Commentary

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The Taylor rule is widely seen as a good summary of what the Federal Reserve does. Though the rule cannot easily be fitted to actual data as subsequently revised, at least for a full postwar sample, it can be fitted to real-time data (i.e., data as seen at the time), as shown by earlier work by Orphanides (2003). But in practice the Fed's Federal Open Market Committee (FOMC), if it is using a Taylor rule, will look at its own forecasts or projections. Orphanides and Wieland (2008) examine whether a Taylor rule can be fitted to the FOMC's own projections since 1988. They find that it can with appropriate parameters that satisfy the Taylor principle—that is, that give a unique stable solution under rational expectations. Furthermore, they find that the rule works better with these projections and resolves various puzzles regarding the data on outcomes.

This is without question an interesting finding; the paper is clear, cogent, and persuasive. Many will be totally persuaded by it; however, I do have a few doubts. Let me begin with some issues of specification and estimation and then proceed with two wider issues.

SPECIFICATION AND ESTIMATION

The Specification of the Taylor Projections Rule for Changes in Targets and Definitions

As the authors note, there remains a puzzle: In spite of the change in the inflation definitions, particularly that from the consumer price index (CPI) to the personal consumption expenditures (PCE) deflator, their estimates find no shift in the Fed rule. Their experiments with a rule estimated for the CPI in the 1990s shows that the rule should have shifted up on the move to the PCE in the 2000s. The rule might have also shifted with the natural rate of employment; however, when they included this rate in the equation along with the inflation definition, the rule did not shift in line with either or both together. Had the equilibrium rate of interest been known, there may have been no puzzle. However, the authors argue that they had no estimate of this to include as a test; but surely index-linked government bond yields provide some idea of shifting real rate equilibria?

This puzzle is particularly odd when viewed side by side with the explicit 0.5 percent shift in target inflation that occurred when the United Kingdom made an essentially similar change—from the retail price index to CPI. The U.S. CPI, too, systematically grows 0.5 percent or so faster than the PCE. The absence of a noticeable shift in the rule makes one wonder exactly what the FOMC projections are—a topic I return to below.

The logic of the Taylor projections rule absolutely requires that the rule shifts when the inflation definition changes; this shift should have been imposed on the equation, together with some estimate of changing real interest rate equilibria based on index-linked bond-yield trends.

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Estimation

I am concerned with the authors' estimation. They use ordinary least squares, a single-equation estimator, which is open to bias because of the correlation of the error term (the FOMC's monetary judgment, or interest rate "shock") with both endogenous variables. These are defined as lagged variables; but truly they are the FOMC's current view of the forecast environment at the time interest rates are set and use contemporaneous data and reports on both output and inflation. Given signal extraction and the semiannual frequency of the data, it is clear that current data will influence projections and so the current interest rate judgment; at the same time, the interest rate shock will affect output and inflation in the semiannual time frame. Furthermore, the error is autocorrelated, except in the projections version when a lagged interest rate is included for "adjustment" reasons. However, in principle, even if the FOMC revises its judgment each semiannual period, each new judgment is unlikely to be independent of the last one; given that it represents views on such things as asset price movements, exchange rate behavior, and special factors like the 9/11 attacks. The FOMC's judgment should show some persistence, and indeed that is what most dynamic stochastic general equilibrium (DSGE) modelers assume about a monetary shock.

Given these issues, I regard the estimation methods of this paper as rather casual. For a start, we need more information on the error process; does "adjustment" really eliminate autoregressiveness in the error? Second, we need some effort to estimate the equation in a bias-free manner; full-information methods are ruled out by the absence of the rest of the model, but on this front it would be helpful to see some instrumental variable or two-stage least-squares results.

Third, however, there are difficulties with any single-equation estimator, as pointed out by Cochrane (2007a). To illustrate his point, consider a standard New Keynesian model with a strict inflation-targeting rule, $R_t = \psi\pi_t + i_t$ (i_t, the shock, will in general be autocorrelated and also correlated with π_t). If we solve the model by imposing a stable solution, inflation is an autoregression, say, $\pi_t = \rho\pi_{t-1} + u_t$ (where the error is also autocorrelated, say, with a root κ), and it follows that the Fisher identity gives interest rates as $R_t = r_t + E_t\pi_{t+1}$, which thus equals $\rho\pi_t + [\kappa u_t + r_t]$, where the term in square brackets is an autocorrelated error, correlated with π_t. How can this regression be distinguished from the inflation-targeting regression? A systems estimator imposing all over-identifying restrictions on the model is the only way.

