

## Comments on the Critics

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# Comments on the Critics

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## Milton Friedman

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My reply to five scholars who have commented on my articles (1) restates the aim and purpose of these articles; (2) explains why the slope of the *LM* curve is not the key difference between monetarists and neo-Keynesians, and elaborates on what I consider to be the key difference; (3) defends my interpretation of Keynes's theory; (4) documents the existence of a distinctive Chicago tradition that had a great influence on my own work in the field of money; and (5) analyzes the quantity theory antecedents of my formulation of the demand function for money and of the role of money in the economy.

We are, as I have said, one equation short. Yet it might be a provisional assumption of a rigidity of money wages, rather than of real wages, which would bring our theory nearest to the facts. . . . A theory cannot claim to be a *general* theory, unless it is applicable to the case where (or, the range within which) money wages are fixed. [KEYNES, *General Theory*, p. 276]

Here, in Keynes's own words, is the kernel of my interpretation of the distinctive feature of what I called the "Keynesian challenge to the quantity theory."<sup>1</sup> My discovery of this passage in the *General Theory* in the course of preparing this reply reinforces my confidence that the interpretation is valid. Yet three of the five scholars who have done me the honor of commenting on my two articles reject that interpretation. I believe that all three are mistaken. I conjecture that Tobin is led into error because

I am in debt for helpful comments on an earlier draft of this reply to Armen Alchian, Robert Clower, and Robert J. Gordon, and also to Allan Meltzer, Paul Davidson, James Tobin, and Don Patinkin. The usual disclaimer of their responsibility for my mistakes is more than usually relevant.

<sup>1</sup> This passage is embedded in a discussion of Pigou's *Theory of Unemployment* with which Keynes was contrasting his own theory.

he interprets the *General Theory* from a methodological point of view alien to Keynes; Davidson and Patinkin, because they mistake minor themes in the *General Theory* for its central message.

Despite this element of overlap, I reply to each criticism separately, both because the critics support their rejection of my interpretation of Keynes on different grounds, but mainly because this rejection is only a minor theme in some of the comments.

I largely agree with the observations of Brunner and Meltzer, who do not comment in any detail on my interpretation of Keynes. The appearance of disagreement simply reflects their gracious assumption that my objective was much more ambitious than it was. Accordingly, I discuss their comment first.

Tobin's comment, like Brunner and Meltzer's, is analytical. Hence, I deal with it next.

In writing these articles, I did not intend to engage in doctrinal history and did not express any such intention. I made no attempt to present a comprehensive survey of the development of either the quantity theory or the Keynesian theory. I used references to earlier writers as expository devices to bring out analytical points rather than for their own sake.

Yet both Davidson and Patinkin regard my articles as devoted not only to analysis but also to the history of thought—indeed, Patinkin terms this the “major” concern of the first paper—and criticize them primarily from this viewpoint. Their contention that I do not present a fully documented account of the contributions of individual scholars, and of the temporal succession of ideas, is entirely correct. That was not my aim, and I should have made it crystal clear that it was not. In my own defense, however, the very absence of such documentation for statements that I intended to be analytical but that Patinkin in particular interprets as concerned with doctrinal history might well have been regarded as evidence of my intentions rather than my performance.

However, both Davidson and Patinkin object not only to what they regard as my forays into the history of thought but also to my analytical interpretation of the doctrines I discuss. In this respect, both seem to me mistaken.

I deal with Davidson first, because his criticism is simpler.

Though I leave Patinkin to the last, that is the reply that concerns me most. This is only one of three articles in which Patinkin has, as I see it, given a misleading impression not only of my work but, more important, of the Chicago tradition that inspired that work (Patinkin 1969, 1972). And one of those articles served as the foundation for the even more misleading discussion of that tradition by Harry Johnson in his Ely lecture (Johnson 1971). Accordingly, I am pleased to have this opportunity to document a Chicago tradition that is more distinctive, more important, and

more encompassing than readers of Patinkin's and Johnson's articles might suppose.

One reward from writing this reply has been the necessity of rereading earlier work, in particular the *General Theory*. The *General Theory* is a great book, at once more naïve and more profound than the "Keynesian economics" that Leijonhufvud contrasts with "the economics of Keynes."

The heart of the *General Theory* is an extremely simple hypothesis—that a highly unstable marginal efficiency schedule of investment and a liquidity preference function that is highly elastic at low rates of interest and unstable at higher rates of interest are the key to short-run economic movements. That is what gives investment its central role, what makes the consumption function and the multiplier the key concepts, what enables Keynes to develop his theory for 165 pages without having to introduce the quantity of money.

Of course, the hypothesis is oversimplified. But Keynes was no Walrasian seeking, like Patinkin, and to a lesser extent Tobin, a general and abstract system of all-embracing simultaneous equations. He was a Marshallian, an empirical scientist seeking a simple, fruitful hypothesis. And his was a new, bold, and imaginative hypothesis, whose virtue was precisely how much it could say about major problems on the basis of so little. Of course, his assumptions were not in literal correspondence with reality. If they had been, he would have been condemned to pedestrian description; his whole theory would have lost its power. Of course, he could be wrong. There is no point to any scientific theory that cannot be. The greater the range of evidence that, if observed, would contradict a theory, the more precise are its predictions and the better a theory it is *provided it is not, in fact, contradicted*.

I believe that Keynes's theory is the right kind of theory in its simplicity, its concentration on a few key magnitudes, its potential fruitfulness. I have been led to reject it, not on these grounds, but because I believe that it has been contradicted by evidence: its predictions have not been confirmed by experience. This failure suggests that it has not isolated what are "really" the key factors in short-run economic change.

The *General Theory* is profound in the wide range of problems to which Keynes applies his hypothesis, in the interpretations of the operation of modern economies and, particularly, of capital markets that are strewn throughout the book, and in the shrewd and incisive comments on the theories of his predecessors. These clothe the bare bones of his theory with an economic understanding that is the true mark of his greatness.

Rereading the *General Theory* has not only reinforced my confidence in the validity of the interpretation in my articles; much more important, it has also reminded me what a great economist Keynes was and how much more I sympathize with his approach and aims than with those of many of his followers.

### Brunner-Meltzer

Apparently, I failed to make clear the purpose and scope of my articles.<sup>2</sup> They do not, as Brunner and Meltzer assume, present a fully developed theory that is intended to have as implications all of the empirical regularities that those of us working in this area have isolated. My aim was much less ambitious. It was to outline a general approach that could suggest what empirical issues required study, an approach that could then be elaborated in further detail in connection with such empirical studies.<sup>3</sup> I viewed the articles as supplementing, not replacing, my other writings, as another piece of a continuing endeavor, not as the final word.

In particular, the so-called common model, which occupies four out of forty-two pages in the first article, receives far more attention than it deserves, not only from Brunner and Meltzer but also from the other commentators. This model was specifically designed to bring out the *defects* in both main approaches. For that very reason, it suppressed all complications not relevant for that purpose and was introduced as "a highly simplified aggregate model." My list of unresolved problems ends by stressing that "the central common defect of the two approaches" is that neither "has anything to say about the factors that determine the proportions in which a change in nominal income will, in the short run, be divided between price change and output change" (Friedman 1970a, pp. 217, 222; 1971b, pp. 29, 44). Yet Brunner and Meltzer have interpreted my presenting the "common model" as meaning that I believe it is desirable and necessary "to divide macroeconomic problems into two sets—unemployment in which prices are fixed *or* inflation in which output is fixed"! Since, like them, I believe the exact opposite, I must have failed dismally to convey what I intended.

In combining the two articles into a National Bureau of Economic Research Occasional Paper, I was led by some initial reactions to the first article to include four additional paragraphs on the difference between our approach and the Keynesian approach. If these paragraphs had been in the original article, they might well have removed some of Brunner's and Meltzer's misconceptions.

Another, more subtle, difference between the approach of the economists in the Keynesian tradition and the approach that we

<sup>2</sup> The two *J.P.E.* articles have been combined and published as Occasional Paper 112 by the National Bureau of Economic Research under the title of the first article, "A Theoretical Framework for Monetary Analysis." In giving page references I shall give first the reference to the original article, then to the NBER Occasional Paper.

<sup>3</sup> Aside from the frequent references to "framework," "abstract level," "highly simplified," etc., note the explicit statement, "still other parts of the theoretical framework are developed more fully in the course of the empirical analysis of some of the issues raised in the other chapters of the book from which this paper is abstracted" (Friedman 1970a, p. 222, n. 23; 1971b, p. 48, n. 31).

have adopted has also contributed to much misunderstanding. This difference is in the transmission mechanism that is assumed to connect a change in the quantity of money with a change in total nominal income (= total spending). The Keynesians regard a change in the quantity of money as affecting in the first instance "the" interest rate, interpreted as a market rate on a fairly narrow class of financial liabilities. They regard spending as affected only "indirectly" as the changed interest rate alters the profitability and amount of investment spending, again interpreted fairly narrowly, and as investment spending, through the multiplier, affects total spending. Hence the emphasis they give in their analysis to the interest elasticities of the demand for money and of investment spending. We, on the other hand, stress a much broader and more "direct" impact on spending, saying, as in section 1 above, that individuals seeking "to dispose of what they regard as their excess money balances . . . will try to pay out a larger sum for the purchase of securities, goods and services, for the repayment of debts, and as gifts than they are receiving from the corresponding sources."

The two approaches can be readily reconciled on a formal level. The transmission mechanism that we have stressed can be described as operating "through" the balance sheet and "through" changes in interest rates. The attempt by holders of money to restore or attain a desired balance sheet after an unexpected increase in the quantity of money will tend to raise the prices of assets and reduce interest rates, which will encourage both spending to produce new assets and spending on current services rather than on purchasing existing assets. This is how an initial effect on balance sheets gets translated into an effect on income and spending.

The difference between us and the Keynesians is less in the nature of the process than in the range of assets considered. The Keynesians tend to concentrate on a narrow range of marketable assets and recorded interest rates. We insist that a far wider range of assets and interest rates must be taken into account—such assets as durable and semi-durable consumer goods, structures and other real property. As a result, we regard the market rates stressed by the Keynesians as only a small part of the total spectrum of rates that are relevant. . . .

This difference in the assumed transmission mechanism is largely a by-product of the different assumptions about price. The rejection of absolute liquidity preference forced Keynes's followers to let the interest rate be flexible. This chink in the key assumption that prices are an institutional datum was minimized

by interpreting the “interest rate” narrowly, and market institutions made it easy to do so. After all, it is most unusual to quote houses, automobiles, let alone furniture, household appliances, clothes and so on, in terms of the “interest rate” implicit in their sales and rental prices. Hence the prices of these items continued to be regarded as an institutional datum, which forced the transmission process to go through an extremely narrow channel. On our side, there was no such inhibition. Since we regarded prices as flexible, though not “perfectly” flexible, it was natural for us to interpret the transmission mechanism in terms of relative price adjustments over a broad area rather than in terms of narrowly defined interest rates. [Friedman 1971*b*, pp. 27–29]

With this background, I can deal briefly with Brunner and Meltzer’s summary of the “four types of criticisms” they offer to my approach.

1. “The restrictions that he imposes on the standard theory to remove any short-term effect of changes in interest rates, fiscal variables, and the stock of securities are not well supported by evidence.”

Granted. I never said they were. Indeed, I did not impose them, except in the one section on “The Common Model,” and there not on empirical grounds but for the analytical purpose of highlighting what I regard as the key difference between the quantity theory and the income-expenditure theory. Note that for this purpose I also neglected “stochastic disturbances.” I surely did not do that because I regarded their unimportance as “well established by the evidence.” I really am puzzled that Brunner and Meltzer could have inflated the role of the common model as much as they did.

2. “The framework does not imply some of the main propositions that have been developed in recent years. . . . For example, there is no mention of the variability of the lag in monetary policy. The gradual adjustment of the price level following the adjustment of real output is either assumed or is not obtained at all.”

Unless I am greatly mistaken this is wrong. I may not have mentioned “the variability of the lag in monetary policy,” but as figure 3 of my paper illustrates, the framework does imply a lag that depends on the path of the monetary disturbances fed into the system. Hence, even in the system as explicitly written, the framework does imply variability of the lag, and surely it can be taken for granted that stochastic disturbances must be added to all of the equations before they are used to interpret historical data, which would add still another source of variability. There is a sense in which the gradual adjustment of the price level is assumed, but surely this feature is consistent with, if not implied by, equations (28), (29), and (31) of the original article (eqq. [45], [46], and [48] of the NBER Occasional Paper).

3. “The framework ignores some main developments in economic theory

during the past ten years that have important bearing on the issues discussed . . . [such as the introduction of] cost of acquiring information, cost of search, and adjustment."

I plead guilty. I agree that these have been important developments. But I must confess that even on further consideration they seem to me irrelevant to my limited purpose, except to the extent that they indirectly motivate the emphasis that I give to anticipated rates of change. In this connection I might well have referred to the important writings of Stigler, Alchian, and others.

