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Introduction

This essay deals with the writings of J. M. Keynes and their subsequent interpretation by economists. These writings have been shrouded in controversy since their publication over half a century ago. Following the publication of his General Theory this controversy took the shape of what Hicks termed the Mr Keynes and the Classics debate. Three decades ago, it appeared that the terms of the truce enunciated by the Neoclassical Synthesis had resolved the major issues of controversy. A new development in this dispute was ignited by the works of Clover and Leijonhufvud. What became known as the re-interpretation of Keynes opened many old war wounds and resulted in an avalanche of research and writings on the exact nature of Keynes contribution to economics.

This continuing controversy is the subject matter of the present paper. Throughout its long history this controversy has reigned on many fronts - cosmological, empirical, methodological, theoretical etc., and from many different viewpoints - historians, theorists, econometricians. As the following excerpt from Blaug highlights, the dispute has had, in many instances, an air of chaos resulting in many becoming despondent regarding the value of such discord:

"In retrospect, the Keynesian-Monetarist debate of the last two decades must rank as one of the most frustrating and irritating controversies in the entire history of economic thought, frequently resembling medieval disputations at their worst ... Much of the debate consists of talking at cross purposes and at times it is difficult to remember what it is that is actually in dispute, a difficulty that is even felt by the main protagonists themselves. Running right through the debate is a continuing quarrel about what Keynes actually meant, as if it were impossible to settle substantive issues of economic policy without first deciding how Keynes differed from 'the classics' ... Reading the debate, one sometimes gets the feeling that macroeconomists are more concerned with exegesis of the General Theory than with advancing knowledge of how the economy actually works." (\*1)

The conclusion to be drawn from the above is that the controversy surrounding the interpretation of Keynes' writings is futile and fruitless and it diverts attention from the 'real' issues of current macroeconomic policy-making. This view of the controversy results, I believe, from a certain conception of the relationship between history and current economic research and analysis. The polarisation of substantive issues and the historical exegetical question is clear in the above passage. I believe that the implicit hierarchy is of dubious validity i.e. the task of ascertaining 'what Keynes really said' is relegated to the task of the economic historian while the more substantive issues are reserved for the labours of the economic theorist/policy-maker. The dichotomization of theory and history reflects, in turn, a certain understanding of economics as a science, the nature of knowledge within that science and the manner in which knowledge accumulates and is communicated within the discipline of economics. In an essay which attempts to discuss the relevance of past controversies in economics, an examination of the above question is evidently essential.

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\*1 Blaug, M. (1980) Methodology of Economics, pp 221 - 222.

The attitude to the debate in the above quotation is one which views it as a destructive and diversionary activity. This is not an attitude I share and however understandable it may be, it fails to capture and explore a vital element of the debate. The fact that this irritating and frustrating debate has, in many instances, resembled a medieval disputation does not, to my mind, constitute a sufficient reason for ignoring or neglecting the controversy. What is needed is a fresh perspective on these old facts, a shift in the focus of attention. This essay will not concentrate on the historical-doctrinal question of Keynes, the focus is rather on the way in which the controversy surrounding his writings was conducted and structured. By examining the form rather than the substance of the controversy, by tracing its evolution over time, I hope to show that the Keynesian controversy is no mere sterile debate but one which raises questions of fundamental importance to economics as a science.

The suggestion in this essay is, therefore, that the Keynesian controversy is not simply an irrelevant chapter of bickering but one which, when viewed in the perspective outlined above, offers the possibility of revealing and illuminating many fundamental questions in economics. One's attitude towards this controversy reflects more one's view of economics as a science and thereby goes beyond the confines of the immediate question. An examination of the underlying assumptions of, and parameters within which, the debates took place offers the possibility of a self-critical and self-reflective analysis of the past. The essay is structured in accordance with these beliefs. Section 2 examines the relationship between history and theory in economics, asking whether the substance of past controversies is irrelevant for current economics. The remainder of the essay will deal with the 'format' which the Keynesian revolution has taken. Section 3 will examine the characterisation of Keynes in terms of a revolution. It is suggested that this is an incomplete and ultimately unhelpful way in which to interpret the controversy. Section 4 then deals with an alternative representation of the controversy, sketching possibly fruitful lines of analysis. Section 5 concludes the essay, summarising the main conclusions of each section.

