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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 that can be used to assess the effect of monetary policy on state-level housing prices. Policy accommodation equivalent to 100 basis points on an equilibrium real federal funds rate basis raises housing prices by about 2.5 percent over the next two years. However, the estimated effect increases to 6.6 percent during the early 2000s housing boom.

JEL Classifications: E52, E58, E43, E44

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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. There are potentially important benefits to this approach. Specifically, it provides sufficient degrees of freedom to examine potential 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 thus addresses the still-debated issue of the role played by monetary policy in the recent 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 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 areas of the United States. We take this differential impact of monetary policy across U.S. states 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. Our state-

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

level specification for the change in house prices controls for state fixed effects, state-level economic conditions, and a time effect that absorbs 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.

There now exists a relatively wide body of empirical literature documenting regional differences in the impact of monetary policy, with Carlino and DeFina (1998) and subsequent work updating their original findings being the most prominent examples. The reasons for such differences are potentially several. Industries have different 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 real effects. Per-capita incomes also differ across states, with income convergence having stalled over the past 30 years (see Ganong and Shoag 2015). With the share of liquidity-constrained households likely different across states, the ability of households to substitute spending intertemporally may also differ. Other transmission channels may also be relevant: domestic in- and out-migration flows differ markedly across U.S. regions (see Franklin 2003), potentially affecting the rate of adjustment of state-level economic conditions to interest rate changes.

In order 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 the state-level unemployment rate gap two years into the future.

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 been including 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.[2] 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 differences in the relative impact 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. Therefore, a state-level notion of the equilibrium real rate is well suited for our purposes. Such a measure also has the advantage of being expressed in terms of a real rate of interest, and thus our results can be compared and contrasted fairly easily with previous findings in the literature. Still, when interpreting movements in this variable, care should be taken in considering the horizon over which this rate needs to be maintained to achieve full resource utilization. In other words, one would expect a 100-basis-point change in the equilibrium real rate to have a different impact on housing prices when the horizon is three years versus, say, two years. For our purposes, however, the choice of the horizon over which the real rate is expected to bring the economy to full resource utilization is immaterial, as, in practice, conversion from one horizon to another can be well approximated by a multiplicative factor.

In practice, there are different ways of computing a short-run equilibrium real federal funds rate. We compute our measure by estimating a simple 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 relationship can be more precisely thought of as relating activity gaps to deviations of the real rate of interest from its longer-run value. Still, what matters in our setup 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 coefficient of the IS curve implies that the state-level equilibrium real rate is related to the state-level unemployment rate differently across different states. This is crucial in order to identify the effect of monetary policy on housing prices, as our specification for the change in housing prices 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

[2]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

differences in the way the equilibrium real rate depends on state-level unemployment rates.

Our approach to identifying the effect of monetary policy on housing prices across U.S. states should also clarify why other potential approaches to capturing differences in the monetary policy stance at the state level are not suitable for this type of exercise. For example, one could think of computing 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 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 achieve separate identification of a 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, for example because of the presence of nontraded goods. Again, this approach is not particularly promising for identifying the effect of monetary policy on housing prices, as the specification for the change in housing prices already controls 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.

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). As we already mentioned, these findings hold within a specification that controls for state-level economic conditions, state-level fixed effects, and a time effect. We further show that during the housing boom, monetary accommodation had a greater impact on house-price growth than in non-boom years. In particular, a 100-basis-point increase in monetary accommodation on an equilibrium real federal funds rate basis is estimated to have raised housing prices over the next two years by about 6.6 percent during the housing boom. In non-boom years, the effect is estimated to be about 2.25 percent. 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, as well as alternative approaches for implementing and estimating our IS curve framework. Overall, the results in this paper point to a non-negligible impact of monetary policy on housing prices during the housing-price bubble in the early 2000s.

There is now 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 this regard, 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 can be taken as exogenous, since they are responding to foreign rather than domestic conditions. It thus becomes relatively straightforward to trace out the effect of monetary policy on domestic housing prices. Our work shares some of the same flavor, 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, the work by Fratantoni and Schuh (2003) is an early application of the use of regional data—more specifically, metropolitan statistical area data—in estimating the effect of monetary policy on housing prices. There, identification and estimation of 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 and discusses some possible extensions.