4. "The explanation of fluctuations in prices and output has very little relation to the static theory of prices and output."

This criticism baffles me. For example, Brunner and Meltzer say, "No doubt a set of postulates can be introduced to reconcile the two, but only at the cost of eliminating interest rates and the negatively sloped *IS* curve from the 'common model' or including interest rates in the adjustment equation." And again, "His differential equations describing the adjustment of output contain neither real balances nor relative prices (including interest rates)."

But of course interest rates are in the adjustment equation for nominal income (eq. [31] of the original paper, [48] of the Occasional Paper), as I say explicitly (Friedman 1970a, pp. 226, 227; 1971b, pp. 52, 53) referring back to the demand for money equation in an earlier section, which includes three different interest rates plus the rate of change of prices. And so also, by the same argument, is wealth, the channel through which the real balance effect operates. Further, the adjustment equation for nominal income enters the adjustment equation for output, so these variables are all in that equation as well.<sup>4</sup>

\* \* \*

All in all, I agree with Brunner and Meltzer that the framework I present is only a beginning. Their own specific model, as outlined in Brunner (1970), the article by Brunner and Meltzer in this issue, and Brunner and Meltzer (1971) is a special extension and development of that framework. I applaud and welcome their efforts in that direction, without necessarily accepting the details of their model.

## Tobin

### 1. *The Main Issue between Monetarists and Neo-Keynesians*

Substantively, the most important point in Tobin's comment is his contention that the main issue between "monetarists and the neo-Keynesians" is

<sup>4</sup> Similarly, I am baffled by their n. 9 in which they regard these differential equations as implying a modification of the monetary rule I have long supported of a fixed rate of growth of the quantity of money, as implying that "the appropriate rate of growth of money is no longer a constant but a variable dependent on past monetary policy."

"the shape of the *LM* locus"—namely, that what he regards as characteristic monetarist propositions require the *LM* curve to be vertical, whereas neo-Keynesian propositions rest on the *LM* curve being positively sloped. I thought that I had disproved this contention in detail in an article on "Interest Rates and the Demand for Money," published in 1966 (reprinted in Friedman 1969). Since Tobin apparently finds that answer unsatisfactory, I shall comment first on this key point.

In my 1966 article, I concluded, "In my opinion, no 'fundamental issues' in either monetary theory or monetary policy hinge on whether the estimated elasticity [of demand for money with respect to the interest rate] can for most purposes be approximated by zero or is better approximated by  $-1$  or  $-5$  or  $-2.0$ , provided it is seldom capable of being approximated by  $-\infty$ " (Friedman 1969, p. 155).

Of the six propositions that Tobin uses to illustrate the opposite contention, it is revealing that one of the three that he alleges to be monetarist is about the effects of real magnitudes on real magnitudes, whereas what I regard as truly "characteristic monetarist" propositions are about the effect of nominal magnitudes on nominal and real magnitudes and are not among any of his six.<sup>5</sup> His third allegedly monetarist proposition asserts that the demand for real balances depends solely on real income. I have never argued that, and I do not know any other monetarist who has. In any event, I demonstrated in my 1966 article that a divorce between monetary and real factors, of the kind embodied in his propositions *a*, *b*, and *c*, is entirely consistent with a positively sloping *LM* curve.<sup>6</sup> Having

It has no such implication *except for the transition* to a constant rate of monetary growth and that requires no modification of what they call the Simons-Friedman tradition. Henry Simons before me, and I in his footsteps, recognized that how one should move to a constant growth path depends on where you start, that it would have been absurd to append a constant growth path to the 1933 money stock, for example, or to the 1948 money stock, that it was desirable to start the long-run growth path from a position of rough monetary equilibrium, not from a position of significant disequilibrium.

In purely mathematical terms, the initial position determines the transient part of the solution to the differential equations. Constant monetary growth is a prescription for the permanent part.

<sup>5</sup> His proposition *a* comes closest to containing a "characteristic monetarist" proposition, but even that is marred by overstatement. I know of no monetarist who regards velocity as an unchangeable constant apart from stochastic disturbances and hence who would regard a change in *M* as both a necessary and sufficient condition for a change in Tobin's *Vp*. I have elsewhere tried to list "systematically the central propositions of monetarism" (Friedman 1970c, pp. 22–26).

<sup>6</sup> In the 1966 article I noted that in the flexible price, full-employment version of the Hicks *IS-LM* model, the "divorce of money from real factors in the sense under discussion requires that there be a way of expressing the equations comprising the theoretical model such that it has a subset of equations sufficient to determine the real magnitudes which do not contain as separate variables either the nominal quantity of money or the price level. In that case, the system of equations simultaneously determining the real and monetary variables can be dichotomized into one set which determines the level of real income and the interest rate and a second set which together with the solution of the first set determines the level of nominal income and

done so, I stressed that I did not myself believe that a model which involved such a divorce was "the most useful, or even *a* useful model to interpret reality. On the contrary," I wrote, ". . . I am myself persuaded that it is far more useful to introduce interactions between the real and monetary sectors" (Friedman 1969, p. 151).

Of Tobin's remaining three propositions, I accept his propositions *d* and *f* as both correct and entirely consistent with a monetarist view (as I shall demonstrate below) but as not in any way a necessary implication of a positively sloped *LM* curve.<sup>7</sup> His proposition *e* is partly factually correct, partly factually wrong, but in any event is a pure assertion that is not dependent in any way on the *LM* curve, which is drawn in real terms, hence cannot imply anything about price changes.<sup>8</sup>

I fear Tobin is right that his six propositions are "the stuff of macroeconomics courses all over the country." Yet they conceal rather than reveal why both monetarists and neo-Keynesians accept the *LM* curve as positively sloped and nonetheless come out with very different conclusions on many issues, particularly the effects of fiscal and monetary policy. Perhaps it will clarify matters if, instead of analyzing Tobin's propositions further, I try to translate a truly characteristic monetarist proposition into Tobin's simple *IS-LM* terms.

Some five years ago, I examined in a number of *Newsweek* columns the likely effect of tax increases that were being recommended as a means of curbing inflation—recommendations that led to the tax surcharge enacted in 1968. Herewith excerpts from two columns, with numbers added in parentheses to facilitate reference.

"I do not share the widespread view that a tax increase which is not matched by higher government spending will necessarily have a strong braking effect on the economy.

(1) "True, higher taxes would leave taxpayers less to spend. But this is only part of the story. If government spending were unchanged, more of it would now be financed by the higher taxes, and the government would have to borrow less. *The individuals, banks, corporations or other lenders from whom the government would have borrowed now have more left to spend or to lend—and this extra amount is precisely equal to the reduction in the amount available to them and others as taxpayers.* If they spend it themselves, this directly offsets any reduction in spending by taxpayers.

(2) If they lend it to business enterprises or private individuals—as they can by accepting a lower interest rate for the loans—the resulting increase

the price level, and this is true regardless of whether the demand equation for money in the second set has the interest rate as one of its variables" (Friedman 1969, p. 150).

<sup>7</sup> Because the effects he attributes to a positively sloped curve could be completely offset by price and wage flexibility which shifted the curve.

<sup>8</sup> The factually wrong part is the assertion that the quantity of money and velocity tend to move in opposite directions; generally, they move in the same direction.

in business investment, expenditures on residential building and so on indirectly offsets any reduction in spending by taxpayers.

(3) "To find any *net* effect on private spending, one must look farther beneath the surface. Lower interest rates make it less expensive for people to hold cash. Hence, some of the funds not borrowed by the Federal government may be added to idle cash balances rather than spent or loaned. In addition, it takes time for borrowers and lenders to adjust to reduced government borrowing. (4) However, any net decrease in spending from these sources is certain to be temporary and likely to be minor.

(5) "To have a significant impact on the economy, a tax increase must somehow affect monetary policy—the quantity of money and its rate of growth" (*Newsweek*, January 23, 1967, p. 86; *italics in original, numbers added*).

"Whether deficits produce inflation depends on how they are financed. (6) If, as so often happens, they are financed by creating money, they unquestionably do produce inflationary pressure. (7) If they are financed by borrowing from the public, at whatever interest rates are necessary, they may still exert some minor inflationary pressure. (8) However, their major effect will be to make interest rates higher than they would otherwise be" (*Newsweek*, August 7, 1967, p. 68; *numbers added*).

To translate:

For points 1, 2, 3, and 4 implicitly, as for point 7 explicitly, it is assumed that the quantity of money or its rate of growth is not affected by the rise in taxes. Hence, what is involved, in Tobin's terms, is a leftward shift in the *IS* curve (part of his proposition *d*) which tends to lower the interest rate (point 2). If prices are assumed unaffected, the *LM* curve is unchanged, but since it slopes positively, the effect is to reduce real income (point 3). If prices were to decline, the *LM* curve would shift to the right, adding to the downward pressure on the interest rate but offsetting the downward pressure on real income.

So far, Tobin would, I believe, agree completely. The difference begins with item 4. Why "certain to be temporary"? Because the leftward shift in the *IS* curve is a once-for-all shift, even though the reduced deficit or increased surplus produced by a tax rise with no change in government spending were to continue indefinitely. Put in monetarist terms, the lowered interest rate resulting from the federal government's absorbing a smaller share of annual savings will reduce velocity; the transition to the lower velocity reduces spending for a given money stock (Tobin's proposition *f*), but once the new velocity is reached there is no further downward pressure.

Why "likely to be minor"? Because the monetarist view is that "saving" and "investment" have to be interpreted much more broadly than neo-Keynesians tend to interpret it, that the categories of spending affected by changes in interest rates are far broader than the business capital forma-

tion, housing construction, and inventory accumulation to which the neo-Keynesians tend to restrict "investment." Hence, even a fairly substantial tax increase will produce only a minor shift in the *IS* curve.<sup>9</sup> Further, while the *LM* curve slopes positively, it is very far from being horizontal, so that the reduction in income associated with a "minor" shift in the *IS* curve will also be minor.

Of course, the terms "temporary" and "minor" are highly imprecise. We get closer to a rigorous statement by comparing the changes resulting from a reduced or increased deficit without any change in monetary growth with those that result when a change in the deficit is matched by a dollar-for-dollar change in monetary growth (points 6, 7, and 8). To interpret these, we must shift gears from the reduction in the deficit assumed in the first excerpt to the increase in the deficit assumed in the second.

Point 7 is the counterpart of the first excerpt: a deficit financed by borrowing, involving, in Tobin's terms, a once-for-all shift to the right in the *IS* curve, a higher interest rate, a higher velocity, and a higher level of spending for a given monetary growth path. Point 6 involves financing the deficit by creating money, which shifts the *LM* curve to the right (so long as prices are assumed constant). But this is not a once-for-all shift. So long as the deficit continues, and continues to be financed by creating money, the nominal money stock continues to grow and the *LM* curve (at initial prices) continues to move to the right. Is there any doubt that this effect must swamp the effect of the once-for-all shift of the *IS* curve? Of course, if prices react, we get into a new set of issues. The rightward movement of the *LM* curve is then offset, but also we need to introduce a distinction between nominal and real interest rates, and the simple *IS-LM* diagram will no longer do.

We may put this point differently. Assume a one-year increase in the deficit, with the budget then returning to its initial position. If this is financed by borrowing from the public with no change in monetary growth, then, in the most rigid Keynesian system, the *IS* curve moves to the right and then back again; real and nominal income rise for one year, then return to their initial values. If the one-year increase in the deficit is financed by creating money, the *LM* curve moves to the right as well, and stays there after the *IS* curve returns to its initial position. If prices remain constant, real and nominal income stay at a higher level indefinitely. If, as is more reasonable, prices ultimately rise, real income may return to its initial level, but nominal income will stay at a higher level indefinitely. Surely, to paraphrase a remark of Tobin's in another connection, the monetary effect is "alchemy of a much deeper significance" than the fiscal effect.

<sup>9</sup> Note that what is at issue is not primarily the "elasticity" of saving or investment, though monetarists would probably set that elasticity higher than neo-Keynesians, but rather the absolute magnitude affected (the position of the relevant investment and saving curves).

But what about the evidences of debt created in the first case? Do they not stay permanently? As Tobin says, "is a 'rain' of Treasury bills . . . of no consequence for the price level, while a 'rain' of currency inflates prices proportionately"? The answer is that the evidences of government debt are largely in place of evidences of private debt—people hold Treasury bills instead of bills issued by, for example, U.S. Steel. The total nominal volume of debt grows by less—and I believe much less—than the size of the deficit. Moreover, even this growth is offset by two other factors: the increase expected in future tax liabilities accompanying the growth of the government debt, which Tobin refers to; and the reduction in the physical volume of assets created because of lowered private productive investment. On the other hand, the dollar bills are a net addition to the total nominal volume of assets.

