The article is based on textual evidence from the quantity-theory and Keynesian literature. It shows, first, that the conceptual framework of a portfolio demand for money that Friedman denotes as the “quantity theory” is actually that of Keynesian economics. Conversely, Friedman detracts from the true quantity theory by stating that its formal short-run analysis assumes real output constant, while only prices change. Friedman also incorrectly characterizes Keynesian economics in terms of absolute price rigidity. He does this by overlooking the systematic analysis by Keynes and the Keynesians of the role of downward wage flexibility during unemployment, and of the “inflationary gap” during full employment. Otherwise Friedman’s interpretation of Keynes is the standard textbook one of an economy in a “liquidity-trap” unemployment equilibrium. The author restates his alternative interpretation of Keynesian economics in terms of unemployment disequilibrium.

“When I use a word,” Humpty Dumpty said, in rather a scornful tone, “it means just what I choose it to mean—neither more nor less.”

“The question is,” said Alice, “whether you can make words mean so many different things.”

“The question is,” said Humpty Dumpty, “which is to be master—that’s all.” [LEWIS CARROLL, Through the Looking Glass]

Milton Friedman’s recent article on “A Theoretical Framework for Monetary Analysis” (Friedman 1970a) 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 nature of—and inter-
relationships between—the quantity theory and Keynesian monetary
theory. The second concern is to present an analytical framework to
analyze the dynamics of monetary adjustment. In this paper I shall con-
centrate primarily on the doctrinal aspects of Friedman's paper. For this
reason I shall not discuss Friedman's subsequent paper (1971), which—
with one exception (see n. 9 below)—does not deal with these aspects.

Clearly, questions about the history of economic doctrine are empirical
questions which can be answered only on the basis of evidence cited from
the relevant literature. Indeed, the "elementary canons of scholarship call
for [such] documentation" (Friedman 1970b, p. 318). My criticism of
Friedman is, accordingly, that on many occasions he has not provided such
evidence; that, indeed, on some occasions he has ignored the detailed
evidence which has been adduced against the views he expresses; and that
on still other occasions he has indulged in casual empiricism in the attempt
to support his doctrinal interpretations. These criticisms will be docu-
mented in what follows.

I. Friedman on the Quantity Theory:
The Doctrinal-History Aspects

In the paper under discussion, Friedman once again (see Friedman 1956,
1968) presents a theory of money whose central feature is a demand func-
tion for money, where this demand is treated "as part of capital or wealth
theory, concerned with the composition of the balance sheet or portfolio of
assets" (Friedman 1970a, p. 202). Accordingly, his demand function
depends on wealth and the alternative rates of return on money and other

If Friedman had simply presented this as a conceptual framework to be
used for monetary analysis, then few would have disagreed with him. On
the contrary, most of us would have enjoyed the systematic clarity of the
exposition; would have considered the suggested influence on the demand
for money of the division between human and nonhuman wealth (which
Friedman carries over from his well-known work on the consumption
function) to be a fruitful one, well worth exploring; and would also have
benefited from the insightful presentation of the rate of change of the price
level as one of the alternative rates of return which affect the demand for
money. For though this last factor has been referred to in both the
quantity-theory and Keynesian literature (Fisher 1922, p. 63; Brown
1939, p. 34), it was not systematically integrated into our thinking until
the work of Friedman and his associates, particularly Cagan (1956).

But, as indicated in my introductory remarks, Friedman is not con-
cerned solely with substantive analytical matters but has a major concern
with the doctrinal-history aspects of monetary theory. Once again, few
would have disagreed if Friedman had described his conceptual framework as being a particular instance of the Keynesian liquidity-preference theory, while noting the specific contributions indicated in the preceding paragraph. This, however, is not Friedman's way. Instead he tells us that “the general theoretical framework that underlies” his empirical studies—and which has been referred to at the beginning of this section—is that of “the quantity theory of money” (Friedman 1970a, p. 193). Accordingly, he presents this framework as the sequel to a fairly detailed presentation, first of Fisher's transactions equation, \( MV = PT \) (which Friedman describes as being primarily concerned with “the mechanical aspects of the payments process”);\(^1\) then of the income form of this equation, \( MV = Py \); and finally of the Cambridge cash-balance equation, \( M = kPy \) (with which Friedman claims the closest affinity) (1970a, pp. 194–202).

