

## Commentary

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Is housing the business cycle, as Leamer says? In their fascinating paper, Jarociński and Smets's (2008) careful analysis suggests that the answer is no. Very briefly, they show that the recent U.S. housing boom is explained by a combination of increases in housing demand and loose monetary policy. However, once they adequately account for the myriad of dynamic interactions, they find that housing demand shocks have a very limited impact on the overall volatility of real growth and inflation. And, finally, Jarociński and Smets use their estimates to suggest that, since 2000, monetary conditions have been close to neutral.

The paper is divided neatly into two parts. The first presents the results of a careful model estimation exercise—a Bayesian vector autoregression (BVAR) that includes real gross domestic product (GDP), the GDP deflator, real consumption, real residential investment, real house prices, real commodity prices, the money stock, the federal funds rate, and the long-term interest rate spread. The second part of the paper uses estimates from the first to estimate a monetary conditions index (MCI). Following this organization, I divide my comments into two parts. First, I discuss the role of housing in the business cycle; and second, I will make a number of comments about the use of MCIs.

### PART I: UNDERSTANDING THE ROLE OF HOUSING

In the first part of the paper, Jarociński and Smets present a careful analysis of the dynamic properties of housing, monetary policy, and growth. They focus on the impact of shocks to housing demand, monetary policy, and the term spread, concluding that they account for a small fraction of real GDP and the real GDP deflator but a large fraction of the variation in house prices and residential construction. (I am referring to the variance decomposition results in their Table 2A. For reasons that will become clear later, I prefer the differences version of their VAR.) Importantly, the Jarociński and Smets estimates show that a combination of a positive housing demand shock and low interest rates accounts for the bulk of the rise in house prices and the increase in residential construction activity. (See the historical decompositions in their Figure 6A.)

I have three separate points to make about this conclusion. First, the results are neatly consistent with my strongly held view that over the period that Jarociński and Smets study, 1987-2006, monetary policymakers stopped being the destabilizing force that they probably were in the 1970s and may even have been successfully neutralizing a variety of demand shocks.<sup>1</sup> That

<sup>1</sup> See Cecchetti, Flores-Lagunes, and Krause (2006).

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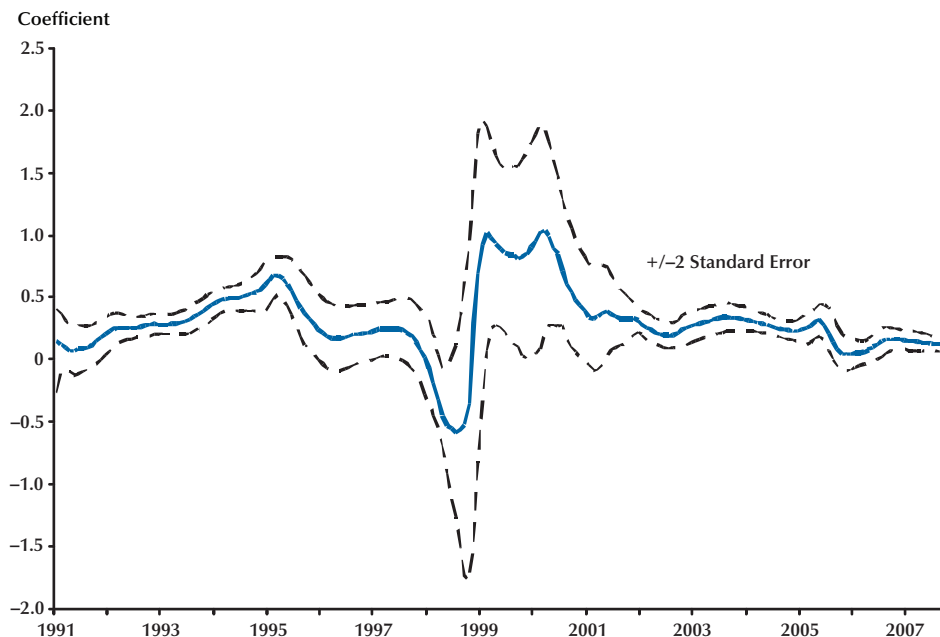
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Federal Reserve Bank of St. Louis *Review*, July/August 2008, 90(4), pp. 371-76.

