

## On Positivism and the Keynesian Revolution

by I. MAES and E. SCHOKKAERT\*

### I. INTRODUCTION

In the history of economic thought the Keynesian revolution is usually presented as the breakthrough of a new theory, marking a break with classical economics. By concentrating on the problem of effective demand, this new theory could explain the phenomenon of large involuntary unemployment. Through providing a rationale for government intervention it also dealt a serious blow to the until then dominating laissez-faire ideology<sup>1</sup>. The main protagonist of this "revolution" was John Maynard Keynes. However, there are some differences between the "true" economics of Keynes and the interpretation of his work by different schools of Keynesians, differences which can be accounted for by theoretical disagreements and ideological prejudices.

In this paper we claim that this traditional account of the Keynesian revolution neglects one of its most important aspects, i.e. the methodological one. We will argue that the so-called "Keynesian revolution" concerns much more than theory alone, but is also characterised by, what we will consider as a "methodological revolution". This "methodological revolution" manifests itself in the breakthrough of positi-

---

\* Respectively Nationale Bank van België and Administratieve en Economische Hogeschool, and Centrum voor Economische Studiën, K.U. Leuven. The authors acknowledge financial assistance of the N.F.W.O. and the Fonds E.G. de Barys. They thank M. Blaug, L. Bouckaert, D. de Crombrugghe, C. Perrotta, G. Van Habberney, V. Van Rompuy, A. Watteyne and an anonymous referee for their valuable comments. They also thank H. Myint and M. Perlman for giving them the opportunity to present an earlier draft at the History of Economic Thought Seminar at the London School of Economics. All this does not imply that the usual restrictions would not remain valid.

vism, a term, however, which should be interpreted carefully. We will try to show that this (methodological) point of view also throws a new light on the difficult question of the exact relationship between the economics of Keynes and the “Keynesian revolution”.

## II. THE “KEYNESIAN REVOLUTION”: MUCH MORE THAN THEORY ALONE

It is quite obvious that the dramatic changes which occurred in economics in the nineteen thirties and fourties had an important theoretical component. The theoretical context of this so-called “Keynesian revolution” is well known. Whereas in classical economics unemployment was a symptom of a badly functioning labour market, it is now perceived as being caused by a deficiency of effective demand on the goods market. The new therapy, following logically from the analysis, is exactly the one suggested by most economists at the time: increase effective demand through government spending.

This theoretical (and political) component is only part of the story however. To draw a complete picture of the “scientific revolution” of the thirties and fourties we have to take into account some methodological developments.

Although Keynes in the “General Theory” did not provide a mathematical model of the theory of unemployment, it is undeniable that “Keynesian” economics has been rather mathematically inclined. After World War II the formalisation of economic theory proceeded rather rapidly and, although its main focus can be located in micro-economic theory, macro-economics certainly did not stay immune for it. In other words, the “mathematical revolution”, emphasised by Ward (1972)<sup>2</sup>, was not independent of the “Keynesian revolution”. We can illustrate this statement with an example. Before 1930 economists wanting to construct a formal theory usually formulated one equation and then started arguing around the different variables of the equation. A beautiful example is Keynes’ “Tract on Monetary Reform”. He there formulates the quantity theory as  $n = p.k$ . with  $n$  as the currency notes or other forms of cash in circulation with the public,  $p$  the price of a consumption unit and  $k$  the amount of real purchasing power the public holds in the form of money. The main point of the exposition is then the discussion of the interrelationships between the different variables. This approach can be contrasted with the one adopted by

Hicks (1937) in his famous article “Mr. Keynes and the ‘Classics’, A Suggested Interpretation”, which probably was the starting point for most research in the following years. Hicks first summarises the classical position and Keynes’ position, each in three equations. He criticises them both and proposes three, more general equations, encompassing both positions as special cases, depending upon the parameters of the money demand equation. The point of this story is clear: mathematical model-thinking was an integral part of the “Keynesian revolution” from its early beginning<sup>3</sup>.

Besides the “mathematical revolution” there was another important phenomenon in the thirties: the increase in the quantity and quality of the available macro-economic data, brought about mainly by the organisation of official government agencies with the aim to measure national income (expenditure, product) and its components. This trend is associated mainly with the names of Simon Kuznets in the United States and Colin Clark and Richard Stone in Great-Britain. Patinkin (1976a) explores the relationships between this “statistical revolution”, as he calls it, and the Keynesian one described earlier<sup>4</sup>. The pre-Keynesian theories of the business cycle directed the statistical efforts towards the measurement of investment. These early estimates were then used (e.g. by Keynes) to support the “theoretical revolution”. An analogous “dialectical” relationship between data gathering and theoretical work also existed after the thirties, the new macroeconomic model offering a useful frame of reference for the elaboration and refinement of statistical measurement, which, in turn, permitted further theoretical developments.

