

## Keynesian Monetary Theory and the Cambridge School\*

The monetarist revival of the last decade and more has been accompanied — especially in recent years — by a renewed interest in the nature of the quantity theory before Keynes. In this connection it seems to me that modern-day adherents of this theory have claimed too much for it — and correspondingly too little for the Keynesian theory.<sup>1</sup>

The exaggerated claims for the quantity theory have expressed themselves in the attempt (especially by Milton Friedman) to present Keynes' monetary theory not as a new theory, but as a variation on the Cambridge cash-balance theory. It is this contention that I shall here examine — and, on the basis of this examination, reject. And lest I be misunderstood (though there is really no reason that I should) I should like to emphasize at the outset that this examination should not be interpreted as a criticism of the Cambridge school. For I would certainly consider it unjustified to criticize these economists for not having fully understood and integrated into their thinking what we have succeeded in learning

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The reader is asked to keep in mind that this paper was delivered as the opening address of the conference — and to excuse accordingly various facetious remarks.

<sup>1</sup> In what follows, I shall make free use of Patinkin (1969, 1972a, and 1972b).

only in the course of the subsequent development of Keynesian monetary theory. My criticism is only of those who make exaggerated claims for the Cambridge economists.

Before embarking on this examination, I would like to say a few words about my general approach to doctrinal history — which is certainly not an unusual one. This approach can be succinctly summarized by the statement that isolated passages do not a theory make. Instead, one of our major concerns in the study of the history of theory is the determination of the extent to which ideas expressed at various points in a work are integrated into — and hence really part of — its main theoretical framework. And this accordingly will be my major concern.

Another point that I should clarify at the outset is my use of the terms “quantity theory” and “Keynesian theory”. By the first of these I mean — quite pragmatically — the monetary theory expounded at the end of the nineteenth century and through the late 1920s by Irving Fisher in the U.S. and by the Cambridge school (Marshall, Pigou, Robertson, Lavington — and the younger Keynes).

By “Keynesian monetary theory” I mean the one developed in the *General Theory* and the literature to which it gave rise — though I should note that the aspect of the theory that is my primary concern here (namely, the treatment of money from the viewpoint of the choice of an optimum portfolio), is in some respects more precisely developed in Keynes' *Treatise on Money* and Hicks' “Suggestion for Simplifying the Theory of Money”.<sup>2</sup> Insofar as the later development is concerned, I have in mind in particular the work of James Tobin and his colleagues. And in a small voice I add: and Milton Friedman too. A small voice — because I do not want right at the beginning of this Conference to run the risk of setting off a violent argument. Nor do I want to offend the man who is really responsible for my being here: for surely you would not have brought me all the way from Israel to speak on this ancient subject in a Conference devoted to such pressing matters as European monetary integration, commercial bank behavior, and U.K. monetary policy in the 1970s —

<sup>2</sup> KEYNES (1930, I, pp. 36, 140-46, 248-57); HICKS (1935). SHACKLE (1967, pp. 222-27), however, claims that there are important differences between these two treatments.

were it not for the reawakened interest in the doctrinal history of the quantity theory that has been sparked in recent years by Friedman's repeated and provocative assertions about its alleged nature.

By the foregoing definition of the quantity theory, I obviously do not mean to imply that Fisher and the Cambridge school are coterminous with the quantity theory. Indeed, at the same time that they were doing their work, Léon Walras in Switzerland was developing his version of the quantity theory in terms of the *encaisse désirée*. And Knut Wicksell in Sweden (whom I continue to regard — by virtue of his own repeated declarations — as a quantity theorist) was making his invaluable contribution to our understanding of the way the quantity theory manifests itself through the interest-rate mechanism in an economy with a banking system.

But since neither Walras nor Wicksell had the sense to write in English, the sad fact — which at the time prompted the gentle chiding of Myrdal [1939 (1933), p. 8] about “the attractive Anglo-Saxon kind of unnecessary originality” — the sad fact is that, say, Wicksell's work did not become known in this country until the mid-1920s. Furthermore — if I make the proper inference from Keynes' *Treatise* (I, pp. 186-88, 196-99, and especially p. 199, footnote 2) — the development of Keynes' thinking even then was not influenced by Wicksell. For this reason the work of Walras and Wicksell has no bearing on the question that concerns us: namely, the relationship of Keynesian monetary theory as here defined to the quantity theory that Keynes knew and, indeed, at one time helped to develop.

I would like to emphasize one further point: what interests me now is monetary theory, not monetary policy. These represent two different spheres of discourse. And whatever the relationship between the two, it is clearly not a one-to-one correspondence: different policy recommendations can emanate from the same conceptual theoretical framework; and different frameworks can lead to the same policy recommendation.

Some of the clearest examples of this can be taken from the monetary field itself. Thus those of us who studied at Chicago under Henry Simons did not need the conceptual framework of the *General Theory* in order to vigorously advocate government deficits to combat depressions; for quite independently of Keynes — and, indeed, before the *General Theory* — Simons taught this

to his students on the basis of the conceptual framework embodied in Fisher's  $MV=PT$ . Indeed, Simons taught us not to suffer patiently those conventional souls who continued to preach the righteous orthodoxy of a balanced budget even in the face of mass unemployment.

