

Revolution and Evolution in Twentieth-Century Macroeconomics

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The twentieth century has seen profound progress in economic thought. This has been associated, among other things, with the progress of economics to a fully autonomous disciplinary status, which had only begun to be established late in the nineteenth century, and with a very substantial improvement in the technical methods employed in the discipline, both in the elaboration of economic theory and in the statistical analysis of economic data. Over the past century economics has also come to play a more important role in the world at large. Economic advisors have become more important in the formulation of government policies and the policies of international organizations such as the IMF and the World Bank; economic theory has proven to be of practical use in the design and use of a world of new financial instruments; and economic ideas have become influential in a number of areas outside the discipline's traditional boundaries, including sociology, political science, and legal studies.

Attempting to catalog, let alone evaluate, all of the important developments in so active a field over the entire century would be a task beyond the scope of a lecture of this length. As a result I have chosen instead to discuss a single, though important, strand in twentieth-century economic thought. This is the field of macroeconomics, the branch of economics concerned with fluctuations in the overall level of business activity, with the determinants of inflation, interest rates, and exchange rates, and with the effects of government policies—such as fiscal policy, monetary policy, and exchange rate policy – that are considered mainly with regard to their effects upon the economy as a whole.

There are several reasons for the selection of macroeconomics for special attention, apart from the fact that I am more knowledgeable about it. First of all, among the applied fields in economics, it has a special place in modern economics curricula. Macroeconomics is also the part of economics most often associated with the field by the general public. For example, a few years ago, Daniel Bell and Irving Kristol edited a book titled *The Crisis in Economic Theory*,¹

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¹ New York: Basic Books, 1981.

but what the book was about was not general economic theory, but the state of macroeconomics.

Second, macroeconomics is an interesting case because the degree to which there has been progress over the course of the century is sufficiently far from transparent as to make it still a suitable topic of conversation. A discussion of the century's progress in general economic theory -- with primary emphasis upon what is taught in courses on "microeconomic theory", which emphasize the decisions of individual households and firms -- would surely be more suitable if my aim were to boost the prestige of my own field among the many distinguished representatives of other disciplines present here. But the story would be one with little suspense. For it would not be too much of an oversimplification to present the field as having progressed smoothly and steadily, developing theories of ever greater power and broader scope within an essentially unchanged explanatory framework, based on the concepts of optimizing individual behavior and market equilibrium, that were already central to economic thought in the previous century. Macroeconomics instead has been famously controversial, with the alarmism of Bell and Kristol going only slightly beyond what one finds in the academic literature. Discussions of twentieth-century developments in macroeconomics make frequent references to "revolutions" and "counter-revolutions",² and the question of whether there has been progress at all (or which broad developments should count as progress) is a more lively topic of debate among economists than one might believe would be possible in the case of a topic with such a canonical status in the curriculum.

Finally, macroeconomics is an appropriate case to consider on this occasion because it has been such a quintessentially twentieth-century development. The rise of macroeconomics as a second, co-equal branch of economic theory in the standard curriculum is a novelty of the twentieth century, the result both of intellectual developments (notably the rise of Keynesian theory) and of a new importance attached to management of the economy in twentieth-century ideas about the role of government. Skeptics may challenge whether it should have an equally prominent role in the curricula of the coming century. I am inclined to believe that it should, but the question is worth considering, and raises central issues about the nature both of this subfield and of economics more generally.

² See, e.g., Lawrence R. Klein, *The Keynesian Revolution*, New York: Macmillan, 1949; Harry G. Johnson, "The Keynesian Revolution and the Monetarist Counter-Revolution," *American Economic Review* 61(2): 91-106 (1971); Mark H. Willes, "'Rational Expectations' as a Counterrevolution," in Bell and Kristol, *op. cit.*; David K.H. Begg, *The Rational Expectations Revolution in Macroeconomics: Theories and Evidence*, Oxford: Philip Allan, 1982.

The Birth of Macroeconomics

The systematic study of business fluctuations and stabilization policy is almost entirely a twentieth-century development. During the previous century, the occurrence of repeated “crises” was a feature of commercial life that a number of authors remarked upon, including Karl Marx. But speculation about the nature of such crises seldom rose to the level of anything that could be called a theory, and had little effect upon the view of the market mechanism presented in mainstream texts on economic theory.

Monetary economics existed as identifiable subfield, and had even developed at least a few important theoretical ideas. These included the quantity theory of money (the proposition that increases in the money supply lead, at least eventually, to proportional increases in the general level of prices), and the dichotomy between “real” and “nominal” variables (with the former being determined, at least in the long run, purely by non-monetary factors). However, discussions of the conduct of monetary policy made little contact with this theory.³ Furthermore, monetary policy was largely viewed as a technical issue relating to the way that “bank rate” (in the British terminology) needed to be managed in order to prevent a loss of the central bank’s gold reserves, in order to make possible continued adherence to the gold standard; it was not discussed in terms of broader economic objectives such as stabilization of economic activity, employment, or the general level of prices. The main intellectual issue relating to monetary policy was the choice of a monetary standard—whether the gold standard, a silver standard, a bimetallic standard, or some other more exotic proposal was preferable in principle.⁴

The early decades of the twentieth century saw important progress in the study of business fluctuations, with a proliferation of proposed explanations for the regular occurrence of “business cycles”.⁵ Perhaps most importantly, by the 1920s, statistical institutes for the study of business cycles had been founded in many countries, and business cycle researchers (most notably those working with Wesley Mitchell at the National Bureau of Economic Research in New York) were able to document in detail the nature of the recurrent patterns of co-movement among a large number of economic series.⁶

³ See, e.g., Allan Meltzer, “A History of the Federal Reserve, chapter 2: The Development of Central Banking Theory and Practice,” mimeo, Carnegie-Mellon University, November 1995.

⁴ For example, the proceedings of the 1904 “Congress of Arts and Sciences” held in conjunction with the St. Louis Exposition include “Money and Credit” as one of the six branches of economics treated. But both of the papers included in that section deal mainly with the desirability of a gold standard.

⁵ Gottfried Haberler, *Prosperity and Depression*, Geneva: League of Nations, 1937, provides a thorough review of the pre-Keynesian literature.

⁶ See Mary S. Morgan, *The History of Econometric Ideas*, Cambridge: Cambridge University Press, 1990, chapter 2. Arthur F. Burns and Wesley C. Mitchell, *Measuring Business Cycles*, New York: National Bureau of Economic Research, 1946, is the classic summary of the early NBER studies.

Business cycle theorists of this period held a wide variety of views, not only about the origin of such fluctuations, but about what could be done about them. Some, notably those influenced by “Austrian” theory, regarded the sequence of events characteristic of the “cycle” as having a fateful inevitability, with which it would be unwise, or perhaps impossible, for government policy to interfere.⁷ Others were more optimistic about the possibility of stabilization policy,⁸ but held widely varying views about the type of policy that would be effective. For example, some (such as Arthur Pigou and Dennis Robertson in Cambridge) argued that monetary policy would be ineffective but that variation of public-works expenditures over the cycle would be highly desirable, while others (such as Ralph Hawtrey, another Cambridge cycle theorist) argued exactly the opposite.

In any event, none of these theories had much effect upon public policy prior to the 1930s. For one thing, the business cycle theories of the 1920s were not yet quantitative models that could be used to analyze the effects of alternative policies. The statistical studies of the business cycle institutes constituted their object of study by stressing the similarities between sequences of measurements made at different times and places; but merely documenting these patterns gave little indication of what might happen differently in the event of government intervention. The proposed theories tended to be narrative accounts of the sequence of events that followed one another through successive “phases” of the business “cycle”, tracing an intricate, but purely linear, chain of causation with a complexity to rival a Rube Goldberg cartoon. They were intended to render intelligible the seemingly invariant patterns that the statisticians were documenting, but were again not well-suited to the generation of concrete predictions about alternative scenarios.

⁷ The fatalism of this strand of the literature is illustrated by the comment of the eminent Harvard business-cycle theorist Joseph Schumpeter, who warned in the early days of the New Deal that “recovery is sound only if it does come of itself. For any revival which is merely due to artificial stimulus leaves part of the work of depressions undone and adds, to an undigested remnant of maladjustment, new maladjustment.” From “Depressions,” in D.V. Brown et al., eds., *The Economics of the Recovery Program*, Cambridge: Harvard University Press, 1934. In Great Britain, Lionel Robbins’ adherence to Austrian theory led him to oppose public-works programs during the Depression, a position that he later characterized as “the greatest mistake of my professional career.” From *Autobiography of an Economist*, London: Macmillan, 1971, p. 154.

⁸ For example, the leading Swedish business-cycle theorist Gunnar Myrdal was scornful of the Austrian doctrine, suggesting that “perhaps the whole attitude was ultimately based upon a primitive puritanism; happiness is somehow evil, something immoral, which should be accompanied by a purifying misery now and then in order that those who have experienced it may be redeemed; and so it is only proper, right and natural that after the upswing, with all its sad mistakes, bad times should follow.” *Monetary Equilibrium*, London: W. Hodge, 1939 [originally published in Swedish in 1931]. Most members of the Cambridge school, to which Keynes belonged, were also advocates of stabilization policy.

The “Keynesian Revolution”

The Great Depression, of course, had a dramatic effect upon thought about such matters. It shifted attention from the dynamics of recurrent cycles to the pressing problem of what to do about a severe slump, and made the question of what government measures such public works could accomplish a matter of urgent debate. And the appearance of John Maynard Keynes’ *General Theory of Employment, Interest and Money*⁹ had a profound intellectual impact, essentially creating the subject of macroeconomics as it is now understood. Keynes offered a theory of depression economics that asserted, famously, that the market mechanism could not be relied upon to spontaneously rebound from a slump, and that advocated public spending, preferably involving a deficit in the government budget, to stimulate demand. Most importantly from a scientific point of view, it presented a framework that could be used to calculate the effects upon economic activity of government spending and taxing of particular amounts, and thus to estimate the size of the required intervention.

