Mortgage Leverage and House Prices *

Stephanie Johnson†

October 4, 2019

Abstract

I measure the effect of mortgage debt-to-income restrictions on house prices using a change in the eligibility requirements imposed by Fannie Mae and Freddie Mac. I show that in 1999 Fannie Mae and Freddie Mac's debt-to-income rules 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 and leads to 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, finding that a relaxation of debt-to-income restrictions is important for explaining the 2000s housing boom.

JEL Classification: G21, G28, R31

Keywords: mortgages, financial regulation, house prices

---

*I would like to thank John Mondragon, Anthony DeFusco, Matthias Doepke, Martin Eichenbaum, David Berger, Lorenz Kueng, Matthew Rognlie, Scott Baker, Douglas McManus, Philip Bond, Mark Bils, Gonzalo Sanchez and Lara Loewenstein, as well as participants in the following seminars: Northwestern University, University of Melbourne, University of Sydney, UNSW, University of Washington, Rice University, Notre Dame, University of Rochester, University of Maryland, Federal Reserve Bank of Richmond, University of Colorado Boulder, University of Connecticut, University of Pennsylvania, Texas A&M, Cornell, 2017 MEA Annual Meeting, 2018 OSU PhD Conference on Real Estate and Housing, NYU Stern Conference on Household Finance, CopenhagenMacro Days and the JPMorgan Chase Institute Conference on Economic Research. I acknowledge support from the Guthrie Center for Real Estate Research at Northwestern University. This research was funded in part by the Ewing Marion Kauffman Foundation. The contents of the paper are solely my responsibility.

†Jones Graduate School of Business, Rice University, 1900 Rice Boulevard, Houston, TX 77005. E-mail: stephanie.g.johnson@rice.edu

I. 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 effect after several years is considerably larger than the short-run effect. While the short-run effect is consistent with the housing demand response of constrained borrowers in a simple model, additional feedback mechanisms are needed to explain the way the effect grows over time. Including adaptive house price expectations generates a similar response to what I find in the data. The framework can also be used to compute the effects of policies which use different cutoffs. This allows me to extrapolate and argue 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. Part of the contribution of this paper is to document this policy change, which I back out using loan-level data. 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

[1] The debt-to-income ratio is defined as the ratio of the monthly mortgage payment and other financial obligations (e.g. child support, alimony, property tax and other debt payments) to gross income.

[2] This has been shown by authors looking at the jumbo market (Loutskina and Strahan (2009); Calem et al. (2013); Adelino et al. (2014)), as well as by Fieldhouse et al. (2018), who show that changes in GSE asset purchases increase mortgage originations and reduce interest rates.

different criteria to determine whether they are willing to purchase a mortgage. Mortgages that satisfy these 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 use broadly similar rules, their criteria sometimes diverge. When this happens, effective lending standards diverge across locations depending on whether local lenders sell to Fannie Mae or Freddie Mac.[3] 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.

The tighter debt-to-income requirements imposed by Freddie Mac initially affected around 5 per cent of borrowers and led to a short-run relative decline in prices of about 2 per cent when comparing locations where lenders sell to Freddie with those where lenders sell to Fannie. This is a relative decline, not an absolute decline, as house prices were in general growing strongly during this period. Freddie Mac's tighter rules continued to weigh on price growth for some time, and dampened the entire price cycle. Areas with tighter policy experienced much lower default rates during the crisis.

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 refuting a direct link to affordable housing policy (Bolotnyy (2013); Ghent et al. (2015)), both Fannie and Freddie did expand their debt-to-income criteria considerably during the 1990s. 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

[3]In Section III I show that at the time of the change lenders often had exclusive relationships with either Fannie or Freddie, and that these relationships were very persistent. This behavior is important for the identification strategy and could possibly be due to a combination of two factors. Firstly, neither Fannie nor Freddie would generally accept the assessment of the other's software (Foster (1997); DeMuth (1999)). Secondly, it was perceived as expensive to use both Fannie and Freddie's software so 'most lenders [opted] to go with one based on where they [had] their primary business relationship' DeMuth (1999). Perceived costs of changing software may explain why lenders did not simply switch from Freddie to Fannie following the change. These considerations have diminished in importance over time. In particular, more than half of Freddie Mac's purchases are now evaluated using Fannie Mae's software (FHFA, 2019).

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 second half of the 1990s. I use a simple model to compute the effect of this change and find it can explain a large share of price growth from 1995 to 2002. However, as software adoption was largely complete by the early 2000s, this channel cannot directly explain a large share share of growth during the 2000s. In a companion paper I use a differences-in-differences approach to measure the effect of the GSEs' software on house prices, and find a similar response.

My paper relates to work in a number of areas. Firstly, it relates to a policy literature that measures the effect of debt-to-income restrictions on house prices empirically (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.

In addition to quantifying the effects of debt-to-income restrictions specifically, I also provide support for the claim that credit conditions influence house prices. In this respect, my paper relates to work looking at the effect of deregulation of house prices (Favara and Imbs (2015); DiMaggio and Kermani (2017)), the effect of lending standards on house prices (Anenberg et al., 2019) and the effect of GSE purchases on house prices (Adelino et al. (2014); Fieldhouse et al. (2018)). Adelino et al. (2014) show that changes in the conforming loan limit affect prices of relatively expensive houses using property-level data. Here, I look at the county house price response and track the response over several years. Fieldhouse et al. (2018) find effects on lending and interest rates using aggregate data, but their results with respect to house prices are inconclusive.

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. 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 more exposed locations.

Several recent papers use a quantitative modeling approach to look at the effect of debt-to-income or loan-to-income 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); 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 reasons why these papers draw different conclusions relates to their assumptions about the rental market. Leverage policies will have a limited effect on demand for housing services when households who would be constrained if buying are able to rent a similar property at a similar price. The fact that I estimate a large effect in practice suggests that models assuming segmented housing markets are likely to draw more accurate conclusions, at least in the US context.

The main caveat when comparing my empirical results with these models is that 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

[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 local aggregates, and has little to do with loan-level differences in leverage and credit score at origination.

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. Fieldhouse et al. (2018) also provide direct support for this.

Section II provides institutional context for the identification strategy. In Section III I describe the data. In Section IV I describe the policy change and in Section V I measure the effect of the change on house prices. In Section VI I measure the relationship between exposure to the policy change and default rates. In Section VII I describe a simple model for computing the effect of debt-to-income policies on house prices. This is useful for validating the empirical results, understanding the way the effect evolves over time, and computing the price response for alternative DTI distributions and policy cutoffs.

II. 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.[5] 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 'prime' and are generally considered to be low risk.

Historically, the GSEs' criteria took the form of manual underwriting guidelines and

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

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 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 relaxed.[6]

Although lenders were initially slow to adopt the software after its release in 1995, usage rose rapidly during the late 1990s and was mostly complete for large lenders by the early 2000s.[7] Both GSEs continued to make changes to their software algorithms over time. To my knowledge many of these changes were not publicized, including the change I identify here using loan-level data. The important point for this paper is that Freddie imposed tighter debt-to-income criteria than Fannie for several years between 1999 and the financial crisis. I document this in Section IV and Appendix E.

Although lenders can make loans that do not meet Fannie or Freddie's rules, in practice they rely heavily on these rules for multiple reasons. 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 or Freddie the originator does not need to hold the loan on its balance sheet, making the origination decision less dependent on lender-specific factors. Even when a lender wishes to retain residential mortgage exposure, it may make sense to hold mortgage-backed securities issued by Fannie or Freddie rather than whole loans. Not only are these securities more liquid, they also receive favorable treatment under regulatory capital requirements.

[6]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. 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)). For other discussions see Straka (2000), Markus et al. (2008) and Foote et al. (2018).

[7]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).

III. Data

III.A. Data Sources

I measure exposure to the policy change using Freddie Mac's county market share by number of loans. Using the dollar value of loans yields very similar results. I compute market shares using the Home Mortgage Disclosure Act (HMDA) dataset, which provides fairly comprehensive coverage of U.S. mortgage originations. While 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). To address the concern that a selected group of lenders changed GSE relationships in response to the underwriting changes, I measure county exposure to Freddie Mac in 1998 before the policy change occurred. 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. 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.

I measure monthly county house prices using the CoreLogic county house price index. This is a repeat-sales index constructed by pairing sales of the same property in different time periods. This means that changes in the characteristics of properties traded should have little effect on the index, but also means that new properties, and properties traded only once, are not included. The index reflects transactions of both detached and attached single family dwellings. The results are robust to using the annual FHFA county price index, which applies a repeat sales methodology using only properties where the mortgage was sold to Fannie or Freddie and excludes attached dwellings.[8]

I characterize the policy change using Fannie and Freddie's Single Family Loan Performance Data and Public Use Databases. The Single Family Loan Performance datasets contain information on the month of origination, debt-to-income, loan-to-value and credit

[8] I exclude counties from the CoreLogic data where the house price index is infilled for all months prior to the policy change due to lack of data. When using the FHFA data I include only those counties for which the house price index is available back to at least 1980.

score of loans purchased by Fannie and Freddie. However, these datasets do not provide a precise measure of the property location, reporting only the state, MSA and three-digit Zip Code.[9] They also contain only a selected subset of loans and are not available prior to 1999. I therefore also rely on the Public Use Databases, which provide better coverage and are available back to 1993, as well as HMDA. The disadvantage of these datasets is that information is annual and debt-to-income and credit score are not reported.

III.B. Descriptive Statistics

The identification strategy relies on limited substitution from Freddie to Fannie following the policy change. To the extent that this substitution occurs, the effect on house prices will be lower than what it would have been were the same policy applied nationally.[10] At the time of the policy change, many lenders had chosen to adopt only one of Fannie and Freddie's software. It was perceived as expensive to use both pieces of software and, prior to policy change, the two pieces of software were thought to apply very similar rules. At that time, Fannie and Freddie would not generally accept the assessment of the other's software (DeMuth, 1999), and so switching from one to the other would involve adopting a new piece of software.[11] This story is supported by the data. The HMDA dataset allows me to determine whether a lender has an exclusive relationship with Freddie or Fannie and to study the persistence of these relationships. 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.

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

[9]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.

[10]The difference in policies is also unlikely to apply to the entire market. While this is challenging to quantify, evidence from industry publications suggests that these rules were also applied to loans not intended for sale to the GSEs. At the time of change some lenders also used the software to assess jumbo loans, subprime loans and loans they intended to hold in portfolio (DeMuth (1999); LaMalfa (1999)). Another possibility is substitution to FHA loans; however, at that time these loans had a 41 per cent DTI limit except where there were significant compensating factors. FHA loans also have a substantially smaller loan size limit than conforming loans (95% of the market median home price, with a lower limit of 38% of the conforming loan limit and an upper limit of 75% of the conforming loan limit).

[11]It may also have taken lenders some time to learn about the change as Fannie and Freddie's software algorithms were proprietary, and dataset I use to back out the change was not publicly available at the time. While it was possible for lenders to sell to either Fannie or Freddie by applying manual underwriting rules, in practice this was often unattractive as the manual rules were considerably more restrictive than those applied by the software.

95 per cent to Fannie Mae. These exclusive relationships were also very persistent. While Freddie Mac relationships are slightly less likely to survive, survival is broadly similar regardless of whether the 1998 relationship was with Fannie or Freddie. Figures A.1 and A.2 in the appendix show the distribution of the share sold to Freddie and Kaplan-Meier estimates of the probability that a 1998 exclusive relationship still survives in later years.

Next I look at how the exposure measure is related to other variables. Table I shows statistics for counties with non-missing house price data located in a core-based statistical area. I separate counties into two groups based on whether they have above or below median exposure to Freddie Mac. The two groups are similar along a number of dimensions, including income, mortgage leverage, the presence of subprime lenders, the share of loans sold to Fannie or Freddie, and the increase in unemployment during the early 2000s recession. The main respect in which the two groups differ is with respect to population density, the rural population share and coastal proximity. As I use state-time fixed effects, I also show in Table A.1 how each variable is related to the exposure measure within state. All variables are normalized by dividing by the standard deviation. Within states, there is a significant negative relationship between the exposure measure and both population density and income. There is also a significant positive relationship between the exposure measure and the market share of thrifts and the within R-squared is 8 per cent. The relationship with the thrift share is unsurprising given a strong historical relationship between thrifts and Freddie Mac.

These differences in characteristics raise the concern that house prices may have moved differently across these areas for reasons unrelated to the policy. To address this concern, 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. My main specification includes state-time fixed effects and conditions on median household income, population density, a coastal indicator equal to one if the county is defined as coastal by the NOAA and market share of lenders classified as subprime by HUD. In Appendix C I re-weight the sample to equalize the means of several variables across high and low exposure groups. This alternative approach yields very similar results. The results are also robust to dropping the top 20 MSAs by 2000 population.

Table II shows characteristics of loans purchased by Fannie or Freddie before and after the policy change in 1999, and illustrates that Fannie and Freddie behaved similarly along several other dimensions. I use the Public Use Database because the Single Family Loan Performance dataset does not cover high LTV loans consistently for Fannie and Freddie, and has substantially reduced coverage for Fannie prior to November 1999. While both

Table I
County Descriptive Statistics

	All Counties	Below Median	Above Median
Median income ('000s, 1998)	40.13	41.37	38.89
	(9.10)	(10.15)	(7.73)
Average DTI (1998)	29.17	29.22	29.12
	(4.11)	(3.89)	(4.33)
Subprime lender share (1998)	19.98	19.11	20.85
	(6.97)	(7.16)	(6.66)
% sold to Fannie/Freddie (1998)	28.29	28.37	28.20
	(9.17)	(9.06)	(9.29)
Persons per sq. mi. (2000)	271.87	319.80	223.84
	(256.12)	(280.07)	(219.64)
% Δ unemployment (2000-2002)	35.80	36.09	35.51
	(18.90)	(18.69)	(19.12)
Thrift share (1998)	18.04	17.91	18.17
	(11.58)	(10.48)	(12.58)
Coastal county	0.35	0.42	0.29
	(0.48)	(0.49)	(0.45)
% County pop. in rural area (2010)	30.34	25.99	34.69
	(22.64)	(22.17)	(22.28)
% County pop. in underserved area (1998)	35.47	35.81	35.12
	(32.82)	(30.93)	(34.63)
Freddie market share (1998)	45.14	32.29	58.01
	(16.32)	(8.26)	(11.55)
Number of Observations	995	498	497

Notes: Median income is real household median income from the U.S. Bureau of the Census. Average DTI is computed using the CoreLogic LLMA Database. Population density is county population density from the 2000 census. Underserved is the share of the county population living in a HUD targeted area (1998 classification). Coastal is equal to one if the county is defined as coastal by the NOAA. The Freddie Mac county market share is constructed using HMDA and excludes lenders originating more than 20000 purchase loans. Population density is winsorized at 95 per cent. Two counties are excluded from the second row due to missing DTI.

Table II
Borrower Descriptive Statistics

	Pre		Post	
	Fannie	Freddie	Fannie	Freddie
% LTV < 60	18.94	20.71	16.95	17.39
	(39.18)	(40.52)	(37.52)	(37.90)
% $60 <$ LTV ≤ 80	52.89	54.28	51.21	49.46
	(49.92)	(49.82)	(49.99)	(50.00)
% $80 <$ LTV ≤ 90	15.43	14.04	14.93	14.69
	(36.12)	(34.74)	(35.64)	(35.40)
% $90 <$ LTV ≤ 95	10.84	9.73	13.50	14.25
	(31.09)	(29.64)	(34.17)	(34.96)
% $95 <$ LTV ≤ 100	1.90	1.24	3.41	4.21
	(13.66)	(11.08)	(18.15)	(20.09)
% Low income family in low income area	1.50	1.39	1.63	1.74
	(12.14)	(11.72)	(12.67)	(13.09)
% Very low income family	9.80	9.34	10.93	11.90
	(29.73)	(29.10)	(31.20)	(32.38)
% In underserved area	24.16	23.65	26.49	26.38
	(42.81)	(42.49)	(44.13)	(44.07)
Number of Observations	3,115,276	2,366,445	1,695,428	1,350,280

Notes: Constructed using the GSE Public Use Database. Columns 1 and 2 are constructed using loans purchased during 1998 and Columns 3 and 4 are constructed using loans purchased during 2000. Low income is defined as income less than 80 per cent of the median family income in the area; very low income is defined as income less than 50 per cent of the median family income in the area (with adjustments for family size and areas with particularly high or low housing costs relative to income). Underserved areas are census tracts with median income less than or equal to 120 per cent of area median income and a minority population of at least 30 per cent.

Fannie and Freddie increased their purchases of loans with an LTV above 90 per cent between 1998 and 2000, they did so in a similar way. Fannie and Freddie also behaved very similarly with respect the affordable housing goals, both having a similar share of loans in areas defined as underserved, and a similar share of loans to borrowers with very low income.

IV. 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 describe 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.[12]

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 typically 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.[13] I also validate the divergence in rules using other datasets.

