Monetary Policy and Regional House-Price Appreciation*

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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 the relative stance of policy can differ across U.S. states. This difference provides an exogenous source of variation to assess the effect of monetary policy on state-level housing prices. Monetary policy has an economically meaningful effect on state-level house-price growth—an effect that is nearly three times as large during the early 2000s housing boom as in non-boom years.

JEL Codes: E52, E58, E43, R31.

1. Introduction

Understanding the effect of monetary policy on housing prices and housing-price bubbles is relevant to inform the debate about the role of monetary policy in promoting financial stability (see, for example, Schularick and Taylor 2012, Clouse 2013, Billi and Vredin 2014). 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

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appreciation can vary over time with changes in the relative size of the bubble component.

In this paper, we revisit the impact of U.S. monetary policy on house-price appreciation. We exploit the fact that states differ in the degree to which monetary policy affects state-level economic conditions, and estimate an economically meaningful impact of monetary policy on state-level house-price growth. States where monetary policy is relatively more accommodative experience higher house-price appreciation—an effect that persists even after controlling for current, past, and future differences in state-level economic conditions. In addition, state-level differences in the relative stance of monetary policy during the early 2000s housing boom had a much greater effect on house-price growth than in non-boom years. We also show that the observed effects of monetary accommodation are stronger in states with more restrictive housing supply conditions—a finding that is consistent with monetary accommodation fueling housing demand. We further provide suggestive evidence that credit supply interacts with credit demand in driving our results.

We reach these findings using a disaggregated empirical approach, one that exploits state-level variation in U.S. housing prices and the relative stance of monetary policy. Our identification strategy is novel, in that it relies on the fact that monetary policy is set at the national level but affects real activity differently across states. Our time-varying, state-level measures of the stance of monetary policy arguably do not suffer from standard concerns about reverse causation, as monetary policy actions in the United States are aimed at aggregate outcomes, with no explicit consideration that a change in the policy rate may have a greater impact in, for example, Illinois than in Connecticut.[1] Another benefit of our

[1]A reverse causation scenario entails, for example, strong housing demand putting upward pressure on housing prices and, more generally, on economic activity. In such a situation, monetary policymakers would respond by raising the federal funds rate. Presidents of the regional Federal Reserve Banks, who are members of the Federal Open Market Committee (FOMC), could in principle have a regional bias when voting on monetary policy. The existing literature on this topic, however, has found such a bias to be small at best, and thus unlikely to have moved the entire committee away from setting policy with a nationwide focus. See Tootell (1991) and Jung and Latsos (2015).

disaggregated approach is that using state-level data provides sufficient degrees of freedom to examine possible changes in the impact of monetary policy on house-price appreciation at different points in time—for example, during the early 2000s housing boom versus other periods.

We calculate state-level differences in the stance of monetary policy by computing time-varying, state-level measures of the equilibrium real rate of interest.[2] We focus on a medium-run concept of the equilibrium real rate, which is often used as an input in setting monetary policy at the aggregate level. This equilibrium rate is the one that allows the economy to achieve full resource utilization within two to three years.[3] Indeed, the Federal Reserve Board staff has included measures of the medium-run equilibrium real federal funds rate in its briefing materials to the FOMC since the end of 2004.[4] We calculate this equilibrium rate by estimating a backward-looking IS curve at the state level. This IS curve approach specifies the state unemployment rate as a function of past unemployment rates and past real interest rates. A key feature of our estimation is that we allow for state-level variation in the IS curve parameters, under the assumption that the sensitivity of economic activity to interest rates varies across states, as does the persistence of shocks to economic activity.[5] There are therefore differences in how our estimated equilibrium real rates depend on state-level unemployment rates. As a result, the stance of monetary policy, as proxied by the equilibrium real rate, can differ across two states even when current economic conditions, as measured by the unemployment rate or other indicators, are the same in both states. This variation is crucial for identifying the effect of monetary policy on housing prices, in part because we can also include a robust set of

[2]We provide a simple example highlighting the intuition behind our approach as well as more specific details regarding how we identify an exogenous effect of monetary policy on state-level house-price growth in Section 2.

[3]This medium-term equilibrium rate will depend on the longer-run measure of the equilibrium real rate.

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

[5]A relatively wide body of empirical literature documents regional differences in the effect of monetary policy; see among others, Carlino and DeFina (1998), Owyang and Wall (2010), and Francis, Owyang, and Sekhposyan (2012).

controls to capture factors that likely influence local house prices, such as state-level output growth or net migration, which we might otherwise worry are being confounded in our measure of monetary policy accommodation.

We define the amount of relative monetary policy accommodation across states as the difference between the state-level equilibrium real rate at a point in time and the actual real federal funds rate. The lower the actual rate compared with the equilibrium rate, the more accommodative policy is in a given period and state. We regress state-level house-price appreciation on this measure of state-level monetary accommodation to examine the impact of monetary policy on house-price growth. Our specification also controls for state fixed effects, leads and lags of state-level economic conditions, state-level net migration, 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 monetary accommodation can reasonably be expected to capture the part of monetary policy that is exogenous at the state level.

Our results show larger effects of monetary policy on house prices during the housing boom (2000–06) than during the non-boom period—a finding that is robust to a variety of approaches within our general IS curve framework for calculating state-level equilibrium real rates. We also demonstrate, using a bootstrap approach, that our results are not biased by measurement error given that our relative monetary accommodation measure is a generated regressor.

There is a growing literature on the effect of monetary policy on house prices (see, among others, Del Negro and Otrok 2007, Dokko et al. 2011, Williams 2015). Work by Jordà, Schularick, and Taylor (2015) is especially relevant, in that it examines the effect of changes in interest rates on housing prices in countries that pegged their exchange rate to a foreign country's currency. Under these circumstances, changes in domestic interest rates are exogenous, since they respond to foreign rather than domestic conditions. It is therefore relatively straightforward to trace out the effect of monetary policy on domestic housing prices. Our work is related, in that we aim to isolate an exogenous monetary policy effect at the state level. The identification strategy in this paper, however, is noticeably different.

In addition, Fratantoni and Schuh (2003) use regional data—more specifically, metropolitan statistical area data—to estimate the effect of monetary policy on housing prices. The authors identify and estimate the effects of monetary policy in the context of a vector autoregression (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 incorporates a wide range of controls. This paper, therefore, also complements work by Galí and Gambetti (2015), who examine the effect of monetary policy on stock market bubbles using a VAR approach with time-varying coefficients. Their work documents substantial differences in the response 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. Our results are different, with one possible explanation being that local conditions are important for determining house prices.

The remainder of the paper proceeds as follows: the next three sections describe our empirical framework, data, and results, respectively; we present robustness analysis in Section 5 and a brief conclusion in Section 6.

2. Estimation Framework

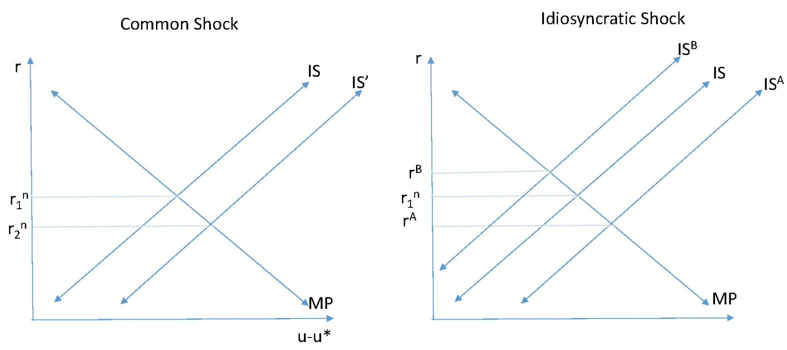
Our analysis is divided into two parts: The first part estimates state-level IS curves and constructs state-level time-varying measures of the equilibrium real interest rate—the interest rate that will close a state's unemployment gap within a specific time horizon. The objective of this first step is to construct a measure of the relative stance of monetary policy across states and over time. The second part considers whether differences in the relative stance of monetary policy predict differences in state-level house-price growth, conditional on other factors. We first, however, provide a simple, stylized example to highlight the intuition behind our approach for identifying the effect of monetary policy.

2.1 The Intuition behind Our Identification Approach

One can gain intuition for our approach to identify the relative stance of monetary policy through a stylized example. Figure 1 shows a reduced-form representation of an IS curve and a monetary policy reaction function in the interest rate and unemployment gap space (the actual unemployment rate less its natural level). The monetary policy reaction function (MP) slopes downward, which implies that policy tightens when activity is high and loosens when activity is low. Needless to say, monetary policy will also respond systematically to inflation, and changes in inflation will act as a shifter to the MP curve drawn in this interest rate–unemployment gap space. For our identification purposes, the feature of the policy reaction function that matters is that monetary policy will close the activity gap (i.e., the unemployment rate gap) over some horizon in a systematic fashion. As already discussed, the FOMC has taken guidance from policy reaction functions that aim at closing the activity gap within a specific horizon—typically two to three years. An inertial Taylor rule also sets the interest rate to close the activity gap over time, other things equal, although the inertial nature of the rule is such that full closure occurs in the limit only.

Meanwhile, the IS curve is upward sloping, given that we compare interest rates and unemployment rather than the more standard approach of plotting interest rates relative to output (lower output or income, and thus higher unemployment, is associated with less

Figure 1. Identifying the Relative Stance of Monetary Policy: Intuition



saving and higher interest rates). The intuition behind our identification strategy hinges on shifts in this IS curve as well as the idiosyncratic nature of some of these shifts across U.S. states.

Consider first the case when there is a common (negative) shock across all states (Figure 1, left panel). Since the shock is common, the IS curve shifts down, and the interest rate falls from r_1^n to r_2^n as the monetary authority responds to the (common) economic weakness with more accommodative policy. With monetary policy following the reaction function and responding to aggregate economic conditions, it is not possible to identify the effect of monetary policy on real activity and/or house prices. Consider, however, the case in which two states, A and B, are hit by idiosyncratic shocks. Because the shocks are idiosyncratic, there is no effect on the national unemployment rate gap, and thus no response from monetary policymakers, who keep interest rates unchanged.[6] This scenario is depicted in the right panel of Figure 1. State A experiences a negative idiosyncratic shock, while state B experiences a positive idiosyncratic shock. With no common component in the shock, the IS curve at the national level (labeled IS) does not move. However, the IS curve for state A shifts down. Since the shock is localized, it is not accommodated and thus a (negative) gap opens up between the national interest rate (r_1^N) and the rate that the monetary policy reaction function would predict is needed to restore equilibrium in state A over a predefined horizon (r^A). That is, the national interest rate is too high relative to what is needed in state A, and thus monetary policy is restrictive for state A. The difference ($r^A - r_1^N$) identifies an independent effect of monetary policy—what is often referred to as a monetary policy "shock"—on activity in state A and potentially also on its housing prices (and other outcomes). The same scenario occurs, with a reversed sign, in state B, where monetary policy will be too accommodative given state B's idiosyncratic economic conditions. The difference ($r^B - r_1^N$) will measure monetary policy's stimulative effect in state B. In short, with state-level idiosyncratic shocks, there will be an independent effect of monetary policy on state-level outcomes. Monetary policy will be providing too much or too little stimulus given the idiosyncratic economic conditions in

[6]This exercise assumes that any idiosyncratic shocks across states cancel out so that there is no net impact on the aggregate economy.

states A and B, and our question of interest is how much this extra (or insufficient) relative monetary policy stimulus affects state-level housing prices.

