

Reassessing the 2000s Housing Cycle

After Two Decades of Research

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Abstract

What caused the 2000s housing cycle? This article synthesizes two decades of research, including reduced-form empirics and structural models. Although often contentious, the literature coheres once one accounts for each study's timing, place, methodology, and measurement. The cycle was a perfect storm that occurred in distinct stages. Starting in the late 1990s, fundamentals improved, and credit supply increased across the income distribution due to automated underwriting and financial deregulation. In the early 2000s, a decline in interest rates was followed by a further expansion in securitized and subprime credit, the origin of which remains controversial. This sequence of shocks produced a persistent boom that triggered two shifts in beliefs: average expectations became overly optimistic, pushing prices higher, and expectations diverged, fueling speculation. In the bust, prices overshot primarily due to mortgage defaults, which removed buyers and contracted credit supply. How these forces interact and how expectations form require further research.

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1 Introduction

From 30,000 feet, the 2000s housing cycle stands out in the history of the U.S. housing market like a mountain suddenly emerging from a broad plain. Although regional housing booms and busts had occurred since the introduction of reliable house price indices in the 1970s ([Case and Shiller, 1987](#)), there had been no significant national cycle. Recent historical work by [Lyons et al. \(2026\)](#) corroborates this: since 1890, nationwide real house prices had grown relatively smoothly aside from a few surges in the wake of both world wars and in the late 1970s. Then, from the late 1990s through 2006, nationwide real house prices skyrocketed above trend—rising by about 80 percent—before cratering by roughly one-third from their peak. This gyration struck the financial system and the macroeconomy with catastrophic force, triggering a wave of research across almost every field of economics.

What caused this tectonic event? This article synthesizes 20 years of research on the root causes of the 2000s housing cycle, integrating empirical and theoretical work from applied microeconomics, macroeconomics, and finance to evaluate what we have learned, where disagreements persist, and what major open questions remain. In the process, it examines and explains the methodologies used to distinguish among potential explanations and provides broader insight into the nature of housing fluctuations.¹

The central conclusion is that the 2000s housing cycle was a perfect storm that unfolded in distinct stages, each building on the last, resulting in a cumulative impact greater than that of any single force in isolation. Starting in the late 1990s, improvements in fundamentals and a broad-based expansion in credit supply, driven by automated underwriting and financial deregulation, led to an initial rise in house prices concentrated in inelastic coastal cities. Two further shocks added fuel to the fire: interest rates de-

¹The article builds on and updates several earlier surveys. [Mayer \(2011\)](#) provides an early survey of housing bubbles focused on the 2000s cycle, and [Glaeser and Nathanson \(2015\)](#) survey bubbles using a user-cost perspective. [Davis and Van Nieuwerburgh \(2015\)](#), [Piazzesi and Schneider \(2016\)](#), and [Guerrieri and Uhlig \(2016\)](#) synthesize theoretical macro models of the housing market, but they do not delve into reduced-form evidence. The first two also treat the 2000s cycle as one application among many. Topical surveys address expectations ([Kuchler et al., 2023](#)) or the origins of the credit supply expansion ([Adelino et al., 2018](#)) but not other forces or the interactions between forces. Finally, [Duca et al. \(2021\)](#) focus on international experience, interactions with the macroeconomy, and macroprudential policy.

clined sharply and remained low from 2001 to 2004, and credit expanded further due to subprime and private-label securitization beginning in 2003—though whether this expansion originated in credit markets or lender expectations remains hotly debated. The persistent price increase led to a wave of overoptimism from 2003 to 2006 and speculation from 2004 to 2006, resulting in explosive price growth, particularly in both inelastic and moderately elastic markets in the “sand states” of California, Arizona, Nevada, and Florida. The 2000s housing boom thus involved *all* of the usual suspects acting together.

Given the voluminous literature on the causes and consequences of the 2000s housing cycle, this article focuses on a positive analysis of its causes, emphasizing the boom and the movement of house prices and rents. The article discusses the bust and eventual rebound primarily to shed light on the boom, though it also provides a brief narrative of the bust in the process. It also adopts a partial-equilibrium, housing-market-centric perspective, treating the macroeconomy and monetary policy as given and setting aside feedback between the housing market, output, and interest rates. It furthermore sets aside normative issues and policy questions even though many of the papers analyzed also discuss policy. Finally, while this article briefly discusses cross-country evidence, the analysis is largely confined to the U.S. experience.

Five main root causes of the boom are considered: fundamental factors such as income and demographics; an expansion in credit supply due to changes in bank regulation, new lending technology, and innovations in subprime and private-label securitization (PLS); declines in interest rates from 2001 to 2004; overly optimistic expectations; and heterogeneity in optimism that manifests through speculative activity and increasingly optimistic marginal homebuyers. For the bust, an additional force is needed: mortgage defaults, which encapsulate both distressed sales due to foreclosures and the effect of non-payments on lender balance sheets. “Root causes” do not just mean exogenous shocks from the housing market’s perspective but instead cover all the essential features—both exogenous and endogenous—for a model or narrative of the cycle. These fall into three categories, with credit supply split across two: exogenous primitives, such as fundamentals and lending technology; forces that are conditionally exogenous given the partial-equilibrium approach, such as financial regulation and the interest rate path;

and endogenous but disciplinable causal chains, including the level and dispersion of house price expectations.

The root causes are difficult to disentangle because they are correlated and endogenous to one another. Interest rates are adjusted to control credit supply and respond to expectations about macroeconomic variables. Credit supply depends on interest rates, lenders' and mortgage-backed security (MBS) investors' expectations of collateral values, and incentives in credit markets; separating these origins is challenging. Expectations should respond when interest rates, credit supply, and fundamentals change. Speculation is spurred by optimism and by speculators' access to credit. Finally, house prices feed back on all of these forces.

The literature has taken two main approaches to distinguishing various stories. The first is empirical. This can take the form of stylized facts, quasi-experimental evidence, or time-series evidence. The second approach uses a structural macro-finance model to decompose the roles of various forces.

Each approach has strengths and shortcomings. Stylized facts are crucial for understanding the contours of the boom-and-bust cycle and for disciplining models, but sometimes they do not sharply distinguish among root causes. Quasi-experimental or cross-sectional evidence is particularly compelling for isolating the role of a single root cause; however, it often relies on identifying variation that captures only one aspect of the boom and is less effective at detecting more gradual changes and longer chains of causation. It is also difficult to aggregate: local average treatment effects (LATEs) may not extrapolate, and general equilibrium (GE) effects common to treatment and control are absorbed by an unidentified intercept (the "missing intercept" problem). Time-series evidence can detect longer impulse responses and other subtle causal chains, but it requires stronger identification assumptions and can yield wide standard errors. Structural models can say much more about aggregates, provide cleaner decompositions of each force's role, and shed light on interactions between forces, but they are more dependent on modeling assumptions and calibration details, and they often fail to capture the cycle's intricate dynamics.

Together, however, these methods complement one another, and a predominant nar-

rative of the cycle emerges despite variation in the cycle's nature across locations. Indeed, a literature that often seems contradictory and contentious coheres, though not without some notable disagreement.

A crucial finding of the analysis is that, rather than being a monolithic phenomenon, the boom and bust consisted of distinct stages. This is not, however, the way much of the literature discusses or analyzes it: empirical papers often use a back-of-the-envelope calculation to assess what fraction of "the boom" or "the bust" can be explained by the channel they identify, while models are often used to quantitatively apportion "the boom" or "the bust" among their contributing factors. The impulse to see the boom and bust as singular has led to confusion: what appears to be conflict in the literature is due to papers analyzing different stages of the cycle or different places. Consequently, the article carefully notes the time and place of each study, which helps to reconcile differences between papers and illuminate the cycle's causes and narrative.

The analysis begins by laying out the key facts of the cycle and then turns to the empirical evidence on each force. Fundamentals improved, but not enough to explain the magnitude of the boom on their own, which makes the boom's causes a puzzle. Although the literature has estimated a broad range of semi-elasticities of house prices to interest rates, interest rate declines played only a supporting role in the boom. Credit expanded in stages, driven first by improvements in lending technology and banking deregulation, which increased credit supply across the income distribution of homeowners. A second shift out in credit supply came from the 2003 expansion of subprime and private-label securitized mortgage credit, although whether this surge originated in credit markets or lender expectations remains a subject of disagreement. Solid but not foolproof survey evidence indicates that expectations became overoptimistic mostly after 2002. Finally, while it is difficult to measure the importance of belief dispersion using surveys, part of its contribution can be inferred from evidence on the magnitude of speculation and sales volume. Speculation mattered primarily in the sand states in the late stages of the boom.

The article then turns to structural models, examining different kinds of models: representative-agent, heterogeneous-agent, and assignment models; models with non-rational or overly optimistic expectations; and models of heterogeneous expectations

and speculation. Once one accounts for methodological differences across models, particularly in how the rental market is modeled, there is less disagreement than one might expect. Models help discipline the magnitude of each force's role in the cycle. Structural models also shed light on the interactions among the various forces and the interlinkages between stages in the presence of endogenous expectations. For instance, they reveal that low interest rates—rather than changes in interest rates—amplified the effects of expectations and credit. Nonetheless, these interactions and interlinkages, which play a key role in theories of asset bubbles and financial crises, remain understudied in the context of the 2000s housing cycle.

The analysis next examines how the bust and eventual rebound further discipline explanations of the boom. One puzzle is what accounts for the apparent overshooting in the bust. None of the five forces behind the boom can explain this: expectations did not overshoot on the downside; interest rates were cut; speculators could not short-sell housing; credit did not tighten below its pre-boom levels due to changes in credit markets; and aggregate income fell less than house prices. Instead, most of the overshooting can be explained by mortgage defaults, which work through two channels: foreclosures create a glut of supply relative to demand, and defaults damage lender balance sheets, contracting credit supply. Declining housing liquidity and overbuilding of durable housing in the boom also played a role. The rebound suggests that many of the shocks that caused the boom were permanent. The bust unfolded in three stages: an initial uptick in defaults and a more gradual price decline from 2006 to 2008 as the temporary shocks that caused the boom began to fade and reverse; a free fall in prices as foreclosures roared in the Great Recession from 2008 to 2010; and a long trough from 2010 to 2012 as improving fundamentals and supportive policies were counterbalanced by persistent defaults.

I conclude by providing a range for how much of the overall boom each factor explains. Despite my misgivings about aggregate numbers that fail to account for the multistage nature of the boom, I do so because these numbers help synthesize the main findings and quantify the magnitude of each root cause. The ranges reflect both disagreement across studies and interactions between forces: a force acting on its own often explains less of the boom than a force acting in concert with others.

I find that fundamentals alone explain 10 to 30 percent of the boom. Credit supply, in both the stronger first wave and the supporting second wave, explains 20 to 50 percent. Interest rate declines explain another 10 to 25 percent, with overly optimistic expectations accounting for 30 to 50 percent. Speculation explains 5 to 10 percent nationally, but far more in the explosive, late-booming metros; it is hard to quantify the impact of heterogeneous optimism that does not work through speculation. These figures are detailed in Section 6 and summarized in Table 1. Section 6 also discusses implications for models and open questions for future research.

2 Facts

Before examining the causes of the 2000s cycle, it is worth reviewing its contours in the aggregate, across cities, and across countries.

Figure 1 displays aggregate trends. Panel A shows nominal and real prices as well as price-rent and price-income ratios. According to the S&P Cotality (formerly CoreLogic) Case-Shiller National Home Price Index, prices grew 120 percent nominally from 1997 to 2006 and 83 percent nominally from 2000 to 2006. They then fell 27 percent from the 2006 peak to the 2012 trough before rebounding.² In real terms, prices grew 80 percent before falling by a third according to Case-Shiller, which is the baseline number used in this article for the size of the boom. Panel A also shows there was a large cycle in both the price-rent and price-income ratios, implying the boom cannot be explained by either measure of fundamentals or by a preference shock to housing.

The cycle in prices also manifested in sales volume and construction, as shown in Panel B. Existing-home sales volume peaked before prices, as detailed by [DeFusco et al. \(2022\)](#), and also fell dramatically, although it was buoyed in part by policy: the spike in 2009–2010 reflects the first-time homebuyer tax credit. Construction rose in the boom, fell off a cliff in the bust, and recovered only slowly.

The boom saw a massive run-up in mortgage debt and an expansion in the share

²The Supplemental Appendix compares various price indices and explains how and why they differ. It is crucial to use the same index for an analysis and for calculating what fraction of the boom the analysis explains.

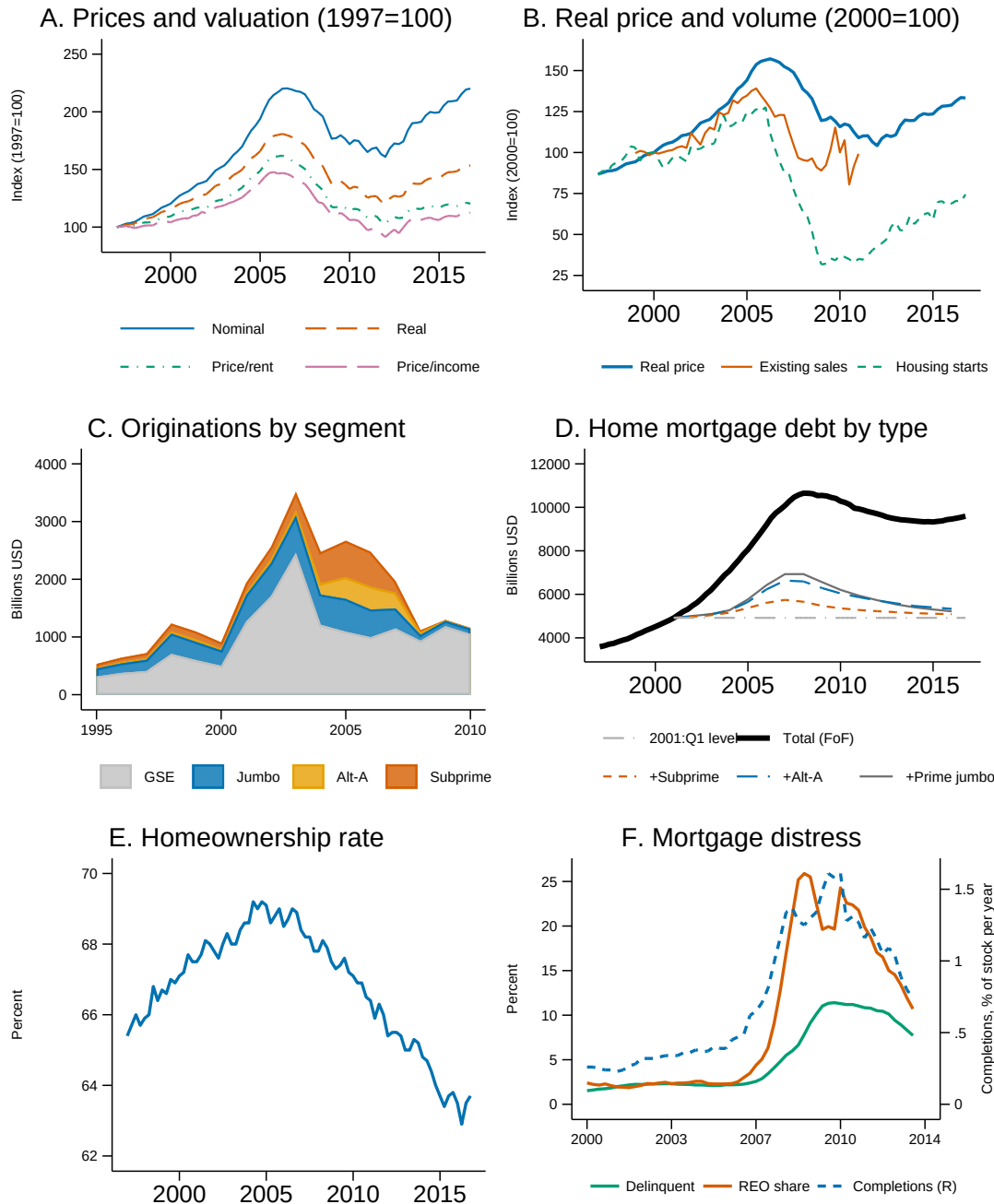


Figure 1: The Aggregate 2000s Housing Cycle

Note: Panel A: Nominal and real house prices and the price-rent and price-income ratios (1997Q1 = 100) (S&P Dow Jones Indices LLC, 2026b; U.S. Bureau of Labor Statistics, 2026; U.S. Bureau of Economic Analysis, 2026b,c). Panel B: Real price, existing-home sales, and housing starts (2000Q1 = 100) (National Association of Realtors, 2013; U.S. Census Bureau and U.S. Department of Housing and Urban Development, 2026). Panel C: Mortgage originations by segment, excluding FHA and VA lending (Keys et al., 2013). Panel D: Outstanding home mortgage debt (Flow of Funds; Board of Governors of the Federal Reserve System, 2026c) and counterfactual paths adding subprime, +Alt-A, and +prime-jumbo growth (all from private-label securitization), from the working paper of Foote et al. (2021). Panel E: Homeownership rate (U.S. Census Bureau, 2026b). Panel F: Share of loans 90+ days delinquent or in foreclosure and REO share of sales (left axis), and foreclosure completions (right axis), from Guren and McQuade (2020). See the Supplemental Appendix for details.

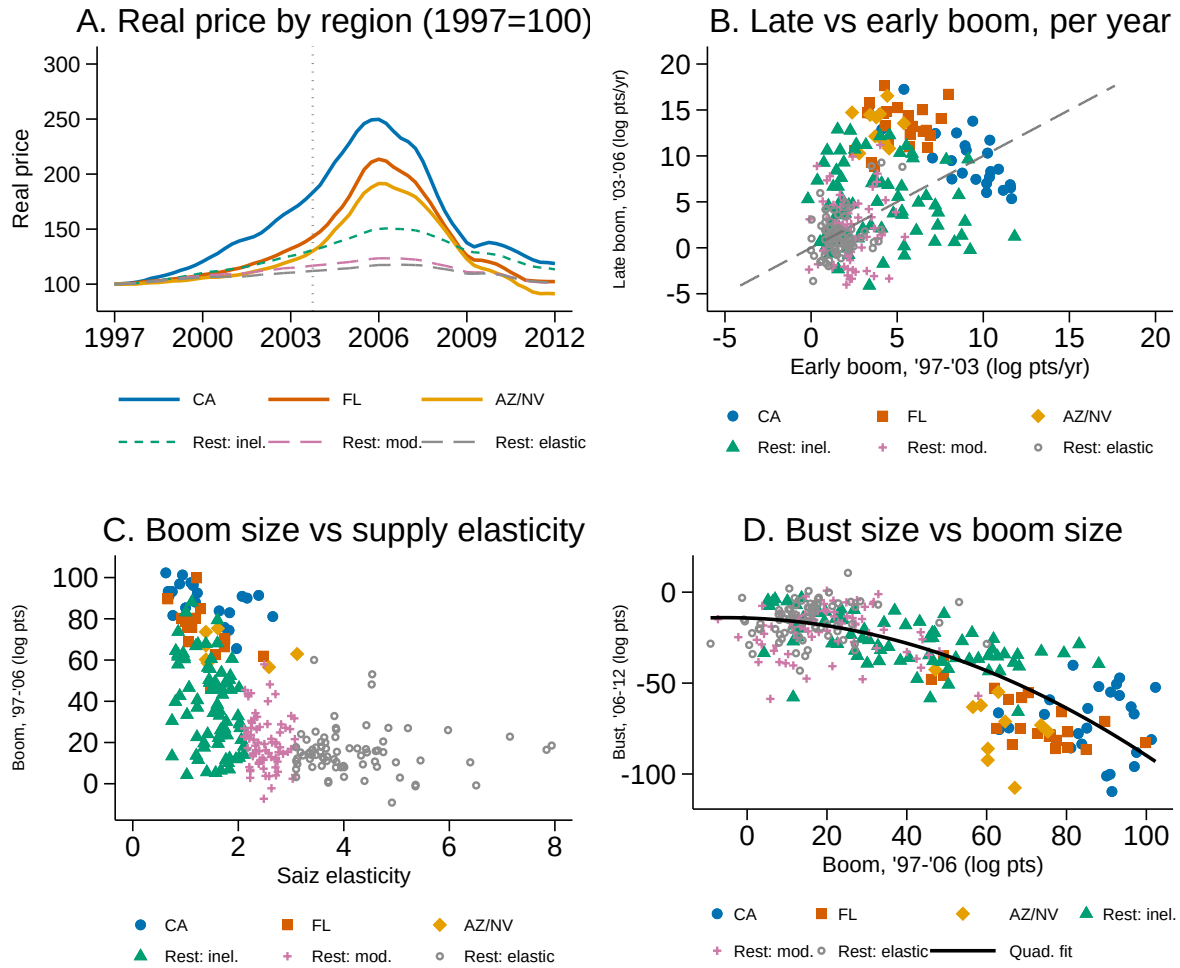


Figure 2: The Cross-Section of the 2000s Housing Cycle

Note: Metropolitan areas (core-based statistical areas, or CBSAs) are segmented into California, Florida, and Arizona/Nevada, with the rest of the country split into terciles of [Saiz \(2010\)](#) supply elasticity (inelastic, moderate, elastic). Real house prices are from the Freddie Mac House Price Index, deflated by the GDP deflator ([Freddie Mac, 2026b](#); [U.S. Bureau of Economic Analysis, 2026c](#)). Panel A: Equal-weighted mean real price by group, each metro indexed to 1997Q1 = 100; the dotted line marks the 2003Q4 split between the early and late boom. Panel B: Late-phase against early-phase boom, each expressed in log points *per year*. Panel C: Total boom against Saiz supply elasticity. Panel D: Bust against total boom, with a quadratic fit, following [Guren and McQuade \(2020\)](#). See the Supplemental Appendix for additional details on samples and construction.

of subprime loans. Panel C shows the flow of mortgage originations by segment, and Panel D shows the stock of debt, isolating the contribution of privately securitized segments.³ Panel C, reproduced from [Keys et al. \(2013\)](#), shows that the *flow* of originations peaked in 2003 due to a wave of refinancing at historically low rates. The origination share of the Government-Sponsored Enterprises (GSEs), Fannie Mae and Freddie Mac, fell from 70 percent in 2003 to 40 percent in 2005–2006 as subprime and Alt-A grew to 40 percent of originations. These lower-quality private-label loans virtually vanished in 2008. Panel D, reproduced from the working paper version of [Foote et al. \(2021\)](#), shows that growth in privately securitized subprime, Alt-A, and prime-jumbo debt cannot account for most of the aggregate increase in the *stock* of outstanding mortgage debt. This reflects in part the distinction between flows and stocks: subprime loans tended to refinance frequently, so subprime can be a large fraction of the origination flow but a smaller fraction of the stock ([Foote et al., 2016](#)). Risky loans held on GSE or bank balance sheets or securitized by the GSEs can also account for some of the rise in the stock of debt that is not visible in the private-label market breakdown in Panel D.