IV.A. Timing

The difference in policies can be seen clearly by looking at the debt-to-income distributions of Freddie and Fannie's mortgage purchases. Figure I, constructed using loans

[12] 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)

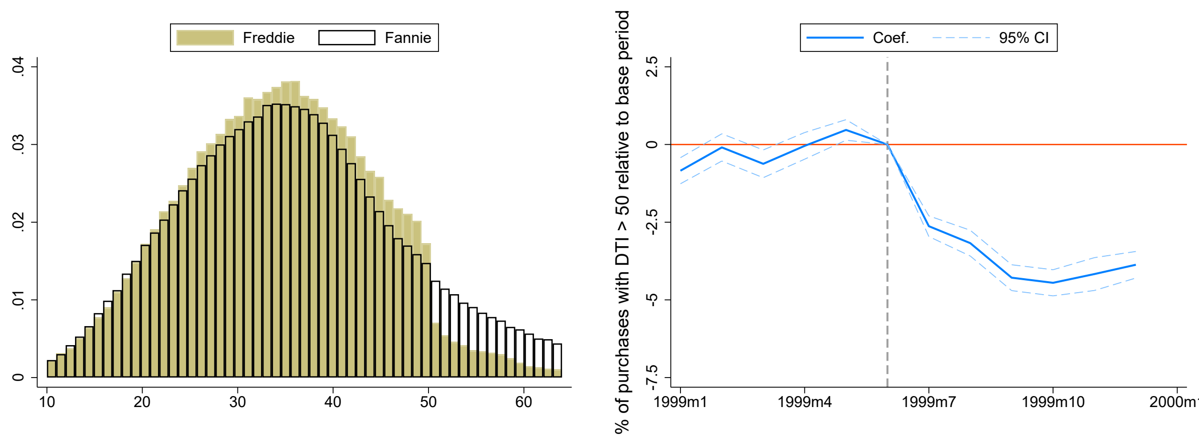
[13] While both Fannie and Freddie expanded their criteria during the 1990s, by 2002 Freddie was using more conservative language regarding these developments. A 2002 *Mortgage Banking* article quoted a Freddie representative, saying: 'Freddie Mac "worries quite a lot" about credit risk' and that 'Freddie 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)

originated from 2000–2001, shows a sharp drop in the mass above 50 per cent for Freddie but not for Fannie. Next, I document the timing of the change by plotting the share of high debt-to-income purchases for Freddie Mac over time. I back out the timing using Freddie loans only, as Fannie has very limited coverage prior to November 1999 (though despite this concern a difference-in-differences approach yields very similar results).

Figure I

Debt-to-Income Restrictions Imposed by Freddie Mac in 1999

A. DTI Distribution 2000–2001 *B. Change in Freddie's Share DTI > 50%*



Notes: These figures are constructed using purchase loans to owner-occupiers bought by Fannie Mae or Freddie Mac. The top panel includes loans originated in 2000 or 2001. Loans with debt-to-income ratios above 64 per cent are excluded because Fannie and Freddie report them differently. Both figures exclude loans sold by very large sellers who are identified in the dataset by name. 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 (consistent with the presence of special agreements). Including all loans does not lead to qualitatively different results. The bottom panel shows estimates of β_t from $\mathbb{1}[\widetilde{DTI}_i > 50] = \gamma_s + \beta_t + \epsilon_i$ for Freddie purchases only, where $\widetilde{DTI}$ is an adjusted measure which abstracts from movements in interest rates, and loan i is originated in month t in state s.

When 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 an increase in interest rates during 1999 which appears to have raised debt-to-income ratios. I construct the following adjusted debt-to-income ratio, which holds average interest rates fixed at August 1999 levels.

$$\text{High } \widetilde{\text{DTI}} = \frac{f(r_{\text{Aug 1999}})}{f(r)} DTI \tag{1}$$

where $f(r)$ is the 30 year fixed mortgage payment on \$1 of debt.[14] I plot estimates of β_t from:

$$\text{High } \widetilde{\text{DTI}}_i = \gamma_s + \beta_t + \epsilon_i \tag{2}$$

where High $\widetilde{\text{DTI}}$ is an indicator equal to one for loans with an adjusted DTI greater than 50 per cent and loan i is originated in month t in state s. The lower panel of Figure I shows that Freddie tightened its debt-to-income policy in July 1999.

IV.B. Magnitude

Column 1 of Table III shows the estimate of β_1 from:

$$\text{High } \widetilde{\text{DTI}}_i = \gamma_s + \alpha \text{Post}_i + \beta_1 \text{Freddie}_i + \epsilon_i \tag{3}$$

Column 2 shows the estimate of β_1 from:

$$\text{High DTI}_i = \gamma_{s,0} + \gamma_{s,1} \text{Post}_i + \beta_0 \text{Freddie}_i + \beta_1 \text{Freddie}_i \cdot \text{Post}_i + \epsilon_i \tag{4}$$

where Post_i is an indicator equal to 0 for loans originated during 1999Q2, and equal to 1 for loans originated during 1999Q3. Overall, the policy reduced Freddie Mac's purchases of high DTI loans by around $3\frac{1}{2}$ percentage points in the first quarter. Both specifications yield similar results. Columns 3 and 4 show the estimates of β_1 with log DTI as the dependent variable. The policy reduces the average DTI of Freddie Mac's purchases by 3–4 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 leaves open the possibility that the changes I identify reflect selection into the dataset. While this is a potential concern, Freddie's dataset does provide high coverage of its single family 30-year fixed-rate mortgage purchases during the period I focus on. For the

[14] 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; however, the adjustment should still broadly capture movements in the debt-to-income distribution which are driven by changes in interest rates.

Table III

High debt-to-income lending response in Fannie and Freddie's loan-level data

	$\mathbb{1}[DTI > 50]$		Log DTI	
	(1)	(2)	(3)	(4)
Post	–3.41***		–3.90***	
	(0.18)		(0.19)	
Post × Freddie		–2.63***		–2.21**
		(0.47)		(0.84)
State-Post FE		X		X
Number of Observations	304,045	309,680	304,045	309,680

Notes: Column 1 reports estimates from regressions of an indicator equal to 1 if the loan has a DTI greater than 50 per cent on an indicator equal to 1 if the loan was originated after the policy change. Column 1 includes only loans sold to Freddie Mac, and the DTI is adjusted to abstract from interest rate movements. Column 2 reports estimates from regressions of an indicator equal to 1 if the loan has a DTI greater than 50 per cent on an indicator equal to 1 if the loan was purchased by Freddie Mac interacted with indicator equal to 1 for loans originated after the policy change. Columns 3 and 4 report estimates from analogous specifications with log DTI as the dependent variable. The pre period is 1999Q2 and the post period is 1999Q3. Columns 2 and 4 only include loans with a DTI less than or equal to 64 as loans above this cutoff are coded differently by Fannie and Freddie.

years prior to 2004 over 90 per cent of these loans are included.[15] It is also possible to approximately quantify 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.

I also use both the GSE Public Use Database and HMDA to validate my conclusions about differences in Fannie and Freddie's debt-to-income policy. While these datasets provide a more comprehensive picture, they do 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 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 related.[16]

The top panel of Figure II, shows that the difference in high DTI purchases between Fannie and Freddie in the Single Family Loan-Level dataset moves very similarly to the difference in high LTI purchases observed in more comprehensive datasets. This is particularly true during the early 2000s when the Single Family Loan Performance dataset has better coverage. When constructing the chart, I aggregate the Single Family Loan Performance dataset to an annual frequency so it is comparable to the other datasets. The figure also shows that Fannie and Freddie's criteria were similar for several years prior to the 1999 change I document. The bottom panel of Figure II shows the level of Fannie and Freddie's high DTI purchase share computed using the Single Family Loan-Level dataset. This highlights the fact that, although Freddie tightened its rules, shifts in the underlying debt-to-income distribution led to an increase in high debt-to-income purchases for both Fannie and Freddie over the period I consider. This led to a larger share of borrowers falling in the region where they are affected by the policy difference, and explains why the observed gap between Fannie and Freddie's high DTI purchases widens in 2000. These movements are also quite consistent with the CoreLogic LLMA dataset, despite concerns about the representativeness of the GSE dataset.

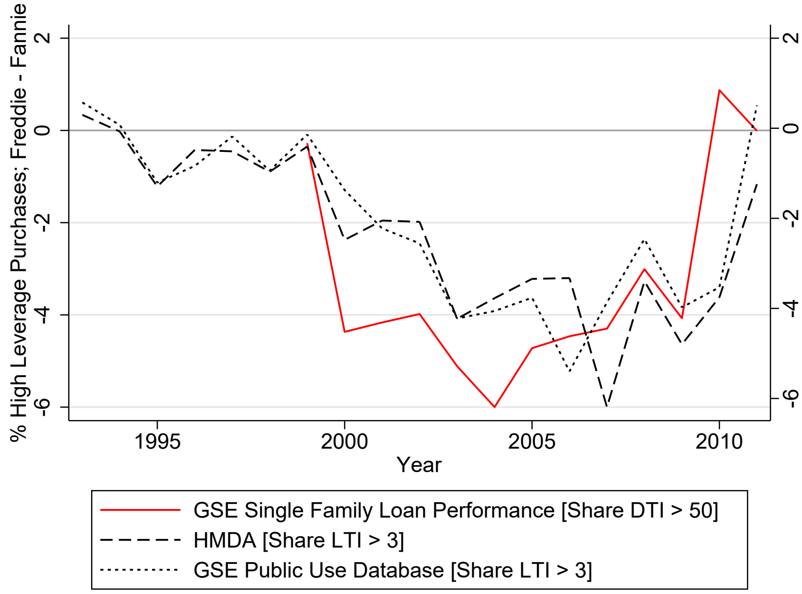
[15] Single Family Loan-Level Dataset Frequently Asked Questions (FAQs), p.3. Fannie Mae does not report similar statistics to my knowledge.

[16] 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 converts the interest rate to the monthly mortgage repayment per \$1 of loan principal.

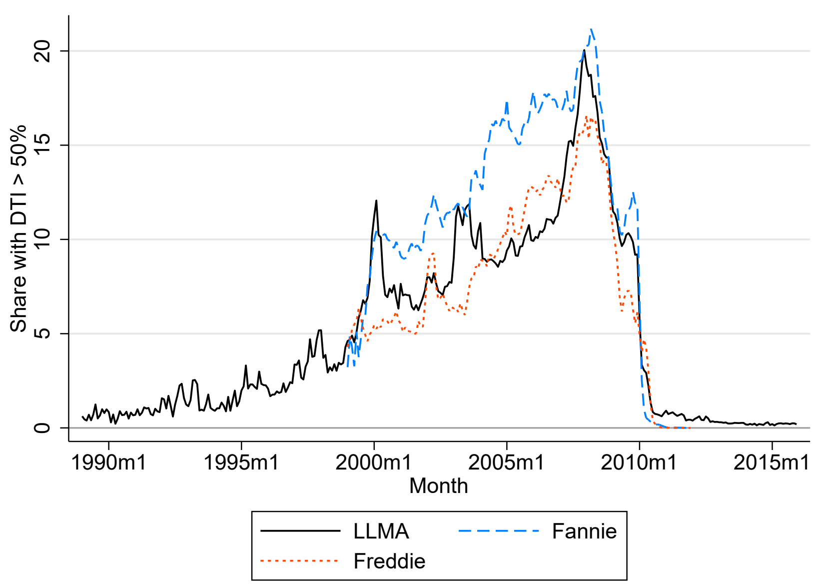
Figure II

Comparison of Freddie and Fannie's High Leverage Mortgage Purchases

A. Difference between Freddie and Fannie's high leverage purchase shares



B. Share of mortgages with a debt-to-income ratio above 50 per cent



NOTES: 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 sold to Fannie Mae or Freddie Mac. The top panel shows the difference between Freddie and Fannie's high leverage purchase shares, where high leverage is defined as $DTI > 50$ in the Single Family Loan Performance dataset, and $LTI > 3$ in HMDA and the GSE Public Use database. I aggregate the Single Family Loan Performance data to an annual frequency by averaging monthly high DTI shares. This is particularly important in 1999, as coverage increases substantially for Fannie Mae throughout the year. The bottom panel shows the share of mortgages with a DTI above 50 per cent in the Single Family Loan Performance datasets and the CoreLogic LLMA dataset. Both charts exclude loans with a DTI above 64 in the Single Family Loan Performance datasets as these are not coded consistently for Fannie and Freddie.

IV.C. Effect on county debt-to-income

It is important to verify that counties more exposed to Freddie Mac experienced a relative decline in high debt-to-income lending coinciding with the policy. It is possible that the difference between Fannie and Freddie's purchases may not translate into differences across counties, for example if borrowers or lenders substitute from one to the other. This is more challenging to test, however, as I do not observe a precise measure of location in the GSE datasets. Instead I use the CoreLogic LLMA database to construct measures of high debt-to-income lending at the county level. While this dataset has the advantage of including location and also loans not sold to Fannie or Freddie, the number of loans with non-missing DTI during the relevant period is quite small, which makes it hard to obtain precise estimates. I estimate:

$$\begin{aligned}\text{High DTI}_{c,t} = \gamma_{0,s} + \gamma_{1,s}\text{Post}_t + \beta_0\text{Freddie}_{c,1998} + \beta_1\text{Post}_t \cdot \text{Freddie}_{c,1998} \\ + \alpha_0\text{Controls}_{c,1998} + \alpha_1\text{Post}_t \cdot \text{Controls}_{c,1998} + \epsilon_{c,t}\end{aligned}$$

Where High $\text{DTI}_{c,t}$ is the share of mortgages originated in a county with a debt-to-income ratio above 50 per cent. The coefficient of interest is β_1. Post_t is an indicator equal to zero for the period 1998Q1–1999Q2 and one for the period 1999Q3–2000Q4. I use only counties located in a core-based statistical area (metropolitan or micropolitan area). I include state fixed effects and cluster by core-based statistical area.

The results are shown in Table IV. Columns 1 and 2 show the effect of Freddie exposure on the share of high DTI loans at the county level, including only counties for which I observe at least 50 loans with non-missing DTI originated between 1998 and 2000. Columns 3 and 4 show the estimates are similar without this restriction, but are less precise. The county high DTI share responds substantially following the policy change. Overall, the results suggest that the difference in Fannie and Freddie's policies translated into differences in county high debt-to-income lending.

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 E I back out this affected group and analyze the policy in more detail. I also calculate the share of purchases prior to the change where the borrower falls in the affected group backed out in Appendix E, 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 Figure I. Establishing that tighter debt-to-income rules were

Table IV

County high debt-to-income lending response: 1998 – 2000

	Counties with > 50 loans		All counties	
	(1)	(2)	(3)	(4)
Post × Freddie	–3.79***	–2.80**	–3.59**	–3.00*
	(1.21)	(1.23)	(1.59)	(1.59)
County FE	X	X	X	X
State-Post FE	X	X	X	X
Controls		X		X
Number of Counties	1,197	1,195	1,764	1,761
Number of States	50	50	50	50
Number of Observations	2,394	2,390	3,528	3,522

Notes: This table reports estimates from regressions of the share of loans originated in a county with a DTI greater than 50 per cent on Freddie Mac's 1998 market share by number of loans. Columns 1 and 2 exclude counties where DTI is observed for fewer than 50 loans between 1998 and 2000. Columns 3 and 4 report estimates using the full sample. Controls include median household income, population density, an indicator for whether the county is coastal and the market share of lenders classified as subprime by HUD. The pre period is January 1998 to June 1999 and the post period is July 1999 to December 2000. Standard errors are clustered by CBSA. I use both purchase and refinance loans.

the main difference between Fannie and Freddie during the early 2000s is important for interpreting the price results. In Appendix E 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 focus on.

V. 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, housing market segmentation and the housing supply elasticity. In this Section I measure the effect of tighter debt-to-income policy on county house prices by comparing counties with different exposure to lenders who sell to Freddie Mac. In all specifications I use a policy implementation date of June 1999, informed by the analysis in Section IV. I also use within state variation in Freddie Mac exposure. When looking at county level outcomes I condition on median household income, population density, a coastal indicator and market share of lenders classified as subprime by HUD.

V.A. Research Design

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 will face Freddie Mac's tighter rules following the policy change. I look at the effect on house prices using the following specification:

$$\Delta \log(\text{Price}_c) = \gamma_s + \beta \text{Exposure}_{c,1998} + \alpha \text{Controls}_{c,1998} + \epsilon_c \quad (5)$$

where I measure the effect over 6 months from June 1999 to December 1999 and from June 1999 to December 2002.

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.[17] To verify that the price response precisely matches the timing of the policy change, I plot the response by month using the following specification:

$$\log(\text{Price}_{c,t}) = \gamma_c + \gamma_{s,t} + \beta_t \text{Exposure}_{c,1998} + \alpha_t \text{Controls}_{c,1998} + \epsilon_{c,t} \quad (6)$$

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. In Appendix C I also show that the results are robust to using a high/low Freddie share indicator and reweighting the sample so that means of a number of other variables are equalized across the two groups.

One potential concern is that the change in debt-to-income is simply a consequence 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 I is sharp and clearly predates the price response. Furthermore, as described in Section IV, 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 E 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 focus on.

V.B. Results

Table V summarizes the main results. The first and second columns show that moving from a Freddie share of zero to one is associated with a relative decline in house prices of around 2 per cent in the 6 months following the policy change. The third column shows the main result is robust to including all lenders when computing the exposure measure. Next I examine whether the effect is larger in locations which had higher average debt-to-income ratios prior to the change. We would expect these areas to experience larger relative house price declines because a larger share of households are constrained by the

[17] Using local variation in exposure to Freddie Mac sellers is important. When using national variation there is sometimes a pre-trend depending on the sample of counties considered and weighting. This is consistent with Freddie Mac's market share being nationally correlated with the housing boom in these samples.

policy. I compute average debt-to-income ratios by MSA using Freddie Mac data for the first half of 1999. The fourth column shows the policy effect separately by debt-to-income tercile (indicators for each tercile are also included). The policy effect is higher for counties in the top tercile of MSA debt-to-income, but the coefficients are imprecisely estimated. There is also not a large amount of variation in average DTI across MSAs. The median average DTI in the bottom tercile is 31 per cent, compared with 33 per cent in the top tercile. Finally, I look at how the size of the policy effect is related to a commonly used measure of the long-run housing supply elasticity from Saiz (2010). I split the sample with non-missing elasticity into terciles and include counties with missing elasticity in a fourth group. The fifth column shows that the policy leads to a relative house price reduction in all groups, and this reduction is larger in MSAs with relatively unresponsive housing supply. Locations with missing elasticity respond similarly to locations with high elasticity.

