loans did not diverge much further after 2000. This leaves two main candidates for the unexplained part of the long-run effect – either it is a direct result of the initial change and reflects price momentum, or it is the result of some correlation which was not relevant before mid 1999 (as there is no pre-trend in Figure 7(a)) but became relevant afterwards. The second explanation seems unlikely, especially given that a lot of the policy effect shown in Figure 7(a) occurs before 2003. This limits the role that stories relating to the private securitization boom can play in explaining the effect. In Section 7 I demonstrate that the long-run effect is consistent with a model of price momentum disciplined by survey estimates.

6. Effect of DTI Rules on Default Rates

6.1 Research Design

One of the motivations for restricting household leverage is to reduce default rates. Leverage restrictions may reduce default rates directly, by reducing the probability that a household either cannot repay, or chooses not to repay because the amount owed is larger than the property value. An indirect effect on default is also possible if leverage restrictions dampen price cycles, as this should reduce the share of households who end up with negative home equity in a bust. I estimate the relationship between exposure to Freddie Mac's more restrictive underwriting criteria and default using the CoreLogic Loan Level Market Analytics database:

$$\text{Default}_i = \gamma_{s,t} + \beta_t \text{Exposure}_{c,1998} + \alpha_t \text{Controls}_c + \epsilon_i$$

Where loan i is originated in county c in state s in year t and Default_i is equal to one if loan i was ever more than 90 days past due in a five-year period after the loan was taken out.

6.2 Results

Figure 9(a) shows the estimated coefficients on $\text{Exposure}_{c,1998}$. Exposure to tighter underwriting standards has, if anything, a positive effect on default in the short-run (possibly reflecting weaker price growth in more exposed areas). However, for the 2006 – 2008 cohorts default rates are about 5 percentage points (or about 25 per cent) lower. I run the same regression conditioning on credit score bins, and a flexible interaction of loan-to-value and debt-to-income bins. Similar estimates of β_t after conditioning on individual leverage suggest the reduction in default comes from the effect on house prices rather than differences in leverage at origination (Figure 9(b)).

7. Theoretical Framework

In this section I describe a model of housing demand in which mortgage leverage policies affect house prices. The short-run effect of the policy depends on the characteristics and behavior of households who are constrained by debt-to-income and loan-to-value limits. In order to accurately capture the effect of the policy on housing demand I therefore incorporate heterogeneity in income, assets and housing preferences. The long-run effect of the policy also depends on how the short-run price effect feeds back to expectations, and the model allows me to treat expectations parametrically in a way that can be matched to the data.

First I show that the model can replicate the short-run effect estimated in Section 5. I then show that after adding adaptive expectations the model can explain the entire empirical response profile estimated in Section 5, reconciling the short-run and long-run effects. Finally, I use the model to argue that Fannie and Freddie's relaxation of debt-to-income limits during the 1990s can explain a sizable share of the housing boom. I discuss this policy in more detail in Section 2 and Appendix A.[29]

Households in the model choose how to allocate their income to housing services and non-housing consumption in a single period. The frictionless allocation depends only on income, the housing preference parameter and the price of housing services (user cost), but because the housing asset must be purchased in order to consume housing services, the available downpayment and the mortgage policy will also matter. While it is not necessary for the rental market to be completely absent in order for leverage policy to

[29]It is also more challenging to estimate the effect of this earlier change directly. In Appendix A I provide direct empirical evidence of similar effects; however, there are some considerations which make this setting less than ideal as a pure debt-to-income experiment.

affect house prices, some form of market segmentation is required and I choose to exclude the rental market entirely for simplicity.[30]

In the model, the effect of leverage policy on house prices is determined primarily by housing preferences and the joint distribution of assets and income. Intuitively, whether a household is constrained by the leverage policy depends, firstly, on how much of their income they would choose to spend on housing services in a frictionless world and, secondly, on how much of the property value they need to borrow. The simplicity of the model allows me to accurately capture heterogeneity along these dimensions by directly matching survey and mortgage data.

The level of the user cost also matters for the size of the effect. As the user cost declines, households want to buy more of the housing asset, and their available downpayment may not be sufficient to support this. Furthermore, if the decline in the user cost is driven by an increase in expected capital gains, the debt-to-income ratio calculated by the bank will rise. Intuitively, when the household expects mortgage interest, depreciation and property taxes to be largely offset by capital gains they may end up with a high debt-to-income ratio even if they have a relatively weak preference for housing services. This makes debt-to-income constraints more binding during a housing boom.

The household's problem is to choose housing H_i and non-housing consumption C_i to maximize:

$$u(H_i, C_i) = \alpha_i \log H_i + (1 - \alpha_i) \log C_i \tag{2}$$

subject to an LTV constraint, DTI constraint and budget constraint:

$$PH_i \leq A_i/(1 - \theta^{ltv}) \tag{3}$$

$$PH_i \leq \frac{(\theta^{dti} - \nu)y_i}{f(r) + \tau} + \frac{f(r)}{f(r) + \tau} A_i \tag{4}$$

$$0 = y_i - C_i - (r + \tau + \delta - g)PH_i \tag{5}$$

where A_i is the net assets the household is endowed with and can use for a down-

[30] When calibrating the model I focus on recent homebuyers. This is important because renters in the data, if forced to own in the model, would likely be very responsive to changes in leverage policy. The appropriate way to think about the absence of the rental market in the model is that when leverage policy is tightened, households are not allowed to switch from being owners to renters. To the extent that households would have done so in practice, the model price effect will be too large.

payment, y_i is the resources household i has available to spend on housing H_i and other goods C_i; P is the price of one unit of the housing asset; τ is the property tax rate, δ is the depreciation rate, g is the expected capital gain, ν is the share of income allocated to other financial commitments (e.g. non-mortgage debt payments and child support) and $f(r)$ is the 30 year fixed rate mortgage payment on a \$1 loan when the interest rate is r.

The intuition for the budget constraint is that the expression for the price of housing services, $(r+\tau+\delta-g)P$, corresponds to a fairly standard definition of the user cost. It can also be derived using a dynamic model which I describe in Appendix B.[31] The user cost can be defined in different ways, but usually includes mortgage interest, property taxes, the forgone return on home equity, maintenance costs and depreciation, offset by the rate of house price appreciation. The expression I use here corresponds to this definition if we think of maintenance costs as being included in δ, and the forgone return on home equity as being equal to the mortgage rate. In the model, subtracting g when computing the user cost implicitly assumes that households can consume their expected capital gain in the current period. This is appropriate as it makes housing demand depend on expected price growth in a way that closely corresponds to the impact of price growth in a dynamic model. It is also consistent with common definitions of the user cost.

An accurate user cost calculation would also incorporate mortgage interest and property tax deductions. I abstract from that here as the main goal is to broadly match the overall level of the user cost, and incorporate expected house price appreciation appropriately. The calibrated 1998 user cost of around 6 per cent of the property value ends up being similar to HUD calculations based on the American Housing Survey.[32]

Assets in the model cannot be used to fund consumption, and exist only for the purpose of determining feasible housing options. This is relaxed in the dynamic problem in Appendix B. The price effect of a debt-to-income policy change is similar under certain conditions which are described further in the Appendix and Section 7.5. I define the leverage policy as:

$$\mathbf{X} := \{\theta^{ltv}, \theta^{dti}\}$$

Combining LTV and DTI constraints, H_i must satisfy:

$$PH_i \leq \overline{PH}(A_i, y_i, \mathbf{X}) = \min\left\{\frac{A_i}{(1-\theta^{ltv})}, \frac{(\theta^{dti}-\nu)y_i}{f(r)+\tau} + \frac{f(r)}{f(r)+\tau}A_i\right\} \tag{6}$$

[31] The expression in the dynamic model is slightly different due to timing assumptions.

[32] https://www.huduser.gov/periodicals/ushmc/summer2000/summary-2.html. These calculations also depend on the forgone return on home equity, which is fairly subjective anyway.

I first solve the problem assuming the household is not constrained by either LTV or DTI. Then for households violating one of the constraints, I set $PH_i = \overline{PH}(A_i, y_i, \mathbf{X})$. The unconstrained problem is standard and a constant budget share is allocated to housing:

$$PH_i = \alpha_i \frac{y_i}{(r + \tau + \delta - g)} \tag{7}$$

Constrained households choose to spend the maximum feasible amount on housing:

$$PH_i = \overline{PH}(A_i, y_i, \mathbf{X}) \tag{8}$$

Summing over constrained and unconstrained households, and letting c denote the user cost as a proportion of the property value, $r + \tau + \delta - g$, gives:

$$\int_i PH_i = \int_i \left(\alpha_i \frac{y_i}{c} \mathbb{1}[i \text{ unconstrained}] + \overline{PH}_i \mathbb{1}[i \text{ constrained}] \right) di \tag{9}$$

Given the leverage policy, there is a cutoff $\bar{\alpha}$ for each level of assets A_i and income y_i, where households with $\alpha_i > \bar{\alpha}$ are constrained by the mortgage policy and those with lower levels of α_i are not. This gives the following expression for total nominal housing demand, where $f(\alpha)$ is the housing preference pdf and $g(A, y)$ is the joint asset and income pdf:

$$\int_i PH_i = \int_A \int_y \int_\alpha d(A, y, \alpha, c, \mathbf{X}) f(\alpha) g(A, y) d\alpha dy dA \tag{10}$$

and the nominal demand of an individual household with assets A, income y, and housing preference α given user cost c and leverage policy $\mathbf{X}$ is:

$$d(A, y, \alpha, c, \mathbf{X}) = \alpha \frac{y}{c} \mathbb{1}[\alpha \leq \bar{\alpha}(A, \frac{y}{c}, \mathbf{X})] + \overline{PH}(A, y, \mathbf{X}) \mathbb{1}[\alpha > \bar{\alpha}(A, \frac{y}{c}, \mathbf{X})]$$

This gives the following expression for housing demand:

$$\int_i PH_i = k_d(\mathbf{X}, c) \Rightarrow H^d = \frac{k_d(\mathbf{X}, c)}{P} \tag{11}$$

where H_d is total housing demand and k_d is a function of the leverage policy $\mathbf{X}$ and user cost c. A relaxation of leverage constraints or a decline in the user cost will increase housing demand. I assume a constant elasticity supply function with elasticity ϵ:

$$H^s = k_s P^\epsilon \tag{12}$$

which gives the following expression for the equilibrium house price:

$$P = \left(\frac{k_d}{k_s}\right)^{\frac{1}{1+\epsilon}} \tag{13}$$

The log price difference between places with leverage policies 1 and 2 is then:

$$\Delta \log P = \frac{1}{1+\epsilon}\Bigg(\log k_d(\mathbf{X}_1, c) - \log k_d(\mathbf{X}_2, c)\Bigg) \tag{14}$$

This means that I can calculate the price effect of a policy change by evaluating the right hand side of 10 for some particular choice of preference distribution, joint asset and income distribution, and values for r, τ, δ and g.

When showing how the policy effect changes from 1999 to 2007, I keep parameter values constant with the exception of expected house price growth g. Because house price growth is treated as a parameter, the model can be used to generate predictions under alternative assumptions about how the policy feeds back to expectations. Below, I focus on two cases. First, I update g using the observed house price history and an adaptive expectations rule, but hold it constant across locations with different leverage policies. In this case the dynamic effects of the policy derive primarily from an increase in the share of constrained households over time. This is because the path for g generates an increase in average debt-to-income similar to that observed in the data. In the second case, I allow the house price history to reflect the past effect of the policy change when computing g. This leads expected house price growth to diverge across locations after the policy is implemented.

Specifically, when computing the price effect for year $t+1$ in the case where g is constant across locations, I use:

$$g = \frac{\lambda}{1-(1-\lambda)^{t+1-t_0}} \sum_{j=0}^{t-t_0} (1-\lambda)^j g_{t-j} \tag{15}$$

where g_{t-j} is actual house price growth in year $t-j$ and t_0 is the first year for which house price growth is observed in the data.[33] I set $\lambda = 0.11$ to match survey evidence on the relationship between house price expectations and lagged house price growth (Armona et al. (2017); Case et al. (2012)).[34] Using this formula it is also straightforward

[33]Given a long price history, this is approximately equal to the more intuitive expression $g = \lambda \sum_{j=0}^{t-t_0}(1-\lambda)^j g_{t-j}$. The additional factor $\frac{1}{1-(1-\lambda)^{t+1-t_0}}$ adjusts for the finite price history so that the weights sum to 1. Not including this factor simply means that growth over the unobserved period is implicitly assumed to be zero. In my application g is not very sensitive to this adjustment.

[34]Specifically, this value of λ generates an estimated coefficient of 0.23 when regressing expected house

to incorporate feedback from the policy to g. That is, after the policy is implemented I allow g to diverge across locations, so for locations affected by the policy change:[35]

$$\tilde{g} = \frac{\lambda}{1-(1-\lambda)^{t+1-t_0}} \sum_{j=0}^{t-t_0} (1-\lambda)^j (g_{t-j} + \text{policy effect}_{t-j}) \tag{16}$$

Consequently, households exposed to the more restrictive policy start to expect weaker price growth going forward which raises their user cost. This means that for each period following the implementation period, the user cost is no longer held constant across affected and unaffected locations:

$$\Delta \log P = \frac{1}{1+\epsilon}\Bigg(\log k_d(\mathbf{X}_1, c(g)) - \log k_d(\mathbf{X}_2, c(\tilde{g}))\Bigg) \tag{17}$$

Next I characterize $\bar{\alpha}(A, \frac{y}{c}, \mathbf{X})$, which is the maximum value of α for which a household with net assets A and income y is unconstrained, and $\overline{PH}(A, y, \mathbf{X})$, which is the maximum amount a household with net assets A and income y can pay for a house. These are needed to compute the effect of the policy on house prices.

7.1 *Characterizing the constrained group*

Understanding which types of households are constrained by different leverage policies is important for gaining intuition about how prices respond to a policy change. The price effect of a policy change in this framework follows directly from the share of households who are constrained and how far away they are from their housing demand in frictionless world. Households are constrained if their assets are insufficient to buy the house they would have bought in the absence of leverage constraints. I start by characterizing $\bar{\alpha}(A, \frac{y}{c}, \mathbf{X})$, the value of α_i above which households are constrained, and the maximum home value $\overline{PH}(A, y, \mathbf{X})$ for the case where the household faces two leverage constraints (a single DTI limit θ^{dti} and LTV limit θ^{ltv} which must both be satisfied). Combining these gives Equation 6 above. Figure 10(a) shows graphically how the maximum home value is related to the assets available for a downpayment. There is a single asset cutoff $\bar{A}$ such that for $A_i < \bar{A}$ the maximum home value is $\frac{A_i}{(1-\theta^{ltv})}$ and for $A_i > \bar{A}$ the maximum home

price growth on lagged house price growth, which matches Case et al. (2012) and is similar to Armona et al. (2017). I use FHFA house price data for the counties considered by Case et al. (2012) and the same sample period, which is 2003 – 2012.

[35]Strictly speaking, Equations 15 and 16 should take into account the fact that the price history in the data reflects an average of price growth across areas with different exposure to Freddie Mac. I ignore this for simplicity.

value is $\frac{(\theta^{dti}-\nu)y_i}{f(r)+\tau} + \frac{f(r)}{f(r)+\tau}A_i$. The cutoff $\bar{A}$ satisfies:

$$\frac{\bar{A}}{(1-\theta^{ltv})} = \frac{(\theta^{dti}-\nu)y_i}{f(r)+\tau} + \frac{f(r)}{f(r)+\tau}\bar{A}$$

$$\Rightarrow \bar{A} = \frac{(\theta^{dti}-\nu)(1-\theta^{ltv})y}{\theta^{ltv}f(r)+\tau}$$

This says that for households with assets below $\bar{A}$, the maximum home value is determined by the loan-to-value limit, θ^{ltv}. For households with assets above $\bar{A}$ the maximum home value is instead determined by the debt-to-income limit, θ^{dti}. The intuition is that the debt-to-income limit corresponds to a dollar limit on loan size, given income, interest rate and other financial obligations. In contrast, the loan-to-value limit allows the household to keep increasing the loan size as long as they are able to match each additional $\$\theta^{ltv}$ of debt with $\$(1-\theta^{ltv})$ of downpayment. Households with limited assets reach their maximum downpayment before they hit the loan limit implied by the debt-to-income constraint.

Because the policy I consider in the empirical section of the paper involves a choice between two sets of constraints, I now consider the case where the household can choose either $\{\theta_l^{dti}, \theta_h^{ltv}\}$ or $\{\theta_h^{dti}, \theta_l^{ltv}\}$, where $\theta_h^{ltv} > \theta_l^{ltv}$ and $\theta_h^{dti} > \theta_l^{dti}$. This corresponds to the observation in Section 4 that households can only have a debt-to-income ratio above 50 per cent if their loan-to-value ratio is sufficiently low. Specifically, when calculating the effect of the policy change in the model I use the parameters shown in Table 8.

The initial policy is equivalent to a two parameter policy with $\theta^{ltv} = 0.95$ and $\theta^{dti} = 0.65$. This is consistent with the fact that around $\frac{1}{4}$ of purchase loans to owner-occupiers in the GSE single family loan performance dataset in 1999 had an LTV of 95 per cent or above. Debt-to-income ratios are top-coded above 65 per cent. As I discuss in Section A, debt-to-income ratios above 65 per cent were allowed but in 1999 very few borrowers in the dataset had a debt-to-income ratio of 65 per cent or above in practice. The short-run price response is therefore not particularly sensitive to whether θ_h^{dti} is set to 0.65 or a higher level.

Under the new policy, the value of θ_l^{ltv} depends on credit score. I incorporate this by calculating housing demand separately for each value of θ_l^{ltv} and multiplying by the share of borrowers in each credit score group in the data. For example, looking at Figure 19(b), a borrower with a credit score of 700 is allowed to have a debt-to-income ratio above 50 per cent if their loan-to-value ratio is below 60 per cent, but if their loan-to-value ratio is higher than this they must have a debt-to-income ratio below 50 per cent. This corresponds to $\theta_l^{dti} = 0.5$ and $\theta_l^{ltv} = 0.6$.

Faced with both high and low DTI constraint sets, the household chooses the more favorable constraint set given their assets and income:

$$PH_i \leq \max\left\{\min\{\frac{A_i}{(1-\theta_h^{ltv})}, \frac{(\theta_l^{dti}-\nu)y_i}{f(r)+\tau} + \frac{f(r)}{f(r)+\tau}A_i\}, \min\{\frac{A_i}{(1-\theta_l^{ltv})}, \frac{(\theta_h^{dti}-\nu)y_i}{f(r)+\tau} + \frac{f(r)}{f(r)+\tau}A_i\}\right\}$$

This case is illustrated in Figure 10(b). There are now four regions instead of two, and three points where $\overline{PH}(A, y, \mathbf{X})$ has a kink. These cutoffs are:

$$\bar{A}_1 = \frac{(\theta_l^{dti}-\nu)(1-\theta_h^{ltv})y}{\theta_h^{ltv}f(r)+\tau}$$

$$\bar{A}_2 = \frac{(\theta_l^{dti}-\nu)(1-\theta_l^{ltv})y}{\theta_l^{ltv}f(r)+\tau}$$

$$\bar{A}_3 = \frac{(\theta_h^{dti}-\nu)(1-\theta_l^{ltv})y}{\theta_l^{ltv}f(r)+\tau}$$

The largest house price for a household with very low assets is determined by θ_h^{ltv}. As assets increase, households eventually reach point $\bar{A}_1$, where if they buy the biggest house they can with LTV $= \theta_h^{ltv}$ they hit the low DTI limit θ_l^{dti}. After hitting the DTI limit (which is a $ limit on loan size determined by income and the interest rate), the household needs to add a dollar of downpayment for every additional dollar of housing. As they do this, their LTV at the maximum house price is gradually reduced. At point $\bar{A}_2$, their LTV is sufficiently low that it becomes preferable to switch to the low LTV–high DTI constraint set. Households between $\bar{A}_2$ and $\bar{A}_3$ get the maximum housing by setting LTV $= \theta_l^{ltv}$. Eventually they hit the high DTI limit θ_h^{dti} and once again need to add a dollar of downpayment for every additional dollar of housing. This gives:

$$\overline{PH}(A, y, \mathbf{X}) = \begin{cases} \frac{a}{1-\theta_h^{ltv}} & a \leq \bar{A}_1 \\ \frac{(\theta_l^{dti}-\nu)y}{f(r)+\tau} + a\frac{f(r)}{(f(r)+\tau)} & \bar{A}_1 \leq a \leq \bar{A}_2 \\ \frac{a}{1-\theta_l^{ltv}} & \bar{A}_2 \leq a \leq \bar{A}_3 \\ \frac{(\theta_h^{dti}-\nu)y}{f(r)+\tau} + a\frac{f(r)}{(f(r)+\tau)} & a > \bar{A}_3 \end{cases}$$

Not all households will want to buy the most expensive house possible, only those who want to spend a sufficiently large share of their income on housing. For each level of assets A, only households with $\alpha_i > \bar{\alpha}(A, \frac{y}{c}, \mathbf{X})$ will be constrained, where:

$$\bar{\alpha}(A, \frac{y}{c}, \mathbf{X}) = \frac{(r+\tau+\delta-g)\overline{PH}(a,y,\mathbf{X})}{y}$$

Figure 11 illustrates how households are affected by changes in each of the four policy parameters $\theta_l^{dti}, \theta_h^{ltv}, \theta_h^{dti}, \theta_l^{ltv}$. In each case, changing one of the four parameters will only affect households located on a single segment of $\overline{PH}(a,y,\mathbf{X})$, with the exception of households who switch segments as a result of the change. Table 9 shows the asset cutoffs affected by changing each policy parameter. In each case, relaxing the leverage policy with respect to one parameter weakly increases the maximum home value at each level of assets. However, only households with assets in a particular range experience a strict increase. Consequently, the effect of a given policy on house prices is quite sensitive to the asset distribution. Although not shown in this figure, as assets increase, the cutoff $\bar{\alpha}$ above which households are constrained also increases. This is also relevant for understanding which policy changes have large effects. Intuitively, households whose assets are high enough to potentially benefit from a relaxation in θ_h^{dti} are more likely to already be unconstrained because they do not need to borrow a large share of the home value.

