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MD DIALOGUE

REAL BUSINESS CYCLES AFTER THREE DECADES

A Panel Discussion with Edward Prescott, Finn Kydland, Charles Plosser, John Long, Thomas Cooley, and Gary Hansen

Moderated by Sumru Altug
Koç University
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January 4, 2013

The transcript of a panel discussion marking three decades of the real business cycle approach to macroeconomic analysis as manifested in Kydland and Prescott's "Time to Build" (*Econometrica*, 1982) and Long and Plosser's "Real Business Cycles" (*Journal of Political Economy*, 1983). The panel consists of Edward Prescott, Finn Kydland, Charles Plosser, John Long, Thomas Cooley, and Gary Hansen. The discussion is moderated by Sumru Altug and Warren Young. The panel touches on a wide variety of issues related to real business cycle models, including their history and methodology, starting with the work of Prescott and Kydland at Carnegie Tech and Plosser and Long at Rochester; their applications to policy; and their role in the recent financial crisis and likely future.

The panel discussion was held in a session sponsored by the History of Economics Society at the Allied Social Sciences Association (ASSA) meetings in the Randle A Room of the Manchester Grand Hyatt Hotel in San Diego, California.

Keywords: Real Business Cycles, Aggregate Dynamic Economics, Quantitative Methods, History and Methodology of RBC Models, Kydland–Prescott Contributions, Long–Plosser Contributions

Warren Young: I would like to welcome you to this session celebrating three decades of real business cycle approaches. In a recent paper, Bénassy (2007, 189) observed that

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> if one looks at the history of macroeconomics, one sees that two paradigms have dominated the profession. First, until the early seventies, the IS-LM model (Keynes 1936, Hicks 1937), then the dynamic stochastic general equilibrium (DSGE) paradigm (Lucas, 1972, Kydland and Prescott, 1982), where "the early DSGE models, then called 'real business cycles' (Long and Plosser, 1983), assumed full market clearing."

In January 2012, when we asked him to participate in today's session, Ed Prescott wrote us and said,

> There are multiple definitions of RBC. Long–Plosser introduced the term in their JPE paper, which had a fully articulated artificial real economy. . . . Finn and I did not use the term until later. We and others found that real factors were the principal factors contributing to business cycles fluctuation. Then we accepted the term. I like micro based dynamic stochastic general equilibrium models of aggregate phenomena. RBC has become a methodology that is used to draw scientific inference about monetary and nominal interest rate policy rules. The so called New Keynesian models use RBC to evaluate monetary policy in worlds with Friedman's sticky wages.

Kydland and Prescott's paper was published in *Econometrica* in November 1982; Long and Plosser's in the *Journal of Political Economy* in February 1983. The short time gap in publication is inconsequential. This is because both were *widely* circulated as working papers and presented at seminars prior to publication. For example, Long–Plosser was presented at Carnegie Mellon and MIT, Kydland–Prescott at "several universities," with working paper versions from 1980 and 1981 cross-referenced in *both* published papers. Moreover, cross fertilization was also a feature of their development, as manifest in the correspondence between the authors of the respective papers. Both approaches can be, and were, augmented to deal with cycles brought on by monetary shocks; this extension was suggested in the early stages of their development.

Today—in the spirit of Lekachman's "reports of three decades" of the Keynesian paradigm (1964)—we have the privilege of meeting the architects of the RBC paradigm—Edward Prescott, Finn Kydland, Charles Plosser, and John Long—three decades after its inception: to hear an account of its history, an assessment of its present state, and their prognosis for its future. We are also fortunate to have as participants Gary Hansen and Thomas Cooley, who extended Kydland–Prescott, and our senior co-organizer, Sumru Altug, who was an early commentator on how to supplement it [Altug (1985, 1989)].

In order to facilitate the round table, questions were put to participants in advance, relating to the past, present, and future of the RBC paradigm. Their answers form the basis for today's session. The first set of questions form the basis for the panel discussions; a second set of questions was asked after the panel discussion. The first set of questions is as follows:

(1) How did *institutional setting* and approach influence the development of your models, i.e., *cross fertilization* manifest in the Carnegie–Rochester nexus, and the influence of the GSIA eclectic approach and other inputs?

(2) To what extent did the *intra-* and *inter*generational aspects of your approaches contribute to their success?

(3) At the time, did you ascertain the *transformative power* of your models and methodology?
(4) The K–P model is an aggregate, the L–P model a multisector model. What, in your view, are the reasons for success of *both*?

In the following, we present an edited version of the presentations and remarks of the roundtable's participants. Material in curly brackets records interventions and audience reactions in the course of the discussion; the material in square brackets is subsequent editorial interpolations.

Edward Prescott: [Accompanying Prescott's remarks was a presentation he titled "RBC and the Revolution in Macroeconomics." He opened by asserting that the revolution had resulted in "aggregate dynamic economics" becoming "a hard science . . . tested through successful use. The theory predicts well given the behavior of productivity and the policies" and "its predictions are useful in policy regime selection." Prescott first turned to the "revolution in methodology" which "is called real business cycle theory," and said the following:]

I want to talk about methodology. This methodology has been used by many researchers, and I call it the RBC methodology. We have learned a lot in the process. [In] that famous article by Long and Plosser, they seemed to argue that fluctuations that we see in output and employment are due to real factors. Research over the last, I guess now nearly 30 years, has proved they were right. In the 1970s, equilibrium business cycle models were recursive linear–quadratic economies. In "Time to Build," Finn and I had a linear–quadratic model economy. Sargent (1978) had one as well. If you look at his model carefully, it is the real factors that are important. Even the empiricists, Sargent and Sims (1977), found that the real factors are what are most important for output and employment. They're the best in using empirical methods in macroeconomics.

The key to making macroeconomics a hard science is the neoclassical growth model. Before the 1980s, growth theory was not part of macroeconomics—Miller and Upton (1974), of course, were the exceptions. I guess there's also something they called microeconomics. Now there is just economics. So, basically, we integrated fluctuations with growth.

