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Commentary

Kenneth D. West

I applaud the goal of this paper. I take this goal to be: compare tightly parameterized but easily interpretable "New Keynesian" (NK) models, with loosely parameterized, but more difficult to interpret, structural vector autoregressions (VARs). A systematic comparison about what each tells us about monetary policy is long overdue. I also applaud the technical and econometric skill of the analysis in the paper.

Unfortunately, despite the technical excellence of the paper, Leeper and Zha did not make quite as much progress towards this goal as I had hoped. The NK models in this paper differ in important ways from what seems to me to be common in the literature. So in the end I did not learn as much as I had hoped to learn about NK models. A crude summary of the paper's empirical message is that certain VARs say plausible and persuasive things about monetary policy while NK models usually do not. I do not find this message convincing. (I should stress that, in personal conversation, one of the authors [Eric Leeper] has emphasized that he does not interpret the empirical results this way. So my reading of the results differs from his.)

Let me begin by summarizing the paper. Let x_t denote the output gap, i_t a short-term interest rate under the control of the Fed, and π_t the inflation rate, $\pi_t = p_t - p_{t-1}$, where p_t is the log of the price level. Sections II and III in the paper analyze a three-equation NK model. (Actually, there is a fourth equation, for potential output, but since that is not central I will ignore it.) The three equations are: goods market equilibrium (IS), aggregate supply (AS) (Phillips curve), and monetary policy (MP). The following specification is broad enough to include virtually all the empirical work in these two sections of the paper:

(1) (IS) $x_t = -b_1 i_t + b_2\pi_t + b_3 x_{t-1}$ + i.i.d. shock,

(2) (AS) $\pi_t = \lambda_1 x_{t-1} + \delta\pi_{t-1}$ + i.i.d. shock,

(3) (MP) $i_t = \gamma_{\pi 1}\pi_t + \gamma_{x1} x_t$ + i.i.d. shock.

Kenneth D. West is the Ragnar Frisch Professor of Economics in the Department of Economics at the University of Wisconsin, Madison.
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These notations match those in the paper except for the new symbols b_1, b_2, and b_3 in the IS curve.

The IS equation is most easily understood if one sets $b_3 = 0$, $b_1 = b_2 > 0$. Then we have

(4) (IS′) $x_t = -b_1(i_t - \pi_t)$ + i.i.d. shock.

Thus, the output gap is negatively related to the real interest rate, with current inflation proxying for expected inflation. This restriction is imposed in some of the empirical work. In some other parts, a value for b_1 was imposed and b_2 was estimated; this was done both with b_3 set to zero and with b_3 freely estimated.

The backward-looking AS function is familiar; in some of the empirical work, δ was set to unity, a restriction sometimes rationalized by the natural rate hypothesis. As well, the MP equation (3) is one version of the now-standard Taylor rule. In some of the empirical work, values for $\gamma_{\pi 1}$ and γ_{x1} were imposed.

Among the findings of Leeper and Zha:

- If the IS parameters b_1 and b_2 are unrestricted, IS cannot be distinguished empirically from MP: an identification problem.
- If one allows $b_1 \neq b_2$ or $\delta \neq 1$, then $\gamma_{\pi 1} > 1$ is not necessary for stability. This is a quantitative result.
- One can sometimes get sensible impulse response functions. A key requirement is that there is enough structure to separate IS from monetary policy.
- System and single equation estimations sometimes yield qualitatively different results, especially about $\gamma_{\pi 1}$.
- All specifications are strongly rejected by likelihood ratio tests.

Sections IV and V of the paper add money demand measured by M2, allow more dynamics, and experiment with an alternative money supply rule. The money demand equation is of the form:

(5) (MD) $M_t - p_t = \alpha_1 i_t + \alpha_2 y_t$ + unrestricted lags of M, p, i, and y + i.i.d. shock.

"More dynamics" comes first by allowing unrestricted second-order dynamics in "x" and "p" equations. The alternative money supply (MS′) rule is of the form:

(6) (MS′) $i_t = \gamma_m(M_t - M_{t-1})$ + unrestricted lags of M, p, i, and y + i.i.d. shock.