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## Figure 1

### Rolling Regression of a 10-Year Bond on Federal Funds, 48-Month Window



NOTE: The figure plots coefficients of a regression of the fixed-maturity 10-year U.S. Treasury bond rate on the federal funds rate using a 48-month rolling window. Estimates are plotted on the last date of the sample.

SOURCE: Board of Governors of the Federal Reserve System.

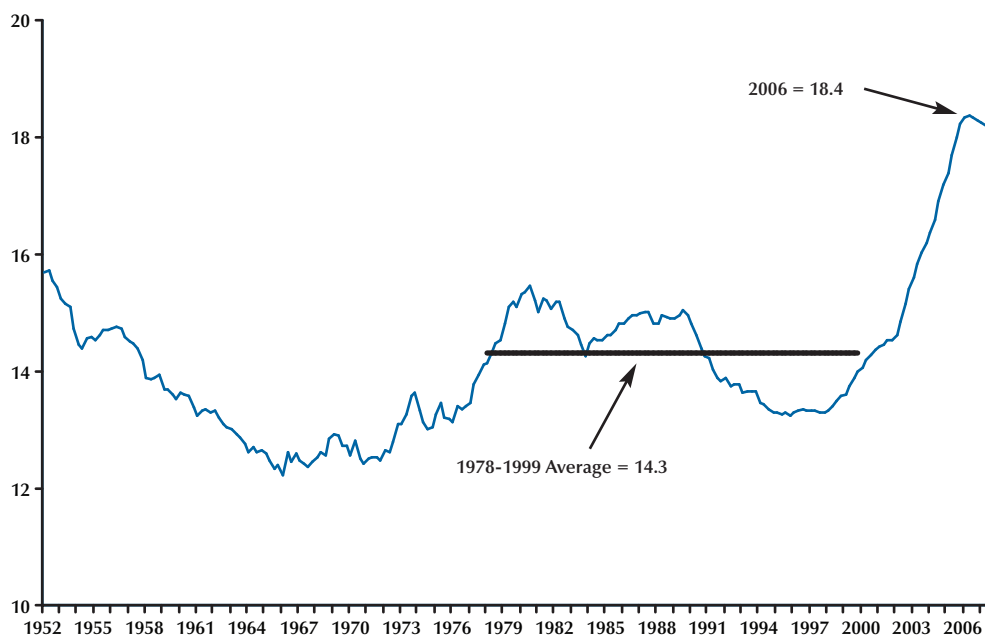
is, the authors use their VAR to allocate the volatility of growth and inflation to its various sources and find no role for monetary policy disturbances. I take this as evidence of the success of central bank stabilization policy.

Second, there is the always-vexing question of whether the sample record used in estimating the model is representative of the experience during the more recent period for which we would like to use the model. A number of concerns arise here. First, there is the problem of trying to separate changes in the federal funds rate from changes in the term spread. To see the possible problem, I have run a very simple regression of the 10-year bond rate on the federal funds rate, using a 48-month moving window, and plotted the results in Figure 1. I simply note that the late-1990s look very different from the period either before or after and suspect that the identification that allows Jarociński and Smets to estimate the

impact of the spread is coming from this part of the sample.

Continuing with the issue of the sample period, there is the question of how we should interpret house price data since 2000. Figure 2 plots the ratio of the value of the U.S. housing stock (from the Federal Reserve Flow of Funds data) to the housing rental service flow (imputed for the computation of the National Income and Product Accounts). The results are striking. The post-2000 data look dramatically different from what came before.

