

# Can we consider the Keynesian Revolution To Be Scientific Progress?<sup>1</sup>

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As soon as "Keynes' theory", "Keynesianism", "Keynesian economics" became a label within economic theory a host of literature began to appear to discuss the true meaning of his theory. The different interpretations are related to the different economic theories the interpreters of Keynes advocate. There is a tendency to read one's own position into the writings of Keynes. A post-Keynesian will find different aspects in Keynes' work more relevant than an economist convinced by the neoclassical synthesis of the 50s and 60s, or a monetarist like Meltzer. Closely related to the appraising literature are descriptions of the development of Keynes' theory, as different ideas about the 'true' meanings of Keynes' theory occasion to put the emphasis on different aspects of his scientific development (Blaug, 1980).

All schools of economics with a positive affinity towards Keynes have a common idea: A pure market economy will not necessarily bring about full employment of labour - an attack on Say's law (Gerrard, 1995). However, if one tries to give this idea more precision, it would be necessary to delve into the differences between the different schools. But all of them consider Keynes to have pushed economic theory to new comprehension, namely that the Keynesian revolution supplanted one theory, - supply creates its own demand - by another, that of effective demand. The contribution of Keynes to the development of economic theory is to be found in those aspects of his work which later economists, suspicious about the beneficial results of a pure market economy, could use for their work.

But does this imply that economists who are convinced of the market clearing perspective and are therefore non-Keynesian or, if more polemical, anti-Keynesian, have to disregard Keynes' contribution to economics, such as Keynes, in a well known dictum, disregarded the contribution of one of the foremost economists arguing with the market clearing approach - namely Ricardo<sup>2</sup>. Hence, did the Keynesian revolution bring about scientific progress only for economists with a Keynesian outlook, while an anti-Keynesian should be convinced that Keynes hindered scientific progress, or can one even say from a modern anti-Keynesian outlook that Keynes furthered economic theory? To give an answer one would need a concept of scientific progress for economics. This article should be seen as an attempt to work out such a concept and to apply this concept to the Keynesian revolution.

In the first part of the paper I argue that, although most of the propositions of Keynes' theory, as far as they were politically relevant, were accepted to be true by many people before the Keynesian revolution, however, their arguments were not sound. Therefore, the Keynesian revolution can be characterised as a scientific evaluation of widely held beliefs concerning the possibility to manipulate aggregate demand. In the second part I show that neither Marshall nor Wicksell could integrate the well established fact of business cycles into their macroeconomic theories. In the third part I distinguish between two strands of macroeconomic theorising, both are to be found in the works of the same authors, often in one text. In the first the quantity theory as an equilibrium approach is pursued, in the other business cycles are

analysed as a sequence of reactions of agents trading in situations out of equilibrium. However, there was no systematic theory for this type of analysis. In the final chapter I argue that in the Keynesian revolution these two different approaches were brought together. This was done by checking carefully the basic concepts which were used in the theories.

## **1 The theoretical problem**

Very naively, one can speak of scientific progress, if scientists at later times know more than scientists did at earlier dates. They know most of what their predecessors knew and something more, namely what had been discovered since<sup>3</sup>. This very general criterion for scientific progress is normally used when the contributions of scientists to the development of their science is evaluated by an historian of science. The latter analyses in which way a scientist contributed to knowledge. It is asked what was known before a scientist had made her contribution, and what was known afterwards. This is a kind of fictitious accrual accounting for knowledge, in which the reference for the valuation is some later knowledge. It is asked by historians of science what was the contribution of a scientist to knowledge, given the state of the art at a later date. For example when the contributions of Copernicus to astronomy and physics is evaluated, one has to look at the books written shortly before he had published his findings and compare them with the books written perhaps fifty years after his discoveries. It is then an empirical question, whether this change was due to the influence of Copernicus or somebody else. One might say the following: Before the Copernican revolution took place, it was considered to be true that the sun moved around the earth. Nobody could claim seriously that this was not the case. After the revolution it was the other way round. After a couple of decades, it was generally accepted to be true that the earth moved around the sun<sup>4</sup>. The content of the revolution was to refute the old model of the relation of the sun to the planets and to provide a better one. The new one was considered to be true for centuries until it got supplanted by an even better one. However, aspects of the Copernican world are still taught, because this model suffices for a lot of problems of the relation between the sun and its planets. Nobody who points to weaknesses of the Copernican model will seriously turn towards the Ptolomean model. The Copernican revolution resulted in a better model. In this case historians of the science can be precise as to what is to be understood by the label 'better'. The new model did not remain within the bounds of the scientific public, but became known with some time lag by a wider public. We can therefore say that something not known before the Copernican revolution was known afterwards.