What is macroeconomics? I think it is largely defined by [the] national income accounts of Kuznets. I use them the way he laid them out, though some people improved on the income side—Henry Simons. You basically measure output and inputs and report them, and use prices to aggregate. That's what the national accountants do. These accounts are consistent with capital theory where you have a special recursive technology, and you can talk about income. It is hard to talk about income in Arrow–Debreu, as it is not part of that language.

What is macroeconomics? I've said that already. What are business cycles? Lucas brought the term back into economics in the late 1960s. The term had been largely dropped in economics in the postwar period. Business cycles are fluctuations in per capita output relative to trend largely accounted for by changes in the fraction of productive time allocated to the market.

A different language was used in dissertation [Prescott (1967)] when I used those macroeconometric models, and where macroeconomists empirically determined the laws of motion of the economy. With dynamic economic theory the law of motion is an endogenous element and not a data.

One of the key findings is that the real factors are the important factors giving rise to fluctuations in output and employment. Of particular importance are persistent changes in productivity. These changes are in part the result of variations in the growth of the stock of knowledge, but changes in the regulatory and legal environment are of greater importance. Other real factors that have been found to be important are changes in the tax system, terms of trade, and demographics.

What is theory? It's a set of instructions for constructing a model to be used to answer a given question—this is Lucas' definition. You have a question? Use theory and observation, both micro and macro, to construct a model, and find an answer to that question for the fully articulated model economy. That's how we build economic intuition. Sometimes theory gives a precise answer to the question. Sometimes theory does not, and better measurement is needed to restrict the model economy before it can provide a precise answer.

Models are instruments to draw scientific inference. I don't see them as a product and treat them as an intermediate good that we use. As I said, the core is the neoclassical growth theory; Solow is a key person there. His aggregate production function is a theory of the income side of the national income accounts. Factors are paid their marginal products and firms can freely enter and exit—these are the microfoundations of his aggregate production function.

I call Solow model the *classical* theory growth model. Solow did not have the household making any decisions The two important household decisions are the allocation of productive time between market and nonmarket activities and the allocation of output between consumption and investment.

Kydland and I in the summer of 1979 added an aggregate household to the growth model. Before that we started with a linear–quadratic economy and couldn't make the connection with the micro observations. We used something called [a] "representative household" in that model, and implicitly we were assuming common homothetic preferences, where aggregation holds and the representative household and the individuals are the same.

One of the problems with the representative agent assumption is that it is inconsistent with micro-observation. It predicts that everybody will adjust the same percent of hours worked in a given week. That's not the case. Most of the margin of the adjustment is in the fraction working. I think Cho and Cooley (1994) came up with over 80%. Needless to say, the inconsistency of the micro observations with the macro bothered Finn and me. I really loved it when [Richard] Rogerson (1988), in a static context, got an aggregation theory when there is labor market indivisibility. Individuals couldn't substitute, but in the aggregate, the elasticity of substitution was big and the margin of adjustment was the right one, empirically. Things better match up [with] observation. Hansen (1985) was fast off the block when he saw the Rogerson aggregation theory with labor indivisibilities

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and introduced it into the neoclassical growth model. Charlie Plosser and his co-editor recognized the importance of the Hansen contribution. The co-editor was Bob King. By the way, Ljundquist and Sargent (2000) went a bit further and made the connection with the "permanent income" literature. You can support the equilibrium allocation without lotteries if you have borrowing and lending, and things take place over time. With this arrangement, theory does not say who works when but it does predict the total amount [of time] people work over their lifetime and what fraction of [the] workforce works at each point in time.

Finn and I wondered why the economy is behaving approximately as if there were a labor indivisibility and the length of the workweek could not be varied. Hornstein and I came up with a story and modeled it rigorously. The abstraction has capital being allocated across workers or groups of workers. Having a worker using the bulldozer and not everybody using shovels—it's more efficient and it is the equilibrium outcome.

However, not all variation in hours is in the extensive margin. So Finn Kydland and I (1991) used this framework and added very small costs of moving between the market and household sectors. This got the observed split between the hours margin—hours per worker—and the fraction of people working.

One thing economists found using this methodology is that monetary things didn't matter. If the modeler abstracts from them, and treats productivity and tax rates as exogenous, for the United States, at least, observations are in remarkable conformity with the predictions of the growth model.

It is easy to bring a transaction demand into the growth model. Cooley and Hansen (1995) did that. They found [that] monetary policy with this transaction demand for money had very small consequences for real output and employment. Introducing staggered wage contracting within this RBC [framework] has been carried out in the Chari et al. (2000) *Econometrica* paper. Their aggregate models are restricted by micro observations. They found [that] sticky wages and prices did not result in fluctuations of the nature observed.

I would like to mention another spectacular success. We now know about the Great Depression due to Cole and Ohanian (1999), those Rochester graduates, who did so much to advance macroeconomics. They introduced cartelization policies, which resulted in insiders and outsiders and depressed employment. These policies account for a sizable fraction of the depression in employment and output in the 1934–1939 period in the United States.

The latest statistics today, the civilian employment rate, went down again. It's probably just noise. So I call the current depression the "not so great one." But the great one was about the dynamic coalitions that they brought in, cartelization and taxes turned out be important. Hoover was a stimulus man, and spend and tax.

Japan lost a decade of growth while Western Europe and United States did well in the period from 1992 to 2002. The stagnation of productivity was the reason for the lost decade of growth, just as productivity was responsible for the Japanese growth miracle.

The theory is not useful just in understanding business cycle fluctuation. The theory also has had [an] impact upon finance. Merton Miller would have loved that, because he was the dynamic general equilibrium man in the finance field. He had [a] major influence on the development of macroeconomics. Other successes include understanding why employment in Europe is depressed 40% relative to the United States. The answer is primarily high marginal tax rates. Why is Japan depressed 40% relative to the United States? Their productivity per hour is low relative to Western Europe and the United States.

To conclude: So much has been learned by so many using RBC methodology. So much is being learned. So much remains to be learned. Aggregate economists are not out of business. There are a wealth of important open questions that can be addressed. This is the golden age of aggregate dynamic economics. In another generation policy makers may take our scientific findings seriously. RBC methodology is a powerful scientific tool. And I don't see any competing discipline.

