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ESSAYS ON AND IN THE CHICAGO  
TRADITION\*  
A REVIEW ESSAY

by  
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\*By Don Patinkin [Durham, NC: Duke University Press, 1981], pp. xii-315

\*\*I am grateful to my colleagues Peter Howitt and David Laidler for their savage criticisms of a preliminary effort at this review, and to Milton Friedman and an anonymous referee for their most helpful comments on an earlier draft. I am also grateful to Roger Farmer (of the University of Pennsylvania) for helping me track down the first edition of a relatively obscure book that I read as shedding some light on traditional Chicago views concerning the theory of money.

This is a collection of twelve essays (all published before), and a reminiscence of the Chicago that Patinkin knew as a student, enlivened with a series of photographs of some of the key members of the Chicago community of the 1940's. The range of the essays is considerable. Three are on price theory,<sup>1</sup> three on macroeconomics,<sup>2</sup> five on the history of thought and social history,<sup>3</sup> and one biographical--on Patinkin's most illustrious teacher, Frank Knight.

As the title of the collection implies, Patinkin portrays the Chicago Tradition by the employment of two contrasting devices. The first device leaves the reader to infer what that tradition is from the sample of it presented--essays In the tradition. The second spells out and documents in varying detail Patinkin's perception of what the tradition actually was--essays On the tradition.

On my reading, two (but according to Patinkin three) Chicago Traditions are portrayed in this book. First, there is "the traditional 'Chicago School'--that School which, with due regard to the differences among them, can be identified with the teaching of Frank H. Knight, Jacob Viner, Henry C. Simons, and Lloyd W. Mints" (p. 3).<sup>4</sup> The second Chicago Tradition is one that emerged when the above group was augmented by Oskar Lange (who arrived in 1939) and the members of the Cowles Commission, directed by Jacob Marschak (a group that began arriving in Chicago in 1943 and that included Ted Anderson, Trygve Haavelmo, Leo Hurwicz, Lawrence Klein, Tjalling Koopmans, Herman Rubin and (later) Kenneth Arrow). Also, arriving in 1943, were the agricultural economists--Theodore W. Schultz, D. Gale Johnson and William H. Nicholls--exiled from Iowa. H. Gregg Lewis should also be mentioned in connection with this strand of Chicago economics--a mathematical as well as econometric approach to the subject.

A third tradition identified--indeed highlighted--by Patinkin is one that emerged in the 1950's under the influence of the seminal contributions to macroeconomics and monetary theory of Milton Friedman. Whether or not that is a separate Chicago Tradition is a matter in which I shall comment at some length in what follows.

Patinkin's essays In the Chicago Tradition reflect, in part, both the traditional Chicago School and the new mathematical school of the 1940's. Of these, the doctrinal history essays, as well as those dealing with price theory, have clear origins in the scholarly traditions of Viner and the rigorous price theory of, in particular, Simons. The macroeconomics essays, like Money, Interest and Prices, reflect something of the traditional school but are more obviously and directly works in that later mathematical tradition. None of the essays In the tradition provide examples of either the monetary theory or econometric traditions, though three of the essays On the tradition are explicitly about monetary doctrines.

It is this latter aspect of Patinkin's work that, for me at least--and as foreshadowed above--raises problems and puzzles. I will focus attention, therefore, in this area--that is, on Patinkin's "disquisition...on... the School's...monetary doctrines" (p. xi).

Patinkin's central message in this respect--well known and extensively debated in the subsequent literature<sup>5</sup>--is that what Milton Friedman wrote in 1956 ("The Quantity Theory of Money--A Restatement") was not in the Chicago Tradition. Rather, it was Friedman's own invention and was heavily influenced by, and reflected the teachings of, not Friedman's Chicago mentors, but Keynes.