What, then, does this leave as Keynes's contribution to his theoretical framework? Friedman's answer to this question is expressed in the following passage:

J. M. Keynes's liquidity preference analysis (discussed further in section 5, below) reinforced the shift of emphasis from the transactions version of the quantity equation to the cash-balances version—a shift of emphasis from mechanical aspects of the payments process to the qualities of money as an asset.\(^2\) Keynes's analysis, though strictly in the Cambridge cash-balances tradition, was much more explicit in stressing the role of money as one among many assets, and of interest rates as the relevant cost of holding money. [1970a, p. 202]

In his subsequent discussion of the demand function for money in section 5—entitled “The Keynesian Challenge to the Quantity Theory”—Friedman goes on to say:

Keynes's basic challenge to the reigning theory [was in his proposition that the] demand function for money has a particular empirical form—corresponding to absolute liquidity preference—that makes velocity highly unstable much of the time, so that changes in the quantity of money would, in the main, simply produce changes in \( V \) in the opposite direction. [P. 206]

Thus the picture which Friedman attempts to create is clear: namely, the conceptual framework he uses for monetary analysis is that of the quantity theory; its basic difference from the Keynesian theory lies in the

\(^1\) As a general characterization, this is somewhat unfair; for, as I have shown elsewhere, Fisher's analysis of the effects of a monetary change is actually far less mechanical than that of the Cambridge School (Patinkin 1965, pp. 166–67).

\(^2\) From much the same viewpoint, one could say that Newton's theory reinforced the shift from Ptolemaic to Copernican astronomy.
fact that the latter assumed the demand function for money to become highly (infinitely) interest elastic. As against this picture, I would like to present the following one: the conceptual framework which Friedman uses to analyze the demand for money is that of the Keynesian theory of liquidity preference—with Friedman's addendum that empirically this demand does not become highly (infinitely) elastic, and is indeed relatively inelastic. And, as important as are the policy implications of this addendum, we should not let it wag the theory.

It is obviously no criticism of Friedman—nor does it derogate from his stature as a monetary economist—to say that his analytical framework is Keynesian. All that is being criticized is Friedman's persistent refusal to recognize this is so.

Let me also say that to accept the Keynesian conceptual framework for the analysis of the demand for money does not imply that one must reject the quantity-theory conclusions about the long-run impact of monetary changes on the economy. This is a proposition which has been emphasized for many years in the literature (Modigliani 1944, sec. 14; Patinkin 1949, pp. 23–26; 1954; 1965, chaps. 10–11). But the converse of this proposition is also true: namely, that there is no need for modern-day quantity theorists to attempt to reinterpret the history of monetary doctrine so as to minimize the Keynesian nature of their analytical framework.

As I have shown elsewhere (Patinkin 1969a, pp. 58–61), there are two (related) justifications for the usual practice of treating the Keynesian theory as a distinct one, and not simply as a variation of the Cambridge cash-balance theory. The first is the different relationship of these two theories to one of the central distinctions of economic analysis—that between stocks and flows. In particular, Keynesian liquidity-preference theory is concerned with the optimal relationship between the stock of money and the stocks of other assets, whereas the quantity theory (including the Cambridge school) was primarily concerned with the direct relationship between the stock of money and the flow of spending on goods and services. Furthermore, the quantity-theory discussion of this relationship either did not make the distinction between stocks and flows—or at least was imprecise about it. This stands in sharp contrast with the Keynesian analysis of the effect of monetary changes in terms of initial balance-sheet adjustments among assets which generate changes in their relative yields, which in turn ultimately affect the flows of expenditures and receipts. Similarly, quantity theorists paid little, if any, attention to the effects on the rate of interest and other variables of shifts in the tastes of individuals as to the form in which they wish to hold their assets.

