

## COMMUNICATIONS

### INTERPRETING KEYNES: REFLECTIONS ON THE LEIJONHUFVUD-YEAGER DISCUSSION

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#### Introduction

In a recent exchange in this *Journal*, Leland Yeager (1988) and Axel Leijonhufvud (1988) consider the significance of Keynes's *General Theory* (1936) as a contribution to macro/monetary analysis. Yeager argues that "the Keynesian Revolution was a *diversion* from the path of progress in money/macro theory" (1988, p. 207, original italics).<sup>1</sup> According to Yeager, the *General Theory* contributed importantly to the neglect of traditional theories of monetary disequilibrium developed by pre-*General Theory* writers. It also was highly conducive to the disregard accorded to efforts by post-*General Theory* writers such as Clark Warburton to revive and extend monetary disequilibrium analysis (Yeager 1986a, 1986b). Leijonhufvud, on the other hand, has long maintained (e.g., 1968) that there is much in the *General Theory* to support the view that Keynes's monetary analysis is considerably more sophisticated than assumed in the Hicks-Hansen IS/LM caricature of income expenditure theory. An accurate and balanced interpretation of the *General Theory* and of Keynes's collected writings leads to the view, argues Leijonhufvud, that Keynes certainly did postulate a theory of monetary disequilibrium and a policy accentuating the importance of money. In contrast, it was the widespread proliferation of the IS/LM model within the profession

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<sup>1</sup>See also Yeager (1973, 1986a, 1986b).

that led during the 1940s and 1950s to the eccentric new doctrines of the “unimportance of money” and the “ineffectiveness of monetary policy” (Leijonhufvud 1968, p. 25).

Unlike old soldiers, some arguments do not fade away—even after more than 50 years of discourse. Fortunately, in this case the end can be shown to be close at hand, with the outcome resolved decisively in favor of the view espoused by Yeager. For Leijonhufvud’s exegesis rests on the Hicks-Hansen interpretation of the *General Theory* and the influence exerted by the IS/LM model on the profession. But John Hicks and Alvin Hansen were but two of the early interpreters of the *General Theory*. Moreover, Hansen was separated by large geographical distances from Cambridge, England, where the process, encompassing a number of years, of thinking out and writing the *General Theory* took place: Hansen left the University of Minnesota in 1937 in order to begin his tenure at Harvard. Throughout most of the 1930s Hansen had been a quantity theorist; his Keynesian conversion did not emerge until the late 1930s. Meanwhile, Hicks did spend several years visiting Cambridge (on leave from Manchester) during the mid-1930s. But during these years (1935–38), he had little contact with Keynes and Keynes’s followers. In particular, Hicks was invited to Cambridge by Arthur Pigou; Hicks accepted Pigou’s offer because of his (Hicks’s) friendship with Dennis Robertson (Hicks 1979, p. 200). As Hicks has stated, he was not well received by the Keynesian camp: “Cambridge, however, was already driven by disputes between Keynesians and anti-Keynesians; and since I was associated with Pigou and Robertson, I found myself regarded, at least by some Keynesians, as being in the ‘anti’ camp” (Hicks 1979, p. 200). Thus neither Hicks nor Hansen made a significant contribution to the formation of Keynes’s views as articulated in the *General Theory*.

There were, however, others who were closely associated with Keynes as he developed the doctrines set forth in the *General Theory*. In particular, there was the so-called Cambridge “Circus”—a group of younger economists at Cambridge (England)—consisting of Richard Kahn, Joan and Austin Robinson, James Meade, Piero Sraffa, and Roy Harrod. The Cambridge Circus met regularly between 1931 and 1936 to discuss, criticize, and propose changes to the successive drafts of what was later to become Keynes’s *General Theory*, with Kahn constituting the channel of communication between the group and Keynes.<sup>2</sup> What did this group of economists—having had direct

<sup>2</sup>Keynes himself never participated in the discussions. Patinkin (1978, p. 6) argues that his circumstances “may simply have been due to the fact that he did not have the time.”

access to Keynes, having influenced Keynes, and which, in turn, was influenced by Keynes—think about the effectiveness of monetary policy? What was the Circus's interpretation of Keynes's views on money as articulated in the *General Theory*? As the remainder of this paper documents, the impotency attributed to monetary policy by Keynesian economists during the 1940s and 1950s was by no means a Hicks-Hansen diversion. It was very much a *General Theory* diversion.