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 in the aggregate 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:

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

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 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 is necessary to account 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 allow longer-run equilibrium values for the unemployment rate and the real rate to differ across states, but these differences are constant over the period over which the IS curve relationship is being estimated. Nevertheless, the presence of a time effect ν_t can still capture time variation in the natural rates, at least the portion of the variation that is common across states. The IS relationship in equation (1) is estimated at an annual frequency, with the two lags in 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. We have already noted in the introduction that these differences can be justified on several economic grounds and that a body of work has already documented the presence of a differential effect of monetary policy across U.S. states. Here, we note that the estimated impulse-responses, as reported in the original work of Carlino and DeFina (1998), indicate state-level differences in both the amplitude and persistence of the real response to a monetary policy shock. Within our IS curve setup, these differences could potentially 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). Still, allowing all the coefficients in the IS curve to vary across states may create some attenuation bias in the estimates, as our estimation 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 $\left(\frac{\delta u}{\delta r}\right)_{2yr} = 2\theta_{1i} + \theta_{1i}\lambda_{1i}$. Ranking states based on the sensitivity of their unemployment rate to the interest rate, θ_{1i}, yields similar results. Dividing states based on an external measure, such as the share of their state product from manufacturing, also yields similar results, as discussed in Section 4.3. For our baseline estimates, we group states into five bins, given the $\left(\frac{\delta u}{\delta r}\right)_{2yr}$ rankings. The bins are denoted by b, where $b \in [1, 5]$—a higher bin number corresponds to higher two-year unemployment rate effects—and we then re-estimate (1), restricting the coefficients to be the same for each state in a given group b:[3]

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

Using the parameter estimates from equation (2) we then construct for each state, ${r^*}_{it}$, which is the interest rate needed at time t to reach full employment—that is, the state-specific equilibrium value of the unemployment rate—over the next two years. Given the annual frequency of our estimation framework 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 detail in the Appendix) one can transform equation (2) such that the state-level equilibrium real interest rate at each point in time is given by:

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

The constant μ_i is state specific and will depend on the IS curve parameters, including α_i. From our standpoint, however, what matters is the first term on the right-hand-side of the equation. The second-stage regression, which we describe below, includes state fixed effects, and, as such, it will automatically control for μ_i. Section 4 reports the parameter estimates from equations (1) and (2) along with the related values for r^*_{it}. Here, it is important to note 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 in the second stage of our analysis to identify the effect of r^*_{it} on the change in state housing 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 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.

The discussion so far should also make it clear that while we try to estimate a measure of the differential impact of policy across states that has strong empirical content, such a measure will nevertheless be contaminated by measurement error. For example, as we have already mentioned, our measure does not allow for the possibility of state-specific time variation in the equilibrium unemployment rate. It is difficult to ascertain the im-

[3] We group states for estimating both the lagged unemployment rate effects and the interest rate effects, since our ranking approach depends on both unemployment and interest rates.

portance of measurement error in the present context, but it should be kept in mind when evaluating the estimated impact of monetary policy on housing prices. Measurement error should bias our estimates towards zero, and as a result our estimates should be interpreted as a lower bound to the effect of monetary policy on housing prices. Still, to the extent that one is mainly interested in evaluating the possibility of a differential impact of monetary policy on housing prices in "bubble" versus "non-bubble" periods, the presence of measurement error may not necessarily constitute a problem. For this to become an issue, one would have to argue that the extent of measurement error in our state-level measure of the equilibrium real rate of interest is systematically different in "bubble" versus "non-bubble" periods. We control for potential measurement error in Section 4.2.1, using a bootstrap approach, 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 stage of our analysis uses the estimated state-level equilibrium real rates to gauge the effect of monetary policy on house-price growth. The state-level measures of the equilibrium real rate of interest still contain the common systematic component of monetary policy. This common component needs to be controlled for in order for the equilibrium real rates to capture a state-specific stance of policy that does not represent the endogenous response to current and expected future economic conditions. We do so by controlling for a time effect in our specification for state-level house-price growth. This effect will soak up common influences on state housing prices, including the common component of monetary policy. 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}. The specification that relates monetary policy and house-price growth thus takes the following form:

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

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 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 sources of fluctuations in state house-price growth that are common to all U.S. states. 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 the horizon we have chosen for computing the equilibrium real rate of interest. Note that 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. This larger amount of policy accommodation should likely stimulate greater demand for housing. As a result, we would expect σ to be positive. That is, additional (relative) monetary policy accommodation should be positively related to house-price growth. The above relationship also makes it clear that 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.

In our baseline specifications, the vector of additional control variables 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. 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, in our baseline specifications we include 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 in equation (4).

The main factor that we do not control for relative to the previous literature on local house-price growth is a measure(s) of housing supply (see, for example, Saks 2008). State income growth and monetary policy accommodation capture mainly factors that influence housing demand. In an extension of our baseline results, we incorporate a proxy for local housing-supply conditions. In particular, we examine whether our estimated effects differ based on the degree of land-use regulation in a given state. Demand shifters, such as additional monetary policy accommodation should have a greater impact on house-price growth in areas where housing supply is relatively more inelastic.

