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DON PATINKIN

The Chicago Tradition, The Quantity Theory, And Friedman^{1, 2}

I MUST BEGIN THIS PAPER with an apology for being over a decade late; for I should have written it as an immediate reaction to Milton Friedman's by now well-known 1956 essay on "The Quantity Theory of Money—A Restatement."³ But the recent appearance of Friedman's *International Encyclopedia* article on the quantity theory⁴ (though, as will be shown in Part IV below, it differs in some relevant respects from the earlier paper) provides an appropriate, if tardy, occasion to raise some basic questions—from the viewpoint of the history of monetary doctrine—about the validity of Fried-

¹ This paper was written while I was visiting at M.I.T. during 1968 under a research grant from the National Science Foundation (NSF Grant GS 1812). I am grateful to both these institutions for making this work possible. I am happy to express my deep appreciation to Mr. Stanley Fischer of M.I.T. for his invaluable assistance at all stages of the preparation of this paper—and particularly in the examination of the relevant literature. In addition, I have benefited from discussions with him and from his criticisms of earlier drafts.

I am also indebted to my Jerusalem colleagues Yosef Attiyeh, Yoram Ben-Porath, and Giora Hanoch, whose thoughtful suggestions have greatly improved the general organization of this paper, as well as the discussion of specific points. As usual, it is a pleasure to thank Miss Susanne Freund for her careful and conscientious checking of the final manuscript and its references. Needless to say, responsibility for the interpretations and views presented in this paper remain entirely my own.

² I would like to dedicate this paper to the memory of Miguel Sidrauski. His untimely death in August, 1968 was a great loss, not only to his family and friends, but to the economics profession in general—and particularly to the development of monetary theory. Though I do not think Miguel had a strong interest in the history of doctrine, I hope that—as a Chicago graduate—he would have been interested in reading the final product of a work whose beginnings he witnessed.

³ In *Studies in the Quantity Theory of Money*, ed. M. Friedman [8], pp. 3–21; referred to henceforth as *Quantity Theory I*. In self-defense, I might, however, note that I have on previous occasions discussed in passing some of the points presented below, and that I have also emp. 81, n.8. See also the implicit criticism in Patinkin [29], p. 480b. See also pp. 60–61 below.

⁴ Friedman [10], referred to henceforth as *Quantity Theory II*.

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man's interpretation of the quantity theory of money, and of its Chicago version in particular.

The argument of the present paper is as follows: in both of the foregoing articles, Friedman presents what he calls a "reformulation of the quantity theory of money." In Part IV I shall show that this is a misleading designation. What Friedman has actually presented is an elegant exposition of the modern portfolio approach to the demand for money which, though it has some well-known (though largely underdeveloped) antecedents in the traditional theory, can only be seen as a continuation of the Keynesian theory of liquidity preference.

The main purpose of this paper, however, is to describe (in Parts II and III) the true nature of the Chicago monetary tradition. In this way I shall also demonstrate the invalidity of Friedman's contention (in his 1956 essay) that this tradition is represented by his "reformulation of the quantity theory." As a minimum statement let me say that though I shared with Friedman—albeit, almost a decade later—the teachers at Chicago whom he mentions (namely, Knight, Viner, Simons, and Mints), his representation of the "flavor of the oral tradition" which they were supposed to have imparted strikes no responsive chord in my memory.

Friedman offers no supporting evidence for his interpretation of the Chicago tradition. This is unfortunate. For questions about the history of economic doctrine are empirical questions. And the universe from which the relevant empirical evidence must be drawn is that of the writings and teachings of the economists in question. No operational meaning can be attached to the existence of a "tradition" which does not manifest itself in one or both of these ways.

From this it will be clear that my examination of this evidence in what follows should not be interpreted as a criticism of the individuals involved. On the contrary, I would consider it unjustified to criticize them for not having fully understood and integrated into their thinking what we have succeeded in learning only in the course of the subsequent development of Keynesian monetary theory. My quarrel is only with those who imply that such an understanding and integration existed before, or independently of, this development.

I would like finally to emphasize that my concern in this paper is with the analytical framework of the Chicago monetary tradition, and not with its policy proposals as such. Correspondingly, I shall not—except incidentally—discuss the relation between these proposals and those of Friedman. Let me, however, note that though there are, of course, basic similarities, there are also significant differences—particularly about the degree of discretion to be exercised by the monetary authorities.⁵

⁵ Cf. note 24 below. Friedman himself discusses some of these differences explicitly in his paper on Simons referred to in note 27 below—and implicitly in his *Program for Monetary*

I. FRIEDMAN'S CHICAGO

Friedman begins his 1956 essay with the explanation that:

Chicago was one of the few academic centers at which the quantity theory continued to be a central and vigorous part of the oral tradition throughout the 1930's and 1940's, where students continued to study monetary theory and to write theses on monetary problems. The quantity theory that retained this role differed sharply from the atrophied and rigid caricature that is so frequently described by the proponents of the new income-expenditure approach—and with some justice, to judge by much of the literature on policy that was spawned by quantity theorists. At Chicago, Henry Simons and Lloyd Mints directly, Frank Knight and Jacob Viner at one remove, taught and developed a more subtle and relevant version, one in which the quantity theory was connected and integrated with general price theory and became a flexible and sensitive tool for interpreting movements in aggregate economic activity and for developing relevant policy prescriptions.

To the best of my knowledge, no systematic statement of this theory as developed at Chicago exists, though much can be read between the lines of Simons' and Mints' writings It was a theoretical approach that insisted that money does matter

The purpose of this introduction is not to enshrine—or, should I say, inter—a definitive version of the Chicago tradition The purpose is rather to set down a particular “model” of a quantity theory in an attempt to convey the flavor of the oral tradition⁶

Friedman then goes on to present this model. Since I am interested only in the doctrinal aspects of the question, it is sufficient to cite the model's basic features. In Friedman's words:

1. The quantity theory is in the first instance a theory of the demand for money. It is not a theory of output, or of money income, or of the price level. Any statement about these variables requires combining the quantity theory with some specifications about the conditions of supply of money and perhaps about other variables as well.
2. To the ultimate wealth-owning units in the economy, money is one kind of asset, one way of holding wealth
3. The analysis of the demand for money on the part of the ultimate wealth-owning units in the society can be made formally identical with that of the demand for a consumption service. As in the usual theory of consumer choice, the demand for money (or any other particular asset) depends on three major sets of factors: (a) the total wealth to be held in various forms—the analogue of the budget restraint; (b) the price of and return on this form of wealth and alternative forms; and (c) the tastes and preferences of the wealth-owning units.⁷

Stability, [8a] pp. 86–90. See also M. Bronfenbrenner, “Observations on the Chicago Schools,” [3a, pp. 72–73].

Another interesting question which lies beyond the scope of the present paper is the extent to which the policy views of the Chicago school in the 1930's represented those of other quantity-theorists of the period.

⁶ *Quantity Theory I*, pp. 3–4.

⁷ *Ibid.*, p. 4.