I might digress for a moment to note that Simons was far from being a voice in the wilderness at that time in the U.S. — and that there were then also similar voices in England. Thus in a most remarkable passage in work systematically based on the quantity theory, Pigou (1933, p. 213) made the distinction that only Keynesians are supposed to make between the efficacy of monetary policy in countering expansionary forces in the economy by raising the bank rate sufficiently — and the limitations to which monetary policy is subject in countering “a contraction in aggregate money income”. For though it is “always possible for the Central Bank, by open market operations, to force out money into balances held by the public ... there may be *no* positive rate of money interest that will avail to get this money used”. (A “liquidity trap” at zero interest!) In such circumstances, Pigou continued, a purely monetary policy is “bound to fail. If, however, at the same time that the banking system keeps money cheap, the government adopts a policy of public works, the risk of failure is greatly reduced”.<sup>3</sup>

Thus both quantity theorists and Keynesians — each from their own conceptual framework — advocated policies of combating unemployment by public-works expenditures and/or deficit financing. Conversely — and once again I must speak in a small voice — the common conceptual framework of most monetary theorists today — Friedman as well as Tobin — is (as I shall argue) the Keynesian one; but this has clearly not precluded the emergence from this framework of quite different policy recommendations, based both on different political philosophies and on different interpretations of the empirical findings.

[As an aside, I might quite frankly note that what generates in me a great deal of skepticism about the state of our discipline

<sup>3</sup> Italics in original. Actually, this passage is not as Keynesian as it sounds: for Pigou presents his argument in the Wicksellian terms of the effect of such public-works expenditure on the difference between the actual bank rate and the “proper” one. At the same time, he does recognize that a decrease in public-works expenditures “directly contracts to [the?] real demand for labour” (Pigou, 1933, pp. 213-14, especially footnote 1).

is the high positive correlation between the policy views of a researcher (or, what is worse, of his thesis director) and his empirical findings. I will begin to believe in economics as a science when out of Yale there comes an empirical Ph.D. thesis demonstrating the supremacy of monetary policy in some historical episode — and out of Chicago, one demonstrating the supremacy of fiscal policy.]

In any event, the examples of Simons, Pigou, and others have led me to suspect that the real Keynesian Revolution took place not in the sphere of economic policy (where changes were already occurring in the early 1930s), but in that of economic theory. I suspect that the real change wrought by Keynes was in the conceptual framework from which we viewed the problems of employment, interest, and money. But that is a question that I shall defer for discussion on another occasion.

### 1. On the Meaning of a New Theory

Let me return to our main question. Consider the familiar representation of the Keynesian model in terms of a simultaneous analysis of the markets for commodities and money. I think that everyone would agree that the conceptual framework of effective demand that Keynes developed to analyze equilibrium in the commodity market was indeed a new one (leaving aside the case of Kalecki, and perhaps one or two other possible precursors). The question is whether the same can be said for the conceptual framework of liquidity preference that Keynes developed to analyze equilibrium in the money market. In order to answer that question, we must first clarify what we mean by a “new theory”.

Those of us who live in Jerusalem need not be reminded of the words of a wise man of our city — many, many centuries ago — that there is nothing new under the sun. Clearly, every theory advanced at one point in time has some antecedents in earlier theories. Nevertheless, there are stages in the development of a science where, by consensus, a “new” theory is said to develop. And one of the major questions discussed by philosophers and historians of science are the characteristics which justify calling a theory “new”.<sup>4</sup> Some related questions that I have already alluded to are the difference between “the asides” referring to an idea in

<sup>4</sup> Cf. THOMAS S. KUHN (1970); see also JOSEPH AGASSI (1968). See also G. J. STIGLER (1955).

the antecedent theories, as compared with its “systematic development” in the “new theory”; and the difference between “mentioning an idea in passing” or “as an aside”, as compared with “integrating it into one’s thinking”.

By its very nature, questions of this type are not susceptible of hard-and-fast answers. The general type of answer that has on occasion been given — and that I would like to give here — is that a theory is “new” if it deals in a different manner with one of the central concepts of the science. Similarly, it is “new” if it stimulates concentrated research along hitherto neglected directions.

Now, that does not take us very far — for it has merely replaced the mystery of “new” by that of “central” and “neglected”. Still I think it can help us on the question now at issue.

Before turning to this issue, I would like to illustrate these distinctions in terms of another recent episode in the history of economic theory — namely, the development of Friedman’s permanent-income hypothesis. As we all know, the essence of this theory is that the individual’s current consumption depends not on his current (or “measured”) income, but on his wealth or its surrogate, permanent income.

Now, in a sense this theory can be said to be incorporated already in Fisher’s 1907 analysis in his *Rate of Interest*<sup>5</sup> of the individual maximizing his utility over two periods subject to the present value of his income stream. And, indeed, by his generous references to Fisher, Friedman (1957, p. 7) even encourages us to think in this way.