Can we say something similar for the Keynesian revolution? If this is the case, the until then generally accepted truth must have been supplanted by a new model accepted by at least a sizeable part of the profession to be true (or at least not falsified), and must still be regarded today as a better theory than the pre-Keynesian theories. Indeed, according to the eye-witnesses of the Keynesian revolution, there existed a generally accepted truth - namely that the price system of a market economy would provide sufficient incentives so that unemployment could not exist for a prolonged period of time. By discovering the principle of effective demand, this old theory got challenged. A better theory, namely that aggregate output depended on aggregate demand was then considered to be true (Samuelson, 1946; Klein, 1950; Patinkin, 1987).

According to the principle of effective demand, aggregate output and therefore aggregate employment depends on demand. New about this principle was that, unlike in the older formulation of Say's law, demand depends on actual supply only via specific parameters, like the propensity to consume, the propensity to hoard etc. Aggregate demand can fall short of aggregate supply forthcoming at the specific price level. One could therefore draw the political conclusion that aggregate demand can be manipulated by the political authorities. This was generally considered to be wrong before the Keynesian revolution, at least by most

economists of that period who today are considered to have been the important scientists. They were convinced by Say's law which states that supply creates its own demand.

However, can we say that the possibility to manipulate effective demand was not known before the Keynesian revolution took place, such as we can say that before the Copernican revolution it was not known that the earth moved around the sun? If we take the above mentioned core of pre-Keynesian economists, the question can be answered positively. They did not know the principle of effective demand. According to Samuelson (1946) those economists who were young in the thirties were taken by storm by the new truth (see also Moggridge, 1995). But one cannot say that there was nobody before the Keynesian revolution who considered the proposition that aggregate demand could fall short of aggregate supply a true statement or that aggregate demand could not be manipulated. Most of them were outsiders to the profession, like the Attwoods or William Blake who already after 1815 argued for a policy of easy money to overcome the prolonged recession after the Napoleonic wars, or Karl Marx who thought that crises are inevitable in capitalism. Malthus and Hobson considered the lack of consumptive demand the cause for crisis. Both wanted the income distribution to be changed to secure adequate demand, the former in favor of landlords the latter in favor of the working classes. The authors of the Minority Report to the Royal Commission on the Poor Laws in 1909 argued for public expenditure during recessions to overcome temporarily insufficient demand. Something like a principle of effective demand had been widely considered to be true.

The economists who stuck to Say's law rather considered it their duty to convince the public of the futility to interfere with aggregate demand. They accused those who believed in this possibility that they lacked a proper understanding of the economy, that they had a wrong theory. However, the above mentioned outsiders had come to the same political conclusions as Keynes and the Keynesians later did, conclusions which were by the standard of a greater part of the academic economists in the forties, fifties, and sixties the right ones. For quite a number of people the achievement of the Keynesian revolution must have provoked the feeling that finally even the economists had learned what they already had known before.

It is therefore wrong to say that during the Keynesian revolution the principle of effective demand was discovered by economists who then provided a wider public with the news of their discovery. It was already 'known' before by many people. If we characterise a scientific revolution by propositions known before and after the revolution, the Keynesian revolution was only a scientific revolution for the believers in the old theory. But if we do so, it looks like as if some of the outsiders and laypersons knew, by the standards of macroeconomic knowledge in the forties and fifties, more than the contemporaneous leaders of the profession did, e.g. that Hobson and the Webbs knew more macroeconomics than Marshall and Pigou did. If we accept this, why would it be that in the forties and fifties Marshall and Pigou were considered to have been important economists whereas Hobson and the Webbs were seen as outsiders?

This question can be answered by distinguishing between the substance of propositions and the arguments put forward to support them. If one reads with the knowledge of Keynesian theory the pamphlets of the Attwoods or the books of Hobson, they still would not appear to be argued profoundly. They were convinced that their propositions were true, but their arguments put forward were mostly wrong, on both accounts, namely by the standards of advanced theoretical contemporaneous argumentation, and by the standards of the science after the Keynesian revolution as well. Although they 'knew', namely were convinced by their own argumentation, that effective demand can be increased by easing monetary policy or by redistributing income in favor of groups with higher inclination to consume, they did not have a proper economic theory to prove it.

The distinction between propositions and arguments to support them provides a different approach to evaluate the achievements of the Keynesian revolution. It was not the principle of effective demand that was a new substantive proposition about the economy, which has to be seen as an intellectual revolution, but the argumentation for this principle within the context of traditional economic analysis. Only for the economists were these two aspects the same. They considered Say's law to be true, because they had a theory which told them so. Supplanting this theory by a new one resulted in a refutation of the consequences of this theory - namely that effective demand cannot be manipulated. Therefore, Keynes did not prove that Malthus, or Hobson, or the Minority Report were right - though he favored the 'plain sense' of Malthus to the rigour of Ricardo - but that within the conceptual framework of economic theory laid out by Ricardo, Marshall and all the others fighting against anything like the principle of effective demand, this very principle could be supported.

