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Abstract

This paper tests the extent to which credit market shocks affect different quantiles in the housing price distribution. We use the new “recentered influence function” methodology to recover the unconditional distribution of housing prices in response to (1) unexpected monetary policy decisions and (2) changes in credit supply. We find that tight monetary policy leads to an increase in housing prices across most of the distribution, with larger increases for higher-priced homes, resulting in an increase in price dispersion. In contrast, increases in loan volume lead to higher home prices across the entire distribution, with the largest increases for the mid-priced homes. Importantly, we show that the credit supply effect changes during the 2000-2006 “bubble” period, leading to higher prices at the bottom of the distribution. These price effects are large and significant—and can explain much of the change in wealth inequality over time. More generally, they challenge the common assumptions that policies can be properly evaluated by average effects and that housing affordability can be sufficiently summarized by median statistics.

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1 Introduction

The distribution of housing prices, like the distribution of wealth more generally, has widened considerably in the past half century. How much are financial markets to blame? This paper unpacks two key components of housing finance—the federal funds rate targeted by the Federal Reserve and the supply of credit issued by mortgage lenders—and their contribution to this increasing dispersion in the housing market. Using a private database of millions of housing transactions from Zillow, we test how monetary policy and credit supply affect the distribution of house prices in California from 1970 to 2016, controlling for structural housing characteristics and county-level fixed effects. We find that tight monetary policy tends to increase dispersion, benefiting high-priced homes more than low-priced homes. We also find that an increase in mortgage lending tends to boost middle-priced homes more than low- and high-priced homes. As a result, we find that mortgage lending can explain much of the 2000-2006 “bubble” in housing prices, but monetary policy cannot.

Until recently, econometricians have been unable to recover these marginal effects on the unconditional distribution. Previous methodologies revealed the effect of treatment variables within categories, but the resulting change in the entire distribution was not well-understood. For example, conditional quantile regressions can only estimate the effects of a treatment variable within increments of the covariates, and binned OLS regressions only estimate effects within increments of the distribution of the outcome variable. Neither methodology shows how the overall dispersion of the \( Y \) distribution changes (Bento, Gillingham, and Roth 2017). In this paper, we apply the unconditional quantile regression method pioneered by Firpo, Fortin, and Lemieux (2009) to reveal how the distribution of house prices shifts over time in response to monetary policy and credit supply. We focus especially on the 2000-2006 period, when both of these factors were blamed for the “bubble” in housing prices.

Do these factors affect all house prices equally? Or do they affect some quantiles more than others? Who is experiencing these shocks most acutely? We answer these questions in three steps.

First, we estimate the effect of monetary policy shocks on the home price distribution, controlling for hedonic characteristics as well as the lagged natural log of GDP and a quadratic time trend. To identify the causal effect of monetary policy, we use the methodology developed by Romer and Romer (2004) to estimate “unexpected” changes in the federal funds rate from 1970 to 2007. This measure of monetary policy is positive when the Federal Reserve unexpectedly raises interest rates and negative when they unexpectedly decrease. We find that it has a positive association with most of the housing price distribution, suggesting that tight monetary policy tends to increase most home prices. Moreover, this effect increases at higher quantiles, suggesting that tight monetary policy increases dispersion in the distribution as higher-priced homes appreciate more. The effect only appears to be negative for the very bottom of the distribution where potential homeowners are the most financially constrained. These effects hold when we add county-level fixed effects.

To address questions about the recent boom-and-bust more directly, we run a specification with interaction effects for each decade. In the 2000s, the effect was positive for most of the housing price distribution. This evidence suggests that, contrary to critics’ claims, artificially low interest
rates were not to blame for the “bubble” in housing prices at that time. In the 1970s and 1980s, the effect was positive but approximately equal across most of the distribution. In fact, tight monetary policy may have compressed much of the distribution during this time. In the 1990s, the effect was negative.

Second, we estimate the effect of credit supply on the home price distribution. By credit supply, we mean the loan volume reported under the Home Mortgage Disclosure Act (HMDA). Although it is not exogenous, lagged HMDA data have predictive “Granger causality.” Again, we control for hedonic characteristics, the lagged log of GDP, and a time trend. We run two specifications of this model: one with state-level loan volume, and one with Census tract-level loan volume. In all estimations from 1990 to 2015, credit supply has a positive effect across the distribution, but it has a stronger effect on the middle-priced homes than the low-priced homes. Its effect on the high-priced homes is more sensitive to the specification. To infer causality, we use the new methodology developed by Greenstone, Mas, and Nguyen (2014) to create a plausibly exogenous instrumental variable for lending. This IV confirms that mortgage lending leads to larger price increases for middle-priced homes than for low- or high-priced homes.

Again, we break this effect down by subperiod. In the 1990s, or “pre-bubble” period, the results from the full estimation hold, with the middle-priced homes experiencing a bigger increase from credit supply than the low end. From 2000 to 2006, the “bubble” period, however, the story inverts: Now, the low-priced homes are receiving the biggest boost, consistent with researchers like Mian and Sufi (2009) who pinpoint the subprime mortgage boom as a key culprit. In the “bust” (2007-2010) and “recovery” (2011-2015) periods, the effect turns negative, suggesting that credit supply has not been beneficial to housing values since the Great Recession began.

Finally, we convert these estimated effects into economic magnitudes and simulate their overall effect on the wealth gap across the distribution of U.S. households. We find that these effects are sizable and can explain a nontrivial portion of the increase in wealth inequality in recent decades.

These findings contribute to the long literature in urban economics that explores the differences in prices across the distribution. Gyourko and Tracy (1999) set the stage for this research by computing quantile indices over time, both in real terms and in constant-quality terms, to understand why housing has become unaffordable for owners at the bottom of the distribution. They primarily focus on structural characteristics of the building itself. Similarly, McMillen (2008) runs hedonic regressions in Chicago and shows that home price appreciation was higher for more expensive homes from 1995 to 2005, largely due to increases in the regression coefficients, not increases in the hedonic characteristics themselves. This paper builds on these findings using a longer time period, a larger geography, and a newer different methodology. The most extensive exploration of housing price variation comes from Bogin, Doerner, and Larson (2016), who use federal housing data to calculate real home price indices at city, county, and ZIP code level from 1975 to 2015. While they are able to show interesting variation across location, they do not illuminate the distributional question.

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1Nicodemo and Raya (2012) and Villar and Raya (2015) apply a similar approach to McMillen’s in multiple cities, though they are a European context (and in the latter case, use less reliable appraisal data).
asked here. Moreover, their data are not representative of the population, as they only include transactions that were financed by a loan purchased or guaranteed by Fannie Mae or Freddie Mac.

These findings also contribute to a newer literature in financial economics that seeks to identify the causes of the housing “bubble” and subsequent financial crisis. Mian and Sufi (2009) initiated this field by showing that credit growth was not correlated with income growth at a ZIP code level, suggesting that price growth was being driven by credit given to subprime borrowers. Adelino, Schoar, and Severino (2016) challenge this result by showing that it does not hold at an individual loan level. They argue, instead, that credit growth was most prevalent at the middle-income level. Mian and Sufi (2017) respond by suggesting that individual loans were contaminated by income overstatement fraud, making income-credit comparisons unreliable at a loan level. Foote, Loewenstein, and Willen (2016) challenge their underlying premise of using the number of loans by showing that the total dollar value of loans did not change its distribution during this period. To our knowledge, however, none of these authors have explicitly made their case based on the effect of credit markets on housing prices, which is the outcome that they are all ultimately trying to understand. Landvoigt, Piazzesi, and Schneider (2015) are one exception, as they show that low-priced homes in 2000 appreciated the most between 2000 and 2005. Their findings are limited, however, by the facts that (1) they are only studying San Diego, (2) they are only looking at properties that transacted more than once, and (3) they are looking at a very short and unusual time period, masking any long-term context in how housing markets traditionally operate that would be necessary to determine whether the “bubble” period was actually different or part of a secular trend in housing price dispersion. This paper improves on all three fronts.

The paper is organized as follows. Section II discusses the tension in the literature that leads to contradictory theoretical predictions for the effects of capital markets on the housing price distribution. Section III describes the data we will use to test these predictions. Section IV derives the new methodology that allows us to estimate these marginal effects on the entire distribution of housing prices over time. Section V presents our empirical results, and Section VI uses these reduced-form results to simulate the impacts of credit market shocks on the housing price distribution. Section VII concludes.

2 Theoretical Predictions

It is useful to categorize the theories of housing cycles based on the fundamental formula that governs housing value, the user cost of owner-occupied housing:

\[ C = [(1 - t)(i + h) + d - g]v, \tag{1} \]

where \( t \) is the income tax rate of the homeowner, \( i \) denotes the interest rate, \( h \) refers to the property tax rate, \( d \) accounts for depreciation, \( g \) is the annual growth rate of the property’s value, and \( v \) is the purchase price per unit of housing. Assuming \( h \) do not change in the short run, a cycle in property
value must occur via either the interest rate and/or the expectation of changing capital gains. This paper focuses on the interest rate side of the equation, which we can model as a function of the short-term risk-free rate of borrowing and the risk premium, or “spread,” that compensates the lender for prepayment and default risk.

\[ i = r_f + \beta, \tag{2} \]

where \( r_f \) is set by the Federal Reserve through its federal funds rate target and \( \beta \) is determined in equilibrium by the supply of and demand for credit. Because we are interested here in the effect of credit markets on asset markets and not vice versa, we will confine our investigation to the supply side.

