Systematic Monetary Policy and the Macroeconomic Effects of Shifts in Residential Loan-to-Value Ratios

Rüdiger Bachmann  Sebastian Rüth*

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Abstract

What are the macroeconomic consequences of changing aggregate lending standards in residential mortgage markets, as measured by loan-to-value (LTV) ratios? Using a structural VAR, we find that GDP and business investment increase following an expansionary LTV shock. Residential investment, by contrast, falls after a small initial uptick, a result that depends on the systematic reaction of monetary policy. We show that, historically, the Fed tended to respond directly to expansionary LTV shocks by raising the monetary policy instrument, and, as a result, mortgage rates increased and residential investment declined. The monetary policy reaction function in the United States appears to include lending standards in residential markets, a finding we confirm in Taylor rule estimations. Without the endogenous monetary policy reaction, residential investment increases. House prices behave in a similar way. This suggests that an exogenous loosening of LTV ratios is unlikely to explain booms in residential investment and house prices, at least in times of conventional monetary policy. By contrast, exogenous monetary policy shocks account for higher fractions of variation in residential investment and house prices.

Keywords: loan-to-value ratio, monetary policy, residential investment, structural VAR, Cholesky identification, Taylor rules.

JEL codes: E30, E32, E50, E52.

*Respectively: University of Notre Dame, Department of Economics, Notre Dame, IN 46556, USA, Phone: +1 (574) 631 0380, CEPR, CESifo and ifo, e-mail: rbachman@nd.edu (corresponding author); Heidelberg University, Alfred-Weber-Institute for Economics, Bergheimer Str. 58, 69115 Heidelberg (Germany), Phone: +49 6221 54 2943, e-mail: sebastian.rueth@awi.uni-heidelberg.de. We thank our editor, Jesús Fernández-Villaverde, two anonymous referees, Christiane Baumeister, Kai Carstensen, Gert Peersman, Eric Sims, and seminar and conference participants at Ghent University, Kiel University, the 2016 Annual Meeting of the European Economic Association in Geneva, the 2016 Annual Meeting of the German Economic Association (Verein für Socialpolitik) in Augsburg, University of Würzburg, University of Western Ontario, Ohio State University, Indiana University, the Federal Reserve Bank of Chicago, Carleton University, the 2017 Asian Meeting of the Econometric Society in Hong Kong, the 2017 China Meeting of the Econometric Society in Wuhan, the 2017 PhD Meetings of the Royal Economic Society in London, the KOF Swiss Economic Institute (ETH), the macroeconomics committee (Makroausschuss) meeting of the German Economic Association in Luxembourg, Yale University, the National University of Singapore, the 2018 T2M Meeting in Paris, the 2018 Meeting of the Royal Economic Society in Brighton, Chemnitz University, the Federal Reserve Bank of Philadelphia, the 2018 Meeting of the Society for Economic Dynamics in Mexico City, the Federal Reserve Bank of Kansas City, Heidelberg University, Pompeu Fabra/CREI, and the Banque de France for their helpful comments.
1 Introduction

What are the macroeconomic consequences of exogenous changes to aggregate lending standards / borrowing constraints in residential mortgage markets, as measured by loan-to-value (LTV) ratios? The most recent cycle in U.S. housing markets saw a relaxation and subsequent tightening of borrowing conditions, leading many observers to attribute the growth in residential investment, mortgage debt, and house prices prior to the Great Recession to the loosening of lending standards.\(^1\) In addition, recent macroprudential policy discussions include the use of regulatory limits on (mortgage) LTV ratios.\(^2\) However, relatively little is known empirically about the macroeconomic consequences of exogenous variations in LTV ratios (see Fischer, 2015). This paper attempts to make some headway.

We empirically quantify the effects of exogenous shifts in LTV ratios on aggregate economic activity, in particular various investment aggregates and house prices. Moreover, we uncover a systematic interaction between movements in LTV ratios and monetary policy, providing, thus, an explanation for the effects of LTV shocks on economic activity. To measure LTV ratios, we rely, as a baseline, on survey data from the Federal Housing Finance Agency (FHFA), which polls a sample of U.S. mortgage lenders to report terms and conditions on lending standards for conventional, newly originated mortgages within the Monthly Interest Rate Survey (MIRS).

Our baseline empirical strategy consists of estimating structural vector autoregressions (VARs) to identify exogenous shocks to LTV ratios with only a few theoretical restrictions. Specifically, we isolate exogenous shifts in LTV ratios from endogenous reactions to other macroeconomic fluctuations by imposing a recursive Cholesky identification scheme. Following, among others, Lown and Morgan (2006), Gilchrist and Zakrajsek (2012), and Walentin (2014), we recover the structural VAR representation by assuming that LTV shocks affect “slow-moving” macroeconomic aggregates with a time lag of one quarter, while “fast-moving” financial variables respond to shifts in lending standards on impact. Purging LTV ratios from financial sector conditions and forward-looking variables in a first-stage regression as in Bassett et al. (2014) shows, in a robustness check, that the Cholesky identification is sufficient to isolate exogenous movements in residential sector lending standards.

Our baseline result can be seen in a nutshell in Figure 1. In a parsimonious three-variable VAR with real residential investment, the LTV ratio, and the nominal Federal funds rate, a Cholesky-identified LTV shock generates, after a small initial uptick, a decline in residential investment, and a hump-shaped contractionary reaction of the monetary policy instrument. The dashed lines show a scenario that eliminates the monetary reaction to the LTV shock, using the procedure in Bernanke et al. (1997) (details below). Without the monetary policy reaction, increases in LTV ratios have the expected expansionary effect on residential investment. Also, omitting the Federal funds rate altogether from the VAR (lines with crosses), leads to the same expected expansionary effect. This suggests that studying the impact of

\(^1\) See Duca et al. (2011), Chu (2014), Landvoigt et al. (2015), and Favilukis et al. (2017). See Kiyotaki et al. (2011) for a more skeptical, and Landvoigt (2017) and Sommer et al. (2013) for a more mixed view.

\(^2\) See, e.g., IMF (2011), BIS (2011), and Claessens (2014) for this discussion.
Notes: The x-axis measures time in quarters. The lines with crosses represent point estimates of impulse responses for a bivariate VAR, using $x_t = [i_t \ ltv_t]'$, while the solid lines represent responses for a three-variable VAR, using $x_t = [i_t \ ltv_t \ r_t]'$. Shaded areas display one standard deviations confidence intervals obtained from 5,000 replications of the recursive-design wild bootstrap procedure of Goncalves and Kilian (2004). For the three-variable VAR, the dashed lines represent IRFs for a passive monetary policy authority that does not react to the shock as in Bernanke et al. (1997) and Sims and Zha (2006).

credit supply shocks, while disregarding the systematic monetary policy reactions they trigger, might lead to an omitted variable problem.

In the body of the paper, we obtain this baseline result also from a more richly specified VAR that includes, in addition to the three already mentioned variables, GDP, non-residential investment, inflation, real price inflation for both non-residential and residential investment, and mortgage rates, thus capturing the joint dynamics of the macroeconomy and housing market quantities, prices, and interest rates. After an expansionary 25 basis point LTV shock, the LTV ratio rises quite persistently, and we find positive spillover effects on real non-residential aggregate quantities, with business investment rising by more than 0.3 percent after a year, and GDP increasing by almost 0.1 percent.

The picture is different, however, for residential investment: after a small initial increase, residential investment turns negative to minus 0.8 percent in the second year after the shock. We identify the Fed’s monetary policy instrument as a candidate to explain the decline in residential investment after the LTV shock. The Federal funds rate responds to looser lending standards in the residential mortgage market with a hump-shaped and rather persistent tightening of 16 basis points at the maximum, counteracting the eased quantity restriction on mortgage loans. In addition, the endogenous policy contraction passes through to mortgage rates—raising the price of mortgage loans—and, furthermore, households anticipate this systematic increase in interest rates as data from the Michigan Survey of Consumers show.
We also show that relative price inflation for residential investment, if anything, decreases after a loosening of LTV ratios, just as residential investment does. Our results thus suggest that an exogenous loosening of LTV ratios is unlikely to explain a boom in residential investment or a prolonged house price boom, at least under conventional monetary policy.

We corroborate this view also through a variance decomposition: not more than 10 percent (at the four-year horizon) of the forecast error variance of residential investment are explained by LTV shocks, and at most 6 percent for house prices. By contrast, monetary policy shocks are the much more potent driver of residential investment (house price) fluctuations on average, explaining over 26 (approximately 15) percent at the two-year horizon.\(^3\) Historical decompositions, however, reveal a somewhat more nuanced picture: we show that the decline of LTV ratios in the aftermath of the 2001 recession was subsequently countered by relatively loose, endogenously reacting monetary policy conditions that fueled the boom in residential investment prior to the Great Recession. Compared to other time periods, monetary policy shocks were of relatively lower relevance for residential investment dynamics during this episode.

We analyze the systematic monetary policy response to an exogenous LTV shock along two additional dimensions. First, to answer the question what the Fed actually responds to after an LTV easing, we perform an impulse response decomposition as proposed by Kilian and Lewis (2011). Based on this decomposition, the policy response is best characterized as a direct response to the altered lending conditions, rather than an indirect response operating through the shock propagation via other variables in the system. For short horizons at least, it is the LTV ratio that accounts for the systematic interest rate reaction almost entirely. We conjecture that aggregate lending standards, as represented by LTV ratios, are thus part of the Fed’s reaction function. This conjecture is indeed corroborated when we estimate Taylor rules in the spirit of Coibion and Gorodnichenko (2012) and show that, even controlling for the usual inflation, output gap and output growth terms, changes in residential LTV ratios, unlike most other financial variables, enter robustly, positively, and significantly. This suggests that Taylor rules in models of the housing market featuring monetary policy should potentially contain residential credit market conditions.

Second, to isolate the impact of systematic monetary policy in the transmission of an LTV shock to the broader economy, we rely on the statistical decomposition proposed in Bernanke et al. (1997) and Sims and Zha (2006), and recently applied in, e.g., Bachmann and Sims (2012) and Bassetto et al. (2016). This methodology consists of comparing the actual impulse response to an LTV shock with one for which the Fed’s interest rate reaction to an LTV shock has been “zeroed out.” The differences between both impulse response functions, then, identify the quantitative importance of the systematic monetary policy reaction for the transmission of the LTV shock. We find that the positive non-residential investment response is magnified in the case sans monetary policy reaction. More importantly, residential investment now exhibits a quite persistent increase, peaking at around 0.3 percent,\(^3\) See also Bhutta and Keys (2016), showing the power of interest rates to influence home equity extractions, within a micro data approach.
and it deviates, from quarters three to twelve, statistically significantly from the impulse response with counteracting monetary policy. The systematic monetary policy response, hence, determines residential investment activity not only quantitatively, but also qualitatively. Put differently, while the reaction of non-residential investment is almost entirely driven by aggregate lending conditions in terms of quantities, the dynamics of residential investment following from a LTV shock are eventually dominated by an endogenous interest rate reaction. In addition, we show that this differential reaction is not due to different pass-through effects from the monetary policy interest rate to the relevant long-term interest rates for households and businesses, but rather the result of residential investment being substantially more interest rate sensitive than non-residential investment.

Our results without the monetary policy reaction further shed some light on the Great Recession. While our VAR is a linear model, our findings are in line with the perception that a tightening of LTV ratios may have exacerbated the downturn in housing markets during the Great Recession (see, for instance, Guerrieri and Iacoviello, 2017). The reason is the asymmetry represented by the zero lower bound on nominal interest rates. Historically, the Fed would have likely lowered interest rates in the face of the LTV tightening, however, with interest rates at zero, this cushioning mechanism was absent. According to our fixed interest rate evidence, such a situation should then be associated with a drop in residential investment, which was indeed observed during the financial crisis.

Related Literature

Our paper is related to a recent and growing literature studying the effects of shocks to bank lending standards and financial market conditions on the macroeconomy. Perhaps most closely related is Walentin (2014), who also uses Cholesky-identified VARs to study the effects of increases in mortgage rate spreads. Unlike Walentin (2014), we focus on lending standards in terms of mortgage LTV ratios rather than mortgage spreads. Walentin (2014) also finds a relaxation of monetary policy after an increase in the mortgage spread, similar to our counteracting monetary policy result. Our paper further differs in that we study the importance of this systematic monetary policy reaction for the transmission of aggregate lending shocks formally and that we connect our residential investment result directly to this systematic monetary policy reaction.

Another closely related paper is Duca et al. (2011), who use first-time home buyer LTV ratios to argue that they improve the fit in house-price-to-rent ratio regressions and that looser LTV ratios were a driver of the house price bubble prior to the Great Recession. Again, our paper differs with its focus on systematic monetary policy as a crucial propagation mechanism for the dynamic effects of LTV ratio shocks.

More broadly, Lown and Morgan (2006), Gilchrist and Zakrajsek (2012), Bassett et al. (2014), Peersman and Wagner (2015), Bassetto et al. (2016), Gambetti and Musso (2017), and López-Salido et al. (2017) study the effects of credit supply shocks (without a particular focus on residential markets) on economic activity. Many have the result that monetary policy counteracts whatever credit market shock is identified, but—with one exception in Bassetto et al. (2016)—the role of the
systematic monetary policy reaction for the credit shock is not quantified, mainly because none of these papers, including Bassetto et al. (2016), feature a prima facie “counterintuitive” finding comparable to our residential investment result; instead, they usually find that their particular form of credit market tightening reduces economic activity and vice versa for expansionary shocks.

Lown and Morgan (2006) and Bassett et al. (2014) use Cholesky-identified VARs with the tightening variable from the Senior Loan Officer Opinion Survey (SLOOS) to identify aggregate credit supply shocks. Importantly, Bassett et al. (2014) introduce the purging exercise into the literature that we use in one of our robustness checks. Gilchrist and Zakrajsek (2012) also use Cholesky-identified VARs with carefully computed credit spreads that our paper also relies on for robustness checks. Peersman and Wagner (2015) and Gambetti and Musso (2017) use VARs that are sign-identified (in addition to zero restrictions in Peersman and Wagner, 2015) with lending aggregates and lending rates to identify credit supply shocks. López-Salido et al. (2017) take a longer historical perspective and use credit spreads to measure credit sentiment all the way going back to the Great Depression, not discussing, however, the endogenous reaction of monetary policy to credit supply shocks. Finally, Bassetto et al. (2016) study, again in Cholesky-identified VARs, the endogenous monetary policy reaction to broader financial condition shocks and also show that monetary policy is important for our understanding of these shocks.