To evaluate the achievements of the macroeconomic discussions in the twenties and thirties one has to look for the development of the theoretical apparatus, the new tools, the clarification of concepts, and how they are related to the new propositions. By doing this we can find out whether there was merely a revolution in the sense of changing beliefs and a changing general outlook, or whether there was a scientific revolution in the sense of scientific progress, independently of current (or personal) conviction concerning Keynesian economics. If it can be shown that the concepts and tools developed in this discussion in the twenties and thirties were used for quite some time after the Keynesian revolution by economists independently of the macroeconomic theory they considered to be true, or still are used, then we are justified to speak of a revolution in the sense of scientific progress.

## 2 The starting point

If we look out for a reference point of pre-Keynesian macroeconomics we have to take the testimonies of Marshall at the Gold and Silver Committee in 1887, in the midst of a severe and prolonged depression, and at the Indian Currency Committee in 1899, at the beginning of a new upswing<sup>6</sup>, and Wicksell's "Prices and Interest" (1898). Both renewed the Ricardian macroeconomic position against the banking school, namely that the amount of outside money only determined the price level, not relative prices. They integrated into their reasoning the existence of a financial sector and thus gave the rate of interest a more important role for macroeconomic equilibrium than Ricardo had done. The newly developed theory of capital remuneration could thereby be given a prominent place in the determination of equilibrium (e.g. Marshall, 1887, nrs.9651, 9684; Wicksell's concept of the 'natural rate of interest'<sup>6</sup>).

Most of the fundamental macroeconomic propositions of Marshall and Wicksell can be covered by modern non-Keynesian macroeconomic theory, because they were both arguing within the tradition of Say's law (e.g. Marshall, 1887, nr.9816; Wicksell, 1898, p.143). Nevertheless, it is not possible to consider the Keynesian revolution simply as an aberration, even from a modern market clearing approach. This is due to two circumstances. Firstly, it was accepted by Marshall and Wicksell that all economies experienced ups and downs in real economic activity - the business cycles; secondly Marshall and Wicksell had accepted to be true that changes in the amount of money can influence real economic activity in the short run, namely that an increase in the amount of the circulating medium can increase real product and vice versa<sup>7</sup>. This is not to say that Marshall and Wicksell can be seen as proto-Keynesians. However, they could not integrate the connection between the state of the financial markets and changing real economic activity into their theories of long term equilibrium. Marshall put a moral verdict on all the extra production due to an easing of money markets<sup>8</sup>, Wicksell reduced this relation to a 'tendency to expand production' (Wicksell, 1898, pp.90, 143). Both retold the story Henry Thornton had told in 1802, a story which never was doubted to be true

(e.g., Mill J.St., 1848, p.527)<sup>9</sup>, namely the existence of a Phillips-curve-like short-run economic relation between price changes and real economic activity was taken for granted.

The theoretical problem was to integrate the phenomena of business cycles with the theory of long term development which was conceptualised as economic equilibrium. How could the phenomenon that easing the terms of credit results in an extension of aggregate demand be theoretically analysed in a framework in which the amount of money only influences the price level? These phenomena were seen as disturbances of an equilibrium due to wrong calculations of economic agents, too much speculation, erroneous hopes etc. All these types of economic activity were considered to be something like misbehaviour and had surely to come to an end within a short time. The idea was that an equilibrium can be characterised as a situation which can go on unless there are external shifts. However, in which way this was related to the economic actions of agents when this condition is not fulfilled remained completely unclear.

The problem for economists can thus be summarised: Whereas the theoretical concepts and propositions considered true were construed around the idea of an equilibrium, all economies were subjected to fluctuations which generally were considered as phenomena of disequilibrium. There were no new phenomena, hitherto unknown, which posed a challenge to the established theory, like in Kuhn's concept of scientific revolutions, because it was always accepted that the price level is linked to real aggregate activity. This does not mean that there was unanimity about the phenomena to be analysed. It was not clear what a crisis is - changes of the price level; a high rate of bankruptcies, particularly of banks; unemployment; a particularly high or low rate of interest; an outflow of gold due to a balance of payments disequilibrium; a change in the rate of exchange; low profits; low investment. All these phenomena were not only seen as phenomena of disequilibrium but often as the very essence of a crisis. How all these phenomena are linked to each other was a theoretical problem.

### 3 The two macroeconomic approaches

There were two strands of macroeconomic discussion during the first quarter of this century. In the one, a new foundation of the quantity theory was worked out. In the Cambridge equation the price level is related to the quantity of money via the desire to hold money (Pigou, 1917; Keynes, 1923). This must be seen as a microeconomic underpinning of a macroeconomic relation, which is not paralleled in Fisher's formulation of the quantity theory. This line of reasoning could not account for the fact of changes in aggregate activity. The other strand of macroeconomics investigated business cycles. It took as a fact that aggregate activity, i.e. aggregate demand and supply, changed in quite a regular pattern. Though the quantity theory itself does not exclude the possibility of a business cycle, to put this theory into the framework of macroeconomic equilibrium, as it was the tradition since Ricardo, clearly created problems for a theory of business cycles<sup>10</sup>.