The degree to which Keynes’ ideas were completely novel is sometimes challenged;¹⁰ and it is certainly true that the aspects of his book that were most novel, and indeed shocking, to his contemporaries were not the parts that have proven to be of most lasting significance. Many of the important themes of the book, from the slow adjustment of wages as an explanation for continuing disequilibrium to the prescription of spending upon public works to increase employment, could be found in other writing of the time, and even of the previous decade.¹¹ But Keynes’ genius lay in showing how these pieces fit together.

Furthermore, he illustrated the fruitfulness of a new style of macroeconomic theorizing, that de-emphasized *dynamics*, in order to emphasize the *simultaneous determination* of a set of key variables (employment, income, interest rates, and prices) by one another at a given point in time. The obscuring of business-cycle dynamics was not an intellectual advance in itself, of course,¹²

⁹ London: Macmillan, 1936.

¹⁰ See, e.g., David Laidler, *Fabricating the Keynesian Revolution: Studies of the Inter-war Literature on Money, the Cycle, and Unemployment*, Cambridge: Cambridge University Press, 1999, for a nuanced discussion of this issue.

¹¹ Wage stickiness had been emphasized by Cambridge monetary economists well before the *General Theory*, beginning with the work of Alfred and Mary Marshall in the 1870s, and including the work of Marshall, Pigou, and Hawtrey in the 1910s and 1920s. See David Laidler, *The Golden Age of the Quantity Theory*, Princeton: Princeton University Press, 1991, chap. 4. Advocacy of countercyclical public-works expenditures as a remedy for unemployment was also common in the Cambridge literature by the 1920s, being espoused by Pigou, Lavington, and Robertson; see Laidler, *Fabricating the Keynesian Revolution*, *op. cit.*, pp. 103-4. For an example of American economists’ views during the Depression, see Arthur D. Gayer, “Monetary Policy and Public Works,” in Robert M. MacIver *et al.*, *Economic Reconstruction: Report of the Columbia University Commission*, New York: Columbia University Press, 1934.

¹² Indeed, Keynes’ truncated analysis of dynamics and expectations was the feature of the book least liked by some of his most sophisticated contemporaries. See, e.g., Bertil Ohlin, “Some Notes on the Stockholm

and the project of extending the Keynesian statics into a coherent dynamic model has been much of what has kept macroeconomic theorists busy in the second half of the century. But there can be little doubt, with hindsight, that the redirection of attention was a masterstroke at the time, because of the improved understanding that it allowed of relations of simultaneous causation.

Much of what economic analysis meant at the time was the use of the paradigm of the determination of prices and quantities by supply and demand to explain the determination of some *single* price and some corresponding quantity by the supply and demand curves appropriate to a single market. Thus wages (and employment) were determined by labor supply and demand; interest rates (and savings) were determined by the supply of savings and the demand for funds to finance investment; and the general level of prices (and money balances) were determined by money supply and demand relations. In any of these cases, a denial that prices clear the market in reality would have been tantamount to abandonment of the explanatory paradigm, so that economic analysis would then have little to say.

But Keynes' analysis showed that if one instead considered the simultaneous determination of all of these prices and quantities at once (taking into account the fact that the level of income implied by a given level of employment also affects the supply of savings and the demand for money balances, and so on), one could deny the postulate of market-clearing at one point in the system (the labor market), and still put the requirements for equilibrium in the *other* markets to good use.¹³ Furthermore, one could reach conclusions about interventions that could affect employment, say, that went beyond the mere adjustment of factors relating to that specific market. Thus while the idea that downward wage rigidity could lead to unemployment was well understood by many of Keynes' contemporaries, Keynes was not interested in promoting the conclusion that action should therefore be taken to lower wages. Consideration of the interactions between several markets made possible an understanding of why other kinds of interventions, such as government spending, could also be effective, and indeed of why wage cuts would not necessarily be.

The result was a powerful engine for the analysis of the effects of any of a variety of disturbing factors (originating either within the economy itself, or in government action) upon each of several important variables, under circumstances that allowed for the persistence of a sustained period in which

Theory of Savings and Investment," *Economic Journal* 47: 53-69, 221-240 (1937), which anticipated important lines of subsequent development.

¹³ The system of simultaneous equations is not actually written in the *General Theory*; it was instead left to John R. Hicks to write them down, in his famous commentary, "Mr. Keynes and the Classics: A Suggested Interpretation," *Econometrica* 5: 147-159 (1937), which became the basis of post-war macroeconomic pedagogy. Keynes' own exposition of his reasoning about the simultaneous determination of the variables mentioned is largely verbal, and quite complex.

wages (and possibly prices as well) failed to fully adjust. Its availability greatly facilitated the growth of a new attitude toward economic policy, in which government accepted a greater degree of responsibility for management of the level of economic activity, and economic advisors provided estimates of the effects of particular contemplated actions.¹⁴ It also provided impetus for the further development of national income accounting, another critical twentieth-century advance for understanding business fluctuations, through the stress placed in Keynes' system upon variations in the various components of national expenditure as the source of short-run variations in economic activity.¹⁵

Finally, Keynesian economics provided the conceptual framework within which simultaneous-equation econometric models of the economy could be developed, for purposes of forecasting and quantitative policy analysis. It is true that the first simultaneous-equation econometric models, developed by Jan Tinbergen in the 1930s, were mainly inspired by pre-Keynesian business cycle theories, and Keynes himself was quite skeptical of this work.¹⁶ However, the main developers of postwar macro-econometric models, such as Lawrence Klein and Franco Modigliani, based the structure of their models directly upon the skeleton provided by Keynes' *General Theory* and subsequent elaborations such as those of Hicks and of Modigliani himself. Furthermore, Tinbergen's models had a recursive structure in which each variable was individually determined by a combination of exogenous disturbances and prior variables, even though each endogenous variable could be affected by the past values of several others; they did not yet incorporate the simultaneous determination of variables that I have identified as a crucial aspect of the Keynesian revolution, and that was responsible for the central intellectual problem (the "identification" problem) connected with simultaneous-equation econometric models.¹⁷

Thus the development of Keynesian economics was central to the development of modern economics as it appeared at mid-century. This is so not simply because it brought a new importance to the study of business fluctuations, which now had results and a mission of sufficient importance to make macroeconomics a central subject in the economics curriculum. Keynesian economics also provided crucial impetus to the development of two other key developments of the middle part of the century -- general-equilibrium reasoning in economic theory, and simultaneous-equation modelling in econometrics.

¹⁴ This new role for economics and economists in the world outside the academy was formalized by institutional developments such as the Full Employment Act of 1946 in the United States, that committed the federal government to the maintenance of full employment, and created the President's Council of Economic Advisors.

¹⁵ See, e.g., Don Patinkin, "Keynes and Econometrics: On the Interaction between the Macroeconomic Revolutions of the Interwar Period," *Econometrica* 44: 1091-1123 (1976).

¹⁶ See Morgan, *op. cit.*, pp. 121-130.

¹⁷ See the diagrams of the causal structure of Tinbergen's models in Morgan, *op. cit.*, chapter 4.

It may seem odd, in light of the disputes about the “microfoundations of macroeconomics” since the 1960s, which have tended to present Keynesian macroeconomics and general equilibrium theory as two alternative and apparently incompatible ways of modeling the economy as a whole, to speak of Keynesian economics as part of a broader current of general-equilibrium reasoning. But the most important early presentations of general-equilibrium theory as a paradigm for economic analysis, such as John R. Hicks’ *Value and Capital*¹⁸ and Paul A. Samuelson’s *Foundations of Economic Analysis*,¹⁹ gave prominence to Keynesian macroeconomic theory as an important application of general-equilibrium analysis. Furthermore, some of the most important critics of general-equilibrium analysis in this period on methodological grounds happen also to have been leading critics of Keynesian macroeconomics.²⁰

The “Neoclassical Synthesis”

Many of the most important developments in macroeconomics since World War II can be described as attempts to come to terms with the “Keynesian revolution” that we have just described. The rise of Keynesian economics created a place for macroeconomics as a second main branch of economic theory, alongside microeconomic theory (the modern version of the “theory of value” that had constituted virtually the entire content of economic theory in the nineteenth century). It did this not only by giving macroeconomics new content and importance, but also by leaving unclear the nature of the connection between the principles of macroeconomics and the more familiar principles of microeconomic theory (those of rational individual choice and market equilibrium), so that pedagogy was made less awkward by treating the two subjects on different days, if not in different semesters.

In the beginning, the methodological gap was not really so extreme; while Keynes contemplated a failure of some markets, such as the labor market, to clear, his analysis nonetheless presumes equilibrium in a reasonably conventional sense in other markets, and the *General Theory* consists largely of analysis of individual and firm decisions that is not different in style from the applied microeconomic analysis of the time. But as macroeconomics became a quantitative subject, with the development of econometric models as its central aim, the connections with the elements of microeconomic analysis became less

¹⁸ Oxford: Clarendon Press, 1939.

¹⁹ Cambridge: Harvard University Press, 1947.

