Volume Title: NBER Macroeconomics Annual 2008, Volume 23

Volume Author/Editor: Daron Acemoglu, Kenneth Rogoff and Michael Woodford, editors

Volume Publisher: University of Chicago Press

Volume ISBN: 0-226-00204-7

Volume URL: http://www.nber.org/books/acem08-1

Conference Date: April 4-5, 2008

Publication Date: April 2009

Chapter Title: Comment on "On the Need for a New Approach to Analyzing Monetary Policy"

Chapter Author: Bennett T. McCallum

Chapter URL: http://www.nber.org/chapters/c7292

Chapter pages in book: (p. 449 - 458)

Comment

Bennett T. McCallum, *Carnegie Mellon University and NBER*

I. Introduction

This is an interesting and challenging paper, in which Atkeson and Kehoe put forth a very strong critique of current mainstream monetary policy analysis. Monetary economists have, of course, been rather pleased with the development of their subject over the past 10–15 years, current U.S. policy difficulties notwithstanding. Indeed, the tone of a prominent recent expository paper by my colleague, Marvin Goodfriend, is somewhat triumphal in spirit.[1] The spirit of the Atkeson and Kehoe paper, by contrast, is conveyed by a recent publication of theirs, together with coauthor Fernando Alvarez, which bears the title "If Exchange Rates Are Random Walks, Then Almost Everything We Say about Monetary Policy Is Wrong" (Alvarez, Atkeson, and Kehoe 2007). That paper focuses on exchange rate failures, whereas the current one stresses the term structure of interest rates, but the line of argument is basically the same.

The title of the 2007 paper leads me rather naturally to ask myself what it is that I would say in answer to the implied question, "What important things do monetary economists really know—or at least believe—about monetary policy?" My own answer to that question would go along the following lines: (i) We believe that if the monetary authority keeps monetary policy expansionary for a substantial length of time, the main effect will be to generate a higher inflation rate than would have prevailed otherwise, with little or no overall effect on aggregate production and employment. (ii) Nominal interest rates will be higher, also, with real rates being affected very little. (iii) If, however, the monetary authority changes policy unexpectedly and abruptly in an expansionary direction, there will most likely be an expansion in aggregate output and employment—but it will be only temporary. (iv) If these changes are in the direction of tighter policy, the signs of the above-mentioned effects

978-0-226-00204-0/2009/2008-0603$10.00

will be reversed. (v) In particular, the monetary authority has the power to generate a recession, in which output and then the inflation rate will fall. (vi) The precise nature of the mechanism that generates the real effects of monetary policy changes of this type is not very well understood. Then, if my questioner had not wandered away in boredom, I would want to add something like the following: (vii) The foregoing points refer to an expansionary or contractionary monetary policy stance—loose or tight—but how is this measured? Well, a sustained high growth rate of the stock of base money will (under most institutional arrangements) be expansionary, but matters are a little less clear-cut when the central bank actually carries out its policy by manipulating overnight interest rates. Nevertheless, there are ways in which we can characterize tighter versus looser policy in terms of interest rate rules by reference to the implied target inflation rate, the strength of responses to deviations from target, and so forth.

Now, I suspect that Atkeson and Kehoe probably do not disagree with most of these statements as to what monetary economists know (or believe), even on a substantive basis.[2] But their title of the current paper, as distinct from the 2007 item, refers to a need for a new *approach* to monetary policy analysis. So let us turn to a consideration of what today's mainstream approach is. As it happens there is a short statement of that type, in a paper of mine, that gives the following description. The approach is one in which "the researcher specifies a quantitative macroeconomic model that is intended to be structural (invariant to policy changes) and consistent with both theory and data. Then, by stochastic simulation or analytical means, he determines how crucial variables (such as inflation and the output gap) behave on average under various alternative policy rules. Usually, rational expectations (RE) is assumed in both stages. Evaluation of the different outcomes can be accomplished by means of an optimal control exercise, or by reference to an explicit loss function, or left to the judgment (i.e., loss function) of the implied policymaker" (McCallum 2001, 258). Here, too, I doubt that Atkeson and Kehoe have any major disagreement with this general approach. What they do disagree with, if I understand at all, is the model that is typically used in recent work and taken to be structural.[3]