Different theories were put forward to account for business cycles<sup>11</sup>. Causes influencing the supply of agricultural products, technological shocks, the instability of the monetary system, underconsumption due to a too low wage share, the lack of capital at the end of the boom, the increase of indebtedness during the upswing, the varying demand for inventories and for fixed capital were held responsible for the emergence of business cycles or a depression. The arguments put forward to support the specific theories were not borne by any systematic theory. Confirmation was sought by looking at empirical evidence. Usually, it was possible to get empirical confirmation, particularly for those theories which considered the business cycle to be endogenous. For example, as long as the boom lasts the nominal rate of interest increases. This can be interpreted as scarcity of capital being responsible for the break of a boom, if the rate of interest is seen to be the price of capital. Or, while a boom lasts profits increase. As consumption out of profits is lower than out of wages, one could find

support for the proposition that it is underconsumption which is responsible for the end of the boom. Or, installation of new capital equipment is more volatile than consumption. The lumpiness of new machinery thus looks as if it could serve as a good explanation for the cycle.

The problem was that though these theories gave different explanations and proposed different - often contradictory - remedies to dampen the cycles, the theories did not contradict each other as theories. They rather were incommensurable, because they lacked a common theoretical approach. However, they had a common style of argumentation. A sequence of reactions is described, taking into account reactions of diverse economic agents to new information.

A few examples elucidate this point:

"The rise of interest rates has already narrowed the margins of security behind credits by reducing the capitalized value of given profits. When profits themselves begin to waver, the case becomes worse. Cautious creditors fear lest the shrinkage in the market rating of the business enterprises which owe them money will leave no adequate security for repayment; hence they begin to refuse renewals of old loans to the enterprises which cannot stave off a decline of profits, and to press for a settlement of outstanding accounts." (Mitchell, 1923, p.15)

"...consider cuts in consumption on the part of entrepreneurs and rentiers [due to inflation in the course of an upswing P.R.]. It may be assumed that these classes would need a considerable inducement in order to make them reduce their consumption voluntarily in a significant proportion; for the standards of most of them are likely to be well set and not readily modified except under compulsion. Even when there is compulsion, through bank loan creations, which raise prices and so secretly tax persons with fixed incomes, a good deal of the enforced variation in expenditure on the part of these people is likely to be a variation, not in expenditure upon consumption goods, but upon capital equipment (i.e. investments)." (Pigou, 1927, p.110)

"Let us suppose...that the movement starts with goods of the third class [goods of higher order P.R.], and that it is in this class that the insufficiency of merchandise and exhaustion of stocks are observed. The prices of these goods may not increase. But merchants begin to buy them in greater quantity to restock their stores, and producers begin to order the products necessary for their manufacture, that is goods of the fourth and fifth class. The prices of goods in these last classes may be the first to rise. ...

The increase of orders and the rise of prices will bring about, during the phase of the business cycle characterized by prosperity, an expansion of production. In this phase manufacturers are induced to increase industrial equipment, to enlarge factories, or build new ones, either because the existing does not make it possible to fill all the orders, or because the rise in prices has led to the expectation of a further rise....." (Aftalion, 1927, p.130)

One does not have to look very long to find such quotations. They are easily found in all the literature concerning cycles - in Marshall, Robertson, Hawtrey, Fisher, Spiethoff, Hansen and all those who have written about the business cycle. It is also characteristic of Keynes up to the *Treatise on Money*. It is a common feature of the monetary and non-monetary approaches to the business cycle.

Characteristic for these argumentations is the following: A small disturbance gets momentum through reactions of persons either as consumers or investors, as firms supplying goods, as firms investing in installment, or as banks granting credit or changing reserves. The reactions comprises prices and quantities as well, with the presumption that as long as there are idle factors of production, quantity reactions prevail during an upswing. For the dynamics there are three important elements: (i) There are always some prices which are sticky. (ii) Decision making and the implementation of decisions need time. (iii) All changes are accompanied by changing moods - in the upswing optimism prevails which itself propels the boom whereas in the downswing pessimism prevails.

For the Keynesian revolution the monetary theories of business cycles were of prime importance. Their argumentation results in a description of economic dynamics resembling the incomes-expenditure approach: Changes of prices and/or quantities will result in changes of incomes, thereby provoking reactions. For these theories it was of prime importance to characterise the propagation mechanism as a change in the flow of payments. It was seen as a change of the flow of incomes or causing a change of stocks. Thereby the distribution of incomes between wages and profits alter. This was important for all authors of the monetary approach as it implied a change in the demand for money, because agents in different social positions have different demand for money holdings. The change of sentiments is also pursued, but without making moral valuations as Marshall, and before him J.S. Mill and many others had done.

