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Modern Business Cycle Analysis: A Guide to the Prescott-Summers Debate*

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The purpose of this paper is to provide a basic framework useful to understand some recent debates about models of business cycles, particularly, the papers by Prescott and Summers (that follow in this issue). I describe a very general model that has as particular cases most of the current research in this area. Within this framework, I contrast the current research to traditional macroeconomic analysis as well as discuss some problems associated with the modern strategy. Finally, I interpret Summers' discussion of Prescott's paper in terms of the general framework.

The General Framework

Modern economic analysis of aggregate behavior (the type of problems that both business cycle theory and growth theory are concerned with) is methodologically similar to the study of microeconomic phenomena. Specifically, in the modern approach, the restrictions imposed by theory on aggregate behavior must be the result of aggregating the restrictions imposed on individual behavior.

The general framework that this modern analysis uses is some variant of the following. Individual economic agents view themselves as playing a dynamic (possibly stochastic) game. More explicitly, individual agents understand the rules of the game in the sense that they can evaluate the consequences of all the players' actions, including their own. These generic agents each have an objective function and choose their strategy in order to maximize it.[1]

While this framework seems general enough to address any interesting economic problem, it is too general to imply restrictions on the aggregate data. To get such restrictions, more structure must be imposed on the general framework, or model. To see how to do that, consider, for example, the problem of typical consumers/workers in the environment just described. At any point in time, these agents may or may not be employed. If unemployed, they must decide whether to accept any job available (even if the only available offers are for very low-paying jobs) or to search for a better offer. At the same time, they must decide how much to consume of each good and how much to save (possibly, how much to dissave). If employed, the choices are qualitatively not very different. Agents must decide whether to continue the employment relationship or to quit and look for another job, how many hours they are willing to work, and how much they want to consume and save (including how to hold their wealth). Of course, many other important decisions can be ignored—like whether to go to the beach or the mountains for a vacation or whether to have children or not—because they are not crucial to an understanding

*I have benefited from many thoughtful conversations on this and related subjects with Larry Jones. He, Robert Hodrick, and Jim Peck provided useful comments on an earlier version. None of them are responsible for the remaining errors.

[1]This description corresponds to that presented in the excellent monograph by Lucas (1986).

of movements in aggregates like income, output, or consumption. The typical decision problems faced by other agents (firms and even government agencies) can be described in a similar way.

Yet this description of decision problems is missing a crucial element. Individual choices at any point in time are influenced by what agents believe will be their available opportunities in the future. It is almost impossible to think of a well-defined decision problem that does not depend on the expectations that agents have about the environment they will be facing. And changes in expectations about a future event will generally affect current decisions, even if none of the contemporaneous constraints has moved.[2] A complete description of individuals' decision problems, therefore, must explicitly model how agents form their expectations.

Assume that enough is known about each individual problem to analyze it. How, then, is a theory of the behavior of the aggregates obtained? Simple: Add up the decisions of all the players—consumers, firms, and government agencies—and impose a solution that makes all these decisions consistent. Then the framework can be confronted with some questions. Consider, for example, the effect of a change in a government policy. How will the hypothetical economy react to such a change? To answer this type of question, include the new policy as part of the rules of the game and compute a new solution assuming the agents make their best choices given the new rules. This may be a very difficult exercise to analyze, but the mechanics of it are well defined.[3]

With regard to such policy questions, the general framework restricts economists in at least two ways: how they specify and how they evaluate policies.

Because individual decisions depend on expectations, policies analyzed in this framework must be defined in terms of what the government does both in the current period and in future periods.[4] For suppose this is not specified, and questions like this are asked: What is the effect on the gross national product of increasing this week's money supply 10 percent? Trying to follow the procedure above will reveal that more information is needed to answer that type of question. For example, agents will need to learn or guess what will happen to other relevant variables in the future. Will this change be temporary or permanent? Since the government is collecting an inflation tax, will other taxes be changed or will government expenditures be changed? Without answers to these and many other questions, the policy cannot be analyzed; it is not well defined. An analyst must either learn more about it or guess, without much information, how individual agents will answer the questions.