In a sense my last statement could be regarded as merely quibbling over their title. But the point seems to be one of some importance: if Atkeson and Kehoe can generate an optimizing model that incorporates reliable, quantitative estimates reflecting time-varying "risk" (i.e., state-dependent variances and covariances) and endogenously explains

inflation and output fluctuations, then monetary economists would presumably be happy to incorporate such features in their models—and would not consider this to reflect any basically new approach. Be that as it may, in what follows I will briefly review their featured empirical regularities, discuss issues concerning their suggested modeling strategy, and provide a brief conclusion.

II. Empirical Regularities

Atkeson and Kehoe begin, in Section I, with "four key regularities regarding the dynamics of interest rates and risk that we use to guide our construction" of a model and its pricing kernel. The first two pertain to a principal components analysis of a collection of interest rates, specifically, a 3-month T-bill rate and zero-coupon yields on U.S. Treasury securities with k-year maturities for $k = 1, 2, \ldots, 13$. Time series observations are monthly over 1946.12–2007.12. The first regularity is that "the first principal component accounts for over 90% of the variance of the short rate [i.e., the 3-month rate]." The second regularity is that "the second principal component is very similar to the yield spread between the short rate and the long [i.e., 13-year] rate." Having demonstrated these facts—and also that the first component is correlated even more strongly with the long rate—the authors henceforth use just the short and long rates.

More substantively (and more questionably), the third and fourth regularities pertain to expected excess returns in the context of term structure and international exchange rate contexts. Specifically, movements in yield spreads and exchange rate premia are "associated with movements in risk." The way in which these regularities might be regarded by some readers as questionable is that, in many studies, "risk" is operationally the name that is given to differentials in expected returns that the analyst's model is not able to explain.

Later in the paper, in Section V.A, Atkeson and Kehoe plot short-rate and long-rate time series for the United States over an extended period from 1836 through 2007. In addition, they include analogous plots for the United Kingdom, France, Germany, and the Netherlands. In all of these, the fluctuations of the long rate represent "a much smaller fraction of overall fluctuations in the short rate than they are in the postwar period." Thus, they state: "A central question in the analysis of monetary policy at the secular level then is, What institutional changes led to this pattern?" In the preliminary version of this comment, I responded to a more pointed and strongly emphasized version of this query by

stating that, to me, it is no surprise that expectations of future interest rates became unanchored during the post–World War II period, because, to again quote myself,

> [the] collapse of the Bretton Woods system created, for the first time in history, a situation in which the world's leading central banks were responsible for conducting monetary policy without an externally imposed monetary standard (often termed a "nominal anchor"). Previously, central banks had normally operated under the constraint of some metallic standard (e.g., a gold or silver standard), with wartime departures being understood to be temporary, i.e., of limited duration. Some readers might not think of the Bretton Woods system as one incorporating a metallic standard, but by design it certainly was, since the values of all other currencies were pegged to the U.S. dollar and the latter was pegged to gold at $35 per ounce. (McCallum 1999, 175–76)

All in all, it seems that there is no difficulty in understanding why an altered monetary policy regime generated different expectations regarding inflation and therefore future short interest rates in the post–World War II era. The variability in long rates during the 1960s developed as market participants began to see that the United States was not going to be bound by its commitment to maintain the $35 per ounce price of gold. Then the variability jumps up around the time of the Bretton Woods collapse in 1971—see Atkeson and Kehoe's figures 6*A*–6*E*—and continues to rise into the Volcker disinflation that was painful (with extremely high nominal interest rates) but that ultimately succeeded in restoring some semblance of a nominal anchor.