The descriptions the authors, like Hawtrey, Robertson, Fisher, and the pre-*General Theory* Keynes gave, look rather realistic. All these theories revert to direct experience of firms and households as they typically get reported in news. Banks do make decisions concerning reserves, firms really change production plans due to new information as households change their consumption and investment plans, whenever they experience a change in income or prices. And it is also true that sentiments are of importance. However, all these dynamic relations as they were reported not only contradict modern economic theory, they were also in contrast to the contemporaneous approach of the quantity theory of money as worked out by Pigou (1917) and Keynes in the *Tract*. There the demand for money is, given a specific institutional structure of the banking system and given the transaction technologies, related to a given nominal income. Within the quantity theory concept the typical *ceteris paribus* question was asked: How will prices react *cet.par.*, if the amount of money supplied changes. This was argued in a way that it would be argued today: The equilibrium amount of money households and firms keep is upset when money supply changes. A new equilibrium will be established when households and firms bring their moneyholdings into their individual equilibrium, thereby changing the price level.

The quantity theory in the Cambridge version must be seen as a simple macroeconomic equilibrium theory, because it links the aggregate amount of money with the price level by asking how much money agents will hold in equilibrium. The amount of money agents hold is related to their incomes, therefore the aggregate total stock of money could be related to aggregate income. However, the income of the agents to which money holding is related must be an equilibrium income, otherwise it does not make sense to speak of money holdings being in equilibrium. The theory thereby assumed that all markets are cleared. Of course there was no precise idea of equilibrium underlying the quantity theory. It was rather the idea of a good average without unemployment, where aggregates remained stable and, because of competition, prices reacted nicely to excess demand. Whenever excess demand is greater than zero, prices will finally rise to equilibrate the markets<sup>12</sup>.

The monetary business cycle theories, on the other hand, took it for granted that a change in the amount of money can influence real economic activity. It therefore was accepted that there are situations in which aggregate demand can be increased by political action. This was always related to the stickiness of some prices, thereby assuming that there are situations realised in which agents are not in equilibrium. These two strands of macroeconomic theory were held by the same authors often in the same text.

The problem for economic theory was, how to bring these two strands together. The quantity theory could not account for the periodic changes of economic activity, particularly it seemed not very helpful in the crisis beginning in 1929. On the other hand, the business cycles theories were theoretically not satisfactory, because their relation to a concept of economic equilibrium was in the dark. This can be seen in the use of the assumption of price stickiness and in the descriptions of the turning points of the business cycle. The first problem relates to

the question of what the assumptions concerning expectations are. Though this point was raised in the decades after the Keynesian revolution against Keynes, it can be shown that it has been always very important. Theories of business cycles were considered unsatisfactory, if the driving force was agents acting erroneously. It was regarded to be important that a theory should not assume acts of agents being irrational. For example this point was raised by Keynes in 1913 against Fisher's theory of business cycles.

The other problem was, how an upswing or a downswing can come to an end, when agents act on information about incomes and prices. Given price stickiness it is not difficult in the context of an income-expenditure approach, typical for the monetary approach to business cycles, to argue the propagation of a positive or a negative disturbance.<sup>13</sup> But how this should come to an end was difficult to argue, because though prices and incomes were rising (falling), agents must have already expected a fall (a rise) of incomes. Usually it was assumed that agents had somehow an idea of normality from which the actual situation has diverged too much<sup>14</sup>.

It was accepted that trading takes place outside equilibrium - or away from the average. To analyse these, reactions of agents to realised positions, to information and to expectations built on information were considered. These reactions were in one way or another plausible and 'realistic'. A few examples:

- (I) When Hawtrey discusses the effect of a rise in the rate of interest, he describes the reactions in the following way:
- "What precise effect will this rise in the rate of interest have on the borrowers? The two principal classes of borrowers are the producers and the dealers. The producers will... find the cost of production of commodities slightly increased. A manufacturer who receives an order from a wholesale dealer will quote a slightly higher price in order to cover his extra cost. If this rise in wholesale prices is reflected in retail prices there will be some slackening of demand, since the national purchasing power remains unchanged. But, in general changes in the rate of interest such as we are considering are too small to affect retail prices immediately...
- But the dealers themselves will be influenced by the rate of interest. One of the special functions of a dealer is to keep a stock or 'working balance' of the goods in which he deals... Now a dealer borrows money to buy goods, and repays the money as the goods are sold. Consequently when his stocks are large his indebtedness to his banker will be correspondingly large... When the rate of interest goes up he will be anxious to reduce his indebtedness... He can reduce his indebtedness if he can reduce his stocks of goods, and he can reduce his stocks of goods by merely delaying replenishment when they are sold." (Hawtrey, 1913, p.61f)
- (ii) Robertson (1926) analyses the cycle in terms of a change of savings due to price increases - or as he called it lacking. He writes:
- "...as the price level rises, even an entrepreneur who merely desires to keep the real value of his Circulating Capital intact will be obliged to expend an increased daily stream of money. It is true that since his money receipts are likely to be increasing at least as far as his money expenditure, it will generally be within his power to provide this increased stream himself; but it is at least as likely that instead of doing so he will increase his personal consumption or his permanent investment, and will apply to the bank for an increased money loan to cover his trade expenses. ..." (Robertson, 1926, p.73)
- (iii) When Keynes in the *Treatise* discusses the effects of an increase of loans, he starts with the following comment:
- "Obviously, the first effect of a new loan by a bank is to increase the deposits of the borrower by the amount of the loan. Now it is uncommon for a borrower to borrow, not for any business or investment purpose, but to meet his personal expenditure on consumption; at any rate, bank loans of this kind are so small a proportion of the whole that we can in general neglect them. Further, it would be unusual to borrow merely in order to add the



