

2 The Evolution of Ideas in Macroeconomics*

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Historians of economic thought frequently use 'revolution' or 'counter-revolution' to highlight fundamental changes in economic thinking. A reader of any one of the many surveys of macroeconomic thought during the past 40 years will find many references to revolutions and counter-revolutions. Of course there was the *Keynesian* revolution which spread rapidly during the 1940s and 1950s. Since then, there appears to have been at least one revolution or counter-revolution per decade.

When Harry Johnson surveyed macroeconomic developments at the end of the 1960s, he focused on the *monetarist* counter-revolution to the Keynesian revolution.¹ By the end of the 1970s many reviewers were writing of the rational expectations revolution referring to any one or more of three related developments during the 1970s: (1) new classical models, (2) rational expectations models with sticky prices and wages, and (3) new policy concepts such as the Lucas critique and time inconsistency.² Now at the end of the 1980s some

reviewers are drawing attention to a *new Keynesian* counter-revolution, emphasizing the micro-economic foundations of sticky wages and prices.³ But other reviewers of the 1980s, such as Edward Prescott, see more to a *real business cycles* revolution.⁴

In this brief review of developments in macroeconomics I would like to try a somewhat different tack which emphasizes a more gradual evolutionary development of ideas, rather than a series of sharp revolutions and counter-revolutions. This emphasis may result in a less exciting story to tell than the common one of revolutions and counter-revolutions, but I think, for many purposes, it is more accurate.

I take this tack after having been asked many times, at sessions like this one, to review the significance of recent developments in macroeconomics, and then to observe, after the passage of more time, how these developments actually affect the mainline macroeconomic model or the way that practical macroeconomists think about the economy. Given the time limitations of this symposium I will try to illustrate the evolutionary nature of macroeconomic thought by focusing on three intellectual developments, one from the 1970s and two from the 1980s. Because a passage

Macroeconomic Stabilization, *Journal of Money, Credit and Banking* 12, 716-46. John B. Taylor (1980), 'Recent Developments in the Theory of Stabilization Policy', in *Stabilization Policies: Lessons from the '70s and Implications for the '80s*, Federal Reserve Bank of St Louis.

³ The remarks by Alan Blinder at this symposium are a good example.

⁴ Edward Prescott (1986), 'Theory Ahead of Business-Cycle Measurement', *Carnegie Rochester Conference Series on Public Policy* 25, 11-44.

* This is a written summary of remarks made at a Symposium on Macroeconomics, sponsored by the Econometric Society and the Australian Economic Society in Canberra, Australia, 29 August 1988. This research has been supported by a grant from the National Science Foundation at the National Bureau of Economic Research.

¹ Harry G. Johnson (1971), 'The Keynesian Revolution and the Monetarist Counter-Revolution', *American Economic Review*, May, Papers and Proceedings, pp.1-14.

² Robert E. Lucas Jr and Thomas Sargent (1978), 'After Keynesian Macroeconomics', in *After the Phillips Curve: The Persistence of High Inflation and High Unemployment*, Federal Reserve Bank of Boston. Bennett T. McCallum (1980), 'Rational Expectations and

of time is necessary to observe gradual evolutionary change, the first example will serve my purpose better than the latter two and will involve less speculation on my part.

1 From the Policy Ineffectiveness Debate to the Development of Empirical Rational Expectations Models

Rational expectations entered macroeconomic research in the form of a debate over the ineffectiveness of monetary policy. The debate began in the early 1970s with several papers by Robert Lucas and, in particular, with a paper by Thomas Sargent and Neil Wallace in the 1975 *Journal of Political Economy*.⁵ Sargent and Wallace showed that monetary policy rules were ineffective in stabilizing real GNP. Their theorem was based on a combination of two key ingredients: (1) rational expectations and (2) perfect wage and price flexibility with instantaneous market clearing. Both ingredients were necessary for the theorem; without either ingredient the theorem would not hold.