It is the essence of Patinkin's view that Friedman's restatement of the quantity theory may be summarized in two propositions; (i) the quantity

theory of money is, first and foremost, a theory of the demand for money and (ii) the velocity of circulation as a stable function of a limited number of variables and particularly the nominal rate of interest. He contrasts the traditional Chicago view with Friedman's by summarizing that earlier view in two very different propositions, namely: (i) the quantity theory of money is, first and foremost, a theory of the determination of nominal income (or, equivalently, of  $MV$ ) "in accordance with Fisher's  $MV = PT$ " (p. 245), and (ii) the velocity of circulation fluctuates considerably and may be unstable.

Friedman is contrasted with the traditional Chicago School on policy matters as well. In this regard, Friedman's well-known advocacy of a constant growth rate for the money stock is contrasted with the view of the Chicago scholars of the 1930's that fluctuations in the quantity of money may be required to offset fluctuations in velocity. The implication of these comparisons is that Friedman is not in the Chicago Tradition. Indeed, pressing the matter harder, in the area of monetary theory, Friedman is a Keynesian. Further, in the area of policy, though not in the area of theory, the traditional Chicago School was Keynesian!

We can, of course, define words--even words like "Chicago Tradition" and "Keynesian"--to mean whatever we like, provided that we are explicit enough for the purpose at hand. Is it a convenient use of language to regard Friedman as being a Keynesian rather than being in the Chicago Quantity Theory Tradition?

In summarizing his case, Patinkin writes:

"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 (1968, p. 440a) 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.' Correspondingly, as I have noted elsewhere, 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 relationship 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.

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." (p. 255)

It will be helpful first to clarify my understanding of what Patinkin means by the phrase "Keynesian monetary theory." I take it that "Keynesian monetary theory" is not the same thing as Keynes' General Theory of ... money, but something narrower and having its origins in the Treatise. This interpretation seems natural in the light of Patinkin's own work on the General Theory. In that work, Patinkin argues that the "Central Message of the General Theory [sic] is the equilibrating role of changes in output generated by discrepancies between aggregate demand and aggregate supply." [Patinkin (1979, p. 175).] This contrasts markedly with what Patinkin identifies as the differentia of Keynesian monetary theory--details concerning the demand for money function. It is of some importance, I think, to note that, in establishing the essential contribution of the General

Theory Patinkin demonstrates that, aside from seeing income as the equilibrating variable, all other aspects of the General Theory are to be found in the earlier writings of Keynes and others. In particular, the demand for money analysis is a direct outgrowth of the Treatise and has antecedents, as Patinkin recognizes, in the Cambridge cash balance School. (See p. 255.)

Was the Cambridge cash balance approach different from the quantity Theory? Patinkin recognizes that it is now "a commonplace of monetary theory that these two approaches can be made analytically equivalent. Nevertheless, (Patinkin goes on) 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!" (p. 247)

Do we find that monetary economists in the pre-Keynesian age approached monetary theory consistently from the perspective of the Fisher equation and not "from the viewpoint of the demand for money"? Was the now commonplace understanding of the analytical equivalence between the quantity theory approach and the Cambridge approach thoroughly appreciated in the pre-Keynesian period?

There seems to be ample evidence from the literature of the period that it was. Thus, for example, Pigou (1917) insisted that:

"Tho the machinery that I shall suggest in the following pages is quite different from that elaborated by Professor Irving Fisher in his admirable Purchasing Power of Money, and, as I think, more convenient, I am not in any sense an 'opponent' of the 'quantity theory' or a hostile critic of Professor Fisher's lucid analysis. He has painted his picture on one plan, and I paint mine on another. But the pictures that we both paint are of the same thing, and the witness of the two, as to what that thing in essentials is, substantially agrees." [Pigou (1917), p. 163.]<sup>6</sup>

Pigou goes on to show in explicit terms the formal equivalence between the demand for money and the velocity of circulation. He concludes this demonstration with:

"It is thus evident that there is no conflict between my formula and that embodied in the quantity theory. But it does not follow that there is nothing to choose between them. Mine is not of course any 'truer' than its rival. They are both equally true. The claim that I make on behalf of mine is merely that it is a somewhat more effective engine of analysis. It focusses attention on the proportion of their resources that people choose to keep in the form of [money] instead of focussing it on 'velocity of circulation'. This fact gives it, as I think, a real advantage, because it brings us at once into relation with volition--an ultimate cause of demand--instead of with something that seems at first sight accidental and arbitrary. But to argue in the air about the merits of a machine is always a waste of time. I offer this specification of it in order that those interested in monetary problems may test its powers in actual work upon concrete problems." (p. 174)

