Comment on "Multicountry Modeling of Financial Markets" by Helliwell, Cockerline, and Lafrance in Financial Sectors in Open Economies: Empirical Analysis and Policy Issues, Board of Governors of the Federal Reserve System, 1990.

Comment

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John Helliwell, Jon Cockerline, and Robert Lafrance have produced an excellent and comprehensive analysis of the properties of five multicountry econometric models:

- the Federal Reserve Board Staff's multicountry model (MCM)
- the Japanese Economic Planning Agency's world model (EPA)
- the Interlink model of the Organisation for Economic Co-operation and Development (OECD)
- the global model of the United Kingdom's National Institute for Economic and Social Research (NIESR)
- the Taylor multicountry model developed at Stanford (Taylor).

The authors have placed the properties of the models in a broader theoretical and empirical context by comparing the simulation results with basic macroeconomic theory and with the properties of other empirical multicountry models, such as the new INTERMOD model fit to annual data by the Working Group in International Macroeconomics in Ottawa and the closely related MULTIMOD model, a rational expectations annual model being developed at the International Monetary Fund.

The structures of the five econometric models differ greatly, and one of the attractive features of the paper is that the authors attempt to explain differences in the simulation properties in terms of the structural differences. Although all the models are empirically fit to quarterly data (except the OECD model, which is semiannual), they differ in the following essential ways:

- the level of aggregation—For example, the OECD model is very disaggregated, the MCM model less so.
- the size of the estimated parameters—For example, the slope of the Japanese *LM* curve is steep in the OECD model but not in the EPA model.
- the treatment of capital mobility—For example, the MCM and the Taylor models assume perfect capital mobility whereas the other models have a portfolio balance structure.
- the treatment of expectations—For example, the Taylor model uses the rational expectations assumption whereas the other models do not.

Before discussing these differences, and to avoid confusion, let me first clarify that the term *rational expectations* as I use it in this comment means

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exactly the same thing as *model-consistent expectations* or simply *consistent expectations* as generally used in the Helliwell, Cockerline, and Lafrance paper. I have a preference for continuing to use the term *rational expectations* in the traditional and widely accepted original meaning of John Muth, and I use it thus throughout this comment.[1]

Aggregation

One of the most interesting findings of the paper is that the degree of disaggregation in the various models makes little difference for the broad patterns of results. This finding suggests that smaller, cheaper, and less complex aggregated models may serve as well as more disaggregated models. In general, I agree with this suggestion, but I have two qualifications. First, the suggestion appears to be based mainly on *annual* summaries of the properties of the models. The quarterly patterns could look quite different for disaggregated models and aggregated models even if the annual results were similar. The timing difference between residential and nonresidential investment, for example, might not show up in annual data.

Second, the similarity between the aggregated and disaggregated models is based only on comparative deterministic simulations. For other uses of the models, such as forecasting or stochastic simulation, disaggregation could indeed make a big difference.

Differences in the *LM* Curves

Much of the discussion of different parameters in the paper addresses the differences in the slopes of the *LM* curves. The authors argue against the popular view that fiscal policy is less powerful in the United States than in Germany and Japan because the *LM* curve is steeper in the United States.

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1. This view is also expressed in my survey paper for the 1986 Brookings conference on multicountry models: "The term 'model-consistent' expectations might capture the idea more vividly, but Muth's term has now achieved such widespread usage that there is little reason to introduce a new one." See John B. Taylor, "The Treatment of Expectations in Large Multicountry Econometric Models," in Ralph Bryant and others (eds.), *Empirical Macroeconomics for Interdependent Economies* (Brookings Institution, 1988), pp. 161–82.

After making similar comments at the Federal Reserve Board's conference in May 1988, I was told by modeling groups who have had considerable experience reporting their rational expectations simulation results to decisionmakers that the term *consistent* is more appealing to decisionmakers than is the term *rational*. For example, John Helliwell wrote to me on June 13, 1988, in explaining his use of terms that "our objective [in using the term *consistent* rather than *rational*] ... is to encourage people to take expectations more seriously, without turning off those on either side of the 'rational' expectations debate." It is hard to disagree with this important practical reason for using a new term for the old rational expectations idea, but I still emphasize that, among technical researchers, the two terms mean exactly the same thing. See also Paul Masson and others, "MULTIMOD: A Multi-Region Econometric Model," IMF Working Paper 88–23 (1988).

The authors make two points: First, the models do not provide much evidence that the slope of the *LM* curve is steeper in the United States; second, there is not much difference among the effects of fiscal policy across countries anyway.