²⁰ See, e.g., Milton Friedman, “Lange on Price Flexibility and Employment: A Methodological Criticism,” *American Economic Review* 36: 613-631 (1946); and “The Marshallian Demand Curve,” *Journal of Political Economy* 57: 463-495 (1949). See also E. Roy Weintraub, *Microfoundations: The Compatibility of Microeconomics and Macroeconomics*, Cambridge: Cambridge University Press, 1979, chapter 4, for a discussion of the relation between the rise of Keynesian macroeconomics and Walrasian general equilibrium theory along similar lines.

and less explicit. Statistical relations between aggregate economic time series came to take priority over theoretical notions such as the incentives provided by prices or the constraints imposed by budgets, or even the nature of the economic units in which particular decisions were made. When price measurements were found not to be of great help in regression equations seeking to explain the evolution of aggregate quantities, prices came to be de-emphasized, in favor of relations directly relating quantities to other quantities (for example, explaining variations in consumption and investment spending by variations in national income). But such relations were different in form from those suggested by microeconomic theory, where the response of individual households and firms to price incentives had always been stressed. Thus by the time that Keynesian macroeconomics had reached full maturity, in the 1960s, it was widely agreed that the apparent inconsistency between the structure of macroeconomic models and the kind of economic theory used in other branches of applied economics posed a fundamental challenge to the coherence of modern economics. This problem was referred to as the need for “microfoundations for macroeconomics”.²¹

Macroeconomics also sometimes appeared to present a fundamentally different view of the market mechanism than that conveyed by mainstream microeconomic theory – a depiction of the economy as subject to arbitrary forces and in need of constant management through government policy to keep it on track, rather than a self-regulating mechanism leading spontaneously to an efficient allocation of resources. Keynes’ stress in the *General Theory* on the possibility of market failure, and indeed upon the idea that unemployed resources could exist as an “equilibrium” state, not spontaneously eliminated by the market mechanism, did a certain amount to further this impression. The sharp contrast that he chose to draw between his own system and previous orthodoxy, which he caricatured as “the classical economics”, did a good bit more.²²

The presentation of Keynesian economics as a radical challenge to conventional economic theory led some to conclude that at most one of the two could be true. Radical Keynesians, especially in Cambridge, England, hoped that the insights of the *General Theory* would provide the foundation for an entirely new economics; at the same time, some traditionalists maintained that Keynesian theory was fundamentally wrong. But the mainstream view, after the end of the Depression and especially in America, was what came to be called “the neoclassical synthesis”. The neoclassical synthesis, as developed by John R. Hicks and Paul A. Samuelson, among others, in the first decade after Keynes

²¹ See, e.g., Weintraub, *op. cit.*

²² As noted above, many themes and recommendations in the *General Theory* could also be found in the writings of his contemporaries, and many of these were quite resentful of Keynes’ characterization of “the classical economics”. See Laidler, *Fabricating the Keynesian Revolution, op. cit.*, chap. 11.

wrote, proposed that both the Keynesian theory and neoclassical general equilibrium theory could be viewed as correct, though partial accounts of economic reality. The conventional theory of general competitive equilibrium was argued to correctly explain the determination of prices and quantities in the long run, once wages and prices had had sufficient time to adjust to clear markets; thus the self-regulating property of the market system was not denied, but simply argued to sometimes be slow enough to profitably allow for interventions intended to speed the sort of adjustment that markets would ideally arrange on their own. At the same time, the Keynesian model was argued to explain the short-run effects of both disturbances to the economy and policy interventions, before prices and wages had much time to adjust.

The details of how one got from the Keynesian short run to the “classical” long run were not really worked out. Economists including Hicks, Samuelson and Oskar Lange made important early efforts at the development of dynamic versions of the Keynesian model,²³ but in this early work, the dynamics that were emphasized involved the adjustment of quantities over time in response to some initial disturbance, in a setting where wages and prices remained fixed, rather than the process by which wages and prices would eventually adjust. The integration of Keynesian short-run analysis with a Walrasian model of long-run equilibrium only began to be systematically addressed in work such as that of Don Patinkin’s in the 1950s,²⁴ and the problem remains central to the current research agenda in macroeconomics, as we shall discuss below.

Nonetheless, the “neoclassical synthesis” was not simply a pious profession of faith that the theoretical difficulties would be resolved eventually. Its redefinition of the scope of Keynesian analysis as relating purely to the period before wages and prices were able to adjust offered an important corrective to many of the more extreme views of Keynesian zealots. For example, one of the more shocking aspects of Keynesian doctrine for traditionalists was Keynes’ disparagement of personal thrift, as a drag on the economy due to the reduction of aggregate demand for produced goods and services. Once it was recognized that conventional neoclassical theory should still apply in the long run -- so that in the long run, incomes should depend upon productive capacity, productive capacity should depend upon capital formation, and capital formation should depend upon private savings -- it could be recognized that discouragement of saving would not generally increase national prosperity, while at the same time recognizing that promotion of savings during a depression (as some had done during the 1930s) would be a mistake.

²³ Hicks, *Value and Capital*, *op. cit.*; Samuelson, *op. cit.*; Oskar Lange, *Price Flexibility and Employment*, Bloomington: Principia Press, 1944.

²⁴ *Money, Interest and Prices: An Integration of Monetary and Value Theory*, New York: Harper and Row, 1957.

While the “neoclassical synthesis” allowed postwar Keynesians to maintain that there was no fundamental incompatibility between microeconomic and macroeconomic theory, the perceived incompleteness of the theoretical foundations of Keynesian economics continued to motivate important work of criticism and refinement. The 1950s saw several important analyses in terms of individual optimizing behavior of crucial elements of a Keynesian macroeconomic model. Examples of such work include the analyses by Franco Modigliani and Milton Friedman of the microfoundations of the Keynesian “consumption function” (relating consumption expenditure to household income), or the work of James Tobin on the demand for liquid assets, to mention only some of the work from the 1950s later cited by the Nobel committee.²⁵

This work was generally presented as a refinement of Keynesian models, rather than a fundamental challenge to such models; indeed, such work typically considered only the question of the proper form of a single equation from a Keynesian model, while tacitly accepting the overall structure within which such an equation would be used.²⁶ Thus while this work drew attention to the importance of new variables not emphasized by Keynes (for example, income expectations as opposed to current income, in Friedman’s work on the consumption function, or wealth and demographic variables in Modigliani’s), the overall impression in this period was one of cumulative progress within a relatively settled paradigm. The theoretical understanding of the individual schedules that made up a Keynesian macroeconomic model and the econometric estimation of quantitative models in this spirit proceeded to some extent along separate tracks, but as long as both programs seemed to be making good progress there was ample ground for faith that eventually the theory and the quantitative models would be shown to be consistent with one another.

The Great Inflation and the Crisis of Keynesian Economics

Most of the important new developments since 1960 have instead been associated with currents of thought that have been harshly critical of Keynesian macroeconomics, that was by this time a well-established orthodoxy. There have been three main waves of such criticism, each often described as another

²⁵ Franco Modigliani and Richard Brumberg, “Utility Analysis and the Consumption Function: An Interpretation of Cross-Section Data,” in Kenneth K. Kurihara, ed., *Post-Keynesian Economics*, New Brunswick: Rutgers University Press, 1954; Friedman, *A Theory of the Consumption Function*, Princeton: Princeton University Press, 1957; James Tobin, “Liquidity Preference as Behavior Towards Risk,” *Review of Economic Studies* 25: 65-86 (1957).

²⁶ Of course, whether such work appeared to represent mere refinements or a fundamental critique depended to some extent upon one’s attitude toward Keynes. For example, the NBER’s summary of Friedman’s study of the consumption function stated: “It is evident that Friedman abandons the conception of the consumer as a mechanical link between current income and consumption, a notion that Keynes set forth in 1936.... Friedman returns to the older theory of consumer behavior, in which consumers’ plans and decisions are influenced by the future as well as the present and the past.”

“revolution” or “counter-revolution” in macroeconomic thought. Each has been clearly anti-Keynesian in that it has expressed skepticism about the benefits of activist stabilization policy, while being correspondingly more optimistic than Keynesian thought about the self-regulating capacities of the market system. The influence of such currents has doubtless been greater as a result of a general tendency, in America and elsewhere, toward greater skepticism about the role of government during this period. At the same time, each of these waves of criticism has drawn much of its intellectual force from the perception that Keynesian macroeconomics was not well integrated with the main body of economic theory. Each has sought, though in somewhat different ways in each case, to reintegrate macroeconomics with the broader theory of market functioning, and each has made some claim, in so doing, to be returning to pre-Keynesian currents of thought about business fluctuations that had been unwisely suppressed during the dark days of Keynesian hegemony.

The increase in the harshness of the polemics directed against Keynesian economics, especially since the 1970s, was perhaps to some extent a simple reaction to the influence that Keynesian views had attained by the 1960s, not only within the academy, but among policymakers as well.²⁷ With the apparent triumph of the Keynesian vision of aggregate demand management with the aid of econometric models as a route to sustained prosperity came an obvious sense that the stakes were now higher in intellectual debates about the fundamental soundness of Keynesian ideas. But the sharpness of the debate -- and the degree to which even radical alternatives began to find sympathetic ears --- had a great deal as well to do with the appearance of a serious problem of public policy, which Keynesian policies had not prevented, and were even suspected to have caused. This was the sustained increase in the rate of inflation, in the U.S. as in many of the other developed economies, beginning in the late 1960s and continuing until the early 1980s.

The appearance of chronic inflation as an economic problem, following a couple of decades of relatively stable prices, and not long after Keynesian ideas about demand management began to be put into practice, highlighted a critical weakness of the somewhat oversimplified Keynesian model that was used in practical policy analyses. This was its relative neglect of the effects of demand stimulus upon the general level of prices. As noted earlier, Keynes’ crucial insight in the *General Theory* had been that it was possible to say many useful things about the short-run determinants of economic activity by treating wages and prices as given, rather than as adjusting so as to clear markets. But this meant that Keynesian models, in their most basic form, abstracted from wage

²⁷ By the late 1960s, Keynesian views on economic policy were accepted in the U.S., with only small qualifications, by Republicans and Democrats alike. This was made clearest when Richard Nixon, during his 1971 State of the Union Address, declared that “by spending as if we were *at* full employment, we will help to *bring about* full employment,” and in an interview afterward stated, “Now I am a Keynesian.” See Herbert Stein, *Presidential Economics*, New York: Simon and Schuster, 1985, pp. 172-173.

and price adjustment, and thus assumed away the problem of inflation as a possible consequence of excessive aggregate demand stimulus.