### 2.1 Monetary Policy

The Federal Reserve has been a target for business cycle researchers as long as it has existed. Friedman and Schwarz (1963) made the most influential critique of Fed behavior, showing that the worst contractions in the Great Depression followed almost immediately after the Fed raised the discount rate or reserve requirements. In each case, the money supply shrunk significantly. Had it continued to grow at a consistent rate, they argued, the economy would have continued on its upward trend. From this argument, they extrapolated that consistent money supply growth was the key to stable economic growth.

It “would be difficult to overstate” the influence of this prescription, said Bernanke (2002) on Friedman’s 90th birthday. Taken literally, it was probably too simplistic. The money supply only matters insomuch as it generates consumption and investment, and the historical record suggests that that relationship—i.e. the “velocity” of money—varies over time (Tobin 1965). The sentiment, however, inspired a generation of monetary economists to specify a monetary policy that would maintain stable growth without accelerating inflation. The most famous attempt was the “Taylor rule,” proposed in 1993 as a way to calculate the optimal interest rate \( i_t \), using the equilibrium real interest rate \( r_t^* \), the rate of inflation \( p_t \), the desired rate of inflation \( p_t^* \), and the logarithms of real GDP \( y_t \) and potential GDP \( \bar{y}_t \):

\[ i_t = p_t + r_t^* + a_p(p_t - p_t^*) + a_y(y_t - \bar{y}_t). \tag{3} \]

Using the parameters that fit the historically stable period known as the “Great Moderation,” Taylor (2009) shows that the Fed deviated from his rule in the expansion that preceded the Great Recession. He then regresses interest rates on housing starts and uses the coefficient to simulate what the housing cycle would have looked like if the Fed had followed the Taylor rule—and critically, if the relationship had remained the same over time, i.e. if the coefficient were time-invariant. It is

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2Hendershott and Slemrod (1983) and Poterba (1991) originally developed this formula.

3This is a standard general asset pricing formula that serves useful in a variety of housing finance models. See, for example, Campbell and Cocco (2015).
important to highlight this assumption because we have known since Lucas (1976) that economic relationships tend to change significantly over time, such that a given policy might have drastically different effects under one set of parameters than another. We will return to this question later, as there is ample reason to believe that the underlying parameters did change significantly during this time period.

Bernanke (2015) further criticizes this simulation on two grounds. First, the Fed has not followed the Taylor rule for most of the years since Taylor proposed it, and the results have not been nearly as negative as Taylor would have us believe. Second, the original Taylor rule uses the GDP deflator as its measure of inflation, whereas the Fed uses core PCE inflation, which has been shown to be a more stable and reliable guide to the future path of consumer prices. Additionally, inflation tends to be more stable than economic output, which means the Fed should not use the same coefficient for both components of the equation. Making these adjustments, Bernanke finds that the Fed has followed the Taylor rule almost exactly—until the end of the Great Recession when it prescribed negative interest rates.

The problem with Taylor’s critique runs deeper, however, than the econometrics. First, consider Figure 1. This graph extends housing starts back to 1959, when the Fed starts tracking them. Does the so-called “boom” in Taylor’s story seem so extreme in this context? On the contrary, it appears to be the natural, linear continuation of growth that began in 1991, and it appears consistent with the height of cycles in 1973, 1978, and 1984. These swings do not line up with the history of loose monetary policy as most economists would tell it. Why the divergence? Perhaps it is because Taylor has chosen an unusual measure of the housing cycle. Most financial economists would use a definition that includes housing prices; and most urban economists would argue that prices tend to appreciate the most when housing starts do not grow rapidly—that is, when housing supply is inelastic, funneling most of the demand into prices.

What, then, can we say about the effect of monetary policy on housing prices? Jordà, Schularick, and Taylor (2015) use a novel instrumental variable strategy to identify the causal effect, which has been notoriously difficult to pin down for the reasons Lucas (1976) elucidated. They look at episodes when countries with a fixed exchange rate experienced a loosening of monetary policy because they were pegged to the currency of a foreign central bank, whose actions were plausibly exogenous to any country other than their own. In such cases, monetary loosening had a significant positive effect on house prices. Of course, this finding does not apply directly to the United States, which has never pegged the dollar to a foreign currency, but it suggests that low interest rates do matter. Whether they have historically been significant drivers of U.S. housing prices—where Bernanke’s evidence suggests that they were not artificially low in the latest cycle—remains a fruitful question for empirical research.

4For example, the Fed consistently undershot the Taylor rule throughout most of the 1990s. If the rule ought to be followed precisely, then the Fed was generating excess unemployment throughout most of this period. This conclusion seems hard to square with the booming economy of the 1990s that most economists consider to have approximated full employment. Similarly, the Fed consistently undershot the Taylor rule during the Great Recession, yet no housing bubble formed at that time.
Theoretically, the effect of monetary policy on housing prices is even less clear. Bernanke and Gertler (1995) famously argued that housing plays an important role in the transmission of monetary policy via the credit channel. Higher interest rates increase the burden of mortgage payment, thus depressing the demand for housing. An alternate possibility exists, however; what if the higher interest rates signal to the market that the economy is improving, triggering an increase in inflation expectations? Such a transmission mechanism could have the opposite effect, increasing housing prices as well as consumer prices more generally. This effect would contradict most of mainstream macroeconomic theory, but it is not so far-fetched. A new strand of the literature, led by Kocherlakota (2016), Cochrane (2017), and Smitt-Grohé and Uribe (2017), suggests a “neo-Fisherian” relationship, building on Fisher’s (1930) classic equation,

\[ i_t = r_t + E_t \pi_{t+1}, \]

(4)

where \( i_t \) is the nominal interest rate at time \( t \), \( r_t \) is the real interest rate, and \( E_t \pi_{t+1} \) is the expected rate of inflation. If we make the (critical) assumption that \( r_t \) reaches a stable equilibrium in the long run, then a change in \( i_t \) by the Federal Reserve translates directly into a change in \( E_t \pi_{t+1} \). To use the example that motivated this literature: If inflation is stable at the zero lower bound, perhaps it reflects a stability in real interest rates, in which case an increase in the federal funds rate will lead to an increase, rather than a decrease, in inflation—and by extension, in housing prices.

Though this theory is relatively new, it has much empirical evidence to support it. While vector autoregression (VAR) models have consistently shown that monetary policy shocks have a negative effect on output, they tend to show a zero-to-positive effect on inflation in the short and medium run. It is only after six quarters that the effect seems to turn negative, a phenomenon known as the “price puzzle” in the literature.\(^5\) When Vargas-Silva (2008) applies the VAR methodology to housing starts and residential investment, he too finds weak evidence. Though these variables respond negatively to contractionary monetary policy, the results are sensitive to the horizon choice, and as in the case of Taylor (2009), they do not map directly onto housing prices. This latter effect is very much an open question for empirical investigation. When the Federal Reserve raises interest rates, which matters more: the financial burden on borrowers or the psychological boost of confidence in future growth?

### 2.2 Credit Supply

Whether or not monetary policy is an important driver, there is strong evidence suggesting that credit supply matters. The latest housing cycle was characterized by a drastic rise and fall in mortgage debt, relative to the size of the economy (Emmons and Noeth 2013). While this correlation alone does not establish causality—it is certainly possible that credit rose to keep up with price

\(^5\)Even in the extreme case that Friedman and Schwartz identified, recent empirical work by Amir-Ahmadi and Ritschl (2009) and Amaral and MacGee (2017) suggest that monetary policy played a minor role at best in the Great Depression.
expectations—it merits the large literature that has developed to explain it.

Mian and Sufi (2009) are among the first to show the linkage between debt, house prices, and subsequent foreclosures in the most recent cycle. Using Home Mortgage Disclosure Act (HMDA) data, they found that ZIP codes with a higher share of subprime borrowers (with a credit score under 660) experienced faster credit growth despite exhibiting lower income growth than ZIP codes with a higher share of prime borrowers. This negative correlation only appears in their data from 2002 to 2005, a truly extraordinary period in the history of mortgage lending. This evidence is only suggestive, but what it suggests comports with many anecdotal accounts of the lending industry at the time: namely, that underwriting standards were loosened, loans were increasingly supplied to less creditworthy borrowers, and these borrowers were predictably unable to keep up with the payments, resulting in the higher foreclosure rates that Mian and Sufi observe in these ZIP codes.

Adelino, Schoar, and Severino (2016) dispute this account. They augment Mian and Sufi’s HMDA data with income data from the IRS, as well as a 5% random sample of all loans from Lender Processing Services. As a result, they can look at individual borrowers, not just ZIP codes. They find that lenders actually issued more debt to middle- and high-income borrowers than low-income borrowers from 2002 to 2006, and these prime borrowers then accounted for a growing share of mortgage debt defaulted on, relative to subprime borrowers. The key difference here is that they are measuring total dollar value of debt, while Mian and Sufi are measuring the number of originations. The lenders seem to be originating an increasing number of mortgages in low-income communities, but the average mortgage size is small enough that the middle- and high-income borrowers still account for the majority of the capital.

