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THE DEVELOPMENT OF
KEYNESIAN MACROECONOMICS

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ABSTRACT

This paper provides an outline of the historical development of Keynesian macroeconomics. It first argues that the business-cycle model of J.M. Keynes's General Theory featured analytical ingredients that were present in earlier writings and attained its theoretical precision only in contributions made later. Remaining sections of the paper focus on the key characteristic of Keynesian theory, namely, a postulated stickiness of nominal prices that enables aggregate demand to play a greater role in output determination than it does in flexible-price classical analysis. Three approaches that have been historically important are ones relying upon (i) equilibria conditional on given prices, (ii) algebraic Phillips-type price adjustment relations, and (iii) equilibrium analysis with incomplete information. The paper reviews difficulties with each of these and concludes with a discussion of relevant issues of today.

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In this paper I will address the topic of this session by outlining the historical development of Keynesian macroeconomics and adding a few remarks on the issues of today. My version of the story will agree with textbook accounts in some ways and differ fairly sharply in others. Very few references will be provided in support of assertions both because space is tightly limited and because my version is something of a "stylized history of thought." The hope is that it will be, like carefully selected "stylized facts," analytically illuminating although lacking in detail.

I. John Maynard Keynes

During the past 20 years, there has grown up a body of literature that promotes the notion that Keynes's own theorizing was vastly superior to that of the "Keynesian" variety that typified mainstream macroeconomic analysis in the 1950s and 1960s. In my opinion, the ranking implied by this literature is precisely the opposite of that which is warranted.

The foregoing contention is based on an evaluation of the contribution to business-cycle theory provided by Keynes's General Theory (GT). Any such evaluation must, it seems clear, be made in light of pre-existing theory. My own non-extensive reading of pre-GT writings has led me to the view that the main analytical ingredients of the GT were distinctly present in the writings of Alfred Marshall and his other students. In particular, Marshall (1887) and Frederick Lavington (1922) described the mechanism of cyclical fluctuations in a manner that (i) emphasized the sluggishness of nominal wage adjustments and

(ii) utilized multiplier effects to explain the magnitude of departures from normality.¹ Furthermore, the idea that these fluctuations were viewed as unimportant by the pre-GT Cambridge writers is soundly refuted by the introductory chapter of Lavington's little book (1922, pp. 9-12).

Of course the GT had an enormous influence in terms of introducing new concepts and terminology, posing new issues and puzzles, and generally redirecting economists' attention. In addition, the GT represented an ambitious attempt to bring the Marshallian building blocks together in the form of a detailed, rigorous, and comprehensive model that would be useful for aggregative analysis. But in this admirable attempt at formal theory, Keynes failed. His top-priority goal of articulating a model with an unemployment equilibrium--in the sense of a situation from which there is no tendency to depart--foundered on the Pigou-Patinkin real balance effect. And as a comprehensive analytical structure, the GT was plagued by various logical inconsistencies,² which were straightened out only in the more careful works of John Hicks (1937), Franco Modigliani (1944), and Don Patinkin (1956). These clarifications left the profession with the analytical structure that Keynes had evidently been seeking. But this structure owed its fundamental ideas to Marshall and other earlier writers, and its analytical precision to Hicks, Modigliani, and Patinkin.³

II. Keynesian Macroeconomics

The key characteristic of Keynesian macroeconomics that distinguishes it from Classical theory is a postulated stickiness

in some nominal price that enables its value to differ, for significant spans of time, from the level that would otherwise (i.e., in the absence of this friction) be market-clearing. Demand and supply quantities (defined in the absence of the friction) can differ, therefore, so fluctuations in nominal aggregate demand can be much more important for aggregate output and employment determination than under flexible-price Classical conditions.

Sluggishness of price adjustments is inherently a dynamic concept, but the refined Hicks-Modigliani-Patinkin version was, like the GT itself, expressed in the form of a static model. Consequently, the way in which price sluggishness had to be reflected was in the model's concept of a "short run" equilibrium. Formally, what this amounted to was a mode of analysis centering on equilibria of a conditional variety: the refined GT model was designed to determine values of endogenous variables conditional upon "given" values of specified prices (most often, nominal wages). This is, to reiterate, the way in which the hypothesis of slow price adjustments was expressed in a framework that was formally static.