Even more to the point is Hicks’ detailed and systematic 1939 discussion in his *Value and Capital* of the meaning of income — to which Friedman also refers (1957, p. 10, footnote 4): for in this discussion Hicks addressed himself directly to the question of the proper measure of an individual’s income during a given period of time — and explains why, in a non-stationary economy, “we should not regard the whole of his current receipts as income” (1939, p. 172). Instead, Hicks went on to explain, “the calculation of income consists in finding some sort of *standard* stream of values whose present capitalized value equals the present value of the

<sup>5</sup> This analysis is essentially repeated in FISHER’s later *Theory of Interest* (1930).

stream of receipts which is actually in prospect" (*ibid.*, p. 184, italics in original).

But despite these discussions, the earlier Keynesian literature — and the related econometric studies in particular — all analyzed consumption as a function of current income — with no one (including — to the best of my knowledge — Hicks) criticizing this procedure. And even after the postwar failure to predict consumption correctly<sup>6</sup> led to the introduction of additional variables into these functions — including lagged income, and even lagged consumption — there was no full understanding of what this meant. Thus, for example, Klein and Goldberger (1955, p. 8) explained their introduction of lagged consumption into the function in terms of their contention that "consumer behavior tends to be repetitive to some extent".

Now "all" that Friedman did was to infer from Fisher's analysis that the proper measure of income for use in the consumption function is an estimate of permanent income provided by an average of income over several periods of time. (Friedman's rationalization of this procedure is far more sophisticated — as is also the average he uses — but for our present purposes we can look at it in this oversimplified way.) But this "simple suggestion" — as well as some related ones — sufficed to bring about a revolution in the way economists viewed the process of consumption, and to stimulate accordingly concentrated empirical research along hitherto neglected directions. Consumption has never been the same since.

This episode illustrates, better than any other that I can think of, one of the fundamental facts of the history of ideas: namely, that in general the full implications of a set of ideas are not immediately seen. Indeed, as frequently noted, if they were, then all mathematics would be a tautology; for its theorems are implicit in the assumptions made.

I think that all of us must be aware of these aspects of *la condition humaine*. I am sure all of us will empathize with my colleague Nissan Liviatan, who in one of our recent departmental seminars quietly answered the criticism that something he said was

<sup>6</sup> The role of these failures in generating the new theories of consumption can well be interpreted in terms of Kuhn's emphasis on the general feeling of dissatisfaction ("crisis in the scientific community") that leads to the development of a new theory (KUHN, 1970, Chapters 7-8).

a tautology with the words: "Whether or not something is a tautology depends on how fast you think".

Similarly, I am sure that all of us have had the experience of saying in the course of our work on a certain problem: "Now why didn't I see that before?" If we are fortunate, we say this at some later stage of the work when we indeed achieve that "moment of truth" that enables us to see the deeper meaning of what we have done. And if we are less fortunate, we say it only after someone else has pointed it out.

## 2. The Novelty of Keynesian Monetary Theory: Stocks and Flows

This is the viewpoint from which I approach the question of the novelty of Keynesian monetary theory.

To begin with, I think that we are all agreed that one of the central distinctions of economic theory is that between stocks and flows. Correspondingly, what I would consider to be one of the hallmarks of Keynesian monetary theory is the sharp distinction it draws between the two sets of decisions an individual has to make: the decision as to the forms in which to hold his stock of wealth at a given instant of time, and particularly the amount to be held in money; and the decision as to the rate at which to add to this wealth over time — i.e., the decision as to the flows of savings and investment.

The Keynesian approach has led to the development of the theory of the demand for money as part of a general theory of the choice of an optimum portfolio of assets. Correspondingly, the emphasis of this theory is on the optimal relationship between the stock of money and the stocks of other assets, as influenced primarily by the alternative rates of return available on these assets. Other determinants of this demand are the total wealth of the individual (which defines the wealth restraint that must be satisfied by the portfolio of assets) and the flow of income (which is the major determinant of the transactions demand for money).<sup>7</sup>

All agree that this conceptual framework is quite different from that of Fisher. The question is whether it is also different

<sup>7</sup> This approach has received its most formal development at the hands of JAMES TOBIN (1955, 1963, 1969) and, more recently, DUNCAN FOLBY and MIGUEL SIDRAUSKI (1971). The individual in this development is conceived as making his optimizing decisions while being subject to a wealth (stock) restraint as well as an income (flow) restraint.

See also GURLEY and SHAW (1960).

from that of the Cambridge school. (I might incidentally note that the sharp contrast that is traditionally drawn between the "mechanical" Fisher and the "behavioristic" Cambridge economists is — in my opinion — largely a Cantabrigian tale; but the discussion of that question too must be deferred to another occasion.) Let me, then, contrast the foregoing conceptual framework with that of the Cambridge school — and with that of Keynes of the *Tract on Monetary Reform* in particular.

There is no doubt that Keynes of the *General Theory* is at one with Keynes of the *Tract* (1923, pp. 78-79) in taking as his point of departure the individual's demand for money holdings. Thus in his exposition of the *Tract* [which he explicitly bases on Marshall (1923) and Pigou (1917-18)] Keynes wrote that the demand for real "purchasing power" in the form of money holdings depends

partly on the wealth of the community, partly on its habits. Its habits are fixed by its estimation of the extra convenience of having more cash in hand as compared with the advantages to be got from spending cash or investing it. The point of equilibrium is reached where the estimated advantages of keeping more cash in hand compared with those of spending or investing it about balance. The matter cannot be summed up better than in the words of Dr. Marshall (1923, p. 78).