Modeling the FOMC Projections Rule

To use this FOMC projections rule in a model requires some transfer function relating the Fed's projections to the actual state of the economy. Thus, if the version here is to be taken seriously as a representation of policy, we need to know its properties in a full model, but of course those properties depend on how the FOMC projections are related to the actual economy.

It matters a lot whether they are, for example, biased and/or subject to learning or rational expectations. A key reason for knowing these details is that they would make it possible to estimate the rule appropriately by full-information methods, as already argued.

SOME WIDER ISSUES

There are some wider issues I see as interestingly raised by this paper. The first is what exactly the Taylor rule is and how it fits into economic thought on policy rules. The second is whether this paper and associated work clinches the debate on which monetary rule was actually being pursued by the FOMC; I will argue that this turns on a difficult issue of identification.

What Exactly Is the Taylor Rule? Origins and Application

John Taylor wrote his paper (Taylor, 1993) proposing the rule in the early 1990s. It seems to have been heavily influenced by a 1989/90 Brookings conference event, which discussed the performance of different monetary rules (money supply, exchange rate targeting, or pegging, mainly) within large models of the world economy, one of which was John Taylor's "Taylor World Model" (another was my "Liverpool World Model"). As a new departure, Dale Henderson and Warwick McKibbin asked the modelers—around a dozen teams—to evaluate a new suggestion that money be bypassed by setting interest rates directly in response to macro data. Various formulations were tried.

The modeling teams drew a blank initially in solving their models under these rules; it seems that they were tripping over indeterminacy and had not discovered the Taylor principle, but it may also have been that the algorithms being used at that time (mostly variants of the Fair and Taylor, 1993, method) simply had difficulty homing in on the solution.

These proposed rules, we may well now have forgotten, were a quite unfamiliar way of thinking about monetary policy. It is true that rules for setting interest rates had had a long history (as pointed out by Stanley Black at this Federal Reserve Bank of St. Louis conference); indeed such rules were dominant in the postwar Keynesian era up to the 1970s. However, there was a strong reaction against such ideas in the late 1970s and 1980s as the rational expectations revolution took effect; interest rate rules were felt to give a poor nominal anchor (and would give rise to indeterminacy unless tied to a nominal target) and instead the setting of the money supply was emphasized. This accounts for the fact that the primary rules investigated in the Brookings conference were either money supply rules or exchange rate rules.

When the teams had succeeded in solving their models for these new rules, they were found to perform surprisingly well and the results were written up by Henderson and McKibbin (1993a,b) at great length (1993a is in Bryant, Hooper, and Mann, 1993, Chap. 2; 1993b was a version of this chapter given at the same Carnegie-Rochester conference where Taylor presented his own paper, Taylor, 1993). It seems that the success of these rules in a wide variety of models indicated a surprising robustness, and it was this robustness that Taylor later emphasized as a major attraction of his own rule. He elaborated on this in further tests on other models. After the Brookings conference, in any case, John Taylor formulated his rule, which could reasonably be termed the Henderson-McKibbin-Taylor rule.

Nevertheless, there seems to be a difference between these authors' views. Whereas Henderson and McKibbin were solely discussing what would be a good rule and never, as far as I am aware, argued that it was actually pursued, John Taylor went further and argued not only that it worked well but also that monetary policy could be thought of as being done this way. A paraphrase of his distinctive message could be "Look, here is a rough approximation of what a good central bank actually does and has done in the United States in recent years."

Thus, the attraction of the Taylor rule was that it was descriptive as well as normative; this was the new ingredient that Taylor added.[1] Orphanides has in his earlier (2003) work argued that it can indeed describe FOMC behavior for the whole postwar period if real-time data are used. Yet, as I shall argue below, it is this implicit claim that the rule is descriptive that is problematic.

We can pursue this history further with a review of how New Keynesian authors use the Taylor rule to account for inflation in the postwar period. Here, I follow the points made by Cochrane (2007b). He notes that these authors (e.g., Clarida, Galí, and Gertler, 2000) have argued that up to around 1980 the Taylor rule being pursued by the Fed violated the Taylor principle and thus produced or permitted high inflation; after 1980, the Fed raised the coefficient on inflation above unity and inflation was brought down. Yet, if the Fed before 1980 had such a Taylor rule, then inflation would have been indeterminate. So, in what sense does this account for any inflation path at all?[2] (This is resolved by Orphanides, who says that, throughout, the Fed had a good rule but just

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1 Yet, there is ambivalence even here. For example, McCallum stated in answer to my question at the Carnegie-Rochester 2002 conference on the Taylor rule that the rule was essentially normative, not descriptive. Ireland (2003), at the same conference, however, took the view that it was both a normative rule (enabling monetary economists to coalesce around inflation targeting after years of wrangling about other rules) and positive, in that central banks actually thought of policy in terms of Taylor rules.