In addition to the time series literature, there is a growing cross-sectional literature that investigates the effects of credit supply shocks on economic activity. The results are somewhat mixed: while Driscoll (2004) does not find large effects, Favara and Imbs (2015) and Maggio and Kermani (2017) come out in favor of a large credit supply channel. Of course, cross-sectional, instrumental variable approaches come with a strong credibility advantage over time series methods. Yet, almost by construction, they cannot speak to the effects of systematic monetary policy reactions, which are always aggregate, and which, as we show, are crucial for our understanding of at least aggregate shocks to lending standards. In the end, we view, of course, both time series and cross-sectional approaches as complementary in studying the important question on how credit supply influences economic activity. Importantly, our results do not contradict these results based on microdata, nor the local results in, for instance, Landvoigt et al. (2015). Our sans monetary policy results are consistent with their findings. Our argument is simply that the transmission from credit supply to aggregate economic activity may depend crucially on the monetary policy reaction to credit supply shocks.

The remainder of the paper is structured as follows. Section 2 describes the data, explains the empirical strategy, and presents the core empirical findings. Section 3 takes a closer look at the LTV ratio series used in the baseline specification and shows that our main results are robust for a variety of alternative LTV measures. Section 4 shows robustness of our results to a number of alternative VAR implementations. Section 5 concludes the paper.

4Fieldhouse et al. (2018), by contrast, report an accommodating monetary policy response after exogenous regulatory changes that increase mortgage purchases by federal housing agencies. Notice, however, that the identifying variation in this paper, regulatory changes, is not necessarily the same as in our paper and the cited credit supply shock literature.
2 LTV shocks and monetary policy

This section presents the empirical framework and our main findings. Section 2.1 describes the data. Section 2.2 discusses the VAR identification strategy and presents the main macroeconomic effects of LTV ratio shocks. Section 2.3 looks in detail under the hood of the systematic monetary policy response to LTV shocks that we uncover in Section 2.2. Variance and historical decompositions follow.

2.1 Data

We study the effects of exogenous shocks to residential mortgage LTV ratios on aggregate economic, in particular, investment activity, housing markets, and monetary policy. Our baseline model thus features nine variables at the quarterly frequency: GDP ($y_t$), non-residential investment ($i_{nr}^t$), residential investment ($i_r^t$), CPI-based inflation rate ($\pi_t$), non-residential investment relative price inflation ($\pi_{nr}^t$), residential investment relative price inflation ($\pi_r^t$), the LTV ratio ($ltv_t$), the nominal Federal funds rate ($r_t$), and the mortgage rate ($r_m^t$). Including these variables allows us to study jointly the effects of LTV and monetary policy shocks on general aggregate economic activity and housing market quantities, prices, and interest rates, while at the same time—through the VAR identification—alleviating concerns about LTV shocks being driven by aggregate demand or mechanical house price effects on LTV ratios (more on this in Section 3.1). Quantity series enter the VAR in seasonally adjusted real log levels, while inflation rates, LTV ratios, and interest rates enter in percent. In Appendix A, we provide details on the data sources, definitions, and transformations for all data used in the paper.

Our benchmark LTV measure is the quarterly average of the seasonally adjusted monthly national average LTV ratios on conventional mortgage loans from the Monthly Interest Rate Survey (MIRS) conducted by the Federal Housing Finance Agency (FHFA). The mortgage rate data stems from the same survey.

The survey provides extensive data on terms and conditions of U.S. mortgages. For instance, toward the end of our sample, the survey covers roughly 82,000 loan contracts. Our baseline VAR considers the period 1973Q1 to 2008Q4, where the availability of LTV data dictates the start of the sample. We choose 2008Q4 as the end of the sample, when the Fed’s policy instrument reached the zero lower bound. Since then, the Fed engaged in several unconventional policies and historical monetary policy reaction functions are likely to no longer hold (see, e.g., Kilian and Lewis, 2011; Peersman and Wagner, 2015).

The FHFA survey polls a sample of mortgage lenders (savings associations, commercial banks, and mortgage companies) to report interest rates and conditions on all fully amortized single family loans closed within the last five business days of each month. As part of the survey, mortgage lenders are asked to report the LTV

5The survey does not comprise the following loan types: mortgages insured by the Federal Housing Administration or guaranteed by the Department of Veterans Affairs, multifamily mortgages, mortgages for mobile homes or farms, and mortgages created by refinancing existing mortgages. As discussed in Duca et al. (2011, 2013), the FHFA survey further does not include so-called Alt-A (a mortgage class between prime and subprime) nor subprime mortgages.
Notes: The figure displays the seasonally adjusted average loan-to-value ratio on conventional single family mortgage loans, which we obtain from the Federal Housing Finance Agency. Data are at the quarterly frequency, and we express them in percent, i.e., as a ratio of the granted mortgage loan and the underlying house price multiplied by 100. The shaded areas represent NBER-dated recession episodes in the US.

ratios agreed upon at purchase of the properties. Importantly, focusing on newly originated mortgages is another guard against house price valuation effects driving LTV ratios mechanically. Also, these LTV ratios include all types of home owners, i.e., owner occupiers as well as first-time home buyers. According to Mian and Sufi (2011), existing home owners contributed substantially to the buildup in household leverage during the 2002 to 2006 house price acceleration, as about 65 percent of households already owned a property prior to the cycle. In addition, the survey includes both fixed-rate and adjustable-rate mortgage loans.

Figure 2 plots the FHFA LTV series, i.e., the national average ratio of granted mortgage loans for single family houses and the underlying property prices multiplied by 100. The shaded areas represent NBER-dated recession episodes in the United States. The LTV ratio is procyclical and exhibits pronounced swings over time. Borrowing limits eased during the housing boom of the years 2002 to 2006, even though the LTV ratio did not reach its 1994 level. At the onset of the Great Recession the LTV ratio tightened sharply.
2.2 Identification of exogenous shifts in the LTV ratio

2.2.1 Structural VAR

To analyze the macroeconomic consequences of exogenous shifts in the LTV ratio, we rely on the vector autoregression framework. A structural representation of the variables of interest can be formulated as:

\[
A_0 x_t = \sum_{l=1}^{p} A_l x_{t-l} + \varepsilon_t,
\]

where we drop the intercept without loss of generality for notational convenience. \(A_l\) is a \(n \times n\) matrix including autoregressive coefficients at lag, \(l = 1, ..., p\), and \(A_0\) captures contemporaneous impact coefficients. \(p\) is the lag length, and \(\varepsilon_t\) represents mutually uncorrelated structural shocks. The \(n \times 1\) vector \(x_t\) comprises the following \(n = 9\) variables in this order, \(x_t = [y_t \ i^{nr}_t \ i^r_t \ \pi_t \ \pi_t^{nr} \ pi^r_t \ ltv_t \ rt \ r^m_t]'^t\). As in Gilchrist and Zakrajsek (2012) and Bassett et al. (2014), we estimate the reduced-form VAR representation of Equation (1) with two lags—a lag length also suggested by the final prediction error information criterion.

Uniquely recovering the structural VAR representation requires us to impose restrictions on elements in \(A_0\) to disentangle exogenous LTV movements from endogenous reactions to other variables in \(x_t\). Structural LTV shocks could arise from internal reassessments of the quality of borrowers, new business models in the residential lending market, or shifts in the supervisory environment under which mortgage lenders operate (see Bassett et al., 2014). We follow Gilchrist and Zakrajsek (2012) and Walentin (2014) by assuming that shocks in “slow-moving” macroeconomic variables impact financial variables contemporaneously, whereas shocks in “fast-moving” financial variables affect the real economy with a time lag (see also Christiano et al., 1996; Peersman and Wagner, 2015). We implement the identification strategy by applying a Cholesky factorization to the variance-covariance matrix of the reduced-form regression residuals, \(u_t\). Then we use the Cholesky factor for \(A_0\), which delivers the linear mapping \(u_t = A_0^{-1} \varepsilon_t\) and recovers the structural representation. Within the recursive identification scheme, we allow the monetary policy instrument to react on impact to LTV shocks, \(\varepsilon_{L,t}\), where the subscript \(L\) is the position of \(ltv_t\) in \(x_t\). As we show in Section 2.3.3 through the estimation of Taylor rules, the Fed historically indeed appears to have reacted to housing market conditions in general, and LTV ratios in particular. Moreover, as is well-known and as we also document in Section 2.3.3, housing market variables in general, and LTV ratios in particular lead the business cycle. It is therefore plausible that the Fed not only monitors but also reacts to housing market shocks on impact.

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\[^6\]We note here that our ordering also means that monetary policy surprises impact other variables (except for the mortgage rate) with a time lag of one quarter, and monetary policy reacts to realizations of macroeconomic aggregates contemporaneously, i.e., contemporaneous as well as previous realizations of macroeconomic variables in \(x_t\) are in the Fed’s time \(t\) information set. We thus follow an established literature of recursively identified monetary policy VARs, e.g., Bernanke et al. (1997), Christiano et al. (2005), and Erceg and Levin (2006). We have experimented with all possible orderings inside the block of financial variables, \([ltv_t \ rt \ r^m_t]'^t\), and found that none of our results depends on the within-block ordering.
2.2.2 Quantifying the effect of the systematic monetary policy response

If the Fed indeed reacts to housing market innovations, a natural question to ask is: how big a deal might such a response be? To flesh out the quantitative importance of the Federal funds rate reaction in the transmission of the LTV shock, we follow the methodology in Bernanke et al. (1997) and Sims and Zha (2006), and recently applied in, e.g., Bachmann and Sims (2012) and Bassetto et al. (2016), to statistically decompose the effects of a given shock into those stemming from the endogenous reaction of another variable in the VAR, say a policy variable, and those holding the latter variable constant. To do so, we generate hypothetical sequences of monetary policy shocks that “zero out” the Federal funds rate response after the LTV shock.

We can recursively calculate the monetary policy shocks required to force the policy response to zero over the whole forecast horizon as follows:

\[ \varepsilon_{F,h} = - \sum_{j=1}^{n} B_{F,j} y_{j,F} - \sum_{m=1}^{\text{min}(p,h)} \sum_{j=1}^{n} B_{F,mn} z_{j,h-m}. \]  

(2)

\[ y_{j,0} \] is the time \( t = 0 \) impact of the LTV disturbance on variable \( j \) in the benchmark VAR, while the same impact without the endogenous monetary policy reaction is:

\[ z_{j,0} = y_{j,0} + \frac{\Phi_{j,F,0} \varepsilon_{F,0}}{\sigma_F}. \]  

(3)

The standard deviation of the monetary policy disturbance is \( \sigma_F \). For horizons beyond the impact period, \( h > 0 \), we calculate:

\[ y_{j,h} = \sum_{m=1}^{\text{min}(p,h)} \sum_{i=1}^{n} B_{j,mn+i} z_{j,h-m} + \sum_{i<j}^{n} B_{j,i} y_{i,h} \quad \text{and} \quad z_{j,h} = y_{j,h} + \frac{\Phi_{j,F,0} \varepsilon_{F,h}}{\sigma_F}. \]  

(4)

In an application to oil price shocks, Kilian and Lewis (2011) propose an alternative procedure to quantify the effects of an endogenous monetary policy response. Whereas in Bernanke et al. (1997) and Sims and Zha (2006) counteracting monetary policy surprises completely offset the endogenous interest rate response, Kilian and Lewis (2011) propose a procedure where only the direct impact of the LTV shock is zeroed out with counteracting monetary policy shocks, while allowing the Fed to respond to the indirect effects of the LTV shock operating through its propagation to other variables in the VAR. Using the definitions of \( z_{j,h} \) and \( y_{j,h} \) from Equation (4), we can recursively calculate the sequence of monetary policy shocks required to remove the direct influence of the LTV shock from the Fed’s reaction function:

\[ \varepsilon_{F,h} = - B_{F,L} y_{L,h} - \sum_{m=1}^{\text{min}(p,h)} B_{F,mn+L} z_{L,h-m}. \]  

(5)

where subscript \( L \) represents the position of LTV in the VAR. For the baseline result, we will use both decomposition procedures. We note that the monetary policy shocks calculated for both decompositions are small and ordinary, below 10 basis points, which mitigates a potential Lucas critique to this exercise.
2.2.3 LTV shocks: baseline empirical evidence

Figure 3 traces out the impulse responses of the variables in $x_t$ following from an exogenous 25 basis point increase in the LTV ratio. The solid lines display the point estimates of impulse response functions and the dark (light) shaded areas are one (two) standard error confidence intervals, which we obtain from 5,000 replications of the recursive-design wild bootstrap procedure of Goncalves and Kilian (2004). The LTV ratio exhibits a sluggish adjustment pattern and is significantly positive for two and a half years before leveling off, i.e., the exogenous shock has a fairly persistent effect on the LTV ratio. The shock significantly affects non-residential investment, which features a hump-shaped increase with a peak around 0.4 percent after one year, and then reverts back to the pre-shock level. GDP features a qualitatively similar impulse response, but less pronounced in terms of amplitude and statistical significance. We thus find significant positive spillovers from lending standards in the residential sector to aggregate economic activity. These spillovers are consistent with the notion that borrowing against real estate, including for business purposes, constitutes an important part of private borrowing and thus of the transmission mechanism from finance to economic activity (see, e.g., Fan et al. (2017) for a particular case study).

By contrast, the impulse response of residential investment displays a small uptick at the very short horizon, but then falls significantly to negative 0.8 percent reaching its trough after two years before reverting back to its pre-shock level. This result is perhaps surprising as it is inconsistent with the view that loose LTV ratios lead to prolonged construction booms, and, perhaps, housing bubbles.

Indeed, our finding for residential investment is mirrored in real residential investment price inflation: its reaction is negative and remains persistently so, paralleling the decline in the quantity of residential investment. By contrast, non-residential inflation hardly reacts. The drop in real residential investment inflation adds an additional drag on residential investment through an increase in the user costs of housing due to an expected prolonged decline in its relative price, even though the effect is quantitatively small (not more than 10 basis points of annualized relative price deflation for residential investment). Our results, thus, support some skepticism toward the view that the lowering of lending standards generates (new) house price booms.

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7We present a 25 basis point LTV shock instead of a one standard deviation shock for better comparability across specifications. This shock size is frequently used in monetary policy VARs. A one standard deviation shock to the LTV ratio would amount to 68 basis points, while a one standard deviation monetary policy shock amounts to 75 basis points. Thus the impulse of our LTV shock is of similar strength as a conventional monetary policy shock.

8We use the most comprehensive residential investment aggregate as our baseline. Figures B.1 to B.3 in Appendix B show that our results are robust to using residential investment in structures, in single family structures, or only new structures instead.