To summarize, monetary policy in our setup follows a reaction function that responds to national economic conditions and can be viewed as fully predictable. In other words, we are not focusing on monetary policy "shocks" at the national level as typically defined in the monetary economics literature. Instead, by looking at state-level economic activity, we can identify a state-level monetary policy effect that has the same interpretation as a monetary policy shock. This approach allows us to examine the effect of arguably exogenous variation in relative monetary policy accommodation across states on house-price growth. In practice, as we discuss further in the next few subsections, identification is more complicated than what this simple example would suggest—we need more than just idiosyncratic shocks—since we want to control for state-level conditions when examining the impact of monetary policy on state-level house price growth. This simple example, however, conveys the still-valid intuition that the identification of our monetary policy effect is predicated on the fact that state-level economic activity will be driven not just by common nationwide factors, to which monetary policy responds, but also by idiosyncratic factors, such as state-specific productivity or demand shocks, to which monetary policy does not respond. The lack of a response of policy to idiosyncratic shocks gives rise to the equivalent of a monetary policy shock at the state level, from which we can assess the impact of monetary policy on state-level housing prices.

2.2 Estimation Part 1: Estimating r^*_{it} and $rgap_{it}$

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 that take the following form:

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

where u_{it} is state i's unemployment rate gap at time t, and r_{it} is a time t measure of the real federal funds rate for state i. Very similar IS curve specifications have been used with aggregate data in the context of small-scale representations of the U.S. economy (for early applications, see Fuhrer and Moore 1995, Rudebusch and Svensson 1998).[7]

Our IS curve specification also controls for state fixed effects (the α_i's) and a time effect ν_t that absorbs sources of fluctuations in state unemployment rates that are common to all U.S. states as well as common fluctuations in the natural rate of interest. Allowing for state fixed effects captures the possibility that the equilibrium level of the unemployment rate differs, on average, across states. Our IS equation is written in terms of the unemployment gap. This gap is calculated assuming that state-level full employment corresponds to the unemployment rate level prevailing on average during 1995 and 1996. This is a period when, at least from a national perspective, the unemployment rate was fairly close to the Congressional Budget Office's estimate of the equilibrium unemployment rate. Any non-time varying differences between the true full employment level and the 1995–96 average will be captured in the fixed effect. As robustness, in Section 5, we consider an augmented IS curve that accounts for potentially time-varying state-level equilibrium unemployment rates. The state fixed effect also controls for state-level differences in the longer-run equilibrium value of the real federal funds rate.

We construct the state-level real federal funds rate, r_{it}, based on the nominal (aggregate) funds rate, i_t, and a measure of state-level inflation π_{it}: $r_{it} = i_t - \pi_{it}$. Inflation data at the state level are only available using gross state product (GSP) deflators for our baseline sample period (1980–2007). At the aggregate level the behavior of the GDP deflator and the core PCE deflator are similar; however, this is not necessarily the case at the state level. Indeed, we find that state-level estimates of the equilibrium real interest rate based on GSP inflation tend to be fairly volatile, as they inherit the volatility of the inflation data. In order to have our inflation measure better reflect core PCE inflation and to reduce the potential for measurement error in the equilibrium real interest rate series, we compute a predicted measure of state-level inflation, $\widehat{\pi}_{it}$, based on the portion of GSP inflation that is predictable given aggregate core PCE inflation

[7] While the IS curve specification in Equation (1) is backward-looking, and thus lacks microfoundations, the difficulties associated with estimating microfounded, forward-looking IS curves have been well documented in the literature (see Fuhrer and Rudebusch 2004).

and the difference between state-level GSP growth and aggregate GDP growth. The measure of the state-level real federal funds rate therefore becomes $r_{it} = i_t - \widehat{\pi}_{it}$. This approach, which we discuss and evaluate in more detail in Appendix A.2, assumes that changes in aggregate core PCE inflation feed one-for-one into state-level inflation and succeed at reducing some of the noise in our measure of state-level inflation (see Figure A.1).

We estimate the IS relationship in Equation (1) at an annual frequency, with the two lags of the unemployment rate capturing the persistent features of this variable, and the lag in the real federal funds rate reflecting the delayed effects of interest rate changes on real activity. In addition, we allow the IS curve parameters $\{\lambda_{1i}, \lambda_{2i}, \theta_{1i}\}$ to vary across states—a feature that is important for identifying the effect of monetary accommodation on house-price appreciation. This imposed parameter variation can be justified given the documented differential effect of monetary policy across U.S. states in the existing literature. In particular, Carlino and DeFina (1998) find state-level differences in both the amplitude and persistence of the real response to a monetary policy shock. Indeed, industries likely respond differently to changes in interest rates, and thus variation in the regional industry mix could lead to varied monetary policy responses across locations. The mix of small (bank-reliant) firms versus large (not bank-reliant) firms in a region can also lead to differential effects. The ability of households to substitute spending intertemporally in response to interest rate changes may also differ across states.[8] Domestic in- and out-migration flows also vary markedly across U.S. regions (see Franklin 2003)—likely affecting the rate of adjustment of state-level economic conditions to interest rate changes across states. These differences translate into different parameter values at the state level for both the interest rate sensitivity coefficient (θ_1) and the coefficients measuring the intrinsic persistence of the real activity variable (λ_1 and λ_2).

Allowing the IS curve coefficients to vary across all states may create bias in the parameter estimates, as our sample is relatively short. For this reason, as well as to decrease potential measurement error in our estimates, we first estimate Equation (1) without any

[8] Indeed, per capita incomes also differ across states, with income convergence having stalled over the past 30 years; see Ganong and Shoag (2017).

restrictions and rank states based on the impact of a change in the real interest rate on the unemployment rate after two years. This effect can be shown to amount to $\left(\frac{\delta u}{\delta r}\right)_{2yr} = 2\theta_{1i} + \theta_{1i}\lambda_{1i}$. Given these $\left(\frac{\delta u}{\delta r}\right)_{2yr}$ rankings, we group states into five bins. The bins are denoted by b, where $b \in [1, 5]$.[9] We then reestimate (1), restricting the coefficients to be the same for each state in a given group b (we explore an alternative approach for ranking states in Section 5):

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

Using the parameter estimates from Equation (2) we compute r^*_{it}, which is the interest rate needed at time t in a given state to reach full employment (the state's equilibrium unemployment rate) over the next two years. Given the annual frequency of our data and the delayed impact of monetary policy implied by our IS curve specification, this amounts to closing the unemployment rate gap in about three years, which is the horizon typically used by policymakers when estimating the medium-run equilibrium real policy rate. We discuss this notion of the equilibrium real rate further in Appendix A.

Some simple algebra (also in Appendix A), yields the following expression for the state-level equilibrium real interest rate:

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

Equation (3) shows that the equilibrium real interest rate is a function of the state-level unemployment gap and the estimated group-level IS parameters.

To analyze the impact of monetary accommodation on house prices, we define the relative stance of monetary policy across locations, $rgap_{it}$, as the difference between the location's equilibrium real rate and the actual real rate:

$$rgap_{it} = r^*_{it} - r_{it}.$$

[9]Higher bin numbers correspond to states with higher two-year unemployment rate effects from interest rate changes.

The stance of monetary policy is relatively more accommodative in a location i at a point in time t, the lower the actual real rate is relative to the equilibrium rate. To the extent that there is variation in the estimated parameters from Equation (2), it is possible to identify the effect of $rgap_{it}$ on house prices even when we control for state-level business cycle conditions and time effects in the second part of our analysis.[10] For example, with sufficient variation in θ_{1b}, two states can have the same current economic conditions but different equilibrium rates because of the mix of local industries and hence different sensitivities to interest rate changes. That is, the variation in r^*_{it} relative to local conditions is not the same across states.

We provide additional detail and robustness checks for our IS curve estimation results in Section 5.

2.3 Estimation Part 2: The Effect of $rgap_{it}$ on House-Price Growth

The main part of our analysis focuses on using $rgap_{it}$ to gauge the effect of the relative stance of monetary policy on local house-price growth. To capture the state-specific stance of monetary policy that does not represent the endogenous response of policy to current and expected future economic conditions, we must control for the common component of monetary policy that is still potentially embedded in our estimate of r^*_{it} and hence our measure of $rgap_{it}$. We do so by including a time effect in our specification—a fairly parsimonious approach that should help isolate the effect of relative monetary accommodation on house-price growth across locations.

More specifically, we estimate the following empirical equation relating house-price growth to the relative stance of monetary policy:

$$\begin{aligned} h_{it,t+2} = \sigma_1 rgap_{it} + \sigma_2 rgap_{it} \times D_{00-06} + \alpha h_{it,t-2} \\ + \beta \mathbf{X}_{it} + \kappa_i + \zeta_t + \varepsilon_{it}, \end{aligned} \tag{4}$$

[10]The constant μ_i in Equation (3) will depend on the IS curve parameters, including α_i. Since our house-price growth specification, which we describe next, includes state fixed effects, it automatically accounts for μ_i.

where $h_{it,t+2}$ is real house-price growth in state i from time t to time $t+2$, D_{00-06} is a dummy variable for the housing-boom years,[11] $rgap_{it}$ is our measure of state-level monetary accommodation, $h_{it,t-2}$ is lagged real house-price growth in state i from time $t-2$ to t, and $\mathbf{X}_{it}$ is a vector of additional state-level controls. Lagged house-price growth captures any serial correlation or momentum in house prices that is unrelated to local economic conditions and monetary accommodation. The specification also controls for state fixed effects (the κ_i's) and a time effect ζ_t that absorbs fluctuations in state house-price growth that are common to all U.S. states, including the common component of monetary policy. The timing of the variables in Equation (4) allows for the effect of monetary policy on house prices to build over time. The two-year future change in housing prices also covers the same time span implicit in our horizon for computing the state-level equilibrium real interest rates.