2 The Taylor principle and this stable-sunspot corrollary can be illustrated for a simple model in which real interest rates are an exogenous AR(1) process (more complex models can produce

had bad estimates of the output gap in the 1970s; to account for inflation, then, a full model including private sector information and learning is needed, which then makes this a branch of the learning literature and not a rational expectations model like the New Keynesian one.) For the post-1980 period, Cochrane (2007b) argues that the way the rule works to discipline inflation is in any case incredible: In effect, the Fed threatens to raise inflation and interest rates without limit should inflation deviate from the stable path. Because people believe this threat, inflation goes to this unique path. Yet, what stops them from choosing one of these deviant paths, so that the Fed has to go along with them? Deviant paths in models with money supply targets can be suppressed by Fed action on the money supply; here it is not clear what the Fed will do to rule out deviant paths.

Thus, there is a doctrinal puzzle in the Taylor rule approach. The Taylor rule emerged from a money-supply-rule world because models were found to behave rather well when the rule was imposed together with some unspecified device to rule out unstable paths. However, it was forgotten that in previous models that device had involved action on the money supply. I think what this shows is that the Taylor rule is an essentially incomplete statement about monetary policy. One has to assume that the authorities have some additional tool in their locker to rule out unstable paths. Cochrane (2007b) argues this can be a non-Ricardian fiscal policy. It could also be a money supply policy of the central bank.

Does This Work Compel Us To Believe the Fed Really Was Pursuing a Taylor Rule?

Identification Across Possible Models. The problem with the claim that the Fed projections rule is descriptive is in a general sense one of identification across possible models. DSGE models give rise to the same correlations between interest rates and inflation, even if the Fed is doing something quite different, such as targeting the money supply. For example, Minford, Perugini, and Srinivasan (2002 and 2003) show this in a DSGE model with Fischer wage contracts. Gillman, Le, and Minford (2007) use a real business cycle growth model with cash and credit in advance to derive a steady-state, or cointegrating, relation between interest rates and inflation and the growth rate when money supply growth is fixed—a "speed limit" version of the rule. The route they use to obtain an apparent Taylor rule is the Fisher equation, which links nominal interest rates with expected future inflation and real interest rates; they then use the relation elsewhere in the model, equating growth with the real interest rates to obtain a "Taylor rule."

This identification would still be a problem under the projections rule because of how the transfer function relates the actual data to the projections; that is, any relationship between interest rates and FOMC projections could be translated by this function into one between interest rates and actual data. I will return below to what this transfer function might look like. For now, let us just compare the normal Taylor rule using actual data with the other rule's implied Taylor-type equation.

To illustrate the point in detail, consider a popular DSGE model but with a money supply rule instead of a Taylor rule:

(IS) $y_t = \gamma E_{t-1} y_{t+1} - \phi r_t + v_t$

(Phillips) $\pi_t = \zeta\left(y_t - y^*\right) + \nu E_{t-1}\pi_{t+1} + \left(1-\nu\right)\pi_{t-1} + u_t$

(Money supply target) $\Delta m_t = m + \mu_t$

(Money demand) $m_t - p_t = \psi_1 E_{t-1} y_{t+1} - \psi_2 R_t + \varepsilon_t$

(Fisher identity) $R_t = r_t + E_{t-1}\pi_{t+1}$

This model implies a Taylor-type relationship that looks like

slightly different Taylor conditions): $r_t = \rho r_{t-1} + \varepsilon_t$. Now add a Taylor rule for inflation only, $R_t = \alpha\pi_t$, and the Fisher identity, $R_t = r_t + E_t\pi_{t+1}$. The general solution of the model is $\pi_t = kr_t + \xi_t$, where $k = (1/\alpha - \rho)$ and the sunspot $\xi_t = \alpha\xi_{t-1} + \eta_t$ with η_t chosen randomly (the solution can be verified by substituting it into $r_t = -E_t\pi_{t+1} + \alpha\pi_t$). If $\alpha \geq 1$, then the sunspot is ruled out by the condition that the solution must be stable. But if $\alpha < 1$, then inflation is a stable process with a sunspot and hence indeterminate in that each period the path can jump anywhere.