Finn Kydland: I'd like to start by announcing that at 9:42 last night, my second grandchild was born {applause}. So, in the correspondence leading up this meeting, Warren had suggested that we spend ten minutes each to present three things: (i) our own personal story and role in the development of RBC, (ii) what we think were the key ideas involved, and (iii) how they evolved. I predicted that he [Prescott] would do a tremendous job on item (ii), talking about the ideas involved. So that leaves me with talking more about (i) and (iii). And now, given the historic orientation of this session, sponsored by the History of Economics Society, I thought it could be interesting to go a back a little further and see how this all developed.

In fact, in the spirit of that, I would like to go back to the spring of 1970. I had arrived in the autumn of 1969 [at Carnegie Mellon] as a Ph.D. student. And with a good math background, I figured I could take courses that were designed for second-year students, general equilibrium theory from Ledyard in the autumn. But the important thing, and we'll see its relevance, was that I took a course—the fourth in the sequence of Advanced Economic Analysis, subtitled Growth and Fluctuations—given by Bob Lucas. Now, that course on Growth and Fluctuations was like no course, I think, ever seen in any university in the 1970s. Bob starts off with a month of constrained optimization math, of constrained optimization involving utility functions, constrained utility maximization—about a month of that, then he gets into overlapping generations models, functional equations, etc. with well-chosen examples. I especially remember, I still have the notes I took during the course, and one thing I remembered, and I went back to check, was that on March 18, he started out presenting a new example, and went on for an hour and twenty minutes, and the class was done. And then about two weeks later—I guess spring break intervened—he came back, and said, "scratch everything I did last time, it did not work out." And what he got going on next time was—or what turned into, or the gist of—his "Expectations and Neutrality of Money" paper [Lucas (1972)]. Just a tremendous experience to see a great mind at work. The point of this is that, that was the kind of macroeconomics seen at Carnegie

Mellon in those days. We did not learn any system of equations stuff, we didn't know about IS-LM analysis, so it was quite comfortable—it's always been quite comfortable—for us to stay in the framework of beginning with preferences, technology, information sets. For me, it was reinforced by Dave Cass, who had arrived at the beginning of my third year, the same year Prescott arrived. I sat in on his course on Growth Theory. Ed has already mentioned the textbook by Miller and Upton. They were at Carnegie Mellon, then called Carnegie Tech—Miller as a faculty member, Charles, a Ph.D. student [supervised by Kamien]—and [they produced] just a great book of the kind of macroeconomics a great economist, or great economists, would write. The last time I taught intermediate macro to undergraduates, I used six chapters from that book, and it was only about five years ago.

Now, Lucas, I like to emphasize Lucas, because he was a forerunner in the kind of theory we use, he was also instrumental and he was influential for Ed and me when we got started on the "Rules vs. Discretion" project. He had written his econometric policy evaluation critique; we used his investment-tax credit example as our main example. He criticized the way policy was analyzed in econometric methods in those days. Of course we picked up an analogous problem for optimal control theory—the time-inconsistency issue—which, by the way, is one of the questions we have been given in advance as possibly to talk about.

I recall a multiple-page article, I believe it was in 1972, extolling the power of this new tool—optimal control theory—to solve the problem of policymaking. Annual conferences were being held on stochastic dynamics and control. At the one in Cambridge, MA, in May 1975, I was on the program with a paper based on one of my thesis chapters. Present were the big gurus in the field—Gregory Chow, David Kendrick, Stanley Fischer, and others. Early in the conference, Chow announced that there would be a session on work in progress. Ed and I at that point had everything pretty much worked out, although not yet a complete written version. So I signed up for that session and got to go first. Admittedly, our tentative title was somewhat inflammatory: "On the Inapplicability of Optimal Control to Policymaking." I barely got past the title and all hell broke loose. People kept searching for where the error had to be. Of course I knew from my thesis work on dominant-player games that time inconsistency could be quite a pervasive problem, so I insisted on my line, although I doubt that anyone in the audience believed me!

That project was pretty much done in 1975. We finished our draft of the "Rules vs. Discretion" paper. And we talked about a business cycle model. Ed had written out some notes, an outline of a possible model.

But we didn't really get going until I was invited to come to see him at Carnegie Mellon as a visiting scholar in the academic year 1977–1978. Toward the end of that academic year, it became evident that they were thinking of giving me a job or hiring me permanently. So in April 1978, I was to give what I regarded as my job market talk. I worked furiously to come up with some written notes (co-authored with Ed) that I could distribute in advance, and the result was a 21-page draft entitled "Persistence of Unemployment in Equilibrium." Even though there was no variable called unemployment in that paper, there was fluctuating labor input, and that paper—Warren has seen it—I still have the paper, is dated April 1978. Among the things in there, it has technology shocks as a key driver and it also talks about the role of the steady state as a point about which to approximate in the actual computations, and its usefulness in quantifying the parameters of the model. But, this was still at a rudimentary stage, and at the same time, one problem was that Ed Prescott left for Chicago in 1978 and went to Northwestern in 1979; thus it wasn't so easy to work jointly in those days as it is these days.

One interesting thing that happened around that time, and at Carnegie Mellon, which we knew about was that Hodrick and Prescott were doing this work on decomposing fluctuating series into slow-moving and fast-moving components, and you can't believe how revealing that was. One of my favorite blues guys, Watermelon Slim, has this tune where he goes, "I don't wear no sunglasses, I want to see what's going on." Well, looking at the H–P filtered output was like taking off the sunglasses. Investment fluctuating about three times as much as output, nondurable consumption about half as much, productivity procyclical, measured labor input almost as volatile as output, so it was a good question—what else other than shocks to the nation's production possibility set could account for such a big chunk? And so what we set out to do was estimate how large that chunk was. Now, we did have computational tools to solve such a model, as we had to work out how to calculate dynamic equilibria as part of the "Rules vs. Discretion" paper [Kydland and Prescott (1977)]. And we had some of the subroutines that we had used for years. In the "Rules vs. Discretion" paper, we had to worry about [the] so-called big K, little k problem—the externality arising from the presence of a policy rule. So from the computational standpoint, the business cycle model was easier. The challenges were more along the lines of refining the model selection, which Ed talked about just now, issues about the quadratic approximation, how to quantify the model parameters, later called calibration, and how to compare the model with the data. So, by around, by early 1980 we had a draft we were happy about, to take on the road and eventually send to *Econometrica*. I suppose the main thing some people were critical of was the lower labor-input volatility in the model compared with the data. Now, we had only one margin, the hours per worker margin, which accounts for maybe a quarter to a third of measured labor-input volatility. Here's where Gary Hansen's paper (1985) came in so importantly: he focused on the other margin, the employment margin, and of course, that can account for much more of the labor-input volatility.