What about the return to stability that may have occurred around 1990? That year is, of course, the year in which the first central bank (New Zealand) officially adopted a monetary policy regime of "inflation targeting" (IT). At that time, this was taken to mean a policy whose only objective was a low and stable inflation rate. Since then, the IT term has come to be applied to regimes that give more weight to output/employment stabilization, but most monetary economists understand it as continuing to emphasize, as the primary goal, inflation control. So again the timing is about right for the possible recovery of anchored expectations that the first empirical regularity is said to reflect.

To this general line of argument, Atkeson and Kehoe object: "But this answer is, at best, superficial. In the prewar era, countries chose to be on the gold standard most of the time and chose to leave it when it suited their purposes. Thus, the relevant questions are, rather, What deeper forces led agents to have confidence that their governments would choose stable policy over the long term? And what forces led them to lose

this confidence after World War II? Only if we can quantitatively account for this history can we give advice on how to avoid another great inflation."

In this regard it must be said that I consider an explanation of the evolution of beliefs regarding the monetary standard, held by citizens of the United States, Great Britain, Germany, and so forth, to be somewhat beyond the scope of monetary policy analysts. To think about this issue, one must recognize that historically "the gold standard" required not just that the monetary authority would stand ready to exchange gold and currency at a specified rate but also that this rate should be unchanged "forever." That arrangement made it such that severe inflation would not occur—even the major historical gold discoveries did not generate sustained inflation on the order of 10% per year—but it did generate more cyclical instability of real variables than we have had in the postwar era. Could policy of that type win popular support in today's environment in the United States? If not, which would be my answer, then we need an entire unified social science to provide an explanation at "a deeper level." And such an explanation—which would need to emphasize enormous developments in the media, extensions of suffrage, evolution of religious beliefs, attitudes toward the role of government, and so on—would not be of much help to central bankers. Let us turn then to monetary policy analysis considered more narrowly.

III. Basic Analysis

The heart of Atkeson and Kehoe's paper is a recommended response to the third and fourth of the regularities mentioned above, that is, that measured excess returns on multiperiod bonds fluctuate strongly with yield spreads for bonds of different maturities and for international exchange rates. These regularities are translated by Atkeson and Kehoe into an argument that the consumption Euler equation, some version of which (often termed an expectational IS equation) is one basic ingredient of current macro-monetary models, performs very poorly empirically. This is, of course, true for the simplest versions, but that problem has been widely recognized by monetary economists. A nice overview of empirical weaknesses of so-called New Keynesian models was provided some years ago in a working paper by Richard Dennis (2003), which is briefly and nontechnically summarized in Dennis (2004). (The weaknesses discussed there relate to the Calvo-style price adjustment relation, as well as the consumption Euler equation.) Dennis distinguishes between the bare-bones "canonical model" and a "hybrid" version that adds habit formation in consumption behavior to the basic

consumption-saving relationship and also adds a somewhat dubious dependence on lagged inflation to the basic Calvo price adjustment relation. He recognizes, following Estrella and Fuhrer (2002), that "the problem with the canonical model is that the behavior of output, consumption, prices, and interest rates suggested by the model are fundamentally at odds with observed data" (Dennis 2004, 1). The hybrid model performs better, in terms of matching quarterly data, but "there are a number of areas where the hybrid model's responses differ importantly from" impulse responses of an identified vector autoregression (VAR; Dennis 2004, 3).

The point here is that monetary economists are quite aware that current models, even with elaborations of the type utilized by Christiano, Eichenbaum, and Evans (2005) or Smets and Wouters (2007), have empirical weaknesses, and they have been active in trying to eliminate these problems by improved specification. One pertinent and recent example concerns the discouraging results reported by Canzoneri, Cumby, and Diba (2007), that is, that inclusion of habit formation in consumption behavior unrealistically increases the variability of interest rates.[4] Subsequent results by Collard and Dellas (2007) indicate, however, that this deterioration obtains when the household utility function is taken to be additively separable in consumption and leisure. If instead consumption and leisure enter the function in a Cobb-Douglas manner, then inclusion of habit results in an improved—not worsened—match of the model's interest rate variability to that of the data.