The above makes it clear that Pigou, at least, understood the formal equivalence between the quantity theory and the Cambridge cash balance approach. Did anyone else? It seems rather evident that Lionel Edie (1928) did. Having taken his reader through Irving Fisher's quantity equation he goes on "whether one adopts the quantity theory or some other causation theory of the value of money, one finds it necessary to have rather definite concepts of supply and demand factors in this special branch of value theory. The theory of the value of money is a particular application of value theory in general, and the exact nature of the manner in which general value theory applies to money in particular cannot be understood until adequate concepts of the demand and supply of money are understood." [Edie (1928), p. 191.] As an aside, let me note the striking similarity in the language used here by Edie and that used by Friedman in 1956.

Another writer whose views on this matter are interesting (for a reason that will become apparent below) is a little known then Assistant



Professor of Finance in the University of Pennsylvania, F. Cyril James.

In 1930 Cyril James published his The Economics of Money, Credit and Banking. Chapter 4 of that book--a general introductory text--deals with "the standard of value". It presents a brief but matter-of-fact exposition of Fisher's statement of the quantity theory. It then goes on to develop certain objections to the quantity theory and finally to some "modern formulations of the quantity theory" [James (1930), p. 54]. Some of the material on "modern formulations" is worth reproducing. James writes as follows:

"On the whole,...economic opinion in the United States has tended to hold a version of the theory substantially similar to that of Professor Fisher, and for a complete restatement of the theory in satisfactory and convenient form, we must turn to the European writers who have been forced by the economic changes of the last two decades to reconstruct their exposition of the principles of monetary science."

James refers to R. G. Hawtrey (1927) as well as Edwin Cannan (1927) and develops the idea "that changes in the value of money will be proportional to changes in the willingness of people to hold stocks of money" (emphasis in original, p. 55). He goes on:

"Variations in the individual stocks of money are not... of primary importance, for it is the aggregate amount of all the individual stocks...which exercises an influence upon the general price level.

The desire of people to hold stocks of money is not...a constant.

...

When...it is widely believed that the general level of prices will rise in the near future, the natural reaction will be for individuals to decrease the size of their stock of money in order to increase their stock of goods - and if there is a general feeling that prices are due to fall, there will be a tendency for the stock of money held by the community to increase."

Finally, James tells us that:

"This formulation of the quantity theory of money receives support from the experiences in several of the countries of Europe during the period from 1914 to 1928."

It seems apparent from the passages quoted above that Cyril James certainly understood that the quantity theory of money could be formulated in terms of a theory of demand for money and, furthermore, that such a formulation was explicitly a portfolio analysis--an analysis of substitution between money and goods.

Although it is clear, from the above evidence, that Pigou, Edie and a relatively obscure textbook writer all appreciated that the quantity theory of money was a theory that could only be given satisfactory content by organizing an analysis in terms of the demand for money, and solid though that evidence might be, it does not, of course, necessarily imply that the traditional Chicago scholars of the 1930's appreciated the equivalence. What is the evidence on this?

It seems (to me) hard to make any sense at all of the passage quoted by Patinkin from Henry Simons' "Banking and Currency Reform" unless it is imagined that Simons thought that he was talking equivalently about demand for money or velocity of circulation. Let me quote Simons (from Patinkin):

"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 lending and investment at the expense of their idle reserves of cash. In a word, the velocity of circulation will increase." (p. 257)

There is a certain lack of explicitness in the passage quoted concerning stocks and flows. But that Simons is talking about portfolio reallocations (away from idle cash and into inventories and plant capacity) as being the equivalent of a change in the velocity of circulation is transparent.