money to the savings deposits, since the interest payable by borrowers always exceeds the interest allowed to depositors ...Generally speaking, therefore, the proceeds of a new loan are added in the first instance to the business deposits." (Keynes, 1930, vol.V, p.238)

Arguments like the preceding ones can be found again and again. They were not in line with the quantity theory of money being a theory of equilibrium, as they looked at the transition from one equilibrium to another one<sup>15</sup>. The relation between these two aspects of the monetary approach to macroeconomics was the driving force in the development of monetary business cycle theories. This can clearly be seen in the Robertson's '*Banking Policy and the Price Level*' and in Keynes' '*Treatise of Money*'. The mathematical formulation in these works, i.e. the appendix to chapter V in Robertson, 1926 and the fundamental equations in the *Treatise* are by definition right, i.e. they are accounting identities. However, they were set up to depict causal relations.

This was the way empirical business cycle research could have proceeded, namely to look for typical behaviour of various types of agents, to draw conclusions and to look at data for support or refutation. In this way regularities could have been found (e.g. Spiethoff, 1925). These type of business cycle theories could have resulted in a meaningful research program. For example, it could be inquired whether all the empirical relations which are mentioned in these theories are really such as assumed. But this is not the way economics proceeds. Since the early classics, equilibrium is the most important concept to analyse the economy. It serves as a thought-organising principle. Of course, this may turn out one day as a complete failure, however, regarding the question, what was the contribution of the Keynesian revolution to current economics as it is contained in libraries, taught in universities and used for applied research, one has to use this concept as a point of reference.

#### 4 *The General Theory*

It was not merely a choice between two research programs, of which one became victorious. The business cycle approach, although more realistic, not only was unsatisfactory from the point of view of equilibrium theory, but the underlying concepts were unclear and resulted in contradictions. The challenge for the different business cycle theories were not contradictory data, as one would suppose for an empirical science, rather the theoretical examination of the underlying concepts. This inspection could only be done on the basis of economic theory, that means in line with the equilibrium approach to economics. All the concepts which were used in the business cycle literature were at one instant or another scrutinised to search for contradictory and unclear use of concepts.

This is true for all concepts of macroeconomics which today serve as a basis for macroeconomic research, and which are topical in all basic macroeconomic courses, whether they are built on an IS-LM framework or not. The concepts of income, savings, investment, hoarding, how they are related to each other got scrutinised again and again in the twenties and thirties - to mention a few examples: Are demand deposits part of savings? Does credit creation result in an addition to voluntary savings? How is income as a theoretical concept, as given e.g. in Pigou 1920, related to data of income accounting, and how should a system of national accounting proceed to provide data which is in line with theoretical concepts? The problem of accounting could not be solved without looking carefully at expectations, as without them depreciation and therefore income could not be defined properly. A clear-cut distinction between expected and realised values, as worked out by the Swedish economists, helped to overcome some of the difficulties inherent in the fundamental equations of the *Treatise*.

To overcome the old belief in Say's law it was important to distinguish clearly between consumption and investment on the one hand, and to separate the activities of saving and investment on the other hand. Whereas up to the Keynesian revolution the argument of Smith,

namely that what is saved is consumed as well, was repeated again and again<sup>16</sup>, after the systematic distinction between consumption and investment, it became clear that this saying of Smith is no argument. Of course, having made the distinction does not imply that investment can fall short of savings, but it made it necessary to look at the link between savings and investment. It became clear that economics was in need of such a theory (e.g. Hawtrey in Keynes, vol.XIII, p.136).

However, the '*General Theory*' was the first book in economics which used most of the macroeconomic concepts in the way they are used today. This resulted in a completely different type of analysis compared to all earlier macroeconomic texts. Though it is not a model in a formal sense, i.e. Keynes does not make use of algebra or graphics, nevertheless all the ideas are laid down in the way it became customary in macroeconomics. All economic magnitudes are related to economic behaviour, this behaviour is mostly considered to be maximising, the distinction between realised and expected values is thoroughly taken into consideration, and there is some analysis of the relation between these two types of magnitudes. The macroeconomic concepts are used coherently. It is not possible to find contradictions as can be found in earlier books on business cycles.