Discussions among policymakers were more sophisticated than these simple textbook models, of course, even in the early 1960s. But Keynesian policymakers argued that demand stimulus should not lead to inflation as long as output remained “below potential”, and devoted considerable effort to the construction of quantitative estimates of the economy’s level of potential output, as the basis for judgments about the extent to which further demand stimulus would be appropriate.²⁸ In retrospect, it now appears that those estimates were systematically over-optimistic for many years, and to a striking extent during much of the 1970s.²⁹ This lent an inflationary bias to policy during most of the period, insofar as the economy was perceived to have more slack in it than really existed, even as inflation accelerated.

To what extent can this failure be attributed to a flaw in the Keynesians’ conceptual framework, as opposed to a simple misreading of current conditions (perhaps honest, perhaps occasionally the result of political opportunism)? To an important extent the misjudgment resulted from changes in the economy, the occurrence of which did not invalidate the Keynesian model. For example, the “natural” rate of unemployment, the rate above which labor market tightness should not result in inflationary wage demands, is now believed to have increased significantly by the 1970s relative to what it had been a couple of decades earlier, as a result, among other factors, of changes in the demographic composition of the workforce.³⁰ This made estimates of the degree of excess capacity based on the unemployment rate, simply extrapolating from recent experience, overly optimistic. At the same time, the rate of growth of labor productivity is now recognized to have significantly declined in the 1970s, for reasons that are still largely mysterious; this too led to systematic over-estimation of the economy’s productive capacity.

The fact that such quantitative guidelines are unstable over time need not invalidate either the basic objectives or the basic conceptual framework behind Keynesian policies. However, it has tempered to a considerable extent the optimism of the 1960s about the extent to which available quantitative models

²⁸ In the U.S., official estimates of potential output were prepared by the Council of Economic Advisors, beginning in 1961 and continuing until 1981. Data on the gap between actual and potential output were part of the first presentation by President Kennedy’s Council before the Joint Economic Committee on March 6, 1961; the 1962 *Economic Report of the President* provided a comprehensive discussion. Arthur Okun, “Potential Output: Its Measure and Significance,” *Proceedings of the Business and Economic Section*, Washington: American Statistical Association, 1962, explained the methodology. See also Okun, *The Political Economy of Prosperity*, Washington: Brookings Institution, 1970, for a review of the principles under which U.S. macroeconomic policy was conducted during the 1960s.

²⁹ See Athanasios Orphanides, “The Quest for Prosperity without Inflation,” unpublished, Federal Reserve Board, May 1999.

³⁰ See, e.g., Robert Shimer, “Why is the U.S. Unemployment Rate So Much Lower?” *NBER Macroeconomics Annual* 13: 11-61 (1998).

would allow precise management of aggregate economic performance. This increased the receptivity of both economists and policymakers to views that questioned the whole enterprise of trying to use such models to conduct stabilization policy. Perhaps more importantly, the fact that the gap between actual and potential output cannot always be measured well “in real time” strengthened the case for greater attention to long-run price stability as a policy goal -- not because it is all that matters in principle, but because it is easier to be sure of the extent to which one is achieving it. But such a change in policy goals created a demand for models of the economy that could bring into sharper focus features that the Keynesian models of the 1960s tended to de-emphasize, if not abstract from altogether.

Most notably, a framework was needed that would clarify the links between macroeconomic policy and the eventual changes in the general price level that would result from it. Attention to this problem soon pointed up other weaknesses of Keynesian models, such as their neglect of the endogeneity of expectations and of the determinants of supply costs. Together with the lingering conceptual problem of the relation between macroeconomic and microeconomic theory, these issues provided the fuel for a series of fundamental critiques of Keynesian economics, that have often been described as “revolutions” or “counter-revolutions” in their own right.

Monetarism

The first of the fundamental criticisms of Keynesian theory was that of the “monetarist” school, associated in particular with Milton Friedman, Karl Brunner, Allan Meltzer and their students and collaborators, which achieved prominence in the 1960s. The monetarists criticized Keynesian theory, most famously, for its relative neglect of variations in the money supply as a determinant of aggregate spending in the economy, and as a related point, for excessive emphasis upon other sources of variations in spending and an exaggerated faith in the usefulness of fiscal policy.³¹

Denial of the effectiveness of monetary policy was hardly a fundamental tenet of Keynesianism. But Keynes had argued that monetary stimulus would have been of little help in getting out of the Depression, and some of the more dogmatic Keynesians sought to make of this a general principle, rather than a circumstance special to the Depression period (with interest rates on government

³¹ On the importance of the money supply, probably the seminal work was Milton Friedman and Anna J. Schwartz, *A Monetary History of the United States 1867-1960*, Princeton: Princeton University Press, 1963. On fiscal policy, see Milton Friedman, *Capitalism and Freedom*, Chicago: University of Chicago Press, 1962, chapter 5; Leonall C. Anderson and Jerry L. Jordan, “Monetary and Fiscal Actions: A Test of their Relative Importance in Economic Stabilization,” *Review*, Federal Reserve Bank of St. Louis, November 1968, pp. 11-23; and Milton Friedman and Walter W. Heller, *Monetary vs. Fiscal Policy: A Dialogue*, New York: W.W. Norton, 1969.

securities already very low, and banks reluctant to lend for reasons unrelated to scarcity of bank reserves). A larger number agreed that monetary policy should affect aggregate demand in principle, but felt that econometric estimates, which had found little statistical relation between interest rates and aggregate spending, implied that the effects were weak, and so less important than factors such as fiscal policy. The monetarists instead began from an insistence that the classical proposition of the neutrality of money had to hold at least in the long run, which meant that an increase in the money supply would eventually result in a proportional increase in prices. But this increase in prices would only come about through an increase in the money value of aggregate spending, and so changes in the money supply had to increase spending, even if it was hard to disentangle the exact mechanism through which this occurred. Monetarists offered their own statistical evidence that money-supply variations were associated with changes in the volume of spending, and proposed that there was clearer evidence that money mattered than that anything else did.

Perhaps more fundamentally, the monetarists criticized Keynesian theory for its exclusive emphasis upon the short-run consequences of government policies. As with much pre-Keynesian thought, monetarists instead gave considerable emphasis to the eventual effects of policies, once wages and prices have adjusted to their equilibrium levels, while not denying that effects were different during a transitory period of adjustment. This emphasis upon the long run was based not least upon the argument that economic theory gave clearer answers about what should happen eventually than about the process of adjustment through which one would get there. The monetarist concern with the long run meant in particular that they stressed the eventual inflationary consequences of sustained stimulus to aggregate demand, and the inability of government demand management policies to affect real incomes or employment other than in a transitory (and often unpredictable) way. This led to skepticism about “fine-tuning” approaches to demand management, and an emphasis instead upon maintaining a low rate of inflation over the longer run.³² As we have discussed, the appeal of such an emphasis greatly increased in the 1970s, with the perceived failure of demand-management policies to maintain high employment, while inflation accelerated to unacceptable levels.

Related to the monetarist interest in longer-run effects was a new emphasis upon the role of expectations in a number of crucial structural relations. Keynes had also recognized the importance of expectations, in various passages in the *General Theory*; but on the whole his emphasis upon the short run required him to adopt a mode of analysis in which the public’s expectations at a point in time were simply taken as given. Monetarist analyses, instead, considered the adjustment of expectations over time as one of the respects in

³² See, e.g., Milton Friedman, “The Role of Monetary Policy,” *American Economic Review* 58: 1-19 (1968).

which an economy should eventually adjust to the position of competitive equilibrium described by neoclassical theory; and so monetarists were among the first to incorporate explicit models of how expectations adjust in response to experience into macroeconomic analysis.³³

Probably the most important application of this idea was Milton Friedman's prediction, in his 1967 Presidential address to the American Economics Association, of the instability of the "Phillips curve".³⁴ The "Phillips curve" was a more sophisticated version of the notion, mentioned earlier, that the inflationary consequences of a given policy depended upon the gap between actual and "potential" output, which could in turn be inferred from the unemployment rate. Studies of historical data suggested the existence of a surprisingly stable inverse relation between the unemployment rate and the rate of wage and/or price inflation.³⁵ Keynesian economists of the 1960s interpreted this as a structural relation that could be relied upon in determining the inflationary impact of alternative policies. By linking real and nominal variables in a way that allowed for their simultaneous determination, this notion represented an important improvement upon simple models that allowed nominal spending to affect prices but not quantities (the "classical" view), or quantities but not prices (the basic Keynesian model), or quantities in the short run and prices in the long run, without explaining how one got from one to the other (the "neoclassical synthesis"). It was also an important advance, at least from the point of view of conceptual clarity, upon the simple doctrine that inflation should result from output in excess of potential, without distinguishing between greater or lesser degrees of excess demand, temporary and permanent increases in inflation, and so on.

In one respect, the use of such a model represented a step in the direction that monetarists advocated, in that the process of price adjustment was no longer ignored -- instead, it was recognized that demand stimulus to raise employment would always be associated with higher inflation (or at least less *deflation*). But the "Phillips curve" also justified a lesser degree of concern that policies would push output above the economy's potential, insofar as the predicted consequence of such overreaching was simply a little higher inflation -- and only temporary inflation at that, if the output target were subsequently to be brought into line

³³ For example, explicit quantitative models of income expectations and inflation expectations respectively are used in the empirical analyses of Friedman, *A Theory of the Consumption Function*, *op. cit.*, and Phillip Cagan, "The Monetary Dynamics of Hyperinflation," in Milton Friedman, ed., *Studies in the Quantity Theory of Money*, Chicago: University of Chicago Press, 1956.

³⁴ "The Role of Monetary Policy," *op. cit.* The same prediction was derived from a more explicit model of the endogenous adjustment of expectations in Edmund S. Phelps, "Phillips Curves, Expectations of Inflation and Optimal Unemployment over Time," *Economica* 34: 254-281 (1967).