Finally, like others before them, Jarociński and Smets find significant housing wealth effects. Their estimate is that a persistent 1-percentage-point increase in house prices leads to a 0.1 percent increase in real GDP after four quarters—an elasticity of 0.1. Interestingly, because of the richness of their model, Jarociński and Smets are able to estimate that this effect is split roughly

**Figure 2****The Ratio of the Value of the U.S. Housing Stock to the Rental Service Flow**

SOURCE: Value of residential real estate: Federal Reserve Flow of Funds data, line 4 of Table B100 plus line 4 of Table B103. Rental service flow: the National Income and Product Accounts estimate of the total housing services in personal consumption expenditure, Table 2.3.5, line 14.

equally between investment in residential construction and consumption. So, the elasticity of consumption with respect to housing wealth is only about 0.05, which is at the low end of the range found by previous researchers (and cited in the paper).

To digress only slightly, I should note that it is not obvious that changes in the value of housing should affect nonhousing consumption at all. We all have to live somewhere. When home prices rise, it does not signal any increase in the quantity of economy-wide output. Although someone with a bigger house could sell it and move into a smaller one, there must be someone else on the other side of the trade. That is, for each person trading down and taking wealth out of their house, someone is trading up and putting wealth in. And renters planning to purchase should save more. All of this should cancel out so that in the aggregate there is no change!

Put another way, people own their homes to hedge the risk arising from potential changes in the price of purchasing housing services. They want to ensure that they can continue to live in the same size home. A rise in property prices means people are consuming more housing, not that they are wealthier.

And yet, everyone finds that when the housing market booms, people raise their consumption. Is this increase justified? Well, it depends. If the consumption and house price increases are both a consequence of higher estimated long-run growth, then the answer is yes. That is, if everyone now expects higher future incomes, then they will demand more housing along with more of everything else, and there is no bubble. So, if the house price boom is accompanied by an increase in the rate of growth of potential output, then it is not a bubble. An equity price boom would have to accompany this as well. And, importantly, this

would likely imply an increase in the long-run real interest rate, too. So, if housing, equity, and bonds all boom at the same time, we probably need not be concerned.

Regardless of my fairly minor concerns, I am convinced by Jarociński and Smets’s conclusion: Stabilizing real growth requires at least some focus on residential construction and housing demand. Housing may not be the business cycle, but it does play a measurable role. But, as Jarociński and Smets show, this depends primarily on long-term interest rates and housing demand, both of which seem to have a life of their own. Monetary policy-makers are left wondering what tools they have at their disposal to do anything about this.

## PART II: MCIs

The second part of the Jarociński and Smets paper presents a very clear discussion of MCIs. They conclude that, since 2000, Federal Reserve policy has been roughly neutral. Before working through this paper, I had not understood what MCIs are. Now I do, so I will make some attempt to share this new-found insight.

As Jarociński and Smets describe, in the past, several (but not many) central banks used MCIs as guides to policy formulation. More recently, business economists have been churning these out, combining a variety of financial indicators into something that is supposed to measure conditions in financial markets (the Goldman-Sachs Financial Conditions Index, DeutscheBank Financial Conditions Index, Morgan Stanley Financial Conditions Index, etc.).

The idea behind what I will call the “traditional MCI” is that it should provide a measure of the relative ease or tightness of monetary conditions. For policymakers, this MCI is supposed to answer the following question: Given the current state of the economy, how should policy-makers set their operational instrument?

The traditional MCI employed by the Bank of Canada, for example, was of the following type:

$$(1) \quad MCE = \alpha(r - r^*) + \beta(e - e^*),$$

where  $r$  is the interest rate instrument,  $e$  is the exchange rate, and the “\*” signifies an equilibrium level.

In practice, the problem is that (1) implies the same reaction to any deviation of the exchange rate from its equilibrium, regardless of the source. This creates problems, because supply shocks should (one assumes) require different responses from demands shocks. It matters *why* the exchange rate has moved.