From the description of $\overline{PH}(a,y,\mathbf{X})$ above, and Figure 11, we can see that the approximate effect of a one percentage point change in θ_l^{dti} on the equilibrium house price is closely related to the following (approximate) percentage change in price paid by households with assets in the potentially affected region, that is $A \in [\bar{A}_1, \bar{A}_2]$:

$$\frac{\frac{\partial \overline{PH}(A,y,\mathbf{X})}{\partial \theta_l^{dti}}}{\overline{PH}(\bar{A}_1,y,\mathbf{X})} = \frac{\frac{y}{(f(r)+\tau)}}{\frac{(\theta_{l,old}^{dti}-\nu)y}{\theta_h^{ltv}f(r)+\tau}} = (\theta_{l,old}^{dti}-\nu)\frac{\theta_h^{ltv}f(r)+\tau}{f(r)+\tau}$$

If $\tau = 0$ this simplifies to:

$$\frac{\frac{\partial \overline{PH}(A,y,\mathbf{X})}{\partial \theta_l^{dti}}}{\overline{PH}(\bar{A}_1,y,\mathbf{X})} = (\theta_{l,old}^{dti}-\nu)\theta_h^{ltv}$$

To convert this to a percentage change in housing demand (and therefore the equilibrium house price through 14), we need to also incorporate the fact that only households with $A_i \in [\bar{A}_1(y_i), \bar{A}_2(y_i)]$ are affected, and then only if $\alpha_i > \bar{\alpha}(A_i, \frac{y_i}{c}, \mathbf{X})$. This illustrates how the user cost and asset, income and preference distributions are important for determining the price effect.

7.2 Calibration

I interpret households in the model as those currently in the market for a house, so it is desirable to calibrate the asset and income distributions using information on recent buyers. This is also consistent with the fact that the GSE datasets I use relate to recent mortgage originations.

I calibrate the model using mortgage origination data from January 1999 (before the policy change), the 1998 Survey of Consumer Finances (SCF) and the 1991 – 1995 American Housing Survey (AHS). The parameter values are shown in Table 10. I set parameters ν and τ using the SCF. For each household I calculate monthly payments on other debts, alimony and child support as a share of monthly income. I set ν to be the average share across home-owning households. I set τ by calculating average monthly property tax payments as a share of the property value for home-owning households. I use an annual depreciation rate of 2 per cent and show results for different housing supply elasticities.

Next I calibrate the joint distribution of assets and income. Both the AHS and the SCF allow me to identify recent buyers, but with some caveats. The AHS contains a much larger sample of recent buyers, but does not have information on assets other than home equity. While the SCF has more comprehensive asset information, it only has a small sample of households who bought recently. I compute the policy effect using each dataset separately and both yield very similar results. I use the AHS in preference because of the larger sample.

I assume that assets and income follow a joint log-normal distribution. Given the information available in the AHS I define assets as home equity at origination.[36] Figures 12(a) and 12(b) calculated using the AHS show that a log-normal distribution fits the data well for most households, except those with assets or monthly income less than \$1000,[37] or very high levels of assets or monthly income. This is because both the empirical log income and log asset distributions are somewhat negatively skewed.

The policy parameters I use are shown in Table 8. Appendix C explains how these policies are backed out using the loan-level data. I apply the policies in Table 8 to 60 per cent of Freddie Mac borrowers. I then assume that 20 per cent of Freddie Mac borrowers are subject to Fannie Mae's policy, and 20 per cent are subject to the tighter

[36] When calibrating the distribution using the SCF I include assets that would likely be available for a downpayment if the household were to move to a different property: liquid assets less credit card debt, plus home equity. For households who recently purchased a property these two definitions of assets are on average broadly similar.

[37] i.e. log assets or income less than zero, when assets or income are measured in thousands.

debt-to-income limit regardless of their credit score or loan-to-value ratio. This choice is informed by the data and motivated in more detail in Appendix C. Intuitively, these two other groups likely correspond to loans processed using other software, and the presence of additional eligibility rules based on variables not included in the dataset. The results obtained assuming the policies in Table 8 apply to all loans are qualitatively similar. The main difference is that this assumption implies a full reversal of the policy after 2005, which is in contrast to the data.

I assume the housing preference parameter α is distributed according to a beta distribution and calibrate the distribution parameters to match the median pre-policy debt-to-income ratio, and the share of mortgages with a debt-to-income ratio exceeding 50 per cent. Figure 12(c) compares the pre-policy debt-to-income distribution in the data with the one in the model.

It is important to realize that as long as the user cost parameters are held constant when changing the policy, they have very little effect on the short-run policy response. The relevant item for computing the effect of the policy is the frictionless housing demand:

$$H_i^* = \frac{\alpha_i y_i}{(r + \tau + \delta - g)P}$$

Because the mortgage moments are directly informative about the size of nominal housing demand $P \cdot H$ relative to income, the calibrated parameters of the α distribution will adjust to accommodate different assumptions about the user cost, leaving the short-run policy effect nearly unchanged. An important departure from this is allowing expected capital gains, g, to vary with the policy. In this case, the way g is specified does matter for the policy effect. The amplification effect is also quite sensitive to how large the user cost is. When the user cost is close to zero, changes in g have a larger effect on demand and therefore prices.

The model is able to match movements in the debt-to-income distribution over the course of the housing boom reasonably well. It is important that the model broadly replicates the increase in the high debt-to-income share because I use it to assess the role these movements played in causing the policy effect to build over time. Figure 12(d) compares the high debt-to-income share of purchase originations in the GSE datasets with the high debt-to-income share implied by the model. I calculate the model share as the average under the two policies, weighted by Fannie Mae and Freddie Mac's market share in the data.

7.3 *Model results*

Table 11 shows the model price effect of the policy change described in Section 4 for different horizons and housing supply elasticities. The first column of Table 11 shows the short-run effect. The immediate price decline of 0.7 per cent assuming fixed housing supply is somewhat smaller than the empirical estimate of 1.6 per cent shown in the bottom row. In the model, the short-run effect does not incorporate any feedback from the policy to expectations by definition. In the data, however, the short-run effect is measured 6 months after the policy change and could potentially reflect feedback.[38] As Figure 7(a) shows, because the effect in the data expands over time the comparison is very sensitive to the choice of horizon. For example, the price decline of 0.6 per cent measured from June to September 1999 is very similar to the immediate effect in the model.

Looking at longer horizons, the model effect continues to increase even without feedback to expectations. The first row shows that when expectations are held constant across locations the effect increases to 1.2 per cent in 2001, 1.7 per cent in 2003 and 3.4 per cent in 2005. This reflects the fact that expected national house price growth rises over the course of boom (all the other parameters are held constant at pre-policy levels). As people demand more housing because of anticipated price growth, debt-to-income ratios rise and a larger proportion of households falls into the region affected by the policy. This is similar to the idea that a constant upper limit on the debt-to-income ratio tends to exert more downward pressure on prices in boom, when it is binding, than in a bust, when it is not binding. This channel alone cannot explain the way the empirical effect builds over time. By 2003 the empirical effect is already 4.5 per cent, but the model is unable to capture this despite the fact that it matches the high debt-to-income share in 2003 very well (Figure 12(d)).

Something else is needed to explain the empirical response. In the second row of 11, I allow expected house price growth to vary with the policy according to equation 17, generating an effect in 2001 of 3.5 per cent, and an effect in 2005 of 18.3 per cent. This pattern of expansion is qualitatively similar to what we observe in the data, but generates a long-run effect which is too large. This is not that surprising given that I assume a housing supply elasticity of zero. The fifth row of Table 11 shows that a supply elasticity of 0.25 gives effects in 2003 and 2005 which are very similar to the data. A low housing supply elasticity is also consistent with the fact that the policy had little overall effect on

[38] It could also reflect a temporary increase in mortgage rates which is not captured in the model. Movements in interest rates cannot explain in the expansion in general, however, as they declined over the period I focus on.

the number of residential building permits. The response differs across locations. Building permits do not respond in metropolitan counties at all, while there is some response in micropolitan counties.[39] These results are discussed further in Appendix D.

I also show results where I update expected house price growth g in equation 16 using the past empirical response, rather than the model response. With an elasticity of 0.25 this also generates a similar response profile to what we observe in the data. Overall, the model generates a short-run effect which is consistent with the empirical response, and the shape of the entire empirical response profile can be captured well by allowing feedback from the policy to house price expectations.

7.4 Effect of 1990s debt-to-income relaxation

In addition to interpreting and checking the plausibility of the empirical results, we can use the calibrated model to assess the effects of additional policies that are challenging to identify empirically. In the second half of the 1990s, Fannie Mae and Freddie Mac removed their historical debt-to-income limit of 36 per cent for lenders using their automated underwriting software. I calculate the effect of this relaxation using the model, and find that it can explain a sizable share of the housing boom.

I assume the policy change increases the maximum debt-to-income ratio from 36 per cent to 65 per cent while holding the maximum loan-to-value ratio fixed at 95 per cent. That is, I think of the experiment as moving from a debt-to-income limit of 36 per cent to a policy consistent with the GSEs' 1999 purchases. So the final policy in this second experiment corresponds to the initial policy in the main experiment.[40] In Appendix A I discuss the background in more detail.

As the software was adopted gradually, this rule change was initially limited to a relatively small group of lenders. Adoption increased rapidly in 1998 and was largely complete by 2000. In Appendix A I describe a second natural experiment based around this gradual adoption. While suggestive, these empirical estimates could reflect forces other than the debt-to-income relaxation I am interested in, so it is useful to see what the model implies.

Here I show the model price response assuming that debt-to-income rules were relaxed

[39] Micropolitan counties are counties in an urban area with an urban core population of at least 10000 but less than 50000. Metropolitan counties are counties in an urban area with an urban core population of at least 50000.

[40] Although it is possible that the GSEs had borrower specific debt-to-income limits below 65 per cent even after the relaxation, this is implicitly accounted for to some extent because I calibrate the model to match the observed 1999 debt-to-income distribution for GSE purchases.

in 1996, and only for lenders using the software. While newspaper reports indicate that Freddie Mac's software had relaxed debt-to-income requirements when it was first released in 1995, Gates et al. (2002) report that the original software was considerably more conservative than the 2000 version. It is unclear exactly when the relaxation occurred. The choice of 1996 is consistent with the empirical analysis in Appendix A. However, because software usage was low prior to 1998, the results are not particularly sensitive to the exact implementation date.

In contrast, using an accurate software adoption profile is important. I construct a measure of the share of loans processed using the software each year based on statistics reported by Fannie Mae and Freddie Mac. These statistics are reported in Table 15 and the adoption measure is illustrated on Figure 13(a).[41] I also show results for the hypothetical case of full software adoption in 1996, as it more closely corresponds to the empirical setting in Appendix A.

For this application I continue to use the same joint income and asset distribution, and the same preference distribution. I use interest rates, property tax rates and other obligations from 1995, and calculate expected house price growth g using Equation 16 and pre-1996 house price history. Unlike the previous exercise, I do not update g based on the true house price history. Because I am interested in seeing whether the GSEs' expansion of debt-to-income criteria can generate a boom, it is appropriate to assume that no boom occurred in the absence of the expansion. Updating g would make the effect even larger, because it would lead to a larger share of borrowers having an ideal debt-to-income ratio above 36 per cent. I do, however, incorporate feedback from the policy to house price expectations.

The first row of Table 12 shows the effect of the policy under gradual software adoption. Although the size of the policy change is large, the initial price increase is fairly small due to the low rate of adoption. As more lenders start applying the new rules, the effect increases. This is also compounded by feedback from the policy to expectations. The policy ultimately leads to an increase in prices of around 15 per cent by 2000, and (assuming fixed housing supply) around 38 per cent by 2005. However, as we saw in Table

[41] At the start of 1998 both Fannie and Freddie were projecting adoption rates of up to 85 per cent, and Freddie reported a rate of 75 per cent at some point in 1999. However, throughout 1999 both Fannie and Freddie made agreements with large lenders to accept loans underwritten using different software, and the share of purchases underwritten using Desktop Underwriter or Loan Prospector fell to around 60 per cent. The Single Family Loan Performance dataset indicates that both Fannie and Freddie continued to purchase a large share of high DTI loans from the lenders they had made these arrangements with. For the purposes of this exercise, I therefore assume that an adoption rate of 75 per cent continues to apply to both Fannie and Freddie after 1998.

11 assuming fixed housing supply led to an overstatement of the long-run empirical effect in the main experiment. The third row of Table 12 shows that using an elasticity of 0.25, which is consistent with the main experiment, implies an effect in 2005 of 17 per cent. Comparing the fourth row of Table 12 with the bottom row shows that an elasticity of 0.25 also generates results which are consistent with the experiment in Appendix A.[42]

Table 13 shows the share of U.S. house price growth after 1995 which can be explained by the debt-to-income relaxation. While the appropriate choice of supply elasticity is unclear, selecting an elasticity of 0.25 in line with long-run effect in the main experiment suggests that the policy accounts for up to 72 per cent of price growth between 1995 and 2003, depending on the price index used, and up to 28 per cent of growth from 1995 to 2006. However, it is important to note that this policy cannot account for the rapid price growth that occurred after 2003. The final column of Table 13 shows that the GSE debt-to-income expansion alone actually predicts a decline in prices between 2003 and 2006.

Figure 13(a) plots the price effect of the policy for various elasticities alongside the cumulative change in real house prices since 1995.[43] Although the magnitude is very sensitive to the choice of elasticity, the price response matches the timing of the housing boom well. Overall, the results are consistent with the GSE debt-to-income expansion making a large contribution to house price growth during the early stages of the housing boom.

The results from the model with adaptive expectations point to a similar conclusion to Greenwald (2016), who finds that this type of debt-to-income expansion can explain around a third of the boom. While the basic idea is qualitatively similar, the quantitative similarity of the effects is partly coincidental. The effect in Greenwald (2016) is much larger than the effect in the version of my model without adaptive updating of g. This is because only constrained households determine the price is his model, whereas in my model the effect is attenuated by unconstrained households, and a large response is only achieved by including adaptive expectations.

[42] However, it helps to keep in mind that they are not completely comparable. In the model, the gradual expansion of the effect is due to adaptive expectations. In the data it is not possible to say whether the expansion reflects a continued response to the 1996 change, or the effects of additional software changes. There are additional caveats regarding the empirical response which are discussed in Appendix A.

[43] When constructing Figure 13(a) and Table 13, I convert the model effects from log changes to percentage changes and the use the percentage change in national prices. Tables 12 and 11 show log changes, as this is appropriate when comparing the model results with the empirical estimates.

7.5 Comparison with other models

Given that liquidity constrained households are the focus of much of the existing work relating mortgage leverage constraints to house prices (e.g. Justiniano et al. (2016); Greenwald (2016), Iacoviello (2005)), it is useful to discuss the conditions under which a static model can adequately capture the effect of debt-to-income constraints on house prices. In this subsection I describe how the static model compares with a simple lifecycle model.

In my framework mortgage leverage restrictions reduce housing demand by creating an upper limit on the price households can pay for a property given their assets and income. Whether the constraint binds or not depends on the value of the household's ideal home relative to their resources. Intuitively, the price effect of a given leverage policy will be large when households want to spend a large share of income on housing, and when the desired house size is a large relative to assets available for a downpayment. This channel from leverage restrictions to house prices is similar to the one discussed by Stein (1995) and it is the intratemporal decision that is affected.

In a dynamic setting, mortgage leverage restrictions affect both intratemporal and intertemporal decisions. In Appendix B I present a model where households may be intratemporally constrained or liquidity constrained, and discuss this distinction more formally. Overall, both types of models produce similar effects in the case of a debt-to-income relaxation – which is the focus of this paper.

While the dynamic and static models generate similar responses in the case of a debt-to-income tightening, this is not true of a loan-to-value tightening. The difference arises because some liquidity constrained households respond in the opposite direction, and therefore offset the response of intratemporally constrained households. This is because liquidity constrained households are sensitive to the location of the kink point in Figure 10(a), above which each additional dollar of downpayment only translates into one additional dollar of housing.

Intuitively, it is very costly for liquidity constrained households to have assets tied up in home equity. When starting out with assets above the kink point (i.e. on the debt-to-income constraint), moving to the kink point frees up a lot of assets for current non-housing consumption and reduces housing consumption by a relatively small amount. Figure 11(a) shows that in the case of a loan-to-value relaxation, the first kink point moves to the left. As households relocate to the kink point, their housing demand actually declines.[44] In contrast, the static model always generates a positive effect because at every level of assets, the maximum feasible housing is weakly greater.

In Appendix B I discuss some other ways in which the dynamic response is different. These include intratemporally constrained households saving more in response to tighter leverage policy, and the fact that the available downpayment is influenced by house price movements. These additional effects work in opposite directions from each other, and it is not clear how important they are in practice. The advantage of the static model is its simplicity, including the fact that debt-to-income and asset distributions can be matched directly. The complexity of the dynamic model also needs to be used appropriately if it is to improve accuracy. For example, the model would need to accurately replicate the share of constrained households who are liquidity constrained and locate at the kink point.[45]

8. Policy Implications

Debt-to-income restrictions tend to have both consumer protection and financial stability motivations. In this paper, I show that changes in debt-to-income limits have a large effect on house prices, and are therefore an effective macroprudential tool. This is an important finding in light of the weak relationship between debt-to-income ratios and default, which raises some doubts about the consumer protection motive (DeFusco et al. (2017); Foote et al. (2010)). In Section 5, I showed that while locations with tighter debt-to-income limits experienced much lower default rates during the financial crisis, this effect is attributable to a smaller price cycle rather than differences in mortgage characteristics.

This tension is also present with respect to the GSEs' 1990s debt-to-income expansion. Incorporating more relaxed debt-to-income limits into automated underwriting software reflected an improved understanding of mortgage default, and the ability of computers to apply complex lending rules based on number of different characteristics. But while this change may not have led to a large increase in individual default risk, my results suggest that it did lead to a large increase in house prices.

[44] This is the same type of channel as in Greenwald (2016).

[45] It is also hard to measure the dynamic model's performance in this respect. In particular, measuring the share of liquidity constrained households relative to intratemporally constrained households requires looking beyond liquid assets. Both intratemporally and liquidity constrained households are expected to have limited liquid assets, but respond to a change in loan-to-value limits in very different ways. While both types of households want to hold their assets in the form of home equity, only liquidity constrained households would choose to run down home equity if lenders gave them the opportunity to do so.