Now the object of constructing the model was to provide analytical guidance for the design of macroeconomic policy. But actual economies are dynamic, not static, so some way had to be found to relate the model to reality. One possible way of proceeding would be to choose policy actions at any point in time (say, t) by treating the current value of the sticky price (say, W_t) as historically given and essentially ignoring the

future--since it can be attended to when it arrives. Then in period $t+1$ the wage W_{t+1} would be treated as historically given, and a new policy action chosen conditional upon its value. In this way it would be possible to use the model without ever developing any explanation for the economy's W_t values.

Of course, it is apparent that this way of proceeding would be highly undesirable. For even if W_t were actually given in t as a residue of the past, the particular value prevailing would certainly have been influenced by economic conditions of the past. The temporarily-fixed price in the Keynesian model is properly viewed as a predetermined variable, not an exogenous variable. So policy actions taken in t will have effects on future prices--on W_{t+1} , W_{t+2} , etc.--and those effects are ignored in the procedure under discussion. This point is worth mentioning in our history because the procedure is a stylized version of a common method of policy analysis as actually conducted in the 1950s and 1960s. Furthermore, the efforts of many distinguished theorists were devoted to the refinement of conditional equilibrium models as recently as the late 1970s.⁴

III. Phillips Curves

Many Keynesian analysts recognized the undesirability of conditional equilibrium analysis, of course, and adopted a different approach. Rather than treating W_t as coming out of the blue, this second approach was to add to a static Keynesian model another equation or sector designed to explain movements over time⁵ in the slowly-adjusting price W_t . Then the model would be dynamic, even if incompletely based on dynamic optimization

analysis, and could be used for policy experiments that would avoid the particular difficulty described above.

Equations or sectors of this type are versions of the famous Phillips Curve. As all readers know, most early formulations were severely flawed in the sense of positing adjustment procedures that involve dynamic money illusion. As Milton Friedman (1968) effectively noted, these versions carried the highly implausible implication that a society could permanently keep its real rate of output high--i.e., enrich itself in real terms--by continually printing paper money at a rapid pace. A more proper specification of the Phillips Curve, according to Friedman, would relate changes in expected real wages to prevailing levels of output relative to normal.

IV. Rational Expectations

Friedman's contribution improved matters considerably but not, according to Lucas (1981, pp. 90-95), enough. Suppose output relative to normal is systematically related to the unexpected rate of change of some nominal variable, as Friedman's reformulation would imply. Then output could still be kept high (relative to normal) permanently if actual inflation could be kept permanently above the rate expected. Such a possibility was, moreover, permitted by Friedman's model of expectational behavior, adaptive expectations. To rule out the implausible possibility of real enrichment by monetary means, Lucas suggested adoption of the hypothesis of rational expectations--i.e., the absence of any systematic relation between expectational errors and information available to agents at the time of expectation

formation. This hypothesis was also necessary, Lucas indicated (1981, p. 285), to avoid the implication of sub-optimal behavior by individuals.

In addition, and as importantly, Lucas (1981, pp. 66-89) proposed a new theory of price stickiness. Instead of some algebraic representation of price adjustments (executed by some unspecified agent) in response to excess demand, Lucas suggested a model based on information limitations faced by individual sellers. The crucially desirable feature of this new approach was its strategy of explaining incomplete price responses to monetary shocks--and thus non-zero quantity responses--in terms of choices made by optimizing agents in light of their own objectives and constraints. This strategy was adopted not for aesthetic reasons, but in order to produce a model that would be well-designed for the Keynesian objective of guiding macroeconomic policy. Such would not be possible with an algebraic price adjustment equation, for the latter would give the analyst no basis for knowing whether the relation would itself shift if policy were substantially altered--which is crucial because such a shift would invalidate his predictions about the effects of the policy change. It is necessary, according to this view, to understand the nature of price sluggishness to know if its quantitative manifestation will remain intact in the face of altered conditions.

V. Recent Developments

Lucas's approach gained much support during the late 1970s but today (i.e., December 29, 1986) matters are rather unsettled.

A major reason for this condition is that the specific informational specification proposed in Lucas's model--which requires agents to be devoid of knowledge concerning current monetary conditions--has come to be viewed as inapplicable to today's developed economies.⁶ And no other model has been devised that combines empirical accuracy with a price adjustment sector that is derived from individuals' objectives and constraints.

Consequently, there has been a splintering of opinion, with prominent researchers promoting widely divergent strategies. One small but significant group has embraced an ultraclassical "real business cycle" position, according to which aggregative output fluctuations are induced almost entirely by technology shocks, with money-output correlations occurring only because the monetary system responds to these fluctuations. Most macroeconomists are highly skeptical of this position; some of my own reasons are outlined in McCallum (1986).