Simons was not alone (at Chicago) in viewing variations in velocity and variations in the demand for money as being equivalent. It is clear that Mints also shared that view. The best evidence supporting that conclusion that I know of is from Mints's review of Cyril James work from which I provided some quotations above. (Indeed, the main reason for citing James is not to be able to establish what his views were--interesting though they are in and of themselves--but for the inferences that they enable us to make concerning the views of Mints's.)

Mints reviewed that book (1931) in the following terms:

"Mr. James has written an introductory text which possesses two points of merit in marked degree. The first relates to composition...the second... [to the fact that it] emphasizes...the significant economic problems which arise out of the functioning of monetary and commercial banking systems. ...

Approximately one-fourth of the book is devoted to the subject of money, and a fairly inclusive presentation is given. The discussion of the primitive theoretical aspects of the value of money is somewhat brief, even for an elementary text. A concise summary of Fisher's exposition of the quantity theory is given, and criticized upon the ground that it is inaccurate in a number of ways. Preference is given, therefore, to a presentation of the theory which emphasizes variations in the cash balances held by individuals, ..." (p. 819)

In going on to evaluate this book, Mints raises many objections and criticisms but nowhere does he suggest that he finds it puzzling, objectionable, or in any other way disturbing to treat the theory of money as a "theory which emphasizes variations in the cash balances held by individuals" rather than in terms of Fisher's exposition.

Frank Knight also seems to share this same basic view. His most explicit and extensive writing on this topic from the period in question is "The Business Cycle, Interest, and Money: A Methodological Approach".<sup>7</sup> Knight presents his views--relevant to the issue at hand--in the following terms:

"The economic process in a pecuniary economy involves the holding or owning, by somebody, of wealth--all the wealth in the economy--and also the entire stock of money. Hence, every property owner has the alternative either of holding money up to the amount of his fortune or of choosing the concrete kind of wealth other than money that he will hold. ...Any belief that the value of money will rise in the future, relative to real wealth, tends to lead men to hold money instead of real wealth, the natural effect of which is a fall in the money value of wealth, which tends to confirm the belief and aggravate the tendency and so on accumulatively. ...

An incipient tendency of prices (of products in general) to rise creates the impression of an upward trend (a downward trend in the value of money). The root of the phenomena in this case is the fact that the money, while not literally consumed, is in part effectively 'used'--i.e., employed in a real, technical, or quasi-technical role in organized production and distribution--and in part is held 'idle' the motive for holding money idle, or especially the main variable motive [my emphasis], is speculation for a rise in its future value." (pp. 57-8)

It is clear from the above quotations that Knight is thinking in terms of a demand for money and in terms of a capital-theoretic approach to the theory of money.

Knight did say more, however, than the above. My purpose in citing that passage is simply to demonstrate that Knight was organizing his thinking in terms of a capital-theoretic demand for money framework. I do not want to assert that Knight had in mind a well-defined and stable demand for money function. The evidence clearly shows that he did not. Although Patinkin does cite passages from this work by Knight, he does not cite the one that provides the clearest evidence on this matter. In this regard Knight says

"The tendency for increase and decrease in speculative holding of money to feed upon itself cumulatively is subject to no such effective check as results from the cumulation of a consumable commodity with a fairly definite demand curve, which is fairly well known, as is the stock held speculatively. Indeed, in the case of money, just what does set a boundary to a

movement of general crisis in either direction and especially the downward movement, becomes something of a mystery...

That the division between active and idle funds is not definite or determinate goes without saying."  
(p. 59)

The evidence presented above seems to lead strongly to the conclusion that Friedman's identification of the quantity theory as being first and foremost a theory of the demand for money is entirely in line with the way in which all the major quantity theorists of the 1930's organized their thinking. It is certainly in line with the way in which they wrote.

A further factor relevant to this matter concerns the historical context in which Friedman was writing as compared with his Chicago mentors. Viewed from a pre-General Theory perspective the Cambridge approach and the quantity theory were indeed different (though logically equivalent). Even Pigou was clear on this for, notwithstanding the passages quoted above, he ended his "Value of Money" essay by noting that

"The 'quantity theory' furnishes a tool which in the skilled hands of Professor Irving Fisher has accomplished great things. But less experienced craftsmen need, I think, a better--a more completely foolproof tool. It is this that, in the preceding pages, I have endeavored to provide." (Pigou, 1917, p. 183.)