### The Cambridge Circus and Monetary Economics

Prior to the publication of the *General Theory*, monetary analysis had for centuries been dominated by the quantity theory of money. There were, of course, a number of variants to the quantity theory. But all had in common the view that inflation is primarily a monetary phenomenon. Further, all variants shared the view that monetary policy is an important instrument for purposes of economic stabilization (either cyclical or secular). One variant of the quantity theory was developed by Keynes's colleagues at Cambridge—Alfred Marshall, A. C. Pigou, and F. Lavington—as well as Keynes himself in his early writings (e.g., Keynes 1923). The Cambridge approach stressed a behavioral, demand-oriented interpretation of the quantity theory. Under this approach, the demand for money rested upon capital-theoretic foundations, emphasizing choice-making behavior of individuals.<sup>3</sup>

With the publication of the *General Theory*, established monetary doctrine, including the Cambridge version of the quantity theory, came under intense attack. Prominent players in this attack included those members of the Cambridge Circus who wrote on monetary economics during the 1930s—notably Richard Kahn, Joan Robinson, Roy Harrod, and James Meade. The criticisms of the quantity theory made by these economists, which in many instances either appeared concurrently with or prior to Hicks's famous *Econometrica* article (1937), and which were to become hallmarks of the Keynesian Revolution, included (i) negation of the quantity-theory view that inflation is a monetary phenomenon in favor of a cost-push hypothesis of

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Although Harrod taught at Oxford during the 1930s, Eltis (1987, p. 595) writes: "During the 1930s Harrod frequently stayed with Keynes and he was increasingly drawn into the group of brilliant young economists which included Richard Kahn and Joan Robinson who were helping him develop the new theories which culminated in *The General Theory*."

<sup>3</sup>The earlier Cambridge formulation of the quantity theory, in terms of capital-theoretic approach to the demand for money, contributed to Milton Friedman's (1956) reformulation of the quantity theory (see Tavlas 1981).

inflation, (ii) rejection of the earlier Cambridge behavioral demand-for-money analysis as formulated by Marshall, Pigou, and Lavington in favor of a mechanical interpretation of velocity, and (iii) the view that monetary policy is a weak stabilization instrument.

*Richard Kahn*

An initial blow against the quantity theory view that inflation is caused by changes in the supply of money was struck by Richard Kahn in his 1933 article "Public Works and Inflation." According to Kahn: "The rise of prices is determined by the supply curve [for labor] and nothing else" (1933, p. 169). Similar arguments were presented in two later articles by Kahn that appeared in the *Economic Journal* (1937, 1938). Subsequently, Keynes (1939) acknowledged that Kahn was probably responsible for the Keynesian view that prices are solely determined by marginal cost. Keynes (1939, p. 39) credited Kahn as the writer "who first attacked" the problem of studying "the relation of the the general level of prices to wages" and who reached the conclusion that it was indeed proper to use such concepts "that in competitive conditions prices are governed by marginal cost" and "that for a closed system as a whole marginal cost in the short period is substantially the same as marginal wage cost rather than to be derived from monetary factors." Moreover, Kahn himself has recently stated that one of the important contributions of his work during the early and mid-1930s was "... finally disposing of the idea that the price level is determined by the quantity of money. I never had been able to understand the quantity theory. What I explained in my article is that the price level is determined by the conditions of demand and supply in much the same way as the price of an individual commodity" (1978, p. 14).

In a 1936 article, Kahn compartmentalized the demand for money into "active" and "idle" components, contributing to a passive, mechanical interpretation of the velocity of circulation of money. Kahn stated that "the size of the active circulation (which is the only portion of the total stock of money that can be directly related to prices and output) is determined by the level of prices and of output and cannot be regarded as their determinant (Kahn 1936, p. 145). Kahn's segmentation of the demand for money thus resulted in a passive interpretation of velocity as follows: "V is a weighted average, and depends on the ratio of the inactive to the active circulation. . . . Now it can easily be demonstrated that if  $M$  increases [through an open market purchase], and if the active circulation does not increase in consequence, i.e., if  $PT$  does not increase . . . then  $V$ , the weighted *average*, will diminish, as a result of the change in the

relative weights of the active and inactive circulation, in exactly the same proportion as  $M$  has increased; so that  $MV$  is unaltered" (1936, p. 146, original italics).

