Comment on Computable General Equilibrium Models and Monetary Policy Advice,
by Edward C. Prescott

The authors construct an applied general equilibrium model to be used in the monetary policy advice process. Applied general equilibrium models are the tools used to evaluate alternative tax policies and to estimate the consequences of changes in trade policies. However, to date they have not been used in the monetary policy advice process. A question then is why haven’t applied general equilibrium models been used in the monetary policy selection process? Some history is needed in order to answer this question and to explain why the direction taken by the authors is somewhat radical and, I think, promising.

In the sixties and early seventies the basic Keynesian macroeconometric model was established theory. Even the monetarist detractors, when they were explicit, argued about the values of the coefficients of some of the equations. These models with their consumption function, investment equation, money demand function, and Phillips Curve fully integrated the forecasting and policy analysis process. With this framework, given the current position of the economy and the value of the policy variables, the models predicted the position of the economy next period. Thus the Keynesian macroeconometric framework was ideally suited to evaluate the consequences of alternative policy actions. Indeed, a macroeconometric model, along with control theory, could be used to determine the optimal policy action today given an objective function specified by the political process.

Two problems developed with this practice for macroeconometric forecasting and policy evaluation. The first was that attempts at providing theoretical underpinnings for these largely empirically determined macro equations discovered a fundamental inconsistency between the theoretical and macroeconometric tradition in economics. Lucas (1976) in his famous critique of macroeconometric policy evaluation articulated and illustrated this inconsistency and argued that the econometric tradition was in need of major reform. Essentially an implication of theory is that the equations that define the law of motion of the economy are not invariant to the rule by which policy is selected. Questions that the Fed is required by the 1946 Employment Act and 1978 Humphrey-Hawkins Act to answer are just not well posed in the language of economic theory. Indeed, we now know even if a policy rule is best (i)

1. Applied general equilibrium models have been used to evaluate the welfare consequences of different rates of inflation, that is, the welfare consequences of the inflation tax. They have not been used to evaluate monetary policies designed to stabilize the economy.

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given the objective, (ii) given the assumed law of motion for the economy is the one associated with that policy rule, and (iii) given the incorrect assumption that the law of motion of the economy is invariant to the policy rule, that policy rule almost surely is not optimal.

The second problem was that the Keynesian macroeconometric approach failed in the seventies and failed in precisely the way that theory had predicted. Central to these models was a trade-off between inflation and unemployment. In the late sixties, economists confidently predicted that sustained 4 percent unemployment and 4 percent inflation was feasible. If society chose to tolerate higher inflation, a prediction of the macroeconometric models was that unemployment would be lower than 4 percent. In the seventies there were extended periods when the average inflation rate was well above 4 percent while the average unemployment rate was significantly in excess of 4 percent. This is counter to the predictions of the macroeconometric models. This was a spectacular failure which was acknowledged in the Federal Reserve Bank of Boston's 1978 conference After the Phillips Curve: Persistence of High Unemployment.

With the failure of the macroeconometric models, atheoretical methods, in particular the vector autoregression (VAR) methods of Sims (1980), came to prominence in macroeconomic forecasting. These techniques proved invaluable in testing the implications of Lucas' (1972) monetary surprise theory of business cycle fluctuations as well as being useful in making unconditional forecasts, but VAR methods provided virtually no guidance to policy selection.

At that time theory was not much help in providing guidance in the monetary policy advice process. About all that theory said was to follow policy rules that have been used in the past and that resulted in the economy's operating reasonably well. Policymakers do not like to be told such things and legislated that the macroeconometric models with their Phillips Curve be used to do what theory says cannot be done. There is still the Board of Governors' model with a discredited Phillips Curve at its center.

To summarize, with the demise of the Keynesian macroeconometric models, economists were faced with a problem of what to do when it came to macroeconomic forecasting and policy advising. Under the leadership of Sims, the vector autoregression methods moved to prominence. These models forecasted as well as the macroeconometric models, but how to use them for making policy advice, given the Lucas critique, is a problem with no solution.