With time and state fixed effects in Equation (4), identification of the monetary policy effect 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 a particular effect difficult and potentially hard to interpret. Therefore, Equation (4) includes the regressors that we deem most relevant for predicting state-level house-price growth. The vector of additional control variables, $\mathbf{X}_{it}$, includes leads and lags of real GSP growth including contemporaneous and lagged real GSP growth as well as real GSP growth between t and $t+1$ and $t+1$ and $t+2$—the period over which we measure house-price growth. It is important to control for current and past state-level economic growth since we are interested in the effect of monetary accommodation on house prices that is orthogonal to local demand conditions. Including leads of real GSP growth ensures that $rgap_{it}$ does not capture features of the future economic environment that are unrelated to cross-sectional variation in the relative stance of monetary policy. That is, we are interested in variation in monetary accommodation across locations that is due to the differential regional effects of monetary policy, not in the variation in the equilibrium real that is

[11]The dummy variable takes a value of one from 2000 until 2004—covering house-price growth from the 2000–02 period through the 2004–06 period—and is zero otherwise.

driven by, for example, state-specific demand or productivity shocks. We also interact the elements of $\mathbf{X}_{it}$ with the dummy variable for the housing-boom years and include a measure of state-level net migration, and as a robustness check, we also control for the state-level unemployment rate.[12] Controlling for migration across states captures potential spillover effects of house-price changes or other differences in economic conditions across states.

Based on our setup in Equation (4), we would expect σ_1 to be positive—additional (relative) monetary policy accommodation should stimulate house-price growth. For a given value of r_{it}, a higher level of $rgap_{it}$ implies a higher value of the equilibrium real rate r^*_{it} and thus a greater relative degree of monetary policy accommodation in state i at time t. Similarly, if variation in monetary accommodation across states had a differentially larger effect during the housing boom, then σ_2 should also be positive.

2.3.1 Identification

Our identification of the effect of monetary policy on house prices differs from other approaches that rely on aggregate policy shocks or other time-series evidence. To help interpret our results, we can rewrite the expression for the state-level equilibrium real rate of interest, Equation (3), as follows:

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

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

[12] These alternative specifications have limited impact on our estimated effect of relative monetary policy accommodation, as discussed in Section 4.1.

the coefficients that pre-multiply the unemployment gap in Equation (3)—separating them into their common (across states) and idiosyncratic components. These coefficients are defined similarly, so that $\sum_{i=1}^{N} w_i \chi_i = 0$. In addition, the actual real federal funds rate for a given state can be written as $r_{it} = i_t - \pi_t - e_{it} = r_t - e_{it}$, with $\sum_{i=1}^{N} w_i e_{it} = 0$.

Abstracting from the interaction term for clarity, we can rewrite Equation (4) substituting the expressions above for $rgap_{it}$ to obtain

$$\begin{aligned} h_{it,t+2} = \sigma \left[\chi_i \left(\overline{u}_t + z_{it}\right) + \overline{\chi}\,\overline{u}_t + \overline{\chi} z_{it} - r_t + e_{it}\right] \\ + \beta \mathbf{X}_{it} + \kappa_i + \zeta_t + \varepsilon_{it}, \end{aligned} \tag{5}$$

where σ is the elasticity of house prices with respect to the stance of monetary policy.

Equation (5) shows that the effect of monetary policy on housing prices is identified mainly via variation in the first term in the bracketed expression, $\chi_i \left(\overline{u}_t + z_{it}\right)$. The other terms, $\overline{\chi}\,\overline{u}_t$, $-r_t$, $\overline{\chi} z_{it}$, and e_{it}, are subsumed in the time effect, ζ_t, and state-level economic conditions, $\mathbf{X}_{it}$, respectively.[13] The first term in the bracketed expression captures the portion of the interest rate effect that is specific to state i, given that χ_i, but not $\overline{\chi}$, is included. As such, this effect should be orthogonal to the systematic component of monetary policy, since monetary policy is set with aggregate goals in mind.

Our identification approach highlights why other potential approaches to capturing state-level differences in monetary accommodation are not suitable for evaluating the relationship between monetary policy and house-price appreciation. For example, one could compute a state-level policy rate as predicted by a Taylor rule, and then take the difference between this rate and the actual state-level federal funds rate as a measure of the relative stance of monetary policy. However, the use of a common Taylor rule implies

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

that the way the relative stance of policy depends on state-level economic conditions is the same across states. Since the specification for house-price growth controls for state-level economic conditions, it will not be possible to separately identify the monetary policy effect. In sum, what matters for identification is not that there are state-level differences in economic conditions, but rather that there is a differential impact of monetary policy across states for a given set of economic conditions.

2.3.2 Interpretation of σ

We are primarily interested in whether there was a significant increase in the sensitivity of house prices to monetary accommodation during the bubble period, as captured by the estimated coefficient σ in Equation (5). Using state-level variation provides sufficient degrees of freedom to test for whether the effect of monetary policy on house-price appreciation changes across time periods.

Translating our estimated σ into an aggregate monetary policy effect is not completely straightforward. Our estimate of σ can be interpreted as a measure of the aggregate effect of monetary policy on housing prices to the extent that a (weighted) average of the state-level measures of the equilibrium real rate of interest provides a good proxy for the aggregate equilibrium real rate. The aggregate effect of monetary policy, as measured by the difference between the aggregate equilibrium real federal funds rate and the actual real federal funds rate, $r_t^* - r_t$, falls out from Equation (5) if $\overline{\chi}\,\overline{u}_t$ properly measures the equilibrium real federal funds rate for the United States as a whole. This may not be the case, though, as the estimated parameters in our state-level IS curve do not necessarily average to values that correspond to representative aggregate IS curve parameters. For example, state-specific productivity shocks may generate cross-state migration—a margin of adjustment that is not present with aggregate productivity shocks.

However, we control for net migration across states and since our outcome of interest is house-price growth and houses are immobile, aggregating our state-level estimates may be less of an issue than in a situation where there is mobility for the outcome variable (e.g., factory jobs) as well. Either way, one can also think about obtaining an estimate of the aggregate effect of monetary policy on house prices by adjusting σ to take into account the average bias arising from the difference between the weighted average of the state-level measures of the equilibrium real interest rate, $\overline{\chi}\,\overline{u}_t$, and a measure of the equilibrium real federal funds rate derived from estimating an IS curve at the aggregate level.[14]

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

2.3.3 Other Considerations

While $rgap_{it}$ 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} and hence $rgap_{it}$ differently across states), we assume that the relationship between $rgap_{it}$ and housing prices is the same across states as summarized by the semi-elasticities σ_1 and σ_2. We relax this assumption later on, allowing for the parameter to vary with local housing supply constraints (Section 4.2). If monetary accommodation influences housing demand, then we would expect to see larger effects in areas with more limited housing supply.

Also, given that $rgap_{it}$ is a generated regressor, our results could be contaminated by measurement error. Ascertaining the magnitude of any such measurement error in our estimates is difficult, but any error that exists should bias our results towards zero. Therefore, our findings can be interpreted as a lower bound of the effect of relative monetary accommodation on housing prices. In addition, the presence of measurement error is likely not a problem for our analysis when evaluating the differential effect of monetary policy on housing

[14]The differences between the cross-sectional and the aggregate effects of a particular shock have been highlighted in work by, among others, Nakamura and Steinsson (2014) and Beraja, Hurst, and Ospina (2019). These authors focus on shocks that are state-specific in nature, whereas in our case monetary policy is set at the national level and is common to all states.

prices in "bubble" versus "non-bubble" periods. For measurement error in $rgap_{it}$ to be an issue, one would have to argue that it is systematically different in "bubble" versus "non-bubble" periods. To further address this concern, we use a bootstrap approach that yields results very similar to our baseline findings (see Section 4.4).

3. Data and Estimation Sample

3.1 Data Sources

Data on state-level unemployment come from the Bureau of Labor Statistics (BLS) and are available starting in the late 1970s. We construct state-level inflation data using GSP data from the Bureau of Economic Analysis (BEA). These data are available starting in 1963. We define state-level inflation as the change in each state's GSP deflator.[15] We use the effective federal funds rate (from the Federal Reserve Board) as our measure of the nominal interest rate. Aggregate data on core inflation come from the BEA and are measured as the year-over-year percent change in the deflator for personal consumption expenditures (PCE) excluding food and energy.

In addition, we obtain nominal state-level house-price indices from CoreLogic and, following the existing literature, we deflate them using the aggregate CPI-U index from the BLS. (Our results are qualitatively the same and quantitatively very similar if we use our fitted state-level inflation data, $\widehat{\pi}_{it}$, to deflate house prices.) We use two-year real house-price growth (not annualized) in our baseline estimates of Equation (4). Using one-year or three-year real house-price growth yields similar (annualized) results. Data on housing supply restrictions come from the Wharton Land-Use Regulation Index (WLURI). 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 index is not time varying, but land-use restrictions change little over time and we employ the index only to divide states based on their relative level of housing

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

supply constraints.[16] Finally, we also compute net population inflows to each state (inter-state migration) using data from the Internal Revenue Service.

3.2 Estimation Sample

We estimate our baseline IS equations using annual data to reduce noise, over the sample period 1980–2007. This timing takes into account data availability (given the lag structure) at the beginning of the sample. The estimates end in 2007 because the Great Recession and the ensuing zero-lower-bound period may have altered the relationship between the real federal funds rate and economic activity. Away from the zero lower bound, changes in the real federal funds rate elicit changes in other borrowing rates that are relevant for consumers' and businesses' spending decisions. At the zero lower bound, the relationship between the real federal funds rate and other borrowing rates changed noticeably, as the FOMC 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.[17]

4. Results

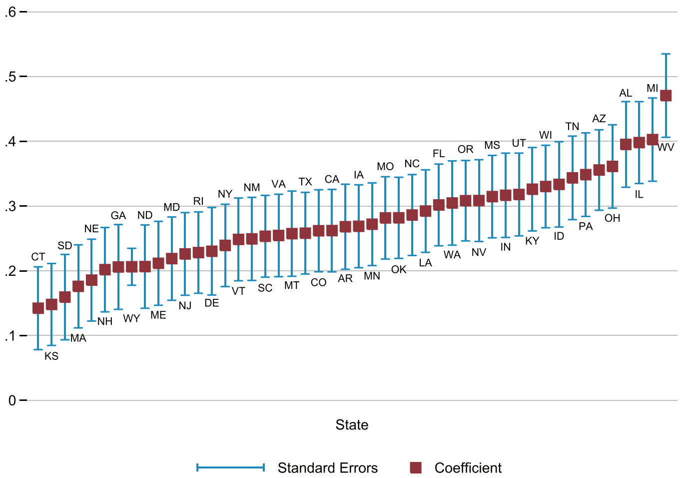
4.1 Relative Monetary Policy Accommodation and House-Price Growth

Our IS curve estimates document substantial variation across states in the impact of interest rates on unemployment, as shown in

[16]The WLURI is constructed at the town/city level. We aggregate it to the state level weighting by population.