Whether consumer protection or macroprudential considerations dominate has implications for the form current policy should take. In the United States, the Ability-to-Repay rule, which primarily has a consumer protection motivation, limits high debt-to-income lending by exposing lenders to legal risk. Because the lender can face sizable legal costs if they do not comply with respect to a single loan, compliance can be onerous, and credit access is significantly reduced for groups with non-standard income streams, such as the self-employed (Johnson, 2018). In contrast, countries with stronger macroprudential motivations, such as the U.K., have chosen to impose limits on the share of high-leverage loans a lender makes.

9. Conclusion

In this paper I show that adjusting mortgage debt-to-income limits has a large effect on house prices, which continues to grow over a period of several years. This finding is important for understanding both the causes of the 2000s housing boom and the effects of macroprudential policy. I also highlight a strong relationship between Fannie Mae and Freddie Mac's eligibility criteria, credit access and house prices in the U.S. context. My results suggest that the housing boom would have been smaller if Fannie Mae and Freddie Mac had maintained tighter underwriting criteria with respect to debt-to-income ratios throughout the 1990s and 2000s. It is important, however, to emphasize that while changes to Fannie and Freddie's criteria seem important for understanding the early stages of the housing boom, they cannot explain the rapid house price growth that occurred after 2003.

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A. 1990s GSE Policies and House Prices

During the 1990s, most U.S. lenders transitioned from manually evaluating mortgage applications to using an automated process. Automated underwriting software not only reduced the time needed to process mortgage applications, it also changed lending standards. The GSEs were an important part of this process and incorporated a substantial relaxation of debt-to-income criteria into their software. These changes were motivated by statistical loan performance analysis, along with the potential for the software to condition on multiple characteristics in complex ways, and were not necessarily associated with an increase in default risk. This explains why, by 1999 when information on the debt-to-income ratios of loans purchased by the GSEs is first available, both Fannie and Freddie were purchasing a large share of loans above the 36 per cent cutoff in their manual guidelines.

In this section, I measure the effect of the GSEs' software on house prices using an identification strategy based on the fact that some lenders starting using the software earlier than others. My estimates are based on county exposure to two large lenders who participated in the 1994 Loan Prospector trial, and then continued to use it after its public release in 1995. Both lenders reported using Loan Prospector to underwrite the majority of eligible loans by 1996. I show how county house prices evolve over time based on exposure to these lenders. After June 1996, house prices diverged rapidly based on the presence of these lenders, implying a price increase of around 9 per cent in the first 6 months, and around 25 per cent by June 1999.

This timing is consistent with the fact that both Fannie Mae and Freddie Mac used a relatively conservative algorithm in the first version of their software. Gates et al. (2002) report that the original version of Loan Prospector was more conservative than the 2000 version; however, they provide few details about the algorithm and they do not make clear when this adjustment occurred.[46] The house price results in this section suggest

[46] The expansion in debt-to-income ratios was referred to as early as 1994, when *Mortgage Banking* reported comments from lenders participating in the Loan Prospector trial prior to its official release in 1995 (Maselli, 1994). In March 1996, Flagstar Bank stated that Loan Prospector 'has allowed it to approve recent applications from people whose debt ratios range from 50 per cent to 72 per cent' (Harney, 1996). In June 1996, Crestar Bank bank noted that 'some loans have even been approved when the borrower's debt-to-income ratio exceeded 50 percent.' However, as of March 1996, eligibility at high debt-to-income ratios was still limited to borrowers with significant offsetting factors, such as a very high credit score, low LTV or substantial cash reserves (Harney, 1996). In contrast, public GSE data show that by 1999 high debt-to-income ratios were available to a much broader range of applicants. When Freddie Mac published the factors used by Loan Prospector in 2000, they stated that the relative importance of debt-to-income ratios was low, with the most important factors being equity, loan type and credit score (Freddie Mac, 2000).

that the main adjustment started with the second generation of Loan Prospector, which was released in July 1996 (Freddie Mac, 1996). However, it is also possible that the update was simply associated with an increase in the extent to which these lenders used the software. In April 1997, Fannie Mae also announced significant changes to Desktop Underwriter, which had at first simply replicated Fannie's manual guidelines (American Banker (1997); Straka (2000)). This was followed by further expansions, including a Loan Prospector upgrade in 1998 reported to accept 25 per cent more loans (Brockman, 1998).

The price effect I measure in this section is most directly interpreted as the effect of updates to the Loan Prospector algorithm. This is subject to the caveat that estimates at longer horizons are likely to be smaller than the true effect, as by 1998 software use was already relatively widespread. This means that many of the other lenders in the 'control' group were also using the same software by this point. Consequently, the strategy is more appropriate for looking at the effects of changes made prior to 1998. The effect I measure includes the large relaxation in debt-to-income criteria already discussed. However, it is hard for me to separate this from other underwriting changes which may have been incorporated into the software over the same period, for example relating to credit score and loan-to-value ratios.

I verify below that loan-to-income ratios increased substantially for loans made by lenders using the software, relative to loans made by other lenders in the same county. But the big challenge is that the algorithm itself is not public, and the Single Family Loan Performance dataset I use to characterize the policy change in Section 4 is not available prior to 1999. For this reason, the empirical result here is best interpreted as saying that the GSEs' automated underwriting software led to a large increase in house prices. Although the empirical analysis focuses on Freddie Mac's software, the results can also likely be extended to Fannie Mae, as both pieces of software were using similar rules by 1999.

When thinking about how the software might have affected national house prices, it is important to take the rate of software adoption into account. Underwriting changes occurring through the software propagated gradually as most lenders were still using the GSEs manual guidelines prior to 1998. At the time, lenders were considering a number of automated underwriting options and it was not clear that the GSEs' offerings were the best ones. Lenders disliked the fact that the GSEs charged substantial fees for using the software, which also created a commitment to sell the loan to either Fannie or Freddie (depending on the software used) and felt that using the manual guidelines gave them more flexibility when selling loans (LaMalfa, 1997). Because of these concerns, the GSE software was not widely used until the late 90s. Figure 13(a) shows statistics on the share of loans purchased by Fannie Mae and Freddie Mac underwritten using their own software, and an adjusted measure which is more likely to correspond to the share of loans underwritten using relaxed debt-to-income criteria.[47]

A.1 Approach

I use information from mortgage industry publications to understand the technological choices of U.S. lenders in the late 1990s and identify lenders who adopted GSE automated underwriting software in or before 1995. Using a series of interviews published in *Mortgage Banking* between 1995 and 2000, I identify two large lenders, Flagstar and InterFirst, who committed to the GSE software when it was first released and by 1996 were using it to evaluate most applications.[48] These early adopters were primarily using Freddie Mac's software and also stated that they were using the software to underwrite portfolio loans. I calculate the 1996 HMDA county market shares of these early adopters and use a similar specification to Section 5 to look at the effect on house prices:[49]

$$\log(\text{Price}_{c,t}) = \gamma_c + \gamma_{s,t} + \beta_{1,t}\text{Early Adopter}_{c,1996} + \alpha_t\text{Controls}_c + \epsilon_{c,t} \tag{18}$$

I include both county and state by time fixed effects and cluster by CBSA. I also estimate short and long-run effects using the following specification:

$$\Delta \log(\text{Price}_c) = \gamma_s + \beta\text{Early Adopter}_{c,1996} + \alpha\text{Controls}_c + \epsilon_c$$

where the short-run effect is measured over 6 months from June 1996 to December 1996 and the long-run effect is measured over three years from June 1996 to June 1999. The lenders I look at have an average 1996 market share of 1.5 per cent, but this varies across counties with a 95th percentile of 5 per cent and a 99th percentile of 12 per cent. Because there is useful variation in the shares of these lenders at state borders, I also

[47] After 1999, the official numbers are likely lower than the overall share of loans underwritten using more relaxed debt-to-income limits. This is because both GSEs made agreements with large lenders to purchase loans underwritten using other pieces of software. The Single Family Loan Performance dataset indicates that both Fannie and Freddie continued to purchase a large share of high DTI loans from the lenders they had made these arrangements with. The adjusted measure corresponds broadly to the adoption rate expected by the GSEs prior to making these agreements.

[48] See LaMalfa (1996); LaMalfa (1997); LaMalfa (1998); LaMalfa (1999).

[49] When identifying these lenders in HMDA I account for mergers, acquisitions and changes in the HMDA lender ID number over time. From 1991 – 1995 some HMDA reporters failed to provide information about the property location on a large scale. I use the 1996 market share rather than an earlier year because Flagstar did not report the property location for a large share of its originations prior to 1996.

show results using border by time fixed effects for counties close to a border.

A.2 House price response

Figure 14 plots estimates of $\beta_{1,t}$ from Equation 18 for 1991 to 2000, showing that locations exposed to early GSE software adopters started to experience a housing boom prior to the national boom. Figure 14 also shows that the effect on prices first emerged in 1996. Figures 15(a) and Figure 15(c) focus on the period close to 1996 and plot estimates from the following specification where the shares of the two lenders enter separately:

$$\log(\text{Price}_{c,t}) = \gamma_c + \gamma_{s,t} + \beta_{1,t}\text{InterFirst}_{c,1996} + \beta_{2,t}\text{Flagstar}_{c,1996} + \alpha_t \text{Controls}_c + \epsilon_{c,t} \quad (19)$$

These figures show that similar responses are obtained for each lender individually, supporting a connection to the July 1996 Loan Prospector upgrade. Figures 15(b) and Figure 15(d) show a similar result using variation close to state borders. As a further robustness check I use a list of the lenders who trialled Loan Prospector before its first release. Figures 15(e) and 15(f) show the results obtained using the market share of these lenders (conditional on the shares of Flagstar and InterFirst). While the within state estimates are inclusive, the border specification is consistent with the results for Flagstar and InterFirst, suggesting that the price effect first emerges in July 1996. The mixed results could be consistent with the fact that some of these lenders did not adopt the software on a large scale. In contrast to the large lenders interviewed in *Mortgage Banking*, I do not know whether these lenders decided to fully commit to the software or not.

A.3 Loan-to-income response

After 1995, Flagstar and InterFirst began to sell loans to Freddie with substantially higher loan-to-value ratios than loans sold by other lenders in the same county. This is consistent with the idea the increase in house prices is related to a relaxation of debt-to-income criteria, though it does not rule out the possibility that other changes to the software also contributed to the price effect. Figure 16(a) shows estimates of $\beta_{1,t}$ from the following specification using HMDA purchase loans sold to Freddie Mac in the year of origination:

$$\frac{\text{Loan}_i}{\text{Income}_i} = \gamma_{c,t} + \gamma_b + \beta_{1,t}\text{Early Adopter}_b + \epsilon_i \tag{20}$$

Figure 16(b) shows estimates from the same specification but with $\mathbb{1}[\frac{\text{Loan}_i}{\text{Income}_i} > 3]$ as the dependent variable. Both figures include only loans originated by the large lenders interviewed in *Mortgage Banking* who either adopted the GSE software early, or had not adopted the software at scale by 1997. Both indicate a substantial increase in high loan-to-income originations by the early adopters. In 1997 the average loan-to-income ratio was 0.1 points, or 5 per cent, higher than other lenders. The share of loans with a loan-to-income ratio greater than three was three percentage points, or 44 per cent, higher.

Figures 16(c) and 16(d) show somewhat smaller results when comparing the early adopters with all HMDA lenders. Finally, Figures 16(e) and 16(f) demonstrate that high loan-to-income lending increases for both the early adopters individually. A qualitatively similar result also holds for other lenders involved in the Loan Prospector trial.

B. Dynamic Problem

The purpose of this appendix is to illustrate the housing demand response to DTI and LTV policies in a dynamic setting. In Section 7 I describe a static problem where the demand response is generated by households who are intratemporally (downpayment) constrained. However, the demand response of intertemporally (liquidity) constrained households could potentially be different, leading to a different house price response. Below I show that both downpayment and liquidity constrained households respond similarly to changes in DTI policy, particularly when they are allowed a high LTV ratio. In contrast, downpayment and liquidity constrained households have opposing responses to changes in LTV policy. This means that the price effect of an LTV policy change is dependent on the share of households who are liquidity constrained. To the extent that liquidity constrained households are present in the data, the static model in Section 7 could overstate the price response to an LTV relaxation.

In addition to the behavior of liquidity constrained households, there are other mechanisms in a dynamic setting that affect the evolution of the price effect. The first is that households' assets are partly held in the form of home equity, and their value is therefore affected by the introduction of the policy. This channel amplifies the house price effect; however, it is partly a product of the simplicity of the model. It is unclear whether this is important for the effect in the data. Firstly, first-time buyers accounted for around 40 per cent of home buyers during the period I look at.[50] Secondly, existing owners would not be forced to satisfy leverage requirements every period as long as they chose not to move or refinance. The asset distribution also changes over time because changes in leverage policy affect households' incentives to save. This channel works in the opposite direction to the first. As households' save more in response to tighter leverage constraints, the share of constrained households falls over time, all else equal.

Finally, a dynamic model introduces the possibility that the amount of resources allocated for consumption in period t is not equal to current income. This means that the debt-to-income constraint is more likely to bind for households whose current income is low relative to their permanent income. The static model captures this in a reduced form way as the preference parameter is calibrated to directly match the debt-to-income distribution in the data. While the preference distribution in the static model may not be interpretable in a structural sense, it can still produce an accurate price response. The main caveat is that the preference distribution in the static model is assumed to be

[50] National Association of Realtors.

independent of current income and assets. This will not be true of the static preference distribution obtained using the output from the dynamic model, but the dynamic model will also not necessarily generate a realistic correlation in this regard.

B.1 Setup

I now describe the setup of the dynamic household problem. As in the static problem, household behavior depends on which, if any, of the financial constraints are binding. Below I consider the optimality conditions for each scenario, and relate them to the optimality conditions of the static model used in the main text.

Let $a_{i,t}$ be net non-housing assets and $h_{i,t}$ be the quantity of housing owned. Housing can be freely adjusted at the start of each period, though the household cannot directly use assets tied up in housing to smooth consumption during the period. It is possible to borrow against home equity for the purpose of smoothing consumption; however, the household cannot run down equity below a minimum level (which is determined by the DTI and LTV constraints). The endowment of net assets (available downpayment) A_i in the static model corresponds to:

$$A_{i,t-1} = (1 + r_t)a_{i,t-1} + (1 - \delta - \tau)p_t h_{i,t-1}$$

Note that the home equity component of the available depends on the equilibrium house price in the current period. Terminal utility is an increasing function of net assets $W(A_{i,T})$. The problem is:

$$\max_{\{h_{i,t},c_{i,t}\}_{t=0}^{T}} \sum_{t=0}^{T} \beta^t u(h_{i,t}, c_{i,t}) + \beta^{T+1} W(A_{i,T}) \tag{21}$$

where:

$$u(h_{i,t}, c_{i,t}) = \alpha_i \log h_{i,t} + (1 - \alpha_i) \log c_{i,t}$$

subject to:

$$c_{i,t} + a_{i,t} + p_t h_{i,t} \leq y_{i,t} + (1 + r_t)a_{i,t-1} + (1 - \delta - \tau)p_t h_{i,t-1} \tag{22}$$

$$p_t h_{i,t} \leq \overline{ph}_{i,t} = \min\left\{\frac{A_{i,t-1}}{(1 - \theta^{ltv})}, \frac{(\theta^{dti} - \nu)y_i}{f(r_{t+1}) + \tau} + \frac{f(r_{t+1})}{f(r_{t+1}) + \tau} A_{i,t-1}\right\} \tag{23}$$

$$a_{i,t} \geq -\min\left\{\theta^{ltv} p_t h_{i,t}, \frac{(\theta^{dti} - \nu) y_{i,t}}{f(r_{t+1}) + \tau}\right\} \tag{24}$$

$$E_t[p_{t+1}] = p_t(1 + g_t) \tag{25}$$

$$g_t = f(g_{t-1}, g_{t-2}, ..., g_0) \tag{26}$$

Equation 22 is the period t budget constraint with multiplier λ_t. Equation 23 is the period t downpayment constraint with multiplier $\lambda_t \gamma_t$. Equation 24 is the borrowing constraint with multiplier $\lambda_t \mu_t$. The downpayment and borrowing constraints both follow directly from the mortgage LTV and DTI constraints. However, the downpayment constraint restricts the intratemporal decision whereas the borrowing constraint restricts the intertemporal decision. The downpayment constraint is a function of assets at the start of period t, whereas the borrowing constraint places a lower bound on assets at the end of period t. I refer to households constrained by 23 as downpayment constrained and households constrained by 24 as liquidity constrained. 1_{DTI} and 1_{LTV} are indicators equal to 1 if the household is constrained by DTI or LTV respectively.

The household bases its housing demand on the adaptively formed price expectation g_t, and it is possible this growth will not actually materialize. Because the price entering Equation 24 is the current price, the household cannot borrow against expected capital gains. The first order conditions are:

$$\frac{(1 - \alpha_i)}{c_{i,t}} = \lambda_{i,t} \tag{27}$$

$$\begin{aligned} \frac{\alpha_i}{h_{i,t}} &= \lambda_{i,t} p_t - E_t \lambda_{i,t+1} p_{t+1}(1 - \delta - \tau) \\ &- E_t[\lambda_{i,t+1}\gamma_{i,t+1} 1_{LTV} \frac{p_t}{1 - \theta^{ltv}} + \lambda_{i,t+1}\gamma_{i,t+1} 1_{DTI} \frac{f(r_{t+1}) p_t}{f(r_{t+1}) + \tau}] - 1_{LTV} \lambda_{i,t} \mu_{i,t} \theta^{ltv} p_t \end{aligned} \tag{28}$$

$$\lambda_{i,t}\mu_{i,t} = \lambda_{i,t} - E_t \lambda_{i,t+1}(1 + r_{t+1}) - E_t[\lambda_{i,t+1}\gamma_{i,t+1} 1_{LTV} \frac{1}{1 - \theta^{ltv}} + \lambda_{i,t+1}\gamma_{i,t+1} 1_{DTI} \frac{f(r_{t+1})}{f(r_{t+1}) + \tau}] \tag{29}$$

Dividing Equation 28 by $\lambda_{i,t}$.

$$\frac{\alpha_i c_{i,t}}{(1-\alpha_i)h_{i,t}} = p_t\bigg(1 - E_t[\gamma_{i,t+1}1_{LTV}\frac{1}{1-\theta^{ltv}} + \gamma_{i,t+1}1_{DTI}\frac{f(r_{t+1})}{f(r_{t+1})+\tau}] - 1_{LTV}\mu_{i,t}\theta^{ltv}\bigg)$$
$$- E_t\frac{\lambda_{i,t+1}}{\lambda_{i,t}}p_{t+1}(1-\delta-\tau) \tag{30}$$

Using $E_t p_{t+1} = p_t(1+g_t)$:

$$\frac{\alpha_i c_{i,t}}{(1-\alpha_i)h_{i,t}} = p_t\bigg(1 - (1+g_t)E_t\frac{\lambda_{i,t+1}}{\lambda_{i,t}} + (1+g_t)(\delta+\tau)E_t\frac{\lambda_{i,t+1}}{\lambda_{i,t}}$$
$$- E_t[\gamma_{i,t+1}1_{LTV}\frac{1}{1-\theta^{ltv}} + \gamma_{i,t+1}1_{DTI}\frac{f(r_{t+1})}{f(r_{t+1})+\tau}]$$
$$- 1_{LTV}\mu_{i,t}\theta^{ltv}\bigg) \tag{31}$$

B.2 Neither downpayment nor liquidity constrained households

If a household is neither downpayment nor liquidity constrained, Equation 29 implies that $E_t\frac{\lambda_{t+1}}{\lambda_t} = \frac{1}{1+r_{t+1}}$. Rewriting 31 for these households gives:

$$\frac{\alpha_i c_{i,t}}{(1-\alpha_i)h_{i,t}} = p_t\left(\frac{r_{t+1} - g_t + (1+g_t)(\delta+\tau)}{1+r_{t+1}}\right) \tag{32}$$

Because $g_t(\delta+\tau) \approx 0$ (and in any case this term is purely a result of the depreciation and tax being paid at the start of the next period):

$$\frac{\alpha_i c_{i,t}}{(1-\alpha_i)h_{i,t}} = p_t\left(\frac{r_{t+1} + \delta + \tau - g_t}{1+r_{t+1}}\right) \tag{33}$$

With resources $\omega_{i,t}$ allocated to the current period:

$$c_{i,t} = (1-\alpha_i)\omega_{i,t} \tag{34}$$

and:

$$p_t h_{i,t} = \frac{\alpha_i \omega_{i,t}(1+r_{t+1})}{r_{t+1} + \delta + \tau - g_t} \tag{35}$$

This is analogous to Equation 7 from the static model, with two exceptions. Current income, $y_{i,t}$, has been replaced by resources allocated for period t consumption, $\omega_{i,t}$. The term $1 + r_{t+1}$ in the numerator does not appear in the static version and is related to timing assumptions in the dynamic model.