## Section 2: History and Theory

Blaug's diametrical treatment of history and theory quoted earlier obscures the nature of the relationship between the two and thereby acts as an impediment to exploring that relationship. History is viewed as the realm of dead men's (erroneous) riders while the task of the contemporary theorist is with developing models applicable to the real world. The belief that there is no analytical justification for further exegesis of Keynes' work and that the only reason for such exegesis is a concern that the relative merits of analytical discovery be properly apportioned between economists, is not peculiar to Blaug. Hutchison concludes his paper with:

"Our main and primary concern in this paper has been with the history of economic thought, that is, with contributing to the formation of a less inaccurate record, which is an important task from the point of view of intellectual standards, and also one not devoid of practical and political significance." (\*1)

This simply confirms, as Cesarano notes, that:

"It is a widely held opinion among economists that the history of the

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\*1 T. W. Hutchinson (1977) Keynes v the 'Keynesian' p. 46.

subject is of little relevance to the present day scholar. This opinion is reflected in the sharp decline in interest in the history of economics."(\*1)

The cosmological foundation of this neglect of history is open to question. The status of economics as a science is what is in question - is it a 'hard' or a 'soft' science? The neglect of history is clearly related to a movement to view economics as a hard science, on a par with the physical sciences. The quantification revolution (for want of a more appropriate title) in economics is, no doubt, partly responsible for such a viewpoint; its ascendancy may be correlated with the demise of history. An increasingly sophisticated methodological apparatus gives a false impression of a broadening gap between past and present works in economics.

The most important aspect of such a view of science, in the context of the present argument, is the understanding of knowledge relevant to the science and the manner in which it accumulates:

"... Implicit in the use of these methods is the positivistic and/or naturalistic view that since no logical distinction exists between the methodologies of the physical and social sciences, economics is a cumulatively progressive science in the same way, as, say, physics. By way of analogy then, an economist need not devote his time to outworn theories of the past any more than his counterpart in the physical sciences."(\*2)

The belief that progress in economic theory is cumulative has been embraced rather naively and overenthusiastically by some economists. Such a belief forms the basis of a theory/history dichotomy. As Cesarano points out, the whole concept of cumulativity is ambiguous and its existence within economics dubious:

"Hence, the awkward question of the existence of convergence towards a 'true' theory can be neglected, since what is at stake is the shape of the Path of Scientific Development (\*3) and not the presence of a link between convergence (i.e. increase in knowledge) and increase in 'truth content'. A sufficient condition for the usefulness of the history of a subject is the failure to comply with the requirements of a non-decreasing monotonic development path."(\*4)

The issue of the importance of history therefore relates directly to the existence and shape of a PSD in economics. The shape of a PSD will evidently vary according to the field, or specialized sub-field, of study. Only in a situation where the discipline displays a non-decreasing monotonic PSD does the past bear no relevance for the present. Whether or not this is so in economics is a controversial and empirical question which I am not in a position to address in the present context. Three things may

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\*1 Cesarano F. HOPE 15:1, 1983 'On the role of the history of economic analysis'.

\*2 Tarascio, quoted by Cesarno, loc. cit., p 64).

\*3 PSD:  
Path of scientific development. He says that this may be thought of as a time function of the variable 'stock of accumulated knowledge'.

\*4 Cesarano, p. 68.

be noted, however. First, a cursory glance at the history of monetary economics reveals the oscillatory nature of economic doctrines which suggests that old ideas die a slow death in economics. Second, since the hypothesis of a non decreasing monotonic PSD for economics appears to be a rather extreme and stringent one, the burden of proof may be said to rest with those who hold it. Finally, in section 4 these ideas will be dealt with again, once more suggesting that the history of economics, given the particular characteristics of the discipline, is not irrelevant to contemporary economics.

### Section 3: A Keynesian Revolution

The previous section has established a prima facie case for the relevance of history within economics. This section develops this argument along the lines that not only is the substance of past theories of importance, the form of those theories are also relevant. Furthermore, it is argued that an examination of the manner in which theories are encapsulated in a particular form presented, communicated and interpreted is both instructive and necessary if economics is to be a self critical discipline.

