Monetary Policy and Regional House-Price Appreciation

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Abstract

This paper examines the link between monetary policy and house-price appreciation by exploiting the fact that monetary policy is set at the national level, but has different effects on state-level activity in the United States. This differential impact of monetary policy provides an exogenous source of variation to assess the effect of monetary policy on state-level housing prices. Policy accommodation has an economically meaningful effect on state-level house price growth—an effect that is nearly two-and-a-half times as large during the early 2000s housing boom than in non-boom years.

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1 Introduction

In this paper, we revisit the impact of U.S. monetary policy on house-price appreciation. Our empirical analysis relies on a relatively disaggregated approach, in that it exploits state-level variation in U.S. housing prices and relative monetary policy accommodation. There are potentially important benefits to this approach. Specifically, it provides sufficient degrees of freedom to examine possible changes in the impact of monetary policy on house-price appreciation at different points in time. Just as with any other asset price, house prices can be interpreted as the sum of a fundamental component and a bubble component. In principle, monetary policy can have a different impact on these two components. As a result, the role of monetary policy in house-price appreciation can vary over time with changes in the relative size of the bubble component. Our approach addresses the role played by monetary policy in the 2000s housing bubble. More generally, the paper contributes to the growing literature on monetary policy and financial stability. Understanding the effect of monetary policy on housing prices and housing-price bubbles is necessary to inform the ongoing debate about the role of monetary policy in promoting financial stability.

In addition to allowing for the possibility of an effect that may not be stable over time, there are other challenges to estimating the impact of monetary policy on housing prices. In particular, one would like to identify policy actions that are not the response to current and expected future economic conditions, so that the estimated effect of monetary policy is not contaminated by reverse causation.[1] This implies, among other things, that the estimated relationship between monetary policy and housing prices should occur in the context of a specification that provides a rich set of controls to soak up variation in the

[1]A reverse causation scenario entails strong housing demand putting upward pressure on housing prices and, more generally, on economic activity. In such a situation, monetary policymakers would respond by raising the federal funds rate. Therefore, reverse causation should work in the direction of weakening the estimated effect of a monetary policy easing on housing prices.

policy rate that is an endogenous reaction to economic conditions. Our identification strategy is novel, in that it relies on the fact that monetary policy is set at the national level, but it has different effects on real activity across different states in the United States. We take this differential impact of monetary policy across locations as providing a source of variation in policy that can be treated as exogenous. Monetary policy actions are undertaken with a national perspective, and the effect of a change in the policy rate is measured over aggregate outcomes, with no explicit consideration that a change in the policy rate may have a greater impact in, say, Illinois than in Connecticut.[2] Our state-level specification for the change in house prices controls for state fixed effects, state-level economic conditions, and a time effect that absorb common sources of variation across states. Given that the time effect will also control for the common national stance of monetary policy, our measure of state-level differences in the stance of monetary policy can reasonably be expected to capture an element of monetary policy that is exogenous.

We find that monetary policy has an economically relevant impact on state-level house-price growth. Additional monetary accommodation results in higher state-level house-price growth (all else equal). We further show that during the housing boom, monetary accommodation had a much greater impact on house-price growth than in non-boom years. In addition, we find that the impact of monetary policy on house-price growth is strongest for locations with the most housing-supply constraints (land regulation). Our results are robust to controlling for lagged house-price growth, leads and lags of state-level economic activity, and regional difference in credit availability. Overall, we find a non-negligible impact of monetary policy on housing prices during the housing-price bubble in the early 2000s.

[2]Members of the Federal Open Market Committee include presidents of the regional Federal Reserve Banks, who could in principle have a regional bias when voting on monetary policy. The existing literature on this topic, however, has found such a bias to be small at best, and thus unlikely to have moved the entire Committee away from setting policy with a nationwide focus. See Tootell (1991) and Jung and Latsos (2015).

A relatively wide body of empirical literature documents regional differences in the effect of monetary policy (see Carlino and DeFina (1998) and subsequent work updating their original findings). There are several reasons for these differences. For one, industries have diverse sensitivities to interest rates, and differences in the regional mix of industries could lead to different regional effects of monetary policy. To the extent that banks are an important transmission channel for monetary policy, the mix of small (bank-reliant) versus large firms in a region can also lead to differential effects. Per-capita incomes also differ across states, with income convergence having stalled over the past 30 years (see Ganong and Shoag 2015). With variation in the share of liquidity-constrained households across states, the ability of households to substitute spending intertemporally may also differ. Domestic in- and out-migration flows also vary markedly across U.S. regions (see Franklin 2003)—likely affecting the rate of adjustment of state-level economic conditions to interest rate changes across states.

To exploit such state-level differences in the effect of monetary policy, we compute a time-varying, state-level measure of the equilibrium real rate of interest. There are several notions of the "equilibrium" or "natural rate" of interest. From a longer-run perspective, the equilibrium real rate is typically defined as "the real short-term interest rate consistent with the economy operating at its full potential once transitory shocks to aggregate supply or demand have abated" (Laubach and Williams 2015). In a monetary policy setting, this notion of the equilibrium real rate is often accompanied by another, shorter-run concept that specifies a window of time—typically two or three years—over which this rate achieves full resource utilization. This latter notion of the equilibrium rate, which will also depend on the longer-run measure of the equilibrium real rate, is the one that we use in our analysis. Specifically, our time-varying state measure of the equilibrium real interest rate is the value of the real federal funds rate that, if sustained, would close a state's unemployment rate gap after two years.

A short-run concept of the equilibrium real rate is often used as an input in setting monetary policy at the aggregate level. For example, the Federal Reserve Board staff has included measures of the short-run equilibrium real federal funds rate in its briefing materials to the Federal Open Market Committee (FOMC) since the end of 2004.[3] The short-run equilibrium real rate has some limitations as a guide to policy, in that, even if this rate can achieve full capacity, it may not necessarily yield a desired outcome in terms of inflation. As a result, actual monetary policy is likely to deviate from this prescription. However, in our disaggregated setup all we need is a measure of the difference in the relative stance of monetary policy across states, as the common effect of monetary policy in determining changes in housing prices will be absorbed by the time effect.

This state-level notion of the equilibrium real rate is well suited for our purposes. Since this equilibrium measure is expressed in real terms, our results can be easily compared with previous findings in the literature. Still, when interpreting movements in the state-level equilibrium rate, it is important to consider the horizon over which this rate needs to be maintained to achieve full resource utilization. In other words, a given change in the equilibrium real rate should have a different impact on housing prices when the horizon is, for example, two years versus three years. For our purposes, however, the choice of the horizon is immaterial, as, in practice, conversion from one horizon to another can be well approximated by a multiplicative factor.

We calculate the short-run equilibrium real federal funds rate by estimating a backward-looking IS curve at the state level. The IS curve specifies the state unemployment rate as a function of past unemployment rates and past real interest rates. The (state-specific) estimated constant in the IS curve captures long-run estimates of the unemployment rate and the real interest rate, so the IS curve relationship can be more precisely thought of as

[3]See the box "New Estimates of the Equilibrium Real Federal Funds Rate" in the Bluebook of December 9, 2014, available online at:
https://www.federalreserve.gov/monetarypolicy/files/FOMC20041214bluebook20041209.pdf

relating activity gaps to deviations of the real rate of interest from its longer-run value. Still, what matters is that the state-level equilibrium real federal funds rate is a function of the state-level unemployment rate. The state-level variation in the coefficients of the IS curve implies that the state-level equilibrium real rate is related to the state-level unemployment rate differently across states. This variation is crucial for identifying the effect of monetary policy on housing prices, as our house price growth specification already controls for state-level economic conditions. Thus, identification of the monetary policy effect as captured by our state-level equilibrium real rate exploits cross-sectional differences in the way the equilibrium real rate depends on state-level unemployment rates.

Our identification approach also highlights why other potential approaches to capturing differences monetary accommodation at the state level are not suitable for this type of exercise. For example, one could compute a state-level policy rate as predicted by a Taylor rule, and then take the difference between the actual and the predicted federal funds rate at the state level as a measure of the relative stance of monetary policy. However, the use of a common Taylor rule across states implies that the way the relative stance of policy depends on state-level economic conditions is the same across states. If the specification for the change in housing prices already controls for state-level economic conditions, it will not be possible to separately identify the monetary policy effect. Similarly, one could argue that the real federal funds rate differs across states, as different economic conditions will lead to different state-level rates of inflation, because of the presence of nontraded goods. Again, this approach suffers from the house price growth equation already controling for state-level economic conditions. In sum, what matters for identification is not state-level differences in economic conditions, but rather the differential impact of monetary policy across states for given economic conditions.

There is a growing literature on the effect of monetary policy on house prices (see, among others, Williams 2015, Dokko et al. 2011). Recent work by Jordà, Schularick, and Taylor (2015) is especially interesting, in that it examines the effect of changes in interest rates on housing prices in countries that pegged their exchange rate to a foreign country's currency. Under these circumstances, changes in domestic interest rates are exogenous, since they respond to foreign rather than domestic conditions. It is therefore relatively straightforward to trace out the effect of monetary policy on domestic housing prices. Our work is similar, in that we isolate a state-level interest rate effect that can be interpreted as exogenous. Our identification strategy, however, is noticeably different. In addition, Fratantoni and Schuh (2003) use regional data—more specifically, metropolitan statistical area data—to estimate the effect of monetary policy on housing prices. The authors identify and estimate the effects of monetary policy occur in the context of a vector auto-regression (VAR) model that incorporates regional heterogeneity in housing markets. While each identification approach has its strengths and weaknesses, we believe that our setup is well suited to address the relationship between monetary policy and house prices in "bubble" versus "non-bubble" times, using a specification that allows for a wide range of controls. This paper, therefore, is also related to recent work by Galí and Gambetti (2015), who examine the effect of monetary policy on stock-market bubbles using a VAR with time-varying coefficients. Their work documents substantial differences in the responses of the fundamental and the bubble components of stock prices to a monetary policy shock. However, they find little evidence that monetary policy easing sustains an increase in the bubble component of stock prices.

The remainder of the paper proceeds as follows: the next three sections present our empirical framework, data, and results, respectively. The final section concludes.

2 Estimation Framework

Our analysis is divided into two parts: The first part estimates state-level IS curves and constructs state-level measures of the equilibrium real interest rate—the interest rate that will close state i's unemployment gap within two years. The second part considers whether differences in the equilibrium real interest rate across states and over time predict differences in state-level house-price growth, conditional on other factors.

2.1 Estimation Part 1: Estimating r^*_{it}

We generate state-level estimates of the equilibrium real interest rate, which we denote by r^*_{it} for each state i at time t, by estimating state-level IS curves. The IS relationship adopted here—with minor modifications—has been used with aggregate data in the context of small-scale representations of the U.S. economy (for early applications see Fuhrer and Moore 1995, Rudebusch and Svensson 1998). The IS curve approach is also used by the Federal Reserve Board to compute an aggregate estimate of the equilibrium real federal funds rate. This IS curve representation is backward-looking, in that it relates a measure of the deviation of the economy from full resource utilization to its lags and lags of the real federal funds rate. As such, it lacks micro foundations, but the difficulties associated with estimating micro-founded, forward-looking IS curves have been well documented in the literature (see Fuhrer and Rudebusch 2004).

Our specification for the state-level IS curve takes the following form:

$$u_{it} = \alpha_i + \nu_t + \lambda_{1i} u_{i,t-1} + \lambda_{2i} u_{i,t-2} + \theta_{1i} r_{i,t-1} + \epsilon_{it}, \tag{1}$$

where $u_{i,t}$ is state i's unemployment rate at time t, and r_{it} is a time t measure of the real federal funds rate for state i. The specification controls for state fixed effects (the α_i's) and a time effect ν_t that absorbs sources of fluctuations in the state unemployment rate that are common to all U.S. states. Allowing for state fixed effects accounts for the possibility that the equilibrium level of the unemployment rate differs across states. It is well known that certain U.S. states experience persistently higher unemployment rates than others. For example, since 1976 the yearly unemployment rate in Alabama has never fallen below the yearly unemployment rate in Utah. The fixed effect also allows for potential state-level differences in the longer-run equilibrium value of the real federal funds rate. The scope for such differences likely depends on the type of inflation rate that one considers when translating the nominal federal funds rate into a real rate. In particular, a value-added deflator is likely to yield inflation rates that are more dispersed across states than a consumption-based deflator, the presence of nontraded goods notwithstanding.

The state fixed effects, however, do not account for any time-variation in the state-level equilibrium values for the unemployment rate and the real federal funds rate. The time effect ν_t in equation (1) captures time variation in the natural rates—at least the portion of the variation that is common across states. In the robustness section, we consider an augmented IS-curve specification that controls for state-level demographics as one potential source of variation in the state-level equilibrium unemployment rates.

We estimate the IS relationship in equation (1) at an annual frequency, with the two lags of the unemployment rate capturing the persistent features of this variable, and the lag in the real federal funds rate capturing the delayed effects of interest rate changes on real activity. We discuss how we construct a state-level measure of the real federal funds rate in Section 4.

Our identification strategy relies crucially on the state-level IS curve having different estimated parameters $\{\lambda_{1i}, \lambda_{2i}, \theta_{1i}\}$ across states. These differences can be justified on several economic grounds given the documented differential effect of monetary policy across U.S. states in the existing literature. In particular, the the estimated impulse-responses in Carlino and DeFina (1998), show state-level differences in both the amplitude and persistence of the real response to a monetary policy shock. Within our IS curve setup, these observed differences translate into different parameter values at the state level for both the interest rate sensitivity coefficient (θ_1) and the coefficients measuring the intrinsic persistence of the real activity variable (λ_1 and λ_2).