Thus far, Kahn's pre-Hicks-Hansen work has been shown to include two of the Keynesian criticisms of established monetary doctrine—the negation of the quantity-theory interpretation of inflation and the reversal of the earlier Cambridge behavioral approach to the demand for money. What about the effectiveness of monetary policy? In his 1936 article, Kahn concluded that there is little scope for monetary policy other than keeping the level of interest rates down during situations of government financing. "The fact that  $MV = PT$  simply tells us that *if nothing is calculated to increase  $PT$ , an increase in  $M$  will fail to increase  $MV$* " (original italics). The reason for the extremely loose connection between money and activity was ascribed to the various elasticities upon which monetary policy must depend in order to exert an impact on nominal income. Kahn (1936, p. 146) listed the following: (a) the elasticity of the demand for money with respect to the interest rate, (b) the interest elasticity of investment, (c) the elasticity of output with respect to investment, and (d) the elasticity of prices with regard to output.

The importance of Kahn's views are accentuated by the following facts: (i) As noted, Kahn constituted *the* channel of communication between the Cambridge Circus and Keynes (Patinkin 1978, p. 6); (ii) the foregoing views were pre-Hicks-Hansen; and (iii) Kahn, the economist closest to Keynes during the formulative years culminating in the *General Theory*, attributed his views directly to Keynes. Thus, after noting the reasons (listed above) as to why there is likely to be a loose connection between money and economic activity, Kahn (1936, p. 146) stated: "I clearly [do] not regard the expansion of credit as the *cause of the increase in employment*. As Mr. Keynes has clearly pointed out, if a man grows broader it is desirable that he should lengthen his belt; but the lengthening of his belt is not the reason why his girth has extended" (original italics).

### *Joan Robinson*

Let us now move on and consider Joan Robinson's early work on monetary economics. As a useful point of departure, consider Robinson's review article (1938) of Bresciani-Turroni's *The Economics of Inflation* (1937), which dealt with the German hyperinflation of the early 1920s. Bresciani-Turroni's explanation of the German hyperinflation was that the enormous rise in the German price level was entirely a consequence of government borrowing of newly created money from the Reichsbank in order to finance the budget

deficit. Thus, after outlining Bresciani-Turroni's interpretation of the German episode, Robinson (1938, p. 507) noted, "Professor Bresciani-Turroni is a strong adherent of the quantity-theory school." Robinson (1938, p. 509), however, expressed dissent with the monetary interpretation of the German hyperinflation: "The author assumes, rather than argues, that an increase in the quantity of money was the root cause of the inflation. But this view is impossible to accept. . . . There is no evidence whatever that events in Germany followed this sequence." Robinson (1938, p. 509) conceded that an increase in the quantity of money might result in a rise in the price level, but only if "it leads to a reduction in the rate of interest, which stimulates investment and discourages saving." The causal role of money was thus seen to operate strictly through an indirect portfolio-adjustment channel. Robinson, however, doubted whether the quantity-theory interpretation was actually the case during the German inflation.

Instead, Robinson (1938, pp. 510–11) elucidated her contrary view of the inflationary process. "The essence of inflation," she argued, "is a rapid and continuous rise of money wages." "Without rising money wages, inflation cannot occur." The quantity-theory interpretation was regarded as "merely a theoretical possibility, not an account of the course of events in Germany" (1938, p. 511). Robinson argued that the increase in the German money supply which took place during the early 1920s was an endogenous response to prior increases in nominal wages.