Many traditional macroeconomic policy analyses have asked questions that are not well defined in this sense. The basic principle of modeling economic agents as facing dynamic problems and trying to maximize their objectives limits the class of questions that can be asked. This is not the consequence of any particular model—the very general description above probably includes most interesting models as special cases. It is, rather, a restriction imposed by the requirement that aggregate phenomena be explicitly modeled as the sum of decisions made by maximizing individuals.

This approach to modeling macroeconomic phenomena restricts economists in another way, too. It suggests a natural way to evaluate the consequences of alternative policies: compare the utility levels achieved by each player under the different policies. Such a measure of desirability is a departure from traditional approaches to aggregate analysis, but it is one that can hardly be argued against.

To summarize, the modern way of modeling economic behavior (and not any particular model in this class) forces economists to analyze macroeconomic policies very precisely. Departures from this strategy—for example, some instances of traditional macroeconomic analysis—are hard to justify and very hard to interpret.[5]

Specializing the General Framework

The framework just described is too general to be useful to analyze particular alternative policies; it is too abstract to give predictions about the effects on aggregate variables of some shocks. To be able to give such predictions, the model must be specialized. How is this best done? It does not seem possible to find a single criterion that the whole economics profession will accept. According to the statistical tradition, however, models that generate behavior similar to the actual time

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[2] A good example is the effect on portfolio decisions in 1986 generated by the change in the tax code starting in 1987. Some economic agents decided to realize capital gains in 1986 just because postponing the decision would have implied a higher tax rate.

[3] Analyzing policy changes under rational expectations requires specifying the prior probability agents put on the new policy. One approach is to interpret new policies as regime changes. On this, see Flood and Garber 1983.

[4] Of course, this policy need not be deterministic. It can specify how the government will react in each possible future situation.

[5] The ideas in this section are in no way new. They can be traced to Lucas 1976. See also Lucas and Sargent 1979, 1981 and Miller and Rolnick 1980 for related arguments.

series are probably to be preferred.[6]

Even if that criterion is accepted, though, the process of specialization is highly conflictive. This is so because the basic modeling approach does not discriminate between micro and macro observations: both have to be explained within the model because the same agents generate both the micro and the macro data. But specializing the model is basically giving up on the possibility of explaining some micro phenomena in order to obtain a structure that is tractable. Once specialized, therefore, the model can be attacked on the grounds that it does not explain some micro observation.

As an example, consider the models Prescott surveys (in this issue). Most of them are *representative agent* models. Formally, the models assume a large number of consumers, but they are specialized by assuming also that the consumers are identical. One of the consequences of this specialization is a very sharp prediction about the volume of trade: it is zero. If explaining observations on the volume of trade is considered essential to an analysis, this prediction is enough to dismiss such models. But if accounting for individual fluctuations beyond the component explained by aggregate fluctuations is not considered essential to understand the effects of business cycles,[7] the abstraction is not unreasonable. A case can even be made that if what matters, in terms of utility, is the behavior of aggregate consumption and leisure, then any model that helps explain movements in these two variables is useful in evaluating alternative policies. This usefulness is independent of the ability of the model to explain other observations.[8]

The near future will not likely bring models that can be useful to analyze policy questions and also are consistent with every single piece of available evidence, both micro and macro. Nor will the future likely soon bring a rule based on the structure of the models that will select the pieces of information that must be consistent with them. For now, some other selection criteria are necessary. Today is too early to summarize the relevant alternatives. What will likely happen is what has happened with many other methodological decisions that guide scientific practice: discussion among the scientists will result in some guidelines that will be accepted by most of the profession.

Many current discussions in aggregate economics can be interpreted as debates about the best set of criteria to select useful models within the general class presented above. This is how I will interpret the Prescott-Summers exchange (in this issue).

Real Business Cycle Models

Before discussing that exchange, I will describe some special features of real business cycle models and the different strategies that can be used to confront them with the data.