Allowing the IS curve coefficients to vary across all states may create some attenuation bias in the parameter estimates, as our sample is relatively short. For this reason, as well as to decrease potential measurement error in our estimates, we first estimate equation (1) without any restrictions and rank states based on the impact of a permanent change in the real interest rate on the unemployment rate after two years. This effect can be readily shown to amount to $\left(\frac{\delta u}{\delta r}\right)_{2yr} = 2\theta_{1i} + \theta_{1i}\lambda_{1i}$. Given these $\left(\frac{\delta u}{\delta r}\right)_{2yr}$ rankings, we group states into five bins. The bins are denoted by b, where $b \in [1, 5]$—a higher bin number corresponds to higher two-year unemployment rate effects. We then re-estimate (1), restricting the coefficients to be the same for each state in a given group b:[4]

$$u_{it} = \alpha_i + \nu_t + \lambda_{1b} u_{i,t-1} + \lambda_{2b} u_{i,t-2} + \theta_{1b} r_{i,t-1} + \epsilon_{i,t}. \tag{2}$$

Using the parameter estimates from equation (2) we compute r^*_{it}, which is the interest rate needed at time t in a given state to reach full employment (the state's equilibrium unemployment rate) over the next two years. Given the annual frequency of our data and the delayed impact of monetary policy implied by our IS curve specification, this amounts to closing the unemployment rate gap in about three years, which is the horizon typically used by policymakers when estimating the equilibrium real policy rate. With some simple algebra (shown in the Appendix), one can manipulate equation (2) to obtain

[4]Ranking states based on the sensitivity of their unemployment rate to the interest rate, θ_{1i}, yields similar results as does dividing states based on an external measure, such as the share of their state product that comes from manufacturing. See Section 4.3 for more details.

the state-level equilibrium real interest rate at each point in time as:

$$r^*_{it} = -\left[(\lambda^2_{1b} + \lambda_{2b})u_{it} + \lambda_{1b}\lambda_{2b}u_{i,t-1}\right] \times \left(\frac{1}{\lambda_{1b}\theta_{1b} + \theta_{1b}}\right) + \mu_i. \tag{3}$$

The constant μ_i is state specific and will depend on the IS curve parameters, including α_i. Since our house-price growth specification, which we describe below, includes state fixed effects, it automatically accounts for μ_i.

Section 4 reports the parameter estimates from equations (1) and (2) along with the related values for r^*_{it}. Importantly, equation (3) shows that the equilibrium real interest rate is a function of the state-level unemployment rate. To the extent that there is variation in the estimated parameters $\{\lambda_{1b}, \lambda_{2b}, \theta_{1b}\}$, it is then possible to identify the effect of r^*_{it} on the change in state house prices even when we control for state-level business cycle conditions and time effects. In other words, it is not sufficient for r^*_{it} to vary across states in order to estimate its impact on housing prices; it is also necessary that the relationship between r^*_{it} and other variables, such as measures of state-level real activity, differ across states.

In addition, while we try to estimate a measure of the differential impact of monetary policy across states that has strong empirical content, this measure will nevertheless be contaminated by measurement error as it is a generated regressor. Also, our estimate of r^*_{it} does not allow for the possibility of state-specific time variation in the equilibrium unemployment rate. We verify however, that state-level time-varying demographic controls, which may affect a state's natural rate of unemployment, do not alter our findings. It is difficult to ascertain the importance of measurement error in our estimates, but it should be kept in mind when evaluating the effect of monetary policy on housing prices. Measurement error should bias our estimates towards zero, and as a result our findings should be interpreted as a lower bound to the effect of monetary policy on housing prices.

The presence of measurement error may not constitute a problem for our analysis when evaluating the differential effect of monetary policy on housing prices in "bubble" versus "non-bubble" periods. For measurement error in r^*_{it} to be an issue, one would have to argue that it is systematically different in "bubble" versus "non-bubble" periods. As an additional check, we control for potential measurement error using a bootstrap approach (see Section 4.2.1), and obtain results that are very similar to our baseline findings.

2.2 Estimation Part 2: Effect of r^*_{it} on House-Price Growth

The second part of our analysis uses the estimated state-level equilibrium real rates to gauge the effect of monetary policy on house-price growth. Our estimates of r^*_{it} still contain the common systematic component of monetary policy. We must control for this common component in order to capture the state-specific stance of policy that does not represent the endogenous response to current and expected future economic conditions, and we do so by including a time effect in our specification for state-level house-price growth. The inclusion of time effects in our specification should provide a more stringent test of the effect of monetary policy on housing prices as captured by r^*_{it}.

Our specification that relates monetary policy and house-price growth takes the following form:

$$h_{i,t+2} = \sigma r^*_{it} + \beta \mathbf{X}_{it} + \kappa_i + \zeta_t + \varepsilon_{it}, \tag{4}$$

where $h_{i,t+2}$ is real house-price growth in state i from time t to time $t+2$, r^*_{it} is our measure of state-level monetary accommodation, and $\mathbf{X}_{it}$ is a vector of additional state-level controls at time t. The specification controls for state fixed effects (the κ_i's) and a time effect ζ_t that absorbs fluctuations in state house-price growth that are common to all U.S. states, including the common component of monetary policy. The timing of the variables in equation (4) allows for the effect of monetary policy on house prices to build over time. The two-year future change in housing prices also covers the same time span implicit in our horizon for computing the equilibrium real rate of interest.

A higher relative value of the equilibrium real rate r^*_{it} means a higher relative degree of monetary policy accommodation in state i at time t. Greater policy accommodation should stimulate housing demand, and thus we would expect σ to be positive. That is, additional (relative) monetary policy accommodation should be positively related to house-price growth. In addition, while r^*_{it} captures the differential effect of monetary policy on state-level activity (that is, a given change in the federal funds rate will move the state-level equilibrium real rate r^*_{it} differently across states), the relationship between r^*_{it} and housing prices as summarized by the semi-elasticity σ is the same across states.

The vector of additional control variables in our baseline estimates of equation (4) includes state GDP growth between period $t-1$ and t, and in some cases lagged house-price growth $h_{i,t-2}$, which captures two-year house-price growth in state i between $t-2$ and t. It is crucial to control for state-level economic conditions, as these will also affect the state-level equilibrium real interest rate. When assessing the effect of monetary policy on housing prices, we are interested in the variation in r^*_{it} that is due to the differential regional effects of monetary policy, not in the variation in r^*_{it} that is driven by, say, state-specific demand or productivity shocks. We maintain a parsimonious specification for house-price growth, given that our estimation equation includes both time effects and state fixed effects. Indeed, with time and state fixed effects, identification of the monetary policy effect (and others) comes from variation in relative monetary accommodation across states after subtracting state-specific averages over time. Including too many additional regressors makes the identification of any one particular effect difficult and potentially hard to interpret. Therefore, equation (refss) includes the regressors that we deem most relevant for predicting state-level house-price growth. In Section 4.3 we explore including both additional and alternative control variables.[5]

We discuss our estimates of r^*_{it} and our findings regarding its impact on house-price growth in detail in Section 4.

2.3 Interpreting the Identified Effects of Monetary Policy

Our identification of the effect of monetary policy on house prices differs from other approaches that rely on aggregate time-series evidence. This section discusses how our estimated effect can be interpreted given our estimation strategy.

We rewrite the expression for the state-level equilibrium real rate of interest, equation (3), as follows:

$$r^*_{it} = (\overline{\chi} + \chi_i)(\overline{u}_t + z_{it}),$$

where, without loss of generality and in order to simplify the exposition, we have omitted the lagged unemployment rate term and a constant. The expression decomposes the state unemployment rate into a component common across states, $\overline{u}_t$, and a state-specific component z_{it}, so that $\sum_{i=1}^{N} w_i z_{it} = 0$ at any point in time t, where w_i is state i's labor force weight in total U.S. labor force. Given such a decomposition, the common component $\overline{u}_t$ can be interpreted as the national measure of the unemployment rate. The term $(\overline{\chi} + \chi_i)$ captures the coefficients that pre-multiply the unemployment rate in equation (3)—separating them into their common (across states) and idiosyncratic components. These coefficients are defined similarly, so that $\sum_{i=1}^{N} w_i \chi_i = 0$.

Within this framework, the effect of monetary policy on state house price growth is the difference between the state-level equilibrium real federal funds rate and the actual real federal funds rate, $r^*_{it} - r_t$. We can rewrite equation (4) in terms of $r^*_{it} - r_t$ using the

[5]One additional factor we explore including in equation 4 is a control for local housing supply conditions—something the previous literature has identified as relevant for thinking about local house price growth (see, for example, Saks 2008).

previous expression for r_{it}^* to obtain:

$$h_{i,t+2} = \sigma\left(\chi_i\left(\overline{u}_t + z_{it}\right) + \overline{\chi}\,\overline{u}_t + \overline{\chi} z_{it}\right) + \beta \mathbf{X}_{it} + \kappa_i + \zeta_t + \varepsilon_{it}, \tag{5}$$

where the term $-\sigma r_t$ is subsumed in the time effect ζ_t. Equation (5) shows that the effect of monetary policy on housing prices is identified via variation in the first term in the bracketed expression, $\chi_i\left(\overline{u}_t + z_{it}\right)$. The other two terms, $\overline{\chi}\,\overline{u}_t$ and $\overline{\chi} z_{it}$, are subsumed in the time effect, ζ_t, and state-level economic conditions, $\mathbf{X}_{it}$, respectively.[6] The first term in the bracketed expression captures the portion of the interest rate effect that is specific to state i, given that χ_i but not $\overline{\chi}$, is included. As such, this effect should be orthogonal to the systematic component of monetary policy, since monetary policy is set with aggregate goals in mind.

The main goal of our analysis is to determine whether the effect of monetary policy on state-level house prices was different during the early 2000s housing bubble than in prior periods. Specifically, we are interested in whether there was a significant increase in the sensitivity of house prices to monetary policy easing during the bubble period, as captured by the estimated coefficient σ in equation (5). Our empirical approach is well suited to answer this type of question, as the state-level variation provides sufficient degrees of freedom to test for a changing effect of monetary policy on house prices. We can also easily compare the relative size of the monetary policy effect across time periods and address how much more of an effect, if any, monetary policy has on house prices during the boom period. Indeed, our setup is quite informative about a change in the parameter σ, over time. Translating our estimated σ into an aggregate monetary policy is not completely straightforward, even though we weight our regressions by the size of each

[6]When $\mathbf{X}_{it}$ does not include the state unemployment rate but another state-level measure of activity such as GDP, then identification of the interest rate effect will also come from the variation in $\overline{\chi} z_{it}$ that is orthogonal to $\mathbf{X}_{it}$. Still, this variation is state-specific and as such it should capture an interest rate effect that is not driven by the systematic response of monetary policy to aggregate economic conditions.

state's labor force to capture the fact that some states, given their size, likely exert more (or less) influence on the overall relationship between monetary policy and house-price growth.

The differences between the cross-sectional and the aggregate effects of a particular shock have been highlighted in work by, among others, Nakamura and Steinsson (2014) and Beraja, Hurst, and Ospina (2016). These authors focus on shocks that are state-specific in nature, whereas in our case monetary policy is set at the national level and is common to all states. Our estimate of σ can be interpreted as a measure of the effect of monetary policy on housing prices in the aggregate to the extent that a (weighted) average of the state-level measures of the equilibrium real rate of interest provides a good proxy for the aggregate equilibrium real rate. In terms of equation (5), the aggregate effect of monetary policy, as measured by the difference between the aggregate equilibrium real federal funds rate and the actual real federal funds rate, $r_t^* - r_t$, falls out from our estimation setup if $\overline{\chi}\,\overline{u}_t$ properly measures the equilibrium real federal funds rate for the US as a whole. This is unlikely to be the case, though, as the estimated parameters in our state-level IS curve do not necessarily average to values that would be representative of an IS curve in the aggregate. For example, state-specific productivity shocks may generate cross-state migration—a margin of adjustment that is not present with aggregate productivity shocks. Still, one could obtain an estimate of the aggegate monetary policy effect on house prices by adjusting σ to take into account the average bias arising from the difference between the weighted average of the state-level measures of the equilibrium real interest rate, $\overline{\chi}\,\overline{u}_t$ in (5), and a measure of the equilibrium real federal funds rate derived from estimating an IS curve at the aggregate level.

Given that our identification approach well identifies changes σ over time, it is not necessary to obtain an exact measure of the aggregate effect of monetary policy on house prices to address whether monetary policy had a larger impact on house prices during the housing boom than before. While we discuss the size of the monetary policy effect as a way of quantifying and comparing our findings across specification, our main focus and interest is how our estimates of σ differ across periods.

3 Data and Estimation Sample

3.1 Data Sources

Data on state-level unemployment come from the Bureau of Labor Statistics (BLS) and are available on a monthly basis starting in the late 1970s. We construct state-level inflation data using Gross State Product (GSP) data from the Bureau of Economic Analysis (BEA). These data are available at a quarterly frequency starting in 1963. We define state-level inflation as the change in each state's GSP deflator.[7] We use the effective federal funds rate (available at a monthly frequency from the Federal Reserve Board) as our measure of the nominal interest rate. Aggregate data on core inflation come from the BEA and are measured as the year-over-year percent change in the deflator for personal consumption expenditures (PCE) excluding food and energy.

