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ABSTRACT

To what extent did an expansion and contraction of credit drive the 2000s housing boom and bust? The existing literature lacks consensus, with findings ranging from credit having no effect to credit driving most of the house price cycle. We show that the key difference behind these disparate results is the extent to which credit insensitive agents such as landlords and unconstrained savers absorb credit-driven demand, which depends on the degree of segmentation in housing markets. We develop a model with frictional rental markets that allows us to consider cases in between the extremes of no segmentation and perfect segmentation typically assumed in the literature. We argue that the relative elasticities of the price-rent ratio and homeownership with respect to an identified credit shock is a sufficient statistic to measure the degree of segmentation. We estimate this moment using three different credit supply instruments and use it to calibrate our model. Our results reveal that rental markets are highly frictional and closer to fully segmented, which implies large effects of credit on house prices. In particular, changes to credit standards can explain between 34% and 55% of the rise in price-rent ratios over the boom.

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Appendices are available at: http://www.nber.org/data-appendix/w29391

1 Introduction

To what extent did an expansion and contraction of credit drive the 2000s housing boom and bust? This question is central to understanding the dramatic movements in housing markets that precipitated the Great Recession and to the effectiveness of macroprudential policy tools, yet more than a decade later there is no consensus on its answer. Some papers, such as Favilukis, Ludvigson, and Van Nieuwerburgh (2017), argue that changes in credit conditions can explain the majority of the movements in house prices in the 2000s.[1] In contrast, papers such as Kaplan, Mitman, and Violante (2020) argue that credit conditions explain virtually none of the boom and bust in house prices, which are instead dominated by changes in beliefs.[2]

This paper makes sense of these divergent findings by elucidating the source of disparate results and quantitatively assessing the role of credit in driving the 2000s housing boom and bust.[3] Our analysis proceeds in four steps. First, we intuitively illustrate why existing models are at odds. Second, we develop and implement an empirical strategy to estimate where reality falls on the spectrum of possible models. Third, we construct a modeling framework flexible enough to nest this spectrum and match our empirical findings. Fourth, we use our calibrated model to quantify the role of credit changes in driving the 2000s housing boom and bust.

To begin, we show that the key difference between these disparate findings is the degree to which credit insensitive agents can absorb credit-driven demand by constrained

[1]Favilukis et al. (2017) find that 60% of the rise in price to rent ratios can be explained by credit alone and all of the rise can be explained by a combination of credit and business cycle shocks.

[2]For more examples, Landvoigt, Piazzesi, and Schneider (2015), Greenwald (2018), Guren, Krishnamurthy, and McQuade (2020), Garriga, Manuelli, and Peralta-Alva (2019), Garriga and Hedlund (2020), Garriga and Hedlund (2018), Justiniano, Primiceri, and Tambalotti (2019), and Liu, Wang, and Zha (2019) analyze models that imply credit conditions played a key role in the boom and bust, while Kiyotaki, Michaelides, and Nikolov (2011) study a model in which credit conditions played only a limited role.

[3]Since credit standards are endogenously set by lenders, the division between credit factors and other factors may not be obvious. For example, overoptimistic beliefs can both raise house prices on their own, and cause lenders to relax credit standards (Foote, Gerardi, and Willen (2012)). In this paper, we define the role of credit to be the difference in outcomes between what occurs when credit conditions change compared to a counterfactual in which credit conditions were exogenously held fixed, regardless of the ultimate cause of the improvement in credit conditions.

agents, which in turn depends on the degree of segmentation in housing markets. This mechanism is clearest and most important in the rental market. In models with perfectly segmented rental markets (most commonly, there is no rental market), favorable credit conditions increase demand for housing by borrowers who compete with each other for the same properties, bidding up house prices. In contrast, models with perfectly integrated rental markets feature deep-pocketed landlords who are willing to trade an unlimited amount of housing at a price equal to the present value of rents. Since landlords are assumed not to use credit and credit has little effect on rents, changes in credit conditions do not influence landlords' reservation price, and the price is effectively fixed. As a result, a credit expansion leads many households to buy housing from landlords, increasing the homeownership rate but not house prices.

The vast majority of models in the literature fall under one of these two paradigms, which can be represented by perfectly inelastic and perfectly elastic "tenure supply" curves in price-rent ratio versus homeownership rate space. These tenure supply curves reflect the relative price schedules at which landlords are willing to supply owned relative to rented housing at a given amount of total housing supply and are distinct from the absolute supply of housing via the construction sector. In this paper, we allow for the possibility of intermediate levels of frictions between these two extremes, with the relative strength of the price-rent and homeownership responses determined by the slope of the tenure supply curve. This slope consequently provides a new and important empirical moment to be matched by any model seeking to study the influence of credit on house prices.

To measure the slope of the tenure supply curve, we follow the traditional approach of using demand instruments. We estimate this slope as the relative elasticity of the price-rent ratio to an identified credit shock, compared to the elasticity of the homeownership rate to that same shock. For our identified credit shock, we look to the existing literature for three different identification strategies. The first and most statistically powerful approach follows Loutskina and Strahan (2015) (hereafter LS) in instrumenting for local credit using differential city-level exposure to changes in the conforming loan limit. The second approach follows Di Maggio and Kermani (2017) (hereafter DK) in exploiting the 2004 preemption of state-level anti-predatory-lending laws for national banks by the Office of the Comptroller of the Currency. The third and final approach follows Mian and Sufi (2019) (hereafter MS) who follow Nadauld and Sherlund (2013) in using differential city-level exposure to a rapid expansion of the private label securitization market through heterogeneity in bank funding sources.

Despite relying on different sources of identification and operating through different segments of the mortgage market, our three empirical approaches deliver largely consistent findings. All three sets of approaches indicate that shocks to credit supply significantly increase house prices and the price-rent ratio, but have a much smaller and rarely statistically significant effect on the homeownership rate. The resulting slope estimates range from three to infinity, depending on the instrument and horizon, implying a substantial degree of segmentation.

To interpret these slopes economically, we construct a dynamic equilibrium model building on Greenwald (2018) in which house prices, rents, and the homeownership rate are all endogenous. Our primary modeling contribution is to tractably incorporate heterogeneity in landlord and borrower preferences for ownership, which allows our model to feature a fractional and time-varying homeownership rate. Our framework nests both full segmentation and zero segmentation between rental and owner-occupied housing, as well as a continuum of intermediate cases. We calibrate the key parameter determining segmentation to directly match our empirical impulse responses, then use the model to compute the role of credit in driving the 2000s housing boom. We find that a relaxation of credit standards in isolation explains 34% of the rise in the price-rent ratio observed in the boom, with a lower bound of 26% accounting for parameter uncertainty. These results contrast to -1% explained by the model with no segmentation, and the 38% explained by the model with full segmentation, implying that our estimated frictions are strong and closer to the fully segmented case.

The role played by credit is even larger after incorporating factors beyond credit standards. Combining a 2ppt decline in mortgage rates alongside the relaxation of credit standards allows our benchmark model to explain 72% of the observed rise, compared to 4% under no segmentation, and 82% under full segmentation. Last, we consider experiments in which we add sufficient exogenous borrower and landlord demand shocks — for instance due to expectations of future prices or rents — to explain the entire boom in price-rent ratios and homeownership. Removing credit relaxation from this "full boom" scenario reduces the observed rise in price-rent ratios by 55%, which is larger than the 34% explained by credit standards in isolation due to strong nonlinear interactions between credit standards and non-credit factors. In contrast, under the no segmentation model removing credit relaxation from the full boom would reduce the rise in price-rent ratios only negligibly (5%). These results imply that macroprudential policies that tighten mortgage credit standards such as loan-to-value (LTV) and payment-to-income (PTI) ratios can be effective at slowing house price growth but only in the presence of the significant segmentation we find in our benchmark economy.

Our baseline model assumes that landlords do not use credit and that the saver housing stock is entirely segmented from the borrower housing stock. To close our analysis, we relax each of these assumptions in turn. When landlords use credit, a credit supply expansion also shifts supply outward. This leads to a larger price-rent ratio response and smaller homeownership rate response than under our benchmark model, implying our baseline results are a lower bound for the effect of credit on the price-rent ratio. Unconstrained savers can also dampen credit-induced house price fluctuations if their housing demand is not segmented from borrowers as shown by Justiniano, Primiceri, and Tambalotti (2015), Kiyotaki et al. (2011), and Landvoigt et al. (2015). We extend our model to allow for frictionlessly integrated saver and borrower housing stocks and find that while this reduces the effect of credit on price-rent ratios by roughly 25%, changes in the price and quantity of credit still explain 54% of the observed rise in price-rent ratios over the boom period. In practice, however, the saver market is quite segmented due to the indivisibility, quality, and location of saver houses, which are absent from our model.[4] We thus consider this extension to represent an extreme lower bound on the influence of credit on house prices.

In summary, the ability of owners who do not use credit, such as landlords or unconstrained households, to absorb credit-driven demand by constrained households determines the extent to which shifts in credit supply influence house prices and the homeownership rate. Our empirical finding that price-rent ratios respond significantly more to a credit supply shock than homeownership rates implies that this margin is subject to substantial frictions, so that prices do respond to credit in a meaningful way. Mapped into our structural model, these frictions are sufficient for credit changes to have driven an important share of the rise in house prices during the 2000s housing boom.

Related Literature. Our paper relates to several literatures. Empirically, our analysis builds on prior analyses of the causal effect of credit and interest rates on house prices including Glaeser, Gottlieb, and Gyourko (2012), Adelino, Schoar, and Severino (2015), Favara and Imbs (2015), Loutskina and Strahan (2015), Di Maggio and Kermani (2017), Mian and Sufi (2019), and Gete and Reher (2018). These results, however, cannot be directly mapped into the share of the boom and bust explained by credit unless the quasi-random variation they use corresponds exactly to the shocks that drove the boom and bust. We build on this literature by adding measures of the causal effect of credit on the homeownership rate, and showing that the ratio of the responses of the price-rent ratio to those of the homeownership rate can identify structural elasticities. These elasticities can be mapped into a structural model to assess the effect of credit on house prices for an arbitrary set of shocks, including those that correspond to the 2000s boom and bust.

Given this focus, our closest empirical counterpart is Gete and Reher (2018), who also measure the impact of an identified credit shock on both the price-rent ratio and home-

[4]These absences are not specific to our framework but are nearly ubiquitous in the macro-housing modeling literature, which typically allows the total housing stock to be frictionlessly reshuffled between various sized houses. This is inconsisetnt with indivisibility.

ownership. Based on a different identification scheme than the three we use in this paper, Gete and Reher (2018) estimate a response of the price-rent ratio that is 85 times larger than the response of the homeownership rate. These estimates correspond to very strong frictions consistent with our estimates in Section 4.

In terms of applied theory, our work relates to papers that study the effect of credit supply on house prices such as Favilukis et al. (2017), Kaplan et al. (2020), Kiyotaki et al. (2011), Greenwald (2018), Guren et al. (2020), Garriga et al. (2019), Garriga and Hedlund (2020), Garriga and Hedlund (2018), Justiniano et al. (2019), Liu et al. (2019), and Huo and Rios-Rull (2016). Closest is Landvoigt et al. (2015), who use an assignment model calibrated to micro data to study endogenous segmentation between constrained and unconstrained homeowners, who sort into homes of different quality. Landvoigt et al. (2015) find that credit is important in explaining the larger boom observed at the bottom of the quality distribution. We see these results as highly complementary to our work. While our model of borrower-saver segmentation is much more simplistic, our tractable approach allows us to embed a similar set of frictions in a complete general equilibrium model of housing and mortgages that provides for a richer set of counterfactuals.

The modeling framework we employ also connects to work using tractable macro-housing models, including Campbell and Hercowitz (2005), Eggertsson and Krugman (2012), Garriga, Kydland, and Sustek (2015), Ghent (2012), Kiyotaki et al. (2011), Iacoviello (2005), Iacoviello and Neri (2010), Liu, Wang, and Zha (2013), Monacelli (2008), and Rognlie, Shleifer, and Simsek (2014). Our contribution relative to this literature is to provide a tractable methodology for incorporating fractional and time-varying homeownership rates and providing a new empirical moment to discipline it.

Last, our paper relates to work on macroprudential policies. Because mortgage credit dominates household balance sheets, many macroprudential policies only work if credit affects house prices. Similarly, the effectiveness of ex-post debt reduction and foreclosure policies (Guren and McQuade (2020), Mitman (2016), Agarwal, Amromin, Ben-David, Chomsisengphet, Piskorski, and Seru (2017a), Agarwal, Amromin, Chomsisengphet, Landvoigt, Piskorski, Seru, and Yao (2017b), Hedlund (2016)) and mortgage design (Guren et al. (2020), Greenwald, Landvoigt, and Van Nieuwerburgh (2020), Campbell, Clara, and Cocco (2020), Piskorski and Tchistyi (2017)) is amplified to the extent that these policies affect house prices.

Overview. The rest of the paper is structured as follows. Section 2 presents the supply and demand model diagrammatically in order to generate intuition and to motivate our estimation of the causal effects of credit on the homeownership rate and price-rent ratio. Section 3 describes our data and empirical methodology. Section 4 presents our empirical results. Section 5 presents the model, Section 6 describes its calibration, and Section 7 presents our model results. Section 8 extends the model to include landlord credit and flexible saver housing demand. Section 9 concludes.

2 Intuition: Supply and Demand

Before we turn to the empirics and model, we present the intuition for how the rental market influences transmission from credit into house prices. This intuition motivates the structure of our model as well as our empirical focus on the causal effects of credit supply on the price-rent ratio and homeownership rate as sufficient statistics for calibration.

To begin, Figure 1 displays the evolution of the price-rent ratio and homeownership rate since 1965. Assuming that housing is either owned by households or by landlords/investors, each point on this plot represents an equilibrium between demand, the relative price (price-rent ratio) the marginal renter is willing to pay to own a home, and supply, the relative price at which the marginal landlord is willing to sell that home.

The figure shows that these equilibria were fairly stable in the pre-boom era (1965-1997), with most observations clustered in the lower left portion of the figure. However, this pattern changed dramatically during the 1997-2006 housing boom, during which the price-rent ratio and homeownership rate increased in tandem to unprecedented levels.