Adelino, Schoar, and Severino then make a surprising leap of logic. They conclude that home price appreciation must have been driven by the expectation of rising prices and not by an increase in credit supply. While their evidence changes the nature of our understanding about credit growth, it does not prove anything about causation. For that, we will have to turn to research with more careful identification strategies.

As early as the 1920s, we have evidence that the credit supply can drive real estate prices. Rajan and Ramcharan (2015) use differences in bank regulations to identify which regions had greater exogenous access to credit during the cycle in agricultural commodity prices from 1917 to 1920. Because some areas were more dependent on these commodities than others, they have a second difference that allows them to tease out the channels through which credit availability operates. They find that credit availability significantly increased land prices, especially but not exclusively in counties that were more exposed to the booming commodities. Similarly, they find that these same regions experienced the most bank failures when prices declined, with repercussions that lasted for many years after.

Favara and Imbs (2015) apply a similar methodology to the latest housing cycle, using state regulations as exogenous sources of variation from 1994 to 2005, when they were allowed to limit

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6 As mentioned above, we will not be addressing expectations in this paper, as they are important and complicated enough to deserve separate treatment.
interstate branching. When states removed these restrictions, commercial banks issued more loans, a greater volume of debt, and riskier loans, as measured by debt-to-income. The entire change seems to have come from out-of-state banks that opened new branches, underscoring that it truly was caused by the deregulation. This increase in lending led directly to greater activity in the housing market. In areas where it was easier to build (i.e. more elastic housing supply), the housing stock grew more after deregulation. In areas with less elastic supply, prices increased. As a result, the authors find that deregulation can account for one-half to two-thirds of the increase in lending and one-third to one-half of the increase in prices. This is one of the most robust findings to date.

3 Data: The Housing Price Distribution

Until recently, it would have been difficult, if not impossible, to estimate the effect of credit market shocks on the entire distribution of housing prices because there was no comprehensive database of housing transactions over an extended period of time. Now, such databases exist. This paper uses a dataset of publicly recorded single-family home transactions throughout the state of California from 1970 to 2017. For each observation, the dataset contains variables for transaction value ($), transaction date, number of bedrooms, number of bathrooms, building size (square feet), lot size (square feet), latitude, longitude, address, city, and ZIP code.

We truncated the data by dropping all transactions with prices of $0, which typically signify transactions that are not arm’s length and therefore do not represent true market value. We also dropped all transactions with prices greater than $100 million for two reasons: their extreme values make them difficult to compare to the rest of the market, and such high numbers might reflect miscoding. In total, the truncated dataset contained 15,740,808 observations.

We need richer geographic detail in order to conduct some of the specifications of our models. For example, we run specifications with county fixed effects to control for local heterogeneity that affected housing prices in a way that might bias the coefficients from capital market shocks. We also run specifications with loan volume at the Census tract level, as the data discussed below are identified at such a local level. As a result, we used the geocoding of the housing price data to match them with Census tracts and counties using GIS mapping. 77% of the observations were able to be matched. When running regressions with geographic controls, we therefore have $N \approx 12.1$ million.

3.1 Home Price Quantiles Over Time

Figure 2 shows the mean, as well as the 10th, 25th, 50th, 75th, and 90th quantile, of housing prices in each year that they transacted from 1970 to 2016. In 1970, single-family homes in California sold for $25,988 on average. Almost half a century later, in 2016, the average was $429,489, just shy of the previous year’s peak of $477,619. In between those years, the average home price followed two distinct boom-and-bust cycles and several smaller ups-and-downs along the way, as shown by the
dotted black line. As shown by the solid lines, however, the average is certainly not the experience of the majority. In fact, it roughly follows the 75th percentile, suggesting that there is significant skewness, with a small minority of very high-priced homes pushing the mean far above the median. Because not every house transacts in every year, we cannot literally conclude that only 25% of houses experienced the average appreciation or better, but we can say with confidence that any studies reporting “average” effects are in danger of failing to capture most of the distribution. We can also conclude that our regressions should use the natural logarithm of prices to reduce some of the skewness and bring the distribution closer to normality.

The severity of the two boom-and-bust cycles is striking, as is the difference in experience across the distribution. From 1970 to 2000, the 10th percentile experienced very little appreciation, and then after the unprecedented boom from 2000 to 2006, prices swiftly plummeted back almost to their original levels. By contrast, the 90th percentile lost less than a third of its value—and unlike the bottom half of the distribution, it has already surged past its original peak.

We might be concerned that inflation is driving much of this variation over time. Figure 3 reports inflation-adjusted prices in constant 1970 dollars using the core PCE index to deflate the series in Figure 2. The story remains the same. If anything, the boom-and-bust periods appear even more extreme. Why has this distribution increased in dispersion? And why were some homes more sensitive to the cycle than others? To answer those questions, it is natural to begin with the characteristics of the homes themselves.

3.2 Home Characteristics Over Time

According to economic theory, homes with more valuable features should be worth more money, holding all else constant. If this theory is correct—and we have ample evidence that it is—we might expect that changes in the homes themselves might explain some of the changes in the price distribution. For example, if the most expensive 10% of houses have become larger relative to the rest of the market, they should become more expensive, explaining part of their outsized appreciation in Figures 2 and 3.

This does not appear to be the case. According to Figure 4, the 90th percentile of the home size distribution increased slightly from the mid-1990s to the mid-2000s, but it is nowhere near the increase in prices. Moreover, it does not appear to have increased before or after that period, contradicting the experience of the price distribution. Further, the mean building size is well below the 75th percentile and not too far from the median, suggesting that the skewness of the size distribution is far less than that of the price distribution.

Lot size tells a very different story, but it also does not appear to explain the change in prices. First, lot size is far more skewed than prices, so much so that Figure 5 makes it difficult to see 90% of the distribution relative to the mean. To get a better sense of the rest of the distribution, Figure 6 drops the mean. Even within this 90-10 range, there is significant skewness. Large plots seem to be getting larger over time, though the timing does not coincide with the “bubble” growth period at all, and the rest of the distribution has remained roughly the same during the entire
period. The bottom of the distribution has actually fallen, suggesting that small lots are getting even smaller.

Bedrooms and bathrooms do not merit a graph because they have not changed at all. Homes have roughly the same allocation of these types of rooms as they have had for several decades. This evidence is consistent with McMillen (2008), who found that a change in structural characteristics did not explain most of the appreciation or increased dispersion in home prices in Chicago. This conclusion suggests that other factors, such as credit market shocks, may explain some of these changes over time.

4 Empirical Approach

Our goal is to estimate the impact of marginal changes in monetary policy and credit supply on the unconditional distribution of housing prices. Let us begin with familiar representations for our outcome variable (housing prices), $Y$, and explanatory variables (monetary policy and credit supply), $X$. We want to estimate the impact of marginal changes in $X$ on the unconditional distribution of $Y$. We will estimate this effect at each quantile of $Y$ individually. Let us represent each quantile with $\tau$. Since $X$ and $Y$ are observed together, we assume they have a joint cumulative distribution $F_{Y,X}$. First, we need the unconditional distribution function of $Y$,

$$F_Y(y) = \int F_{Y|X}(y | X = x) \, dF_X(x), \quad (5)$$

which sums over all the conditional distribution functions,

$$F_{Y|X}(y | X = x) = \Pr[Y > y | X = x]. \quad (6)$$

Second, we need a method to estimate the impact of infinitesimal (marginal) changes on nonparametric statistics, such as quantiles, of a function. This method is called the “influence function.” In this section, I describe the basic theory behind the influence function and the methodology developed by Firpo, Fortin, and Lemieux (2009) to use it in regression analysis. Then, I explain how I apply this type of regression to quantiles of the housing price distribution with respect to changes in credit markets.

4.1 The Influence Function

Influence functions hail from the field of robust statistics, where they are used to understand the stability of statistical procedures (Hampel 2001). They begin with a statistical functional, which is a function of the distribution function itself,

$$\theta = T(F_Y). \quad (7)$$
For example, we are interested in the quantile functional,

\[ T(F_Y) = F_Y^{-1}(\tau). \] (8)

We want to infer the marginal effect of a change in the distribution of \( X \) on this functional. Let us denote \( G_Y(y) \) as the counterfactual distribution function of \( Y \) due to this change in the distribution of \( X \),

\[ G_Y(y) = \int F_{Y\mid X}(y \mid X = x) \, dG_X(x). \] (9)

Fortunately, the formula for the Gâteaux derivative yields the rate of change of the functional \( T \) from a small amount of contamination \( \epsilon \) that moves the distribution function \( F_Y \) in the direction of \( G_Y \),

\[ L_{T,F_Y}(G_Y) = \lim_{\epsilon \to 0} \left[ \frac{T\{(1 - \epsilon)F_Y + \epsilon G_Y\} - T(F_Y)}{\epsilon} \right]. \] (10)

The trick to estimating the influence function is to pick one \( y \) at a time out of the distribution of \( Y \). In other words, we use a probability measure \( \delta_y \) that assigns the point mass 1 to \( y \),

\[ \delta_y(u) = \begin{cases} 
0 & \text{if } u < y \\
1 & \text{if } u \geq y
\end{cases}, \] (11)

which finally yields the equation of the influence function,

\[ IF_{T,F_Y}(y) = \lim_{\epsilon \to 0} \left[ \frac{T\{(1 - \epsilon)F_Y + \epsilon \delta_y\} - T(F_Y)}{\epsilon} \right]. \] (12)

It is worth pausing to appreciate why Hampel (1974) first proposed this function for robust statistics. It is, in his words, “essentially the first derivative of” the functional \( T \) at the distribution \( F_Y \). As such, it tells us how the statistic in question changes if we add an additional observation at \( y \). The “influence” of this “contamination” reveals the stability of the estimate \( T(\hat{F}_Y) \) for that statistic based on our sample.