Table VI shows the policy effect measured to December 2002. The effect is much larger over this horizon at around 8 per cent. The effect also continues to be most pronounced in locations with high debt-to-income ratios and unresponsive housing supply. The way the effect expands over time is quite striking and I provide some possible explanations for it below. Appendix Tables A.2 and A.3 show that the main results are similar when using FHFA county house price data. As these data are annual I measure the short-run effect from 1998 to 2000, and the long-run effect from 1998 to 2002. Given the gradual expansion in the size of the effect, this means the short-run effect is somewhat larger than when measured using monthly 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 IIIA plots the estimates of β_t from Equation 6 and a 95 per cent confidence interval for months close to the policy change. Supporting the channel, prices only start to diverge after Freddie Mac adjusted its underwriting criteria with respect to the debt-to-income ratio. This helps to address the concern that locations with higher Freddie Mac exposure simply experienced a smaller housing boom during the 2000s for other reasons. It is also important to point out that if the housing boom had started at the same time as the policy change the lack of pre-trend would not be as convincing. Figure A.3 in the Appendix illustrates, firstly, that the boom started well before the policy change and, secondly, that the policy effect only lines up with national movements in house prices after the policy change.

Figure IIIB 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. I

Table V

% House price response: Jun 1999 – Dec 1999; CoreLogic

	(1)	(2)	(3)	(4)	(5)
Freddie Share	−2.48***	−1.94**			
	(0.78)	(0.80)			
Freddie Share (All Loans)			−2.89**		
			(1.21)		
Freddie × Low Leverage				−1.12	
				(1.37)	
Freddie × Mid Leverage				−1.78	
				(1.72)	
Freddie × High Leverage				−2.66	
				(2.35)	
Freddie × Low Supply Elasticity					−5.00***
					(1.72)
Freddie × Mid Supply Elasticity					−0.66
					(1.30)
Freddie × High Supply Elasticity					−2.05
					(1.52)
Freddie × Missing Supply Elasticity					−1.67
					(1.10)
State FE	X	X	X	X	X
Controls		X		X	X
Number of Counties	996	996	996	754	996
Number of States	49	49	49	48	49
Number of Observations	996	996	996	754	996

Notes: This table reports estimates from regressions of the change in log county house price on Freddie Mac's 1998 market share by number of loans. Columns 1 and 2 use the preferred exposure measure which excludes loans origination by lenders with more than 20000 purchase originations in 1998. Column 3 uses an alternative exposure measure which is constructed using HMDA loans originated by all lenders. Column 4 compares the policy effect for MSAs in different debt-to-income terciles. MSA debt-to-income is constructed using both purchase and refinance loans. The housing supply elasticity measure is from Saiz (2010). I split the sample with non-missing elasticity into terciles and include counties with missing elasticity in a fourth group. Controls include median household income, population density, an indicator for whether the county is coastal and the market share of lenders classified as subprime by HUD. Standard errors clustered by CBSA.

Table VI
House price response: Jun 1999 – December 2002; CoreLogic

	(1)	(2)	(3)	(4)	(5)
Freddie Share	−8.93***	−7.79***			
	(2.72)	(2.57)			
Freddie Share (All Loans)			−12.31***		
			(4.04)		
Freddie × Low Leverage				−10.60***	
				(3.38)	
Freddie × Mid Leverage				−9.98**	
				(4.30)	
Freddie × High Leverage				−15.61*	
				(8.43)	
Freddie × Low Supply Elasticity					−16.07**
					(7.72)
Freddie × Mid Supply Elasticity					−6.37*
					(3.42)
Freddie × High Supply Elasticity					−9.10**
					(3.57)
Freddie × Missing Supply Elasticity					−4.11
					(2.69)
State FE	X	X	X	X	X
Controls		X		X	X
Number of Counties	996	996	996	754	996
Number of States	49	49	49	48	49
Number of Observations	996	996	996	754	996

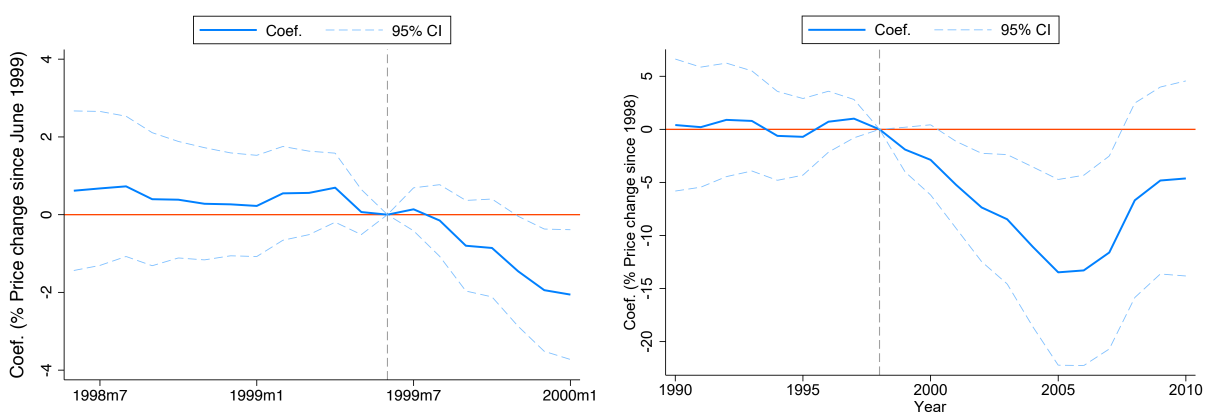
Notes: This table reports estimates from regressions of the change in log county house price on Freddie Mac's 1998 market share by number of loans. Columns 1 and 2 use the preferred exposure measure which excludes loans origination by lenders with more than 20000 purchase originations in 1998. Column 3 uses an alternative exposure measure which is constructed using HMDA loans originated by all lenders. Column 4 compares the policy effect for MSAs in different debt-to-income terciles. MSA debt-to-income is constructed using both purchase and refinance loans. The housing supply elasticity measure is from Saiz (2010). I split the sample with non-missing elasticity into terciles and include counties with missing elasticity in a fourth group. Controls include median household income, population density, an indicator for whether the county is coastal and the market share of lenders classified as subprime by HUD. Standard errors clustered by CBSA.

explore some possible explanations for these results in the next section. I also plot the coefficients excluding the top 20 MSAs by 2000 population (Figure A.4). These are similar to the main results. Figure A.5 plots the average short-run and long-run price changes within each quartile of the exposure measure and shows that both are broadly monotonic. Table A.4 looks at price growth over the 6 months prior to the policy change and shows that locations with higher Freddie Mac exposure did not experience significantly weaker price growth prior to the change.

As well as estimating the policy effect, in principle it is also possible to estimate the elasticity of house prices with respect to average DTI. In practice this is challenging given the data I have available, and I discuss it further in Appendix B. An alternative approach when extrapolating to other policy settings would be to use a simple theoretical framework to calculate a response. In Section VII I discuss a formula which can be used to compute a back-of-the-envelope price response from mortgage, income and house price statistics. In the case of the policy I consider here, it matches the data reasonably well.

Figure III
Effect of Freddie Mac's Debt-to-Income Restrictions on County House Prices

A. Short-run Response *B. Long-run Response*



NOTES: These figures show 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 median income, population density, the market share of lenders classified as subprime by HUD, and an indicator for whether the county is coastal.

V.C. Interpreting the house price response

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 natural 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 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, which could reflect households incorporating the past effect of the policy into their expectations, or some other type of feedback mechanism. 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 VII, a simple model suggests that the first explanation cannot fully account for the large effect observed by 2002, and is in line with Figure II, which shows that Fannie and Freddie's purchases of high leverage loans did not diverge much further after 2000. In Section VII I use an adaptive expectations rule, disciplined by survey estimates, to show that the long-run effect is broadly consistent with feedback to expectations.[18]

VI. Effect of DTI Rules on Default Rates

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. Leverage restrictions can also affect default rates indirectly, through their effect on house prices, or other aggregate variables. In my setting, areas

[18] One concern is that the expansion in the effect is the result of some correlation which was not relevant before mid 1999 (as there is no pre-trend in Figure IIIA) but became relevant afterwards. Focusing on how the response evolves before 2002, that is prior to the dramatic growth in private label securitization, limits the set of plausible stories. An alternative perspective is that if the increase in debt-to-income ratios was an important factor contributing to the magnitude of the entire price cycle, it is not surprising that tighter DTI policy should have the effect of dampening the entire cycle. This would naturally lead to a response profile which looks remarkably similar to the cumulative change in house prices over the same period. Indeed, to the extent that further relaxation of lending standards during the 2000s reflected past price growth feeding back to expectations, we would expect areas with less exposure to Freddie Mac to experience a larger boom and bust.

more exposed to Freddie Mac's policy experienced both a smaller boom and a smaller bust, which should reduce the share of households with negative equity in the bust.

In this section I focus on the effect through aggregate variables. I discuss the direct effect on default rates in Appendix A using Freddie Mac's Single Family Loan Level Data. There I find a small effect, consistent with other work (Foote et al. (2010), DeFusco et al. (2017)). While data limitations prevent me from directly comparing Fannie and Freddie's default rates for loans originated before and after the policy, there is no significant difference in the level of Fannie and Freddie's default rates for loans originated between 2000 and 2002.

VI.A. Research Design

I estimate the relationship between county 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. I also consider a specification with loan-level leverage and credit score controls:

$$\text{Default}_i = \gamma_{s,t} + \beta_t \text{Exposure}_{c,1998} + \alpha_{1,t} \text{Controls}_c + \alpha_{2,t} \text{Controls}_i + \epsilon_i$$

The idea is to try to separate the direct effect of leverage at the loan-level from indirect effects coming through local aggregates, such as house prices.

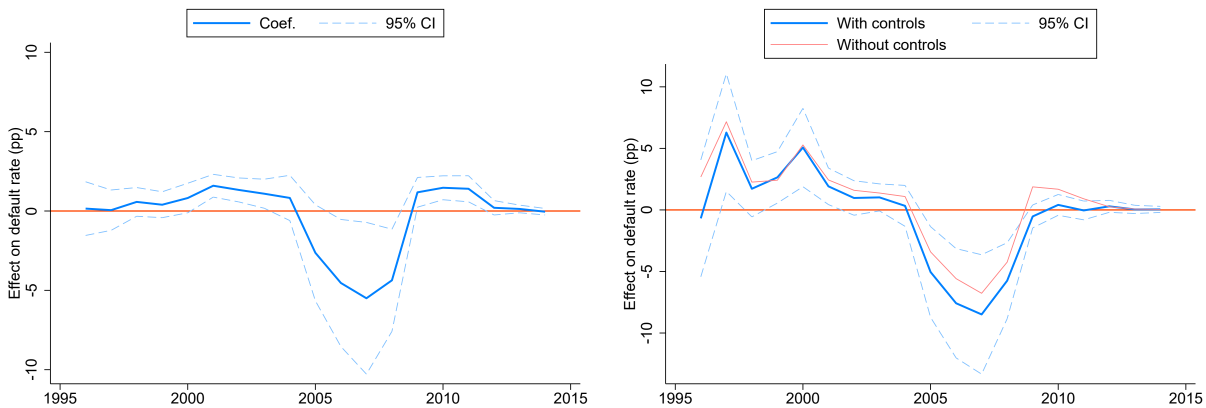
VI.B. Results

Figure IVA 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 more than 5 percentage points (or about 25 per cent) lower. I run the same regression conditioning on credit score, 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 county-level outcomes, rather than differences in leverage at origination (Figure IVB).

Figure IV

Effect of Freddie Mac's Debt-to-Income Restrictions on the Mortgage Default Rate

A. County Controls Only *B. Includes Loan-level Controls*



NOTES: Figure IVA 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 IVB 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 IVB plots the estimates without controls using the sample of loans for which all controls are non-missing. The figures are constructed using the CoreLogic LLMA Database.

VII. Theoretical Framework

In this section I describe a model of housing demand in which mortgage leverage policies affect house prices. In this setting, an approximate short-run price response can be computed directly from Fannie and Freddie's debt-to-income distributions. Although the model is static, it is still possible to calculate approximate effects at longer horizons taking into account simple feedback mechanisms. For example, as house price expectations enter parametrically, it is possible to use a back-of-the-envelope calculation to illustrate how the policy effect changes over time when expectations are a function of past price growth.

First, I show that the short-run effect estimated in Section V is broadly consistent with the direct response of constrained households in the model. Then I incorporate adaptive expectations, calibrated to match survey data, and show that this produces price responses consistent with the empirical estimates at longer horizons. 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 II and a companion paper.[19]

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). However, 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 the leverage policy to affect house prices, some form of market segmentation is required and I choose to exclude the rental market entirely for simplicity. The household's problem is to choose housing H_i and non-housing consumption C_i to maximize:

$$u(H_i, C_i) = \alpha_i \log H_i + (1 - \alpha_i) \log C_i \tag{7}$$

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

$$PH_i \leq A_i/(1 - \theta_i^{ltv}) \tag{8}$$

[19] It is also more challenging to estimate the effect of this earlier change directly. In the companion paper 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.

$$PH_i \leq \frac{(\theta_i^{dti} - \nu)y_i}{f(r) + \tau} + \frac{f(r)}{f(r) + \tau} A_i \tag{9}$$

$$0 = y_i - C_i - (r + \tau + \delta - g)PH_i \tag{10}$$

where A_i is the net assets the household is endowed with and can use for a downpayment, 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 expected house price growth, 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 leverage policy $\{\theta_i^{dti}, \theta_i^{ltv}\}$ is individual specific (reflecting the fact that DTI and LTV cutoffs are conditional on individual characteristics such as credit score). 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 D. The price effect of a debt-to-income policy change is similar under certain conditions which are described further in the Appendix and Section VII.D..

I assume Cobb-Douglas preferences. This assumption is used by other authors in the housing literature (for example Eeckhout (2004), Davis and Ortalo-Magne (2011), Michaels et al. (2012), Berger et al. (2018)), and is broadly supported by empirical evidence. Davis and Ortalo-Magne (2011) find that housing expenditure shares are constant both over time and across locations. Piazzesi et al. (2007) also find relatively little variation in the housing expenditure share and Albouy et al. (2016) find that housing demand is price inelastic, but that the elasticity is still fairly close to one.

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 the dynamic model I outline in Appendix D.[20] 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

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

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.[21]

VII.A. Approximate price response

In the model, the direct effect of a leverage policy on house prices is summarized by the unconstrained debt-to-income distribution (which reflects the distribution of housing preferences and the joint distribution of assets and income). Below, I show that it is possible to compute an approximate short-run price response using the pre-policy debt-to-income distribution observed in the mortgage data, the share of borrowers affected by the policy, and the share of owners with a mortgage.[22] I show that this is consistent with the short-run price response I observe in the data.

On top of this, there are a number of possible feedback mechanisms which could amplify the effect on house prices. For example, assets, income and expectations may diverge across locations as a result of the policy. These enter as parameters in the static framework. Given the simplicity of the model I do not formally analyze these channels; however, I do compute an approximate long-run price response assuming an adaptive expectations rule calibrated to match survey evidence. Under this assumption the user cost becomes a function of past price growth and I simply update the user cost each year to reflect the price effect in previous years. With adaptive expectations the policy triggers a (temporary) divergence in user costs, and therefore demand, across locations which can lead the policy effect to grow for some time following implementation before reversing. This channel generates a similar response to what I observe in the data.

Direct effect from constrained borrowers

I derive the approximate price response to a change in leverage policy as a function of quantities observed in the data. First, the debt-to-income ratio of household i is by definition:

[21] An accurate user cost calculation would also incorporate 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. (see 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.

[22] This response is approximately the same as what is obtained by choosing distributions for α_i and A_i and values of r, ν, τ and δ which match moments from the SCF, computing g as described below, and then computing housing demand under the initial and final policies.

$$DTI_i = \frac{f(r_i)\max\{P \cdot H_i - A_i, 0\}}{y_i} + \nu_i$$

Assuming that a household above the imposed cutoff θ_i^{dti} responds by cutting loan size (i.e. holding income and the available downpayment fixed), the implied change in their nominal housing demand is:

$$P \cdot H_{i,new} - P \cdot H_i = \frac{y_i}{f(r_i)}\left(\theta_i^{dti} - DTI_i\right)$$

If only constrained households adjust their housing demand, the total percentage reduction in nominal housing demand is:

$$\frac{\sum_i (P \cdot H_{i,new} - P \cdot H_i)}{\sum_i P \cdot H_i} = \frac{1}{\sum_i P \cdot H_i} \sum_i \frac{y_i}{f(r_i)} \max\{\theta_i^{dti} - DTI_i, 0\} \tag{11}$$

All borrowers face the same DTI cutoff conditional on being affected, $\theta_i^{dti} = 0.5$. Under the simplifying assumptions that income and DTI are uncorrelated, and all borrowers face the same interest rate:[23]

$$\%\Delta(P \cdot H) = P(\text{constrained by DTI}) \frac{\bar{y}}{f(r)P\bar{H}} \left(0.5 - \frac{\sum_i DTI_i \mathbb{1}[DTI_i > 0.5]}{\sum_i \mathbb{1}[DTI_i > 0.5]}\right) \tag{12}$$

Where $P(\text{constrained by DTI})$ is the share of households constrained by the policy, $\frac{\bar{y}}{f(r)P\bar{H}}$ is the debt-to-income ratio for a household with average income, buying a house at the average price with no downpayment and no other financial obligations, and $\frac{\sum_i DTI_i \mathbb{1}[DTI_i > 0.5]}{\sum_i \mathbb{1}[DTI_i > 0.5]}$ is the average pre-policy debt-to-income ratio for constrained households.