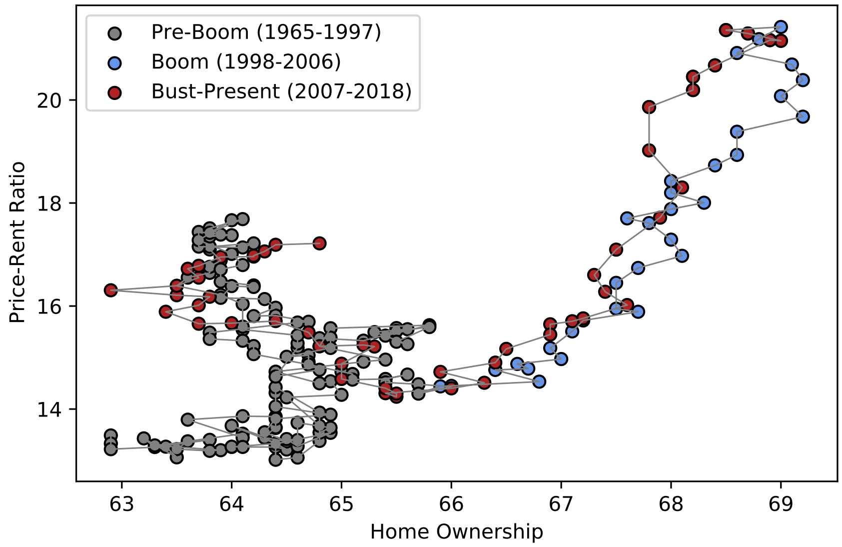


Figure 1: Price-Rent Ratio vs. Homeownership Rate

Note: The figure displays national data at the quarterly frequency. The price-rent ratio is obtained from the BEA and Flow of Funds, as the ratio of the value of residential housing owned by households (FRED code: BOGZ1FL155035013Q) to the value of owner-occupied housing services (FRED code: A2013C1A027NBEA). We use an interpolation scheme to convert owner-occupied housing services from annual to quarterly (see Appendix B.5). The homeownership rate is obtained from the census (FRED code: RHORUSQ156N).

Following the start of the bust in 2007, these variables reverse course, traveling nearly the same path downward that they ascended during the boom, and finally ending up close to the typical values from the pre-boom era.

To understand what forces could cause these patterns, we present a simple supply and demand treatment that illustrates the key forces in the equilibrium model we develop in Section 5. As in Figure 1, we use the price-rent ratio on the y-axis and the homeownership rate on the x-axis. These axes thus represent the relative price and relative quantity of owned vs. rented housing. We use relative rather than absolute prices and quantities of housing to ensure that changes are driven by the rent versus own margin rather than the construction margin.[5] As such, the "tenure supply" curve is the menu of price-rent ratios at which landlords are willing to supply differing amounts of rented housing to the owner-occupied market — leading to different homeownership rates — at a given

[5]For example, new housing construction typically increases the quantity of housing and decreases its price, but has no clear impact on either the price-rent ratio or the share of housing that is owner occupied. Our definitions eliminate these fluctuations which are not relevant for our analysis.

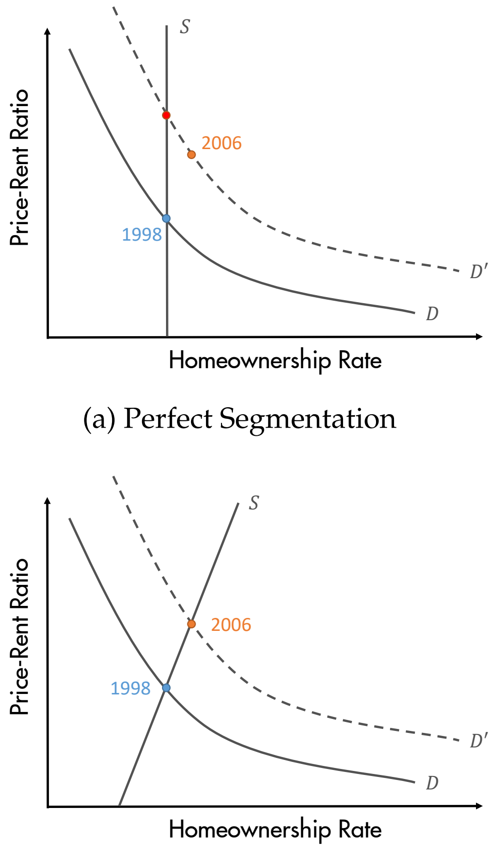


Figure 2: Supply and Demand Treatment of Model Intuitions

amount of aggregate housing supply. We note that this margin is distinct from changes in the absolute quantity of housing units via the construction sector.

Demand for owner-occupied housing comes from constrained households who require mortgages to own. As the price-rent ratio rises, more of these households prefer renting to owning, creating a downward slope. Supply comes from landlords who decide whether to sell units of rental housing to households as owner-occupied housing. The slope of the tenure supply curve reflects the willingness of landlords to sell more units as the price-rent ratio rises, while shifts in the tenure supply curve reflect changes in the landlords' fundamental value of houses relative to rents.[6]

[6]If landlords require credit, a credit supply shock would also shift the tenure supply curve upward. We abstract from landlord credit in our baseline model but return to it in Section 8.

Our supply-and-demand framework is displayed graphically in Figure 2. To begin, Panel (a) shows the case of perfect segmentation, in which units cannot be converted between owner-occupied and renter-occupied, and the homeownership rate is exogenously fixed. This example nests specifications such as Favilukis et al. (2017), Justiniano et al. (2015), and Greenwald (2018), in which households cannot rent housing, corresponding to a fixed homeownership rate of 100%. In our framework, this corresponds to a perfectly inelastic tenure supply, indicated by the vertical line in Panel (a). This curve intersects the downward sloping demand curve to generate an equilibrium in price-rent versus homeownership rate space.

From this starting point, we can consider the impact of a credit expansion that shifts demand outward from the solid curve D to the dashed curve D', as improvements in access to or cost of financing makes more households willing to purchase instead of renting at a given price. Despite this increase in demand, these households have no one to trade with except each other. As a result, under a perfectly inelastic (segmented) tenure supply curve, this increased demand translates directly into an increase in house prices, while the homeownership rate remains fixed. These models thus produce large effects of credit on house prices during the housing boom, but cannot reproduce the rise in homeownership displayed in Figure 1.

Panel (b) considers the alternative extreme case of a frictionless rental market in which identical risk-neutral and deep-pocketed landlords transact with households, similar to the baseline model of Kaplan et al. (2020). This specification leads to a perfectly elastic (horizontal) tenure supply curve, since landlords are willing to buy or sell an unlimited amount of housing at a price equal to the present value of rents, which pins down prices. Since this present value does not directly depend on credit, an expansion of credit that induces an outward shift of demand increases the homeownership rate but not the price-rent ratio. Consequently, a credit expansion cannot explain the dual rise in price-rent ratios and homeownership. Instead, reproducing the empirical pattern requires a *separate* upward shift in the tenure supply curve, indicated by the horizontal dashed line in Panel (b). Since prices are equal to the present value of rents in this model, a shock to future rents is required to move prices *relative* to current rents, such as the shock to future rental beliefs used in Kaplan et al. (2020).[7]

While the literature to date is centered around these polar cases of perfect segmentation and zero segmentation, this paper introduces a framework that allows for intermediate levels of frictions, which corresponds to an upward tenure sloping supply curve as in Panel (c). In this case, a credit expansion that shifts housing demand causes an increase in both the price-rent ratio and the homeownership rate as the equilibrium moves up the tenure supply curve. Panel (c) shows that such a model can in principle explain the joint empirical pattern observed during the housing boom with only a single shock. However, we could also combine a flatter tenure supply curve with a shift in supply to obtain the same equilibrium movement in house prices and homeownership, as in Figure (d). Indeed, any slope of the tenure supply curve could be combined with the appropriate shift in tenure supply to generate the observed dynamics. Ultimately, to disentangle these competing explanations, we need to discipline the slope of the tenure supply curve.

We do so empirically. As is typical in the simultaneous equations literature, the slope of the tenure supply curve can be uncovered using a shock to demand. In Section 3 we use a set of credit supply shocks from the literature that provide exactly this type of variation by increasing demand for owner-occupied housing to estimate the elasticity of tenure supply. With this slope in hand, we then write down a structural model that we calibrate to this estimated slope and use the calibrated model to decompose the role of credit in the 2000s housing boom and bust.

3 Empirical Approach

Motivated by the intuition in Section 2, our goal is to estimate the slope of the tenure supply curve, equal to the ratio of the elasticity of the price-rent ratio to an identified

[7] A shift in landlord discount rates for the same set of rental cash flows would have a similar effect.

demand shock (an expansion of credit) to the elasticity of the homeownership rate to that same shock. Doing so using ordinary least squares (OLS) is problematic because credit is endogenous and housing market conditions can naturally affect credit supply. Furthermore, credit measures may be subject to measurement error. To address these issues, we seek an instrument for credit supply.

We use three different off-the-shelf identification approaches from the literature to instrument for credit supply. While all three approaches are limited in their statistical power, particularly for homeownership rates, all three provide consistent results and thus reinforce one another. In the remainder of this section, we present our data, our basic regression framework, and then describe each empirical approach. Details on our instruments' first stages and robustness checks can be found in Appendix B.

3.1 Data

We construct an annual panel at the core-based statistical area (CBSA) level that merges together data on house prices, rents, homeownership rates, credit, and controls. The data set is slightly different for each empirical approach we use, so we describe the common data sources first, then present these variations in Sections 3.2 - 3.4. Further details on our data construction can be found in Appendix B.

For house prices, we use the CoreLogic repeat sales house price index collapsed to an annual frequency, and check robustness to using the FHFA indices in the Appendix. For credit we use Home Mortgage Disclosure Act (HMDA) data, which we collapse to the CBSA level. Our main measure of credit is the dollar volume of loan originations; in the Appendix we assess robustness using the number of loans and the loan-to-income ratio.

For rents, we use the CBRE Economic Advisors Torto-Wheaton Same-Store rent index (TW index), a high-quality repeat rent index for multi-unit apartment buildings.[8] It

[8]CBRE EA uses data on effective rents, which are asking rents for newly-rented units net of other leasing incentives. CBRE builds a historical rent series for each building and computes the index as the average change in rents for identical units in the same buildings. CBRE does not use the standard repeat sales methodology because rent data is available for most buildings continuously, so accounting for many periods of missing prices is unnecessary.

is available quarterly for 53 CBSAs beginning in 1989 and 62 CBSAs beginning in 1994. Although using the TW index limits our sample sizes, it improves on rent measures typically used in the literature in two ways. First, its repeat sales methodology is preferable to median rent measures typically used in the literature, which are biased by changes in the composition of leased units. Second, while median rents tend to be sticky and slow moving due to contractual rigidies, the TW index uses asking rents on newly-leased apartments, which better reflect current market conditions. Since the price-rent ratio is meant to capture the rent a unit could command if leased instead of sold, rent measures using newly-leased apartments are more appropriate for constructing this ratio.

Our homeownership data come from the Census Housing and Vacancy Survey (HVS), which provides annual estimates of the homeownership rate at the CBSA level from 1986 to 2017. These data are only available for an unbalanced panel of 82 CBSAs and are somewhat noisy as they are obtained from a supplement to the Current Population Survey with only 72,000 households nationwide. The HVS is also complicated by decennial changes in CBSA definitions. In the baseline results, we treat CBSAs where changing definitions increase or decrease the homeownership rate significantly as separate CBSAs, but present robustness checks in the Appendix dropping any CBSA that experiences a changing definition.[9] For robustness, we supplement our data with alternative homeownership rates from the American Community Survey (ACS), which are available for a larger pool of cities than our baseline measure but begin only in 2005.

3.2 First Empirical Approach: Loutskina and Strahan (2015)

Our first and most statistically powerful empirical approach follows LS in using a shift-share instrument based on the conforming loan limit (CLL). The CLL represents the maximum loan size eligible for securitization by Fannie Mae and Freddie Mac. Because mort-

[9]Specifically, we use county-level data on homeownership rates for the full population of households from the adjacent decennial Censuses. If the homeownership rate changes by more than 5%, we treat the CBSA before and after the change as different CBSAs, creating an unbalanced panel. This approach uses the full data but ensures that fixed effects and impulse responses are not affected by a jump in the homeownership rate due to CBSA definition changes.

gages backed by Fannie Mae and Freddie Mac receive subsidized interest rates, an increase in the CLL represents an increase in the incidence of this subsidy and a positive shock to the supply of mortgage credit for borrowers newly able to take advantage of it.

The operating principle of the instrument is that the same nationwide change in the CLL should have stronger effects in cities where a larger fraction of loans are close to this threshold since more new loans should shift from being unsubsidized to subsidized.[10] For a concrete example, an average of 7.2% of loans originated in San Francisco over our sample fall within 5% of the next year's conforming loan limit, compared to an average of only 0.4% in El Paso. Our instrument exploits the fact that a change in the CLL should therefore have a bigger average effect in San Francisco than in El Paso.

To construct an instrument that exploits this CLL variation, we follow LS and define:

$$Z_{i,t} = \begin{bmatrix} ShareNearCLL_{i,t} \times \%ChangeInCLL_t \\ ShareNearCLL_{i,t} \times \%ChangeInCLL_t \times SaizElasticity_i \end{bmatrix}.$$

To measure the share of homes with loans near CLL, we follow LS in using the fraction of mortgage originations in the prior year that are within 5 percent of the current year's CLL in the HMDA data. We also follow LS in using both the standard share-shift and a triple interaction with the housing supply elasticity as estimated by Saiz (2010) as instruments to allow for potential heterogeneity in the effect of the CLL change by housing supply elasticity. Since the CLL has occasionally varied by region, we use only changes in the national CLL to construct our instrument.[11]

We then estimate the impulse response of a change in credit $C_{i,t}$ on an outcome variable of interest $Y_{i,t}$ using a local projection instrumental variables (LP-IV) approach.[12]

[10] See Adelino et al. (2015) for an implementation of this empirical strategy based directly on house prices rather than loan sizes.

[11] As part of the HERA legislation in 2008, Congress created more transparent procedures for changing the national CLL. The legislation also allowed the CLL to rise by more in high-cost cities if their local house price index grew sufficiently quickly. This would violate an instrumental variable's exclusion restriction because the change in the CLL would be mechanically correlated with lagged local outcomes. Consequently, in constructing the instrument we use the change in the national CLL regardless of the change in the local CLL in high-cost areas.

[12] In our empirical application, $C_{i,t}$ is the volume of new purchase loan origination in location i at time t;

This approach generalizes the Jordà (2005) local projection methods to use exogenous instrumental variables for identification as in Ramey (2016) and Ramey and Zubairy (2018). We extend this to the panel context and add CBSA and time fixed effects following Chen (2019).

In particular, we use two-stage least squares to estimate:

$$\log(Y_{i,t+k}) = \xi_i + \psi_t + \beta_k \Delta \log(C_{i,t}) + \theta X_{i,t} + \epsilon_{i,t} \tag{1}$$

$$\Delta \log(C_{i,t}) = \phi_i + \chi_t + \gamma Z_{i,t} + \omega X_{i,t} + e_{i,t}, \tag{2}$$

for $k = 0, ..., 5$, where β_k is our coefficient of interest, ξ_i and ϕ_i are location fixed effects in the second and first stages, respectively, ψ_t and χ_t are time fixed effects, and $X_{i,t}$ are controls. We cluster the standard errors by CBSA.