The influence function only reveals this influence at \( y \), however. It is, in a sense, a partial derivative. To reveal how the functional \( T \) changes, we have to sum over these infinite partial derivatives,

\[ T(G_Y) - T(F_Y) = \int IF_{T,F_Y}(y) \, dG_Y(y). \] (13)

The influence function therefore acts like a residual between the original functional and the new, counterfactual functional that we are trying to estimate (Borah and Basu 2013). Our goal, remember, is to run a regression of this new functional on \( X \), and therefore we need to add \( T(F_Y) \) back to the influence function to create a “recentered” influence function (RIF),

\[ RIF_{T,F_Y}(y) = T(F_Y) + IF_{T,F_Y}(y), \] (14)
which can now serve as the dependent variable in our regression.

4.2 The RIF Regression

What makes RIFs well suited for regression analysis? It is the fact that their conditional expectation with respect to the distribution of $X$ is the exact functional we want to use as our outcome variable,

$$T(F_Y) = \int E[RIF_{T,F_Y}(Y) \mid X = x] \ dF_X(x). \quad (15)$$

This convenient fact is what motivates Firpo, Fortin, and Lemieux (2009) to create the RIF and to use it as the dependent variable in an ordinary least squares (OLS) regression, yielding the “unconditional partial effect” of $X$ on $T(F_Y)$,

$$\beta_T = \int \frac{dE[RIF_{T}(Y) \mid X = x]}{dx} \ dF(x). \quad (16)$$

We want to estimate this effect for the $\tau$th quantile, $T(F_Y) = q_\tau$. First, we combine equations 5 and 8 to create the IF for $q_\tau$,

$$IF_{q_\tau}(y) = \frac{\tau - 1 \{y \leq q_\tau\}}{f_Y(q_\tau)}, \quad (17)$$

where $f_Y(q_\tau)$ is the density of $Y$ at $q_\tau$. Next, we insert the IF into equation 14 to calculate the RIF,

$$RIF_{q_\tau}(y) = q_\tau + \frac{\tau - 1 \{y \leq q_\tau\}}{f_Y(q_\tau)}. \quad (18)$$

Finally, we take the expectation, conditional on the distribution of $X$,

$$E[RIF_{q_\tau}(Y) \mid X = x] = q_\tau + \frac{\tau - \Pr[Y > q_\tau \mid X = x]}{f_Y(q_\tau)}. \quad (19)$$

Now, we have a dependent variable for our RIF-OLS regression, which we will refer to as an “unconditional quantile regression,” following Firpo, Fortin, and Lemieux (2009). Plugging this formula into equation 16 yields the “unconditional quantile partial effect”,

$$\beta_\tau = f_Y^{-1}(q_\tau) \int \frac{d\Pr[Y > q_\tau \mid X = x]}{dx} \ dF_X(x), \quad (20)$$

which is the effect of $X$ on the $\tau$th quantile of the unconditional distribution of $Y$, our ultimate goal.

4.3 Estimating the Unconditional Quantile Partial Effect of Monetary Policy

Now that we have a methodology with an appropriate outcome variable, we need a treatment variable for monetary policy shocks that is plausibly exogenous. The most direct measure is the federal funds rate that the Federal Reserve targets with its open market operations. As Figure 7
shows, however, this measure is endogenous to the macroeconomy—and by extension, the housing market—tending to rise in good times and fall in bad times as a reaction to good and bad news, respectively. This positive correlation would fail to capture any countercyclical effect that the federal funds rate may have on the economy. Even a lagged federal funds rate may fail to capture the effect of monetary policy shocks, as the Fed may anticipate future movements in the economy and move their target accordingly.

Romer and Romer (2004) resolve these endogeneity issues with a new measure of monetary policy shocks that controls for the Fed’s forecasts and the market’s expectations. The resulting variable is plausibly exogenous because it is not made in response to any observable macroeconomic trends and therefore it is truly unexpected. Specifically, they estimate an OLS regression using the change in the federal funds rate target, $\Delta f f_m$, after each Federal Open Market Committee (FOMC) meeting, $m$,

$$ \Delta f f_m = \alpha + \beta f f b_m + \sum_{i=-1}^{2} \gamma_i \Delta \tilde{y}_{m,i} + \sum_{i=-1}^{2} \lambda_i (\Delta \tilde{y}_{m,i} - \Delta \tilde{y}_{m-1,i}) + \sum_{i=-1}^{2} \varphi_i \tilde{\pi}_{m,i} + \sum_{i=-1}^{2} \theta_i (\tilde{\pi}_{m,i} - \tilde{\pi}_{m-1,i}) + \rho \tilde{u}_{m,0} + \epsilon_m, \tag{21} $$

with $f f b_m$ as the intended federal funds rate target before the meeting, and with $\tilde{y}$, $\tilde{\pi}$, and $\tilde{u}$ representing forecasted output, inflation, and unemployment, respectively. Romer and Romer infer $f f b_m$ by reading the Record of Policy Actions of the Federal Open Market Committee, and they use the Fed’s own forecasts for $\tilde{y}$, $\tilde{\pi}$, and $\tilde{u}$ from the “Greenbook” prepared by the Fed’s staff before each meeting\textsuperscript{8}. The residual $\epsilon_m$ is their new monetary policy shock variable, as it represents the amount of $\Delta f f_m$ that could not have been predicted based on the Fed’s own prior intentions and forecasts. It is truly an unexpected shock to the market. We will therefore use it as our treatment variable to estimate the effect of monetary policy shocks on the housing price distribution.

The original Romer and Romer series covers the period from 1969 to 1996. We replicate their results and extend them to 2007, giving us 39 years for our treatment variable. Figure\textsuperscript{9} shows the time series at a monthly frequency. It appears to have become slightly less volatile after the early 1980s, consistent with the “Great Moderation” narrative in the macroeconomic literature. Also consistent with widely accepted history of this period, it shows the “Volcker disinflation” of 1980-82 to be an extraordinary episode, with large unexpected shocks designed to change the expectations embedded in the Phillips curve. While certainly not definitive evidence, the later years appear to register consistently negative, as Taylor (2009) alleges. This paper will determine whether these negative shocks translated into unusually high house price growth.

Compare these shocks to the actual change in the federal funds rate, shown in Figure\textsuperscript{9}. From this measure, it is difficult to identify the period that Taylor refers to, aside from the fact that it is

\textsuperscript{8}They find these forecasts to be superior to private forecasts, as well as forecasts made by individual FOMC members.

\textsuperscript{9}In months where there was more than one FOMC decision, the residuals are summed over the month.
unusually static. It would be difficult to judge it without the context of the market’s expectations. The story of the Great Moderation and the Volcker disinflation, in contrast, are as clear in this graph as they were in the last. It may not be clear from this graph, however, that the Fed tended to surprise on the positive side more often than the negative during the Great Moderation, keeping rates higher than expected, even when the actual rate was flat or falling.

Romer and Romer find that this new measure is a significant predictor of output and prices. Consistent with standard New Keynesian theory, an unexpected increase in interest rates has a negative effect that lasts at least two years—and is sizably larger than the effect estimated with the actual change in the federal funds rate. Their results suggest that previous empirical studies may have had difficulty revealing the predicted relationship because they were confounded by the omitted variable of the market’s expectations. The robust association between this measure of monetary policy shocks and economic theory gives us confidence to use it as a plausibly exogenous treatment variable.

With this treatment variable, we can finally estimate an unconditional quantile regression,

\[
E[RIF_{q,\tau}(\ln p_{i,t}) \mid M_{t-1}, X_{i,t}] = \beta_{\tau} M_{t-1} + X'_{i,t} \gamma_{\tau},
\]

with \( M \) as the “exogenous” monetary policy shock modeled after Romer and Romer (2004) and \( X \) as a matrix of controls to eliminate omitted variable bias, including hedonic characteristics such as the number of bedrooms, bathrooms, building size, and lot size, as well as the lagged natural log of GDP and a quadratic time trend. This specification follows Bento, Gillingham, and Roth (2017) in treating the RIF regression like a time series relationship where the treatment affects all individuals in the sample, thus precluding the possibility of a difference-in-difference approach with a control group. We aggregate the monetary policy shocks by summing over each year to estimate \( t \) at an annual frequency, and we test the robustness of our results by summing over each quarter. It is well known that housing markets take longer than stock or bond markets to incorporate news into valuations, making it unlikely that a monthly frequency is appropriate (Case and Shiller 1989, Case and Shiller 1990).

### 4.4 Estimating the Unconditional Quantile Partial Effect of Credit Supply

Unlike monetary policy, credit supply does not have a readily exogenous component that has been identified by the literature. Some studies have used state-level policies as exogenous changes in availability of credit, but these policies do not allow us to identify causality within one state where there is no control group. We will therefore use a similar time series approach as we did for monetary policy. We will also attempt to predict loan volume with a shift-share approach similar to the method developed by Bartik (1991) to predict housing demand. We will not be able to estimate the local average treatment effect, however, because an instrumental variable version of
unconditional quantile regressions does not currently exist for continuous variables. Instead, we will estimate the reduced form effect of the “instrument” directly on the quantiles of the housing price distribution.