The cost-push hypothesis of inflation was also articulated in Robinson's *Introduction to the Theory of Employment* (1937). Additionally, this volume includes the two other components forming the core of the early Keynesian attack on the quantity theory—the impotency of monetary policy and the mechanical interpretation of velocity. The former component is a derivation of post-*General Theory* portfolio-adjustment reasoning, to which Robinson alluded in her review of Bresciani-Turroni. In *Theory of Employment*, monetary policy again was seen to work only through its effect on the rate of interest, depending on the interest elasticity of investment. But, according to Robinson (1937, p. 121), the latter elasticity was likely to be insignificant: "The monetary authorities normally try to foster the remedial action of the rate of interest by deliberately increasing the quantity of money where activity has fallen to a low level, and restricting it when the boom is at its height. . . . It is found that such action as the authorities normally take is not sufficient to induce a steady rate of investment, for once pessimism has taken hold of the entrepreneurs a moderate fall in the rate of interest is not sufficient to restore the inducement to invest, and when they are dazzled by

golden visions of profit a moderate rise in the rate of interest will not check their enthusiasm.”

Leijonhufvud (1968, p. 405) has argued that “the interest-inelasticity of investment became the pivotal argument in the New Economics position on the issue [of the efficacy of monetary policy]. As we have repeatedly emphasized, this postulate did not enter into Keynes’s analysis at all. The dogma of the interest-inelasticity of expenditures as the bane of monetary policy did not originate in Cambridge but in Oxford.”<sup>4</sup> In light of the evidence presented above with regard to both Kahn and Robinson, Leijonhufvud’s thesis needs to be revised as follows: The dogma, of the interest-inelasticity of expenditures as the bane of monetary policy as having originated in Oxford (and not in Cambridge), itself originated at UCLA.

Robinson’s view concerning the passivity of velocity follows (as in the case of Kahn) from the proclivity of Keynes’s early interpreters to compartmentalize the demand for money into “active” and “idle” components. To demonstrate this relation between the segregation of the two components of money demand and a passive velocity of circulation, consider an illustration given in *Theory of Employment* relating to the sequence of events following an increase in the right-hand side variables of the quantity equation. “A rise in . . .  $PT$ ,” argued Robinson (1937, p. 95), “is determined, roughly speaking, by the level of trade activity.” If the banking system fails to increase the money supply to meet the induced increase in the active circulation, “the rate of interest rises and inactive deposits fall” as much as the active deposits have increased (p. 95). Hence, “the average velocity of circulation of money is raised [and] an increase in  $V$  is brought about as a consequence of an increase in  $PT$ ” (p. 95). In this manner, velocity becomes a passive concept, adjusting so as to preserve the identity of the quantity equation.

#### *Roy Harrod*

In a 1937 article titled, “Mr. Keynes and Traditional Theory,” Roy Harrod presented a virtually identical representation of the equations contained in Hicks’s 1937 *Econometrica* article. Indeed, Warren Young (1987) has claimed that Harrod’s paper was written before Hicks’s piece and that, accordingly, Harrod deserves credit as the originator of the IS/LM approach. In his 1937 article, Harrod argued that the analysis of price determination contained in the *General Theory* was based on cost-markup theory, and not on the quantity

<sup>4</sup>As evidence, Leijonhufvud (1968, p. 41) cites the Oxford Surveys of the late 1930s, dealing with the interest elasticity of investment.

theory of money. According to Harrod, in Keynes's system "the price level should be determined otherwise than by the monetary equation." And "the price level is related to the level of activity by the marginal cost of production" (1937, p. 83).

Also, in his 1936 essay, *The Trade Cycle*, Harrod argued that given "those forces which . . . govern the volume of output and the level of prices, . . . these in turn cause the velocity of circulation to be what it is" (1936, p. 120). Thus, in the case of Harrod, velocity adjusts passively so as to preserve the identity of the quantity equation. Further, Harrod, too, was skeptical about the efficacy of monetary policy during recessions. He argued that in "certain instances" the demand for money becomes "extremely elastic. . . . Thus it seems improbable that banking policy, however inspired and well informed, could secure a sufficient fluctuation in long-term interest rates to ensure a steady advance" (1936, pp. 124–25).

### *James Meade*

James Meade's writings present a most interesting case, for several reasons. First, in his 1937 book, *Economic Analysis and Policy*, Meade, in contrast to the other members of the Cambridge Circus, put considerable weight on the efficacy of monetary policy during depressions. Chapters II and III of *Economic Analysis and Policy* show how open market purchases serve to "reduce the rate of interest on long-term industrial securities . . . increasing the demand for capital goods and consumption goods" (1937a, p. 28). He thus argued that open market operations should be used to "increase the amount of money and lower interest rates when there is general unemployment" (1937a, p. 30).