In the second part of our analysis, we obtain nominal state-level house-price indices from CoreLogic and following the existing literature we convert them to real using the aggregate CPI-U index from the BLS.[8] We use two-year real house-price growth (not annualized) in our baseline estimates of equation (4). Using one-year or three-year real house price growth yield similar (annualized) results. We control for state-level economic activity using growth in each state's real GSP. Data on housing supply restrictions (land-use regulation) come from the Wharton land-use regulation Index (WLURI). This index is

[7]The BEA publishes nominal and real GSP data, and we divide nominal GSP by real GSP to obtain the implicit price deflator.

[8]Our results are qualitatively the same and quantitatively very similar if we use our fitted state-level inflation data to deflate house prices (see Section 4.1 for more details on this inflation measure).

based on Gyourko, Saiz, and Summers (2008), and provides a good proxy for the relative level of housing-supply constraints in a given area. The data are not time-varying, but land use restrictions change little over time and we employ the index only to divide states based on their relative level of housing-supply constraints.

3.2 Estimation Sample

We estimate our baseline equations using annual data to reduce noise. Monthly and quarterly data are converted to an annual frequency by taking the annual average, and subsequent data transformations are made from these annual averages. Equations (1) and (2) are estimated over the 1980–2007 period. This timing takes into account data availability (given the lag structure) at the beginning of the sample. The estimates end in 2007 because the Great Recession and the ensuing zero-lower-bound period may have altered the relationship between the real federal funds rate and economic activity. Away from the zero lower bound, changes in the real federal funds rate elicit changes in other borrowing rates that are relevant for consumers' and businesses' spending decisions. At the zero lower bound, the relationship between the real federal funds rate and other borrowing rates changed noticeably, as the Federal Open Market Committee engaged in large-scale asset purchases to reduce the yields on longer-maturity assets. As a consequence, the relationship between activity and interest rates as captured by our IS curve is likely to have changed in the post-2007 period.

Consistent with the timing of our IS curve estimates, we estimate equation (4) between 1980 and 2005. Since our dependent variable is house-price growth over the next two years, the analysis incorporates house-price growth through 2007. Our baseline analysis focuses on the 48 contiguous states and excludes the District of Columbia.[9]

[9] Including or excluding the District of Columbia has little impact on our results.

4 Results

4.1 IS Curve Estimation

This section first discusses the estimates from equations (1) and (2), and explores the related estimates of r^*_{it} constructed using equation (3). In order to estimate equations (1) and (2) we take a few preliminary steps. First, we construct a state-level measure of the real federal funds rate r_{it}. In particular, we define,

$$r_{it} = i_t - \pi_{it}, \tag{6}$$

where i_t is the nominal federal funds rate, and π_{it} is state-level inflation. In other words, we take time t realized inflation as providing the expected value for inflation at time $t+1$. For the sample period that we consider in the estimation, inflation data at the state level are only available using GSP deflator. At the aggregate level, the behavior of inflation based on the GDP deflator or on the core PCE (consumption) deflator is fairly similar, largely because consumption represents a sizable share of GDP. Therefore, estimates of aggregate IS curves where the real rate is computed by either GDP inflation or core PCE inflation tend to be similar. Still, the aggregate IS curve estimated by the Federal Reserve Board to compute an estimate of the equilibrium real interest rate uses the core PCE inflation-based measure (see Brayton, Laubach, and Reifschneider 2014). This is not surprising, as the FOMC's inflation target is in terms of PCE inflation, with the core measure providing a less volatile, near-term indicator of underlying inflation. On a state-level basis, one would not expect a close correspondence between consumption-based and GSP-based measures of inflation. In practice, the available data limit our options at the state level. We find that state-level estimates of the equilibrium real interest rate based on GSP inflation tend to be fairly volatile, as they inherit the volatility of the inflation data.

In order to have our inflation measure better reflect core PCE inflation and to reduce the potential for measurement error in the equilibrium real interest rate series, we compute a measure of state-level inflation, $\widehat{\pi}_{it}$, as the fitted value from the following regression:

$$\pi_{it} = \varphi_i + \pi_t^{core} + \delta_1 \tilde{y}_{it} + \delta_2 \tilde{y}_{i,t-1} + \upsilon_{it}, \tag{7}$$

where π_{it} is inflation in state i at time t as measured by the GSP deflator, π_t^{core} is aggregate core PCE inflation at time t, $\widetilde{y}_{it}$ is real GSP growth in state i at time t relative to aggregate real GDP growth, and φ_i is a state-specific intercept. We constrain the coefficient on aggregate core PCE inflation to equal one so that changes in aggregate core inflation feed one-for-one into state-level inflation. In addition, we weight the estimates in equation (7) based on the relative size of each state's labor force. This approach smooths our state inflation measure in a way that captures the relevant variation due to core consumer price fluctuations as well as the changes in inflation due to differences across states in their business cycle conditions relative to national economic conditions. Indeed, over the 1980–2007 period, actual state-level inflation (π_{it}) has a mean of 3.2 percent and a standard deviation of 2.1 percent. While the mean of predicted inflation $\widehat{\pi}_{it}$ is the same, its standard deviation is slightly lower, at 1.9 percent. Figure A.1 in the Appendix plots the two inflation measures across states over time.

Our fitted state-level inflation data compare more favorably to recently published BEA data on state-level consumer inflation, available only for the 2009–2013 period, than to the raw, GSP-based price data.[10] The overall correlation of BEA state inflation rates and the GSP deflator-based state inflation measure is 0.38 for this period, while the correlation of BEA inflation rates with the fitted values from equation (7) is 0.50.

[10] The BEA data capture state-level PCE inflation—an appropriate measure for calculating the real interest rate. Unfortunately, these data are only available starting in 2009.

(Figure A.2 in the Appendix depicts the relevant state-by-state correlations.) This test of the external validity of our predicted inflation measure confirms that our approach removes noise, rather than signal, from the GSP-based state inflation data. Section 4.3 shows that our results are robust to instead using aggregate core PCE inflation data for all states. The advantage of our baseline approach is that we can account for differences in price fluctuations across states due to differences in local business conditions, which arguably provide a more accurate picture of local inflation and the implied state-level real interest rate. Table A.2 in the Appendix reports the actual parameter estimates from equation (7).

We construct our measure of the state-level real interest rate, $\widehat{r}_{it}$, based on the fitted values $\widehat{\pi}_{it}$ from equation (7) as follows:

$$\widehat{r}_{it} = i_t - \widehat{\pi}_{it}, \tag{8}$$

and we replace r_{it} with $\widehat{r}_{it}$ when estimating the unconstrained and constrained state-level IS curves, equations (1) and (2). As noted earlier, these equations are estimated using annual data from 1980 to 2007. The results are qualitatively similar if we instead begin the estimation period in 1986, which is a common starting date for analyzing macro data over the "Great Moderation" period.[11] We allow the coefficients in equation (1) to vary by state, but we do not report the individual parameter estimates, for brevity. Table 1 shows summary statistics for the lagged interest rate effect, θ_{1i}, and the sum of the lagged unemployment gap effects, $\lambda_i = \lambda_{1i} + \lambda_{2i}$. The table also shows the estimated effect of a persistent increase in the interest on the unemployment gap after two years, which is given by $2\theta_{1i} + \theta_{1i}\lambda_{1i}$. This estimated two-year effect indicates the speed with which interest rate changes impact the real economy.

The estimated parameters are quite reasonable. All of the lagged unemployment rate

[11] These results are not shown, but are available from the authors upon request.

effects lie within the unit circle, so there are no explosive dynamics. The estimated interest rate effects all have the expected sign (positive)—higher rates restrain economic activity and lead to higher unemployment relative to its natural rate, all else equal. In addition, Figure 1 plots the estimated interest rate effects (and their standard errors) for each state. The figure shows that there is substantial variation across states in the impact of interest rates on unemployment. All of the state-level effects are also significantly different from zero. Finally, the estimated size of the interest rate effect on the unemployment rate after two years is at the higher end of estimates of the impact of short-term rates on real activity at the national level. For example, the Federal Reserve Board's FRB/US model implies that a persistent 100-basis-point decline in the federal funds rate would lower the national unemployment rate by roughly a half percentage point after three years.[12] The larger effect estimated at the state level could be due to the presence of spillover effects, such as labor mobility across states, which may not operate at the national level. Indeed, recent work by Beraja, Hurst, and Ospina (2016) illustrates how the estimated regional effect of a shock can differ from the estimated national effect of the very same shock.

We estimate equation (2) after grouping states based on the size of the impact of the interest rate on the unemployment rate after two years (two-year effect). Recall that based on equation (1), the two-year effect equals $2\theta_{1i} + \theta_{1i}\lambda_{1i}$. We combine states into five groups of roughly equal size with the cut-off points for the two-year effect at 0.64, 0.77, 0.87, and 0.96 respectively.[13] We explore alternative approaches to grouping states in Section 4.3.

Table 2 shows the estimated lagged interest rate and lagged unemployment rate effects for each of the five groups. In addition, the table reports the corresponding two-year effect. The estimated interest rate effect, θ_{1b}, increases monotonically across the groups—a

[12]See Brayton, Laubach, and Reifschneider (2014).
[13]Table A.3 reports the states in each group.

finding that is consistent with our grouping approach, as larger estimated interest rate effects result in bigger two-year unemployment gap effects. That is, states in group five will be the ones with the greatest interest rate sensitivity. In addition, the estimated lagged unemployment rate effects are all substantially less than one and are roughly similar across groups, with the effect for group 5 being the largest. The estimated two-year effects range from 0.68 for group 1 to 1.22 for group 5. These estimates imply that a 100-basis-point reduction in the short-term interest rate reduces states' unemployment gaps by about $\frac{2}{3}$ and $1\frac{1}{4}$ percentage points, respectively, over two years. These effects are somewhat, but not substantially, smaller than the estimated long-run impact of interest rates on the unemployment gap (not shown), which suggests that interest rate changes impact economic activity fairly quickly. Overall, the parameter estimates from equation (2) are quantitatively similar but somewhat less dispersed than when we estimate separate interest rate and unemployment gap effects for each state. We use these restricted parameter estimates to calculate each state's equilibrium interest rate, r^*_{it}.

Figure 2 shows the calculated values for r^*_{it}. These values are computed assuming that for each state full employment is equal to the level of the state unemployment rate prevailing on average during the years 1995 and 1996. This is a period when, at least from a national perspective, the unemployment rate was fairly close to the CBO estimate of the equilibrium unemployment rate.[14] Note also that we calculate r^*_{it} based on parameter estimates for the 1980–2007 period, but we use these parameters to project r^*_{it} over a longer sample horizon (1980–2015) to check the out-of-sample behavior of the series. The figure plots the average r^*_{it} across states in a given year (solid line) as well as the interquartile range and minimum and maximum values for r^*_{it} by year.

On average, the real interest rate needed to close the unemployment rate gap falls during economic downturns and rises during periods of economic recovery and growth.

[14] We consider an alternative approach to control for states' equilibirum unemployment rates in section 4.3.

The average rate is negative during the Great Recession as well as during the economic downturn in the early 1980s—two periods when there was substantial job loss and rising unemployment. During these episodes, especially low levels of the real federal funds rate were needed to restore equilibrium (closing the unemployment gap over the next two years). Not surprisingly, the average estimated r^*_{it} is somewhat more negative during the Great Recession, the most severe economic downturn since the Great Depression, than during the early 1980s. The difference, however, is not large. Most importantly, the qualitative (and quantitative) pattern of r^*_{it} over time is broadly consistent with our own estimates using aggregate data (not shown) and previous estimates in the literature.

4.2 Monetary Accommodation and House-Price Growth

Table 3 reports our baseline estimates of the impact of monetary policy accommodation on house-price growth using equation (4). Recall that we estimate the effect of monetary accommodation and other factors at time t on local house-price growth between time t and time $t+2$. In addition, all specifications include state fixed effects and a full set of time dummy variables (neither is shown), and are estimated over the 1980–2005 period for house-price growth through 2007. The estimates are weighted based on the size of a state's labor force. Standard errors are clustered at the state level. For simplicity, we hereafter refer to monetary policy accommodation as r^*_{it}.

Column (1) shows the impact of r^*_{it} on house-price growth controlling only for time and state fixed effects. As expected, the estimated r^*_{it} effect is positive. It is also very precisely estimated, although the standard errors may overstate the degree of precision because r^*_{it} is a generated regressor. We discuss the standard errors in more detail below, including a correction based on bootstrapping. Given the importance of controlling for state-level economic conditions in order to identify the monetary policy effect, in column (2) of the table we add state output (GSP) growth as a control variable. We take the specification in this column as our baseline, against which we evaluate specifications that introduce additional covariates.

Controlling for state output growth raises the portion of the variation in house prices that we are able to explain by 8 percent. The effect of monetary accommodation on house-price growth is reduced by almost one half, but remains positive and precisely estimated. The estimated coefficient implies that an increase in monetary policy accommodation by an amount equivalent to the equilibrium real federal funds rate rising by 100 basis points results in about 2.5 percent higher house-price growth over the next two years. Since the amount of variation in r^*_{it} across states and over time depends somewhat on our approach to estimating the IS curve, a 100-basis-point change may be large or small depending on the distribution of r^*_{it}. Therefore, we also evaluate a standardized change in r^*_{it}—specifically, the effect on house prices when moving from the 25th percentile of the r^*_{it} distribution to the 75th percentile—to more easily compare our baseline estimates to those from alternative estimation approaches. To keep the period over which we measure variation in r^*_{it} consistent across specifications, we always calculate the difference between the average 25th and 75th percentiles over the 2000–2004 period. During this time frame, there is a 130-basis-point difference between the average 25th and average 75th percentiles of the r^*_{it} distribution for our baseline sample. On this standardized basis, the corresponding gain in house prices over two years is about 3.3 percent.

