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Reassessment of Keynes's Views on Money

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If we seek to understand Keynes's views on money our primary source of information must be his *Treatise on Money*. This is the book in which he deals with that topic fully. I judge that it is also his most distinguished and important book. The essentials of his breakaway from traditional economics are present in it. Its theoretical work is more finespun and scholarly than that of the *General Theory*. It displays considerable learning. It displays also Keynes's intimate knowledge of the actual processes in the financial world, which he got from his extended experience of participation in it. The book shows his intense realism. It is the combination of theoretical power with his great sense of realism that gives Keynes perhaps a unique position as an economist. The book is judicious and not dogmatic.

My judgment is that historians of thought should, if they are to get things straight, give priority of reference to the *Treatise on Money* as against the *General Theory*. But can historians of thought be relied on to get things straight?

The *General Theory* is more in the nature of a tract. It is less judicious than the *Treatise*, and he left himself more exposed to criticism. Keynes hoped to gain the ear of a wider audience. In fact he did this. But I guess that this was rather because the word went round from person to person that there was something important in the book than that a great many non-economists actually read it. Despite Keynes's general reputation for lucidity, even economists have found the *General Theory* difficult to understand.

Keynes spent a long time in writing the *Treatise*. It was published at the close of 1930. I recall going to stay with him for some days in his country house in 1927 (or it may have been 1928) and reading copious galley proofs of the *Treatise*. I do not think that the fundamental equations had

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yet seen the light. I knew that he intended to make numerous changes in the galley proofs and asked him if it was not rather expensive. From the "Economic Consequences of the Peace" onward he always paid all the costs of production of his own books. He denied that it would be expensive. He said that publishers were in the habit of grossly exaggerating the cost to them of authors' corrections to proofs. (Perhaps things have changed since then?) There is one passage in the *Treatise* which reads "at the time of writing (1926)." On the other hand there are a number of references to the slump of 1930. I well remember his coming to Oxford some time in that year and saying "many people do not realise that we are in the midst of the worst world slump in history."

Thus the background of his writing was somewhat different in the case of the *Treatise* from that of the *General Theory*. It is true that during the twenties the United Kingdom suffered from an unsatisfactory level of unemployment (around 10 percent of those for which statistics were available, which would probably mean around 7 percent on the present basis). The position had been made somewhat worse by the United Kingdom's return to the gold standard in 1925 at a slightly excessive valuation for sterling. But the main emphasis in the book is on curing the trade cycle. The *General Theory*, on the other hand, was written during and after the far greater horrors that were to follow 1930. It was published in 1936. I do not think that he conceived his central idea for it until after intensive discussions on the *Treatise* had been carried on in a seminar at Cambridge for a year or so after its publication; Professor Meade, at that time a visitor to Cambridge from Oxford, participated in these discussions. As I have said, much of the doctrine of the *General Theory* which made such a profound impression is already present in the *Treatise*. There are three main points of difference.

1. There is no position of "equilibrium" in the *Treatise*. I will presently explain this more fully. I sometimes wonder whether the concept of an "equilibrium" is not over used in economics, at least in relation to the constructive propositions that can be made about it. The equilibria of Marshall and Walras are rather tenuous concepts. They are concerned with a static equilibrium, and it is to be noted that in a static equilibrium saving should be set down as zero; this is rather a special case. We do not yet have an agreed concept of a dynamic equilibrium; so perhaps it is a good point that there is no equilibrium in the *Treatise*, rather than the other way round.

2. There is not very much in the *General Theory* specifically relating to money. Keynes is more concerned with the equilibrium of the economic system as a whole. It is, in fact, by the later volume that Keynes can stake out his claim to being the father of macrostatics. This was an absolutely indispensable bridge to economic dynamics. But we must not linger on the bridge for too long. Of course there are many dynamic considerations in

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the *General Theory*, but the formal equilibrium remains static. I pointed this out at a meeting of the Econometric Society in the year of its publication (1936). (This address was reprinted in *Econometrica* [January 1937].)

I notice that my book on the *Trade Cycle*, which deals formally with "dynamic determinants," was published in the same year as the *General Theory*, although later in the year. But then, I knew the *General Theory* from cover to cover by proofreading before it was published. To compare great with small, there is a similarity between the relation of the *Treatise* to the *General Theory* and the relation of my *Trade Cycle* to my *Essay in Dynamic Theory* (1939). The former contains a number of seminal ideas that figured in my later writing, but there is no concept of an equilibrium in it, no "warranted growth rate."