And here Keynes quotes at length from Marshall's well known discussion in his *Money, Credit and Commerce* (1923, pp. 44-45) — which goes back to much earlier statements — about "the fraction of their income which people find it worth while to keep in the form of currency", as well as Marshall's example of an economy whose inhabitants "find it just worth their while to keep by them on the average ready purchasing power to the extent of a tenth part of their annual income, together with a fiftieth part of their property".<sup>8</sup> Keynes also notes — when referring to Marshall's discussion of the antecedents of this approach in the writings of Petty and others — that

in modern conditions the normal proportion of the circulation [of money] to this national income seems to be somewhere between a tenth and a fifteenth. (Keynes, 1923, p. 79, footnote 1).

<sup>8</sup> These passages are cited at length and further discussed in the Appendix referred to in the opening footnote of this paper.

Actually, the clearest statement of the Cambridge approach — though one that Keynes does not cite, possibly because he was restricting himself to the writings of his teachers, as distinct from his contemporaries — is that of Lavington,<sup>9</sup> who also based himself on Marshall. After discussing "the general principle on which an individual distributes his resources among their various uses", Lavington wrote:

Resources devoted to consumption supply an income of immediate satisfaction; those held as a stock of currency yield a return of convenience and security; those devoted to investment in the narrower sense of the term yield a return in the form of interest. In so far therefore as his judgement gives effect to his self-interest, the quantity of resources which he holds in the form of money will be such that the unit of resources which is just and only just worth while holding in this form yields him a return of convenience and security equal to the yield of satisfaction derived from the marginal unit spent on consumables and equal also to the net rate of interest (1921, p. 30; the reference to Marshall is on p. 27).

I might note that no such passage appears in the first (1922) edition of Dennis Robertson's celebrated little volume on *Money* in the Cambridge Economic Handbooks series though there is one in the third (1929) and later editions.

At first sight, these passages seem to indicate that the conceptual approach of the Cambridge school did not differ much from that of the later Keynesian monetary theory. What I shall, however, now show is that this is not the case: First of all, there are some substantive differences — in the description of the optimum portfolio — though less so with respect to Lavington than the others. Secondly — and more important — the Cambridge economists did

<sup>9</sup> FREDRICK LAVINGTON (1881-1927) began his university studies (at Cambridge) relatively late (in 1908) after eleven years service in a bank. He began his academic career at Cambridge in 1918, after a further period in administrative work. This late start — as well as his illness and early death — undoubtedly helps explain why his role in the development of Cambridge monetary thought was less than it otherwise would have been. But I suspect that an at least equally important factor was Lavington's self-effacing outlook on his own work as reflected in his favorite saying that "It's all in Marshall, if you 'll only take the trouble to dig it out".

Cf. the obituaries of Lavington by H[arold] W[right] and C.R. F[ay] in *Economic Journal*, XXXVII (1927), 503-505. [I am indebted to Lord Robbins for the identification of Harold Wright, and for the information that Wright was the author of the Cambridge Economic Handbook on *Population* (1923).

not recognize the full implications of the optimum-portfolio approach to monetary theory; they did not really integrate it into their thinking. In particular, as I shall show in the next section, they failed to take account of the implications of this approach at the appropriate points in their discussions.

By my first point I mean that Cambridge monetary theory did not draw the sharp and basic distinction Keynesian theory draws between stocks and flows — and sometimes even indiscriminately interchanged “wealth” and “income”. Thus, it is not clear from the foregoing passages whether the Cambridge economists conceived of the individual as holding a quantity of money that is optimum with reference to his stocks of other assets, or optimum with reference to his income, or optimum with reference to some combination of the two.<sup>10</sup>

In order to prevent any misunderstanding, I must emphasize that in actual fact the individual's holdings of money should be optimum with respect to both his wealth and his income. This, after all, is the view implicit in Keynes' liquidity-preference function  $L_1(Y) + L_2(r)$  — and explicit in the presentation of the demand for money by Tobin (1955, p. 208; 1969) and Foley and Sidrauski (1971, pp. 30-31). My point is, however, that some Cambridge economists (Pigou, Robertson, and possibly Lavington) expressed the demand for money as a function of income — without referring at all to wealth; and that even those who did refer to both wealth and income (Marshall and Keynes of the *Tract*) did so in a way that does not reveal awareness of the basically different roles these magnitudes play in determining the demand for money: namely, that tangible wealth is the variable that constitutes the total budget restraint on the holding of assets, including money — so that an increase in wealth generally results in increased holdings of all assets; whereas income is one of the relevant variables explaining the (transactions) demand for money in a portfolio of a given size — so that an increase in income increases the demand for money, at the expense of other assets.