The formal identification conditions for the panel LP-IV specification following Stock and Watson (2018) are not only relevance and contemporaneous exogeneity but also exogeneity at all leads and lags. This requires that our instruments be independent of one another. To address the potential failure of this condition due to serial correlation, we follow Ramey (2016) and Ramey and Zubairy (2018) in including two lags each of our instruments $Z_{i,t}$, our outcome variable $\log(Y_{i,t})$, and our first stage variable $\Delta \log(C_{i,t})$ as controls. We supplement these with a number of additional control variables to ensure that our estimates are based purely on the share-shift variation in our instrument. We include both CBSA effects, which absorb any average differences across areas including their supply elasticity, and time fixed effects, which absorb aggregate conditions including any average effects of the CLL on the national housing and mortgage markets. We also directly control for $ShareNearCLL_{i,t}$ and its lag as control variables so that time variation in this share is not driving our estimates.

The identifying assumption for this first empirical approach is that conditional on our controls there is no unobservable variable that varies with both the fraction of loans orig-

we consider other measures of credit in the Appendix.

inated in the previous year close to the CLL (or its interaction with the Saiz elasticity) and that also varies with changes in the national CLL. For example, one might be concerned that cities with higher prices tend to be more exposed to national business cycles, and that CLL changes are also correlated with these cycles. To address such concerns, we conduct robustness checks in the Appendix demonstrating that time-varying city characteristics are not driving our results.

Since we are also interested in estimating the slope of the tenure supply curve directly, we also modify the LP-IV approach to directly estimate the impulse response of the slope. To do so, we use the homeownership rate as $Y_{i,t+k}$ and replace $\Delta \log(C_{i,t})$ with the price-rent ratio $\log(PRR_{i,t+k})$ in the first stage.[13] The coefficients β_k then represent the inverse of the slope of the supply curve. We estimate this inverse slope rather than the slope itself because in practice the instrument has a far stronger effect on price-rent ratios than homeownership rates. As a result, so the first stage is much stronger for estimating the inverse slope by IV than for estimating the slope directly, which would suffer from severe weak instrument problems.

For our sample, we use an unbalanced panel of 62 CBSAs with both rents and homeownership rates from 1992 to 2016 for our analysis, although we note that most of the identifying variation is obtained over the period 1992 to 2006 since the national CLL is fixed through the bust and rebound.[14]

3.3 Second Empirical Approach: Di Maggio and Kermani (2017)

Our second approach follows DK by exploiting an expansion of credit that occurred due to the OCC's 2004 preemption of state-level anti-predatory-lending laws (APLs) for national banks. States implemented APLs in 1999 to limit the terms of mortgages made to riskier borrowers. The preemption thus allowed national banks to expand credit more

[13]Because we want to obtain the ratio of the price-rent and homeownership rate response at the same horizon, we use the price-rent ratio k periods ahead in our first stage, instead of the contemporaneous credit growth $\Delta \log(C_{i,t})$ in our previous regressions. In all cases we use the time t instrument.

[14]In practice, the CLL never adjusts downward, so it typically remains flat during housing downturns until prices pass their previous peak.

readily to riskier borrowers, providing a shock to the supply of credit. DK identify credit supply shocks by comparing counties with different exposures to national banks that were regulated by the OCC before and after the change. We adapt their approach but do so at the CBSA level in order to use homeownership rate data that is only available for metropolitan areas.

We define the DK instrument as:

$$Z_i = APL_{2004} \times OCC_{2003},$$

where APL_{2004} is an indicator for whether the state that the majority of the CBSA resides in has an anti-predatory-lending law by 2004, and OCC_{2003} is the share of mortgage originated by OCC-regulated banks in 2003, obtained from HMDA data.

Because this instrument only induces variation across cities in response to a single credit supply event, we follow DK in using an event study approach in place of the LP-IV approach we use for the LS instrument, which requires variation in the instrument both across cities and over time. We follow much of the literature and focus on the reduced form regressing the outcome variables directly on the instrument. This is sufficient to obtain the slope of the supply curve, but implies that we cannot interpret coefficient magnitudes in units of credit.

In particular, we estimate the regression:

$$\log(Y_{i,t}) = \xi_i + \psi_t + \sum_{k \neq \tau} \beta_k Z_i \mathbf{1}_{t=k} + \theta X_{i,t} + \epsilon_{i,t}. \tag{3}$$

The coefficients β_k represent the reduced form effect of the instrument at each date in time relative to a base period τ for which β is normalized to zero. To ensure that only the interaction of APL_{2004} and OCC_{2003} is used for identification, we follow DK in controlling for both of these variables directly in addition to including CBSA and year fixed effects (ξ_i and ψ_t). Our controls ($X_{i,t}$) include the lag of the outcome variable as well as all additional controls used by DK in their original analysis, with the exception of a proprietary measure of the share of loans originated to subprime borrowers that is not crucial for their results. Since this identification strategy is similar to a difference-in-difference with Z_i measuring exposure, a key test of the identifying assumptions is that there are parallel pre-trends or equivalently that β is equal to zero prior to date τ.

The identifying assumptions are similar to a differences-in-differences approach: there must be parallel trends in the absence of treatment. DK provide extensive support for this identifying assumption in their paper, and we replicate this finding of no pre-trends prior to 2003 in our empirical specifications.

We follow DK in estimating equation (3) using growth rates for $Y_{i,t}$, so that the outcome variable is the log change in house prices or homeownership rates. To obtain an impulse response in levels, we then add up the coefficients of interest from the base period to each indicated period and compute standard errors by the delta method.

DK kindly provided us with their data set, and we use their data directly collapsed to the CBSA level to be as consistent with their paper as possible. We then merge in CBSA-level CoreLogic house price index and census homeownership rates. This yields 262 CBSAs from 2001 to 2010 for house prices and 82 CBSAs from 2001 to 2010 for homeownership rates. Due to the limited power of a single event study, we focus on house prices rather than price-rent ratios, which would require cutting our sample further to the subset with available rent data. We follow DK by weighting our regressions by population and by clustering standard errors by CBSA. For homeownership rates, we drop cities that have a change in Census homeownership definitions in 2005.

3.4 Third Empirical Approach: Mian and Sufi (2019)

Our third approach exploits differential city-level exposure to the 2003 expansion in private label securitization (PLS) to identify the effect of credit supply on prices and homeownership rates based on MS and Nadauld and Sherlund (2013). MS build on evidence from Justiniano, Primiceri, and Tambalotti (2017) of a sudden, sizable, and persistent expansion in PLS markets in late 2003 that persists until the crash. MS argue that the PLS expansion had a larger effect on lending by mortgage lenders that rely on non-core deposits to finance mortgages, measured at the bank level as the ratio of non-core liabilities to total liabilities (NCL). They hypothesize that NCL banks, which are funded less by deposits, should have a greater exposure to the PLS expansion, and show that high NCL banks did in fact expand their lending more after following roughly parallel trends prior to 2002. Like the DK instrument, this approach isolates a non-prime credit supply shock, in contrast to the LS shock that directly affects prime borrowers only.

The MS instrument is defined as:

$$Z_i = NCLShare_i^{2002},$$

where $NCLShare_i^{2002}$ is MS's measure of CBSA-level exposure to high NCL lenders, equal to the origination-weighted average of lender-level NCLs in a CBSA based on 2002 originations. MS argue that the city-level NCL exposure satisfies the relevant exclusion restriction and is a valid instrument for credit. Because this instrument also induces variation across cities in response to a single event, we use the same reduced form event study approach (3) that we use for the DK instrument. We follow MS in weighting by the number of housing units and including year and CBSA fixed effects, and cluster at the CBSA level.

The MS instrument is underpowered using only the CBSAs for which we have Census HVS homeownership rates. Consequently, for this empirical approach we expand our data set by using ACS data for homeownership rates and FHFA data for house prices. This ACS-FHFA data sample covers 245 CBSAs from 1990 to 2017 for prices and 245 CBSAs from 2005 to 2017 for the homeownership rate. However, this means that we must use house prices in place of our preferred outcome variable, the price-rent ratio.[15] Using this data sample also prevents us from setting the base year of 2002 used by MS because

[15]We do not use the ACS homeownership rates for the LS instrument because the ACS begins in 2005 and most of the variation in the conforming loan limit comes before 2005. The ACS does have rents, but they are average rents rather than new rents. Due to the long term nature of leases, average rents move much less than new rents, so a price-rent ratio constructed with ACS data looks nearly identical to the same regression using prices as an outcome.

our ACS homeownership rate data begins in 2005. We instead use 2013 as the base year, since our estimates imply that the house price response returned to its 2002 level in 2013. Our results are robust to using peak-to-trough changes rather than a particular base year.

4 Empirical Results

4.1 Loutskina-Strahan LP-IV Results

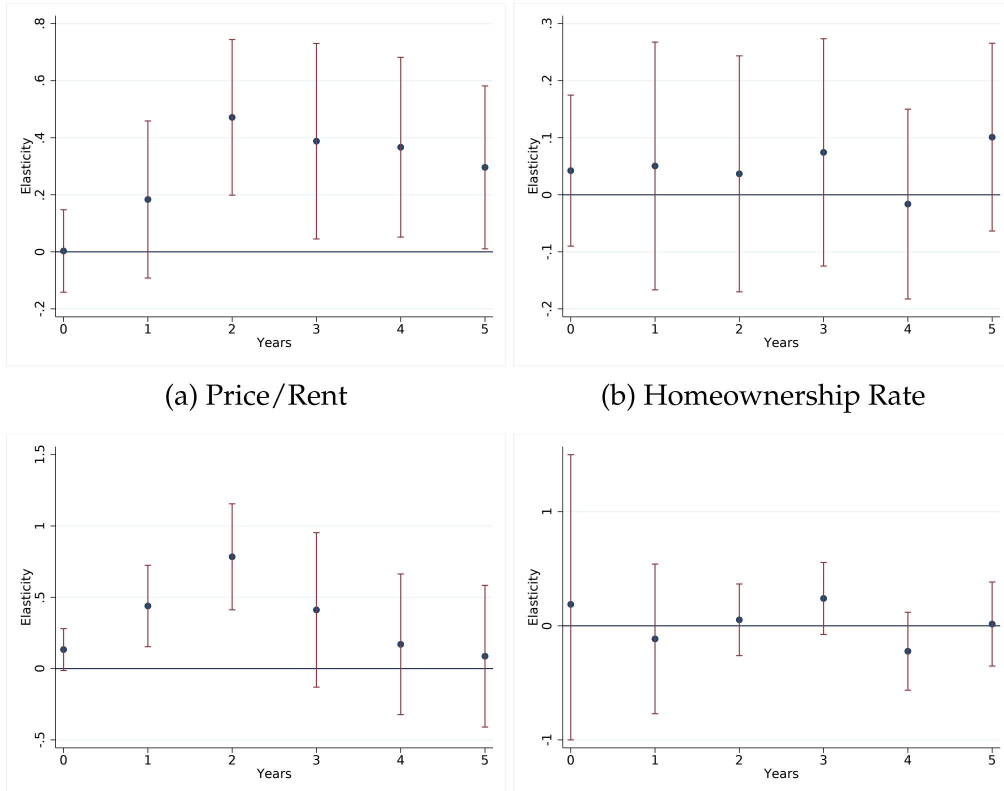
Figure 3 plots the impulse responses of our outcome variables to credit growth (β_k in equation (1)) using LP-IV and the LS instrument. Beginning with Panel (a), we observe that the price-rent ratio rises for two years following the shock before peaking at an increase of 0.471. Our estimates are significant at the 5% level in years 2 through 5. The smaller and statistically insignificant responses in years 0 and 1 are typical of house price dynamics, which exhibit sluggish reactions and short run momentum (Guren (2018)).

Decomposing this result, this behavior of the price-rent ratio is due to a larger hump-shaped response of house prices, which peak at 0.784 after two years (Panel (c)), and a hump-shaped response of rents, which reaches 0.160 at the same horizon (Appendix Figure B.1). Our results for the effect of credit on house prices are consistent with those found in the literature, such as Glaeser et al. (2012), Adelino et al. (2015), Favara and Imbs (2015), and Di Maggio and Kermani (2017). For instance, Favara and Imbs (2015) find an elasticity of house prices to loan volumes over one year of 0.134, which is extremely close to our estimate of 0.133 at the same horizon.[16]

In contrast to our results on price-rent ratios and house prices, we find no significant response of homeownership rates to credit shocks. While this is in part due to lower statistical power stemming from a noisier data set, the point estimates are also consistently small, reaching 0.037 after two years, and peaking at 0.101 after 5 years. For a naive "back-of-the-envelope" measure of the relative slope, we can simply divide the point estimates

[16]We find a larger response of rents than Favara and Imbs, likely because we are using the TW rent index, which provides the rent of a newly-rented multi-family unit using a repeat sales methodology, rather than stickier median rents as used by prior literature.

Figure 3: Loutskina-Strahan Instrument LP-IV Impulse Responses



(a) Price/Rent (b) Homeownership Rate

(c) House Price (d) Inverse Supply Slope

Notes: 95 % confidence interval shown in red bars. The figure shows panel local projection instrumental variables estimates of the response of the indicated outcomes to dollar credit volume. For panels (a) to (c), the first and second stages are indicated in equations (2) and (1), respectively. The two instruments are $ShareNearCLL_{i,t} \times \%ChangeInCLL_t$. and $ShareNearCLL_{i,t} \times \%ChangeInCLL_t \times Z(SaizElasticity_i)$. Control variables include $ShareNearCLL_{i,t}$ and its lag, lags of both instruments, and two lags of both the outcome variable and the endogenous variable. For panel (d), use the homeownership rate as our outcome variable and replace the log credit growth at time t with the log price-rent ratio at time $t+k$ to obtain a coefficient for the inverse supply curve slope. All standard errors are clustered by CBSA.

in Panel (a) by those in Panel (b) to obtain ratios of 12.83 at the 2-year horizon, 5.22 at the 3-year horizon, -22.50 at the 4-year horizon, and 2.93 at the 5-year horizon, corresponding to a range of slopes between 2.9 and infinity.[17]

Beyond these naive ratios, we pursue a more econometrically precise approach by directly estimating the inverse of this ratio. We reestimate our IV specification (1) - (2) using

[17]Since a downward sloping supply curve is implausible, negative inverse ratios are best interpreted as infinite (perfectly inelastic) slopes, since these offer the smallest possible ratio of the homeownership rate response to the price-rent ratio response.

the homeownership rate as the outcome variable $Y_{i,t+k}$ and the price-rent ratio in period $t+k$ in place of log credit growth as the independent variable. As described in Section 3, we estimate the *inverse* slope (response of homeownership relative to response of price-rent) because our imprecise results in Figure 3 Panel (b) would yield a weak first stage if the homeownership rate were used as the independent variable. Panel (d) shows that the inverse slope is small and not statistically different from zero, with point estimates of 0.05 at the 2-year horizon, 0.24 at the 3-year horizon, -0.22 at the 4-year horizon, and 0.02 at the 5-year horizon.[18] Inverting these estimates yields supply curve slopes between between 4.2 and infinity. The upper bounds of the 95% confidence intervals for the inverse slope are 0.37 at the 2-year horizon, 0.56 at the 3-year horizon, 0.12 at the 4-year horizon, and 0.38 at the 5-year horizon, corresponding to lower bound estimates of the (non-inverted) slope between 1.8 and 8.4.