3. There is the question of terminology. In the *General Theory* Keynes bases himself and lays stress on the fact that investment must always and necessarily be equal to saving. In the *Treatise* these magnitudes are taken to be unequal, and many of the arguments of the book turn on their inequality. Of course Keynes knew perfectly well the bookkeeping identity that expost investment must be equal to expost saving, and he states this more than once in the *Treatise*. He is able to postulate an inequality by providing a special definition of income. The components of his national income are those to which we are accustomed, except for entrepreneurial income. When he comes to entrepreneurs, he postulates that entrepreneurial income is always "normal," whether it actually is so or not. "I define the 'normal' remuneration of entrepreneurs at any time as that rate of remuneration which, if they were open to make new bargains with all the factors of production, at the currently prevailing rates of earnings, would leave them under no motive either to increase or to decrease their scale of operations" (1:125). This is a disconcertingly static definition; but we can easily dynamize it by substituting for the last fifteen words the words "gives them a motive to expand output at a rate consistent with dynamic equilibrium."

I rather regret that he changed his stance in this matter in the *General Theory*. We have continued, right to the present day, to feel the need of a pair of terms, over and above the necessarily equal expost investment and saving. Some use the Swedish concept of exante. I prefer Keynes's concepts of the *Treatise* because they refer to the moment of time that is being considered. What people thought they wanted to do in the past is not strictly relevant. I myself have been in the habit of using concepts which are slightly different from those of Keynes, namely, desired saving, which is the amount of saving that, at the particular point of time that we are considering, people wish they had made as compared with the saving that they actually have made, and required investment, which is the amount of capital fixed or working that people would like to have at the same particular point of time as distinguished from what they have actually got.

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Both Keynes's *Treatise* definitions and mine have the merit of concentrating on the particular point of time under consideration.

I fancy that the reason why Keynes abandoned his special definition of normal saving, to the loss, as I hold, in the power of his expository tools, was that he became embroiled in controversies with some critics like D. H. Robertson and Professor von Hayek. The trouble about a special definition of the exante type is that there are liable to be as many definitions of this type as there are authors. There is no freedom of choice of concepts as regards the expost definitions, but there is freedom in relation to exante definitions. Writers criticized Keynes on the basis not of *his* definitions but of their own. There was even a tendency to criticize Keynes because he had not adopted the definition selected by the critic, which was of course absurd; in this matter there is no "right" definition, but only a convenient one. In this way the controversies in which he was engaged became meaningless. And so he thought it expedient to get away from special definitions and stick firmly to the expost identity. Here, I think that he was wrong, and that he bothered his head too much about these controversies, which were destined soon to be forgotten.

I must now tackle the central doctrines of the *Treatise*. The first fundamental equation is: $P = E/O + (I' - S)/R$ (1:135), where P is the price level of liquid consumer goods, R is the volume of such goods, E is the total earnings of the community, but with the earnings of entrepreneurs reckoned as "normal" (see above), O is total output, I' is the cost of production of new investment, and S is total saving, again subject to the special definition of entrepreneurial income. Then there is another fundamental equation (p. 136): $\pi = E/O + (I - S)/O$, where π is the price level of output as a whole and I is the value of new investment.

In these two equations we find set out the direct determinants of the price level. The Keynesian breakthrough consists essentially of the fact that in these equations there is no reference to the money supply. Keynes states in various places that the bank rate or the quantity of the money supply can influence prices only indirectly, namely, by their influence on the values of the terms in the fundamental equations. In certain circumstances, however, they may have a negligible influence only. This is the central point of Keynes's monetary theory.

Recently, when endeavoring to explain how money as such does not enter directly into Keynes's fundamental theory of demand, I have expressed myself as follows. Let x be the fraction of productive resources required to fulfil current orders for investment goods, under the influence of expectations about their yield and the rate of interest, and let y be the fraction of income that people (including firms) desire to save. Aggregate demand exceeds the supply potential if $x + y > 1$, and conversely. There is no reference to money here.