The Home Mortgage Disclosure Act of 1975 (HMDA) empowered the government to collect the data we need to estimate this effect. It required depository institution (and subsidiaries in which they held a majority stake) to create a “loan/application register” in which they record each mortgage application and report it to the Federal Reserve. Institutions were exempt if they were smaller than an asset threshold set by the Fed. Over the years, the rules were amended to cover nondepository institutions and to raise the asset threshold. In 2016, for example, depository institutions were exempt if they had less than $44 million in assets, and nondepository institutions were exempt if they had less than $10 million in assets or originated less than 100 home purchase loans.

Figure shows the total mortgage volume in California from 1990 to 2016, both in dollars and in number of loans originated. The most striking feature is the so-called “bubble period.” Loan originations spike at exponential rates from 2000 to 2003 and then remain at that unusually high level through 2005. The unusual nature of this period is far more apparent in this graph than in any of the monetary policy graphs. Also interesting is the fact that loan originations have been higher in the post-recession era than the pre-recession era, suggesting that the market for home lending has not been persistently squelched by the Great Recession.

With this time series as our treatment variable, we can estimate an unconditional quantile regression following the same approach as we used for monetary policy,

\[ E[RIF_{q,t} | \ln p_{i,t}, \ln L_{t-1}, X_{i,t}] = \beta_\tau \ln L_{t-1} + X'_{i,t} \gamma_\tau, \quad (23) \]

where \( L \) is the loan volume for the state of California from the HMDA dataset and \( X \) is the same matrix of controls as we used in equation. The lag of the treatment variable establishes causality in the predictive Granger (1969) sense, but it does not elucidate the underlying mechanism. For that, we need a more plausibly exogenous instrument.

Greenstone, Mas, and Nguyen (2014) create a county-level instrument for bank lending to small businesses that we can apply to mortgage lending. They construct this instrument in two steps. First, they regress their lending variable on county and bank fixed effects,

\[ \ln L_{i,j} = c_i + b_j + e_{i,j}, \quad (24) \]

where \( i \) indexes each county and \( j \) indexes each bank. Then, they predict the “lending supply shock” in each county by multiplying each bank’s estimated fixed effects, \( \hat{b}_j \), by their market share.

\footnote{Frolich and Melly (2013) propose a method for binary treatment variables and binary instruments; to date, that is the only known IV approach to unconditional quantile regressions that is well-behaved.}

\footnote{The law was intended to address credit shortages in low-income neighborhoods by allowing the government and the public to observe the the mismatch between lending and needs.}

\footnote{See \url{https://www.ffciec.gov/hmda/history2.htm} for more details on the evolution toward these thresholds.}
at the beginning of the period, $ms_{i,j}$,

$$Z_i = \sum_j (ms_{i,j} \cdot \hat{b}_j) . \tag{25}$$

The resulting instrument, $Z_i$, captures the predicted credit supply in a given county based on how each bank is lending overall, not based on any endogenous economic conditions in that county that might be motivating lending. We can then use this $Z_i$ in place of $L_{t-1}$ in equation 23 to see how the credit supply pressure on each county affects the overall distribution of housing prices.

5 Main Empirical Results

5.1 Basic Hedonic Model

Before we estimate the unconditional quantile partial effects, it would be useful to understand the control variables a little better—and in the process, see how to interpret the output from an unconditional quantile regression. The hedonic pricing model, the workhorse of urban economics, serves this purpose perfectly. It explains the cross-sectional variation in prices across homes using each building’s structural characteristics, revealing how much buyers value different features of the house. Rather than explaining the cross-section on average, however, we will use an unconditional quantile regression to reveal how much different buyers value each feature at different points in the distribution,

$$E[RIF_{q,r}(\ln p_{i,t} | X_{i,t})] = \beta_{0,r} Beds_{i,t} + \beta_{1,r} Baths_{i,t} + \beta_{2,r} BldgSize_{i,t} + \beta_{3,r} LotSize_{i,t}, \tag{26}$$

where, for example, $\beta_{0,r}$ indicates the effect of adding another bedroom, controlling for baths, building size, and lot size, on the price of homes at the $r$th quantile. An intuitive way to visualize this effect is by graphing the coefficient across the quantiles, as we have done in Figure 11. On the x-axis, we have quantiles ranging from the 10th percentile to the 90th in increments of 5. On the y-axis is the estimated coefficients, i.e. the unconditional quantile partial effects, in equation 26.

The graph shows that the coefficient for bedrooms is larger at higher quantiles—that is, as home prices increase. In fact, it switches from positive to negative, meaning low-priced homes become more valuable from adding an additional bedroom, while high-priced homes lose value from the marginal bedroom. It is important to remember that we are controlling for building size. As a result, this effect does not capture an additional bedroom expanding the home, but rather subdividing a home of a given size to create one more bedroom. This graph is consistent with the housing literature, which indicates that poorer households need to subdivide to accommodate more people living in one house, while richer households prefer more space (Myers, Baer, and Choi 1996).

The graph for bathrooms, in contrast, suggests that an additional bathroom is more valuable

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13 We drop everything below the 10th percentile and above the 90th because Bento, Gillingham, and Roth (2017) show that RIF-OLS regressions are not well-behaved at the tails of the distribution.
for high-priced homes. These positive and negative slopes are key to understanding their effect on the dispersion of the distribution. A positive slope suggests that the higher-priced homes are going up in value relative to the lower-priced homes, increasing dispersion across homes, while a negative slope suggests the opposite, compressing the distribution.

The graph for building size is the best example of this interpretation. It is nearly exponentially positive. For more expensive homes, it seems, an additional square foot is increasingly more valuable. This effect is consistent with hedonic theory, which suggests that more valuable materials should result in more expensive homes, and the additional square foot is typically built out of better materials for more expensive homes (Gyourko and Linneman 1993, Gyourko and Tracy 1999). We do not see the same effect in the graph for lot size, which is very close to zero throughout the distribution.

5.2 Monetary Policy

Figure 12 gives the punchline upfront. It shows the unconditional quantile partial effect of monetary policy, as estimated by equation (22). Three things are striking about this graph.

First, the effect is positive for most of the distribution. Most homes increase in price following an unexpected increase in the federal funds rate. This effect is very significant, so much so that it would be nearly impossible to see error bands on this graph, so we leave them out. This finding may be surprising if considered in the context of mainstream theory, which suggests that output and prices should fall when monetary policy is tightened. It is not surprising, however, in the context of the VAR literature, which finds a similar “price puzzle” and the recent theoretical work by Cochrane (2017) showing that strong, outdated assumptions are necessary to justify the expected negative relationship.

Second, the slope of the graph is positive, indicating that the effect is greater for higher-priced homes. In other words, tight monetary policy increases dispersion in the housing price distribution, adding more value to homes that are already more expensive—and by extension, loose monetary policy decreases dispersion, benefitting the cheaper homes more.

Finally, the left tail of the graph is the only negative portion. For the bottom tenth of the distribution, an unexpected increase in the federal funds rate lowers home values. This is consistent with much of the housing finance literature, which shows that the poorest households are the most financially constrained and therefore the most likely to be hurt by tight credit conditions (Di and Liu 2007, Acolin, Bricker, Calem, and Wachter 2016, Bostic and Orlando 2017). When constraints are binding, it appears that a marginal change in the cost of financing is more important than any positive signal it may give to the market as a whole.

As a robustness check, we run the same equation with fixed effects at the county level, and it confirms all of these results. The one caveat is that the standard errors are larger, making the effect statistically indistinguishable from zero for about 40% of the distribution. Table 1 shows these coefficients and standard errors for the 10th, 25th, 50th, 75th, and 90th percentiles. We will use this conservative estimate as our preferred specification in later calculations.
We also run the regressions without different control variables, and we run the regressions at a quarterly level. For all specifications, the results are qualitatively similar. There is no indication of a negative effect until the sixth quarter, again consistent with “price puzzle” findings in the VAR literature.

These findings suggest that Taylor’s (2009) concerns are unfounded. It is not the case that “artificially” low interest rates lead to a bubble in housing prices. On the contrary, they have tended to depress housing prices over the past 47 years. However, that is a long time for one set of coefficients to remain stable. Time-varying coefficients have been shown to matter in a wide variety of economic contexts. It is especially important to control for regime changes in monetary policy, which only started targeting the federal funds rate as its primary lever in the 1980s after the Volcker disinflation. Even Taylor himself does not assert that the Fed followed the Taylor rule in the 1970s. It is therefore important to determine whether the effect of monetary policy shocks might differ by decade, thereby opening the possibility that they did in fact boost housing prices in the 2000s.