Special Features

As their name suggests, models of real business cycles abstract away from monetary phenomena. They are only interested in the behavior of real magnitudes (like consumption, employment, investment), and they cannot address questions related to such nominal variables as the price level or to policies like open market operations. If these models are to be useful in the analysis and evaluation of macroeconomic policies, the following form of monetary neutrality must hold: no matter how monetary policy is conducted, the behavior of real quantities is determined by real shocks to the economy.[9]

As a subclass, real business cycle models need not assume anything about the nature of the interaction among agents; in particular, they need not assume that markets are competitive. They also need not assume anything about the nature of the shocks—if any—that affect the system.

The line of research initiated by Kydland and Prescott (1982) is a special case within this subclass.[10] It builds on the growth theory literature[11] and enriches the basic structure so that it can account for fluctuations about a trend. The crucial elements are the assumptions that markets are competitive, all information is public (so markets are complete), and technology shocks drive the economic system. These shocks are interpreted as the production function residuals that Solow (1957) first identified. This special case has other assumptions, like the existence of a representative consumer, that are very important in the sense that the techniques that Kydland and Prescott use to characterize the equilibrium depend on them. But those assumptions are not

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[6]For a description of a criterion for selecting useful models from the class of well-articulated models, see Lucas 1980.

[7]For an argument suggesting how this might be true, see Lucas 1986.

[8]In particular, the usefulness is independent of the ability of the model to account for the behavior of asset prices.

[9]For a critical treatment of models of real business cycles, including their inability to account for monetary phenomena, see McCallum 1986.

[10]This line of research has been extended by others, including Long and Plosser (1983), Kydland (1984), Kydland and Prescott (1984), and Hansen (1985).

[11]The pioneering work is Solow 1956. The analysis of the optimal growth model was initiated by Cass (1965) and Koopmans (1965). Within this framework, stochastic shocks to the production function were first analyzed by Brock and Mirman (1972).

crucial, because relaxing them should not change the results drastically.[12]

The Kydland-Prescott (1982) model and the updating by Prescott (in this issue) are extremely simple. The model has only one good, and investment may take several periods. Some versions of the model—for example, Hansen's (1985) and Rogerson's (1984)—can accommodate unemployment. Long and Plosser (1983) have analyzed a multisector version with a simpler technology.

Data Strategies

The approach of concentrating on the real aspects of business cycles does not point to a preferred way to confront these models with data. Nothing in the approach suggests that standard econometric techniques are not appropriate.[13] However, these are not the techniques that Kydland and Prescott use in their original (1982) paper or that Prescott describes in his update (in this issue).

Prescott's strategy is to use growth and cross-section observations to tie down the parameters of preferences and technology that determine the stochastic behavior of the model.[14] As an important methodological point, he emphasizes that data related to the phenomenon under study should not be used to calibrate the model.[15]

Criticisms of Real Business Cycle Models

Now I will review Summers' four criticisms of real business cycle models (in this issue) from the perspective of the general framework for modern aggregate analysis.

Summers first criticizes the appropriateness of using growth observations to tie down parameters of preferences and technology and the resulting accuracy of Prescott's estimates of some key parameters. Summers suggests that different data sets would have resulted in different parameter values. If that is so, some criterion must be developed to pick the most appropriate data set. This type of critical analysis will probably make aggregate models more consistent with micro evidence.

Summers' second criticism is that technology shocks cannot explain the observed movements of output about a trend. He insists that model builders should be more specific about the nature of these shocks. This is a criticism that does not challenge the ability of real business cycle models to generate behavior that resembles actual time series; instead, it focuses on the sources of the driving shocks. As with the questions about the models' parameters, this type of criticism will probably result in a more careful analysis of the micro evidence, here, for the nature of technology shocks.[16]

The third criticism that Summers makes is that the Kydland and Prescott model generates predictions about the joint behavior of interest rates and asset prices that do not agree with the available evidence. This argument basically says that any acceptable model of business cycles must explain not only the co-movements of quantities like consumption, investment, and employment, but also the movements of asset prices. This argument clearly falls within the previous discussion of how much a model must explain and is therefore difficult to evaluate.