I hasten to add that this does not imply that Keynes did not think the

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amount of the money supply very important. I ought to hasten particularly, being at this moment in Chicago; here there has been new thinking about the money supply. I would not presume to comment on it. Not only in advancing years, but all my life, I have found it difficult to assess the value of new thought of a fundamental character except after a long period. Keynes thought the quantity of the money supply of the greatest importance, and the whole *Treatise* is impregnated with discussions of its influence. I believe that certain followers of Keynes have given an entirely wrong idea of the nature of his work, by suggesting that he ceased to think the money supply important. There was the idea in the minds of some that, now that we can analyze the equilibrium of the economy in real terms, we can put all that stuff about money into the wastepaper basket. On the contrary, there is nothing in the *General Theory* to suggest that Keynes was repudiating all that finely wrought work of his about money in the *Treatise*. There are some minor adjustments and changes of categorizations and terminology. For instance, the "financial bearishness" of the *Treatise* becomes the "speculative motive" in the *General Theory*. It is to be noted that the main doctrines of "liquidity preference" that are set out in the *General Theory* are to be found in the *Treatise*. Keynes's work for Bretton Woods is evidence that Keynes still regarded the money supply as of great importance. His "breakthrough" consisted of new doctrines about the modus operandi of the influence of changes in the money supply, which he deemed to be indirect only, on the activity of the economy and on the price level.

Before leaving the equations, I should revert to the question of equilibrium. The equations evidently do not represent an equilibrium position, except when the right-hand term is zero. By definition, if I' is not equal to S, then producers must be on the move, either increasing or decreasing their output. But prices are not represented as being on the move. Given the size of the gap between I' and S, prices are at a certain fixed level. They will be on the move only if the gap between I' and S is growing, or shrinking. If we take the inflationary case, the upward movement of the entrepreneurs will presumably reduce, and finally close, the gap between I' and S. This means that prices have to fall in the process of adjustment in an inflationary condition, unless there is a countervailing increase in wage rates per unit of productivity. This seems to be what Keynes thought in the *Treatise*, but there is some obscurity.

Finally, attention should be drawn to the fact that the fundamental equations very clearly show demand pull and wages push as separate influences on the price level.

I would next mention two difficulties that I have in regard to the *Treatise*.

1. Keynes seems throughout to regard the value of investment goods as one and the same thing as the price level of securities. I quote a passage to

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illustrate this: "The price level of investments as a whole, and hence of new investments, is that price level at which the desire of the public to hold savings deposits is equal to the amount of savings deposits which the banking system is willing and able to create" (1:143). It may be that this identification of the value of investment goods with the price level of securities is logically correct. The current market valuation of a given security should be taken to be the current market value of the real capital assets that this security represents the ownership of. But do entrepreneurs act accordingly? It may be that they do, to the extent that they finance capital extensions by new issues of securities. But many (perhaps on the national average most) entrepreneurs do not do so. It is doubtful whether each entrepreneur values his marginal investment, which he has to decide whether to make or not, on the basis that the current price quotation on the stock exchange for the security of his firm represents the current value of his existing assets.

2. While I expressed gratification that the two expressions on the right-hand side of the equations are formally independent, and show the influence of wages push and demand pull respectively, it has to be admitted that Keynes seems to regard the amount of wages push as predominantly influenced by the forces at work in the expression for demand pull. Throughout the *Treatise* he speaks of entrepreneurs, on occasions when profit is supernormal (that is, in the language of the *Treatise*, when there is positive profit), "offering" more wages. For instance, it is "the anticipated profit . . . which influences entrepreneurs in deciding . . . the offers which it is worth while to make to factors of production." This attitude continues into the *General Theory*. The attitude seems to be that wages (and rewards to other factors) rise only as and when entrepreneurs think it profitable to offer more. There is no hint of the implacable onward march of the labor unions, even in times of relative stagnation, in demanding and getting higher wages. It is true that he does recognize the possible existence of "spontaneous" wage changes (for example, 1:166 ff.). In volume 2, page 351, he writes: "if there are strong social or political forces causing spontaneous changes in the money rates of efficiency wages, the control of the price level may pass beyond the power of the banking system." But this aspect of things is very much deemphasized. Could it be argued that his views were right at the time, and that the wages push had not come forward so prominently then as it has since World War II? But Professor Hines has shown that in the United Kingdom there has been no correlation between the level of aggregate demand (which would be prepresented by the right-hand term of the fundamental equation), and the rate of wage increases since 1921. Keynes did, however, recognize at various points that wages might be inflexible downward, and indeed thought that no bad thing (*General Theory*, chap. 19). If Keynes had thought more about spontaneous wage increases, considerable modifications of his text would probably have been required.

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Subject to what I have already said, I accept the main principles of Keynes's work. I am a Keynesian.

I would call attention also to the practical effect of certain relatively minor proposals in the *Treatise*.