B.3 Downpayment constrained households

Next I consider households who are downpayment constrained but not liquidity constrained. For these households Equation 29 implies that

$$E_t \frac{\lambda_{t+1}}{\lambda_t} = \frac{1}{1 + r_{t+1} + E_t \gamma_{i,t+1} (1_{LTV} \frac{1}{1-\theta^{ltv}} + 1_{DTI} \frac{f(r_{t+1})}{f(r_{t+1})+\tau})}$$

The household's decision is distorted because saving relaxes the downpayment constraint tomorrow, providing an extra incentive to accumulate assets. This means that $\omega_{i,t}$ will depend on the leverage policy. However, because the household is already constrained with respect to housing, this $\omega_{i,t}$ adjustment will occur through a reduction in $c_{i,t}$ leaving $h_{i,t}$ unaffected. So it is appropriate to say (as I did in the static section) that if equation 35 implies that the downpayment constraint is violated then housing demand is given by:

$$p_t h_{i,t} = \overline{ph}_{i,t} = \min\{\frac{A_{i,t-1}}{(1-\theta^{ltv})}, \frac{(\theta^{dti} - \nu) y_{i,t}}{f(r_{t+1}) + \tau} + \frac{f(r_{t+1})}{f(r_{t+1}) + \tau} A_{i,t-1}\} \tag{36}$$

This is analogous to Equation 8 from the static model. In this case the marginal effect of relaxing θ^{ltv} is

$$\frac{\partial p_t h_{i,t}}{\partial \theta^{ltv}} = 1_{LTV} \frac{A_{i,t-1}}{(1-\theta^{ltv})^2}$$

The marginal effect of relaxing θ^{dti} is

$$\frac{\partial p_t h_{i,t}}{\partial \theta^{dti}} = 1_{DTI} \frac{y_{i,t}}{f(r) + \tau}$$

B.4 Liquidity constrained households

Next consider households who are liquidity constrained but not downpayment constrained. In this case Equation 29 gives:

$$\lambda_{i,t} \mu_{i,t} = \lambda_{i,t} - E_t \lambda_{i,t+1} (1 + r_{t+1}) \Rightarrow E_t \frac{\lambda_{i,t+1}}{\lambda_{i,t}} = \frac{1 - \mu_{i,t}}{1 + r_{t+1}}$$

The first order condition for housing is then:

$$\frac{\alpha_i c_{i,t}}{(1-\alpha_i)h_{i,t}} = p_t\left(1-(1+g_t)\frac{1-\mu_{i,t}}{1+r_{t+1}} + (1+g_t)(\delta+\tau)\frac{1-\mu_{i,t}}{1+r_{t+1}} - 1_{LTV}\mu_{i,t}\theta^{ltv}\right) \tag{37}$$

Simplifying:

$$\frac{\alpha_i c_{i,t}}{(1-\alpha_i)h_{i,t}} = p_t\left(\frac{r_{t+1} - g_t(1-\mu_{i,t}) + (1+g_t)(1-\mu_{i,t})(\delta+\tau) + \mu_{i,t} - 1_{LTV}\mu_{i,t}\theta^{ltv}}{1+r_{t+1}}\right) \tag{38}$$

Using $g_t(\delta+\tau) \approx 0$:

$$\frac{\alpha_i c_{i,t}}{(1-\alpha_i)h_{i,t}} = p_t\left(\frac{r_{t+1} + (\delta+\tau-g_t)(1-\mu_{i,t}) + \mu_{i,t} - 1_{LTV}\mu_{i,t}\theta^{ltv}}{1+r_{t+1}}\right) \tag{39}$$

The numerator on the RHS of Equation 39 differs from the unconstrained case in two respects. First, the expected capital gain, depreciation and tax are multiplied by $(1-\mu_{i,t})$. Second, the liquidity constrained household particularly dislikes the fact that it has to purchase the housing asset to consume housing services, as this ties up resources it could otherwise have consumed. This is captured by the term $\mu_{i,t}$. If the household is not DTI constrained some of this cost is offset by the term $-\mu_{i,t}\theta^{ltv}$ because additional housing can be partly financed with mortgage debt. If $\theta^{ltv} = 1$ and the DTI constraint does not bind, the liquidity constrained household does not experience any additional cost from having to buy the housing asset, as it can fund the purchase entirely with debt. Liquidity constrained households are responsive to the location of the kink in $\overline{ph}_{i,t}(A_{i,t-1})$ because their user cost of housing jumps by $\mu_{i,t}\theta^{ltv}\frac{p_t}{1+r_{t+1}}$ at that point. The value of $\overline{ph}_{i,t}$ at the kink point is:

$$\theta^{ltv}p_t h_{i,t} = \frac{(\theta^{dti}-\nu)y_{i,t}}{f(r_{t+1})+\tau} \Rightarrow p_t h_{i,t} = \frac{(\theta^{dti}-\nu)y_{i,t}}{\theta^{ltv}(f(r_{t+1})+\tau)}$$

This means that when the LTV constraint is relaxed, the kink point moves to the left, whereas when the DTI constraint is relaxed it moves to the right. It follows that the effect of an LTV relaxation on $ph_{i,t}$ is actually negative, whereas the effect of a DTI relaxation is positive. The marginal effect of relaxing θ^{ltv} is:

$$\frac{\partial p_t h_{i,t}}{\partial \theta^{ltv}} = -\frac{(\theta^{dti}-\nu)y_{i,t}}{\theta^{ltv2}(f(r_{t+1})+\tau)}$$

The marginal effect of relaxing θ^{dti} is:

$$\frac{\partial p_t h_{i,t}}{\partial \theta^{dti}} = \frac{y_{i,t}}{\theta^{ltv}(f(r_{t+1}) + \tau)}$$

This is similar to the downpayment constrained case, though it is larger to the extent that $\theta^{ltv} < 1$. What about liquidity constrained households not at the kink point (and not downpayment constrained)? For these households:

$$p_t h_{i,t} = \frac{\alpha_i \omega_{i,t}(1 + r_{t+1})}{r_{t+1} + (\delta + \tau - g_t)(1 - \mu_{i,t}) + \mu_{i,t} - 1_{LTV}\mu_{i,t}\theta^{ltv}} \tag{40}$$

An LTV relaxation raises their housing demand by allowing them to borrow more, and also by reducing the liquidity cost of housing $\mu_{i,t}(1 - \theta^{ltv})$. A DTI relaxation has no effect because these households are not at the kink point. In this respect they respond similarly to households who are downpayment constrained only, but have a low level of assets and are therefore constrained by LTV, not DTI.

C. Additional Policy Documentation

C.1 Comparing credit score and loan-to-value

Because my identification strategy is based on comparing areas where lenders are more or less tied to Freddie Mac, it is important to have a broader understanding of differences between Fannie Mae and Freddie Mac. Figure 17 compares characteristics of Fannie and Freddie's purchases over time. The debt-to-income and credit score figures are constructed using the Single Family Loan Performance datasets. Loan-to-value figures are constructed using the GSE Public Use Database. Using the GSE Public Use Database is preferable because it presents a more comprehensive picture of Fannie and Freddie's purchases; however, it does not contain information on debt-to-income and credit score.

Figure 17 shows that credit score distributions for Fannie and Freddie are very similar in each time period. The largest discrepancy is for 1999, where Fannie credit scores are slightly more dispersed. However, this seems to be specific to the first three quarters of 1999 when coverage for Fannie is much lower, suggesting it should be interpreted cautiously and does not necessarily indicate a general policy difference. If anything, Freddie credit scores are actually slightly lower after 2002.

The loan-to-value bins match those used in the dataset. The first bin contains loans with a loan-to-value ratio less than 60 per cent. The second contains loans with loan-to-value ratios between 60 and 80 per cent, and includes 80 per cent. The third contains loans with loan-to-value ratios between 80 and 90 percent, the fourth loans with loan-to-value ratios between 90 and 95 per cent. The fifth contains loans with loan-to-value ratios above 95 per cent. Fannie and Freddie's purchases had similar loan-to-value characteristics in each time period. The main difference is that Freddie had fewer purchases of loans with a loan-to-value ratio above 95 per cent after 2002. This divergence cannot explain the way the price difference between Freddie and Fannie areas expands over time because much price response occurs between 1999 and 2003, while Freddie and Fannie's loan-to-value characteristics were very similar up until 2003.

Overall, looking at these other variables suggests that Fannie and Freddie's rules diverged mainly with respect to debt-to-income. Furthermore, if anything their debt-to-income policies became more similar over time. This suggests that the long-run price effect documented in Section 5 is unlikely to reflect later policy changes.

C.2 *Which Freddie applicants were allowed DTI > 50%?*

Figure 17 suggests that Freddie applied a debt-to-income limit of 50% to only some borrowers. Here, I use the data to identify this affected group, showing that whether a borrower is allowed a high debt-to-income ratio depends on their credit score and loan-to-value ratio. While it is possible to show this directly by plotting average credit score and LTV against DTI, I want to characterize the rule more precisely so I can appropriately incorporate it into the model in Section 7. To do this, I assign loans to credit score by loan-to-value bins, and calculate the following measure of the mass above 50 per cent:

$$\text{Ratio} = \frac{\#DTI \in [51, 60]}{\#DTI \in [40, 49]}$$

I then calculate:

$$\text{Relative Ratio} = \frac{\text{Ratio}_{\text{Freddie}}}{\text{Ratio}_{\text{Fannie}}}$$

That is, I use the Fannie Mae distribution as a counterfactual. I then classify each credit score by loan-to-value bin as affected (DTI > 50% not allowed) or unaffected (DTI > 50% allowed) based on the value of the ratio. Figure 18 shows for four different time periods how the relative ratio varies with credit score and loan-to-value, and Figure 19 shows which bins are classified as affected. Figure 18(a) shows that under the initial policy Fannie and Freddie applied similar rules, as the ratio is close to one in most cases and is not closely related to credit score or the loan-to-value ratio. In Figure 19(a) all credit score and loan-to-value combinations are classified as unaffected.

Figures 18(b) and 19(b) show the short-run policy change. I classify a group as affected if the relative ratio calculated above is less than 0.4 and a group as unaffected if the relative ratio is greater than 0.4. Looking at Figure 19(a) it is possible to see that the classification would not change very much if the cutoff were adjusted somewhat. This gives a relatively clear idea of how high debt-to-income eligibility is determined. High debt-to-income and loan-to-value combinations seem to be removed regardless of credit score. However, applicants with a high credit score may be eligible at a high debt-to-ratio if their loan-to-value ratio is sufficiently low. For example, if an applicant has a credit score of 700 they would be eligible for a high debt-to-income ratio at Freddie as long as their loan-value ratio is less than 70.

Figure 20 compares the debt-to-income distributions for Freddie Mac loans classified as affected or unaffected with comparable loans purchased by Fannie Mae during 2000. This supports the idea that the discontinuity at 50 per cent in Figure 17(b) reflects pooling of borrowers who have a 50 per cent debt-to-income limit under the new policy with those who are unaffected.

The reverse engineering approach is subject to two main caveats. Firstly, the dataset does not contain all the variables used as inputs into the algorithm. Secondly, the dataset also likely includes loans that were not processed using the GSEs' own software, or could possibly reflect some human discretion. That is, even if Freddie Mac's software cut out certain groups of loans, these loans still might show up in the purchase data if they were underwritten using different software. These two factors likely explain why the lower bound on the relative ratio in Figure 19(b) is 0.2 rather than zero, and the upper bound is around 0.8. In other words, around 20 per cent of borrowers whose loans were sold to Freddie Mac can have a high debt-to-income ratio regardless of credit score and loan-to-value, and around 20 per cent cannot.

Next I look at what happens over the longer-term. Figures 18(c) and 19(c) show that the policy applied between 2002 and 2005 is different from the policy applied between 2000 and 2001. Only loans with very high loan-to-value ratios or very low credit scores are classified as affected. However, the relative ratio is still consistently less than 1. This indicates a sizable share of borrowers are not allowed a debt-to-income ratio above 50 per cent, but this is no longer closely related to their credit score or loan-to-value ratio. Figures 18(d) and 19(d) show that by 2006 the 1999 policy has been largely reversed. However, a small proportion of borrowers are still affected by the 50 per cent limit, consistent with Figure 17(d). It is also still the case that borrowers with low credit scores are more likely to be affected by the 50 per cent limit.

C.3 Comparing subprime and Alt-A securities purchases

Both Fannie Mae and Freddie Mac purchased a large amount of subprime and Alt-A securities during the 2000s. This was separate from their purchases of loans meeting their standard eligibility criteria. One concern for identification is that this somehow affected the supply of credit in a way that is correlated with the 1998 county exposure to Freddie Mac sellers. Figure 21 shows the value of private label securities purchases as a share of total purchases in the GSE Public Use Database. Freddie Mac was a more active purchaser of both subprime and Alt-A private label securities, and this is also true in an absolute sense as Freddie Mac had a smaller market share during the 2000s. This means that private label securities purchases cannot explain the long-run effect. In any case it is not obvious that there should be any direct connection between lender relationships and the location where these subprime and Alt-A loans were originated.

D. Housing Supply Response

In this section I show that housing supply did not respond strongly to the change in Freddie Mac's debt-to-income rules. This supports my assumption of very inelastic housing supply in Section 7. I examine the effect of the policy change on building permits issued for new housing units using the Building Permits Survey. The permits represent approval to begin a residential construction project. While some locations do not require building permits, the dataset provides good coverage as, according to the U.S. Census Bureau, over 98 per cent of privately-owned residential buildings are constructed in places which issue building permits. I focus on permits rather than actual construction as information on permits is available at the county level. I use an analogous specification to the one in Section 5:

$$\log(\text{Units}_{c,t}) = \gamma_c + \gamma_{s,t} + \beta_t \text{Exposure}_{c,1998} + \alpha_t \text{Controls}_{c,1998} + \epsilon_{c,t} \tag{41}$$

where $\text{Units}_{c,t}$ is the number of housing units for which building permits were issued in county c in year t. Figure 22 plots the estimates of β_t. The response varies depending on the type of area. Figure 22(a) shows the estimates for counties located in micropolitan areas. These are counties in an urban area with an urban core population of at least 10000 but less than 50000. In these areas building permit issuance responds to the policy change. The policy leads to a 30 per cent relative reduction in the flow of residential building permits in 1999, and this continues up until 2002. At this time, the number of building permits each year was about 1 per cent of total housing units outstanding on average.

Figure 22(b) shows the estimates for counties located in metropolitan areas. These are counties in an urban area with an urban core population of at least 50000. Building permit issuance does not appear to respond to the policy in these areas. However, areas with a higher Freddie Mac share do have a significantly higher flow of building permits after 2005 (that is, during the period where the policy effect starts to reverse and prices grow more strongly in areas with a higher Freddie Mac share). Figure 22(c) shows estimates for all counties included in the main house price regression. There is some reduction in building permits in response to the policy but the response is small. There is an average reduction in building permits of around 6 per cent per year over the four year period from 1999 – 2002. This corresponds, broadly, to around 0.06 per cent of the housing stock each year for four years. The effect is later reversed.

Overall, housing supply is fairly unresponsive during the period immediately following the policy change, which justifies the assumption made in Section 7. It is more responsive later on when the policy effect starts to reverse. This may be related to the fact that the policy change occurs during a period when building permit issuance was growing very strongly for other reasons. In contrast, during the later period building permit issuance was declining.

Table 1 – Relationship between Freddie Mac exposure and county characteristics

	Below median	Above median	Difference	t-stat
Median income ('000s)	40.87	39.38	1.49	2.37
	(8.61)	(7.40)		
Housing supply elasticity	2.18	2.26	-0.08	-0.99
	(1.03)	(1.02)		
Persons per sq. mi.	83.43	40.70	42.73	4.68
	(156.17)	(52.61)		
Average credit score	682.74	675.71	7.04	3.57
	(25.56)	(24.87)		
Traditional bank share	50.94	55.47	-4.53	-5.42
	(11.02)	(10.36)		
Underserved area	46.01	43.08	2.94	1.50
	(25.03)	(25.45)		
Number of Observations	333	333	666	666
	Includes counties with missing elasticity or credit score			
	Below median	Above median	Difference	t-stat
Median income ('000s)	38.60	36.88	1.72	3.74
	(8.24)	(7.01)		
Persons per sq. mi.	50.34	26.42	23.92	5.48
	(93.22)	(42.72)		
Traditional bank share	52.98	57.05	-4.07	-5.34
	(12.70)	(12.64)		
Underserved area	42.61	40.11	2.50	1.19
	(34.01)	(36.33)		
Number of Observations	564	564	1,128	1,128

Note: Exposure measure is the 1998 Freddie Mac county market share excluding lenders originating more than 20000 purchase loans. Median income is real household median income from the U.S. Bureau of the Census. Population density is county population density from the 2000 census. The housing supply elasticity measure is from Saiz (2010). The average county credit score is calculated using the CoreLogic LLMA database. Traditional bank share is the HMDA market share by number of loans of banks with a loan-to-asset ratio about 33 per cent and a core deposit-to-asset ratio above 50 per cent. Underserved is the share of the county population living in a HUD targeted area (1999 classification). The top and bottom 1 per cent of the distribution of each variable is removed before calculating the mean.

Table 2 – Loan-level evidence

	6 months		**Pre-period (3 months)**	
	Price paid	**DTI > 50**	**Price paid**	**DTI > 50**
Freddie × Post	-6.15**	-5.81***	4.54	1.46
	(2.70)	(0.53)	(3.29)	(1.05)
Number of ZIP3	880	880	865	865
Number of States	50	50	50	50
Number of Observations	134,757	134,757	114,867	114,867
Controls	X	X	X	X
State FE	X	X	X	X

Notes: Constructed using GSE Single Family Loan Performance Datasets. Standard errors are clustered by state.

Table 3 – Effect on county share of new loans with debt-to-income ratio above 50%

	Short-run		**Long-run**		**Pre-period**	
			% DTI > 50			
Exposure	-4.25**	-4.30**	-1.85	-1.96	1.03	0.93
	(1.99)	(2.05)	(1.62)	(1.66)	(2.31)	(2.36)
Number of Counties	1,103	1,103	1,103	1,103	1,098	1,098
Number of States	50	50	50	50	50	50
Number of Observations	1,103	1,103	1,103	1,103	1,098	1,098
Controls		X		X		X
State FE	X	X	X	X	X	X

Note: Constructed using the CoreLogic LLMA Database. I exclude counties which not in a CBSA or have missing house price data. I also exclude counties with an average annual loan count less than 50 (calculated between 1997 and 2013). Standard errors are clustered by CBSA.

Table 4 – Effect on county house price index

	Short-run		**Long-run**		**Pre-period**	
			Main Results			
Exposure	-1.85***	-1.59***	-9.53**	-7.37**	-0.52	-0.22
	(0.59)	(0.57)	(3.78)	(3.32)	(0.59)	(0.59)
Number of Counties	1,132	1,132	1,132	1,132	1,132	1,132
Number of States	50	50	50	50	50	50
Number of Observations	1,132	1,132	1,132	1,132	1,132	1,132
			Zillow Price Index			
Exposure	-1.86**	-1.46*	-7.60*	-7.64*	-0.51	-0.20
	(0.85)	(0.84)	(4.01)	(4.14)	(0.72)	(0.70)
Number of Counties	885	885	885	885	885	885
Number of States	46	46	46	46	46	46
Number of Observations	885	885	885	885	885	885
Controls		X		X		X
State FE	X	X	X	X	X	X

Notes: Constructed using Zillow county house price data and a proprietary index. Standard errors are clustered by CBSA. Exposure is defined as the 1998 market share of Freddie Mac by number of loans. The market share is computed using only loans sold to either Freddie or Fannie and excluding loans made by lenders originating more than 20000 purchase loans in 1998. Shows estimates of β from:

$$\log(\text{Price}_{c,t}) = \gamma_c + \gamma_{s,t} + \beta_t \text{Exposure}_{c,1998} + \alpha_t \text{Controls}_{c,1998} + \epsilon_{c,t}$$

The short-run effect is measured from June 1999 to December 1999. The long-run effect is measured from June 1999 to June 2005. The pre-period effect is measured from January 1999 to June 1999.