[23]In practice, subject to qualifying for a conforming loan borrowers faced similar rates at this time (the interquartile range in Freddie Mac's dataset is typically 25–50 basis points, which is less than 10 per cent of the average rate). Conditional on having a mortgage, unconstrained DTI and income in the model are related in the following way: $DTI_i = \frac{\alpha_i f(r)}{r+\tau+\delta-g} - f(r)\frac{A_i}{y_i} + \nu$. That is, the relationship between DTI_i and y_i depends on how assets scale with income. If, for example, $A_i = \kappa y_i$, DTI_i and y_i will be unrelated. If, on the other hand, DTI_i and y_i are negatively (positively) correlated, high debt-to-income households will account for a smaller (larger) share of nominal housing demand. This means prices will be less (more) responsive than implied by Equation 12. In any case, the correspondence between Equation 12 and county house prices depends on how the index is constructed. The house price index I use is value-weighted; however, I also report results using the FHFA house price index which does not assign a higher weight to more expensive properties.

Effect on unconstrained households

The policy may also affect the decisions of households who are not directly constrained. Empirical evidence suggests that households have adaptive expectations about house prices (Armona et al. (2018); Case et al. (2012)). In this case, the policy change generates feedback to expectations through its effect on the history of price growth. In the context of the natural experiment the policy may cause expected price growth g to diverge across locations with different exposure to the initial change in credit conditions. This leads to further divergence in prices as the user cost is a function of g.

I provide a back-of-the-envelope calculation showing how the price difference between affected and unaffected locations changes over time when households update their expectations adaptively using a rule calibrated to match survey evidence. The nominal housing demand of an unconstrained household is:

$$P \cdot H_i = \frac{\alpha_i y_i}{r + \tau + \delta - g} \tag{13}$$

So when the user cost $r + \tau + \delta - g$ increases by 1 per cent, nominal housing demand declines by approximately 1 per cent. Using annual county house price data I compute a backward looking measure of expected house price growth g:

$$g = \frac{\lambda}{1 - (1-\lambda)^{t+1-t_0}} \sum_{j=0}^{t-t_0} (1-\lambda)^j g_{t-j} \tag{14}$$

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.[24] I set $\lambda = 0.11$ to match survey evidence on the relationship between house price expectations and lagged house price growth (Case et al., 2012).[25] Using this formula it is also straightforward 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:[26]

[24] 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.

[25] Specifically, this value of λ generates an estimated coefficient of 0.23 when regressing expected house price growth on lagged house price growth, which matches Case et al. (2012) and is similar to Armona et al. (2018). I use FHFA house price data for the counties considered by Case et al. (2012) and the same sample period, which is 2003 – 2012.

[26] Strictly speaking, Equations 14 and 15 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.

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

Here, the policy effect in the initial period reflects only the direct effect given by Equation 12. In subsequent periods it also incorporates the effect on the user cost. After computing the difference in the user cost implied by different values for g, the approximate effect on nominal housing demand in a given year is:

$$\begin{aligned} \%\Delta(P \cdot H) = & -P(\text{responds to user cost change}) \cdot \%\Delta\text{user cost} \\ & + P(\text{constrained by DTI}) \cdot \frac{\bar{y}}{f(r)P\bar{H}} \left(0.5 - \frac{\sum_i DTI_i \mathbb{1}[DTI_i > 0.5]}{\sum_i \mathbb{1}[DTI_i > 0.5]}\right) \end{aligned} \tag{16}$$

I compute the annual average DTI conditional on being above the 50 per cent cutoff using Fannie Mae's DTI distribution. The difference in user cost does not affect everyone, as nominal housing demand will not change for households who are constrained by the leverage policy at both the initial and final values of the user cost. It is unclear exactly how to measure this in the data. One approach is to use the share constrained by the DTI policy or with a high LTV.[27]

Housing supply

Converting the effect on nominal housing demand to a price response requires an assumption about the housing supply response. Assuming a constant housing supply elasticity of ϵ means that an approximate house price response is obtained by dividing the nominal housing demand response from 12 and 16 by $1 + \epsilon$.[28]

VII.B. Comparison with empirical results

When computing the model effect I assume the share of borrowers constrained by DTI is equal to the difference between Fannie and Freddie's high DTI shares. This is around 6 per cent in the last two months of 1999 and increases slightly after that.[29] To calculate

[27] In practice it is hard to measure the share of LTV-constrained households in the data. Households are observed bunching at commonly used LTV cutoffs of 80, 85, 90, 95 and 97 per cent. These households may well have been able to make a larger downpayment, or qualify at a higher LTV, but chose not to (for example, in order to take advantage of better terms available at a lower LTV).

[28] $\%\Delta PH \approx \%\Delta P + \%\Delta H = (1+\epsilon)\%\Delta P$.

[29] The Fannie Mae dataset has very low coverage prior to this date.

the share of households constrained by DTI I multiply by the share of recent homebuyers in the Survey of Consumer Finances who have a mortgage.[30] This gives a value for the share of DTI constrained households of around 5 per cent.

Computing the effect with feedback to unconstrained households requires a value for the share of households who respond to a change in the user cost. I conservatively assume that LTV constrained borrowers are all those located at LTV values greater or equal to 80 per cent where bunching occurs (i.e. 80, 85, 90, 95, and 97). This is around two-thirds of borrowers in the GSE dataset (and this group also includes most of those constrained by the DTI policy). Adding DTI constrained borrowers not already included and multiplying by the share of recent homebuyers with a mortgage, gives a responsive share of around 45 per cent.

Table VII shows the approximate theoretical price effect of the policy change for different horizons and housing supply elasticities. The first column of Table VII shows the short-run effect. The immediate price decline of 1.5 per cent, assuming housing supply does not respond, is similar to the empirical estimate of 1.9 per cent shown in the bottom row. At 2 per cent the price difference implied by the model for 2002 is larger, even without any type of feedback to unconstrained households. This reflects the fact that the average debt-to-income ratio was higher than in 1999, and a larger proportion of households falls into the region affected by the policy. Intuitively, a constant upper limit on the debt-to-income ratio tends to exert more downward pressure on prices in boom, when it is more binding. This channel alone cannot explain the way the empirical effect builds over time, however. Even assuming no supply response, the effect is still substantially smaller than the 7.8 per cent estimated in Section V.[31]

Something else is needed to explain the empirical response. In the third column of Table VII, I allow expected house price growth to vary with the policy according to Equation 16, generating an effect in 2002 of up to 7.7 per cent depending on the housing supply elasticity. Assuming a fairly limited supply response consistent with the analysis in Appendix F gives effects broadly consistent with the data at both horizons. This point

[30] As the sample size is not very large I use data from all the surveys between 1995 and 2010. I restrict attention to households who purchased in the two years prior to the survey. This makes sense given that the mortgage data is restricted to newly originated purchase mortgages. Focusing on recent buyers in the calibration partly reflects the available information, but it may also make sense for other reasons. Given the costs associated with moving (and the relatively modest price changes involved) it may be reasonable to assume no demand response for households who would not have moved in the absence of the policy.

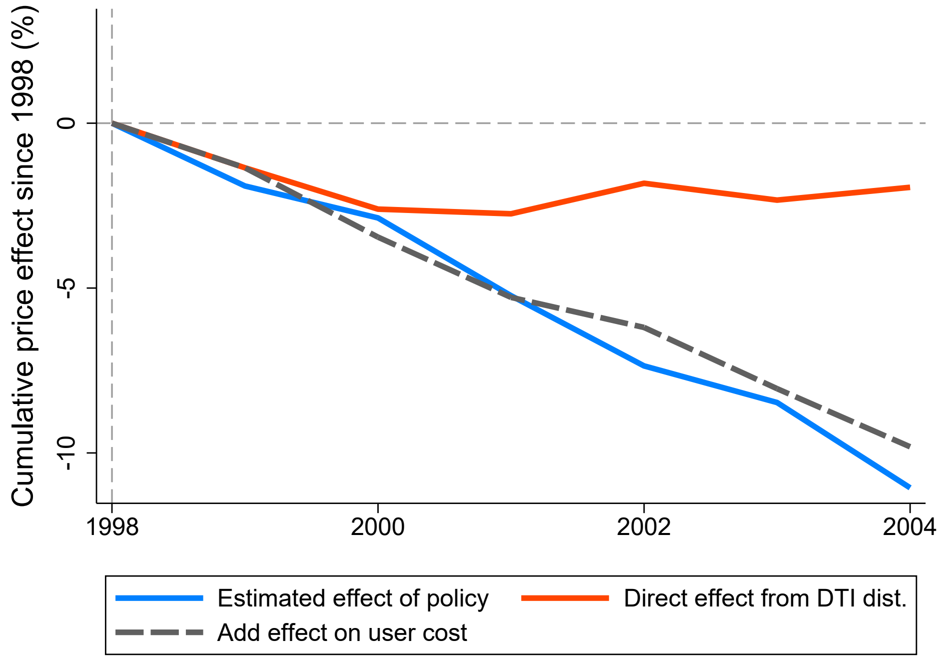
[31] The supply elasticity could also increase with the horizon. However, in Appendix F I measure the response of residential building permits empirically and find a very modest response. This could be because the policy was introduced at a time when housing supply was already growing strongly for other reasons.

TABLE VII
EMPIRICAL AND THEORETICAL PRICE RESPONSES

	% Price effect of Freddie policy change		
	1999	2002	
		Direct	+ User cost
Supply elasticity = 0	−1.5	−2.0	−7.7
Supply elasticity = 0.1	−1.4	−1.8	−6.2
Supply elasticity = 0.25	−1.2	−1.6	−4.7
Supply elasticity = 0.5	−1.0	−1.3	−3.3
Supply elasticity = 1	−0.7	−1.0	−2.0
Data	−1.9		−7.8

NOTES: This table shows the cumulative percentage house price response computed using the theoretical framework in Section VII. The final row shows the county house price response estimated in Section V.

Figure V
Empirical and Model Price Response



NOTES: This figure compares the cumulative percentage house price response computed using the theoretical framework in Section VII with the county house price response estimated in Section V.

is also illustrated graphically in Figure V, which assumes a housing supply elasticity of 0.1. While there are other possible explanations for the increase in the effect over time, this exercise illustrates that the empirical response is reasonable.

VII.C. Effect of 1990s debt-to-income relaxation

In addition to interpreting and checking the plausibility of the empirical results, the framework can be used to analyze additional policies. 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 an approximate theoretical effect of this relaxation, and find that it can explain a sizable share of the housing boom. I assume the policy change is a move from a debt-to-income limit of 36 per cent to a policy consistent with the GSEs' 1999 purchases (the first year for which GSE data with the debt-to-income ratio is available). So the final policy in this second experiment corresponds to the initial policy in the main experiment.[32]

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. Figure VIA shows the adoption timing. In a companion paper 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 compute a theoretical response. When computing the price response in this case I assume the new policy only applies to the share of loans for which the software was used in that year.

Figure VIB shows the house price response to the DTI relaxation assuming only constrained borrowers change their housing demand. With fixed housing supply and assumed peak adoption of 75 per cent, the policy raises house prices by around 8 per cent. Even without additional feedback channels, the policy can explain a large share of the early part of the housing boom, up to three quarters of growth from 1995 to 2000 depending on the housing supply elasticity. The price response also matches the timing of the housing boom well. Assuming that the ratio of the total response to the theoretical response from constrained borrowers is the same as in the main experiment, the true response may have been over 30 per cent (though given gradual adoption in this case, the timing of the response is likely to have been different). 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.

VII.D. Other approaches

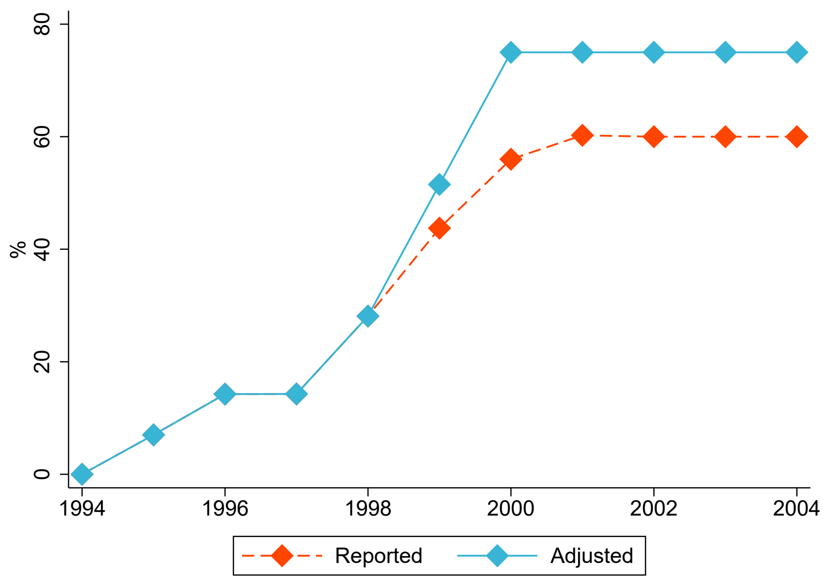
Given that liquidity constrained households are the focus of much of the existing work relating mortgage leverage constraints to house prices (e.g. Justiniano et al. (2016); Greenwald (2016), Iacoviello (2005)), it is useful to discuss the conditions under which a static model can capture the effect 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

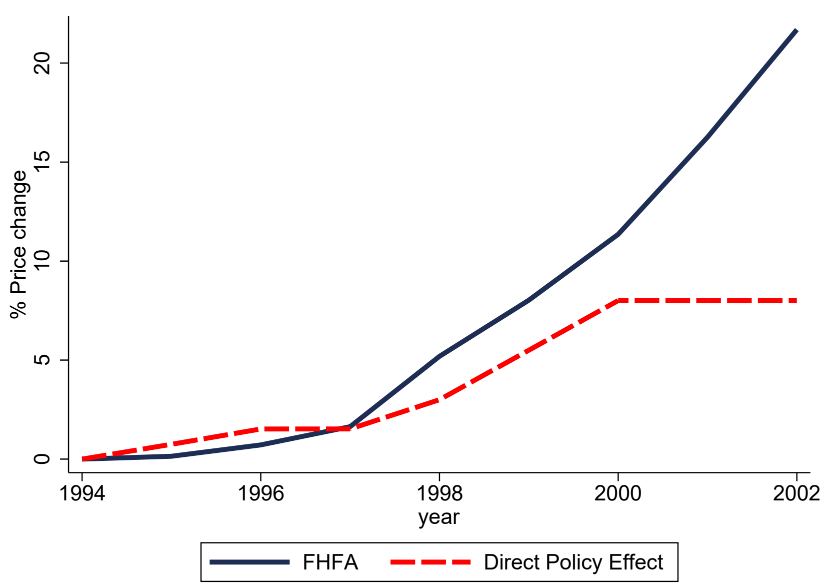
[32] Although it is possible that the GSEs had borrower specific debt-to-income limits below 65 per cent even after the relaxation, this is implicitly accounted for to some extent because the model calculations are based on the observed 1999 debt-to-income distribution for GSE purchases.

Figure VI
How Did Relaxing Fannie and Freddie's 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. Effect of Relaxing 36 per cent DTI Limit and Observed House Price Growth



NOTES: Figure VIA is constructed using statistics from GSE 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. Actual Desktop Underwriter and Loan Prospector usage plateaued at a lower level than the GSEs had anticipated following agreements they made to purchase loans underwritten using other software. Since the GSE monitored these loans for deviations relative to their own software, and the loans seem to have had similar characteristics, I use a higher adjusted rate of 75 per cent consistent with the GSEs earlier forecasts. Figure VIB plots the real FHFA cumulative house price change since 1994 alongside the price response to the software's relaxed DTI rules computed using the model in Section VII.

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 D 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.[33]

In Appendix D I discuss some other ways in which the dynamic response is different. These include 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, and in particular an intuitive, transparent expression for the house price response due to adjustments by constrained borrowers. This expression can be used to compute approximate responses with a small number of statistics from mortgage and survey data.

VIII. 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 V, 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 county-level factors (such as a smaller price cycle) rather than

[33]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. Intuitively, it is very costly for liquidity constrained households to have assets tied up in home equity. When starting out with assets such that they are constrained by debt-to-income, reducing housing demand to the point where they are on the loan-to-value constraint frees up a lot of assets for current non-housing consumption and reduces housing consumption by a relatively small amount. When the loan-to-value limit is relaxed, the level of housing associated with this kink point declines. In contrast, the static model always generates a positive effect because at every level of assets, the maximum feasible housing is weakly greater.

differences in individual mortgage characteristics.