I have several reactions to the authors' arguments. Their arguments should not be interpreted as showing that the slope of the *LM* curve does not matter once other factors are controlled for. When one controls for other factors, one finds evidence from the models that the *LM* curve does make a big difference for the effect of fiscal policy. For example, in the Taylor multicountry model a flat *LM* curve in the United States results in relatively powerful fiscal policy in the United States, whereas a steeper *LM* curve in Germany and Japan results in less powerful fiscal policy in those two countries (see chart 1 and table 2 in the authors' chapter). Textbook theory, of course, says that the slope of the *LM* curve should be a factor in the effectiveness of fiscal policy, and this relation shows up within a given model.

That the Taylor model goes against the popular view is part of the reason for the authors' criticism. I agree with the authors' statement that "the models contain no general evidence that the *LM* curves are steeper in the United States than in Japan and Germany." But I emphasize that this statement is based on the fact that some models have relatively steep *LM* curves in the United States and others have relatively flat *LM* curves in the United States. There is no formal statistical test here. In principle, propositions about differences among estimated parameters should be qualified by measures of statistical significance. Unfortunately, these measures are not available for the more disaggregated models.

Also, because the *LM* curves are dynamic, a comparison based solely on the slopes of the long-run *LM* curve is potentially misleading. As the paper by Brayton and Marquez in this volume indicates, most of the models show a great difference between the long-run and the short-run *LM* slopes. In one case, the slope even flip-flops between positive and negative values. Because of these differences, the long-run slope of the *LM* curve is not an unambiguous measure of the constraint on interest rate and income movements that fiscal policy faces.

Treatment of Capital Mobility

As the authors point out, two of the multicountry models assume perfect capital mobility. In practice, this assumption means that the difference between the interest rates of any two countries is equal to the expected change in the exchange rate between the two countries, that is, ex ante interest rate parity holds. Given that the models are empirically estimated, some qualification of this assumption is in order. A skeptic might question how an empirically estimated model could have equations in which ex ante interest rate parity holds. Much research, and even casual observation,

shows that ex ante interest rate parity, with any reasonable theory of expectations, does not hold. Exchange rate equations based on this assumption, therefore, do not fit very well and have large serially correlated errors.

For deterministic simulations of the type examined by the authors, little can be done about these large errors. The errors might be interpreted as variations in the risk premium, but there is no way to forecast how a change in the money supply or in government spending will affect this risk premium. However, in stochastic simulations of the effect of policy it is possible to shock the interest rate parity equations with serially correlated errors distributed according to the distribution estimated over the sample period. In this limited sense, for stochastic simulations, the models with "perfect capital mobility" (at least, the Taylor multicountry model) need not assume perfect capital mobility.

Treatment of Expectations

Helliwell and his colleagues focus much of their discussion on the differences among the treatments of expectations in the models. Their suggestions for research on expectations are interesting.

The Taylor model is the only one examined in the paper to use the assumption of rational expectations. As the authors point out, this assumption leads to larger fluctuations in asset prices, including exchange rates, in response to changes in the policy instruments. In their conclusion, the authors comment that "given the volatility of asset prices and the dependence of those prices on expectations, an explicit treatment of expectations seems almost inescapable for any fully satisfactory model of financial markets and their international linkages." They feel that "going further will necessitate retaining the explicit forward-looking features of the model-consistent [that is, rational] expectations," but they also feel that the rational expectations assumption will have to be modified because it is "probably... not a realistic way of modeling market expectations."

The authors are not explicit about the modification of rational expectations, which, they argue, is a good area for research. Clearly, more research on workable alternatives to rational expectations would be useful; but my own experience leads me to views about the nature of this future research that differ somewhat from those of the authors.

First, the authors recommend running econometric models in both adaptive and rational modes because "for some purposes [adaptive and rational expectations] provide brackets about the behavior of market participants." I disagree with the rationale for this recommendation and would be cautious about its implementation. One cannot assume that adaptive and rational expectations, taken literally, set bounds for actual expectations. One easy way to see the problem with such an assumption is

to note that in some cases adaptive and rational expectations are equal. According to the argument of upper and lower bounds, this situation should pinpoint actual expectations exactly. More generally, both adaptive expectations and rational expectations could be below or above actual expectations.

My own approach in dealing with the inadequacies of rational expectations in certain applications is to use that assumption as a benchmark and to adjust the expectations from this benchmark as appropriate for a given application. For example, one use of rational expectations that has been made in recent years is in the analysis of the effects of the Gramm–Rudman legislation on interest rates and the economy. The attractiveness of a forward-looking expectations assumption in this application is that the Gramm–Rudman legislation affects expectations of future taxes and government spending. Clearly, it would be incorrect to assume that people think that the Gramm–Rudman targets will be met with perfect certainty, as would be the case in a naive application of rational expectations. Instead, one can assume that people gradually build up credibility about the targets according, perhaps, to the degree to which those targets are met each year. This buildup would be an adjustment of the rational expectations baseline to make the assumption more workable in a given application. In this case, such an adjustment seems to be superior to the assumption of adaptive expectations, which might state simply that the expectation for next year's budget deficit is a fraction of this year's deficit.