Let me just finish by commenting on expressions, or names we give to things. I mentioned calibration, and Warren wanted to know if we were influenced by Shoven and Walley and I must admit, I didn't know their work, and when we started doing calibration, we didn't know there was a name for it. I believe it was John Taylor who suggested we call it "calibration." And he did so in time for the finished version of the "Time to Build" paper.

A couple of years ago there was a similar event to this one on rational expectations. For some reason, I don't think I ever used that term, and I think this is all due to Bob Lucas. So Lucas is, maybe generally, regarded as the father of rational expectations theory [in macroeconomics]. I scanned through the notes [I took in his course]. He never used that term. When you do equilibrium theory at the level he did, we all ended up doing, well, you have an equilibrium and that's it. There is no need to talk about rational expectations. My conjecture is that that's a term that was much more needed in the context of what we call the "system of equations" approach.

I am never enamored with the term "real business cycle theory" either; I prefer the term "quantitative aggregate economics". That's what I've always used as the title of my courses. Lucas, sort of, Lucas is a very perceptive guy, and I always listen to what he says. He argued forcefully that what, the lasting importance—I am a Norwegian, so it's hard for me to talk about my stuff in this way, but what the hell—Lucas said in his very nice tribute after the Nobel [in 2004] and he made clear the lasting importance, *is the methodology, the whole package.* The nature of the models, the calibration part of it, how to come up with answers to the questions and so on. How to compare with the data. And I agree with that. More so than just the fact that the first example we used was to see what fraction of the business cycle was accounted for by technology shocks. I hardly ever used the term "real business cycles." I do have a paper with that term in the title. The first paper Backus, Kehoe, and I wrote a couple of decades ago was a paper on international questions. The title of that paper is "International Real Business Cycles" (1992), even though I don't think the term is used in the body of the paper. It ended up with that title at the insistence of the marketing expert amongst the three of us, Patrick Kehoe. Thank you.

Charles Plosser: Ed covered a lot about the history of real business cycles and the thought processes involved and how they have evolved. So I thought I would give a little more personal perspective, and talk a bit about how and why I came to consider real models of the business cycle as interesting. Not surprisingly, for me, the roots can be traced to Bob Lucas, who was on my thesis committee at Chicago in the early to mid [19]70s. Also, near the end of my career at Chicago—I finished my Ph.D. (1976)—I was very enamored with Merton Miller and Charles Upton's new macro textbook, which I ended up using when I taught macroeconomics at Stanford. I found the book very enlightening and very helpful. So, all those things had an impact on the way I thought about macroeconomics in the late [19]70s, and even subsequently.

There was another dimension to my thinking too, that was an empirical dimension. As most of you probably know, during much of the 1970s, there was a lot of focus on monetary theories of the business cycles—a huge industry had developed that stressed the importance of unanticipated money shocks as the key source of business cycle fluctuations. I didn't find that work terribly compelling. And so I was beginning to question whether such theories were the right way of approaching macroeconomic fluctuations. Single shock models, single sector models, seemed really suspicious to me and incomplete at best.. Lucas' work and discussions of business cycle brought back to mind the seminal work [of]

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Burns and Mitchell (1946)—where business cycles were defined as comovements among lots of variables. For the most part, macroeconomics ignores [the fact] that one-sector models, by their nature, just assume the sort of comovement that Burns and Mitchell identified as the heart of what defines a business cycle. Once you simplify to a one-sector model you are inevitably led to consider aggregate shock theories of business cycle[s]—just a money shock.

If you go back to Burns and Mitchell, there was a lot more complexity in the business cycle—trying to understand the dynamics about why certain sectors move together, when they did, and when they didn't. In most macroeconomics, we brushed a lot of that under the rug. So I was more interested in trying to understand more about how sectors interacted, how business cycles evolved, and what caused them. While we had come to think all business cycles looked alike, and there was some truth to that, I began to think that view may have some limitations. To understand how and why they may be different, we needed to understand the dynamics and how the economy evolves.

The other dimension was the empirical dimension. I got to know Charles Nelson at Chicago. I think he and I left about the same year, he went to Washington and I moved to Stanford. We had actually started some work together about that time. I had done some work with Arnold Zellner, on Bayesian estimation of unit roots, stochastic trends, and random walks. Charles and I had this ongoing discussion for several years about what are the implications of unit roots and random walks for economics. Charles had done some earlier work on GNP, showing that GDP and GNP had [a] random walk component [Beveridge and Nelson (1981)]. We spent a lot of time trying to figure out what were the econometric issues associated with unit roots.

We searched and searched and searched, until we discovered Dickey and Fuller, who had been working on this inference problem. The one important implication that Charles [Nelson] and I thought about was whether or not there were stochastic trends in the economy more broadly, and if so how do you decompose permanent movements in a variable from transitory movements? So, when we did our empirical work, we came to the conclusion that GDP and lots of other variables appeared to contain stochastic trends; that is, we couldn't reject the hypothesis that there were pretty significant permanent movements in many series.