This analysis is, of course, greatly oversimplified. The effects of changes in asset structure are far more complex. They clearly deserve investigation and cannot be simply dismissed. Yet, I believe that even these brief remarks indicate that they are of a different order than the effect of financing deficits by creating money.<sup>10</sup>

So far as this particular analysis is concerned, the difference between Tobin and myself, if there is one, is partly different empirical assumptions; mostly, whether one considers only the impact effect of a change or the cumulative effect. This same difference arises in another connection and is discussed further in section 4 below on the "first-round effect." At any rate, the main issue between us clearly is not and never has been whether the *LM* curve is vertical or has a positive slope.

Emphasis on the "first-round effect" by the Keynesians and neo-Keynesians contributes, I believe, to their tendency to regard prices and wages as an institutional datum and to neglect the effects of price flexibility. But it may be that, at least for the neo-Keynesians, I was mistaken in regarding their treatment of price flexibility as *the* main issue between them and the monetarists. Perhaps the emphasis on first-round effect is *the* main issue and their treatment of price flexibility a minor corollary.

Tobin objects to my interpretation of the main issue because he "had thought that both monetarists and neo-Keynesians agreed that short-run variations of money income . . . were generally divided between changes in output and changes in price. . . . It is equally a caricature of the neo-Keynesian view to say that  $\rho$  is an 'institutional datum' in the short run. Keynes certainly did not make this assumption. . . . Keynes did not even assume a constant wage rate."

These objections seem to me largely beside the point. I said, "Whatever the first group [the neo-Keynesians] may say in their asides and in their

<sup>10</sup> In terms of real rather than nominal magnitudes, the difference can be interpreted as reflecting the effect of using different tax structures to finance government expenditures.

qualifications, they treat the price level as an institutional datum in their formal theoretical analysis" (Friedman 1970a, p. 211; 1971b, p. 20). I did not intend this statement to mean that neo-Keynesians assert that prices and wages are in fact constant, or even that in their empirical work they do not introduce relations designed to predict the movements of prices and wages. If I gave that impression, Tobin is right to correct it. Treating the price level or the wage level as an institutional datum, or, as Keynes did, as the "numeraire," is not equivalent to asserting that wages or prices are constant. It means rather, that the theory in question has nothing to say about what determines the wage level; that the forces determining the wage level are forces abstracted from in the theory. This is clearly reflected in the fact that the relations neo-Keynesians introduce into their empirical work to predict prices and wages tend to be largely ad hoc (see also point 11 in Appendix 2 below).

It is important to distinguish between the logical implications of a theory and the statements about observable phenomena that a professed adherent of the theory may make. As Keynes says, "We can keep 'at the back of our heads' the necessary reserves and qualifications and the adjustments which we shall have to make later on" (Keynes 1936, p. 297). Of course, both the neo-Keynesians and Keynes himself recognize that, as a factual matter, changes in income are partly in prices and partly in output; and, of course, both have instructive ideas and insights about the factors that determine the division in particular cases. But Keynes's formal theory, as I demonstrate more fully in my reply to Davidson, has nothing to say about what determines the absolute price or wage level, though it does have some implications for the behavior of prices relative to wages. Tobin's statement to the contrary in his footnote 5 is wrong. The elasticity Tobin refers to is purely definitional, and Keynes's discussion of it consists simply of expressing an arithmetical identity in terms of elasticities (see n. 15 in my reply to Davidson). Similarly, Tobin's assertion later on that "Keynes certainly included in his system a relationship between real output and the price level, derived from a theory of labor demand and supply" is correct about the price level in wage units; it is wrong about the price level in money units.

## 2. *Tobin's Interpretation of My Words*

Much of the rest of Tobin's criticism of my articles leaves me utterly baffled. We seem to be talking at cross-purposes. I disagree far less with the substance of what he says than with the views that he attributes to me—which repeatedly seem to me in clear and present conflict with what I have written. And, no doubt, he has the same difficulty with my remarks (see also Tobin 1970; Friedman 1970b).

Let me try to document my bafflement with some specific examples.

*a)* Tobin devotes nearly a third of his comment to analyzing the implications of my “third way” for real income, real saving, and real investment. He treats my model as if it had been developed for that purpose.

Yet I stated that this third way “involves bypassing the breakdown of nominal income between real income and prices and using the quantity theory to derive a theory of nominal income rather than a theory of either prices or real income” (Friedman 1971*a*, p. 323; 1971*b*, p. 34). I discussed briefly the real model Tobin elaborates and concluded that “for both empirical and theoretical reasons, I am inclined to reject this way of marrying the real and the nominal variables and to regard the saving-investment sector as unfinished business, even on the highly abstract general level of this paper” (Friedman 1971*a*, p. 330; 1971*b*, p. 40).

Is Tobin’s reaction not equivalent to criticizing a proposed cure for the measles because it does not also cure the mumps?

*b)* TOBIN: Friedman “doubts that the real rate should really be regarded as a constant in the short run [for analyzing real investment and saving]. . . . Friedman finds it easy to accept the assumption of his model that the only short-run fluctuations of nominal interest rates relevant to the demand for money are those associated with the inflation premium. This is not consistent with his acknowledgment that real rates relevant for investment and saving decisions vary in the short run.”

FRIEDMAN: “It seems entirely satisfactory to take the anticipated real interest rate . . . as fixed for the demand for money. There, the real interest rate is at best a supporting actor. Inflation and deflation are surely center stage. Suppressing the variations in the real interest rate . . . is unlikely to introduce serious error. The situation is altogether different for saving and investment. Omitting the real interest rate in that process is to leave out Hamlet” (Friedman 1971*a*, p. 330; 1971*b*, p. 40).

*c)* TOBIN: “This relates the procyclical movement of velocity to the procyclical movement of interest rates—superficially, at least, the orthodox Keynesian interpretation which Friedman has so stubbornly resisted for so long.”

FRIEDMAN (in a 1959 article on “The Demand for Money” [reprinted in Friedman 1969] to which Tobin’s “so long” presumably refers): “These results [about the cyclical movements of velocity and interest rates] are of the kind that might be expected if the returns on alternative ways of holding assets were the chief factor other than permanent income affecting desired cash balances. . . . The remaining movement in velocity, though . . . it may well be accounted for by movements in interest rates, is much too small to reflect any very sensitive adjustment of cash balances to interest rates (Friedman 1969, pp. 136, 137).

*d)* FRIEDMAN: “On an analytical level, it [the quantity theory] is an analysis of the factors determining the quantity of money the community wishes to hold; on an empirical level, it is the generalization that changes

in desired real balances (in the demand for money) tend to proceed slowly and gradually or to be the result of events set in train by prior changes in supply, whereas, in contrast, substantial changes in the supply of nominal balances can and frequently do occur independently of any changes in demand" (Friedman 1970a, p. 195; 1971b, p. 3).

Tobin gives six alternative meanings of "the long-run quantity theory." One of them, his number 4, can be regarded as corresponding to the "analytical" level in my statement. None of them corresponds to the "empirical" level (his item 3 on first reading can be read as if it does, but his further explanation makes it clear that he does not interpret it that way).

### 3. *What Explains the Difficulty of Communication?*

One explanation for the failure of communication that long appealed to me is the one I gave in the "Theoretical Framework" article, namely, that our qualification of our statements "by referring to their effect on *nominal* income . . . appeared meaningless to economists who implicitly identified nominal with real magnitudes. Hence they have misunderstood our conclusions" (Friedman 1970a, p. 216; 1971b, p. 27). But that explanation must clearly be rejected since it can hardly apply to Tobin's present comment.

The alternative that now appeals to me is that the difficulty is a different approach to the use of economic theory—the difference between what I termed a Marshallian approach and a Walrasian approach in an article I wrote many years ago (Friedman 1949, reprinted in Friedman 1953). From a Marshallian approach, theory is, in Marshall's words, "an engine for the discovery of concrete truth." In this view, "Economic theory . . . has two intermingled roles: to provide 'systematic and organized methods of reasoning' about economic problems; to provide a body of substantive hypotheses, based on factual evidence, about the 'manner of action of causes.' In both roles the test of the theory is its value in explaining facts, in predicting the consequences of changes in the economic environment. Abstractness, generality, mathematical elegance—these are all secondary, themselves to be judged by the test of application" (Friedman 1953, pp. 90–91). On this view, there is no such thing as "the" theory, there are theories for different problems or purposes; there is nothing inconsistent or wrong about using a theory that treats the real interest rate as constant in analyzing fluctuations in nominal income but using a theory that treats the real interest rate as variable in analyzing fluctuations in real income; the one theory may be more useful for the one purpose, the other theory for the other. We lose generality by this procedure but gain simplicity and precision.

From a Walrasian approach, "abstractness, generality, and mathematical elegance have in some measure become ends in themselves, criteria by which

to judge economic theory. Facts are to be described, not explained. Theory is to be tested by the accuracy of its ‘assumptions’ as photographic descriptions of reality, not by the correctness of the predictions that can be derived from it” (Friedman 1953, p. 91). If the real interest rate enters one part of the model it must be used in all, hence it is logically inconsistent and presumably invalid to regard it as constant for one purpose but as variable for another.

The economic principle of equating marginal costs in all directions in order to achieve minimum cost for given output applies to the use of theory just as much as to other productive activities. Generality reduces cost in one direction, specificity in another. Just where the right margin comes is a matter of judgment about which scholars may differ. Presumably, we all tend to develop our own methodological style or bias. The items in section 2 labeled *a* and *b*, are consistent with the hypothesis that Tobin’s style goes farther in Walras’s direction than mine does and that this difference in methodological style is an important reason why we seem to talk at cross-purposes.

#### 4. *First-Round Effects*

Further indirect evidence in support of this hypothesis is provided by Tobin’s discussion of whether “the genesis of new money makes a difference” in the effect of the new money on the economy, whether “an increase in the quantity of money has the same effect whether it is issued to purchase goods or to purchase bonds.” The basic issue is ancient—whether the “first-round effect” of a change in the quantity of money largely determines the ultimate effect. As John Stuart Mill put a view very much like Tobin’s in 1844, “The issues of a *Government* paper, even when not permanent, will raise prices; because Governments usually issue their paper in purchases for consumption. If issued to pay off a portion of the national debt, we believe they would have no effect” (Mill 1844, p. 589).

Tobin’s concentration on the first-round effect also parallels the emphasis by von Mises in his theory of the cycle. For example, Lionel Robbins, in his Misean analysis of the Great Depression, says, “In normal times, expansion and contraction of the money supply comes, not *via* the printing press and government decree, but *via* an expansion of credit through the banks. . . . This involves a mode of diffusion of new money radically different from the case we have just examined—a mode of diffusion which may have important effects” (Robbins 1934, pp. 35–36).

Of course, Tobin is right that the way the quantity of money is increased will affect the outcome in some measure or other. If one group of individuals receives the money on the first round they will likely use it for different purposes than another group of individuals. If the newly printed money is spent on the first round for goods and services it adds directly at that point

to the demand for such goods and services, whereas if it is spent on purchasing debt it has no such immediate effect on the demand for goods and services. Effects on the demand for goods and services come later as the initial recipients of the "new" money themselves dispose of it. Clearly, also, as the "new" money spreads through the economy, any first-round effects will tend to be dissipated. The "new" money will be merged with the old and will be distributed in much the same way.

One way to characterize the Keynesian approach is that it gives almost exclusive importance to the first-round effect. This leads it to attach importance primarily to flows of spending rather than to stocks of assets. Similarly, one way to characterize the quantity-theory approach is to say that it gives almost no importance to first-round effects.

The empirical question is how important the first-round effects are compared to the ultimate effects. Theory cannot answer that question. The answer depends on how different are the reactions of the recipients of cash via different routes to larger cash balances, on how rapidly the larger money stock is distributed through the economy, how long it stays at each point in the economy, on how much the demand for money depends on the structure of government liabilities, and so on. Casual empiricism yields no decisive answer. Tobin can say, "The monetization of commercial loans . . . seems to me to be alchemy of much deeper significance than semi-monetization of Treasury bills" (Tobin 1965, p. 467). But I could answer, "True, but remember that the transactions velocity of money may well be over twenty-five to thirty times a year, to judge from the turnover of bank deposits, so the first round covers at most a two-week period, whereas the money continues circulating indefinitely." Maybe the first-round effect is so strong that it dominates later effects; but maybe it is highly transitory. We shall have to examine empirical evidence systematically to find out.

The issue looks different to Tobin. "The crucial issue," he writes, "is whether government interest-bearing time debt is of *any* significance" (my italics). If that is the crucial issue, then of course he is right. Government interest-bearing time debt is of some significance. But although he phrases the issue that way, on reflection Tobin will undoubtedly agree that the crucial issue is not whether government interest-bearing time debt is of any significance but whether it is of enough significance to introduce significant error into a relation between money and income—or, put differently, whether knowledge of the sources of the change in money permits an economically and statistically significant improvement in predictions of the future course of income.