This tension between individual and macroeconomic implications 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. While this change may have had small effects on individual default risk, my results suggest that it did lead to a large increase in house prices. This could be particularly concerning if the response is partly temporary and prices eventually decline, behavior which could arise as the result of adaptive expectations.[34]

IX. Conclusion

I show that adjusting mortgage debt-to-income limits has a large effect on house prices, and that the effect continues to grow over 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 directly explain the rapid house price growth that occurred after 2003.

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

References

Acharya, V., Bergant, K., Crosignani, M., Eisert, T., and McCann, F. (2018). The Anatomy of the Transmission of Macroprudential Policies. Unpublished.

Adelino, M., Schoar, A., and Severino, F. (2014). Credit Supply and House Prices: Evidence from Mortgage Market Segmentation. NBER Working Paper.

Adelino, M., Schoar, A., and Severino, F. (2016). Loan Originations and Defaults in the Mortgage Crisis: The Role of the Middle Class . Review of Financial Studies, 29:1635–1670.

Albouy, D., Ehrlich, G., and Liu, Y. (2016). Housing demand, cost-of-living inequality, and the affordability crisis. NBER Working Paper, (22816).

American Banker (1997). Fannie Mae Releases Upgraded Software. American Banker, page 8.

Anenberg, E., Hizmo, A., Kung, E., and Molloy, R. (2019). Measuring Mortgage Credit Availability: A Frontier Estimation Approach. Journal of Applied Econometrics (Forthcoming).

Armona, L., Fuster, A., and Zafar, B. (2018). Home Price Expectations and Behavior: Evidence from a Randomized Information Experiment. Review of Economic Studies (Forthcoming).

Barberis, N., Greenwood, R., Jin, L., and Shleifer, A. (2018). Extrapolation and bubbles. Journal of Financial Economics, 129(2):203–227.

Barta, P., Zuckerman, G., and Schroeder, M. (2000). As Agencies' Portfolios Swell, Worry on Risks Also Rises. The Wall Street Journal, page A28.

Berger, D., Guerrieri, V., Lorenzoni, G., and Vavra, J. (2018). House Prices and Consumer Spending. Review of Economic Studies, 85:1502–1542.

Bolotnyy, V. (2013). The Government-Sponsored Enterprises and the Mortgage Crisis: The Role of the Affordable Housing Goals. Real Estate Economics, 41(3):724–755.

Bordalo, P., Gennaioli, N., and Shleifer, A. (2018). Diagnostic expectations and credit cycles. Journal of Finance.

Calem, P., Covas, F., and Wu, J. (2013). The Impact of the 2007 Liquidity Shock on Bank Jumbo Mortgage Lending. Journal of Money, Credit and Banking, 45:59–91.

Campbell, J. and Cocco, J. (2015). A Model of Mortgage Default. Journal of Finance, 70(4).

Case, K., Shiller, R., and Thompson, A. (2012). What Have they been Thinking? Home Buyer Behavior in Hot and Cold Markets. NBER Working Paper No. 18400.

Cocco, J. (2005). Portfolio Choice in the Presence of Housing. The Review of Financial Studies, 18(2):535–567.

Cocheo, S. (1995). Automated Underwriting: Tower of Techno-Babel? ABA Banking Journal, 87:68.

Corbae, D. and Quintin, E. (2015). Leverage and the Foreclosure Crisis. Journal of Political Economy, 123:1–65.

Costanzo, C. (2004). Automated Underwriting is now Business as Usual. Community Banker, 13:49–50.

Davis, M. A. and Ortalo-Magne, F. (2011). Household expenditures, wages, rents. Review of Economic Dynamics, 14:248–261.

DeFusco, A., Johnson, S., and Mondragon, J. (2017). Regulating Household Leverage. Unpublished.

DeMuth, J. (1999). The selling of two systems. Mortgage Banking, 59:16.

DiMaggio, M. and Kermani, A. (2017). Credit-Induced Boom and Bust. Review of Financial Studies, 30(11):3711–3758.

Eeckhout, J. (2004). Gibrat's Law for (All) Cities. American Economic Review, 94(5):1429–1451.

Favara, G. and Imbs, J. (2015). Credit Supply and the Price of Housing. American Economic Review, 105(3):958–992.

Favilukis, J., Ludvigson, S., and Van Nieuwerburgh, S. (2016). The Macroeconomic Effects of Housing Wealth, Housing Finance, and Limited Risk Sharing in General Equilibrium. Journal of Political Economy, 125(1).

FHFA (2019). An Overview of Enterprise Debt-to-Income Ratios . White Paper.

Fieldhouse, A., Mertens, K., and Ravn, M. O. (2018). The Macroeconomic Effects of Government Asset Purchases: Evidence from Postware U.S. Housing Credit Policy. Quarterly Journal of Economics, 133(3):1503–1560.

Foote, C., Gerardi, K., Goette, L., and Willen, P. (2010). Reducing foreclosures: No easy answers. In NBER Macroeconomics Annual 2009, Volume 24, pages 89–138. University of Chicago Press.

Foote, C., Loewenstein, L., and Willen, P. (2016). Cross-Sectional Patterns of Mortgage Debt during the Housing Boom: Evidence and Implications. NBER Working Paper.

Foote, C., Loewenstein, L., and Willen, P. (2018). Technological Innovation in Mortgage Underwriting and the Growth in Credit: 1985-2015 . Federal Reserve Bank of Cleveland Working Paper No. 18-16.

Foster, D. (1997). The debate over automated underwriting. Mortgage Banking, 57:10–15.

Ghent, A., Hernandez-Murillo, R., and Owyang, T. (2015). Did Affordable Housing Legislation Contribute to the Subprime Securities Boom? Real Estate Economics, 43(4):820–854.

Glaeser, E. L., Gottlieb, J. D., and Gyourko, J. (2013). Can Cheap Credit Explain the Housing Boom? In Glaeser, E. L. and Sinai, T., editors, Housing and the Financial Crisis. NBER.

Greenwald, D. (2016). The Mortgage Credit Channel of Macroeconomic Transmission. Working Paper.

Hainmueller, J. (2012). Entropy balancing for causal effects: A multivariate reweighting method to produce balanced samples in observational studies. Political Analysis, 20:25–46.

Iacoviello, M. (2005). House Prices, Borrowing Constraints, and Monetary Policy in the Business Cycle.. American Economic Review, 95(3):739–764.

Iacoviello, M. and Neri, S. (2010). Housing Market Spillovers: Evidence from an Estimated DSGE Model. American Economic Journal: Macroeconomics, 2:125–164.

Igan, D. and Kang, H. (2011). Do Loan-to-Value and Debt-to-Income Limits Work? Evidence from Korea. IMF Working Paper.

Johnson, S. (2018). Are Mortgage Regulations Affecting Entrepreneurship? Unpublished.

Justiniano, A., Primiceri, G., and Tambalotti, A. (2015). Credit Supply and the Housing Boom. Federal Reserve Bank of New York Staff Reports.

Justiniano, A., Primiceri, G., and Tambalotti, A. (2016). Household Leveraging and Deleveraging. Review of Economic Dynamics, 18(1):3–20.

Kaplan, G., Mitman, K., and Violante, G. L. (2017). The Housing Boom and Bust: Model Meets Evidence. Unpublished.

Kuttner, K. and Shim, I. (2016). Can Non-Interest Rate Policies Stabilize Housing Markets? Evidence from a Panel of 57 Economies. Journal of Financial Stability, 26:31–44.

LaMalfa, T. (1999). Wholesale giants 1998. Mortgage Banking, 59.

LaMalfa, T. (2000). Wholesale giants 1999. Mortgage Banking, 60:70.

Loutskina, E. and Strahan, P. (2009). Securitization and the Declining Impact of Bank Finance on Loan Supply: Evidence from Mortgage Originations. Journal of Finance, 64(2).

Markus, M., Dutta, A., Steinfield, C., and Wigand, R. (2008). The Computerization Movement In The US Home Mortgage Industry: Automated Underwriting From 1980 To 2004. In Computerization Movements and Technology Diffusion: From Mainframes to Ubiquitous Computing, pages 115–144. Information Today.

Maselli, P. (1994). Mortgages in Minutes. Mortgage Banking, 55(1):102.

McDonald, D. W., Pepe, C. O., Bowers, H. M., and Dombroski, E. J. (1997). Desktop Underwriter: Fannie Mae's Automated Mortgage Underwriting Expert System. In for Artificial Intelligence, A. A., editor, Innovative Applications of Artificial Intelligence – Conference Proceedings. American Association for Artificial Intelligence.

Mian, A. and Sufi, A. (2009). The Consequences of Mortgage Credit Expansion: Evidence from the U.S. Mortgage Default Crisis. The Quarterly Journal of Economics, 124(4):1449–1496.

Mian, A. and Sufi, A. (2017). Household Debt and Defaults from 2000 to 2010: The Credit Supply View. In Fennell, L. and Keys, B., editors, Evidence and Innovation in Housing Law and Policy, pages 257–288. Cambridge University Press.

Michaels, G., Ferdinand, R., and Stephen, R. (2012). Household expenditures, wages, rents. The Quarterly Journal of Economics, 127:535–586.

Morse, N. (2002). Crowding Out in the Nonconforming Market? Mortgage Banking, 62(9):80–87.

Muolo, P. (1996). Automated Analysis Ramps Up. US Banker, 106:55.

Piazzesi, M., Schneider, M., and Tuzel, S. (2007). Housing, consumption and asset pricing. Journal of Financial Economics, 83:531–569.

Pinto, E. (2011). Government Housing Policies in the Lead-up to the Financial Crisis: A Forensic Study . American Enterprise Institute Discussion Paper.

Saiz, A. (2010). The Geographic Determinants of Housing Supply. Quarterly Journal of Economics, 125(3):1253–1296.

Slemrod, J. (1982). Down-Payment Constraints: Tax Policy Effects in a Growing Economy with Rental and Owner-Occupied Housing. Public Finance Quarterly, 10(2):193–217.

Stein, J. (1995). Prices and Trading Volume in the Housing Market: A Model with Down-Payment Effects. The Quarterly Journal of Economics, 110(2):379–406.

Straka, J. W. (2000). A Shift in the Mortgage Landscape: The 1990s Move to Automated Credit Evaluations. Journal of Housing Research, 11(2):207–232.

Van Order, Robert & Thomas, J. (2010). Housing Policy, Subprime Markets and Fannie Mae and Freddie Mac: What We Know, What We Think We Know and What We Don't Know. Unpublished.

Wallison, P. (2015). Hidden in Plain Sight. Encounter Books.

Online Appendices

A. Effect of the policy on Default Rates

A.1 Research Design

A.1.1 Effect through individual leverage

In this section I measure the direct effect of the policy on default at the individual level. I find, firstly, that the policy targeted a set of borrowers with above-average default rates. This is relevant for understanding the policy motivation. However, given the small share of the market affected, the default rates in the targeted group were not large enough to generate a meaningful decline in the aggregate default rate. Consequently the policy did not directly translate into significant differences in loan performance between Fannie and Freddie.

As there is limited performance information for loans purchased by Fannie Mae prior to the policy change, I use a back-of-the-envelope calculation to translate the DTI-default relationship for Freddie Mac into an aggregate default rate effect. The default effect of the policy at the individual level depends on the relationship between debt-to-income and default prior to the policy change:

$$\text{Default}_i = \gamma_t + \kappa_z + \beta_1 \mathbb{1}[DTI_i \in (10, 15]] + \beta_2 \mathbb{1}[DTI_i \in (15, 20]] + ... + \epsilon_i$$

as well as the change in default in high-DTI bins induced by removing borrowers who are high-risk along other dimensions:

$$\begin{aligned}\text{Default}_i = \gamma_t + \kappa_z + \beta_1 \mathbb{1}[DTI_i \in (10, 15]] + \beta_2 \mathbb{1}[DTI_i \in (15, 20]] + ...\\ + \alpha_1 \mathbb{1}[DTI_i \in (10, 15]] \cdot \text{Post}_t + \alpha_2 \mathbb{1}[DTI_i \in (15, 20]] \cdot \text{Post}_t + ... + \epsilon_i\end{aligned}$$

Where 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 and Post_t is an indicator equal to 1 if the loan was originated within the six months following the change (and 0 if it was originated in the six months prior to the change). I also include time and three-digit Zip Code fixed effects (γ_t and κ_z). The (45,50] DTI bin is omitted and loans with a DTI less than 10 are dropped. The back-of-the-envelope calculation assumes that all loans moving out of the region affected by the policy disappear entirely. In practice this is an upper bound on the default effect, as given the estimated relationship between DTI and default, shifting loans to just below the cutoff generates a smaller reduction in the default rate than removing them entirely.

I denote the default rate of the affected loans in DTI bin d by $\tilde{\theta}_d$. θ_d denotes the default rate in bin d before the policy and $\hat{\theta}_d$ denote the default rate in bin d after the policy. $\bar{\theta}$ denotes the default rate across all DTI bins. s_d is the share of loans in bin d before the policy and $\hat{s}_d$ is the share of loans in bin d after the policy. Assuming that all the affected loans drop out, the change in the default rate is:

$$\Delta\bar{\theta} = (\hat{\theta}_L \hat{s}_L + \hat{\theta}_H \hat{s}_H) - (\theta_L s_L + \theta_H s_H)$$

Assuming the affected loans have the same default rate as others in the same bin, this simplifies to:

$$\Delta\bar{\theta} = (\theta_H - \theta_L) \cdot (\hat{s}_H - s_H) \tag{1}$$

However, because the policy removes loans based on characteristics such as credit score, it is likely that the default rate of the affected loans is different from remaining loans in the same DTI bin. In this case:

$$\Delta\bar{\theta} = (\theta_H - \theta_L) \cdot (\hat{s}_H - s_H) + \hat{s}_H \cdot (\hat{\theta}_H - \theta_H) \tag{2}$$

I estimate $\theta_H - \theta_L$ and $\hat{\theta}_H - \theta_H$ using the following specification:

$$\text{Default}_i = \gamma_t + \kappa_z + \beta \mathbb{1}[DTI > 50] + \alpha \mathbb{1}[DTI > 50] \cdot \text{Post}_t + \epsilon_i$$

and substitute $\hat{\beta}$ for $\theta_H - \theta_L$ and $\hat{\alpha}$ for $\hat{\theta}_H - \theta_H$ in Equation 2. I compute $\hat{s}_H$ directly using loans purchased by Freddie Mac in the second half of 1999.

A.2 Results

Figure A.6A plots the relationship between DTI and default for loans purchased by Freddie Mac prior to the policy change. While loans with a DTI above 50 per cent have a default rate around 2 percentage points higher than loans with a DTI of 20 per cent, the DTI-default relationship is fairly flat once DTI exceeds 40 per cent[1]. This means that if most of the response to the policy occurred along the intensive margin (i.e. through reduced loan size rather than number of loans) the effect on the default rate would be

[1] The default rate in the top bin is significantly higher, but includes all loans with a DTI above 65 per cent due to top-coding. The overall share of loans in this bin is also small.

negligible. This is consistent with other work examining the relationship between DTI and default (Foote et al. (2010), DeFusco et al. (2017)).

Next, I compute the direct effect on default under the assumption that all loans affected by the policy were no longer made. I estimate $\hat{\beta} = 0.91$ and $\hat{\alpha} = -0.61$. I compute $\hat{s}_H = 0.055$ and take $\hat{s}_H - s_H = -0.034$ from Table III. The average pre-policy default rate is 2.3 per cent. Using Equation 2 gives an effect of 0.065 percentage points, or 2.8 per cent. Even assuming a purely extensive margin response, the direct effect of the policy on default is small. Using Equation 1 gives an effect of 0.031 percentage points, implying that around half of the effect is due to the application of the policy to higher-risk borrower types. Figure A.6B shows how the default rate in each bin changes after the policy. There is a relative drop in defaults for loans originated with a DTI above 50 percent. There is no obvious spike in relative default rates immediately below the cutoff, which is consistent with the response occurring primarily along the extensive margin. Overall, the policy removes loans with a higher than average default rate, but given the share of the market affected, the default reduction is small.[2]

B. House price elasticity with respect to DTI

In addition to estimating the policy effect, in principle it is also possible to estimate the elasticity of house prices with respect to average DTI. However, this is challenging in practice because of the limited number of originations per county in the LLMA dataset. Instead, I use the DTI response in the GSE loan-level data to compute the elasticity. This approach assumes that the policy passes through fully, and so the computed elasticity will likely be too small. Furthermore, because the GSE loan-level data represent a subset not only of the mortgage market, but also of the loans purchased by Fannie and Freddie the results should still be interpreted cautiously. I obtain an elasticity of about 0.5 by dividing the estimated short-run price response to the policy of -1.9 per cent (Column 2 of Table V), by the effect of the policy on the average DTI of -3.9 per cent (Column 3 of Table III). The elasticity increases to 2 when using the price response by December 2002 of 7.8 per cent.

[2] In particular, the reduction is small relative to month-to-month fluctuations in the default rate, and there is no obvious visual or statistically significant difference in average default rates for loans purchased by Fannie and Freddie from 2000 – 2002 (when clustering by three-digit Zip Code).

C. Reweighted House Price Results

As I discuss in Section III, counties with different Freddie Mac market shares also differ along some other dimensions. This raises the concern that house prices would have moved differently across these areas in the absence of the policy change. In the main analysis I address this by conditioning on a number of county characteristics, using within state variation in exposure and by demonstrating that house prices do not diverge significantly across areas with different exposure prior to the policy change.