The cycle also manifested in homeownership, as shown in Panel E. The homeownership rate grew from 65 percent prior to the boom to 69 percent in 2004. It then plateaued through 2006 before beginning a steady decline. By 2016, the homeownership rate had fallen to 63 percent, its lowest level since the mid-1960s.

The bust was characterized by a wave of defaults and foreclosures, as shown in Panel F. By late 2009, over 10 percent of outstanding mortgages were 90 or more days delinquent or in foreclosure. At peak, 1.7 percent of the housing stock was foreclosed upon annually, and 8 percent of the owner-occupied housing stock experienced a foreclosure from 2006 to 2013 ([Guren and McQuade, 2020](#)). The large number of lender sales of foreclosures helped lift sales volume.

The boom was heterogeneous in its size, timing, and dynamics across cities. [Ferreira and Gyourko \(2023\)](#) and [Dokko et al. \(2019\)](#) demonstrate this using a time-series break-

³In the figure, “GSE” loans are prime loans purchased or guaranteed by Fannie Mae and Freddie Mac. “Jumbo” loans are prime-credit-quality loans that are too large for Fannie Mae and Freddie Mac to purchase or are otherwise not securitized by them and are thus private-label. “Alt-A” loans are for near-prime borrowers who fail to receive a prime loan for another reason, typically low documentation. “Subprime” loans are for lower-credit-quality borrowers.

point methodology to date the start of the boom across locations. I illustrate this pattern by grouping cities as in Figure 2, which shows the sand states separately—California, Florida, and Arizona/Nevada—because their timing differs sharply, and splits the rest of the country into terciles of the [Saiz \(2010\)](#) housing supply elasticity.⁴ Three timing patterns emerge. California and the inelastic tercile boomed starting in the late 1990s with a modest acceleration after 2003. Arizona and Nevada tracked the rest of the country until 2003 and then boomed explosively, as did Florida, which had already been growing somewhat faster than the rest of the country. The moderate and elastic terciles outside of the sand states had a muted cycle.

Panel A of Figure 2 plots average price paths, showing the differences in amplitude and timing across groups.⁵ Panel B plots log house price growth per year in the early boom (1997–2003) and the late boom (2003–2006) for each city, with the same groups indicated by different markers. Within groups, there is heterogeneity in the size of the early boom relative to the late boom—for instance, Boise, Idaho, had a sharp acceleration, while Boston, Massachusetts, had a large early boom that stalled. However, on average, California and the inelastic tercile cluster around the 45-degree line, while Florida and especially Arizona/Nevada sit well above it. Arizona and Nevada cities grew by 3.9 log points annually from 1997 to 2003 but by 13.3 log points annually from 2003 to 2006. The California cities farthest above the 45-degree line are inland areas rather than the coastal urban centers.

While inelastic areas did, on average, have a larger cycle, [Davidoff \(2013, 2016\)](#) challenges the view that common shocks and heterogeneous elasticities can explain the cycle. Panel C illustrates this by showing the boom size versus the [Saiz \(2010\)](#) supply elasticity. Boom size and elasticity are inversely related, but this is largely driven by the sand states, as Davidoff notes.⁶ Without these states, the relationship would be weaker but still present. Several cities—like Merced, Bakersfield, Fresno, and, to a lesser extent, Las Vegas

⁴Saiz is used in most of the literature; using other more recent measures of housing supply elasticity, such as [Baum-Snow and Han \(2024\)](#), provides similar though not identical patterns.

⁵The Freddie Mac House Price Index is used here because it behaves much like the Case-Shiller index, but it is publicly available for many cities. The downside is that it is limited to transactions with GSE-guaranteed loans.

⁶Davidoff goes further by arguing that within states, the residual cycle size is at best loosely related to elasticity and in some specifications has the wrong sign.

and Phoenix—had late, explosive booms despite not being especially supply constrained. This behavior is a puzzle.

The bust was far more synchronized than the boom, beginning in mid-2006 almost everywhere and lasting until roughly 2012. The bust also exhibited a strong nonlinearity between boom size and bust size, as shown in Panel D of Figure 2. This implies that places with larger booms overshot in the bust (Guren and McQuade, 2020). Some of this is due to a compositional effect. Panel F of Figure 1 shows the “Real Estate Owned (REO)” share, the share of home sales accounted for by foreclosures, which peaked at 27 percent nationally in 2009 and was as high as 75 percent in cities like Las Vegas. Because foreclosures sell at a discount (Campbell et al., 2011; Anenberg and Kung, 2014), a high REO share drags down the price index in the cities with the most foreclosures, helping explain some of the nonlinearity. However, Guren and McQuade show that the nonlinearity is also evident in non-distressed price indices that do not include foreclosures.

Similar boom-bust patterns can be seen across countries. Duca et al. (2021) survey the international evidence and find two main patterns. First, house prices boomed and busted more in countries where credit standards relaxed. Second, house price cycles were larger in countries with inelastic supply. These effects interact: countries with both inelastic supply and credit relaxations had the largest booms. Several countries, like Spain, Ireland, and the United Kingdom, had larger booms than the United States, while a few countries, notably Japan and, to a lesser extent, Germany, had almost no boom. Knoll et al. (2017) create panels of house prices going back to the 1870s for 14 countries. Their indices show that the 2000s cycle was historically exceptional across most countries. Nonetheless, Duca et al. (2021) point out that international comparisons are challenging because mortgage and housing market institutions differ across countries, which is why this article focuses mostly on the United States.

3 The Boom: Empirical Evidence

This section reviews the empirical evidence—stylized facts, quasi-experimental designs, and time-series analyses—on the root causes of the boom, organized by cause.

3.1 An Improvement in Fundamentals

The first force is fundamentals, such as income or demographics. Rents—the flow value of housing—are a common summary statistic for fundamentals.

[Shiller \(2007\)](#) shows that construction costs and rents did not rise nearly as much as prices, and [Figure 1](#) shows income did not either. In other words, the price-rent and price-income ratios both dramatically increased. There is also no demographic break. [Glaeser \(2013\)](#) argues there was less uncertainty about fundamentals than in past booms.

[Loewenstein and Willen \(2023\)](#) take this further by decomposing the price changes of properties with both observed prices and rents into changes in the relative prices of rented units, the price-rent ratio, and rents, which reflect fundamentals. They find that real rents played relatively little role from 2000 to 2006 but were central to other cycles such as the post-pandemic boom.

[Chodorow-Reich et al. \(2024\)](#) (CRGM), however, show that national real rent growth accelerated in the late 1990s from 0.46 percent annually from 1988 to 1997 to 1.23 percent from 1997 to 2017.⁷ While this suggests fundamentals did improve, the magnitude is too small to explain the boom on its own. Rents grew only 11 percent over the boom from 1997 to 2006, and a simple Gordon growth calculation detailed in the Supplemental Appendix implies that this would translate into a price increase of about 30 percent under the extreme assumptions that the acceleration is permanent and fully capitalized.

The cross-section tells a similar story. The Supplemental Appendix shows there is a correlation between income growth and house price growth across cities, but house price growth far outpaced income growth, especially in the sand state cities with the largest booms. [Ferreira and Gyourko \(2023\)](#) show that while the boom's onset correlates with increases in local income, income mean-reverts within three years, and the initial price spurts are small relative to the full boom. CRGM find that rent growth accelerated more in cities with higher long-run house price growth and fundamental growth, although

⁷CRGM describe their fundamental shock as combining increases in urbanization, changes in industry growth, and rising tastes for city dwelling due to falling crime or increased preferences for urban amenities. This is related to [Howard and Liebersohn \(2023\)](#), who argue that increasing regional divergence increased fundamentals by increasing expected future rent growth.

again the magnitudes are far smaller than the boom.⁸ Finally, [Duca et al. \(2021\)](#) find that across countries, population growth is secondary to credit liberalization and supply elasticity in explaining the boom.

In sum, fundamentals alone explain 10 to 30 percent of the boom, which motivates the search for other culprits or amplification channels.

3.2 The Decline in Interest Rates from 2001 to 2004

The second force is the decline in interest rates from 2001 to 2004. Lower interest rates affect house prices through user costs and by relaxing payment-to-income (PTI) constraints.⁹ This section focuses on the decline in interest rates; the impact of low rate *levels* is discussed in Section 4.4.

Amid a recession and deflation concerns, the Federal Reserve reduced the federal funds rate (FFR) from 6.5 percent in December 2000 to 1 percent by mid-2003, then raised it beginning in June 2004. Peak to trough, the 10-year Treasury yield fell about 365 bps, and the average 30-year fixed mortgage rate fell about 340 bps, although the decline of real long-run rates was 120–190 bps ([Glaeser et al., 2013](#)).

Estimates of the semi-elasticity of house prices with respect to interest rates vary widely. Some well-known papers, such as [Taylor \(2007\)](#) and [Himmelberg et al. \(2005\)](#), argue that interest rates explain much of the boom, though Taylor looks at construction rather than house prices. Others, such as [Glaeser et al. \(2013\)](#), find that interest rates explain at most 20 percent of the boom, and [Kuttner \(2013\)](#) and [Del Negro and Otrok \(2007\)](#) conclude that the estimated effects are too small to account for much of it.

These starkly different conclusions reflect three factors. First, although papers appear to estimate the same semi-elasticity, they do not. Some papers use short rates, others use long rates, and still others use mortgage rates; some use real house prices and others nominal, and they use different price series.

⁸In their structural model, described in Section 5, CRGM estimate that at the peak, fundamentals would have explained 28 percent of realized house price growth in the highest-growth cities in a model with rational learning.

⁹This article uses “PTI” to refer to both payment-to-income and debt-to-income constraints. Typically, these constraints limit *combined* minimum payments on all debts—rather than just mortgage payments—relative to income.

Second, micro and macro estimates differ in whether they include general equilibrium effects and in the horizons they examine. Quasi-experimental micro estimates, which have the most credible identification but measure LATEs, find semi-elasticities of nominal prices with respect to *mortgage rates* of roughly 1 to 5. The variation used holds macroeconomic responses fixed, and the outcomes are typically short-run. [Adelino et al. \(2025\)](#) use interest rate increases at conforming loan limits to estimate a semi-elasticity of house prices to mortgage rates of between 1.3 and 5.3. [Davis et al. \(2020\)](#) use a change in Federal Housing Administration (FHA) mortgage insurance premiums to estimate a semi-elasticity of 3.4.¹⁰ Finally, [Fuster and Zafar \(2021\)](#) use a willingness-to-pay survey to obtain a semi-elasticity of 2.5.

By contrast, time-series or panel vector autoregression (VAR) approaches find hump-shaped impulse responses that peak after 2–5 years at semi-elasticities to *short rates*—which moved considerably more than mortgage rates—of 5 to 10. Unlike the micro estimates, these studies incorporate general-equilibrium feedback through income and employment, which provides a natural explanation for their larger magnitudes. The cost is weaker identification: macro estimates are sensitive to specification, particularly of time trends, and have wide standard errors. These include [Kuttner \(2013\)](#), who finds smaller estimates surveying VAR evidence and estimating a vector error correction model;¹¹ [Jordà et al. \(2015\)](#) and [Burgert et al. \(2024\)](#), who instrument for short rates with exchange rate pegs and U.S. monetary policy spillovers, respectively, in a cross-country panel; and [Aastveit and Anundsen \(2022\)](#), who use monetary shocks in the United States. [Časta \(2025\)](#) estimates a maximal semi-elasticity of house prices to mortgage rates close to 10 and provides a comprehensive homogenized survey of estimates.

Third, the role of interest rates is entangled with a debate over whether the Fed kept interest rates too low for too long. Assessing the impact of interest rates requires a counterfactual rate path, and most analyses use a Taylor rule.¹² Often, the choice of Taylor rule,

¹⁰[Bhutta and Ringo \(2021\)](#), discussed in Section 3.3.3, study the same FHA mortgage insurance premium change but use a within-market regression discontinuity and find little effect on the prices individual borrowers pay. The gap between the two studies reflects GE effects in Bhutta and Ringo’s intercept.

¹¹Kuttner reports that a 25 bps expansionary monetary shock raises house prices by 0.3 to 0.8 percent across VAR studies, which implies a semi-elasticity of 1.2 to 3.2. He emphasizes that these magnitudes are too small to explain much of the boom.

¹²The literature on Taylor rules shows that different formulations yield different conclusions and fit

rather than the sensitivity of house prices to interest rates, accounts for the disagreement in the literature.¹³ Most modern estimates of Taylor-rule deviations are closer to [Dokko et al.'s \(2009\)](#) 100–200 bps deviation, which this article takes as the baseline.

[Dokko et al. \(2009\)](#) and [Kuttner \(2013\)](#) note two other facts that cast doubt on the central role of interest rate declines. First, the timing is off. Rates were kept low through mid-2004, so a 2- to 3-year impulse response could explain a boom that peaked in mid-2006. However, rates rose sharply starting in 2004, and this contractionary monetary policy should have offset the expansion from 2003 to 2004, though the pass-through into long rates was muted in this period. Second, across countries, the decline in interest rates and the size of the boom are at most weakly correlated.

Two counterfactuals bound the impact of interest rates. A lower bound holds the macroeconomy fixed: a semi-elasticity of 3 to 5, applied to a 75–150 bps decline in mortgage rates attributable to the 100–200 bps Taylor-rule deviation, gives a 2.3 to 7.5 percent impact of interest rates on nominal house prices. When endogenous macroeconomic responses are incorporated, a semi-elasticity of 5 to 8 multiplied by a 100–200 bps change in short rates leads to a 5 to 16 percent change in house prices. This is an upper bound because this article counts the resulting income feedback as fundamentals, not rates. A reasonable consensus estimate between these two bounds is that low interest rates increased house prices 5 to 12 percent, which is 6 to 15 percent of the real national price boom from 1997 to 2006.

3.3 An Expansion in Credit Supply

The third force is an expansion in credit supply *conditional* on interest rates. The credit supply narrative gained traction in the popular press due to dramatic increases in sub-

poorly in some periods ([Nakamura et al., 2025](#)). Interest rates may also have been low due to macroeconomic conditions rather than the Taylor-rule deviation. Some papers use the full decline in rates (e.g., [Glaeser et al., 2013](#); [Garriga et al., 2019](#)), which gives interest rates a larger role. Doing so, however, conflates short-run monetary policy with secular trends in interest rates.

¹³For instance, [Taylor \(2007\)](#) argues that “a higher federal funds rate path would have avoided much of the housing boom” based on a simulation where the FFR was almost 300 bps too low for several years. By contrast, [Dokko et al. \(2009\)](#) conclude that “housing market developments would have been only modestly different” under a Taylor-rule formulation in which the FFR was 100–200 bps too low in 2003–2004. Using a VAR, they find that interest rates and other macro variables cannot account for most of the post-2002 boom.

prime mortgages and PLS ([Keys et al., 2013](#)). The debate is not whether credit expanded—it clearly did—but whether it *originated* in credit markets (the “credit market view”) or responded to lender expectations about future collateral values (the “lender expectations view”). “Lender expectations” refers not only to the lenders but also to secondary-market participants who purchased the securitized loans and provided the ultimate demand for MBS. Changes in loan-to-value (LTV) limits, PTI limits, and underwriting standards; the growth of securitization; and the expansion of subprime and other nontraditional mortgage products all fall predominantly under this force, though one must be careful because these measures also respond to lender expectations.

3.3.1 Credit and House Prices

Before turning to the particulars of the 2000s cycle, it is useful to assess whether credit affects house prices in general. A quasi-experimental literature convincingly shows it does. [Favara and Imbs \(2015\)](#) use the staggered rollout of interstate bank branching deregulation across states and banks’ legal statuses to instrument for mortgage credit and find that a 1 percent change in the dollar value of lending volume increases house price growth by 0.2 percentage points, peaking after two years. [Di Maggio and Kermani \(2017\)](#) use the federal preemption of state anti-predatory lending laws in 2004 as an instrument for credit. They find that a 1 percent change in lending increases annual house price growth by 0.3 percentage points. [Mian and Sufi \(2022\)](#) examine the impact of the 2003 PLS expansion using the pre-period local share of lending by lenders financed with non-core deposits as an instrument for local exposure to the PLS expansion and find a significant effect on house prices over three years. Finally, [Johnson \(2025\)](#) examines differences in lending policies between Fannie Mae and Freddie Mac, interacted with local exposure to the two firms, and finds that credit affects house prices. Because the last two papers pursue reduced-form analyses, it is harder to evaluate magnitudes.

These estimates are hard to translate into the impact of credit supply during the boom for three reasons: it is unclear how large an exogenous expansion in credit should be applied to these estimates, these LATEs may not be representative, and there are standard general equilibrium aggregation concerns with a missing intercept. The literature has

thus turned to how credit supply behaved in the boom to assess its role.

3.3.2 Subprime and Securitization, 2003–2006

A contentious and still-unsettled literature assesses the importance of the credit market view relative to the lender expectations view.¹⁴

The credit market view is summarized by [Levitin and Wachter \(2012\)](#), who describe the late boom as a “mortgage-finance supply glut... resulting from the failure of markets to price risk correctly due to the complexity, opacity, and heterogeneity of the unregulated [PLS].”¹⁵ This view comprises several narratives for why the PLS market mushroomed, including lenders’ shift to generating fees through quantity because they could pass on risk; the incentives of the rating agencies; and the role of the GSEs. Sometimes the credit market and lender-expectation distinction blurs, implying both stories matter. For instance, out-of-line lender expectations about hard-to-estimate covariances between the loans underlying MBS, which reduce risk diversification from pooling, could count as either story.

[Levitin and Wachter \(2012\)](#) and [Demyanyk and Van Hemert \(2011\)](#) emphasize that bond and rate spreads, respectively, shrank despite rising volumes. While spreads show the credit supply curve shifted right, they cannot identify its origin. The literature has turned to two broad methods of identifying the source.

The first way is to look at its incidence: credit market stories imply disproportionate credit growth at the bottom because the PLS market disproportionately attracts nonprime loans, whereas lender expectations affect the entire quality distribution.

[Mian and Sufi \(2009\)](#) (MS) famously argue that the subprime expansion originated in credit markets. They show that 2002 to 2005 was the only period during which the correlation between income and credit growth across ZIP codes was negative, suggesting

¹⁴It is crucial to distinguish the effect of subprime on *house prices*, discussed here, from the effect of subprime delinquencies on *bank balance sheets*, which was central to the financial crisis but is not the focus of this article. The two are often conflated.

¹⁵[Diamond and Rajan \(2009\)](#) provide an early exposition. See [Gorton and Ordoñez \(2014\)](#) for a theory of the informational sensitivity of debt and why collateral quality was scrutinized in the bust but not the boom. [Coval et al. \(2009\)](#) and [Ashcraft and Schuermann \(2008\)](#) describe the informational frictions in these markets in practice.

that credit expanded differentially for low-quality borrowers. MS show the subprime expansion is closely related to securitization and observe similar patterns in cities with elastic housing supply, where expectation-driven booms are unlikely. These two patterns, which they summarize in [Mian and Sufi \(2018\)](#) and expand on in [Mian and Sufi \(2017b\)](#), lead MS to argue in favor of the credit market view.

This conclusion has been challenged by [Adelino et al. \(2016, 2017\)](#), [Albanesi et al. \(2022\)](#), and [Foote et al. \(2021\)](#). These papers present countervailing evidence that there was no *differential* expansion of credit to low-income borrowers from 2001/2002 to 2006, which they take as evidence that the increase in mortgage credit originated in lender expectations. These papers collectively differ from MS in three ways: they use borrower-level data rather than ZIP code aggregates, stocks rather than flows, and credit scores rather than income.