As further evidence on this point, consider Marshall's statement that the “relation between the volume of this currency and the

<sup>10</sup> Some of the evidence on which this paragraph is based has been provided by the quotations from Marshall and Keynes above; fuller documentation is provided in the Appendix referred to in the opening footnote of this paper.

general level of prices may be changed permanently by changes in ... population and wealth, which change the aggregate income” (1923, p. 45). Marshall is clearly assuming here that an increase in wealth increases the demand for money only by first increasing the subsequent flow of income, and hence the transaction needs for money; there is no awareness here of the possibility that an increase in wealth may directly increase the demand for money as one of the assets in which form this wealth is held. In brief, if in modern monetary theory we sometimes use income as a proxy for wealth, here Marshall is using wealth as a proxy for income.

Significantly enough, my criticism on this point would seem to be related to one that Keynes of the *Treatise* makes of the Cambridge school, including explicitly Keynes of the *Tract*: namely, that the Cambridge equation  $P = kR/M$  — in which  $R$  represents “the current income of the community” — can explain the demand for income (or demand) deposits, which are held for transactions needs; however, contends Keynes, it does not explain the demand for savings (or time) deposits. This demand, too, can be said to depend on the “resources” of the individuals; “but resources in this connection ought not be interpreted, as it is interpreted by Prof. Pigou, as being identical with current *income*” (1930, I, pp. 231-32, italics in original).

I wish I could go on to say that Keynes explicitly states here that “resources” in this connection should be interpreted as wealth. Unfortunately for me, he does not; but that this is what he meant is, I think, quite clear from Keynes' analysis earlier in the *Treatise* of the holding of saving deposits, in which he explicitly relates these holdings to the individual's total wealth, and not to the “current increment” to this wealth (1930, I, pp. 140-41).

I must admit that Keynes' criticism here does not apply to Lavington, who explicitly stated that that part of the demand for money that is held as a contingency reserve changes “in some measure independently of the volume of payments” (1921, p. 33). On the other hand, Lavington — unlike Keynes of the *Treatise* — does not relate this contingency reserve to the individual's wealth; nor does he relate its magnitude to the price of the securities that can be held as an alternative (Keynes, 1930, I, pp. 141-43). Instead Lavington speaks only in general terms of the magnitude of this reserve being “regulated largely by the general level of confidence” (1921, p. 33). It is also significant that Lavington himself considers

his description of the demand for money as a contingency reserve to be "rather different from (though not inconsistent with) that laid down by the Quantity theory" (1921, p. 32). Finally, it is significant that though Robertson (1940, pp. 92-93) defends the Cambridge equation against Keynes' criticisms here, he (Robertson), too, makes no mention of Lavington in this context.<sup>11</sup>

### 3. The Novelty of Keynesian Monetary Theory: The Recognition of the Implications of the Optimum-Portfolio Approach

The preceding discussion has explicated the distinction between Keynes and the Cambridge school that is reflected in their respective treatments of stocks and flows. A related distinction manifests itself in the already-noted fact that despite its description of an optimum portfolio, the Cambridge school did not realize the full implications of the portfolio approach to monetary theory. Conversely, it is the systematic application of this approach that is the hallmark of Keynesian monetary theory.

This distinction reflects itself, first of all, in the way these two approaches analyze the effects of a monetary increase on the economy. Keynesian theory analyzes the initial impact of this increase on the balance sheet of the individual: it emphasizes that in order to persuade the public to hold a portfolio with such an increased stock of money, the rates of return on the other assets in this portfolio must fall. That is, stock equilibrium can be achieved now only at lower rates of return on these other assets. This decline in interest and other rates of return then increases the demand for the flow of consumption and (primarily) investment goods, thus disturbing the equilibrium in the commodity-flow markets, and thus causing an increase in output and/or prices (depending on the state of unemployment).

In brief, Keynesian theory analyzes the impact of a monetary increase in terms of the substitution effects that it generates. I am sure that it will come as no surprise to anyone if I say that Keynesian economics is to be criticized for this concentration on the substitution effects, to the exclusion of the possible wealth — or real-balance — effect. For though there are indeed cases in which

monetary changes do not generate a wealth effect (namely, some open-market operations),<sup>12</sup> there are other cases (namely, monetary changes generated by deficit financing) in which it does.

On the other hand, I cannot think of a case in which a monetary change generates only a wealth effect, and not a substitution effect. And it is the fact that the Cambridge school nevertheless frequently analyzed a monetary change precisely in this way that distinguishes it so sharply from the later Keynesian economics.

Ironically enough, this distinction is clearest from a passage by Keynes himself in the *Tract* that reads as follows:

When people find themselves with more cash than they require ..., they get rid of the surplus by buying goods or investments, or by leaving it for a bank to employ, or, possibly, by increasing their hoarded reserves [(1923), pp. 75-76; see also (1911)].

Thus Keynes here conceives of the individual as directly using his "cash surplus" to buy more investment goods: the individual need not first be induced by a lower rate of interest to do so. Similarly — and even more to the point — Keynes conceives the individual as directly increasing his "hoarded reserves" — and makes no mention whatsoever of the variation in interest required to induce him to do so. In this way Keynes fails to realize the full implications of his own description (cited above) of "equilibrium [as being] reached where the estimated advantages of keeping more cash in hand compared with those of spending or investing it about balance" (1923, p. 78). He does not recognize the fact (that he was later to emphasize so systematically in the *General Theory*) that the monetary increase will disturb the foregoing balance at the margin — and that the individual's holdings of money and other assets can accordingly be in equilibrium once again only at a lower rate of interest.