Despite his repeated assertions that the effect is significant in this sense, Tobin has not, so far as I know, presented any systematic empirical evidence in support of that assertion. It has long seemed to me that the apparently similar response of income to changes in the quantity of money over a long span of time in different countries and under different monetary

systems established something of a presumption that the first-round effect was not highly significant. More recently, several empirical studies designed explicitly to test the importance of the first-round effect have supported this presumption.<sup>11</sup>

Perhaps other studies will reverse this tentative conclusion. In any event, the importance of the first-round effect will be provided by empirical evidence, not by argumentation or theory.

## Davidson

Paul Davidson reproaches me for having neglected “several important chapters in Keynes’s *General Theory*”—namely, chapters 12, 17, 20, and 21. Having reread those chapters, and Davidson’s comments, I am unrepentant.

The four chapters Davidson refers to contain many correct, interesting, and valuable ideas, although also some wrong ones, and many shrewd observations on empirical matters, particularly the operation of financial markets. But all four chapters are strictly peripheral to the main contribution of the *General Theory*. They contain a sequence of organized but uncoordinated comments on their several subjects, and none makes any contribution to the formal theory of the book. Far from being in any way inconsistent with my interpretation of Keynes’s views, they reinforce that interpretation, particularly with respect to the key role that Keynes assigned to the liquidity trap.

Let me document this judgment by considering Davidson’s own six-item summary of his critique “from the Keynesian view”—the first three referring to “basic factors which Friedman omits,” the fourth to a “mis-specification”; the fifth and sixth to “two of Friedman’s assertions about Keynes’s model [that] are incompatible with the *General Theory*.”

The three basic factors I am said to omit are:

1. “The essence of uncertainty.”

This is simply wrong. Uncertainty may not be explicitly mentioned, but it is certainly taken for granted throughout the analysis. Indeed, the assumption that there exists a demand for a finite amount of real-cash balances itself implies the existence of uncertainty.<sup>12</sup> In exactly the same

<sup>11</sup> Cagan investigated the first-round effect on interest rates. He was able to identify the existence of such an effect, but it was of minor quantitative importance. Auerbach found no evidence of a first-round effect on nominal income of the division of the change in the quantity of money between high-powered money and bank credit, or the division of high-powered money between financing current government expenses and debt redemption. Bordo, in a thesis underway dealing with the pre-World War I period for the United States, finds at best very limited traces of the first-round effect (see Auerbach 1969; Cagan 1972; Bordo, in preparation). I am indebted to Anna Schwartz for calling to my attention the 1844 quotation from John Stuart Mill with which I began this section.

<sup>12</sup> Compare my statement, “It is worth noting that both reasons [for holding

sense I could be said to omit "the existence of men who calculate," since I nowhere state explicitly that the analysis is for a society of such men (and women). Surely, a technical article for technical economists can take some things for granted.

Davidson justifies his assertion that "in Friedman's framework, . . . all expectations are realized and therefore there is no uncertainty" by referring to my statement that "at a long-run equilibrium position, all anticipations are realized." But this is a definition of long-run equilibrium, not an assumption about the real world. In any event, my papers were largely devoted to the problem of short-run adjustment. I introduced the concept of "long-run equilibrium" solely as a preliminary step in sketching a theory of the short-run "adjustment process." This is a straw man if I ever saw one (see also point 2 below).

Keynes emphasizes "uncertainty" in his chapter 12 primarily to stress the importance of expectations about the future for the marginal efficiency of capital. This is the point also of his remark in chapter 21, to which Davidson refers, that "the importance of money essentially flows from its being a link between the present and the future" (Keynes 1936, p. 293). I regard as the major new items in my two papers the "third approach" of a "monetary theory of nominal income" in the second paper, and the section on the "adjustment process" in the first. The rest is mostly a retelling of a well-known story. Both new items give a key role to expectations about the future movements of prices, output, money, and income, and to the consequences of deviations between actual magnitudes and anticipated magnitudes. Thus they extend the role of anticipations from the market for investment and loans, with which Keynes dealt, to a broader range of markets.

In a respect that Davidson regards as distinctively Keynesian, therefore, my papers are more Keynesian than the *General Theory*!

Yet, as noted in my reply to Brunner and Meltzer, these extensions owe much more to recent work that has been done on information and search, and to my own work on the consumption function, than they do to Keynes.

2. "The existence of particular market institutions, organizations, and constraints . . . which exist only because uncertainty is present."

The gravamen of Davidson's complaint under this heading is an alleged incompatibility of "the Walrasian auctioneer and flexible money-wages and prices" with uncertainty and a stable monetary system. "Thus," he concludes, "these institutions cannot be used to close logically the Keynesian system."

A methodological answer to this complaint is given by Keynes: "After we have reached a provisional conclusion by isolating the complicating factors one by one, we then have to go back on ourselves and allow, as well

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money] depend critically . . . on the existence of individual uncertainty" (Friedman 1969, p. 3).

as we can, for the probable interactions of the factors amongst themselves. This is the nature of economic thinking" (Keynes 1936, p. 297). The long-run equilibrium in which, as I put it, "all anticipations are realized" and that is determined by "the earlier quantity theory plus the Walrasian equations of general equilibrium" is not a state that is assumed ever to be attained in practice. It is a logical construct that defines the norm or trend from which the actual world is always deviating but to which it is tending to return or about which it tends to fluctuate. The hypothesis that the logical construct does specify the norm or trend in this sense is entirely compatible with the existence of uncertainty, just as the hypothesis that  $s = \frac{1}{2} gt^2$  specifies the law of falling bodies is entirely compatible with the existence of air. This does not of course mean that the hypothesis is correct. That is a question of fact to be determined by the consistency of the hypothesis with experience. But the hypothesis cannot validly be rejected on the purely a priori grounds on which Davidson rejects it.<sup>13</sup>

The substantive fallacy that underlies Davidson's belief in the incompatibility of flexible wages and prices with "uncertainty and a stable monetary system" is the confusion between flexibility and instability that has done so much to impede understanding of the desirability of flexible exchange rates. A price may be flexible, in the sense that it can and does change promptly in response to changes in demand and supply and that there are no institutional obstacles to its changing, yet be relatively stable, because demand and supply are relatively stable over time (e.g., this was the case with the exchange rate of the Canadian dollar in the 1950s). Violent instability of prices in terms of a specific money would greatly reduce the usefulness of that money; however, flexibility of prices in terms of that money has no such effect.

Keynes does not make Davidson's mistake. Whereas Davidson says flatly, "A 'perfect' labor market with flexible prices would be incompatible with a system where there are contracts, and money is used as a store of value," Keynes says that "the fact that contracts are fixed, and wages are usually somewhat stable, in terms of money unquestionably plays a large part in attracting to money so high a liquidity premium," and he goes on to refer to "the relative stability . . . in the future money-cost of output." He then argues that flexible wages and prices would in practice be highly unstable because, to simplify and summarize, the marginal efficiency schedule is highly unstable and the liquidity preference schedule is highly elastic

<sup>13</sup> Of course, as Davidson notes, there may be and are "false trades" and "disappointments" "in the real world," just as there may be and is air pressure. But the relevant issue is not whether there are false trades and disappointments but whether such false trades and disappointments are "sufficiently" important to change "significantly" the trend about which the observed magnitudes fluctuate, or instead tend to be of the nature of random disturbances that largely average out over time. The answer need not, of course, be the same for all problems or under all circumstances. Unfortunately, armchair reasoning cannot settle such issues.

so that it will take wide fluctuations in money-wages and prices "to establish a relation between the rate of interest and the marginal efficiency of capital that would maintain investment at the critical level" (Keynes 1936, pp. 236, 237, 370). Keynes does not treat instability in wages and prices as a logical implication of flexibility and uncertainty *per se*; he presents it as an empirical hypothesis that may be, as I believe it is, false. Its plausibility even to Keynes rested on the omission of adjustment mechanisms from his theoretical system that were added later (notably, the Keynes and Pigou real wealth effects, and the role of permanent income in the consumption function).

Keynes's recognition of the logical consistency of flexibility, uncertainty, and the use of money is demonstrated by his continued belief in the desirability of flexible exchange rates (Keynes 1936, p. 270).

3. The third basic factor I am said to omit is "the existence of money which, in an uncertain world, has a dual function—namely, a medium of exchange and a store of value."

Clearly I did not omit this—indeed, one of Patinkin's main criticisms of my article is that I treated the asset role of money as consistent with the quantity theory, whereas, in his view, it is Keynesian.

Davidson's real criticism is that I did not recognize that "money has two essential properties, namely (a) a zero (or negligible) elasticity of production and (b) a zero (or negligible) elasticity of substitution" and that "the existence of a money possessing these properties underlies Keynes's basic propositions that (a) as a purely theoretical matter if there is uncertainty there need not exist, in a monetary, production economy, a long-run equilibrium position characterized by full employment of labor; (b) stickiness of the money-wage rate is necessary if money is to play its peculiar role in such an economy; and (c) if wages and prices are flexible, 'the quantity of money is, indeed, nugatory in the long period.'"<sup>14</sup>

<sup>14</sup> In a footnote, Davidson says, "Friedman 'summarily' dismisses propositions *a* and *b* as already having been proved false while ignoring proposition *c* altogether."

I am puzzled by this remark in view of the references I gave to the extensive and excellent literature dealing with propositions *a* and *b* (Friedman 1970a, pp. 206, 207, 215; 1971b, pp. 15, 16, 25). I did not introduce Davidson's proposition *c* in my article, so of course I ignored it. But in any event, it is not an independent proposition. It is simply a statement of the converse of proposition *a*.

In discussing the effect of money-wage reductions, Keynes said that they would increase employment directly "only if the community's marginal propensity to consume is equal to unity" (Keynes 1936, p. 261). In effect, the proof of the falsity of proposition *a* that I referred to can be regarded as a demonstration that if necessary (that is, if Keynes's mechanisms via liquidity preference, the rate of interest, and investment are not sufficient) this condition can always be satisfied. If "equilibrium" is regarded as described by a fixed price level and a fixed nominal quantity of money, then the Pigou effect means that there will always exist a price level low enough so that wealth is high enough so that the marginal propensity to consume is unity.

I have long regarded an alternative way to assure the same result as a more elegant answer on a purely theoretical level, though of not much greater practical significance than the real balance effect. The alternative involves widening our perspective to ac-

Let us take a deep breath and see if we can sort out this jumble of ideas.

In the first place, Keynes himself never says that the two properties Davidson refers to are essential properties of an asset money. He says that they "commonly characterize money as we know it" and that they are essential to attribute "a peculiar significance to the money-rate of interest," the peculiar significance in turn being required to justify the possibility that there will not exist a long-run equilibrium position characterized by full employment—a very different proposition from Davidson's (Keynes 1936, pp. 229–30).

In the second place, neither alleged property of money is essential for an asset money and neither has in fact characterized actual moneys. With respect to a zero elasticity of production, over most of the world's history, money has consisted of a commodity that was capable of being produced by the exertion of labor, often at roughly constant costs (the cowrie shells so widely used as money in Asia and Africa and the wampum of the American Indians are the most obvious primitive examples, metallic moneys the most obvious modern examples). Keynes himself recognized this with respect to a gold standard but dismissed the possibility except for "a country of which gold-mining is the major industry" (Keynes 1936, p. 230). But surely this is wrong. In a gold-standard world, a country that does not have literal gold mines has the economic equivalent in industries capable of producing goods for export, and these are likely to account for a larger fraction of employment than literal gold mines.

With respect to a zero elasticity of substitution, Keynes became enmeshed in a confusion between two kinds of prices—the purchase price of an asset (its capital value) and the price of the service flow yielded by the asset (its rental value)—that has more recently been a key element in the dispute over the contentions of Pesek and Saving about inside money and outside money (see Pesek and Saving 1967, p. 118; Johnson 1969, p. 37; Friedman and Schwartz 1970, pp. 114–16).

This confusion is brought out by Keynes's comparison of money with other "pure rent factors, the production of which is completely inelastic," and so in this respect share the first property he attributes to money. He differentiates the two by the alleged zero elasticity of substitution of money, saying that "as the exchange value of money rises there is no motive or

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cept as equilibrium postions states of the world in which prices are falling or rising at a fixed rate. In that case, for a fixed quantity of money, falling prices make income as viewed by the consumer (say  $Y_1$ ) higher than income defined by the value of productive resources (say  $Y_2$ ), because  $Y_1$  includes not only  $Y_2$  but also the capital gain from the increasing real value of cash balances. Consumers can then add to their wealth a fraction of  $Y_1$  equal to the capital gain on cash balances, yet spend on consumption the whole of  $Y_2$ . Hence there will always exist a rate of price fall which will enable  $Y_1$  to exceed  $Y_2$  at full employment by the difference between the amount that consumers, when fully employed, want to add to their wealth and the amount that the business community wants to add to physical capital when there is full employment.

tendency, as in the case of rent factors, to substitute some other factor for it" (Keynes 1936, p. 231). But this is equally true for rent factors if what changes is their capital value as a result of a change in the interest rate used to discount future rents. If the annual rent charged for the services of a piece of land remains the same relative to the annual rent charged for the services of other productive services, there will be no tendency to substitute other factor *services* for land *services*, even though the price of an acre of land goes up sharply. On the other hand, if the annual rental value of a piece of land rises relative to the rental value of other productive sources, there will be an incentive to substitute other services for land services, even though the price of an acre of land does not change. Similarly, if the annual cost of holding money increases, because for example of a more rapid rate of inflation, there will be a tendency to substitute the service of other factors for money, even at the same price level. Money and "pure rent factors" are on all fours.