[Adelino et al. \(2016, 2017\)](#) argue that, *in shares*, credit expanded across the income distribution and that credit growth and income did not decouple at the household level.¹⁶ [Foote et al. \(2021\)](#) focus on debt stocks rather than flows and show that they grew evenly across the income distribution, implying that subprime was the extensive margin of a broader expansion in credit across the distribution of mortgage borrowers. [Albanesi et al. \(2022\)](#) show that prime borrowers and investors, rather than subprime borrowers, drove the increase in the stock of loans that MS found in subprime ZIP codes. [Adelino et al. \(2026\)](#) show that LTV ratios were remarkably constant over the cycle, even conditional on observables, which casts doubt on the role of relaxed LTV limits.

This evidence is controversial in part because of a debate over the reliability of the mortgage data on income. For instance, [Mian and Sufi \(2017a\)](#), [Jiang et al. \(2014\)](#), and [Ambrose et al. \(2016\)](#) show that households frequently misreport income, particularly in low-credit-quality and low-income mortgage applications or low-documentation loans. [Garmaise \(2015\)](#) has similar findings for assets. [Mian and Sufi \(2017a\)](#) argue that income overstatement is severe enough to invalidate the findings of [Adelino et al. \(2016\)](#). [Adelino et al. \(2016\)](#) reply that income misrepresentation does not drive their results, and [Foote](#)

¹⁶[Mian and Sufi \(2017b\)](#) argue that there is more growth at the bottom once one looks at combined loan values that include junior liens and say that the evidence presented by their critics is not inconsistent with their “credit supply view.”

et al. (2021) and Albanesi et al. (2022) avoid self-reported income on mortgage applications and use alternative measures of income and lagged credit scores, respectively, presumably for this reason, although credit scores can miss other dimensions of risk. Given these data-quality concerns and the difficulty of inferring the origin of the credit expansion from incidence, the literature remains unsettled.

A second approach to evaluating the credit market view is to focus on securitization and the institutions surrounding it. Some papers look at how demand from the secondary market affected securitization. Keys et al. (2010) show that securitization relaxed screening for low-quality borrowers, as evidenced by discontinuities at an ad hoc FICO score threshold that varied the ease of securitization.¹⁷ Two papers attribute weaker screening to lenders' ability to pass on risk: Purnanandam (2011) shows that lenders that securitized more originated lower-quality loans conditional on a battery of observables, and Rajan et al. (2015) show that lenders focused on information disclosed to investors rather than soft information. Demyanyk and Van Hemert (2011) show that subprime borrower quality declined monotonically from 2001 to 2007 and that securitizers were aware of it. Finally, Nadauld and Sherlund (2013) identify a shock to investment banks' demand for securitized loans that they argue is exogenous to the PLS market and show that these banks reduced quality relative to non-investment banks operating in the same ZIP codes.

Other papers show that misrepresentation was rife in securitization markets, with the implication that this allowed originators to reduce quality by hiding risks (Piskorski et al., 2015; Griffin and Maturana, 2016; Griffin, 2021). In contrast, Foote et al. (2012) argue that the relevant risks were disclosed and understood.

An additional body of research looks at institutional factors. The popular press and Financial Crisis Inquiry Commission (2011) (FCIC) have pointed to competition for market share among a handful of rating agencies as an explanation for an erosion of standards from 2005 to 2007, which Ashcraft et al. (2010) document. Others focus more squarely on the GSEs. The FCIC pointed to their incentives being distorted by their status as private companies with an implicit government backstop and by their political influence in

¹⁷See Bubb and Kaufman (2014) for a critique of this paper's methodology and Keys et al. (2012) for a rejoinder.

Washington that warded off effective oversight. [Elul et al. \(2025\)](#) argue, based on regional variation in GSE acquisitions, that the GSEs' large exposure to house prices led them to make risky loans in 2007 in an effort to prop up prices. Relatedly, [Bhutta and Keys \(2022\)](#) find that private mortgage insurers failed to act as a check on the GSEs and instead made it easier for them to finance increasingly risky loans in the late boom. Finally, nontraditional loans may have played a role. [Dokko et al. \(2019\)](#) show that across counties that boomed after 2000, an expansion of mortgages with nontraditional features immediately preceded the start of the boom, as measured by a breakpoint analysis, providing timing-based evidence that these loans fueled the late boom.

The literature on structured finance is convincing that securitization expanded credit; the difficulty lies in aggregating the literature's estimates into an impact on prices. This is challenging for three reasons. First, there is the classic missing intercept problem. Second, an increase in risky loans at the margin does not reveal how much the credit supply curve shifted. Third, as Panel D of Figure 1 shows, the PLS market accounts for only about a third of the overall increase in the stock of mortgage debt. Together, these make it difficult to find a moment summarizing the PLS expansion for models to target, which is part of why models have focused more on easily observable shifts in LTV and PTI constraints.

My reading is that the evidence for a broad-based expansion is strong, particularly because it holds up when credit scores are used instead of the problematic income data. However, a broad-based expansion does not rule out some relaxation at the bottom or imply that the origin of the expansion was *only* lender and investor expectations. Typically, rising prices would push out lower-income and lower-credit-quality borrowers, leading their share of borrowing to fall. Borrowers at the bottom instead kept pace, implying their credit did relax somewhat. This is corroborated by evidence that securitization created incentives to expand credit and loosen screening. An expansion in low-income credit originating in credit markets thus remains part of the puzzle, but in a supporting rather than leading role.

3.3.3 Why Did Credit Expand? Three Distinct Episodes

Rather than ascertaining the origins of the credit expansion from the nature of the equilibrium shift or by analyzing how securitization changed incentives as in Section 3.3.2, other papers examine specific triggering events. Three major episodes have been documented.

The first episode is the late 1990s, when banking deregulation and the introduction of automated underwriting converged in a broad-based expansion in credit supply.

The most significant change in banking regulation was the legalization of interstate bank branching in 1994, although states could and did blunt its impact.¹⁸ Favara and Imbs (2015) show that this increased the number of loan originations and the loan-to-income ratio. They report that branching deregulation explains one-half to two-thirds of the increase in loan volume and one-third to one-half of the increase in house prices, with the effects concentrated in inelastic cities. They show it explains most of the price increase from 1994 to 2002, but far less of the increase from 2002 to 2005. As with most aggregations of cross-sectional coefficients, these calculations neglect GE effects.

Beyond bank branching, two other changes in regulation—the 1992 introduction and subsequent rollout of the GSEs’ affordable housing goals and the 1995 Community Reinvestment Act (CRA) overhaul—are often cited in the popular press as playing a role in the credit expansion. However, the academic literature and the FCIC have found that they played a minor role. Bhutta (2012) and Avery and Brevoort (2015) find small effects for the GSEs’ affordable housing goals, although these papers’ cross-regional comparisons cannot detect nationwide shifts. Similarly, evidence on the CRA is slim: Bhutta (2011) and Avery and Brevoort (2015) find weak effects, and Agarwal et al. (2012) find increases in risky lending around CRA exams that are concentrated in 2004–2006 rather than 1999–2003.¹⁹

Automated underwriting systems (AUSs) were introduced around the same time that banks were deregulated. These systems spread gradually within the prime credit seg-

¹⁸Deregulation was gradual from 1994 through 2005: nine states were fully deregulated by 1996, most of the changes occurred between 1996 and 2002, and 26 states had removed at least three of four restrictions by 2005.

¹⁹Other regulatory changes that have been discussed include the Office of Thrift Supervision’s 1996 preemption of state limits on prepayment and late fees, and the 1999 repeal of Glass–Steagall; there is, to my knowledge, no convincing evidence regarding these two interventions in the late 1990s.

ment in the late 1990s, with market share growing from under 25 percent of GSE loans in 1997 to 60 percent in 2001, where it stabilized. [Johnson and Tzur-Ilan \(2026\)](#) show that the primary impact of these systems was to replace heuristic manual rules with statistical risk modeling; accelerating the lending process was second order. The shift loosened PTI limits: because high PTI ratios added little predictive power to default models, AUSs using statistical risk modeling underwrote loans with higher PTI ratios ([Johnson and Tzur-Ilan, 2026](#); [Foote et al., 2026](#)).

[Greenwald \(2026\)](#) shows that during the boom, the PTI distribution of originated Fannie Mae loans is smooth, with 55 percent of home-purchase debt at the 2006 peak above the typical pre-boom 36 percent PTI limit, suggesting that PTI limits were effectively unenforced. After the bust, there is a sharp falloff in mass above the post-bust PTI limit of 45 percent.²⁰ Greenwald shows that the income distributions of borrowers largely overlap, supporting the idea that PTI relaxations caused a broad-based credit supply expansion. [Anenberg et al. \(2019\)](#) echo this finding empirically by showing shifts in the “loan frontier.” Greenwald also documents a strong cross-sectional link between an area’s pre-boom high-PTI share and its subsequent price growth, explaining much of the geographic variation in the boom. While robust to controls, this evidence is suggestive rather than causal. [Bhutta and Ringo \(2021\)](#) confirm the importance of binding PTI limits.

[Johnson and Tzur-Ilan \(2026\)](#) combine differences in the extent to which Fannie Mae’s and Freddie Mac’s AUS software used statistical risk modeling with regional variation in each system’s adoption to show a causal link between automated underwriting, growth in high-PTI lending, and house price growth. A back-of-the-envelope aggregation of their estimates, which does not account for GE effects, implies that the adoption of GSE AUSs cumulatively increased house prices by 16 percent from 1993 to 2005 and increased the correlation of house prices across cities. Similarly, [Lewellen and Williams \(2021\)](#) show that the introduction of the Mortgage Electronic Registration Systems (MERS) in the late 1990s, which simplified securitization, helped expand PLS by non-bank lenders to low-income borrowers and was associated with faster house price growth in exposed areas. Extrap-

²⁰Greenwald lacks pre-boom data but provides anecdotal evidence that the manual underwriting standards in place before automated systems typically enforced a 36 percent PTI cap.

olating their results on Massachusetts to the whole country would imply that MERS increased dollar lending volumes by 3 percent annually.

The second episode is the expansion of automated underwriting from prime lending into subprime in the early 2000s, a potential trigger, beyond securitization, for the subprime expansion discussed in Section 3.3.2. The evidence of AUSs' spread to subprime is, however, circumstantial and limited (Temkin et al., 2002).

The third episode is an expansion in PLS in the summer of 2003, after the Fed indicated that it had finished its easing cycle. Several authors argue that when the refinancing wave ended, lenders repurposed infrastructure previously used for refinancing to originate privately securitized home-purchase loans, and that pressure to maintain refinancing-boom earnings pushed them toward riskier loans (Levitin and Wachter, 2012).²¹ Justiniano et al. (2022) show that there was a sharp 80 bps drop in the spread between rates on privately securitized mortgages and Treasuries, conditional on observables, in summer 2003. Dokko et al. (2019) link this to the expansion in subprime and nontraditional loans, particularly in later-booming, more elastic and state housing markets like Las Vegas and Phoenix. Mian and Sufi (2022) show that the PLS expansion pushed up house prices. This evidence does not settle the debate in Section 3.3.2: repurposed refinancing capacity and pressure on earnings are credit-market explanations, while a sudden fall in MBS spreads is also consistent with more optimistic secondary-market investors.

Overall, there is strong evidence that credit supply significantly expanded in the late 1990s, driven by bank branching deregulation and changes in lending technologies, and that this primarily relaxed PTI constraints and affected inelastic areas. Consistent with this, Davis et al. (2023) show that mortgage risk had risen over the 1990s due to increases in average PTI and combined LTV rather than unconventional mortgage features. There is good evidence for a subprime boom and a PLS boom originating in credit markets in the early 2000s, although quantitatively they played a supporting role in a broad-based credit expansion. Quantifying the aggregate impact of this credit expansion on house prices, however, is challenging, which is why structural models are necessary.

²¹The FCIC and the popular press have pointed to other changes in 2003–2004 that may have played a role in stoking the PLS market, including accounting scandals and regulatory changes at the GSEs.

3.4 Overly Optimistic Expectations

The fourth force is overly optimistic expectations about future house prices. This refers specifically to the *average* level of expectations and is distinct from *heterogeneous* optimism, which is discussed subsequently. My focus is on an empirical literature that measures expectations, assesses behavioral biases and their consequences for house prices, and notes when and how expectations are clearly out of line. Other researchers have used models—most often a user-cost framework—to argue that fundamentals alone cannot explain the boom, implying that overoptimism is necessary ([Glaeser and Nathanson, 2015](#); [Duca et al., 2021](#)). Because this line of reasoning is model-dependent and has been covered extensively elsewhere, I omit it here.

Several stylized facts suggest that expectations were overly optimistic, particularly in the 2003–2006 period. [Foote et al. \(2012\)](#) present a set of scenarios from a 2005 Lehman Brothers report forecasting potential losses on subprime investments. The scenarios are wildly optimistic: the report put 50 percent weight on house price growth slowing to 5 percent by the end of 2005 and stabilizing, 30 percent weight on more optimistic scenarios, 15 percent weight on prices stalling but not falling, and only 5 percent weight on a “meltdown,” which had house prices fall 5 percent a year for three years and grow 5 percent a year thereafter. Even the unlikely meltdown scenario was far too rosy! [Cheng et al. \(2014\)](#) look at the housing market actions of people who work in securitized finance and find they did not time the market; instead, some groups aggressively *increased* their housing market exposure in proportion to their growing incomes. They argue this behavior reflects overoptimism rather than lenders being aware of a looming crash yet originating and securitizing loans anyway because of bad incentives.

The most direct evidence on overoptimism comes from survey expectations, which [Kuchler et al. \(2023\)](#) review comprehensively. The main challenge is that most high-quality surveys of house price expectations using modern methods begin in or after the bust. To my knowledge, only two sources provide direct survey evidence on how expectations evolved over the boom. First, [Case et al. \(2012\)](#) (CST), updated in [Shiller and Thompson \(2022\)](#), survey recent homebuyers in four counties on one-year-ahead and 10-

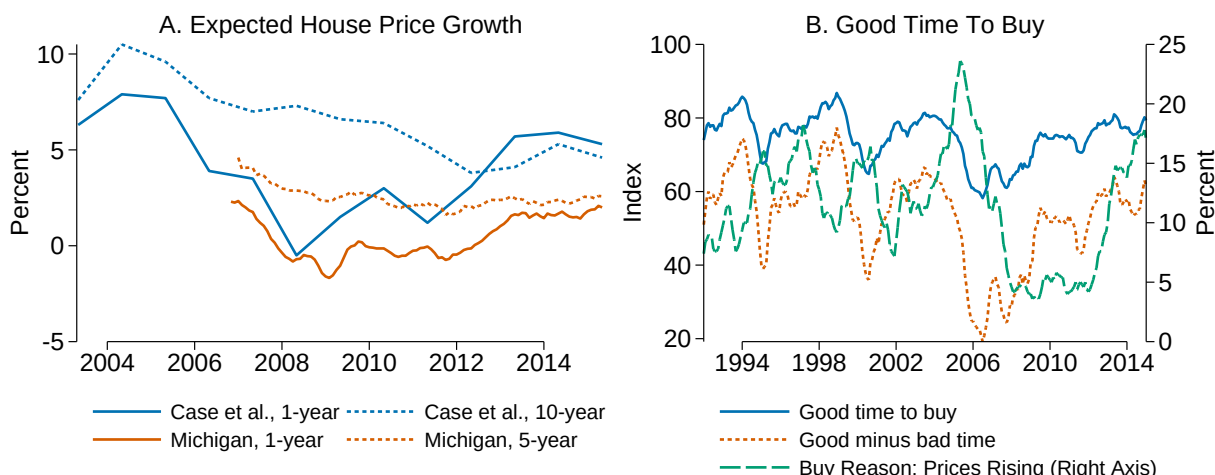


Figure 3: Survey Evidence on Expectations Over the Cycle

Note: The CST data come from [Shiller and Thompson \(2022\)](#), which, relative to [Case et al. \(2012\)](#), sets any 10-year value 10 or more times its one-year value equal to the one-year value. This helps address respondents’ misunderstanding of the question. The Michigan Survey data ([University of Michigan Surveys of Consumers, 2026](#)) are smoothed with a five-month moving average to reduce noise. In Panel B, “Buy Reason: Prices Rising” (right axis) is the share of respondents who say it is a good time to buy because prices are going up or housing is a good investment, following [Piazzesi and Schneider \(2009\)](#). It is computed from the published tables, which count a respondent who gives both reasons twice, so it runs somewhat above Piazzesi and Schneider’s household-level measure.

year-ahead expectations beginning in 2003. Second, the Michigan Survey of Consumers asks whether it is a good time to buy a house and began to ask about one- and five-year-ahead expectations in 2007.

Figure 3 shows the expectations data, with Panel A showing expected price growth from CST (pooled across four counties) and the Michigan Survey, and the first two lines of Panel B showing the “good time to buy” question from Michigan. In the boom years, expected price growth is *extremely* high: one-year expectations rise to nearly 8 percent annualized in 2004 and 2005, and 10-year-ahead expectations (that is, expected growth *each year* over the next 10 years) rise to over 10 percent.

The 10-year number is often cited as clear evidence that expectations were overly optimistic. CST point out that it is difficult to prove irrationality and that realized price growth in some cities from 1996 to 2006 was close to 10 percent annually. However, it is hard to imagine a model in which a prediction of 10 percent house price growth per year for a decade in 2004 or 2005—after 10 years of accelerating and stratospheric price growth—would be rational. Still, CST’s 10-year estimates must be interpreted with cau-

tion. Observers such as their discussant [Laibson \(2012\)](#) have pointed out that the 10-year expectations are so high as to be somewhat implausible, likely because respondents conflated average and total return. In response, CST later emphasized the “on average” phrasing to be clearer and adopted stronger data cleaning in their 2022 update to ameliorate this problem (these updated data are used in the figure). Nonetheless, the 10-year expectation series is internally consistent within the boom and, in 2004 and 2005, is far above its later levels, so it still signals extreme bullishness in the late boom.

The Michigan Survey’s “good time to buy” question is less clear-cut. Although it was high in the boom, with approximately 80 percent saying it was a good time to buy, it was not historically high. The timing is also a bit off: it peaked in 2003 before falling off precipitously in 2005, suggesting a connection to interest rates and the level of prices. Another possible explanation is that the Michigan Survey is a nationally representative sample of U.S. households, while CST survey recent homebuyers. If buyers are more optimistic than the general public—as suggested in [Section 3.5](#)—this may explain why the Michigan Survey’s average provides a weaker signal of overoptimism.

Beyond this time-series evidence, the major finding from the literature on survey expectations is that households underestimate the strength of short-run momentum and neglect medium-term mean reversion. These patterns appear both in regression analysis of survey expectations (e.g., [Case et al. 2012](#), [Shiller and Thompson 2022](#), and [De Stefani 2021](#)) and in information experiments ([Armona et al., 2019](#)). [Kuchler and Zafar \(2019\)](#) also show that personal experience shapes optimism: individuals who have seen larger local house price increases expect higher national price growth. An additional feature is the failure to consider dire scenarios: house price expectations rarely turn negative, and when they do, the expected declines are modest.

[Ben-David et al. \(2026\)](#) take a different approach by inferring expectations using a Bayesian VAR in which inventory dynamics help distinguish expectations shocks from interest rate, housing consumption, and housing supply shocks. They find that expectations account for a quarter of the boom nationally, about the same as mortgage rates.²²

²²Their mortgage-rate contribution is larger than that implied by the semi-elasticity estimates in [Section 3.2](#) because the VAR incorporates dynamic feedback and reflects unexpected movements in the mortgage rate over 1996–2006, not just during the 2001–2004 decline. Their mortgage-rate shock may also absorb

Expectations mattered more in the sand states and in the late boom, and the authors argue that their magnitude points to overoptimism.

On balance, there is solid but not foolproof evidence that expectations were overly optimistic—and in particular that households did not anticipate a bust anywhere near as large as what occurred—and that this was crucial in the late stages of the boom. There is little direct evidence on the early stages. More broadly, it is difficult to quantify the importance of optimistic expectations using this evidence, which further motivates structural analyses.

3.5 Heterogeneous Optimism and Speculation

The last force is heterogeneity in house price expectations, rather than their level as in Section 3.4. Belief dispersion affects the housing market in two ways. First, because few homes trade each period, optimistic households can become the marginal buyers who determine prices. Second, heterogeneous expectations open the door to speculation, which the popular press has portrayed as rampant in the boom.

A finance literature has shown that heterogeneous optimism, where agents “agree to disagree,” can push up prices through both the marginal-buyer and speculation channels. This is true in multiple classes of models. Models in the mold of [Harrison and Kreps \(1978\)](#) and [Scheinkman and Xiong \(2003\)](#) show how optimistic speculators’ valuations rise in a boom because they expect to sell to an even more optimistic buyer, amplifying the price surge. Models with equilibrium leverage that build on [Geanakoplos \(2010\)](#) and [Simsek \(2013\)](#) find that endogenous leverage can amplify a boom because optimists—be they speculators or ordinary homeowners—take on larger loans, which makes the marginal buyer more optimistic and pushes up prices.