9Given the initial uptick in residential investment, a first-blush explanation for this impulse response could be pure intertemporal substitution on LTV ratios: build a house now, before LTV ratios tighten again. This could lead, theoretically, to a boom-bust cycle in residential investment. However, notice that the magnitudes of the boom and the bust phase are very different, which makes it unlikely that the bust phase is just residential investment shuffled forward in time.

10The results are essentially the same had we used the real house price inflation rate of the
Figure 3: Loan-to-value ratio shock in the benchmark VAR

Notes: The x-axis represents time in quarters. The solid lines represent point estimates of impulse response functions for the VAR, using \( x_t = [y_t \ \pi_t^{nr} \ \pi_t^{nr} \ \pi_t^{r} \ \pi_t^{r} \ \pi_t^{ltv} \ \pi_t^{r} \ \pi_t^{m}]' \). Dark (light) shaded areas display one (two) standard deviations confidence intervals, which we obtain from 5,000 replications of the recursive-design wild bootstrap procedure of Goncalves and Kilian (2004). The dashed lines represent impulse responses for the case of a passive monetary policy authority that does not react to the shock at all, as in Bernanke et al. (1997) and Sims and Zha (2006), and dotted lines display adjustment patterns for the Kilian and Lewis (2011) decomposition.
But why does a shock that *eases* borrowing constraints in the residential mortgage market lead to a *decline* in residential investment? The impulse response in the middle of the last row of Figure 3 provides a candidate explanation. Monetary policy reacts to the eased lending standards by significantly and persistently raising the Federal funds rate by 16 basis points at the maximum. The persistent contractionary shift in monetary policy appears to counteract the initial easing in mortgage markets and appears to be dominating the expansionary effects of the LTV increase, at least, for residential investment. The endogenous monetary policy tightening furthermore transmits significantly to mortgage rates (see the last panel of Figure 3). Thus an increase in mortgage borrowing costs counteracts the loosening of the LTV ratio on mortgage loans.

The dashed lines in Figure 3 represent the impulse response functions when monetary policy does not respond to the dynamics triggered by the LTV shock at any horizon as in Bernanke et al. (1997). By construction, the impulse response of \( r_t \) is zero over the entire time horizon in the decomposition experiment and the mortgage rate barely reacts in this scenario. As a consequence of passive monetary policy, GDP, \( y_t \), and non-residential investment, \( i_{nr}^r \), both increase more strongly and more persistently, while the responses of consumer price inflation, \( \pi_t \), and non-residential relative price inflation, \( \pi_{nr}^r \), are muted and remain rather flat.

The dynamics of residential investment, \( i_r^r \), however, change substantially when monetary policy does not react: \( i_r^r \) still displays the small initial surge, but then continues to increase in a hump-shaped manner to more than 0.3 percent at the maximum. The response remains strictly positive over the whole forecast horizon, whereas, in the presence of a systematic monetary policy reaction, \( i_r^r \) turns significantly negative in the first year. The effect of the LTV shock on residential investment thus crucially depends on the endogenous reaction of monetary policy, both in a quantitative and qualitative sense. Put differently, while the reaction of non-residential investment is almost entirely driven by the lending conditions in terms of quantities, the dynamic response of residential investment following an LTV shock is eventually overwhelmed and ultimately determined by an endogenous price, or interest rate reaction. Similarly, real residential investment price inflation displays a qualitatively different behavior in the sans monetary policy case: it persistently increases. All these results are very similar for the Kilian and Lewis (2011) decomposition (dotted line). Taken together our results mean that an exogenous loosening of LTV ratios is unlikely to explain a boom in residential investment and house prices, at least under conventional monetary policy.\textsuperscript{11}

\textsuperscript{11}Figure B.9 in Appendix B shows that our results also hold qualitatively for firm-level and household-level mortgage debt aggregates: the stock of business mortgages increases after an expansionary LTV shock, sans or cum endogenous monetary policy reaction. The stock of household mortgages decreases significantly after an expansionary LTV ratio shock with the counteracting monetary policy reaction suggested by the VAR. Without it, the stock of household mortgages remains flat. The results would be the same, had we used overall (and not just mortgage) debt aggregates.

\textsuperscript{11}MIRS or the Case-Shiller real house price inflation. In addition, including all the price series in log levels (rather than growth rates) does not change results, and neither does the use of GDP deflator inflation or the use of PCE inflation instead of CPI inflation (see Appendix B, Figures B.4 to B.8).
2.3 Looking under the hood of the monetary policy reaction

This section studies the systematic monetary policy reaction to LTV ratio shocks documented in the previous section in more detail. First, we give a brief argument as to why the Fed might want to counteract fluctuations in financial conditions in the housing market, in the way our VAR suggests it has historically done. Second, we isolate the drivers of the Federal funds rate response to LTV ratio shocks. Third, we provide independent evidence through the estimation of Taylor rules that the Fed indeed reacted to credit conditions in the housing market. Finally, we shed more light on the pass-through from the Federal funds rate response to other interest rates and interest rate expectations and analyze the different responses of non-residential investment and residential investment to monetary policy, showing that the latter is substantially more interest-sensitive than the former. This concludes the argument and rationalizes the differential results for non-residential and residential investment in Figure 3.

2.3.1 Why might the Fed counteract LTV ratio fluctuations?

We begin by noting that, according to Figure 3 above, the Fed, by raising interest rates to an expansionary LTV ratio shock, has the effect of stabilizing GDP (even though, as we will show in the next subsection, it is not reacting to LTV-shock-induced GDP changes per se). This stabilization effect can also be found for non-durable consumption, a perhaps more directly welfare-related macroeconomic aggregate: in Figure 4, we trace the impulse response function of non-durable consumption to an LTV ratio shock, where we have replaced real GDP with real non-durable consumption in our baseline nine-variable VAR (we only show the reaction of consumption because the impulse response functions of the other variables remain essentially unchanged). The same picture as for real GDP emerges: the systematic monetary policy reaction helps to keep consumption smooth. While we do not intend to make welfare judgments based on VARs, at the very least, this result shows that, if the LTV ratio shocks are inefficient or take place in an inefficient economy, the Fed might actually provide welfare-enhancing stabilization with the systematic reaction it has historically displayed according to our VAR.

2.3.2 What drives the monetary policy reaction?

Which variables in the VAR actually trigger the policy reaction to the change in the LTV ratio, i.e., what is the central bank responding to after an LTV shock? We answer this question by decomposing the impulse response of the Fed’s policy instrument into contributions from the variables in $x_t$, as in Kilian and Lewis (2011). The rationale behind this exercise is as follows: LTV disturbances cause the Federal funds rate to deviate from its steady state. This response can be considered as the sum of a policy reaction, first, to lags of the policy instrument itself, and, second, to deviations of other variables in $x_t$ from their steady state values. The relative contributions of variables in $x_t$ to the Federal funds rate response, then, identify the forces underlying the monetary policy contraction.

\[12\] The resulting additional inflation volatility is minuscule.
Figure 4: Loan-to-value ratio shock: effect on non-durable consumption

Notes: The x-axis represents time in quarters. The solid lines represent point estimates of impulse response functions for the VAR, using $x_t = [c_t \ i_{tr}^{nr} \ i_{rr}^{nr} \ \pi_{tr}^{nr} \ \pi_{rr}^{nr} \ \pi_{tr}^{lt} \ \pi_{rr}^{lt} \ \pi_{tr}^{rt} \ \pi_{rr}^{rt} \ \pi_{tr}^{mt} \ \pi_{rr}^{mt}]'$. Dark (light) shaded areas display one (two) standard deviations confidence intervals, which we obtain from 5,000 replications of the recursive-design wild bootstrap procedure of Goncalves and Kilian (2004). The dashed lines represent impulse responses for the case of a passive monetary policy authority that does not react to the shock at all, as in Bernanke et al. (1997) and Sims and Zha (2006), and dotted lines display adjustment patterns for the Kilian and Lewis (2011) decomposition.

It is convenient to express the structural VAR as follows:

$$x_t = Cx_t + \sum_{i=1}^{p} A_ix_{t-l} + \varepsilon_t,$$

where the $n \times n$ matrix $C$ is strictly lower triangular. Furthermore, we can compactly summarize the structural parameters as $B = [C \ A_1 \ldots A_p]$.

To isolate the contribution of variable $j$ to the Federal funds rate response at horizon $h$ after a time $t = 0$ shock to the LTV ratio ($\Xi_{F,j,h}$), we define:

$$\Xi_{F,j,h} = \sum_{m=0}^{\min(p,h)} B_{F,mn+j} \Phi_{j,L,h-m},$$

with subscripts $F$ and $L$ denoting the position of the Federal funds rate and LTV ratio in the system, and $h = 0, 1, 2, ..., 16$ as well as $j = 1, 2, ..., n$. $\Phi_{j,L,h-m}$ is the \{j, L\} entry of the parameter matrix of impulse responses, $\Phi_{h-m}$.

Figure 5 shows that, in the first quarter after the shock, the LTV ratio accounts for the bulk of the Federal funds rate response (line with crosses). This impact persists for a few quarters. In addition, the lags of the Federal funds rate itself take over and explain the Federal funds rate response in later quarters (dashed line). This is consistent with the well-documented policy interest rate smoothing behavior of the Federal Reserve.
Figure 5: Decomposition of the Federal funds rate response following an LTV shock

Notes: The x-axis represents time in quarters. The solid lines are the point estimate of the Federal funds rate impulse response function after an LTV shock in the benchmark VAR. The dashed, dotted, and lines with nodes or circles represent the contribution of the respective variable to the reaction of the Fed’s policy instrument.

The direct contributions of the other variables, in particular the inflation rate, are negligible. As Figure 3 shows, the inflation rate reaction to an LTV shock is initially flat and moving into negative territory in the third year after the shock. This suggests that the monetary policy contraction cannot be explained by a direct “leaning against the wind” towards expected inflationary pressure. Rather lending standards in the housing market, as reflected by residential mortgage LTV ratios, appear to be part of the Fed’s reaction function. The next subsection presents complementary evidence for this claim.

2.3.3 Does the Fed really respond to LTV ratios? Estimating a Taylor rule

In this section, we now directly estimate the Fed’s reaction function in the spirit of Taylor (1993) and explicitly allow the monetary authority to respond to lending conditions as proxied by residential mortgage LTV ratios. We do so following the recent work by Coibion and Gorodnichenko (2012). In doing so, we will draw the same conclusion as from the VAR evidence—monetary policy reacts to residential credit market conditions—but with an alternative empirical framework. While early estimations of Taylor rules included contemporaneous measures of economic activity and inflation, recent research favors the use of fundamentals reflecting the real-time expectations of the monetary authority (e.g., Orphanides, 2003; Coibion and Gorodnichenko, 2011).

13This is an important lesson for models that study the interaction between credit conditions and monetary policy. For instance, Gambetti and Musso (2017) use a theoretical inflation-pressure argument to justify the assumption of a contractionary reaction of the Federal funds rate to positive loan supply shocks in the sign restrictions that identify their VAR.
We therefore characterize the Fed’s desired policy rate, $r_{tar}$, as:

\[ r_{tar} = c^* + \psi^*_\pi E_t\{\pi_{t+h_\pi}\} + \psi^*_\tilde{y} E_t\{\tilde{y}_{t+h_{\tilde{y}}}\} + \psi^*_{\Delta y} E_t\{\Delta y_{t+h_{\Delta y}}\} + \psi^*_{\Delta ltv} \Delta ltv_{t-1} \]  

(8)

whereas the realized policy rate, $r_t$, can be written as:

\[ r_t = r_{tar} + \eta^T_R. \]  

(9)

The central bank sets interest rates according to the expected realizations of the inflation rate, $\pi_t$, the output gap, $\tilde{y}_t$, and the growth rate of GDP, $\Delta y_t$. The inclusion of the latter term is motivated by empirical evidence in Ireland (2004) and subsequent studies. The reaction coefficients $\psi^*_\pi$, $\psi^*_\tilde{y}$, and $\psi^*_{\Delta y}$ determine to what extent the Fed changes the policy rate in response to fluctuations in the target variables. $c^*$ is an intercept capturing time invariant target values of the central bank’s objectives, and $\eta^T_R$ is a monetary policy disturbance. Finally, in addition to the standard Taylor rule elements, we include a term for past changes in LTV ratios, $\Delta ltv_{t-1}$, with a coefficient $\psi^*_{\Delta ltv}$, to let the data tell us whether the Fed historically responded to shifts in residential mortgage lending standards.

To measure the Fed’s real-time information set prior to official meetings of the FOMC, $E_t\{\cdot\}$, we follow the methodology in Coibion and Gorodnichenko (2012) and update their data set to the end of our sample, 2008Q4. Specifically, we use the forecasts conducted by the Board of Governors’ staff, which are released in the so-called Greenbook with a five-year publication lag. These staff projections are exogenous to the subsequent interest setting of the Fed, so that we can estimate the Taylor rule with OLS (e.g., Coibion and Gorodnichenko, 2011, 2012). We select those meeting dates that occurred closest to a quarter’s midpoint to transform the forecasts from the FOMC meetings frequency into a quarterly time series. $h_\pi$, $h_{\tilde{y}}$, and $h_{\Delta y}$ denote the forecast horizons considered.

Finally, to model the well-documented gradualism of monetary policy decisions, two leading empirical strategies exist: first, defining the current policy rate as a weighted average of the desired interest rate and past interest rate realizations—the interest rate smoothing hypothesis—or, second, allowing for serial correlation in $\eta^T_R$—the persistent monetary policy shock hypothesis. To quantify their relative significance, Rudebusch (2002) formulates a nested model, allowing for both interest rate smoothing ($\rho_{r,k}$) and autocorrelated shocks ($\rho_{\eta,j}$).