4.2 Di Maggio-Keramani APL Preemption Results

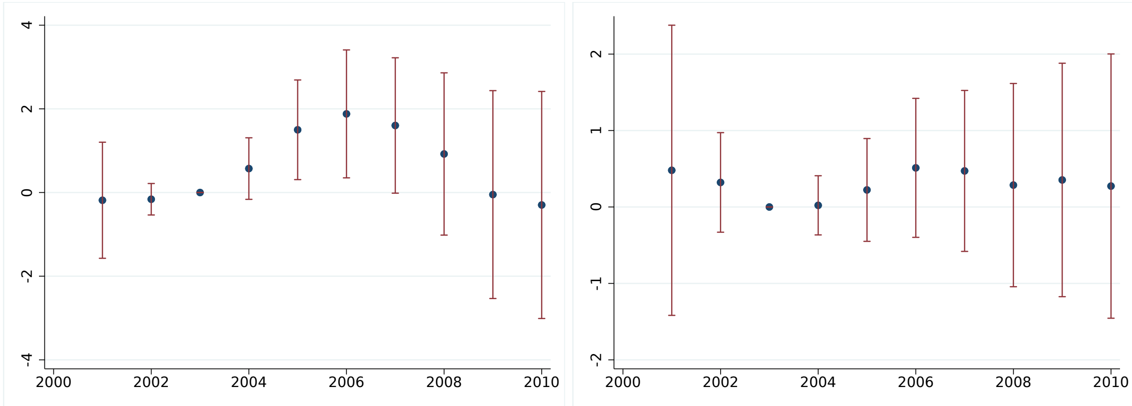
Figure 4 shows the results for the reduced form event study using the DK instrument. As a reminder, we estimate equation (3) with house prices as the outcome in log changes and then cumulate the coefficient β coefficients from the base period to the indicated period to obtain an IRF in levels.

Panel (a) displays the response of house prices for the sample of CBSAs for which we have homeownership rate data. This differs slightly from the original DK estimation, which used a broader sample of cities.[19] The impulse response shows no significant pre-trends are evident prior to 2003. After 2004, the results demonstrate a classic hump-shaped impulse response for house prices peaking in 2007 at 1.60, which is significant at the 5% level. Panel (b) of Figure 4 shows the impulse response for homeownership rates is generally smaller, peaking in 2006 at 0.51, and is far from statistically significant. A

[18]We omit the ratios for the 0-year and 1-year horizons since Panel (a) implies that our first stage is not statistically significant at these horizons.

[19]Using the broader sample of CBSAs we are able to largely reproduce DK's results (see Figure B.10). Cities with homeownership rate data tend to be larger, more inelastic cities, leading us to find larger responses of house prices than in the original DK estimation.

Figure 4: Di Maggio-Kermani APL Preemption Reduced Form



(a) House Prices

(b) Homeownership Rate

Notes: 95 % confidence interval shown in red bars. Each panel shows estimates of the cumulative sum from 2003 of β_k for each indicated year estimated from equation (3), with the instrument being $Z_i = APL_{2004} \times OCC_{2003}$ and 2003 being the base year. The regression is weighted by population and standard errors are clustered by CBSA. The controls include median income growth, population growth, the Saiz (2010) elasticity interacted with a dummy for post-2004, the fraction of loans originated by HUD-regulated lenders interacted with a dummy for post-2004, and the fraction of HUD-regulated lenders interacted with a dummy for APLs. The controls are as Di Maggio and Kermani (2017) except our data is at the CBSA level and omits a control for the fraction of loans originated that are subprime (FICO under 620), which is based on proprietary data. All regressions are weighted by population and standard errors are clustered by CBSA as in the DK paper.

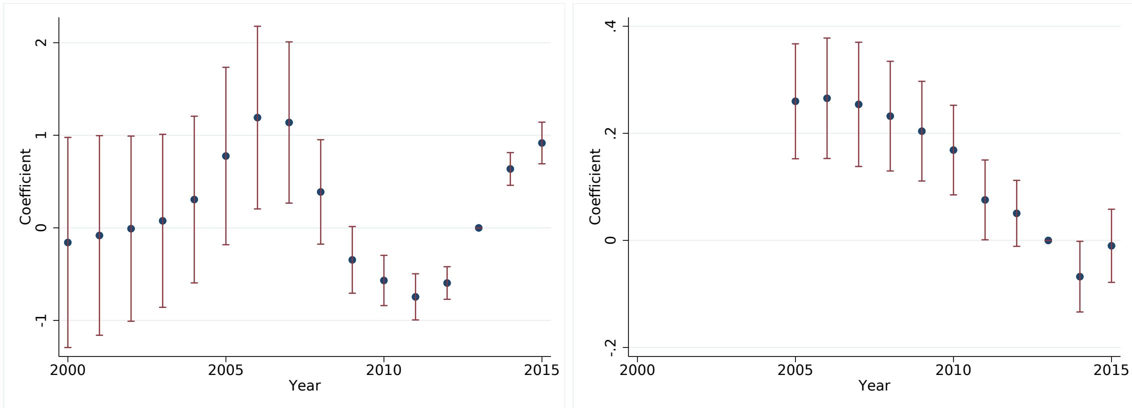
simple division of the values yields slopes of 6.72 in 2005, 3.67 in 2006, and 3.40 in 2007.[20]

4.3 Mian-Sufi PLS Expansion Results

Figure 5 displays our estimated β_k coefficients by year for the Mian and Sufi NCL share instrument. As a reminder, while the PLS market expansion is in 2002, we have normalized 2013 to be the base year due to data availability, and also use house prices as the outcome due to power concerns. Panel (a) shows a zero effect on prices prior to 2002, followed then a hump-shaped impulse response, in line with our previous results. Panel (b) shows a positive and statistically significant effect of the NCL share on homeownership rates be-

[20]As in Section 3.2, we focus on slopes in periods with house prices responses that appear at or close to the peak and are not still in the process of sluggish adjustment, as these provide the best analogue to the corresponding moment in the model. We similarly report years satisfying these criteria for our MS results in Section 4.3 below.

Figure 5: Mian-Sufi PLS Expansion Reduced Form



(a) House Prices

(b) Homeownership Rate

Notes: 95 % confidence interval shown in red bars. Panels A and B shows estimates of the effect of a city's NCL share on each outcome based on estimating equation (3) with the instrument being $Z_i = NCLShare_i^{2002}$ and 2013 being the base year. For panel C, we use the same controls but the outcome variable is the homeownership rate and the credit variable is replaced with log house prices to obtain a coefficient for the inverse supply curve slope All standard errors are clustered by CBSA as in the MS paper.

ginning in 2005 that also mean reverts over time. Examining the coefficients reveals that the effect of house prices peaks in 2006 at 1.19 while the effect on the homeownership rate also peaks in 2006 at 0.27. A simple division of these point estimates yields a slope of 4.49, while the same exercise using the 2007 coefficients yields a ratio of 4.48.

4.4 Summary

While all three of our empirical strategies have limitations, they each find evidence for a slope of the supply curve of at least three, and often higher. We obtain these consistent results even though our instruments rely on completely distinct sources of variation and influence different segments of the mortgage market, with the LS instrument affecting credit to prime borrowers and the DK and MS instruments affecting credit to more risky borrowers. While future work is needed to refine these estimates, our finding of broad agreement across specifications leads us to conclude that our general finding that house prices response more than homeownership rates in response to a shock to credit supply is robust. In Section 6, we use a model to provide economic interpretation of these numeric slope values, showing that our empirical estimates provide clear lower bounds on the degree of segmentation needed to match the data.

5 Model

This section develops an equilibrium model that we use to quantitatively evaluate the role of credit in driving house prices, with a focus on the 2000s boom-bust cycle.

Demographics. There is a representative borrower, landlord, and saver, denoted B, L, and S, respectively. Each is infinitely lived. We assume perfect risk sharing within each type, allowing for aggregation to a representative agent of each type.

Housing Technology. Housing is produced by construction firms (described below) whose supply at the end of period t is denoted $\bar{H}_t$. Housing can be owned either by borrowers, by savers, or by landlords, where landlords in turn rent the housing they own to borrowers. We denote borrower-owned housing as $H_{B,t}$ and landlord-owned rental housing as $H_{L,t}$. Housing produces a service flow proportional to its stock, and is sold ex-dividend (i.e., after the service flow is consumed). It requires a per-period maintenance equal to fraction δ of its current value.

While housing is traded by borrowers and landlords, our main specification imposes that savers always demand the fixed quantity of housing $\bar{H}_S$. This is equivalent to assuming a completely segmented housing market, in which savers and borrowers consume different types of housing (e.g., live in different neighborhoods, occupy different quality tiers). This restrictive and important assumption shuts down any margin for borrowers and savers to transact housing, equivalent to fully segmented housing markets between these two groups. In Section 8, however, we relax this assumption to allow savers to freely trade housing with borrowers. For notational convenience, we denote the stock of housing not owned by savers, and therefore ultimately inhabited by borrowers as either owners or renters, as $\hat{H}_t = \bar{H}_t - \bar{H}_S$.

Preferences. Borrowers and savers both have log preferences over a Cobb-Douglas aggregator of nondurable consumption and housing services:

$$U_j = \sum_{t=0}^{\infty} \beta_j^t \log\left(c_{j,t}^{1-\xi} h_{j,t}^{\xi}\right), \qquad j \in \{B, S\}$$

where c represents nondurable consumption, and h represents housing services. To nest typical specifications in the literature, we assume that landlords are risk neutral and maximize:

$$U_L = \sum_{t=0}^{\infty} \beta_L^t c_{L,t}.$$

Risk neutrality aligns with the interpretation of landlords as a foreign-owned, profit-maximizing firm as in e.g., Kaplan et al. (2020).

Asset Technology. Borrowers and landlords can trade long-term mortgage debt with savers at equilibrium, with the mortgage technology following Greenwald (2018). Borrower debt is denoted $M_{B,t}$ while landlord debt is denoted $M_{L,t}$. Debt is issued in the form of fixed-rate perpetuities with coupons that geometrically decay at rate ν. This means that a mortgage that is issued with balance M^* and rate r^* will have payment stream of $(r^* + \nu)M^*,\ (1-\nu)(r^* + \nu)M^*,\ (1-\nu)^2(r^* + \nu)M^*$, etc. Mortgage loans are prepayable, with exogenous fraction ρ prepaying their loans in a given period, and are also nominal, meaning that real balances decay each period at the constant rate of inflation π.

As in Greenwald (2018), the average size of new loans for borrower i (denoted $M_{i,t}^*$) is subject to both loan-to-value (LTV) and payment-to-income (PTI) limits at origination:

$$M_{i,t}^* \leq \theta^{LTV} p_t H_{i,t}^* \tag{4}$$

$$M_{i,t}^* \leq \frac{\left(\theta^{PTI} - \omega\right) \text{income}_{i,t}}{r_{B,t}^* + \nu + \alpha}, \tag{5}$$

where p_t is the price of housing, $H^*_{i,t}$ is the borrower's new house size, and ω and α are offsets used to account for non-housing debts, and taxes and insurance, respectively.

Ownership Benefit Heterogeneity. Without additional heterogeneity, the model would be unable to generate a fractional and time-varying homeownership rate. If all borrowers have the same valuation for housing and all landlords have the same valuation for housing, then whichever group has the higher valuation will own all the housing, leading to a homeownership rate of either 0% or 100%. In order to generate a fractional homeownership rate, we thus need to impose further heterogeneity in how agents value housing *within* at least one of these types. Our key modeling contribution in this paper is to introduce this within-type heterogeneity.

We impose this heterogeneity in a simple way, by assuming that agents receive an additional service flow (either positive or negative) from owning housing. For parsimony, we assume that if borrower i owns one unit of housing, he or she receives surplus equivalent to $\omega_{i,t}$ times the market rent for that unit, where $\omega^B_{i,t} \sim \Gamma_{\omega,B}$ is drawn i.i.d. across borrowers and time. Symmetrically, if a landlord owns unit i of housing, he or she receives surplus equivalent to $\omega^L_{i,t} \sim \Gamma_{\omega,L}$ times the market rent for that unit. Because we perceive these benefits and costs, particularly those of the borrower, as likely non-financial, we rebate them lump-sum to households, so that they do not have any effect on the resource constraint at equilibrium. Since we apply borrower heterogeneity at the household level, but landlord heterogeneity at the property level, the two dimensional sorting problem is trivial: all properties with sufficiently low ω_L are owned, and they are owned by the households with the largest ω_B.

There are several forms of heterogeneity that would map intuitively into this framework. On the borrower side, heterogeneity in the value of ownership likely stands in for household age, family composition, ability to make a down payment, and true personal preference for ownership. On the landlord side, we conjecture that the biggest source of heterogeneity is on the suitability of different properties for rental, as documented for instance by Halket, Nesheim, and Oswald (2020). For example, while urban multifamily units can be efficiently monitored and maintained in a rental state, the depreciation and moral hazard concerns for renting a detached suburban or rural house may be much higher. Under this interpretation, at high homeownership rates, the marginal converted property is easy to convert and maintain, and is valued highly by landlords relative to the rent it produces. At low homeownership rates, by contrast, the marginal converted property is relatively costly to maintain as a rental property, and landlords are willing to part with it at a lower price-rent ratio.

The degree of dispersion of the distributions $\Gamma_{\omega,B}$ and $\Gamma_{\omega,L}$ map into the slopes of the demand and tenure supply curves, respectively, in Section 2. The more dispersed are the ownership benefits, the steeper is the slope, as the marginal valuation changes rapidly as we move along the distribution. In contrast, a distribution with low dispersion will yield a flatter, more elastic curve, as agents share highly similar valuations, implying that prices move little as the homeownership rate, and the identities of the marginal owner/renter and landlord vary.