The greater clarity was achieved at some cost. Whereas the older monetary business cycle theory tried to deal with complex institutional set-ups, particularly concerning the problem of financial intermediation, the *GT* cut through all these problems by reducing the complexity of the financial system drastically. This applies also to the behaviour of households and firms. Older theories distinguished households of different social groups concerning consumption habits and habits of money holding, the ways incomes are paid out etc. They also distinguished between firms of different forms of ownership, whether they are private business firms - Marshallian firms - or public companies, to argue problems connected with financial intermediation and investment behaviour. All this is absent in the *GT*. Furthermore, there is no analysis of the long run equilibrium. There are only households and firms, money and non-money financial assets. This is the way many macroeconomic models have been set up since the *GT*. The 'reality', as it was depicted in the older theories, and some of the important theoretical questions are left out.

But because of these simplifications, problems became manageable, and could be put into the perspective of economic theory. Namely, assuming utility maximisation and profit maximisation, all households and all firms must be treated alike. This does not imply that social differences do not exist or are irrelevant for macroeconomic problems, however, these differences cannot simply be taken for granted. To give an example for the relevance of these problems: In the pre-*GT* literature it was customary to assume that fixed capital had to be financed by 'real' savings, whereas working capital could be provided by loans. This may have been true institutionally, and perhaps there exist today theories to explain this, but this distinction was one of the reasons for many problems concerning savings, investment and capital.<sup>17</sup>

Most of the ideas in the *GT* had been worked out before, a lot of them by other economists, however, the *General Theory* was the first book to make use of all these theoretical clarifications. Thereby Keynes brought the two strands of macroeconomics together - the equilibrium approach of the quantity theory and the possibility of unemployment from the business cycle theories. Unemployment was not seen as a phenomenon which occurs within a development in the passage of time. The sequence-of-reactions analysis, so typical for the pre-*GT* business cycle theories gave way to a clearer analysis, more in line with basic assumptions of equilibrium analysis<sup>18</sup>.

Of course, it would be ridiculous to say that Keynes was successful in solving all problems looked at in the *GT*. Not only the questions of business cycles and of long run equilibrium remained important, but it is not difficult for a trained economist to find a lot of

deficiencies and unclarity. Take for example expectations. Keynes argues that the stability of an equilibrium depends amongst other things on the diversity of expectations concerning the future.<sup>19</sup> One does not have to be a dogmatic adherent to the concept of rational expectations, to find this methodologically problematic. One cannot even say that he has proved that a stable unemployment equilibrium can exist, nor that he has proved that there can be a temporary equilibrium with unemployment. These are questions one can only ask with the knowledge of economic theory developed after the Keynesian revolution. However, he put the problem into the framework as it is looked at today. Modern macroeconomics can build on the results of the *GT*, independent of the belief of the economist. In this sense the Keynesian revolution brought progress to the science.

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## Notes

<sup>1</sup> This paper was presented at the conference of the European Conference of the History of Economics, Lisbon, February 1996. I thank David Colander for his useful comments.

<sup>2</sup> "One cannot arise from a perusal of this correspondence [between Ricardo and Malthus P.R.] without a feeling that the almost total obliteration of Malthus' line of approach and the complete domination of Ricardo's for a period of a hundred years has been a disaster to the progress of economics." (Keynes, 1933, p.98)

<sup>3</sup> 'To know' in this context means nothing but that some propositions are accepted as being true by a relevant scientific community bound by methodological rules as to which propositions to accept or refute. The notions of truth and knowledge, as they are used here, have nothing to do with 'truth' in any objective sense.

<sup>4</sup> Actually this is only true under a eurocentric position. Peoples without regular contact with Europe, some of which had elaborate systems of calendars, probably had some geocentric worldview until they were conquered by a European power or were brought somehow differently into the reach of European science.

<sup>5</sup> Marshall published his long awaited book about topics which were later dealt within macroeconomic reasoning, 'Money, Credit and Commerce', not before 1923. It remained strictly within the quantity theory at a time when the younger economists already strived to find better answers to the macroeconomic problems of their time. It therefore arose disappointment as it was considered to say nothing new (Edgeworth, 1923, p.204).

<sup>6</sup> As profits are a residual in Ricardo's theory, there is no choice theoretic basis for his theory of profits.