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

3 Data and Estimation Sample

3.1 Data

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. State-level inflation data are constructed using data on Gross State Product (GSP) 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.[4] 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 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 convert them to real using the aggregate CPI-U index from the BLS. We use two-year real house-price growth (not annualized) in the baseline regressions of equation (4). We control for local economic activity with growth in real GSP. Data on land-use regulation are based on the Wharton land-use regulation Index (WLURI) from 2006. This index is 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 to simply 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 the necessary data transformations are made from the annual averages. Equations (1) and (2) are estimated over the 1980-to-2007 period. This timing takes into account data availability (given the lag structure) at the beginning of the sample. In addition, the regressions end in 2007. We do so 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

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

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.[5]

4 Results

4.1 IS Curve Estimation

Before turning to our main question of interest—how monetary accommodation impacts house-price growth—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,

$$r_{it} = i_t - \pi_{it}, \tag{5}$$

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, data are available only for a measure of state-level inflation based on the 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, it should be noted that 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

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

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 above-mentioned constraint in terms of state-level inflation data limits the available options. Preliminary analysis indicates 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 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:

$$\pi_{it} = \varphi_i + \pi_t^{core} + \delta_1 \tilde{y}_{it} + \delta_2 \tilde{y}_{i,t-1} + \upsilon_{it}, \tag{6}$$

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 (6) 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.[6] The overall correlation of BEA state inflation rates and GSP deflator-based state inflation is 0.38 for this period, while the correlation of BEA inflation rates with the fitted values from equation (6) is 0.50. (Figure A.2 in the Appendix depicts the relevant state-by-state correlations.) This test of the external validity of our predicted inflation measure helps validate the notion that our approach removes noise, rather than signal, from the GSP-based state inflation data. Section 4.3

[6]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, whereas state-level GSP deflator data are available since 1963.

explores the robustness of our results to simply using aggregate core PCE inflation data for all states. Overall, the results are quite similar. 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. The actual parameter estimates from equation (6) are shown in Table A.2 in the Appendix.

We construct our measure of the state-level real interest rate, $\widehat{r}_{it}$, based on the fitted values $\widehat{\pi}_{it}$ from equation (6) as follows:

$$\widehat{r}_{it} = i_t - \widehat{\pi}_{it}, \tag{7}$$

and we replace r_{it} with $\widehat{r}_{it}$ when estimating the unconstrained and constrained state-level IS curves (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.[7] We allow the coefficients in equation (1) to vary by state, but we do not report the individual parameter estimates, for brevity. However, we summarize our findings in Table 1 and Figure 1. In particular, 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 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 shows the estimated interest rate effects (and their standard errors) for each state. The figure demonstrates 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

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

federal funds rate would lower the national unemployment rate by roughly one-half of one percentage point after three years.[8] 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. Recent work by Beraja, Hurst, and Ospina (2016) is a notable example illustrating how the estimated regional effect of shocks can differ from the estimated national effect of the very same shocks.

To estimate equation (2) we group 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 $2\theta_{1i} + \theta_{1i}\lambda_{1i}$. Our baseline specification combines states, based on the estimates from equation (1) into five groups of roughly equal size with the cut-points for the two-year effect at 0.64, 0.77, 0.87, and 0.96 respectively.[9] 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 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 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

[8] See Brayton, Laubach, and Reifschneider (2014).

[9] Table A.3 in the Appendix reports the states in each group.

a national perspective, the unemployment rate was fairly close to the CBO estimate of the equilibrium unemployment rate. Note also that r^*_{it} is based on the parameter estimates for the 1980–2007 period, even though we use these parameters to calculate 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. 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 periods, especially low levels of the real federal funds rate were needed to restore equilibrium (close 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. Either way, the magnitude of our estimated average equilibrium real rate and, more importantly, the qualitative pattern of the rates over time are 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-to-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 and large. 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 standard errors in more detail below, including a correction based on bootstrapping.

In terms of the economic magnitude of the effects, 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 4.6 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 curves, 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 in Section 4.3. 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, which corresponds to a roughly 6 percent increase in house-price growth over two years.[10]

Column (2) adds state output (GSP) growth as a control variable. Not surprisingly house-price growth is positively correlated with higher state-level output growth. Adding GSP to the regressions explains an additional 8 percent of the variation in house-price growth, but it also reduces the r^*_{it} effect by almost one half. Still, the effect of monetary accommodation on house-price growth remains positive and precisely estimated. The impact on house prices of a 100-basis-point change in monetary accommodation increases house-price growth by about 2.5 percent over two years. On a standardized basis (moving from the 25th to 75th percentile of the r^*_{it} distribution) 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-to-2002 period through the 2004-to-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.[11] In short, the answer is yes. The

[10]We calculate the 25th and 75th percentiles of the r^*_{it} distribution in each of the relevant years and then take the difference in the average values across time.

[11]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,

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.[12] As a point of comparison, real two-year growth in house prices between the 2000-to-2002 and 2004-to-2006 periods was 9.6 percent on average across states with a standard deviation of 7.9 percent.