Borrower's Problem. The borrower maximizes expected lifetime utility subject to the borrowing constraints (4), (5), and the budget constraint

$$\begin{aligned} c_{B,t} \leq \; & \underbrace{(1-\tau)y_{B,t}}_{\text{after-tax income}} + \underbrace{\rho_B\left(M^*_{B,t} - \pi^{-1}(1-\nu_B)M_{B,t-1}\right)}_{\text{net mortgage iss.}} - \underbrace{\pi^{-1}(1-\tau)X_{B,t-1}}_{\text{interest payment}} - \underbrace{\nu_B\pi^{-1}M_{B,t-1}}_{\text{principal payment}} \\ & - \underbrace{\rho_B p_t\left(H^*_{B,t} - H_{B,t-1}\right)}_{\text{net housing purchases}} - \underbrace{\delta p_t H_{B,t-1}}_{\text{maintenance}} - \underbrace{q_t\left(h_{B,t} - H_{B,t-1}\right)}_{\text{rent}} + \underbrace{\left(\int_{\bar{\omega}_{B,t-1}} \omega \, d\Gamma_{\omega,B}\right) q_t \hat{H}_{t-1}}_{\text{owner surplus}} + \underbrace{T_{B,t}}_{\text{rebates}}, \end{aligned}$$

where $y_{B,t}$ is exogenous outside income and q_t is the rental rate (i.e., the price of housing services). The optimal policy is for all borrowers with owner utility shock $\omega_{i,t} > \bar{\omega}_t$ to choose to buy housing. By market clearing, $\bar{\omega}_{B,t} = \Gamma^{-1}_{\omega,B}(1 - H_{B,t}/\hat{H}_t)$, which ensures that the fraction of borrowers choosing to own is equal to the fraction of borrower-owned housing. Income is taxed at rate τ, while mortgage interest payments are tax deductible.

The laws of motion for the mortgage balance $M_{B,t}$, interest payment $X_{B,t}$, and stock of owned housing $H_{B,t}$ are:

$$
\begin{aligned}
M_{B,t} &= \underbrace{\rho_B M^*_{B,t}}_{\text{new loans}} + \underbrace{(1-\rho_B)(1-\nu_B)\pi^{-1}M_{B,t-1}}_{\text{old loans}} \\
X_{B,t} &= \underbrace{\rho_B r^*_{B,t} M^*_{B,t}}_{\text{new loans}} + \underbrace{(1-\rho_B)(1-\nu_B)\pi^{-1}X_{B,t-1}}_{\text{old loans}} \\
H_{B,t} &= \underbrace{\rho_B H^*_{B,t}}_{\text{new housing}} + \underbrace{(1-\rho_B)H_{B,t-1}}_{\text{old housing}}.
\end{aligned}
$$

Landlord's Problem. The landlord's problem is similar to that of the borrower, with two key exceptions: (i) the landlord only sells housing services to the borrower instead of consuming them, and (ii) the landlord does not use credit — an assumption we relax in Section 8. The landlord's budget constraint is:

$$
c_{L,t} \leq \underbrace{(1-\tau)y_{L,t}}_{\text{after-tax income}} - \underbrace{p_t\left(H_{L,t} - H_{L,t-1}\right)}_{\text{net housing purchases}} - \underbrace{\delta p_t H_{L,t-1}}_{\text{maintenance}} + \underbrace{q_t H_{L,t-1}}_{\text{rent}} + \underbrace{\left(\int_{\bar{\omega}_{L,t-1}} \omega \, d\Gamma_{\omega,L}\right) q_t \hat{H}_{t-1}}_{\text{owner surplus}} + \underbrace{T_{L,t}}_{\text{rebates}},
$$

and the market clearing condition $\bar{\omega}_{L,t} = \Gamma^{-1}_{\omega,L}(1 - H_{L,t}/\hat{H}_t)$.

Saver's Problem. The saver's budget constraint is:

$$
\begin{aligned}
c_{S,t} \leq\ & \underbrace{(1-\tau)y_{S,t}}_{\text{after-tax income}} - \underbrace{p_t\left(H^*_{S,t} - H_{S,t-1}\right)}_{\text{net housing purchases}} - \underbrace{\delta p_t H_{S,t-1}}_{\text{maintenance}} + \underbrace{T_{S,t}}_{\text{rebates}} \\
& + \underbrace{\pi^{-1}(\bar{r}_B + \nu_B)M_{B,t-1}}_{\text{total payment}} - \underbrace{\rho_B\left(\exp(\Delta_{B,t})M^*_{B,t} - \pi^{-1}(1-\nu_B)M_{B,t-1}\right)}_{\text{net mortgage iss.}},
\end{aligned}
$$

where the wedge $\Delta_{j,t}$ is a time-varying tax, rebated to the saver lump sum at equilibrium, that allows for time variation in mortgage spreads. A value of $\Delta_{j,t} > 0$ implies that the mortgage rate exceeds the rate on a risk-free bonds with the payment structure, allowing for exogenous variation in mortgage spreads. In addition to the budget constraint, the saver must also satisfy the fixed housing demand constraint $H_{S,t} = \bar{H}_S$ at all times.

Construction Firm's Problem. New housing is produced by competitive construction firms. Similar to Favilukis et al. (2017) and Kaplan et al. (2020), we assume that housing is produced using nondurable goods Z and land L_t according to the technology:

$$I_t = L_t^{\varphi} Z_t^{1-\varphi}$$
$$\bar{H}_t = (1-\delta)\bar{H}_{t-1} + I_t,$$

where $\bar{L}$ units of land permits are auctioned off by the government each period, with the proceeds returned pro-rata to the households. Each construction firm therefore solves:

$$\max_{L_t, Z_t} p_t L_t^{\varphi} Z_t^{1-\varphi} - p_{\text{Land},t} L_t - Z_t,$$

where $p_{\text{Land},t}$ is the equilibrium price of land permits, and the price of nondurables is normalized to unity.

Equilibrium. A competitive equilibrium economy consists of endogenous states $(H_{B,t-1}, M_{B,t-1}, X_{B,t-1}, \bar{H}_{t-1})$, borrower controls $(c_{B,t}, h_{B,t}, M^*_{B,t}, H^*_{B,t})$, landlord controls $(c_{L,t}, H_{L,t})$, saver controls $(c_{S,t}, M^*_{B,t})$, construction firm controls (L_t, Z_t), and prices $(p_t, q_t, r^*_{B,t})$ that jointly solve the borrower, landlord, saver, and construction firm problems, as well as the market clearing conditions:[21]

$$\begin{aligned}
\text{Housing:} &\quad \bar{H}_t = H_{B,t} + H_{L,t} + \bar{H}_S \\
\text{Housing Services:} &\quad \bar{H}_t = h_{B,t} + \bar{H}_S \\
\text{Housing Permits:} &\quad \bar{L} = L_t \\
\text{Resources:} &\quad Y_t = c_{B,t} + c_{L,t} + c_{S,t} + Z_t + \delta p_t \bar{H}_t.
\end{aligned}$$

[21] In a slight abuse of notation we allow both the saver and borrower to choose $M^*_{B,t}$ as controls, and implicitly impose that these values must be equal in equilibrium.

5.1 Key Equilibrium Conditions

We now present the key equilibrium conditions of the model, while reserving the full set of equilibrium conditions to Appendix A.1. These key equations are the optimality conditions for borrower and landlord housing, respectively which correspond to the inverted demand and tenure supply curves:

$$p_t^{\text{Demand}}(H_{B,t}) = \frac{\mathbb{E}_t \left\{ \Lambda_{B,t+1} \left[(1+\bar{\omega}_{B,t}) q_{t+1} + \Big(1 - \delta - (1-\rho_B)\mathcal{C}_{B,t+1}\Big) p_{t+1} \right] \right\}}{1 - \mathcal{C}_{B,t}} \tag{6}$$

$$p_t^{\text{Supply}}(H_{B,t}) = \mathbb{E}_t \left\{ \Lambda_{L,t+1} \Big[(1+\bar{\omega}_{L,t}) q_{t+1} + (1-\delta)\, p_{t+1} \Big] \right\}. \tag{7}$$

p_t^{Demand} is the price at which borrowers are willing to purchase quantity $H_{B,t}$, and p_t^{Supply} is the price at which landlords are willing to provide quantity $H_{B,t}$ to the market, which by market clearing is equivalent to landlords choosing quantity $H_{L,t} = \hat{H}_t - H_{B,t}$ of housing.

These are standard asset pricing equations that state that the asset price is equal to the expected future payoff discounted by the relevant stochastic discount factors, here $\Lambda_{B,t+1}$ for borrowers and $\Lambda_{L,t+1}$ for lenders. The supply schedule (7) simply sets the current price equal to the present value of the next period cash flow (rent) for the marginal landlord $(1 + \bar{\omega}_{L,t})q_{t+1}$ plus the next period value of the housing after maintenance costs. The demand schedule (6) is similar, but is influenced by the ability of borrower housing to collateralize debt, which is valued by borrowers. This enters through the marginal collateral value term $\mathcal{C}_{B,t}$, which represents the shadow value of the additional credit that can be collateralized by an additional dollar of housing (see Section A.1 for details).[22] A relaxation of credit standards or a decrease in the cost of credit allows housing to collateralize more or cheaper credit, raising this marginal value $\mathcal{C}_{B,t}$, and increasing the reservation price. As a result, changes in credit conditions directly shift the demand schedule (6).

These equations map directly into the supply and demand framework of Section 2,

[22] The exact definition is $\mathcal{C}_{B,t} \equiv \mu_{B,t} F_t^{LTV} \theta^{LTV}$ as in Greenwald (2018). An extra dollar of housing can collateralize θ^{LTV} of new debt for an LTV-constrained borrower, of which there are fraction F_t^{LTV}. Finally, the Lagrange multipler $\mu_{B,t}$ on the borrowing constraint converts from the quantity of new credit to the *value* of that credit from the borrower's perspective.

where the demand and tenure supply schedules map into the $\bar{\omega}_{j,t}$ terms. Recall that these terms are defined by $\bar{\omega}_{B,t} = \Gamma_{\omega,B}(1 - H_{B,t}/\hat{H}_t)$ and $\bar{\omega}_{L,t} = \Gamma_{\omega,L}(1 - H_{L,t}/\hat{H}_t)$. As $H_{B,t}$ increases, so does the fraction of owner-occupied housing. This pushes down $\bar{\omega}_{B,t}$, as the marginal household becomes increasingly less suited for ownership, generating a downward sloping demand curve. At the same time, $\bar{\omega}_{L,t}$ rises as the marginal unit becomes more and more favorable for rental, generating an upward sloping supply curve. In equilibrium, the level of owner-occupied housing $H_{B,t}$ adjusts so that $p_t^{\text{Demand}} = p_t^{\text{Supply}}$, and the market clears. The degree of dispersion in the $\Gamma_{\omega,B}$ and $\Gamma_{\omega,L}$ distributions determine how much the $\bar{\omega}_{j,t}$ terms change with the homeownership rate, which governs the slopes of the demand and supply curves, respectively.

6 Model Quantification

We calibrate our model at quarterly frequency to jointly match several targets from the literature along with our key empirical moment, with the full set of calibrated parameters displayed in Table 1. Our main calibration of all parameters except for $\sigma_{\omega,L}$ is presented in Section 6.1, while our calibration of $\sigma_{\omega,L}$ to directly match our regressions in Section 4 follows in Section 6.2.

6.1 Main Calibration

Demographics and Preferences To determine the borrower population share, we use the 1998 Survey of Consumer Finances. In the model, borrowers are constrained households whose choice of rental vs. ownership is influenced by credit conditions. Correspondingly, we identify a household as a "borrower" in the data if it either (i) owns a home and its mortgage balance net of liquid assets is greater than 30% of the home's value, or (ii) does not own a home. We believe both of these groups would likely find it difficult to purchase a home without credit. This procedure yields a population share of $\chi_B = 0.626$ and an income share of $s_B = 0.525$. For landlord demographics, we con-

Table 1: Parameter Values: Baseline Calibration (Quarterly)

Parameter	Name	Value	Internal	Target/Source
Demographics and Preferences				
Borrower pop. share	χ_B	0.626	N	1998 SCF
Borrower inc. share	s_B	0.525	N	1998 SCF
Landlord pop. share	χ_L	0	N	Normalization
Borr. discount factor	β_B	0.974	Y	PMI Rate (see text)
Saver discount factor	β_S	0.992	Y	Nom. interest rate = 6.46%
Landlord discount factor	β_L	0.974	Y	Equal to β_B
Housing utility weight	ξ	0.2	N	Davis and Ortalo-Magné (2011)
Saver housing demand	$\bar{H}_S$	5.299	Y	Steady state optimum
Ownership Benefit Heterogeneity				
Landlord het. (location)	$\mu_{\omega,L}$	-0.109	Y	Avg. homeownership rate
Landlord het. (scale)	$\sigma_{\omega,L}$	2.877	Y	Empirical elasticities (Section 6)
Borr. het. (location)	$\mu_{\omega,B}$	0.217	Y	Borr. VTI (1998 SCF)
Borr. het. (scale)	$\sigma_{\omega,B}$	0.319	Y	Implied subsidy (see text)
Technology and Government				
New land per period	$\bar{L}$	0.090	Y	Residential inv = 5% of GDP
Land share of construction	φ	0.371	N	Res inv. elasticity in boom
Housing depreciation	δ	0.005	N	Standard
Inflation	$\bar{\pi}$	1.008	N	3.22% Annualized
Tax rate	τ	0.204	N	Standard
Mortgage Contracts				
Refinancing rate	$\bar{\rho}$	0.034	N	Greenwald (2018)
Loan amortization	ν	0.45%	N	Greenwald (2018)
Borr. LTV Limit	θ_B^{LTV}	0.85	N	Greenwald (2018)
Borr. PTI Limit	θ_B^{PTI}	0.36	N	Greenwald (2018)
Borr. PTI offset (taxes etc.)	α_B	0.09%	N	Greenwald (2018)
Landlord LTV Limit	θ_L^{LTV}	0.000	N	No landlord credit

sider the limit $\chi_L \to 0$ and assume that landlords do not receive labor income, instead subsisting entirely on their rental earnings.[23]

For preferences, the key parameter is the borrower's discount factor, β_B, which determines the level latent demand for credit in the economy, and in turn, how much a relaxation of credit will influence household demand for owned housing. We infer this parameter from the pricing on private mortgage insurance (PMI) — the additional fees

[23]Because landlord utility is linear in consumption, assumptions about their income and consumption have essentially no impact on the results.

and interest rates that a borrower must pay in order to obtain a high-LTV loan. This approach is motivated by the fact that many borrowers choose to pay for PMI, while many do not, meaning that the typical borrower should be close to indifferent.[24] We choose β_B so that the typical borrower would be indifferent between receiving a loan at 80% LTV, and paying the exact FHA insurance scheme for a loan at 95% LTV: an up front fee of 1.75% of the loan, plus a spread of 80 basis points.[25]

For the other preference parameters, we assume a standard consumption weight parameter of $\xi = 0.2$ on housing, following the evidence in Davis and Ortalo-Magné (2011). We set the saver discount factor to target a nominal interest rate of 6.46%, equal to the average rate on 10-year Treasury Bonds in the immediate pre-boom era (1993 - 1997). We set the saver's fixed level of demand $\bar{H}_S$ equal to the level they would choose in steady state at prevailing prices. This implies that while saver demand is fixed in the short run, it is at the correct "long run" equilibrium value. Last, we set the landlord discount rate β_L to be equal to β_B. This calibration ensures that both borrowers and landlords discount future housing services and rental cash flows at essentially equal rates. As a result, shocks that shift the path of future rents will affect borrowers and landlords symmetrically, and have little impact on the equilibrium homeownership rate.[26]

Ownership Cost Heterogeneity. The paper's most novel modeling mechanism relates to heterogeneity in the benefits to borrower and landlord ownership, represented by the distributions $\Gamma_{\omega,B}$ and $\Gamma_{\omega,L}$. We specify each of these as a logistic distribution, so that each

[24]For example, 37.7% of Fannie Mae purchase loans required PMI over the 1999-2008 boom period (source Fannie Mae Single Family Dataset).