We can control for these time-varying differences by interacting the monetary policy shocks with dummy variables for each decade,

$$ E[RIF_q, \ln(p_{i,t})|M_{t-1}, X_{i,t}] = \beta_{r,7} M_{t-1} + \beta_{r,8} D_{1980_a} M_{t-1} + \beta_{r,9} D_{1990_a} M_{t-1} + \beta_{r,0} D_{2000_a} M_{t-1} + X_{i,t}' \gamma $$

such that $\beta_{r,d}$ captures the unconditional quantile partial effect of monetary policy in a given decade. These betas vary widely. In figure Figure 13 for example, we see that the positive effect still holds for most of the distribution, as does the negative effect for the bottom 5%, but the slope is not positive. In fact, the effect seems pretty equal across 80% of the distribution. Given the fact that inequality was not increasing much in the 1970s, but these results suggest that the institutional environment matters. Monetary policy did not seem to affect house price dispersion in this more egalitarian time period.

Figure 13 tells the same story for the 1980s. If anything, it appears that much of the slope is negative. For over half of the distribution, unexpected increases in the federal funds rates are leading to a compression in the housing price distribution. This evidence supports Paul Volcker’s decision to engage in tight monetary policy, perhaps suggesting that inflation was so high that it was beneficial to everyone to get it under control. The story changes significantly in the 1990s. Tight monetary policy had the expected negative effect on most housing prices.

Then in the 2000s, our original story comes back into play. We see a positive effect for most of the distribution, a negative effect for the cheapest homes, and a positive slope increasing dispersion. The only significant difference appears to be the negative effect on a larger portion, approximately 40%, of the population. If Taylor is arguing that low interest rates caused the bubble by pushing up prices on the low end, then he may have a case. But if he is saying that low interest rates

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14 These results are available from the author upon request.
pushed up most housing prices—which is what he seems to be saying—then the evidence in this paper’s findings contradict that hypothesis. The effect of monetary policy differs depending on the institutional environment, but on the whole and especially during the “bubble” period, it does not appear that low interest rates have led to high housing prices.

5.3 Credit Supply

At first glance, the unconditional quantile partial effect for credit supply is very different. Figure 14 shows the results from equation 23 with state-level loan volume as the treatment variable and no fixed effects. When more mortgage debt is supplied to California, it appears that the middle-priced homes are the ones that appreciate the most. This is a comforting graph for policymakers who are trying to “build the middle class” by making affordable financing available for homeownership. When we add county-level fixed effects, the same conclusion holds.

Unlike monetary policy, however, credit supply is not the same treatment for all homes. Some cities and neighborhoods receive more loans than others. As a result, we can run the same regression with HMDA data broken down by Census tract, assigning the tract-level loan volume as the treatment for each home that transacts within that tract. Figure 15 shows that the story changes significantly. The increasing slope still holds for the bottom half of the distribution, suggesting that the middle-priced homes benefit more than the lower-priced homes. The top half of the distribution, however, appears to experience roughly the same effect. It is no longer true that the middle-priced homes benefit more than the higher-priced homes. What accounts for this difference? It may be that the original equation did not capture the full differential effect of the treatment across the distribution. The way loans are distributed geographically may benefit the higher-priced homes, even if they do not experience as much benefit from more mortgage debt overall.

County-level fixed effects add a cautionary note to these explanations, however. The error bands increase at the top of the distribution, and the lower bound starts to look like our original results, bending down for higher-priced homes. It seems we simply cannot draw strong conclusions about how credit supply is affecting the top of the distribution. We have more confidence, however, in our conclusion that the mortgage debt increased the value of middle-priced homes relative to low-priced homes, consistent with conventional wisdom about homeownership in the United States.

In the debate between Mian and Sufi (2009) and Adelino, Schoar, and Severino (2016), these results seem to come down on the latter side. They suggest that mortgage debt was affecting the middle of the distribution more than the low end. They are not specifically addressing the “bubble” period, however, as the treatment effect ranges from 1990 to 2015. To better address this debate, we need to break up the treatment into subperiods as we did for monetary policy,

$$ E[RIF_{q_t}(\ln p_{i,t})|\ln L_{t-1}, X_{i,t}] = \beta_{x,0} \ln L_{t-1} + \beta_{x,1} D_{2000-06} \ln L_{t-1} + \beta_{x,2} D_{2007-10} \ln L_{t-1} $$

$$ + \beta_{x,3} D_{2011-15} \ln L_{t-1} + X_{i,t}' \gamma $$

where our subperiods more closely align with “pre-bubble,” “bubble,” “bust,” and “recovery”

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stages. According to Figure 16, the only subperiod that aligns with our overall results is the “pre-bubble” period. In the “bubble,” the graph flips. Now, it appears that Mian and Sufi are correct: The low-priced homes experienced much higher price appreciation in response to tract-level loan volume, and the slope is negative, suggesting that the subprime boom compressed the distribution as it was intended to do.

The “bust” and “recovery” stages are completely different. First, they are negative. More mortgage debt has led to depressed home values since the Great Recession, and the effect has not dissipated since the housing market has begun to recover. In fact, it only seems to have gotten worse. Second, both graphs are U-shaped, suggesting that the middle-priced homes have been most negatively affected by the debt. These results are both puzzles for future research to unravel.

In Figure 17, we present the same specification using state-level loan volume. The story remains consistent, except the “pre-bubble” experience for the top of the distribution, which experiences less appreciation than the rest of the distribution. In this graph, the negative slope of the “bubble” is especially pronounced, confirming our conclusion that mortgage lending was associated with higher prices for the low-priced homes than for the rest of the distribution.

It is reasonable to wonder whether these results have casual implications, given the endogenous nature of county-level lending. To test this endogeneity, we calculate the Greenstone, Mas, and Nguyen (2014) instrumental variable that removes county-level changes from year-to-year and replaces them with bank fixed effects multiplied by the bank’s market share in the previous period. Figure 18 shows the results when this IV replaces the HMDA variable from our first specification. This instrument tells the same story as the state-level HMDA regression: The middle of the distribution appreciates more than the bottom or the top. While it is difficult to interpret the magnitude of the effect because of the transformation of the variable, all the coefficients are statistically significant at the 0.1% level. We can say with confidence that credit supply shocks have the strongest positive effect on the middle-priced homes.

6 Simulations

How much do these effects matter? Are they economically meaningful? How do they relate to recent trends in inequality more generally? In this section, we use the coefficients from our preferred specifications to simulate the impacts of these credit market shocks on housing prices and wealth across the distribution.

6.1 Price Effects

For monetary policy, we take the estimation for the full time period with county fixed effects as our preferred specification. We want to use a typical monetary policy shock, so we turn to the distribution of annual monetary policy shocks in Figure 19. A 50-basis-point increase in interest rates appears to be common and near the median. Table 3 shows the effect of an unexpected increase in the federal funds rate by one-half percentage point between 2016 and 2017. At the
10th percentile, home prices were $170,000 in 2016. A monetary policy shock of 50 basis points would decrease home prices at the 10th percentile by 0.3%, or $544, to $169,456 in 2017. At the 90th percentile, in contrast, home prices were $1,050,000 in 2016. The same monetary policy shock would increase their home prices by 6.2%, or $64,681, to $1,114,681. The change in the housing price distribution is sizable, with an increase in 90-10 interquantile dispersion by $65,225.

For credit supply, Table 2 shows our preferred specification with state-level loan volume and county fixed effects, as graphed in Figure 14. These coefficients accord most closely with the results of our plausibly exogenous instrument, as shown in Figure 18 which unfortunately are not as easily interpretable in terms of economic magnitude due to the transformation of the variable. Again, turning to the distribution in Figure 20, it appears that 30% is a reasonable delta to use for our simulation. Using these state-level effects, Table 3 shows the effect of an increase in loan volume by 30% between 2016 and 2017. At the 10th percentile, home prices increase by 0.73%, or $1,244, to $171,244 in 2017. At the 90th percentile, home prices similarly increase by 0.52%, or $5,457, to $1,055,457. The change in the housing price distribution is much smaller than it was for a monetary policy shock. The 90-10 interquantile dispersion still increases because equal percentages result in more money at the top of the distribution, but the increase is only $4,213. This may seem to suggest that credit supply is a small contributor to increasing price dispersion, but recall from Figure 10 that loan volume was increasing much faster than the simulated 30% during the “bubble” years. In some years, it increased as much as 100%.

6.2 Wealth Effects

Not all of these value changes are of equal importance to the homeowners across the distribution, however. Most households only own a fraction of their home’s value as equity. An increase in value therefore lowers their loan-to-value ratio and increases their net worth. The loan-to-value ratios in Tables 3 and 4 correspond with the estimates in the Survey of Consumer Finances. As is well known, leverage magnifies losses and gains. For a one-half percentage point monetary policy shock, for example, the homeowner at the 75th percentile experiences a 10.3% increase in equity, while the homeowner at the 90th percentile only experiences a 9.2% increase, despite a larger dollar gain. Similarly, a 30% increase in credit supply increases wealth at the 10th percentile by 2.9% and at the 90th percentile by 0.8%. It is important to remember that the absolute size of the wealth gap still increases, but the incremental gains are more valuable to the lower quantiles, which have both lower baselines and higher marginal utilities.

6.3 A Simulation of the Housing “Bubble”

How well can these models explain the “bubble” in housing prices that occurred from 2000 to 2006? As an example, we will use the monetary policy model, for which the inputs (i.e. the monetary policy variable) are more stable over time than the credit supply model. Specifically, we will use the

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15This version of the paper uses the SCF from the 1990s to correspond with the “bubble” simulation below; future versions will use more recent SCFs as well.
“Bubble” coefficients from equation 27, the results of which appear in Figure 17. These coefficients were positive for most of the distribution, with the highest values for the bottom of the distribution, becoming negative only above the 80th quantile.