Columns (4) to (6) add lagged two-year house-price growth (from $t-2$ to t) to the estimates. House-price growth might be serially correlated, and this specification captures the fact that there may be some 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. Indeed, 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).[13,14]

4.2.1 Alternative Approaches to Calculating Standard Errors

Since the real equilibrium interest rate measure (r^*_{it}) is a generated regressor, there is potential bias in the standard errors reported by OLS, and it is common practice to bootstrap the standard errors. Also, r^*_{it} is potentially "measured" with error, so the point estimate of the coefficient on r^*_{it} could be biased towards zero. We, therefore, use a bootstrap (Monte Carlo) procedure that can address the potential measurement error and the bias in the standard errors.

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

as shown in Table A.6 in the Appendix. We therefore do not include this additional interaction in our specifications going forward.

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

[13]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.2 in the Appendix.

[14]Our results 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).

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:[15]

$$u_{it}^{(l)} = \hat{\alpha}_i + \hat{\nu}_t + \hat{\lambda}_{1b} u_{i,t-1} + \hat{\lambda}_{2b} u_{i,t-2} + \hat{\theta}_{1b} \widehat{r}_{i,t-1} + \epsilon_{it}^{(l)}. \tag{8}$$

We then re-estimate the restricted IS curve:

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

and with the new estimated coefficients, we use equation (3) to obtain an equilibrium interest rate 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 $\varepsilon_{it}^{(l)}$ from an i.i.d. N(0, s_ε) distribution and generate the variable:

$$h_{i,t+2}^{(l)} = \widehat{\kappa}_i + \widehat{\zeta}_t + \hat{\sigma} r_{it}^{*} + \hat{\beta} \mathbf{X}_{it} + \varepsilon_{it}^{(l)}. \tag{9}$$

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

$$h_{i,t+2}^{(l)} = \kappa_i + \zeta_t + \sigma r_{it}^{*(l)} + \beta \mathbf{X}_{it} + \varepsilon_{it}. \tag{10}$$

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. 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.[16]

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

[15] 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 tried stratifying the residual sample by state, by year, or not at all, and obtained similar results.

[16] When comparing the bootstrap 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.

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.

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. States are divided into groups (terciles) based on this index, with states in the highest tercile having the most restrictive housing-supply conditions.[17] We anticipate that demand shifts should 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 $r^*_{it} \times D_{2000-06}$, which captures the incremental effect of monetary accommodation on house-price growth during the housing boom.

Column (1) in Table 6 shows that the impact of r^*_{it} on house-price growth rises with the degree of housing-supply restrictions. 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). In particular, the differential effects of r^*_{it} during the housing boom increase monotonically with the degree of land restrictions. The differential effect for the states with the least restrictive land-use regulations is positive, but small and not statistically different from zero, while the differential effect is 4.3 percent (for a 100-basis-point increase in accommodation) for states with the most restrictive regulations.[18]

The overall effect of house-price growth during the housing boom is also increasing

[17]Dividing states into quintiles yields similar results.

[18]This difference is statistically significant at conventional levels.

monotonically based on housing-supply restrictions. House-price growth increases 3.3 percent for a 100-basis-point increase in accommodation for states with the least restrictive regulation, ($S_i = 1$), and increases 7.1 percent for 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 overall effect during the housing boom in our baseline estimates. Overall, these findings are consistent with demand driving the growth in house prices during the housing boom—the demand impact was simply larger, as one might expect, in states with less elastic (more restrictive) housing supply.

Including lagged house-price growth in the regressions, columns (4)–(6), once again improves the fit of the regressions, but does not change the results materially. Indeed, the estimated effects—especially during the housing boom—are qualitatively and quantitatively quite similar to those where we do not control for lagged house-price growth. 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 alternatives to our baseline house-price growth estimates, such as a different measure of business cycle conditions at the state level, while 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.

Alternative Baseline Specifications

Table 7 shows estimates of our baseline specification using alternative controls. The first two columns of the table repeat our baseline results from columns (3) and columns (6) in Table 3 for comparison purposes. We focus on the results from these columns here, and for the rest of our analysis, both for simplicity and because the specifications include all of our baseline controls with and without lagged house prices. Each subsequent set of two columns in Table 7 reports results from our baseline specification with alternative and/or additional 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 control, 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. Incuding the realizations of state-level real activity between t and $t+2$ controls for potential state-level differences in the propagation of shocks, thus ensuring that r^*_{it} does not capture features of the 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 control for the state-level unemployment rate change as a local business cycle indicator 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-level GSP growth has little effect on our estimated relationship between r^*_{it} and future house-price growth. 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 (lower adjusted R-squared). Overall, our results are not sensitive to how we control for local business cycle conditions.