There were many things about the rational expectations assumption which I liked from the start. First, it seemed more realistic than existing adaptive expectations models—people do appear to be forward-looking in many aspects of their behaviour. Second, it was one way to deal with the Lucas critique which raised a serious problem with conventional ways of doing econometric policy evaluation in macroeconomics. Third, it placed emphasis on evaluating macroeconomic policy as a rule or system or institution, rather than as one-time changes in the levels of the policy instruments. But the second aspect, perfectly flexible prices and market clearing, was something that made little sense to me, even as an empirical approximation. Wages do seem to be sticky at least for a few quarters, and understanding the inflation cycles of the late 1960s and early 1970s required some theory of why it was difficult to instantaneously reduce inflation without recession. For these reasons, Edmund Phelps and I developed a model that combined rational expectations and sticky prices and wages.⁶ The model had all the good

features of rational expectations—forward-looking behaviour, an examination of the Lucas critique and policy rules for the longer term. Stanley Fischer developed a similar model about the same time. The Fischer paper was published in the same issue of the *Journal of Political Economy* as the Phelps-Taylor paper.

Somewhat over enthusiastically, I thought at the time (around 1975) that these papers would end the policy-ineffectiveness debate and that people would immediately begin to start doing empirical policy analysis with models based on rational expectations and Keynesian-like wage and price rigidity. I even set out to build a small rational expectations econometric model with these features myself.⁷ But the change in macroeconomic thinking was much slower than I anticipated, partly because there was an enormous resistance to rational expectations by Keynesians and partly because the instantaneous market-clearing view was taken more seriously than I anticipated. But more importantly than either of these reasons, econometric procedures—estimation and solution techniques—had to be developed to make these rational expectations types of models as operational as the conventional models that were used in practice at the time. In retrospect it turned out to be almost ten years before these techniques were fully developed and could be used in large-scale empirical models with enough detail for practical policy work.⁸ In the meantime the policy ineffectiveness debate raged. Robert Barro's unanticipated money regressions were a big factor in the debate.⁹ Perhaps another factor was my own work and that of others on staggered wage setting showing that wage rigidities could be quite modest in length, could have some microfoundations and

⁵ John B. Taylor (1979), 'Estimation and Control of Macroeconomic Model with Rational Expectations', *Econometrica* 47, 1267-86.

⁶ Estimation procedures like those by Lars Peter Hansen (1982), 'Large Sample Properties of Generalized Methods of Moments Estimators', *Econometrica* 50, 1029-54 and solution procedures like those by Ray Fair and John B. Taylor (1983), 'Solution and Maximum Likelihood estimation of Dynamic Nonlinear Rational Expectations Models', *Econometrica* 51, 1169-85 were to come later and like many technical innovations in macroeconomic research would not individually be considered revolutionary.

⁷ Robert Barro (1978), 'Unanticipated Money Growth and Unemployment in the United States', *Journal of Political Economy* 86, 549-80.

⁵ These papers are reprinted in Robert E. Lucas and Thomas Sargent (1981), *Rational Expectations and Econometric Practice*, University of Minnesota Press, Minneapolis, and in Robert E. Lucas, Jr (1984), *Studies in Business Cycle Theory*, M.I.T. Press, Cambridge, Massachusetts.

⁶ Edmund S. Phelps and John B. Taylor (1977), 'Stabilizing Powers of Monetary Policy under Rational Expectations', *Journal of Political Economy* 85, 163-90.

still general business cycle persistence.¹⁰ But the debate over whether only unanticipated money mattered did eventually die out, certainly by the mid-1980s. Evidence against the view seemed to overwhelm evidence in favour of the view. Robert Barro and Zvi Hercowitz as well as Frederic Mishkin found strong evidence against it.¹¹ During the period (1985-88) that I was co-editor for macroeconomics at the *American Economic Review*, very few papers on the subject of policy ineffectiveness were submitted.

But even though the debate over the ineffectiveness of monetary policy has ended, rational expectations theory and econometrics has had a lasting effect on macroeconomic thinking. One can appreciate these changes by observing the evolution of models over this period. It appears that many of the rational expectations ideas have been embedded in mainline models. For example, at a conference at the Brookings Institution in 1986, four of the twelve large-scale multicountry econometric models examined were based on rational expectations.¹² Several of these models used wage and price rigidities along with the rational expectations. Since then more rational expectations models have been developed. In my view, this gradual evolution represents significant progress, but it is hard to see without at least a 15-year historical perspective.