However, it has proven difficult to measure heterogeneity in expectations directly using surveys. Distinguishing heterogeneity from measurement error is challenging, and large sample sizes are needed to measure higher-order moments precisely. While newer surveys, such as the New York Fed Survey of Consumer Expectations, may shed light on credit-supply-driven declines in mortgage spreads.

heterogeneity in expectations in a future boom, comparable data from the boom period do not exist.

A different approach is to look at *why* people bought. [Piazzesi and Schneider \(2009\)](#) show that the share of Michigan Survey respondents citing rising housing values when asked why it is a good time to buy, which they argue reflects momentum traders or speculation, rose from about 10 percent in late 2003 to over 20 percent in mid-2005—its highest level in 25 years. This can be seen in the “Buy Reason: Prices Rising” line on the right axis of Panel B of Figure 3. Piazzesi and Schneider point out that because so little of the housing stock is transacted every period, a few optimistic homebuyers can have a big price impact—the marginal-buyer channel from above. This is tantalizing evidence of the role of momentum traders, but given the potential for confounding factors or noise to explain the increase, it should not be overinterpreted.

Given the limited evidence from surveys and the difficulty of observing marginal homebuyers, the empirical literature has focused on speculation. Speculation is an endogenous force: the question is not whether it increased exogenously, but whether models and narratives must include speculative investors, or whether the boom can be explained by owner-occupants with a single home. Speculation’s strength is also closely related to other mechanisms, such as credit supply and interest rates, as speculators tend to use leverage and are sensitive to price movements. The literature has used two approaches to measure speculation’s role in the 2000s boom.

The first approach directly measures speculative activity over time and across markets. The literature identifies speculators in the data in several ways: [Haughwout et al. \(2011\)](#) measure speculators as buyers who hold multiple first-lien mortgages; [Gao et al. \(2020\)](#) count non-owner-occupants; [Bayer et al. \(2021\)](#) and [Mian and Sufi \(2022\)](#) identify speculators based on holding times and multiple-property ownership; and [Chinco and Mayer \(2016\)](#) count speculators as out-of-town second-home buyers and emphasize the role of an influx of non-local capital. Regardless of the definition used, these studies reach similar conclusions: speculative activity surged in the late boom, from late 2003 or 2004 to 2006, and was particularly significant in sand state markets, such as Las Vegas and Phoenix, that experienced more explosive, later booms despite having moderately elastic

housing supply. Speculation was strongest at the same times and in the same places as optimism, which one would expect if optimism were heterogeneous. [Bayer et al. \(2020\)](#) refine these definitions by differentiating between destabilizing speculators and stabilizing middlemen who provide liquidity, and find that the composition of investors shifted toward speculators in the late boom.

A few studies go further by identifying the mechanisms behind speculation. [Bayer et al. \(2021\)](#) provide evidence of hyper-local spillovers: having a nearby neighbor engaging in speculation or a nearby home flip increases the chance that an individual becomes a speculator, and these socially induced speculators tended to be novices whose investments performed relatively poorly. [Nathanson and Zwick \(2018\)](#) examine whether speculation can explain the [Davidoff \(2013\)](#) puzzle (Section 2) of why relatively elastic sand state cities experienced explosive booms. They provide empirical evidence and a model showing that speculation on undeveloped land can generate an inverted-U-shaped relationship between supply elasticity and boom size. Intuitively, in cities with intermediate housing supply elasticity, optimistic homebuilders can bet on whether supply constraints will bind in the future, which amplifies today's boom. Finally, [Mian and Sufi \(2022\)](#) provide a direct link between credit and speculation, showing that areas more exposed to the PLS expansion experienced greater speculation and volume.

The second approach examines the joint dynamics of price and transaction volume, as speculation should show up in shorter holding times and higher volume. [DeFusco et al. \(2022\)](#) show that price and volume co-move, that volume leads prices by 24 months, and that these patterns are more pronounced in cities with larger cycles and more speculators, as measured by holding period and occupancy. In the 2000s boom, they find that speculative volume explains 40 to 50 percent of total sales volume growth in an accounting decomposition. They calibrate a model to match their findings and use it to argue that, absent speculation, the volume boom would have been far smaller and price overshooting would have been minimal.²³ They conclude that speculators amplified the effects of

²³Rather than modeling heterogeneous expectations, [DeFusco et al. \(2022\)](#) introduce “speculation” through exogenous heterogeneity in expected holding times due to moving shocks. Their “speculators” are short-horizon households who endogenously become more important in the boom because they weight resale price more heavily than flow values, similar to optimists in belief-heterogeneity models.

optimism in the late boom, particularly for sales volume.

To summarize, speculation mattered during the late boom, from 2004 to 2006, and primarily in the handful of sand state markets that experienced late and explosive price growth. While the exact contribution of speculators varies with how they are defined, speculation may account for up to half of purchase-mortgage originations in the most bubbly states ([Haughwout et al., 2011](#)). However, while speculation is key to these markets, it is less central to the nationwide narrative of the boom and mattered only at its tail end. Because the marginal-buyer channel is so difficult to measure, this evidence captures only part of the role of heterogeneity in optimism.

4 The Boom: Structural Models

While empirical evidence can directly support each channel, distinguish exogenous forces from endogenous mechanisms, and detail the boom's sequencing and heterogeneity, it falls short of quantifying each channel's role and elucidating how root causes interact. To answer these questions, structural macro-finance models are necessary. Typically, models are calibrated to match microdata; exogenous shocks to interest rates, credit, and expectations are fed in; and the models are used to decompose the cycle's sources through a series of counterfactuals.

This section groups models by their key assumptions. First, the simplest models are rational expectations models with a single house price and a representative borrower and saver. Second, heterogeneous agents are introduced into rational expectations models with a single house price. Third, assignment models eliminate the single-house-price assumption and price houses of different sizes or qualities differently. Fourth, rational expectations are replaced by non-rational expectations that generate overoptimism. Finally, models with heterogeneous expectations and speculation are considered.

Throughout, it is essential to consider the behavior of *both* prices and rents, or, equivalently, price-rent ratios. In the 2000s boom, real prices rose with little real rent growth. This rules out models that explain the boom with shocks that move both prices and rents, such as preference or fundamental shocks. Such models are useful—they do a good job

explaining the post-pandemic housing boom, for instance—but they cannot explain the 2000s cycle.

4.1 Single-House-Price Models with Rational Expectations and a Representative Borrower and Saver

The simplest models make four assumptions to provide clear, closed-form expressions and sharp intuition. First, they assume rational expectations. Second, housing is modeled as a homogeneous good that has a single price and can be consumed in continuous quantities, abstracting from indivisibility in location, size, and quality. Third, there are two representative households: a saver and a borrower who faces an LTV constraint at mortgage origination $m_t \leq \theta^{LTV} p_t h_t$, where m_t is the mortgage principal, $p_t h_t$ is the dollar value of housing purchased, and θ^{LTV} is the maximum allowed LTV ratio.²⁴ Fourth, they assume the borrower is the marginal homebuyer whose behavior prices houses.²⁵ They also assume a fixed housing supply, which is relaxed below, and implicitly assume one household per house, which rules out speculation.

The representative borrower’s Euler equation can then be written as a standard consumption-based asset pricing formula with a collateral constraint:

$$p_t = \frac{\text{Present Value of Shadow Rents}}{1 - \mathcal{C}_t}. \quad (1)$$

The numerator is the present value of shadow rents discounted at the borrower’s stochastic discount factor.²⁶ The term $\mathcal{C}_t$ is the shadow value of a dollar of housing collateral, which is equal to the multiplier on the LTV constraint times θ^{LTV} . Relaxing the LTV constraint means that a dollar of cash on hand can be further leveraged to demand more

²⁴Stein (1995) introduced down payment constraints into the analysis of housing markets, and Iacoviello (2005) incorporated them into a business cycle model.

²⁵For instance, Justiniano et al. (2019), Greenwald (2026), and Greenwald and Guren (2025) assume savers always choose a fixed quantity of housing. This is justified, respectively, by segmentation in housing markets, the implausibility of large shifts in home size between the two groups over the cycle, and indivisibility and differences in quality and location. Greenwald and Guren show in an extension that letting savers trade housing dampens the effect of credit supply and interest rates on the price-rent ratio by 30 percent.

²⁶Formally, this is equal to u_h^B / u_c^B , the marginal rate of substitution between housing and consumption, which is the shadow rent, plus $(1 - \delta)p_{t+1}$, the resale value next period net of depreciation. This sum is discounted at the borrower’s stochastic discount factor $\Lambda_{t+1}^B = \beta^B u_{c,t+1}^B / u_{c,t}^B$.

housing, thereby increasing the collateral value C_t and pushing up house prices through a collateral effect. This class of models thus provides clear and sharp intuition.

[Garriga et al. \(2019\)](#) present a two-good representative-agent model with mortgage rates exogenous to the housing market. In an experiment in which households learn the entire future path of interest rates and LTV limits in 1998, they find that lower mortgage rates and looser LTV limits account for about half of the price boom, with most due to rates. It is difficult to isolate the impact of Fed policy from this counterfactual, as it combines the secular trend in rates and changes in spreads with short-run monetary policy.

[Justiniano et al. \(2019\)](#) (JPT) extend the basic model outlined above by adding a lending constraint on the supply of loanable funds $m_t \leq \bar{M}_t$ alongside the LTV constraint. They argue that in the relevant area of the parameter space, collateral values and prices adjust so that both constraints bind simultaneously. Relaxing the LTV constraint θ^{LTV} lowers prices and raises mortgage interest rates because greater credit demand chases a fixed credit supply, pushing up its price (the interest rate) and pushing down house prices. Relaxing the credit supply constraint $\bar{M}_t$ increases housing collateral values and pushes prices up and interest rates down. They argue that only an expansion in credit supply can match the joint behavior of prices and interest rates and the stability of LTVs.

[Greenwald \(2026\)](#) takes the argument further by introducing long-term mortgages and replacing JPT's loanable funds constraint with a PTI constraint: $d_t \leq \theta^{PTI} y_t$, where d_t is the required combined debt payment and y_t is income. PTI constraints differ from JPT's credit supply constraint because, as interest rates fall, minimum payments fall and the PTI constraint slackens. Greenwald also introduces within-period idiosyncratic income shocks but maintains the representative-agent assumption by assuming that, at the end of the period, the members of the borrower "family" insure one another. This means that rather than the entire economy being on the LTV constraint, the PTI constraint, or both, a fraction of the population is in each of the three situations, which introduces selection of the marginal buyer into this class of models.²⁷

²⁷A large fraction of households are bound by *both* constraints simultaneously, which Greenwald argues matches the data. This occurs because many borrowers optimally choose the kink point in the budget set: \$1 of cash on hand increases housing demand by \$1 on the PTI constraint but by $\$1/(1 - \theta^{LTV})$ on the LTV constraint.

Greenwald shows that this setup leads to a constraint-switching effect that amplifies relaxations in PTI constraints θ^{PTI} and dampens relaxations in LTV constraints θ^{LTV} . Intuitively, the value of collateral C_t includes the fraction of households that are LTV-constrained. As the PTI constraint slackens, fewer households are PTI-constrained, and more households become LTV-constrained, which increases the collateral value of housing C_t and pushes up prices. Greenwald shows that PTI constraint relaxations have large effects on prices, whereas LTV constraint relaxations are dampened by the opposite effect as more households become PTI-constrained and the collateral value of housing falls.

To quantify this effect, Greenwald calibrates a realistic version of his model and introduces an unexpected relaxation in PTI limits that matches the shift from a 36 percent PTI limit (prior to automated underwriting) to virtually unconstrained PTIs, followed by an unexpected tightening that triggers the bust. He shows that relaxing PTI constraints can explain 33 to 53 percent of the boom in price-rent ratios. These numbers are roughly in line with the combined effects of automated underwriting and bank branching deregulation discussed in Section 3.3.3.

A shortcoming of this broad class of models is the lack of heterogeneity. In a market like housing where few assets are transacted at any time, the identity of the marginal buyer can be critical. Assuming a representative borrower who is always marginal abstracts from potentially important changes in who buys.

4.2 Single-House-Price Models with Rational Expectations and Heterogeneous Households

The next class of models sacrifices the transparency of an Euler equation for pricing housing in favor of the richness of numerically solved heterogeneous-agent models. Retaining a single house price and rational expectations and assuming away speculation allows these models to remain computationally tractable even when realistic features are added: aggregate and idiosyncratic income risk; overlapping generations with a lifecycle that matches the age profile of mortgage debt and house size; a rental market; and priced long-term mortgages with credit constraints, refinancing, and default. [Ortalo-Magne and](#)

[Rady \(2006\)](#) is an influential early paper in this vein that showed young first-time home-buyers are particularly exposed to credit and have a disproportionate effect on house prices, but it did not consider the 2000s cycle.²⁸

[Favilukis et al. \(2017\)](#) (FLVN) present a general equilibrium (two-sector, fully endogenous-interest-rate) heterogeneous-agent model with aggregate and idiosyncratic income risk, a lifecycle, and an LTV constraint. The key new force is a housing risk premium, driven by households who can only partially insure against income risk and therefore demand compensation to hold risky, illiquid, levered housing. When credit loosens, households can better insure themselves, which endogenously lowers the housing risk premium, pushing the effective discount rate on housing down and the price-rent ratio up. This risk premium channel is distinct from the collateral channel of Section 4.1 and amplifies the impact of changes in LTV constraints because aggregate risk and a realistic wealth distribution together make the risk premium effect quantitatively large. In a quantitative exercise in which they relax the LTV constraint, they find that credit alone can explain 40 to more than 60 percent of the national boom through the risk premium channel. However, the relaxation counterfactually pushes up interest rates, so they also include an influx of foreign capital, which lowers rates but does little to house prices.

[Kaplan et al. \(2020\)](#) (KMV), by contrast, find a minuscule role for credit conditions and instead conclude that overoptimistic expectations were a primary cause of the boom. Relative to FLVN, they add two central features: a rental market, in which deep-pocketed landlords convert housing from owned to rented according to a user-cost equation; and long-term mortgages. Their structural model also includes overlapping generations with bequests, aggregate and idiosyncratic income risk, endogenous tenure choice, refinancing and default, priced mortgages, and a construction sector, and is calibrated to match an array of lifecycle and cross-sectional microdata.

KMV use their model to decompose the role of several shocks that could have caused the boom. An unexpected shock triggers the boom, and a second unexpected reversal

²⁸[Sommer et al. \(2013\)](#) is an early paper that extended a Bewley–Huggett–Aiyagari model to assess the drivers of the boom. They find that lower interest rates, relaxed lending standards, and higher incomes together explain half of the rise in the price-rent ratio from 1995 to 2006.

triggers the bust. These shocks affect fundamentals through labor productivity, credit by relaxing LTV and PTI constraints and lowering mortgage origination costs and spreads, and expectations through a news shock about future housing demand. This news shock is modeled as an increase in the probability of moving to a state with a higher utility weight on housing. A rough average of recent buyers' expected real house price growth over the next 10 years across CST's four counties disciplines the preference parameter in the high-demand state, and the average size and length of cycles in 25 countries over 1970–2012 from [Burnside et al. \(2016\)](#) discipline the transition probabilities. Given the news shock, expectations are rational.

KMV find that the price boom was almost entirely driven by beliefs, with credit and fundamentals required to match the rise in homeownership and leverage. KMV attribute the unimportance of credit for prices in their model to two factors. First, building on [Kiyotaki et al. \(2011\)](#), they argue that the presence of a rental market means far fewer households in their calibration are constrained in the amount of housing they wish to consume, so demand changes less when credit constraints relax. Second, long-term mortgages, which are absent in FLVN, mean that households do not need to satisfy the collateral requirement each period, making consumption and the risk premium less volatile.

[Greenwald and Guren \(2025\)](#) (GG) reconcile these disparate findings and show that they depend not only on the presence of a rental market but also on how it is modeled. For tractability, papers typically make one of two polar assumptions. Papers like KMV assume perfectly frictionless conversion between the owned and rented housing stocks by deep-pocketed landlords. This pins prices at the present discounted value of rents, so when credit expands, homeownership rates move as people buy housing from their landlords, but prices do not. The polar-opposite assumption, made by papers like FLVN and [Greenwald \(2026\)](#), is a complete segmentation between rental and ownership stocks. This implies that the homeownership rate is fixed and prices move when credit expands.

GG argue that reality likely lies between these two extremes and present a model that extends [Greenwald \(2026\)](#) to allow frictional conversion, nesting the two polar cases on conversion while also allowing for intermediate cases. They introduce a moment to discriminate between models: the impact of credit supply shocks on the price-rent ratio

relative to their impact on the homeownership rate. This is the slope of what they call the “tenure supply” curve, a supply curve in price-rent versus homeownership rate space. Credit shifts the tenure demand curve in the same space, so the equilibrium moves along the tenure supply curve, whose elasticity determines credit’s impact on house prices. Intuitively, in the frictionless conversion case, only homeownership responds to credit, whereas in the complete segmentation case, only the price-rent ratio responds.

Using the instruments of [Loutskina and Strahan \(2015\)](#), [Di Maggio and Kermani \(2017\)](#), and [Mian and Sufi \(2022\)](#), GG find that the data are closer to perfect segmentation than perfect conversion; that is, the tenure supply curve is steep. When they calibrate their model to match this moment and feed in two unexpected shocks that relax and tighten lending constraints as in KMV, they find that credit can explain between one-third and one-half of the boom and that credit and interest rates together can explain up to 70 percent of the boom, with the rest attributed to a residual that they interpret as expectations.²⁹

This class of models can also analyze interactions between forces. For instance, GG find that the credit shock they feed in can explain one-third of the boom if it is the only shock introduced, but if interest rate reductions and expectational shocks are added to match the full size of the boom and credit is removed, credit explains half the boom. This is because of an interaction between credit supply and expectations: PTI constraints limit how much optimists can borrow, so relaxing PTI constraints lets optimistic expectations have a bigger impact on housing demand and trigger a larger boom. However, not all models feature such interactions. For instance, the various shocks in KMV affect house prices largely independently because demand has little effect on prices in their model.

While these models are realistic, they have limitations. First, expectations either enter as a named residual or are calibrated as unexpected, exogenous, one-time shocks rather than arising endogenously. For instance, KMV’s news shock calibration leans heavily on the CST 10-year expectations, whose limitations are discussed in Section 3.4, and on [Burn-](#)

²⁹Their model is not a true heterogeneous-agent model, but Greenwald shows that extending his similar model to include a lifecycle does not substantially change his results, so GG’s results should be robust. GG’s counterfactuals are similar to Greenwald’s, except that homeownership adjusts to dampen the price-rent response. As in Greenwald, the PTI relaxation drives most of the response.

[side et al.’s \(2016\)](#) analysis of cycle size and length. Furthermore, all agents in the economy are assumed to have the same expectations as CST’s recent homebuyers. While these models carefully examine how heterogeneity in household balance sheets affects housing demand, they abstract away from heterogeneity in expectations, which appears important in the data and is at the heart of other models considered subsequently. Second, they feed in exogenous credit supply shocks, which are difficult to calibrate. Typically, these papers introduce large LTV and PTI relaxations based on observed data, implicitly assuming that these changes were exogenous (although evidence from [Adelino et al. \(2026\)](#) on the stability of LTVs suggests PTI should be the primary mechanism). I therefore treat these quantifications as an upper bound on credit’s role. Third, these models cannot explain the dynamics of the boom and abstract from its multistage nature, as they predict that house prices jump when unexpected shocks hit at the beginning of the boom and again when they suddenly revert at the beginning of the bust.

4.3 Assignment Models

Assignment models dispense with the assumption that housing is a homogeneous good and instead take indivisibility—and its implication that different types of households buy different homes—seriously. This makes the cross-section of house prices a central model object and a new moment to shed light on the causes of the cycle. This moment is informative because forces like borrowing constraints affect only credit-constrained households. Put differently, rather than assuming the representative borrower is the marginal buyer, these models make each buyer marginal in their own market, allowing comparisons of price dynamics across borrower and saver segments to discipline the model.

Assignment models feature a continuous distribution of home types and household types, with households sorting by rank—the lowest-ranked household to the lowest-quality house and so on up the distribution. Each household has a demand function, and the pricing function ensures that each household optimally chooses the exact type of house to which it is assigned. Each housing type consequently satisfies a user-cost equation—similar to the asset pricing condition for the single house price in Section [4.1](#)—

but because households must be assigned across the whole housing stock, these conditions are linked by market clearing. This implies that the price schedule solves a differential equation affected by credit, the distribution of mover types, and the distribution of house types, so that a shock to one segment ripples across the entire distribution.

[Landvoigt et al. \(2015\)](#) (LPS) introduce an assignment model with a lifecycle, uninsurable income risk, and credit constraints and quantify it using data from San Diego. Only movers participate in the housing market each period, so selection into who transacts and which houses are put up for sale matters. They show that in the data, the bottom of the market experienced a larger boom than the top, which the model attributes to two factors. The first is a relaxation of credit, modeled as a decline in interest rates and mortgage spreads together with a relaxation of an LTV constraint. Intuitively, cheap credit has larger effects on demand at the lower end of the market because more agents are credit-constrained. The second is the composition of buyers and homes; they find that more low-quality homes were sold, which drew richer marginal buyers into that segment and drove up its prices. LPS calibrate their model and show it matches not only the cross-section of price growth from 2000 to 2005 but also the joint distribution of house quality and borrower characteristics, which allows them to disentangle buyer-seller matching from credit. In a counterfactual that changes only credit conditions (their Figure 10B), LTV relaxation and declining interest rates together explain 40 to 50 percent of the excess appreciation of low-quality homes relative to high-quality homes. While it is hard to extrapolate from their results to a number for the portion of the boom attributable to credit and interest rates, their model ascribes a prominent role to these forces.