[17]Including the District of Columbia has little impact on our results.

Figure 2. IS Curve Interest Rate Effect Estimates



Note: 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.

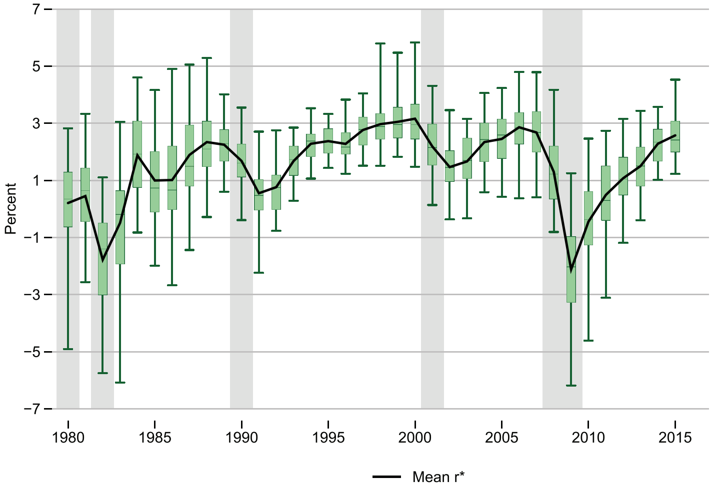
Figure 2.[18] This is consistent with the aforementioned previous literature showing a differential effect of monetary policy on state-level economic activity. The variation in these estimates is a crucial piece of our identification strategy.

Figure 3 shows the calculated values for our state-level measure of the equilibrium real federal funds rate (r^*_{it}) from our IS curve framework, which is the main driver of our measure of $rgap_{it}$—the relative stance of monetary policy across states. These data are constructed using Equation (3) given the IS curve estimates from Equation (2). The figure plots the average r^*_{it} across states (solid line) as well as the interquartile range and minimum and maximum values by year.

On average, the real interest rate needed to close the unemployment rate gap falls during economic downturns and rises during

[18] All of the state-level interest rate effects are also significantly different from zero.

Figure 3. r^*_{it} Estimates



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

periods of economic recovery and growth.[19] 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. Especially low levels of the real federal funds rate were needed during these episodes to restore equilibrium. Not surprisingly, the average estimated r^*_{it} is somewhat more negative during the Great Recession, the most severe economic downturn since the Great Depression (at the time of our analysis), than during the early 1980s. The difference, however, is not large. Most importantly, the qualitative (and quantitative) pattern of r^*_{it} over time is broadly consistent with our own estimates of the medium-term equilibrium real rate using aggregate data (not shown) and previous estimates in the literature.

[19]We calculate r^*_{it} based on parameter estimates from Equation (2) for the 1980–2007 period, but we use these parameters to project r^*_{it} over a longer sample horizon (1980–2015) to check the out-of-sample behavior of the series.

Turning our attention to house prices, our estimates of Equation (4) show that conditional on local economic growth there is a positive and precisely estimated relationship between the relative stance of monetary policy across locations and state-level house-price growth (see Table 1). The memo lines in the table report the effect of $rgap_{it}$ on house prices taking into account the lagged dependent variable, and all estimates include state fixed effects and a full set of time dummies (not shown). In addition, we cluster the standard errors at the state-by-year level to account for the fact that there might be spatial correlation in the errors across states within a year due to the common component of monetary policy. This approach to clustering is quite conservative, but we still find significant effects of monetary accommodation on house-price growth. Alternative approaches to calculating the standard errors such as Driscoll-Kraay (slightly more conservative) or clustering by state alone (less conservative) yield similar results.[20]

The estimates in columns 1 and 2 of Table 1 imply that 100 basis points higher relative monetary accommodation leads to about 1.8 percent higher house-price growth over the next two years. This effect is non-trivial given average two-year house-price growth over our sample of about 2.8 percent. In addition, the estimated real GSP growth effects, both the leads and the lags, have a positive effect on house-price growth as expected, but including or excluding them (not shown) has little effect on the relationship between monetary accommodation and house-price growth.

When we allow for a differential impact of monetary accommodation during the early 2000s housing boom, column 3, we find that 100 basis points higher relative monetary policy accommodation leads to 1.5 percent higher house-price growth over the next two years outside of the housing-boom period. The same 100 basis point difference in accommodation leads to roughly 6.4 percent higher house-price growth during the boom,[21] suggesting that monetary accommodation was particularly correlated with house-price growth during a

[20]See a previous version of this manuscript, Cooper, Luengo-Prado, and Olivei (2016), for further details on our estimates with alternative standard errors.

[21]Calculating the total effect of $rgap_{it}$ during the boom period requires adding the coefficients in the first two rows of the table and also accounting for the presence of the lagged dependent variable.

Table 1. The Effect of Monetary Accommodation on House-Price Growth

	(1)	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**
$rgap_{it}$	1.430***	1.295***	1.168***	0.920	1.420***	0.922**
	(0.383)	(0.359)	(0.370)	(0.564)	(0.369)	(0.387)
$rgap_{it} \times D_{2000-06}$			3.692***	3.407***	2.663***	2.722***
			(0.645)	(0.675)	(0.651)	(0.638)
House-Price Growth$_{t-2,t}$	0.245***	0.277***	0.243***	0.306***	0.236***	0.248***
	(0.065)	(0.058)	(0.055)	(0.056)	(0.054)	(0.051)
GSP Growth$_{t-1,t}$	1.908***	0.732***	0.706***		0.722***	0.698***
	(0.215)	(0.231)	(0.220)		(0.256)	(0.261)
GSP Growth$_{t-2,t-1}$	–0.039	0.344	0.399*		0.265	0.010
	(0.229)	(0.219)	(0.205)		(0.223)	(0.241)
GSP Growth$_{t,t+1}$		1.074***	1.102***	1.486***	1.032***	0.879***
		(0.238)	(0.231)	(0.186)	(0.260)	(0.254)
GSP Growth$_{t+1,t+2}$		1.113***	1.058***	0.830***	1.010***	1.038***
		(0.193)	(0.189)	(0.190)	(0.209)	(0.204)
Chg. Unemp. Rate$_{t-1,t}$				–0.902		
				(0.599)		
Chg. Unemp. Rate$_{t-2,t-1}$				–1.568***		
				(0.442)		
GSP Growth$_{t-1,t} \times D_{2000-06}$					0.050	0.009
					(0.498)	(0.492)
GSP Growth$_{t-2,t-1} \times D_{2000-06}$					1.114**	1.244***
					(0.481)	(0.468)
GSP Growth$_{t,t+1} \times D_{2000-06}$					0.972*	1.100*
					(0.575)	(0.568)

(*continued*)

Table 1. (Continued)

	(1)	(2)	(3)	(4)	(5)	(6)
GSP Growth$_{t+1,t+2} \times D_{2000-06}$					0.228	0.179
					(0.454)	(0.451)
Net Migration, pct$_{t-1,t}$						2.314**
						(1.088)
Memo:						
$rgap_{it}$ Effect†	1.894	1.791	1.542	1.326	1.858	1.226
P-value	0.000	0.000	0.001	0.080	0.000	0.015
$rgap_{it}$ Total Effect 2000–06‡			6.416	6.239	5.342	4.849
P-value			0.000	0.000	0.000	0.000
Adjusted R-squared	0.621	0.687	0.707	0.703	0.716	0.722
Observations	1,248	1,248	1,248	1,248	1,248	1,248

Note: The table shows estimated coefficients and standard errors (in parentheses) from the equation $h_{it,t+2} = \sigma_1 rgap_{it} + \sigma_2 rgap_{it} \times D_{00-06} + \alpha h_{it,t-2} + \beta \mathbf{X}_{it} + \kappa_i + \zeta_t + \varepsilon_{it}$, where $h_{it,t+2}$ is real house-price growth in state i between t and $t+2$, $h_{it,t-2}$ is real house-price growth between $t-2$ and t, $rgap_{it}$ is state-level monetary policy accommodation as of time t, D_{00-06} is a dummy variable that takes a value of 1 between 2000 and 2004 (house-price growth between 2000 and 2006) and is 0 otherwise, $\mathbf{X}_{it}$ is a vector of additional control variables as of t, κ_i are state fixed effects, and ζ_t are time effects. Estimation period: 1980–2005 (house-price growth through 2007). AK, DC, and HI are excluded from the analysis. ***, **, and * indicate significance at the 1 percent, 5 percent, and 10 percent level, respectively. Standard errors clustered by state and year.

†Effect of $rgap_{it}$ on house prices, taking into account that there is a lagged dependent variable.

‡Total effect of $rgap_{it}$ on house prices during the housing-boom period, taking into account the lagged dependent variable.

period when the bubble component of house prices was likely to have been larger, and credit was more readily available than in other years included in our estimation sample.

Using contemporaneous and lagged changes in the state-level unemployment rate rather than contemporaneous and lagged GSP growth to capture local business cycle conditions, we find, not surprisingly, that an increase in the unemployment rate lowers future house-price growth (Table 1, column 4). However, the estimated relationship between $rgap_{it}$ and future house-price growth is little changed.[22] Overall, our results are not sensitive to how we control for local business cycle conditions, but the fit is marginally better with the GSP controls. The $rgap_{it}$ effect on house prices is a bit larger during the non-boom period, and is somewhat smaller during the boom period when we allow the estimated GSP lead and lag effects to also differ during the housing-boom period (column 5). Finally, we also control for inter-state migration flows in column 6. Positive net migration is associated with higher house-price growth, as one would expect. While the estimated coefficients of the effect of $rgap_{it}$ on house prices are a tad smaller during both periods when adding this control, its inclusion does not alter our main finding that the effect of monetary accommodation is substantially larger during the boom period.

While we use column 6 as our baseline specification for the remainder of our analysis, Table 2 provides an alternative set of estimation results based on measuring our dependent variable over different time horizons. Specifically, we assess the impact of monetary policy on house-price growth over horizons of one to four years. The four columns in Table 2 provide estimation results for the four cases, with the second column (the two-year house-price growth horizon) replicating our baseline findings. Explanatory variables are kept the same across specifications, and thus the estimated coefficients for $rgap_{it}$ can be interpreted as we have before in terms of tracing the response of house-price growth over time to 100 basis point higher relative monetary accommodation as of time t. Overall, the results

[22]Including leads of the change in the unemployment rate instead of leads of GSP growth yields even larger house-price effects for both periods (not shown), but reduces the overall amount of variation in state-level house-price growth that is explained by our regressions.