We therefore specify a generalized version of this approach, i.e., one that accommodates a flexible number of lags, as follows:

\[ r_t = c + \psi_\pi^* E_t\{\pi_{t+h_\pi}\} + \psi_{\tilde{y}}^* E_t\{\tilde{y}_{t+h_{\tilde{y}}}\} + \psi_{\Delta y}^* E_t\{\Delta y_{t+h_{\Delta y}}\} + \psi_{\Delta ltv}^* \Delta ltv_{t-1} \]

\[ + \sum_{k=1}^K \rho_{r,k} r_{t-k} + \eta^T_R, \text{ where } \eta^T_R = \sum_{j=1}^J \rho_{\eta,j} \eta_{t-j}^R + \zeta_t. \]  

(10)

For target variable $i$, the $\psi_i$ coefficient measures the short-run response of the Fed, i.e., we define $\psi_i \equiv (1 - \sum_{k=1}^K \rho_{r,k}) \psi_i^*$. Coibion and Gorodnichenko (2012) provide strong empirical support for the interest rate smoothing motive relative to the serially correlated shocks hypothesis. We
thus follow their specification for our benchmark Taylor rule, set $h_{\pi} = 1, 2$, $h_{\tilde{y}} = 0$, $h_{\Delta y} = 0$, $K = 1$, and $J = 0$, and re-estimate it for our sample period, 1973Q1 to 2008Q4, while also including changes in LTV ratios. The estimated equation is then (see Column (1) in Table 1):

$$
rt = -0.72 + 0.35Et-\{\pi_{t+1,t+2}\} + 0.12Et-\{\tilde{y}_t\} + 0.17Et-\{\Delta y_t\} \\
+ 0.26\Delta lt v_{t-1} + 0.90r_{t-1} + \eta_{t}^{TR}. 
$$

We find strong support for an interest rate smoothing motive of the Fed, with a first-order autocorrelation coefficient estimate of 0.90. The short-run inflation response coefficient is 0.35 so that the Fed satisfies the Taylor principle by reacting to inflation fluctuations significantly more than one-for-one in the long run, $\psi_{\pi}^{long} = \psi_{\pi}/(1 - \rho_{r,1}) > 1$, where $\pi_{t+1,t+2}$ is the average inflation rate over $t + 1$ and $t + 2$. The Fed adjusts policy rates to the expected contemporaneous output gap and GDP growth, with reaction coefficients of $\psi_{\tilde{y}} = 0.12$ and $\psi_{\Delta y} = 0.17$, respectively. All coefficients are highly significant, where the numbers in brackets are Newey-West standard errors. Our estimates align very closely with those in Coibion and Gorodnichenko (2012).

More importantly, we find a significant positive response coefficient to changes in LTV, $\psi_{\Delta ltv} = 0.26$, implying an increase in the policy rate after a loosening of lending conditions.14 This result confirms the message from the previous subsection that points to a direct reaction of the Fed to housing lending conditions as proxied by LTV ratios over and above any indirect effects operating through contemporaneous output or inflation. This ultimately also suggests that models that analyze the housing market and contain a meaningful monetary policy should explore the quantitative implications of such a modified Taylor rule.

Could our finding be driven by the fact that the Fed simply reacts to general housing market conditions? Given the well-documented empirical property of housing indicators as leading the business cycle and in light of the view that “Housing (REALLY) IS the Business Cycle” (Leamer, 2007, 2015), it is, at the very least, plausible that the Fed would make use of housing market indicators even beyond the Greenbook estimates for inflation, the output gap, and the output growth rate, which are made in real time and with substantial uncertainty. Two such leading indicators are housing starts, $start_t$, and housing permits, $perm_t$. Their cyclical components—based on a Hodrick-Prescott filter with $\lambda = 1, 600$—lead the cyclical real GDP component with a maximum cross-correlation at a horizon of two quarters at 0.71 and 0.73, respectively. Financial housing market indicators like mortgage rate spreads, $spr_t^m$, also display this property: they lead, countercyclically, the business cycle by one quarter and a cross-correlation of $-0.35$. Also the LTV ratio leads

---

14This result (and really most estimated coefficients in the Taylor rule) remains stable across a number of variations in the econometric model specification. In particular, $\psi_{\Delta ltv}$ stays stable and highly significant, when we, e.g., allow for higher-order autocorrelation terms of the policy instrument and/or autocorrelated shocks, or estimate the equation on different data samples; in particular one with the same start date as Coibion and Gorodnichenko (2012), 1987Q4. Table B.1 in Appendix B shows these results.
Table 1: Taylor rules incorporating housing market indicators

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<td>( \psi_{\Delta y} ) ( \Delta y_t )</td>
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<td>( \rho_{r,1} ) ( r_{t-1} )</td>
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<td>( \psi_{\Delta ltv} ) ( \Delta ltv_{t-1} )</td>
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<td>( \psi^m_{\Delta spr} ) ( \Delta spr^m_{t-1} )</td>
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<td>( \psi_{\Delta perm} ) ( \Delta perm_{t-1} )</td>
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Notes: The table shows, in column (1), ordinary least squares estimates for the interest rate rule from Equation (11). \( \psi_\pi \) denotes the central bank reaction to inflation expectations, \( \psi_\tilde{y} \) is the response to the expected output gap, and \( \psi_{\Delta y} \) is the coefficient for expected output growth. \( \rho_{r,1} \) measures the degree of AR(1) interest rate smoothing. We perform all estimations using data from 1973Q1 to 2008Q4 and present Newey-West standard errors in parentheses. In columns (2) to (4), we replace \( \Delta ltv_{t-1} \) with reaction coefficient \( \psi_{\Delta ltv} \) with changes in mortgage interest rate spreads, \( \Delta spr^m_{t-1} \), the growth rate of new housing starts, \( \Delta start_{t-1} \), and the growth rate of new housing permits, \( \Delta perm_{t-1} \), where the reaction coefficients are \( \psi^m_{\Delta spr} \), \( \psi_{\Delta start} \), and \( \psi_{\Delta perm} \), respectively. Column (5) includes all housing market indicators at the same time. \( \tilde{R}^2 \) denotes the adjusted \( R^2 \) and s.e.e. stands for the standard error of the corresponding equation.

*** Significant at the 1 percent level.

** Significant at the 5 percent level.

* Significant at the 10 percent level.
real GDP by one quarter and a positive cross-correlation of 0.51. We find similar correlations based on quarterly growth rates.

Columns (2) to (4) in Table 1 present regression results for the Taylor rule from Equation (11), where we replace the changes in LTV ratio against the three aforementioned housing market indicators, one at a time. All three indicators are significant, just as the LTV ratio. When we estimate Equation (11) with all leading housing market indicators simultaneously in Column (5), only the LTV ratio and the mortgage rate spread stay statistically significant. This evidence supports the notion that U.S. monetary policy reacts to developments in housing markets. Furthermore, among different housing market indicators, the Fed apparently puts an emphasis on financial relative to real housing variables. Finally, both LTV ratio changes and changes in mortgage spreads lead to a statistically significant countervailing reaction of monetary policy.

This finding is, however, somewhat housing-specific. Table B.2 in Appendix B presents estimates for Taylor rules, where we replace \( \Delta \text{ltv}_{t-1} \) with changes in several non-housing-related financial market indicators, in particular, corporate bond spreads (see Section 3.1 for a description of the controls). The point estimates mostly reveal the same pattern as for the credit variables in residential markets: worsening credit conditions lead to an easing of the monetary policy instrument, but, unlike, with LTV ratios and mortgage spreads, we find no statistical significance associated with these policy responses, which is in line with similar findings in Coibion and Gorodnichenko (2012).15

2.3.4 Monetary policy and the interest sensitivity of sectoral investment

With the goal of better understanding the systematic policy tightening by the Fed after an expansionary LTV ratio shock, we scrutinize the monetary policy reaction along two further dimensions. First, we analyze whether households are plausibly aware of the endogenous interest rate hike, and, second, we study whether the shift in monetary policy passes not only through to interest rates that are relevant for housing markets, i.e., mortgage rates, but also to interest rates that directly affect businesses. After all, if there was a lower degree of pass-through to non-housing interest rates, then the differential reaction of non-residential vis-à-vis residential investment to an LTV shock would not be surprising.

To do so, we replace in our baseline nine-variable VAR—one at a time—the mortgage rate with a measure of consumers’ interest rate change expectations \( (r_e^t) \), which we obtain from the Michigan Survey of Consumers, and corporate bond yields \( (r_{tt}^{baa}) \). The Michigan survey asks consumers the following question on a monthly basis: “No one can say for sure, but what do you think will happen to interest rates for borrowing money during the next 12 months—will they go up, stay the same, or go down?” We use a balance score, i.e., the share of consumers expecting rates to go up minus the share of consumers expecting rates to go down, plus 100. Thus the

15When we run a horse race between \( \Delta \text{ltv}_{t-1} \) and corporate bond spread changes in the same Taylor rule framework, we find that changes in LTV ratios continue to have a significantly positive coefficient of essentially the same magnitude as in Table 1 and the corporate bond spread changes remain statistically insignificant.
Figure 6: Mortgage rates, interest rate expectations, and corporate bond yields

Notes: The x-axis represents time in quarters. The solid lines represent point estimates of impulse response functions for the VAR, using the benchmark model and rotating in—
one at a time—interest expectations, $r_t$, or corporate bond yields, $r_{t}^{baa}$. Dark (light) shaded areas display one (two) standard deviations confidence intervals, which we obtain from 5,000 replications of the recursive-design wild bootstrap procedure of Goncalves and Kilian (2004). The dashed lines represent impulse responses for the case of a passive monetary policy authority that does not react to the shock at all, as in Bernanke et al. (1997) and Sims and Zha (2006).

scale is qualitative and positive values indicate a less favorable expected interest rate environment. We use the quarterly average of the monthly data. Second, since we are not aware of any consistent series of bank business loan interest rates, we resort to corporate bond yields as a proxy. Specifically, we use Moody’s Baa corporate bond yields for bonds with a remaining maturity of roughly 30 years. Figure 6 presents the LTV shock propagation to the alternative interest (expectations) measures together with the mortgage rate response of the baseline model. For the qualitative interest change expectations, we plot the cumulative impulse response to proxy for qualitative expectations about the level of interest rates.

The monetary policy reaction passes through to corporate bond yields in a similar order of magnitude as it does to mortgage rates, and, in addition, this monetary policy reaction is reflected in consumers’ expectations on borrowing interest rates, which move instantaneously (rather than hump-shaped as the interest rates themselves) and remain significantly positive for three quarters. The evidence on interest expectations along with rising mortgage rates supports the hypothesis that systematic contractionary monetary policy reactions are a candidate for explaining the decrease in residential investment after an expansionary LTV shock. But the explanation cannot lie in different degrees of pass-through for the relevant sectoral interest rates. Rather, we show next that a much higher interest rate elasticity of

---

16 The results are almost identical had we used the Aaa corporate bond yield series, $r_{t}^{aaa}$. 

20
residential investment causes the difference between non-residential and residential investment.

To do so, we study now an exogenous monetary policy surprise in our baseline nine-variable VAR. A contractionary monetary policy shock in that VAR triggers indeed negative hump-shaped responses of GDP, non-residential, $i_t^{nr}$, and residential investment, $i_t^r$, (see Figure 7). In terms of magnitude, the response of $i_t^r$ is roughly three times as strong as that of $i_t^{nr}$, i.e., residential investment appears to be significantly more interest sensitive than non-residential investment. This finding is consistent with the strong sensitivity of the housing sector to monetary policy shocks documented in, e.g., Erceg and Levin (2006), Monacelli (2009), and Calza et al. (2013), and explains the different reaction of non-residential investment and residential investment to LTV shocks. Note that these papers document a larger monetary policy sensitivity of durable consumption expenditures, including residential investment, compared to non-durable consumption. We add to this literature the finding that residential investment is also more sensitive to monetary policy than non-residential investment.

We close this section by noting that the interest-rate-sensitivity-induced difference between non-residential investment and residential investment is unlikely due to physical differences in the type of investment good, for example, differences in depreciation rates and thus intertemporal substitutability. An LTV shock has similar effects on non-residential equipment and non-residential structures investment, which are themselves similar to the effect on non-residential overall investment shown in Figure 3 (see Figures B.10 to B.11 in Appendix B). We therefore surmise that the different interest sensitivity in non-residential versus residential investment must stem from the difference in who is undertaking the investment rather than the physical characteristics of the investment good. For instance, with customer markets, firms are likely to be much less reactive to fluctuations in the interest rate and will be willing to accommodate increased aggregate demand after looser lending conditions. By contrast, households, can always rent to ride out periods of high interest rates.

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17 Also, $ltv_t$ initially falls significantly after a contractionary monetary policy shock, suggesting initially a “risk-taking channel” (on the part of lenders) of monetary policy, that is, after a monetary contraction, less risky mortgage contracts are signed. $\pi_t$ reacts positively for four quarters (price puzzle) before turning negative—a response that is mimicked by real non-residential investment inflation. Real residential investment inflation displays a negative response from the beginning.

18 The magnitudes between the endogenous interest rate effect and the exogenous shock line up rather well: in Figure 3, a 16 basis points endogenous policy tightening produces a negative 0.8 percent reaction of residential investment; in Figure 7, a 25 basis points exogenous contractionary monetary policy shock produces a negative 1.2 percent reaction.

19 The last panel of Figure 7, where we superimpose the impulse response function to a monetary policy shock in the baseline VAR, with corporate bond yields replacing mortgage rates (see Figure 6), shows again that the explanation does not lie in different degrees of pass-through from the monetary policy rate to longer term interest rates.

20 This result is robust to using Romer and Romer (2004)-shocks or Romer and Romer (2004)-shocks purged from financial indicators, such as the Baa corporate bond spread and the LTV ratio, for the monetary policy shock identification (see, e.g., Caldara and Herbst (2019)).
Figure 7: Monetary policy shock

Notes: The x-axis represents time in quarters. The solid lines represent point estimates of impulse response functions for the VAR, using $x_t = [y_t^nr_t^i_t^r_t^l_t^v_t^r_t^m_t]'$. Dark (light) shaded areas display one (two) standard deviations confidence intervals, which we obtain from 5,000 replications of the recursive-design wild bootstrap procedure of Goncalves and Kilian (2004).
2.4 Variance and historical decomposition

To assess the quantitative relevance of LTV shocks for macroeconomic and housing market fluctuations further, we next provide the results from a forecast error variance decomposition. For the benchmark VAR, the upper panel of Table 2 shows the percentage contribution of LTV shocks to variations in the VAR variables, at different horizons. LTV shocks explain a large fraction of LTV ratio fluctuations, which supports our view that LTV ratios are largely exogenous to the macroeconomy. They explain, however, only a small fraction of the variances of the \(k\)-step-ahead forecast errors in the block of macroeconomic variables: between 1.1 and 4.3 percent of the forecast error variance of GDP, between 6.0 and 9.0 percent for non-residential investment, and, most notably, at most 10.1 percent of the fluctuations in residential investment. The shares for the inflation rates are generally even smaller, in particular, for the real residential investment inflation rate. Again, this finding is inconsistent with the hypothesis that shifts in LTV ratios represent a substantial exogenous driver of housing market dynamics, at least on average in U.S. post-war history. Also consistent with our impulse response and our Taylor rule estimation results is the finding that monetary policy reacts systematically to movements in LTV ratios, with LTV surprises accounting for up to 14.9 percent of variations in the Federal funds rate after one year.