Perhaps the most familiar characterisation of Keynes' work is in terms of a revolution. Since the publication of his General Theory, economic literature has been replete with examples of revolutions, counter-revolutions, resurgences, revivals. There have been as many revolutions as there have been authors so that the exact nature of the revolution has become obscured, as is its timing - was it notionally with the General Theory but effectively with the works of Clover-Leijonhufvud, perhaps? Even before the publication of Kuhn's work The Structure of Scientific Revolution, Keynes's work had been labelled a revolution (see Klein's work, among others). Since the publication of Kuhn's work the notion of a Keynesian revolution or a revolutionary paradigm has become commonplace in economic literature. Blaug rightly points out that economists' adaptation of Kuhn must be viewed in a similar fashion to their earlier adaptation of Popper and their subsequent embracing of Lakatos. (\*1)

Many academic works made oblique reference to Kuhn's work, or employed his terminology, without the full conceptual impact of his work being realised. There developed a crude textbook-like version of the Keynesian revolution as a new paradigm in economics. The conclusions to be drawn from this section are twofold. First, that Kuhn's schema is of dubious validity to the history of economics. Second, this being so, the wholesale usage of Kuhnian terminology, though superficially attractive, is ultimately unhelpful, since important aspects of the debate are either missed or underestimated.

The basic stages in Kuhn's history of sciences are as follows: preparadigm stage; normal science; crisis; extraordinary science; normal science again once the crisis is resolved. The normal science from which Keynes supposedly deviated was Marshallian neoclassical economics. The basic anomaly one may discern in this heritage is the postulation of full employment based upon Say's Law and the quantity theory. Given complete wage-price flexibility, full employment was automatically assured, by introducing aggregate demand not income-spending as the crucial linkage in

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\*1 Blaug, M. HOPE, Vol. 7, No. 4, (1975) 'Kuhn versus Lakatos'.

the economy's prosperity, Keynes is attributed with totally changing the world view. The basic puzzle of the discipline now shifts. It is no longer a question of determining the distributive shares of optimum resource allocation given full employment, but rather the level of employment of resources, the role of aggregate demand in determining this level and the role of the government in relation to aggregate demand. The crisis was undoubtedly accelerated, and the paradigm battle shortened, by the Great Depression.

As already noted, this account has all the appearance of a Kuhnian scientific revolution. The crisis period witnessed a growing number devoted to working in the anomaly area. The rules of normal science were blurred with attendant insecurity regarding definitions of approach and terminology. Keynesianism emerged as one competing paradigm which eventually became the dominant paradigm, offering its resolution to the conflict. The old paradigm lingered until a new one emerged. Keynes made a substantive contribution to the tools of economic analysis adding new fundamental behavioural functions, i.e. consumption, liquidity preference, multiplicity and new applications of the discipline in areas such as policy and econometrics. In the paradigm battle 'conversion' happened more as a matter of faith than proof. It was a question of persuasion, of adapting what appeared as the most analytically useful set of assumptions and axiomatic position. Two decades ago it was, perhaps unwisely, proclaimed that "we are all Keynesians now".

I find the above account both unconvincing and a potential diversion from many of its own "anomaly" areas. The first set of criticisms concern internal criticisms of Kuhn's theory. These relate to the terminological imprecisions and flaws in Kuhn's exposition. Much of the difficulty in this respect stems from the successive versions of Kuhn's theory, whereby later editions have sought to elucidate what was unclear and redefine what was imprecise, often resulting in apparent internal contradictions. Thus, the word paradigm is replaced by disciplinary matrix and controversy continues to rage over whether old paradigms disappear or continue to co-exist with the new paradigm. This type of criticism is not unique to Kuhn's work, it is a natural consequence of any attempt to develop and articulate a new set of ideas. In the present context the criticism is of significance. If Kuhn's theory is to be of use to economists then internal consistency and clarity of exposition are essential. It will be something of a blunt instrument if the major features of its own construction are in dispute.

A second line of attack relates to a repudiation of Kuhn's central thesis of revolutionary change. This is a fundamental disagreement over the nature of change in science. Kuhn's paradigm-based science and the catastrophic facets of his historiographical framework is anathema to the incrementalists or informatarians who believe that scientific advance is gradual and continuous. These opposing views do not necessarily constitute an either/or choice. It does show, however, that Kuhn has no monopoly in the theory of change and points to the possible existence of other types of change which this theory cannot accommodate.

A third set of criticisms refer to a distinction between the physical and social sciences. However applicable his theory may be for the physical sciences, a direct transplantation of Kuhn's ideas to the discipline of economics is premature and undesirable. Bronfenbrenner states:

"It may also be that Kuhn has generalised too rapidly from too small a sample of revolutions within the natural sciences which he

treats."(\*1)

A consideration of why it is that Kuhn's theories are not applicable to the social sciences, economics in particular, leads to the final and most trenchant criticism of Kuhn. While, in many respects, Kuhn's theory provides a broad general framework within which change and the accumulation of knowledge may be viewed, it fails to provide a mechanism whereby the structure of that change may be explained. It does not deal with the factors which result in change, nor the relationship between these factors. This is precisely what is needed for a reflection on the structure of past controversies in economics. The following section outlines how this task might be undertaken. While it does not purport to be a full specification of a new approach, it does attempt to sketch an alternative to the revolution story.