Column (3) interacts r^*_{it} with a dummy variable for the housing-boom years. That is, we estimate whether the impact of r^*_{it} on house-price growth was stronger during the early 2000s run-up in house prices. The dummy variable takes a value of one from 2000 until 2004—covering house-price growth from the 2000–2002 period through the 2004–2006 period—and is zero otherwise. Broadly speaking, this approach tests whether monetary accommodation was particularly correlated with house-price growth during a period when the bubble component of house prices is likely to have been larger than in other years included in our estimation sample.[15] In short, the answer is yes. The estimated effect of r^*_{it} on house-price growth is more than two-and-a-half times larger during the housing boom than in non-boom years. During the housing boom, a 100-basis-point increase in accommodation raised house-price growth over the next two years by about 6.6 percent—suggesting that even after controlling for state-level business cycle conditions, monetary accommodation had a much greater impact on house-price growth during the housing boom. On a standardized basis, an increase in monetary accommodation led to 8.6 percent higher house-price growth during the boom years.[16] As a point of comparison, real two-year growth in house prices between the 2000–2002 and 2004–2006 periods was 9.6 percent on average across states with a standard deviation of 7.9 percent.

Columns (4) to (6) add the lag of (two-year) house-price growth (from $t-2$ to t) to the estimation. House-price growth might be serially correlated, and this approach captures any momentum in house prices that is unrelated to local economic conditions and monetary accommodation. Indeed, including lagged house-price growth boosts the adjusted R-squared of the regressions. The memo lines in the table report the relevant r^*_{it} effect on house prices, taking into account that the specifications include a lagged dependent variable. After accounting for the lagged dependent variable, the impact of monetary accommodation on house-price growth in columns (4) to (6) is very similar to the estimated effects in columns (1) to (3).[17,18]

[15] We also interacted state GSP growth with the dummy variable to control for state income growth potentially having a differential effect on house prices during the housing boom. When included as an additional regressor in the specification in column (3), this interaction effect is small and insignificant, as shown in Table A.6 in the Appendix. We therefore do not include this additional interaction in our specifications going forward.

[16] The total effect of r^*_{it} during the boom is the sum of the first and third rows in column (3).

[17] The presence of a lagged dependent variable in columns (4) to (6) raises concerns about potential dynamic panel bias. However, our results are little changed after controlling for such potential bias, as we discuss in Section A.3 in the Appendix.

[18] Our findings are very similar to those in Table 3 if we use the unrestricted IS curve estimates to calculate r^*_{it} (see Table A.7 in the Appendix).

4.2.1 Alternative Approaches for Calculating Standard Errors

Since r_{it}^{*}—the real equilibrium interest rate measure—is a generated regressor, there is potential bias in the standard errors from an ordinary least squares (OLS) regression, and it is common practice to bootstrap the standard errors. Also, r_{it}^{*} is potentially "measured" with error, so our estimate of σ could be biased toward zero. We, therefore, use a bootstrap (Monte Carlo) procedure that can address the potential measurement error as well as the bias in the standard errors.[19]

First, we retrieve the estimated coefficients $\hat{\alpha}_i$, $\hat{\nu}_t$, $\hat{\lambda}_{1b}$, $\hat{\lambda}_{2b}$, and $\hat{\theta}_{1b}$, as well as the estimated standard error s_ϵ of the residuals from equation (2)—the IS curve with restricted coefficients. At each iteration l ($l = 1, \ldots, 1,000$), we draw from an i.i.d. N(0, s_ϵ) distribution a vector of residuals $\epsilon_{it}^{(l)}$ and generate the variable $u_{it}^{(l)}$, where:[20]

$$u_{it}^{(l)} = \hat{\alpha}_i + \hat{\nu}_t + \hat{\lambda}_{1b} u_{i,t-1}^{(l)} + \hat{\lambda}_{2b} u_{i,t-2}^{(l)} + \hat{\theta}_{1b} r_{i,t-1} + \epsilon_{it}^{(l)}. \tag{9}$$

We then re-estimate the restricted IS curve:

$$u_{it}^{(l)} = \alpha_i + \nu_t + \lambda_{1b} u_{i,t-1}^{(l)} + \lambda_{2b} u_{i,t-2}^{(l)} + \theta_{1b} r_{i,t-1} + \epsilon_{it},$$

and with the new estimated coefficients, we use equation (3) to obtain equilibrium interest rates at each iteration, $r_{it}^{*(l)}$.

Next, we retrieve the estimated coefficients, $\widehat{\kappa}_i$, $\widehat{\zeta}_t$, $\hat{\sigma}$, and $\hat{\beta}$, and the estimated standard error s_ε of the residuals from equation (4), and we draw a vector of residuals $\varepsilon_{it}^{(l)}$

[19] When comparing the bootstrapped standard errors to the OLS standard errors (see Table 5), it is apparent that the OLS standard errors are too conservative. However, the OLS standard errors are not our baseline.

[20] Note that the simulated unemployment rate is generated dynamically. This requires initializing the values of the unemployment rate. We do so by taking the actual realization of the state unemployment rate in 1978 and 1979. Also, our results are very similar if we draw from the actual sample of residuals with replacement in each iteration instead of drawing a new residual sample. We further tried stratifying the residual sample by state, by year, or not at all, and obtained similar results.

from an i.i.d. N(0, s_ε) distribution and generate the variable:

$$h_{i,t+2}^{(l)} = \widehat{\kappa}_i + \widehat{\zeta}_t + \hat{\sigma} r_{it}^* + \hat{\beta}\mathbf{X}_{it} + \varepsilon_{it}^{(l)}. \tag{10}$$

Finally, we re-estimate the house-price growth equation using the different equilibrium interest rates calculated in the initial step:

$$h_{i,t+2}^{(l)} = \kappa_i + \zeta_t + \sigma r_{it}^{*(l)} + \beta\mathbf{X}_{it} + \varepsilon_{it}. \tag{11}$$

At each iteration, we record the estimates $\hat{\sigma}^{(l)}$ and $\hat{\beta}^{(l)}$. Table 4 reports the mean and standard deviation (in parentheses) of the estimated coefficients over all the iterations.

Comparing these results to our baseline results in Table 3, it seems that the bias towards zero in our baseline estimates is negligible (particularly after controlling for state-specific economic conditions). While the bootstrap standard errors are somewhat larger than the standard errors clustered by state (our baseline), all coefficients of interest remain significant at the 1 percent level or better.

We further explore clustering standard errors differently instead of bootstrapping. In particular, we cluster by state (our baseline), by year, by state-year, and we also employ Driscoll-Kraay standard errors that are robust to very general forms of cross-sectional and temporal dependence. The results are summarized in Table 5. The overall picture that emerges is that while the standard errors increase somewhat using these alternative approaches, particularly when using Driscoll-Kraay standard errors, the coefficients on our variables of interest—r_{it}^* and $r_{it}^* \times D_{2000-06}$—are always significant at the 5 percent level or better. Going forward, we continue to cluster standard errors by state, as we do in our baseline results.

4.2.2 Controlling for Housing Supply

The baseline analysis in Table 3 captures factors (state income growth and monetary policy accommodation) that mainly influence housing demand. However, housing supply may also impact house-price growth, and we extend our baseline results to control for local housing-supply conditions. In particular, we interact our measure of r^*_{it} with a dummy variable (S_i) indicating the restrictiveness of land use in a given state, based on the WLURI index. We divide states into groups (terciles) based on this index, with states in the highest tercile having the most restrictive housing-supply conditions.[21] We anticipate that demand shifts have a bigger impact on house-price growth in states with more restrictive housing supply. Therefore, the impact of r^*_{it} on house-price growth should increase with the degree of housing-supply restrictions. We interact S_i with both our direct measure of r^*_{it} and with $r^*_{it} \times D_{2000-06}$.

Column (1) in Table 6 shows that the impact of r^*_{it} on house-price growth rises with the degree of housing-supply restriction. Column (2) indicates that the impact of r^*_{it} increases monotonically with the degree of land regulation after controlling for local economic conditions. Indeed, the effect of monetary accommodation is larger in the states with the most restrictive land regulation ($S_i = 3$) than in the states with the least restrictive ($S_i = 1$) regulations (p-value 0.07). This pattern continues when we control for the housing-boom period in column (3).

The overall effect of house-price growth during the housing boom is also increasing monotonically based on housing-supply restrictions. House-price growth increases 3.3 percent for a 100-basis-point increase in accommodation in states with the least restrictive regulation, ($S_i = 1$), and increases 7.1 percent in states with the most restrictive regulation ($S_i = 3$). Again, this difference is statistically significant. Finally, the estimated impact of r^*_{it} for the most restrictive land-use states is a bit larger than the effect

[21] Dividing states into quintiles yields similar results.

during the housing boom in our baseline estimates. Overall, these findings are further consistent with monetary policy having a larger effect on house prices during the housing boom than before. The housing demand impact was simply larger, as one might expect, in states with more restrictive housing supply.

Including lagged house-price growth in the regressions, columns (4)–(6), once again improves their fit, but does not change the results materially. Overall, the results in Table 6 demonstrate that our findings are robust to controlling for differences in housing-supply restrictions across states. Moreover the results suggest that both differences in monetary accommodation across states and differences in housing-supply restrictions impact the relationship between changes in short-term interest rates and house-price growth.

4.3 Robustness

The first part of this section considers alternative and/or additional controls in our baseline house-price growth equation, such as including different measures of business cycle conditions at the state level. The second part of the section considers alternative approaches to estimating the IS curves, and hence r^*_{it}, and how these alternatives impact our estimated house-price growth effects. The final part explores the role of credit availability.

4.3.1 Alternative Controls

Table 7 shows estimates of the house-price growth regressions using alternative or additional controls. The first two columns of the table repeat, for easy reference, the baseline results in columns (3) and (6) of Table 3. We focus on these two specifications both for brevity and because they include all of our baseline controls with and without lagged house prices. Each subsequent set of two columns in Table 7 reports results with the different controls.

Columns (3) and (4) incorporate a lag of state GSP growth, in addition to contemporaneous GSP growth, which captures past state-level economic conditions that might influence house-price growth. The timing of the data are such that including this lag means that we effectively control for state-level economic activity from $t-2$ to t. Including this additional lag, however, has essentially no effect on our results of interest—suggesting that our estimate of the impact of r^*_{it} on house-price growth is not proxying for omitted data on recent local economic conditions that might lead to higher house-price growth in the future. The results are also very similar if we include the contemporaneous value and two lags of state GSP growth (not shown).

Our identification strategy relies on monetary policy having a different impact across different states. Some of the reasons we have mentioned for such a differential effect could also be consistent with productivity and demand (rather than monetary policy) affecting states' real activity, and thus state housing prices, differently. Indeed, our IS-curve estimation implies that a time t shock to state unemployment, regardless of its nature, may propagate differently in different states. Controlling for state-level economic conditions at time t in our baseline specification fully accounts for time t shocks' differential impact, but may not adequately capture differences in propagation from time t to $t+2$—the interval over which we measure the state-level change in housing prices. For this reason, columns (5) and (6) add future values of state GSP growth—between t and $t+1$ and between $t+1$ and $t+2$—to our baseline specification. This ensures that r^*_{it} does not capture features of the future economic environment that are unrelated to cross-sectional variation in the relative stance of monetary policy. The estimated effect of r^*_{it} on house-price growth is very similar even after controlling for future state-level economic conditions: an increase in monetary accommodation leads to higher house-price growth over a two-year period, especially during the housing boom.

The final two columns of Table 7 use state-level unemployment rate changes to measure local business cycle conditions instead of state GSP growth. Not surprisingly, there is a negative relationship between the change in the unemployment rate and future house-price growth. That is, an increase in unemployment leads to lower house-price growth. Still, the inclusion of the change in the unemployment rate instead of state GSP growth has little effect on the estimated relationship between r^*_{it} and future house-price growth; if anything, the estimated effect is larger. Including the change in the unemployment rate instead of state GSP growth also reduces the overall amount of variation in state-level house-price growth explained by our regressions. Overall, our results are not sensitive to how we control for local business cycle conditions.

4.3.2 Alternative Approaches for Estimating r^*_{it}

The next set of results considers the robustness of our findings to alternative approaches for calculating r^*_{it}, the degree of state-level monetary policy accommodation. These robustness checks involve re-estimating both the state-level IS curves and the house-price growth regressions. However, for brevity, we do not report all of the alternative IS curve parameter estimates.[22] The first two columns of Table 8 show our baseline house-price growth results, while each subsequent set of two columns reports results for the alternative specifications. Recall that our baseline estimates show that the effect of monetary policy on house price growth is two-and-a-half times larger during the housing boom period than before—a difference that is even bigger if we include lagged house-price growth in our estimates. In addition to the considering how the differential effect across periods changes with these alternative approaches, we will also focus on the standardized impact of r^*_{it} on house prices (that is, moving from the average 25th to average 75th percentile of the r^*_{it} distribution during the housing boom), since the alternative IS curve

[22] Additional details about these estimates are available from the authors upon request.

estimation approaches can result in more or less variability in the estimated values of r^*_{it} across states. The baseline standardized effect is 8.6 percent.