4.4 Endogenous or Non-Rational Expectations

Models with endogenous or non-rational expectations formalize and quantify overoptimism, typically while maintaining homogeneous expectations. Unlike KMV, who model overoptimism as a rational response to an exogenous shift to a state with potentially high future fundamentals, these models endogenize and ground overoptimism using behavioral theories. These models face a major challenge: without discipline, they can explain

almost *any* observed pattern by assuming appropriate expectational biases, so empirical evidence is essential to narrow the scope of admissible behavioral biases.³⁰ Although several candidate forms of non-rational expectations have been studied, a shortcoming of this literature is a lack of consensus on the “best” model of non-rational expectations.

Motivated by early evidence on house price momentum from [Case and Shiller \(1989\)](#), echoed in their subsequent work on survey expectations ([Case et al., 2012](#)), an early candidate was extrapolative expectations, which assume past house price appreciation positively affects current expectations ([DiPasquale and Wheaton, 1994](#); [Abraham and Hendershott, 1996](#); [Barberis et al., 2018](#)). These models often also include a measure of deviation from fundamentals, so that as a boom grows, it is initially dominated by extrapolation but eventually dampened as prices become further detached from fundamentals until a bust occurs. Building on the macro and finance literatures ([Piazzesi and Schneider, 2009](#); [Barberis et al., 2015](#)), other models feature a small share of extrapolators.

Simple extrapolation faces two challenges. First, the evidence in Section 3.4 shows that households *underestimate* short-run momentum in house prices and neglect medium-run mean reversion, which simple extrapolation struggles to match. In response, [Glaeser and Nathanson \(2017\)](#) provide a microfoundation for extrapolation based on individuals assuming that past prices reflect only past demand, not past beliefs, which is consistent with evidence on underreaction to short-run momentum and neglect of mean reversion in the survey expectations literature. Their microfoundation is elegant, though in my experience difficult to embed in a macro model, and their model leads to oscillatory dynamics that struggle to match the data. The second empirical challenge is the lack of evidence of expectational overshooting due to extrapolation in the bust shown in Figure 3.

The literature has consequently turned to other forms of non-rational expectations from behavioral economics. One option is natural expectations, proposed by [Fuster et al. \(2010\)](#), which assumes that agents use a parsimonious, boundedly rational model that captures short-run momentum but neglects medium-run mean reversion, so prices over-

³⁰[Landvoigt \(2017\)](#) infers beliefs from household choices using a lifecycle model rather than positing a bias. He finds that the average expected price growth was close to its long-run average (2.5 percent annually) and not unusually optimistic. However, he finds large subjective uncertainty, which, given the option to default, raises optimal leverage. Because beliefs are backed out as whatever rationalizes observed choices, the exercise jointly tests expectations and the model. He also does not decompose the boom.

shoot. However, [Glaeser and Nathanson \(2015\)](#) find that in the context of housing fluctuations, implausibly extreme parameters are necessary to make natural expectations work.

CRGM provide a model of the 2000s housing cycle that relies on a second option: diagnostic expectations, proposed by [Bordalo et al. \(2018\)](#). Diagnostic expectations formalize Kahneman and Tversky’s representativeness heuristic, in which people latch onto characteristics that represent a population when estimating probabilities. In particular, agents overweight states that recent news makes relatively more likely, which in this setting means they overreact to changes in the growth rate of fundamentals. Diagnostic expectations have the advantage of being tractable—they add a single term to the mean of a typical Kalman filter—and have consequently been used across finance and macroeconomics. CRGM argue that diagnostic overreaction to an improvement in the growth rate of fundamentals can explain the cycle.³¹ CRGM show that diagnostic expectations are consistent with two features of the cycle that strain pure extrapolation or other learning stories: expectations do not overshoot on the downside in the bust, and the length of the boom and bust is uncorrelated with the boom’s size. Diagnostic expectations are only one ingredient of CRGM’s unified model of the boom, bust, and rebound; because the model’s identification rests on the bust and rebound, the model and its quantification are deferred to Section 5.

A third option is adaptive learning, which [Jacobson \(2025\)](#) adds to KMV’s model. Unlike rational expectations, which can be thought of as a fixed point between agents’ policy functions and their expectations, adaptive learning treats agents as econometricians who update their forecasts as they receive new data. Overoptimism comes not from a model of non-rational expectations but from boundedly rational agents learning from limited histories of house price data. Observing sustained house price growth causes them to infer that high growth is a persistent feature of the true price process and update their expectations accordingly. Jacobson finds that when forecast errors in survey data discipline the speed of learning, improvements in credit supply and fundamentals, amplified by adaptive learning, can explain 75 percent of the nationwide boom.

³¹Although CRGM focus on fundamentals, their mechanism of diagnostic overshooting to a change in the dividend drift rate could also apply to a persistent credit expansion due to changes in bank regulation or lending technology.

Finally, [Adam et al. \(2025\)](#) introduce a model in which households assume that house price growth is the sum of a persistent long-run drift and transitory noise, and they estimate the drift as Bayesians. They show that the data support weak updating, which leads to initial underreaction and subsequent overreaction over three years, resulting in a form of extrapolation that they argue is consistent with survey evidence. They do not, however, explore how these expectations would operate in the 2000s cycle; they apply them only to optimal monetary policy.

These types of models can explain the cycle's dynamics, which rational expectations fail to capture. For instance, CRGM and Jacobson both explain the gradual, accelerating price growth during the boom as optimism builds, driven by a shock to the growth rate of fundamentals (CRGM) or credit (Jacobson). This matches the two-stage boom revealed by the empirical evidence. It also fits the empirical findings in [Greenwald \(2026\)](#) and [Johnson and Tzur-Ilan \(2026\)](#), both of whom find impacts of their credit measures that strike me as too large to be explained by credit alone but that are potentially consistent with credit triggering optimism. While these models typically feature a single exogenous shock for parsimony, Section 3 indicates that a cluster of successive shocks to fundamentals, interest rates, and credit supply occurred in the late 1990s and early 2000s. This swarm of shocks may amplify endogenous expectations, as agents interpret persistent price appreciation as a new paradigm and become overoptimistic.

Extrapolation makes the housing market highly sensitive to changes in fundamentals and credit. While this is a feature that lets modest shocks generate a large cycle in the 2000s, it can also be a bug: the quantitative findings are sensitive to small changes in parameter values and in the magnitude of the shocks that trigger the cycle. Furthermore, this class of models may struggle to explain periods of relative stability. Despite these drawbacks, their realistic dynamics suggest that these models are a step in the right direction.

Non-rational models also highlight an additional factor in the boom: low *levels* of interest rates. Low interest rates make demand, and consequently prices, more sensitive to expected future price growth because agents discount future periods by less.³² This

³²It matters that optimism is about growth rates. Optimism about levels would not be amplified by lower

holds in user-cost models (e.g., [Himmelberg et al., 2005](#)) but is amplified by overoptimism about future growth rates. For instance, CRGM show that setting real interest rates equal to their level in the 1980s would roughly halve the size of the boom in their model due to this logic. This suggests that while *declines* in interest rates played only a small role in the boom, the secular decline in interest rates from the 1980s through the 2000s and the resulting low *level* of interest rates in the early 2000s may have amplified the effect of overoptimism on prices.

4.5 Models of Heterogeneous Optimism and Speculation

The last class of models considers dispersion in optimism, which, as discussed in Section 3.5, works through both the optimism of the marginal buyer and speculation. To the extent that a small group of optimistic homeowners drove house prices up, models without heterogeneity in beliefs may misdiagnose the causes of the boom.

Because speculation played a limited role nationwide and because modeling heterogeneous beliefs with resale options or a leverage cycle is technically cumbersome, most macro models of the boom abstract from speculation by assuming that all housing is owned by owner-occupants with a single home. These models consequently struggle to explain the explosive booms in late-booming and moderately elastic sand state cities where speculation mattered most.

Several papers have explored heterogeneous expectations in models designed to explain the role of speculation and the abrupt, surprising boom in these cities. [Nathanson and Zwick \(2018\)](#) analyze a model of speculation in undeveloped land, but it is a two-period model. [DeFusco et al. \(2022\)](#) provide a discrete-time model in which extrapolative expectations interact with heterogeneous investment horizons. [Piazzesi and Schneider \(2009\)](#) show how, in an illustrative search model, a small fraction of optimists can have a large price impact. [Dong et al. \(2022\)](#) introduce a theory with heterogeneous expectations in which credit supply expansions make the marginal homebuyer more optimistic, increasing their leverage and pushing up the price-rent ratio. However, they do not pro-

rates: doubling cash flows doubles an asset's value regardless of the discount rate.

vide a quantitative analysis.

[Burnside et al. \(2016\)](#) study a model in which long-run uncertainty about fundamentals interacts with heterogeneous optimism, which spreads through random social interactions among agents much as a disease spreads through a population in an epidemiological model. It is tractable because there are three types—optimists, skeptics, and the vulnerable—and the spread of optimism through social interactions is exogenous to the housing market, leading to dynamics consistent with the gradual increase in optimism and prices. Burnside et al. argue that the model is consistent with the difficulty of identifying fundamentals that are correlated with booms and busts. They develop a quantitative version of the model that incorporates search-and-matching frictions and a rent-own decision, using as moments the mean of CST’s one-year-ahead expectations and the gap between their mean and median. They find that their model can help explain the dynamics of speculation and the price-volume comovement. However, the model takes its social-dynamics mechanism as given—a mechanism that, to my knowledge, has not been directly tested—and omits forces beyond long-run fundamentals, speculation, and overoptimism, which makes it hard to use for quantitatively decomposing the role of other forces.

Several open questions remain about overoptimism and heterogeneity in optimism. First, how did optimism spread across cities and specifically to the sand states? The models in [Section 4.4](#) suggest that a series of credit, interest rate, and fundamental shocks led to persistent price growth that triggered overoptimism, but we do not observe persistent price growth in the cities with late, explosive booms. Typically, models that study the boom across cities treat cities as siloed economies and thus cannot answer these questions. One provocative piece of evidence from [Bailey et al. \(2018\)](#) is that social networks helped spread overoptimism across cities. Another explanation comes from [Schubert \(2024\)](#), who finds that Las Vegas and Phoenix were highly exposed to in-migration from booming cities in coastal California.

Second, how does speculation interact with the other forces? Speculation should be amplified by credit expansion and by both low levels of interest rates and rate declines, but these interactions are understudied. Similarly, there is relatively little evidence re-

garding the interaction between overoptimism and heterogeneity in expectations, even though they are correlated in timing.³³

Third, what is the quantitative role of changes in the optimism of marginal homebuyers and the selection of more optimistic agents into buying houses? Papers like [Dong et al. \(2022\)](#), [Scheinkman and Xiong \(2003\)](#), [Geanakoplos \(2010\)](#), and [Simsek \(2013\)](#) study these channels but do not quantify them. Incorporating heterogeneous expectations and speculation into models alongside other forces to quantify their role remains an open challenge for the literature.

5 What the Bust and Rebound Reveal About the Cycle

The evidence and models analyzed so far have ignored an additional data point: the bust. Grappling with the bust is difficult because so much bears on it. The housing market's interactions with the financial system and the macroeconomy, and the role of policy during the housing bust and Great Recession—including housing and mortgage policy, banking policy, fiscal policy, and monetary policy—could fill an entire article. However, the bust provides too much evidence to ignore, so the approach taken here is to provide a positive, partial-equilibrium analysis of what happened in the housing market, holding the financial system, the macroeconomy, and policy—along with their two-way feedback with the housing market—fixed. In particular, this section examines research on the bust and the rebound that followed, focusing on two questions. First, what does the bust reveal about the causes of the overall cycle? Second, and necessary to answer the first question, what factors contributed to the bust?

In addition to the five factors discussed for the boom—fundamentals, changes in interest rates, credit supply, overoptimistic expectations, and speculation—a sixth factor must be examined to understand the bust: mortgage defaults. Mortgage defaults affect the housing market through two distinct channels. First, foreclosures create supply-demand imbalances that may affect house prices. Second, mortgage delinquency and defaults

³³[Gao et al. \(2020\)](#) provide some limited evidence on extrapolation driving heterogeneity and speculation, but more work is needed.

create losses for banks, which may feed back onto credit supply.

A puzzle about the bust is what drove overshooting on the downside. When the shocks that generate the boom in many of the models discussed in Section 4 are reversed, they produce a bust—but one too small to match the data, missing the overshoot (Kaplan et al., 2020; Greenwald and Guren, 2025; Greenwald, 2026; Jacobson, 2025). Furthermore, across cities, there is a strong nonlinearity in the size of the bust relative to the size of the boom even in non-distressed prices (Guren and McQuade, 2020), implying that overshooting was important. Finally, CRGM argue that rather than a simple boom-bust, the cycle was a boom-bust-rebound, which makes the size of the bust even more puzzling. This section focuses on the factors that can explain the overshooting.

5.1 Forces That Did *Not* Drive Overshooting

Four of the five candidate forces in the boom played a minimal role in overshooting in the bust. While speculation largely evaporated in the bust, unwinding the speculative boost to house prices, it did not play a significant role in pushing house prices down further because short-selling houses is difficult. Fundamentals mattered after 2008, when the Great Recession triggered job losses that fed into the housing market through increased foreclosures, as discussed below. However, national income fell only 4 percent year over year, which is insufficient to explain the magnitude of the decline in house prices. Finally, interest rates had mixed effects: rate increases in 2004–2006 may have slowed the boom, but aggressive easing beginning in mid-2007—and particularly the rate cuts and first round of quantitative easing, which reduced mortgage spreads—worked to arrest rather than accelerate the decline.

Expectations also cannot explain the overshooting. As discussed previously and shown in Figure 3, surveys indicate that rather than becoming overly pessimistic, households *underestimated* the extent of price declines during the bust. In the Shiller and Thompson (2022) survey, one-year-ahead house price expectations were negative in only three counties, and only in 2007 and 2008. The most extreme pessimism was in Orange County, California, where expected price growth was -2.3 percent—even as realized

price growth was -24.3 percent! CST's 10-year-ahead expectations never overshot their long-run path and instead smoothly declined through 2012. The Michigan Survey of Consumers' one-year-ahead expectations did go negative more frequently in the bust, but the mean expected change never fell below -2 percent, no more than 30 percent of respondents said they expected prices to fall in the next year, and average five-year expectations declined smoothly and did not turn negative. This failure to appreciate price declines puts an important restriction on theories of the cycle, as any non-rational beliefs must be either non-extrapolative or asymmetrically extrapolative.

The role of credit is more nuanced. Credit contracted sharply during the bust, particularly in 2008 and 2009, when house prices fell fastest. But it is hard to tell how much of this reflects a contraction in credit supply relative to credit demand. It is even more challenging to tell how much of any contraction in credit supply is due to defaults and declining housing collateral values devastating lender balance sheets and how much is due to changes originating in credit markets as the PLS market dried up and policymakers tightened regulatory requirements. Rather than wading into this thorny debate—which is outside the scope of this article—I focus on whether lending standards contracted relative to pre-boom levels, judged by the characteristics of originated loans.

The disappearance of private-label securitization, subprime mortgages, and nontraditional mortgages ([Keys et al., 2013](#)) appears to be a supply contraction, but a closer look at the data reveals little strong evidence that changes in regulation and PLS markets explain overshooting in the bust. Perhaps surprisingly, LTV ratios on new loans were stable, a pattern explained by Federal Housing Administration (FHA) and Department of Veterans Affairs (VA) loans substituting for the vanishing private-label products ([Adelino et al., 2026](#)). The GSEs also took up some of the slack. Consistent with substitution, areas with greater pre-existing exposure to FHA and GSE lending saw smaller declines in credit and smaller busts ([Passmore and Sherlund, 2021](#)).

As in the boom (Section 3.3.3), most of the credit supply change came through PTI limits. [Greenwald \(2026\)](#) dates the imposition of tighter GSE PTI limits to the spring of 2010, which is consistent with evidence from [FHFA Office of Inspector General \(2019\)](#) that Fannie Mae and Freddie Mac tightened their PTI limits to 50 percent between December

2009 and April 2010. The enforcement of stricter PTI limits was persistent, due both to GSE policy and to the Dodd–Frank ability-to-repay rule, which took effect in 2014 and enforced strict PTI limits on non-GSE loans ([DeFusco et al., 2020](#)). But these rules cannot explain the overshooting: they were imposed after the deepest price declines and were *looser* than the pre-boom limit of 36 percent. Credit tightened sharply and persistently below pre-boom levels with respect to borrower quality ([Anenberg et al., 2019](#)), but I read this as lenders retreating from riskier borrowers as mounting defaults and falling collateral values damaged their balance sheets. On balance, I see little strong evidence of a tightening of mortgage credit supply below pre-boom levels for reasons originating in credit markets, so changes in credit markets are unlikely to have driven price overshooting on their own.

5.2 Defaults, Liquidity, and Overbuilding

Three factors explain the majority of the overshooting: mortgage defaults (both through the effect of delinquencies on bank balance sheets and credit supply and through the effect of foreclosures on the housing market), declining liquidity as sales volume collapsed and time to sell surged, and an overhang of supply due to overbuilding in the boom.

Before turning to the effects of defaults, it is worth briefly reviewing their causes; [Foote and Willen \(2018\)](#) provide a comprehensive treatment. There are three main theories: *strategic* default, the ruthless exercise of a default option when underwater; *cash-flow* default, driven by a life event such as unemployment or an expenditure shock; and *double-trigger* default, which requires both a life event and negative equity (otherwise the defaulter could sell their house). The evidence for widespread strategic default is scant: households with negative equity have low default rates, most do not default until well underwater, and most defaults are associated with low liquidity ([Foote et al., 2008](#); [Bhutta et al., 2017](#); [Gerardi et al., 2018](#); [Elul et al., 2010](#)). The literature long favored the double-trigger view, but [Ganong and Noel \(2023\)](#), who observe borrower cash flows directly, find that among underwater delinquent borrowers, only 6 percent are strategic, 24 per-

cent double-trigger, and 70 percent cash flow only.³⁴³⁵ Regardless of whether negative equity is necessary or not, default requires a shock rather than the exercise of an equity-triggered option. Defaults in the bust were therefore fueled by the Great Recession surge in unemployment.

Guren and McQuade (2020) (GM) use a structural model with double-trigger default to argue that overshooting due to defaults explains the nonlinear relationship between boom size and bust size and, in their calibration, roughly half of the nationwide decline in non-distressed prices. GM introduce a search model in which defaults reduce the price of non-distressed homes through two main effects, triggering a price-foreclosure spiral as price declines cause further defaults.³⁶ First, there is a foreclosure effect: lenders immediately sell the foreclosed houses, but the foreclosed households are locked out of purchasing, so demand contracts relative to supply at the market level. Second, there is a default effect: lenders are pushed against regulatory capital constraints and contract lending, which, because of banks' geographic reach, pushes down prices nationwide. GM fit their model to the nationwide downturn and the cross-section of cities and find that the contraction in demand and lender losses each account for roughly a quarter of the nationwide decline in non-distressed prices.

Other structural models find a somewhat smaller effect of foreclosures. Chatterjee and Eyigungor (2015) provide a structural model in which foreclosures account for 12 percent of the bust.³⁷ In their model, foreclosed households are forced to rent, and prices must fall to induce other renters to buy from a fixed owner-occupied stock. Their smaller estimate for the role of foreclosures reflects the absence of two features of GM's model: the lender balance-sheet channel and search frictions that amplify the impact of the demand con-

³⁴Ganong and Noel define "default" as three missed payments, not foreclosure, but they write that "we observe similar patterns in the data when we define default as foreclosure initiation or as one, two, four, or five missed payments."

³⁵Negative equity is not strictly binary and interacts with the household's financial position and the liquidity of housing, which together determine whether a household with a shock can sell before defaulting. This variant is sometimes called a "modified double-trigger" model.

³⁶GM do not distinguish between foreclosure and default effects as I do here, so the names of these channels differ from those in their paper.

³⁷They attribute 69 percent of the decline to a financial wedge—an exogenous disruption to mortgage credit, not the balance-sheet damage from foreclosures. Marginal contributions in their model need not sum to 100 percent.

traction. KMV also examine the impact of foreclosures on house prices and find almost no effect. This is because they abstract away from a direct foreclosure effect and because their frictionless rental sector absorbs foreclosed units at a price equal to deep-pocketed landlords' willingness to pay, a mechanism that GG's estimates imply is unrealistic.

Empirically, the strongest evidence on the market-level impact of foreclosures—as opposed to hyper-local spillovers onto immediate neighbors—is [Mian et al. \(2015\)](#), who instrument foreclosures with state judicial foreclosure requirements. They find that moving from a state at the median to one at the 90th percentile in foreclosures per homeowner reduces 2007–2009 house price growth by 8 percentage points. They further estimate that foreclosures account for 33 percent of the aggregate house price decline from 2007 to 2009, closer to GM's estimate, although that aggregation neglects the general equilibrium effects absorbed in the missing intercept.