In first instance, the data were mainly used to “illustrate” the theoretical discussions. But as their quality increased and the macroeconomic discussions were more and more cast in the form of mathematical models, the foundations were laid for a new fundamental “revolution”, the “econometric revolution”<sup>5</sup>. Here also there was a dialectical relationship<sup>6</sup>. On the one hand, it is undeniable that the growth of Keynesian macroeconomics was impregnated by econometrics. A lot of empirical research tried to estimate the parameters of the Hicksian model: the consumption function, the investment function and the liquidity-preference function. On the other hand it can also be argued that econometricians needed the Keynesian revolution. Of course, a lot of econometric research was purely methodological, and the work done after World War II around the Cowles Commission by a group of active and highly influential researchers was directed partly towards

the solution of rather abstract statistical problems. Nevertheless, the enthusiasm would have been much smaller, if there had been no potential immediate applications. These possibilities were offered by Keynesian macroeconomics. Econometricians were quite aware of the fact that they only elaborated a tool and that they had to rely on theory to provide them with adequate specifications. Because of its theoretical and social relevancy the Keynesian framework in this respect dominated all its competitors. Moreover it had the decisive advantage to be formulated on a highly aggregative level and to suggest easy and adequate definitions of the variables. Because of the interaction with the statisticians observational data on these variables were readily available. It then can cause no surprise that the pathbreaking work of Tinbergen in the thirties was concerned with business cycle theory and that macroeconomics remained the favourite field for application of econometrics.

It must have become clear by now that a separate investigation of the theoretical developments during the “Keynesian revolution” can only be undertaken at the risk of getting biased interpretations of the overall evolution. This overall evolution cannot be understood if we neglect the complex interplay of theoretical (Keynesian), mathematical, statistical and econometric “revolutions”. Moreover, these “revolutions” have to be projected on the philosophical background of the time.

### III. A POSITIVIST BREAKTHROUGH IN THE THIRTIES

During the thirties and fourties positivist ideas, which originated in the nineteenth century, were evolving towards more logical-positivist or logical-empiricist philosophies of science, under the influence of the Vienna Circle<sup>7</sup>. In economics T. Hutchison has become the most influential advocate of these ideas. He argued (in 1938!) that logical analysis and empirical investigations are the two pillars of science: “The scientist proceeds by means of the two inextricable interconnected activities of empirical investigation and logical analysis, the one, briefly, being concerned with the behaviour of facts, and the other with the language in which this is to be discussed”<sup>8</sup>. It is illuminating to confront this quotation with the picture of the Keynesian revolution sketched earlier. Obviously, mathematical model building has become the language of Keynesian economics, their vehicle of logical analysis,

while econometric testing has become its favorite method of empirical investigation. Apart from its theoretical aspects, the “Keynesian” revolution secured the definitive (?) triumph of the logical-empirical methodology and its success partly can be accounted for by the growing attraction of a more positivist methodology. On the other hand, it is also true that the success of Keynesian economics contributed to the acceptance of a more positivist methodology in economics.

In this respect two important points should be stressed. In the first place we must admit that the concept of “positivism” is very vague and indeed is covering (and has covered) a lot of different contents. Perhaps we can argue that in economics it stands for a procedure, whereby one starts with some well-defined premises, assumptions, then constructs a logical-deductive reasoning (preferably mathematic), to arrive finally at a set of operationally-formulated “causal laws”. These laws make prediction possible. Predictions should be confronted with reality to test whether the theory holds (preferably with statistical methods). The vagueness of this description reproduces rather well the vagueness with which the term is used by economists. One could even argue that the confusion around the concept partly explains its success. Indeed, and this is the second important remark, the practice of economists does only partially come up to the expectations of the methodologists. Since the strong criticism of Hutchison (1938) this point has repeatedly been stressed by different authors. They generally agree that economists pay too much attention to deduction and (mathematical) theory construction and neglect the systematical confrontation of theory with fact. “Much empirical work in economics is like playing tennis with the net down”<sup>9</sup>.