II Unit Root Econometrics

In the early 1980s, Charles Nelson and Charles Plosser as well as John Campbell and Greg Mankiw showed that real GNP shocks have a large permanent component.¹³ More technically, there

is a quantitatively significant unit root component in the estimated time series for real GNP. A shock to the world economy such as the one in 1981-82 would imply that GNP would stay permanently below the previous trend. At the bottom of the 1982 recession, for example, one would predict that the economy would never return to the previous trend in potential.

In many respects this simple statistical finding itself was considered revolutionary. Most macroeconomic theories developed in the past 40 years had characterized the economy as fluctuating about a given trend of potential GNP and that the economy would return to this trend after a shock. If the unit root component is significant, and if one takes the deviation from the trend line as a measure of the business cycle, then clearly the business cycle is less severe if unit roots are very significant. For example, the 1981-82 recession would be interpreted as a change in the level of the potential GNP rather than as a business cycle fluctuation around potential GNP. Because it painted such a different picture of the cycle, the Nelson-Plosser finding attracted the attention of many researchers and was the subject of considerable debate.

More recent work using state space methods, Kalman filtering, and other statistical procedures by Peter Clark, John Cochrane and others has shown that there is more reversion to trend than the Nelson-Plosser or Campbell-Mankiw methods estimated.¹⁴ Nonetheless, the permanent effect is not zero as was implicitly assumed in theoretical models, at least as an abstraction to make the models simpler. This unit roots debate has now reached an impasse with the view that the existing data cannot reveal any more information about the permanent and temporary components. However, I believe one can already see a permanent effect on mainline macroeconomic thinking as a result of this debate.

First, there is no question that the permanent effects are not trivial and that shocks to real GNP have a permanent component that existing theories need to deal with. Second, and I think more important, it is striking that when one looks at a plot of potential real GNP that comes out of, say, Peter Clark's Kalman filter, it appears to be incredibly smooth over the post-World-War-II

¹⁰ John B. Taylor (1980), 'Aggregate Dynamics and Staggered Contracts', *Journal of Political Economy* 88, 1-23.

¹¹ Robert Barro and Zvi Hercowitz (1980), 'Money Stock Revisions and Unanticipated Money Growth', *Journal of Monetary Economics* 6, 257-67 and Frederick Mishkin (1982), 'Does Anticipated Monetary Policy Matter? An Econometric Investigation', *Journal of Political Economy* 90, 22-51.

¹² The conference proceedings are published in Ralph Bryant, Dale Henderson, Gerald Holtham, Peter Hooper, Steven Symansky (eds), *Empirical Macroeconomics for Interdependent Economies*, Brookings Institution, Washington.

¹³ Charles Nelson and Charles Plosser (1982), 'Trends and Random Walks in Macroeconomic Time Series', *Journal of Monetary Economics* 10, September, 139-62. John Campbell and N. Gregory Mankiw (1987), 'Are Output Fluctuations Transitory', *Quarterly Journal of Economics*, November, 857-80.

¹⁴ Peter Clark (1987), 'The Cyclical Component in US Economic Activity', *Quarterly Journal of Economics*, November, 797-814. John Cochrane (1988), 'How Big is the Random Walk in GNP', *Journal of Political Economy* 96, October, 893-920.

period. Even though permanent shocks have an important statistical effect, they do not change the shape of the business cycle by much. Clark's trend looks a lot like what one would find by drawing a few crude trend lines to reflect long-term productivity growth. There is a slowdown in growth to reflect a lower productivity growth starting around the late 1960s and early 1970s.

What are some implications for economic theory? Some authors have suggested that one identify real shocks as permanent and monetary shocks as temporary.¹⁵ There are problems with this identification—monetary shocks can be permanent because they affect investment and capital accumulation—but it is a useful starting point. What this identification implies is that the recent research on unit roots suggests that real factors have an important role to play in any macroeconomic theory. And this brings me to my third and most current example of evolution in macroeconomics.