Alternative Approaches for Estimating r^*_{it}

The next set of results considers the robustness of our findings to alternative approaches for calculating r^*_{it}. This mainly covers different ways of calculating the state-level real interest rate data that enter the IS curve equations. First, we re-estimate equation (2), where we group 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 relevant—state economies with higher manufacturing shares are likely more sensitive to interest rate changes—but is also outside the model estimation process.[19] We also consider estimates of r^*_{it} where, rather than using our fitted inflation measure to calculate the real rate (r_{it}) in the IS curve equations, we use aggregate core PCE inflation for all states. Finally, we re-estimate our baseline specification beginning in 1986 to cover only the "Great Moderation" period.

These robustness checks involve re-estimating both the state-level IS curves and the house-price growth regressions. We discuss the results from each of these alternatives below. However, for brevity, we do not report all of the alternative IS curve parameter estimates.[20]

Table 8 shows our relevant baseline house-price growth results, columns (1) and (2), along with the house-price growth estimates from the alternative specifications used to generate estimates of r^*_{it} that we just discussed. Recall that our baseline estimates imply that a 100-basis-point increase in r^*_{it} leads to a roughly 6.6 percent increase in house-price growth over two years during the housing boom. Outside of the housing-boom period, the effect is smaller—only about 1.6 percent for a 100-basis-point change in accommodation. In addition, 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) is roughly 8.6 percent. We will focus on the standardized impact of r^*_{it} on house prices when considering the alternative specifications, since the alternative IS curve estimation approaches can result in more or less variability in the estimated values of r^*_{it} across states.

Columns (3) and (4) show results where the estimates of r^*_{it} are generated using an IS curve setup that groups states based on their manufacturing share of output rather than

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

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

on the two-year effect. Overall, the results are qualitatively very similar to our baseline findings—the impact of monetary accommodation on house-price growth is much stronger during the housing-boom period. Quantitatively the standardized effect is a little bit larger than in our baseline estimates. In particular, moving from the average 25th to the average 75th percentile of r^*_{it} during the housing boom results in 9.5 percent higher house-price growth over two years—an effect that, once again, is non-trivial.

The estimates in columns (5) and (6) are based on the IS curve estimates that use aggregate core PCE inflation to calculate the real interest rate in each state rather than the fitted values from equation (6). These results 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 particular, Figure 3 plots our baseline estimates of average r^*_{it} over time versus the estimates using core PCE inflation.[21] 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, in terms of house-price effects, the point estimates in columns (5) and (6) are smaller than in our baseline results of columns (1) and (2). However, the standardized house-price growth effects are nearly the same after taking the differences in the estimated r^*_{it} variation into account. The results using core PCE inflation also continue to show much larger house-price growth effects during the housing boom than in non-boom periods.

The final set of results in columns (7) and (8) examines the effect of r^*_{it} on house-price growth when we begin the estimation period for the IS curves and 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. As already mentioned, this period 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 much larger during the housing-boom period than in the non-boom period. Overall, our findings and conclusions do not seem particularly sensitive to our chosen baseline estimation approach.

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

5 Concluding Thoughts and Policy Implications

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. 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 in the context of a framework 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. We also estimate a larger impact of monetary policy on housing prices during the housing boom. Whether the cause behind the emergence of financial imbalances is holding interest rates "too low for too long," however, is far from obvious. It is a regular feature of all business cycles that the unemployment rate eventually falls below its long-run natural level. Thus, from the IS-curve standpoint that we are using, there is always a stage in the business cycle when the real rate of interest is not high enough to prevent the unemployment rate from falling below its natural level. This does not mean that monetary policy purposely tries to overheat the economy, but rather that unexpected developments and/or a misjudgment of the underlying strength in activity eventually materialize that push the economy beyond full employment. Typically, overshooting full employment leads to a recession. However, not all recessions have been associated with financial crises.

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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: [a] The impact of a permanent increase in the real interest rate on the unemployment gap after two years. Estimation Period: 1980–2007. Baseline estimates exclude AK, DC, and HI.

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: † 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. 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.

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‡				4.28***	1.87***	1.64***
r^*_{it} total effect 2000-06$^{\diamond}$						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 effects. †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. 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 4: The Effect of Monetary Accommodation on House-Price Growth, Bootstrap Estimates and Standard Errors

	(1)	(2)	(3)	(4)	(5)	(6)
r^*_{it}	4.51***	2.45***	2.15***	2.86***	1.36***	1.22***
	(0.56)	(0.43)	(0.41)	(0.47)	(0.39)	(0.39)
GSP growth$_{t-1,t}$		2.11***	2.09***		1.90***	1.91***
		(0.19)	(0.18)		(0.19)	(0.19)
$r^*_{it} \times D_{2000-06}{}^{\dagger}$			4.21***			3.78***
			(0.88)			(0.84)
Lagged house-price growth$_{t-2,t}$				0.32***	0.25***	0.22***
				(0.04)	(0.04)	(0.04)
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. *** (**) [*] indicate significance at the 1 (5) [10]% level, respectively.