Viewed from a post-General Theory perspective, however, and especially a post-bastardization perspective, the similarities between the Quantity Theory and cash balance approach dominate the distinctions between them especially when contrasted with the model of Keynes in the General Theory--a model which, if Patinkin's appraisal of it is correct, essentially amounts to an introduction into monetary theory of the notion that real income is the key equilibrating variable.

Perhaps the most thoroughly documented fact about the Chicago quantity theorists' writing in the 1930's is their neglect of the rate of interest as a factor upon which variations in velocity might depend.

Patinkin takes this as evidence that these scholars did not have in mind the demand for money approach. That is clearly one way of reading the evidence. It does not, however, seem to be the most natural and obvious way. An alternative interpretation, if correct, would not lead to Patinkin's conclusion. Let me hasten to add that I am not claiming that the alternative (about to be suggested) is correct. I merely advance it as a hypothesis.

An alternative interpretation runs as follows. First, Chicago quantity theorists were aware of the weak procyclical comovements of interest rates and velocity. They viewed each of these variables as, what we would today call, endogenous variables. Their monetary theory was couched in a language which, again using today's modes of expression, we would call reduced form analysis. That is, they did not set out explicit structural models (even in words) and proceed to analyze the effects of exogenous variables on endogenous variables by way of an explicit manipulation of such a model. Rather they conducted their analysis in terms of an attempt to sort out the effects of variables perceived as exogenous on endogenous variables with a less than fully articulated structural model and a good deal of what we might today call "arm waving". (There is, incidentally, still something of this in Friedman's quite modern work on monetary theory. His continued reluctance to be pinned down on an explicit theory concerning the division of a change in nominal demand between real output and prices is an example.)

Viewed from this perspective it would have struck a Chicago quantity theorist as quite uninteresting to talk about the connection between the rate of interest and velocity. The data told him that such a relationship existed but, since both are simultaneously induced by some deeper and more primitive exogenous force, to emphasize the connection--even to mention it--would, from the perspective of that time, have seemed like superficial description.

The best documentary evidence that I can find supporting my hypothesis is in Knight's 1941 paper cited above. Towards the end of that article he notes that he has said very little about rates of interest and asks what role they play in the cycle. His answer is "very little". It is clear that he does not see interest rates as playing any role of prime mover and, therefore, sees little need to investigate their influence.

A view advanced strongly by Patinkin is that the traditional Chicago quantity theorists did not have in mind the existence of a stable demand for money (velocity) function. There is a feature of their writing, however, that raises questions (at least for me) about even that conclusion. This feature is the failure in those traditional writings to distinguish between stability in two senses: the stability of the functional relationship and the dynamic stability (or stationarity) of a difference or differential equation. One of the major contributions of Friedman (and Phillip Cagan (1956)) was to emphasize the importance of this analytical distinction in the context of the demand for money. Asserting that the demand for money is a stable function of a limited number of variables (Friedman's position) does not imply that velocity may not be highly volatile or indeed dynamically unstable (non-stationary). Whether or not velocity is dynamically unstable will depend on the precise relationship between money, velocity, and prices on the one hand and the expected rate of inflation on the other. Although not devoid of analytical considerations, this will be primarily an empirical matter.