$$R_t = r^* + \pi^* + \gamma\chi^{-1}\left(\pi_t - \pi^*\right) + \psi_1\chi^{-1}\left(y_t - y^*\right) + w_t,$$

where $\chi = \psi_2\gamma - \psi_1\phi$ and the error term, w_t, is both correlated with inflation and output and autocorrelated; it contains the current money supply/demand and aggregate demand shocks and also various lagged values (the change in lagged expected future inflation, interest rates, the output gap, the money demand shock, and the aggregate demand shock). This particular Taylor-type relation was created with a combination of equations—the solution of the money demand and supply curves for interest rates, the Fisher identity, and the IS curve for expected future output.[3] But other Taylor-type relationships could be created with combinations of other equations, including the solution equations, generated by the model. They will all exhibit autocorrelation and contemporaneous correlation with output and inflation, clearly of different sorts depending on the combinations used.

Identification is of course a quite separate matter from estimation; the usual assumption is that we have infinite amounts of data to carry out completely accurate estimation. In fact, OLS estimation would be inappropriate, as we have seen, because it forces the error term to be orthogonal to the regressors, yet because this cannot be the case, it induces bias. Instead, estimation is done by a full-information estimator, which allows for the model's simultaneity, including of the error term in this equation. With infinite data, we retrieve the parameters exactly and also the error terms. The error term in the Taylor rule proper is, as we have seen, the "monetary shock" created by FOMC special judgments on current events. This is, therefore, like the errors in the Taylor-type relationships, correlated with current events, including the output gap and inflation, both because these influence FOMC judgments (even if they do not observe the correct values, they know enough to extract signals from current reports, snapshot statistics, etc.) and because these shocks may affect current output and inflation.

Distinguishing between the two equations is likely to be difficult in general. The error terms of both the Taylor rule and Taylor-type relations are autocorrelated and correlated with output and inflation. The coefficients on output and inflation in both are positive and that on inflation in the Taylor rule will be higher than the one in the Taylor-type relation if $\psi_2\gamma - \psi_1\phi$ is less than γ. The constant in both is the steady-state value of inflation plus the real rate of interest.

Identification by "Narrative Evidence" and by Projections? Could we nevertheless be confident that there is a Taylor rule because of what we definitely know about policymakers' behavior (what we might call narrative evidence)? In his replies to my comments, Athanasios Orphanides stated that FOMC minutes during this sample period (from 1988) supported the interpretation that the projections determined interest rate setting. However, the problem is that we cannot see directly in this way what FOMC policymakers were doing. They vote and there are minutes, but we do not know what they are really trying to do. We are familiar from psychology that people may describe their actions in one way when in truth they are being compelled to act (in a "deterministic" way) by other forces; also there may be reasons of prudence or politics that lead people to disguise the motives for their actions.

Even when there is a legal objective, as in the United Kingdom, policymakers pursue all sorts

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[3] From the money demand and money supply equation,

$$\psi_2\Delta R_t = \pi_t - m + \psi_1\Delta E_{t-1}y_{t+1} + \Delta\varepsilon_t - \mu_t:$$

Substitute $E_{t-1}y_{t+1}$ from the IS curve and then inside that for real interest rates from the Fisher identity, giving

$$\psi_2\Delta R_t = \pi_t - m + \psi_1\left(\tfrac{1}{\gamma}\right)\left\{\phi\left(\Delta R_t - \Delta E_{t-1}\pi_{t+1}\right) + \Delta y_t - \Delta v_t\right\} + \Delta\varepsilon_t - \mu_t;$$

then, rearrange this as

$$\begin{aligned}&\left(\psi_2 - \tfrac{\psi_1\phi}{\gamma}\right)\Delta\left(R_t - R^*\right)\\ &= \left(\pi_t - m\right) - \tfrac{\psi_1\phi}{\gamma}\Delta E_{t-1}\pi_{t+1} + \tfrac{\psi_1}{\gamma}\Delta\left(y_t - y^*\right) - \tfrac{\psi_1}{\gamma}\Delta v_t + \Delta\varepsilon_t - \mu_t,\end{aligned}$$

where the constants R^* and y^* have been subtracted from R_t and y_t, respectively, exploiting the fact that when differenced they disappear. Finally, obtain

$$\begin{aligned}R_t &= r^* + \pi^* + \gamma\chi^{-1}\left(\pi_t - \pi^*\right) + \psi_1\chi^{-1}\left(y_t - y^*\right)\\ &+ \left\{\begin{matrix}\left(R_{t-1} - R^*\right) - \psi_1\phi\chi^{-1}\Delta E_{t-1}\pi_{t+1} - \psi_1\chi^{-1}\left(y_{t-1} - y^*\right)\\ -\psi_1\chi^{-1}\Delta v_t + \gamma\chi^{-1}\Delta\varepsilon_t - \gamma\chi^{-1}\mu_t\end{matrix}\right\},\end{aligned}$$

where we have used the steady-state property that $R^* = r^* + \pi^*$ and $m = \pi^*$.

of private agendas. Thus, in the United Kingdom recently we have had different members of the Bank of England Monetary Policy Committee being particularly concerned with measures like house prices, other asset prices, the state of the labor market, and latterly "moral hazard." All these have jostled in the voting for a place in interest rate setting.