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} ×$D_{2000-06}$			0.98			1.03
			(1.00)			(0.88)
Land Reg.=2 × r^*_{it} ×$D_{2000-06}$			2.38***			2.22***
			(0.88)			(0.82)
Land Reg.=3 × r^*_{it} ×$D_{2000-06}$			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‡				3.66***	1.64***	1.96***
Land Reg.=2 × r^*_{it} effect‡				3.95***	1.55***	1.63***
Land Reg.=3 × r^*_{it} effect‡				5.15***	2.60***	2.16***
Land Reg.=1 × r^*_{it} effect 2000-06‡						1.25
Land Reg.=2 × r^*_{it} effect 2000-06‡						2.70***
Land Reg.=3 × r^*_{it} effect 2000-06‡						4.85***
r^*_{it} (total) effect 2000-06◇ [Land Reg.=1]						3.21***
r^*_{it} (total) effect 2000-06◇ [Land Reg.=2]						4.33***
r^*_{it} (total) effect 2000-06◇ [Land Reg.=3]						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. † 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). ◇ Total effect of r^*_{it} on house prices given S_i during the housing-boom period. 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 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‡		1.64***		1.64***		1.89***		1.79*
Overall r^*_{it} effect 2000-06$^\diamond$		6.95***		6.94***		6.94***		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

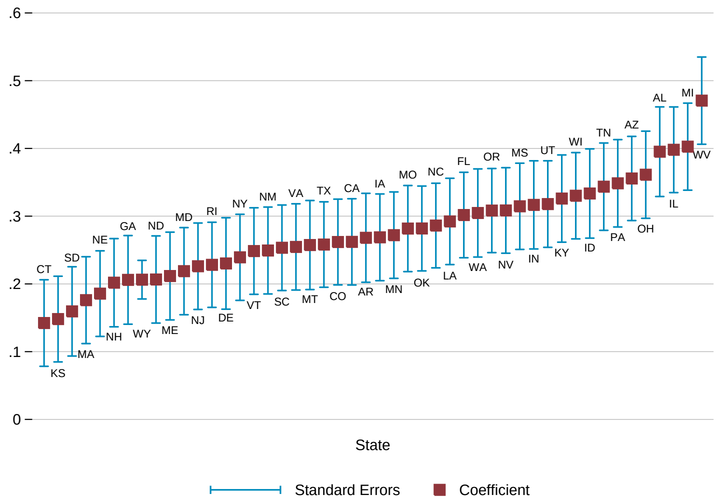
Notes: The table shows estimated coefficients (and standard errors in parentheses) from the equation $h_{i,t+2} = \alpha_i + \nu_t + \sigma r^*_{it} + \beta \mathbf{X}_{it} + \varepsilon_{it}$, where $h_{i,t+2}$ is real house-price growth between t and $t+2$, r^*_{it} is state-level monetary policy accommodation 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. † 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 (2), (4), (6), and (8). $^\diamond$ Total effect of r^*_{it} on house prices during the housing-boom period. 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 8: Monetary Accommodation and House-Price Growth, Alternative Approaches to Estimating r^*_{it}

	Baseline		Alt. State Groupings		Agg. Inflation		Begin Est. 1986	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
r^*_{it}	2.24***	1.30***	1.45***	0.78**	1.76***	1.04***	1.20***	0.58**
	(0.33)	(0.42)	(0.26)	(0.30)	(0.25)	(0.35)	(0.26)	(0.22)
$r^*_{it} \times D_{2000-06}$	4.34***	3.90***	4.30***	3.98***	2.52***	2.25***	1.37	1.32*
	(1.20)	(1.13)	(0.96)	(0.87)	(0.76)	(0.73)	(0.85)	(0.73)
GSP growth$_{t-1,t}$	2.06***	1.89***	2.10***	1.91***	2.02***	1.87***	2.30***	1.99***
	(0.28)	(0.33)	(0.28)	(0.34)	(0.27)	(0.32)	(0.31)	(0.36)
Lagged house-price growth$_{t-2,t}$		0.21**		0.21**		0.21**		0.26***
		(0.08)		(0.08)		(0.09)		(0.06)
Memo:								
Adj. r^*_{it} effect[‡]		1.64***		0.99***		1.31***		0.79***
		4.95***		5.06***		2.85***		1.80*
Total standardized effect 2000-06[∓]		8.55***		9.44***		8.53***		4.24***
R-squared	0.626	0.643	0.636	0.654	0.623	0.640	0.593	0.622
N	1248	1248	1248	1248	1248	1248	1248	1248