III Real Business Cycle Models

As I discussed earlier (in Section I), I underestimated the length of the policy ineffectiveness debate of the 1970s, and I also miscalculated what the end of that debate would bring. The end of the debate, with the conclusions as I characterize it here that monetary policy rules are effective, didn't lead all researchers to a new class of models in which rigidities were combined with rational expectations. Instead many researchers have abandoned money altogether and kept the market-clearing assumption, developing the so-called real business cycle models of Kydland and Prescott.¹⁶

Kydland and Prescott's point is that you can explain business cycles with a stochastic growth model based on market clearing without money. Of course, a better term would be non-monetary models, because most monetary models have real factors in as well.

I find this extreme view far from reality. Even if we ignore the evidence of Friedman and Schwartz¹⁷ for the Great Depression in the US, there seem to be problems with the extreme real

business cycle view. I find it difficult to explain the 1981-82 recession without a reference to the role of the Federal Reserve Board in attempting to reduce the rate of inflation. I also find it difficult to explain the difference between economic fluctuations in the US and Japan without reference to differences in nominal wage rigidities and monetary policy. Finally, there are other factors about the business cycle, the correlations between prices and output, that the real business cycle can not explain.

Despite this negative assessment of the extreme real business cycle view, it appears (and here I need to speculate rather than look at the historical experience) that mainline macroeconomic thinking, as it gradually evolves over time, will be significantly affected by the research being completed in this real business cycle area. Note first that some researchers are putting money or credit into real business cycle models, thereby borrowing some of the ideas and techniques from this literature while permitting the models to be more realistic and empirically useful.¹⁸

The emphasis on the interaction between cycles and growth which is the hallmark of the real business cycle models and which the research on unit roots suggests is empirically important is likely to be a lasting impact on macroeconomics. I would also see the interaction of finance and macroeconomics that is a feature of real business cycle models¹⁹ enabling a better description of risk premia and thereby a better explanation of exchange rate and interest rate behaviour in macroeconomic models.

IV Conclusion

Rational expectations econometric models are now starting to be used in practical policy discussions, even if the name has to be changed to consistent expectations. Unit root economics has caused a rethinking of the interaction between growth and cycles in macroeconomic research. Real business cycle models are now beginning to incorporate money and to permit a better

¹⁵ Olivier Jean Blanchard and Danny Quah (1989), 'The Dynamic Effects of Aggregate Demand and Supply Disturbances', *American Economic Review*, forthcoming.

¹⁶ Finn Kydland and Edward Prescott (1982), 'Time to Build and Aggregate Fluctuations', *Econometrica* 50, 1329-33.

¹⁷ Milton Friedman and Anna Schwartz (1960), *A Monetary History of the United States*, Princeton University Press, Princeton, New Jersey.

¹⁸ Ben Bernanke and Mark Gertler (1988), 'Agency Costs, Net Worth, and Business Fluctuations', *American Economic Review*, forthcoming. Thomas Cooley and Gary Hansen (1988), 'The Inflation Tax in a Real Business Cycle Model', Rochester Center for Economic Research, Working Paper No.155, University of Rochester.

¹⁹ Lars Peter Hansen and Kenneth Singleton (1982), 'Generalized Instrumental Variables Estimation of Nonlinear Rational Expectations Models', *Econometrica* 50, 1269-86.

description of phenomena like risk premia in financial markets. These are all examples of the recent and possibly the future evolution of ideas in macroeconomics as a result of ongoing research in the field.

In concluding my remarks, it is appropriate to comment briefly on another potential influence in the development of macroeconomics. It is frequently said the swings in political sentiments influence the flow of new ideas in macroeconomics. Perhaps some people latched on to market-clearing models in macroeconomics because of the non-intervention bias in these models. And perhaps the attraction to rational expectations itself comes from

a conservative attitude which tends to emphasize long-run behaviour.

But, in my view, such political influence on research can be counterproductive even in the more technical areas that I have discussed here. Take the first and the last of the 'revolutions' in macroeconomic thought which I began these remarks by listing: just as there were many good ideas in Keynesian macroeconomics that conservatives should not have ignored because of its bias towards government intervention, so there are many good ideas in real business cycle literature that liberals should not ignore because of its bias towards non-intervention.