As Jarociński and Smets describe in clear detail, this led researchers to suggest the computation of a “conditional MCI”—that is, conditional on some sort of information. A conditional MCI is the forecast  $k$  periods ahead for the output gap (actual output,  $y$ , less potential output,  $y^*$ ) or the inflation gap (the deviation of inflation,  $\pi$ , from its target,  $\pi^*$ ):

$$(2) \quad E[(y_{t+k} - y^*_{t+k})|I_t]$$

and

$$(3) \quad E[\pi_{t+k}|I_t].$$

Importantly, these expectations are conditional on the policymaker’s implied monetary policy reaction function. But, the information set used to compute the expectations need not have everything in it.

Looking at (2) and (3) leads me to ask the following question: If policymakers are doing their job, why would the conditional MCI ever deviate from zero?

Because the conditional MCI should be zero, what might we get from computing it? As it turns out, quite a bit. To see, we can start with a generic formulation of the policymaker’s problem. Assume that monetary policy sets the interest rate,  $r$ , to minimize the quadratic loss function,

$$(4) \quad L = E[\alpha\pi_t^2 + y_t^2],$$

subject to the constraints imposed by the dynamic structure of the economy:

$$(5) \quad \begin{bmatrix} y_t \\ \pi_t \end{bmatrix} = A(L) \begin{bmatrix} \varepsilon_t \\ r_t \end{bmatrix},$$

where  $A(L)$  is a polynomial in the lag operator,  $L$ , and  $\varepsilon$  is a vector of disturbances.

This problem yields a policy “rule” of the form

$$(6) \quad r_t^* = \phi(L)\varepsilon_t.$$

Substituting (6) into (5) yields a reduced form:

$$(7) \quad \begin{bmatrix} y_t \\ \pi_t \end{bmatrix} = \tilde{A}(L)[\varepsilon_t].$$

The conditional MCI is related to the properties of (7). Jarociński and Smets note that when  $\alpha = 0$  and  $A(L)$  in (5) has no lags, then

$$E[(y_{t+k} - y_{t+k}^*)|I_t] = 0$$

for all  $k$ . They interpret this as neutral policy.

Although this is fine as far as it goes, the conditional MCI is actually capable of addressing two additional questions:

- (i) Does the central bank need to change its reaction function to meet its stated goal? Is the reaction function (6) appropriate to minimize the loss function, (4)?
- (ii) What is the tradeoff or relative weight,  $\alpha$ , in the central bank’s loss function, (4)?

Looking at question (i), we see that this is not a question of whether policy is loose, tight, or neutral. The issue is whether it is properly responding to the shocks that are hitting the economy. Are policymakers moving their instrument to neutralize demand shocks completely? Are they changing the short-term interest rate to offset supply shocks appropriately? It is not about action, it is about reaction.

To understand (ii), take a look at the following static version of (5) written as an aggregate demand–aggregate supply model:

$$(8) \quad y = -\lambda r + \varepsilon_d \quad (\text{aggregate demand})$$

$$(9) \quad \pi = \omega y + \varepsilon_s \quad (\text{aggregate supply}).$$

The parameters  $\lambda$  and  $\omega$  represent the slopes of the aggregate demand and aggregate supply curves, respectively.

This setup implies a simple policy rule of the form

$$(10) \quad r^* = a\varepsilon_d + b\varepsilon_s.$$

Using this, we can now compute the implied conditional MCI for output and inflation (conditional on the optimal policy response, that is):

$$(11) \quad E(y|r^*) = -\frac{\alpha\omega}{(1+\alpha\omega^2)}\varepsilon_s$$

and

$$(12) \quad E(\pi|r^*) = \frac{1}{(1+\alpha\omega^2)}\varepsilon_s.$$

Now, take the ratio of (11) to (12) to obtain

$$(13) \quad \frac{E(y|r^*)}{E(\pi|r^*)} = -\alpha\omega.$$

So, once we know the slope of the aggregate supply curve, the ratio of these two conditional MCIs tells us the relative importance of inflation variability in the policymaker’s objective function—their inflation volatility aversion, if you will.