Here, I show that the results are also robust to reweighting in order to achieve identical means of several variables in high and low exposure samples. I divide the sample into terciles of the Freddie Mac share and repeat the analysis of Section V using a binary exposure measure equal to one for the top tercile and zero for the bottom tercile. The mean Freddie Mac share in the bottom tercile is 0.28 and the mean share in the top tercile is 0.64. I construct weights so that the weighted means of the following variables are equal in the high and low exposure samples: median household income, average loan-to-income ratio (winsorized at 99.5%), market share of subprime lenders, population density (winsorized at 95%), market share of thrifts, share of loans sold to Fannie or Freddie, coastal indicator, % of population in rural areas, underserved indicator. The reweighting approach is described in Hainmueller (2012).

Table A.5 shows the short-run results. The short-run price effect in the weighted sample is about 0.7 per cent. Given the difference in the average Freddie share between the two groups of around $\frac{1}{3}$, this is similar to the main results, and if anything somewhat larger. Table A.6 shows that the cumulative response by 2002 is around five times larger than the short-run response. This is also consistent with the main results.

D. 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 VII 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.

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 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.[3] 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 by directly matching the debt-to-income distribution in the data. While the preference distribution in the static model may therefore not be interpretable in a structural sense, the model can still produce an accurate price response.

D.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:

[3] National Association of Realtors.

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

Note that the home equity component of the available downpayment 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:

$$\max_{\{h_{i,t},c_{i,t}\}_{t=0}^{T}} \sum_{t=0}^{T} \beta^t u(h_{i,t}, c_{i,t}) + \beta^{T+1} W(A_{i,T}) \tag{3}$$

where:

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

subject to:

$$c_{i,t} + a_{i,t} + p_t h_{i,t} \leq y_{i,t} + (1 + r_t)a_{i,t-1} + (1 - \delta - \tau)p_t h_{i,t-1} \tag{4}$$

$$p_t h_{i,t} \leq \overline{ph}_{i,t} = \min\left\{ \frac{A_{i,t-1}}{(1 - \theta^{ltv})}, \frac{(\theta^{dti} - \nu)y_i}{f(r_{t+1}) + \tau} + \frac{f(r_{t+1})}{f(r_{t+1}) + \tau} A_{i,t-1} \right\} \tag{5}$$

$$a_{i,t} \geq -\min\left\{ \theta^{ltv} p_t h_{i,t}, \frac{(\theta^{dti} - \nu)y_{i,t}}{f(r_{t+1}) + \tau} \right\} \tag{6}$$

$$E_t[p_{t+1}] = p_t(1 + g_t) \tag{7}$$

$$g_t = f(g_{t-1}, g_{t-2}, ..., g_0) \tag{8}$$

Equation 4 is the period t budget constraint with multiplier λ_t. Equation 5 is the period t downpayment constraint with multiplier $\lambda_t \gamma_t$. Equation 6 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 5 as downpayment constrained and households constrained by 6 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 6 is the current price, the household cannot borrow against expected capital gains. The first order conditions are:

$$\frac{(1-\alpha_i)}{c_{i,t}} = \lambda_{i,t} \tag{9}$$

$$\begin{aligned}\frac{\alpha_i}{h_{i,t}} &= \lambda_{i,t}p_t - E_t\lambda_{i,t+1}p_{t+1}(1-\delta-\tau)\\ &- E_t[\lambda_{i,t+1}\gamma_{i,t+1}1_{LTV}\frac{p_t}{1-\theta^{ltv}} + \lambda_{i,t+1}\gamma_{i,t+1}1_{DTI}\frac{f(r_{t+1})p_t}{f(r_{t+1})+\tau}] - 1_{LTV}\lambda_{i,t}\mu_{i,t}\theta^{ltv}p_t\end{aligned} \tag{10}$$

$$\lambda_{i,t}\mu_{i,t} = \lambda_{i,t} - E_t\lambda_{i,t+1}(1+r_{t+1}) - E_t[\lambda_{i,t+1}\gamma_{i,t+1}1_{LTV}\frac{1}{1-\theta^{ltv}} + \lambda_{i,t+1}\gamma_{i,t+1}1_{DTI}\frac{f(r_{t+1})}{f(r_{t+1})+\tau}] \tag{11}$$

Dividing Equation 10 by $\lambda_{i,t}$.

$$\begin{aligned}\frac{\alpha_i c_{i,t}}{(1-\alpha_i)h_{i,t}} &= p_t\Big(1 - E_t[\gamma_{i,t+1}1_{LTV}\frac{1}{1-\theta^{ltv}} + \gamma_{i,t+1}1_{DTI}\frac{f(r_{t+1})}{f(r_{t+1})+\tau}] - 1_{LTV}\mu_{i,t}\theta^{ltv}\Big)\\ &- E_t\frac{\lambda_{i,t+1}}{\lambda_{i,t}}p_{t+1}(1-\delta-\tau)\end{aligned} \tag{12}$$

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

$$\begin{aligned}\frac{\alpha_i c_{i,t}}{(1-\alpha_i)h_{i,t}} = p_t\Big(1 &- (1+g_t)E_t\frac{\lambda_{i,t+1}}{\lambda_{i,t}} + (1+g_t)(\delta+\tau)E_t\frac{\lambda_{i,t+1}}{\lambda_{i,t}}\\ &- E_t[\gamma_{i,t+1}1_{LTV}\frac{1}{1-\theta^{ltv}} + \gamma_{i,t+1}1_{DTI}\frac{f(r_{t+1})}{f(r_{t+1})+\tau}]\\ &- 1_{LTV}\mu_{i,t}\theta^{ltv}\Big)\end{aligned} \tag{13}$$

D.2 *Neither downpayment nor liquidity constrained households*

If a household is neither downpayment nor liquidity constrained, Equation 11 implies that $E_t\frac{\lambda_{t+1}}{\lambda_t} = \frac{1}{1+r_{t+1}}$. Rewriting 13 for these households gives:

$$\frac{\alpha_i c_{i,t}}{(1-\alpha_i)h_{i,t}} = p_t\left(\frac{r_{t+1} - g_t + (1+g_t)(\delta+\tau)}{1+r_{t+1}}\right) \tag{14}$$

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):

$$\frac{\alpha_i c_{i,t}}{(1-\alpha_i)h_{i,t}} = p_t\left(\frac{r_{t+1} + \delta + \tau - g_t}{1+r_{t+1}}\right) \tag{15}$$

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

$$c_{i,t} = (1-\alpha_i)\omega_{i,t} \tag{16}$$

and:

$$p_t h_{i,t} = \frac{\alpha_i \omega_{i,t}(1+r_{t+1})}{r_{t+1} + \delta + \tau - g_t} \tag{17}$$

This is analogous to Equation 13 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.

D.3 Downpayment constrained households

Next I consider households who are downpayment constrained but not liquidity constrained. For these households Equation 11 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 17 implies that the downpayment constraint is violated then housing demand is given by:

$$p_t h_{i,t} = \overline{ph}_{i,t} = \min\{\frac{A_{i,t-1}}{(1-\theta^{ltv})}, \frac{(\theta^{dti} - \nu)y_{i,t}}{f(r_{t+1}) + \tau} + \frac{f(r_{t+1})}{f(r_{t+1}) + \tau}A_{i,t-1}\} \tag{18}$$

This is analogous to Equation 13 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

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

D.4 Liquidity constrained households

Next consider households who are liquidity constrained but not downpayment constrained. In this case Equation 11 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:

$$\frac{\alpha_i c_{i,t}}{(1-\alpha_i)h_{i,t}} = p_t \left(1 - (1+g_t)\frac{1-\mu_{i,t}}{1+r_{t+1}} + (1+g_t)(\delta+\tau)\frac{1-\mu_{i,t}}{1+r_{t+1}} - 1_{LTV}\mu_{i,t}\theta^{ltv} \right) \quad (19)$$

Simplifying:

$$\frac{\alpha_i c_{i,t}}{(1-\alpha_i)h_{i,t}} = p_t \left(\frac{r_{t+1} - g_t(1-\mu_{i,t}) + (1+g_t)(1-\mu_{i,t})(\delta+\tau) + \mu_{i,t} - 1_{LTV}\mu_{i,t}\theta^{ltv}}{1+r_{t+1}} \right) \quad (20)$$

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

$$\frac{\alpha_i c_{i,t}}{(1-\alpha_i)h_{i,t}} = p_t \left(\frac{r_{t+1} + (\delta+\tau-g_t)(1-\mu_{i,t}) + \mu_{i,t} - 1_{LTV}\mu_{i,t}\theta^{ltv}}{1+r_{t+1}} \right) \quad (21)$$

The numerator on the RHS of Equation 21 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:

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

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:

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

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

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

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:

$$p_th_{i,t} = \frac{\alpha_i\omega_{i,t}(1+r_{t+1})}{r_{t+1}+(\delta+\tau-g_t)(1-\mu_{i,t})+\mu_{i,t}-1_{LTV}\mu_{i,t}\theta^{ltv}} \tag{22}$$

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.

E. Additional Policy Documentation

E.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 A.7 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 A.7 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 of the response occurs between 1999 and 2002, 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 V is unlikely to reflect later policy changes.

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

Figure A.7 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 VII. 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. 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, though the classification does not change much with small adjustments of the cutoff. Figure A.8 shows which bins are classified as affected for four different time periods. Figure A.9 shows how the relative ratio varies with credit score and loan-to-value. Panel A of Figure A.9 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. Looking at Figure A.9 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 A.10 compares the debt-to-income distributions for Freddie Mac loans classified as affected or unaffected with comparable loans purchased by Fannie Mae during 2000.

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 A.9 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 A.8 and A.9 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. By 2006 the 1999 policy has been largely reversed, though a small proportion of borrowers are still affected by the 50 per cent limit.

E.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 A.11 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.

F. Housing Supply Response

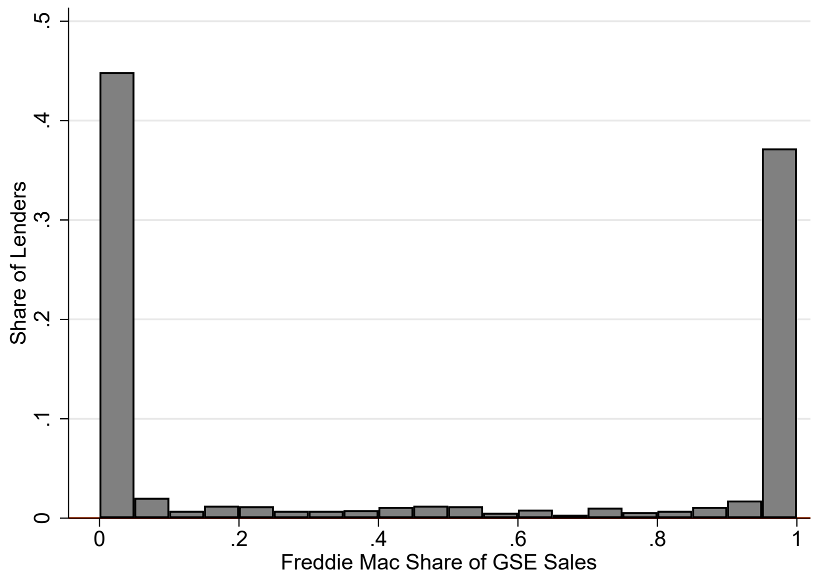
In this section I show that housing supply did not respond strongly to the change in Freddie Mac's debt-to-income rules. This supports my assumption of very inelastic housing supply in Section VII. I examine the effect of the policy change on building permits issued for new housing units using the Building 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 V:

$$\Delta \log(\text{Housing Units}_c) = \gamma_s + \beta \text{Exposure}_{c,1998} + \alpha \text{Controls}_{c,1998} + \epsilon_c \tag{23}$$

where Housing $\text{Units}_{c,t}$ is the imputed number of housing units in county c at time t. This is constructed by assuming that each building permit is issued in county c in year t translates into an additional housing unit in that year, and taking the 2000 number of housing units from the census. The response is reported in Tables A.7 and A.8 and varies depending on the type of area. Column 1 in each table shows estimates for the full sample of counties. Column 2 shows 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 an 0.36 per cent (relative) reduction in housing supply by the end of 1999. In the full sample the response is 0.02 per cent and is insignificant. Table A.8 shows that the response by 2002 is larger. For micropolitan counties there is a relative reduction in housing units of 1.3 per cent. In the full sample the reduction is 0.24 per cent and is insignificant.

Overall, housing supply is fairly unresponsive during the period immediately following the policy change. 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.

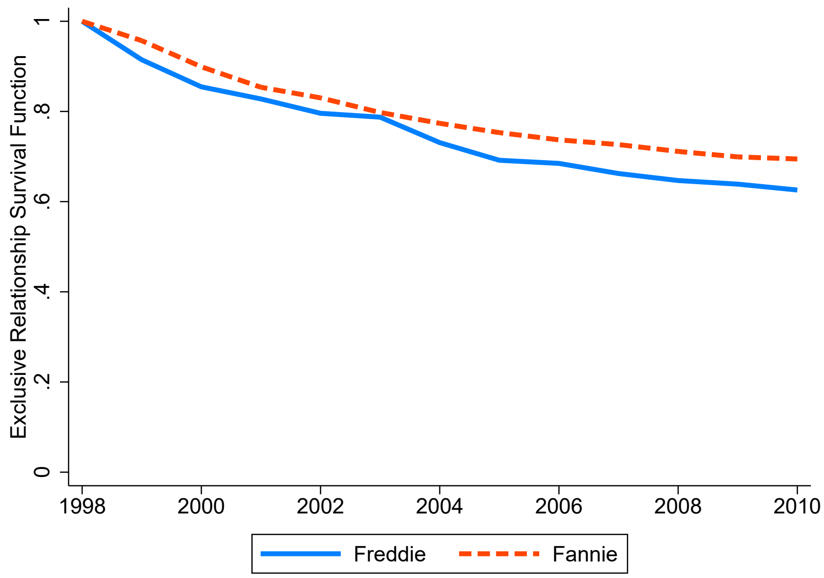
A.1
Freddie Mac Share of Lenders' GSE Sales in 1998



NOTES: This figure is constructed using HMDA loans sold to Fannie Mae or Freddie Mac in the calendar year of origination. Lenders who did not sell to either GSE and lenders with $<$ 10 originations are excluded. Each observation corresponds to a single lender ID.

A.2

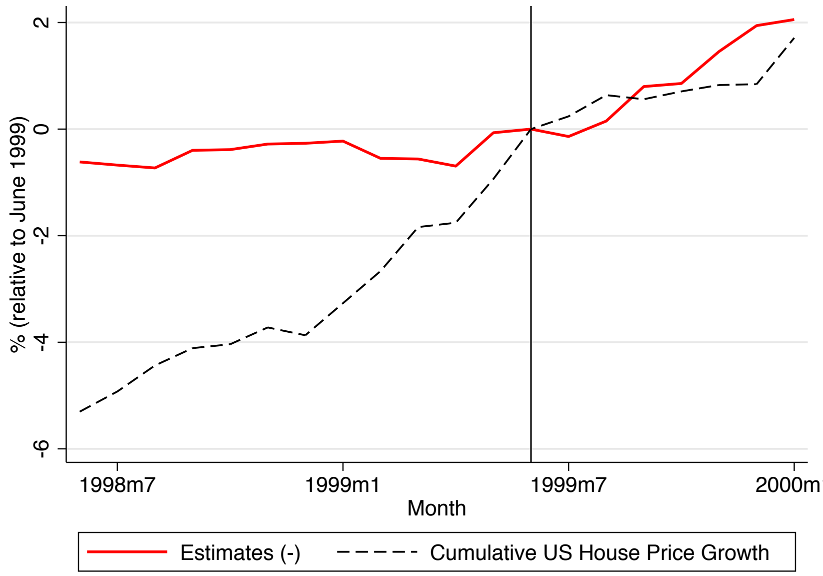
Survival of 1998 Lender Exclusive Relationships with Fannie Mae or Freddie Mac



NOTES: This figure is constructed using HMDA loans sold to Fannie Mae or Freddie Mac in the calendar year of origination. Lenders who did not sell to either GSE and lenders with $<$ 10 originations are excluded. Each observation corresponds to a single lender ID.

A.3

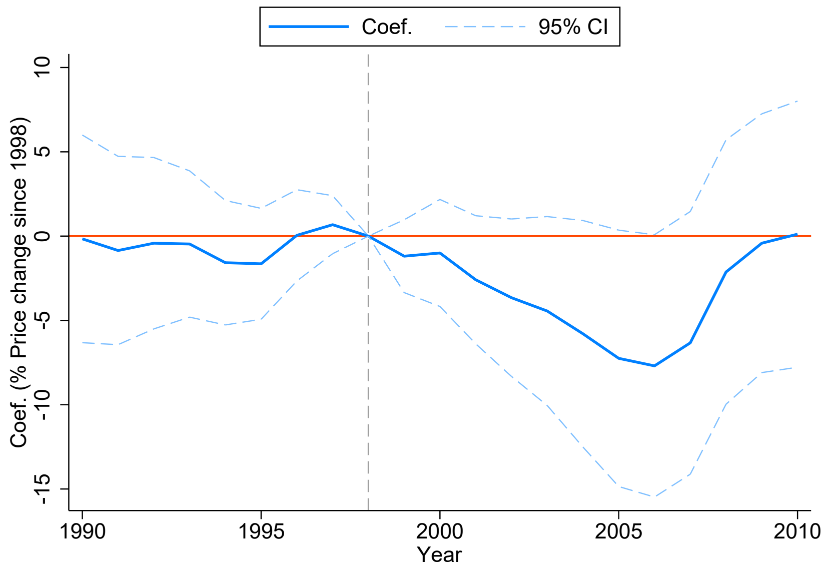
Comparison of Estimated Policy Effect and US House Price Movements



Notes: This figure shows 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 regression is unweighted and I condition on median income, population density, the market share of lenders classified as subprime by HUD, and an indicator for whether the county is coastal. The dashed line shows cumulative US house price growth relative to June 1999 measured using the CoreLogic national house price index.