Table 2. The Effect of Monetary Accommodation on House-Price Growth: Alternative Horizons

	HPG (t,t+1) (1)	HPG (t,t+2) (2)	HPG (t,t+3) (3)	HPG (t,t+4) (4)
$rgap_{it}$	0.334*	0.922**	1.035*	1.248*
	(0.188)	(0.387)	(0.584)	(0.719)
$rgap_{it} \times D_{2000-06}$	1.166***	2.722***	4.036***	4.210***
	(0.317)	(0.638)	(0.984)	(1.369)
House-Price Growth$_{t-2,t}$	0.243***	0.248***	0.066	–0.204**
	(0.024)	(0.051)	(0.085)	(0.103)
GSP Growth$_{t-1,t}$	0.316**	0.698***	0.919**	1.263***
	(0.154)	(0.261)	(0.365)	(0.476)
GSP Growth$_{t-2,t-1}$	0.002	0.010	0.255	0.468
	(0.126)	(0.241)	(0.354)	(0.441)
GSP Growth$_{t,t+1}$	–0.109	0.009	–0.069	–0.168
	(0.272)	(0.492)	(0.791)	(1.303)
GSP Growth$_{t+1,t+2}$	0.605***	1.244***	2.036***	2.895***
	(0.224)	(0.468)	(0.768)	(1.097)
GSP Growth$_{t-1,t} \times D_{2000-06}$	0.468***	0.879***	1.279***	1.474***
	(0.154)	(0.254)	(0.363)	(0.463)
GSP Growth$_{t-2,t-1} \times D_{2000-06}$	0.272**	1.038***	1.975***	2.943***
	(0.117)	(0.204)	(0.298)	(0.375)

(*continued*)

Table 2. (Continued)

	HPG (t,t+1) (1)	**HPG (t,t+2) (2)**	**HPG (t,t+3) (3)**	**HPG (t,t+4) (4)**
GSP Growth$_{t,t+1} \times D_{2000-06}$	0.497	1.100*	1.313*	0.559
	(0.342)	(0.568)	(0.700)	(1.041)
GSP Growth$_{t+1,t+2} \times D_{2000-06}$	–0.097	0.179	1.006*	1.921***
	(0.257)	(0.451)	(0.563)	(0.714)
Net Migration, pct$_{t-1,t}$	0.932*	2.314**	3.936**	4.650**
	(0.519)	(1.088)	(1.760)	(2.157)
Adjusted R-squared	0.736	0.722	0.703	0.693
Observations	1,248	1,248	1,248	1,248

Note: The table shows estimated coefficients and standard errors (in parentheses) from the equation $h_{it,t+h} = \sigma_1 rgap_{it} + \sigma_2 rgap_{it} \times D_{00-06} + \alpha h_{it,t+2} + \beta \mathbf{X}_{it} + \kappa_i + \zeta_t + \varepsilon_{it}$, where $h_{it,t+h}$ is real house-price growth in state i between t and $t+h$ as indicated in the column heading, $h_{it,t-2}$ is real house-price growth between $t-2$ and t, $rgap_{it}$ is state-level monetary policy accommodation as of time t, D_{00-06} is a dummy variable that takes a value of 1 between 2000 and 2004 (house-price growth between 2000 and 2006) and is 0 otherwise, $\mathbf{X}_{it}$ is a vector of additional control variables as of t, κ_i are state fixed effects, and ζ_t are time effects. Estimation period: 1980–2005 (house-price growth through 2007). AK, DC, and HI are excluded from the analysis. ***, **, and * indicate significance at the 1 percent, 5 percent, and 10 percent level, respectively. Standard errors clustered by state and year.

in the table remain consistent with our baseline estimates for the two-year house-price growth horizon, with a larger estimated effect of monetary policy during the early 2000s housing boom.[23]

4.2 Controlling for Housing Supply

Our baseline estimates in Table 1 focus on factors—relative monetary accommodation and changes in local economic conditions—that tend to affect the *demand* for housing. However, as Saks (2008) and others have highlighted, local housing supply conditions are also relevant for thinking about local house-price growth. Indeed, if the demand channel explanation is valid for why a more accommodative monetary policy stance leads to greater house-price growth, then we should observe greater effects in locations where housing supply is more constrained.

To explore this further, we divide states into groups (terciles) based on the restrictiveness of land use in each state as measured by the WLURI index. Higher-numbered groups have more restrictive housing regulations.[24] We then interact these groups ($LandReg = 1, 2,$ or 3) with $rgap_{it}$ and $rgap_{it} \times D_{2000-06}$.[25] Table 3 shows these results.[26]

While the effect of monetary policy accommodation does not seem to vary during the non-boom period based on housing supply restrictions, the housing demand effect is larger in states with more restrictive housing supply during the boom period. More specifically, our results show that house-price growth in states with the least restrictive regulation ($LandReg = 1$) increased by 1.6 percent with a 100 basis point higher (relative) monetary accommodation during the early 2000s, but increased by 5.4 percent in states with the most restrictive regulation ($LandReg = 3$)—a difference that is statistically significant. The effect of monetary accommodation during the boom period for states with the least supply restrictions is roughly

[23]Using a local projection method as in Jordà (2005), with the dependent variable being one-year house-price growth at t+1, t+2, t+3, and t+4, rather than the partially overlapping growth rates in Table 2, yields very similar results that are available upon request.

[24]See Table A.3 for a list of states in each tercile.

[25]Dividing states into quintiles yields similar results.

[26]The estimates incorporate all the same baseline controls as column 6 of Table 1 even though they are not shown.

Table 3. Monetary Accommodation and House-Price Growth, Controlling for Housing Supply Restrictions

	(1)
$LandReg = 1 \times rgap_{it}$	1.109***
	(0.373)
$LandReg = 2 \times rgap_{it}$	1.036**
	(0.409)
$LandReg = 3 \times rgap_{it}$	1.155**
	(0.532)
$LandReg = 1 \times rgap_{it} \times D_{2000-06}$	0.130
	(0.632)
$LandReg = 2 \times rgap_{it} \times D_{2000-06}$	1.631***
	(0.620)
$LandReg = 3 \times rgap_{it} \times D_{2000-06}$	3.028***
	(0.681)
House-Price Growth$_{t-2,t}$	0.225***
	(0.050)
Net Migration, pct$_{t-1,t}$	2.244**
	(1.051)
Memo	
$[LandReg = 1]$ $rgap_{it}$ Effect†	1.430***
$[LandReg = 2]$ $rgap_{it}$ Effect†	1.335***
$[LandReg = 3]$ $rgap_{it}$ Effect†	1.489***
$[LandReg = 1]$ $rgap_{it}$ (Total) Effect 2000–06‡	1.597**
$[LandReg = 2]$ $rgap_{it}$ (Total) Effect 2000–06‡	3.439***
$[LandReg = 3]$ $rgap_{it}$ (Total) Effect 2000–06‡	5.393***
R-squared	730
N	1,248

Note: The table shows estimated coefficients and standard errors (in parentheses) from the equation $h_{it,t+2} = \sigma_1 rgap_{it} + \sigma_2 rgap_{it} \times D_{00-06} + \alpha h_{it,t-2} + \beta \mathbf{X}_{it} + \kappa_i + \zeta_t + \varepsilon_{it}$, where $h_{it,t+2}$ is real house-price growth in state i between t and $t+2$, $h_{it,t-2}$ is real house-price growth between $t-2$ and t, $rgap_{it}$ is state-level monetary policy accommodation as of time t, D_{00-06} is a dummy variable that takes a value of 1 between 2000 and 2004 (house-price growth between 2000 and 2006) and is 0 otherwise, $\mathbf{X}_{it}$ is a vector of additional control variables as of t, κ_i are state fixed effects, and ζ_t are time effects. Estimation period: 1980–2005 (house-price growth through 2007). AK, DC, and HI are excluded from the analysis. ***, **, and * indicate significance at the 1 percent, 5 percent, and 10 percent level, respectively. Standard errors clustered by state and year.

†Effect of $rgap_{it}$ on house prices given supply elasticity, taking into account that there is a lagged dependent variable.

‡Total effect of $rgap_{it}$ on house prices during the housing-boom period, taking into account the lagged dependent variable.

of the same magnitude as the effect during the non-boom period. Overall, these findings point to a likely housing demand channel for the relative degree of monetary accommodation across states, but they also demonstrate the relevance of housing supply restrictions across locations in determining the magnitude of the housing demand effect. Also, to the extent that the non-fundamental component of house-price growth during the boom was larger in areas with more restrictive housing supply, these findings suggest that monetary accommodation contributed more to house-price growth at a time and in areas where there was likely a larger "bubble" component to house-price growth.

4.3 Possible Mechanisms: The Role of Credit Availability

Why did monetary policy accommodation matter more during the housing-boom period and particularly in areas with more restrictive housing supply? It has been extensively documented that a credit boom occurred during the 2001–06 period, in part driven by credit supply factors (see Mian and Sufi 2009). While disagreement on what population segment, prime versus subprime, saw the largest credit expansion, there is little doubt at this point that credit supply expanded during the early 2000s.[27] Primary mortgage rates vary little if at all across states, but households' expectations about house prices and their ability to obtain credit—especially housing-related (mortgage) loans—could affect housing demand and hence house-price appreciation across locations.[28] If credit indeed expanded relatively more to "hotter" areas with relatively more accommodative monetary policy and/or to areas with more restrictive housing supply regulations, then relative monetary policy accommodation should matter more during the housing-boom period even after controlling for time fixed effects.

[27]See Albanesi, De Giorgi, and Nosal (2017) and Foote, Loewenstein, and Willen (2021).

[28]Mortgage credit risk is not and was not priced differently by location. In particular, Freddie Mac and Fannie Mae (the so-called GSE lenders) and FHA loans are priced the same across the country. The price of mortgage insurance also is the same across location. For a recent example of the lack of regional differences in loan pricing, see https://www.mgic.com/-/media/mi/rates/rate-cards/71-61284-rate-card-pdf-bpmi-monthly-july-2018.pdf.

Using data from the Federal Reserve Bank of New York's Consumer Credit Panel (CCP) provided by Equifax, we find that credit expanded relatively more in areas with more housing supply regulations.[29] From 2001 to 2006, housing-related debt balances (mortgages plus home equity loans) increased on average by 78, 83, and 107 percent, respectively, in our three state groups based on land regulation ($LandReg = 1, 2,$ or 3; with 3 representing the most restrictive regulation). During the same period, the correlation across states of housing-debt growth and relative monetary accommodation (the average of $rgap_{it}$ in a given state over the period) was 0.4. That is, housing-debt growth was generally higher in areas with a relatively more accommodative stance of monetary policy. More formal regression analysis using these data is not possible, because the CCP sample starts only in 1999. Nevertheless, the evidence is suggestive of housing demand and credit supply factors interacting and driving our results.