Although we agree that it is difficult to give an adequate definition of “positivism” and that there sometimes is a gap between the practice of economists and the prescriptions of the methodologists, we nevertheless think that the main idea is clear enough and may help us to interpret the “Keynesian revolution”. In our view, historians of economics have laid (relatively) too much stress on theoretical shifts in the thirties and neglected the acceleration in the trend towards positivism. In the following section we will try to show that our point of view also throws a new light on the Keynes versus the Keynesians-debate.

#### IV. KEYNES AND THE “KEYNESIAN REVOLUTION”: A METHODOLOGICAL PERSPECTIVE

The question of the exact relationship between the economics of Keynes and the “Keynesian revolution” has been vigorously debated, especially since the appearance in 1968 of Leijonhufvud’s “On Keynesian economics and the Economics of Keynes”<sup>10</sup>. But the aim of this section is rather different from that of most other publications: indeed, we do not try to reconstruct the “true” Keynes, nor do we want to derive any normative conclusions about the value of Keynesian macroeconomics by comparison of its main tenets with those of its sacred prophet. We only want to argue that the deviation from Keynes’ work can partly be explained by the methodological developments described in the previous section.

A natural starting point for our discussion are Keynes’ own writings on methodology<sup>11</sup>. Although these writings are not so abundant, they suffice to sketch rather clearly his views. Keynes was very explicit in a letter to Harrod: “It seems to me that economics is a branch of logic, a way of thinking (...). Economics is a science of thinking in terms of models joined to the art of choosing models which are relevant to the contemporary world.”<sup>12</sup>. We should not conclude from this quotation that Keynes would have applauded the mathematization of economics. On the contrary, in the *General Theory* he jeers at the “symbolic pseudo-mathematical models of formalizing a system of economic analysis”: “Too large a proportion of recent mathematical economics are merely concoctions, as imprecise as the initial assumptions they rest on, which allow the author to lose sight of the complexities and interdependencies of the real world in a maze of pretensions and unhelpful symbols”<sup>13</sup>. Indeed, according to Keynes, “...economics is essentially a moral science and not a natural science. That is to say, it employs introspection and judgements of value”<sup>14</sup>. Of course, “... the specialist in the manufacture of models will not be successful unless he is constantly correcting his judgment by intimate and messy acquaintance with the facts to which his model has to be applied”<sup>15</sup>. This interest for the facts of the real world does not imply the adoption of econometric methods at all: on the contrary Keynes (1939) wrote a devastating critique of Tinbergen’s work on business cycles, wherein he denies the possibility of testing economic theories with regression techniques. Later on, he compares econometrics with “black magic” and “alchemy” and challenges statisticians to an experiment: “It will be remem-

bered that the seventy translators of the Septuagint were shut up in seventy separate rooms with the Hebrew text and brought out with them, when they emerged, seventy identical translations. Would the same miracle be vouchsafed if seventy multiple correlators were shut up with the same statistical material? And anyhow, I suppose, if each had a different economist perched on his a priori, that would make a difference to the outcome”<sup>16</sup>.

These few quotations clearly illustrate the sharp distinction between Keynes’ methodological ideas and the practice of economists in the post-war period. Patinkin (1976a) tries to sugar the pill by noting that the “Treatise on Money” was a formalistic and mathematically oriented work and that Keynes was one of the first fellows of the Econometric Society and even elected president in 1944. It still remains a rather ironical trick of history that Keynes, who claims to repel very firmly “attempts to turn economics into a pseudo-natural science”<sup>17</sup>, has given its name to a “revolution”, which definitely put the seal upon the succes of positivism in economics.

Let us now have a look at some theoretical issues. One of the dominant features of the “Keynesian revolution” is the victory of the Walrasian research programme over the Marshallian, thanks to the innovative work of Hicks, Allen and Samuelson<sup>18</sup>. This development was in line with the trend towards mathematization and formalisation, but departed from Keynes’ terminology and methodology. His use of an aggregate supply and demand function and of the concepts of demand price and supply price, his partial equilibrium approach and the tackling of the problem of time with the different periods concept, all show how deeply impregnated he was by Marshalls Principles<sup>19</sup>.

A more basic problem is the role of money and the analysis of disequilibrium. For Keynes, the monetary aspect of the economy was immediately connected with the overwhelming importance of uncertainty, and exactly this point was for him (one of) the main innovation(s) of his General Theory. Most participants in the “Keynes and the Keynesians” – debate agree on this point: while Keynes uses equilibrium analysis as an analytical tool, he was fundamentally interested in the analysis of a monetary economy in turmoil. It is therefore striking that this concern with disequilibrium processes remained largely out of the interest of mainstream macroeconomics. Money is not a source of instability anymore: it plays no role at all in an exposition of the 45°-graph, while in the IS-LM-model there is only a place for a

demand for money function, which is a stable function of income and the interest rate<sup>20</sup>.