Turning next to the explanatory power of monetary policy shocks, the lower panel of Table 2 shows the comparatively rather substantial contributions of monetary policy shocks toward explaining GDP and in particular residential investment fluctuations, which amount to 26.5 percent at the two-year horizon. Monetary policy shocks also explain almost 16 percent in real house price fluctuations, at the four-year horizon.

Table 2: Variance decompositions

<table>
<thead>
<tr>
<th>LTV Shock</th>
<th>(y_t)</th>
<th>(i_t^{nr})</th>
<th>(i_t^r)</th>
<th>(\pi_t)</th>
<th>(\pi_t^{nr})</th>
<th>(\pi_t^r)</th>
<th>(ltv_t)</th>
<th>(r_t)</th>
<th>(r_t^{in})</th>
</tr>
</thead>
<tbody>
<tr>
<td>Impact</td>
<td>0.0</td>
<td>0.0</td>
<td>0.0</td>
<td>0.0</td>
<td>0.0</td>
<td>0.0</td>
<td>92.6</td>
<td>2.6</td>
<td>0.0</td>
</tr>
<tr>
<td>1 Year</td>
<td>4.3</td>
<td>6.8</td>
<td>1.6</td>
<td>0.6</td>
<td>0.2</td>
<td>2.2</td>
<td>78.5</td>
<td>14.9</td>
<td>4.2</td>
</tr>
<tr>
<td>2 Years</td>
<td>1.8</td>
<td>9.0</td>
<td>8.4</td>
<td>0.8</td>
<td>0.6</td>
<td>4.6</td>
<td>72.3</td>
<td>13.0</td>
<td>6.3</td>
</tr>
<tr>
<td>4 Years</td>
<td>1.1</td>
<td>6.0</td>
<td>10.1</td>
<td>6.5</td>
<td>2.9</td>
<td>5.9</td>
<td>59.7</td>
<td>9.8</td>
<td>4.0</td>
</tr>
</tbody>
</table>

<table>
<thead>
<tr>
<th>FFR Shock</th>
<th>(y_t)</th>
<th>(i_t^{nr})</th>
<th>(i_t^r)</th>
<th>(\pi_t)</th>
<th>(\pi_t^{nr})</th>
<th>(\pi_t^r)</th>
<th>(ltv_t)</th>
<th>(r_t)</th>
<th>(r_t^{in})</th>
</tr>
</thead>
<tbody>
<tr>
<td>Impact</td>
<td>0.0</td>
<td>0.0</td>
<td>0.0</td>
<td>0.0</td>
<td>0.0</td>
<td>0.0</td>
<td>85.5</td>
<td>10.7</td>
<td>17.9</td>
</tr>
<tr>
<td>1 Year</td>
<td>5.7</td>
<td>0.4</td>
<td>19.1</td>
<td>4.7</td>
<td>1.1</td>
<td>7.2</td>
<td>0.8</td>
<td>36.2</td>
<td>25.4</td>
</tr>
<tr>
<td>2 Years</td>
<td>14.3</td>
<td>3.9</td>
<td>26.5</td>
<td>4.6</td>
<td>1.1</td>
<td>14.7</td>
<td>0.7</td>
<td>72.1</td>
<td>18.2</td>
</tr>
<tr>
<td>4 Years</td>
<td>16.9</td>
<td>10.5</td>
<td>25.2</td>
<td>10.0</td>
<td>4.8</td>
<td>15.9</td>
<td>3.6</td>
<td>15.8</td>
<td>9.6</td>
</tr>
</tbody>
</table>

Notes: The table displays the fraction of the forecast error variance (in percent) for the variables in the benchmark VAR, using \(x_t = [y_t, i_t^{nr}, i_t^r, \pi_t, \pi_t^{nr}, \pi_t^r, ltv_t, r_t, r_t^{in}]\), that is explained by LTV shocks (upper panel) / Federal funds rate shocks (lower panel) at different horizons.
Notes: The x-axis represents time in quarters. The Figure shows selected data as it enters the VAR (solid line), a counterfactual evolution without the contribution of LTV shocks (dashed line), and a counterfactual evolution without the contribution of monetary policy shocks (solid line with asterisks). We obtain these results from the benchmark VAR using $\mathbf{x}_t = [y_t, i^r_t, i^r_t, \pi_t, \pi^r_t, \pi^r_t, \pi^r_t, \pi^r_t, \pi^r_t, r_t, r_t, m_t]$. 
The on average (over the entire sample) muted macroeconomic consequences of LTV shocks and the relatively large explanatory power of monetary policy shocks for residential investment fluctuations indicated by the variance decomposition, however, do not imply that LTV shocks also have little relevance (or lower relevance than monetary policy shocks) in particular historical episodes. We therefore now turn to a historical decomposition of the housing cycle in the 2000s, where indeed LTV ratios played a more substantial role. In Appendix C, we provide an additional historical decomposition for the years from 1984 to 1990, which also saw major swings in LTV ratios, where, however, the impact of these swings on macroeconomic variables was, in line with the results from the variance decomposition, small.

Figure 8 presents the historical decomposition for the episode starting in 2001 and leading to the onset of the Great Recession in 2007. Measured LTV ratios did not start taking off from a historically rather low level until the second quarter of 2003 (solid lines). Viewed through the lens of the historical decomposition, this was caused by contractionary LTV shocks that prevailed from the second half of 2002 until almost the beginning of 2007, impeding higher LTV ratios and thus more favorable lending conditions (dashed lines). The sequence of adverse LTV shocks translated into a looser monetary policy environment, which appears to have boosted residential investment, with a maximum impact at the end of 2004. Reflecting the loosening of LTV ratios toward the end of this episode, the expansionary effect on the evolution of the Federal funds rate narrowed and ultimately flipped sign in 2006, slowly reducing the LTV shocks’ impact on residential investment. At the same time, the historical decomposition reveals also a sequence of expansionary monetary policy shocks (solid lines with asterisks) from 2004 on. This accommodative monetary policy ultimately also translated—with some lag—into an amplification of the residential investment cycle from 2005 to 2007.

Comparing monetary policy and LTV ratio shocks during this historical episode, it is LTV ratio shocks that played a larger role for the dynamics of residential investment. However, the narrative suggested by our VAR is somewhat different from the conventional one: LTV ratios were tight from 2002 onwards, which was accompanied by a systematically expansionary monetary policy reaction. This endogenously loosened policy stance then amplified the boom in residential investment, which later was further sustained by exogenously expansionary monetary policy, i.e., by an active deviation from the historical policy rule. The evidence is thus compatible with the “too low for too long”-hypothesis, according to which the Fed is to blame for fueling the housing bubble. Quantitatively however, loose policy rates prior to the crisis may be thought of as a reaction to tight lending and only to a lesser extent as an unstable conduct of monetary policy. Importantly, this is a relative statement only ranking the importance of LTV and monetary policy shocks. It is clear from Figure 8 that neither LTV ratio shocks nor monetary policy shocks can explain the entirety or even the majority of residential investment and house price dynamics prior to the Great Recession. Other effects such as housing demand, the unsustainable accumulation of subprime mortgages, or home equity lines of credit are likely to have played a role during this episode.

25
3 A closer look at LTV ratios

In this section, we investigate whether our results also hold for other LTV ratio series than our benchmark. While the FHFA data are the most comprehensive LTV series available, the following concerns might arise: first, have we really isolated exogenous changes in aggregate borrowing constraints / lending standards in our VAR? Second, have we used the “right” LTV ratio series when it does not contain, for example, the now notorious subprime mortgages? What, more generally, about cyclical composition effects in the aggregate LTV ratio series? The following two subsections address these concerns: in Section 3.1, we purify the LTV ratio series from many macroeconomic, expectational, and financial market influences before using it in the VAR. Section 3.2 analyzes the effects of an LTV shock restricted to the group of first-time home buyers, but now including mortgages to lower-quality borrowers. In addition, we check whether the result for the aggregate FHFA mortgage rate series also holds for fixed-rate mortgages and adjustable-rate mortgages separately.

3.1 LTV ratio: a purification exercise

Thus far, we have interpreted Cholesky-identified shifts in the LTV ratio as an indicator of exogenous changes in aggregate residential lending standards. And, of course, the VAR is meant to condition on macroeconomic variables that might drive the LTV ratio because of, for example, loan demand changes. But other confounding effects might exist: general financial conditions, such as the ability of banks to lend or a changing risk aversion in financial markets, expectations about macroeconomic and financial conditions, etc. Motivated by similar concerns, Bassett et al. (2014) propose a procedure to purge their lending standard measure—banks’ lending standards from the Fed’s Senior Loan Officer Opinion Survey (SLOOS)—from influences that, on the one hand, drive lending standards, but on the other hand, might also reflect changes in loan demand or lending ability. We apply their methodology to our LTV series and remove the effects of variables capturing (a) the current state of the economy, (b) the economic outlook, (c) a number of financial sector condition variables, and (d) house price developments. We then re-estimate our benchmark VAR with the so adjusted LTV measure.

Specifically, to control for changes in LTV ratios that are reflective of the current state of the economy, we follow Bassett et al. (2014) and account for the quarterly percentage change of real GDP, $\Delta y_t$, the quarterly change in the unemployment rate, $\Delta u_t$, and the quarterly change in the real Federal funds rate, $\Delta rr_t$, measured as in Gilchrist and Zakrajsek (2012). Including the latter addresses a specific recent critique by Gertler and Karadi (2015) against zero restrictions in VARs that contain both financial and monetary policy variables. By orthogonalizing LTV ratios first to monetary policy, the LTV ratio series that enters into the VAR should indeed not react to contemporaneous changes in monetary policy, thus justifying our baseline Cholesky ordering.²¹

²¹Following Caldara and Herbst (2019) we have also experimented with using Romer and Romer (2004)-shocks in this purification exercise with essentially identical results.
Next, we turn to variables capturing the outlook about the future evolution of the economy. We purge the LTV series from the one-year ahead expectations on the growth rate of real GDP, $E_{t-1}\{y_{t+4} - y_t\}$, and the expected change in the unemployment rate, $E_{t-1}\{u_{t+4} - u_t\}$. Both expectation measures are available from the Survey of Professional Forecasters. Furthermore, we include the change in the term spread, $\Delta spr^t$, which we measure as the spread between three-month and ten-year Treasury yields, that is the slope of the yield curve. This spread controls for financial market expectations about the future evolution of policy rates. In addition, we account for changes in households’ interest rate expectations by including their qualitative interest rate change expectations from the Michigan Survey of Consumers, i.e., $r_t^e$. Controlling for interest rate expectations, guards us against the following reverse causality story: households expect the Fed to tighten and take out more loans while they are cheap, thus perhaps mechanically increasing the LTV ratio on mortgages.

We also control for the following indicators reflecting financial sector conditions. First, we include the change in the credit spread index, $\Delta spr^c_t$, developed by Gilchrist and Zakrajsek (2012), which represents a corporate bond spread calculated on the basis of secondary market (individual) bond prices. The index serves as an indicator of tensions in financial markets as well as perceived default risks. Second, we use changes in the excess bond premium, $\Delta ebp_t$, also proposed in Gilchrist and Zakrajsek (2012), to address potential movements in financial sector risk aversion, and, third, we include the percentage change in private depository institutions’ net worth, $\Delta nw_t$, to account for the influence of banks’ capital position on lending policies, capturing an ability-to-lend component on the part of the banks. Fourth and finally, we add changes in the S&P Composite Stock Price Index, $\Delta stocks_t$, and the Index of Consumer Sentiment, $\Delta sent_t$, as surveyed by the Michigan Survey of Consumers, to the regression to take into account movements in financial market sentiment.

Finally, we deal with the following more mechanical concern: suppose agents have a certain amount set aside as down payment for their home purchase and they are not credit constrained; in this case, LTV ratios would mechanically positively comove with house prices, and shocks to the aggregate LTV ratio could simply reflect movements in house prices. To exclude such spurious shocks to the LTV ratio, we also purge for contemporaneous house price inflation, $\pi^r_t$, and its first lag, $\pi^r_{t-1}$.\(^{22}\)

In an intermediate step, we estimate a regression of $\Delta ltv_t$\(^{23}\) only on the first set of variables controlling for the current state of the economy. Here and in the second,

\(^{22}\)In Equation (13), we use the BEA implicit price deflator for residential investment, but all the results in this section are robust to using the Case-Shiller house price index or the house value data from the MIRS. They are also robust to not using the first lag of house price inflation in the purging regression. They are, finally, robust to not using house price inflation at all; after all, it could be argued that purging LTV ratios from house prices is “too much.” Also, notice that both coefficients on $\pi^r_t$ and $\pi^r_{t-1}$, albeit insignificantly so, have negative signs, which means there is no prima facie evidence for the above-described mechanical effect. This is also confirmed by the raw correlations between $\Delta ltv_t$ and the three house price inflation measures, which are all statistically insignificant, and, if anything, come with a negative sign contemporaneously as well as at the first lag and first lead.

\(^{23}\)Since the purification regression is not a VAR, we stationarize the LTV series through differencing. When we estimate the equation with the level of the LTV ratio on the left-hand side and controlled for a lag of the LTV ratio, the results were essentially the same.
richer specification, we perform the estimation by ordinary least squares and report Newey-West standard errors in parentheses. The results of the intermediate-step regression are given by:

$$\Delta ltv_t = -0.02 + 0.03 \Delta y_t - 0.51 \Delta u_t - 0.05 \Delta rr_t + \hat{\varepsilon}_{ltv}^t, \quad (12)$$

where the residuals, $\hat{\varepsilon}_{ltv}^t$, denote the LTV series purged from macro variables. Only changes in the unemployment rate have a significant (negative) impact on LTV ratios. With an adjusted $R^2$ of 0.024 the overall explanatory power of the regressors is, however, weak.