First, in columns (3) and (4) we re-estimate equation (2), grouping states not based on the two-year unemployment rate effect, but rather based on the average manufacturing share of GDP in the state over the sample period. This approach provides a way of grouping states that is outside the model estimation process, but is also relevant—state economies with higher manufacturing shares are likely more sensitive to interest rate changes.[23] Qualitatively and quantitatively the results are very similar to our baseline findings: the effect of monetary policy is larger during the housing-boom period than before. The standardized effect is somewhat larger than in our baseline estimation, so these findings are not the result of altering the variability of r^*_{it}.

We also consider estimates of r^*_{it} where, rather than using our fitted inflation measure to calculate the real interest rate (r_{it}) in the IS curve equations, we use aggregate core PCE inflation for all states. Columns (5) and (6) show these results, which along with Figure 3, demonstrate that our findings are not sensitive to the measure of inflation that we use to calculate the real interest rate in the IS curves. In particular, Figure 3 plots our baseline estimates of average r^*_{it} over time versus the estimates using core PCE inflation.[24] The average r^*_{it} estimates are very similar across time, although there is slightly more variation with the measure that uses core PCE inflation. Indeed, even though the point estimates in columns (5) and (6) are smaller than in our baseline results, the magnitude of the differential effect of monetary policy across periods and the standardized effect are quite similar to our baseline findings—we continue to find much larger house-price growth effects during the housing boom than in non-boom period.

The results in columns (7) and (8) examine the effect of r^*_{it} on house-price growth

[23] The states in each "manufacturing share" group are listed in Table A.5.

[24] For scaling purposes, the r^*_{it} estimates in the figure are calculated using the same time effects across specifications.

where we start the estimation for both the IS curves and the house-price growth equation in 1986. This shorter period may capture a time when credit markets had become fully developed, possibly leading to different sensitivities to interest rate movements. The time frame also coincides with the "Great Moderation," and likely features a more stable systematic component of monetary policy. While starting the estimates in 1986 yields smaller house-price growth effects even after standardizing the point estimates (the effect is about half as large) the message of the results is qualitatively the same: changes in monetary accommodation impact house-price growth, and this effect is larger during the housing-boom period than in the non-boom period.

In columns (9) and (10), we report results from regressions that add time-varying state-specific demographic controls to the IS curve regressions, both equation (1) and equation (2) (the unrestricted and the restricted IS curves). In particular, we add the yearly growth rate in the percentage of individuals in each state with a college degree, and the yearly growth rate in the proportion of older individuals (those 55 and older) in each state.[25] Since the changing demographics of the labor force likely impact a state's natural rate of unemployment, these additional controls help capture potential time variation in states' equilibrium unemployment rates. While we use these additional controls to improve our estimated IS curve parameters, we still calculate r^*_{it} based on equation (3). Qualitatively and quantitatively the results are similar to our baseline findings—if anything, there is an even larger differential monetary policy effect between the housing boom and non-boom periods. To further address concerns about time-varying natural rates of unemployment across states, we also re-estimate the IS curves using the state unemployment rate data for males aged 25–55 (prime age males). This segment of the labor force tends to be relatively stable over time and there is likely much less variation in their natural rate of unemployment. The results, shown in columns (11)

[25]Both the percentage of individuals with a college degree and the proportion of older individuals in a given state are obtained from the Census Bureau. The growth rates are calculated as log differences.

and (12), are once again very similar to our baseline findings. In sum, our findings and conclusions do not seem particularly sensitive to our chosen baseline estimation approach.

4.3.3 Controlling for Credit Availability

An additional potential concern with our results is that we are simply picking up differences in credit availability across states—especially as it relates to differences in mortgage lending—rather than a a differential monetary policy effect across locations and time. In other words, our results may be driven by differences in mortgage rates or the availability of housing credit across locations. While primary mortgage rates vary little if at all across states,[26] we address the credit availability concern by controlling for state-level differences in banks' financial wellbeing. In particular, we include an annual measure of the deposit-weighted share of nonperforming loans on banks' balance sheets (bank health) in each state.[27] Banks with a greater share of nonperforming loans on their balance sheets are less likely and/or less able to extend credit, so credit conditions are likely tighter in states where banks have greater amounts of nonperforming loans.[28]

If the relationship between house-price growth and monetary accommodation is just a story about differences in mortgage credit availability across states, then the effect of banks' ability and desire to lend should dominate the monetary policy effect (σ). However, this is not the case, as shown in Table 9.[29] When we add a measure of bank health

[26]Mortgage credit risk is not and was not priced differently by location. In particular, Freddie Mac and Fannie Mae (the so-called GSE lenders) and FHA loans are priced the same across the country. The price of mortgage insurance also is the same across location. For a recent example of the lack of regional differences in loan pricing see https://www.mgic.com/pdfs/71-61210_bpmi_monthly.pdf.

[27]Since a local bank's financial situation ("health") is likely impacted by local economic conditions, we use a measure of bank health that focuses on multilocational banks—those banks that exist in multiple locations and thus their financial situation is arguably exogenous with respect to the local business cycle because their balance sheets are less directly tied to the local area. Our results are robust to different thresholds and approaches to defining multilocational banks.

[28]Balance sheet data from banks with branches in multiple states are apportioned to a given state based on the share of deposits of a given bank in the state. See Appendix A.2 for more details on how the bank health data are constructed.

[29]The sample size for these estimates is somewhat smaller than in our baseline results as the bank financial condition data, which are based on Call Report data that are only available since 1984.

to our baseline estimates, the credit availability effect has the expected negative sign—states with banks in worse financial condition (higher levels of nonperforming loans) have less house-price growth. A one percentage point higher nonperforming loan share leads to 0.3 percent lower house-price appreciation. During the housing boom the bank health effect is particularly pronounced: a one percentage point higher share of nonperforming loans lowers house-price growth by 3.8 percent, all else equal. (Both bank health effects are borderline statistically significant.)

While the credit availability effect is much stronger during the housing boom—states with weaker banks and presumably less available credit experienced less house-price growth—it does little to change our estimated effect of r^*_{it} on house-price growth. The effect of monetary policy on house- price growth continues to be two-and-a-half times larger (or more) during the housing boom years than during the non-boom years. Therefore, it does not seem that our baseline results are proxying for differences in credit availability across states and over time. However, it is entirely possible that overall increased lending across the country may help explain why monetary policy accommodation seems to matter more during the housing-boom period.

5 Conclusion

We propose a novel identification strategy to assess the effect of monetary policy on housing prices. The identification relies on monetary policy having a differential impact across different U.S. states. We measure this differential effect in terms of a state-specific equilibrium real federal funds rate. This is the rate that would need to prevail to close the state economic activity gap two years out in the future. States with higher rates would need relatively less monetary accommodation, and vice versa. After controlling for state-specific economic conditions and for common conditions across states, state differences in the equilibrium real federal funds rate capture the documented fact in the literature that monetary policy has different (temporary) effects across locations (states). Given our controls, such variation in the equilibrium real rate can be taken as exogenous: monetary policy is set with the objective of achieving price stability and full employment at the aggregate level, with little regard to the fact that a certain policy may be, for example, more stimulative in certain states than in others. This identification strategy has advantages relative to the more standard practice of isolating a monetary policy "shock," for example, as the residual from an estimated policy reaction function. The reason is that the approach allows us to estimate the effect of monetary policy on housing prices in a panel of U.S. states with a rich set of controls, which stacks the odds against finding a role for our measure of monetary policy.

In all, our findings confirm other results in the literature that argue for a non-negligible role for monetary policy in affecting housing prices. More importantly, our estimation approach allows us to demonstrate that monetary policy had a much larger impact on housing prices during the early 2000s housing boom period than during the non-boom period. While one may not be able to directly translate our estimated effect across states into an exact number for the overall effect of monetary policy on house prices, we clearly show that the effect is present and it is stronger during the housing boom period.

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Table 1: Parameter Estimates for the Unrestricted IS Curve: Summary Statistics

Variable	Mean	SD	Min	Median	Max	N
Lagged Interest Rate Effect (θ_{1i})	0.28	0.07	0.14	0.27	0.47	48
Lagged Unemp. Gap Effect ($\lambda_{1i} + \lambda_{2i}$)	0.70	0.09	0.51	0.69	0.88	48
Two-Year Unemployment Rate Effect[a] ($2\theta_{1i} + \theta_{1i}\lambda_{1i}$)	0.82	0.22	0.39	0.80	1.52	48

Notes: Summary of the estimated coefficients for the unrestricted IS curve $u_{it} = \alpha_i + \nu_t + \lambda_{1i} u_{i,t-1} + \lambda_{2i} u_{i,t-2} + \theta_{1i} r_{i,t-1} + \epsilon_{i,t}$, where $u_{i,t}$ is state i's unemployment rate at time t, and $r_{i,t}$ is a time t measure of the real federal funds rate for that state. α_i and ν_t denote state and time fixed effects, respectively. Estimation Period: 1980–2007. Baseline estimates exclude AK, DC, and HI.
[a] The impact of a permanent increase in the real interest rate on the unemployment gap after two years.

Table 2: Parameter Estimates for the Restricted IS Curve

	Group[†]				
	1	2	3	4	5
Lagged Interest Rate Effect (θ_{1b})	0.22***	0.27***	0.29***	0.34***	0.39***
	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)
Lagged Unemp. Gap Effect ($\lambda_{1b} + \lambda_{2b}$)	0.70	0.69	0.73	0.73	0.75
	(0.03)	(0.03)	(0.03)	(0.02)	(0.02)
Two-Year Unemployment Rate Effect[a] ($2\theta_{1b} + \theta_{1b}\lambda_{1b}$)	0.68	0.82	0.90	1.02	1.22

Notes: Estimated coefficients for the restricted IS curve $u_{it} = \alpha_i + \nu_t + \lambda_{1b} u_{i,t-1} + \lambda_{2b} u_{i,t-2} + \theta_{1b} r_{i,t-1} + \epsilon_{i,t}$, where $u_{i,t}$ is state i's unemployment rate at time t, and $r_{i,t}$ is a time t measure of the real federal funds rate for the state. α_i and ν_t denote state and time fixed effects, respectively. Standard errors of the estimates are in parentheses where applicable. *** (**) [*] indicate significance at the 1 (5) [10]% level, respectively. Estimation Period: 1980–2007. Baseline estimates exclude AK, DC, and HI.
[†] Groups based on two-year unemployment rate effects from unrestricted IS curve regressions. Group 1 has the smallest two-year effect, while group 5 has the largest effect.
[a] Impact of a permanent increase in the real interest rate on the unemployment gap after two years.

Table 3: The Effect of Monetary Accommodation on House-Price Growth

	(1)	(2)	(3)	(4)	(5)	(6)
r^*_{it}	4.63***	2.54***	2.24***	2.96***	1.42***	1.30***
	(0.45)	(0.31)	(0.33)	(0.65)	(0.39)	(0.42)
GSP growth$_{t-1,t}$		2.08***	2.06***		1.89***	1.89***
		(0.27)	(0.28)		(0.32)	(0.33)
$r^*_{it} \times D_{2000-06}{}^{\dagger}$			4.34***			3.90***
			(1.20)			(1.13)
Lagged house-price growth$_{t-2,t}$				0.31***	0.24***	0.21**
				(0.07)	(0.07)	(0.08)
Memo:						
r^*_{it} effect‡	4.63***	2.54***	2.24***	4.28***	1.87***	1.64***
r^*_{it} total effect 2000–06$^{\diamond}$			6.58***			6.58***
R-squared	0.522	0.598	0.626	0.560	0.621	0.643
Observations	1248	1248	1248	1248	1248	1248

Notes: The table shows estimated coefficients and standard errors (in parentheses) from the equation $h_{i,t+2} = \alpha_i + \nu_t + \sigma r^*_{it} + \beta \mathbf{X}_{it} + \varepsilon_{it}$, where $h_{i,t+2}$ is real house-price growth between t and $t+2$, r^*_{it} is state-level monetary policy accommodation as of time t, $\mathbf{X}_{it}$ is a vector of additional control variables as of t, α_i are state fixed effects, and ν_t are time fixed effects. Estimation period: 1980–2005 (house-price growth through 2007). AK, DC, and HI are excluded from the analysis. *** (**) [*] indicate significance at the 1 (5) [10]% level, respectively. Standard errors clustered by state.

†Dummy variable that takes a value of 1 between 2000 and 2004 (house-price growth between 2000 and 2006) and is 0 otherwise.

‡Effect of r^*_{it} on house prices, taking into account that there is a lagged dependent variable in the regressions in columns (4)–(6).

$^{\diamond}$ Total effect of r^*_{it} on house prices during the housing-boom period.

Table 4: The Effect of Monetary Accommodation on House-Price Growth, Bootstrap Estimates and Standard Errors

	(1)	(2)	(3)	(4)	(5)	(6)
r^*_{it}	5.05***	2.65***	2.35***	3.19***	1.49***	1.36***
	(0.65)	(0.49)	(0.46)	(0.53)	(0.43)	(0.42)
GSP growth$_{t-1,t}$		2.15***	2.12***		1.90***	1.91***
		(0.18)	(0.18)		(0.18)	(0.18)
$r^*_{it} \times D_{2000-06}{}^{\dagger}$			4.92***			4.37***
			(1.07)			(1.01)
Lagged house-price growth$_{t-2,t}$				0.33***	0.25***	0.22***
				(0.04)	(0.04)	(0.04)
Memo:						
r^*_{it} effect‡	5.05***	2.65***	2.35***	4.76***	1.98***	1.91***
r^*_{it} total effect 2000–06$^{\diamond}$			7.27***			7.51***
Replications	1000	1000	1000	1000	1000	1000

Notes: See Section 4.2.1 for a description of the bootstrap procedure. We report the average of the estimated coefficients across 1,000 replications, and the standard deviation of those coefficients in parentheses from running the regression $h^{(l)}_{i,t+2} = \alpha_i + \nu_t + \sigma r^{*(l)}_{it} + \beta \mathbf{X}_{it} + \varepsilon_{it}$, where $h^{(l)}_{i,t+2}$ is real house-price growth between t and $t+2$ for replication l, $r^{*(l)}_{it}$ is state-level monetary policy accommodation as of time t, $\mathbf{X}_{it}$ is a vector of additional control variables as of t, α_i are state fixed effects, and ν_t are time fixed effects. Estimation period: 1980–2005 (house-price growth through 2007). AK, DC, and HI are excluded from the analysis. *** (**) [*] indicate significance at the 1 (5) [10]% level, respectively.