Furthermore, there have been many phases in U.S. policy, as in U.K. policy. Under Bretton Woods, the dollar's fixed rate against the Deutsche mark put some brakes on U.S. policy. After the end of Bretton Woods, leading to the Louvre and Plaza accords there were still flurries of concern with exchange rates; intermittently right up to present times there has been policy concern with the current account deficit and the need for exchange-rate movement. In 1979-81 there was a big debate about money supply targets and an episode of reserve targeting. Congress mandated that the Fed give an account of its efforts to hit various money supply targets in the 1970s and 1980s. Electoral pressures seem to have played a part at times. Further, we know that for much of the earlier postwar period some policymakers believed that inflation could be contained by wage/price controls and interest rates could be used to bring down unemployment. Even in recent times, influential policymakers have been opposed to an inflation target—including some policymakers inside the Fed itself—on the grounds that there needs to be "flexibility" to deal with unemployment.

Finally, I note that the Fed, more or less now alone among central banks within the Organisation for Economic Co-operation and Development, does not have a formal inflation target set by law. This certainly makes it harder, even in this recent sample period from 1988, to use narrative evidence to identify the FOMC's rule.

Can We Be Confident Because We See Such a Close Correlation Between Projections and Interest Rates? It may be argued that such a high correlation (an R^2 of over 90 percent) proves beyond doubt that Fed governors were using their projections to produce their view on interest rates. This too is problematic; indeed such a high R^2 arouses suspicion.[4] We do not know how these projections are produced, only that each governor sends them to the meeting having produced them with the help of his or her staff. They are then cropped and averaged to give the published values for the Humphrey-Hawkins legislation's requirements. A reasonable suspicion would be that the fit is so close because the governors want to present a plausible public case for their views on interest rates; hence, governors that wish to raise rates will generate forecasts of higher inflation and/or higher output gap (overheating). Their reasons for raising rates may be quite different from these. Thus, their projections are molded by their views, not as assumed here, the other way round, views by projections.

On this skeptical view of such a close fit, we have no evidence of what was driving the governors' views. It could be that they are closet monetarists. It could be that they worry about asset prices or their latest regional data—any number of things. In the end, it still comes out looking like they follow a Taylor projections rule.

This way of thinking about FOMC decisions could account for the lack of shift in the inflation forecasts after the change from CPI to PCE: If the governors are just rationalizing their interest rate decisions by producing projections, they will choose numbers not in the spirit of a good forecast but more in order to signal clearly the need they perceive to raise or lower rates. The actual number would be of little significance; the direction would be solely what mattered.

Consider now what the transfer function might look like. It translates the governors' average inflation and unemployment projections into the state variables producing them. Hence, these variables would be a mixture that could include domestic asset prices, the exchange rate, the money supply, unemployment and its dispersion—any variables that governors believe would trigger their desired interest rate change.

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[4] I owe this point to Clemens Kool. In his conference comment, Steven Cecchetti (2008) also questioned the meaning of these forecasts.

CONCLUSIONS

This interesting paper shows that, if one thinks the Taylor rule definitely describes the FOMC's behavior over the past two decades, then a rather convincing relationship can be found, though there are concerns about estimation, how the transfer function relates projections to the actual data, and the puzzling lack of shift in the projections in response to well-known shifts in the environment. Yet the Taylor rule, as its intellectual history suggests, is an incomplete description of monetary policy, at least within a New Keynesian model; it cannot account for determinate inflation before 1980, and after 1980 it lacks a clear mechanism for ruling out unstable paths.

If one is not a priori convinced it describes the FOMC's behavior in the past two decades, then there is a nontrivial issue of identification: Taylor-type relationships can emerge from a DSGE model where no Taylor rule is guiding monetary policy. To test the Taylor rule descriptive hypothesis convincingly, one really needs to compare results for a full model with alternative formulations of monetary policy. That way we can see whether the data rejects one or other policy formulation when embedded in a full-model structure.

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