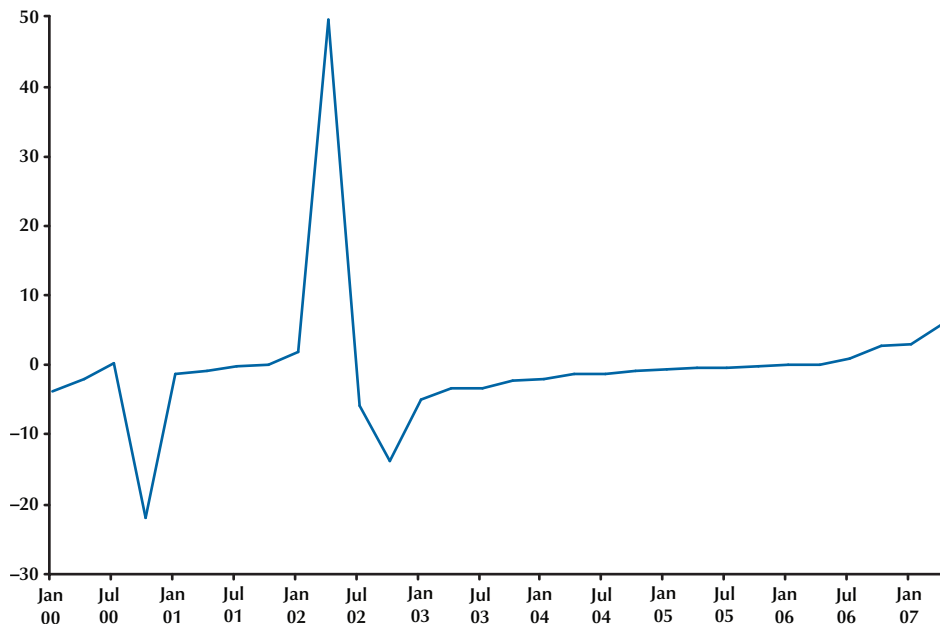
To figure out a reasonable value for  $\omega$ , take a look at the impulse responses in their Figure 4. The first row tells us that an interest rate shock (which is basically an aggregate demand shock) has roughly the same impact on inflation and output. This leads to the conclusion that  $\omega \approx 1$ . Next, take a look at the first row of their Figure 7A—the MCI conditional on monetary policy, but not on other financial conditions. (Because my very simple construction really models the unconditional, steady-state behavior, I have chosen to use the differences VAR estimates.)

The implied time series for  $\alpha$  is plotted in Figure 3. These point estimates move around quite a bit. But the primary problem is that they are negative. That is, inflation and output seem to be moving in the same direction at the horizons over which Jarociński and Smets report their conditional MCI computations.

There are several possible reasons for this. The first is that Jarociński and Smets’s Figure 7A reports the conditional MCI over different horizons for output and inflation. For the former it is

### Figure 3

#### Implied Inflation Volatility Aversion



SOURCE: Author's calculations using data from Jarociński and Smets (2008, Figure 7A).

one year, whereas for the latter it is two. So, although there might be a contemporaneous volatility tradeoff, it isn't showing up here. A second possibility is that monetary policymakers were not in fact acting appropriately to neutralize the housing demand shock. This interpretation is consistent with Jarociński and Smets's results that the boom which began in fall 2001 was the consequence of a combination of an increase in housing demand and expansionary monetary policy. My conclusion is that this means Federal Reserve policy was not on the output-inflation volatility frontier.

In conclusion, I found this a very rewarding paper to read. Although I may not subscribe to Jarociński and Smets's interpretation of the conditional expectation of output or inflation as an indicator of monetary conditions, I do agree with their conclusion that housing is at the core of the business cycle, so it should have a prominent role in the formulation of monetary policy.

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