The lack of appropriate references in the Cambridge literature to the dependence of the demand for money on interest is of great significance in the present discussion, not because such a dependence is necessarily of empirical importance, but because the recognition of such a dependence seems to me to constitute a critical and unambiguous indicator of whether the Cambridge economists really

<sup>11</sup> For further details, see Appendix referred to in the opening footnote of this paper.

<sup>12</sup> Cf. PATINKIN (1965), Chapter XII: 4, especially p. 294, footnote 23.



understood the analytical apparatus they described. Let me then provide some additional instances in which their writings fail to indicate such a recognition.

Thus, despite what I have said above, Cambridge economists did indeed assign an important role to changes in the rate of interest in their analysis of the effects of a monetary increase. But the way in which they discussed this role is itself evidence of how different their conceptual framework really was from that of the Keynesians. In particular, Marshall, Pigou, the younger Keynes, and other quantity theorists all analyzed the effects of a monetary increase that reflected itself in the first instance in an increase in bank reserves. Indeed, this was the major case they considered. They argued that the resulting excess reserves would lead to an increased desire on the part of banks to make loans, hence to a decrease in the rate of interest<sup>13</sup> (Wicksell's "money rate" or "bank rate", though the Cambridge economists did not describe it in these terms), hence to increased borrowings, hence to increased demand for goods by the borrowers, and hence to a rise in prices.

Now, the interesting aspect of this description of the adjustment process is that none of these Cambridge economists even alluded to the fact — implicit in their analyses of the demand for money cited above — that the changes in the rate of interest would affect not only the amount of the public's borrowing, but also the quantity of money it chooses to hold.

A similar picture emerges when we consider the instances — unfortunately few — in which Cambridge economists supplemented their theoretical monetary analysis with empirical observations.

Thus Pigou (1929, pp. 163-72) tried to apply to British data for the period 1878-1914 the same techniques used by Carl Snyder (1924) in his study of the equation of exchange for the U.S. Pigou concludes from the data that the higher price level of 1914 as compared with 1878 was the result of a higher velocity of circulation at that time (1914). Now, what is interesting about Pigou's discussion is that he does refer to the possibility that velocity is increased by

an increasing price level [by Pigou's data (*ibid.*, p. 592) — though he does not explicitly refer to them — prices had been *falling* before 1878, and had remained *constant* in the period 1912-1914, which could have explained part of the higher velocity in the latter period]. On the other hand, Pigou does not refer to the possible effect of changes in the rate of interest on velocity [though it must be conceded that the yield on consols in 1914 (3.3%) was only slightly higher than in 1878 (3.2%)] (*British Historical Statistics*, p. 455). Thus the evidence here is ambiguous.

The situation with reference to Keynes is clearer. In his *Tract* (pp. 83-84) Keynes compares the data for prices and money supply in October 1920 with those for October 1922 — and concludes that his  $k$  (which, of course, equals the Cambridge  $KT$  in the equation  $M=KPT$ ) had increased significantly during this period. Now, Keynes does mention (though as the effect of the increase in  $k$ , and not as its cause) the sharp (33%) decline in the price level during this period. On the other hand, he does not cite as a possible explanatory factor the fact that the yield on consols fell from 5.3% in 1920 to 4.4% in 1922 — or that the maximum rate on three-month bills fell from 6.5% in 1920 to 2.7% in 1922. Of course, one might say that in accordance with Fisher's distinction between nominal and real rate of interest, this decline in nominal interest reflected in part the fact — which Keynes did mention — that the price level was declining. But it would be carrying things too far to try to explain away in this way Keynes' failure even to mention the rate of interest in this context.

Furthermore, in this immediately following discussion about the ways to stabilize  $k'$  (i.e., the real value of the demand for current deposits), Keynes states that "a tendency of  $k'$  to increase may be somewhat counteracted by lowering the bank-rate, because easy lending diminishes the advantage of keeping a margin for contingencies in cash" (*ibid.*, p. 85). Now this sentence can be interpreted as reflecting the effect on the demand for money of the more ready availability of money substitutes like easy credit facilities. But however it is interpreted it will not yield a reaffirmation of the contention that, *ceteris paribus*, lowering the rate of interest causes an increase in  $k$  — and hence in the real amount of money demanded.

I might note that Pigou and Keynes are representative of what seems to have been a systematic tendency of quantity theorists

<sup>13</sup> This can be interpreted as reflecting the optimum-portfolio adjustment of the banks; but the Cambridge economists did not present such an interpretation — nor should they be expected to have done so. Once again, however, LAVINGTON (1921, pp. 30-31) is something of an exception; see Appendix referred to in the opening footnote of this paper.



to explain observed variations in the velocity of circulation in terms not of the rate of interest, but of variations in the rate of change of prices (Patinkin, 1972a). Keynes' procedure on this score is particularly enigmatic: for in his description of the post-World-War I inflations in his *Tract on Monetary Reform* (pp. 45 ff.), he provides a precise analysis of the influence of a high rate of increase of prices in causing the public to develop "economizing habits" with reference to its demand for money; yet in his systematic presentation of the Cambridge demand for money (*ibid.*, pp. 78 ff.) he does not mention this factor at all, but does analyze the influence of the rate of interest. In this discrepancy, too, I see additional evidence of the failure of the Cambridge economists to integrate the different elements of their monetary theory into their thinking.