A more sizeable group has reacted against the postulate that sluggish price adjustments need to be explained in terms of individuals' objectives and constraints. It is better, according to this view, to use a poorly understood but empirically substantiated price adjustment relation than to pretend--counterfactually--that all nominal adjustments take place promptly. One's econometric model will then track the data better and the adjustment relation will be unlikely to shift much or rapidly when policy changes are undertaken.

It is hard to keep from having considerable sympathy with

this view. But the logic of the "Lucas critique" objection is inescapable. One possible way out of the dilemma, perhaps, is to proceed with models incorporating price adjustment equations that can be rationalized by subsidiary arguments, even though these arguments cannot clearly find expression in terms of the model's explicit taste and technology representation. This is an interpretation that can perhaps be given to John Taylor's (1979) well-known formulation, though I believe some modifications would be appropriate.

A pervasive problem in devising well-rationalized models of price stickiness and monetary effects on real variables is that taste and technology analysis (even when augmented by monopoly, asymmetric information, and insurance considerations) typically proceeds entirely in real terms. Accordingly, any such models that rationalize the predetermination of prices do so, appearances notwithstanding, in terms of real (i.e., indexed) prices and therefore fail to explain the crucial phenomena. In an attempt to remedy this weakness, I have constructed an argument that justifies the possible reinterpretation of some models of this type in terms of nominal prices (McCallum, 1986). The basic idea is unimpressively simple: the benefits to an individual obtained from indexation are exceedingly small. Therefore, for small and non-ongoing transactions, the tiny computational costs of expressing prices in indexed form will outweigh the benefits. For such transactions, stickiness will then pertain to nominal prices.

This last argument is not entirely immune to the Lucas

critique: in a regime with more rapid inflation, the benefits from indexation may be greater. The implied model incorporates, in other words, a "rule of thumb" that would tend to be revised if placed under severe strain. But the argument does not abandon rationality as an essential ingredient. In this respect it differs from some more extreme suggestions that have recently been put forth by other writers in response to the dilemma noted above. I will conclude by briefly considering two of these other positions.

One, expressed most prominently by Akerlof and Yellen (1985), suggests that certain small departures from rational behavior on the part of individual agents will have very small effects on these individuals' utility levels. Yet if many individuals are engaged in these small departures, the aggregative consequences can be quite large. In my opinion, this argument is sensible but in one respect misstated. The point is that if the model used in the implicit definition of "rational behavior" neglects some small computational or adjustment costs, then the agents' choices hypothesized by Akerlof and Yellen may in fact be entirely rational. Under this interpretation, the argument becomes rather similar to the one given two paragraphs above.

The second example is the proposed abandonment of rational expectations. Here I would emphasize that to concentrate on the question "Are expectations rational?" is to miss the true issue. Of course there are empirical departures from the hypothesized orthogonality conditions, but can the same departures plausibly be relied upon to hold in the future? The answer is No. A

better way to proceed would be to suggest that recognition of adjustment and computational costs might lead to weaker formal representations of expectational rationality--e.g., that expectational errors have unconditional (but not conditional) means of zero. But such an approach would (again) not actually represent the abandonment of rationality. A true abandonment would, in my opinion, constitute suicide for the profession.

It is necessary to stop at this point. Some readers may feel that the foregoing is not actually a history of Keynesian macroeconomics, and I would have to agree that it neglects many interesting and significant matters. But I would strenuously argue that it outlines the main developments concerning the single most important aspect of Keynesian economics--or, perhaps, macroeconomics more generally.

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Footnotes

1. See Marshall (1887, p. 358) and Lavington (1922, pp. 48-51 and 81-86).
2. Numerous examples are detailed by Patinkin (1956) (1982).
3. Patinkin (1982) has emphasized the originality of Keynes's insight that, with fixed prices, output adjustments can provide an equilibrating mechanism. This argument holds in a clean form, however, only in a model in which interest rate adjustments are suppressed by the unsatisfactory device of treating investment as exogenous.
4. Here reference is to the surge of interest in so-called "disequilibrium" or "fixed-price" analysis. My claim is not that distinguished theorists actually embraced the policy procedure described, but that their writings could have easily been interpreted by policymakers as providing support for such a procedure.
5. Over actual time, not the meta-time of stability analysis such as Patinkin's (1956, pp. 152-158 and 342-351).
6. The specification is much more applicable to the economies that Keynes was concerned with in the 1920s and 1930s.