Next, we additionally purge $\Delta ltv_t$ from variables proxying for the economic outlook, for financial market conditions, and aggregate house price developments. The resulting regression equation is given by:

$$\Delta ltv_t = -0.80 - 0.46 \Delta \{y_{t+4} - y_t\} - 0.49 \Delta \{u_{t+4} - u_t\} - 0.09 \Delta y_t - 0.16 \Delta u_t + 0.02 \Delta rr_t - 0.15 \Delta spr_t^r + 0.007 r_t^r - 0.17 \Delta ebp_t + 0.005 \Delta nw_t - 0.37 \Delta spr_t^c - 0.002 \Delta stocks_t + 0.02 \Delta sent_t - 0.03 \hat{\pi}^r_t - 0.03 \hat{\pi}^r_{t-1} + \hat{\varepsilon}_{ltv}^t, \quad (13)$$

with the residuals of this regression, $\hat{\varepsilon}_{ltv}^t$, representing the more extensively purged LTV series. Only the expected unemployment rate, GDP growth (which was not significant in Equation (12)), and households’ qualitative interest change expectations are significant at conventional levels, and the adjusted $R^2$ increases only slightly to 0.107. Overall, both regressions support the notion that the raw LTV series is a fairly exogenous and thus a clean measure of movements in aggregate residential lending standards. This notion is further supported by the unconditional time series correlation between the more extensively purged LTV ratio, $\hat{\varepsilon}_{ltv}^t$ and changes of the raw LTV series changes: 0.9. This number means that LTV ratios are unlikely to be driven by other macroeconomic or financial conditions, or anticipation effects.

Next, we use both adjusted LTV ratios, i.e., $\hat{\varepsilon}_{ltv}^t$ and $\hat{\varepsilon}_{ltv}^t$, cumulated up, to re-run the benchmark VAR and study the transmission of the LTV shock for these new LTV series. As Figures B.12 and B.13 in Appendix B show, the results for both purged LTV ratio series are essentially the same as in the benchmark VAR. In summary, the main findings of this paper are not affected by a purging exercise in the spirit of Bassett et al. (2014). We therefore conclude that shifts in the raw LTV ratio series are a good measure of exogenous changes in aggregate residential lending standards / borrowing constraints.

### 3.2 First-time home owners, fixed-rate and variable-rate mortgages

Using data from the American Housing Survey (AHS), Duca et al. (2011, 2013) emphasize the role of *first-time home owners* for mortgage markets, because a large...
share of this group of home buyers might be subject to borrowing constraints. Replacing our benchmark LTV ratio from the FHFA with their first-time buyer LTV ratio series, \( \text{ltv}_{t}^{\text{first}} \), in the VAR also allows us to identify, whether and to what extent potentially different trends in LTV ratios of first-time owners and owner occupiers—both included in the FHFA survey—affect our results. For example, the FHFA LTV ratio may underestimate the increase of first-time owner LTV ratios at the beginning of the 2000s as the survey does not include so-called Alt A nor subprime mortgages, whereas the AHS contains such mortgage loans. In the spirit of Bassett et al. (2014), Duca et al. (2011, 2013) also adjust their raw first-time buyer LTV ratio series for certain cyclical factors, such as, e.g., the unemployment rate, seasonal factors, and some exceptional events.\(^{24}\) The sample starts in 1978Q4, because the AHS data is available only from then on.

Furthermore, our overall LTV ratio series could exhibit time series fluctuations that are merely driven by cyclical changes in the composition of the type of mortgages entering the series. This is related, but not identical to the concern about the cyclical composition of the types of borrowers that we addressed using the first-time buyer LTV ratios based on the AHS. The FHFA-MIRS provides separate LTV ratio series for fixed-rate mortgage loans versus mortgage contracts with adjustable-rates. Calza et al. (2013), for instance, emphasize the relevance of this distinction, in particular, for the transmission of monetary policy into housing markets.

Figures B.14 to B.16 in Appendix B plot the impulse response functions of residential investment for a shock to each of the three alternative LTV ratios in the same nine-variable VAR as the benchmark exercise, where only the LTV ratio was replaced, by one alternative LTV ratio at a time. The results for all three series are very much in line with the benchmark results. The only exception is that, quantitatively, the effects of innovations to the first-time home buyer LTV ratio are much less pronounced, presumably reflecting the smaller number of first-time home buyers in the data.\(^{25}\) For instance, around 65 percent of households already owned a property before the early 2000 housing boom (Mian and Sufi, 2011). This number has fluctuated around a similar level since (Acolin et al., 2017).

4 Robustness

Finally, we assess whether our main findings hold up in a number of additional robustness checks concerning (i) the VAR specification and (ii) the data sample. First, we re-estimate the benchmark VAR using lag orders of \( p = 1 \) and \( p = 4 \) quarters and by estimating the model in first differences. We also experiment with the time aggregation of the data. Second, we check the robustness of our results with respect to the sample choice.

\(^{24}\)We kindly thank the authors for providing us with their data. As our FHFA LTV series, their first-time home buyer series does not include mortgages that are insured by the Federal Housing Administration or guaranteed by the Department of Veterans Affairs.

\(^{25}\)Also, the quantitative importance—as measured by the forecast error variance decomposition—of shocks to the first-time buyer LTV ratio series is for most variables in the VAR smaller compared to shocks to the FHFA LTV ratio, casting some doubt on the notion that the borrowing conditions for borrowing-constrained home buyers drive aggregate dynamics.
Following the literature and the final prediction error information criterion, the lag lengths in our benchmark VAR was set to $p = 2$. Other information criteria such as Hannan-Quinn and Schwarz suggest $p = 1$. Also, since we are dealing with quarterly data, $p = 4$ is another natural lag length to use. Next, we estimate the VAR in first differences (and present cumulative impulse response functions). For the LTV ratio, the null hypothesis of a unit root cannot be rejected based on the augmented Dickey-Fuller test. While this does not prevent us from estimating the VAR in levels consistently, in a finite sample, the event of estimating a root equal to 1.0 for the LTV ratio is zero probability, thus essentially “forcing” LTV ratio shocks to be non-permanent (see for this point Born et al., 2015). This justifies as a robustness check an estimation in first differences.

We also present a robustness exercise, which, we believe, makes our baseline Cholesky identification assumption for the LTV shock a bit more direct. We have solved the time aggregation problem for the monthly LTV ratio and Federal funds rate data by averaging over the three month in the corresponding quarter. Now we take an alternative approach: use the first-month value of the LTV ratio and the third-month values of the Federal funds rate and the mortgage rate in each quarter. Anticipation effects aside, with this time aggregation it is perhaps more natural to assume that the LTV ratio does not react contemporaneously to Federal funds rate innovations. To the extent that the impulse response functions of this VAR do not differ substantially from our baseline VAR, our baseline time aggregation and identification assumption is supported.26

Finally, motivated by relatively low U.S. inflation rates and modest output fluctuations since the 1980s, Clarida et al. (2000), among others, document a significant shift in the conduct of monetary policy for post-1979 data. Beginning with the appointment of Paul Volcker as the Fed’s chairman, their estimated monetary policy reaction function changes considerably toward a more proactive attitude of controlling the inflation rate (expectations). Following Clarida et al. (2000), we therefore re-estimate the VAR by excluding the pre-Volcker era and starting the sample in 1979Q3 (see also Lubik and Schorfheide, 2004; Boivin and Giannoni, 2006, who use the same break date).27

Figures B.17 to B.21 in Appendix B display the results of these robustness checks. The baseline results—a negative reaction of residential investment and real house price deflation after an expansionary LTV ratio shock, accompanied by a contractionary reaction of the Federal funds and the mortgage rates, all of which is overturned without the systematic monetary policy reaction—remain the same. Perhaps interestingly, when we estimate the VAR in first differences, the LTV ratio shock might be permanent and the difference of the residential investment reactions between the case cum and the case sans monetary policy is particularly stark, suggesting that, if anything, we conservatively understate our baseline result in the level estimation.

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26 We have experimented with a number of such modifications with no changes to our baseline result, e.g., an implementation, where we order the first-month value of the LTV ratio seventh, the second-month value of the mortgage rate eighth, and the third-month value ninth.

27 The sample start in 1979Q3, moreover, is known to reduce the price puzzle in monetary policy VARs (Hanson, 2004; Castelnuovo and Surico, 2010).
5 Conclusion

This paper studies the macroeconomic consequences of shifts in housing-related aggregate lending standards as measured by residential mortgage LTV ratios. Using LTV data from the Federal Housing Finance Agency, we find that exogenous expansionary LTV shocks feature positive spillovers to non-residential activity aggregates. Perhaps surprisingly, however, we also find that surprise shifts in the LTV ratio are not likely to be a substantial driver of residential investment, real house price inflation, and household debt in the conventionally assumed way. The reason for this is, historically, a systematic monetary policy response, which tightens as a reaction to looser LTV ratios. We find this result both in VARs and by estimating Taylor rules that include changes in the LTV ratio. As a result, residential investment, real house price inflation, and household debt decline (or at least do not increase) after an increase in the LTV ratio. We also show, however, that without the systematic monetary policy reaction, increases in LTV ratios are expansionary for residential investment and house prices, just as cross-sectional studies in the literature have documented. A variance decomposition shows that, on average, monetary policy shocks have a much higher explanatory power for residential investment and real house price fluctuations, compared to LTV ratio shocks. The historical decomposition for the period prior to the Great Recession, however, shows a somewhat larger impact of LTV ratios in that particular episode.

Furthermore, while our VAR is a linear model, while we, therefore, exclude the Great Recession quarters from our benchmark estimation, and while we overall find little role for surprise changes in the LTV ratio as a driver of residential investment, our results are nevertheless in line with the perception that a tightening of LTV ratios may have exacerbated the downturn in housing markets during the Great Recession.28 The reason is the asymmetry represented by the zero lower bound on nominal interest rates. Historically, the Fed would have likely lowered interest rates in the face of the LTV tightening; however, with interest rates bounded at zero, this cushioning mechanism was absent. According to our fixed interest rate results, such a situation would then be associated with a drop in residential investment, which was indeed observed during the financial crisis.

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28In Appendix D, we provide additional evidence when we include the zero lower bound episode in our analysis.
References


Appendix: For Online Publication Only

A Appendix: data sources and transformations

This appendix provides a detailed overview of the time series we have used in the paper, their sources, and the transformations we have applied. All time series enter at the quarterly frequency, where we calculate the quarterly average for data that is available at a higher frequency. We measure inflation rates and interest rate series in percent, while we include quantity series as natural logarithms (multiplied by 100).

We begin the data description with the nine variables that enter the benchmark VAR model: GDP \( y_t \), non-residential investment \( i^{nr}_t \), residential investment \( i^r_t \), the inflation rate \( \pi_t \), non-residential investment relative price inflation \( \pi^{nr}_t \), residential investment relative price inflation \( \pi^r_t \), the LTV ratio \( ltv_t \), the Federal funds rate \( r_t \), and the mortgage rate \( r^m_t \).

\( y_t \) is U.S. real gross domestic product, \( i^{nr}_t \) is the non-residential component of gross private domestic investment, and \( i^r_t \) is the residential counterpart. For them, we use data provided by the Bureau of Economic Analysis (BEA), specifically the series from NIPA Table 1.1.3., lines 1, 9, and 13. We obtain the corresponding price/deflator series from Table 1.1.9., lines 9, and 13. All series are already provided in seasonally adjusted terms. We divide the two investment price deflators by the GDP deflator from Table 1.1.9., line 1, before calculating quarterly growth rates by applying the first-difference operator to the natural logarithms of both relative prices to calculate \( \pi^{nr}_t \) and \( \pi^r_t \). The LTV ratio, \( ltv_t \), is the national average of the ratio of the loan volume relative to the underlying house price for newly originated single family home mortgage loans from the MIRS survey of the Federal Housing Finance Agency.\(^{29}\) Specifically, we use the spreadsheet All Homes - Table 17 and apply the Census X-12 filter to seasonally adjust the LTV series before calculating the quarterly average of this monthly LTV series. From the Federal Reserve Bank of St. Louis database (FRED) we obtain the quarterly average of the nominal Federal funds rate (FEDFUNDS) to measure \( r_t \) and the average quarterly change of the Consumer Price Index (CPIAUCSL) to measure \( \pi_t \). We also use the monthly FHFA survey to obtain nominal contract mortgage rates, \( r^m_t \), of which we calculate the quarterly average.\(^{30}\)

In a model extension in Section 2.2.3, we rotate into the VAR two alternative series, replacing \( r^m_t \). \( r^{baa}_t \) denotes the quarterly average of Moody’s Baa corporate bond yield for bonds with a maturity of, roughly, 30 years, which we obtain from the Federal Reserve Bank of St. Louis database FRED with identifier WBAA. We also use a measure of consumer interest rate expectations, \( r^e_t \), which we obtain from the Michigan Survey of Consumers as a quarterly average. To extend our sample period to 2015Q3 in Section D, we use the shadow rate of the Federal funds rate, as proposed in and provided by Wu and Xia (2016).\(^{31}\)

For the estimation of Taylor rules in Section 2.3.3, we rely on the data set used in


\(^{31}\)See https://sites.google.com/site/jingcynthiawu/home/wu-xia-shadow-rates.
Coibion and Gorodnichenko (2012). Their sample ends in 2006, as the Greenbook forecasts that are used in this paper are released with a minimum publication lag of five years by the Board of Governors. Therefore, we update the data set by employing the same aggregation principle and by using the same data sources as described in Coibion and Gorodnichenko (2012). In some Taylor rule regressions, we use the total housing starts of new privately owned housing units (HOUST) and the new private housing units authorized by building permits (PERMIT), each measured in thousands of units at a seasonally adjusted annual rate, from the FRED database. We also calculate a mortgage rate spread, which is the difference between $r^m_t$ and the 10-year Treasury yield, $r^{10}_t$, from the data set used in Gilchrist and Zakrajsek (2012).

For the purification exercise conducted in Section 3.1, we use the following data sources: from the FRED database, we obtain the civilian unemployment rate, $u_t$ (identifier: UNRATE), and from the Flow of Funds database, we obtain banks’ net worth $nw_t$ (identifier: Z1/Z1/FL702090095.Q). For $spr^r_t$, $ebp_t$, $rr_t$, and $spr^d_t$, we draw on the data set of Gilchrist and Zakrajsek (2012), while the historical Survey of Professional Forecasters data can be downloaded from their website (see Bassett et al., 2014, who use the same data). The Standard & Poor’s 500 Index (INDEXCBOE: .INX), $stocks_t$, is from Robert Shiller’s homepage.

\[32\] See https://www.aeaweb.org/articles?id=10.1257/mac.4.4.126.

\[33\] See https://www.aeaweb.org/articles?id=10.1257/aer.102.4.1692.

\[34\] See https://www.philadelphiafed.org/research-and-data/real-time-center/.