One passage from Simons quoted by Patinkin is certainly capable of being read as if Simons understood this matter. Simons writes:

"The bottom of an uncontrolled deflation, for all practical purposes, is nonexistent--with adverse expectations causing price declines and with the actual declines aggravating expectations, etc. ["Hansen on Fiscal Policy," 1942, p. 188]." (p. 258)

Simons is not here using the words "there is a stable functional relationship linking velocity to inflation and that relationship may be relied upon to generate a nonstationary price process". It is difficult to understand, however, how he could have talked about "adverse expectations causing price declines...with actual declines aggravating expectations, etc." if he was not thinking in those terms. I have no way of knowing whether Simons really understood that but his words are so clear that it is hard to see how else they may be interpreted. If this passage from Simons is read as saying that Simons did indeed appreciate the distinction between dynamic stability and stability of a functional relationship it is the dynamic instability and not instability of the functional relationship to which he was drawing attention and from which he thought problems for price stability would arise. Interpreted in that way Friedman and Simons differ only in that one of Friedman's students<sup>8</sup> convinced Friedman that dynamic instability of the price process does not appear to occur as an empirical matter while Simons had no such empirical knowledge.

Patinkin's analysis of the distinctions between Friedman and the traditional Chicago approach extend beyond matters of theory and deal also with differences on policy. Patinkin documents the differences between Friedman and traditional Chicago and the quite remarkable similarities between that traditional Chicago view and the Keynes of the General Theory and beyond. Friedman advocates a fixed rule whilst his Chicago mentors advocated active variation in the quantity of money to offset fluctuations in velocity. There is one aspect, however, of



the distinction between traditional Chicago and Keynes that Patinkin does not emphasize--but, in my view ought to have done. He says

"I have already stressed the common view of Keynes and the Chicago School on the short-run instability of the economy. Another common feature was the fact that each chose what we today would call a discretionary policy to deal with this instability." (p. 303.)

I believe that Patinkin is wrong to identify each school as choosing what we would today call a discretionary policy. On just one page earlier (p. 302)

Patinkin quotes Simons' criticism of Keynes noting that

"He [Keynes] overlooks the need [clearly suggested by his own analysis] for the minimizing of monetary uncertainties and the achievement of a monetary system based on definite and stable rules." (My emphasis.)

I view this statement by Simons as showing that he was not in favor of discretionary policies but rather the pursuit of explicitly stated and well understood rules.<sup>9</sup> It is interesting to note that this distinction between Keynes and traditional Chicago serves to emphasize a continuity between the Chicago School that extends well beyond that running from the 1930's to Friedman and coming all the way up to the current period. The emphasis on policy rules that arises out of the rational expectations framework developed by Robert E. Lucas, Jr. is the content of that continuity.

What is certainly true is that Friedman is a monetarist and traditional Chicago was not. Indeed, there is, on my reading, a remarkable similarity between traditional Chicago and those modern-day anti-monetarists who worry about innovations and deregulation in the financial sector as sources of instability in the demand for money function and, therefore, as justifications for ad hoc discretionary variation in the supply of money. Friedman, in contrast, belittles the effects of these matters on the demand for money and emphasizes the importance of sticking to a steady and predictable behavior of the money supply. Based on their views, as reported by Patinkin, it is hard to resist the conclusion that,

had the deregulation and financial innovations of the past few years been occurring in the 1930's Simons and the others would have viewed them in much the same way as anti-monetarists do today.

Friedman has suggested, however (in his Henry Simons lecture), that

"Had Simons known the facts as we now know them, he would, I believe, have been confirmed in 'his earlier persuasion as to the merits of the rule of a fixed quantity of money...' rather than have accepted...stabilization of the price level..."  
(Friedman, 1967, p. 92.)

Patinkin painstakingly rehearses the facts that were known to Simons and concludes that he was in possession of all the relevant facts and, despite this, and even more strongly despite having considered a fixed money stock as a possible rule, advocated variations in the quantity of money to achieve his price level target.