From these and other considerations Friedman arrives at a demand function for money of the form

$$M = g(P, r_b, r_e, (1/P) (dP/dt), w, Y; u), \quad (1)$$

where M is the nominal quantity of money; P , the price level; r_b is the interest rate on bonds; r_e , the interest rate on equities; $(1/P) (dP/dt)$, the rate of change of prices—and hence the negative of the rate of return on money balances; w , the ratio of non-human to human wealth; Y , money income; and u , “variables that can be expected to affect tastes and preferences.”⁸ Friedman then makes the familiar assumption that this function is homogeneous of degree one in P and Y , and hence rewrites it as⁹

$$M/P = f(r_b, r_e, (1/P) (dP/dt), w; Y/P; u). \quad (2)$$

Alternatively, dividing (1) through by Y , he obtains

$$Y = v(r_b, r_e, (1/P) (dP/dt), w, Y/P; u) \cdot M. \quad (3)$$

“In this form the equation is in the usual quantity theory form, where v is income velocity.”¹⁰

As an aside, I might note that at no point in the foregoing exposition does Friedman mention the name of Keynes. Indeed, one cannot escape the impression that even the term “liquidity” is being avoided.¹¹

Friedman does recognize that “almost every economist will accept the general lines of the preceding analysis on a purely formal and abstract level.” But Friedman defines three distinguishing features of the quantity theorist, of which the first is that the quantity theorist

accepts the empirical hypothesis that the demand for money is highly stable
The quantity theorist need not, and generally does not, mean that the . . . velocity of circulation of money is to be regarded as numerically constant over time
For the stability he expects is in the functional relation between the quantity of money demanded and the variables that determine it

The other two features are that the quantity theorist believes that “there are important factors affecting the supply of money that do not affect the demand for money” and that the demand for money does not become infinitely elastic (viz., absence of a “liquidity trap”).¹²

There is no question that these last two features are generally found (either explicitly or implicitly) in presentations of the quantity theory. But it is equally

⁸ *Ibid.*, pp. 4–10; the quotation is from p. 9.

⁹ *Ibid.*, p. 11, equation 11.

¹⁰ *Ibid.*, p. 11; see equation 13.

¹¹ Cf., e.g., *ibid.*, pp. 5, 14, and 19.

¹² *Ibid.*, pp. 15–16.

clear to me that the first—which is crucial to Friedman’s interpretation—is not. Correspondingly, one of the basic points that will be examined in the following discussion of the Chicago economists is whether they did indeed think in terms of a stable velocity in Friedman’s functional sense.

II. THE OTHER CHICAGO

As against the foregoing, let me now describe a Chicago tradition of monetary theory whose approach, contents, and language can be represented by the following summary-propositions:¹³

1. The quantity theory is, first and foremost, not a theory of the demand for money, but a theory which relates the quantity of money (M) to the aggregate demand for goods and services (MV), and thence to the price level (P) and/or level of output (T): all this in accordance with Fisher’s $MV = PT$.
2. V is not constant; on the contrary, a basic feature of economic life is the “danger of sharp changes on the velocity side”; or in other words, the danger “of extreme alternations of hoarding and dishoarding.”¹⁴ These “sharp changes” in turn are due to anticipations of changing price levels, as well as to the changing state of business confidence as determined by earnings.¹⁵ Thus, if individuals expect prices to rise and earnings to be good, they will dishoard—that is, increase the velocity of circulation. But the crucial point here is that these expectations will be self-justifying: for the very act of dishoarding will cause prices to rise even further, thus leading to further dishoardings, and so on. In this way a “cumulative process” of expansion is set into operation which “feeds upon itself” and which has no “natural” limit.¹⁶ Conversely, an indefinite “cumulative process” of hoarding, price declines and depression, and further hoarding is set into operation by the expectation that the price level will fall and/or that earnings will be poor. Thus the economic system is essentially unstable.¹⁷
3. Such a cumulative process might possibly take place, albeit in a much

¹³ The following is primarily a summary of Simons’ views, which were largely accepted by Mints. Knight’s analysis is the same, though—quite characteristically—he seems to have had less faith than Simons and Mints in the policy proposals. For Viner, I have been able to find evidence only on the first proposition. For references to the relevant writings, see the Appendix below. Cf. also Davis [4a].

¹⁴ Simons, “Rules versus Authorities in Monetary Policy,” [33], p. 164 (this passage is cited in full in the Appendix below). That by “hoarding and dishoarding” Simons means changes in velocity is clear from p. 165. See also note 15.

¹⁵ See Simons, “Banking and Currency Reform,” as quoted in the Appendix below.

¹⁶ Knight [18], pp. 210–11, 223–24.

¹⁷ For supporting quotations from Simons [34], p. 222, and [35], p. 188, and Knight [18], pp. 211 and 224 see the Appendix below.

less severe form, even if the quantity of money in the economy were to remain constant.¹⁸ In the actual world, however, the process is highly exacerbated by the “perverse” behavior of the banking system, which expands credit in booms and contracts it in depressions. As a result the quantity of money (M) and near-moneys (and hence V) increases in booms, and decreases in depressions.

4. In accordance with (2) and (3), the government has an obligation to undertake a contracyclical policy. The guiding principle of this policy is to change M so as to offset changes in V , and thus generate the full-employment level of aggregate demand MV . If prices are downwardly flexible, the operational rule which will assure the proper variation in M is that of increasing M when P falls, and decreasing it when P rises. In any event, it is “inconceivable” that a sufficiently vigorous policy of (say) expanding M in a period of depression would not ultimately affect aggregate spending in the required manner.
5. The necessary variations in M can be generated either by open-market operations or by budgetary deficits. The latter method is more efficient, and in some cases might even be necessary. Budgetary deficits, in turn, can be generated by varying either government expenditures or tax receipts. From the viewpoint of contracyclical policy, this makes no difference—for either method changes M ; but from the viewpoint of the general philosophy of the proper role of government in economic life, the variation of tax receipts is definitely preferable. Hence, a tax system which depends heavily on the income tax is desirable not only from the viewpoint of distributive justice, but also from the viewpoint of automatically providing proper cyclical variations in tax receipts.

Before going on to bring out the flavor of these propositions as contrasted with that of Friedman’s presentation, I would like briefly to indicate three reasons for the emphasis I have given in the foregoing to the writings of Simons. First, at the Chicago which concerns us, Simons was undoubtedly the dominant figure in discussions of monetary and fiscal policy. (In Friedman’s presentation too there is more emphasis on the writings of Simons and Mints than on those of Knight and Viner.) Second, Simons’ writings on these questions were the earliest by far of the writers here considered. And, third, they were sufficiently early to represent the Chicago tradition in its pristine—and pre-Keynesian—form.

The significance of this last point will become clear from our discussion of Mints at the end of this part. In connection with the first reason, I might note that Mints repeatedly makes clear his indebtedness to Simons.¹⁹ Again, I would

¹⁸ See the quotation from Simons, [33], p. 164, in the Appendix below. See also *ibid.*, p. 331, footnote 16; and Mints [25], pp. 120–22.

¹⁹ Cf., e.g., *Monetary Policy for a Competitive Society*, [25], p. vii.

conjecture that Knight's writings referred to above also reflect Simons' influence. Similarly, in my recollections of student days at Chicago—and I think I can speak safely for my fellow students at the time—it is Simons who stands out sharply as the major source of intellectual stimulation and influence in all that regards monetary and fiscal policy. In the slang of those days, most of us were “Simonized” to some degree or other.