The second reason why Meade's writings are of special significance is to be found in his 1937 article "A Simplified Model of Mr. Keynes' System." In presenting a mathematical and general equilibrium model of the *General Theory*, in that article (1937b), Meade, like Harrod, articulated an analytic framework strikingly similar to that found in Hicks's 1937 *Econometrica* contribution. As David Vines has recently noted regarding Meade's 1937 article: "In this article Meade set out the equations of the IS/LM model (and *apparently this paper was circulated before the celebrated exposition of the IS/LM [model] was presented by Hicks*") (Vines 1987, p. 411, italics supplied).

The following observations are in order. First, two other economists—both members of Keynes's inner circle—independently presented the theoretical structure of the *General Theory* in IS/LM terms. Given that three researchers concurrently and independently



derived the IS/LM model from Keynes's book, could it be that the IS/LM model is not merely *an* interpretation of the *General Theory* but a *message* of the *General Theory*? It is worthwhile to point out in this connection that Keynes, on reading Hicks's paper, wrote to Hicks that he had "next to nothing to offer by way of criticism" (Bliss 1987, p. 644). Second, it is interesting that in Meade's case we have a writer who espoused both the IS/LM framework *and* the efficacy of monetary policy. Apparently, the Hicks-Hansen framework was not a necessary condition for the widespread impotency attributed by the profession to monetary policy during the 1940s and 1950s.

#### *Whither the Quantity Theory?*

As the foregoing presentation has documented, the impotency ascribed to monetary policy by Keynesian economists was not a Hicks-Hansen creation. It was a *General Theory* creation. Members of the Cambridge Circus such as Kahn, Robinson, and Harrod, who were partakers in the generation of ideas contained in the *General Theory*, all interpreted that book as denigrating the efficacy of monetary policy—prior to the appearance of Hicks's 1937 article. Further, all thought that the *General Theory* served to effectively obliterate the quantity theory of money. Thus, consider the following:

For discussion of changes in trade activity, the quantity theory is a weak and treacherous instrument [Robinson (1937, p. 97)].

Dr. Neissers' article [i.e., Neisser (1936)] is an extreme example of the type of confusion which is liable to arise from blind faith in the Quantity Theory of Money [Kahn (1936, p. 146)].

And that was the end of the *Quantity Theory* until its recent resuscitation. Keynes in his long struggle for release had conquered [Kahn (1984, p. 59) on the impact of the *General Theory*].

The set of ideas to which the doctrines of this essay are most repugnant are those connected with the Quantity Theory of Money. This is a curious upshot. For many years monetary doctrine . . . has been thought to be the securest part of economic theory. Yet all now seems changed [Harrod (1936, p. 125)].

Finally, it is extremely noteworthy that several of the articles cited above—Kahn (1937, 1938), Robinson (1938)—were published in the *Economic Journal*, of which Keynes was editor. At a time when his book was creating a revolution in economic thought, are we to expect that the editor elected to stand detached from interpretations of his views that may have misrepresented his message?

### Casualties of the Keynesian Revolution

The intellectual environment created by the *General Theory* contributed to both the intellectual and personal abuse of writers who

continued to espouse what had long been established monetary doctrine. As noted, Yeager cites the work of Clark Warburton as one such writer. There were others as well. Three such writers were Dennis Robertson, Arthur Marget, and, during the 1950s and into the 1960s, Milton Friedman.

Robertson had been a student of Keynes. He then became a fellow teacher at Cambridge and a collaborator in research. However, Robertson thought that the theoretical presentation of liquidity preference in the *General Theory* was flawed.<sup>5</sup> His reaction to the *General Theory*, and his continued adherence to the Cambridge behavioral demand-for-money approach to the quantity theory, led to an estrangement between Robertson and Keynes (Danes 1987, p. 210).