With respect to the elasticity of substitution in response to a change in rental value rather than in response to a change in capital value, it is Keynes who assumes a high elasticity (infinite when liquidity preference is absolute) and the quantity theorists who are (incorrectly) charged with assuming a zero elasticity of substitution.

In the third place, Keynes recognizes, as Davidson does not, that these two properties are not sufficient for his proposition about long-run equilibrium. The two properties are stated initially in terms of the nominal quantity of money, so Keynes goes on to ask whether the conclusion that these two properties prevent falling prices from adding to employment is "upset by the fact that" falling wages and prices will add to the real stock of money. He admits the theoretical possibility but argues that the possibility is not likely to be realized for several reasons, of which "the most fundamental consideration" is the high elasticity of the liquidity preference function—the liquidity trap (Keynes 1936, pp. 231–33).

In my interpretation of Keynes, I put great emphasis on highly elastic liquidity preference, calling this his "special twist" and "a key element" in his proposition about long-run equilibrium. Davidson does not refer to "absolute liquidity preference," yet two of the three statements of Keynes to which he gives page references when he first introduces this proposition about long-run equilibrium assign a key role to absolute liquidity preference. One quotation is particularly pertinent: If wages are flexible, "there will . . . be only two possible long-period positions—full employment and the level of employment corresponding to the rate of interest at which liquidity preference becomes absolute (in the event of this being less than full employment)" (Keynes 1936, p. 191). The impression given by this quotation is confirmed by all the other statements I have been able to find in the *General Theory* bearing on the issue, and assembled in my Appendix 1 below.

I am not sure I have understood Davidson's own position (as distinct from his interpretation of Keynes), but if I have it seems to stand theory on its head. He appears to *start* from the proposition that there does not exist a long-run equilibrium position characterized by full employment, and then try to *deduce* the empirical characteristics of money (and other elements of the economy) from that proposition. But perhaps I have misunderstood him.

4. Davidson says that I misspecify "the demand function for money and the supply processes for altering the money supply" because my omission of "*planned* expenditures as a specific independent variable can lead to wrong theoretical constructs and policy implications" and because I do not treat "the supply of money and debt and production offer contracts" as "intimately and inevitably related."

I, like every other scientist, readily grant, even know, that any simplification of whatever character "can lead to wrong theoretical constructs and policy implications." But Davidson makes no case for attaching particular significance to the simplification in the demand function that he singles out.

On the supply function, Davidson confuses money with credit. There is no necessary connection between a change in the quantity of money and in the volume of outstanding debts—as is crystal clear when the change in the quantity of money reflects either gold discoveries or the printing of fiat money by government to pay for current expenses.

5 and 6. The two assertions of mine that Davidson regards as incompatible with the *General Theory* are "5. Keynes assumed that before full employment all adjustments to changes in demand take place via quantity changes," and "6. The price analysis underlying Keynes's model is 'arbitrary' and has 'no underpinning in economic theory'" (the words in single quotes are from my articles).

Re 5, Keynes of course recognized that increases in income were in practice divided between price and quantity changes and stated explicitly that the "wage-unit," that is, money wages, "may tend to rise before full employment." But, as he said, while these phenomena have "a good deal of historical importance, . . . they do not readily lend themselves to theoretical generalizations" (Keynes 1936, p. 302). He drew a sharp distinction between such changes in money wages and "a condition which might be appropriately designated as one of true inflation" which occurs "when a further increase in the quantity of effective demand produces no further increase in output" (Keynes 1936, p. 303). He did not incorporate any changes in the wage unit in his formal theoretical structure but simply added, as a verbal qualification, recognition of this possibility and some discussion of how it might occur. This discussion is all reasonable and sensible, but I believe that I have done no violence to Keynes's theory by neglecting these qualifications in discussing his central structure. Of course, the use of a wage unit to express all quantities does not require that money

wages be regarded as in fact constant or rigid. It simply means that changes in money wages are exogenous to the theoretical system and are to be explained by other factors.

Re Davidson's point 6, Keynes's discussion of the price level *relative* to the wage level does have a theoretical underpinning in the law of diminishing returns, and this does play a critical role in his theory, leading him to regard a reduction in real wages as a necessary condition for a rise in employment. This aspect of his theory has stimulated a great deal of empirical work on the behavior of real wages during the business cycle, which has failed to confirm his hypothesis. There is no clear negative relation between real wages and the level of employment over the course of the cycle of the kind that Keynes postulates. The kind of framework that I use, which introduces a distinction between real wages as measured *ex post* and real wages as viewed *ex ante* by employer and employee, can explain this result without contradicting the basic notion of diminishing returns. During an expansion, when the actual rate of price rise exceeds the anticipated, it is possible for real wages as viewed by employers to fall (because they deflate wages by the price of the product they produce) and thus induce them to increase employment, and for real wages as viewed by employees to rise (because they deflate wages by cost of living) and thus induce them to increase the amount of labor offered. This result can be regarded as implied by information and search theory. The actual result *ex post* can be either a rise or a fall in measured real wages.

But the behavior of prices relative to wages is a very different question from the behavior of the absolute price or wage level, and it is one that I abstracted from entirely in my two articles. The relevant issue is whether Keynes had a theory of the absolute price level that was not "arbitrary." Davidson says, "Keynes skillfully constructed the elasticities of output, wages, and prices from traditional price theory concepts. Empirical estimates of these elasticities would go a long way toward answering some of the questions on the adjustment process which Friedman raises in the concluding section of his paper. Unfortunately, such elasticity concepts are not specified at all in Friedman's framework."

This is nonsense pure and simple. The elasticities are simply definitions; the formula connecting them that Davidson cites is a truism derived from the identities that the price in money is equal to the price in wage units times the money-wage rate and that the price in wage units is equal to total demand in wage units divided by output. The formula is a bit complex only because two of the three elasticities it contains are defined with respect to the change in aggregate demand in money and one with respect to the change in aggregate demand in wage units.<sup>15</sup> These elasticities and

<sup>15</sup> Keynes defines:  $p_w$  = price of goods in wage units,  $O$  = output,  $W$  = wage rate in money.

this truism are “specified” as much—whatever that may mean—in my framework as they are in Keynes’s. They are pure arithmetic and are completely empty both theoretically and empirically. To regard them as a “theoretical underpinning” for Keynes’s assumptions about the price level is on a par with regarding  $(a + b)^2 = a^2 + 2ab + b^2$  as theoretical underpinning for the law of falling bodies.

I thus remain persuaded that Keynes’s assumption about the absolute price or wage level is “arbitrary” and has no “theoretical underpinning,” however useful, or mischievous, that assumption may be.<sup>16</sup>

$$D_w = p_w O = \text{aggregate demand in wage units.} \quad (1)$$

$$p = p_w W = \text{price of goods in money.} \quad (2)$$

$$D = D_w W = \text{aggregate demand in money,} \quad (3)$$

$$e_p = d \log p / d \log D = \text{elasticity of price with respect to } D,$$

$$e_o = d \log O / d \log D_w = \text{elasticity of output with respect to } D_w,$$

$$e_w = d \log W / d \log D = \text{elasticity of wages with respect to } D.$$

From (1) and (2),

$$p = \frac{D_w W}{O}. \quad (4)$$

Taking logs, we have

$$\log p = \log D_w - \log O + \log W. \quad (5)$$

Differentiate with respect to  $\log D$  and replace relevant symbols with elasticities:

$$e_p = \frac{d \log D_w}{d \log D} (1 - e_o) + e_w. \quad (6)$$

But, from (3),

$$\log D_w = \log D - \log W, \text{ or} \quad (7)$$

$$\frac{d \log D_w}{d \log D} = 1 - e_w. \quad (8)$$

Substituting in (6), we have

$$e_p = (1 - e_w)(1 - e_o) + e_w, \quad (9)$$

or

$$e_p = 1 - e_o(1 - e_w), \quad (10)$$

which is Keynes’s formula (Keynes 1936, p. 285).

<sup>16</sup> In the epilogue which Davidson added after reading the first draft of my reply, he asks a series of “when did you beat your wife” questions. For the record, let me answer them succinctly.

a) My general framework does not assume an exogenous money supply in any relevant sense (see Friedman 1970a; 1971b, section 3). One simplified model, used for a special purpose, takes money supply to be exogenous. I have done work on the factors determining the money supply and have encouraged much work by others on this subject.

b) I have answered this question in my response to Davidson’s point 2 and in n. 13.

c) Keynes, as I have explained, has no theory of the absolute level of prices.

d) Talk is not a substitute for evidence. I know no empirical study of the demand for money that has ever identified variables corresponding to “the finance motive,” let alone found them to have a significant influence. An attempt to do so would certainly be an appropriate piece of research. In view of Davidson’s strong a priori feelings on this issue, I hope that he will be led to investigate the question systematically.

## Patinkin

Patinkin's (1969) article on "The Chicago Tradition, The Quantity Theory, and Friedman" served as an important source for Harry Johnson's 1970 Richard T. Ely lecture (Johnson 1971). Patinkin—both in his 1969 article and in the present comments—and Johnson criticize me for linking my work to a "Chicago tradition" rather than recognizing that, as they see it, my work is Keynesian. In the course of their criticism, they give a highly misleading impression of the Chicago tradition. Grant for the sake of the argument—as I do not in fact grant—that my 1956 "Restatement" (see "The Quantity Theory of Money—a Restatement," in Friedman 1956; reprinted in Friedman 1969, pp. 51–67), which is the main object of criticism in Patinkin's 1969 article, had nothing whatsoever of the flavor of the Chicago tradition. That would indict my perception, or integrity, or scholarship, but it would in no way contradict the existence of an important Chicago tradition in the field of money that had a great influence on subsequent work in monetary economics and on my own work in particular. Similarly, it might justify Johnson's charge that I engaged in "scholarly chicanery," but it would not justify his charge that the "University of Chicago oral tradition" was my "invention."

Whether I conveyed the flavor of that tradition or not, there was such a tradition; it was significantly different from the quantity theory tradition that prevailed at other institutions of learning, notably the London School of Economics; that tradition had a great deal to do with the differential impact of Keynes's *General Theory* on economists at Chicago and elsewhere; and it was responsible for the maintenance of interest in the quantity theory at Chicago. My restatement is a restatement of the quantity theory and is not Keynesian in any meaningful sense of that term.

One reason Patinkin gives such a misleading view of both the Chicago School tradition and of my theoretical framework is his propensity to take the "quantity theory" to mean one thing and one thing only, namely, the long-run proposition that money is neutral, even though he fully recognizes, indeed insists, that the quantity theorists (myself included) were concerned mostly with short-run fluctuations. My explicit statement of what I mean by the quantity theory (quoted in my reply to Tobin, p. 919) is not referred to by Patinkin; he interprets almost all my references to the quantity theory as if I meant by it what he means by it. For example, he criticizes me for "not presenting the long-run quantity theory in the most general way that one can, once one has decided to reformulate it" and proceeds in the very next paragraph to document this criticism by referring to a model which I said—and he even quotes this statement—"refers to a short period"!<sup>17</sup>

<sup>17</sup> For another striking example, see his discussion of the comment he quotes from my "Theoretical Framework" article (Friedman 1970a, p. 225; Friedman 1971b, p. 51),

A second, and closely related, reason why Patinkin gives a misleading view of both the Chicago School tradition and of my framework is his emphasis on the role of the interest rate in the demand function for money as "one of the central issues of monetary economics." He regards the inclusion of the interest rate in the demand function for money as distinctively Keynesian and its neglect as a key omission in pre-Keynesian monetary theory. The contrast between that alleged omission and my inclusion of the interest rate in the demand function for money is a major ground on which he criticizes my "persistent refusal to recognize" that my "analytical framework is Keynesian." As I show below, the inclusion of the interest rate in the demand for money is not distinctively Keynesian. But, far more important for the present purpose, the inclusion of the interest rate is a minor feature of my framework, and its exclusion would have been a minor feature of pre-Keynesian monetary theory. As I also show below, the inclusion or exclusion of the interest rate in the demand function for money is not the respect in which my work is in the Chicago tradition. That tradition influenced my work primarily with respect to the interpretation of short-run movements, the reasons for the great depression, and the role of monetary and fiscal policies.