³⁵ The relation was first observed by A.W. Phillips, "The Relation between Unemployment and the Rate of Change of Money Wage Rates in the United Kingdom, 1861-1957," *Economica* 25: 283-299 (1958).

with potential. The risk might well seem worth taking, even in the face of uncertainty about the economy's potential.³⁶

Friedman argued instead that it did make sense to regard the statistical "Phillips curve" as an invariant menu of unemployment and inflation combinations from among which one might choose, for this would imply that the neutrality of money would not hold *even in the long run*. He proposed that the short-run tradeoff between alternative levels of employment and inflation depended upon the rate of inflation that people had come to *expect*, and that the apparent stability of such a relation in historical data only reflected the relative stability of inflation expectations over a period in which prices had generally been quite stable. If, however, expansionary policies were to bring about constant inflation, inflationary expectations should eventually catch up to the actual rate of inflation, *shifting* the employment-inflation tradeoff so that a permanently higher rate of inflation would be associated with no higher employment than could have been obtained with stable prices. In the long run, the unemployment rate could not be anything other than what Friedman called its "natural rate", determined by the purely real factors of preferences and technology. Aggregate demand stimulus could instead result in a *permanent* increase in inflation, as the price of a purely transitory increase in employment; for once inflation expectations had been allowed to increase, the adverse shift in the employment-inflation tradeoff would mean that inflation could not be lowered below the level people had come to expect without a painful period of unemployment even *greater* than the natural rate.

The prediction that a period of sustained inflation should result in an adverse shift of the employment-inflation tradeoff was spectacularly (though tragically) confirmed by the events of the 1970s, when the higher inflation of that decade was accompanied by higher, rather than lower, unemployment rates. Similarly, the disinflation of the 1980s has shown that sustained lower rates of inflation result in an apparent shift of the employment-inflation tradeoff in the favorable direction.³⁷ The importance of taking account of endogenous expectations would thus seem to be confirmed. The episode is also an interesting instance of a prediction on the basis of economic *theory* turning out to have been more reliable than simple extrapolation from historical correlations. As such it has provided considerable impetus to work in the past quarter century that has sought to clarify the theoretical foundations, in terms of individual optimizing behavior, of structural relations such as the short-run tradeoff between employment and inflation.

³⁶ See, e.g., Franco Modigliani, "The Monetarist Controversy or, Should We Forsake Stabilization Policies?" *American Economic Review* 67: 1-19 (1977).

³⁷ This is in addition to the smaller shifts in the location of the short-run tradeoff due to shifts over time in the natural rate of unemployment itself, owing for example to the demographic factors mentioned earlier.

The monetarist critique of early postwar Keynesian views was highly influential, and on at least several central issues, the monetarist viewpoint had become the new orthodoxy by the mid-1970s. The importance of monetary policy was soon generally accepted, and nowadays the advocates of active stabilization policy almost invariably consider monetary policy, rather than fiscal policy, to be the instrument of choice; for it is widely accepted that fiscal measures are not suitable for accurate “fine-tuning”, even if it is not agreed that they have little effect. In addition, the monetarist emphasis upon the longer-run effects of policy clearly represented an important supplement to Keynesian analyses of shorter-run effects. And since the acceleration of inflation in most of the industrial countries during the 1970s, few economists or policymakers have doubted that the consequences of macroeconomic policy for inflation should be an important concern, if not the primary criterion for judging alternative policies. Finally, it has come to be generally accepted that one should expect the public’s expectations of inflation to catch up with the level of inflation actually caused by a given policy, and that this makes it difficult for monetary stimulus alone to sustain a higher level of economic activity or employment for too long a time.

Thus in at least these respects, the “monetarist counter-revolution” has been completely victorious. Yet its victory did not at all mean the disappearance of Keynesian economics. Rather, the Keynesian models of the 1970s came to incorporate the most important of the monetarist insights, thus achieving a new synthesis. Monetary policy could already be treated as one, if not the sole, determinant of aggregate demand within a Hicks’s “IS-LM” framework. And the “Phillips curve” could be adapted to be consistent with the “natural rate hypothesis” by simply postulating a relation between unemployment and the rate of inflation *relative to inflation expectations*, and adding an equation for the endogenous adjustment of expectations in response to experience.³⁸

Indeed, it is probably in this form that monetarism has had its most lasting impact. The more specific monetarist prescriptions, such as advocacy of monetary targeting as an approach to monetary policy, have not fared well in the light of experience. Practically no central banks currently make any important use of monetary aggregates as guides to policy, even though in the U.S., the chairman of the Federal Reserve System is still required, as a *pro forma* matter, to report twice a year to Congress upon the Fed’s projections for growth of the U.S. money supply, as a vestigial result of the monetarist victory codified in the Humphrey-Hawkins Act of 1978.

In matters of methodology, monetarist writings often appeared to offer a stark contrast with Keynesian work, in many ways recalling a pre-Keynesian style. Friedman and Schwartz plotted time series alongside one another and looked at the relative timing of “turning points”, using the non-statistical

³⁸ Such an equation system had already been written down in the 1967 article by Phelps, cited above.

empirical methodology of Wesley Mitchell's NBER, rather than estimating econometric models. Monetarist analysis was typically based upon single-equation models rather than models of the simultaneous determination of several variables, and monetarists preferred not to write down an explicit model of short-run equilibrium at all. However, these quaint aspects of monetarist methodology did little to impress younger scholars.³⁹ The methodological battle was instead won by the Keynesians, who were able to embellish their models in reasonably straightforward ways to incorporate monetarist insights -- simply adding additional variables to be simultaneously determined, and additional dynamic structure where necessary -- while retaining the Keynesian core, and an essentially Keynesian analysis of the short run. And while the models representing the new synthesis no longer implied that the maintenance of continual prosperity should be such a simple matter, they still left plenty of scope, at least in principle, for management of the economy's short-run response to shocks through enlightened monetary policy.

Rational Expectations and the “New Classical Economics”

The second wave of attack on Keynesian macroeconomics was associated with the introduction into macroeconomic theory of the concept of “rational expectations”, most notably by Robert E. Lucas, Jr. , Thomas J. Sargent, and their co-authors in the early 1970s. These authors stressed the role of expectations as a crucial element in many key structural relationships of macroeconomic models, and proposed that expectations be modeled, not by *any* specified function of past experience (as in earlier monetarist efforts), but instead by assuming that people's expectations coincide at all times with what one's economic model implies should occur (at least on average).

This way of modeling expectations represented a rather obvious extension (at least in retrospect) to the context of the intertemporal coordination of plans of the concept of *equilibrium* -- i.e., a state in which no one has any reason to act differently, given a *correct* understanding of the environment in which they act, as determined by the collective actions of others -- that is central to modern economic theory. The theory of rational expectations had already found important applications in the 1960s to models of agricultural cycles⁴⁰ and of

³⁹ The problem was well identified by Harry Johnson, a relatively neutral observer writing during the monetarists' heyday of academic influence: “The monetarist counter-revolution has served a useful scientific purpose, in challenging and disposing of a great deal of the intellectual nonsense that accumulates after a successful scientific revolution. But its own success is likely to be transitory, precisely because [among other mistakes, it has] espoused a methodology that has put it in conflict with long-run trends in the development of the subject” (Johnson, *op. cit.*, p. 106).

⁴⁰ The term “rational expectations” was in fact introduced by John F. Muth, “Rational Expectations and the Theory of Price Movements,” *Econometrica* 29: 315-335 (1961).

fluctuations in financial markets.⁴¹ Indeed, the use of this theory to predict that stock market prices should follow something close to a “random walk” -- a prediction that has been subjected to extensive statistical testing, and that has had profound consequences for practical investment management -- has perhaps been the most celebrated insight obtained using this theory. At around the same time that Lucas introduced the concept into macroeconomic theory,⁴² Roy Radner developed a general formulation of an “equilibrium of plans, prices and expectations” as an approach to modeling sequential trading within general equilibrium theory.⁴³ Thus this methodological innovation in macroeconomics had close ties to other developments in economics at the time.

In its macroeconomic application, this view of expectations had radical consequences, at least in the context of a simple model in which variations in aggregate spending were able to affect economic activity only by bidding up prices relative to what they had been expected to be. In this case, the new view implied that *no* government policy should be able to make prices systematically different than they are expected to be, and thus that aggregate demand management could not hope to stabilize the economy’s responses to shocks at all.⁴⁴ The proponents of this view called their theory a “New Classical” macroeconomics -- proclaiming their anti-Keynesian intent by embracing the label with which Keynes had caricatured his less radical precursors.⁴⁵

More generally, the “New Classical” macroeconomics argued for greater attention to providing explicit foundations in terms of individual choice for the structural relations assumed in macroeconomic models. As noted above, some attention had been given, especially in the 1950s, to the optimizing foundations of various key relations assumed in Keynesian models. But these theoretical derivations were used mainly to justify the existence of a causal relationship between certain variables (for example, to explain why the “consumption

⁴¹ Paul A. Samuelson, “Proof that Properly Anticipated Prices Fluctuate Randomly,” *Industrial Management Review* 6: 41-49 (1965).

⁴² “Expectations and the Neutrality of Money,” *Journal of Economic Theory* 4: 103-123 (1972).

⁴³ Roy Radner, “Existence of Equilibrium of Plans, Prices and Price Expectations in a Sequence of Markets,” *Econometrica* 40: 289-303 (1972).

⁴⁴ This “policy irrelevance proposition” was announced in Thomas J. Sargent and Neil Wallace, “Rational Expectations, the Optimal Monetary Instrument, and the Optimal Money Supply Rule,” *Journal of Political Economy* 83: 241-254 (1975). The “New Classical” challenge to Keynesian orthodoxy was most aggressively advanced in Lucas and Sargent, “After Keynesian Macroeconomics,” in *After the Phillips Curve: Persistence of High Inflation and High Unemployment*, Boston: Federal Reserve Bank of Boston, 1978.