Let me turn now to the propositions themselves. The contrast drawn by Proposition I is that between the transactions approach to the quantity theory and the cash-balance approach emphasized by Friedman. Now, it is a commonplace of monetary theory that these two approaches can be made analytically equivalent. Indeed, in his general discussion of monetary influences, Fisher himself vividly shows that he was thinking in terms of a demand for money.²⁰ Nevertheless, if we consistently find a treatment in terms of the transactions approach, we can take this as some indication that the economists in question did not primarily approach monetary theory from the viewpoint of the demand for money. Or at least we cannot take it as an indication that they did!²¹

Indeed, it is a much closer approximation to the flavor of the Chicago tradition to say that basically it was not interested in a systematic analysis of the demand for money:²² for it believed so strongly that “the supply of money matters,” that—for the policy purposes which were its main concern—the exact form of the demand function for money did not matter at all, aside from the critical (though sometimes implicit) assumption “that additional money in unlimited amounts would [not] be hoarded in its entirety.” For then no matter what the demand for money—in the language of Simons and Mints: no matter what the extent of hoarding—its adverse effects could be offset by a sufficient increase in M . “Much hoarding would simply require a larger addition to the stock of money.”²³ The possibility that destabilizing lags could interfere with the efficacy of such a monetary policy—a problem which has received so much attention in recent years—was either not seen (Simons) or not given much weight (Mints²⁴).

It should therefore not surprise us that Simons did not present a detailed analysis of the demand for money. Indeed, despite his frequent references to “hoarding,” there does not seem to be any point in his writings in which he even uses the term “demand for money.” Another, and related, manifestation

²⁰ Fisher, *The Purchasing Power of Money*, [6], pp. 153–54.

²¹ Actually, Friedman draws a sharper distinction on this score between the transactions and cash-balance approaches than I would; thus compare his *Quantity Theory II*, pp. 437–38 with my *Money, Interest, and Prices*, [28], pp. 166–67.

²² It is noteworthy that the work on the empirical nature of the demand function for money that was done at Chicago during the 1940's was carried out under the inspiration not of the “Chicago oral tradition,” but of the Keynesian model-builders at the Cowles Commission, which was then located at the University of Chicago. See in particular Klein [16], pp. 125 ff. and [17], pp. 95–101.

²³ The last two quotations in this paragraph are from Mints [24], p. 67; see also *ibid.*, p. 61 and [25], pp. 48–49.

²⁴ Mints, [25], pp. 138 ff. Mints ascribes to Milton Friedman (who by then was his colleague) the suggestion that such a destabilizing influence might occur (*ibid.*, p. 138, n. 8).

of the lack of interest in such an analysis is the fact that Simons did not spell out the details of the mechanism by which an increase in the quantity of money was supposed to increase the volume of spending on goods and services. Instead, he sufficed with the simple, sometimes implicit, and frequently mechanical statement that an increase in M increased aggregate demand MV .

Again, even the influence of the rate of interest on the demand for money was not consistently recognized by the Chicago school of the 1930's and 1940's. Thus even though Knight discussed cyclical variations in the rate of interest, he did not take account of the possible influence of such variations on the velocity of circulation.²⁵ Similarly, though (as indicated in Proposition 3) Simons and Mints did emphasize the influence of near-moneys on velocity, it was the *volume* of these money-substitutes to which they referred, not to the *rate of interest* upon them.

Let me turn now to the "extreme alternations" in velocity described in Proposition 2. It is not clear from the writings of the Chicago school whether it believed that the very fact that prices were, say, increasing would cause an indeterminate flight from money—so that there could exist no stable functional relationship between velocity and the anticipated rate of change of prices; or whether velocity was unstable because of the nature of the expectations function which generated a sequence of ever-increasing anticipated rates of price changes which operated through a stable demand function; or whether there were other forces in the economy (of which those described in Proposition 3 are an example) which generated such a divergent sequence—or whether it believed that a combination of some or all of these factors was at work. But in any event one point is clear: there is no place in their writings in which the aforementioned Chicago economists even hint that they were thinking in terms of Friedman's crucial assumption of a velocity which is a stable function of (among other variables) the anticipated rate of change of prices.^{26, 27}

There were other respects in which the Chicago tradition lacked some of the basic ingredients of the flavor of the "model" which Friedman has presented of it. In particular, whereas (as indicated above) this tradition was primarily

²⁵ See the discussion of Knight in Appendix below.

²⁶ See, in this context, the statement by Knight [20], p. xlv, quoted in full in the Appendix.

²⁷ I might at this point note that in his recent paper on Simons, Friedman [9] cites the passage referred to in note 18 above and then concludes, "There is clearly great similarity between the views expressed by Simons and by Keynes—as to the causes of the Great Depression, the impotence of monetary policy, and the need to rely extensively on fiscal policy. Both men placed great emphasis on the state of business expectations and assigned a critical role to the desire for liquidity. Indeed, in many ways, the key novelty of Keynes' *General Theory* was the role he assigned to 'absolute' liquidity preference under conditions of deep depression" (p. 7). But Friedman gives no indication of the fact that this interpretation of Simons is hardly consistent with that of his 1956 essay, with its emphasis on the functional stability of V and, even more to the point, on the absence of a "liquidity trap" (see conclusion of Part I, above).

In this paper, Friedman also contends that "had Simons known the facts as we now know them [about the monetary history of the United States during the Great Depression], he would, I believe," have been less concerned with "the danger of sharp changes on the velocity side" (p. 12). Without discussing the validity of this conjecture, I shall merely note that it is not relevant to the question which concerns me here, namely Simons' actual approach to the quantity theory.

concerned with the relation between the stock of money and the flow of expenditures, Friedman's primary concern is with the relation between the stock of money and the stocks of other assets. I shall return to this point in Part IV. But let me now admit that, with respect to this comment—and even more so, with respect to much of what has been said above about the lack of interest at Chicago in the demand for money—Lloyd Mints was at least a partial exception. Thus, even though it is not at all comparable in either detail or precision with Friedman's exposition, Mints's *Monetary Policy for a Competitive Society* contains a more explicit analysis of the asset-demand for money than any earlier Chicago discussion.²⁸ But it is highly significant that the chapter in which this analysis is presented (Chapter 3) is followed by a special appendix on Keynes's theory of liquidity preference. Similarly, as shown in Appendix II, it was in this context (and not in that of the quantity-theory of money) that Mints's lectures on the asset-demand for money were given. It is also noteworthy that the few Chicago doctoral theses of the period 1939–1950 that were concerned with the choice of money as a component of a portfolio of assets generally took Keynes as their point of departure and gave no indication that they saw this approach as stemming from the Chicago tradition (see Part III below).

Thus, the picture which emerges from all this is that by the 1940's the Chicago school itself had, quite understandably, been influenced by Keynesian monetary theory. Accordingly, not only did it begin to evince an interest in a systematic analysis of the demand for money, but it frequently did so from the Keynesian viewpoint of money as one component of an optimally chosen portfolio of assets. Indeed, it had to use this viewpoint in order to explain why it rejected some aspects of the Keynesian theory: namely, the Keynesian concentration on the choice between money and bonds, and the related interpretation of interest as a monetary phenomenon; and the emphasis on the possibility of indefinite hoarding (the "liquidity trap"), and the related Keynesian conclusion that money could not matter enough, so that only a policy of increased government expenditures could deal adequately with the problem of unemployment.²⁹

III. THE ORAL TRADITION OF CHICAGO

The preceding discussion of the Chicago school has been based on its writings. It is, however, the "oral tradition of Chicago" which Friedman primarily claims to represent and to which, accordingly, I shall now turn.