In arguing that inclusion of the interest rate in the demand for money is "Keynesian," Patinkin implicitly takes everything in the *General Theory* as *ipso facto* "Keynesian," as we use that term. Keynes was a quantity theorist long before he was a Keynesian, and he continued to be one after he became a Keynesian. Many parts of the *General Theory* are a continuation of his earlier interests and beliefs. The fact that Patinkin can find parallels between some of my discussion of the demand for money and Keynes's discussion in the *General Theory* does not establish that my discussion is therefore Keynesian. I shall argue, rather, that those parts of the *General Theory* are a direct outgrowth of Keynes's earlier quantity-theory views.

A more fundamental reason for Patinkin's emphasis on long-run "neutrality," the interest rate, and the real balance effect, and for his slighting the short-run context of most of my framework is that Patinkin, even more than Tobin, is Walrasian, concerned with abstract completeness, rather than Marshallian, concerned with the construction of special tools for special problems.

In a recent article, "On the Short-Run Non-Neutrality of Money in the Quantity Theory," Patinkin (1972) cites evidence that he regards as decisively contradicting my interpretation that Fisher and quantity theorists "simply took over Marshall's assumption" that "prices adjust more rapidly

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in which he regards as an advantage of his own model that it demonstrates "the validity of the quantity theory," meaning by that simply the long-run neutrality of money, a result that is wholly irrelevant to my quotation in which I used "quantity-theory approach" in my sense, not his.

than quantities." Yet I regard the evidence he cites as strikingly confirming my interpretation. One sample of his evidence will do:

"The sequence of effects visualized by Fisher" after an increase in the quantity of money is "as follows:

- “1. Prices rise.
- “2. Velocities of circulation ( $V$  and  $V'$ ) increase; the rate of interest rises, but not sufficiently.
- “3. Profits increase, loans expand, and the Q's [i.e., the real volume of trade] increase.
- “4. Deposit currency ( $M'$ ) expands relatively to money ( $M$ ).
- “5. Prices continue to rise; that is, phenomenon No. 1 is repeated. Then No. 2 is repeated, and so on.”

I described Marshall's assumption as being "that prices adjust more rapidly than quantities, indeed, so rapidly that the price adjustment can be regarded as instantaneous. An increase in demand (a shift to the right of the long-run demand curve) will produce a new market equilibrium involving a higher price but the same quantity. The higher price will, in the short run, encourage existing producers to produce more with their existing plants, thus raising quantity and bringing prices back down toward their original level, and, in the long run attract new producers and encourage existing producers to expand their plants, still further raising quantities and lowering prices. Throughout the process, it takes time for output to adjust but no time for prices to do so" (Friedman 1970a, pp. 207–8; 1971b, p. 17). Is not Fisher's sequence precisely the counterpart for the aggregate to this analysis for a particular product?

Further proof is that just prior to listing the five steps that Patinkin quotes, Fisher states that "an increase in currency cannot, even temporarily, very greatly increase trade. . . . [A]lmost the entire effect of an increase of deposits must be seen in a change of prices" (Fisher 1911, pp. 62–63).

Consider how a Keynesian would describe the effects of an increase in the quantity of money. It would go:

- 1. Interest rates fall.
- 2. Investment increases.
- 3. Output and real income increase.
- 4. Consumption increases.

It is not clear when he would come to the statement "prices rise," but it would surely be late in his list. Moreover, his step number 1 implies that velocity falls, but he would be most unlikely ever to refer to that phenomenon.

Is this not precisely the contrast that I drew between the quantity theorists and the Keynesians when I said that Keynes "deviated from

Marshall . . . in reversing the roles assigned to price and quantity" (Friedman 1970a, p. 209; 1971b, p. 18)?<sup>18</sup>

I must plead guilty to one careless expression that Patinkin quotes and that does lend itself to the interpretation that I was accusing the quantity theorists of assuming strict neutrality in the short run. This was my statement: "There is nothing in the logic of the quantity theory that specifies the dynamic path of adjustment, nothing that requires the whole adjustment to take place through  $P$  rather than through  $k$  or  $y$ ." In light of my quotation above about Marshall's assumption (which is separated from the prior sentence by only one paragraph), I intended this sentence to refer to the instantaneous market equilibrium and to the assertion that "throughout the process, it takes time for output to adjust but no time for prices to do so"; it is not intended to mean that there are no effects on output during the process. But my wording is ambiguous and I should have been more careful.

Patinkin goes on also to quote my statement, "It was widely recognized that the adjustment during what Fisher, for example, called 'transition periods' would in practice be partly in  $k$  and in  $y$  as well as in  $P$ . Yet this recognition was not incorporated in formal theoretical analysis." He italicizes the last sentence for emphasis and asserts, "The facts of the case, however, are quite different," giving as evidence that "Fisher wrote incomparably more on his monetary proposals for mitigating the cyclical problems of the 'transition period' than on the long-run proportionality of prices to money. This concentration on short-run analysis was even more true for the policy-oriented Chicago quantity-theory school of the 1930s and 1940s."

The issue here is one that we have already encountered in my replies to Tobin and Davidson (and that is discussed also in points 7 and 11 of Appendix 2 below). There can be a great difference between what is implied by or contained in a formal theory, what proponents of the theory may believe it implies or contains, and what they write about. Of course, Fisher, the Chicago monetary economists, and the host of other economists who studied business cycles wrote a great deal about short-run movements and constructed many ingenious theories about business cycles that have much to teach us. In particular, Fisher's distinction between nominal and real interest rates, which dates back to some of his earliest writing, remains a seminal and penetrating insight. Yet, so far as I know, none of this voluminous writing and none of these theories provide a formal theoretical extension of the quantity theory to explain the division of changes in nominal income between changes in prices and in output or of changes in the quantity of money between changes in velocity, in prices, and in

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<sup>18</sup> The references in Patinkin's article to statements by Chicago economists, Pigou, Keynes, Robertson, and Lavington, all equally strike me as clearly confirming my interpretation.

output, just as none of Keynes's extensive discussion of changes in money-wage rates prior to the point of full employment provides a formal theoretical analysis of such changes.

Rather than continuing to examine Patinkin's remarks in detail, I believe it will contribute more to clarify the issues raised by Patinkin if I address myself directly to the two key questions: (1) What was the distinctive quality of the Chicago tradition as it affected my own writings? (2) What are the quantity theory antecedents of Keynes's and my writings on the demand for money? I shall therefore relegate further detailed comments on those of Patinkin's statements that do not bear directly on these two issues to Appendix 2.

### 1. *The Chicago Tradition*

I was myself first strongly impressed with the importance of the Chicago tradition during a debate on Keynes between Abba P. Lerner and myself before a student-faculty seminar at the University of Chicago sometime in the late 1940s (or perhaps early 1950s). Lerner and I were graduate students during the early 1930s, pre-*General Theory*; we have a somewhat similar Talmudic cast of mind and a similar willingness to follow our analysis to its logical conclusion. These have led us to agree on a large number of issues—from flexible exchange rates to the volunteer army. Yet we were affected very differently by the Keynesian Revolution—Lerner becoming an enthusiastic convert and one of the most effective expositors and interpreters of Keynes, I remaining largely unaffected and if anything somewhat hostile.

During the course of the debate, the explanation became crystal clear. Lerner was trained at the London School of Economics, where the dominant view was that the depression was an inevitable result of the prior boom, that it was deepened by the attempts to prevent prices and wages from falling and firms from going bankrupt, that the monetary authorities had brought on the depression by inflationary policies before the crash and had prolonged it by "easy money" policies thereafter; that the only sound policy was to let the depression run its course, bring down money costs, and eliminate weak and unsound firms.

By contrast with this dismal picture, the news seeping out of Cambridge (England) about Keynes's interpretation of the depression and of the right policy to cure it must have come like a flash of light on a dark night. It offered a far less hopeless diagnosis of the disease. More important, it offered a more immediate, less painful, and more effective cure in the form of budget deficits. It is easy to see how a young, vigorous, and generous mind would have been attracted to it.

It was the London School (really Austrian) view that I referred to in my "Restatement" when I spoke of "the atrophied and rigid caricature [of

the quantity theory] 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 the quantity theorists" (Friedman 1969, p. 51).

The intellectual climate at Chicago had been wholly different. My teachers regarded the depression as largely the product of misguided governmental policy—or at least as greatly intensified by such policies. They blamed the monetary and fiscal authorities for permitting banks to fail and the quantity of deposits to decline. Far from preaching the need to let deflation and bankruptcy run their course, they issued repeated pronunciamientos calling for governmental action to stem the deflation—as J. Rennie Davis put it, "Frank H. Knight, Henry Simons, Jacob Viner, and their Chicago colleagues argued throughout the early 1930's for the use of large and continuous deficit budgets to combat the mass unemployment and deflation of the times" (Davis 1968, p. 476).

They recommended also "that the Federal Reserve banks systematically pursue open-market operations with the double aim of facilitating necessary government financing and increasing the liquidity of the banking structure" (Wright 1932, p. 162). There was nothing in these views to repel a student; or to make Keynes attractive. On the contrary, so far as policy was concerned, Keynes had nothing to offer those of us who had sat at the feet of Simons, Mints, Knight, and Viner.

It was this view of the quantity theory that I referred to in my "Restatement" as "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" (Friedman 1969, p. 52).

I do not claim that this more hopeful and "relevant" view was restricted to Chicago. The manifesto from which I have quoted the recommendation for open-market operations was issued at the Harris Foundation lectures held at the University of Chicago in January 1932 and was signed by twelve University of Chicago economists. But there were twelve other signers (including Irving Fisher of Yale, Alvin Hansen of Minnesota, and John H. Williams of Harvard) from nine other institutions.<sup>19</sup> I have done no exhaustive research on the policy views at the time of economists at other institutions. But we do know that the London School view, really the Austrian view of Ludwig von Mises, had many adherents—including Gottfried Haberler, who was at the time a visiting lecturer in economics at Harvard and who gave a talk on "Money and the Business Cycle" at the same Harris Foundation lectures.

<sup>19</sup> However, several of these were Chicagoans at one remove, for example, Charles D. Hardy and Harold G. Moulton of Brookings Institution.

To assure you (and myself) that this is not simply hindsight, let me quote from pre-Keynesian publications about the depression, reflecting the London-Austrian view and the Chicago view.

Lionel Robbins's *The Great Depression* (1934) is an extraordinarily lucid and penetrating analysis of the depression from the Austrian point of view.<sup>20</sup> Published in 1934, large stretches of it make instructive reading today, especially the analysis of the consequences of restrictive foreign and domestic policies. But when it comes to the role of money and to causes and cures of the depression, here is what he has to say:

"If we take deflation to mean a deliberate curtailment of the supply of money, there seems to be no evidence of its existence on a large scale either before or since the slump commenced. . . . Since the slump, Central Banks and Governments have vied with each other in promoting policies calculated to bring about easy money conditions" (p. 17).

"It is clear that the authorities of the Federal Reserve Bank and the Bank of France did nothing to prevent their increased reserves [as a result of gold inflows during the depression] from becoming effective. It is not really sensible, therefore, to attribute what happened after 1929 to their policy" (p. 23).

"A fluctuation of the kind described in the last chapter [the alleged inflation of the 'twenties] is bound to be followed by a period of extensive depression" (p. 55). But it "does not explain why [the slump] has been so severe." Robbins goes on to argue that "no single explanation of this phenomenon will be sufficient" (p. 56), gives a long catalog of special factors intensifying the depression, and turns to "certain tendencies of policy . . . which have greatly enhanced these difficulties" (p. 65). After stressing restrictions on international trade, he turns to wage policy. "If profitability is to be restored, costs must be cut and the capital resources rehabilitated" (p. 69).

"In earlier depressions this has been the rule. . . . But at the outset of this depression other measures were adopted" (p. 69) to keep up wage rates.

"Now this policy was the reverse of what was needed. . . . The maintenance of wage rates and dividends was at the expense of capital. . . . Consumption is maintained at the expense of capital. . . ." (p. 71).

"In the present depression . . . We eschew the sharp purge. We prefer the lingering disease. . . .

"The moment the boom broke in 1929, the Central Banks of the world, acting obviously in concert, set to work to create a condition of easy money, quite out of relation to the general conditions of the money market. . . . The process of liquidation was arrested" (p. 73).

<sup>20</sup> Compare the footnote in the *General Theory*, "It is the distinction of Prof. Robbins that he, almost alone, continues to maintain a consistent scheme of thought, his practical recommendations belonging to the same system as his theory" (Keynes 1936, p. 20).

"It is agreed that to prevent the depression the only effective method is to prevent the boom" (p. 171). There is needed "a greater flexibility of wage rates" (p. 186). "Our main contention [is] the necessity for the elimination of all kinds of inflexibility" (p. 189).