[25]We choose the FHA scheme because it is much simpler to implement in the model than the GSE insurance scheme, where pricing is less transparent, and insurance premia are only paid until the borrower's LTV drops below 80%. However, the overall costs of the two forms of insurance are similar, as can be seen in e.g., Goodman and Kaul (2017).

[26]For example, if borrower and landlord discount factors differ, shocks to expectations about future rents will cause a large shift in ownership toward the type with a higher discount factor. In the absence of additional disciplining information about the relative discount factors, we seek a calibration that avoids these seemingly arbitrary shifts.

c.d.f. is defined by:

$$\Gamma_{\omega,j}(\omega) = \left[1 + \exp\left\{-\left(\frac{\omega - \mu_{\omega,j}}{\sigma_{\omega,j}}\right)\right\}\right]^{-1} \qquad j \in \{B, L\}.$$

For the borrower distribution, we set $\mu_{\omega,B}$, the typical ownership utility for borrowers, to target the average ratio of home value to income among borrowers who own homes in the 1998 SCF, equal to 8.81 (quarterly).

We next calibrate the ownership dispersion parameter $\sigma_{\omega,B}$, which determines the rate at which borrower households would switch between owning and renting as the price of housing changes. Because $\sigma_{\omega,B}$ must be identified from shifts in tenure supply (shocks to house prices exogenous to borrower demand), our empirical estimates in Section 4 using shifts in demand are not informative about this parameter. Instead, we use the empirical estimates of Berger, Turner, and Zwick (2020), who study the impact of the First Time Home Buyer credit, a 2009 - 2010 policy that subsidized the purchase of housing by up to 10% (capped at $8,000) for renter households that did not own in the three prior years.

Berger et al. (2020) estimate that this subsidy led 3.2% of eligible renters to switch to ownership during the policy window (February 17, 2009 to July 1, 2010), a rate of 0.64% per quarter. For our calibration, we choose $\sigma_{\omega,B} = 0.319$ so that exactly 0.64% of renters (equal to 0.64% / 3.4% = 18.8% of "active" renters) would switch from renting to owning if their housing purchases were given a 10% subsidy. While this calibration strategy ignores some of the more nuanced details around this episode, the borrower dispersion parameter $\sigma_{\omega,B}$ is much less important for our results than the landlord dispersion parameter $\sigma_{\omega,L}$.[27] This is because our core experiments shift the demand curve, then travel along the supply curve, making the supply slope much more influential than the demand slope (see Appendix Figure A.1 for sensitivity analysis).

For the landlord, we set $\mu_{\omega,L}$ to attain the correct homeownership rate among "bor-

[27]Such details include that not all households received the full subsidy due to income and value caps, the finding by Berger et al. (2020) that the policy increased house prices, offsetting part of the subsidy, and the potential that the share of "active" households in this episode differed from our steady state level of 3.4%.

rowers" in the 1998 SCF (49.64%). Since all savers own in the model, this ensures an overall homeownership rate of 68.50% at steady state, matching the 1998 SCF. Finally, the landlord dispersion term $\sigma_{\omega,L}$ — the key parameter in the model — is calibrated to match our empirical estimates from Section 4, as described in detail in Section 6.2 below.

Technology and Government. For the construction technology, we set the amount of new land permits issued per period so that residential investment Z_t is 5% of total output in steady state. For the land weight in the construction function φ, we note that $\varphi/(1-\varphi)$ is the elasticity of residential investment to house prices, and choose 0.371 so that this elasticity is equal to the ratio of the peak log increase in the residential investment share of output to the peak log increase in prices over the boom. We set housing depreciation and the tax rate to standard values, and set inflation to be equal to the average 10-year inflation expectation in the pre-boom era (1993-1997) following Greenwald (2018).

Mortgage Contracts. For the mortgage contract parameters, we follow Greenwald (2018), who provides a detailed calibration for this mortgage structure.

6.2 Calibration of Landlord Heterogeneity to Our Empirical Results

We calibrate the dispersion in the landlord's ownership cost $\sigma_{\omega,L}$ — the model's key parameter governing the slope of credit supply and the response of house prices to a change in credit conditions — so that the model is able to reproduce as closely as possible the responses to an identified credit shock that we estimate in Section 4. In particular, we target our estimates from the LS IRF of the price-rent ratio and homeownership rate displayed in Panels (a) and (b) of Figure 3. We focus on the LS results because they are the most statistically precise, stem from a shock that is more straightforward to map into the model, and produce responses that are measured in the ideal price-rent ratio outcome variable, rather than the house price outcomes used with the DK and MS results.

Since a change in the conforming loan limit effectively adds a subsidy to the newly included mortgages, the model analogue of the empirical regression is a linearized impulse response following a shock to the mortgage spread $\Delta_{B,t}$, which we assume follows an AR(1). Once we compute our model impulse responses at quarterly frequency, we take annual averages to map our model impulse responses into the annual frequency used in our empirical regression. We then choose $\sigma_{\omega,L}$ along with the size and persistence of the shock to the mortgage spreads $\Delta_{B,t}$ corresponding to the change in the conforming loan limit to minimize the distance between the model and data impulse responses. The size and persistence of the shock to mortgage spreads can be considered nuisance parameters that are not of direct interest for our experiments, but important to pin down the scale and temporal shape of the shock response, which are not directly identified by $\sigma_{\omega,L}$.

We set these three parameters to minimize the squared error between the model and the data scaled by the statistical uncertainty around the empirical estimates, summarized by the objective function:

$$Q = \sum_{v \in \{PR, HOR\}} \sum_{k=2}^{5} \left(\frac{IRF_{v,k}^{Model} - \beta_{v,k}}{SE_{v,k}} \right)^2, \tag{8}$$

where $IRF_{v,k}^{Model}$ is the model impulse response for variable v (either the price-rent ratio or the homeownership rate), $\beta_{v,k}$ is the corresponding estimate from Figure 3, and $SE_{v,k}$ is the estimated standard error of $\beta_{v,k}$. We restrict our estimation to the response at horizons from two years to five years. We do so because our model produces responses that jump on impact. This is typical in models of frictionless housing markets but contrasts with the data, where house prices and price-rent ratios typically display hump-shaped responses and momentum due to search and other frictions. Including the first two periods in our estimation would thus lead the model to "compromise" by systematically understating price-rent ratio growth at horizons of two to five years, in an attempt to reduce errors the first two periods. To avoid this bias due to misspecification, we therefore only ask the model to fit the data in periods after the two-year peak.

Our procedure estimates a landlord cost dispersion ($\sigma_{\omega,L}$) of 2.877, a mortgage spread

Figure 6: Impulse Responses: Model vs. Data

(a) Price-Rent Ratio

(b) Homeownership Rate

(c) Inverse Ratio (Bands)

(d) Inverse Ratio (Model Comparison)

Notes: Grey squares and bands indicate our benchmark LS estimates from Figure 3 and their 95% confidence intervals. These empirical estimates are plotted alongside the corresponding outputs of each model. For each model, we compute a quarterly impulse response, then average over each year to obtain annual responses. Panel (a) above corresponds to Panel (a) of Figure 3, while Panel (b) above corresponds to Panel (b) of Figure 3, and Panels (c) and (d) correspond to Panel (d) of Figure 3. Shaded bands indicate the range of outcomes from the lower bound estimate of $\sigma_{\omega,L} = 0.810$, obtained from a least squares fit of the top of the 95% empirical confidence intervals in Panel (c), to the upper bound estimate of $\sigma_{\omega,L} = \infty$, equivalent to the Full Segmentation case.

shock persistence of 0.965, and a mortgage spread shock size of -0.041, where a negative shock size captures that spreads fall due to the subsidy. Our persistence and shock size estimates indicate a persistent but non-permanent shock, and an annualized CLL subsidy of 17bp, which falls in the typical range of 10bp - 24bp found in the literature (Adelino, Schoar, and Severino, 2012).

To interpret our estimate of $\sigma_{\omega,L}$, Figure 6 displays our estimated empirical IRFs alongside the IRFs obtained from a model at our minimum-distance estimate (henceforth the "Benchmark" model), as well as two polar economies, one with "Full Segmentation", corresponding to $\sigma_{\omega,L} \to \infty$, and one with "No Segmentation," corresponding to $\sigma_{\omega,L} = 0$. These polar economies correspond to the perfectly inelastic and perfectly elastic tenure supply examples in Section 2, respectively. To isolate the role of our main parameter, we only vary the value of $\sigma_{\omega,L}$, and use the same estimates for the persistence and size of the shock across all three economies.

Panels (a) and (b) display results for the price-rent ratio and homeownership rate. Our estimation is successful, as the Benchmark model (red line) delivers a close fit of the empirical point estimates. The fit on the price-rent ratio is extremely close, while errors are slightly larger for the homeownership rate, reflecting the lower statistical precision around these estimates. In terms of slope, the Benchmark model delivers a response of the price-rent ratio that is between 6.98 and 9.31 times that of the homeownership rate depending on the horizon. Turning to polar models for comparison, the Full Segmentation model (blue line) delivers a nearly identical path of the price-rent ratio, showing that our Benchmark estimates imply heavy frictions and a large degree of segmentation. However, the Full Segmentation model fails to deliver any increase in the homeownership rate. Last, the No Segmentation model delivers a much smaller rise in the price-rent ratio, and a much larger rise in the homeownership rate, in line with the intuition from Section 2.

We next compute a "credible set" for $\sigma_{\omega,L}$ that reflects our 95% confidence interval for the inverse tenure supply slope estimates in Figure 3 Panel (d).[28] We choose values of

[28]We compute our credible set based on these ratio estimates, rather than the individual IRFs for two reasons. First, these estimates jointly summarize uncertainty about both the price-rent ratio response and the homeownership rate response into a single statistic at each horizon, which would otherwise be nontrivial to combine. Second, part of standard errors for the individual IRFs represents uncertainty about the absolute scale of the shock rather than the *relative* size of the responses. Removing the size of the shock as a nuisance parameter allows for more statistical precision.

$\sigma_{\omega,L}$ to minimize the distance to the upper and lower ends of the 95% confidence interval:

$$Q_{UB} = \sum_{k=2}^{5} \left(\frac{IRF_{IR,k}^{Model} - (\beta_{IR,k} + 1.96 \times SE_{IR,k})}{SE_{v,k}} \right)^2$$
$$Q_{LB} = \sum_{k=2}^{5} \left(\frac{IRF_{IR,k}^{Model} - (\beta_{IR,k} - 1.96 \times SE_{IR,k})}{SE_{v,k}} \right)^2,$$

where "IR" denotes the inverse slope ratio. For both re-estimations we hold the persistence of the shock constant at our prior estimate, while the shock size is irrelevant for the computation of this inverse ratio.

The resulting estimates are displayed as the shaded area in Figure 6 Panel (c). The upper bound, targeting the tops of the confidence intervals, yields an estimate of $\sigma_{\omega,L} = 0.810$, while the lower bound, targeting the bottoms of the confidence intervals, is most closely matched using the Full Segmentation case $\sigma \to \infty$. The figure shows that our Benchmark calibration provides a close fit of the inverse ratio as well.[29]

To interpret the magnitudes implied by our credible set, Panel (d) displays the credible set and Benchmark responses for the inverse ratio alongside the responses from our No Segmentation and Full Segmentation economies. While the Full Segmentation economy forms the lower bound of our credible set by construction, the No Segmentation economy falls far outside of the credible set, with inverse ratios between 6 and 13 times those of our credible set upper bound, and between 4 and 32 times the upper bounds of our empirical confidence intervals, depending on the horizon.

[29] In principle, matching the separate price-rent ratio and homeownership rate IRFs in Figure 3 Panels (a) and (b) and matching the ratio responses in Panel (d) are both theoretically valid approaches. In practice, directly fitting the ratio responses delivers a slope so steep it is effectively identical to full segmentation. By matching the IRFs separately we are therefore opting for the more conservative set of estimates in terms of the influence of credit on house prices.

7 Model Results

Now that we have calibrated the model to match our empirical results, we run a series of experiments to quantitatively assess the role that credit played in the 2000s housing boom. We describe our experiments and results in detail below, and provide a summary table across all of our boom bust experiments in Table 2, as well as robustness of our main experiments to alternative values of $\sigma_{\omega,L}$ in Appendix Table A.1.

To begin, we simulate a realistic relaxation of credit standards and evaluate the model's implications for the evolution of debt and house prices. Our baseline experiment follows KMV in relaxing LTV limits from 85% to 99% and PTI limits from 36% to 65% unexpectedly and permanently in 1998 Q1. The new standards are left in place until 2007 Q1, at which time they unexpectedly and permanently revert to their original values. The model responses are computed as nonlinear perfect foresight paths.

The results of this experiment are shown visually in Figure 7. To highlight the role of landlord heterogeneity, we again plot the responses in our Benchmark model against the polar No Segmentation (perfectly elastic supply) and Full Segmentation (perfectly inelastic supply) alternatives. As in Figure 6, the shaded bands account for the credible set for $\sigma_{\omega,L}$. Our Benchmark model displays a large price response to the credit standard shifts, accounting for 34% of the peak rise in price-rent ratios observed in the boom, while a model setting $\sigma_{\omega,L}$ to the lower bound of the credible set would explain 26%. This stands in sharp contrast to the No Segmentation model, where the same credit relaxation explains -1% of the peak growth in price-rent ratios, as landlords are able to completely satisfy the increase in demand, preventing a rise in prices. Instead, house price dynamics in the Benchmark model are much closer to the Full Segmentation model, where this credit relaxation would account for 38% of the observed rise in price-rent ratios.