The punch line of our trends and random walks paper that came out in 1982 [Nelson and Plosser (1982)] was that when you approach your data like that, you find that well over 50% [of] the fluctuations in GDP were permanent. That didn't mean every shock was permanent, but a large fraction of the variance was accounted for by these permanent shocks. Of course, being trained at Chicago, I said well, if the shocks were permanent, [they] better not be monetary. Because if they were monetary, then I would have to throw out everything about the neutrality of money that I thought I knew. So I was motivated to think [of] alternatives. Put differently, if a lot of the fluctuations that we identified were actually the result of permanent shocks, then we better look somewhere other than money as [the] primary source of fluctuations.

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And, of course, that led me to real factors—productivity shocks, technology shocks, changes in taxes, other real factors—rather than searching high and low for the channels in which monetary impulses explained real fluctuations. So those three trains of thought related to Lucas—rational expectations, theories of the business cycle, neoclassical growth models—and the empirical background led me to think hard about alternatives.

There was one other dimension to this that I found striking. The more I talked to people in the late [19]70s about the theory of business cycles, the more apparent it became to me that for the most part they were mostly theories about market failures. I asked myself, well, how do we know there are market failures or how much they might contribute to fluctuations, unless we understand what a fully dynamic general equilibrium model might look without such failures? But we didn't have a good specification, or benchmark, of such a stochastic, multisector, dynamic market-clearing model.

So one of the ways I thought about what John Long and I were working on was, "OK. We will forget about market failures, forget about money, forget about many of the perceived essential elements of the current crop of business cycle models, and attempt to establish a better benchmark that illustrates the potential dynamic properties of a 'perfectly' functioning dynamic market model." So I have always viewed real business cycle modeling, and the focus of our effort, as kind of laying out the benchmark. Trying to lay out the baseline of what a general equilibrium model, and its dynamics, could look like, and then, and only then, can you start making statements about the importance of various market failures, or market structures, or institutional arrangements, that might contribute the business cycles we in fact observe.

I am pleased that subsequent to the early work we did, more and more research has been done using these models as the starting point, but exploring various extensions and variations to try to improve their ability to match the data. I think we have made a lot of progress over the years, we discovered a lot of things and we pursued some dead ends. But such is the nature of research.

One of the things that Bob King and I did later in the 1980s was to explore the types of business cycle features the baseline or benchmark model might generate, More specifically, Bob and I were interested in asking, "OK. Will these models generate things that look like Burns and Mitchell business cycles?" Burns and Mitchell had spent years looking at data and identifying business cycle turning points—peaks and troughs. They then explored how the various data series behave across business cycles or relative to the peaks and troughs. They produced what we called business cycle plots. And today they are quite common. So with the help of Victor Zarnowitz and Anna Schwartz, we discovered in the depths of the NBER [Archives] a deck of cards. This deck of cards was a computerized method of constructing the Burns and Mitchell turning points of the business cycles. It was all coded. The program was probably written in the 1960s, I suspect. We used the program to reconstruct the Burns and Mitchell methodology. We ran historical series through the program to see if it generated what Burns and Mitchell generated. I think it did a pretty good job, actually. We then asked ourselves, "Do the artificial data that are generated by these real business models look like what Burns and Mitchell might have found?" And surprisingly, they did. I remember giving a seminar on something like this at MIT, [where] Franco Modigliani came up to me and said, "I don't believe it. It is magic." I said here is the code, here is the model, see for yourself. I didn't think I was a magician. But it was pretty straightforward and a fascinating exercise. It suggested that if Burns and Mitchell had analyzed these artificial data they might well have identified business cycles of the very sort they found in the real data they had used.

So I think I will just make one more footnote before I stop on the history. You go back to the work that Charles Nelson and I did and much of what Charles has done throughout his career and it suggests the importance of permanent shocks and the challenge of how to interpret them. In that light, I have looked at the data on the most recent recession. It certainly appears like we have experienced a permanent shock. The problem is we probably won't reach a consensus on this interpretation for many years. What will happen, and is already happening, is that over time various studies conducted by economists and organizations [such as] the Congressional Budget Office will gradually reassess measures of things [such as] potential GDP and they will determine that the level and path of potential GDP as was perceived in 2007 will get revised down, so that by 2016, say, the so-called output gap will be much smaller that we thought it was in real time. Indeed, this is already happening. So I think there is a lot of work left to be done. I think there is still a lot of focus on monetary theories, particularly in the context of the popular New Keynesian models. But I would like to see us return to more multisector models, with more emphasis on the real factors at work. Thank you.

John Long: First I'd like to thank our organizers, Sumru Altug and Warren Young, and also to thank them especially for inviting me, too, for addressing your questions. You see, I am not a macroeconomist.

My work before my collaboration with Charles Plosser, and since, has been primarily in asset pricing theory. At the time of our collaboration, however, I found our joint work to be quite compatible with that. The background I brought to the work was from financial economics—in which, starting in the early 1960s, the consumer side, that is, models of consumption, saving, and portfolio choices in multiperiod stochastic settings, started to be developed and by the early 1970s were quite well developed.. My own exposure to this development is reflected in my Carnegie dissertation (1971), which was a representative agent equilibrium model of asset pricing in a multiperiod stochastic endowment economy. In the 1970s there were representative consumer models of general asset pricing and the term structure of interest rates that included production in the form of multiple processes for producing a single good. A good example is the pair of Cox–Ingersoll–Ross papers (1985a, 1985b) that were ultimately published in *Econometrica* in the [19]80s but that circulated in various forms in the 10 years before that. Brock (1979, 1982) also had a dynamic stochastic equilibrium model of asset pricing with multiple production processes in a one-sector economy. For my own part, at

Rochester in the 1970s I was teaching, and still do, production, distribution, and equilibrium theory in our Ph.D. price theory sequence. So I was also familiar with modeling [of] capitalistic production that employs a variety of distinct produced inputs. What was novel to me when Charles and I authored our model was the focus of the model—joint business cycle quantity fluctuations rather than asset price behavior. In fact, we devoted very little attention, per se, to asset pricing, almost none.

I'd like to reiterate a couple of points that Charles made.