Great progress has been made in the development and use of applied general equilibrium models in public finance, trade, and business cycle research. Central to these aggregate models is the neoclassical production function, or a multisector generalization of it.2 Not surprisingly this construct is used in the Altig, Carlstrom, and Lansing paper. In a simple parsimonious way the aggregate production function

2. Very recently applied general equilibrium models that begin at the plant level and explicitly carry out the aggregations are being used. Examples are Cooley, Hansen, and Prescott (1995), Fitzgerald (1995) and Veracierto (1995). To address many issues, such as those involving variations in capital utilization rates, the movement of workers between plants, and the welfare consequences of constraints on the length of the work week, requires theorizing at the plant level.
summarizes people's ability to substitute between goods where the key goods are today's and future consumptions and labors. In order to summarize people's willingness to substitute between these goods, the authors use the preference order which through successful use has become the standard one for aggregate analysis. On the real side the model is quite standard.

More problematic is the monetary side of the model. Here the authors use what I see as the currently leading construct for a theory of money, namely, the limited participation structure with firms borrowing from financial intermediaries to finance their wage bill and households facing a cash-in-advance constraint. The reasons I think the theory underlying the monetary side model is not at the same level as the theory underlying the real side are as follows. One reason is the counterfactual implication of the theory that the average real return on government debt will almost equal the average return on capital. In fact, this is not the case. Over the last hundred years the average real return on government debt was close to 5 percent while the average real interest rate on short-term government debt was only 1 percent. It is unfair to criticize a model just because it is false. All abstractions are by definition false and do not match reality on many dimensions. It is far from clear whether or not this particular discrepancy should lessen our confidence in the guidance the authors' model provides in the monetary policy advice process. Given that the authors abstracted from the costs of financial intermediation, the failure to match on this dimension may be a plus rather than a minus.

A second reason is that the model does not match reality in the behavior of the differences in the real return on capital and the federal funds return over time. In fact, five-year moving averages of these differences vary from as low as 3 percent to as high as 7 percent. With the monetary theoretical framework used by the authors it is the surprises that give rise to this difference, and only extremely unlikely events could give rise to such large variations in five-year moving averages. This counterfactual observation diminishes my confidence in the use of this structure for short-term monetary advice. Successful use of the theory, however, would change my views.

There are two sources of uncertainty in the authors' model. One is the shock to the production technology and the other is the shock to the interest rate pegging equation. The shock to the technology of production is standard, the shock to the interest rate pegging equation is not. If the Fed has limited abilities to peg the interest rate, and is only able to peg the one-period-ahead expected value, then this second shock makes sense. It seems to me that pegging of the federal funds rate can be done on an almost weekly basis, and this permits the quarterly rate to be set virtually at the desired level. This implies a negligible shock in the interest rate pegging equation. Another possibility that would lead to following such a rule is that randomness in the interest rate pegging rule is perceived to be optimal. There are cases for which randomness in policy is best.

One problem that the authors discuss is that there may be an infinite number of stationary money growth rules that will support a given interest rate target. In their analysis they restrict attention to the money growth rule that is both stationary and not dependent on "sunspot" variables. I compliment the authors for being open
about this possible multiplicity problem. More likely than not, for their model there are not sunspot equilibria. The reason for this statement is that talented economists have been searching for economies that have multiple equilibria and that are consistent with the data. Benhabib and Farmer (1994), for example, find that there are multiple equilibria for the class of economies that they consider only if the increasing returns to scale are ridiculously large. Perhaps this multiplicity of equilibria will prove to be a big problem, but to date it has been more a theoretical possibility than a practical problem. This is not to advocate not worrying about the possibility of multiple equilibria. Rather it is to advocate going ahead with the applied general equilibrium analysis and worrying about the multiplicity problem if it arises.

In summary I commend the authors for bringing theory into the forecasting and policy advice process. That their extremely simple structure forecasted as well as it did, being as good as the atheoretical vector autoregression methods and the Board of Governors' large-scale macroeconometric model, is surprising. The authors take a first step in developing an applied general equilibrium model that can be used in the monetary policy advice process. However, there are limits to what successful theory can do. These models cannot provide forecasts of what will happen under alternative policy scenarios. They can provide assessment of the current state of the economy and, given the policy rule, better determine that action called for by the rules being followed. For example, the rule may specify different reactions to increases in the real interest rate that are due to shocks to the technology of production and those due to shocks to the technology of exchange. Currently, theory is providing guidance as to which variables to focus on on the real side, namely, hours worked per adult and total factor productivity as well as the consumption and investment shares. Theory provides less guidance on the monetary side. That the authors' simple model forecasted as well as it did is very promising for theory.

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