⁴⁵ Keynes had admitted that “post-war economists seldom, indeed, succeed in maintaining this [classical] standpoint consistently; for their thought to-day is too much permeated with the contrary tendency and with facts of experience too obviously inconsistent with their former view. But,” he maintained, “they have not drawn sufficiently far-reaching consequences; and have not revised their fundamental theory.” Keynes, *op. cit.*, p. 20. The Chicago economist Frank H. Knight instead denounced Keynes’ references to the existence of a “classical economics” as “the sort of caricatures which are typically set up as straw men for purposes of attack in controversial writing.” From “Unemployment and Mr. Keynes’ Revolution in Economic Theory,” *Canadian Journal of Economics and Political Science* 3: 100-123 (1937).

function” should include financial wealth as an argument, and not just current disposable income), rather than to derive the complete *dynamic* specification of the relationship that would actually be estimated in an econometric model. Lucas and Sargent called instead for models in which all aspects of the model equations were derived from consistent foundations in terms of optimizing behavior. This naturally required an emphasis upon dynamic optimization, which made expectations crucial, and made it natural to assume that the model was also internally consistent in the sense of positing forecasts by agents within the model that agreed with what the model itself would predict.

The new style of model -- illustrated in particular by the celebrated, though highly stylized, business cycle model of Lucas -- essentially imported into macroeconomics the rigorous, tightly structured modeling style of modern intertemporal general equilibrium theory. One feature of standard general equilibrium models that was quite contrary to the spirit of Keynesian macroeconomics was the assumption of instantaneously clearing, perfectly competitive markets. The “New Classical” macroeconomists enthusiastically embraced this feature of general equilibrium models as well, and argued that such effects upon economic activity as were observed to result from monetary instability should be attributed to imperfect information about the aggregate state of the economy on the part of producers, and not to any failure of wages or prices to immediately adjust to clear markets. In this respect their models shared the monetarist emphasis upon the consequences of eventual price adjustment, but brought this adjustment even further into the foreground by asserting that *only* incomplete information prevented it from occurring immediately.

In addition to its critique of Keynesian theory, the “New Classical” macroeconomics also challenged the empirical foundations of the macroeconometric models used by Keynesians for quantitative policy evaluation. In another seminal article,⁴⁶ Lucas argued that such models failed to identify truly “structural” economic relationships – i.e., relations that could be expected to remain invariant despite a change in the way that economic policy is conducted – because of the way that people’s behavior depends upon their expectations regarding the future evolution of inflation, income, interest rates, and so on.

Keynesian macroeconometricians recognized that current and lagged variables appearing in their estimated equations often appeared entirely, or at least in part, as proxies for people’s forecasts of *future* values of those or other variables, which forecasts were, however, not directly observed. As long as the forecasts could be relied upon to evolve as mechanical functions of current and past variables that *were* observed by the econometrician, it mattered little

⁴⁶ “Econometric Policy Evaluation: A Critique,” *Carnegie-Rochester Conference Series on Public Policy* 1: 19-46 (1976).

whether the observed variables entered the equations as proxies for expectations, or had a genuine causal effect upon behavior themselves. However, the theory of “rational expectations” implied that the way that expectations vary with other observed variables should *change* whenever the patterns of correlation between those variables and the quantities being forecasted change, as they should if the systematic component of government policy were to change.

That this should occur, at least eventually, is reasonably clear in principle. The experience of the 1970s with the instability of econometric “Phillips curves” made it plausible that such shifts might occur quickly enough to matter for even medium-term policy analysis. Lucas charged that conventional policy-simulation exercises were fundamentally flawed on this account, and suggested in particular that the apparent possibility of significantly improved economic performance through “fine tuning” depended upon mistaken inference of this kind.

Like monetarism, the “New Classical” critique has had a profound effect upon macroeconomic practice, though this is perhaps clearer in the case of academic work than in the kind of economic analysis that is conducted inside policy-making institutions. The recognition that economic behavior is forward-looking in character, and thus that the effects of government policy *upon expectations* are critical to its effects overall, is fundamental to current ways of thinking about monetary and fiscal policy. For example, it is now widely accepted in discussions of central banking, within the banks as well as in the academy, that the way that policy is *perceived* by the public should be a critical concern of central bankers.⁴⁷

The “New Classical” literature has also had important consequences for the way that policymakers think about the tradeoff between employment and inflation. It is now widely accepted that it would be a mistake to seek to exploit the short-run “Phillips curve” tradeoff, without recognizing that a reputation for doing so will soon lead the public’s inflation expectations to change, in a way that is likely to eliminate the apparent gains achieved by the policy. To an important extent, this point had been made without the theory of “rational expectations”, as we have discussed above. But the “New Classical” literature identified a further pitfall, showing how discretionary optimization by a central bank seeking to exploit such a tradeoff leads to a suboptimal outcome (specifically, a bias toward too much inflation) *even when the central bank correctly understands the determination of private-sector expectations*.

This is because the central bank, in choosing the optimal degree to exploit the tradeoff at a given point in time, would rationally ignore the effect of its

⁴⁷ See Alan S. Blinder, *Central Banking in Theory and Practice*, Cambridge: M.I.T. Press, 1998, chap. 3, sec. 4, for a comparison of academics’ and central bankers’ understanding of the role of central bank “credibility”.

decision then upon private-sector inflation expectations *in the past*, which are at this point a simple historical fact. Yet the private sector, if it has rational expectations, expects the central bank to act in the way that it does, and the expectation of inflationary behavior shifts the short-run tradeoff in an adverse direction. A better outcome could be achieved if the central bank could determine to resist the temptation to inflate, even though past expectations are simply a given *ex post*; for if it could make its commitment credible to the private sector, inflationary expectations would be lower, allowing a lower rate of inflation to be consistent with unemployment at the natural rate.⁴⁸

This theoretical analysis of the possibility of a failure of coordination between the private sector and the government has seemed to many to help explain the trap in which policymakers found themselves in the 1970s. The model implies that avoiding the trap is not a matter simply of trying hard never to let inflation get started, as one would conclude if inflation expectations were simply a mechanical function of past inflation. It implies that it is important not to allow the impression to develop that policymakers would be *willing* to exploit the private sector's expectations of low inflation by choosing to stimulate the economy, taking advantage of a temporary opportunity to obtain high employment without much inflation (though higher than people had expected).

In practice, this has meant that central bankers have come to see the importance of stressing their commitment to control of inflation as the primary goal of monetary policy, regardless of the existence of a short-run tradeoff between inflation and economic activity.⁴⁹ Indeed, in the 1990s, a number of central banks have been willing to accept responsibility for the achievement of explicit inflation targets.⁵⁰

The assumption of "rational expectations" as a modelling device is also now entirely orthodox. And macroeconomic models with explicit foundations in intertemporal optimizing behavior are now generally accepted as the ideal, at least in principle, though the models used in central banks and elsewhere for practical policy analysis must generally compromise the ideal to some extent. However, this does not mean that the specific "New Classical" model of the business cycle, or its nihilistic implications for the possibility of stabilization policy, are now generally accepted. For the original Lucas model of the business cycle proved to be *too* successful at explaining why business fluctuations should not be much of a problem; under plausible assumptions about the speed with which information circulates in a modern economy, the model could not explain

⁴⁸ The classic source of this analysis is Finn E. Kydland and Edward C. Prescott, "Rules Rather than Discretion: The Inconsistency of Optimal Plans," *Journal of Political Economy* 85: 437-491 (1977).

⁴⁹ See Blinder, *op. cit.*, chap. 2, sec. 4. Blinder offers himself as an example of at least one central banker whose approach to his task was consciously affected by the theoretical analysis of the inflationary bias in discretionary policymaking (p. 43).

⁵⁰ See Ben S. Bernanke, Thomas Laubach, Frederic S. Mishkin, and Adam S. Posen, *Inflation Targeting: Lessons from the International Experience*, Princeton: Princeton University Press, 1999.

why monetary disturbances should have any significant effects (and above all, any *persistent* effects) at all. But this meant that it could not explain the character of observed fluctuations in business activity. And more to the point, it could not explain the effects of specifically monetary disturbances that sophisticated recent studies, carefully controlling for possible reverse causality from economic activity to the money supply, continue to find.⁵¹

Instead, it has proven to be possible to incorporate rational expectations -- and indeed, intertemporal optimizing behavior -- into models of nominal wage and price rigidity.⁵² The “New Keynesian” models that one obtains in this way are still recognizably Keynesian in their allowance for prolonged departures of economic activity from its optimal level as a consequence of instability in aggregate spending. And they still allow, in principle, for skillfully conducted active stabilization policy to improve upon what would occur under an arbitrarily chosen passive rule -- though the policy recommendations that they yield are importantly different as a result of taking account of rational expectations. These models, developed since the late 1970s, have been much more successful than the earlier “New Classical” models at accounting for the statistical properties of actual economic time series, and estimated versions of such models are now used for policy analysis in a number of central banks.⁵³

Real Business Cycle Theory

The third and most recent wave of attack on Keynesian macroeconomics has been the “real business cycle theory” developed in the 1980s by Finn Kydland, Edward Prescott, Charles Plosser, and their students and collaborators.⁵⁴ This theory represented a radical departure from most previous speculation about business fluctuations -- including that of most of the pre-Keynesian business-cycle theorists -- in that it proposed that business cycles did not indicate any failure of the market mechanism at all (whether inherent or due

⁵¹ For a recent survey, see Lawrence J. Christiano, Martin Eichenbaum, and Charles L. Evans, “Monetary Policy Shocks: What have we Learned and to What End?” in John B. Taylor and Michael Woodford, eds., *Handbook of Macroeconomics*, Amsterdam: North-Holland, forthcoming.