First, we believe that it is natural and productive to employ a multisector business cycle model. This allows the model to address multisector phenomena [such as] comovement of outputs across diverse sectors. Recalling Burns and Mitchell (1946) and other subsequent evidence, Lucas (1977) listed comovements as the primary empirical feature of business cycles. For aggregate macroeconomics, reliable comovement of variables from different sectors gives aggregate indices of these variables more information content than they would otherwise have. Rather than relying only on evidence of comovement or an implicit assumption of comovement to justify aggregation, a multisector equilibrium model offers a view of the economic forces that may endogenously generate the comovement.

The second point I want to reiterate is the intent of the Long–Plosser model. As Charles said and as we emphasized repeatedly in our article, our model was designed as a basic theoretical benchmark—an answer to the question[s], "If you make conventional, middle-of-the-road assumptions about consumer preferences and production possibilities in a multisector stochastic dynamic general equilibrium model with rational expectations and frictionless market clearing, what kind of joint dynamic behavior should you see in the endogenous variables (real quantities and relative prices) of the model economy?" "Is there comovement between among variables from different sectors?" "Why or why not?" "Does the effect of a shock in one sector persist over time and 'spread' to other sectors?" "Why or why not?" Answers to these questions may be inconsistent in significant ways with empirical observations. If so, however, a benchmark model [such as] ours allows one to better assess the incremental explanatory power of features we deliberately omitted from our model: things [such as] taxes, government spending, money and nominal price level uncertainty, and market frictions and failures.

In closing, let me say that I am very pleased to see that in the years since the Kydland–Prescott and Long–Plosser articles there has been so much more advanced work on this topic. I am just pleased to have been there at the beginning. Thank you.

Gary Hansen: I want to begin by thanking Warren and Sumru for inviting me to be a part of a group where I get to be the youngest member. That doesn't happen very often anymore so I value this. I became a contributor to this research program when I was a graduate student at the University of Minnesota in the early 1980s. This was a pretty magical time at Minnesota, a period that has been celebrated quite a bit in recent years—four faculty that were actively teaching then later won Nobel prizes, one of them sitting here to my right. Lots of exciting research agendas were being pursued and one of those was the real business cycles research program. Also, I met Charles Plosser, Bob King, and Finn Kydland, who all visited Minnesota while I was a graduate student, and so I became familiar with their work. In addition, Sumru Altug was on the faculty at the time. Later, when I went to UC Santa Barbara in 1985, I started working with Tom Cooley, who was on the faculty there.

Thinking back about my days as a graduate student, it seemed surprising to me at first that I could recall only one other dissertation on real business cycles being written at the time I was writing mine. Of course, many of the people who were graduate students with me later went on to write papers on real business cycles or using the quantitative DSGE approach more generally. But, in retrospect, one might expect that everybody would be running to do this stuff.

Of course, at the time, the entry costs associated with this type of research were extremely high. There were no textbooks that covered these methods, and the approach was quite controversial. People were nervous about getting jobs. Bob Lucas has described the work that Ed and Finn did as "economics without a net," meaning that they weren't using an established methodology that everybody understood and that one could rely on and say, "This is what I'm doing and here's my success based on a set of criteria that we all accept."

As introduced to me, the RBC approach (we didn't use that name then) was more of a set of methods for doing applied macroeconomics using DSGE models. I think that to this day, the lasting contribution is this set of tools and methods. This included

1. A new way of summarizing business cycle facts that Hodrick and Prescott developed as an alternative to that employed by Burns and Mitchell. It provided [a] description of business cycles that was robust over time and across countries. Of course the facts do change somewhat over time and across countries, but exactly how they do change became interesting in and of itself. This was [a] new way to think about business cycles.
2. The notion that the same model used to explain long-run growth properties of an economy could also be used to think about business cycles was, as has been mentioned, new and, of course, quite controversial. As Charles mentioned, traditionally economists viewed business cycles as deviations from the kind of time series that a growth model would produce and understanding these deviations required a very different type of model. Both Kydland and Prescott (1982) and Long and Plosser (1983) were hugely controversial in the beginning, but ultimately very influential, by developing this line.
3. Calibration. Just mentioning this word could start a bar fight. What Ed and Finn did was to develop a method where models that were quite simple and clearly false could be used to account for the statistical properties of business cycles observed in actual economies. By "clearly false" I mean that there is no sense in which the equilibrium stochastic process implied by these models could have generated the kind of the data that we see in the [United States] or any other actual economy. The equity premium paper of Mehra and Prescott (1985) was also quite important in illustrating the power of this approach.

4. Finn and Ed also offered a set of methods for computing numerical solutions of these models. These were not models that you could solve with pencil and paper, and brand new computational methods were required. In particular, they suggested approximating a nonlinear model by one that had a quadratic objective and linear constraints. Of course, these numerical methods have been developed significantly from what Ed and Finn first proposed, but they were the first to offer methods for deriving quantitative conclusions from these models.
5. Finally, the notion of business cycles being caused by technology shocks or business cycles resulting from an environment with no distortions whatsoever, where the equilibrium was a solution to a planner's problem, was completely ridiculous to most people's minds. Certainly it was completely inconsistent with the traditional Keynesian approach to macroeconomics that was taught at the time.

So there were a lot of methods to master and a lot of controversial aspects that a graduate student needed to come to grips with if he/she was going to contribute to this research program. In my case, while Ed and Finn didn't have a net, I did. That net was Ed. Ed was convinced of the power and potential of this approach and was adept at coming up with responses to those who argued against it. His enthusiasm was infectious. Now, while the approach is still controversial to some, it has adapted to address many of the concerns that have been raised over the years. While there were no textbooks covering this material in the early 1980s, there are some very good textbooks now. In addition, the Internet is full of lecture notes on these and closely related topics. So the entry costs are no longer an issue. In fact, the methods that I have mentioned (and much more) are now taught in the first quarter of the first year of UCLA's Ph.D. program, and many (most?) others as well.