The second justification lies in the different treatment by these two theories of what continues to be one of the central issues of monetary

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3 As attested particularly by the extensive empirical literature of the past fifteen
For, though quantity theorists did frequently recognize this influence, they did not fully integrate it into their thinking. Most revealingly, they failed to do so in their empirical work—which, by its very nature, confronted them with a concrete situation in which they were called upon to list the major theoretical variables which might explain the data, even if some of the variables might subsequently be rejected as statistically unimportant. It is therefore significant that the first empirical study (to the best of my knowledge) which explicitly deals with the influence of interest on the demand for money is the 1939 Keynesian-inspired study by A. J. Brown.⁵

These hallmarks of the Keynesian liquidity-preference theory also characterize Friedman's exposition. It should be said that Friedman has taken some account of criticisms and has in recent years partly acknowledged this intellectual indebtedness. Thus in his 1956 essay Friedman presented his analytical framework as one that "conveys the flavor" of the Chicago quantity-theory tradition of Simons, Mints, Knight, and Viner—and did not even mention Keynes or the liquidity-preference theory (Friedman 1956, pp. 3–4). In contrast, in his 1968 and 1970 essays he does not mention either the Chicago School or its individual members—and he describes his framework as "a reformulation of the quantity theory that has been strongly affected by the Keynesian analysis of liquidity preference" (Friedman 1968, p. 439b).

At the same time, Friedman has not yet faced up to the implications of the fact⁶ that whatever the similarities in policy proposals (and there are significant differences here too [Patinkin 1969a, p. 47]), the theoretical framework of the Chicago School of the 1930s and 1940s—a major center of the quantity theory at the time—differed fundamentally from his. In particular, the Chicago School—as exemplified especially by Henry Simons—was basically not interested in the demand function for money (Simons never even mentioned this concept!) and carried out its analysis instead in terms of Fisher's \( MV = PT \) equation. Furthermore (and in marked contrast with Friedman) the basic assumption of the Chicago School analysis was that the velocity of circulation is unstable. Correspondingly, it considered sharp changes in this velocity to be a major source of instability in the economy.

Similarly, Friedman has not changed his basic contention that his con-
ceptual framework is that of the quantity theory. Indeed, his 1970 essay includes a further misinterpretation of the nature of this theory. In particular, in his discussion of the transactions approach to the quantity theory, Friedman makes the familiar distinction between the Fisherine equation $MV = PT$ and the "income form of the quantity equation," $MV = Py$, where $y$ is real national income and $V$ accordingly represents the income velocity of circulation. He then goes on to say:

Clearly, the transactions and income versions of the quantity theory involve very different conceptions of the role of money. For the transactions version, the most important thing about money is that it is transferred. For the income version, the most important thing is that it is held. This difference is even more obvious from the Cambridge cash-balances version of the quantity equation. Indeed, the income version can perhaps best be regarded as a way station between the Fisher and the Cambridge versions. [1970a, p. 200]

No evidence is given in support of this assertion about the nature of the income version of the transactions approach—which, if true, would obviously increase the possible relevance of Friedman's interpretation of the quantity theory in terms of the individual's demand to hold money as a component of a portfolio of assets. This is not the occasion to undertake a full-scale study of the development of the income-velocity form of the quantity theory. Let me only say that a brief examination of the interwar quantity-theory literature shows that the income-velocity approach was used as a variant of the transactions approach—and involved no more emphasis on the holding of money (as contrasted with its being transferred) than did the latter. The reasons for using income velocity were either considerations of data availability or the feeling that the volume of final output and/or the price level of this output were more meaningful economic variables than the gross volume of transactions and/or its price level (to use modern terms, more strategic variables). To the extent that the income-velocity approach constituted a "way station," it was one on the road between the Fisherine quantity theory and the Keynesian income-expenditure approach. It was not one on the road to the Cambridge cash-balance approach.\footnote{This paragraph is based on Robertson (1948, pp. 33, 38), Angell (1933, pp. 43–46), Warburton (1945, p. 161), and Chandler (1940, pp. 71–72; 1953, p. 543). I hope on some future occasion to deal more fully with this question, as well as the general question of the relations between the three forms of the quantity theory.}