⁵² For a survey of this literature, see John B. Taylor, “Staggered Price and Wage Setting in Macroeconomics,” in Taylor and Woodford, *op. cit.*

⁵³ For example, on the role of rational expectations and intertemporal optimization in the U.S. model currently used at the Federal Reserve Board, see Flint Brayton, Andrew Levin, Ralph Tryon, and John Williams, “The Evolution of Macro Models at the Federal Reserve Board,” *Carnegie-Rochester Conference Series on Public Policy* 47: 43-81 (1997); and Flint Brayton, Eileen Mausekopf, David Reifschneider, Peter Tinsley, and John Williams, “The Role of Expectations in the FRB/US Macroeconomic Model,” *Federal Reserve Bulletin* 83(4): 227-245 (1997). For an example of policy analysis using the model, see John C. Williams, “Simple Rules for Monetary Policy,” Finance and Economics Discussion Series no. 1999-12, Federal Reserve Board, February 1999.

⁵⁴ Classic references include Kydland and Prescott, “Time to Build and Aggregate Fluctuations,” *Econometrica* 50: 1345-1370 (1982); and John Long and Charles Plosser, “Real Business Cycles,” *Journal of Political Economy* 91: 39-69 (1983). For a sympathetic review of the current state of the literature, see Robert G. King and Sergio Rebelo, “Resuscitating Real Business Cycles,” in Taylor and Woodford, *op. cit.*

to government meddling), but were actually an *efficient* response to exogenous variations over time in production opportunities. It also differed in assigning no importance whatsoever to monetary or financial factors, or to any nominal variables, in explaining business fluctuations (from whence the reference to “real” business cycles).⁵⁵ Indeed, real business cycle models implied that monetary policy has essentially *no effect* upon the economy, either for good or for ill; they thus proposed that the “classical dichotomy” of nineteenth-century monetary theory holds even in the short run.⁵⁶

The attention that these proposals have received is not due solely to the premium placed upon originality in academic life. It had been realized by about 1980 that Lucas’ proposal to reconcile a view of business cycles as due to monetary instability with rational expectations, individual optimization, and instantaneous market clearing depended upon assumptions that were plainly implausible. Thus if one wished to adhere to Lucas’ methodological strictures (in their strong form, including rejection of non-market-clearing models as lacking a motivation in terms of individual rationality), and nonetheless explain business fluctuations, it was necessary to find another source for them.

In addition, the hypothesis of “technology shocks”, while unconventional, offered a simple explanation for well-established business-cycle phenomena that had long taxed the ingenuity of business-cycle theorists who sought to explain them under the assumption that technological possibilities changed only slowly. These included the failure of real wages to move counter-cyclically (i.e., to decrease when economic activity is high) and the fact that productivity measures are strongly procyclical (i.e., increase with economic activity). Finally, the economic effects of the two large OPEC “oil shocks” of the 1970s made economists more receptive to the hypothesis of real disturbances to the costs of production as an importance source of economic instability (even if the RBC models weren’t really about oil shocks, and couldn’t explain why they had so much of an effect).⁵⁷

The real business cycle literature also offered a new methodology, both for theoretical analysis and for empirical testing. It demonstrated how complete

⁵⁵ There were, however, a few precursors of such an analysis, for example, Dennis Robertson, *A Study of Industrial Fluctuation*, London: King and Son, 1915. Robertson not only attributed a primary causal role to variations in technical progress, but argued that at least some of the resulting variations in economic activity represented “appropriate fluctuations”. His 1915 work also completely ignored monetary factors, though this was not at all true of his more mature exposition of his theory, *Banking Policy and the Price Level*, London: Macmillan, 1926.

⁵⁶ The correlations between changes in the money supply and the business cycle emphasized by monetarists were explained, by at least some real business cycle theorists, as due to reverse causation, reviving an argument that Keynesians had once made against the monetarist evidence. See, e.g., Robert G. King and Charles I. Plosser, “Money, Credit and Prices in a Real Business Cycle Economy,” *American Economic Review* 74: 363-380 (1984). As noted above, most of the recent literature rejects this interpretation.

⁵⁷ These events also led to increased attention to real disturbances and the supply factors among other macroeconomists during the 1980s; see, e.g., Michael Bruno and Jeffrey Sachs, *Economics of Worldwide Stagflation*, Cambridge: Harvard University Press, 1985.

business-cycle models could be built up using the intertemporal general-equilibrium methodology that Lucas had advocated, but that only a few of the “New Classical” papers of the 1970s had actually been able to apply. More importantly, it showed how such models could be made *quantitative*, emphasizing the assignment of realistic numerical parameter values and the computation of numerical solutions to the equations of the model, rather than being content with merely qualitative conclusions derived from more general assumptions.

The “equilibrium business cycle models” of Lucas had really only been parables; they could not be regarded as literal descriptions of an economy, even allowing for the sort of idealization that all models of reality involve. Thus, for example, in the Lucas’ classic 1972 business cycle model, the economy is made of successive “generations” of agents who each participate in the economy for two successive “periods”, which might thus be interpreted (if one thinks the human life-cycle is being represented) as each lasting for decades. But in the model, a “period” is also the length of time between when money is obtained from a sale and the first opportunity that the recipient has to spend it (so that it ought to be very short), it is the reciprocal of “the velocity of money” (i.e., the length of time over which the value of the national product equals the money supply, or a couple of months), and it is the length of time that it takes for a change in the money supply to become public knowledge (perhaps a month). Given this, it is hard to evaluate the model’s “quantitative” prediction that business cycles should last for only “one period”. The predicted variations in business activity are probably much too short-lived to match the evidence, but it is hard to say exactly *how* short-lived the model says that they are.

Real business cycle models are instead quantitative models, that are intended to be taken seriously as literal depictions of the economy, even if many details are abstracted from. The literature emphasizes the numerical predictions of the models, when parameter values are assigned on the basis of measurement of the relevant aspects of an actual economy. This “calibration” procedure draws upon other sources of information about realistic parameter values, rather than simply asking which parameter values would result in the best predictions with respect to the character of cyclical fluctuations.⁵⁸

Indeed, one of the great strengths of the approach is the fact that, exactly *because* the model’s structural relations are derived from foundations in terms of individual behavior, the structural parameters have meaning apart from their role in predicting certain kinds of short-run responses to disturbances. Hence other sources of information about the structure of the economy *can* be used to judge whether parameters are realistic or not. How conscientiously such questions of microeconomic realism have been addressed by the real business

⁵⁸ This methodological aspect of the RBC literature is highlighted in Robert G. King, “Quantitative Theory and Econometrics,” *Economic Quarterly*, Federal Reserve Bank of Richmond, 81: 53-105 (1995).

cycle theorists in practice is a matter of some debate;⁵⁹ but the attempt is surely a step forward. Given the complexity of the simultaneous interactions that are necessarily involved in macroeconomic models, and the scarcity of true “natural experiments” in macroeconomics (because the entire economy is involved in the phenomena that one wishes to understand), the discipline imposed upon business-cycle modeling by a requirement of microeconomic realism is essential. Here, if nowhere else in economics, a positivistic insistence that a theory should only be judged upon the success of its predictions, rather than upon the realism of the model’s assumptions, seems unwise.

Finally, the RBC literature has emphasized tests of the models’ ability to explain not merely qualitative features of observed business fluctuations, but statistical properties of observed aggregate time series, notably the relative volatilities and degree of co-movement among various aggregate series. While statistical descriptions of the data have been emphasized, the methods used to compare these with model predictions have been quite informal, by comparison with the canons of evaluation of model adequacy within the prior Keynesian macroeconometric tradition. The rejection of traditional econometric methods by the early RBC literature has surely been overdone; a consideration of the way in which sampling error affects the degree of confidence one can place in any putative rejection of the predictions of a model, for example, must be a critical part of any serious model evaluation. Nonetheless, more informal sorts of model evaluation are appropriate to the earlier stages in the development of a successful theory, and the vigor with which this literature has pursued the investigation of the ways in which alterations of model structure affect a battery of quantitative predictions should be applauded.

It is perhaps too early to reach a conclusive judgment about the ultimate impact of this work. It seems probable, however, that many of the methodological innovations of the RBC literature will have a lasting effect. They are already widely adopted in analyses that depart from the substantive assumptions of real business cycle theory, by introducing market imperfections of various sorts (with the implication that equilibrium fluctuations in economic activity and employment may be significantly *suboptimal*), and by assuming other important sources of disturbances to the economy (or, in some cases, even showing how fluctuations in economic activity may occur spontaneously).⁶⁰

The RBC literature has probably also had an important effect in directing greater attention to modelling the determinants of *supply* decisions, an issue that is surely of considerable importance for macroeconomic dynamics in all but the

⁵⁹ See, e.g., Lawrence J. Summers, “Some Skeptical Observations on Real Business Cycle Theory,” *Quarterly Review*, Federal Reserve Bank of Minneapolis, Fall 1986, pp. 22-27; and Martin Browning, Lars Peter Hansen, and James J. Heckman, “Micro Data and General Equilibrium Models,” in Taylor and Woodford, *op. cit.*

⁶⁰ Several early examples are provided by chapters in Thomas F. Cooley, ed., *Frontiers of Business Cycle Research*, Princeton: Princeton University Press, 1995; and in Pierre-Yves Henin, ed., *Advances in Business Cycle Research*, Berlin: Springer-Verlag, 1995.

shortest run. Keynesian models of the 1940s and 1950s often ignored the supply side altogether (assuming that supply would be perfectly elastic in response to whatever variations in demand might occur), but many of the most important developments since then have involved putting the determinants of aggregate supply back into the picture.⁶¹ The monetarist and New Classical schools both brought about important improvements in the analysis of aggregate supply. But both were still concerned primarily with how price changes (and inflationary expectations) were related to transitory departures of the *actual* quantity supplied from the quantity that the economy ideally *would* supply if wages and prices were flexible and expectations were fulfilled; this latter, ideal supply (the economy's "potential output") still tended not to be modeled but simply taken as a given. This was possible because these authors still accepted the Keynesian presupposition that business fluctuations were primarily about deviations of actual output from the economy's potential.