Table 5 – Core-based statistical area fixed effects

	Short-run		Long-run		Pre-period	
Exposure	-2.38**	-2.17**	-6.91**	-5.07	-0.60	-0.11
	(0.95)	(0.97)	(3.46)	(3.58)	(1.08)	(1.13)
Number of Counties	638	638	638	638	638	638
Number of States	47	47	47	47	47	47
Number of Observations	638	638	638	638	638	638
Controls		X		X		X
CBSA FE	X	X	X	X	X	X

Notes: Constructed using a proprietary house price index. Standard errors are clustered by CBSA. Exposure is defined as the 1998 market share of Freddie Mac by number of loans. The market share is computed using only loans sold to either Freddie or Fannie and excluding loans made by lenders originating more than 20000 purchase loans in 1998. Shows estimates of β from:

$$\log(\text{Price}_{c,t}) = \gamma_c + \gamma_{cbsa,t} + \beta_t \text{Exposure}_{c,1998} + \alpha_t \text{Controls}_{c,1998} + \epsilon_{c,t}$$

The short-run effect is measured from June 1999 to December 1999. The long-run effect is measured from June 1999 to June 2005. The pre-period effect is measured from January 1999 to June 1999.

Table 6 – How does the house price response interact with local housing supply elasticity?

	Short-run		Long-run		Pre-period	
Exposure	-3.95**	-3.65**	-14.86	-39.70***	0.75	-1.05
	(1.64)	(1.78)	(10.90)	(15.00)	(1.74)	(1.77)
Exposure × Housing Supply Elasticity	0.88**	0.86*	1.39	7.55*	0.05	0.30
	(0.43)	(0.50)	(2.82)	(4.20)	(0.54)	(0.54)
Housing Supply Elasticity	-0.46**	-0.50*	-3.31**	-9.63***	-0.40	-0.60**
	(0.22)	(0.26)	(1.58)	(2.41)	(0.29)	(0.28)
Number of Counties	667	669	667	669	667	669
Number of States	47	49	47	49	47	49
Number of Observations	667	669	667	669	667	669
State FE	X		X		X	
Division FE		X		X		X

Notes: Constructed using a proprietary house price index. Standard errors are clustered by CBSA. Exposure is defined as the 1998 market share of Freddie Mac by number of loans. The market share is computed using only loans sold to either Freddie or Fannie and excluding loans made by lenders originating more than 20000 purchase loans in 1998. Housing supply elasticity is from Saiz (2010).

Table 7 – House price results using alternative exposure measure

	Short-run		**Long-run**		**Pre-period**	
Exposure	-2.12**	-1.75**	-10.98*	-8.01	-1.03	-0.64
	(0.91)	(0.88)	(5.94)	(5.33)	(0.92)	(0.90)
Number of Counties	1,132	1,132	1,132	1,132	1,132	1,132
Number of States	50	50	50	50	50	50
Number of Observations	1,132	1,132	1,132	1,132	1,132	1,132
State FE		X		X		X
Division FE	X		X		X	

Notes: Constructed using a proprietary house price index. Standard errors are clustered by CBSA. Exposure is defined as the 1998 market share of Freddie Mac by number of loans. The market share is computed using only loans sold to either Freddie or Fannie, but for all HMDA reporters. Shows estimates of β from:

$$\log(\text{Price}_{c,t}) = \gamma_c + \gamma_{s,t} + \beta_t \text{Exposure}_{c,1998} + \alpha_t \text{Controls}_{c,1998} + \epsilon_{c,t}$$

The short-run effect is measured from June 1999 to December 1999. The long-run effect is measured from June 1999 to June 2005. The pre-period effect is measured from January 1999 to June 1999.

Table 8 – Policy parameters

Policy Parameter	Initial Policy	2000–2001	2002–2005	2006–2008
θ_h^{ltv}	0.95	0.95	0.95	0.95
θ_h^{dti}	0.65	0.65	0.65	0.65
θ_l^{ltv}	0.95	Figure 19(b)	Figure 19(c)	Figure 19(d)
θ_l^{dti}	0.65	0.5	0.5	0.5

Note: θ_l^{ltv} varies with credit score as shown on Figure 19.

Table 9 – How does a small change in policy affect the kink points?

	θ_h^{ltv}	θ_l^{dti}	θ_l^{ltv}	θ_h^{dti}
$\overline{A}_1$	$\downarrow$	$\uparrow$	None	None
$\overline{A}_2$	None	$\uparrow$	$\downarrow$	None
$\overline{A}_3$	None	None	$\downarrow$	$\uparrow$

Table 10 – Calibration

		Source
Log income mean	1.44	AHS
Log income variance	0.37	AHS
Log asset mean	3.05	AHS
Log asset variance	1.44	AHS
Log income and asset covariance	0.18	AHS
Preference dist.	$\beta(12.6, 36.3)$	Pre-policy share DTI > 50
	$E(\alpha) = 0.26$	Pre-policy median DTI
Other commitments (ν)	0.051	Homeowner avg. in SCF
Property taxes (τ)	0.014	Homeowner avg. SCF
Depreciation (δ)	0.02	
Expected price growth (g)	adaptive	See Section 7 ($\lambda = 0.11$)
Interest rate (r)	0.068	30 year fixed rate
θ^{ltv} (pre-policy)	0.95	
θ^{dti} (pre-policy)	0.65	
θ_h^{ltv}	0.95	
θ_l^{dti}	0.5	
θ_l^{ltv}	By credit score	
θ_h^{dti}	0.65	

Note: Income and asset statistics are from the 1991-1995 AHS sample of owners who bought in the current or previous year. AHS assets include home equity at the time the property was purchased. The mortgage rate and DTI statistics are from January 1999. ν and τ are calculated using the 1998 SCF.

Table 11 – Model % price effect of Freddie's tighter DTI requirements after 1999

	Short-run	**By 2001**	**By 2003**	**By 2005**
	Housing supply elasticity = 0			
No feedback	-0.7	-1.2	-1.7	-3.4
Feedback	-0.7	-3.5	-9.1	-18.3
Feedback; using past empirical effect	-0.7	-5.0	-6.6	-9.2
	Housing supply elasticity = 0.25			
No feedback	-0.6	-1.0	-1.4	-2.7
Feedback	-0.6	-2.3	-4.7	-7.8
Feedback; using past empirical effect	-0.6	-4.0	-5.3	-7.4
	Housing supply elasticity = 1			
No feedback	-0.4	-0.6	-0.9	-1.7
Feedback	-0.4	-1.0	-1.6	-2.7
Feedback; using past empirical effect	-0.4	-2.5	-3.3	-4.6
Data	-1.6	-3.6	-4.5	-7.4

Note: This table shows model calculations of the percentage house price response to Freddie Mac's tighter debt-to-income requirements after 1999.

Table 12 – Model % price effect of relaxing 36% DTI limit in 1996

	Short-run	**By 1998**	**By 2000**	**By 2005**
	Housing supply elasticity = 0			
Gradual implementation	1.1	4.0	15.0	38.4
Immediate implementation	7.6	32.4	55.5	45.5
	Housing supply elasticity = 0.25			
Gradual implementation	0.9	2.8	9.7	17.2
Immediate implementation	6.1	20.0	32.4	26.7
	Housing supply elasticity = 1			
Gradual implementation	0.6	1.5	4.6	5.4
Immediate implementation	3.8	8.1	8.9	5.7
Data	8.7	19.5	29.0	

Note: This table shows model calculations of the percentage house price response to a relaxation of the debt-to-income limit from 36 per cent to 65 per cent in 1996. I compute effects both under the (counterfactual) case of immediate implementation in 1996, and using a gradual implementation profile calculated using statistics on GSE software adoption (shown in Figure 13(a) and Table 15).

Table 13 – Percentage of U.S. house price growth explained by debt-to-income relaxation

	1995–2003	**1995–2006**	**2003–2006**
	FHFA house price index		
Elasticity = 0	162.3	83.8	-1.7
Elasticity = 0.25	71.6	28.1	-20.6
Elasticity = 1	22.4	9.2	-6.1
	Case Shiller house price index		
Elasticity = 0	116.0	59.1	-1.3
Elasticity = 0.25	51.2	19.8	-15.6
Elasticity = 1	16.0	6.5	-4.6

Note: This table shows the percentage of FHFA and Case Shiller U.S. house price growth accounted for by the GSEs relaxing debt-to-income rules. The first three rows show the model effect for different values of the housing supply elasticity.

Table 14 – Effect of GSE software on house prices

	Short-run		**3 years**		**Pre-period**	
Exposure	8.69**	8.71**	25.16**	25.54**	-2.39	-2.34
	(3.96)	(3.97)	(11.39)	(11.53)	(3.36)	(3.35)
Number of Counties	1,132	1,132	1,132	1,132	1,132	1,132
Number of States	50	50	50	50	50	50
Number of Observations	1,132	1,132	1,132	1,132	1,132	1,132
Controls		X		X		X
State FE	X	X	X	X	X	X

Note: 'Exposure' is the 1996 combined market share of InterFirst and Flagstar Bank by number of HMDA loans. Standard errors are clustered by CBSA. The short-run effect is measured from June 1996 to December 1996. The three-year effect is measured from June 1996 to June 1999. The pre-period covers December 1995 to June 1996.

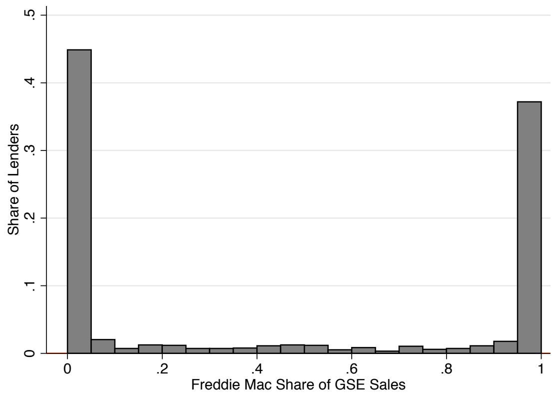
Table 15 – Software adoption – % of purchases processed using Desktop Underwriter or Loan Prospector

	Fannie Mae		**Freddie Mac**		
	Report	Other	Report	Other	Notes on other sources
1995					
1996				25	
1997	9		22	54	In 1997, 54% processed through LP.*
1998	22	26	36		Dec 1998: 26% processed through DU.
					March/April 1998: Freddie expected 80-85% over 1998.*
					And ‘Fannie Mae’s numbers show similar growth patterns’
1999	39		50	>75	Over 75% of Freddie purchases processed through LP.**
2000	56		56		
2001	59		62		
2002	60		60		Freddie Mac 2002 report: around 60% from 2000–2002
2003			64		
2004			61		

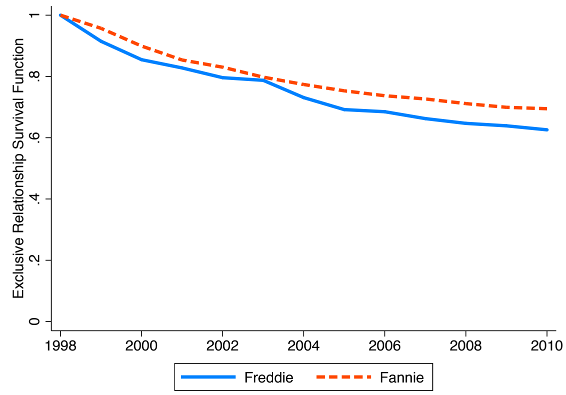
* Wilson, Caroline (1998). Automated Underwriting Goes Mainstream. America’s Community Banker, 7(4):36; Gallaher, Douglas (1998). Getting a Payoff from Technology. Mortgage Banking, 58(6): 66–76.
** Murin, Joseph (1999). A Business Transformed by Technology. Mortgage Banking, 60(1): 152.
The discrepancies between Fannie and Freddie’s annual reports and other numbers reported by Fannie and Freddie representatives to mortgage publications likely reflect fluctuations in LP and DU usage within the calendar year. These alternative figures indicate that both Fannie and Freddie expected usage of 80-85% by 1999. These rates were never realized on average over a calendar year, though during 1999 Freddie stated that over 75% of its purchases were processed through LP. Later annual reports suggest that DU and LP usage stabilized at a lower rate of around 60 per cent because both Fannie and Freddie made agreements with individual lenders allowing them to use alternative software.

1 – Exclusive relationships

(a) Freddie Mac Share of GSE Sales: 1998



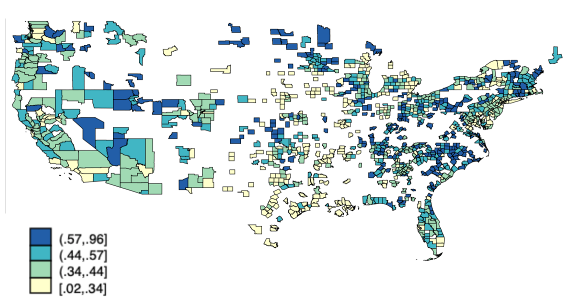
(b) Exclusive Relationship Survival



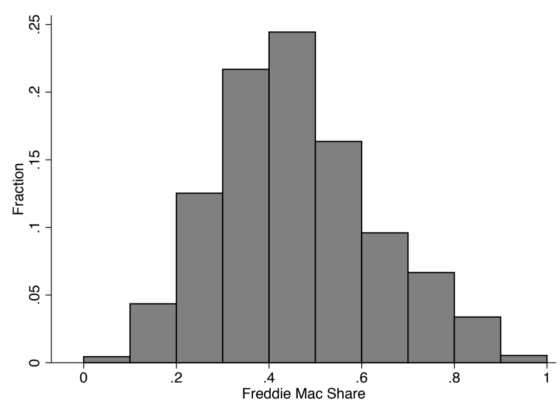
Note: Computed using HMDA loans sold to Fannie Mae or Freddie Mac in the calendar year of origination. Excludes lenders who did not sell to either GSE and lenders with < 10 originations. Each observation corresponds to a single lender ID.

2 – Freddie Mac market share

(a)



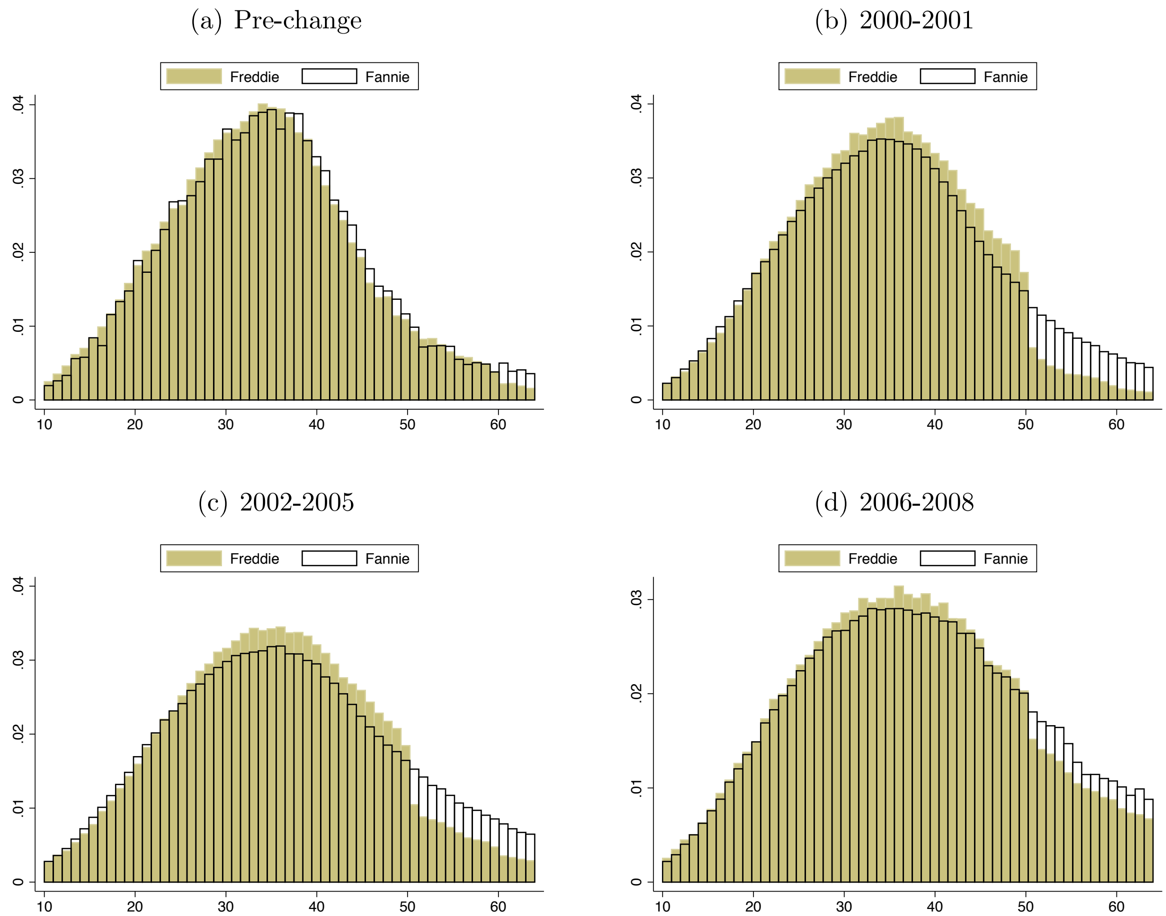
(b)



Sources: HMDA 1998. This figure shows the exposure measure only for counties where house price data are available. The exposure measure is constructed using the following formula:

$$\text{Freddie Mac Share}_c = \frac{\#\text{ Loans in county } c \text{ sold to Freddie in 1998}}{\#\text{ Loans in county } c \text{ sold to Freddie or Fannie in 1998}}$$

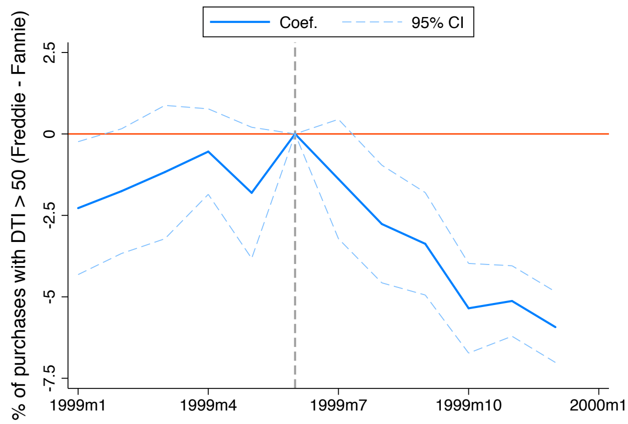
3 – DTI distributions before and after change



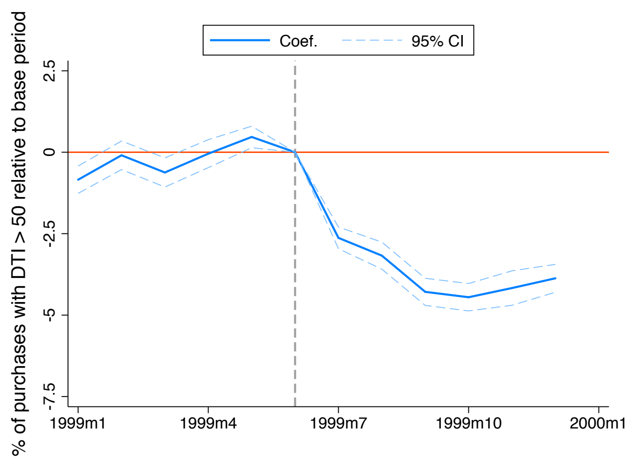
Source: GSE Single Family Loan Performance Datasets. Includes purchase loans to owner-occupiers bought by Fannie Mae or Freddie Mac. Loans with debt-to-income ratios above 64 per cent are excluded. Because Fannie and Freddie report loans above with debt-to-income ratios above 64 per cent differently, I drop them when comparing the two distributions. Figure 3(a) includes loans originated between January and June 1999 for Freddie, and loans originated between January and December 1999 for Fannie. This is because the dataset contains a reduced number of loans purchased by Fannie with origination dates prior to October 1999. The datasets also report the seller name for sellers accounting for a large share of total purchases. These charts are constructed based only on loans sold by smaller sellers. During some time periods loans sold by particular large institutions seem to have special characteristics, suggesting they may have been processed using somewhat different rules. This is consistent with the fact that the GSEs reached agreements with some of these sellers allowing them to use their own software. Including all loans does not lead to a qualitatively different conclusion, however.