#### Section 4: Controversy Revisited

"Our theories, regarded as tools of analysis, are blinkers in this sense. Or it may be politer to say that they are rays of light, which illuminate a part of the target, leaving the rest in the dark. As we use them, we avert our eyes from things that may be relevant, in order that we may see more clearly what we do see. It is entirely proper that we should do this, since otherwise we should see very little. But it is obvious that a theory which is to perform this function satisfactorily must be well chosen; otherwise it will illumine the wrong things."(\*2)

The above succinctly summarises my objection to the application of Kuhn's theory to the works and interpretation of Keynes. It is not that I deny that Kuhn lights up part of the target, it is rather that I believe the darkened perimeter also contains many important insights. A rigid and consistent application of Kuhn to Keynes will systematically fail to explore the perimeter, thereby neglecting many essential features of the issue.

Following Leijonhufvud (\*3) I would make a distinction between theory and model. By theory is meant a patterned set of substantiated beliefs about how an economic system works. In Lakatosian terms this refers to what remains of a research programme if the positive heuristic is included. A model is a formal representation of a theory, or a subset or aspect of it. It is quite probable that no model will succeed in giving an exhaustive account of a theory; it may in fact neglect or misrepresent quite essential characteristics of a theory. This distinction is, I believe, crucial and one which is of great use in analysing the continuation of controversy

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- \*1 Bronfenbrenner, M. HOPE, Spring 1971, p 136.
  - \*2 Hicks, J. R. in Latsis, Method and Appraisal in Economics, CUP, 1976, p. 208.
  - \*3 Leijonhufvud 'Schools, 'Revolutions' and Research Programmes' in Latsis, op. cit. p.70.  
Having already castigated intellectual eclecticism in the case of Kuhn and use of his language and concepts, I am aware that much of Leijonhufvud's paper is based on the work of Lakatos. Any mention of Lakatos in this essay is simply to highlight that these ideas may be 'translated' into Lakatos' system, while the arguments advanced are independent of validity.

surrounding Keynes.

Economic models are at a second remove from economic reality. The first stratum of simplification or reductionism occurs between economic reality and an economic theory. The economic universe is multi-faceted and, in Schumpeter's terms, a 'vision' or theory is a particular understanding of the myriad relationship within this universe. This vision is subsequently distilled in a model, the substance is given a particular form. Economic models are as such surrogate models which are partial in the sense of highlighting certain aspects of the same real world.

In economics the substance and form of a theory is less tightly linked than in the natural sciences. Thus the prevalent use of temporal equilibrium constructions and the comparative static method for the analysis of real-time dynamic processes. This imperfect congruence between substantive theories and formal models means that the latter require interpretation. Two economic models may thus be formally identical or indistinguishable yet subject to substantively different interpretations. For this reason, theories may easily co-exist since decisive falsification or convincingly accumulating confirmation of theories may pose a difficulty (see section 2).

In addition to this theory/model distinction there is another element which will be of use in interpreting the works of Keynes. This is the fact that the universe economists study is a non constant historically conditioned one. As Hicks states:

"Further, since it is a changing world that we are studying, a theory which illumines the right things now may illumine the wrong things another time. This may happen because of changes in the world (the things neglected may have grown relative to the things considered) or because of changes in our sources of information (the sorts of facts that are readily accessible to us may have changed) or because of changes in ourselves (the things in which we are interested may have changed). There is, there can be, no economic theory which will do for us everything we want all the time." (\*1)

The following may be noted in relation to the works of Keynes. Not only is the substance/form distinction applicable to Keynes, and the subsequent interpretation of his work, it is also relevant to Keynes' view of classical economics. Pre-Keynesian theory must be distinguished from the picture of it constructed and propagated by Keynes and his followers. Neoclassical economics of the 1930s was not a homogeneous doctrine. Keynes' revolt against the classics was directed at the Marshallian variant. This must be clearly distinguished from the neo-Walrasian theory which became predominant in the post-war period.