A priori, it seems unlikely that scholars who had presented a consistent, and sometimes lengthy, statement of their views in print would have provided

²⁸ Mints [25], chap. iii; see also chap. ix, especially pp. 210–11. See also Mints [23], pp. 219–22 and [24], p. 63 *et passim*.

²⁹ See again the references to Mints in the preceding footnote. See also the discussion of Knight in the Appendix.

a significantly different presentation in their classroom discussions. Fortunately, there is no need to rely solely on such a priori considerations—or even on my memories of these classroom discussions as contrasted with those of Friedman. For there is concrete evidence on their nature in the form of lecture notes which I took during my graduate studies at Chicago in the period 1943–45.

Of course, these lecture notes are subject to all of the standard reservations about the accuracy with which students understand their teachers. Furthermore, they constitute only one observation on these teachings. But at the present moment this is one more than has yet been provided on the question. It should also be noted that if we accept (as I do) the fact that there was a “tradition” at Chicago, then we can also assume that there was a high degree of continuity between what was taught in my student days and what was taught before.

Mints devoted several lectures to the quantity theory at the beginning of his course on “Money.” After presenting Fisher’s equation of exchange (with no discussion of the determinants of V —or of the Cambridge K , to which he also referred), Mints went on to formulate the quantity theory of money in a way which has remained sharply etched in my memory—and which has always represented for me “the flavor of the Chicago tradition”:

Some attempts [have been made statistically] to verify quantity theory by showing that $MV + M'V' = PT$ is true. But quantity theory says that P is the dependent variable. So would have to show that exist consistent time lags. Have to establish causal relationship. Formula itself is a truism—doesn’t need verification. Formula \neq quantity theory.

Mints prefers following statement of quantity theory: P is the dependent variable (in the long run) of the equation $MV = PT$. But in the short run all the variables tend to move together.³⁰

For our purposes (see end of Part II), it is also most significant that Mints’s discussion of the demand for money from a viewpoint which is closer to the portfolio approach did not occur in his lectures on the quantity theory, but a month later in the context of his discussion of Keynes’ theory of liquidity preference. Here Mints said that there were

... really four factors to be kept in equilibrium: (1) price level (2) rate of interest (3) demand for cash (liquidity preferences) and (4) quantity of money.

Methods of disposing of cash:

- (1) Hold in cash
- (2) Purchase consumer’s goods
- (3) Purchase producer’s goods
- (4) Lend on short term
- (5) Purchase long term bonds
- (6) Purchase corporation shares.

³⁰ Lecture notes from Lloyd Mints, “Money” (Economics 330), June 28 and July 3, 1944, italics in original. It is noteworthy that this distinction between the quantity theory and the identity $MV + M'V' = PT$ is also emphasized by Friedman in his encyclopedia article; see *Quantity Theory II*, pp. 434–36.

Keynes assumes that doubts about the future affect only (5).

But uncertainties affect (2) and especially (3). Demand will fall off for these, prices there will fall, profits decrease, and beginning of unemployment, etc.³¹

Mints then went on to present a discussion which closely parallels that of the first part of Chapter 3 of his *Monetary Policy for a Competitive Society*.

Some other notes from Mints's lectures, as well as relevant notes from the lectures of Knight and Simons, are reproduced in the Appendix. The evidence of these notes leads unmistakably to one simple and unsurprising conclusion: the oral tradition of the Chicago school of monetary theory was entirely reflected in its written tradition; whatever was not in the latter was also not in the former.

Let me turn now to the doctoral theses written at Chicago during the period in question.³² As we all know, students' theses reflect the interests, approach, and all too frequently even the views of their teachers. It is therefore interesting to see what we can learn about the Chicago tradition from this source. This is all the more legitimate in the present context in view of Friedman's assertion that "Chicago was one of the few academic centers at which the quantity theory continued to be a central and vigorous part of the oral tradition throughout the 1930's and 1940's, whose students continued to study monetary theory and to write theses on monetary problems."³³

The list of relevant theses is presented in the Appendix. Even after taking account of the small number of theses which were being submitted in those days (a total of 46 for the 1930's and 52 for the 1940's), one is struck by the paucity of monetary theses written at Chicago during the 1930's. The fact that from 1931 through 1938 only one such thesis was submitted speaks for itself. The number is decidedly greater for the 1940's. Nevertheless, a casual comparison with the list of doctoral theses submitted at Harvard shows that even during the 1940's there were at least as many monetary theses being submitted at Harvard as at Chicago (though admittedly the total number of theses submitted at the former was three times as large).

Let me turn now to the far more important question as to the contents of the Chicago theses. The situation can be described quite simply: several of the theses are primarily descriptive and contain little, if any, analysis. To the extent that the theses refer to the quantity theory of money, they do so in terms of Proposition 1 above;³⁴ none of them do so in terms reminiscent of

³¹ Lecture notes from Mints, "Money" (Economics 330), August 4, 1944.

³² Without disclaiming any responsibility for what follows, I would like to express my appreciation to my assistant, Mr. Stanley Fischer, who has carefully gone through the monetary theses written at Chicago during the period 1930-1950—and on whose excellent notes I have relied heavily. I would also like to thank Professor Lester Telser of the Department of Economics at the University of Chicago and his secretary, Mrs. Hazel Bowdry, for their help in obtaining a complete list of Chicago theses and microfilms of those discussed here, as well as information about these committees.

³³ *Quantity Theory I*, p. 3.

³⁴ Thus see, e.g., Benjamin F. Brooks, "A History of Banking Theory in the United States Before 1860" (1939), p. 354; Marion R. Daugherty, "The Currency School-Banking School

Friedman's "reformulation." Few theses even reflect a portfolio approach to the demand for money. Furthermore, those that do draw their primary inspiration from Keynes or his supporters.³⁵ Similarly, the influence of the rate of interest on the demand for money is rarely mentioned, even in appropriate contexts;³⁶ and when there is some mention, it is again largely inspired by Keynesian monetary theory.³⁷

Of particular interest in the present context is Bach's thesis on "Price Level Stabilization: Some Theoretical and Practical Considerations" (1940). In his general analysis and policy proposals, Bach presents the position of the Chicago school as summarized in the five propositions of Part II above.³⁸ Furthermore, in the process of so doing he refers explicitly to an "oral tradition" at Chicago which he describes in the following terms:

The explanation of the cycle may, for our purposes, be ultimately reduced to the existence of two basic factors, the first redivisible into two more or less separate elements. These two sub-factors in the first are (a) psychological shifts by consumers, entrepreneurs, and investors, leading to changes in the propensities to hoard, consume, and invest, and (b) perverse fluctuations in the volume of money in the system (M plus M' in the Fisherian notation). The second basic factor is the existence of "sticky" prices throughout large sectors of the economy, of which many are cost-prices, so that costs have a tendency to move more slowly than do the more flexible selling prices.*

* On this reduction to essentials I am indebted to Professor Mints, although it has been in the nature of an "oral tradition" at Chicago for some time and can be found in many writers, but only more or less obscured.³⁹

Controversy" (1941), p. 54; Roland N. McKean, "Fluctuations in Our Private Claim-Debt Structure and Monetary Policy" (1948), pp. 51, 98, and 103. See also the references to Bach's thesis in notes 38 and 39 below.