Mixing adaptive and rational expectations in the same model, which is implicit in the authors' recommendation and is actually followed in their simulations with the new INTERMOD model, can also be misleading. The expectations-augmented Phillips curve equation illustrates how misleading this approach can be. This equation is typically estimated with adaptive expectations; but the adaptive expectations are a stand-in for slow adjustment of wages (due to contracts, for example) as well as for the slow adjustment of expectations. If such an equation has the natural-rate property in the long run, then simply replacing the adaptive expectations terms with rational expectations terms will result in a vertical *short-run* Phillips curve and will create a model in which monetary policy is completely neutral. In this case, replacing adaptive with rational expectations goes beyond changing expectations. I believe that the same type of example occurs in most equations in an econometric model.

I also emphasize that, because of advances in algorithms and hardware in recent years, there are no longer great computational savings from using adaptive rather than rational expectations for deterministic simulation in models like Taylor, MULTIMOD, or INTERMOD. These models can now be solved on minicomputers or even personal computers. Although the solution times can be one-fiftieth to one-hundredth times as long with rational expectations than with adaptive expectations, the solution times are

now so short that multiplication by a factor of fifty or one hundred still leaves a very short computation time. The cost-minimization features of adaptive expectations that the authors emphasize are just not significant relative to all other factors that affect costs in a large empirical modeling exercise. To be sure, stochastic simulation or full-information maximum-likelihood estimation is still quite costly; but for the practical uses that the authors have in mind, stochastic simulation or FIML estimation is usually not done in any case.

I agree with the authors that research on learning and on diverse expectations should eventually improve on the rational expectations assumption in ways that will be helpful in practice. I am optimistic that the theoretical work on diverse expectations by Robert Townsend and the applications by Jeremy Rudin will lead to practical ways of dealing with the unrealistic "single-valued paths of future variables" assumption, which concerns Helliwell, Cockerline, and Lafrance.[2] I am also optimistic that theoretical research on learning, such as my own and that by Albert Marcet and Thomas Sargent, will offer practical solutions to the problem of modeling the way agents learn about a change in policy regime or economic structure.[3] The time-varying parameters that evolve from such learning schemes suggest that simple, constant-coefficient adaptive expectations will not be a good approximation of the true learning scheme. Recent empirical applications of learning schemes, like that of Karen K. Lewis, are suggestive of what might be done.[4] However, we seem to be a long way from applying these ideas in large multicountry models. Time-varying parameters are difficult to deal with in large, complex models. Hence, for the near future we are probably left with the simple rational expectations assumption as a benchmark and with less formal adjustment to this benchmark as described above.

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2. Robert Townsend, "Forecasting the Forecasts of Others," *Journal of Political Economy*, vol. 91 (August 1983), pp. 546–88; and Jeremy Rudin, "Diverse Expectations: Empirical and Policy Implications" (Ph.D. dissertation, Stanford University, 1986).

3. John B. Taylor, "Monetary Policy during a Transition to Rational Expectations," *Journal of Political Economy*, vol. 83 (October 1975), pp. 1009–21; and Albert Marcet and Thomas Sargent, "Convergence of Least Squares Learning Mechanisms in Self-Referential Linear Stochastic Models," *Journal of Economic Theory*, vol. 48 (August 1989), pp. 337–68.

4. Karen K. Lewis, "Changing Beliefs about Fundamentals and Systematic Rational Forecast Errors: With Evidence from Foreign Exchanges," *American Economic Review*, vol. 79 (September 1989), pp. 621–36.

Conclusion

In my view, the best direction for future research in model comparisons is toward the evaluation of alternative policy rules rather than toward more work on the effects of one-time changes in the policy instruments as in this model-comparison exercise undertaken by Helliwell, Cockerline, and Lafrance. Although these comparisons of one-time changes tell us much that is useful about the properties of different models, I think they are not as helpful to policymakers as is the comparison of different policy rules or operating strategies, which more closely describe what policymakers actually do. It would be interesting to see how the five multicountry models compare in their evaluation of different systems of exchange rates, for example, or in their evaluation of different rules of interest rate reaction for the Federal Reserve, the Bank of Japan, and the Bundesbank. This exercise would most likely involve stochastic simulation under different policy feedback rules. Not only is this type of policy exercise more relevant to current issues, but it is less affected by problems with the rational expectations assumption. Presumably, a policy rule would be in operation for a long time, long enough so that most agents could reasonably be assumed to know about and believe the rule.