What of the interplay of exogenous and endogenous function which contributed to Keynes' new cosmological view? No doubt there were changes in the real world (unemployment and depression which questioned the automaticity of the system towards full employment); in the perception and vision of that world (one which allowed for the possibility of 'disequilibrium'). The form in which these views were cast obscured rather than highlighted these new developments. Leijonhufvud claims that the technical limitations of inherited models of formal economic analyses left Keynes with little choice but to adapt a static equilibrium framework

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\*1 Hicks, J. R. op. cit. p208.

(Leijonhufvud, op. cit. p. 94).

This brings me to an analysis of that remarkable creature known as the Neoclassical Synthesis. The Keynes versus the Classics debate took place in a forum that was alien to them both. Starting with the work of Hicks and continuing with the works of Lange and Klein, to Patinkin, there emerges a growing 'consensus' as to the microstructure which underlies all macrodiscourse. This is of course the neo-Walrasian model in a certainty-based general equilibrium framework. The primary task of this debate degenerated into analytically isolating the atypical assumptions/magnitudes of Keynes' theory which were responsible for the peculiar unemployment equilibrium policy implications.

One might, again, question what were the factors that were relevant to sustaining such a synthesis. Leijonhufvud suggests the following:

"The 'neoclassical synthesis' proposed a reconciliation of 'Keynesianism' and 'orthodoxy' on a purely formalistic plane ... behind the formal screen they stood poles apart ... yet, surprise at the extent that this modelling formula gained widespread acceptance, despite the incompatibility on a basic theoretical level is possibly misplaced. It may be that it 'worked' in its time, rather, because it allowed the postponement of a confrontation that could not have been decided, but that had tremendous latent potential for diverting energies away from the pursuit of 'normal science' within each 'paradigm'." (\*1)

Whether or not Leijonhufvud is correct in his rather nuanced view of Keynes' economics is of secondary importance. What is relevant is that the debate was carried on at the level of models, at a formal level which, in time, became obsolescent. The reasons for this obsolescence is of great interest, but not one that may be easily answered. How is one to understand the re-interpretation of Keynes by Clover and Leijonhufvud? Was it motivated by an effort to go beneath the received formal view to re-present Keynes' essential vision? Or was it motivated by erroneous and/or political factors such as the theoretical impotence within the synthesis and the tottering empirical value in the face of increasing anomalies?

How, indeed, is one to assess their re-interpretation? The attempts to prove a secure Keynesian microfoundation for macroeconomics have provided to be suicidal, opening the way for the micro-founded rational expectations approach. How indeed has the substance of Leijonhufvud's vision been encapsulated in his own model?(\*2) As Coddington points out, of the time options open to him Leijonhufvud only discusses one, thereby marking Keynes something of a reconstituted reductionist.

The above sketch does little justice, perhaps, to the complexity of the debate. It is, nevertheless, a more promising perspective within which these events might be viewed. The controversy has been a controversy of models, not theories; the contentions have been about the appropriate specifications of possibly unsuitable models; shifts in the choice of models and new specifications of these models motivated by an interaction of exogenous and endogenous factors.

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\*1 Leijonhufvud, op. cit. p. 98.

\*2 The further question of how Leijonhufvud's vision of Keynes' theory actually corresponds to Keynes' theory is a moot point.



## Section 5: Conclusion

The preceding sketch has a number of significant elements. This lack of perfect congruence between substantive theories and formal models lies at the heart of much of the controversy. It highlights the limits which a particular representation of a theory may impose on one's understanding. Progress in theory may impose on one's understanding. Progress in the economic analysis proceeds not by pure examination of ideas but by a complex interplay of endogenous and exogenous factors. The manner in which economic models are presented, communicated, interpreted and reacted to by the economic profession plays a crucial role in this process.

Academic lassitude of the continuing Keynesian controversy is, therefore, misplaced. Not alone is the substance of past theories of interest, but the form which they have taken is of relevance. Controversy is a perennial feature of economic discussion. If economists are to become involved in controversy in a worthwhile and analytically fruitful way, then it is imperative that they be self-aware of the format and implications of controversy. Unless economists are clearly aware of the scientific basis of economics and the nature, process of accumulation, communication and interpretation of knowledge within the discipline, future economic controversies are likely to be uncritical, aimless, futile and destructive. An examination of Keynesian controversy along the lines sketched above offers the possibility of avoiding this and of entering new controversies in a self-aware and self-critical manner that will be of analytical benefit and not a useless distraction.

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