This finding for house prices also has important implications for credit growth. While credit standards are loosened equally for all three cases depicted in Figure 7, credit growth over the boom is much larger in the Benchmark economy relative to the No Segmenta-

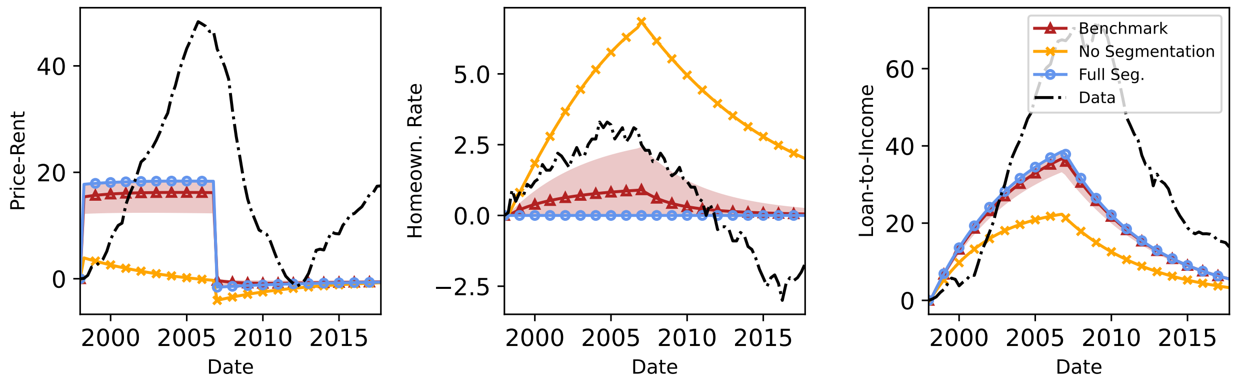
Table 2: Results, Boom Experiments

Experiment	Price-Rent	Homeown.	Loan-Inc.
Peak Data Increase	**48.3%**	**3.3pp**	**72.2%**
Credit Relaxation (Share of Peak Data Increase)			
Full Segmentation	38%	0%	53%
Benchmark	34%	27%	51%
Est. Lower Bound	26%	71%	46%
No Segmentation	-1%	201%	31%
Credit Relaxation + Decline in Rates (Share of Peak Data Increase)			
Full Segmentation	82%	0%	86%
Benchmark	72%	53%	80%
Est. Lower Bound	56%	135%	70%
No Segmentation	4%	353%	38%
Removing Credit Relaxation from Full Boom (Share of Peak Data Increase)			
Full Boom (Benchmark)	100%	100%	98%
No Credit Relaxation (Benchmark)	45%	59%	26%
Full Boom (No Segmentation)	100%	100%	103%
No Credit Relaxation (No Segmentation)	95%	-230%	50%
Credit Relaxation + Decline in Rates: Extensions (Share of Peak Data Increase)			
Landlord Credit, Not Recalibrated	81%	8%	80%
Landlord Credit, Recalibrated	80%	21%	80%
Saver Demand, Not Recalibrated	52%	63%	98%
Saver Demand, Recalibrated	54%	30%	102%

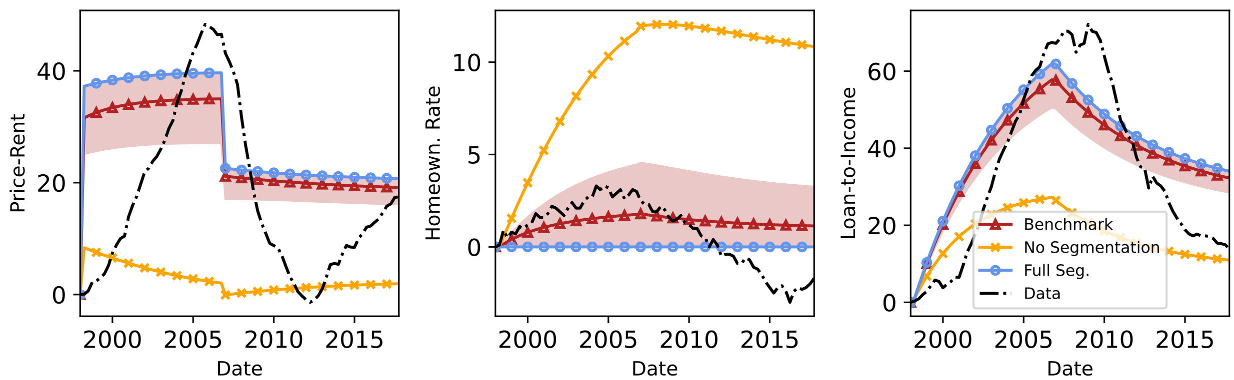
Notes: This table summarizes the results from the various nonlinear transition experiments in Sections 7 and 8. “Price-Rent” is the price-rent ratio, “Homeown.” is the homeownership rate, and “Loan-Inc.” is the aggregate loan to income ratio. The top row displays the actual changes in these variables, in levels from 1998:Q1 to the peak of each series during the boom period (2006 - 2008). The remaining numbers below display the shares of these peak increases explained by each model-experiment combination, calculated from 1998:Q1 to the peak of each model boom in 2007:Q1. The loan-to-income ratio is the ratio of household debt (FRED code: HMLBSHNO) to household gross income (FRED code: PI) in the Flow of Funds. For other data definitions see the notes for Figure 1.

tion economy, explaining 51% and 31% of the observed rise, respectively. This additional credit growth is a direct consequence of the larger house price appreciation in the Benchmark economy, which increases the value of housing collateral and allows larger loans for a given maximum LTV ratio. Consequently, the same credit loosening leads to much

Figure 7: Credit Relaxation Experiment



(a) Credit Standards Only



(b) Credit Standards + Falling Rates

Notes: Each panel displays perfect foresight paths for the price-rent ratio, homeownership rate, and loan-to-income ratio, respectively to relaxing LTV and PTI constraints. The "Benchmark" model sets a value of $\sigma_{\omega,L}$ calibrated to match our empirical IRFs as in Section 6, while the "No Segmentation" model sets $\sigma_{\omega,L} = 0$ and the "Full Segmentation" model sets $\sigma_{\omega,L} \to \infty$. Shaded bands indicate the range of outcomes from the lower bound estimate of $\sigma_{\omega,L} = 0.810$, obtained from a least squares fit of the top of the 95% empirical confidence intervals in Panel (c), to the upper bound estimate of $\sigma_{\omega,L} = \infty$, equivalent to the Full Segmentation case. Results are summarized numerically in Table 2. The loan-to-income ratio is the ratio of household debt (FRED code: HMLBSHNO) to household gross income (FRED code: PI) in the Flow of Funds. For other data definitions see the notes for Figure 1.

more levered households in the Benchmark economy when credit conditions return to baseline. As before, the Full Segmentation response falls very close to the Benchmark path.

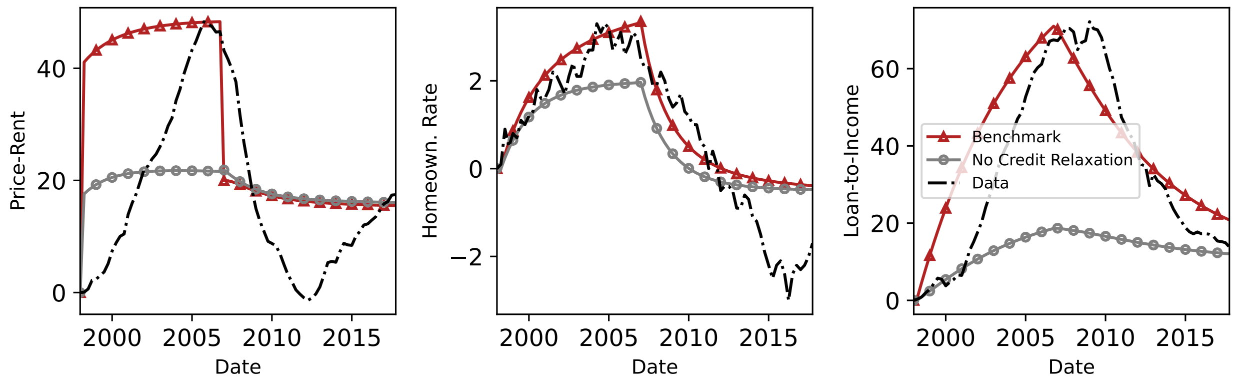
For a more comprehensive view of the role of credit, we next incorporate an additional 2ppt fall in mortgage spreads, assumed to be permanent, which reflects secular declines in interest rates over the boom period. This causes an outward shift of housing demand, which, given our estimated rental frictions generates a large additional increase in house prices. Combining the relaxation in LTV and PTI limits and the fall in rates can explain 72% of the observed rise in price-rent ratios and 80% of the rise in loan-to-income ratios, shown in Figure 7 Panel (b). These results again stand in contrast to the 4% and 38% shares explained in the No Segmentation model, in which neither the price nor quantity of credit is an important determinant of the price-rent ratio.

Beyond changes in credit prices and standards, non-credit factors such as overoptimistic house price expectations are also widely believed to have played a major role in driving the boom (see e.g., Kaplan et al. (2020)). Since strong interactions can be present between credit conditions and these non-credit factors (Greenwald (2018)), we now incorporate these residual non-credit factors on top of the relaxation in LTV and PTI constraints and 2% decline in mortgage rates to create a "Full Boom" experiment. We follow the intuition in Section 2 that for any credit shock and supply curve slope we can use additional shifts to the demand and tenure supply curves to exactly match the total increase in both the price-rent ratio and in the homeownership rate over the complete boom period. We implement these shocks as level shifts in the ownership utility distributions through changes in $\mu_{\omega,B}$ and $\mu_{\omega,L}$, respectively, that are assumed permanent during the boom, then revert to their original values in the bust. To complete this "Full Boom" experiment, we incorporate additional features relevant to the bust: a further 3ppt fall in both mortgage rates and the landlord discount rate, consistent with a broad decline in long-term interest rates, and a 10% tightening of both LTV and PTI limits, consistent with a further tightening of credit standards.[30]

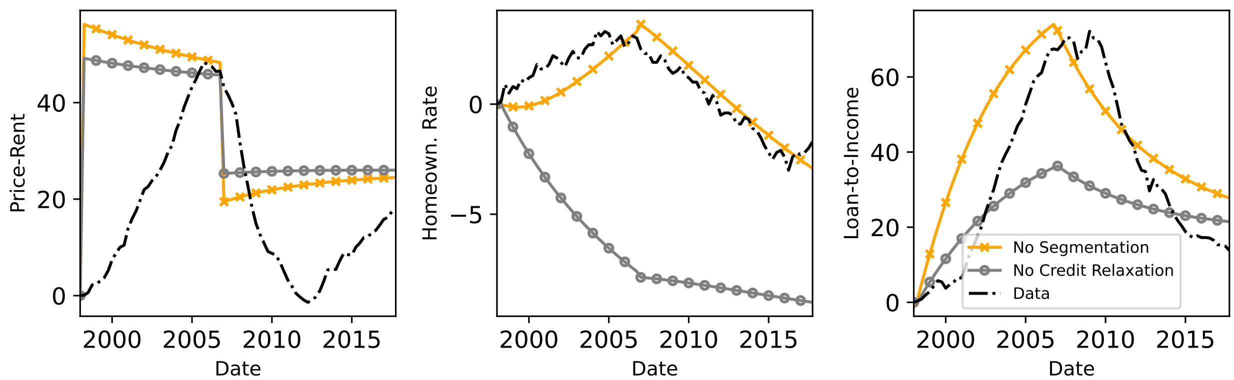
The resulting transition paths are plotted in Figure 8, Panel (a). Overall, these assumptions generate a reasonably good fit of the dynamics of the boom and bust, with two main exceptions: (i) house prices jump in the model rather than adjust sluggishly in the data,

[30] This is best interpreted as increasing standards for credit scores preventing a fraction of the population from obtaining credit at the extensive margin, rather than a decline in maximum LTV and PTI ratios at the intensive margin.

Figure 8: Full Boom Experiment



(a) Benchmark Economy



(b) No Segmentation Economy

Notes: Plots display perfect foresight paths. The "Benchmark" model sets a value of $\sigma_{\omega,L}$ calibrated to match our empirical IRFs as in Section 6, while the "No Segmentation" model sets $\sigma_{\omega,L} = 0$. For each set of plots, the colored plots display an experiment imposing a credit relaxation, decline in the interest rate, and level shifts to the demand and supply curves ($\mu_{\omega,B}$ and $\mu_{\omega,L}$), with these level shifts chosen to exactly match the peak growth of price-rent ratios and the homeownership rate over the housing boom. In each panel, the "No Credit Relaxation" responses display an alternative experiment showing the same decline in the mortgage rate, and level shifts to our demand and supply curves while removing the relaxation of credit standards. Results are summarized numerically in Table 2. The loan-to-income ratio is the ratio of household debt (FRED code: HMLBSHNO) to household gross income (FRED code: PI) in the Flow of Funds. For other data definitions see the notes for Figure 1.

as is typically found in models lacking frictions; and (ii) our model "bust" is much more gradual in the model relative to the data, as we lack the foreclosures and financial market features that transformed the housing crash into a global financial crisis.

To measure the total contribution of credit conditions in this simulated boom-bust, we then remove the simulated credit expansion, while leaving all the other factors in place, to generate the series labeled "No Credit Relaxation." We find that removing the credit expansion from our Full Boom experiment would have reduced the overall rise in price-rent ratios by 55% and in loan-to-income ratios by 74% in our Benchmark economy. These shares, which provide the upper bound for our estimated role of credit during the boom-bust, are larger than the shares explained by relaxing credit in isolation (34% and 51%, respectively), because loose credit amplifies the role of non-credit demand factors. The simple intuition, discussed at length in Greenwald (2018), is that even if borrower households perceive large gains to ownership, they lack the financial resources to pay for large fractions of their housing purchases in cash. When expectations rise without the relaxation in credit standards, binding PTI limits constrain households' ability to finance these properties, dampening the response of price and credit growth relative to the case where e.g., expectations rise and PTI limits are relaxed.

This final set of results indicates that macroprudential policy that restricts credit through LTV and PTI limits is effective at restraining a housing boom. This finding depends heavily on our key parameter $\sigma_{\omega,L}$ and the slope of the supply curve. To show this, Figure 8 Panel (b) replicates the exact same experiment using the frictionless No Segmentation model. As before, we add a set of demand and supply shocks to our relaxation of credit and fall in rates to exactly recreate the entire boom in price-rent ratios and homeownership, and then solve a second transition that removes the relaxation of credit standards. In the absence of rental market frictions, however, removing credit standard relaxation reduces the increase in price-rent ratios by only 5%. Because it fails to stem the rise in collateral values, this tight credit counterfactual is also much less effective at reducing credit growth, with the rise in loan-to-income ratios reduced by only 50%. The fact that alternate calibrations can fully explain the rise in price-rent ratios and homeownership rates while having strikingly different implications for the effectiveness of macroprudential policy illustrates the importance of the tenure supply curve slope to discipline models.