Another force behind overshooting in the bust is an endogenous decline in liquidity, as it took much longer to find a buyer. [Garriga and Hedlund \(2020\)](#) use a housing search model of illiquidity to explain the increase in months of supply from about 5 to nearly 12 during the bust. They show that falling liquidity increases foreclosure frequency because households cannot sell before defaulting. Quantitatively, they find that endogenous liquidity amplifies the bust price decline by 26 percent. Liquidity could also decline if matches do not result in sales because sellers refuse to cut their prices due to loss aversion ([Genesove and Mayer, 2001](#); [Badarinza et al., 2024](#)).

A final force is overbuilding in the boom, which, combined with housing durability, can lead to overshooting in the bust. [Haughwout et al. \(2013\)](#) estimate that 3 to 3.5 million excess units were built in the boom, two to two and a half years of housing production at its trend. One problem with the overbuilding hypothesis is that overbuilding should be most pronounced in elastic markets, most of which had a muted cycle. But overbuilding is central to the bust in the sand states.

Defaults, liquidity, and overbuilding were not the triggers of the bust—the unwinding of the shocks that caused the boom was. Rather, they explain why prices overshot and stayed persistently low *despite* improving fundamentals and supportive policy. The timing supports this: foreclosures peaked in 2009, after the largest price declines occurred in

2008–2009, but stayed elevated for years.

5.3 The Rebound and Implications for the Cycle

CRGM analyze the bust together with the rebound that followed from 2012 to 2019 to shed further light on the cycle’s causes. They point out that cities with larger boom-and-bust cycles also had larger rebounds and greater long-run house price growth over 1997–2019. They empirically measure a long-run fundamental correlated with the boom-bust-rebound pattern and introduce a one-time shock to fundamentals in 1997, modeled as a change in the drift of a housing-dividend process, in a model (discussed in Section 4.4) with endogenous mortgage spreads, double-trigger foreclosures, and diagnostic expectations. Diagnostic expectations drive overshooting in the boom, foreclosures generate overshooting on the downside in the bust, and the rebound emerges as foreclosures subside and prices return to a new balanced growth path with higher fundamental growth.

Quantitatively, CRGM find that absent foreclosures, the bust would not have exhibited overshooting and prices would have fallen by less than a third as much in the quartile of cities with the largest long-run house price growth. They also find that absent overoptimism, there would have been a far smaller boom. A drawback of their approach is that credit constraints are unmodeled and interest rates are held fixed, so they cannot assess whether the expectational boom would have been dampened had credit not also loosened, as suggested by the interactions between credit and optimism in GG. Overall, CRGM’s analysis corroborates the importance of foreclosures in the bust and implies that persistent nationwide shocks are critical to explaining the boom.

The narrative of the bust that emerges has three stages. First, from 2006 to 2008, the shocks that caused the boom began to reverse, prices started to decline, and foreclosures began to ramp up. Given the synchronized nature of the bust across cities, it is hard to determine what triggered the early stages of the reversal, and different models highlight different factors. New negative shocks may have played a role, but it is also possible that people learned about the overshooting as prior shocks and price growth faded while defaults rose. Second, from 2008 to 2010, the temporary shocks that caused the boom

unwound, and the deep recession caused households to lose their jobs while underwater on their homes, unleashing a wave of foreclosures that amplified the bust through a price-foreclosure spiral. Finally, from 2010 to 2012, a “long trough” followed as policies to stabilize the housing market and broader economy, such as quantitative easing and the first-time homebuyer tax credit, supported house prices, which bounced along a rocky bottom because defaults continued to drag on them.

This narrative has three implications for explanations of the overall cycle. First, any non-rational beliefs must be asymmetric or non-extrapolative given the absence of expectational overshooting on the downside. Second, the rebound implies that many of the boom’s most significant drivers did not reverse. Third, the unwinding of the factors that drove the boom need not explain the overshooting in the bust.

6 A Summary of the Perfect Storm and Its Implications

6.1 The Narrative of the 2000s Cycle

This section presents a detailed narrative of the perfect storm based on the evidence. The first stage of the boom, from roughly 1997 to 2002, was concentrated in inelastic, coastal markets. The boom began in the late 1990s, driven by improving fundamentals (reflected in accelerating long-run rent growth) and an expansion in credit supply (fueled by bank branching deregulation and automated underwriting). Relaxations in PTI constraints, which made credit growth broad-based, were central. LTV constraints played a smaller role but are important to include in models alongside PTI.

The second stage of the boom began when the Fed aggressively cut and then kept interest rates low from 2001 to 2004. Simultaneously, automated underwriting may have spread into subprime credit, and private-label securitization expanded sharply in 2003. Whether this second credit expansion originated in credit markets or lender expectations remains controversial; however, the answer does not make or break the overall story because the sequence of shocks still holds regardless of why subprime and securitized credit expanded. Amid sustained price growth—which reflected the clustering of the first-stage

shocks and these second-stage shocks—expectations about future price growth became overly optimistic from roughly 2003 to 2006. Optimism manifested itself not only in average expectations but also in dispersion, which worked both by increasing the optimism of the marginal homebuyer and by fueling speculation. While inelastic markets continued to experience house price growth, this period also saw sudden, explosive price growth in selected more elastic markets concentrated in the sand states, where speculation—and particularly land speculation—was most rampant. Nationwide, however, speculation mattered less.

As the Fed raised rates, defaults accumulated, and the mania cooled, house prices peaked in 2006 and began to decline as temporary shocks reversed themselves. When mortgage losses triggered a financial crisis and mass unemployment, a tsunami of defaults drove price overshooting, as foreclosures reduced demand, defaults weighed on lender balance sheets and crimped credit supply, and liquidity in housing markets evaporated. Overly pessimistic expectations do not appear to have played a role in the bust.

6.2 Quantifying the Role of Each Force

Drawing on my synthesis of the literature, this subsection quantifies the role of each force to help the reader understand the forces' relative magnitudes in explaining the nationwide boom. A theme of the article is that the cycle was not monolithic but occurred in discrete stages that built on one another; my attempt to quantify each force's role in the overall boom should not undermine this message.

The numbers that follow should be treated as rough ranges for two reasons. First, papers disagree due to differences in data and methodology. Second, forces interact, so the impact of a force on its own is often smaller than when it works in concert with other forces. For example, GG find that credit explains one-third of the boom on its own but half in a model with interest rate reductions and overoptimism. Rather than deciding which counterfactual is correct, my approach combines them into the range; as a result, the midpoints add to more than 100 percent. This logic also explains why empirical estimates tend to be lower than model estimates: quasi-experimental designs difference out factors

common to treatment and control (the missing intercept problem), and many interactions show up in the intercept. By contrast, models capture the interactions.

A few details are necessary to interpret these estimates. First, consistent with the partial-equilibrium, housing-market-centric approach of this article, I take the state of the macroeconomy and the path of interest rates as given. Second, when one force triggers another, the interaction is ascribed to the responding force rather than the trigger. For instance, in CRGM's model, fundamentals trigger overoptimism. Taking away the fundamental shock would eliminate most of the boom, but it does not make sense to say that fundamentals can explain almost 100 percent of the boom. The better counterfactual for fundamentals is to turn off overoptimism and see how much of the boom fundamentals explain. In cases where forces are complements rather than one serving as a trigger, the interaction is included with both forces. Details on how this is applied in each case are in the Supplemental Appendix.

Table 1 shows the decomposition, summarizing the ranges from the empirical literature, structural models, and my synthesis of the two. Fundamentals, which began growing in the first stage and continued to grow throughout, explain 10 to 30 percent. Declining interest rates from 2001 to 2004 contribute 10 to 25 percent. Credit supply expansions account for 20 to 50 percent of the overall boom, with the first-stage expansion playing a larger role. Overoptimistic expectations, which kicked in strongly from 2003 to 2006, explain 30 to 50 percent. Speculation accounts for perhaps 5 to 10 percent nationally, but it was central in the late-booming cities in the sand states. This estimate covers only the speculation channel of heterogeneous optimism; the marginal-buyer channel has not been quantified.

The literature has identified several quantitatively important interactions. Expansions in credit supply and reductions in interest rates amplify the impact of expectations in models like GG because borrowers can translate their optimism into demand, which is not the case if credit constraints bind. Low interest rate *levels* also amplify optimism, since expectations about future house prices are discounted by less. Finally, credit interacts with speculation, as speculators often rely on leverage. There are also dynamic interlinkages between these forces. The narrative that the swarm of positive shocks from

Table 1: Contribution of Each Force to the 2000s Housing Boom

Force	Period	Empirics	Models	Overall
Fundamentals	Throughout	10–30%	28%	10–30%
Interest-rate decline	2001–2004	6–26%	25–38%	10–25%
Credit supply	Late 1990s First Wave, 2003 Second Wave	20–30%	32–60%	20–50%
Overoptimism	2003–2006	~25%	30–50%	30–50%
Speculation	2004–2006	5–10%	No Estimate	5–10%

Note: Entries in the last three columns are shares of the 2000s boom, defined as the 80 percent real (120 percent nominal) rise in the national Case-Shiller index from 1997 to its 2006 peak where possible, or otherwise the corresponding rise in the price-rent ratio for models that emphasize its behavior. The “Period” column gives the years in which each force was most active; the “Empirics” column reports quasi-experimental and time-series estimates; the “Models” column reports quantifications from structural models; and the “Overall” column is my synthesis rather than a union of the other two columns. Midpoints of the “Overall” shares sum to more than 100 percent because the forces interact. The model ranges exclude [Kaplan et al. \(2020\)](#). See Supplemental Appendix C for sources and details.

1997 to 2003 generated overoptimism suggests that removing the other forces might have tempered overoptimism in the late boom.

6.3 Implications for Models

Heterogeneous-agent models of the housing market following [Kaplan et al. \(2020\)](#) have become workhorse models for many applications. They capture much of the richness across households in the data and allow for realistic housing and mortgage market institutions. The key model ingredients are overlapping generations, incomplete markets, and realistic long-term mortgages with endogenous refinancing and default. Much of the conflict in the literature on this class of models traces to how rental markets are modeled; once this margin is disciplined, disagreement over the quantitative roles of various forces shrinks. While such a model involves abstractions, notably pooling housing markets rather than segmenting them by quality, and ignoring speculation, it has been successful in analyzing the aggregate dynamics of house prices in the 2000s cycle and many of the channels cited in this article.

Current models have several drawbacks. First, the literature needs better ways to cal-

ibrate exogenous changes in PTI and LTV constraints, especially given that the empirical literature shows LTVs did not vary significantly over the cycle ([Adelino et al., 2018, 2026](#)). Second, the literature offers no consensus on the right expectations model, as detailed below. Third, many of these models feature unexpected permanent shocks and lack realistic dynamics that reflect the boom's two-stage nature. Models that incorporate learning or non-rational expectations have improved on this. Fourth, these models often struggle to explain the explosive, late booms in sand state cities such as Las Vegas and Phoenix. Finally, these models focus on household balance sheets and sweep heterogeneity in beliefs under the rug. They cannot explore whether the emergence of a small group of optimistic households as marginal buyers in relatively thin housing markets helped generate aggregate dynamics.

One lesson of this synthesis is that lending decisions have pecuniary externalities that work through house prices. Automated underwriting relaxed PTI limits because PTI added little predictive power for individual loan default. While this holds in partial equilibrium, it need not hold once house prices are endogenous: when all lenders relax PTI constraints, house prices rise, and other forces like overoptimism and speculation are triggered, leading to a boom and a bust. The macroprudential policy literature has identified these pecuniary externalities as central to the case for regulating household credit ([Jeanne and Korinek, 2019](#); [Dávila and Korinek, 2018](#)).

The importance of interest rates and credit supply in the boom and the role of foreclosures in the bust imply that housing market stabilization policy, including macroprudential, foreclosure, and monetary policy, can be effective at dampening a housing cycle. Although housing stabilization policy is outside the scope of this article, many of the cited papers analyze policy implications.

6.4 Areas for Future Research

Several areas remain for future research. One understudied area is the dynamic linkages between the various stages of the cycle and the interactions among root causes. A few empirical studies examine these linkages and interactions; for instance, [Mian and Sufi](#)

(2022) assess how the PLS expansion led to pockets of both optimism and speculation. Further research is needed. Empirical analyses often struggle to detect effects with long causal chains. Structural models have largely been used to disentangle forces rather than to measure interactions or study dynamics. The nascent literature on learning and non-rational expectations is a step forward, but more work is necessary to understand and test models that seek to capture the dynamics of the boom.

Future research should also settle on a consensus model of housing market expectations. Several reasonable candidates exist, including adaptive learning in an information-poor environment, diagnostic expectations, and extrapolative expectations, although the absence of expectational overshooting in the bust casts some cold water on extrapolative models that are not asymmetric for booms relative to busts. The lack of consensus is likely to persist without additional data, and the dearth of comprehensive, modern survey evidence on expectations during the 2000s boom is a major hindrance. One potential way forward is to integrate loss aversion, which has been shown to matter in housing markets (Genesove and Mayer, 2001; Andersen et al., 2022) and may help explain asymmetric market dynamics without resorting to ad hoc differences in beliefs between booms and busts. Badarinza et al. (2024), for instance, integrate loss aversion into macro-housing models, but they do not apply their framework to the 2000s cycle. Another potential way forward is to apply ideas from the behavioral macro literature on expectation formation to house price expectations.

Several questions about expectations remain to be settled even without additional data. The sensitivity of existing models to shocks makes it hard to explain cross-city heterogeneity and the pre-2000s stability of housing markets. Additionally, across countries, one major outlier—Japan—remained stable in the 2000s after its 1986–1991 cycle, suggesting that lived experience tempers overoptimism, in line with Malmendier and Nagel’s (2011) work on the Great Depression. How lived experience shapes expectations during asset bubbles and what that implies for modeling expectations remain underexplored.

At a deeper level still, any model of house price expectations needs to connect to overarching theories of asset price bubbles and busts across time and history. As Kindleberger argued (Kindleberger and Aliber, 2005, p. 12), in a speculative mania, “rational exuber-

ance morphs into irrational exuberance.” How this transformation occurs is a major question not only for the study of housing markets but also for macro-finance more broadly.

The next area for additional research is the drivers of the credit supply expansion. The subprime expansion is the area of greatest disagreement, and much of that stems from methodology: the literature has focused on whether subprime lending expanded because of incentives created by securitization, rather than examining underlying technological drivers such as automated underwriting. Recent research has established the role of automated underwriting in relaxing credit constraints for prime borrowers, and there is some evidence that automated underwriting technologies spread into subprime in the early 2000s. However, this evidence is at best suggestive; more concrete analysis of the roles of regulation and lending technologies in private-label securitization and the subprime expansion is necessary, as is research that quantifies the aggregate price impact of securitization.

More can also be done to use additional cross-sectional data and understudied moments to discipline models of the cycle. Examples include rent data, sales volume, and house price changes across cities (rather than within a single city, as in [Landvoigt et al. 2015](#)), as well as improved data on beliefs.

Another gap to be filled is incorporating heterogeneous expectations and speculation into heterogeneous-agent models. The quantitative models used to decompose forces largely omit speculation and downplay the changing identity of the marginal household—omissions that should be rectified. Relatedly, most models assume cities are small open economies modeled separately. How did cities become optimistic without an extended run of high local price growth?

A final area for future work is when and where housing bubbles and cycles occur. This can be thought of as three interlocking questions. First, what conditions are necessary versus sufficient for a boom-bust cycle? Why have prior waves of innovation and liberalization in mortgage markets either failed to trigger a similar cycle or led to a boom without an ensuing bust, as pointed out by [Glaeser \(2013\)](#)? The literature offers some answers, including the importance of low interest rates in the early 2000s, but more research is necessary. Second, why was speculation concentrated in specific markets, and

why those markets? Third, as Kindleberger ([Kindleberger and Aliber, 2005](#), p. 13) wrote, “virtually every mania is associated with a robust economic expansion, but only a few economic expansions are associated with a mania.” Booms driven by fundamentals often push up rents, so a rise in rents can indicate a boom without a bubble, as in the post-pandemic housing boom ([Loewenstein and Willen, 2023](#)). But when rents are stable and price-rent ratios are rising, how can we distinguish healthy expansions from manias in real time?

7 Conclusion

The 2000s housing boom and bust stands out in the history of housing prices and rents in the United States as an anomaly, remarkable for its size, scope, and macroeconomic impact. Two decades of research show that it resulted from a confluence of factors, with all the usual suspects acting together to create a housing cycle greater than the sum of its parts. Indeed, this article shows that many—though not all—apparent disagreements in the literature stem from studying different time periods or places, and that the literature as a whole reveals a cycle that emerged in distinct, mutually reinforcing stages. While many important questions remain, our enhanced understanding of this cycle should help us better diagnose and respond to future housing booms.

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Supplemental Appendix

A Additional Facts

Figure [A1](#) shows several different interest rate series, home mortgage debt, and the rental vacancy rate. Figure [A2](#) compares the cycle for several publicly available house price indices, with Table [A1](#) providing supporting summary statistics.

The FHFA indices are the least volatile in the boom, the Zillow and Freddie Mac indices fall in the middle, and the Case-Shiller index is the most volatile. For this reason, it is crucial to use the same index to construct the numerator and denominator of any ratio.

The indices' relative volatilities have to do with measurement. The FHFA and Freddie Mac indices are geometric indices that compute an equally weighted geometric mean price, while the Case-Shiller indices are arithmetic indices that compute a value-weighted arithmetic mean price.³⁸ The Freddie Mac national index then weights states by value, which is why it behaves more like the Case-Shiller index, as value weighting puts more weight on states like California that boomed more. Zillow uses a trimmed mean of individual home values from its proprietary valuation model. The indices also differ in the degree to which they include homes purchased with non-conforming loans, but this matters less than value weighting versus equal weighting in the boom. In the bust, the composition matters, as homes with conforming loans had a weaker bust. Arithmetic value-weighted indices track changes in the aggregate value of the housing stock, while equally weighted indices understate that change because of geometric averaging.

Figure [A3](#) plots the size of each metropolitan area's boom against the growth in its per-capita personal income over the same window. While this correlation is merely suggestive—causality likely flows in both directions—there are two main takeaways. First, house prices grew much more than income: mean real income growth across these metros is 17 log points relative to a mean boom of 35 log points. Second, the sand states had much larger house price growth relative to income (Table [A2](#)).

³⁸The arithmetic value-weighted repeat-sales estimator is due to [Shiller \(1991\)](#); the FHFA geometric weighted-repeat-sales method is compared with it by [Calhoun \(1996\)](#). The Case-Shiller and Freddie Mac methodologies are described in [S&P Dow Jones Indices LLC \(2026a\)](#) and [Freddie Mac \(2026c\)](#).

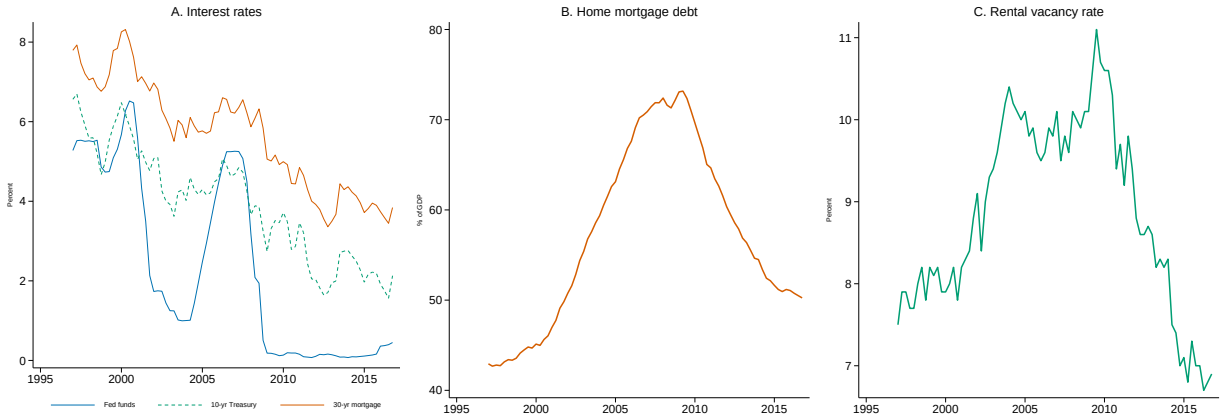


Figure A1: Secondary Aggregate Series

Note: Panel A: The effective federal funds rate, the 10-year Treasury yield, and the 30-year fixed mortgage rate (Board of Governors of the Federal Reserve System, 2026b,e; Freddie Mac, 2026a). Panel B: Home mortgage debt of households as a share of GDP (Board of Governors of the Federal Reserve System, 2026c; U.S. Bureau of Economic Analysis, 2026d). Panel C: The rental vacancy rate (U.S. Census Bureau, 2026c).