A.4

Effect of Freddie Mac's Debt-to-Income Restrictions on County House Prices; Excludes Top 20 CBSAs

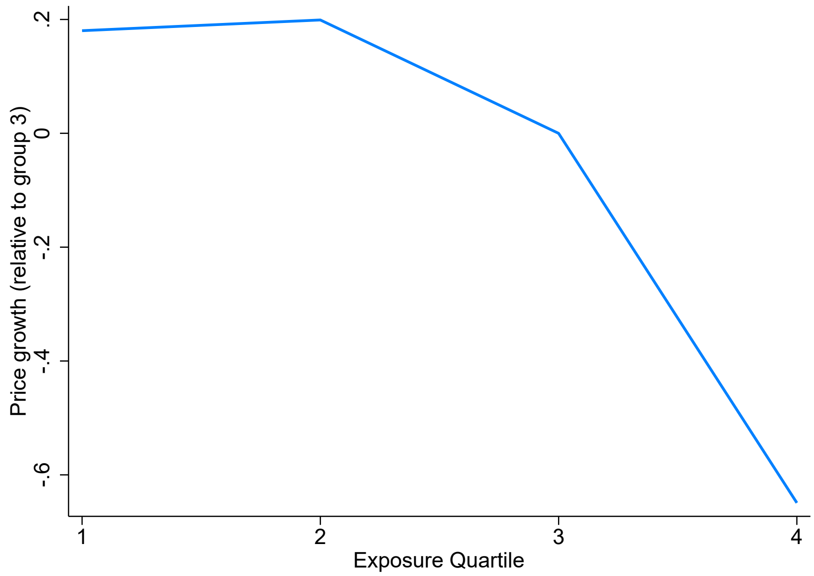


Notes: This figure shows estimates from $\log \text{Price}_{c,t} = \gamma_c + \gamma_{s,t} + \beta_t \text{Exposure}_{c,1998} + \alpha_t \text{Controls}_{c,1998} + \epsilon_{c,t}$, where June 1999 is the base month. Standard errors are clustered by CBSA. The regressions are unweighted and I condition on median income, population density, the market share of lenders classified as subprime by HUD, and an indicator for whether the county is coastal. I exclude counties in the top 20 CBSAs by 2000 population.

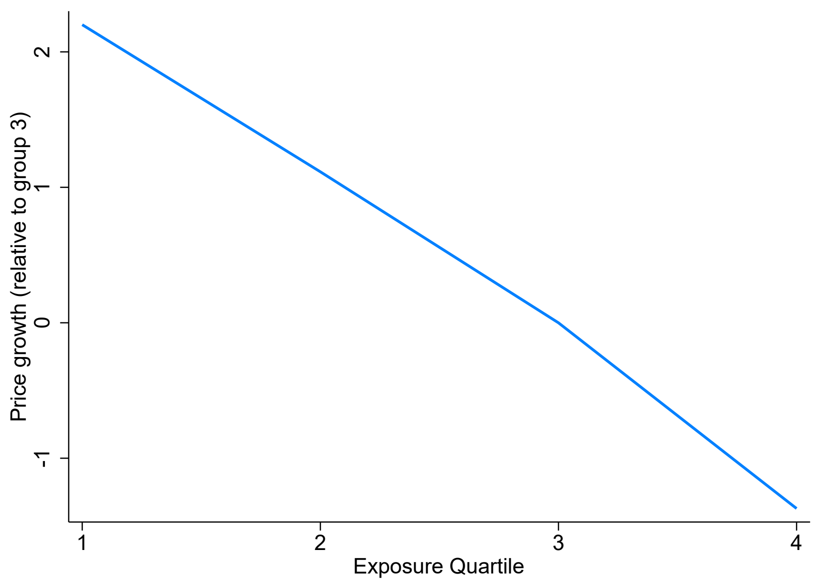
A.5

County House Price Growth by Quartile of Freddie Mac Share

A.



B.

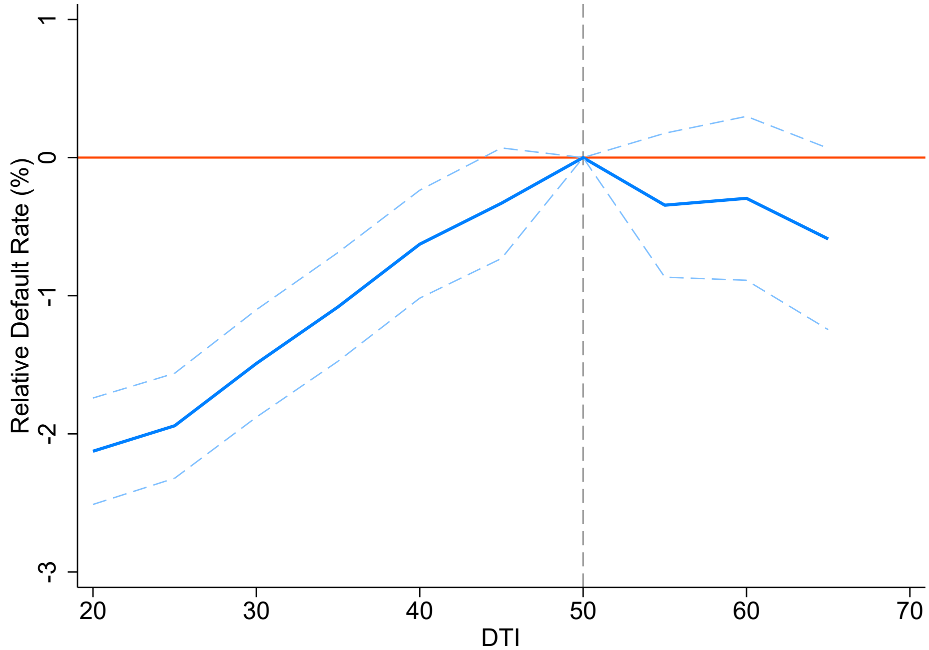


NOTES: These figures show estimates of coefficients on the exposure quartiles from $\Delta \log(\text{Price})_c = \gamma_s + \sum_{q \neq 3} \beta_q \mathbb{1}[q\text{th exposure quartile}]_c + \alpha \text{Controls}_c + \epsilon_c$, where county c is located in state s. The regressions are unweighted and I condition on median income, population density, the market share of lenders classified as subprime by HUD, and an indicator for whether the county is coastal.

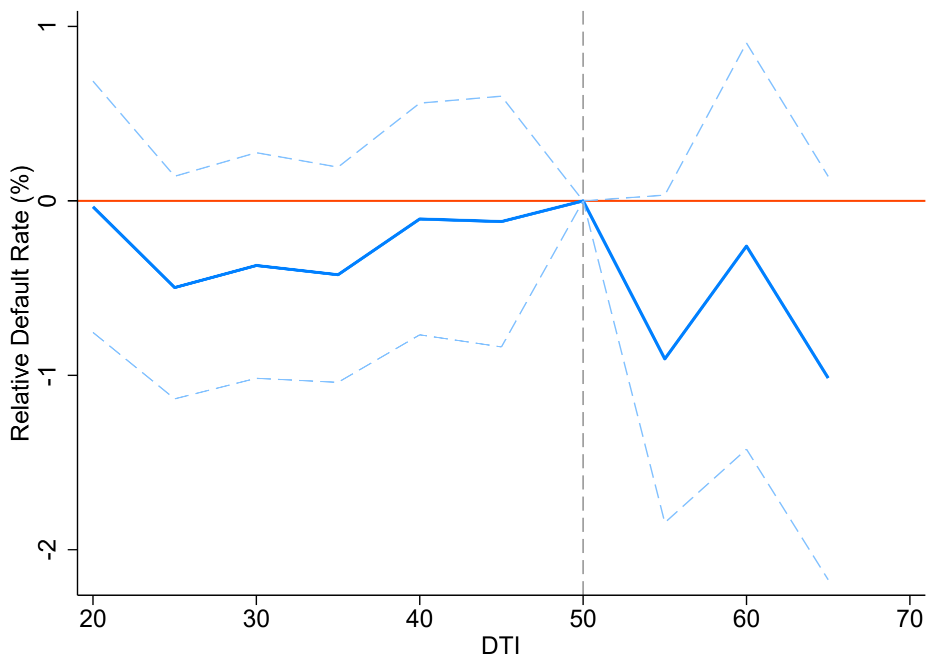
A.6

Effect on 5-year default rate

A. Freddie Mac relative five-year default rate by DTI bin; 1999H1



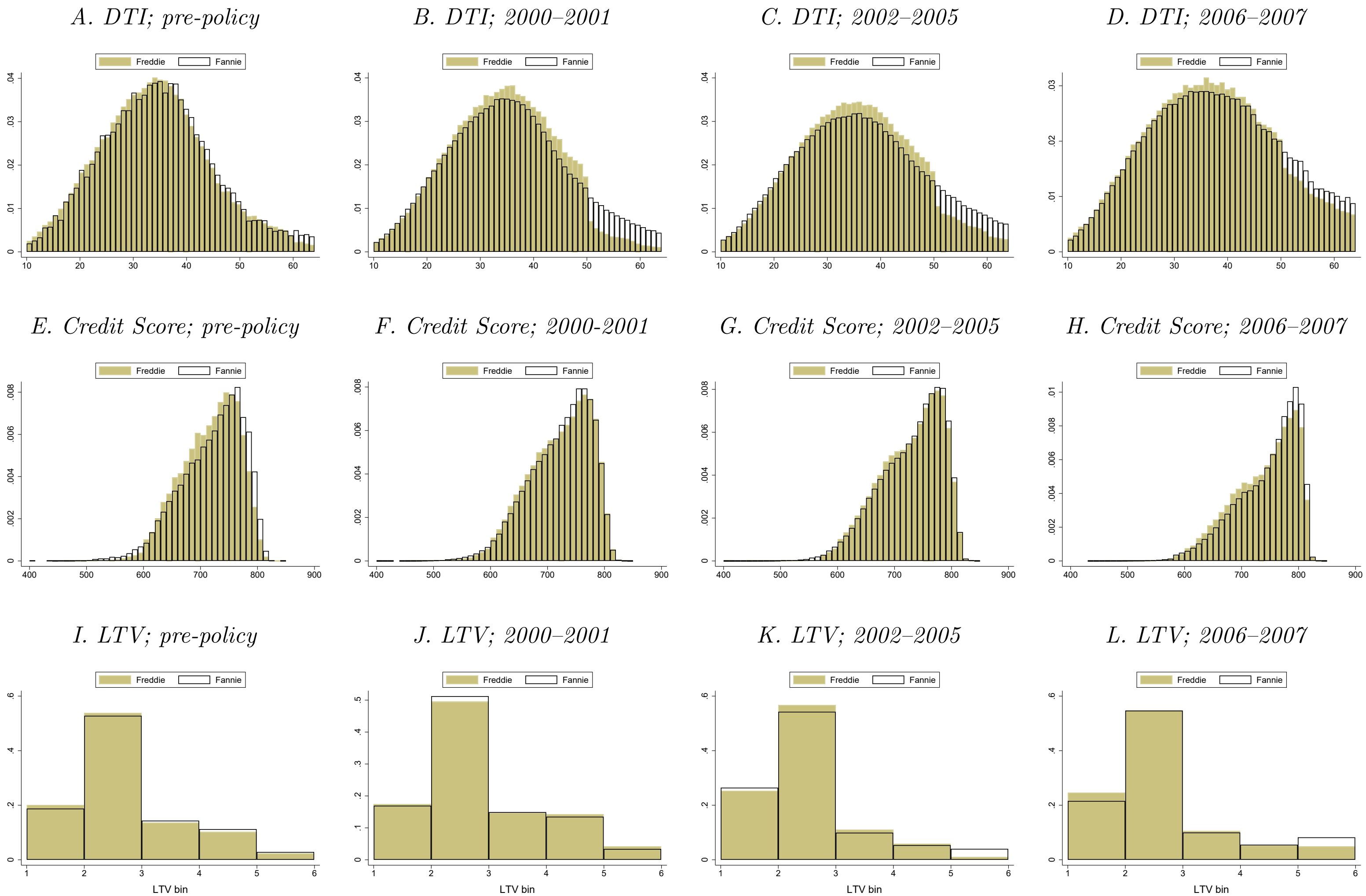
B. Freddie Mac relative five-year default rate by DTI bin; difference between 1999H2 and 1999H1



NOTES: Both figures are constructed using Freddie Mac's Single Family Loan Performance Data. Default is defined as 90+ days past due or worse and is measured over the five years following origination.

A.7

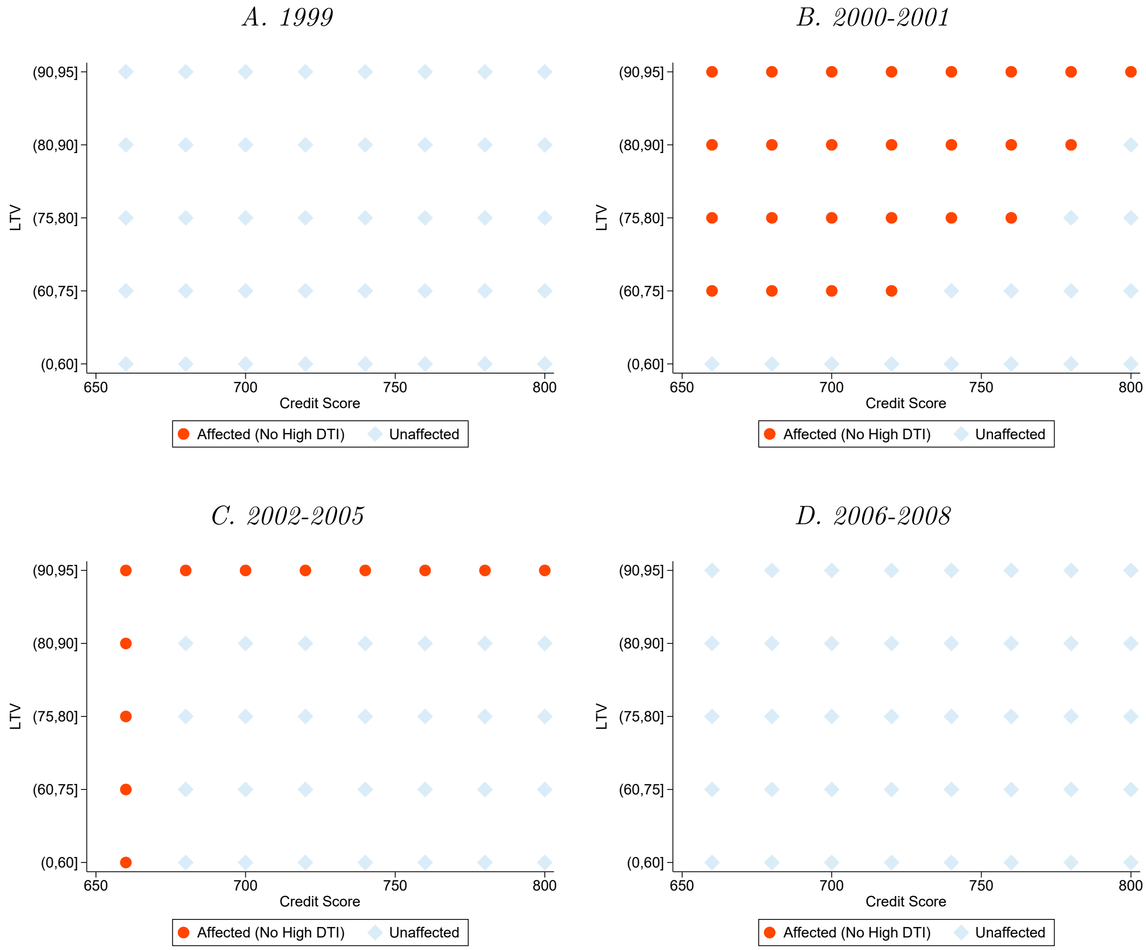
Characteristics of GSE purchases



NOTES: This figure uses Fannie and Freddie's 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.