4 – Timing of policy change

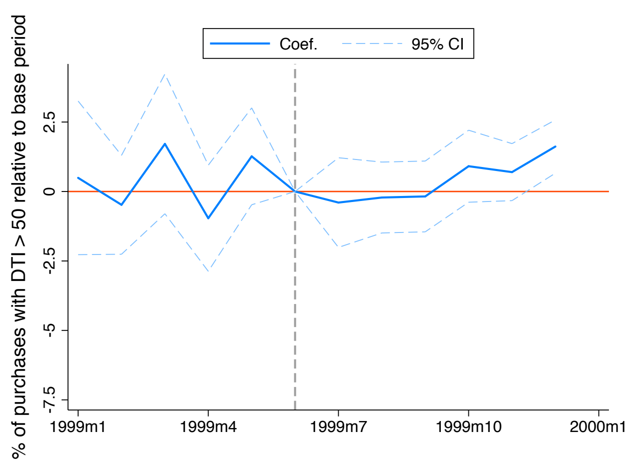
(a) Freddie - Fannie



(b) Freddie (adj. for rate movements)



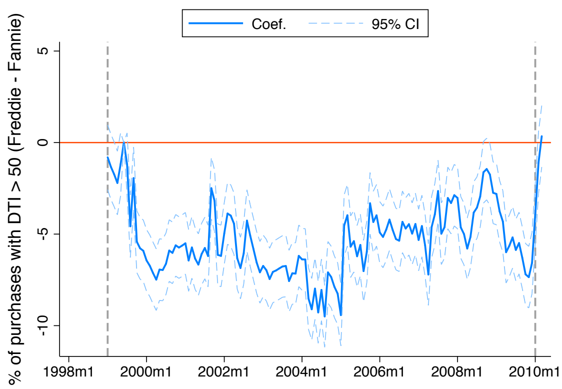
(c) Fannie (adj. for rate movements)



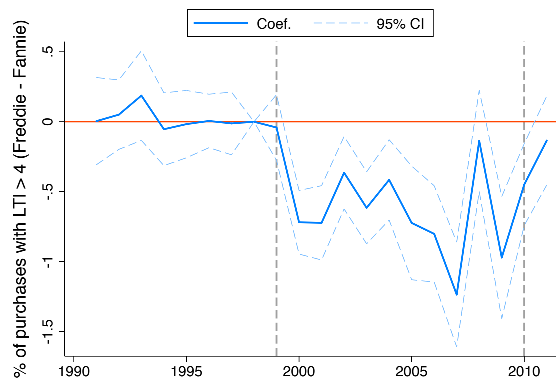
Sources: GSE Single Family Loan Performance Database. Purchase mortgages to owner-occupiers. Figure 4(a) shows estimates of β_t from $DTI_i > 50 = \gamma_{s,t} + \beta_t \text{Freddie Mac}_i + \epsilon_i$. Figures 4(b) and 4(c) show estimates of β_t from $\widetilde{DTI}_i > 50 = \gamma_s + \beta_t + \epsilon_i$ separately for Freddie and Fannie, where $\widetilde{DTI} = \frac{f(r_{\text{Aug 1999}})}{f(r)} DTI$ as discussed in Section 4. $\widetilde{DTI}$ is an adjusted DTI designed to abstract from the effect movements in interest rates have on the high DTI share.

5 – Freddie Mac high leverage mortgage purchases relative to Fannie Mae

(a) GSE Single Family Loan Performance Data

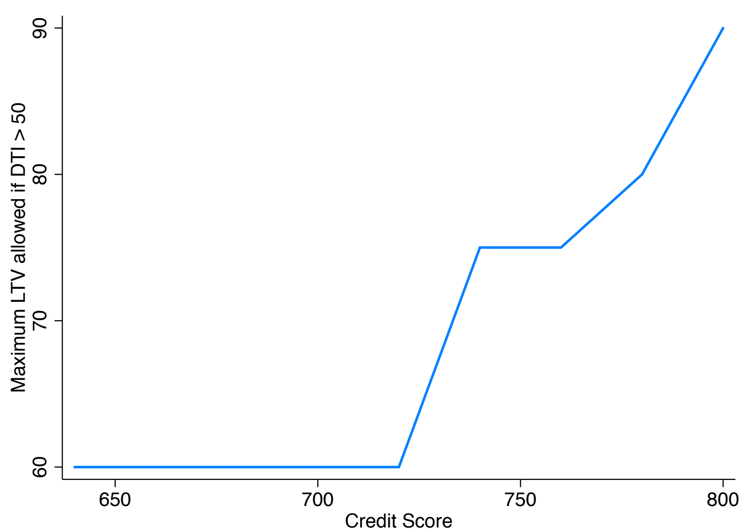


(b) HMDA



Loans in the top and bottom 0.5 per cent of the loan size or income distributions are removed before calculating the loan-to-income ratio. The sample includes only loans to owner-occupiers sold to Fannie Mae or Freddie Mac. Figure (a) shows estimates from: Share$(DTI > 50) = \gamma_{s,t} + \beta_t \text{Freddie Mac}_i + \epsilon_i$. Figure (b) shows estimates from: Share$(\frac{\text{Loan}}{\text{Income}} > 4) = \gamma_{c,t} + \beta_t \text{Freddie Mac}_i + \epsilon_i$.

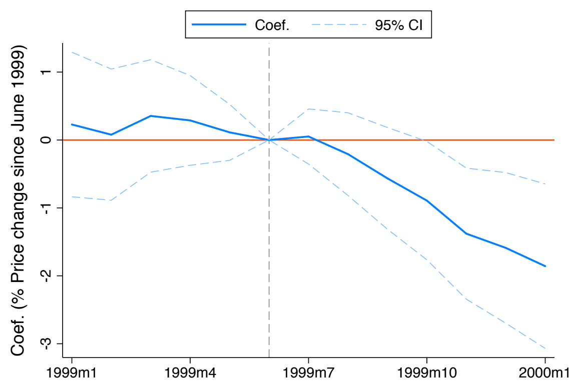
6 – Maximum LTV that can be combined with DTI > 50 by credit score group



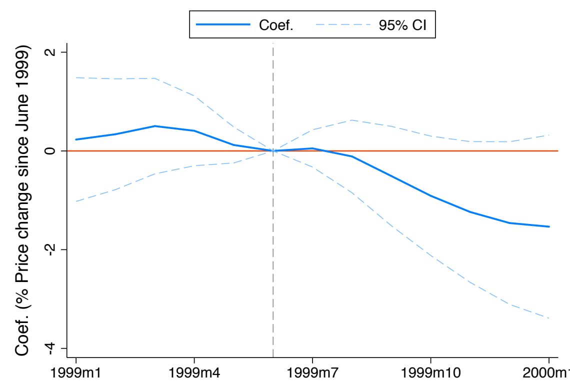
Sources: Single Family Loan Performance Datasets. This chart shows rules for high debt-to-income eligibility under Freddie's post June 1999 policy backed out using the method described in Appendix C.

7 – Effect on county house prices

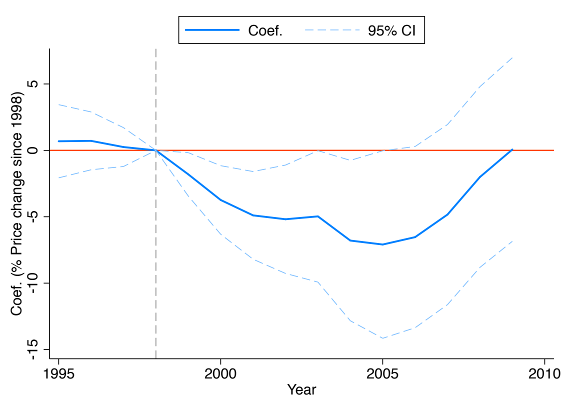
(a) Short-run



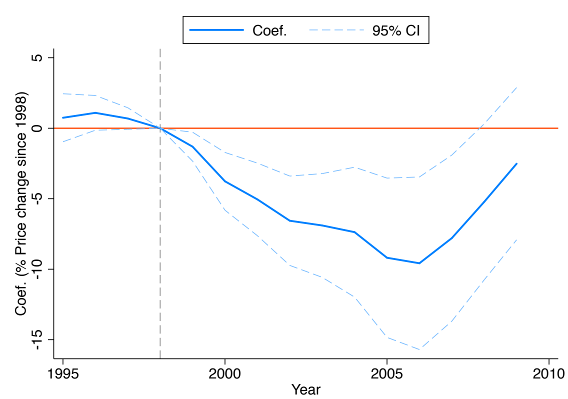
(b) Short-run (Zillow)



(c) Long-run



(d) Long-run (FHFA)



Plots estimates from:

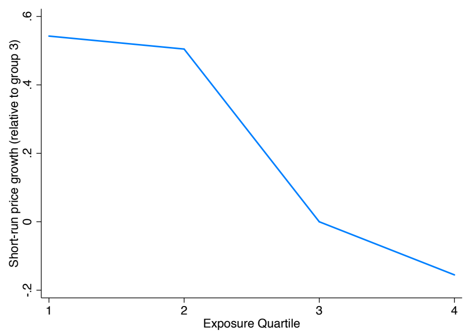
$$\log \text{Price}_{c,t} = \gamma_c + \gamma_{s,t} + \beta_t \text{Exposure}_{c,1998} + \alpha_t \text{Controls}_{c,1998} + \epsilon_{c,t}$$

Where June 1999 is the base month. Standard errors are clustered by CBSA. The regressions are unweighted and I condition on 2000 county population density.

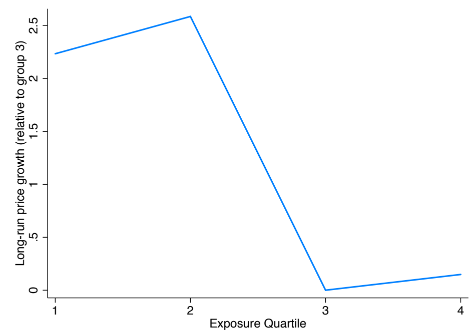
8 – House price response by exposure quartile

Short-run | Long-run

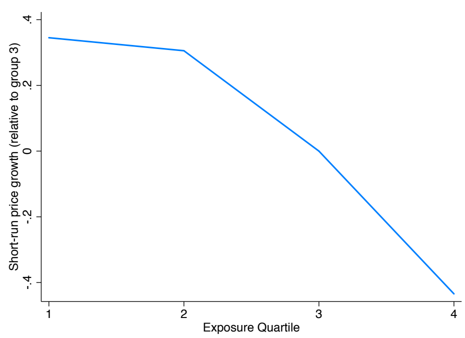
(a) CBSA FE, Short-run



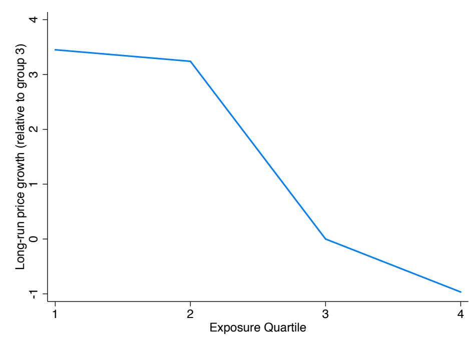
(b) CBSA FE, Long-run



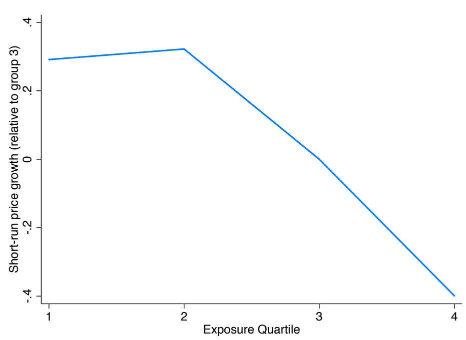
(c) State FE, Short-run



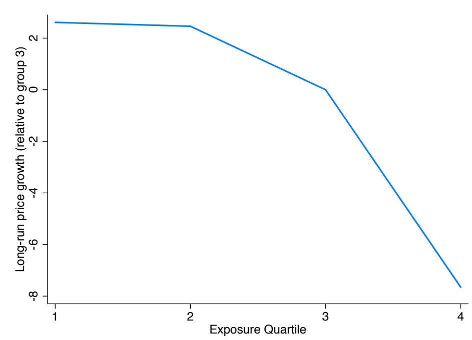
(d) State FE, Long-run



(e) Division FE, Short-run



(f) Division FE, Long-run



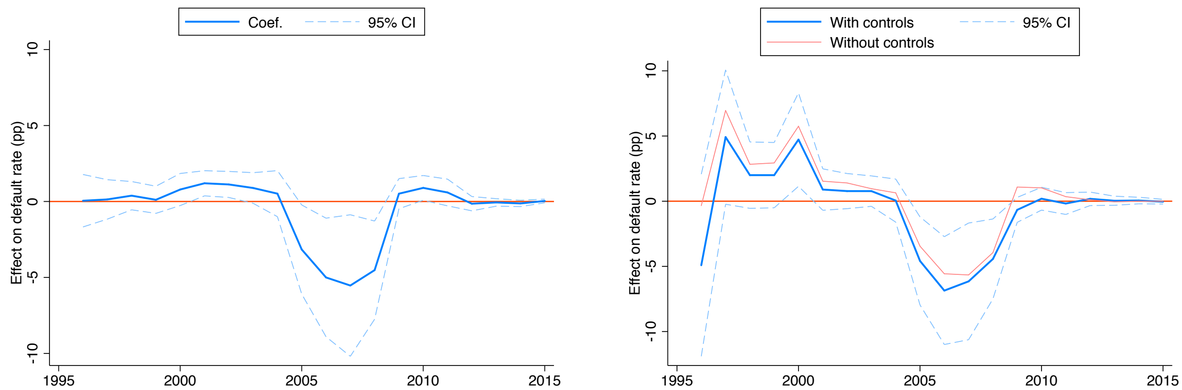
Plots estimates of coefficients on exposure quartiles from

$$\Delta \log(\text{Price})_c = \gamma_g + \sum_{q \neq 3} \beta_q \mathbb{1}[q\text{th exposure quartile}]_c + \alpha \text{Controls}_c + \epsilon_c$$

, where g is the CBSA (Row 1) state (Row 2) or census division (Row 3) of county c.

9 – Effect on 5-year default rate

(a) County controls only (b) Includes loan-level controls



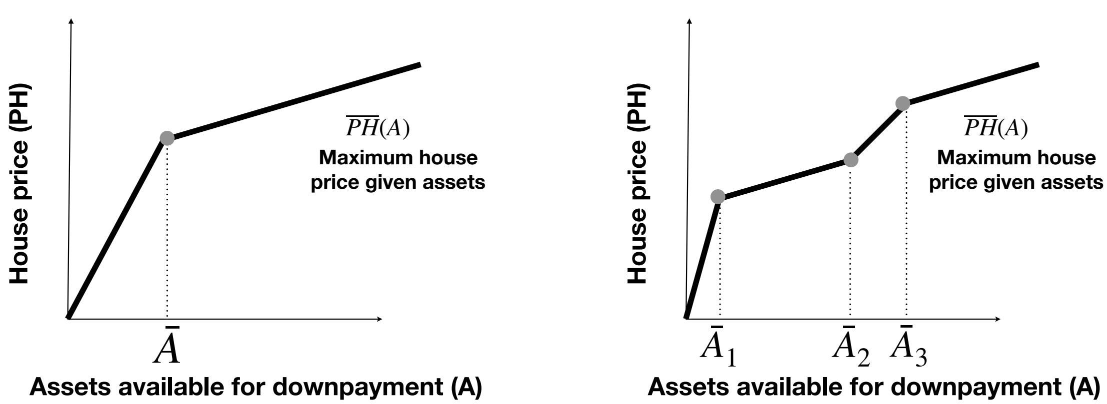
Source: CoreLogic LLMA Database. Figure 9(a) plots estimates from:

$$\text{Default}_i = \gamma_{s,t} + \beta_t \text{Exposure}_c + \alpha_t \text{Controls}_c + \epsilon_i$$

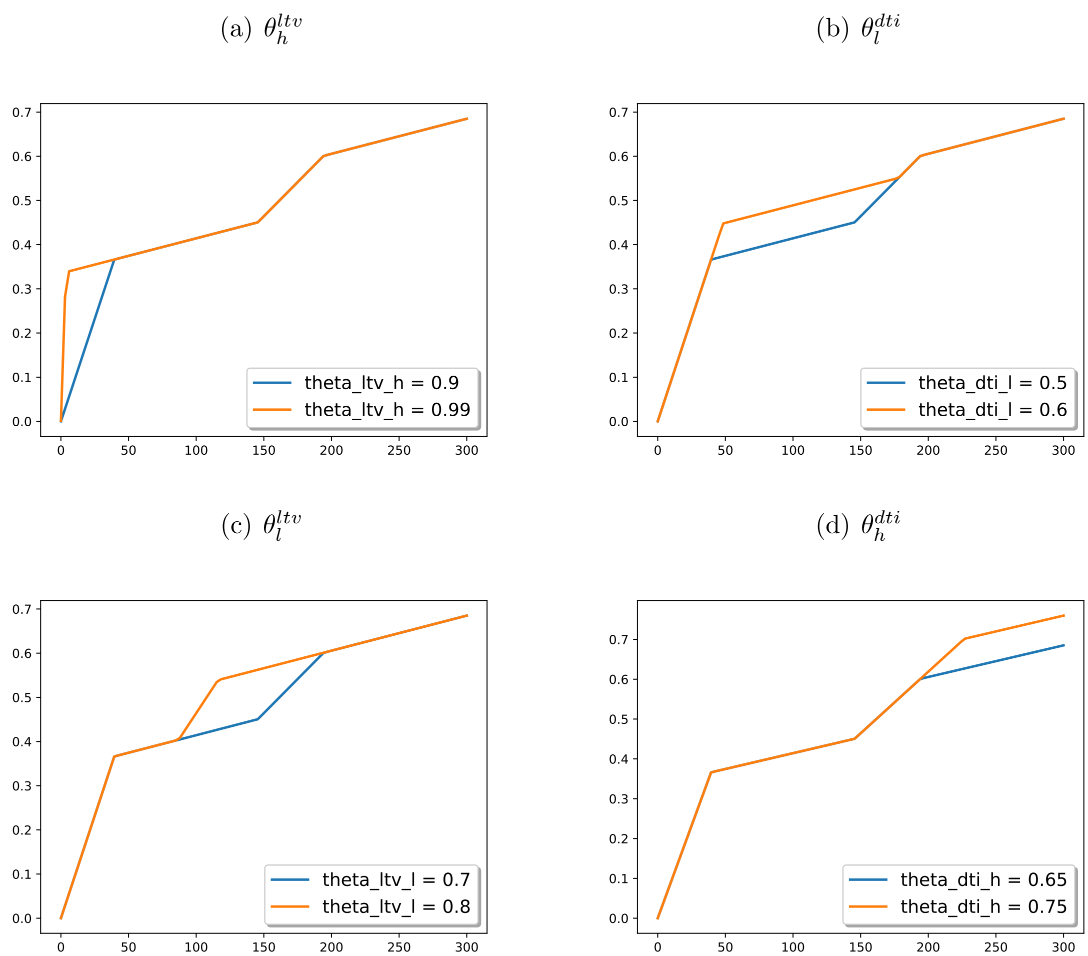
Where loan i is originated in county c in year t. Figure 9(b) adds loan-level loan-to-value, debt-to-income and credit score controls. Standard errors are clustered by county and year. Default_i is equal to 1 if loan i was ever more than 90 days past due in a 5-year period following origination. The red line on Figure 9(b) plots the estimates without controls using the sample of loans for which all controls are non-missing.