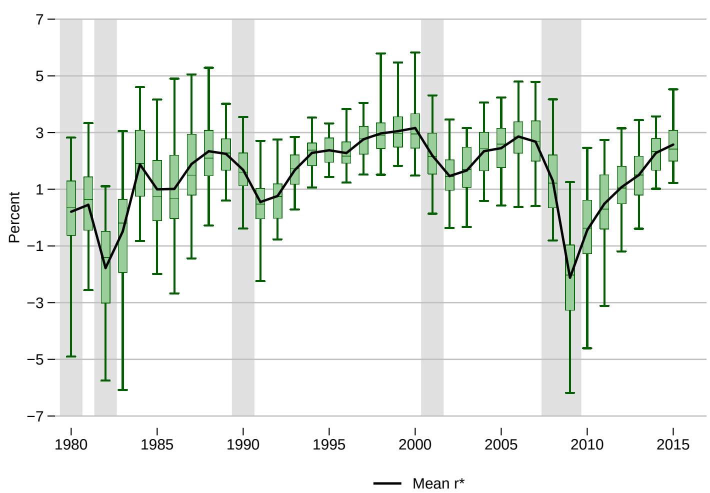
Notes: The table shows estimated coefficients (and standard errors in parentheses) from the equation $h_{i,t+2} = \alpha_i + \nu_t + \sigma r^*_{it} + \beta \mathbf{X}_{it} + \varepsilon_{it}$, where $h_{i,t+2}$ is real house-price growth between t and $t+2$, r^*_{it} is state-level monetary policy accommodation 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. [†] 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 columns (2), (4), (6), and (8). [∓] Overall effect of an increase in r^*_{it} equivalent to the interquartile range of r^*_{it} during the housing boom. 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.

FIGURE 1: IS Curve Interest Rate Effect Estimates



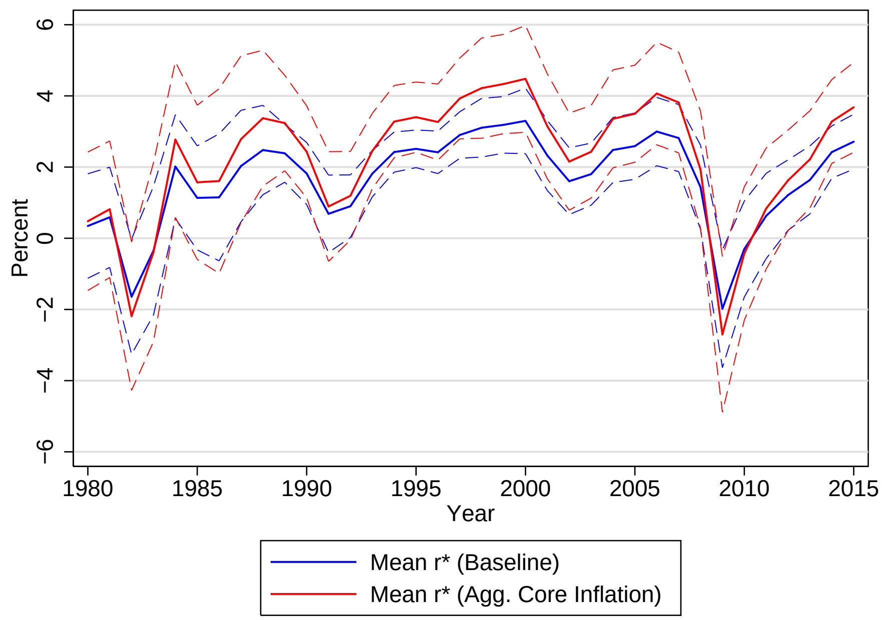
Notes: The figure shows the estimated two-year interest rate effect on unemployment by state. The squares show the estimated effect, while the whiskers show the estimated effect plus or minus the standard error of the estimate. Baseline estimates exclude AK, DC, and HI.

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),

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

and

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

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 error terms $\epsilon_{i,t+1}$ and $\epsilon_{i,t+2}$, as the computation of the equilibrium real rate is from an expectations perspective as of time t. Note that the error 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 fixed effects. Further rearranging yields:

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

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 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 our baseline results (Table 3) could be correlated with the error term, resulting in biased estimates.[22] 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.[23] 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 findings. 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.

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

[23] 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: *** (**) [*] 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).

A.3 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: *** (**) [*] indicate significance at the 1 (5) [10]% level using standard t-tests. [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. 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.

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.