³⁵ Thus see, e.g., Martin Bronfenbrenner, "Monetary Theory and General Equilibrium" (1939), pp. iii, 43, 45, 156-57; McKean, *op. cit.*, chap. iv, and especially pp. 52 (note 5) and 57-59; and William W. Tongue, "Money, Capital and the Business Cycle" (1947), chaps. i and iii.

³⁶ Cf., e.g., Arthur I. Bloomfield, "International Capital Movements and the American Balance of Payments, 1929-40" (1942), pp. 578-79; see also McKean, *op. cit.*, p. 80. In all but the last chapter of his thesis (see especially chapter iv), McKean follows Simons (to whom he repeatedly refers, pp. 3, 32, 52, 68, *et passim*), in being concerned with the volume of liquid assets and debts, and not the rates of return upon them. (Cf. above, p. 53.) On the other hand, his discussion of 100 per cent money in the last chapter (see especially pp. 174-77) explicitly takes account of the effect on the demand for money (and hence velocity) of changes in the rates of interest on money substitutes.

³⁷ See again the references cited in note 35. See also McKean, *op. cit.*, p. 100, who, however, refers to the influence of the quantity of money on the interest rate as "the very old argument, revived in the thirties" (*ibid.*, p. 99).

It might be noted that at some points McKean's thesis also reflects the influence of Milton Friedman, who had joined the Chicago staff in 1946. Thus, see the reference to Friedman in McKean's discussion of the simultaneous influence of the interest rate on the demand for money and on savings (p. 101, n. 1). Somewhat less relevant for our present purpose are McKean's many references, in chapters i and ii and on p. 191, to Friedman's discussions of the problem of lags in monetary policy and of the proper framework for monetary and fiscal policy.

³⁸ See especially pp. 42-45 and 72-75 of his thesis.

³⁹ *Ibid.*, pp. 35-36.

It might be noted that at the time Bach wrote, the tradition was indeed largely oral: for all that then existed in print was Simons' brief discussions; the writings of Knight and Mints had yet to appear (see Appendix).

In concluding this discussion of the Chicago school, I would like to emphasize once again that its purpose has not been either to praise or to criticize—and surely not to criticize the writers of the theses—but only to convey the flavor of the Chicago tradition as it really was.

IV. THE QUANTITY THEORY, FRIEDMAN, AND KEYNESIAN ECONOMICS

As indicated in my opening remarks, the nominal occasion for the appearance of this paper is the recent publication in the *International Encyclopedia of the Social Sciences* of Friedman's article on the "Quantity Theory." From the substantive viewpoint, the "reformulation of the quantity theory" which Friedman presents on pp. 439–42 of this article is essentially the same as the one he presented in his 1956 essay (see Part I above). But from the doctrinal viewpoint which engrossed us in Parts II–III, there is a fundamental difference: for Friedman now makes no attempt to present this reformulation as a "model of the oral tradition of Chicago." Indeed, neither the Chicago school nor its individual members are even mentioned.

On the other hand, as just indicated, Friedman does continue to denote his presentation as a "reformulation of the quantity theory." The only support he adduces for this nomenclature is that "Fisher and other earlier quantity theorists explicitly recognized that velocity would be affected by, among other factors, the rate of interest and also the rate of change of prices."⁴⁰

That such a recognition existed, there can be no doubt.⁴¹ But, as I have indicated elsewhere,⁴² the real question is the extent to which the "earlier quantity theorists" recognized the full and precise implications of these effects: the extent to which they consistently took account of these effects at the appropriate points in their discussions. For one of the fundamental facts of the history of ideas is that in general the full implications of a set of ideas are not immediately seen. Indeed, as has been frequently noted, if they were, then all mathematics would be a tautology; for its theorems are implicit in the assumptions made. The failure to see such implications is also familiar from many episodes in the history of economic doctrine: for example, from the tortuous and faltering manner in which the full implications of the marginal productivity theory were developed.⁴³

Thus, there is indeed a striking passage in Fisher's *Rate of Interest* about

⁴⁰ *Quantity Theory II*, p. 436b.

⁴¹ For specific references to writings of Walras, Wicksell, the much-neglected Karl Schlesinger, Fisher, and the Cambridge school (Marshall, Pigou, and especially Lavington) which discuss or at the least refer to the influence of interest on the demand for money, see Patinkin [28], pp. 372, 545, 556, and 576–80.

⁴² Patinkin [29], p. 480b.

⁴³ See Stigler [39].

the "convenience" of money holdings which makes an individual willing to forego the interest that he could earn.⁴⁴ But the only echo of this passage in *The Purchasing Power of Money* is a passing reference to the influence of the "waste of interest" on velocity.⁴⁵ Furthermore, it is clear that Fisher did not integrate this influence into the general analysis of this book. Indeed, this influence is not mentioned at any other point in it: neither in the analysis of the effects of the higher interest rates which mark the "transition period" (Chapter iv), nor in the detailed description of the determinants of the velocity of circulation (Chapter v),⁴⁶ nor finally in the statistical investigation of the theory, with its description of how velocity varied during the periods examined (Chapters xi–xii).

I find this last omission particularly significant. For the empirical investigator is confronted with a concrete situation in which he is called upon to take account of the major theoretical variables which might explain the data (even if some of these variables will subsequently be rejected as statistically insignificant); hence this situation provides a proper and operationally meaningful test of whether the influence of a variable has been "fully recognized." It should therefore be emphasized that the failure even to mention the rate of interest as a possible explanation of the observed variations in the velocity of circulation also characterizes the writings of Carl Snyder,⁴⁷ to whose empirical work (as well as that of Fisher) Friedman refers.⁴⁸ And a similar picture obtains for the earlier studies by James W. Angell⁴⁹ and Clark Warburton,⁵⁰ to which Selden refers in his survey of empirical investigations of the income-velocity of circulation in the United States.⁵¹ Furthermore, the fact that Angell and Warburton mention the influence of interest only in their later studies—and in explicit response to issues raised by Keynesian monetary theory⁵²—reinforces my basic contention that the "early quantity theorists" did not of themselves fully recognize this influence.

p. 44 Fisher [5], p. 212. This passage is slightly elaborated upon in *The Theory of Interest* [7], an 216, where Fisher refers to the "liquidity of our cash balances [which] takes the place of the rate of interest in the ordinary sense of the term."

⁴⁵ [6], p. 152.

⁴⁶ Fisher's discussion here is in terms of the "habits of the individual," the "systems of payments in the community," and "general causes"—by which he means "density of population" and "rapidity of transportation" (*ibid.*, p. 79).

⁴⁷ Thus see Snyder [36, 37, 38].

⁴⁸ *Quantity Theory II*, p. 436b.