Figure 21 compares the simulated housing prices using the coefficients from this model with the actual historical prices that occurred during this time period. Clearly, monetary policy cannot account for the majority of the appreciation, though it does appear that it was pushing in that direction for the top quarter of the distribution in 2004-2006. For the bottom half of the distribution, it may have even been a moderating force. These homes must have appreciated for other reasons.

7 Conclusion

The housing price distribution has changed significantly over time, and credit markets have played a starring role. This paper has shown that tight monetary tends to increase dispersion in the distribution—and to increase housing prices overall. Credit supply, on the other hand, increases the middle of the distribution relative to the top and the bottom. The 2000-2006 period was an outlier, however, with credit supply increasing the bottom of the distribution the most, consistent with the findings of Mian and Sufi (2009). These findings suggest that credit supply played an important role in the “bubble” in housing prices at that time, while there is no evidence that monetary policy was an important factor. On the contrary, monetary policy may have been a moderating factor.

These findings have important policy implications. Low interest rates have been blamed for inflating asset markets, but this evidence does not support that characterization. More work should investigate this relationship, and more caution should be used when criticizing this tilt in monetary policy. For decades, affordable housing advocates have supported increased credit availability as a way to build the middle class. This paper suggests that increased credit supply has indeed had this effect. The type of credit matters, however, as we learned from the subprime boom, which has had negative effects that continue to this day.

From a methodological standpoint, the unconditional quantile regression has proven itself useful in ways that can be applied across the housing literature and program evaluation more generally. Other housing policies, such as land-use regulations or mortgage modification programs, likely have different effects across the distribution as well. Average effects often do not describe the lived experiences of much, if not most, of the population. This paper demonstrates that public policies can be more carefully, richly, and accurately evaluated using quantile methodologies such as RIF regressions. These methodologies reveal that the entire distribution matters in the housing context. Researchers can use this approach to determine the extent to which heterogeneous effects matter in other policy contexts.
Table 1: Effect of Monetary Policy Shocks on Housing Price Distribution

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<td></td>
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<tr>
<td>Bedrooms</td>
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<td>-0.047**</td>
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<tr>
<td>Bathrooms</td>
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<td>0.136***</td>
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<tr>
<td></td>
<td>(4.46)</td>
<td>(4.49)</td>
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<tr>
<td></td>
<td>(3.51)</td>
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<tr>
<td>LotSize</td>
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<td>(0.01)</td>
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<tr>
<td>(R^2)</td>
<td>0.178</td>
<td>0.240</td>
<td>0.334</td>
<td>0.275</td>
<td>0.161</td>
</tr>
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</table>

Notes: All regressions include a quadratic trend and county fixed effects. 

\(t\) statistics in parentheses: * \(p < 0.05\), ** \(p < 0.01\), *** \(p < 0.001\)
Table 2: Effect of State-Level Credit Supply on Housing Price Distribution

<table>
<thead>
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<tbody>
<tr>
<td></td>
<td>Q10</td>
<td>Q25</td>
<td>Q50</td>
<td>Q75</td>
<td>Q90</td>
</tr>
<tr>
<td>$l(\text{LoanVolume})_{t-1}$</td>
<td>0.024***</td>
<td>0.043***</td>
<td>0.029***</td>
<td>0.017***</td>
<td>0.120***</td>
</tr>
<tr>
<td></td>
<td>(5.34)</td>
<td>(6.79)</td>
<td>(10.41)</td>
<td>(9.94)</td>
<td>(6.14)</td>
</tr>
<tr>
<td>$l(\text{GDP})_{t-1}$</td>
<td>2.031***</td>
<td>2.240***</td>
<td>2.639**</td>
<td>1.563***</td>
<td>0.445***</td>
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<tr>
<td></td>
<td>(8.39)</td>
<td>(11.13)</td>
<td>(20.79)</td>
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<td><strong>Bedrooms</strong></td>
<td>0.088**</td>
<td>0.062**</td>
<td>0.027</td>
<td>-0.009</td>
<td>-0.069**</td>
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<tr>
<td></td>
<td>(3.41)</td>
<td>(3.45)</td>
<td>(1.61)</td>
<td>(-0.56)</td>
<td>(-3.52)</td>
</tr>
<tr>
<td><strong>Bathrooms</strong></td>
<td>0.138*</td>
<td>0.110***</td>
<td>0.146***</td>
<td>0.176***</td>
<td>0.271***</td>
</tr>
<tr>
<td></td>
<td>(3.57)</td>
<td>(4.33)</td>
<td>(5.91)</td>
<td>(6.33)</td>
<td>(5.94)</td>
</tr>
<tr>
<td><strong>BuildingSize</strong></td>
<td>0.000*</td>
<td>0.000**</td>
<td>0.000***</td>
<td>0.000***</td>
<td>0.000***</td>
</tr>
<tr>
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<td>(2.44)</td>
<td>(3.15)</td>
<td>(3.33)</td>
<td>(3.86)</td>
<td>(4.58)</td>
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<td><strong>LotSize</strong></td>
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<tr>
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<td>(-1.25)</td>
<td>(-1.22)</td>
<td>(-0.26)</td>
<td>(-0.14)</td>
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<table>
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<tbody>
<tr>
<td>$R^2$</td>
<td>0.072</td>
<td>0.155</td>
<td>0.229</td>
<td>0.183</td>
<td>0.133</td>
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Notes: All regressions include a quadratic trend and county fixed effects. 

$t$ statistics in parentheses: * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 3: Simulation of Monetary Policy Shock on Housing Price Distribution

<table>
<thead>
<tr>
<th></th>
<th>(1)</th>
<th>(2)</th>
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<th>(5)</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Q10</td>
<td>Q25</td>
<td>Q50</td>
<td>Q75</td>
<td>Q90</td>
</tr>
<tr>
<td>$P_{2016}$</td>
<td>170,000</td>
<td>265,000</td>
<td>412,000</td>
<td>650,000</td>
<td>1,050,000</td>
</tr>
<tr>
<td>$\hat{P}_{2017}$</td>
<td>169,456</td>
<td>265,038</td>
<td>423,053</td>
<td>691,605</td>
<td>1,114,681</td>
</tr>
<tr>
<td>$\Delta_{2016-17}$</td>
<td>-544</td>
<td>38</td>
<td>11,053</td>
<td>41,605</td>
<td>64,681</td>
</tr>
<tr>
<td><strong>LTV</strong></td>
<td>1.25</td>
<td>0.80</td>
<td>0.59</td>
<td>0.38</td>
<td>0.33</td>
</tr>
<tr>
<td><strong>%\Delta(Equity)</strong></td>
<td>-1.3%</td>
<td>0.1%</td>
<td>6.5%</td>
<td>10.3%</td>
<td>9.2%</td>
</tr>
</tbody>
</table>

Notes: Simulated effect of one-half percentage point unexpected increase in federal funds rate. Based on coefficients from Table 1.
Table 4: Simulation of Credit Supply Shock on Housing Price Distribution

<table>
<thead>
<tr>
<th></th>
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<th>(2)</th>
<th>(3)</th>
<th>(4)</th>
<th>(5)</th>
</tr>
</thead>
<tbody>
<tr>
<td>$P_{2016}$</td>
<td>170,000</td>
<td>265,000</td>
<td>412,000</td>
<td>650,000</td>
<td>1,050,000</td>
</tr>
<tr>
<td>$\hat{P}_{2017}$</td>
<td>171,244</td>
<td>267,899</td>
<td>417,402</td>
<td>655,746</td>
<td>1,055,457</td>
</tr>
<tr>
<td>$\Delta_{2016-17}$</td>
<td>1,244</td>
<td>2,899</td>
<td>5,402</td>
<td>5,746</td>
<td>5,457</td>
</tr>
<tr>
<td>LTV</td>
<td>1.25</td>
<td>0.80</td>
<td>0.59</td>
<td>0.38</td>
<td>0.33</td>
</tr>
<tr>
<td>$%\Delta(\text{Equity})$</td>
<td>2.9%</td>
<td>5.5%</td>
<td>3.2%</td>
<td>1.4%</td>
<td>0.8%</td>
</tr>
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</table>

Notes: Simulated effect of 30% increase in state-level loan volume. Based on coefficients from Table 2.