I might also remark that Atkeson and Kehoe's way of considering the empirical failure of the Euler equation seems questionable. Specifically, they discuss the relationship in a manner that would be appropriate if the role of this equation were to explain movements in nominal interest rates of various maturities. In fact, however, the role of this equation in standard monetary policy models is to explain consumption in response to (real) interest rates and expected future consumption (and, in habit specifications, lagged consumption). No mention of the adequacy or inadequacy of the standard model's properties with regard to consumption is provided.[5]

Be that as it may, it is essential to consider the analytical heart of Atkeson and Kehoe's paper, which is their presentation of "a simple model of the pricing kernel that is consistent with these [observed] dynamics" pertaining to interest rates. For the one-period nominal interest rate, i_t in their notation, the pricing kernel m_{t+1} is an unobservable random variable that is generated by a stochastic process such that the interest rate i_t can be

determined by a relation of the form $i_t = -\log E_t \exp(m_{t+1})$. Assuming conditional lognormality, then, we have

$$i_t = -E_t(m_{t+1}) - 0.5\,\mathrm{Var}_t(m_{t+1}). \tag{1}$$

Except for lognormality, the content of their model for i_t is then the specification of the stochastic process generating m_{t+1}. They take it to be

$$-m_{t+1} = \delta + z_{1t} + \sigma_1 \varepsilon_{1t+1} - [1 - (\lambda^2/2)]z_{2t} + z_{2t}^{0.5}\lambda\varepsilon_{2t+1} + \sigma_3\varepsilon_{3t+1}, \tag{2}$$

where ε_{1t}, ε_{2t}, and ε_{3t} are independent, standard normal, white-noise innovations and where

$$z_{1t+1} = z_{1t} + \sigma_1\varepsilon_{1t+1}, \tag{3}$$

$$z_{2t+1} = (1 - \varphi)\theta + \varphi z_{2t} + z_{2t}^{0.5}\sigma_2\varepsilon_{2t+1}. \tag{4}$$

These processes are chosen with an eye to their implications for the term structure via the relation

$$1 = E_t \exp\left(m_{t+1} + p_{t+1}^{k-1}\right), \tag{5}$$

which characterizes an absence of arbitrage possibilities for k-period bonds with prices, p_{t+1}^{k-1}. From these prices the analyst can calculate term structure measures.

Finally, Atkeson and Kehoe calibrate the model by assuming that $\lambda = \sqrt{2}$, $\varphi = 0.99$, and $\sigma_2 = 0.017$. This specification suffices, they report, to generate interest rates of different maturities such that the term structure features long and short rates that possess properties that have the general characteristics found in their exploration of monthly data for rates of various maturities in the U.S. data.

How does this model compare in specification with the standard three-equation framework used in recent years to model one-period interest rates, consumption (and/or output), and inflation by Clarida, Gali, and Gertler (1999), McCallum (2001), Woodford (2003, 238–47), and dozens of other monetary economists? That framework, as is well known, consists of (i) a consumption Euler equation (aka expectational IS relation), (ii) a price adjustment relation (usually of the Calvo variety), and (iii) a monetary policy rule that specifies adjustments of the one-period nominal policy rate i_t to its determinants, which include the steady state real interest rate, the central bank's inflation target, departures of inflation from target, and departures of output from its natural (flexible price) rate. (The lagged rate i_{t-1} is often included as well to represent smoothing.) This framework implicitly adopts the expectations theory of the term structure, which is known to be inconsistent with the data. Notable examples of larger

models that include more variables and equations but that have the same basic underlying logic are provided by Christiano et al. (2005) and Smets and Wouters (2007).