Compare this with what Jacob Viner said in early 1932 in his Harris Foundation lecture:

"The Federal Reserve Board has revealed to the outsider no greater capacity [than other Central Banks] to formulate a consistent policy, unless a program of drift, punctuated at intervals by homeopathic doses of belated inflation and deflation and rationalized by declarations of impotence, can be accepted as the proper constituents of central bank policy. While the New York Federal Reserve Bank has made more effort than any other central banking institution to develop a program and a technique of credit control with a view to stabilization, it has at critical moments found itself at cross-purposes with, and inhibited from action by, a Federal Reserve Board with an attitude toward its functions resembling with almost miraculous closeness that of the Bank of England during its worst period" (Wright 1932, p. 28).

Even more pertinent is a talk Viner delivered in Minneapolis on February 20, 1933, on "Balanced Deflation, Inflation, or More Depression" (Viner 1933). While agreeing with Robbins on the harm done by wage and price rigidity, and in particular by the Hoover Administration pressure against wage reductions, he also spoke vigorously against letting the cure take its course:

"We have already had three years of patient waiting, probably three years too much. It is arguable that even dangerous remedies now threaten less risk of disaster than does continuance of inaction" (p. 10).

"Had it not been for this campaign of fear . . . it would have been sound policy on the part of the federal government deliberately to permit a deficit to accumulate during depression years, to be liquidated during prosperity years. . . . The outstanding though unintentional achievement of the Hoover Administration in counteracting the depression has in fact been its deficits of the last two years, and it was only its own alleged fears as to the ill effects of these deficits, and the panic which the big business world professed to foresee if these deficits should recur, which have made this method of depression finance seriously risky" (pp. 18–19).

"I will use the term inflation to mean an increase in the total amount of spendable funds, whether consisting of coin, paper money, or bank deposits subject to check.

"The basic argument for inflation is that it would operate to raise product-prices more than cost-prices, would in this way restore a profit margin for business, and thus would bring about an increased volume of production and of employment. Against inflation many things are urged . . .

"I can see little force in most of these objections . . ." (p. 20).

"It is often said that the federal government and the Federal Reserve

system have practiced inflation during this depression and that no beneficial effects resulted from it. What in fact happened was that they made mild motions in the direction of inflation, which did not succeed in achieving it, did not succeed even in accomplishing 'reflation'; but which probably did slow up somewhat the rate of price decline . . . [p. 21]. At no time . . . since the beginning of the depression has there been for so long as four months a net increase in the total volume of bank credit outstanding. On the contrary, the government and Federal Reserve bank operations have not nearly sufficed to countervail the contraction of credit on the part of the member and non-member banks. There has been no net inflation of bank credit since the end of 1929. There has been instead a fairly continuous and unprecedently great contraction of credit during this entire period" (p. 22).

"Assuming for the moment that a deliberate policy of inflation should be adopted, the simplest and least objectionable procedure would be for the federal government to increase its expenditures or to decrease its taxes, and to finance the resultant excess of expenditures over tax revenues either by the issue of legal tender greenbacks or by borrowing from the banks" (p. 24).

However, Viner adds, such a policy might be vitiated by "general fear of an early departure from the gold standard and therefore a flight from the dollar." In light of this, "if going off the gold standard were as simple a matter for us as for England and Canada, I would not only advocate it, but if the mere cessation of gold payments did not suffice to lower substantially the internal purchasing power of the dollar I would recommend its accompaniment by increased government expenditures financed by the printing press or by loans" (p. 25).

However, "the actual process of going off the gold standard, while it is under way, is extremely painful, costly, panic-breeding. . . . In this country, it would undoubtedly require weeks, if not months, of public and congressional debate, during which utter confusion would be likely to prevail. . . .

"If we are to have inflation, therefore, we must have it within the gold standard" (p. 27).

Two weeks after Viner spoke these words, the United States was off the gold standard! Another graphic illustration of why economists should not let their amateur judgments of political feasibility overrule their professional judgments of economic desirability.<sup>21</sup>

But this is a digression. My main point is different. What, in the field of interpretation and policy, did Keynes have to offer those of us who learned their economics at a Chicago that was filled with these views? Can anyone who knows my work read Viner's comments and not see the direct links between them and Anna Schwartz's and my *Monetary History*

<sup>21</sup> The other famous example, of course, is Keynes's advocacy of a tariff in 1931 as a second-best solution to Britain's departure from the gold standard, which he ruled out as not feasible politically, only to see it happen very shortly.

(1963), or between them and the empirical *Studies in the Quantity Theory of Money* (1956)? Indeed, as I have read Viner's talk for the purposes of this paper, I have myself been amazed to discover how precisely it foreshadows the main thesis of our *Monetary History* for the depression period, and have been embarrassed that we made no reference to it in our account. Can you find any similar link between Robbins's comments and our work?

I shall not defend my "Restatement" as giving the "flavor of the oral tradition" at Chicago in the sense that the details of my formal structure have precise counterparts in the teachings of Simons and Mints. After all, I am not unwilling to accept some credit for the theoretical analysis in that article. Patinkin has made a real contribution to the history of thought by examining and presenting the detailed theoretical teachings of Simons and Mints, and I have little quarrel with his presentation. But I certainly do defend my "Restatement" as giving the "flavor of the oral tradition" at Chicago in what seems to me the much more important sense in which, as I said, the oral tradition "nurtured the remaining essays in" *Studies in the Quantity Theory of Money*, and my own subsequent work. And, in any event, it is clearly not a tradition that, as Johnson charges, I "invented" for some noble or nefarious purpose.

## 2. *Keynes and the Quantity Theory*

Is everything in the *General Theory* Keynesian? Obviously yes, in the trivial sense that the words were set down on paper by John Maynard Keynes. Obviously no, in the more important sense that the term Keynesian has come to refer to a theory of short-term economic change—or a way of analyzing such change—presented in the *General Theory* and distinctively different from the theory that preceded it. To take a noncontroversial example: in his chapter 20 on "The Employment Function" and elsewhere, Keynes uses the law of diminishing returns to conclude that an increase of employment requires a decline in real-wage rates. Clearly that does not make the "law of diminishing returns" Keynesian or justify describing the "analytical framework" of someone who embodies the law of diminishing returns in his theoretical structure as Keynesian.

In just the same sense, I maintain that Keynes's discussion of the demand curve for money in the *General Theory* is for the most part a continuation of earlier quantity theory approaches, improved and refined but not basically modified. As evidence, I shall cite Keynes's own writings in the *Tract on Monetary Reform* (1923)—long before he became a Keynesian in the present sense.

### a) Absolute Liquidity Preference

There is one respect—and I believe only one—in which the discussion of the demand curve for money in the *General Theory* is distinctively Keynes-

ian and that is the importance attached to "absolute liquidity preference" or a high-interest elasticity of the demand for money. This element is distinctively Keynesian in the double sense that it is, so far as I know, introduced for the first time in the *General Theory* and also, as I stated in the "Theoretical Framework," that it can "be regarded as a direct consequence of his assumption about the relative speed of adjustment of price and quantity" (Friedman 1970a, p. 210; 1971b, p. 20).

Patinkin objects to my treating "the case of 'absolute liquidity preference'—as part of 'Keynes's basic challenge to the reigning theory.'" He cites as counterevidence Keynes's own statement (which I also quoted) that "whilst this limiting case might become practically important in the future" he knew "of no example of it hitherto." However, the right hand apparently knows not what the left does. In the very next paragraph, Patinkin says, "Keynesian economics contends that as a result of high-interest elasticity of the demand for money . . ." Pray tell how that statement differs from mine that Keynes "treated velocity as if in practice its behavior frequently approximated that which would prevail in this limiting case" (Friedman 1970a, p. 215; 1971b, p. 26)?

More important, Patinkin does not quote the sentence immediately following Keynes's disclaimer, to wit, "Indeed, owing to the unwillingness of most monetary authorities to deal boldly in debts of long term, there has not been much opportunity for a test" (Keynes 1936, p. 207).<sup>22</sup> Neither does Patinkin note that, so far as I can discover, this is the only disclaimer in the *General Theory*; while there are repeated statements in the opposite direction, such as, to pick only one, "The most stable, and the least easily shifted element in our contemporary economy has been hitherto, and may prove to be in the future, the minimum rate of interest acceptable to the generality of wealth-owners" (Keynes 1936, p. 309).

One consequence of my rereading large parts of the *General Theory* in the course of writing this reply has been to reinforce my view that absolute liquidity preference plays a key role. Time and again when Keynes must face up to precisely what it is that prevents a full-employment equilibrium, his final line of defense is absolute liquidity preference. To document this point, I have assembled the relevant quotations in Appendix 1. The first quotation is from page 172 because Keynes does not introduce liquidity

<sup>22</sup> As Allan Meltzer points out, this comment was in line with views that had long been held by Keynes.

"Keynes of the *Treatise* and even more Keynes of the *Essays in Persuasion* was scornful of the analysis and policies of bankers, central as well as private. In the *Treatise*, Keynes talked repeatedly and at length about the inadequacies of monetary policy during the interwar period and the limitations imposed by bankers. . . .

"Keynes's views on the subject of monetary policy, central banks, and the liquidity trap were much the same when he wrote the *General Theory*. The main point Keynes makes about the possibility of a liquidity trap and the breakdown of monetary policy is sandwiched between two statements that are critical of central bankers for not acting boldly and for not dealing in long-run debts" (Meltzer 1970, pp. 46–47).

preference or the quantity of money, with only trivial exceptions, until page 166. I do not see how anyone can read through these quotations and come to any other conclusion than that his “special twist” was highly elastic liquidity preference and that this “was a key element in Keynes’s proposition” about the possibility that there might not be a full-employment equilibrium even with flexible prices. Patinkin sees the fly on the barn door but not the door!

### b) Stocks versus Flows and Interest-Rate Effects

Patinkin argues that the liquidity-preference analysis of the *General Theory* differs from the earlier quantity-theory approach in two key respects: first, that it “is concerned with the optimal relationship between the stock of money and the stocks of other assets, whereas the quantity theory . . . was primarily concerned with the direct relationship between the stock of money and the flow of spending on goods and services.”

Second, the two theories treated differently “the influence of the rate of interest on the demand for money. For, though quantity theorists did frequently recognize this influence, they did not fully integrate it into their thinking.”

With respect to the first point, Patinkin does not correctly describe the analysis in the *General Theory*. The liquidity function includes both  $M_1$ , which is treated as strictly a function of “the flow of spending on goods and services,” as well as  $M_2$ , which is treated as related to the stock of other assets.

In his *Tract on Monetary Reform* (1923), Keynes discusses the amount of cash people wish to hold, saying that it “depends partly on the wealth of the community, partly on its habits. Its habits are fixed by its estimates of the extra convenience of having more cash in hand as compared with the advantages to be got from spending the cash or investing it. . . . The matter cannot be summed up better than in the words of Dr. Marshall.” There follows a lengthy quotation from Alfred Marshall’s *Money, Credit, and Commerce*, including: “Let us suppose that the inhabitants of a country . . . find it just worth their while to keep by them on the average ready purchasing power to the extent of a tenth part of their annual income, together with a fiftieth part of their property” (Keynes 1923, pp. 85–86).

This is precisely the liquidity preference function of the *General Theory* (p. 199):

$$M = M_1 + M_2 = L_1(Y) + L_2(r),$$

except that Marshall expressed the  $M_2$  part as a fraction of wealth, whereas Keynes expresses it as a function of the interest rate. So Patinkin’s first point reduces to his second.

With respect to the second, Patinkin states that the “rate of change of

the price level as one of the alternative rates of return which affect the demand for money . . . was not systematically integrated into our thinking until the work of Friedman and his associates, particularly Cagan (1956)." Patinkin is clearly wrong, at least if "our" includes the Keynes of *Monetary Reform*. Keynes has an excellent and explicit discussion of inflation as a tax and of the effect of the tax on the quantity of real balances demanded. "The public," he writes, "discover that it is the holders of notes who suffer taxation . . . and they begin to change their habits and to economize in their holding of notes" (Keynes 1923, p. 51).

Keynes also uses Germany as "an illustration of the extraordinary degree in which the money rate of interest can rise in its endeavor to keep up with the real rate, when prices have continued to rise for so long and with such volume that, rightly or wrongly, everyone believes that they will continue to rise further" (Keynes 1923, p. 26). The remark already quoted about "the advantages to be got from . . . investing" cash is further evidence that he recognized the role of the interest rate. True, he discussed the role of the rate of inflation explicitly, and of the interest rate only implicitly, but that was because his book was so largely devoted to an analysis of post-World War I inflations and their implications for exchange rates. True, also, he did not write down an explicit demand function with the rate of interest as an argument, but neither did he write one down with the anticipated rate of inflation as an argument.