I have so far criticized Friedman for claiming too much for the quantity theory; let me now indicate one direction in which he has claimed too little. This occurs in the context of his identification of the quantity theory with the short-run assumption that real income is constant, while
only the price level changes (for details, see the next section). Here Friedman states:

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 $y$. 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. [Friedman 1970a, p. 208; italics added]

The facts of the case, however, are quite different. Thus Fisher's analysis of the "transition period"—which was assumed to last ten years on the average, and during which both the level of real output and the velocity of circulation were changing—assigned a critical role to the difference between the money and real rates of interest. In this way Fisher integrated his analysis of the "transition period" into his formal theoretical analysis of the distinction between these two rates of interest—a distinction that was a basic component of his theoretical framework even before he turned to monetary problems. It might also be noted 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 of the policy-oriented Chicago quantity-theory school of the 1930s and 1940s: indeed, Simons and Mints showed little, if any, interest in the long-run aspects of the quantity theory. Again, representatives of the Cambridge School such as Lavington, Pigou, and Robertson wrote entire monographs on the problems of the "trade cycle" and of "industrial fluctuations"—and devoted substantial parts of these monographs to the analysis of the role of money in these fluctuations. Needless to say, this role was also a primary concern of Wicksell and Hawtrey. Thus, far from being a question dealt with in "asides," the systematic analysis of the short-run variations in output and velocity generated by monetary changes was a major concern of the pre-Keynesian quantity theorists.8

In order to avoid any possible misunderstanding, I wish to emphasize that Friedman in his own application of his "modern quantity theory" obviously assigns a central role to the short-run effects of changes in the quantity of money on $k$ and $y$; what I am criticizing here, however, is

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8 See Fisher (1896; 1907; 1922, chap. 4). For a detailed description of Fisher's voluminous writings on his policy proposals (the main one of which was to stabilize the price level), see Reeve (1943, chap. 11). On the Chicago School, see Patinkin (1969a, secs. 2, 3). This question is discussed in greater detail in my paper "On the Short-Run Non-Neutrality of Money in the Quantity Theory" (Patinkin 1972a).
Friedman's contention that the traditional quantity theorists themselves did not recognize this role in their "formal theoretical analysis."

II. Friedman on the Quantity Theory: Some Analytical Issues

In the preceding section I have dealt with the history of doctrine. In the present section I shall turn briefly to analytical issues and again criticize Friedman for claiming too little: namely, for not presenting the long-run quantity theory in the most general way that one can, once one has decided to reformulate it. To a large extent, what is involved here is a question of tastes. My own are for introducing explicitly into the mathematical model the assumptions of the text—particularly when this yields (without much inconvenience) a more general model.

Friedman's presentation of the "simple quantity theory" is in terms of the following general equilibrium model which, "in Keynes's spirit, . . . refers to a short period in which the capital stock can be regarded as fixed" (1970a, p. 218):

\[ f(y, r) + g(r) = y, \] \( (1) \)

\[ l(y, r) = \frac{M_0}{p}, \] \( (2) \)

where \( y \) is real national income, \( r \) is the rate of interest, and \( M_0 \) is the fixed quantity of money; and where the left-hand side of (1) consists respectively of the consumption and investment functions, while the left-hand side of (2) consists of the demand function for money—all expressed in real terms (Friedman 1970a, pp. 217-18).

This is a system of two equations in three variables: \( y, P, \) and \( r \). Hence, continues Friedman,

there is a missing equation. Some one of these variables must be determined by relationships outside this system. . . . The simple quantity theory adds the equation

\[ y = y_0; \] \( (3) \)

that is real income is determined outside the system. In effect, it appendes to this system the Walrasian equations of general equilibrium, regards them as independent of those equations defining the aggregates, and as giving the value of \( y \), and thereby reduces this system to one of \( [\text{two}] \) equations determining \( [\text{two}] \) unknowns.