†Dummy variable that takes a value of 1 between 2000 and 2004 (house-price growth between 2000 and 2006) and is 0 otherwise.

‡Effect of r^*_{it} on house prices, taking into account that there is a lagged dependent variable in the regressions in columns (4)–(6).

$^{\diamond}$ Total effect of r^*_{it} on house prices during the housing-boom period.

Table 5: The Effect of Monetary Accommodation on House-Price Growth, Different Standard Errors

	(1)	(2)	(3)	(4)	(5)	(6)
r^*_{it}	4.63***	2.54***	2.24***	2.96***	1.42***	1.30**
	(0.25)	(0.27)	(0.26)	(0.29)	(0.29)	(0.29)
	(0.45)	(0.31)	(0.33)	(0.65)	(0.39)	(0.42)
	(0.59)	(0.48)	(0.48)	(0.56)	(0.43)	(0.47)
	(0.62)	(0.42)	(0.43)	(0.73)	(0.43)	(0.48)
	(0.75)	(0.58)	(0.62)	(0.70)	(0.50)	(0.57)
GSP growth$_{t-1,t}$		2.08***	2.06***		1.89***	1.89***
		(0.14)	(0.13)		(0.14)	(0.13)
		(0.27)	(0.28)		(0.32)	(0.33)
		(0.18)	(0.17)		(0.18)	(0.18)
		(0.27)	(0.28)		(0.32)	(0.33)
		(0.23)	(0.21)		(0.25)	(0.24)
$r^*_{it} \times D_{2000-06}{}^{\dagger}$			4.34***			3.90**
			(0.46)			(0.45)
			(1.20)			(1.13)
			(1.18)			(1.19)
			(1.50)			(1.46)
			(1.46)			(1.47)
Lagged house-price growth$_{t-2,t}$				0.31**	0.24*	0.21
				(0.03)	(0.03)	(0.03)
				(0.07)	(0.07)	(0.08)
				(0.11)	(0.11)	(0.10)
				(0.11)	(0.11)	(0.11)
				(0.13)	(0.13)	(0.12)
R-squared	0.522	0.598	0.626	0.560	0.621	0.643
Observations	1248	1248	1248	1248	1248	1248

Notes: Five standard errors are reported: (1) OLS; (2) clustered by state (baseline); (3) clustered by year; (4) clustered by state-year; and (5) Driscoll-Kraay standard errors. *** (**) [*] indicate significance at the 1 (5) [10]% level, respectively, using the highest standard error. See the notes to Table 3 for details on the regression specification.

Table 6: Monetary Accommodation and House-Price Growth,
Controlling for Housing-Supply Restrictions

	(1)	(2)	(3)	(4)	(5)	(6)
Land Reg.=1 × r^*_{it}	3.84***	2.11***	2.30***	2.61***	1.27***	1.61***
	(0.43)	(0.46)	(0.48)	(0.49)	(0.40)	(0.46)
Land Reg.=2 × r^*_{it}	4.30***	2.21***	2.14***	2.82***	1.20***	1.34**
	(0.41)	(0.32)	(0.34)	(0.69)	(0.45)	(0.50)
Land Reg.=3 × r^*_{it}	5.54***	3.31***	2.75***	3.67***	2.01***	1.77**
	(0.74)	(0.59)	(0.51)	(1.06)	(0.71)	(0.75)
GSP growth$_{t-1,t}$		2.04***	2.01***		1.88***	1.88***
		(0.26)	(0.26)		(0.31)	(0.31)
Land Reg.=1 × r^*_{it} $\times D_{2000-06}$ †			0.98			1.03
			(1.00)			(0.88)
Land Reg.=2 × r^*_{it} $\times D_{2000-06}$ †			2.38***			2.22***
			(0.88)			(0.82)
Land Reg.=3 × r^*_{it} $\times D_{2000-06}$ †			4.31***			3.98***
			(0.88)			(0.84)
Lagged house-price growth$_{t-2,t}$				0.29***	0.23***	0.18*
				(0.08)	(0.08)	(0.09)
Memo:						
[Land Reg.=1] r^*_{it} effect‡	3.84***	2.11***	2.30***	3.66***	1.64***	1.96***
[Land Reg.=2] r^*_{it} effect‡	4.30***	2.21***	2.14***	3.95***	1.55***	1.63***
[Land Reg.=3] r^*_{it} effect‡	5.54***	3.31***	2.75***	5.15***	2.60***	2.16***
[Land Reg.=1] r^*_{it} (total) effect 2000–06$^\diamond$			3.28***			3.21***
[Land Reg.=2] r^*_{it} (total) effect 2000–06$^\diamond$			4.52***			4.33***
[Land Reg.=3] r^*_{it} (total) effect 2000–06$^\diamond$			7.06***			7.01***
R-squared	0.531	0.603	0.644	0.564	0.623	0.656
N	1248	1248	1248	1248	1248	1248

Notes: The table shows estimated coefficients (and standard errors in parentheses) from the equation $h_{i,t+2} = \alpha_i + \nu_t + \sigma S_i \times r^*_{it} + \beta \mathbf{X}_{it} + \varepsilon_{it}$, where $h_{i,t+2}$ is real house-price growth between t and $t+2$, r^*_{it} is state-level monetary policy accommodation as of time t, S_i is a dummy variable indicating the degree of land-use regulation in the state, $\mathbf{X}_{it}$ is a vector of additional control variables as of t, α_i are state fixed effects, and ν_t are time effects. Estimation period: 1980–2005 (house-price growth through 2007). AK, DC, and HI are excluded from the analysis. *** (**) [*] indicate significance at the 1 (5) [10]% level, respectively. Standard errors clustered by state.

† Dummy variable that takes a value of 1 between 2000 and 2004 (house-price growth between 2000 and 2006) and is 0 otherwise.

‡Effect of r^*_{it} on house prices given S_i, taking into account that there is a lagged dependent variable in the regressions in columns (4)–(6).

$^\diamond$ Total effect of r^*_{it} on house prices given the level of land-use regulation S_i during the housing-boom period.

Table 7: Monetary Accommodation and House-Price Growth,
Alternative Controls

	Baseline		Lagged GSP		Leads of GSP		State Unemp. Rates	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
r^*_{it}	2.24***	1.30***	2.04***	1.29***	2.61***	1.32***	3.75***	1.16
	(0.33)	(0.42)	(0.34)	(0.41)	(0.37)	(0.39)	(0.43)	(0.79)
$r^*_{it} \times D_{2000-06}$	4.34***	3.90***	4.34***	3.90***	4.10***	3.54***	4.33***	3.37***
	(1.20)	(1.13)	(1.16)	(1.13)	(1.12)	(0.86)	(1.29)	(1.14)
GSP growth$_{t-1,t}$	2.06***	1.89***	1.87***	1.89***	1.18***	0.94***		
	(0.28)	(0.33)	(0.32)	(0.34)	(0.17)	(0.17)		
Lagged house-price growth$_{t-2,t}$		0.21**		0.21**		0.30***		0.35***
		(0.08)		(0.08)		(0.05)		(0.09)
GSP growth$_{t-2,t-1}$			0.36*	0.01				
			(0.18)	(0.17)				
GSP growth$_{t,t+1}$					0.86***	0.82***		
					(0.16)	(0.16)		
GSP growth$_{t+1,t+2}$					1.09***	1.26***		
					(0.17)	(0.20)		
Chg. Unemployment Rate $_{t-1,t}$							–1.32*	–3.10***
							(0.69)	(0.74)
Memo:								
r^*_{it} effect[‡]	2.24***	1.64***	2.04***	1.64***	2.61***	1.89***	3.75***	1.79*
Overall r^*_{it} effect 2000–06[◇]	6.58***	6.58***	6.38***	6.94***	6.71***	6.94***	8.08***	6.99***
R-squared	0.626	0.643	0.628	0.643	0.689	0.722	0.555	0.599
N	1248	1248	1248	1248	1152	1152	1248	1248

Notes: The table shows estimated coefficients and standard errors (in parentheses) from the equation $h_{i,t+2} = \alpha_i + \nu_t + \sigma r^*_{it} + \beta \mathbf{X}_{it} + \varepsilon_{it}$, where $h_{i,t+2}$ is real house-price growth between t and $t+2$, r^*_{it} is state-level monetary policy accommodation as of time t, $\mathbf{X}_{it}$ is a vector of additional control variables as of t, α_i are state fixed effects, and ν_t are time effects. Estimation period: 1980–2005 (house-price growth through 2007). AK, DC, and HI are excluded from the analysis. *** (**) [*] indicate significance at the 1 (5) [10]% level, respectively. Standard errors clustered by state.

[†] Dummy variable that takes a value of 1 between 2000 and 2004 (house-price growth between 2000 and 2006) and is 0 otherwise.

[‡] Effect of r^*_{it} on house prices, taking into account that there is a lagged dependent variable in the regressions in even columns.

[◇] Total effect of r^*_{it} on house prices during the housing-boom period.

Table 8: Monetary Accommodation and House-Price Growth, Alternative Approaches to Estimating r^*_{it}

	Baseline		Alternative State Groupings		Aggregate Inflation		Begin Estimation in 1986		Demographic Controls		Prime Age Males	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
r^*_{it}	2.24***	1.30***	1.45***	0.78**	1.76***	1.04***	1.20***	0.58**	2.87***	1.67***	3.05***	1.57**
	(0.33)	(0.42)	(0.26)	(0.30)	(0.25)	(0.35)	(0.26)	(0.22)	(0.48)	(0.48)	(0.50)	(0.66)
$r^*_{it} \times D_{2000-06}$ †	4.34***	3.90***	4.30***	3.98***	2.52***	2.25***	1.37	1.32*	6.76***	6.12***	5.66***	5.03***
	(1.20)	(1.13)	(0.96)	(0.87)	(0.76)	(0.73)	(0.85)	(0.73)	(1.75)	(1.62)	(1.43)	(1.43)
GSP growth$_{t-1,t}$	2.06***	1.89***	2.10***	1.91***	2.02***	1.87***	2.30***	1.99***	2.12***	1.91***	2.19***	2.00***
	(0.28)	(0.33)	(0.28)	(0.34)	(0.27)	(0.32)	(0.31)	(0.36)	(0.29)	(0.35)	(0.30)	(0.35)
Lagged house-price growth$_{t-2,t}$		0.21**		0.21**		0.21**		0.26***		0.22***		0.22**
		(0.08)		(0.08)		(0.09)		(0.06)		(0.08)		(0.08)
Memo:												
Adj. r^*_{it} effect‡	2.24***	1.64***	1.45***	0.99***	1.76***	1.31***	1.20***	0.79***	2.87***	2.14***	3.05***	2.01***
Overall r^*_{it} effect 2000–06◇	6.58***	6.58***	5.75***	6.03***	4.28***	4.16***	2.57**	2.57**	9.63***	9.99***	8.71***	8.46***
Total standardized effect 2000–06∓,		8.55***		9.44***		8.53***		4.24***		9.99***		8.46***
R-squared	0.626	0.643	0.636	0.654	0.623	0.640	0.593	0.622	0.628	0.646	0.625	0.642
N	1248	1248	1248	1248	1248	1248	1248	1248	1248	1248	1248	1248

Notes: The table shows estimated coefficients and standard errors (in parentheses) from the equation $h_{i,t+2} = \alpha_i + \nu_t + \sigma r^*_{it} + \beta \mathbf{X}_{it} + \varepsilon_{it}$, where $h_{i,t+2}$ is real house-price growth between t and $t+2$, r^*_{it} is state-level monetary policy accommodation as of time t, $\mathbf{X}_{it}$ is a vector of additional control variables as of t, α_i are state fixed effects, and ν_t are time effects. Estimation period: 1980–2005 (house-price growth through 2007), except for columns (7) and (8). *** (**) [*] indicate significance at the 1 (5) [10]% level, respectively. Standard errors clustered by state. AK, DC, and HI are excluded from the analysis.

† Dummy variable that takes a value of 1 between 2000 and 2004 (house-price growth between 2000 and 2006) and is 0 otherwise.

‡ Adjusted effect of r^*_{it} on house prices, taking into account that there is a lagged dependent variable in the regressions in even columns.

◇ Total effect of r^*_{it} on house prices during the housing-boom period.