⁴⁹ Angell [1, 2]. Angell's detailed and systematic analysis of the velocity of circulation is almost entirely in terms of the timing and mechanics of the payment process. The passing references to interest (on pp. 57–58 of the article, and on pp. 164–65 of the book) do not change this basic picture. Note too the discussion of "idle balances" (in [2], pp. 140 ff.) which is devoid of any reference to the interest rate.

⁵⁰ Warburton [43, 44]. Warburton's primary concern is with the secular trend in velocity, which he explains in terms of the mechanism of the payment process and a greater-than-unity income elasticity of demand for cash balances [44, pp. 443–44]. See also [46], pp. 89–90.

⁵¹ Selden [31], pp. 184–85. Nearly half of the studies surveyed by Selden are by Angell and Warburton.

⁵² Thus see Angell [3], chaps. vi and ix. Similarly, Warburton first deals with this influence in his reply to Tobin's criticism (from the viewpoint of Keynes' liquidity-preference theory) of his (Warburton's) earlier work; see Warburton [45]. It is against this background that one must also read Warburton's discussion of the rate of interest in a later article [46], pp. 89–90.

It is true that the aforementioned empirical studies were primarily concerned with explaining the observed price level in the market, and not the demand function for money. But to press this point too far in the present context is to admit that these “early quantity theorists” did not really have a major concern with the properties of this demand function. Furthermore, even within the context of these empirical studies it is quite appropriate to investigate the possibility that observed deviations from the hypothesized V (whether assumed constant or secularly declining) can be explained in terms of changes in the interest rate—provided the observed V is assumed to equal the desired V . In fact, this is what Warburton did in his later (1949) study, though he concluded that this possibility should be rejected.

In any event, it is significant that the first empirical study (to the best of my knowledge) which explicitly deals with the influence of interest on the demand for money is the 1939 Keynesian-inspired study by A. J. Brown on “Interest, Prices, and the Demand Schedule for Idle Money.”⁵³ I might also add that this is the first such study which discusses a *functional relationship* between the demand for money and “the rate at which the general price-level has lately been changing.”⁵⁴ This discussion can well be contrasted with Fisher’s imprecise statement that when money “is depreciating, holders will get rid of it as fast as possible.”⁵⁵ Furthermore, Fisher sees this as an unstable process which will cause a further rise in prices which will again increase V “and so on.”⁵⁶ In my discussion of the Chicago quantity-theorists (above, pp. 52–53), I have already stressed the difference between this view and the stable relationship between the demand for money and rate of change of prices described by Friedman—and by Brown.

I have dwelt at length on the treatment of the rate of interest in Friedman’s “reformulation” as compared with the actual writings of the quantity-theorists because this difference can be well-defined and hence clearly observed in the literature. But I attach no less significance to other, and more subtle, differences which also characterize Friedman’s 1956 essay. Thus, Friedman’s presentation of the demand for money is first and foremost in terms of the demand for an asset; for him the income variable in the demand function is primarily “a surrogate for wealth, rather than [as in the quantity theory] a measure of the ‘work’ to be done by money.”⁵⁷ Correspondingly, as I have noted elsewhere (see footnote 3 above), Friedman is primarily concerned with the optimal relationship between the stock of money and the stocks of other assets, whereas the quantity theorists were primarily concerned with the rela-

⁵³ See Brown [4].

⁵⁴ *Ibid.*, p. 34; unfortunately, Brown goes on to represent this rate by the absolute difference $p_t - p_{t-1}$, instead of the ratio of this difference to p_t (or p_{t-1}), where p_t represents the price level p at time t .

⁵⁵ *The Purchasing Power of Money*, [6], p. 63.

⁵⁶ *Ibid.*

⁵⁷ *Quantity Theory II*, p. 440a.

tionship between the stock of money and the flow of spending on goods and services. Furthermore, their discussions of this relationship either did not make the distinction between stocks and flows—or at least were imprecise about it. Similarly, quantity theorists paid little, if any, attention to the effects on the rate of interest and other variables of shifts in tastes as to the form in which individuals wished to hold their assets.⁵⁸

And now to our main point: all of the foregoing are precisely the differentia of Keynesian monetary theory as compared with the traditional quantity theory. They are the basic components of a theory of portfolio choice of which there are undoubtedly antecedents in the Cambridge cash-balance school and before, but whose analytical structure as it now exists stems from the publication during the 1930's of Keynes's *Treatise on Money*,⁵⁹ Hicks's "Suggestion for Simplifying the Theory of Money,"⁶⁰ and Keynes's *General Theory*.⁶¹ Subsequent valuable contributions to this analysis were made during the 1940's and early 1950's by, among others, H. Makower and J. Marschak [21], Franco Modigliani [26], R. F. Kahn [13], Joan Robinson [30], Harry Markowitz [22], and James Tobin [40]. And in direct continuation of this intellectual line of descent, Milton Friedman provided us in 1956 with a most elegant and sophisticated statement of modern Keynesian monetary theory—misleadingly entitled "The Quantity Theory of Money—A Restatement."⁶²

Actually, a careful reading of Friedman's encyclopedia article would seem to indicate that he has taken account of criticisms of his earlier exposition and that—at least in part—he himself now recognizes this intellectual indebtedness. Thus, first of all, he now describes his reformulation as one "that has been strongly influenced by the Keynesian analysis of liquidity preference."⁶³ Similarly, the term "liquidity," which had been avoided in the 1956 essay, is now used.⁶⁴ Second, he admits that the Keynesian analysis of the demand for money lays "greater emphasis on current interest rates" than did the "earlier quantity theorists."⁶⁵ Third, Friedman now recognizes that the "earlier quantity theory" envisaged the process of monetary adjustment in terms of the relation between the stock of money and the flow of expenditures "to the almost complete exclusion" of the Keynesian approach, which envisages it in terms of the relation between the stock of money and other assets,

⁵⁸ These differences also prevail between Friedman and the Chicago tradition; see above, pp. 53–54.

⁵⁹ Keynes [14], Vol. I, pp. 140–46.

⁶⁰ Hicks [12]. Note the reference to Lavington on p. 15, n. 2.

⁶¹ Keynes [15], pp. 166–72, 222–29.

⁶² Cf., on this intellectual genealogy, n. 3 above.

⁶³ *Quantity Theory II*, p. 439b. Although Friedman refers to Johnson's 1962 *American Economic Review* survey article immediately after this statement, I think that it can safely be assumed to reflect his own view as well.

⁶⁴ "... the services rendered by money relative to those rendered by other assets—in Keynesian terminology, ... liquidity proper" (*Quantity Theory II*, p. 440b). Cf. *Quantity Theory I*, *op. cit.*, p. 14.

⁶⁵ *Quantity Theory II*, p. 438b.

particularly bonds.⁶⁶ Furthermore, Friedman himself accepts as “plausible” the Keynesian approach that “any widespread disturbance in money balances . . . will initially be met by an attempted readjustment of assets and liabilities, through purchase and sale”—though he goes on to explain how the resulting change in prices will also “establish incentives to alter flows of receipts and expenditures.”⁶⁷

In view of all this, one can only regret that Friedman has persisted—even within the confines of an international encyclopedia—in presenting his exposition of the demand function for money as a “reformulation of the quantity theory.”