Equation (3) then permits "a sequential solution" of system (1)–(2). In particular, substituting from (3) into (1), we can solve for \( r = r_0 \). Sub-
stitution of this value into (2) then yields the "classical quantity equation"

\[ M_0 = P l(y_0, r_0) = P \frac{l(y_0, r_0)}{y_0} y_0 = \frac{P y_0}{V}, \]  

(4)

which then determines \( P \) (Friedman 1970a, pp. 219–20).

Instead of initially creating a problem of a "missing equation" which is then solved by determining \( y \) "outside the system," I would prefer including in the model from the very beginning that part of the "Walrasian equations of general equilibrium" needed to determine \( y \) endogenously. This preference is reinforced by the fact that all that need be added to the model for this purpose are the production function and the excess-demand equation in the labor market. For the assumption of wage and price flexibility (which, in the context of the quantity theory, is in any event being made) assures that the equilibrium level of employment will be achieved in the labor market. And since the capital stock is fixed, the production function then determines the equilibrium level of real output, \( y \), corresponding to this level of employment. Indeed, this procedure has been the standard one in the literature since Modigliani (1944).

Again, in view of the crucial role that Friedman assigns to the real-balance effect in assuring the long-run equilibrium of the system (Friedman 1970a, pp. 206, 215), I would prefer introducing this effect explicitly into the commodity-demand functions. 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. [1970a, p. 225]

It will not come as a surprise to the reader that these modifications yield a model which I have developed at length elsewhere (Patinkin 1954; 1965, chaps. 9–10). The long-run proportionality of \( P \) to \( M \) specified by the quantity theory holds true in this model even though it cannot, in Friedman’s terms, be "solved sequentially" (or, to use the more usual term, it cannot be dichotomized), so as to reduce it to one equation (that for money) in one variable (the price level). Thus this model requires us to abandon the traditional single-equation form of the quantity theory which Friedman apparently prefers. On the other hand, the model has what is for me the more than compensating advantage of demonstrating that the long-run validity of the quantity theory does not (as it was at one time thought—and as might mistakenly be inferred from Friedman’s presentation) depend on the restrictive assumption that the system can be dichotomized in the foregoing manner (Patinkin 1965, p. 175).
III. Friedman on Keynesian Economics

A Keynesian, according to Friedman, is one who makes the above system (1)–(2) determinate in the short run by adding the equation

\[ P = P_0 \]  

instead of equation (3), \( y = y_0 \) (Friedman 1970a, pp. 206, 219–20). Thus, the contrast that Friedman tries to draw is between the quantity theorists—who assume real income constant and prices flexible, and indeed changing in direct proportion to changes in the money supply, and the Keynesians—who make the opposite assumptions about real income and prices.9

Let me first of all point out that this description of the quantity theory is misleading. For, though Friedman presents both of the foregoing positions as referring to the short run (Friedman 1970a, pp. 206, 222), quantity theorists did not actually assume real income to remain constant—and the price level to change proportionately with the quantity of money—except in the long run. In the short run (as shown at the end of Section I above) they believed that a (say) decrease in the quantity of money would decrease both the velocity of circulation and the level of real output as well as the price level. Thus part of what Friedman presents as a difference between Keynes and the quantity theory is really a difference between these “runs.”

Let me also say that presentations of the Keynesian theory of unemployment usually begin with an analysis based on the assumption of absolute wage and price rigidity. However, the basic question here at issue is whether these presentations have gone on to generalize the theory to the case of wage and price flexibility.

The clear implication of Friedman’s interpretation (1970a, esp. pp. 206, 209–11) is that Keynesian economics provided no such generalization. But Friedman achieves this interpretation only by overlooking the chapter in the General Theory devoted to—and indeed, entitled—“Changes in Money Wages,”10 by overlooking those portions of interpretations of

9 In Friedman 1971 (p. 324, n. 1) this interpretation of Keynes is slightly modified—but not in a way that really affects the following criticism.