Patinkin has on occasion (and quite rightly) drawn attention to "one of the oldest and most dangerous pitfalls that lies along the path of those who venture into the history of doctrine". This is the pitfall of "reading things out of context" (Patinkin, 1983, p. 48.) There is a hint in Patinkin's treatment of Friedman's assertions concerning Simons' policy position that Patinkin has not completely avoided this "dangerous pitfall". Patinkin quotes Friedman as follows

"Now, Friedman quite reasonably says that Simons' belief in the instability of velocity was based on his interpretation of the Great Depression; but Friedman goes on to contend that had Simons then known the 'facts that we now know and he did not' (1967, p. 84), he would not have interpreted the Great Depression in terms of such an instability of V, and would accordingly have chosen instead of policy rules stated in terms of the quantity of money." (p. 296)

After this statement and after confessing that he (Patinkin) has some difficulty in understanding the operational meaning of such a statement he goes on to demonstrate that Simons knew the facts about the Great Depression as subsequently presented by Friedman and Schwartz in A Monetary History of the United States (1962) and

also that Simons was familiar with the work of Lauchlin Currie which, in effect, provided a virtually identical interpretation of the Great Depression to that subsequently supplied by Friedman and Schwartz. Patinkin's implicit conclusion from all this is that Friedman is wrong. Simons was in possession of all the relevant facts and, despite that, he still reached the conclusion that a quantity of money rule would be inappropriate.

Before going on to consider what Friedman actually said let me note that, although the interpretation of the Great Depression provided by Currie was very similar to that subsequently supplied by Friedman and Schwartz, the basis for that interpretation was much less solid in Currie's paper. He provided an analysis of only ten years of data. This stands in marked contrast to Friedman and Schwartz who analyzed the Great Depression as one (albeit extreme) cycle in a 93 year period of U.S. history.

Let me now turn however to what Friedman actually said in his Simon lecture and contrast that with Patinkin's out of context reading. What Friedman in fact said was this:

"Since Simons wrote, an enormous amount of evidence has accumulated that bears not only on these few years [i.e., the Great Depression] but also on a far wider range of economic history. This evidence, ..., contradicts Simons' interpretation of the sources of instability. It turns out that the rate of growth of the quantity of money has systematically tapered off well before the economy in general slows down and has speeded up well before the economy speeds up. The movements in velocity--which Simons took as an independent source of instability--come later than the movements in the quantity of money and are mild when the movements in the quantity of money are mild. They have been sharp only when there have been sharp movements in the quantity of money. There is no evidence to support Simons' fear that a fixed quantity of money might involve 'the danger of sharp changes on the velocity side'. On the contrary the evidence is precisely the reverse--that it would lessen the danger of sharp changes in velocity." (Friedman, 1967, pp. 91-2.)

Read in context it is clear that Friedman is not claiming that Simons did not know about the Great Depression. The facts of which Simons was ignorant

were the facts that can only be discovered by a systematic investigation of a longer run of data than a single (albeit extreme) cyclical episode. In view of Patinkin's documentation of Simons' anti-empirical approach it is not clear whether Simons would have been as impressed with this evidence as Friedman is and as Friedman believes he would have been. It is clear, however, that Patinkin misquoted and misunderstood the reasoning that led Friedman to that conjecture.

Patinkin regards

"Questions about the history of economic doctrine [as] 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."  
(p. 242.)

Subsequently, when studying Keynes, he went on to declare that:

"I see it [the history of thought] as an empirical science in the broader sense that as historians of thought we are like econometricians fitting a multivariate regression equation to a man's writings: We are trying to pass a regression line through them that will best explain them. Now, one thing about a regression line is that there are always points off it, and then the question is whether they are random departures from the line, or whether they reflect a systematic influence that you have not taken account of. And the same is true when you pass a regression line through a man's writings: there will generally be some passages in his writings that you have not explained. And then you have to decide what is the true meaning of the man, what is his regression line and what is a chance phrase, a chance formulation, or perhaps even a mistake, whose departure from the regression line shouldn't make us change our view about the nature of the line." (Patinkin and Leith (1978), pp. 125-6.)

Patinkin is here writing explicitly in the context of his work on Keynes and the General Theory. He does not, however, write in a way that invites us to regard him as restricting his attention only to that author and work. The passage that I have quoted sounds like a declaration of a general approach to the history of thought.

The analogy between texts and data and using texts to discern the true meaning of a writer and fitting a regression line is certainly a nice one.