4.4 Bootstrap Approach for Addressing Measurement Error

Since r^*_{it}—the real equilibrium interest rate measure—and hence $rgap_{it}$ is a generated regressor, there is potential bias in the estimated coefficients and the associated standard errors in our ordinary least squares regressions. So far, our estimates show a much larger impact of the relative stance of monetary policy on house prices during the housing boom. Given that our primary interest is in determining whether monetary policy affected house prices differently during the "bubble" period, our estimates suggest that measurement error is not a big concern. Even if our estimates are biased downward, the bias would have to be time varying to affect the estimates in the boom and non-boom periods differently and contribute to our observed differential effect of monetary policy accommodation. Nevertheless, we use a bootstrap (Monte Carlo) procedure that can address the potential measurement error.

[29]The CCP is a longitudinal, nationally representative 5 percent random sample of individuals with credit records in the United States. The data are available quarterly (starting in 1999) and include information on most aspects of individuals' credit and debt holdings, including credit scores, balances on credit cards, auto loans, student debt, and mortgages as well as the birth year and the geographical location of these individuals.

The procedure, which we discuss in detail in Appendix B, generates a series of simulated values of the parameters in Equation (4) based on our initial IS curve and house-price growth effect estimates. Table 4 reports the mean and standard deviation (in parentheses) of our estimated coefficients of interest over all the bootstrap iterations. Comparing these results to our baseline results in Table 1, it seems that the bias towards zero in our baseline estimates is not an issue. While the bootstrap standard errors are somewhat larger than the standard errors clustered by state and year (our baseline), all coefficients of interest remain significant at the 5 percent level or better. Therefore, it does not appear that our baseline results are contaminated by measurement error.

5. IS Curve Estimates and Relevant Robustness

This section discusses our baseline results from estimating Equations (1) and (2), and assesses the robustness of our main house-price growth results to different IS curve estimation assumptions and the equilibrium real interest rates that these alternative estimates generate.

5.1 IS Curve Estimation Results

Remember that to estimate the IS curves, we first take the preliminary step of constructing a state-level measure of the real federal funds rate, r_{it}, using fitted inflation values (see Appendix A.2.) Next, we estimate the IS curve with coefficients that vary by state, Equation (1). 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. For brevity, we do not show all these state-level estimated effects, but they are summarized in Table 5.[30] Among other things, the table shows the estimated effect of a persistent increase in the real interest rate on the unemployment

[30]Figure 2, which we discussed earlier, plots the estimated interest rate effects on unemployment (and their standard errors) for each state.

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

	(1)	(2)
$rgap^*_{it}$	1.198***	0.865**
	(0.438)	(0.430)
$rgap^*_{it} \times D_{2000-06}$		2.490**
		(1.119)
House-Price Growth$_{t-2,t}$	0.274***	0.245***
	(0.064)	(0.062)
GSP Growth$_{t-1,t}$	0.751***	0.709***
	(0.167)	(0.187)
GSP Growth$_{t-2,t-1}$	0.344*	0.003
	(0.202)	(0.201)
GSP Growth$_{t,t+1}$	1.076***	0.883***
	(0.157)	(0.190)
GSP Growth$_{t+1,t+2}$	1.131***	1.052***
	(0.197)	(0.199)
GSP Growth$_{t-1,t} \times D_{2000-06}$		0.027
		(0.408)
GSP Growth$_{t-2,t-1} \times D_{2000-06}$		1.232**
		(0.481)
GSP Growth$_{t,t+1} \times D_{2000-06}$		1.105**
		(0.523)
GSP Growth$_{t+1,t+2} \times D_{2000-06}$		0.213
		(0.502)
Net Migration, pct$_{t-1,t}$		2.341**
		(0.970)
Memo:		
r^*_{it} Effect†	1.643***	1.145**
r^*_{it} Total Effect 2000–06°		4.463***
Replications	1,000	1,000

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

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

‡Effect of $rgap^*_{it}$ on house prices, taking into account that there is a lagged dependent variable.

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

Table 5. 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

Note: Summary of the estimated coefficients for the unrestricted IS curve $u_{it} = \alpha_i + v_t + \lambda_{1i}u_{it-1} + \lambda_{2i}u_{it-2} + \theta_{1i}r_{it-1} + \epsilon_{it}$, where u_{it} is state i's unemployment rate gap at time t, and r_{it} is a time t measure of the real federal funds rate for the state. α_i and v_t denote state and time fixed effects, respectively. Estimation period: 1980–2007. Baseline estimates exclude AK, DC, and HI.

[a]The impact of a permanent increase in the real interest rate on the unemployment gap after two years.

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 affect the real economy. The results are reasonable, but are at the higher end of estimates of the impact of short-term rates on real activity at the national level.[31] The larger state-level effects could be due to the presence of spillover effects, such as labor mobility across states, which may not operate at the national level.

To reduce bias in the IS curve parameter estimates, given that our sample is relatively short, we use these estimated two-year interest rate effects to group states into five bins (b) of roughly equal size. We then estimate Equation (2), which restricts the IS curve parameters to be the same within each group.[32] The estimated interest rate effect, θ_{1b}, increases monotonically across the groups—a result that is consistent with our grouping strategy, as larger estimated interest rate effects result in bigger two-year unemployment rate effects (see Table 6). The results imply that a 100 basis point reduction in the short-term interest rate reduces states' unemployment gaps by about $\frac{2}{3}$ percentage point over two years for states with the least interest rate sensitivity and $1\frac{1}{4}$ percentage points for states with the greatest interest rate sensitivity. 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 affect state-level economic activity fairly quickly. 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. Overall, the parameter estimates from Equation (2) are quantitatively similar but somewhat less dispersed than what we get when we estimate separate interest rate and unemployment gap effects for each state.

[31]The Federal Reserve Board's FRB/US model implies that a persistent 100 basis point decline in the federal funds rate would lower the national unemployment rate by roughly half a percentage point after three years (see Brayton, Laubach, and Reifschneider 2014).

[32]Table A.2 shows the states in each group. The cut points for determining the groups are 0.64, 0.77, 0.87, and 0.96, respectively.

Table 6. Parameter Estimates for the Restricted IS Curve

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

Note: Estimated coefficients for the restricted IS curve $u_{it} = \alpha_i + v_t + \lambda_{1b}u_{it-1} + \lambda_{2b}u_{it-2} + \theta_{1b}r_{it-1} + \epsilon_{it}$, where u_{it} is state i's unemployment rate gap at time t, and r_{it} is a time t measure of the real federal funds rate for the state. α_i and v_t denote state and time fixed effects, respectively. Standard errors of the estimates are in parentheses where applicable. ***, **, and * indicate significance at the 1 percent, 5 percent, and 10 percent level, respectively. Estimation period: 1980–2007. Baseline estimates exclude AK, DC, and HI.

†Groups based on two-year unemployment rate effects from unrestricted IS curve regressions. Group 1 has the smallest two-year effect, while group 5 has the largest effect.

[a]Impact of a permanent increase in the real interest rate on the unemployment gap after two years.

5.2 Alternative Approaches for Estimating $rgap_{it}$

The results in this section speak to the robustness of our findings to alternative approaches for estimating r^*_{it} and hence $rgap_{it}$. These robustness checks involve reestimating both the state-level IS curves and the house-price growth equations. For brevity, we do not report all of the alternative IS curve parameter estimates.[33] The first column of Table 7 shows our baseline house-price growth results for easy reference, while each subsequent column reports results for an alternative IS curve specification.

To better compare our results across IS curve specifications, we evaluate a standardized change in $rgap_{it}$—specifically, the effect on house prices when moving from the 25th percentile of the $rgap_{it}$ distribution to the 75th percentile. We do this because the 100 basis point differences in relative monetary accommodation that we discussed earlier may be large or small depending on the distribution of $rgap_{it}$. To keep the time horizon over which we measure variation in $rgap_{it}$ consistent across specifications, we always calculate the difference between the 25th and 75th percentiles over the 2000–04 period.[34] During this time frame, there is a 130 basis point difference between the 25th and 75th percentiles of the $rgap_{it}$ distribution for our baseline estimation. On this standardized basis, the corresponding gain in house prices over two years is about 1.6 percent during the non-boom period and 6.3 percent during the boom period.[35] As a point of comparison, real two-year growth in house prices between the 2000–02 and 2004–06 periods was 9.6 percent, on average, across states with a standard deviation of 7.9 percent.

Moving on to the different IS curve specifications, as a first alternative we do not group states into five bins, but compute r^*_{it} based on the estimated parameters for Equation (1) directly. Qualitatively the results (Table 7, column 2) are similar to our baseline findings: the effect of monetary policy is noticeably larger during the housing-boom period than before. Quantitatively, the estimated effects of monetary accommodation on house prices during the boom period

[33]These estimates are available from the authors upon request.

[34]We calculate the 25th and 75th percentile in each year during this period and take the average.

[35]To obtain these numbers, multiply the interquartile range (reported at the bottom of Table 7) by the estimated $rgap_{it}$ (total) effects and divide by 100.

Table 7. Monetary Accommodation and House-Price Growth, Alternative Approaches to Estimating $rgap_{it}$

	Baseline (1)	Unrestricted IS Curve (2)	Alt. State Grouping (3)	Aggregate Inflation (4)	1986–2007 Subsample (5)	Demographic Controls (6)	Prime-Age Males (7)
$rgap_{it}$	0.922**	0.886***	0.533*	0.728**	0.654**	1.262***	0.985*
	(0.387)	(0.341)	(0.276)	(0.300)	(0.313)	(0.468)	(0.513)
$rgap_{it} \times D_{2000-06}$	2.722***	1.448***	2.974***	1.585***	0.727**	4.057***	3.761***
	(0.638)	(0.475)	(0.540)	(0.395)	(0.330)	(0.877)	(0.759)
House-Price Growth$_{t-2,t}$	0.248***	0.267***	0.248***	0.248***	0.215***	0.252***	0.257***
	(0.051)	(0.051)	(0.050)	(0.051)	(0.050)	(0.050)	(0.052)
Memo:							
$rgap_{it}$ Effect†	1.226	1.210	0.709	0.968	0.833	1.687	1.324
P-value	0.015	0.009	0.048	0.013	0.031	0.006	0.047
$rgap_{it}$ Total Effect 2000–06‡	4.849	3.187	4.663	3.076	1.760	7.111	6.383
P-value	0.000	0.000	0.000	0.000	0.000	0.000	0.000
Interquartile Range for $rgap_{it}$ (Basis Points)	130	150	156	205	164	78	100
Adjusted R-squared	0.722	0.718	0.728	0.720	0.751	0.722	0.722
Observations	1,248	1,248	1,248	1,248	960	1,248	1,248

Note: The table shows estimated coefficients and standard errors (in parentheses) from the equation $h_{it,t+2} = \sigma_1 rgap_{it} + \sigma_2 rgap_{it} \times D_{00-06} + \alpha h_{it,t+2} + \beta \mathbf{X}_{it} + \kappa_i + \zeta_t + \varepsilon_{it}$, where the estimates of $rgap_{it}$ vary based on different approaches to estimating r^*_{it} as noted in the column headings and discussed in the text. As before, $h_{it,t+2}$ is real house-price growth in state i between t and $t+2$, $h_{it,t-2}$ is real house-price growth between $t-2$ and t, $rgap_{it}$ is state-level monetary policy accommodation as of time t, D_{00-06} is a dummy variable that takes a value of 1 between 2000 and 2004 (house-price growth between 2000 and 2006) and is 0 otherwise, $\mathbf{X}_{it}$ is a vector of additional control variables as of t, κ_i are state fixed effects, and ζ_t are time effects. Regressions included leads and lags of GSP growth along with these variables interacted with the dummy variable for the housing boom, and a control for percent, net migration, but these estimates are not shown. Estimation period: 1980–2005 (house-price growth through 2007) except for column 5, where the estimation period is 1986–2005 (house-price growth through 2007). AK, DC, and HI are excluded from the analysis. ***, **, and * indicate significance at the 1 percent, 5 percent, and 10 percent level, respectively. Standard errors clustered by state and year.