One aspect of the comparison is that the Atkeson-Kehoe model, since it pertains to an "endowment economy," implicitly assumes that price level adjustments are complete within each period so that output is always equal to its (exogenous) natural rate, flexible price value. Only a degenerate version of the Calvo equation component of the standard model is therefore present. That removes one endogenous variable, output/consumption. For some purposes, a flexible price model can be useful for monetary policy principles, as in Woodford (2003, chap. 2). But Atkeson and Kehoe also treat inflation as exogenous. Thus, there is no possibility remaining for conducting monetary policy analysis, and i_t is not determined by central bank behavior. Those features are consistent with their expressed view that the central bank "simply responds to exogenous changes in real risk—specifically, to exogenous changes in the conditional variance of the real pricing kernel—with the aim of maintaining inflation close to a target level." But this seems highly unsatisfactory. It is probably true that a substantial portion of the meeting-to-meeting variations in the federal funds rate in the United States represents adjustments that are responses to changes in real rates that are brought about by changes in tastes, technology, shocks from abroad, and even perhaps some random behavioral errors by private agents. In fact, this is implied by much of the analysis that represents today's mainstream monetary policy analysis—see, for example, Woodford (2003, chaps. 4 and 7). But the modeling approach suggested by Atkeson and Kehoe evidently implies that the Fed somehow accomplishes its (entirely random) objectives perfectly (except for a random error ε_{3t} that is arbitrarily inserted) although it takes no tangible actions in a systematic manner.

Furthermore, no evidence is provided that their model would do a decent job of matching data on inflation, much less output. Without those two variables treated as endogenous and potentially influenced by central bank actions, described by a policy rule for a feasible instrument variable, the model is simply not suitable in principle for monetary analysis.

IV. Conclusion

Atkeson and Kehoe are correct to say that the Euler equation specification in many NK monetary models does not perform well empirically. In addition, their specification of stochastic processes for the z_{1t} and z_{2t} variables that yield a pricing kernel that implies term structure features

that match the data in important ways is skillful and ingenious. They seem correct in suggesting that models in which conditional variances of asset returns are variable provide an attractive possibility for improved model specification. This viewpoint is not new, of course, and does not require treatment of inflation and output as exogenous or require adherence to a model that leads to their highly unorthodox conclusions about the nature of actual monetary policy in the United States (and, presumably, other nations and currency areas). There is a growing branch of the finance-oriented monetary policy literature that includes nontrivial term structure formulations that presume arbitrage-free pricing with time-varying risk premia in models along with endogenous consumption/saving choices, gradual price adjustments, and plausible monetary policy rules. Some leading examples are provided by Gallmeyer, Hollifield, and Zin (2005), Piazzesi and Schneider (2006), Gallmeyer et al. (2007), and Palomino (2007).[6] These have gone far beyond Atkeson and Kehoe in attempting to specify models that match the term structure regularities while maintaining a framework usable for monetary policy analysis. Furthermore, the approach featuring time-varying conditional covariances is not the only one worthy of consideration, as the Collard and Dellas (2007) example illustrates. In sum, I am impressed by aspects of the Atkeson and Kehoe critique of some features of today's New Keynesian monetary policy models, but I find their current proposal to be unsatisfactory in essential ways, their characterization of actual U.S. monetary policy to be unconvincing, and their critique of current monetary policy analysis to be drastically overstated.

Endnotes

1. See "How the World Achieved Consensus on Monetary Policy" (Goodfriend 2007).
2. They would probably grumble, justifiably, about the vagueness of point vii.
3. McCallum (2001, 258) goes on to say: "There is also considerable agreement about the general, broad structure of the macroeconomic model to be used." Atkeson and Kehoe clearly would not share in this agreement.
4. The Canzoneri et al. (2007) paper is cited by Atkeson and Kehoe.
5. Atkeson and Kehoe seem, moreover, to believe (counterfactually) that standard specifications have Euler equations that include no disturbance term reflecting preference shocks.
6. For a brief literature review, see Palomino (2007, 5–6).

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