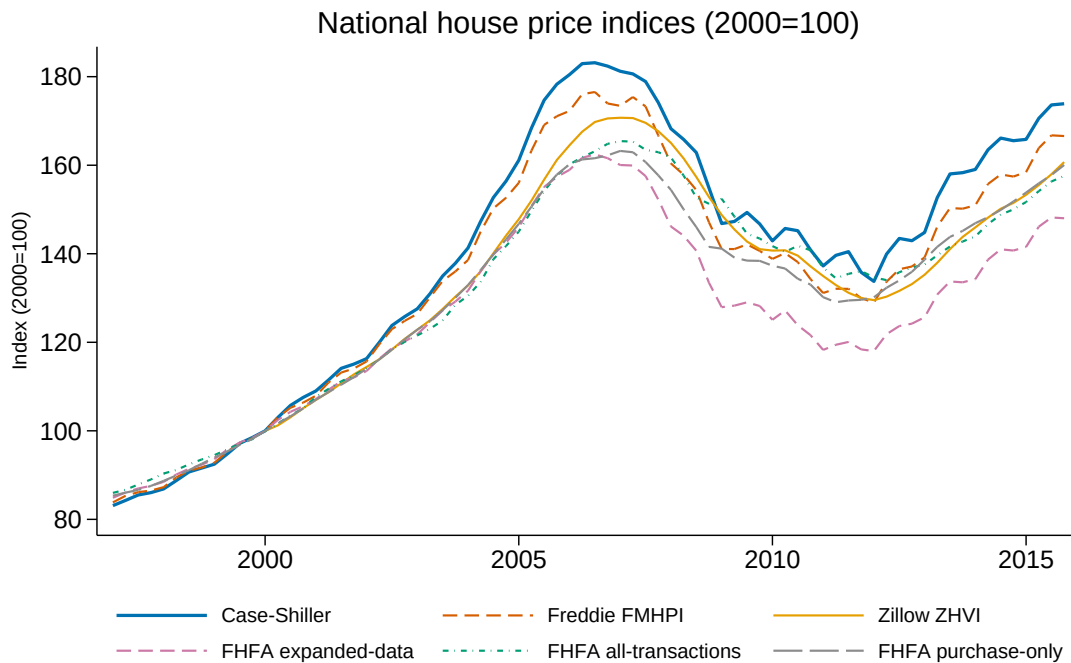


Figure A2: National House Price Indices Compared

Note: Each house price index is normalized to 2000Q1 = 100, and all but Zillow, which begins in 2000, begin in 1997. The indices are the S&P Cotality Case-Shiller index (S&P Dow Jones Indices LLC, 2026b), the value-weighted Freddie Mac FMHPI national index (Freddie Mac, 2026b), the Zillow Home Value Index (Zillow, 2026), and the three FHFA indices—Expanded-Data (U.S. Federal Housing Finance Agency, 2026b), All-Transactions (U.S. Federal Housing Finance Agency, 2026a), and Purchase-Only (U.S. Federal Housing Finance Agency, 2026c).

Table A1: Boom and Bust Magnitudes Across National House Price Indices

Index	Boom (to 2006Q3)			Bust	Weighting / coverage
	1997–06	2000–06	2003–06	06–12	
Case-Shiller National	+120%	+83%	+44%	−27%	Value-wtd; all deeds
Freddie Mac FMHPI	+111%	+77%	+40%	−27%	State value-wtd; GSE
Zillow ZHVI	—	+70%	+38%	−24%	Value-wtd; all homes
FHFA Expanded-Data	+91%	+62%	+33%	−27%	Equal-wtd; GSE/FHA/records
FHFA All-Transactions	+90%	+63%	+34%	−17%	Equal-wtd; + refi appraisals
FHFA Purchase-Only	+89%	+62%	+32%	−19%	Equal-wtd; conforming

Note: All indices are nominal. Boom columns give the change to the 2006Q3 peak from 1997Q1, 2000Q1, and 2003Q1; the bust is 2006Q3 to the 2012Q1 trough. Zillow begins in 2000, so its 1997–06 boom is unavailable.

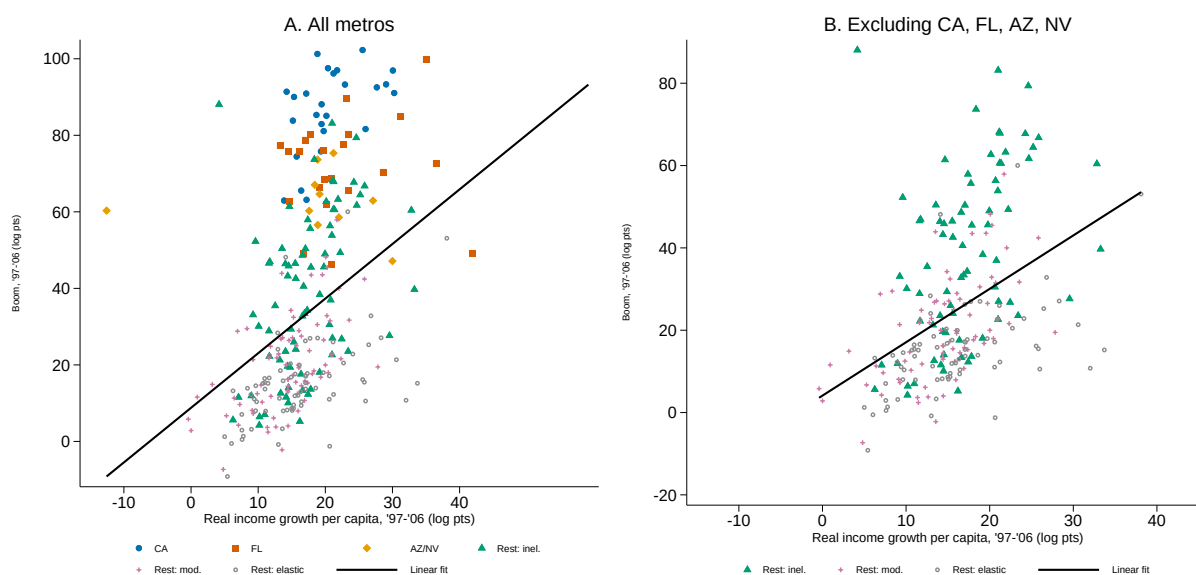


Figure A3: Boom Size and Local Income Growth Across Metropolitan Areas

Note: Total real house price growth from 1997Q1 to 2006Q3 (log points) against real growth in per-capita personal income from 1997 to 2006 (log points), for the same 283 metropolitan areas as Panels A, B, and D of Figure 2 and using the same six groups and markers. Panel A shows all metros; Panel B drops California, Florida, Arizona, and Nevada. Solid lines are linear fits. Real house prices are the FMHPI deflated by the GDP deflator, as in Figure 2. Per-capita personal income is constructed from BEA county personal income and population (table CAINC1, which BEA publishes only at the county level; [U.S. Bureau of Economic Analysis, 2026a](#)) aggregated to CBSAs on the 2013 Census delineation ([U.S. Census Bureau, 2026a](#)) as total income divided by total population, then deflated by the GDP deflator.

B Calculations

Gordon Growth Calculation in Section 3.1. A home's value is the present discounted value of future rents. Let R denote the current rent, r the required return on housing (discount rate plus risk premium, taxes, and maintenance), and g the constant trend growth rate of rents. Discounting the growing rent stream gives the Gordon growth formula,

$$P = \sum_{t=1}^{\infty} \frac{R(1+g)^{t-1}}{(1+r)^t} = \frac{R}{r-g}, \quad \text{so} \quad \frac{P}{R} = \frac{1}{r-g}.$$

The spread $r - g$ is calibrated to a pre-boom price-rent ratio of $P/R = 20$ from [Davis et al. \(2008\)](#). Pre-boom trend rent growth is $g_0 = 0.46\%$ from [Chodorow-Reich et al. \(2024\)](#) (CRGM). Then:

$$r - g_0 = \frac{1}{20} = 0.05 \quad \implies \quad r = 0.05 + 0.0046 = 0.0546.$$

Holding r fixed, the acceleration of trend rent growth to $g_1 = 1.23\%$ in CRGM raises the Gordon growth price-rent ratio to

$$\frac{1}{r - g_1} = \frac{1}{0.0546 - 0.0123} = \frac{1}{0.0423} = 23.6,$$

an increase of $23.6/20 - 1 \approx 18\%$. Combining this with the roughly 11% rise in rents over 1997–2006, the price increase is

$$\frac{P_1}{P_0} = \underbrace{\frac{R_1}{R_0}}_{1.11} \times \underbrace{\frac{r - g_0}{r - g_1}}_{1.18} = 1.31,$$

or about 30%, relative to the 80% rise in real prices. This is an upper bound because it assumes that the increase in the growth rate of fundamentals is permanent and fully capitalized into house prices.

The Boom Denominator. For the share of the boom explained, figures from the cited papers are used whenever possible to ensure that the same series is used for the numer-

ator and denominator. Where such figures are not available, the “share of the boom” is calculated as the share of the roughly 80% real (120% nominal) rise in the national Case-Shiller index from 1997 to the 2006 peak, documented under Section 2 in Appendix C below. Some papers report price-rent ratios, and in those cases this ratio is used instead of prices.

Interest-Rate Counterfactuals in Section 3.2. The micro bound applies a semi-elasticity of 3–5 to a 75–150 bps decline in mortgage rates:

$$3 \times 0.75\% \approx 2.3\% \quad \text{to} \quad 5 \times 1.50\% = 7.5\%.$$

The decline in mortgage rates comes from applying a three-quarters pass-through to the 100–200 bps Taylor-rule deviation. This pass-through comes from noting that the 30-year mortgage rate fell about 340 bps peak to trough (May 2000 to June 2003) while the real federal funds rate (the effective rate less realized year-over-year PCE inflation) fell about 450 bps from December 2000 to June 2003.

The macro bound applies a semi-elasticity of 5–8 to a 100–200 bps change in short rates:

$$5 \times 1.00\% = 5\% \quad \text{to} \quad 8 \times 2.00\% = 16\%.$$

This is an upper bound because macro feedback on income should be counted as fundamentals.

The consensus 5–12% price increase in the text, as a share of the 80% real boom over 1997–2006, is

$$5/80 \approx 6\% \quad \text{to} \quad 12/80 \approx 15\%,$$

or 6–15% of the real boom.

Credit-Supply Back-of-the-Envelope Calculations in Section 3.3.3. The Credit Empirics cell of Table 1 is based on the two papers on the 1990s, each converted to a share of the 80% real boom.

- [Johnson and Tzur-Ilan \(2026\)](#) estimate that the adoption of GSE automated under-

writing cumulatively raised house prices by 16% over 1993–2005. That is a price-level increase, so as a share of the boom it is:

$$16/80 \approx 20\%.$$

- [Favara and Imbs \(2015\)](#) report only shares—branching deregulation explains one-third to one-half of the increase in house prices over 1994–2005—and give no percentage level, so a level must be supplied from a national index. Real Case-Shiller home prices rose 67.4% over 1994Q1–2005Q1, so their lower share implies

$$\frac{1}{3} \times 67.4\% = 22.5 \text{ pp}, \quad 22.5/80 \approx 28\%,$$

and their upper share implies 33.7 pp, or about 42%. Their separate statement that deregulation explains “virtually all” of the pre-2002 increase implies 29.8 pp, or about 37%.

The cell is reported as 20–30%: [Johnson and Tzur-Ilan \(2026\)](#) is the floor and the lower share of [Favara and Imbs \(2015\)](#) is the ceiling. This is deliberately conservative—Favara and Imbs’s own upper share and their pre-2002 statement both imply substantially more—because their estimate is a local average treatment effect (LATE) identified off counties near the borders of deregulating states, and extrapolating it to the national index raises the missing-intercept problem discussed in Section 1. The two studies identify different forces, branching deregulation and automated underwriting, which Section 3.3.3 treats as jointly constituting the first episode; the range is therefore what either force alone supports, not a sum, and to the extent they are complementary it is a floor. Because real prices were essentially flat from 1994 to 1997 (+1.0%), applying a 1994–2005 share against the 1997–2006 boom denominator makes little difference.

C Sources for Numbers in the Text

C.1 Section 2: Facts

Anchor boom and bust magnitudes. The headline magnitudes come from the S&P Co-tality Case-Shiller U.S. National Home Price Index ([S&P Dow Jones Indices LLC, 2026b](#)), quarterly averaged, deflated by the GDP deflator ([U.S. Bureau of Economic Analysis, 2026c](#)). From the 1997Q1 trough to the 2006Q3 peak the nominal index rises about 120% and the real index about 80%; from the 2006Q3 peak to the 2012Q1 trough the nominal index falls about 27% and the real index about 33%.

Figure 1

- **Sales and construction volumes (Panel B).** Housing starts ([U.S. Census Bureau and U.S. Department of Housing and Urban Development, 2026](#)) fall about 79% peak to trough, from a January 2006 peak near 2.27 million SAAR to an April 2009 trough near 0.48 million. NAR existing-home sales ([National Association of Realtors, 2013](#)) peak in 2005 at about 7.25 million SAAR, one year ahead of the 2006 price peak. This reflects the volume-leads-price pattern of [DeFusco et al. \(2022\)](#), whose price-volume cross-correlation is maximized at a 24-month lag (their Figure 2). Note that the NAR data are proprietary and not included in the replication package, but they can be purchased from the NAR.
- **Mortgage originations by segment (Panel C).** The origination-composition series is digitized from Figure 1 of [Keys et al. \(2013\)](#). The GSE share of originations falls from about 70% at its peak to about 40% by 2005–06, and subprime plus Alt-A reaches about 40%.
- **Outstanding mortgage debt (Panel D).** Outstanding home mortgage debt ([Board of Governors of the Federal Reserve System, 2026c](#)) rises from about \$4.9 trillion in 2001Q1 to about \$10.6 trillion by 2007Q4 (and peaks at about the same level in 2008Q1), more than a doubling.

The counterfactual paths are digitized from the bottom panel of Figure 13 of the working paper version of [Foote et al. \(2021\)](#) (NBER Working Paper 22985). Each

path holds total debt at its 2001Q1 level and adds only the growth in the indicated component of privately securitized debt. Private-label growth (subprime, Alt-A, and prime jumbo together) of roughly \$2.0 trillion against a total increase of roughly \$5.7 trillion (2001Q1 to 2007Q4) is about 36%. This is the basis for the statement in the text that growth in privately securitized subprime, Alt-A, and prime jumbo debt cannot account for most of the increase in the stock of mortgage debt.

- **Homeownership (Panel E).** The homeownership rate ([U.S. Census Bureau, 2026b](#)) is about 65% in 1997, plateaus near 69% from 2004 through 2006 (69.2% in 2004Q2 and 2004Q4, 69.0% in 2006Q3), and falls to about 63% by 2016. The 2016 annual average of 63.4% is the lowest since the mid-1960s: no year from 1966 through 2015 is at or below it, and the nearest match in the series is 1966 at 63.5%. The decline continues well past the 2012 price trough, so the fall is not contemporaneous with the bust in prices.
- **Mortgage distress (Panel F).** All three series are digitized from Figure 1 of [Guren and McQuade \(2020\)](#), which plots monthly CoreLogic data from January 2000 to September 2013; Panel F shows quarterly averages.

The delinquency line is the sum of the two series in their Figure 1A. The REO share of sales and foreclosure completions are from their Figure 1B. The REO share peaks at 27% of sales in April 2009, and completions peak at an annual rate of 1.7% of the housing stock in March 2010. The “about 8 percent of the owner-occupied stock foreclosed 2006–13” figure is stated verbatim in that paper (their p. 1331).

The “as high as 75 percent in cities like Las Vegas” figure is also from [Guren and McQuade \(2020\)](#), whose Section 1 states that “[i]n Las Vegas, for instance, prices fell nearly 60 percent, and the REO share was as high as 75 percent,” referencing their Figure 2.

Figure 2

- Real prices are quarterly means of the monthly seasonally adjusted Freddie Mac House Price Index for each CBSA ([Freddie Mac, 2026b](#)).

Table A2: Boom and Bust by Metropolitan Group

Group	Metros	Early boom 97Q1–03Q4		Late boom 03Q4–06Q3		Boom 97Q1– 06Q3	Bust 06Q3– 12Q1	Income growth 97–06	Boom / income
		Total	Per yr	Total	Per yr	06Q3	12Q1	97–06	
California	25	59.3	8.8	27.2	9.9	86.5	−71.5	20.6	4.2
Florida	22	35.2	5.2	36.5	13.3	71.7	−69.9	22.6	3.2
Arizona–Nevada	10	26.1	3.9	36.5	13.3	62.6	−73.0	18.1	3.5
Rest: inelastic	76	25.3	3.7	13.2	4.8	38.5	−27.4	17.2	2.2
Rest: moderate	75	15.3	2.3	4.8	1.8	20.1	−20.9	14.4	1.4
Rest: elastic	75	11.1	1.7	4.3	1.6	15.4	−15.5	16.1	1.0

Note: Group means across metropolitan areas, in log points. House prices are real Freddie Mac HPI, with groups as in Figure 2; income growth is real per-capita personal income, as in Figure A3. Per-year columns annualize the early and late booms. Boom/income is the ratio of the group means.

- Of the 387 CBSAs in the FMHPI, 283 are assigned a group and appear in Panels A, B, and D: all sand-state metros, plus the rest-of-country metros with a Saiz elasticity. Panel C, which plots the boom against elasticity, covers the 270 with an elasticity.
- The 1997 start and 2012Q1 trough follow [Chodorow-Reich et al. \(2024\)](#); the 2003Q4 split between the early and late boom is made by the author to reflect various cuts between the early and late boom in the literature. The boom ends at 2006Q3, the peak of the nominal Case-Shiller index.
- Table A2 reports the group means behind Panels A–D, in log points.
- The figures “3.9 then 13.3 log points annually” quoted in the text are the Arizona–Nevada per-year means.
- Florida’s late pace (13.3 per year) is essentially identical to Arizona–Nevada’s. What distinguishes them is Florida’s higher early pace (5.2 against 3.9), so its acceleration is smaller, which is the distinction Panel B draws.

C.2 Section 3.1: An Improvement in Fundamentals

Rent growth. The rent-growth numbers in the Gordon growth calculation in Appendix B are from [Chodorow-Reich et al. \(2024\)](#). Real rent growth is 0.46% per year over 1988–1997

and accelerates to 1.23% over 1997–2017; total rent growth over 1997–2006 is about 11% (their Supplementary Appendix Section C.5 and Table B.2).

CRGM footnote. The 28% figure is read from Figure 9(a) of [Chodorow-Reich et al. \(2024\)](#), which plots the model’s price index for their baseline calibration to the fourth quartile of CBSAs. It is the share of price growth at the peak in the baseline model accounted for by the “ $\theta = 0$ ” model, which removes diagnostic expectations but includes learning. This is the version of their model without expectational overshooting but with informational frictions.

Fundamentals’ share of the boom. “Fundamentals alone explain 10 to 30 percent of the boom” is the author’s synthesis. The upper bound is shaded down from the Gordon growth calculation because its assumptions are extreme. [Chodorow-Reich et al. \(2024\)](#) report 11% real rent growth, which would be 14% of the boom if the price-rent ratio were held fixed. [Chodorow-Reich et al.’s \(2024\)](#) model implies that 28% of the price growth in the boom can be accounted for by fundamentals in the quartile of CBSAs with the highest house price growth, as noted in footnote 8 of the main text. [Loewenstein and Willen \(2023\)](#) find real rents rose less than 2% over 2000–06 against a more than 50% rise in the real price of renter-occupied properties. [Ferreira and Gyourko \(2023\)](#) report an unconditional price jump at boom onset of about 11% (about 5% conditional), with buyer income growth reverting to trend by relative year 3 (their Table 2).

C.3 Section 3.2: The Decline in Interest Rates from 2001 to 2004

Peak-to-trough rate declines. According to FRED, the effective federal funds rate ([Board of Governors of the Federal Reserve System, 2026a](#)) was approximately 6.5% in December 2000. It declined to 1.75% in December 2001 and to 1.0% in June 2003, matching the Fed’s announced target rate ([Dokko et al., 2009](#)), and was raised starting in June 2004. The 10-year Treasury yield at constant maturity on an investment basis ([Board of Governors of the Federal Reserve System, 2026d](#)) peaked at 6.79% on January 20, 2000, hovered around 4% from late 2002 through early 2003, and bottomed out at 3.13% on June 13, 2003, before rising back to the 4–4.5% range, a peak-to-trough decline of 366 bps. The text’s “about

365 bps” uses the daily DGS10 series; the monthly averages (GS10) plotted in Figure A1 fall 333 bps, from 6.66% in January 2000 to 3.33% in June 2003. The 30-year fixed-rate mortgage average from Freddie Mac (Freddie Mac, 2026a) peaked at 8.64% in May 2000 (the 2000 average was 8.05%), declined to 7.0% in early 2002, and bottomed out at 5.21% in June 2003, a peak-to-trough decline of 343 bps, before hovering between 5.5% and 6% from 2003 to 2005.

For real rates, the size of the decline depends on which measure of inflation one uses and whether it is expected or realized. Realized PCE inflation fell by 40–70 bps, depending on the exact dates used. Glaeser et al. (2013) report that the real 10-year Treasury yield fell 120 bps from 1996 to 2006 and 190 bps from 2000 to 2005, using expected inflation from the Livingston Survey rather than realized inflation. The two figures differ because of the window, not because of the deflator.