†Effect of $rgap_{it}$ on house prices, taking into account that there is a lagged dependent variable.

‡Total effect of $rgap_{it}$ on house prices during the housing-boom period, taking into account the lagged dependent variable.

are a little smaller on a standardized basis than our baseline (4.8 percent versus 6.3 percent, respectively).

Next, we group states by their average manufacturing share of GDP over our sample period instead of the two-year unemployment rate effect and reestimate Equation (2). This approach groups states based on a metric that is outside the model estimation process, but is also relevant—state economies with higher manufacturing shares are likely more sensitive to interest rate changes.[36] Qualitatively and quantitatively the results are strikingly similar (Table 7, column 3). The effect of monetary policy is larger during the housing-boom period, and on a standardized basis the estimated effects are a bit higher during the housing-boom period (7.3 percent versus our baseline of 6.3 percent).[37]

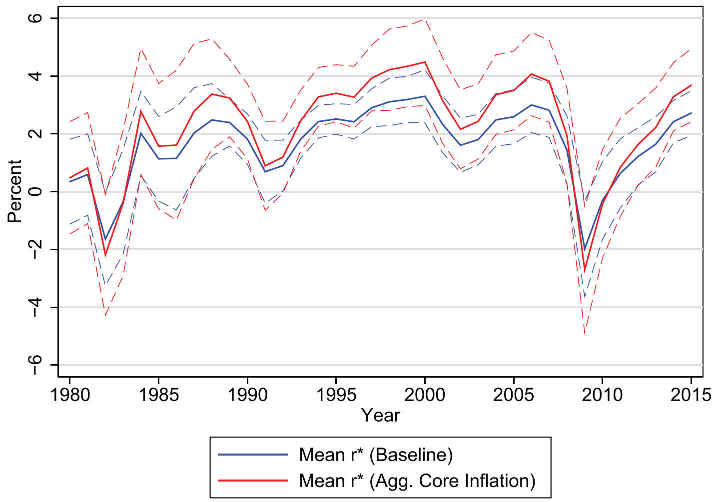
We also consider estimates of $rgap_{it}$ where, rather than using our fitted inflation measure to determine the real interest rate (r_{it}) in the IS curve equations, we use aggregate core PCE inflation for all states. Column 4 shows these results, which along with Figure 4, demonstrate that our findings are not sensitive to the measure of inflation that we use to calculate the real interest rate in the IS curves. In particular, Figure 4 plots our baseline estimates of average r^*_{it} over time versus the estimates using core PCE inflation.[38] The average r^*_{it} estimates are very similar across time, although there is slightly more variation with the measure that uses core PCE inflation. Indeed, even though the point estimates in column 4 are smaller than in our baseline results, the magnitude of the differential effect of monetary policy across periods and the standardized effect are quite similar to our baseline findings.

[36]See Table A.4 for state groupings by manufacturing shares. Other "external" approaches for grouping states yield similar results.

[37]There is also the potential issue that sectoral shifts in employment over time, such as the decline in manufacturing, may have accelerated during the housing-boom period. Grouping states by their average manufacturing share does not completely address the concern that these employment shifts may have directly affected the sensitivity of house prices to interest rates, complicating the interpretation of the estimated effects of monetary policy during this period. To address this concern, we explored an additional specification that included as additional controls the time-varying manufacturing share of state output as well as its interaction with our indicator variable for the housing-boom period (not shown). Our results of interest were little changed.

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

Figure 4. r^*_{it} Estimates: Aggregate Core Inflation



Note: The figure shows the average equilibrium interest rate r^*_{it} across states for our baseline estimates (blue line) and for the 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. (For figures in color, see the online version of the paper at http://www.ijcb.org.)

The results in column 5 examine the effect of $rgap_{it}$ on house-price growth where we start the estimation in 1986—both for the IS curves and for house-price growth. This shorter sample horizon focuses on a time when credit markets were fully developed, possibly leading to different sensitivities to interest rate movements. The time frame also coincides with the "Great Moderation" and likely features a more stable systematic component of monetary policy. While starting the estimates in 1986 yields smaller house-price growth effects, particularly for the boom period (2.9 versus 6.3), the message of the results is qualitatively the same: differences in monetary accommodation across locations impact house-price growth, and this effect is larger during the housing-boom period than in the non-boom period.

In column 6, we report results from regressions that add time-varying, state-specific demographic controls to the unrestricted and restricted IS curve regressions. In particular, we add the yearly growth rate in the percentage of individuals in each state with a college degree, and the yearly growth rate in the proportion of older individuals (those 55 and older).[39] Since changing demographics in the labor force likely affect a state's natural rate of unemployment, these additional controls help capture potential time variation in states' equilibrium unemployment rates. Qualitatively and quantitatively, the results are similar to our baseline findings. We also reestimate the IS curves using state unemployment rate data for males aged 25–55 (prime-age males). This segment of the labor force tends to be relatively stable over time and there is likely much less variation in these adults' natural rate of unemployment. The results, shown in column 7, are once again very similar to our baseline findings. In sum, our findings and conclusions do not seem particularly sensitive to our chosen baseline IS curve estimation approach.

6. Conclusion

We propose a novel identification strategy to assess the effect of monetary policy on housing prices. The identification relies on monetary policy having a differential impact across 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 in the future. For a given real federal funds rate, the degree of monetary accommodation is greater in states with a higher equilibrium real interest rate.

After controlling for state-specific economic conditions, for state-level net migration, and for common conditions across states, differences in the relative stance of monetary policy capture the documented fact that monetary policy has different (temporary) effects across locations (states). Given our controls, such variation in monetary accommodation 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

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

certain policy may be, for example, more stimulative in certain states than in others. This identification strategy has advantages relative to the more standard practice of isolating a monetary policy "shock," for example, as the residual from an estimated policy reaction function. The reason is that the approach allows us to estimate the effect of monetary policy on housing prices in a panel of U.S. states with a rich set of controls, which stacks the odds against finding a role for our measure of monetary policy accommodation. We are also able to examine the effect of monetary policy on house prices across time periods.

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

Appendix A. The "Equilibrium" Real Interest Rate

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 2016). In a monetary policy setting, this notion of the equilibrium real rate is often accompanied by another, medium-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-level measure of the equilibrium real interest rate is the value of the real federal funds rate that, if sustained, would close a state's unemployment rate gap after two years.

As discussed in the main text, a medium-run concept of the equilibrium real rate is often used as an input in setting monetary policy at the aggregate level. The medium-run equilibrium real rate has some limitations as a guide to policy, in that, even if this rate can achieve full capacity, it may not necessarily yield a desired outcome in terms of inflation. As a result, actual monetary policy is likely to deviate from this prescription. However, in our disaggregated setup all we need is a measure of the difference in the relative stance of monetary policy across states, as the common effect of monetary policy in determining changes in housing prices will be absorbed by the time effect.

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

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),

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

and

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

setting $u_{it+2} = 0$ (a measure of state i's equilibrium unemployment rate), setting $\widehat{r}_{it+1} = r^*_{it}$ and $\widehat{r}_{it} = r^*_{it}$, and substituting for u_{it+1} based on Equation (A.1) yields

$$0 = \alpha_i + \lambda_{1b}(\alpha_i + \lambda_{1b}u_{it} + \lambda_{2b}u_{it-1} + \theta_{1b}r^*_{it}) + \lambda_{2b}u_{it} + \theta_{1b}r^*_{it}.$$

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

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

which is Equation (3) in the text, where

$$\mu_i = -(1 + \lambda_{1b})\alpha_i \times \left(\frac{1}{\lambda_{1b}\theta_{1b} + \theta_{1b}}\right).$$

A.2 Computing $\widehat{\pi}_{it}$

To estimate our IS curve equations, we construct a measure of the real federal funds rate, which requires a measure of state-level inflation as discussed in Section 2.2 of the main text. Since our options for state-level inflation measures are limited and the GSP-based measures that we use are fairly volatile, we take an approach to reduce potential measurement error and have our inflation measure better reflect core PCE inflation. In particular, we compute a measure of state-level inflation, $\widehat{\pi}_{it}$, as the fitted value from the following regression:

$$\pi_{it} = \varphi_i + \pi^{core}_t + \delta_1\tilde{y}_{it} + \delta_2\tilde{y}_{it-1} + \upsilon_{it}, \tag{A.2}$$

where π_{it} is inflation in state i at time t as measured by the GSP deflator, π^{core}_t is aggregate core PCE inflation at time t, $\widetilde{y}_{it}$ is real

Table A.1. Prediction Equation Estimates for State-Level Inflation

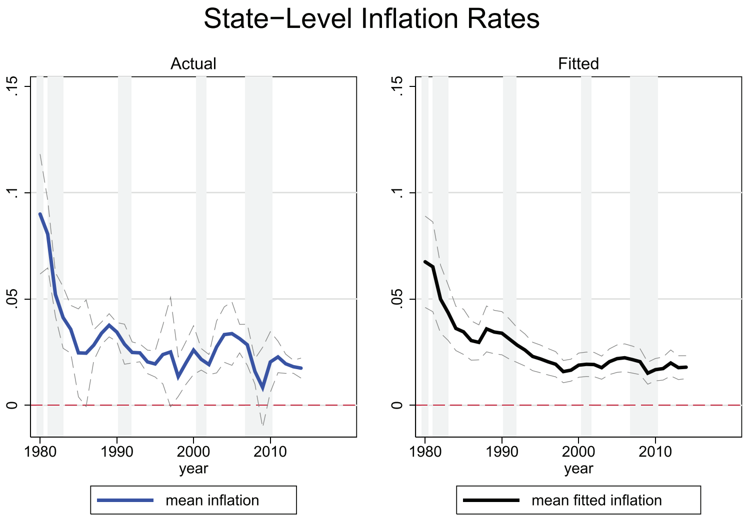
	(1)
Core PCE Inflation[a]	1
Relative GSP Growth$_t$[b]	0.034*
	(0.02)
Relative GSP Growth$_{t-1}$[b]	0.029
	(0.02)
R-squared	0.74
N	1,288

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

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 (A.2) based on the relative size of each state's labor force. Table A.1 reports the parameter estimates from Equation (A.2).