To summarize our results, our calibrated model implies an important role for credit conditions in explaining the housing and credit cycles observed in the 2000s boom-bust. A relaxation of credit standards can explain roughly between one third and one half of the rise in price-rent ratios, depending on the order it is added relative to other shocks — results that are closer to the extreme of full segmentation than to a frictionless model with no landlord heterogeneity.

8 Model Extensions

In the model as presented so far, the only credit insensitive agents who could enter the owner-occupied market are deep-pocketed landlords who face heterogenous costs in converting properties between owner-occupied and renter-occupied. While this assumption makes the economics of the model transparent, it is clearly an abstraction, as many landlords also face financial constraints in reality. As discussed earlier, we have also abstracted from trade in housing between borrowers and savers. In this section, we extend the model to relax each of these assumptions in turn.

8.1 Landlord Credit

In practice, landlords are not deep-pocketed, and the vast majority of investor-owned properties are purchased with mortgages. To capture this, we now allow landlords to purchase properties with mortgage credit that is affected by changes in credit supply.

We begin by reassessing the intuition developed in the supply and demand framework in Section 2. Recall that a credit relaxation shifts the demand curve but not the tenure supply curve, causing movement in the price-rent ratio, the homeownership rate, or both, as the equilibrium travels up the supply curve in Figure 2 Panel (c). Introducing credit for landlords implies that a relaxation in credit not only shifts the demand curve upward but also shifts the tenure supply curve upward, as in Figure 2 Panel (d). The degree of the supply curve shift is endogenous and depends on the model's parameters. Adding landlord credit to the baseline model while holding the parameters fixed will lead to a smaller (or potentially even negative) change in the homeownership rate and a larger change in the price-rent ratio. This is represented by a shift from the solid supply curve to the dashed supply curve in Figure 2 Panel (d).

To illustrate this intuition quantitatively, we implement a version of the model with landlord credit, with a full description in Appendix A.2. We assume that landlords use a parallel borrowing technology to borrowers, with an LTV limit of 65% (a standard constraint for multi-family construction loans) and no PTI limit.[31] We first solve this extension using our Benchmark value of $\sigma_{\omega,L}$, then recalibrate $\sigma_{\omega,L}$ under the new model following the procedure in Section 6. This recalibration requires an adjustment as roughly half of rental units are located in multifamily buildings too large to be affected by the changes in the CLL on which the LS instrument is based (see Appendix A.2 for details).

Figure 9 displays the results from an experiment analogous to that of Figure 7 Panel (b), which both relaxes credit conditions and allows interest rates to fall, with summary statistics again displayed in Table 2. To provide a quantitative example of a loosening of landlord credit, we assume that landlord mortgages face an equal decline in rates and that landlord credit also expands to a new LTV limit of 85% during the boom.

The resulting responses show that, holding parameters fixed (the path denoted "Landlord Credit (No Recal)"), adding landlord credit increases the response of the price-rent ratio, explaining 81% of the rise observed in the data, compared to 72% for the Benchmark model. At the same time, the landlord credit model features a smaller rise in the homeownership rate, explaining only 8% of the rise in the data, compared to 53% for the Benchmark model. These results are consistent with the intuition in Figure 2 Panel (d).

The results holding parameters fixed would, however, make the model inconsistent with our key empirical moment, which now reflects a locus of equilibria rather than the slope of the demand curve. To address this, we repeat our exercise in Section 6 to recalibrate $\sigma_{\omega,L}$ for the landlord credit model. The resulting responses, denoted "Landlord Credit (Recalibrated)," follow a very similar pattern, explaining 80% of the observed rise

[31] These assumptions correspond to the parameter values $\theta_L^{LTV} = 0.65$ and $\theta_L^{PTI} = \infty$, and imply $F_{L,t}^{LTV} = 1$.

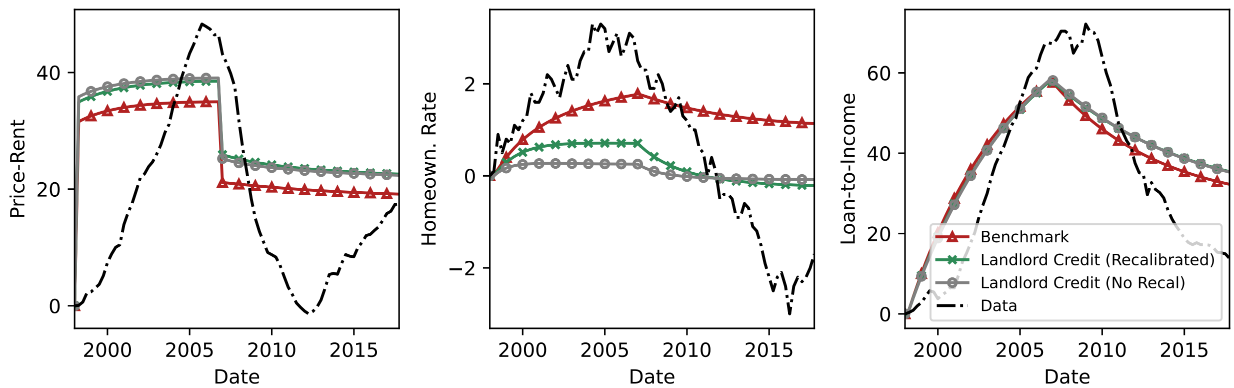


Figure 9: Credit Standards + Falling Rates Experiment, Landlord Credit Extension

Notes: Plots display perfect foresight paths following a relaxation of credit standards and a decline in interest rates. The "Benchmark" model sets a value of $\sigma_{\omega,L}$ calibrated to match our empirical IRFs as in Section 6. The "Landlord Credit (No Recal)" model applies the landlord credit extension holding $\sigma_{\omega,L}$ fixed as in our Benchmark calibration, while the "Landlord Credit (Recalibrated)" model applies the same extension while recalibrating $\sigma_{\omega,L}$ under the new model. Results are summarized numerically in Table 2. The loan-to-income ratio is the ratio of household debt (FRED code: HMLBSHNO) to household gross income (FRED code: PI) in the Flow of Funds. For other data definitions see the notes for Figure 1.

in the price-rent ratio, and 21% of the rise in the homeownership rate.

Overall, these results indicate that incorporating landlord credit and its relaxation during the housing boom period would strengthen the role of credit in driving house prices. As a result, we believe that our Benchmark calibration is, if anything, conservative, and should provide a lower bound on the true contribution of credit over this period.

8.2 Saver Housing Demand

Our baseline model also assumes that housing demand by unconstrained households ("savers") is fixed. Because these savers have a relatively constant marginal utility and are not credit constrained at the margin, if their housing is not segmented from that of the borrowers then they will make the supply curve more elastic. Intuitively, they absorb or supply housing to the constrained borrower households as credit supply fluctuates. Examples of models with this feature include Justiniano et al. (2015), Kaplan et al. (2020), and Kiyotaki et al. (2011).

To embed this mechanism in our model, we relax our assumption of fixed saver demand $H_{s,t} = \bar{H}_s$ and allow savers to freely trade housing. This implies the additional optimality condition from the saver first order condition:

$$p_t^{\text{Saver}} = \mathbb{E}_t \left\{ \Lambda_{t+1}^S \left[(1 + \bar{\omega}_S) \underbrace{u_{h,t}^S / u_{c,t}^S}_{\text{housing services}} + \underbrace{\left(1 - \delta\right) p_{t+1}}_{\text{continuation value}} \right] \right\}. \tag{9}$$

This expression is nearly identical to the borrower's condition (6) with two exceptions. First, the collateral value term $\mathcal{C}$ is equal to zero, as the saver does not use credit. Second, for this extension we assume no saver heterogeneity, so that $\bar{\omega}_S$ is a fixed parameter. Since heterogeneity would steepen the slope of the saver demand curve, diminishing their ability to absorb changes in borrower demand, these results should be considered an upper bound on the role of savers. Under this assumption, reaching an equilibrium where $p^{Saver} = p^{Demand} = p^{Supply}$ occurs entirely through changes in saver housing H_S, which adjusts the marginal utility term $u_{h,t}^S / u_{c,t}^S$.

Figure 10 compares the response to our experiment in which credit standards are loosened and interest rates fall between our Benchmark calibration and this saver demand extension. As before, we plot one version holding $\sigma_{\omega,L}$ fixed ("Not Recalibrated") and a second version ("Recalibrated") after repeating the $\sigma_{\omega,L}$ calibration procedure in Section 6. Beginning with the non-recalibrated response, we observe that the rise in price-rent ratios is diminished as savers react to the rise in prices by selling portions of their housing stock to borrowers, absorbing demand. Since these savers are still homeowners, just with smaller houses, there is no major change in the response of the homeownership rate.

However, introducing savers while holding $\sigma_{\omega,L}$ fixed severely worsens the model's fit of our empirical IRFs in Section 3.2. Specifically, the price-rent ratio increases by too little relative to the homeownership rate. Recalibrating $\sigma_{\omega,L}$ to restore this fit yields the "Recalibrated" response. This recalibration yields a slightly larger rise in price-rent ratios to the non-recalibrated saver model, and a much smaller change in the homeownership

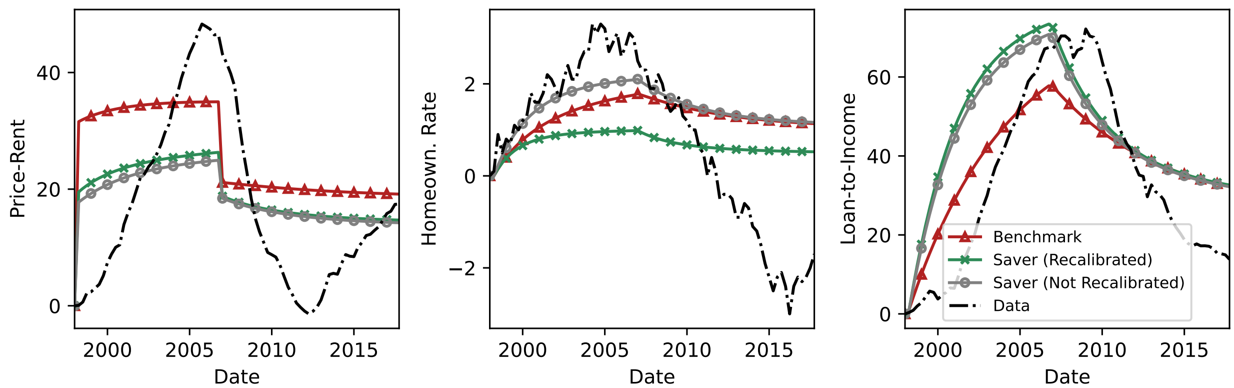


Figure 10: Credit Standards + Falling Rates Experiment, Saver Demand Extension

Notes: Plots display perfect foresight paths following a relaxation of credit standards and a decline in interest rates. The "Benchmark" model sets a value of $\sigma_{\omega,L}$ calibrated to match our empirical IRFs as in Section 6. The "Saver (Not Recalibrated)" model applies the flexible saver demand extension holding $\sigma_{\omega,L}$ fixed as in our Benchmark calibration, while the "Saver (Recalibrated)" model applies the same extension while recalibrating $\sigma_{\omega,L}$ under the new model. Results are summarized numerically in Table 2. The loan-to-income ratio is the ratio of household debt (FRED code: HMLBSHNO) to household gross income (FRED code: PI) in the Flow of Funds. For other data definitions see the notes for Figure 1.

rate, effectively restoring the correct ratio.[32] Even with a perfectly frictionless saver margin, the recalibrated saver model still explains 54% of the observed rise in the price-rent ratio from changes in the price and quantity of credit alone. While this response is about 25% smaller than the 72% observed in the Benchmark model, it still implies that more than half of the boom in price-rent ratios is explained by credit factors and thus does not overturn our core results.

We think of our saver extension as an extreme lower bound on the strength of credit on house prices. While savers in our model are able to frictionlessly adjust the size of their home at the intensive margin in response to the housing cycle, in practice saver housing markets are highly segmented in part because homes are indivisible and in part because of the geographic and quality segmentation of these groups' home purchases. In reality, it is not a viable option for most sellers to sell portions of their primary residences or

[32]The reason the recalibration ends up mostly adjusting along the homeownership margin rather than the price-rent margin is that the price-rent ratio response in the Benchmark model is already very close to the Full Segmentation model, leaving little room for further increases as $\sigma_{\omega,L}$ rises. As a result, most of the remaining adjustment occurs through the homeownership margin.

vacation homes to borrowers when credit relaxes and rebuy these portions when credit tightens, but this is a primary margin of adjustment in the frictionless model. That being said, there is not complete segmentation in practice, as changes in demand do ripple up or down the housing quality ladder. The best existing evidence on the degree of segmentation is Landvoigt et al. (2015), who show that this effect is significantly muted relative to a frictionless benchmark. We consequently suspect that the real world is closer to our benchmark model than our saver model but leave the quantification of where reality lies between these two extremes to future research.

9 Conclusion

More than a decade after the Great Recession there is still a lack of consensus about the role of credit supply in explaining house prices dynamics over the boom and bust. We argue this is because most of the literature has focused on two polar cases with regards to the segmentation in housing markets between credit-insensitive agents such as landlords or unconstrained savers and credit-sensitive borrowers.

In this paper, we generalize these polar cases to allow for arbitrary intermediate levels of rental frictions. Building on supply-demand intuition, we show that the key sufficient statistic for determining where reality falls on this spectrum is the causal effect of credit on the price-rent ratio relative to the effect of the same shock on the homeownership rate. We show in a new data set using instrumental variables methods that credit supply shocks cause a significant increase in price-rent ratios and a more muted and statistically insignificant homeownership response. Calibrating a model to match these estimates, we find that credit supply can explain between 35% and 54% of the rise in price-rent ratios over the 2000s housing boom. Relative to our polar cases, the calibrated model displays house price dynamics that are close to those under perfect segmentation, implying large frictions in rental markets.

Our work highlights the importance of assumptions about rental markets and the elasticity of saver demand for macro models of the housing market. These model features are often overlooked but are critical for many important results. We hope that our findings motivate future work to use and develop intermediate models in place of either polar assumption. We also highlight the use of identified credit supply shocks and a novel empirical moment for calibrating macroeconomic models of the housing market. We hope that subsequent work will improve on our estimates of the relative causal effect of credit supply on price-rent ratios and homeownership rates and use these identified moments to further improve the calibration of macro-housing models.

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