10 – Characterizing the constrained group

(a) (b)

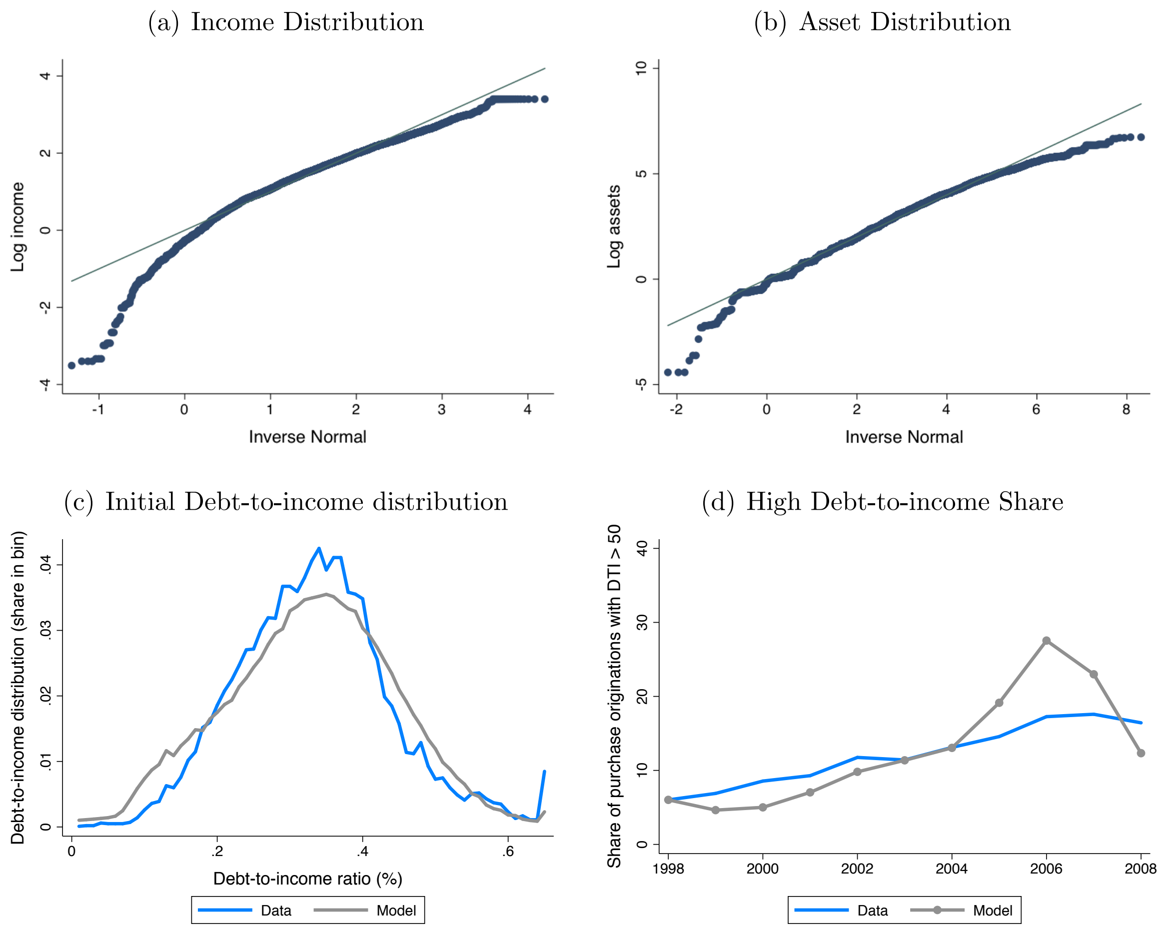


11 – Effect of policy changes on $\bar{\alpha}$ (value of the housing preference parameter above which households are constrained)



Note: This figure shows the effect of changing each of the four policy parameters holding the others fixed. Each line shows the value of the housing preference parameter above which households are constrained, and how this varies with the available downpayment (shown on the x-axis). Base has $\theta_h^{ltv} = 0.9$, $\theta_l^{dti} = 0.5$, $\theta_l^{ltv} = 0.7$, $\theta_h^{dti} = 0.65$.

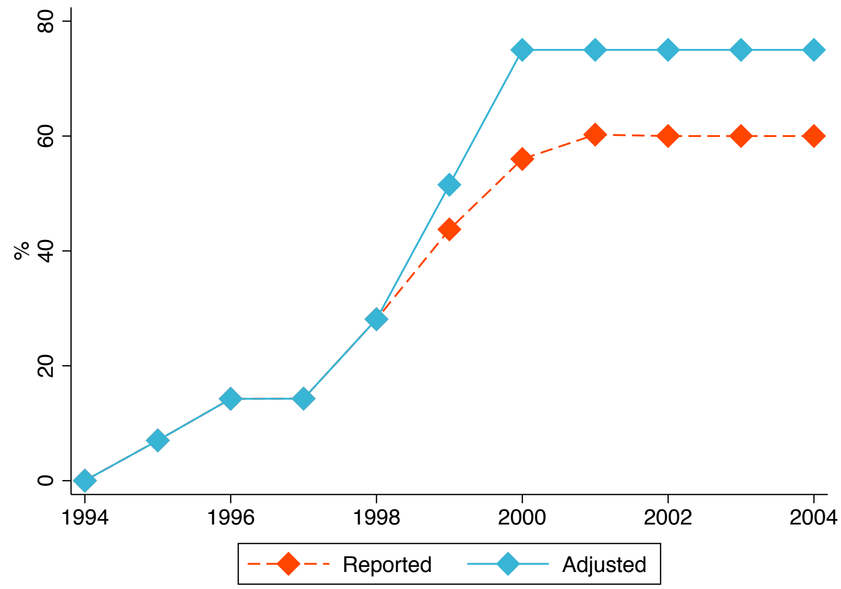
12 – How closely does the model match income, asset and debt-to-income distributions?



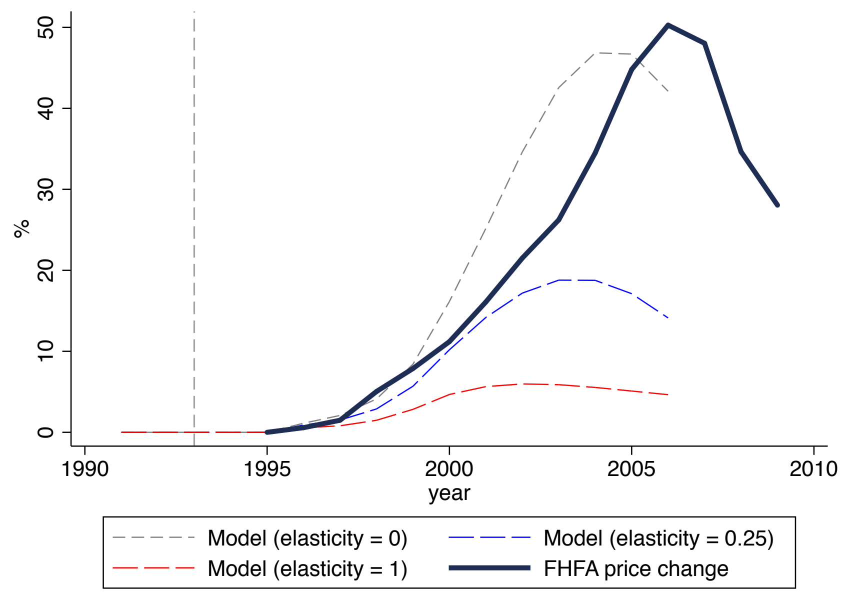
Note: Figure 12(a) compares the income distribution for recent homebuyers in the American Housing Survey (1991-1995) to a log-normal distribution. Figure 12(b) compares the distribution of home equity at the time of purchase for recent homebuyers in the American Housing Survey (1991-1995) to a log-normal distribution. Figure 12(c) compares the debt-to-income distribution in January 1999 to the debt-to-income distribution used to compute the short-run house price effect in the model. Figure 12(d) shows how the share of mortgages with a debt-to-income ratio above 50 per cent changes over time in the data and the model. The model is calibrated to match the 1998 data point. The change in the model share over time is generated primarily by the increase in expected house price growth g, computed using national house price growth and the adaptive expectations rule described in Section 7.

13 – How did relaxing the historical 36 per cent DTI limit affect house prices?

(a) Share of loans purchased by Fannie or Freddie processed using Desktop Underwriter or Loan Prospector



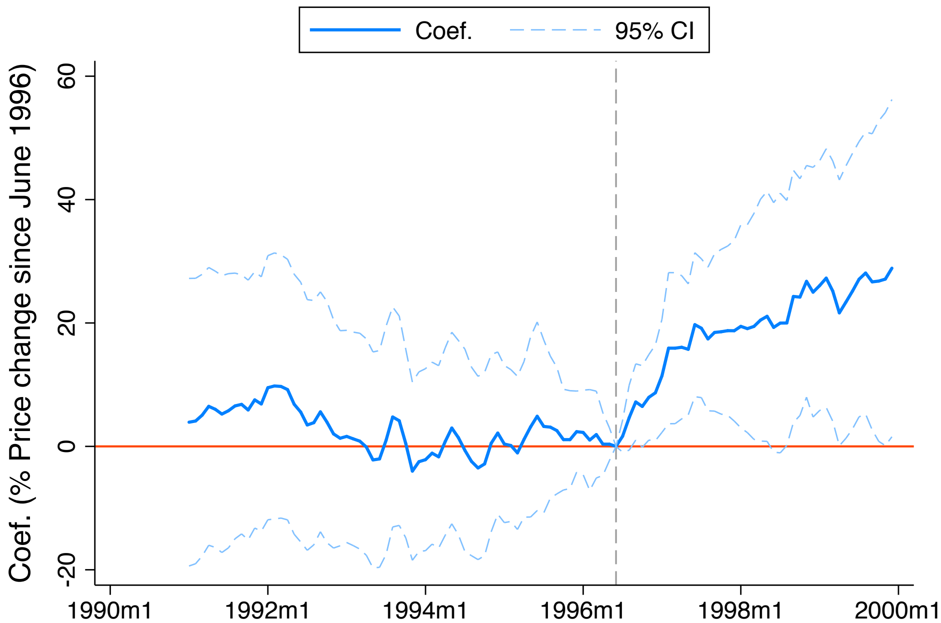
(b) Price response



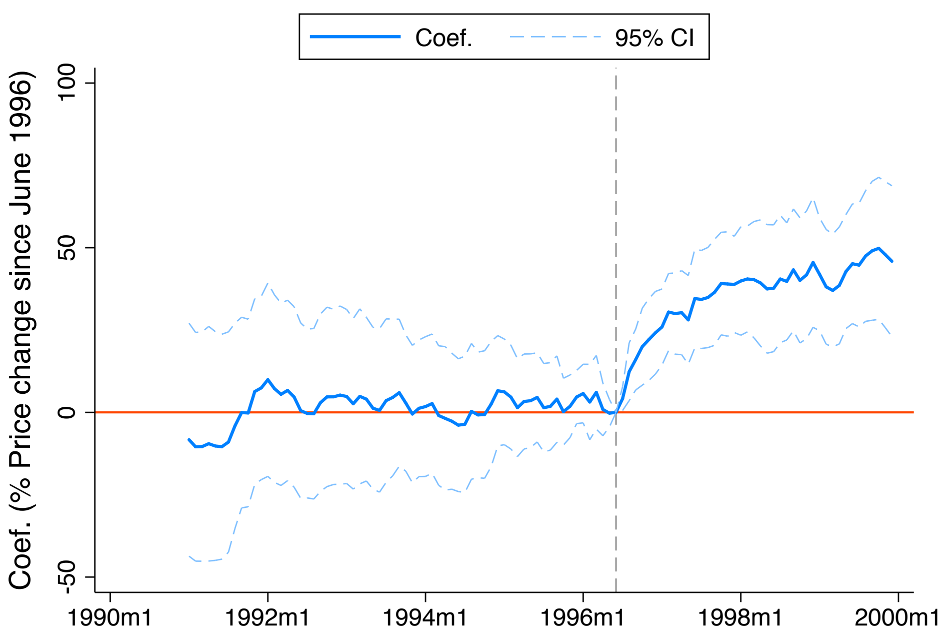
Source: Fannie Mae and Freddie Mac publications, *The Washington Post* and *Mortgage Banking*. The line represents an average of Fannie Mae and Freddie Mac statistics on software usage weighted by the dollar value of their respective purchases reported in the GSE Public Use Database. The adoption rate for 1999 and subsequent years is adjusted. At this time both Fannie and Freddie made arrangements to purchase loans underwritten using other software, leading Desktop Underwriter and Loan Prospector usage to plateau at a rate lower than what the GSEs had anticipated. However, what matters here is the share of loans subject to relaxed debt-to-income rules. GSE data suggests that loans purchased from lenders with special arrangements still displayed characteristics very similar to those purchased from other lenders. This is consistent with the fact that the GSEs reported actively monitoring these loans to understand deviations relative to their own software. The adjusted rate of 75 per cent is consistent with what the GSEs were expecting prior to agreeing to accept loans underwritten using other software.

14 – Effect of GSE software on house prices

(a) Within state



(b) Close to state border

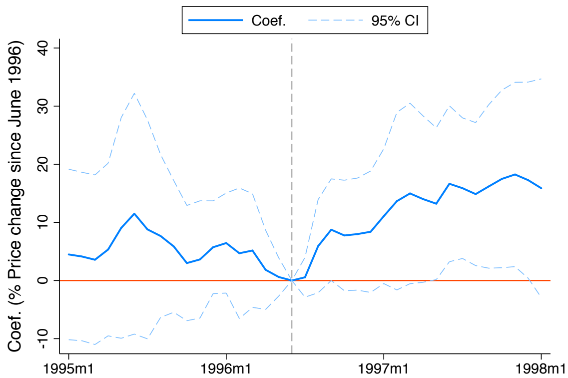


Note: Figure 15(c) uses the market share of the 7 lenders who tested Loan Prospector prior to its release in 1995. This measure excludes Flagstar and InterFirst and I also condition on their market shares. Figure 14(b) uses only counties within 50 miles of a state border and includes border by month fixed effects. Base month is June 1996.

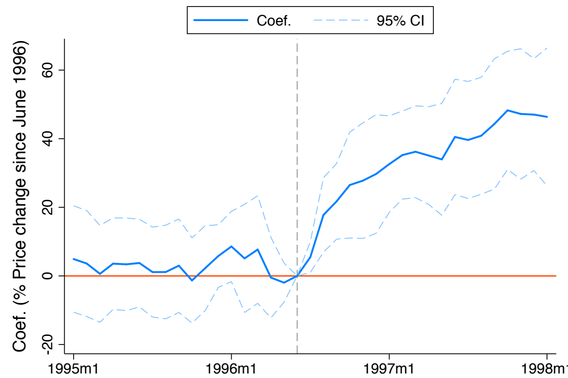
15 – Effect of GSE software on house prices