1. He recommended, I believe for the first time, that central banks should have the legal power to vary reserve requirements. This power was granted the Federal Reserve system some years later; its example has been followed in a number of countries, and, belatedly, nearly a quarter of a century after the Federal Reserve, by Keynes's own country, the United Kingdom.

2. He recommended wider points on either side of the foreign exchange parity. This proposal was adopted when the IMF was set up. Perhaps he had direct influence in bringing this about. There is still active discussion about the desirability of making the spread still wider—what are known as the proposals for a "wider band." I am not altogether clear whether this would work in well with the system of the "adjustable peg" that we now have.

3. He recommended operations by central banks in forward foreign exchange markets. This has been done in recent years.

In the most recent period I have been attempting another little breakthrough of my own, which has been christened by the *Economist* newspaper, in consequence of a letter I wrote to it, "Harrod's Dichotomy." Keynes assumes that a rise of aggregate demand will always have a tendency to raise prices, and conversely. This has been almost universally believed by economists for generations, even when they had no tools of thought that would enable them to define what aggregate demand was. When I wrote the *Trade Cycle* (1936), I was already beginning to have doubts, but felt compelled to toe the line in this matter because of the overwhelming weight of expert opinion. I referred to "wide empirical evidence" (p. 39). But had I really sifted this evidence? "In our experience of the trade cycle," I wrote, "the only very notable exception was the failure of prices to rise in the period from 1925–1929. Such an exception is highly interesting and cries for special investigation." I did add my note of caution: "Some writers indeed have seemed to imply that the proposition (i.e. that an increase of aggregate demand always tends to raise the price level) has even greater strength than this—that it is deducible from certain principles of monetary theory. This, however, is an illusion. Reasons and explanations can be found for the phenomenon; but, without the brute fact to guide them, it is most unlikely that theorists would ever have reached this proposition as a conclusion drawn from general reasoning. On the contrary, they would have been inclined to take the opposite view, for the most general considerations suggest it."

The "dichotomy" is as follows. If aggregate demand is running ahead of supply potential, this will tend to pull prices up. If prices do not rise, there

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will be a vacuum, which nature abhors; it will be just impossible to meet parts of the demand; so prices will have to rise to bring demand into equality with supply. In these circumstances, deflationary policies, designed to reduce aggregate demand, will have the effect of reducing, or, in the absence of wages-push trouble, eliminating, any price increase that is occurring.

But, if initially aggregate demand is *not* above supply potential, it is no longer clear that deflationary policies, so called, will have the effect of reducing or eliminating any price inflation that is occurring. It may even be the other way round. Economic theory, as such, has nothing to say about this, as I stated thirty-four years ago. I must make an exception for the *short*-period effect of reducing the demand for commodities that pass through "perfect markets." In advanced countries these no longer constitute more than a minor part of articles entering into the index of general prices.

For the rest, all depends on whether the majority of commodities are subject to short-period economies or diseconomies of scale. Keynes assumed the normal case to be one of diseconomies. This was in the tradition of Alfred Marshall. But the doctrines of imperfect competition, on which I, along with others, had recently been working at that time made a very great difference. It could no longer be assumed that short-period decreasing returns to scale were the predominant phenomenon. The opposite might well be the case. Keynes himself did not follow closely, or take a great interest in, the work that was being done on imperfect competition in the early thirties.

If a reduction of aggregate demand tends to raise short-period costs in the majority of cases, why should it cause a reduction in the prices asked by suppliers? It could be suggested, I suppose, that, when trade is receding, they start competing more actively with each other and indulge in cut-throat competition. Students of the theory of decision making by firms will be doubtful whether this is the case, at least in countries of the general industrial structure of the United States or the United Kingdom. Those who think that monetary and fiscal "deflation" must have a salutary effect in checking price increases may then bring forward the "Phillips' curve." As I have said, this has been shown not to be valid for the United Kingdom recently; I do not know about the United States. But even if it were valid, the consequent abatement in the rate of price inflation might be offset, or more than offset, by other factors. It is to be remembered that some of the weapons of "deflation," for example, high interest rates and indirect taxes, tend to raise costs. I would not go so far as to affirm that these measures of "deflation" usually have a price-inflationary effect. I will rather maintain the neutrality of a strict agnostic. Things may go one way or they may go the other.

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What is now happening in the United States may be an important test case. The stern deflationary measures undertaken by the U.S. authorities in 1969 seem indeed to have had an effect in reducing the rate of increase of demand during that year. But at the close of that year they had still had no effect in reducing the rate of price increases.

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