B Additional Tables and Figures

Table B.1: Taylor rules: econometric model modifications

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<tr>
<td>$\psi_r : E_t - { \pi_{1,2} }$</td>
<td>0.37***</td>
<td>0.31</td>
<td>0.21</td>
<td>0.32***</td>
<td>0.34***</td>
<td>0.35***</td>
<td>0.53***</td>
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<td>(0.19)</td>
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<td>(0.06)</td>
<td>(0.07)</td>
<td>(0.05)</td>
<td>(0.09)</td>
</tr>
<tr>
<td>$\psi_y : E_t - { \hat{y}_t }$</td>
<td>0.14***</td>
<td>0.33***</td>
<td>0.28***</td>
<td>0.12***</td>
<td>0.14***</td>
<td>0.12***</td>
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<tr>
<td>$\psi_{\Delta y} : E_t - { \Delta y_t }$</td>
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<td>0.09</td>
<td>0.11**</td>
<td>0.16***</td>
<td>0.17***</td>
<td>0.22***</td>
<td>0.28***</td>
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<td>(0.05)</td>
<td>(0.06)</td>
<td>(0.05)</td>
<td>(0.04)</td>
<td>(0.04)</td>
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<td>(0.06)</td>
</tr>
<tr>
<td>$\rho_{r,1} : r_{t-1}$</td>
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<td>0.91***</td>
<td>0.77***</td>
<td>0.86***</td>
<td>0.82***</td>
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<td>(0.11)</td>
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<td>(0.15)</td>
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</tr>
<tr>
<td>$\rho_{\eta,1} : \eta_{t-1}^{TR}$</td>
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<td>1.13***</td>
<td>−0.07</td>
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<td>−0.16</td>
<td>−0.31**</td>
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<td>(0.13)</td>
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<td>$\psi_{\Delta ltv} : \Delta ltv_{t-1}$</td>
<td>0.26**</td>
<td>0.18***</td>
<td>0.17***</td>
<td>0.30***</td>
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<td>(0.09)</td>
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<td>$\hat{R}^2$</td>
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<td>0.94</td>
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<td>0.95</td>
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<td>$s.e.e.$</td>
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<td>0.80</td>
<td>0.75</td>
<td>0.28</td>
<td>0.73</td>
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Notes: The table shows ordinary least squares estimates for the interest rate rule from Equation (10). $\psi_r$ denotes the central bank reaction to inflation expectations, $\psi_y$ is the response to the expected output gap, and $\psi_{\Delta y}$ is the coefficient for expected growth. $\rho_{r,k}$ measures the degree of AR(k) interest rate smoothing, and $\rho_{\eta,j}$ defines the order (J) of serial correlation in $\eta_{t-1}^{TR}$. $\psi_{\Delta ltv}$ represents the monetary policy reaction to lagged changes in the LTV ratio. We perform the estimations of models (1) to (5) using data from 1973Q1 to 2008Q4 and present Newey-West standard errors in parentheses. In column (1), we set $K = 1, 2$ and $J = 0$, in column (2), we set $K = 0$ and $J = 1$, in column (3), we set $K = 0$ and $J = 1, 2$, and column (4)/(5) estimates an ARMA(1,1)/ARMA(2,2) Taylor rule, respectively. In column (6), we estimate the benchmark model from Equation (11) for a data sample starting in 1987Q4 as in Coibion and Gorodnichenko (2012), and in column (7), we start the sample with Paul Volcker's Fed chairmanship in 1979Q3 as in Clarida et al. (2000) or Boivin and Giannoni (2006), among others. $\hat{R}^2$ denotes the adjusted $R^2$ and s.e.e. stands for the standard error of the corresponding equation.

*** Significant at the 1 percent level.

** Significant at the 5 percent level.

* Significant at the 10 percent level.
Table B.2: Taylor rules incorporating non-housing market indicators

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<td>$\psi_\pi$</td>
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<td>0.35***</td>
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<td>(0.07)</td>
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<td>$E_t - {y_t}$</td>
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<tr>
<td>$\psi_{\Delta y}$</td>
<td>$E_t - {\Delta y_t}$</td>
<td>0.18***</td>
<td>0.19***</td>
<td>0.12***</td>
<td>0.16***</td>
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<td>$\psi_{baa}$</td>
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<td>0.82</td>
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Notes: The table shows ordinary least squares estimates for the interest rate rule from Equation (10). $\psi_\pi$ denotes the central bank reaction to inflation expectations, $\psi_y$ is the response to the expected output gap, and $\psi_{\Delta y}$ is the coefficient for expected output growth. $\rho_{r,1}$ measures the degree of AR(1) interest rate smoothing. We perform all estimations using data from 1973Q1 to 2008Q4 and present Newey-West standard errors in parentheses. In columns (1) and (2), we, respectively, use a corporate bond credit spread, $\Delta spr_{t-1}$, and the excess bond premium, $\Delta ebp_{t-1}$, both proposed in and provided by Gilchrist and Zakrajsek (2012), instead of $\Delta t\pi_{t-1}$. Columns (3) to (5) use Moody’s Baa spread relative to 10-year Treasury yields, $\Delta spr_{baa}$, and the Aaa-rated counterpart, $\Delta spr_{aaa} = \Delta (r^{aaa}_t - r^{10}_t)$, the Aaa-rated counterpart, $\Delta spr_{aaa} = \Delta (r^{aaa}_t - r^{10}_t)$, and the difference between Baa and Aaa-rated bond yields, $\Delta spr_{h-a} = \Delta (r^{baa}_t - r^{aaa}_t)$; where we obtain the data from FRED. Finally, we use changes in the S&P Composite Stock Price Index, $\Delta stocks_{t-1}$, which we downloaded from Robert Shiller’s website. Reaction coefficients are $\psi_{spr}$, $\psi_{ebp}$, $\psi_{spr}$, $\psi_{spr}$, $\psi_{spr}$, and $\psi_{stocks}$. $R^2$ denotes the adjusted $R^2$ and $s.e.e.$ stands for the standard errors.

*** Significant at the 1 percent level.
** Significant at the 5 percent level.
* Significant at the 10 percent level.
Notes: The x-axis represents time in quarters. The solid lines represent point estimates of impulse response functions for the VAR, using $x_t = [y_t, \pi_t^{nr}, \pi_t^{r,s}, \pi_t^{dr}, \pi_t^{ir}, t_l, r_t, r_t^{m}]'$. $\pi_t^{r,s}$ is the structures sub-component of the aggregate U.S. residential investment series, which we take from NIPA Table 5.3.3., line 21, and $\pi_t^{r,s}$ is based on NIPA Table 5.3.4., line 21. Dark (light) shaded areas display one (two) standard deviations confidence intervals, which we obtain from 5,000 replications of the recursive-design wild bootstrap procedure of Goncalves and Kilian (2004). The dashed lines represent impulse responses for the case of a passive monetary policy authority that does not react to the shock at all, as in Bernanke et al. (1997) and Sims and Zha (2006).
Figure B.2: Loan-to-value ratio shock: residential investment in single family homes

Notes: The x-axis represents time in quarters. The solid lines represent point estimates of impulse response functions for the VAR, using $x_t = [y_t \ i^{nr}_t \ i^{r,sf}_t \ \pi_t^{nr} \ \pi_t^{r,sf} \ ltv_t \ \rr_t \ \rm_t]$. $i^{r,sf}_t$ is the single family homes sub-component of the aggregate U.S. residential investment series, which we take from NIPA Table 5.3.3., line 23, and $\pi^{r,sf}_t$ is based on NIPA Table 5.3.4., line 23. Dark (light) shaded areas display one (two) standard deviations confidence intervals, which we obtain from 5,000 replications of the recursive-design wild bootstrap procedure of Goncalves and Kilian (2004). The dashed lines represent impulse responses for the case of a passive monetary policy authority that does not react to the shock at all, as in Bernanke et al. (1997) and Sims and Zha (2006).
Figure B.3: Loan-to-value ratio shock: residential investment in new structures

Notes: The x-axis represents time in quarters. The solid lines represent point estimates of impulse response functions for the VAR, using $x_t = [y_t \ i^{r,ns}_t \ \pi_t \ \pi^{r,ns}_t \ lt_t \ rt_t \ \rho_t]’$. $i^{r,ns}_t$ is the new structures sub-component of the aggregate U.S. residential investment series, which we take from NIPA Table 5.3.3., line 30, and $\pi^{r,ns}_t$ is based on NIPA Table 5.3.4., line 30. Dark (light) shaded areas display one (two) standard deviations confidence intervals, which we obtain from 5,000 replications of the recursive-design wild bootstrap procedure of Goncalves and Kilian (2004). The dashed lines represent impulse responses for the case of a passive monetary policy authority that does not react to the shock at all, as in Bernanke et al. (1997) and Sims and Zha (2006).
Figure B.4: Loan-to-value ratio shock: real MIRS house price inflation

Notes: The x-axis represents time in quarters. The solid lines represent point estimates of impulse response functions for the VAR, using \( \mathbf{z}_t = [y_t i_t^{nr} i_t^r \pi_t \pi_t^{mirs} \pi_t^{mirs} \pi_t^{mirs} \pi_t^{mirs} \pi_t^{mirs} \pi_t^{mirs}]' \). \( \pi_t^{mirs} \) is GDP deflator normalized house price inflation, where we use the nominal house price series from the FHFA survey. Dark (light) shaded areas display one (two) standard deviations confidence intervals, which we obtain from 5,000 replications of the recursive-design wild bootstrap procedure of Goncalves and Kilian (2004). The dashed lines represent impulse responses for the case of a passive monetary policy authority that does not react to the shock at all, as in Bernanke et al. (1997) and Sims and Zha (2006).
Figure B.5: Loan-to-value ratio shock: real Shiller house price inflation

Notes: The x-axis represents time in quarters. The solid lines represent point estimates of impulse response functions for the VAR, using \( x_t = [y_t \ i_t^{nr} \ i_t^r \ \pi_t \ \pi_t^{nr} \ \pi_t^{shill} \ \ln y_t \ \pi_t^m]^T \). \( \pi_t^{shill} \) is GDP deflator normalized house price inflation, where we use the nominal house price series proposed by Robert Shiller. Dark (light) shaded areas display one (two) standard deviations confidence intervals, which we obtain from 5,000 replications of the recursive-design wild bootstrap procedure of Goncalves and Kilian (2004). The dashed lines represent impulse responses for the case of a passive monetary policy authority that does not react to the shock at all, as in Bernanke et al. (1997) and Sims and Zha (2006).
Notes: The x-axis represents time in quarters. The solid lines represent point estimates of impulse response functions for the VAR, using $\mathbf{x}_t = [y_t, i_t^{nr}, i_t^r, p_t, p_t^{nr}, p_t^r, \text{ltv}_t, r_t, r_t^{nr}]$. Dark (light) shaded areas display one (two) standard deviations confidence intervals, which we obtain from 5,000 replications of the recursive-design wild bootstrap procedure of Goncalves and Kilian (2004). The dashed lines represent impulse responses for the case of a passive monetary policy authority that does not react to the shock at all, as in Bernanke et al. (1997) and Sims and Zha (2006).
Notes: The x-axis represents time in quarters. The solid lines represent point estimates of impulse response functions for the VAR, using \( \mathbf{x}_t = [y_t \ i^{nr}_t \ i^{r}_t \ \pi^{GDP}_t \ \pi^{nr}_t \ \pi^{r}_t \ ltv_t \ r_t \ r^{m}_t]' \). Dark (light) shaded areas display one (two) standard deviations confidence intervals, which we obtain from 5,000 replications of the recursive-design wild bootstrap procedure of Goncalves and Kilian (2004). The dashed lines represent impulse responses for the case of a passive monetary policy authority that does not react to the shock at all, as in Bernanke et al. (1997) and Sims and Zha (2006).
Figure B.8: Loan-to-value ratio shock: PCE instead of CPI inflation

Notes: The x-axis represents time in quarters. The solid lines represent point estimates of impulse response functions for the VAR, using $x_t = [y_t \ i_{nt}^{nr} \ i_t^P \ \pi_t^{PCE} \ \pi_{nt}^{nr} \ \pi_t^r \ \text{lnt} \ r_t \ r_t^{m}]'$.

$\pi_t^{PCE}$ is the seasonally adjusted quarterly growth rate of the personal consumption expenditures chain-type price index, which we obtain from FRED (identifier: PCEPI_PCH). Dark (light) shaded areas display one (two) standard deviations confidence intervals, which we obtain from 5,000 replications of the recursive-design wild bootstrap procedure of Goncalves and Kilian (2004). The dashed lines represent impulse responses for the case of a passive monetary policy authority that does not react to the shock at all, as in Bernanke et al. (1997) and Sims and Zha (2006).
Notes: The x-axis represents time in quarters. The solid lines represent point estimates of impulse response functions for the VAR, using $\mathbf{x}_t = [y_t \ i_{t}^{bm} \ b_{t}^{hm} \ \pi_t \ \pi_t^{nr} \ \pi_t^{r} \ \pi_t^{ltv} \ r_t \ r_t^{m}]^\prime$. In this VAR we replace $i_t^{nr}$ and $i_t^{r}$ by business and home mortgages, $b_{t}^{bm}$ and $b_{t}^{hm}$, respectively. Both debt series are stock variables (in log levels), measuring the outstanding amount of loans at the end of each quarter. We divide by the GDP deflator to transform them into real quantities (see Justiniano et al., 2015). The debt measures are from the Flow of Funds database with identifiers: Z1/Z1/FL143165005.Q for non-financial business mortgages and Z1/Z1/LA153165105.Q for home mortgages. We apply the Census X-12 filter to seasonally adjust both debt series. Dark (light) shaded areas display one (two) standard deviations confidence intervals, which we obtain from 5,000 replications of the recursive-design wild bootstrap procedure of Goncalves and Kilian (2004). The dashed lines represent impulse responses for the case of a passive monetary policy authority that does not react to the shock at all, as in Bernanke et al. (1997) and Sims and Zha (2006).
Figure B.10: Loan-to-value ratio shock: non-residential investment in structures

Notes: The x-axis represents time in quarters. The solid lines represent point estimates of impulse response functions for the VAR, using $x_t = [y_t \ i_{t}^{nr,s} \ \ i_{t}^{s} \ \ \ \pi_t \ \ \ \pi_{t}^{nr,s} \ \ \ \pi_{t}^{r} \ \ \ \pi_{t}^{ltv} \ \ \ \pi_{t}^{m}]'$. $i_{t}^{nr,s}$ is the structures sub-component of the aggregate U.S. non-residential investment series, which we take from NIPA Table 5.3.3., line 3, and $\pi_{t}^{nr,s}$ is based on NIPA Table 5.3.4., line 3. Dark (light) shaded areas display one (two) standard deviations confidence intervals, which we obtain from 5,000 replications of the recursive-design wild bootstrap procedure of Goncalves and Kilian (2004). The dashed lines represent impulse responses for the case of a passive monetary policy authority that does not react to the shock at all, as in Bernanke et al. (1997) and Sims and Zha (2006).
Figure B.11: Loan-to-value ratio shock: non-residential investment in equipment