Finally, Summers suggests that economic downturns are associated with "breakdowns in the exchange mechanism." In his view, apparently, these breakdowns are associated with the inability of a flexible-price, market-clearing mechanism to account for some observations. Although the criticism suggests that analysis beyond the subclass of models that Prescott summarizes is necessary, the criticism formally corresponds to bringing new evidence and asking whether the models can generate behavior consistent with it. Answering that question is not easy, in part because an operative definition of the breakdowns is not available. As with Summers' third criticism, this one suggests that useful models of business cycles must explain more than the available models do.

Summers' criticisms, then, fall into two categories. The first two challenge how Prescott uses available evidence to specify the model, without challenging Prescott's overall empirical strategies. The second two question the usefulness of the models Prescott describes, on the grounds that they fail to explain some observations. This criticism cannot apply to real business cycle models as a whole, though, because the models Prescott describes are only a small subset of them.

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[12] With many consumers, the equilibrium will be the Pareto optimal allocation for an economy in which the utility function is a weighted average of the individual utility functions. However, the fact that the representative agent is an average has implications about how some partial microeconomic empirical evidence can be used to restrict the parameters of the utility functions.

[13] Altug (1985) uses traditional maximum likelihood methods in a version of the original Kydland-Prescott (1982) model. See also the analysis of aggregate data in Eichenbaum, Hansen, and Singleton 1984.

[14] This is not strictly accurate, because the arguments Prescott uses to determine the variance of the technology shock use data on cyclical variations.

[15] This strategy does not seem to agree very well with the principle that the model should explain both growth and fluctuations. However, Prescott points out that growth can be introduced into the model in a way that validates his separation of growth and fluctuations.

[16] As Lucas (1986) points out, technology shocks are very similar to the development of new technologies or new goods.

Concluding Comments

The models analyzed by Prescott have two major advantages over other candidate models of business cycles. First, they are firmly grounded in the tradition that aggregate phenomena must be explained as the outcomes of the decisions made by individual agents. Of course, this implies that they are explicit models that can be scrutinized. Disagreeing with Prescott is relatively easy because so is understanding how agents behave in the economies he presents. Second, the analyses of the models surveyed by Prescott show how an extremely simple structure can generate behavior that resembles the actual time series. They go even further and show that movements of macroeconomic variables about a trend can be viewed as optimal responses to exogenous shocks. As Lucas (1986, p. 76) puts it, "Certainly no one would argue that Kydland and Prescott's work has *established* this conclusion, but neither do I believe that it can be firmly rejected on the basis of currently available evidence" [Lucas' italics].

How do the papers by Prescott and Summers help improve the understanding of business cycles? Not only do they make explicit how available evidence can be used to choose among models; they also indicate what observations may not agree with the predictions of the models. A better understanding of real business cycle models is certainly a prerequisite of their use in formulating and evaluating economic policies. More careful examination of the evidence along these lines will also increase the understanding of the potential role of other models, monetary models as well as noncompetitive models, in analyzing business cycles.[17] Critical evaluation of real business cycle models can only improve both the understanding of the phenomena and the ability to choose beneficial policies.

Explicit models like those described by Prescott will no doubt be improved by careful examination of micro data. The next generation of models, though, will probably try to account for more observations. Clearly, as Lucas (1980, p. 697) says, "The more dimensions on which the model mimics the answers actual economies give to simple questions, the more we trust its answers to harder questions." The continuous development of new tools of analysis suggests that someday tractable models will be produced that will also explain the behavior of a richer set of time series. Careful scrutiny of existing models will give useful insights on how to allow development of more elaborate ones. Pioneering analyses, like those summarized by Prescott, and critical evaluations, like the one presented by Summers, will help keep economists pointed in the right direction.

[17] For an argument that suggests that there is some evidence of monetary effects in business cycles, see Lucas 1986 and McCallum 1986.

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