Figure 1: Total U.S. Housing Starts

Figure 2: Nominal Single-Family Home Transaction Prices in California, 1970-2016

Notes: Quantiles of the unconditional distribution of single-family home transaction prices in California in each year from 1970 to 2016. p10 = 10th percentile, p25 = 25th percentile, p50 = 50th percentile, p75 = 75th percentile, p90 = 90th percentile, mean = arithmetic average. Data obtained through private restricted-use agreement with Zillow.
Figure 3: Real Single-Family Home Transaction Prices in California, 1970-2016

Figure 4: Single-Family Home Sizes in California, 1970-2016

Notes: Quantiles of the unconditional distribution of building sizes from single-family home transactions in California in each year from 1970 to 2016. p10 = 10th percentile, p25 = 25th percentile, p50 = 50th percentile, p75 = 75th percentile, p90 = 90th percentile, mean = arithmetic average. Data obtained through private restricted-use agreement with Zillow.
Figure 5: Single-Family Lot Sizes in California, 1970-2016

Notes: Quantiles of the unconditional distribution of lot sizes from single-family home transactions in California in each year from 1970 to 2016. p10 = 10th percentile, p25 = 25th percentile, p50 = 50th percentile, p75 = 75th percentile, p90 = 90th percentile, mean = arithmetic average. Data obtained through private restricted-use agreement with Zillow.
Figure 6: Single-Family Lot Sizes in California, 1970-2016

Notes: Quantiles of the unconditional distribution of building sizes from single-family home transactions in California in each year from 1970 to 2016. p10 = 10th percentile, p25 = 25th percentile, p50 = 50th percentile, p75 = 75th percentile, p90 = 90th percentile, excluding mean from previous graph for better scaling and visual interpretation. Data obtained through private restricted-use agreement with Zillow.
Figure 7: Effective Federal Funds Rate

Figure 8: Unexpected Monetary Policy Shocks

Notes: Residual from regression of change in federal funds rate on intended rate and forecasts of inflation, output, and unemployment: $\Delta ff_m = \alpha + \beta ff b_m + \sum_{i=-1}^{2} \gamma_i \Delta \hat{y}_{m,i} + \sum_{i=-1}^{2} \lambda_i (\Delta \hat{y}_{m,i} - \Delta \hat{y}_{m-1,i}) + \sum_{i=-1}^{2} \varphi_i \hat{\pi}_{m,i} + \sum_{i=-1}^{2} \theta_i (\hat{\pi}_{m,i} - \hat{\pi}_{m-1,i}) + \rho \hat{u}_{m,0} + \epsilon_m.$ Based on Romer and Romer (2004) methodology.
Figure 9: Change in Actual Federal Funds Rate

Notes: Monthly change in the federal funds rate, computed from the monthly federal funds rate from January 1969 to December 2007, measured in percentage points, not seasonally adjusted. Retrieved from FRED, Federal Reserve Bank of St. Louis. [https://fred.stlouisfed.org/series/FEDFUNDS](https://fred.stlouisfed.org/series/FEDFUNDS)
Notes: Mortgage originations reported by lenders in California under the Home Mortgage Disclosure Act from 1990 to 2015. Measured in total dollar volume, represented by line corresponding to primary $y$-axis on the left. Measured in number of loans originated, represented by columns corresponding to secondary $y$-axis on the right.
Figure 11: Effect of Hedonic Characteristics on Housing Price Distribution

Notes: Coefficients for RIF-OLS regressions at each quantile of the housing price distribution at 5% increments from the 10th percentile to the 90th percentile. Hedonic model with year fixed effects:

\[ E[RIF_q(ln p_{i,t}) | X_{i,t}] = \beta_0,\tau Beds_{i,t} + \beta_{1,\tau} Baths_{i,t} + \beta_{2,\tau} BldgSize_{i,t} + \beta_{3,\tau} LotSize_{i,t} + \lambda_t + \varepsilon_{i,t} \]

Figure 12: Effect of Monetary Policy Shocks on Housing Price Distribution

Notes: Coefficients for RIF-OLS regressions at each quantile of the housing price distribution at 5% increments from the 10th percentile to the 90th percentile. Estimated with annual Romer and Romer (2004) monetary policy shocks and controls for omitted variable bias: $E[RIF_q(\ln p_{i,t}) \mid M_{t-1}, X_{i,t}] = \beta_t M_{t-1} + X_{i,t}'\gamma_t$. Data covers single-family home transaction prices in California from 1970 to 2007. Obtained through private restricted-use agreement with Zillow.
Figure 13: Effect of Monetary Policy Shocks on Housing Price Distribution by Decade

Notes: Coefficients for RIF-OLS regressions at each quantile of the housing price distribution at 5% increments from the 10th percentile to the 90th percentile. Estimated with annual Romer and Romer (2004) monetary policy shocks, interactions with dummy variables by decade, and controls for omitted variable bias: $E[\text{RIF}_{q_r}(\ln p_{i,t})|M_{t-1},X_{i,t}] = \beta_{7,7}M_{t-1} + \beta_{7,8}D_{1980s}M_{t-1} + \beta_{7,9}D_{1990s}M_{t-1} + \beta_{0,0}D_{2000s}M_{t-1} + X_{i,t}'\gamma \tau$. Data covers single-family home transaction prices in California from 1970 to 2007. Obtained through private restricted-use agreement with Zillow.
Figure 14: Effect of State-Level Loan Volume on Housing Price Distribution

(a) No Fixed Effects

(b) County Fixed Effects

Notes: Coefficients for RIF-OLS regressions at each quantile of the housing price distribution at 5% increments from the 10th percentile to the 90th percentile. Estimated with annual HMDA loan value and controls for omitted variable bias: \( E[RIF_{q,t}(\ln p_{i,t}) | \ln L_{t-1}, X_{i,t}] = \beta_\tau \ln L_{t-1} + X_{i,t}'\gamma_\tau. \) Data covers single-family home transaction prices in California from 1990 to 2015. Obtained through private restricted-use agreement with Zillow.

Figure 15: Effect of Tract-Level Loan Volume on Housing Price Distribution

(a) No Fixed Effects

(b) County Fixed Effects

Notes: Coefficients for RIF-OLS regressions at each quantile of the housing price distribution at 5% increments from the 10th percentile to the 90th percentile. Estimated with annual HMDA loan value and controls for omitted variable bias: \( E[RIF_{q,t}(\ln p_{i,t}) | \ln L_{t-1}, X_{i,t}] = \beta_\tau \ln L_{t-1} + X_{i,t}'\gamma_\tau. \) Data covers single-family home transaction prices in California from 1990 to 2015. Obtained through private restricted-use agreement with Zillow.
Figure 16: Effect of Tract-Level Loan Volume on Housing Price Distribution by Subperiod

Notes: Coefficients for RIF-OLS regressions at each quantile of the housing price distribution at 5% increments from the 10th percentile to the 90th percentile. Estimated with annual HMDA loan value, interactions with dummy variables by subperiod, and controls for omitted variable bias:

\[ E[RIF_q, (\ln p_{i,t}) | \ln L_{t-1}, X_{i,t}] = \beta_{\tau,0} \ln L_{t-1} + \beta_{\tau,1} D_{2000-06} \ln L_{t-1} + \beta_{\tau,2} D_{2007-10} \ln L_{t-1} + \beta_{\tau,3} D_{2011-15} \ln L_{t-1} + X_{i,t}' \gamma_{\tau}. \]

Data covers single-family home transaction prices in California from 1990 to 2015. Obtained through private restricted-use agreement with Zillow.
Notes: Coefficients for RIF-OLS regressions at each quantile of the housing price distribution at 5% increments from the 10th percentile to the 90th percentile. Estimated with annual HMDA loan value, interactions with dummy variables by subperiod, and controls for omitted variable bias:

\[ E[RIF_q, (\ln p_{i,t}) | \ln L_{t-1}, X_{i,t}] = \beta_{\tau,0} \ln L_{t-1} + \beta_{\tau,1} D_{2000-06} \ln L_{t-1} + \beta_{\tau,2} D_{2007-10} \ln L_{t-1} + \beta_{\tau,3} D_{2011-15} \ln L_{t-1} + X'_{i,t} \gamma_{\tau}. \]

Data covers single-family home transaction prices in California from 1990 to 2015. Obtained through private restricted-use agreement with Zillow.
Figure 18: Effect of County-Level IV for Lending on Housing Price Distribution

Notes: Coefficients for RIF-OLS regressions at each quantile of the housing price distribution at 5% increments from the 10th percentile to the 90th percentile. Estimated with Greenstone, Mas, and Nguyen (2014) IV strategy applied to mortgage lending and controls for omitted variable bias, as reduced form, not 2SLS: \( E[RIF_q, (\ln p_{i,t}) | Z_{t-1}, X_{i,t}] = \beta_r Z_{t-1} + X_{i,t}'\gamma_r \). Data covers single-family home transaction prices in California from 1990 to 2015. Obtained through private restricted-use agreement with Zillow.
Notes: On the x-axis, residual from regression of change in federal funds rate on intended rate and forecasts of inflation, output, and unemployment: $\Delta f_{f m} = \alpha + \beta f b_{m} + \sum_{i=-1}^{2} \gamma_{i} \Delta \tilde{y}_{m,i} + \sum_{i=-1}^{2} \lambda_{i} (\Delta \tilde{y}_{m,i} - \Delta \tilde{y}_{m-1,i}) + \sum_{i=-1}^{2} \varphi_{i} \tilde{\pi}_{m,i} + \sum_{i=-1}^{2} \theta_{i} (\tilde{\pi}_{m,i} - \tilde{\pi}_{m-1,i}) + \rho \tilde{\mu}_{m,0} + \varepsilon_{m}$. Based on Romer and Romer (2004) methodology.
Figure 20: Distribution of State-Level Changes in Loan Volume

Notes: Mortgage originations reported by lenders in California under the Home Mortgage Disclosure Act from 1990 to 2015. $x$-axis corresponds with ratio of current year’s total dollar volume to previous year’s. (Multiply by 100 to read in percentages.)
Figure 21: Simulated Housing Prices, 2000-2006

Notes: The monetary policy simulation uses the coefficients from the state-level loan volume model with subperiods in Figure 14. The actual historical values correspond with the 2000-2006 prices in Figure 2.
References