How is this biased Keynes-interpretation of mainstream economists to be explained? The usual answer is well-known: one can claim that ideological considerations, particularly the wish to defend the existing class structure of society, made it impossible for many conservative economists to accept the idea that capitalism is fundamentally unstable. We claim that this answer is only a partial one. The General Theory, although containing a message of disequilibrium, gives a comparative static analysis<sup>21</sup>. Moggridge (1976) notes that it differed in this respect from the "Treatise on Money", which had been cast in a sequential framework, particularly adapted for dealing with disequilibrium situations. This shift was not really a voluntary choice, but the technical problems raised by the "disequilibrium" approach were so difficult, that they forced Keynes to change his mode of analysis in order to reach his overall purpose, which was the diffusion of some fundamental ideas and policy options. Leijonhufvud also writes: "Static, equilibrium modelling was the only technical form that we can reasonably say was available to him"<sup>22</sup>. It is clear that the same technical problems existed for Keynes' successors. These were moreover, as distinct from Keynes, strongly impressed by the aesthetical value of the Walrasian framework. Technical limitations and infatuation with the beauty of the Walrasian scheme seem to be the most fundamental explanation for the stress on equilibrating factors in mainstream economics after Keynes. If this hypothesis is provisionally accepted, one can derive from it a prediction: if new techniques could be developed to treat adequately disequilibrium situations, the emphasis would shift again. The evidence of the last years has certainly not falsified this prediction. Since the publication of the paper of Barro and Grossman (1971) there has been a lot of work on disequilibrium economics and "disequilibrium econometrics" and it is not exaggerated to say that this field of research is nowadays one of the most promising and expanding.

We can now draw some conclusions. The evolution in the methodology of mainstream economics after World War II was opposite to the prescriptions of Keynes, even when it elaborated his very theoretical framework. This is a clear illustration of the strong power of attraction, exerted by the example of the positivistic successes of the natural sciences. The positivistic ideal also coloured the theoretical Keynes-interpretation by mainstream economics: only the components of his theory that could be summarised in a neat, formal framework were



taken into account. “Disequilibrium” ideas only made their way in economics in recent years, because now the technical difficulties appear to be solvable.

## V. CONCLUSION

In this paper we tried to show that methodological aspects are very important if we want to understand the revolution in economics during the thirties and forties. The “Keynesian”, mathematical, statistical and econometric revolutions were not independent of each other but probably inseparable to such an extent that no one of them would have had a deep influence without the others. Together they marked the breakthrough of a more positivist methodology in economics. Also, this tendency towards a positivist methodology partly explains the difference between Keynes and his “Keynesian” successors.

### NOTES

1. *Cfr.* Klein (1949), Robinson (1962), Meade (1975).
2. Ward (1972), pp. 40-44. Ward advances F. Ramsey as the first economist in which the mathematical style of arguing dominated.
3. For an overview of the development of Hicks' ideas and his impact on macroeconomic and monetary theory, *cfr.* Maes (1984).
4. A summary is given in Patinkin (1976a), pp. 1110-1111.
5. For an interesting overview of the early history of econometrics, *cfr.* Darnell (1983).
6. See also, for example, the important testimony of Tinbergen (1947).
7. For an overview of the development of positivism, *cfr.* Caldwell (1980).
8. Hutchison (1938), p. 9.
9. Blaug (1976), p. 173.
10. Of course, the debate existed already before 1968. The Keynes-interpretation of some of his Cambridge followers had always departed from the standard income-expenditure model. See for instance Robinson (1962), chapter 4.
11. For a more detailed analysis of Keynes' methodology, *cfr.* Maes (1983a).
12. Letter to R.F. Harrod, 4/7/38. Reprinted in Keynes (1973), p. 296.
13. Keynes (1936), p. 297.
14. Letter to R.F. Harrod, 4/7/38. Reprinted in Keynes (1973), p. 297.
15. Letter to R.F. Harrod, 16/7/38. Reprinted in Keynes (1973), p. 300.
16. “Comment” in the Economic Journal, march 1940. Reprinted in Keynes (1973), pp. 319-320.
17. Letter to R.F. Harrod, 4/7/38. Reprinted in Keynes (1973), p. 296.