Leeper and Zha find that parameters and impulse response functions are reasonable, especially under the alternative monetary policy rule, equation (6). Finally, the authors allow unrestricted fourth-order (rather than second-order) dynamics and find with the equation (6) money rule that impulse responses are not only reasonable, but are stable across subsamples.

This paper has some interesting logical results. It nicely illustrates a potential identification problem, namely, that a particular NK model implies an inability to use data to distinguish IS from MP. It also makes the important point that the economic system determines the behavior of macro variables simultaneously. This point is relevant to VAR studies, which often assume a recursive structure with little or no explanation. It is also relevant to any NK studies that attempt to define stability in terms of a single coefficient in the monetary policy reaction function. Finally, it reminds us that models without money are making some assumptions about money. One such assumption is that the money demand specification (whatever it is) is restricted in such a way that one can identify the equations under study. A second is that the monetary aggregates do not appear in the monetary policy rule.

But as I noted in my introductory paragraphs, I find the empirical results, and in particular the implicit contrast between NK (poor and often implausible fits) and VAR (good and often plausible fits) models unpersuasive. It is evident that the dynamics in the paper's NK models are generally more restricted than is consistent with the well-known persistence shown by macro variables. Recall that the paper takes seriously the notion that an IS curve specialized as in equation (4) can capture, at least roughly, the dynamics of the output–real interest rate relationship. The paper also presumes that a Taylor rule (equation (3)) without lags will pass econometric diagnostics. I am not surprised to find that neither notion finds support in the data.

In places, the dynamics of this paper's NK models are also more restricted than that of both the theoretical and empirical NK literature. In particular, the specification of the IS curve does not match what seems to me to be the current frontier of that literature. That frontier begins with the consumption–real interest rate relationship that is long familiar from asset pricing studies:

$$E_t c_{t+1} - c_t = (1/\sigma)(i_t - E_t \pi_{t+1}). \tag{7}$$

Here, I digress from the notation in the paper: c_t is log consumption. But as in the paper, σ is the intertemporal elasticity of substitution. This may be rewritten as

$$c_t = -(1/\sigma)(i_t - E_t \pi_{t+1}) + E_t c_{t+1}. \tag{8}$$

With what in my view is a bit of a stretch, the papers in the NK literature often substitute output or the output gap for consumption. This implies

$$x_t = -(1/\sigma)(i_t - E_t \pi_{t+1}) + E_t x_{t+1}. \tag{9}$$

If one sets $b_1 = b_2 = (1/\sigma)$, $E_t \pi_{t+1} = \pi_t$, this becomes $x_t = -b_1(i_t - \pi_t) + E_t x_{t+1}$. This reduces to equation (4) *only* when $E_t x_{t+1}$ is i.i.d., which is not the case in any of Leeper and Zha's models.

The paper is also inconsistent with much of the empirical NK literature. The paper makes frequent reference to Taylor (1999b). It is true that the model emphasized in Taylor (1999b) is a simple special case of equations (2), (3), and (4) (with $\delta = 1$). But in comments from the floor during the conference, Taylor stated that he used this model as an expository device, to illustrate empirical results actually found in a much larger and richer model. As well, existing studies allow richer dynamics either through lagged endogenous variables (e.g., McCallum and Nelson, 1999) and/or serially correlated disturbances (e.g., Rotemberg and Woodford, 1997).

A minor technical comment: Because the authors study overidentified models, it is not clear to me how to interpret the impulse responses and variance decompositions. In an overidentified model, some of the orthogonality conditions do not hold in sample. This might apply in particular to the conditions that state that disturbances are mutually uncorrelated. If so, the fitted model's residuals will be correlated, and one must somehow parcel out the correlation to underlying uncorrelated shocks. It would have been helpful for Leeper and Zha to tell us how they did so.

In sum: The paper by Leeper and Zha considers an interesting and important topic. It makes only a start on this topic. I very much look forward to a more comprehensive analysis in their future papers.