APPENDIX: THE EMPIRICAL EVIDENCE

I. The Writings

The sources for the first four^{67a} summary-propositions at the beginning of Part II are as follows:

HENRY C. SIMONS: *A Positive Program for Laissez Faire* [32], p. 64; “Rules versus Authorities in Monetary Policy” [33], pp. 164–66, 170–72, 326, n. 5, 331, n. 16; *Personal Income Taxation* [34], p. 222; “Hansen on Fiscal Policy” [35], p. 188. Also, “Banking and Currency Reform,” p. 3 and Appendix, p. 2.⁶⁸ The last passage listed (see n. 15 above) reads:

But any general change in business earnings will affect promptly the speculative temper of the community. Larger profits breed optimism; they stimulate investment and induce dishoarding (reduction of idle cash reserves). Producers will become more anxious to borrow for purposes of increasing inventories, expanding production, and increasing plant capacity. Lenders will have fewer misgivings about the ability of borrowers to repay. People generally will increase their

⁶⁶ *Ibid.*, p. 441b.

⁶⁷ *Ibid.*, pp. 441b–442a. Though he does not refer to it at this point, Friedman’s discussion here essentially summarizes the analysis presented in his and Meiselman’s paper on “Relative Stability of Monetary Velocity and the Investment Multiplier in the United States, 1897–1958,” [11], pp. 217–22. In this analysis, Friedman and Meiselman distinguish their approach from the Keynesian one in terms of the range of assets involved in the monetary adjustment.

^{67a} I have not provided specific sources for Proposition 5, which does not really bear on the issue at hand, and which has been included in the text only for the sake of completeness.

⁶⁸ “Banking and Currency Reform” is an unpublished and unsigned memorandum dated by Aaron Director as November, 1933 and ascribed by him largely to Simons. I am greatly indebted to Friedman and Director for providing me with a copy of this memorandum. See the Bibliography in *Economic Policy for a Free Society* [33], p. 313; see also Friedman 9, p. 2, n. 1. Friedman describes the Appendix to the memorandum as a “partial exception to the statement that Simons nowhere set forth a consistent statement of his theory” (*ibid.*). However, except for its explicit relating of velocity to business earnings, the theoretical presentation of this Appendix seems to me to be no more detailed or systematic than Simons’ other writings.

lending and investment at the expense of their idle reserves of cash. In a word, the velocity of circulation will increase. But this change, in turn, means a larger volume of business and higher product-prices, and thus still larger earnings. The further increase of earnings, moreover, will induce further increase in the velocity of money. And so on and on, until the initially sticky prices which govern costs do finally move upward markedly and rapidly—or until some fortuitous disturbance (perhaps a mere speculative scare) happens to establish a sharp reversal of the trend in product prices. On the other hand, once earnings begin to decline, forces will be set in motion to continue and accelerate the trend—and perhaps with more striking results, for the crucial, sticky prices are peculiarly resistant to downward pressure.

The passages from Simons' writings referred to in notes 14, 17, and 18 above are as follows:

Once a deflation has gotten under way, in a large modern economy, there is no significant limit which the decline of prices and employment cannot exceed, if the central government fails to use its fiscal powers generously and deliberately to stop that decline. Only great government deficits can check the hoarding of lawful money and the destruction of money substitutes once a general movement has gotten under way. [*Personal Income Taxation, op.cit.*, p. 222.]

The bottom of an uncontrolled deflation, for all practical purposes, is non-existent—with adverse expectations causing price declines and with the actual declines aggravating expectations, etc. [“Hansen on Fiscal Policy,” *op. cit.*, p. 188.]

With all its merits, however, this rule [of holding the quantity of money constant] cannot now be recommended as a basis for monetary reform. The obvious weakness of fixed quantity, as a sole rule of monetary policy, lies in the danger of sharp changes on the velocity side, for no monetary system can function effectively or survive politically in the face of extreme alternations of hoarding and dishoarding. It is easy to argue that something would be gained in any event if perverse changes were prevented merely as to quantity, but the argument is unconvincing. The fixing of the quantity of circulation media might merely serve to increase the perverse variability in the amounts of “near moneys” and in the degree of their general acceptability, just as the restrictions on the issue of bank notes presumably served to hasten the development of deposit (checking-account) banking. [“Rules versus Authorities in Monetary Policy,” *op.cit.*, p. 164.]

LOYD W. MINTS: *A History of Banking Theory* [23], pp. 218–22; “Monetary Policy” [24], esp. pp. 61, 63, and 67; *Monetary Policy for a Competitive Society* [25], chap. iii (esp. pp. 29, 32–35, 39–41, 48–49, 69–70), chap. vi (esp. pp. 120–22, 138–42), and chap. ix (esp. pp. 194, 202–203, 207, 210–11, 227).

FRANK H. KNIGHT: “Economics” [19], pp. 15, 30; “The Business Cycle, Interest, and Money: A Methodological Approach” [18], pp. 210–11, 213, 223–24; Preface to 1948 reprint of *Risk, Uncertainty, and Profit* [20], pp. xlii–xlv.

The passages from Knight referred to in footnotes 17 and 26 are as follows:

... in the case of money, just what does set a boundary to a movement of general prices in either direction, and especially the downward movement, becomes something of a mystery. [“The Business Cycle, Interest, and Money,” *op.cit.*, p. 211.]

Up to a point, socialist critics have been right in regarding cycles and depressions as an inherent feature of “capitalism.” Such a system must use money, and the circulation of money is not a phenomenon which naturally tends to establish and maintain an equilibrium level. Its equilibrium is vague and highly unstable. Its natural tendency is to oscillate over a fairly long period and wide range, between limits which are rather indeterminate. [*Ibid.*, p. 224.]

My chief ground for disagreement with the Keynesian theory of money is the belief that in view of these facts, [*viz.*, the instability of V]—some, or most, or all of them well recognized by Keynes as well as others—supply and demand curves for “liquidity” have no solid foundation and are not a sound basis for action but are “theoretical” in the bad and misleading sense. [*Risk, Uncertainty and Profit, op.cit.*, p. xlv.]

It might be noted that in his discussion of Keynes’s theory of liquidity preference in his 1941 article Knight readily recognizes that the rate of interest “must equalize the attractiveness of bonds and of money for holding” ([18], p. 221) and earlier in this article he also describes the holding of money as an alternative to holding other assets (p. 210). But this dependence of the demand for money on the rate of interest is not taken account of in Knight’s discussion of variations in the velocity of circulation during the course of the business cycle—despite Knight’s discussion of the cyclical changes in the rate of interest.⁶⁹ All that is discussed in this context is the influence of price expectations.

JACOB VINER: *Studies in the Theory of International Trade* [41], pp. 40–45, 131, *et passim*; “Schumpeter’s *History of Economic Analysis*” [42], p. 365.

The discussion on pages 40–45 of the *Studies* show that Viner thought of the quantity theory as specifying a causal relationship between the quantity of money and its value. The effect of anticipations of price increases in increasing the velocity of circulation is indicated on page 131.

The passage from Viner’s review of Schumpeter is an instance in which it would have been most appropriate for Viner to have indicated (if he had so believed) that the quantity theory specified not the constancy of the velocity of circulation, but the constancy of the functional relationship between this velocity and the variables which determine it.