A.8

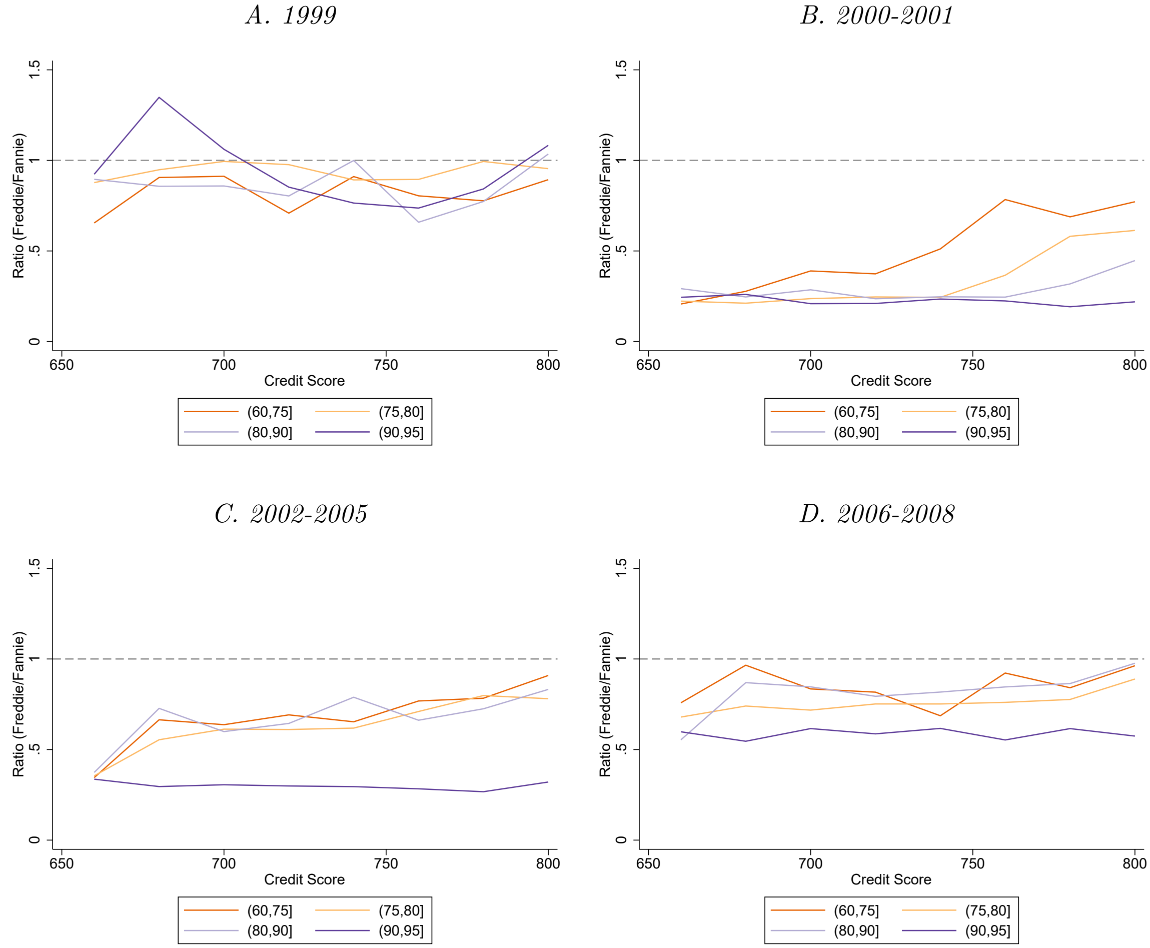
Which LTV by credit score groups are not allowed to have DTI > 50?



NOTES: This figure uses the GSE Single Family Loan Performance Datasets. Shows whether a given credit score × 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 E.

A.9

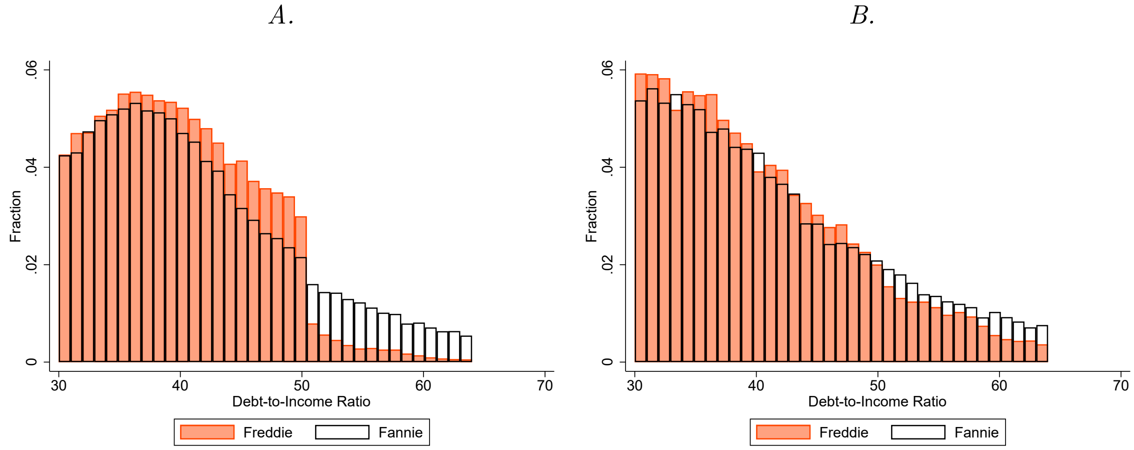
Ratio of Freddie to Fannie DTI > 50% Share



Notes: This figure uses the 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 E.

A.10

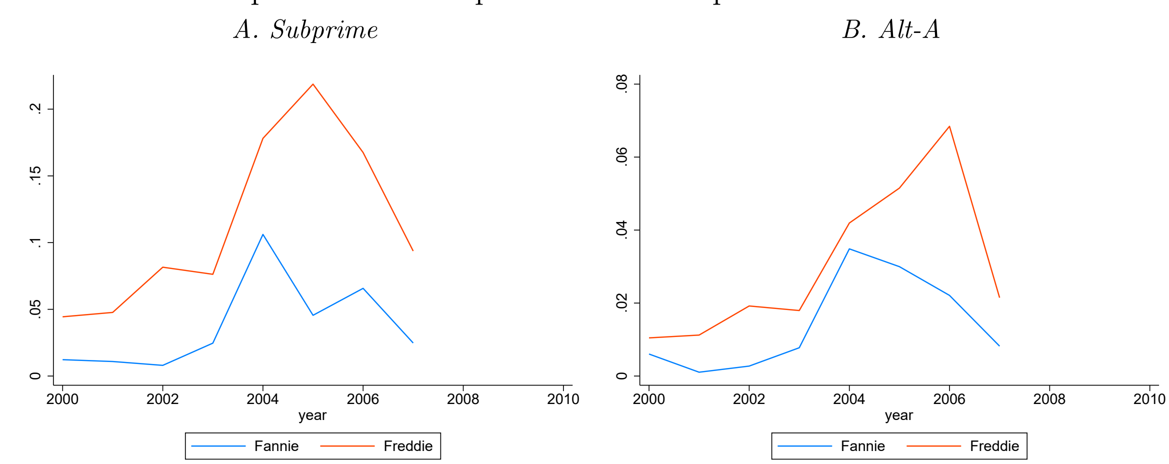
DTI distribution for affected and unaffected credit score - LTV combinations



NOTES: This figure uses the GSE Single Family Loan Performance Datasets.
Plots debt-to-income distributions separately by whether, using the procedure described in Appendix E, I classify a particular credit score × LTV group as being allowed to have DTI > 50 or not. That is, Figure A.10A uses credit score × LTV groups shown in red on Figure A.8B, and A.10B uses credit score × LTV groups shown in blue on Figure A.8B. Includes purchase loans bought by Fannie Mae or Freddie Mac in 2000. Includes debt-to-income ratios up to 64 per cent.

A.11

GSE purchases of subprime and Alt-A private label securities



NOTES: This figure uses numbers reported by 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.

Table A.1
County Descriptive Statistics (II)

	(1)
Median income ('000s, 1998)	–0.10**
	(0.04)
Average DTI (1998)	0.00
	(0.01)
Subprime lender share (1998)	–0.02
	(0.04)
% sold to Fannie/Freddie (1998)	–0.02
	(0.04)
Persons per sq. mi. (2000)	–0.18***
	(0.05)
Thrift share (1998)	0.19***
	(0.04)
Coastal county	–0.06
	(0.04)
% County pop. in rural area (2010)	–0.03
	(0.04)
% County pop. in underserved area (1998)	–0.03
	(0.04)
State FE	X
Number of Counties	994
Number of States	49
Within R-squared	0.08
Number of Observations	994

Notes: Each dependent and independent variable is divided by its standard deviation. The dependent variable is Freddie Mac's 1998 county market share. Median income is real household median income from the U.S. Bureau of the Census. Average DTI is computed using the CoreLogic LLMA Database. Population density is county population density from the 2000 census. Underserved is the share of the county population living in a HUD targeted area (1999 classification). Coastal is an indicator equal to one if the county is defined as coastal by the NOAA. The Freddie Mac county market share is constructed using HMDA and excludes lenders originating more than 20000 purchase loans. Loan-to-income is winsorized at 99.5 per cent. Population density is winsorized at 95 per cent.

Table A.2
% House price response: 1998 – 2000; FHFA

	(1)	(2)	(3)	(4)	(5)
Freddie Share	−5.97***	−3.42**			
	(1.47)	(1.38)			
Freddie Share (All Loans)			−8.83***		
			(2.18)		
Freddie × Low Leverage				−3.22*	
				(1.81)	
Freddie × Mid Leverage				−2.10	
				(2.39)	
Freddie × High Leverage				−8.94**	
				(3.92)	
Freddie × Low Supply Elasticity					−11.73***
					(3.95)
Freddie × Mid Supply Elasticity					−1.32
					(1.92)
Freddie × High Supply Elasticity					−1.90
					(1.53)
Freddie × Missing Supply Elasticity					−2.50*
					(1.51)
State FE	X	X	X	X	X
Controls		X		X	X
Number of Counties	866	866	866	661	866
Number of States	48	48	48	47	48
Number of Observations	866	866	866	661	866

Notes: This table reports estimates from regressions of the change in log county house price on Freddie Mac's 1998 market share by number of loans. Columns 1 and 2 use the preferred exposure measure which excludes loans origination by lenders with more than 20000 purchase originations in 1998. Column 3 uses an alternative exposure measure which is constructed using HMDA loans originated by all lenders. Column 4 compares the policy effect for MSAs in different debt-to-income terciles. The housing supply elasticity measure is from Saiz (2010). I split the sample with non-missing elasticity into terciles and include counties with missing elasticity in a fourth group. Controls include median income, population density, the share of county population in areas classified as underserved, the share of the county population in rural areas, and an indicator for whether the county is coastal.

Table A.3
% House price response: 1998 – 2002; FHFA

	(1)	(2)	(3)	(4)	(5)
Freddie Share	−10.50***	−6.87***			
	(2.29)	(2.09)			
Freddie Share (All Loans)			−15.28***		
			(3.36)		
Freddie × Low Leverage				−8.52***	
				(2.97)	
Freddie × Mid Leverage				−6.39**	
				(3.18)	
Freddie × High Leverage				−14.13**	
				(5.98)	
Freddie × Low Supply Elasticity					−17.79***
					(6.12)
Freddie × Mid Supply Elasticity					−5.02
					(3.37)
Freddie × High Supply Elasticity					−5.70***
					(2.13)
Freddie × Missing Supply Elasticity					−4.83**
					(2.19)
State FE	X	X	X	X	X
Controls		X		X	X
Number of Counties	866	866	866	661	866
Number of States	48	48	48	47	48
Number of Observations	866	866	866	661	866

Notes: This table reports estimates from regressions of the change in log county house price on Freddie Mac's 1998 market share by number of loans. Columns 1 and 2 use the preferred exposure measure which excludes loans origination by lenders with more than 20000 purchase originations in 1998. Column 3 uses an alternative exposure measure which is constructed using HMDA loans originated by all lenders. Column 4 compares the policy effect for MSAs in different debt-to-income terciles. The housing supply elasticity measure is from Saiz (2010). I split the sample with non-missing elasticity into terciles and include counties with missing elasticity in a fourth group. Controls include median income, population density, the share of county population in areas classified as underserved, the share of the county population in rural areas, and an indicator for whether the county is coastal.

Table A.4
% House price response: Jan 1999 – Jun 1999

	(1)	(2)	(3)	(4)	(5)
Freddie Share	–0.40	–0.27			
	(0.67)	(0.68)			
Freddie Share (All Loans)			–1.34		
			(1.10)		
Freddie × Low Leverage				0.53	
				(1.19)	
Freddie × Mid Leverage				0.45	
				(1.22)	
Freddie × High Leverage				–2.88**	
				(1.26)	
Freddie × Low Supply Elasticity					–2.34*
					(1.39)
Freddie × Mid Supply Elasticity					2.93***
					(1.00)
Freddie × High Supply Elasticity					–1.91
					(1.46)
Freddie × Missing Supply Elasticity					–0.06
					(0.86)
State FE	X	X	X	X	X
Controls		X		X	X
Number of Counties	996	996	996	754	996
Number of States	49	49	49	48	49
Number of Observations	996	996	996	754	996

Notes: This table reports estimates from regressions of the change in log county house price on Freddie Mac's 1998 market share by number of loans. Columns 1 and 2 use the preferred exposure measure which excludes loans origination by lenders with more than 20000 purchase originations in 1998. Column 3 uses an alternative exposure measure which is constructed using HMDA loans originated by all lenders. Column 4 compares the policy effect for MSAs in different debt-to-income terciles. The housing supply elasticity measure is from Saiz (2010). I split the sample with non-missing elasticity into terciles and include counties with missing elasticity in a fourth group. Controls include median income, population density, the share of county population in areas classified as underserved, the share of the county population in rural areas, and an indicator for whether the county is coastal.

Table A.5

% House price response: Jun 1999 – Dec 1999; Binary Exposure Measure

	(1)	(2)	(3)
Freddie	–0.72**		
	(0.33)		
Freddie × Low Leverage		–0.67	
		(0.66)	
Freddie × Mid Leverage		–0.09	
		(0.70)	
Freddie × High Leverage		–0.71	
		(0.74)	
Freddie × Low Supply Elasticity			–1.43*
			(0.73)
Freddie × Mid Supply Elasticity			–0.22
			(0.55)
Freddie × High Supply Elasticity			–0.60
			(0.60)
Freddie × Missing Supply Elasticity			–0.84
			(0.52)
Weights	X	X	X
Number of Counties	665	498	665
Number of States	51	51	51
Number of Observations	665	498	665

Notes: This table reports estimates from regressions of the change in log county house price on a binary exposure measure. I divide the sample into terciles of Freddie Mac's 1998 market share by number of loans, and set the exposure measure equal to one for counties in the top tercile and zero for counties in the bottom tercile. The table shows weighted results, where the weights are computed such that the means of the following variables are equalized across areas with high and low Freddie share: median household income, average loan-to-income ratio (winsorized at 99.5%), market share of subprime lenders, population density (winsorized at 95%), share of loans sold to Fannie or Freddie, coastal indicator, % of population in rural areas, underserved indicator, thrift market share. The reweighting method is as described in Hainmueller (2012). Column 2 compares the policy effect for MSAs in the top debt-to-income tercile with those in the bottom tercile. The housing supply elasticity measure is from Saiz (2010). I split the sample with non-missing elasticity into terciles and include counties with missing elasticity in a fourth group.

Table A.6

House price response: Jun 1999 – Dec 2002; Binary Exposure Measure

	(1)	(2)	(3)
Freddie	−3.55***		
	(1.20)		
Freddie × Low Leverage		−2.08	
		(1.94)	
Freddie × Mid Leverage		−2.81	
		(2.35)	
Freddie × High Leverage		−7.15**	
		(3.30)	
Freddie × Low Supply Elasticity			−8.96***
			(3.28)
Freddie × Mid Supply Elasticity			−4.37**
			(2.09)
Freddie × High Supply Elasticity			−2.97*
			(1.70)
Freddie × Missing Supply Elasticity			−2.31
			(1.53)
Weights	X	X	X
Number of Counties	665	498	665
Number of States	51	51	51
Number of Observations	665	498	665

Notes: This table reports estimates from regressions of the change in log county house price on a binary exposure measure. I divide the sample into terciles of Freddie Mac's 1998 market share by number of loans, and set the exposure measure equal to one for counties in the top tercile and zero for counties in the bottom tercile. The table shows weighted results, where the weights are computed such that the means of the following variables are equalized across areas with high and low Freddie share: median household income, average loan-to-income ratio (winsorized at 99.5%), market share of subprime lenders, population density (winsorized at 95%), share of loans sold to Fannie or Freddie, coastal indicator, % of population in rural areas, underserved indicator, thrift market share. The reweighting method is as described in Hainmueller (2012). Column 2 compares the policy effect for MSAs in the top debt-to-income tercile with those in the bottom tercile. The housing supply elasticity measure is from Saiz (2010). I split the sample with non-missing elasticity into terciles and include counties with missing elasticity in a fourth group.

Table A.7
% Housing supply response: 1998 – 1999

	(1)	(2)	(3)
Freddie	0.02	0.15	–0.36*
	(0.16)	(0.22)	(0.19)
State FE	X	X	X
Controls	X	X	X
Number of Counties	1,203	875	321
Number of States	50	50	40
Number of Observations	1,203	875	321

Notes: This table reports estimates from regressions of the imputed change in log housing units on Freddie Mac's 1998 market share by number of loans. I impute the number of housing units in county by assuming that each new residential building permit translates into a new housing unit. Housing units in 2000 are available from the Census. Column 1 shows estimates for all counties included in the house price regressions. Column 2 shows estimates for metropolitan counties and Column 3 shows estimates for micropolitan counties.

Table A.8
% Housing supply response: 1998 – 2002

	(1)	(2)	(3)
Freddie	0.24	0.77	−1.31*
	(0.63)	(0.84)	(0.76)
State FE	X	X	X
Controls	X	X	X
Number of Counties	1,203	875	321
Number of States	50	50	40
Number of Observations	1,203	875	321

Notes: This table reports estimates from regressions of the imputed change in log housing units on Freddie Mac's 1998 market share by number of loans. I impute the number of housing units in county by assuming that each new residential building permit translates into a new housing unit. Housing units in 2000 are available from the Census. Column 1 shows estimates for all counties included in the house price regressions. Column 2 shows estimates for metropolitan counties and Column 3 shows estimates for micropolitan counties.