The outcome of this estrangement is a rather sad commentary on the behavior of certain members of the profession. As Harry Johnson (1978a, p. 136) pointed out: "Keynes had a group of young people around him [the Cambridge Circus]—Richard Kahn and Joan Robinson, in particular. . . . He deliberately egged them on to attack Robertson—not that they needed urging." Johnson also noted that this led to "the harrying of Robertson through the 1930s both in print and personally; the latter was much more serious. He had been prevented from receiving what he (and many others) considered was the final reward of a serious academic career, namely a professorship at Cambridge" (1978a, p. 139). Likewise, David Laidler (1980, p. 1272) has observed that "there is nothing in the history of monetarism to parallel the hounding of Dennis Robertson by Keynes and his younger colleagues."

Arthur Marget was another casualty of the Keynesian Revolution. Marget taught first at Harvard (1920–27) and then at the University of Minnesota (1927–43). During the 1920s and 1930s he was a prolific contributor to the quantity-theory literature. His *magnum opus* was *The Theory of Prices*, published in two volumes (1938, 1942). The first volume (1938) was defense of the quantity theory from the critical positions adopted by Keynes in the *Treatise on Money* (1930). The second volume (1942) was a defense of established monetary doctrine from the criticisms contained in the *General Theory*.

Reviews of the first volume of *The Theory of Prices* were, for the most part, favorable. Interestingly, Alvin Hansen reviewed the book (favorably) for the *American Economic Review*. Hansen wrote that "this volume is the result of a stupendous amount of work in the literature on money and prices. It is the product of prodigious scholarship such as is extremely rare in America" (Hansen 1938, p. 750).

<sup>5</sup>See, for example, Danes (1987).

By contrast, the *Economic Journal* reviewer, Nicholas Kaldor, was, to say the least, considerably less generous in his appraisal. Kaldor, who by the late 1930s had become a disciple of Keynes (see Tavlas 1981), thought the book was “pompous” (1939, p. 455). “It reminds one of the bourgeois solidity and spaciousness of that bygone age” (1939, pp. 455–56). In particular, Kaldor was critical of Marget’s defense of the quantity theory of money. Said Kaldor: “Continued use of the  $MV = PT$  [Irving Fisher’s equation of exchange] type of equation (or of the  $n = pk$  [Cambridge] type), even when shorn of its wings, as in Professor Marget’s interpretation, is positively harmful rather than helpful” (1939, p. 497).

The second volume of *The Theory of Prices* (1942) received praiseworthy reviews in both the *American Economic Review* (Ellis 1945) and the *Journal of Political Economy* (Reed 1942).<sup>6</sup> Thus, Ellis wrote: “Beyond his exuberant scholarship and industry, Marget offers us penetrating thought, sound judgement, and a method more complete than Keynes’s for articulating partial and general equilibrium analysis and monetary theory” (1945, p. 90). Reed “congratulate[d] him [Marget] on his literary achievement” (1942, p. 564). Repeating a point that Hansen had made in his earlier review of the first volume of the *The Theory of Prices*, Reed stated that Marget’s “work represents a type of research that is extremely rare in America” (1942, p. 564). By contrast, the editors of the *Economic Journal*—Keynes and Austin Robinson—sat by on the sidelines. For some reason, they decided not to have the second volume of *The Theory of Prices* reviewed. By the early 1940s, Marget’s quantity-theory orientation was out of fashion. Yet, in the second volume of *The Theory of Prices* he wrote: “It will be the workers of another generation, possessed of a later and broader perspective than our own, who will decide where victory lay in the ‘Keynesian controversy’—one of the greatest, if not the greatest, of the internecine controversies that have ever split the ranks of economic theorists” (1942, p. 768). Years later, Marget’s approach would be vindicated, due in large measure to the work of Milton Friedman.

But it was not to be easy. Friedman’s efforts to revive the quantity-theory approach during the 1950s and 1960s met with a good deal of skepticism, if not outright cynicism. As Robert Lucas (1984, p. 53) has observed: “The [Keynesian] consensus of the 60s was artificial and unhealthy. Look at the way Friedman’s work was criticized during that period. I think it’s just a disgrace to the profession that he was treated as though he were some kind of nut.”

<sup>6</sup>These are the *only* reviews of the second volume that I have been able to locate.