InterFirst

(a) InterFirst: within state

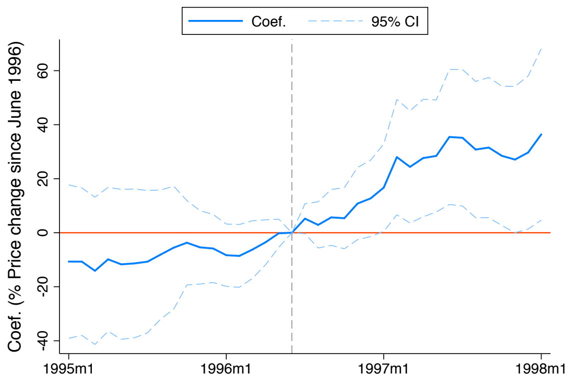


(b) InterFirst: close to border

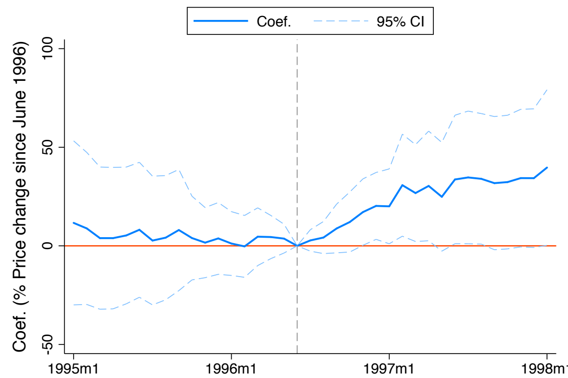


Flagstar

(c) Flagstar: within state

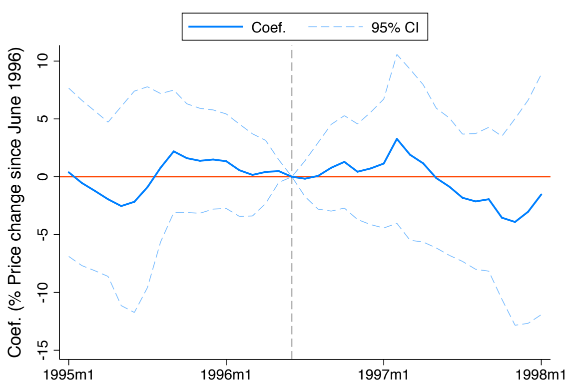


(d) Flagstar: close to border

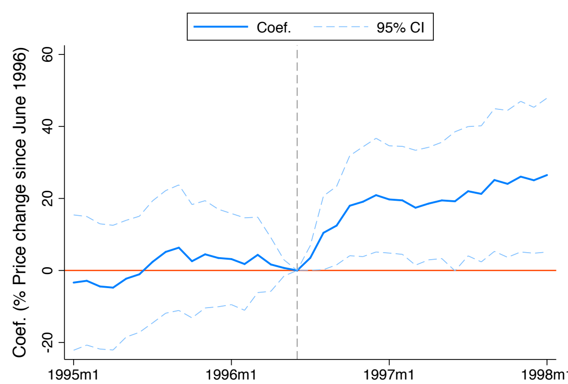


Other trial participants

(e) Other: within state

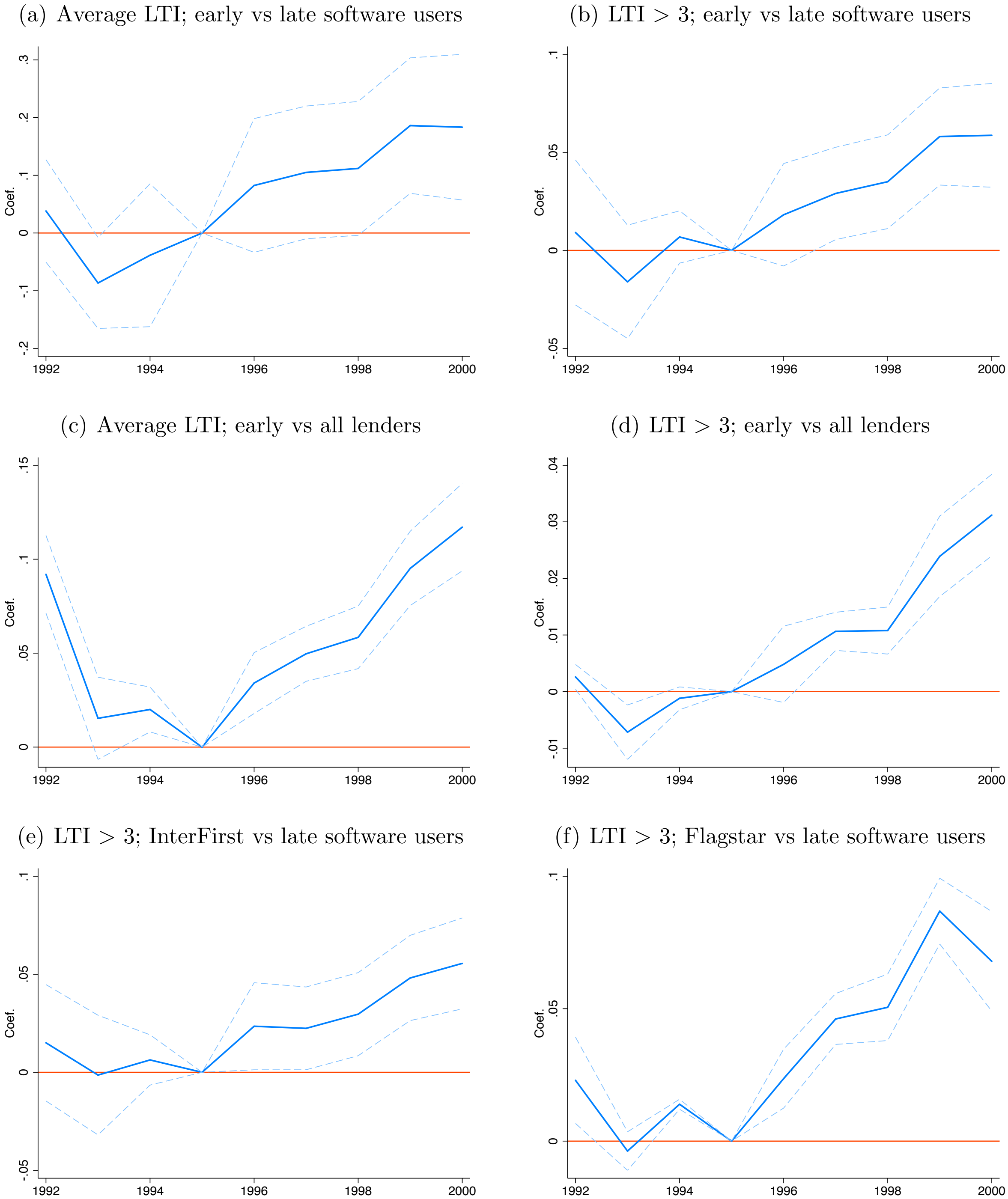


(f) Other: close to border



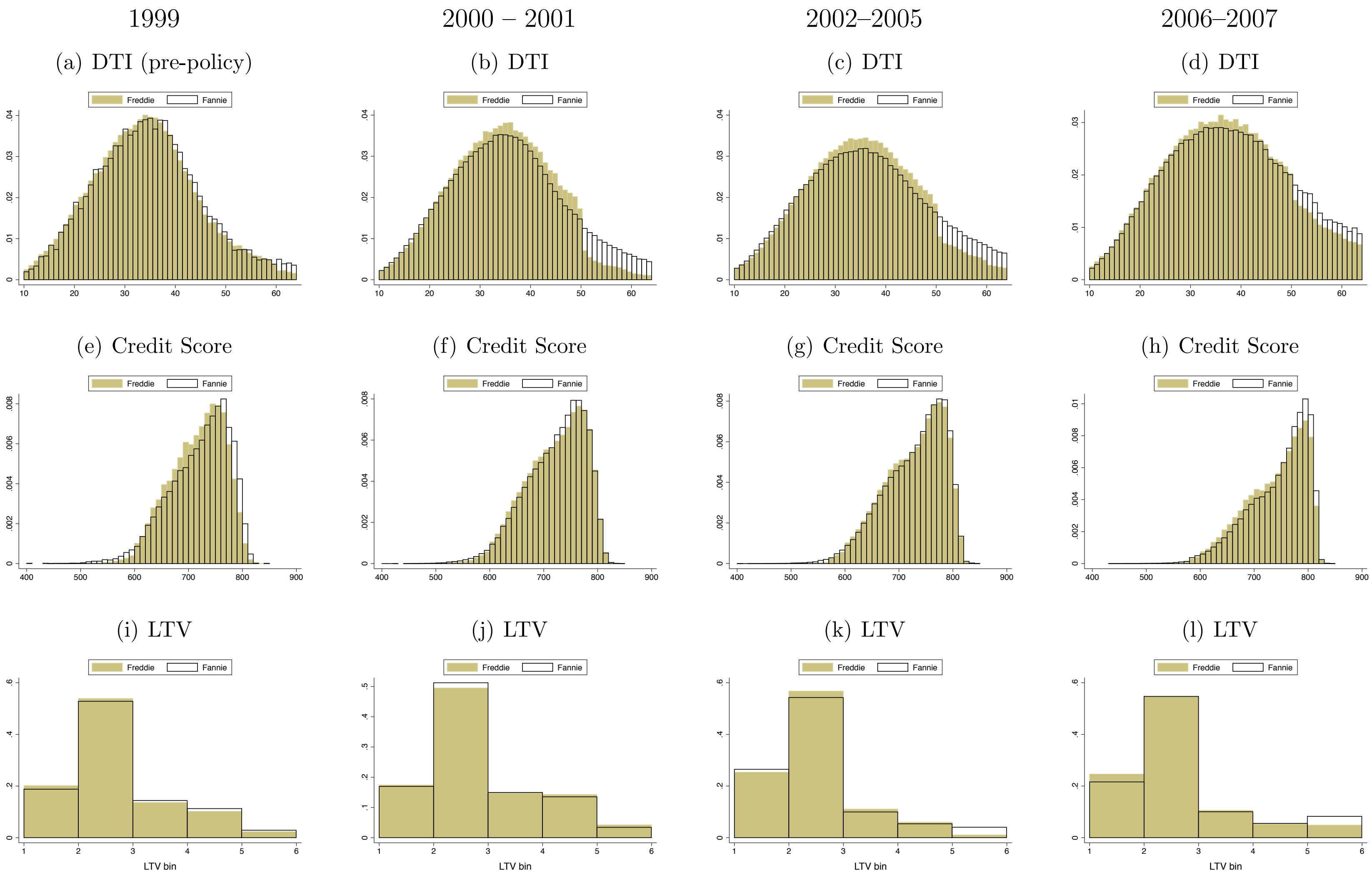
Note: Figure 15(c) uses the market share of the 7 lenders who tested Loan Prospector prior to its release in 1995. This measure excludes Flagstar and InterFirst and I also condition on their market shares. Figures 15(b), 15(d) and 15(f) use only counties within 50 miles of a state border and includes a border by month fixed effect.

16 – Loan-to-income comparison for early software users and other lenders in same county



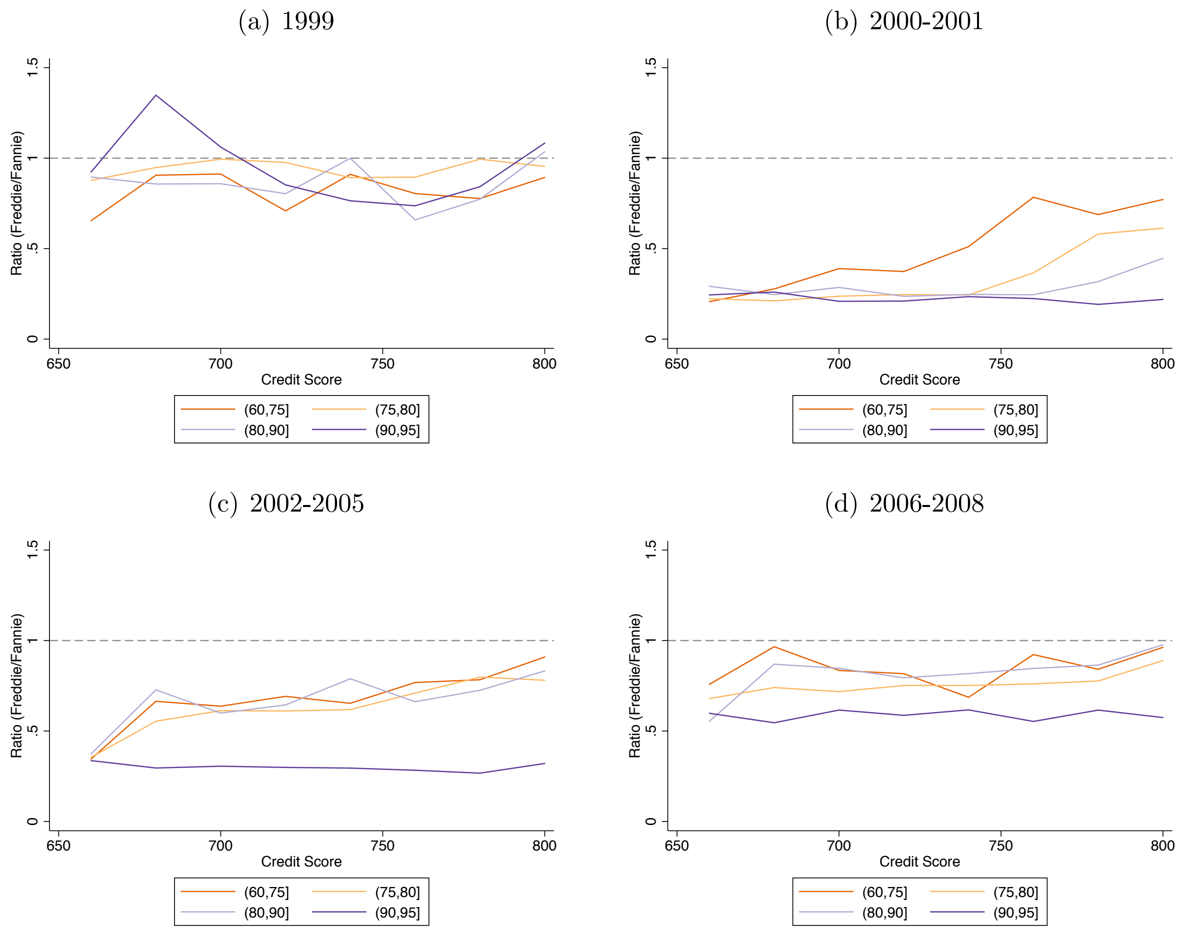
Note: Includes loans sold to Freddie Mac in the calendar year of origination. Early software users and late software users are identified using interviews in a series of *Mortgage Banking* articles from 1995 – 2000. Of the lenders interviewed in these articles, I classify Flagstar and InterFirst as early software users. These were the only lenders who were already using GSE automated underwriting software at scale in 1996. Both adopted the software when it was first released. I classify Fleet Mortgage, Norwest, Chase Manhattan Mortgage, Bank of America, Resource Bancshares and Homeside Mortgage as late software adopters. These lenders were still not using automated underwriting software at scale by 1997.

17 – Characteristics of GSE purchases



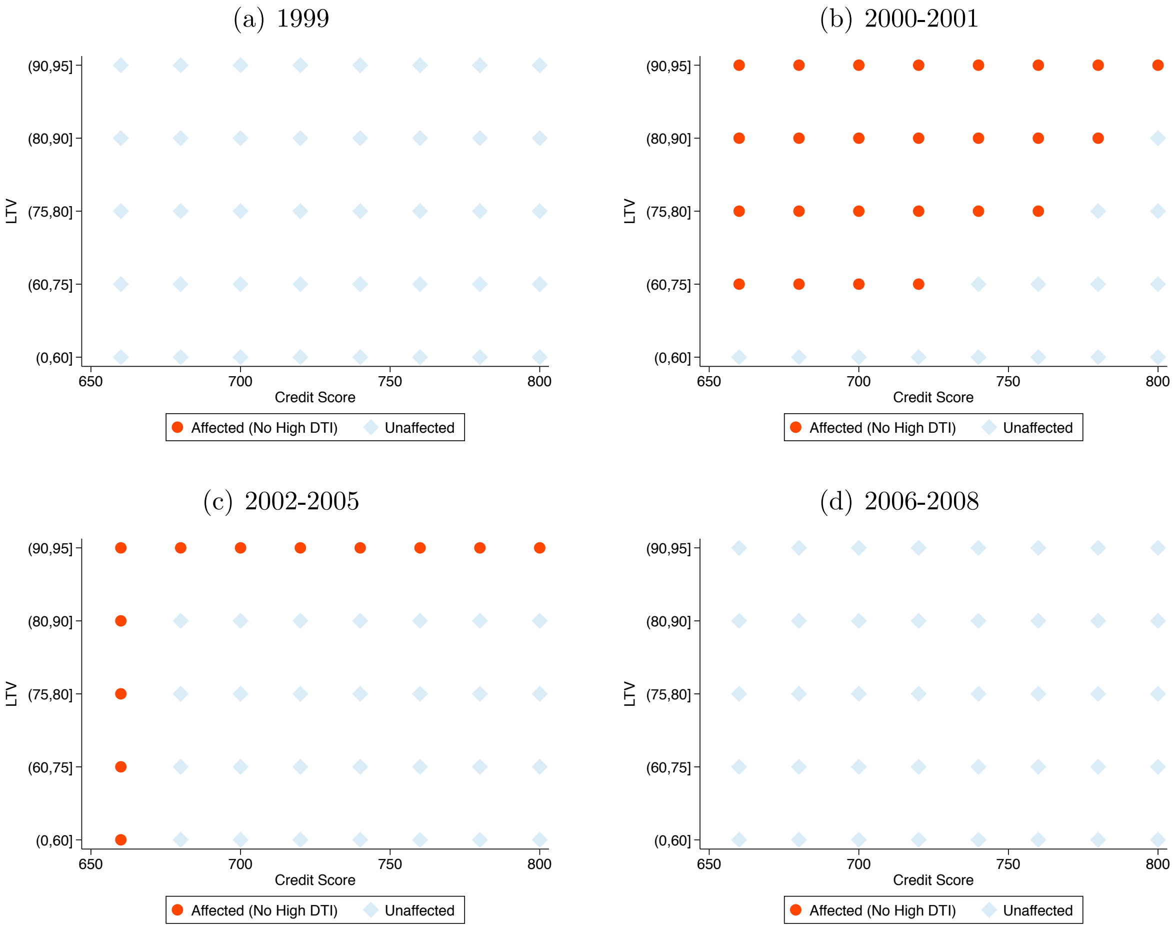
Source: Fannie and Freddie Single Family Loan Performance Datasets (DTI and Credit Score) and GSE Public Use Database (LTV). LTV bins are the same as those used in the GSE Public Use Database: (0,60]; (60,80]; (80,90]; (90,95]; Above 95.

18 – Ratio of Freddie to Fannie DTI > 50% Share



Source: GSE Single Family Loan Performance Datasets. Shows the ratio of Freddie to Fannie's high DTI purchases by credit score and LTV groups. The computation of the ratio is discussed in Appendix C.

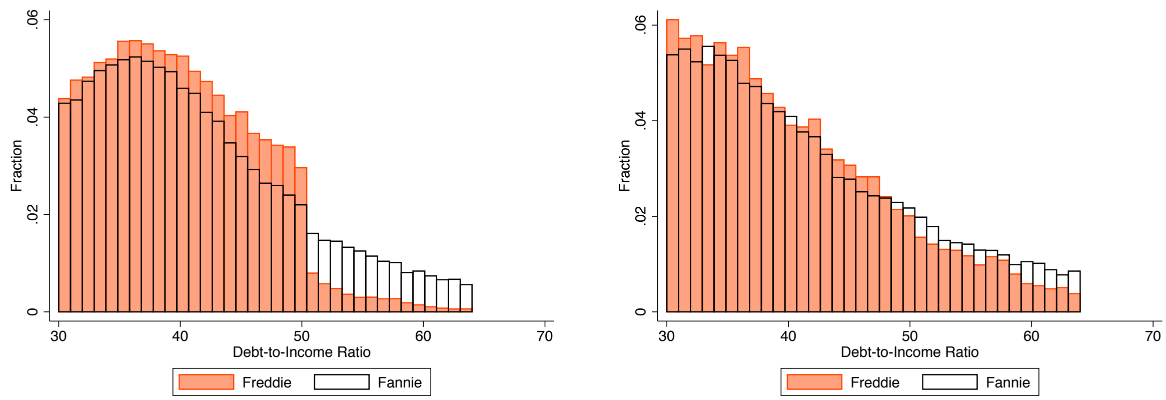
19 – Which LTV by credit score groups are not allowed to have DTI > 50?



Source: GSE Single Family Loan Performance Datasets. Shows whether a given credit score $\times$ LTV group of borrowers is allowed to have a DTI > 50 under Freddie's eligibility criteria. This classification is backed out from the data and is subject to a number of caveats discussed in Appendix C.

20 – DTI distribution for affected and unaffected credit score - LTV combinations

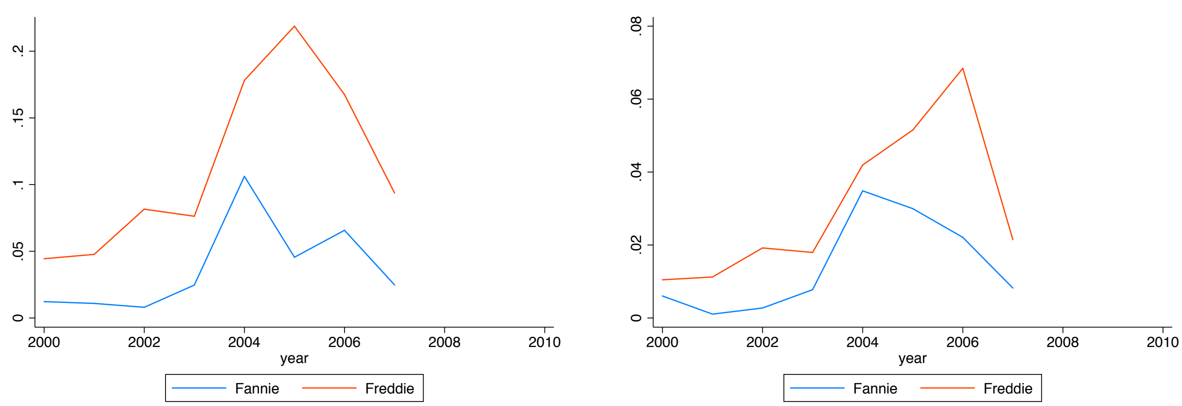
(a) Classified as Affected (b) Classified as Unaffected



Source: GSE Single Family Loan Performance Datasets.
Plots debt-to-income distributions separately by whether, using the procedure described in Appendix C, I classify a particular credit score × LTV group as being allowed to have DTI > 50 or not. That is, Figure 20(a) uses credit score × LTV groups shown in red on Figure 19(b), and 20(b) uses credit score × LTV groups shown in blue on Figure 19(b). Includes purchase loans bought by Fannie Mae or Freddie Mac in 2000. Includes debt-to-income ratios up to 64 per cent.

21 – GSE purchases of subprime and Alt-A private label securities

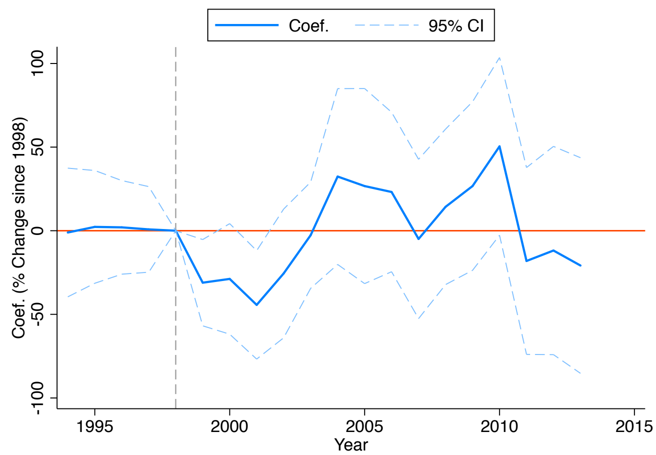
(a) Subprime (b) Alt-A



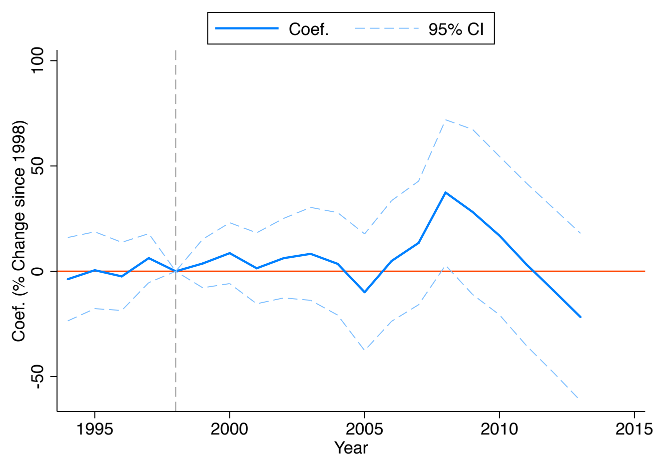
Source: Van Order (2010) (from GAO Analysis of LoanPerformance data, FHFA, Enterprise Credit Supplements). Purchases are expressed as a share of the dollar value of loans reported in the GSE Public Use Database.

22 – Effect on Residential Building Permits

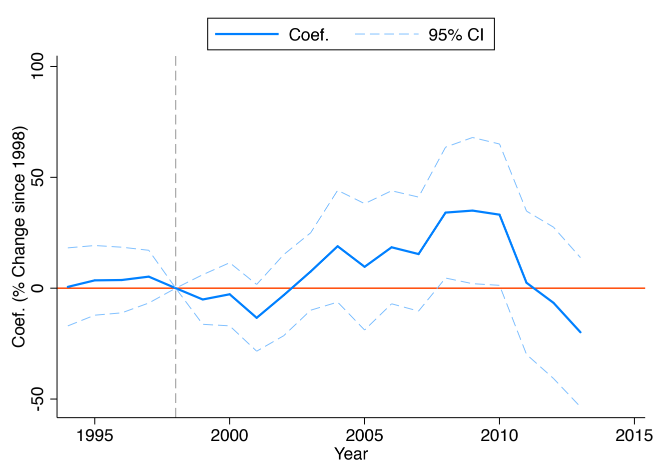
(a) Micropolitan areas



(b) Metropolitan areas



(c) Both metropolitan and micropolitan areas



Source: Building Permits Survey. Figures 22(a) and 22(b) show the effect on the annual number of residential units for which building permits were issued. On average, annual building permits in 2000 are around 1 per cent of the total number of housing units.