∓ Overall effect of an increase in r^*_{it} equivalent to the interquartile range of r^*_{it} during the housing boom. The interquartile range in columns (10) and (12) is roughly 100 basis points—the same as what we use to calculate the nonstandardized effects

Table 9: The Effect of Monetary Accommodation on House-Price Growth, Controlling for (Bank) Credit Availability

	(1)	(2)	(3)	(4)	(5)	(6)
r^*_{it}	2.30***	2.30***	2.36***	1.17*	1.17*	1.24*
	(0.37)	(0.37)	(0.39)	(0.60)	(0.60)	(0.66)
$r^*_{it} \times D_{2000-06}{}^{\dagger}$	4.11***	4.12***	3.99***	3.83***	3.84***	3.73***
	(1.02)	(1.00)	(0.83)	(0.91)	(0.90)	(0.75)
GSP growth$_{t-1,t}$	2.22***	2.22***	2.19***	1.99***	1.99***	1.97***
	(0.31)	(0.31)	(0.30)	(0.40)	(0.40)	(0.39)
Bank Fin. Conditions (BFC)$^{\triangle}$		–0.29	–0.22		–0.27	–0.20
		(0.20)	(0.24)		(0.19)	(0.23)
BFC $\times D_{2000-06}{}^{\dagger}$			–3.81			–3.44
			(2.54)			(2.26)
Lagged house-price growth$_{t-2,t}$				0.22**	0.22**	0.22**
				(0.09)	(0.09)	(0.10)
Memo:						
r^*_{it} effect‡	2.30***	2.30***	2.36***	1.51**	1.51**	1.59**
r^*_{it} total effect 2000–06$^{\diamond}$	6.41***	6.42***	6.35***	6.45***	6.45***	6.37***
R-squared	0.625	0.626	0.629	0.645	0.645	0.648
Observations	1056	1056	1056	1056	1056	1056

Notes: The table shows estimated coefficients and standard errors (in parentheses) from the equation $h_{i,t+2} = \alpha_i + \nu_t + \sigma r^*_{it} + \beta \mathbf{X}_{it} + \varepsilon_{it}$, where $h_{i,t+2}$ is real house-price growth between t and $t+2$, r^*_{it} is state-level monetary policy accommodation as of time t, $\mathbf{X}_{it}$ is a vector of additional control variables as of t, α_i are state fixed effects, and ν_t are time effects. Estimation period: 1980–2005 (house-price growth through 2007). AK, DC, and HI are excluded from the analysis. *** (**) [*] indicate significance at the 1 (5) [10]% level, respectively. Standard errors clustered by state.

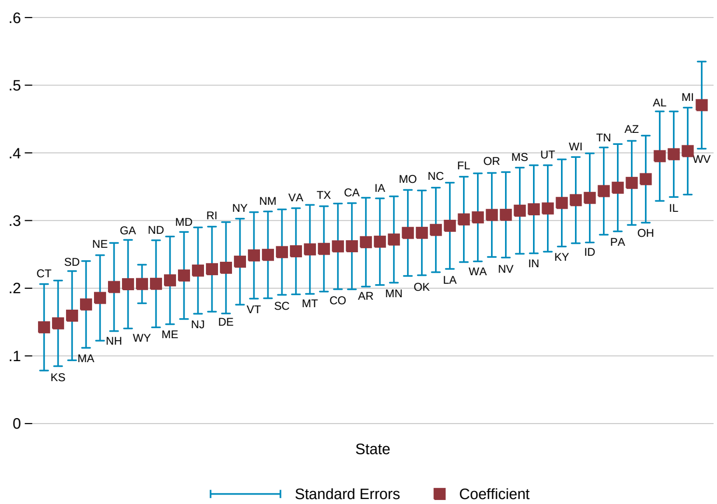
$^{\triangle}$ Continuous measure of bank financial conditions in each state based on nonperforming loan data. Higher values indicate worse conditions.

† Dummy variable that takes a value of 1 between 2000 and 2004 (house-price growth between 2000 and 2006) and is 0 otherwise.

‡ Effect of r^*_{it} on house prices, taking into account that there is a lagged dependent variable in the regressions in columns (4)–(6).

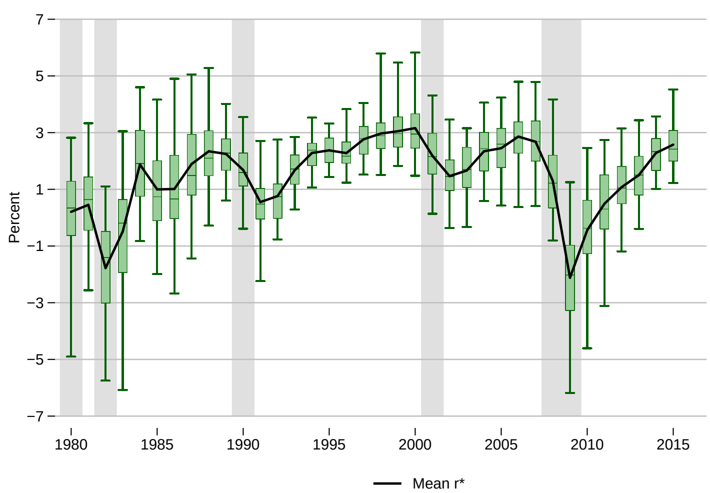
$^{\diamond}$ Total effect of r^*_{it} on house prices during the housing-boom period.

Figure 1: IS Curve Interest Rate Effect Estimates



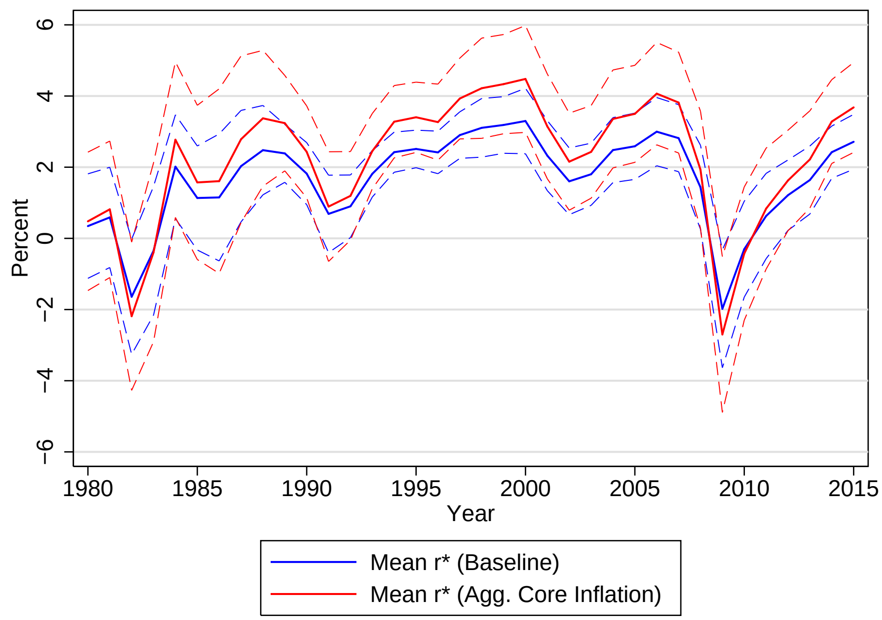
Notes: The figure shows the estimated two-year interest rate effect on unemployment by state. The squares show the estimated effect, while the whiskers show the estimated effect plus or minus the standard error of the estimate. Baseline estimates exclude AK, DC, and HI.

Figure 2: r^*_{it} Estimates



Notes: The figure shows the average equilibrium interest rate r^*_{it} across states (blue line) along with the inter-quartile range (box) and minimum and maximum values (whiskers) by year. Baseline estimates exclude AK, DC, and HI.

FIGURE 3: r^*_{it} Estimates: Aggregate Core Inflation



Notes: The figure shows the average equilibrium interest rate r^*_{it} across states for our baseline estimates (blue line) and for for IS curve estimates that use aggregate core PCE inflation to calculate the real interest rate instead of our baseline measure of fitted state-level inflation (red line). The dashed lines show the average r^*_{it} in a given year plus or minus one standard deviation. For scaling purposes, the estimated time effects used in calculating r^*_{it} for each approach are the same. The estimates exclude AK, DC, and HI.

A Appendix

A.1 Deriving r^*_{it}

As discussed in the main text, r^*_{it} is the interest rate that will close each state's unemployment gap within two years. The formula used to calculate r^*_{it} (repeated below) is based on iterating forward equation (2), setting the unemployment gap equal to zero as of time $t+2$, and setting $r_{it} = r^*_{it}$ for all t, and then making a series of substitutions. In particular, given equation (2),

$$u_{i,t+1} = \alpha_i + \lambda_{1b} u_{i,t} + \lambda_{2b} u_{i,t-1} + \theta_{1b} \widehat{r}_{it} + \nu_{t+1} + \epsilon_{i,t+1}, \tag{A.1}$$

and

$$u_{i,t+2} = \alpha_i + \lambda_{1b} u_{i,t+1} + \lambda_{2b} u_{,it} + \theta_{1b} \widehat{r}_{i,t+1} + \nu_{t+2} + \epsilon_{i,t+2},$$

setting $u_{i,t+2} = \overline{u}_i$ (a measure of state i's equilibrium unemployment rate), $\widehat{r}_{i,t+1} = r^*_{it}$ and $\widehat{r}_{it} = r^*_{it}$, and substituting for $u_{i,t+1}$ based on equation (A.1) yields:

$$\overline{u}_i = \alpha_i + \lambda_{1b}(\alpha_i + \lambda_{1b} u_{i,t} + \lambda_{2b} u_{i,t-1} + \theta_{1b} r^*_{i,t}) + \lambda_{2i} u_{it} + \theta_{1b} r^*_{i,t}.$$

We drop the terms $\nu_{t+1} + \epsilon_{i,t+1}$ and $\nu_{t+2} + \epsilon_{i,t+2}$, as the computation of the equilibrium real rate is from an expectations perspective as of time t. Note that the term includes a time effect, and the expectation for the time effect could be different from zero. As long as such expectation is constant, however, the computation goes through, but with a different constant (state-specific) term. We have already mentioned that this constant term is not crucial in our setup, as in the second stage our specification controls for state and time fixed effects. Further rearranging yields:

$$r_{it}^{*} = -\left[(\lambda_{1b}^{2} + \lambda_{2b})u_{it} + \lambda_{1b}\lambda_{2b}u_{i,t-1}\right] \times \left(\frac{1}{\lambda_{1b}\theta_{1b} + \theta_{1b}}\right) + \mu_{i},$$

which is equation (3) in the text, where

$$\mu_{i} = [\overline{u}_{i} - (1 + \lambda_{1b})\alpha_{i}]) \times \left(\frac{1}{\lambda_{1b}\theta_{1b} + \theta_{1b}}\right).$$

A.2 Constructing Measures of Banks' Financial Wellbeing

Indicators of banks' financial wellbeing come from ongoing work by Cooper and Peek (2017) and are based on Call Report (CR) and Survey of Deposit (SOD) data. The CR data provide information on banks balance sheets, while the SOD data provide information on the location of bank branches. The data are restricted to banks headquartered in the 50 U.S. states as well as the District of Columbia. The primary state-level measure of banks financial wellbeing used in this paper is a weighted average of banks nonperforming loan ratios where the weights equal the share of a banks deposits in a given state.

While one can easily use the weighted average of nonperforming loan ratios for all banks in a state, we focus on multilocation banks because this measure is arguably more exogenous with respect to local economic conditions. Indeed, a local economic shock should have much less of an impact on the financial conditions of a bank with deposits spread across multiple states than a bank that does business only in a given state. A bank is deemed multilocational in a state if less than 5 percent of its total deposits are in that state. For additional details on how the bank health data are constructed see Cooper and Peek (2017).

A.3 Arellano-Bond Estimates

In this section we reconsider our baseline estimates, taking into account the fact that some of our specifications include a lagged dependent variable and the number of time periods in our sample is somewhat short (T = 28). The potential econometric issue is that the lagged dependent variable in columns (4)–(6) of Table 3, our baseline results, could be correlated with the error term, resulting in biased estimates.[30] More specifically, the concern is that the lagged dependent variable is potentially correlated with the fixed effect component of the error term. Simply controlling for state fixed effects, as we do, does not fully address this correlation issue (see Roodman 2009 for additional details). Such correlation is not an issue in data sets with a large time dimension, since in the limit the correlation goes to zero as T increases. However, in finite samples—especially ones with $T < 30$—this correlation can impact the estimated coefficients. Arellano and Bond (1991) propose an estimator to account for these serial correlation issues.

We employ a slightly modified, more transparent version of the Arellano-Bond estimator, which is discussed in detail in Roodman (2009). A priori, it is not clear that we need to implement the Arellano-Bond estimator as we have nearly 30 years of data, and we can reject the possibility that our errors are serially correlated, based on a simple test.[31] Still, for completeness we present estimates using the Arellano-Bond estimator in Table A.1. Columns (1)–(2) replicate the specifications from columns (5) and (6) in Table 3. The estimates include time effects (not reported). State fixed effects are automatically purged by the Arellano-Bond estimation approach, since it is based on differencing the data prior to running the regressions.

The results using the Arellano-Bond estimator are very similar to our baseline find-

[30] This is an example of "dynamic panel bias" (Nickell 1981).

[31] To test for serial correlation in our estimates we first limit our analysis to every other time period to mechanically avoid serial correlation due to overlapping periods of two-year house-price growth. We then take the residuals from these estimates and regress them on their own lag. The estimated effects are close to zero and not statistically significant, suggesting that the errors are not serially correlated.