Friedman was the beneficiary of an earlier (1930s) Chicago monetary tradition which rejected the Keynesian Revolution and which formulated the quantity theory in terms of a behavioral velocity or demand for money function with respect to price expectations.<sup>7</sup> But Friedman's Chicago predecessors were writing at a time when there was as yet no adequate theory of demand for a stock. Friedman (1956), however, was able to reformulate the quantity theory in terms of consumer behavior, where the demand function for money depends on the price level, permanent income, bond and equity yields, the rate of change of the price level, the ratio of human to nonhuman wealth, and a taste variable. This represented an important modification of the pre-*General Theory* Cambridge approach, extended to include other variables, in addition to income and wealth, in the demand for money function.

Friedman was also the heir to an empirical tradition in American monetary economics dating back into the mid-19th century.<sup>8</sup> In this vein, Friedman presented the reformulated quantity theory as a testable empirical hypothesis of a stable demand function for money. While Friedman's approach won the day during the late 1960s and 1970s, the 1980s saw a breakdown in many countries of what previously had been stable demand for money functions.<sup>9</sup> One response to this breakdown has been the so-called buffer-stock, or disequilibrium, approach to monetary theory and money-demand estimation.<sup>10</sup> This modern disequilibrium approach is very much in line with the views of earlier disequilibrium theorists, such as Clark Warburton. As David Cobham (1988, pp. 4–5) explains the modern approach: "It assumes that markets do not clear perfectly; prices are not automatically or immediately adjusted to market clearing levels as if by some Walrasian auctioneer but are set by particular economic agents on the basis of their expectations of the behavior of other agents and their perceptions of their own interests. . . . Disequilibrium monetarism [also] assumes that, although automatic equilibrating forces

<sup>7</sup>For documentation on both these points, see Tavlas (1977a, 1977b).

<sup>8</sup>See Tavlas and Aschheim (1985).

<sup>9</sup>Friedman's empirical work has always used a long-run (i.e., no lagged dependent variable) specification of the demand for money. Further, Friedman has always estimated using cyclically adjusted data. Subsequent researchers have, for the most part, overlooked the longer-run nature of Friedman's approach, employing instead, quarterly or annual data and a short-run specification of money-demand (i.e., including a lagged dependent variable).

<sup>10</sup>See, for example, Laidler (1984). For a discussion of the empirical approaches used to implement the disequilibrium money concept, and for a listing of the recent literature, see Swamy and Tavlas (1989a, 1989b).

may be very weak in the short term . . . they are very powerful over the longer term." Similarly, Warburton stressed the short-run stickiness of prices and wages in his disequilibrium theory, and the long-run tendency toward equilibrium (Yeager 1981, pp. 281–82; Dorn 1983, pp. 3–6; Dorn 1987).

Dennis Robertson once wrote that "highbrow opinion is like a hunted hare; if you stand long enough it will come back to the place it started from."<sup>11</sup> Perhaps so, but 50 years has been a long time to wait.

## Conclusion

The key issue addressed in this paper is: Who is best able to tell us what Keynes really meant on monetary economics: Keynes's colleagues at Cambridge who during the 1930s contributed significantly to the development of the ideas contained in the *General Theory*, or Professor Leijonhufvud, separated from Cambridge, England, by an ocean and a continent, by some 30 years from the writing of Keynes's book, and equipped with subsequent developments in the economic theory of market information and search? One could argue, I suppose, that such economists as Richard Kahn, Joan Robinson, Roy Harrod, and James Meade were not up to the intellectual task of digesting the *General Theory*, even after having spent years contributing to its formation. It is doubtful, however, whether such a line of reasoning would have many (any) takers.

Harry Johnson (1978b, p. 189) has characterized Leijonhufvud's 1968 book as a "monumentally scholarly work." Meanwhile, Yeager has written that Leijonhufvud should step forth and take full credit for the originality of ideas wrongly attributed by Leijonhufvud to Keynes. He certainly should, and for reasons that go beyond the just attribution of ideas. For by having failed to take proper credit, Leijonhufvud has unintentionally perpetuated a second diversion: the belief that it was the Hicks-Hansen exegesis that led to the notions of the unimportance of money and the ineffectiveness of monetary policy. In fact, it was no such thing. It was Keynes!

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<sup>11</sup>Quoted from Nobay and Johnson (1978, p. 470).

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