A similar difference characterizes Pigou's analysis of the trade cycle (1929). Changes in the velocity of circulation play an important role in this analysis — but they are never related to the concurrent changes taking place (according to Pigou) in the rate of interest. Instead, the changes in velocity are attributed solely to the anticipation of price changes — and to changes in "confidence". A similar statement holds for Lavington's analysis (1922). As a side remark, I might also note that the emphasis that both these writers placed on "confidence" makes it clear that — in contrast with the "modern quantity theory" — they did not think of velocity — and hence the demand for money — as a stable function of stipulated economic variables.

I would like to conclude this examination of the Cambridge literature with a more general observation on its nature that also indicates its failure to realize the full implication of its conceptual framework. It seems to me that if an economist has a full understanding of the portfolio approach to monetary theory, then one of the natural questions he will be led to ask is about the effects on the rates of returns of the various assets of a shift in tastes with reference to the forms in which individuals wish to hold their assets (of which Keynes' shift in liquidity preference is the archetype). It should be emphasized that such a shift will affect the rate of interest (and rates of return in general) even under conditions of full employment and perfectly flexible prices.<sup>14</sup> Correspondingly, the complete absence from this literature of an

<sup>14</sup> See PATINKIN (1965), Chapter X: 4.

analysis of such a shift in taste — in contrast with the attention paid to the effects of a shift in tastes with respect to the desired level of  $K$  — is to me clear evidence that the Cambridge quantity theorists did not really approach monetary problems from the viewpoint of an optimally composed portfolio of assets.

#### 4. Concluding Remarks

My conclusion from the evidence presented here is that the conceptual framework of the Cambridge school was not really the Keynesian one described in the opening section of this paper: namely, a framework that conceives of the individual as deciding on the amount of his money holdings as a component of a portfolio of assets that is optimally composed with reference to the alternative rates of return available on these assets. A framework that (in contrast with the Cambridge school) distinguishes between the initial *stock* (or balance-sheet) adjustments generated by a monetary change, and the subsequent effect on the demand for *flows* of commodities of the changes in rates of interest generated by these adjustments.

One indication of this fact is the failure of the Cambridge school to analyze the effect on the equilibrium rate of interest of a shift in the tastes of the individual with reference to the desired asset-composition of his portfolio. Another indication is that despite the fact that the Cambridge school referred to the influence of the rate of interest on the demand for money, it did not really integrate this influence into its thinking: it did not call it into use in explaining observed variations of the velocity of circulation; nor did it cite it as a factor in its theoretical explanations of variations of the velocity of circulation over the trade cycle.

It is because of these differences from the Cambridge school that the Keynesian theory of liquidity preference can properly be considered only as a "new theory" — one that makes it impossible for us today to approach monetary problems without taking account of these factors.<sup>15</sup>

I would like to end with some personal reminiscences that I hope will support this interpretation of the Cambridge school

<sup>15</sup> It is for these reasons that I cannot accept the contrary conclusions of ESHAG (1963), pp. 62-68.

— though I am sure that for some it will merely be an indication of my prejudices.

There is some effrontery in claiming that though scholars described a certain analytical apparatus, they did not really understand its full implications. I dare nevertheless to advance this contention not only because of what I feel to be the convincing evidence presented here, but also on the basis of my own recollections of how I, too, failed at one time to see these implications. For though my studies of economics at Chicago began some years after the appearance of the *General Theory*, I was educated in the analytical spirit of the quantity theory that prevailed there. Hence even though we also studied the *General Theory*, I know that I did not think then in terms of the sharp Keynesian distinction between stock and flow equilibrium. I know that my instinctive way of thinking of monetary influences at that time was directly from the increase in the stock of money to the increase in the demand for the flow of commodities — without the aid of any intervening portfolio-adjustment substitution effects. I know that I thought of a change in the velocity of circulation solely in terms of a change in tastes as to the desired proportion between the stock of money and the flow of expenditure on current commodities; not in terms of the consequence of a change in tastes as to the desired proportion between the stock of money and the stocks of other assets in a portfolio of a given size.

Knowing these things about the workings of my own mind, I hope that I will not be considered presumptuous if I interpret the detailed evidence from the writings of the Cambridge economists that I have here presented as evidence that they too were subject to a similar failure to see what is so clear to us today — as a result of the changes wrought by Keynesian monetary theory.