This estimation 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 plots the two inflation measures across states over time.

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

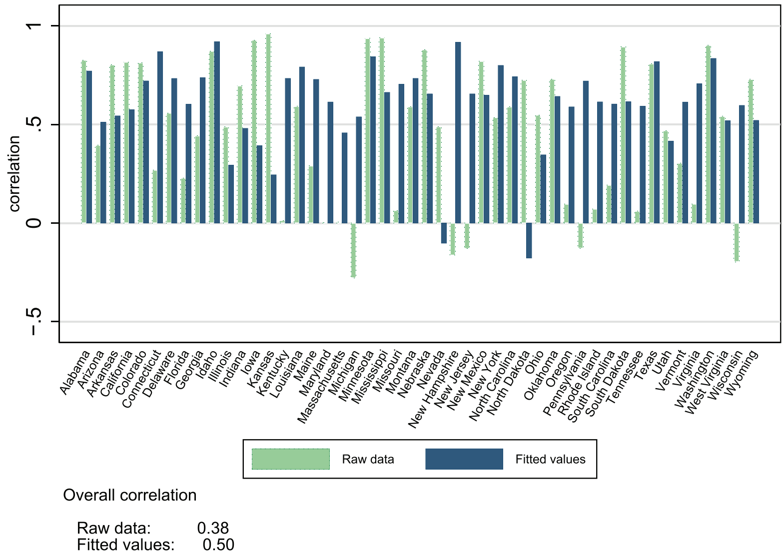


Note: Average across states in a given year, and average plus or minus one standard deviation.

Our fitted state-level inflation data also compare more favorably to published BEA data on state-level consumer inflation, available only for the 2009–13 period, than to the raw, GSP-based price data.[40] The overall correlation of BEA state inflation rates and the GSP deflator-based state inflation measure is 0.38 for this period, while the correlation of BEA inflation rates with the fitted values from Equation (A.2) is 0.50. (Figure A.2 depicts the relevant state-by-state correlations.) This test of the external validity of our predicted inflation measure confirms that our approach removes noise, rather than signal, from the GSP-based state inflation data. Section 5 in the main text shows that our baseline results are robust to instead using aggregate core PCE inflation data for all states. The advantage of using $\widehat{\pi}_{it}$ as our baseline inflation measure, however, is that we can account for differences in price fluctuations across

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

Figure A.2. Correlations of Inflation Rates Computed Using Retail Price Deflators and GSP Deflators, 2009–13



Note: The figure compares BEA state-level data on PCE inflation and inflation rates computed using GSP deflators, raw data, and fitted values, using Equation (A.2). AK, DC, and HI excluded.

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.

A.3 Supplementary Tables

Table A.2. 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 $\theta_b = 5$
NC	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	

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

Table A.3. 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		

Note: 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.4. 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

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

Appendix B. Bootstrap Approach Details

This section describes the bootstrap (Monte Carlo) procedure that we use to address sampling error in our generated regressor. The procedure also allows us to adjust the standard errors of our estimates to take into account the autocorrelation of the data in the spirit of Bertrand, Duflo, and Mullainathan (2004). Note that while we show our approach without any interaction terms for simplicity, the approach is similarly executed when we include them.

At each iteration l ($l = 1, \ldots, 1000$) of our bootstrap procedure, we randomly assign to each state i all the t residual observations from the estimations of Equation (2) and Equation (4) of a randomly selected state j (sampling with replacement). While the procedure assumes cross-sectional independence, drawing the entire time series of a given cross-section preserves potential within-state autocorrelation of outcomes.

With the estimated coefficients $\hat{\alpha}_i$, $\hat{\nu}_t$, $\hat{\lambda}_{1b}$, $\hat{\lambda}_{2b}$, and $\hat{\theta}_{1b}$, as well as the estimated residuals $\hat{\epsilon}$ from Equation (2)—the IS curve with restricted coefficients—we generate the variable $u_{it}^{(l)}$, where[41]

$$u_{it}^{(l)} = \hat{\alpha}_i + \hat{\nu}_t + \hat{\lambda}_{1b} u_{it-1}^{(l)} + \hat{\lambda}_{2b} u_{it-2}^{(l)} + \hat{\theta}_{1b} r_{it-1} + \hat{\epsilon}_{jt}^{(l)}. \tag{B.1}$$

We then reestimate the restricted IS curve:

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

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

Next, we retrieve the estimated coefficients, $\widehat{\kappa}_i$, $\widehat{\zeta}_t$, $\hat{\sigma}$, and $\hat{\beta}$, and the estimated residuals $\hat{\varepsilon}$ from Equation (4) to generate the variable

$$h_{it,t+2}^{(l)} = \widehat{\kappa}_i + \widehat{\zeta}_t + \hat{\sigma}\ rgap_{it} + \hat{\beta}\mathbf{X}_{it} + \hat{\varepsilon}_{jt}^{(l)}. \tag{B.2}$$

Finally, we reestimate the house-price growth equation using generated house-price growth and the different equilibrium interest rates calculated in the initial step:

$$h_{it,t+2}^{(l)} = \kappa_i + \zeta_t + \sigma\ rgap_{it}^{(l)} + \beta\mathbf{X}_{it} + \varepsilon_{it}. \tag{B.3}$$

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

Appendix C. Checking for Dynamic Panel Bias

In this section, we reconsider our baseline estimates, taking into account that our empirical specification includes a lagged dependent variable, and the number of time periods in our sample is relatively short (T = 28). In particular, the lagged dependent variable could

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

be correlated with the error term, resulting in biased estimates.[42] 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 affect the estimated coefficients. Arellano and Bond (1991) propose an estimator to account for these serial correlation issues, which has been modified in subsequent work by Blundell and Bond (1998) and others.

To check the potential bias in our estimates, we employ an estimator based on the approach in Blundell and Bond (1998), which is a somewhat modified version of the original Arellano-Bond estimator used to address lagged dependent variables in panel-regression analysis. The Blundell-Bond estimator assumes that the first differences of the relevant instruments are not correlated with the fixed effects (in the error term) and thus does not require transforming all the variables like with the Arellano-Bond estimator. We implement the Blundell-Bond estimator using the xtabond2 function in Stata, which is discussed in detail in Roodman (2009). A priori, however, it is not clear that our estimates are biased due to the lagged dependent variable, 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.[43] Still, for completeness we reestimate our baseline results using the Blundell-Bond estimator and report the results in Table C.1. The estimates include time and state fixed effects, but they are not reported.

The results using the Blundell-Bond estimator are very similar to our baseline findings. These findings suggest that any serial correlation in the error term of our baseline specification has little effect

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

[43] 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 C.1. Monetary Accommodation and House-Price Growth, Controlling for Potential Serial Correlation

	(1)	(2)	(3)
$rgap_{it}$	1.417***	1.071***	0.995**
	(0.427)	(0.394)	(0.388)
$rgap_{it} \times D_{2000-06}$		3.014***	2.762***
		(0.646)	(0.616)
Lagged House-Price Growth$_{t-2,t}$	0.299***	0.255***	0.229***
	(0.085)	(0.061)	(0.052)
GSP Growth$_{t-1,t}$	0.674**	0.711***	0.693***
	(0.262)	(0.240)	(0.251)
GSP Growth$_{t-2,t-1}$	0.499**	0.520**	0.049
	(0.207)	(0.245)	(0.235)
GSP Growth$_{t,t+1}$	1.009***	0.988***	0.894***
	(0.267)	(0.244)	(0.245)
GSP Growth$_{t+1,t+2}$	0.996***	1.064***	1.019***
	(0.258)	(0.216)	(0.195)
GSP Growth$_{t-1,t} \times D_{2000-06}$			0.009
			(0.477)
GSP Growth$_{t-2,t-1} \times D_{2000-06}$			1.234***
			(0.455)
GSP Growth$_{t,t+1} \times D_{2000-06}$			1.102**
			(0.551)
GSP Growth$_{t+1,t+2} \times D_{2000-06}$			0.197
			(0.439)
Net Migration, pct$_{t-1,t}$			2.281**
			(1.042)
Memo:			
$rgap_{it}$ Effect†	2.022	1.438	1.291
P-value	(0.002)	(0.003)	(0.000)
$rgap_{it}$ Total Effect 2000–06‡		5.486	4.872
P-value		0.000	0.000
Observations	1,248	1,248	1,248

Note: The table shows estimated coefficients and standard errors (in parentheses) from the equation $h_{it,t+2} = \sigma_1 rgap_{it} + \sigma_2 rgap_{it} \times D_{00-06} + \alpha h_{it,t-2} + \beta \mathbf{X}_{it} + \kappa_i + \zeta_t + \varepsilon_{it}$, where $h_{it,t+2}$ is real house-price growth in state i between t and $t+2$, $h_{it,t-2}$ is real house-price growth between $t-2$ and t, $rgap_{it}$ is state-level monetary policy accommodation as of time t, D_{00-06} is a dummy variable that takes a value of 1 between 2000 and 2004 (house-price growth between 2000 and 2006) and is 0 otherwise, $\mathbf{X}_{it}$ is a vector of additional control variables as of t, κ_i are state fixed effects, and ζ_t are time effects. Estimation period: 1980–2005 (house-price growth through 2007). AK, DC, and HI are excluded from the analysis. ***, **, and * indicate significance at the 1 percent, 5 percent, and 10 percent level, respectively. All specifications are estimated using the Blundell-Bond estimator implemented with the xtabond2 function in Stata. Standard errors clustered by state and year.

†Effect of $rgap_{it}$ on house prices, taking into account that there is a lagged dependent variable.

‡Total effect of $rgap_{it}$ on house prices during the housing-boom period, taking into account the lagged dependent variable.

on our estimates of interest. Our estimates are also very similar (not shown) if we drop every other year of data to avoid overlapping periods of house-price growth in our estimates given our focus on two-year house-price growth.

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