Patinkin is correct when he says "quantity theorists paid little, if any, attention to the effects on the rate of interest . . . of shifts in the tastes of individuals as to the form in which they wish to hold their assets." This indeed was a Keynesian development that reflected what I regard as Keynes's critical assumption that prices were an institutional datum. The quantity theorists (including Keynes in *Monetary Reform*) found it natural to regard changes in the quantity of money as affecting prices in the first instance, and to regard the interest rate as determined by saving and investment or lending and borrowing. Monetary changes affected the interest rate by producing inflation (or deflation), which shifted the saving and investment functions by leading lenders to demand higher (or lower) nominal rates and borrowers to be willing to pay higher (or lower) nominal rates. Because the Keynesians take the price level as an institutional datum, they regard a change in the interest rate as the means whereby people are induced to hold a larger or smaller quantity of money. Hence the Keynesians were led to place greater importance than the quantity theorists on the role of changes in the interest rate in the economy's adjustment to monetary change.

But this valid point is a final, and decisive, piece of evidence against Patinkin's claim that my "analytical framework is Keynesian." For in this respect the treatment in my "Restatement" and in my "Theoretical Framework" (except where I am discussing the Keynesian theory) is the quantity-

theory treatment. I too pay no attention to “the effects on the rate of interest” of shifts in the demand function for money. I too tend to minimize changes in market interest rates as the primary channel through which changes in the quantity of money affect spending, output, and prices. To go further, in the “Restatement” I do not even consider the effect of changes in the quantity of money on interest rates. In the “Theoretical Framework” I do, but only (in the passage I added to the NBER version that is quoted in my reply to Brunner and Meltzer) to show how the quantity theory and income-expenditure approaches “can be readily reconciled on a formal level” (Friedman 1971b, p. 28).

I conjecture that Patinkin’s insistence on labeling my analytical framework Keynesian ultimately reflects his concentration on “neutrality.” For if he interprets my framework as in the “quantity-theory” tradition, he cannot continue to regard “the quantity theory” as synonymous with the long-run neutrality of money, since my framework is clearly and obviously not about that—just as I believe the writings of earlier quantity theorists, from Ricardo and Thornton to Keynes, were not about that either. So cut down the forest to let the “neutrality tree” stand proud and tall.

## Appendix 1

### Quotations from *General Theory* Related to Absolute Liquidity Preference

1. “Circumstances can develop in which even a large increase in the quantity of money may exert a comparatively small influence on the rate of interest” (p. 172).

2. “In the extreme case where money-wages are assumed to fall without limit in face of involuntary unemployment through a futile competition for employment between the unemployed labourers, there will, it is true, be only two possible long-period positions—full employment and the level of employment corresponding to the rate of interest at which liquidity-preference becomes absolute (in the event of this being less than full employment)” (p. 191).

3. “This, indeed, is perhaps the chief obstacle to a fall in the rate of interest to a very low level” (p. 202).

4. “The long term rate may be more recalcitrant when once it has fallen to a level which . . . is considered ‘unsafe’ by representative opinion. . . . [A] domestic rate of interest dragged up to a parity with the *highest* rate . . . prevailing in any country belonging to the international system may be much higher than is consistent with domestic full employment. . . .

“ $M_2$  may tend to increase almost without limit in response to a reduction of  $r$  below a certain figure. . . .

“Any level of interest which is accepted with sufficient conviction as *likely* to be durable will be durable. . . .

“[I]t may fluctuate for decades about a level which is chronically too high for full employment . . .

“The difficulties in the way of maintaining effective demand at a level high enough to provide full employment, which ensue from the association of a conventional and fairly stable long-term rate of interest with a fickle and highly

unstable marginal efficiency of capital, should be, by now, obvious to the reader" (pp. 203-4).

5. "There is the possibility, for the reasons discussed above, that, after the rate of interest has fallen to a certain level, liquidity-preference may become virtually absolute. . . . In this event the monetary authority would have lost effective control over the rate of interest. But whilst this limiting case might become practically important in future, I know of no example of it hitherto. Indeed, owing to the unwillingness of most monetary authorities to deal boldly in debts of long term, there has not been much opportunity for a test" (p. 207).

6. "What would this involve for a society which finds itself so well equipped with capital that its marginal efficiency is zero and would be negative with any additional investment; yet possessing a monetary system, such that money will 'keep' and involves negligible costs of storage and safe custody, with the result that in practice interest cannot be negative; and, in conditions of full employment, disposed to save?

". . . the position of equilibrium, under conditions of *laissez faire*, will be one in which employment is low enough and the standard of life sufficiently miserable to bring savings to zero" (pp. 217-18).

7. "We have assumed so far an institutional factor which prevents the rate of interest from being negative. . . . In fact, however, institutional and psychological factors are present which set a limit much above zero to the practicable decline in the rate of interest . . . , which in present circumstances may perhaps be as high as 2 or 2½ percent on long term" (pp. 218-19). (At the time Keynes wrote, consol yields in the United Kingdom were around 3 percent.)

8. "It seems, then, that the *rate of interest on money* plays a peculiar part in setting a limit to the level of employment" (p. 222).

9. "We come to what is the most fundamental consideration in this context, namely, the characteristics of money which satisfy liquidity-preference. For, in certain circumstances such as will often occur, these will cause the rate of interest to be insensitive, particularly below a certain figure, even to a substantial increase in the quantity of money in proportion to other forms of wealth" (p. 233).

10. "The significance of the money-rate of interest arises, therefore, out of the combination of the characteristics that, through the working of the liquidity-motive, this rate of interest may be somewhat unresponsive to a change in the proportion which the quantity of money bears to other forms of wealth measured in money . . ." (p. 234).

11. "If . . . money wages were to fall without limit whenever there was a tendency for less than full employment . . . there would be no resting place below full employment until either the rate of interest was incapable of falling further or wages were zero" (pp. 303-4).

12. "The most stable, and the least easily shifted, element in our contemporary economy has been hitherto, and may prove to be in future, the minimum rate of interest acceptable to the generality of wealth owners" (p. 309).

13. "The destruction of the inducement to invest by an excessive liquidity-preference was the outstanding evil, the prime impediment to the growth of wealth, in the ancient and medieval worlds" (p. 351).

## Appendix 2

### Comments on Some of Patinkin's Other Detailed Criticisms

Except where otherwise indicated, all quotations are from Patinkin's comment.

1. "Milton Friedman's recent article . . . has two concerns. The first and—

from the viewpoint of the space devoted to it—major one is the chapter in the history of monetary doctrine which deals with . . . the quantity theory and Keynesian monetary theory.”

That was not my intention in the two articles under discussion, and I made no attempt to survey the relevant literature exhaustively. I was, and am, interested primarily in the analytical differences between the two approaches, and used references to literature to illuminate them. That is, I was concerned with “doctrinal aspects of monetary theory” but not with “the history of monetary doctrine”—though Patinkin treats these terms as synonymous.

2. Patinkin sees confession of error in my failure in the 1968 and 1970 essays to “mention either the Chicago School or its individual members.”

No such thing. The 1956 essay was the introduction to a series of studies done as Ph.D theses at Chicago. The Chicago background was relevant. It was not relevant to the later articles.

3. “No evidence is given in support of this assertion about the nature of the income version of the transactions approach”—that it stresses that money is held and can be regarded as a way station between the Fisher and the Cambridge version.

In line with point 1, I did not intend this to be a statement about the history of doctrine. Clearly, as an analytical matter, it can be regarded as a way station.

4. “Friedman . . . has claimed too little . . . in the context of his identification of the quantity theory with the short-run assumption that real income is constant.”

As already explained, I identified the quantity theory rather with the assumption that prices react more rapidly than output. The assumption Patinkin attributes to me, I introduced only in the “simple model” designed to highlight the problem of the missing equation.

5. “In view of the crucial role that Friedman assigns to the real-balance effect in assuring the long-run equilibrium of the system, I would prefer introducing this effect explicitly into the commodity-demand functions.”

But for the most part I was concerned with the short run, not the long run, and I never have believed that the real balance effect is of much empirical significance for the short run.

6. (Continuation of quotation in point 5.) “This would also seem to provide an expression of Friedman’s view that ‘the key insight of the quantity-theory approach is that such a discrepancy [that is, between the nominal quantity of money demanded and supplied] will be manifested primarily in attempted spending.’”

It would provide an erroneous expression, in my opinion. I believe the real balance effect plays a negligible role in this process. The substitution effect between money and other assets is, I believe, the key factor. This is a particularly clear example of Patinkin’s propensity to identify the quantity theory with long-run neutrality.

7. “Friedman achieves this interpretation [that Keynes assumed rigid wages and prices and did not generalize the theory to the case of wage and price flexibility] only by overlooking the chapter [chap. 19] in the *General Theory* . . . entitled ‘Changes in Money Wages’” which “I [Patinkin] have long considered . . . to be the apex of Keynes’s analysis” and in which Patinkin believes an economic analysis of wage movements was already provided.

Chapter 19 of the *General Theory* correctly points out that the argument that wage cuts will increase employment cannot be generalized directly from a single industry to the economy as a whole without taking into account effects on

demand. It then lists various possible effects of wage cuts on the propensity to consume, the marginal efficiency of capital, and the rate of interest, stresses that the only significant positive effect (in Keynes's system) is by increasing the real quantity of money and thereby lowering the rate of interest, and points out that this is subject to the same limitations as monetary policy—that is, the liquidity trap. It ends with some reflections on the appropriate policy, which Keynes concludes to be rigid wages.

There are intelligent, thoughtful, comments in the chapter, but to call it "the apex of Keynes's analysis" is an insult to Keynes. The chapter adds nothing important to the rest of the Keynesian apparatus; it rather illustrates how that apparatus can be applied to a particular problem and gives a basis for regarding rigid wages as not only an observable phenomenon but also a desirable policy. (See also my reply to Davidson.)

8. "Thus wage rigidities in this chapter [chap. 19 of the *General Theory*] are not an *assumption* of the analysis but the *policy conclusion* which Keynes reaches after investigating the results to be expected from wage flexibility."

Agreed. That is precisely what I said: "When there was no full-employment equilibrium, there was also no equilibrium nominal price level; something had to be brought in from outside to fix the price level; it might as well be institutional wage rigidity. Put differently, flexible nominal wages under such circumstances had no economic function to perform; hence they might as well be made rigid" (Friedman 1970a, p. 209; 1971b, pp. 18–19).

9. "To 'express all variables in wage units' . . . is surely not to assume that this unit is constant."

Of course not. But it does mean that the system of equations or relations expressed in wage units provides no information on the money value of the wage unit. That has to be brought in from outside, which is precisely my point. (See also Section 1 of my reply to Tobin.)

10. "Furthermore, this absence of money illusion is a necessary condition for the validity of the quantity theory."

Only in Patinkin's sense of that term, not my sense.

11. "Even before the flourishing of the Phillips curve, Keynesian econometric models generally treated the wage rate and price level as endogenous variables of the system," which, in Patinkin's view, contradicts my interpretation of Keynes as assuming wage rigidity.

The crucial question is not whether such equations are included in the models but whether they are ad hoc or derivable from Keynes's theoretical system. The price equations generally simply link prices to costs, mainly wages. This equation can be regarded as derivable from Keynes's system. But the wage equations are either purely ad hoc or, insofar as they are derivable from any theoretical system, it is the pre-Keynes classical system rather than Keynes's. For example, Patinkin refers approvingly to Klein's comment that "the main reasoning behind this equation is that of the law of supply and demand. Money wage rates move in response to excess demand on the labor market." The "law of supply and demand" is hardly Keynesian! More important, Klein misapplies it. The "classical law," as taken over by Keynes, would connect *real-wage rates*, not *money-wage rates*, with excess supply or demand. Klein's inclusion of the rate of change of prices in the equation, which Patinkin cites, is a move toward the correct classical inclusion of real wages, but if it went wholly in that direction it would leave money wages and money prices either undetermined or a simple inheritance from past history—which is precisely what I say Keynes's system assumes.

12. "Friedman cites 'Leijonhufvud's penetrating analysis' in support of his

(Friedman's) view [that the liquidity trap plays a crucial role in Keynes's system]—even though Leijonhufvud's actual position is exactly the opposite."

This is careless textual interpretation. What I actually said in the footnote to which Patinkin refers, was "I am indebted to a brilliant book by Leijonhufvud (1968) for a full appreciation of the importance of this proposition [that, 'as an empirical matter, prices can be regarded as rigid—an institutional datum—for short-run economic fluctuations'] in Keynes's system. This subsection and the one that follows, on the liquidity preference function, owe much to Leijonhufvud's penetrating analysis."

That is literally true and in no way whatsoever implies either that I agree with every conclusion Leijonhufvud reaches or that he agrees with my conclusions. In fact, I believe that Leijonhufvud's "penetrating analysis" justifies a different conclusion about the role of absolute liquidity preference in Keynes's system than the one Leijonhufvud reaches (that it plays no important role), and I am strongly confirmed in that conclusion by my rereading of Keynes (see quotations in Appendix 1).

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