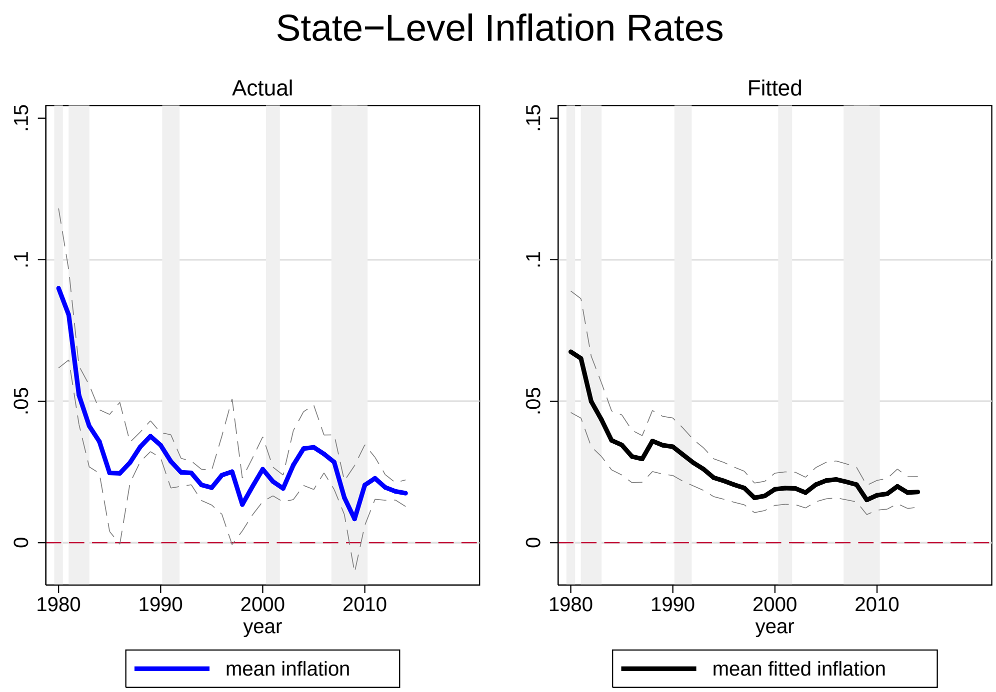
Table A.7: The Effect of Monetary Accommodation on House-Price Growth.
r^*_{it} Estimates Based on *Unrestricted* IS Curves

	(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‡				3.74***	1.83***	1.61***
r^*_{it} total effect 2000-06$^{\diamond}$						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. †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. 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.

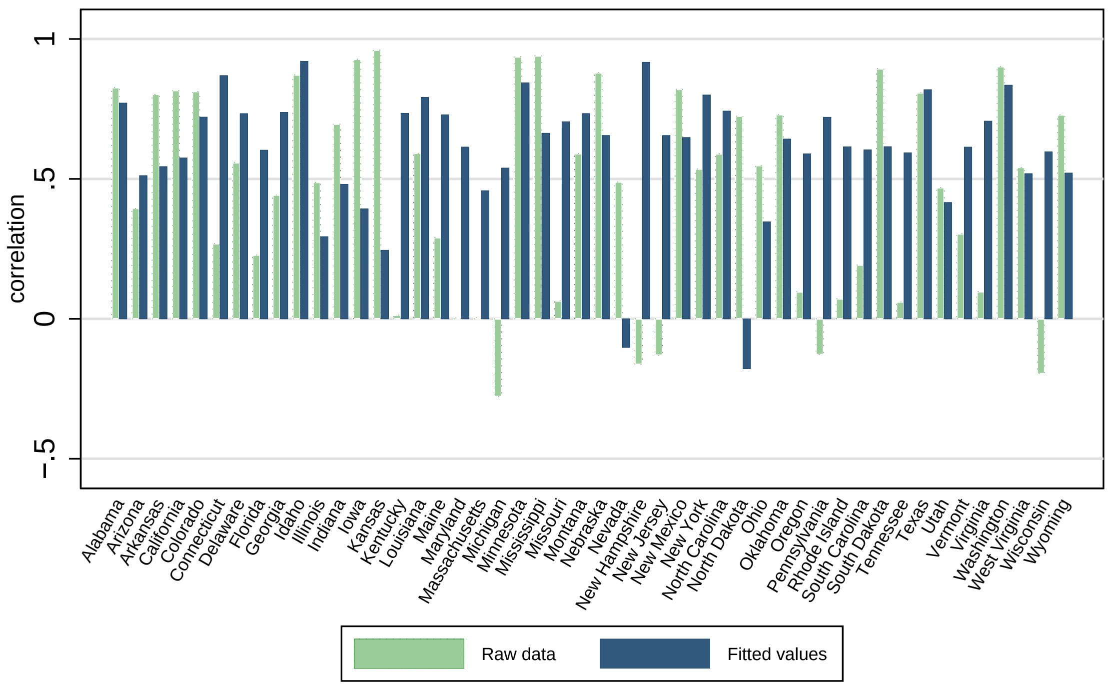
A.4 Additional Figures

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



Overall correlation

Raw data: 0.38
Fitted values: 0.50

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 (6). AK, DC, and HI excluded.