<sup>7</sup> "If there is an extra supply of bullion, bankers and others are able to offer easy terms to people in business including the bill brokers, and consequently there is more money on loan, and consequently people enter into the market as buyers of things, as starting new businesses, new factories, new railways, and so on." (Marshall, 1887, nr.9677) "Now let us suppose that the banks and other lenders of money lend at a different rate of interest, either lower or higher, from that which corresponds to the current value of the natural rate of interest on capital. The economic equilibrium of the system is *ipso facto* disturbed. If prices remain unchanged, entrepreneurs will in the first instance obtain a surplus profit (at the cost of the capitalists) over and above their real entrepreneur profit or wage. This will continue to accrue so long as the rate of interest remains in the same relative position. They will inevitably be induced to extend their business in order to exploit to the maximum extent the favourable turn of events. And the number of people becoming entrepreneurs will be abnormally increased. As a consequence, the demand for services, raw materials, and goods in general will be increased, and the prices of commodities must rise." (Wicksell, 1898, p.105f)

<sup>8</sup> "Q: You mean to say then that there have been periods of rising prices when there has been great prosperity?"

A: Yes, but I think that the prosperity has been caused by other things, and I think the real prosperity has not been greater than it would have been if prices had not risen so fast. I think the rise of prices has caused the apparent prosperity to be much greater than the real; has caused an immense number of incompetent persons to get into the control of a business which they cannot manage except when prices are rising. As soon as prices fall, and sooner or later they must fall, these people fail, and their failure reacts to others and causes widespread distress. That is what I regard as the chief evil of a sudden rise of

prices, the premium it gives to incompetent business men, enabling them to retain the control of concerns which they do not manage well, and which collapse as soon as the artificial support of rising prices is taken away." (Marshall, 1887, nr.9819; similarly nr.9827).

"To attribute this social *malaise* to the fall of prices, instead of to the previous morbid inflation which caused it, is as reasonable as to attribute the headaches which follow a night of feasting and rioting to want of a sufficiently nourishing breakfast, instead of to the bad condition of the digestive organs that took away the appetite for breakfast." (Marshall, 1899, nr.11 781)

9 "Imagine a London banker to acquire his share in them [the additional paper P.R.]. The supply of bank notes which he chooses to reserve in his drawer is always estimated by the scale of his payments; or...by the probable amount of the fluctuations in his stock of notes, which fluctuations are proportionate, or nearly proportionate, to the scale of his payments. So long, therefore, as his payments remain the same (and they will not materially alter while the price of goods suffers no variation, supposing his transactions to retain their former proportion to those of the whole kingdom), he will be perfectly indisposed to hold fifty thousand pounds in bank notes, in the place of each fifteen thousand pounds which he has been accustomed to deem necessary. He will make haste to part with the whole superfluous quantity; he will offer to lend it to any safe merchants, and even at a reduced rate of interest..

...There seems to be only two modes in which we can conceive the additional paper to be disposed of. It may be imagined either, first, to be used in transferring an increased quantity of articles, which it must...be assumed that the new paper itself has tended to create; or, secondly, in transferring the same articles at a higher price." (Thornton, 1802, p.235)

10 Wicksell explicitly denied that his macroeconomic theory of interest and prices is directly related to the problem of business cycles. (See the introduction of B. Ohlin to the English edition of *Interest and Prices* p.viii). His theory of business cycles belongs to the non-monetary branch of business cycle theories.

11 For early American treatment of business cycles see Samuels, 1972

12 There were already some attempts to remain within the framework of equilibrium theory when analysing business cycles. These attempts comprise the theories of Jevons and of Robertson in 1915. They considered real shocks to be decisive for cycles and therefore can be seen as forerunners of real business cycle theories.

13 This argument can already be found in the macroeconomic literature discussing the post-Napoleonic crisis, for example in Attwood, Blake and many others.

14 It was much easier to argue turning points for the non-monetary theories.

15 "Upholders of the quantity theory of money have sometimes been led to argue that prices ought to rise and to fall regularly with the quantity of money and credit in being. The theory has been as often attacked on the ground that experience shows us many examples of prices rising when the quantity of credit and money falls, and *vice versa*. The argument on both sides betray a misconception of the theory. It is indeed sometimes the case that, when prices show a marked rise, there is not at any rate a proportionate increase in the bankers' liabilities, whether deposits or note issues. That occurs at one of these transition periods when credit is expanding. Fluctuating incomes, whether derived from wages or profits, dividends or fees, are swollen by the flood of new credit, but the balances retained in hand by the recipients of these incomes are not immediately increased up to the proportion to which they would approximate with the same incomes in a state of equilibrium. Thus it is only at times of equilibrium..... that the relation of prices and money values to the quantity of credit and money is determined by the individual's considered choice of the balance of purchasing power appropriate to his income." (Hawtrey, 1919, p.45f)

16 Hawtrey, for example, wrote in his book on business cycles: "This purchasing power is exercised in part upon consumable commodities, in part upon capital...Thus the whole income (except such as goes to the working balance) is spent, whether it is saved (i.e. invested), or not." (Hawtrey, 1913, p.34)

17 Machlup (1931) and most of the other German and Austrian authors believed that loans due to money creation were not covered by savings.

18 This is the reason why Hayek was the main challenger to Keynes. He also worked within the equilibrium approach. However, he was not successful in relating his theory to the obvious fact of massive unemployment. (cf. Rosner, 1994)

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