Table A.1: Monetary Accommodation and House-Price Growth, Controlling for Potential Serial Correlation

	(1)	(2)
r^*_{it}	1.79***	1.44***
	(0.32)	(0.32)
GSP growth at t	1.62***	1.60***
	(0.21)	(0.22)
L2.House-price growth	0.04	0.06
	(0.04)	(0.04)
$r^*_{it} \times D_{2000-06}$		2.73***
		(0.51)
Constant	–0.49	–3.58***
	(0.98)	(1.13)
N	1248	1248

Notes: The table shows estimated coefficients (and standard errors in parentheses) from the equation $h_{i,t+2} = \alpha_i + \nu_t + \sigma r^*_{it} + \beta \mathbf{X}_{it} + \varepsilon_{it}$, where $h_{i,t+2}$ is real house-price growth between t and $t+2$, r^*_{it} is state-level monetary policy accommodation as of time t, $\mathbf{X}_{it}$ is a vector of additional control variables as of t, α_i are state fixed effects, and ν_t are time effects. † Dummy variable that takes a value of 1 between 2000 and 2004 (house-price growth between the 2000 and 2006) and is 0 otherwise. *** (**) [*] indicate significance at the 1 (5) [10]% level. All regressions include time fixed effects and are estimated using the Arellano-Bond estimator (xtabond2 in Stata). Estimation Period: 1980–2005 (house-price growth through 2007).

ings. While the housing-boom interaction period effect is somewhat smaller than in our baseline results, the non-boom r^*_{it} effect is slightly stronger and more precisely estimated than in our baseline results. These findings suggest that our baseline estimates that include lagged house-price growth are reasonable and appear not to be impacted by serially correlated errors. Our estimates are also very similar if we drop every other period of data to avoid overlapping periods of house-price growth (not shown), given our two-year measure of house-price growth.

A.4 Additional Tables

Table A.2: Prediction Equation Estimates for State-Level Inflation

	(1)
Core PCE inflation[a]	1
	(0.01)
Relative GSP growth$_t$[b]	0.034*
	(0.02)
Relative GSP growth$_{t-1}$[b]	0.029
	(0.02)
R-squared	0.74
N	1288

Notes: We estimate the equation $\pi_{it} = \varphi_i + \pi_t^{core} + \delta_1 \tilde{y}_{it} + \delta_2 \tilde{y}_{i,t-1} + \upsilon_{it}$, where π_{it} is inflation in state i at time t as measured by the GSP deflator, π_t^{core} is aggregate core PCE inflation at time t, $\widetilde{y}_{it}$ is real GSP growth in state i at time t relative to aggregate real GDP growth, and φ_i is a state-specific intercept. *** (**) [*] indicate significance at the 1 (5) [10]% level using standard t-tests. The estimates cover the 1980–2007 period and exclude AK, DC, and HI. The regression includes state fixed effects and is weighted based on the size of the labor force in each state and year.

[a] The impact of core PCE inflation is constrained to equal 1 across all states.

[b] Relative state GSP growth is measured as the difference between real state-level GSP growth in a given year and aggregate real GDP growth.

Table A.3: States in Baseline (Two-Year Effect) Groupings

Group 1 $\theta_b = 1$	Group 2 $\theta_b = 2$	Group 3 $\theta_b = 3$	Group 4 $\theta_b = 4$	Group 5 $\theta_b = 5$
NE	RI	AR	NV	TN
ND	MT	TX	AZ	IL
NH	NJ	NC	UT	AL
KS	NM	IA	IN	WI
MA	NY	CO	ID	KY
GA	VA	OK	MS	MI
WY	SC	MO	LA	OH
ME	VT	MN	FL	PA
SD	MD	CA	WA	WV
CT	DE		OR	

Notes: AK, DC, and HI are excluded from the baseline analysis.

Table A.4: States in WLURI Terciles

Tercile 1	Tercile 2	Tercile 3
AL	GA	AZ
AR	IL	CA
IA	KY	CO
ID	MI	CT
IN	MN	DE
KS	NC	FL
MO	NM	MA
MS	NV	MD
MT	NY	ME
ND	OH	NH
NE	OR	NJ
OK	TX	PA
SC	UT	RI
SD	VA	VT
TN	WI	WA
WV		

Notes: FL and AZ appear in the top tercile, given the high impact fees faced by developers in those states, which create a barrier to new construction.

Table A.5: States in Manufacturing Share of GDP Groupings

Group 1 $\theta_b = 1$	Group 2 $\theta_b = 2$	Group 3 $\theta_b = 3$	Group 4 $\theta_b = 4$	Group 5 $\theta_b = 5$
MT	UT	IL	MN	SC
CO	NY	LA	CT	WI
WY	WV	WA	PA	IN
FL	NE	GA	AL	KY
NV	AZ	ME	NH	AR
MD	CA	RI	MS	MI
NM	VA	KS	MO	NC
ND	OK	MA	VT	OH
	SD	ID	DE	TN
	TX	NJ	OR	IA

Notes: AK, DC, and HI are excluded from the baseline analysis.

Table A.6: The Effect of Monetary Accommodation on House-Price Growth, Additional Interactions

	(1)	(2)
r^*_{it}	2.24***	2.35***
	(0.33)	(0.33)
$r^*_{it} \times D_{2000-06}$	4.34***	3.97***
	(1.20)	(1.18)
GSP growth$_{t-1,t}$	2.06***	1.95***
	(0.28)	(0.31)
GSP growth$_{t-1,t} \times D_{2000-06}$		0.72
		(0.81)
R-squared	0.626	0.628
N	1248	1248

Notes: The table shows estimated coefficients (and standard errors in parentheses) from the equation $h_{i,t+2} = \alpha_i + \nu_t + \sigma r^*_{it} + \beta \mathbf{X}_{it} + \varepsilon_{it}$, where $h_{i,t+2}$ is real house-price growth between t and $t+2$, r^*_{it} is state-level monetary policy accommodation as of time t, $\mathbf{X}_{it}$ is a vector of additional control variables as of t, α_i are state fixed effects, and ν_t are time effects. † Dummy variable that takes a value of 1 between 2000 and 2004 (house-price growth between the 2000 and 2006) and is 0 otherwise. All specifications include time and state fixed effects. Estimation Period: 1980–2005 (house-price growth through 2007). AK, DC, and HI are excluded from the analysis. *** (**) [*] indicate significance at the 1 (5) [10]% level, respectively. Standard errors clustered by state.

TABLE A.7: The Effect of Monetary Accommodation on House-Price Growth, r^*_{it} Estimates Based on *Unrestricted* IS Curves

	(1)	(2)	(3)	(4)	(5)	(6)
r^*_{it}	3.67***	2.01***	1.81***	2.44***	1.36***	1.23***
	(0.36)	(0.28)	(0.33)	(0.44)	(0.26)	(0.28)
GSP growth$_{t-1,t}$		2.19***	2.18***		1.85***	1.87***
		(0.29)	(0.29)		(0.32)	(0.33)
$r^*_{it} \times D_{2000-06}{}^{\dagger}$			2.65**			2.29**
			(1.09)			(0.96)
Lagged house-price growth$_{t-2,t}$				0.35***	0.26***	0.24***
				(0.05)	(0.06)	(0.06)
Memo:						
r^*_{it} effect‡	3.67***	2.01***	1.81***	3.74***	1.83***	1.61***
r^*_{it} total effect 2000–06$^{\diamond}$			4.46***			3.01***
R-squared	0.507	0.596	0.609	0.565	0.625	0.635
Observations	1248	1248	1248	1248	1248	1248

Notes: The table shows estimated coefficients (and standard errors in parentheses) from the equation $h_{i,t+2} = \alpha_i + \nu_t + \sigma r^*_{it} + \beta \mathbf{X}_{it} + \varepsilon_{it}$, where $h_{i,t+2}$ is real house-price growth between t and $t+2$, r^*_{it} is state-level monetary policy accommodation as of time t, $\mathbf{X}_{it}$ is a vector of additional control variables as of t, α_i are state fixed effects, and ν_t are time effects. Estimation period: 1980–2005 (house-price growth through 2007). AK, DC, and HI are excluded from the analysis. *** (**) [*] indicate significance at the 1 (5) [10]% level, respectively. Standard errors clustered by state.

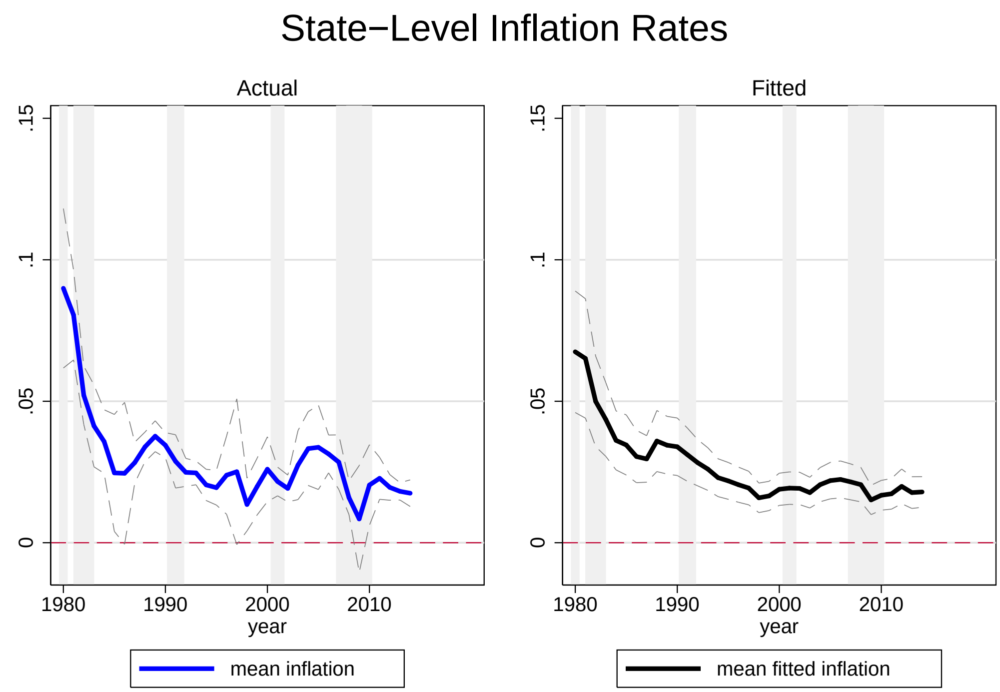
† Dummy variable that takes a value of 1 between 2000 and 2004 (house-price growth between 2000 and 2006) and is 0 otherwise.

‡ Effect of r^*_{it} on house prices, taking into account that there is a lagged dependent variable in the regressions in columns (4)–(6).

$^{\diamond}$ Total effect of r^*_{it} on house prices during the housing-boom period.

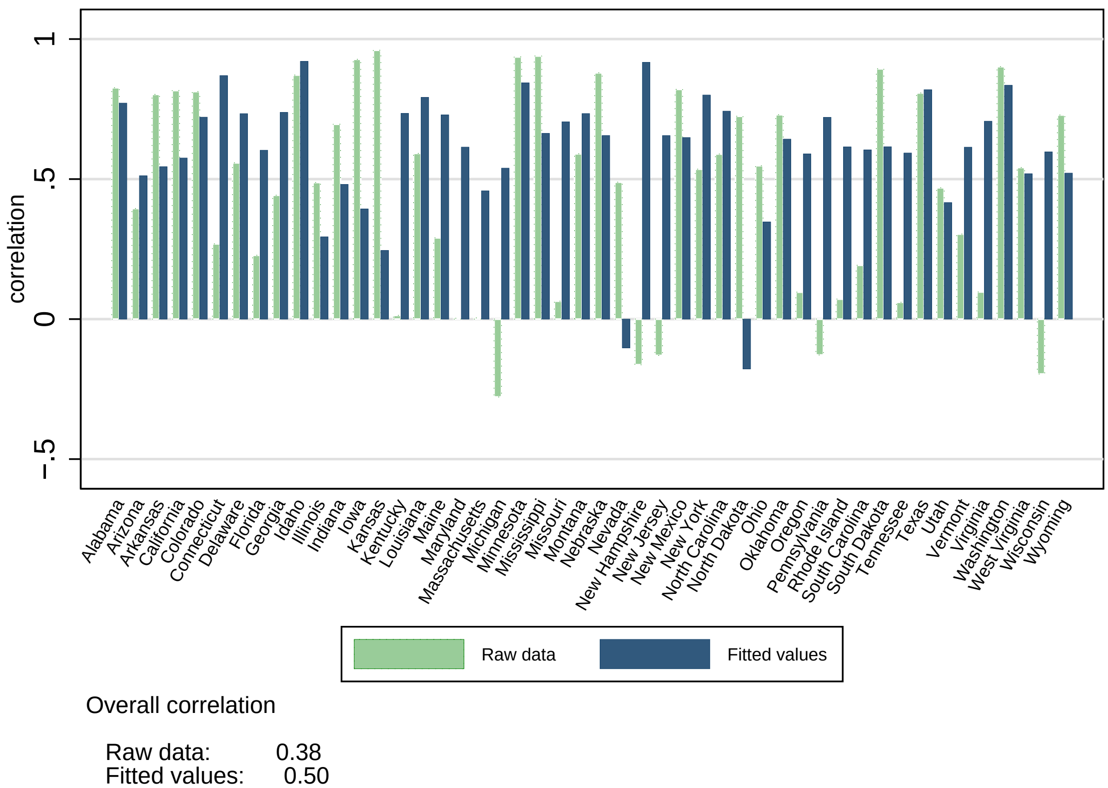
A.5 Additional Figures

FIGURE A.1: State-Level Inflation: Actual and Predicted



Note: average across states in a given year, and average plus or minus one standard deviation.

Figure A.2: Correlations of Inflation Rates Computed Using Retail Price Deflators and GSP Deflators, 2009–2013



Notes: The figure compares BEA state-level data on PCE inflation and inflation rates computed using GSP deflators, raw data, and fitted values, using equation (7). AK, DC, and HI excluded.