II. The Lectures

The only relevant passage from my notes on Simons’ lectures is from his course “Economics of Fiscal Policy.” The passage reads:

The only thing that has stopped deflationary movement is that government begins to get insolvent too (fears that cheap money really would set in). So we inevitably get a government deficit which works to stop deflation. There is no automatic recovery—there is nothing in the system to bring this about. This is Simons’ theory of business cycles: deflation until governmental action. There is no stability in the economy—so that’s why we have fluctuations to begin with.

⁶⁹ Knight [18], pp. 219–220. On p. 223, Knight does refer to high “liquidity preference” and low interest rates in depressions; but he does not refer to a causal relationship between these two phenomena (i.e., to a movement along a demand curve for money), and instead presents them as parallel consequences of the same cause, namely, the depression.

Should we obtain deficits by (1) revenue changes, or by (2) spending changes? Simons is in favor of (1).⁷⁰

My lecture notes from Mints's courses contain the following additional relevant passage, taken from his discussion of the Cambridge cash-balance approach:

In modern theory, demand for money is said to have unitary elasticity. Assume that V and T are constant. Then P changes directly proportionate with M . The real value of total money remains the same. [A diagram of a rectangular hyperbola appears at this point.]

Some have said, on a basis of post-World War I experience, that η for money $\neq 1$. E.g., the total quantity of money increased ten times, while goods that could be purchased with this [i.e., a unit of money] decreased $\frac{1}{15}$ [?]. But in this case there have been changes in V and T —contrary to our assumptions. So we assume that there has been a shift from one demand curve to another (also with $\eta = 1$) according as V and T change. [More diagrams follow here.]⁷¹

Mints concluded his course on “Money” with a discussion of policy, in which (among other things) he stated

If [government] stabilizes price level, it will [also] stabilize aggregate demand and thus prevent unemployment. It is inconceivable that the federal government couldn't so increase the cash balances of the public that it wouldn't want to purchase goods.⁷²

Most of Mints's discussions of policy matters were, however, contained in his course entitled “Banking Theory and Monetary Policy” (Economics 331). The material presented here closely paralleled the corresponding discussions in his books *A History of Banking Theory* and *Monetary Policy for a Competitive Society* [23, 25].

There is nothing of relevance in my lecture notes from Viner's courses (on economic theory, international trade). In the notes from Knight's lectures on economic theory there is a passage which repeats the analysis of his article entitled “The Business Cycle, Interest, and Money: A Methodological Approach,” [18] which had appeared a few years before. Indeed, the notes refer explicitly to this article and read:

Keynesian economics.—The older viewpoint assumed neutral money: money only as an intermediary, so really have barter [economy]. Say's law—*loi des débouchés*. Under ideal competition [conditions?] wouldn't have any money used as medium of exchange—just as unit of account.

Keynes did not do anything not adumbrated in previous writings. Instead of saying, “Every supply of goods is a demand for other goods,” he said, “Every supply of goods is a demand for money.” Keynes hypostatizes money under the name “liquidity preference.” People want money as such—for its own sake—not as immediate purchasing power.

Knight says that the demand for money is highly speculative—especially in an

⁷⁰ Lecture notes from Henry Simons, “Economics of Fiscal Policy” (Economics 361), April 20, 1945.

⁷¹ Lecture notes from Lloyd Mints, “Money” (Economics 330), June 30, 1944.

⁷² *Ibid.*, August 11, 1944.

investors' market and even in a consumers' market. If one considers changes in prices relative to changes in interest rates—the former are much greater than the latter. Thus, if one foresees rising prices, he will borrow money to buy goods; and when he foresees lower prices he will hurry to sell now. The anticipation itself will create the price change—and this is cumulative. “Every speculation on the future value of goods is a speculation on the future value of money.” The essential fact in a slump is just that. In a boom everyone begins to realize that prices are really too high—overcapitalization. All changes in the value of money tend to be cumulative—an unstable equilibrium.

In wheat futures market we have the same thing—anticipation creates changes. But there is an equilibrium there which is dependent on well-known objective facts. So have damped oscillations. But there is no definite, known equilibrium value of money. Everyone might know that money is too high—but the question is whether it will continue to rise; one doesn't know where the breaking point is.

Knight doesn't know how to stabilize the price level—or at what height to stabilize it.⁷³

I cannot resist citing in addition the following typical Knightian remark, which occurred at a later point:

In medieval times men didn't look for remedies since they thought everything came from God, who was good—so everything [must be] good. Now science is the God— and we think that there must be a remedy for every disease. Maybe [there] is no answer to the business cycle: [maybe we] have to let it take its course.⁷⁴

⁷³ Lecture notes from Frank Knight, “Price and Distribution Theory” (Economics 301), July 24, 1945.

⁷⁴ *Ibid.*, July 26, 1945.

THE THESES

DOCTORAL THESES ON MONETARY PROBLEMS SUBMITTED TO THE UNIVERSITY OF CHICAGO, 1930-1950

Author	Title of Thesis	Thesis Committee	Date of Submission
Ernest R. Shaw	The Investment and Secondary Reserve Policy of Commercial Banks	L. D. Edie,* S. P. Meech, L. W. Mints	1930
Francis A. Linville	Central Bank Cooperation	H. D. Gideonse (?), L. W. Mints, J. Viner	1937
Benjamin F. Brooks	A History of Banking Theory in the United States Before 1860	F. H. Knight, L. W. Mints,* J. Viner	1939
Martin Bronfenbrenner	Monetary Theory and General Equilibrium	F. H. Knight, L. W. Mints, H. Schultz,* J. Viner	1939
Joseph E. Reeve	Monetary Proposals for Curing the Depression in the United States, 1929-35	G. V. Cox, L. W. Mints,* J. Viner	1939
George L. Bach	Price Level Stabilization: Some Theoretical and Practical Considerations	L. W. Mints, H. C. Simons, J. Viner	1940
Mrs. Marion R. Daugherty	The Currency School—Banking School Controversy	G. V. Cox, L. W. Mints, J. Viner*	1941
Benjamin Caplan	The Wicksellian School—A Critical Study of the Development of Swedish Monetary Theory, 1898-1932	O. Lange, H. C. Simons, J. Viner	1942
Arthur I. Bloomfield	International Capital Movements and the American Balance of Payments, 1929-40	O. Lange, L. W. Mints, J. Viner*	1942
R. Craig McIvor	Monetary Expansion in Canadian War Finance, 1939-1945	R. Blough,* J. K. Langum, L. W. Mints	1947
Don Patinkin	On the Inconsistency of Economic Models: A Theory of Involuntary Unemployment	P. Douglas, H. G. Lewis, J. Marschak,* T. Yntema	1947
William W. Tongue	Money, Capital and the Business Cycle	O. Lange (?), H. G. Lewis, F. H. Knight, L. W. Mints*	1947
Roland N. McKean	Fluctuations in Our Private Claim-Debt Structure and Monetary Policy	A. Director, E. J. Hamilton, L. A. Metzler, L. W. Mints*	1948
Joel W. Harper	Scrip and Other Forms of Local Money	S. E. Leland,* L. W. Mints, H. C. Simons (?)	1948
Raymond H. McEvoy	The Effects of Federal Reserve Operations, 1929-1936	E. J. Hamilton, L. A. Metzler, L. W. Mints*	1950

* Where known, the committee chairman is designated by an asterisk.

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