At this point, the methodology seems almost limitless in its potential and certainly one is not limited to the study of social planning problems. One can study equilibria [that] are very complicated in nature, including all sorts of frictions and distortions. As a result, most now refer to this research program by the broader name "dynamic stochastic general equilibrium (DSGE)" rather than the more specific term "real business cycles." What was once regarded as very controversial and limited to only a few economics departments can be now found literally everywhere. So, in some sense, what we are doing here by marking the thirtieth anniversary of RBC is really a bit of a victory lap. Given that there are lots of problems still to be solved, the program will likely continue for another thirty years. While it is not finished, it has certainly been an honor and a pleasure to have had the opportunity to contribute to its early development.

Thomas Cooley: It is a pleasure to reflect on the remarkable achievement of Finn and Ed in their 1982 paper and Charlie and John's bold 1983 paper, both of which moved the discussion of business cycles in a very different direction than had been the case. "Time to Build . . ." was remarkable because it contained within it three bold new ideas that created a new way of thinking about and exploring business cycles. Of course, as everyone has noted, the starting point was the exploration of general equilibrium models of cycles where the fundamental shock was a technology shock—a real shock—that set off adjustments in the economy that we call the business cycle. But that was only the starting point.

I once heard Lionel McKenzie give a talk about the development and evolution of general equilibrium theory. It was like describing the construction of a medieval cathedral brick by brick. If we took the Kydland–Prescott, Long–Plosser construct as a starting point and described everything that has been explored since within that framework—the introduction of money, rigidities, financial markets, the reconciliation of micro and macro elasticities, the elaboration of labor supply, vintage capital, firm dynamics, industry dynamics, household decisions and non market activities, trade, financial frictions, shadow banks, distorting taxes, fiscal policy . . . it does begin to resemble a cathedral at least conceptually. To me this metaphor seems fitting because one of the earliest critiques of the real business cycle construct was by Larry Summers, who described it—if I am remembering correctly—as offering not a very sound structure for thinking about cycles, but more like a loosely anchored flapping tent. That was probably correct and that was its appeal to the many generations of students who came out of Minnesota, Carnegie-Mellon, and Rochester and saw in that structure exactly what they needed to pursue an ever broader set of questions.

But it was not an arbitrary structure. There were three parts of the K–P contribution: general equilibrium, consistency with growth theory, calibration. But these are not separate items. They go together in a coherent way: Calibration is a procedure that restricts the mapping between competitive equilibria and the data so that the equilibria display certain desirable properties—in standard business cycle models those properties are the growth facts, but when other questions are addressed they could be any broader set.

As the last few years have reminded us, economics is never a settled field. There are new shocks, frictions, institutions that change the equilibria. But the field of economics has basically embraced the methodology of real business cycles—this methodology insists on intellectual coherence and coherence with data and it is now the preferred way to explore important questions.

Sumru Altug: I am not part of [the] panel but hearing the commentary, I'd like to add a few comments. As Finn has emphasized, in my time as a Ph.D. student at GSIA, we took the dynamic macroeconomic analysis course. I don't think we even had a regular macro course. Instead we had courses on general equilibrium theory and dynamic macroeconomic analysis. I had to learn the IS/LM model years later when I taught it but so . . . {Prescott interrupts: Why did you do that? Laughter.} Well, I don't know, I mean, you're right.

But there are some other people during my time at Carnegie Mellon who were not mentioned in this panel. One of those people is Rob Townsend, who has been extremely influential in furthering the agenda of modern general equilibrium theory and its applications. We also had Lars Hansen and Ken Singleton, whose work revolutionized macroeconomics and structural econometrics through the development of the generalized method of moments (GMM) and its applications. Thus, I would go along with Finn and say that how important quantitative macroeconomics and quantitative theorizing were at GSIA. My own thesis and subsequent *International Economic Review* 1989 paper arose out this environment at GSIA. As Edward Prescott has stated in written correspondence, this thesis was "an ambitious project that used the tools of statistics to select the model economy and to measure deviations from theory. Ex post, the ex ante promising investigation was not that successful, but a lot was learned in the process."

Now we'd like to move on to the part where we ask the panelists from the list of questions that Warren and I prepared. One of the questions that Warren and I prepared has to do with the literature about [the] equity premium puzzle. What was the role of this in the development of this field? The literature that arose subsequently to the Mehra and Prescott paper stimulated many new developments such as nonexpected utility, habit persistence, and other features. Many of these developments have been incorporated now into New Keynesian models that try to do policy analysis. So how do you view the contribution of [the] original equity premium puzzle paper?

Edward Prescott: By the way, King and Plosser are guilty again. They're the ones who got the equity premium puzzle paper published. We'd given up trying to get it published. The key thing was that the paper changed my thinking. I always thought in the empirical approach, you want to get a model that mimics a particular empirical data set. And there we just wanted to use a model that estimates how big something was. The premium for bearing nondiversifiable aggregate risk wasn't that big and this seemed to be a puzzle. By the way, that excess volatility puzzle has been open for nearly 40 years. Now, which one of you will solve that and make my day?

The goal is using theory to say how big things are, how much of the observed variation is accounted for by some factor. You don't have to account for 100% of the variation. It's obvious that model economies are abstraction of a complex reality. I don't see the equity premium puzzle as being a puzzle any more. Taxes account for about a third of it, intermediation costs account for another third, and the remaining part seems to be related to [the[liquidity value of short-term government debt. But the puzzle did foster progress in exploring alternative preferences orderings transaction and savings technologies.

Major advances were made. Larry Summers said there was this big tent flapping in the wind. We started looking at statistical properties, [a] certain set of statistics of the business cycles, of the time series—the business cycle statistics that Hodrick and I defined and reported. But now we are not content with just these business cycle statistics, but determine the predicted paths of economic variables. Now we can respond to Larry Summers type questions, which are good ones, such as, "Why did the economy boom in the early [19]60s? Why was there this fantastic growth?" And [the] answer is technology advances, the interstate highways, advances in chemicals, mainframe computers, jet airplanes, too. A good question in this genre is: Why is the economy depressed now? Think of Japan in the 90s—low productivity. Particularly we can better measure output and include intangible investments in a multisector framework and taking seriously the nature of the tax

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system and build it in there. You could estimate how big that was. But I think that was a concrete thing to focus on specific phenomena. If you permit animal sprits, that is, arbitrary shocks to preferences and technology that are not restricted by micro observations, you can explain anything within dynamic economic general equilibrium theory, as it is vacuous in itself.