Dokko et al.’s Taylor-rule gap and boom accounting. The 100–200 bps Taylor-rule gap in 2003–04 is read from their Figure 2, using the Taylor rule with real-time core PCE inflation and real-time output gap data. “They find that interest rates and other macro variables together cannot account for most of the post-2002 boom” refers to Figure 11 of Dokko et al.

Micro semi-elasticities. Adelino et al. (2025) report 1.3–5.3 (their p. 13); Davis et al. (2020) report 3.4 (their p. 9); Fuster and Zafar (2021) report 2.5 (their p. 261).

Bhutta and Ringo (2021) find little immediate effect on purchase prices (their Section 5.2 and Table 3) and a rise of less than 2 percentage points relative to the control group by the end of 2015 (their Section 8).

Macro semi-elasticities and Taylor-rule deviations. Glaeser et al. (2013): Lower real rates “cannot account for more than one-fifth” of the 1996–2006 boom with the full rate decline (their p. 304; their Table 7.8: 120 bps times a semi-elasticity of 6.8 gives 8.2%, “less than one-fifth of the total” on p. 329).

Kuttner (2013) reports that a 25 bps expansionary shock raises house prices by 0.3–0.8% across the VAR studies in his Table 1; dividing by 0.25 gives the 1.2–3.2 in the text, which is this article’s conversion rather than his figure.

Časta’s (2025) headline is a maximal long-run semi-elasticity “close to 10,” stated in

the abstract and repeated in the results (“at average levels of housing supply elasticity or interest rates, the maximum house price semi-elasticity remains close to 10”). He separately gives an uncertainty band of “a range of 5% to 15%.”

[Taylor \(2007\)](#): “almost 300 bps too low” is read off his Figure 1.

Timing and cross-country evidence. The timing point draws mainly on [Kuttner \(2013\)](#), who notes that house prices began to appreciate in 1998, three years before real user cost fell, and that real user cost stayed low even as the Fed raised its target by 3.25 percentage points from mid-2004 to mid-2006, a divergence known as the bond yield “conundrum” (his pp. 171–172). [Dokko et al. \(2009\)](#) likewise note that the boom’s 1998 start predates the post-2001 easing, although appreciation accelerated after 2002 (their p. 23), and their FRB/US simulations show the actual federal funds rate above the simple-rule paths after early 2005 (their Figure 9). The argument that the 2004–06 tightening should offset the 2003–04 stimulus is this article’s, built on this evidence. [Dokko et al. \(2009\)](#) describe the cross-country evidence as “mixed” (their p. 31).

C.4 Section 3.3: An Expansion in Credit Supply

C.4.1 Section 3.3.1: Credit and House Prices

[Favara and Imbs \(2015\)](#) use interstate branching deregulation (the Rice–Strahan index). Their impact elasticity is about 0.12–0.14, rising to a peak of about 0.2 two years out (their Table 5 and Figure 3).

[Di Maggio and Kermani \(2017\)](#) report in their abstract that the January 2004 federal preemption of state anti-predatory-lending laws produced “an 11% increase in annual lending in the 2004–2006 period, which is associated with a 3.3% rise in annual house price growth rate.” The elasticity is stated directly in their text: “[a] 10% increase in loan origination leads to a 3.3-percentage-point increase in the growth rate of house prices” (their p. 3713), which is their IV point estimate of 0.330 (their Table 5, column 6); the abstract’s 11% is the size of the preemption-induced credit expansion, not the denominator of the elasticity. This article’s text rounds this to 0.3 percentage points of annual house price growth per 1% increase in lending.

[Mian and Sufi \(2022\)](#) report a 12.1-percentage-point top-versus-bottom exposure gap in house prices over 2003–06 (their Table 5 and Figure 11).

C.4.2 Section 3.3.2: Subprime and Securitization, 2003–2006

[Mian and Sufi \(2009\)](#): 2002–05 is the only period of negative income/credit-growth correlation across ZIP codes (their abstract, Figure III, and Table IV). [Demyanyk and Van Hemert \(2011\)](#): Adjusted loan quality declined monotonically from 2001 to 2007 (their Figure 1, right panel). [Justiniano et al. \(2022\)](#): The conditional mortgage-rate/Treasury spread drops about 80 bps in summer 2003 (their Figure 4).

Spread compression as supply evidence. [Levitin and Wachter \(2012\)](#) examine PLS deal pricing from 2003 to 2007 and find that the spread on PLS over maturity-matched Treasuries fell even as mortgage risk and issuance volume rose, a movement they call “generally opposite of what one would have expected in a perfect market” (their Figures 10 and 11). [Demyanyk and Van Hemert \(2011\)](#) document the borrower-side counterpart: the subprime–prime FRM rate spread declined over 2001–2007 (their Section 6 and Figure 6). They draw the same inference, noting that an increase in demand for subprime MBS “is consistent with our finding of lower spreads and higher volume.”

[Piskorski et al. \(2015\)](#) report a 7–14% misrepresentation rate for second-lien/collateral (housing equity) characteristics.

C.4.3 Section 3.3.3: Why Did Credit Expand?

Branching deregulation. [Favara and Imbs \(2015\)](#) report that between 1994 and 2005 deregulation explains one-half to two-thirds of the observed increase in mortgage loans and one-third to one-half of the increase in house prices, and that the credit expansion it caused explains “virtually all of the increase in house prices until 2002, and between a third and one-half of the increase between 2002 and 2005” (their pp. 959, 961, and 979).

Automated underwriting, PTI, and MERS. [Johnson and Tzur-Ilan \(2026\)](#): The automated-underwriting share of GSE purchases grows from under 25% in 1997 to stabilize just above 60% in 2001 (their Table A.1). The back-of-the-envelope 16% cumulative

house price growth over 1993–2005 is stated on their p. 30.

Breadth of the PTI expansion. [Anenberg et al. \(2019\)](#) estimate a loan frontier—the maximum loan available conditional on credit score, income, and down payment—for over 10,000 borrower types across the 100 largest MSAs from 2001 to 2014. They report that “increases in credit availability during the boom were fairly similar across borrower types regardless of their income, credit score, or downpayment” (their abstract).

[Greenwald \(2026\)](#): At the boom’s peak (2006Q4), 55% of debt for home purchases went to loans above the 36% pre-boom PTI limit (his p. 11). His Figure 2 shows the post-bust (2014–2016) PTI distribution sharply truncated at the 45% limit.

[Lewellen and Williams \(2021\)](#): A 10.1% within-lender volume rise scaled by the roughly one-third MERS-member share gives 3.1–3.4% per year (their p. 1253).

C.5 Section 3.4: Overly Optimistic Expectations

[Foote et al. \(2012\)](#): The 2005 Lehman Brothers report’s scenario is from their Table 6.3.

[Case et al. \(2012\)](#) and [Shiller and Thompson \(2022\)](#): The data are from their Table 3. One-year expectations peak near 8% (7.9% in 2004 and 7.7% in 2005 for the four counties pooled, the “All” column) and annualized 10-year expectations exceed 10% (10.5% in 2004).

[Piazzesi and Schneider \(2009\)](#): The share buying “because of expected price growth” peaks at 20.2% in 2005Q2, a 25-year high (their p. 408). The late phase in their article corresponds to 2004–2005.

Momentum line in Figure 3, Panel B. The line sums two columns of Table 42 of [University of Michigan Surveys of Consumers \(2026\)](#), “prices won’t come down; are going higher” and “capital appreciation; good investment,” which together make up the expected-price-change issue in [Piazzesi and Schneider \(2009\)](#). Because of the double counting described in the figure note, it runs somewhat above their household-level series.

[Ben-David et al. \(2026\)](#) attribute 24% of the 1996Q4–2006Q1 boom to expectations and 26% to mortgage rates (their Table 2 and footnote 29). These two figures show up in

the Empirics column of Table 1 for overoptimism (about 25%) and the upper end of the interest-rate range (26%).

C.6 Section 3.5: Heterogeneous Optimism and Speculation

[DeFusco et al. \(2022\)](#): Volume leads prices by 24 months (their Figure 2) and speculative volume accounts for 40–50% of total sales-volume growth (their p. 2). [Haughwout et al. \(2011\)](#): The investor share of purchase-mortgage originations in the four bubble states (Arizona, California, Florida, and Nevada) rose to about 45%, or “almost half,” at the 2006 peak (their Figure 3c and abstract).

C.7 Section 4: Structural Models

Garriga, Manuelli, and Peralta-Alva. [Garriga et al. \(2019\)](#) report that the perfect-foresight version of their model “accounts for about one-half of the observed price increase in the United States in the 2000s” (their abstract). Holding LTV limits at their 1997 level, they attribute approximately 80% of the model’s boom to lower mortgage rates (their Section V.D and Figure 9).

Greenwald. [Greenwald’s \(2026\)](#) “33 to 53 percent of the boom in price-rent ratios” is that paper’s own conclusion. The two endpoints come from two counterfactuals: PTI liberalized in isolation gives 33% of the actual rise (his Table 4, row 2), while holding PTI limits at their historical level within the full boom reduces the rise by 53% (the difference between rows 4 and 5 of the same table).

Greenwald and Guren. [Greenwald and Guren \(2025\)](#) find that credit explains 32% of the boom when relaxed in isolation and 53% when relaxed alongside other shocks, and that credit and interest rates together explain 70% (their abstract; the 53% counterfactual is on pp. 3591–3592 and the 70% on p. 3589). Their Table 2 reports shares of the peak rise in price-rent ratios for credit relaxation alone (full segmentation 36%, benchmark 32%, estimated lower bound 22%) and for credit relaxation plus the decline in rates (77%, 70%, 47%), so the implied increments attributable to rates are 41, 38, and 25 percentage points. They report no rate-decline-only experiment. In their extension that lets savers trade

housing (their Section VI, summarized in the bottom panel of Table 2), the same credit-relaxation-plus-declining-rates experiment explains 50% of the observed rise in the price-rent ratio rather than 70%; the “30 percent” dampening in the text is $(70 - 50) / 70 \approx 29\%$.

Favilukis, Ludvigson, and Van Nieuwerburgh. Favilukis et al. (2017, p. 143) report that relaxing credit constraints “alone generates an increase in the P/MU ratio equal to more than 60 percent of the increase in observed price-rent ratios... from two different US data sources and 40 percent of that from a third data source.” On the foreign-capital inflow, their p. 186 states that “without an influx of foreign capital, relaxed financing constraints would lead to an endogenous increase in interest rates as precautionary saving declines,” and that the inflow “drives down interest rates but has very little effect on home prices.”

Kaplan, Mitman, and Violante. Kaplan et al. (2020) (KMV) find beliefs to be the “dominant force” with credit having “virtually no effect” on prices or rents (their p. 3287 and Figure 3). Greenwald and Guren (2025) attribute this to KMV’s frictionless rental market. KMV’s Figure 5 shows that foreclosures have essentially no price impact.

Landvoigt, Piazzesi, and Schneider. Landvoigt et al. (2015) do not report the share numerically; the “40 to 50 percent” is read off their Figure 10, Panel B, which plots annualized 2000–2005 capital gains by 2000 house value in the data and in the counterfactual that changes only credit conditions. Taking low-quality homes as those worth \$100k–\$150k in 2000 and high-quality homes as those worth \$400k–\$450k, the data show average gains of about 18.8% and 10.9% per year, against 10.0% and 6.0% in the credit-only counterfactual, so credit alone accounts for about 50% of the excess appreciation of low-quality homes (about 42% comparing \$150k with \$450k homes).

Jacobson. Jacobson (2025) reports that “model generated house prices can match 75 percent of the empirical level increase observed from 1997 to 2007” (her p. 2). Note that the window ends in 2007.

Chodorow-Reich, Guren, and McQuade. Chodorow-Reich et al. (2024) find that setting real rates to their 1980s level, which raises the discount rate by 4 percentage points, roughly halves the boom in levels (their Figure 9(d)).

C.8 Section 5: The Bust and Rebound

Model busts are too small. In each of the four models cited, the bust comes from reversing the boom shocks, and each paper's figures, which plot the model's paths against the data, show a bust that stops at roughly the pre-boom level while the data undershoot and then rebound. See Figure 3A of [Kaplan et al. \(2020\)](#), Figure 4a of [Jacobson \(2025\)](#), Figure 8b of [Greenwald \(2026\)](#), and Figure 8a of [Greenwald and Guren \(2025\)](#).

National income. According to FRED ([U.S. Bureau of Economic Analysis, 2026e](#)), national income's largest year-over-year decline was about 4%, in 2009Q1 (−4.1%, after −2.3% in 2008Q4), and it fell about 5% peak to trough from 2008Q3 to 2009Q1.

Bust-era expectations (Section 5.1). [Shiller and Thompson \(2022\)](#): One-year expectations turn negative in only three counties and only in 2007–08 (Orange in 2007; Alameda, Middlesex, and Orange in 2008); the most negative is Orange County in 2008 at −2.3% (their Table 3), against a realized −24.3% (their Table 4). The Michigan Survey bust readings—a one-year mean growth never below −2%, no more than 30% ever expecting price falls, and a five-year average never negative—are based on the underlying tables from the Michigan Survey.

Post-bust PTI limits (Section 5.1). The pre-boom 36% and post-bust (2014–2016) 45% PTI limits are in [Greenwald \(2026\)](#) (his p. 33 and Figure 2). His footnote 11 dates the post-bust bunching as fully in place from 2010Q2, the basis for the spring 2010 dating in the text. [DeFusco et al. \(2020\)](#) establish the 43% figure: the Dodd–Frank ability-to-repay/qualified-mortgage rule sets a back-end debt-to-income limit of 43%, effective 2014. This finding is consistent with [FHFA Office of Inspector General \(2019\)](#), which reports that the absolute maximum allowed PTI limits (rather than the effective limits from the data) under the GSE underwriting software fell to 50% between late 2009 and early 2010 and stayed at 50% until mid-2017.

Defaults and foreclosures (Section 5.2). [Ganong and Noel \(2023\)](#): Among underwater defaults, 6% are strategic, 24% double-trigger, and 70% cash-flow only (their abstract and pp. 1001–1003).

[Guren and McQuade \(2020\)](#): Foreclosure-driven amplification is 48% of the non-

distressed decline (“half”), with the demand-contraction and lender-loss channels at 25.4% and 22.6% (their Table 5). [Guren and McQuade](#) identify three effects rather than two: the 25.4% combines their foreclosure flag effect (22.2%), in which foreclosed owners are locked out of buying, with a smaller choosey buyer effect (3.2%), in which buyers hold out for a discounted foreclosure sale (their pp. 1333 and 1351–1353).

[Chatterjee and Eyigungor \(2015\)](#): Foreclosures are $2.3/19 \approx 12\%$ of the bust (their Figure 5), and the financial wedge is 68.8%, a marginal contribution (such contributions need not sum to 100%; their Table 9 and footnotes 2 and 46). The 68.8% is $(19.00 - 5.93)/19.00$ from their Table 9, where 19.00 is the baseline price decline and 5.93 the decline with no financial disruption. Note that the shock is sized to deliver the baseline: “[w]e chose the size of the initial wedge so that the model produces a decline in the price of owner-occupied housing of 19 percent.”

[Mian et al.’s \(2015\)](#) 8-percentage-point estimate comes from their state-level 2SLS specification (their equation (3), p. 2608). The magnitude is stated on their pp. 2589 and 2611: “moving from a state with the median foreclosure rate to a state with the 90th percentile foreclosure rate leads to 8% lower house price growth from 2007 to 2009” (coefficient -2.245 , their Table VI). Their state-border discontinuity design (their pp. 2602–2604) is a separate exercise establishing the legitimacy of the judicial-requirement instrument by showing a discrete jump in foreclosure propensity at state borders; the paper notes on p. 2614 that “we have focused on state-level evidence when estimating the second-stage effect of foreclosures on house prices.” The 33% aggregate share is from their pp. 2622–2623.

Liquidity (Section 5.2). [Garriga and Hedlund \(2020\)](#): Months of supply is 11.5 at month zero, and endogenous liquidity amplifies the price decline by 25.8%, about 26% (their Table 2, Figure 3, and Table 5).

Overbuilding (Section 5.2). [Haughwout et al. \(2013\)](#) estimate that “3 to 3.5 million excess housing units were produced during the boom” and that excess supply is positively related to supply elasticity. Against their estimate that about 1.4 million housing starts per year were needed to keep pace with household formation, this is roughly two to two and a half years of trend production.

The rebound (Section 5.3). [Chodorow-Reich et al. \(2024\)](#): Absent foreclosures, the top-quartile bust (the quartile with the largest long-run growth) is 15.2 against 60.4 log points in the baseline, so prices fall about a quarter as much, or “less than a third” (their footnote 35).

C.9 Section 6: A Summary of the Perfect Storm and Its Implications

The decomposition table, cell by cell (Table 1). The ranges map to the entries above as follows.

Fundamentals (Empirics and Overall 10–30%; Models 28%): The Section 3.1 synthesis. Following Section 6.2, these estimates exclude the overoptimism that fundamentals trigger: the 14% holds the price-rent ratio fixed, and [Chodorow-Reich et al.’s \(2024\)](#) 28% turns off diagnostic overshooting ($\theta = 0$). Removing fundamentals from their model would eliminate most of the boom, but that is not the relevant counterfactual. The Models entry is that 28%, which covers the highest-growth quartile of cities rather than the nation.

Interest rates (Empirics 6–26%): 6% is the low end of the “6–15% of the real boom” counterfactual and 26% is [Ben-David et al.’s \(2026\)](#) mortgage-rate share. Models 25–38% is the increment of [Greenwald and Guren’s \(2025\)](#) combined credit-and-rate counterfactual over credit alone, spanning their estimated lower bound (47% – 22%) to their benchmark (70% – 32%). Because falling rates slacken PTI constraints, that increment includes the rate–credit interaction rather than rates on their own—rates and credit are complements, so the interaction is included with both forces—which is why it exceeds the empirical estimates and the Overall synthesis of 10–25%.

Credit supply (Empirics 20–30%): The two first-episode back-of-the-envelope calculations, converted to shares of the 80% real boom in Appendix B above: [Johnson and Tzur-Ilan’s \(2026\)](#) automated-underwriting estimate gives about 20% and [Favara and Imbs’s \(2015\)](#) branching-deregulation estimate about 28% at their lower share. The cell is conservative—Favara and Imbs’s upper share implies about 42%—because their estimate is a border-county LATE extrapolated to the national index. Models 32–60%: Greenwald

and Guren (32–53%) and Favilukis et al. (40 to more than 60%). The estimates of [Kaplan et al. \(2020\)](#) are excluded because [Greenwald and Guren \(2025\)](#) find the assumption of perfect arbitrage between owning and renting by deep-pocketed landlords to be unrealistic; including them would widen the credit range to 0–60% and the belief range to 30% up to essentially all of the boom. Credit and the other forces are complements, so Greenwald and Guren’s range spans credit on its own (32%) to credit alongside the decline in rates and overoptimism (53%). The Overall figure is 20–50%, which discounts the models’ upper end because those models feed in the entire observed relaxation of LTV and PTI limits as exogenous. The securitization evidence is not included because it is so hard to aggregate.

Overoptimism (Empirics about 25%): [Ben-David et al.’s \(2026\)](#) 24%. Models 30–50%: The residual after credit and rates in Greenwald and Guren (about 30%) up through [Jacobson \(2025\)](#) and [Chodorow-Reich et al. \(2024\)](#). Because overoptimism responds to the other forces, it is credited with the response they trigger (Section 6.2). The low end treats beliefs as a separate residual shock; the endogenous-belief models of [Jacobson \(2025\)](#) and [Chodorow-Reich et al. \(2024\)](#), in which price growth triggers overoptimism, give the higher end. The 50% upper end is a judgment rather than a number either paper reports. Read literally, both attribute more than half of their boom to beliefs: in [Jacobson’s \(2025\)](#) model, which matches 75% of the national price increase, shutting down beliefs leaves essentially no boom (her Appendix Figure 18a), and in [Chodorow-Reich et al.’s \(2024\)](#) Figure 9(a) the $\theta = 0$ path accounts for 28% of the baseline increase, leaving about 70% to diagnostic overshooting. The upper end discounts these readings somewhat because both models treat expectations as a named residual and leave out some of the other forces. The Overall figure is 30–50%, which follows the models more closely than the empirics because survey evidence is difficult to translate into a share of the boom.

Speculation (Empirics and Overall 5–10%): The author’s judgment, because the national importance of speculators is not well documented. Neither [DeFusco et al. \(2022\)](#) nor [Haughwout et al. \(2011\)](#) reports a share of the national price boom. The judgment draws on DeFusco et al.’s finding that growth in speculative volume explains 40–50% of total volume growth and on Haughwout et al.’s documented rise in the investor share of

purchase-mortgage originations, from about 20% in 2000 to nearly 35% in 2006 nationally and from almost 25% to 45% in the states with the largest booms and busts. “No Estimate” appears in the Models column because no structural model quantifies speculation’s contribution comparably to the other forces: in DeFusco et al.’s model, removing short-horizon buyers makes the price overshoot almost disappear, but this is not expressed as a share of the boom.

The Overall column is the author’s synthesis rather than a mechanical span of the other two, and its midpoints sum to more than 100% because complementary forces are credited with their interaction in both ranges.