**Notes:** The x-axis represents time in quarters. The solid lines represent point estimates of impulse response functions for the VAR, using $x_t = [y_t^r y_t^r i_t^r \pi_t^r \pi_t^r \pi_t^r \pi_t^r \pi_t^r \pi_t^r \pi_t^r]$. $i_t^r$ is the equipments sub-component of the aggregate U.S. non-residential investment series, which we take from NIPA Table 5.3.3., line 9, and $\pi_t^r$ is based on NIPA Table 5.3.4., line 9. Dark (light) shaded areas display one (two) standard deviations confidence intervals, which we obtain from 5,000 replications of the recursive-design wild bootstrap procedure of Goncalves and Kilian (2004). The dashed lines represent impulse responses for the case of a passive monetary policy authority that does not react to the shock at all, as in Bernanke et al. (1997) and Sims and Zha (2006).
Figure B.12: Loan-to-value ratio shock: slightly purged LTV Ratio

**Notes:** The x-axis represents time in quarters. The solid lines represent point estimates of impulse response functions for the VAR, using \( \mathbf{x}_t = [y_t \ i_t^{nr} \ i_t^r \ \pi_t^{nr} \ \pi_t^r \ \tilde{\varepsilon}_t^{ltv} \ r_t^{nr} \ r_t^{nr}]' \), where \( \tilde{\varepsilon}_t^{ltv} \) denotes the residuals of Equation (12). Dark (light) shaded areas display one (two) standard deviations confidence intervals, which we obtain from 5,000 replications of the recursive-design wild bootstrap procedure of Goncalves and Kilian (2004). The dashed lines represent impulse responses for the case of a passive monetary policy authority that does not react to the shock at all, as in Bernanke et al. (1997) and Sims and Zha (2006).
Notes: The x-axis represents time in quarters. The solid lines represent point estimates of impulse response functions for the VAR, using $\textbf{x}_t = [y_t \ i_t^{nr} \ i_t^r \ \pi_t \ \pi_t^{nr} \ \pi_t^r \ \varepsilon_{ltv}^t \ r_t \ r_t^{mn}]'$, where $\varepsilon_{ltv}^t$ denotes the residuals of Equation (13). Dark (light) shaded areas display one (two) standard deviations confidence intervals, which we obtain from 5,000 replications of the recursive-design wild bootstrap procedure of Goncalves and Kilian (2004). The dashed lines represent impulse responses for the case of a passive monetary policy authority that does not react to the shock at all, as in Bernanke et al. (1997) and Sims and Zha (2006).
Figure B.14: Loan-to-value ratio shock: first-time buyer mortgages

Notes: The x-axis represents time in quarters. The black lines represent point estimates of impulse response functions for the VAR, using $\mathbf{z}_t = [y_t \ i_{t}^{nr} \ i_{t}^{r} \ \pi_t \ \pi_t^{nr} \ \pi_t^{r} \ \text{ltv}_{t}^{first} \ r_t \ r_t^{m}]'$, where $\text{ltv}_{t}^{first}$ denotes the AHS LTV ratio series, adjusted as in Duca et al. (2011, 2013), which was provided to us from these authors. Dark (light) shaded areas display one (two) standard deviations confidence intervals, which we obtain from 5,000 replications of the recursive-design wild bootstrap procedure of Goncalves and Kilian (2004). The dashed lines represent impulse responses for the case of a passive monetary policy authority that does not react to the shock at all, as in Bernanke et al. (1997) and Sims and Zha (2006).
Figure B.15: Loan-to-value ratio shock: fixed-rate mortgages

Notes: The x-axis represents time in quarters. The black lines represent point estimates of impulse response functions for the VAR, using $x_t = [y_t, i_t^{nr}, i_t^r, \pi_t, \pi_t^{nr}, \pi_t^r, \ln(\text{ltv}_t^{\text{fix}}), r_t, r_t^{m}]'$, where $\text{ltv}_t^{\text{fix}}$ denotes the fixed-rate mortgage LTV ratio series from the MIRS, Table 20. Because this disaggregate series is only available from 1986Q1, we fill in the rest by backcasting them to 1973Q1, using the overall LTV ratio. Dark (light) shaded areas display one (two) standard deviations confidence intervals, which we obtain from 5,000 replications of the recursive-design wild bootstrap procedure of Goncalves and Kilian (2004). The dashed lines represent impulse responses for the case of a passive monetary policy authority that does not react to the shock at all, as in Bernanke et al. (1997) and Sims and Zha (2006).
Notes: The x-axis represents time in quarters. The black lines represent point estimates of impulse response functions for the VAR, using $x_t = [y_t \ i_t^{nr} \ i_t^r \ \pi_t \ \pi_t^{nr} \ \pi_t^r \ \pi_t^{var} \ \pi_t^{rvar} \ \pi_t \ r_t \ r_t^m]^T$, where $ltv_t^{var}$ denotes the fixed-rate mortgage LTV ratio series from the MIRS, Table 23. Because this disaggregate series is only available from 1986Q1, we fill in the rest by backcasting them to 1973Q1, using the overall LTV ratio. Dark (light) shaded areas display one (two) standard deviations confidence intervals, which we obtain from 5,000 replications of the recursive-design wild bootstrap procedure of Goncalves and Kilian (2004). The dashed lines represent impulse responses for the case of a passive monetary policy authority that does not react to the shock at all, as in Bernanke et al. (1997) and Sims and Zha (2006).
Figure B.17: Loan-to-value ratio shock: 1 lag in VAR

Notes: The x-axis represents time in quarters. The solid lines represent point estimates of impulse response functions for the VAR, using \( x_t = \left[ y_t, \pi_t^nr, \pi_t^r, \pi_t^l, \pi_t^m, l_t, r_t, r_t^m \right]' \). Dark (light) shaded areas display one (two) standard deviations confidence intervals, which we obtain from 5,000 replications of the recursive-design wild bootstrap procedure of Goncalves and Kilian (2004). The dashed lines represent impulse responses for the case of a passive monetary policy authority that does not react to the shock at all, as in Bernanke et al. (1997) and Sims and Zha (2006).
Figure B.18: Loan-to-value ratio shock: 4 lags in VAR

Notes: The x-axis represents time in quarters. The solid lines represent point estimates of impulse response functions for the VAR, using $\mathbf{x}_t = [y_t \ i_{tnr}^r \ i_t^r \ \pi_t^e \ \pi_t^r \ \pi_t^{nr} \ \pi_t^{nr} \ \pi_t^r \ \pi_t^{nr} \ \pi_t^r \ \pi_t^{nr} \ \pi_t^r \ \pi_t^{nr} \ \pi_t^r \ \pi_t^{nr} \ \pi_t^r \ \pi_t^{nr} \ \pi_t^r \ \pi_t^{nr} \ \pi_t^r]$'. Dark (light) shaded areas display one (two) standard deviations confidence intervals, which we obtain from 5,000 replications of the recursive-design wild bootstrap procedure of Goncalves and Kilian (2004). The dashed lines represent impulse responses for the case of a passive monetary policy authority that does not react to the shock at all, as in Bernanke et al. (1997) and Sims and Zha (2006).
Figure B.19: Loan-to-value ratio shock: difference specification

Notes: The x-axis represents time in quarters. The solid lines represent point estimates of IRFs for the VAR, using $x_t = [\Delta y_t \ \Delta i^{lr}_t \ \Delta i^{nr}_t \ \Delta \pi_t \ \Delta \pi^{nr}_t \ \Delta \pi^{r}_t \ \Delta \pi^{lr}_t \ \Delta \pi^{nr}_t \ \Delta \pi^{r}_t \ \Delta \pi^{lr}_t \ \Delta \pi^{nr}_t \ \Delta \pi^{r}_t \ \Delta \pi^{lr}_t]$ where IRFs are cumulated. Dark (light) shaded areas display one (two) standard deviations confidence intervals, which we obtain from 5,000 replications of the recursive-design wild bootstrap procedure of Goncalves and Kilian (2004). The dashed lines represent impulse responses for the case of a passive monetary policy authority that does not react to the shock at all, as in Bernanke et al. (1997) and Sims and Zha (2006).
Figure B.20: LTV shock: LTV (M1), FFR (M3), and Mortgage rate (M3)

Notes: The x-axis represents time in quarters. The solid lines represent point estimates of impulse response functions for the VAR, using $x_t = \begin{bmatrix} y_t \ i_{t,nr} \ i_{t,r} \ \pi_{t,nr} \ \pi_{t,r} \ ltv_{t,1} \ r_{t,3} \ r_{t,m,3} \end{bmatrix}'$. Dark (light) shaded areas display one (two) standard deviations confidence intervals, which we obtain from 5,000 replications of the recursive-design wild bootstrap procedure of Goncalves and Kilian (2004). The dashed lines represent impulse responses for the case of a passive monetary policy authority that does not react to the shock at all, as in Bernanke et al. (1997) and Sims and Zha (2006). $ltv_{t,1}$ represents the LTV ratio measured in the first month of a quarter, where we use this realization as the value for the entire quarter. $r_{t,3}$ and $r_{t,m,3}$ are the corresponding FFR and mortgage rate measured in the third month of a quarter, again, imposed to hold for the entire quarter.
Notes: The x-axis represents time in quarters. The solid lines represent point estimates of impulse response functions for the VAR, using $\mathbf{x}_t = [y_t \ i_{nt}^n \ i_t^r \ \pi_t \ \pi_{nt}^n \ \pi_t^r \ ltv_t \ r_t \ r_{nt}^n]'$. Dark (light) shaded areas display one (two) standard deviations confidence intervals, which we obtain from 5,000 replications of the recursive-design wild bootstrap procedure of Goncalves and Kilian (2004). The dashed lines represent impulse responses for the case of a passive monetary policy authority that does not react to the shock at all, as in Bernanke et al. (1997) and Sims and Zha (2006).
For the period from 1984 to 1990, Figure C.1 plots the evolution of the LTV ratio in the top panel. In 1984, the LTV ratio experienced a historical high, that is, it was the laxest since the start of our data. It then experienced a decline over two years, but altogether the LTV ratio remained expansionary until 1986: the LTV ratio was higher than it would have been without LTV ratio shocks. The other four panels display, respectively, $\pi_t^{nr}$, $i_t^{nr}$, $i_t^r$, and $r_t$ as a solid line along with two counterfactual scenarios: the data without the contribution of, respectively, (i) LTV shocks (dashed lines), and (ii) monetary policy shocks (solid lines with asterisks). The main take-away is: absent LTV shocks, the macroeconomy would have evolved fairly similarly to the historical experience (the dashed lines tend to be close to the solid lines), even in periods of major shifts in LTV ratios, confirming the results of the variance decomposition in the main text.

By contrast, shocks to monetary policy played a much larger role, just as in the variance decomposition, especially for residential investment, as the monetary policy counterfactuals in Figure C.1 show (solid lines with asterisks). The differences between the data and the monetary policy counterfactuals are in general much larger than the differences between the data and the LTV ratio counterfactuals. The exogenous component of monetary policy was expansionary until the end of 1986 and contractionary afterwards. Without monetary policy shocks, residential investment would have been lower from 1984 to mid 1987, and higher afterward.
Notes: The x-axis represents time in quarters. The Figure shows selected data as it enters the VAR (solid line), a counterfactual evolution without the contribution of LTV shocks (dashed line), and a counterfactual evolution without the contribution of monetary policy shocks (solid line with asterisks). We obtain these results from the benchmark VAR using $x_t = [y_t \ i^{nr}_t \ i^r_t \ \pi_t \ \pi^{nr}_t \ \pi^r_t \ ltv_t \ r_t \ r^{nr}_t]$. 

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In the main body of the paper, we established that an expansionary LTV shock—counteracted systematically by the monetary policy instrument—reduces residential investment, under conventional monetary policy. While our VAR is a linear model and even though we exclude the Great Recession quarters from our analysis, our results sans monetary policy nevertheless support the view that a tightening of LTV ratios may have exacerbated the downturn in housing markets during the Great Recession. Historically, the Fed would have likely lowered interest rates in the face of this LTV tightening; however, with interest rates bounded at zero, this cushioning mechanism was absent. According to our fixed interest rate results, such a situation should then be associated with a strong drop in residential investment, which was indeed observed during the financial crisis.

Another (somewhat suggestive) way of seeing this is to compare the benchmark model results, which are based on a sample ending in 2008Q4, with results from a model estimated on a sample that includes the zero lower bound episode, 1973Q1 to 2015Q3 to be specific. We do so by using the shadow rate of the Federal funds rate, as proposed in Wu and Xia (2016). The difference between the impulse responses of both samples can then be interpreted as a qualitative approximation of the marginal effect of an LTV shock in the zero lower bound episode relative to normal times.

In Figure D.1, we show the impulse response functions of our benchmark VAR on the extended sample ending in 2015Q3, along with impulse responses for the benchmark model (solid line) estimated until 2008Q4 and the Bernanke et al. (1997) exercise (dashed line). Indeed, adding quarters that are characterized by the zero lower bound and by unconventional monetary policy to the VAR, moves the unrestricted adjustment patterns toward the direction of the fixed interest rate environment. Most notably, the peak response of the Federal funds rate is muted by more than one third and the drop in residential investment is about two thirds smaller, relative to the benchmark VAR. This finding suggests that the statistically generated fixed interest rate environment can indeed qualitatively approximate what happens in a zero lower bound episode plus unconventional monetary policy, which would mean that, while not without effect, unconventional monetary policy does not appear to be a complete substitute for conventional monetary policy.
Figure D.1: Loan-to-value ratio shock in an extended sample (incl. ZLB)

Notes: The x-axis represents time in quarters. The solid lines represent point estimates of impulse response functions for the VAR, using $x_t = [y_t \ i_{t}^{nr} \ \pi_t \ \pi_t^{nr} \ \pi_t^r \ ltv_{t} \ r_{t} \ r_{t}^{m} \ r_{t}^{m} \ ltv_{t} \ r_{t} \ r_{t}^{m} \ ltv_{t} \ r_{t} \ r_{t}^{m} \ ltv_{t} \ r_{t} \ r_{t}^{m}]$. Dark (light) shaded areas display one (two) standard deviations confidence intervals, which we obtain from 5,000 replications of the recursive-design wild bootstrap procedure of Goncalves and Kilian (2004). The dashed lines represent impulse responses for the case of a passive monetary policy authority that does not react to the shock at all, as in Bernanke et al. (1997) and Sims and Zha (2006), and dotted lines display adjustment patterns for the VAR estimated over 1973Q1 to 2015Q3.