Sumru Altug: We now move on to our second question. The RBC agenda is based on neoclassical assumptions about preferences. Yet there is a lot of recent empirical and experimental evidence regarding deviations from these behavioral assumptions, such as hyperbolic discounting, departures from expected utility, ambiguity aversion, etc. How should this agenda, the original RBC agenda, be modified to account for this new literature?

Edward Prescott: In behavioral finance, they have not provided [an] alternative paradigm to address such questions as how we should manage our retirement portfolios or how we can design a better financial system. Myron Scholes made this case quite elegantly. Until we have some other set of well-defined disciplined procedures, behavior economics will not contribute to economics, though it may contribute to the behavioral sciences. I remember that behavioral science was big at Carnegie Tech back in the [19]60s when I was a student and a lot of smart people explored that line of research in a rigorous way. But we haven't heard much about them because they were unlucky and bet wrong. You have to set up a disciplined alternative. Herb Simon would come in and say, "Firms do not maximize profits, they would have to solve large combinatorial problems, which are not solvable in finite time. It is computationally impossible to solve that." He was right, but then Lucas came back and said, "Well, we study abstract worlds where the problems are sufficiently simple that we can solve them and build our economic intuition based on what happens in these economies." That was a well-defined research program. It's working, pretty well, and it doesn't seem to me that there is any other alternative.

Charles Plosser: I don't have much to add, I agree with Ed 100%. But I would say, though, that this is the general advancement of knowledge—explorations of deviations of different sets of assumptions about the construction of a model and what people want to put in. I think the test is, ultimately, in some quantitative measure how meaningful it is. But certainly such strategies are the way to "let all of the flowers bloom," and to let people pursue different paths, because that's how science progresses.

John Long: Some of these things strike me as adding degrees of freedom and that's a slippery slope. You add enough degrees of freedom and you can explain anything. Some kind of discipline has to be used to evaluate whether the extra degrees of freedom you gain are worth it.

Sumru Altug: Of course, the raging issue is the financial crisis and the global impact of it. So how should the canonical RBC world view this phenomenon, what should be done? Should we just add a financial sector to the canonical model, or should we use the financial crisis to create a new framework?

Edward Prescott: Designing a better financial system is something we should do. And we should be worrying about that, there should be extensive discussion about that and once we agree, we should implement that better system. Financial crises tend to be somewhat disruptive and lead to lots of redistribution; when people are risk averse and redistributions are random, [the outcome is] not good. I don't see the evidence that these—any of these financial factors—impacted. I expected a big deviation from a theory that abstracted from the monetary and financial factors would occur in the 1980–1981 period. I was surprised that the standard theory with the standard factors—taxes, demographics, and TFP or productivity—mimicked or predicted that path.

Thomas Cooley: And a lot of it has been done in the context of disciplined general equilibrium models that adhere to the principles that we talked about. And I think the most important lesson is that there is *no* question that we can't imagine undertaking, but we don't trust answers, we are not likely to trust answers that don't take account of these general equilibrium issues, and then aren't properly calibrated to the data. I think that's where a lot of the looseness creeps into discussions of macroeconomics and maybe discussions of financial issues, financial frictions, and so on. It is if they don't adhere to this kind of discipline that enables you to have a little bit more confidence in the kind of answers that you get.

Sumru Altug: We have one other question but instead of asking that, I'm going to go back to an even earlier paper. When Warren and I were preparing our questions, we went back and also read the "Time Inconsistency" paper carefully again. What struck us from that reading was the fact that the entire RBC agenda seemed to be in the conclusion of the 1977 Kydland and Prescott "Time Inconsistency" paper. The paper asserts that what is optimal is rule-based policy, not discretionary monetary or fiscal policy. So, if it is then rule-based policy, though, we need to understand the underlying economic framework. What is the true model? Well, so, here's what they say: "The structure considered is far from a tested theory of economic fluctuations, something which is needed before policy evaluation is undertaken. The implication of this analysis is that until we have such a theory, active stabilization policy may well be dangerous," etc. So, then, if we are not to attempt to select policy optimally, how should it be selected? Our answer is as Lucas proposed—that economic theory be used to evaluate alternative policy rules and one with good operating characteristics be selected.

So, we understand this as representing what has come to be called as the "RBC agenda." How are we supposed to select the optimal rules? We need to know the economic environment in which they are going to operate, and there we have to use economic theory to evaluate these alternative policy rules, not the empirical policy evaluation that Lucas criticized. So it seems to us that then the whole agenda started from, or was in the conclusion of this paper, and is still continuing, as Professor Prescott said.

Charles Plosser: I'm going to move away from theory and talk a bit about the challenges that policy makers face. One challenge that policy makers are always faced with is to come up with what we think of as the "optimal policy." Sometimes our models allow us to construct optimal policies. But, unfortunately, we don't always know the underlying economic model, and different models can give rise

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to very *different* optimal policies. What's a policymaker to do? If you can't agree on the true model, you have a challenge. I believe that there is lots of evidence that policy making should strive to be systematic in ways that are robust across different models. This is the notion that many people have worked on—John Taylor (1999a, 1999b), Athanasios Orphanides and John Williams (2002)—how do you generate robust rules when you have uncertainty about the models? From a policy-making point of view, those sorts of explorations are important. I don't think we have great answers yet, but it is an important and growing area of research

Warren Young: We want to thank all those who participated for their recollections and insights. The RBC paradigm has indeed come a long way in *three decades*, from being the *challenger* to the mainstream, to becoming the *basis* for the mainstream. And, as in the case of the other modern "revolution in macroeconomics"—rational expectations—we hope that in the future our students, at least, will be able to assess the RBC after *fifty years* of being the *core* of modern quantitative macroeconomics and policy analysis. For, as Ed Prescott said at the end of his presentation today:

> In another generation policy-makers also may take our views seriously, our scientific findings. It's a high-powered scientific tool. And I don't see any competing discipline.

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