18. For a characterisation of the Walrasian and Marshallian approach to economics, see Leijonhufvud (1974).
19. Patinkin is here not so convincing in attributing a general-equilibrium analysis to Keynes, *cf.* Maes (1983b), p. 102.
20. See also Johnson (1962), pp. 39-43.
21. Leijonhufvud (1968), p. 36 writes: "His model was static, but his theory was dynamic". This accords also with Patinkin's analysis, see Patinkin (1976b), pp. 116-119.
22. Leijonhufvud (1976), p. 94.

## REFERENCES

- Barro, R.J. and Grossman, H.I., 1971, A General Disequilibrium Model of Income and Employment, *American Economic Review*, 61, March 1971, pp. 82-83.
- Blaug, M., 1976, Kuhn versus Lakatos, or Paradigms versus Research Programmes in the History of Economics, in Latsis, S.J. (ed.), *Method and Appraisal in Economics*, C.U.P., Cambridge, 1976, pp. 149-180.
- Caldwell, B., 1980, Positivist Philosophy of Science and the Methodology of Economics, in *Journal of Economic Issues*, Vol. XIV, No. 1, March 1980, pp. 53-76.
- Darnell, A.L., 1983, *Statistics and Econometrics in Historical Perspective*, Working Paper 58, University of Durham, 65 pp.
- Hicks, J.R., 1937, Mr Keynes and the "Classics": A Suggested Interpretation, in Lindauer J. (ed.), *Macroeconomic Readings*, Free Press, N.Y., 1968, pp. 53-60.
- Hutchison, T.W., 1938, *The Significance and Basic Postulates of Economic Theory*, Kelly, N.Y., 1965, XXVIII + 192 pp.
- Johnson, H.G., 1962, The General Theory after Twenty-Five Years, in Marshall N. (ed.), *Keynes: Updated or Outdated?*, Heath & Co., 1970, pp. 31-48.
- Keynes, J.M., 1923, *A Tract on Monetary Reform*, MacMillan, London, 1971, XXII + 172 pp.
- 1936, *The General Theory of Employment, Interest and Money*, MacMillan, London, 1976, XXXV + 428 pp.
- 1939, Professor Tinbergen's Method, in *The General Theory and After, Part II, Defence and Development*, C.W. XIV, MacMillan, London, 1973, p. 306-318.
- 1973, *The General Theory and After, Part II, Defence and Development*, C.W. XIV, MacMillan, London, 1973, 584 pp.
- Klein, L., 1949, *The Keynesian Revolution*, MacMillan, London, 1980, XIII + 288 pp.
- Leijonhufvud, A., 1968, *On Keynesian Economics and the Economics of Keynes*, O.U.P., N.Y., 1968, XIV + 431 pp.
- 1974, *The Varieties of Price Theory: What Microfoundations for Macrotheory?*, Discussion Paper, UCLA, 1974, 53 pp.
- 1976, Schools, "Revolutions", and Research Programmes in Economic Theory, in Latsis S.J. (ed.), *Method and Appraisal in Economics*, C.U.P., Cambridge, 1976, pp. 65-108.
- Maes, I., 1983a, *On the Economic Philosophy of J.M. Keynes*, Research Rapport, Centrum voor Economische Studiën, Sept. 1983, 24 pp.
- 1983b, Review of Patinkin D., Anticipations of the General Theory?, *Recherches Economiques de Louvain*, vol. 49, n° 1, mars 1983, p. 101-103.
- 1984, *The Contribution of J.R. Hicks to Macroeconomic and Monetary Theory*, Ph.d. dissertation, K.U. Leuven, 1984, V + 224 pp.
- Meade, J., 1975, The Keynesian Revolution, in Keynes M. (ed.), *Essays on John Maynard Keynes*, C.U.P., 1975, p. 82-88.

- Moggridge, D. E., 1976, *Keynes*, Fontana, Glasgow, 1976, 109 pp.
- Patinkin, D., 1976a, Keynes and Econometrics: On the Interaction Between the Macroeconomic Revolutions of the Interwar Period, *Econometrica*, Vol. 44, N° 6, Nov. 1976, pp. 1091-1123.
- 1976b, *Keynes' Monetary Thought; a Study of its Development*, D.U.P., Durham, 1976, V + 163 pp.
- Robinson, J., 1962, *Economic Philosophy*, Penguin, Harmondsworth, 1976, 140 pp.
- Tinbergen, J., 1947, The Significance of Keynes' Theories from the Economic Point of View, Harris S.E. (ed.), *The New Economics*, London, Dennis Dobson, 1947, p. 219-231.
- Ward, B., 1972, *What's Wrong with Economics?*, MacMillan, London, 1972, X + 273 pp.