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BIBLIOGRAPHY

1. AGASSI, JOSEPH, "The Novelty of Popper's Philosophy of Science", *International Philosophical Quarterly* (September 1968), pp. 442-63.
2. ESHAG, EPRIME, *From Marshall to Keynes*, Oxford: Basil Blackwell, 1963.
3. FISHER, IRVING, *The Rate of Interest*, New York: Macmillan, 1907.
4. FISHER, IRVING, *The Theory of Interest*, New York: Macmillan, 1930.
5. FRIEDMAN, MILTON, *A Theory of the Consumption Function*, (N.B.E.R.: General Series No. 63), Princeton: Princeton University Press, 1957.
6. FOLEY, D.K. and SIDRAUSKI, M., *Monetary and Fiscal Policy in a Growing Economy*, New York: Macmillan, 1971.
7. GURLEY, JOHN G. and SHAW, EDWARD S., *Money in a Theory of Finance*, Washington, D.C.: The Brookings Institution, 1960.
8. HICKS, J.R., "A Suggestion for Simplifying the Theory of Money", *Economica* (1935), pp. 1-19. Reprinted in *Readings in Monetary Theory*, edited by F.A. Lutz and L.W. Mints, London: Allen and Unwin, 1952, pp. 13-32.
9. HICKS, J.R., *Value and Capital*, Oxford: Clarendon Press, 1939.
10. KEYNES, J.M., "Review of I. Fisher's 'The Purchasing Power of Money'", *Economic Journal* (1911), pp. 393-98.
11. KEYNES, J.M., *Tract on Monetary Reform*, London: Macmillan, 1923.
12. KEYNES, J.M., "Alfred Marshall 1842-1924", *Economic Journal* (1924), as reprinted in Pigou, A.C. (ed.), *Memorials of Alfred Marshall*, New York: Kelley and Millman, 1956, pp. 1-65.
13. KEYNES, J.M., *A Treatise on Money*, London: Macmillan, 1930.
14. KEYNES, J.M., *The General Theory of Employment, Interest and Money*, New York: Harcourt, Brace, 1936.
15. KLEIN, L.R. and GOLDBERGER, A.S., *An Econometric Model of the United States 1929-1952*, Amsterdam: North-Holland, 1955.
16. KUHN, THOMAS S., *The Structure of Scientific Revolutions*, 2nd ed. (International Encyclopedia of Unified Science, Vol. 2, No. 2), Chicago: University of Chicago Press, 1970.
17. LAVINGTON, F., *The English Capital Market*, London: Methuen, 1921.
18. LAVINGTON, F., *The Trade Cycle*, London: King, 1922.
19. MARSHALL, ALFRED, *Money, Credit and Commerce*, London: Macmillan, 1923.
20. MARSHALL, ALFRED, *Official Papers*, London: Macmillan, 1926.
21. MITCHELL, B.R. and DEANE, P., *Abstract of British Historical Statistics*, Cambridge: Cambridge University Press, 1962.
22. MYRDAL, GUNNAR, *Monetary Equilibrium*, Glasgow: William Hodge, 1939 (translated from the German work of 1933).
23. PATINKIN, DON, *Money, Interest, and Prices*, 2nd ed., New York: Harper and Row, 1965.
24. PATINKIN, DON, "The Chicago Tradition, the Quantity Theory, and Friedman", *J. Money, Credit and Banking* (February 1969), pp. 46-70.
25. PATINKIN, DON, "On the Short-Run Non-Neutrality of Money in the Quantity Theory", *Banca Nazionale del Lavoro Quarterly Review* (March 1972), pp. 3-22 (1972a).
26. PATINKIN, DON, "Friedman on the Quantity Theory and Keynesian Economics", *J.P.E.*, forthcoming (1972b).

27. PIGOU, A. C., "The Value of Money", *Q.J.E.* (1917-18), as reprinted in *Readings in Monetary Theory*, edited by F. A. Lutz and L. W. Mints, London: Allen and Unwin, 1952.
28. PIGOU, A. C., *Industrial Fluctuations*, 2nd ed., London: Macmillan, 1929.
29. PIGOU, A. C., *The Theory of Unemployment*, London: Macmillan, 1933.
30. ROBERTSON, D. H., *Money*, New York: Harcourt, Brace, 1922; 3rd ed., London: Nisbet and Cambridge: Cambridge University Press, 1929; 4th ed., London: Pitman, 1948.
31. SHACKLE, G. L. S., *The Years of High Theory*, Cambridge: Cambridge University Press, 1967.
32. SNYDER, CARL, "New Measures in the Equation of Exchange", *A.E.R.* (December 1924), pp. 699-713.
33. STIGLER, GEORGE J., "The Nature and Role of Originality in Scientific Progress", *Economica* (November 1955). As reprinted in *Essays in the History of Economics*, Chicago and London: University of Chicago Press, 1965, pp. 1-15.
34. TOBIN, JAMES, "A Dynamic Aggregative Model", *J.P.E.* (April 1955), pp. 103-15.
35. TOBIN, JAMES, "An Essay on Principles of Debt Management", in *Fiscal and Debt Management Policies* (Commission on Money and Credit), Englewood Cliffs, N.J.: Prentice-Hall, 1963, pp. 143-218.
36. TOBIN, JAMES, "A General Equilibrium Approach to Monetary Theory", *J. Money, Credit and Banking* (February 1969), pp. 15-29.
37. WRIGHT, HAROLD, *Population*, New York: Harcourt, Brace, 1923.
38. W[RIGHT], H[AROLD], "[Obituary of] Fredrick Lavington", *Economic Journal* (1927), pp. 503-505.