Real business cycle theory, instead, because of its radical assumption that actual output is always *exactly equal* to potential, has directed considerable resources to modeling the determinants of variations in potential output. Whether or not one accepts the view that there are no important deviations from potential, an improved understanding of the determinants of potential output is an important advance. It is widely agreed that many of the important policy issues of our time, such as the persistent high levels of unemployment in western Europe, have more to do with the real determinants of potential output than with any insufficiency of nominal spending -- even if the models of potential output that are needed to understand them involve market failures not allowed for in first-generation RBC models.⁶²

A New Neoclassical Synthesis?

Even for issues relating to shorter-run stabilization policy, an understanding of the way in which economic disturbances, and possible policy interventions, affect supply as well as demand allows for a more sophisticated analysis of what government policy can hope to accomplish.

Indeed, the 1990s have seen the development of what has been called a "new neoclassical synthesis", which unites important elements of real business

⁶¹ The rhetorical banner under which the "supply-side economists" of the Reagan administration marched thus had at least a superficial connection to important intellectual developments. However, the resurgence of emphasis upon the supply side among academic macroeconomists has had little to do with the belief that tax cuts are a painless route to prosperity. Indeed, it has had more to do with a sober acceptance of limits to the extent to which sustained prosperity can be achieved by a mere re-shuffling of financial claims.

⁶² See, e.g., Richard Layard, Stephen Nickell, and Richard Jackman, *Unemployment: Macroeconomic Performance and the Labor Market*, Oxford: Oxford University Press, 1991; Assar Lindbeck, *Unemployment and Macroeconomics*, Cambridge: M.I.T. Press, 1993; and Edmund S. Phelps, *Structural Slumps: The Modern Equilibrium Theory of Unemployment, Interest and Assets*, Cambridge: Harvard University Press, 1994.

cycle theory with other elements of the “new Keynesian” models of the 1980s.⁶³ Like the neoclassical synthesis of Hicks, Samuelson and Patinkin, the new literature seeks to bridge the methodological divide between microeconomics and macroeconomics, by using the tools of general equilibrium theory to model Keynesian insights. Today, this means using intertemporal general equilibrium analysis to model the complete dynamics of the macroeconomy – just as is done in modern theories of financial markets, industry structure, and so on -- rather than simply using a static general-equilibrium model to describe the long-run position toward which the economy should tend asymptotically. In practice, this means that the methodology of the new synthesis is largely that of the real business cycle literature, even though wage and price rigidities are allowed for, and the determinants of (individually) optimal wage and price-setting decisions are modeled in detail.

The new synthesis also proposes to give both “Keynesian” and “classical” insights their due by assigning each a distinct role within a complete model. The division of labor is no longer one in which Keynesian theory explains the short run while general equilibrium theory explains the long run. Rather, the factors stressed in real business cycle theory explain the evolution over time of *potential* output, while transitory *deviations* from potential result from delays in the adjustment of wages and prices. Furthermore, the process of wage and price adjustment is explicitly modeled, so that even in short-run analysis, wage and price changes can be taken into account, as can variations in potential output.

In such models, real disturbances may play an important role as the ultimate source of short-run variations in economic activity, contrary to monetarist and New Classical models alike, which attributed business fluctuations primarily to erratic monetary policy. As noted above, recent econometric studies of the effects of monetary policy disturbances tend to attribute only a small fraction of overall variability in economic activity to this source, and thus it may make sense to emphasize real disturbances instead, as RBC models do. But in the case of the new synthesis models, this no longer means either that the fluctuations in economic activity that occur are necessarily desirable, nor that monetary policy is irrelevant. Because of delays in wage and price adjustment, the consequences of real disturbances are usually inefficient, and their degree of inefficiency depends upon the response of monetary policy. As a result there is a role for active monetary policy, to mitigate the distortions that would otherwise result from the failure of wages and prices to adjust sufficiently in response to these real disturbances.

It thus seems likely that, just as in the case of earlier putative “counter-revolutions,” the ultimate impact of the RBC literature will be not the establishment of a fundamentally new view of macroeconomic phenomena, so much as an adjustment of emphasis, and an increase in methodological

⁶³ See, e.g., Marvin Goodfriend and Robert G. King, “The New Neoclassical Synthesis and the Role of Monetary Policy,” *NBER Macroeconomics Annual* 12:231-283 (1997).

sophistication, with a relatively continuous mainstream tradition. For the new synthesis shares important elements with Keynesian economics, and indeed with the Cambridge neoclassical tradition from which Keynes' ideas developed. These include an emphasis upon the inefficiency of short-run responses to economic disturbances, and upon the role of wage and price stickiness as a source of such inefficient responses. It also shares with postwar Keynesianism the aspiration of building models of the economy that can be matched to detailed properties of economic time series, and used for quantitative analysis of alternative policies.⁶⁴ This includes a return to the use of optimal control methods to compute optimal stabilization policies, once these methods have been suitably modified to take account of the forward-looking elements in the correctly-specified structural relations of a model founded upon optimizing private-sector behavior.⁶⁵

There is probably little point in insisting upon the specifically Keynesian character of the emerging theoretical synthesis.⁶⁶ As Robert Lucas has remarked, it is a sign of a successful scientific revolution that one does not see physicists still referring to themselves as "Einsteinian physicists" at the century's close. And as respects many of the issues most debated between the professed Keynesians and anti-Keynesians of a few decades ago -- such as the usefulness to macroeconomics of the paradigm of individual optimization and equilibrium analysis, the relative efficacy of fiscal and monetary policy, or the question whether demand stimulus has any consequences for inflation in circumstances of less than full employment --- the modern literature sides with the critics of Keynesianism. Perhaps more to the point would be the observation that the zeal to thoroughly overturn Keynesian economics and start afresh has been a frequent source of strident manifestos that have been no more helpful to ultimate progress in the scientific understanding of aggregate fluctuations than were the excesses of Keynesian fundamentalism at mid-century. Just as in the case of the

⁶⁴ See, e.g., Robert G. King and Mark W. Watson, "Money, Prices, Interest Rates and the Business Cycle," *Review of Economics and Statistics* 78: 35-53 (1996); Julio J. Rotemberg and Michael Woodford, "An Optimization-Based Econometric Framework for the Evaluation of Monetary Policy," *NBER Macroeconomics Annual* 12: 297-346 (1997); and Peter N. Ireland, "A Small, Structural, Quarterly Model for Monetary Policy Evaluation," *Carnegie-Rochester Conference Series on Public Policy* 47: 83-108 (1997).

⁶⁵ See, e.g., Michael Woodford, "Optimal Monetary Policy Inertia," NBER working paper, forthcoming 1999.

⁶⁶ Miles Kimball has called the new style of model "neomonetarist," and Robert G. King and Alexander L. Wolman have also stressed the kinship between new synthesis models and some aspects of monetarist models of the 1960s. Kimball, "The Quantitative Analytics of the Basic Neomonetarist Model," *Journal of Money, Credit and Banking* 27: 1241-1277 (1995); King and Wolman, "Inflation Targeting in a St. Louis Model of the 21st Century," *Review*, Federal Reserve Bank of St. Louis, 78: 83-107 (1996). The primary sense in which the models are monetarist is that they present departures from the level of potential output determined by real factors as purely transitory, and give attention to the adjustment of prices and inflation expectations as the process through which actual output adjusts toward potential. On the other hand, they need not imply any special importance for the money supply, either as a determinant of aggregate spending or as the proper instrument of monetary policy. See, e.g., Rotemberg and Woodford, *op. cit.*

Keynesian revolution, the true significance of the fundamental advances later in the century has not always been best grasped by the most ardent proponents of revolutionary change.

Finally, it is appropriate to reflect upon the prospects for resolution in the new century of the schism between the “micro” and “macro” branches of economics, that has so characterized the pedagogy of the past several decades. In principle, the grounds for reunification of the subject would seem to be largely in place. Macroeconomics no longer claims that the study of aggregate phenomena requires a distinct methodology; instead, modern macroeconomic models are intertemporal general equilibrium models, derived from the same foundations of optimizing behavior on the part of households and firms as are employed in other branches of economics. Furthermore, the aims of stabilization policy can now be discussed in terms -- namely, the attempt to mitigate quantifiable efficiency losses resulting from identifiable distortions of the market mechanism -- that correspond to those used for policy evaluation by microeconomists.

At the same time, as far as the sociology of the discipline is concerned, the split between micro and macroeconomists has probably grown sharper in recent decades, despite the methodological convergence. The primary reason for this is probably the increasing specialization of economic research, as in other fields. As recently as the 1960s, it was still quite commonly the case that leading economists contributed to the analysis of both microeconomic and macroeconomic issues, and felt qualified to teach both kinds of courses. This has become quite uncommon. Furthermore, the branches of various specializations within economics that concern themselves with microeconomic and macroeconomic issues have come to have less frequent contact with one another. In Princeton’s doctoral program, the former field of specialization in “international economics” has only recently been split into two distinct fields (“international trade” and “international finance”), corresponding to the microeconomic and macroeconomic aspects of international economics. Applied econometricians can now usually be clearly identified as studying econometric issues that arise in empirical microeconomics, or that arise in empirical macroeconomics, but not both. In other specializations, such as labor economics, development economics, and public finance, a similar split has failed to develop mainly because, in many American departments, the macroeconomic aspects of these fields have quietly been dropped from the curriculum as these specializations have come to be dominated by microeconomists.

It is not clear that there is anything to be done about this development, as ever-increasing specialization appears to be a fact of life at our research universities. Yet the trend has unappealing consequences, most notably the atrophy of fields of study such as development macroeconomics and “macro labor”, which would still seem crucial fields from the point of view of their importance for the improvement of public policy. The disappearance of these

fields as subjects of academic inquiry has been unfortunate, both because it means less scholarly attention to these policy issues, and because it has reduced the richness of the body of institutional knowledge upon which general macroeconomists can draw. It would be desirable if the emergence of methodological consensus in macroeconomics were to make possible, in the new century, a revival of the links between general macroeconomics and these more specialized fields of inquiry.