It invites an extension. Two aspects of inference have received a lot of attention at the hands of empirical (statistically oriented) economists, both of them appropriately at Chicago. One is the simultaneous equation problem<sup>10</sup> and the other is the sample selection bias problem.<sup>11</sup>

The historian of thought needs to pay special attention to both of these problems in his "as if" regression running. A simultaneous equation problem arises for the historian of thought from the inevitable interaction between various traditions. Sample selection bias arises because scholars do not write down what seems to them to be obvious with the frequency that they write down those things which they are wrestling. I would interpret some of my remarks in the foregoing as being an attempt to address the consequences of these two types of problems. I make no claim, however, to having invented a new estimator, or to having, in any adequate sense, solved these problems.

Patinkin has provided us with an account of traditional Chicago monetary theory and of the work of Friedman that creates an overall picture which, in my view, puts the "regression line" in the wrong place and leaves too much in the error term. I have tried to show the essential continuity between the monetary theory of Milton Friedman and that of his Chicago precursors. There is, of course, also a continuity between the monetary theory of Keynes and that of Friedman. Keynes' monetary theory, however, is itself solidly in the Cambridge cash balance tradition--a tradition that is equivalent to the quantity theory approach.

Over and above all this (and not dealt with above because it is only tangential to Patinkin's treatment of the matter), Friedman seems to me to be in the Chicago tradition in a much more important way. That is, not only is Friedman a quantity theorist but also a quantitative quantity theorist. The quantitative tradition, eclipsed in Chicago with the death

of Henry Schultz in 1938, but re-established in 1943 with the arrival of the Cowles Commission emerges as one of the clear Chicago antecedents of the careful systematic empirical investigations that Friedman subsequently undertook and inspired his students to pursue in establishing some of the propositions of monetary theory and policy that he (Friedman) sees as being distinctly in that earlier Chicago Tradition.

## FOOTNOTES

<sup>1</sup>"Multiple-Plant Firms, Cartels and Imperfect Competition"--Quarterly Journal of Economics, 1947; "Demand Curves and Consumer's Surplus"--Measurement in Economics: Studies in Mathematical Economics and Econometrics in Memory of Yehuda Grunfeld, 1963; "On the Economic Theory of Price Indexes"--Economic Development and Cultural Change, 1961 (with Nissan Liviatan).

<sup>2</sup>"The Indeterminacy of Absolute Prices in Classical Economic Theory"--Econometrica, 1949; "Reflections on the Neoclassical Dichotomy"--Canadian Journal of Economics, 1972; and "Involuntary Unemployment and the Keynesian Supply Function"--Economic Journal, 1949.

<sup>3</sup>"In Search of the 'Wheel of Wealth'.."--American Economic Review, 1973; "Mercantalism and the Readmission of the Jews to England"--Jewish Social Studies, 1946; "The Chicago Tradition, the Quantity Theory, and Friedman"--Journal of Money, Credit and Banking, 1969; "More on the Chicago Monetary Tradition"--Southern Economic Journal, 1973; and "Keynes and Chicago"--Journal of Law and Economics, 1979.

<sup>4</sup>The page references with no further qualification refer to the book under review. Others at Chicago when Patinkin arrived there in 1941 were Paul H. Douglas and John U. Nef.

<sup>5</sup>See, for example, Milton Friedman (1972); Don Patinkin (1972); George S. Tavlas (1977a, and 1977b).

<sup>6</sup>The page reference given here is to the reprint in Readings in Monetary Theory. London; George Allen and Unwin Ltd., 1952.

<sup>7</sup>Frank H. Knight (1941).

<sup>8</sup>I am referring here to the work of Phillip Cagan (1956).

<sup>9</sup>It must be admitted, however, that Simons advocated a fixed rule for the price level while Friedman proposed one for the money stock. With variable velocity, the pursuit of Simons' rule would require a feedback rule for the money stock.

<sup>10</sup>The work of Haavelmo on this problem is referred to in Patinkin's reminiscence chapter.

<sup>11</sup>One of the early discoverers of this problem is even closer to home for Patinkin--see Reuben Gronau (1976). The name most associated with this matter is, however, James Heckman (1979).



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