10 I have long considered this chapter to be the apex of Keynes’s analysis (Patinkin 1951, p. 283, n. 38). In support of this interpretation let me cite its opening paragraphs: “It would have been an advantage if the effects of a change in money-wages could have been discussed in an earlier chapter. For the Classical Theory has been accustomed to rest the supposedly self-adjusting character of the economic system on an assumed fluidity of money-wages; and, when there is rigidity, to lay on this rigidity the blame of maladjustment. It was not possible, however, to discuss this matter fully until our own theory had been developed. For the consequences of a change in money-wages are complicated. A reduction in money-wages is quite capable in certain circumstances of affording a stimulus to output, as the classical theory supposes. My difference from this theory is primarily a difference of analysis; so that it could not be set forth clearly until the reader was acquainted with my own method” (Keynes 1936, p. 257; see also pp. 231–34).
Keynes that he does cite in which the implications of wage and price flexibility are analyzed within the Keynesian system (for example, Tobin 1947, pp. 585–86; Patinkin 1948, 1951, sec. 14; Leijonhufvud 1968, pp. 319 ff., 340 ff.), and, finally, by overlooking entirely the classic interpretation of Keynes by Modigliani, which has provided the basis for so many textbook expositions. Indeed, in this interpretation the case of downward wage and price flexibility that depresses the rate of interest until it ultimately pushes the economy into the "liquidity trap" is even singled out for designation as "the Keynesian case" (Modigliani 1944, sec. 16[A]).

More specifically, in chap. 19 of the General Theory, Keynes analyzes in detail the ways in which a decrease in the wage rate caused by the

\[11\] Again, the opening paragraph of this article—entitled "Money Wage Rates and Employment"—indicates the perspective from which the writer approaches the question: "What is the effect of a general change in money wage rates on aggregate employment and output? To this question, crucial both for theory and for policy, the answers of economists are as unsatisfactory as they are divergent. A decade of Keynesian economics has not solved the problem, but it has made clearer the assumptions concerning economic behavior on which the answer depends. In this field, perhaps even more than in other aspects of the General Theory, Keynes' contribution lies in clarifying the theoretical issues at stake rather than in providing an ultimate solution" (Tobin 1947, p. 572).

\[12\] In presenting his interpretation of Keynes, Friedman expresses his indebtedness to Leijonhufvud's book (Friedman 1970a, p. 207, n. 7). One might therefore note Leijonhufvud's view that "the most common interpretation is perhaps that, once having adopted the assumption of 'wage-rigidity' and built his model on this assumption, Keynes had little further interest in questions relating to money price flexibility. That this is a superficial explanation is apparent both from our discussion in Chapter II and from the fact that Keynes devoted a large portion of the latter half of the General Theory to these problems" (Leijonhufvud 1968, p. 332). One might also note that Leijonhufvud (1968, p. 332, n. 1) supports this view with a reference to my 1951 article, which as just noted, is also referred to by Friedman.

\[13\] For reasons elsewhere presented (Patinkin 1965, chap. 14, sec. 3), I do not agree with this identification of Keynesian economics with the "liquidity trap"—an identification which has also been followed by Friedman (1970a, pp. 206, 212 ff.) This point is further discussed in the next section.

Friedman (1970a, p. 206) identifies Keynesian economics with price rigidity as well as the "liquidity trap." Actually, however, the "liquidity trap" assumes its critical role in Keynesian economics only in the case of price flexibility.

For examples of Keynesian macroeconomic-textbook analyses of wage and price flexibility under conditions of unemployment, see Klein (1947, pp. 87–90), Dillard (1948, chap. 9), Hansen (1949, pp. 122–29), McKenna (1955, chap. 12), Dernburg and McDougall (1960, pp. 144–47), Siegel (1960, pp. 225–31), Ackley (1961, pp. 191–98, 377–93), and Shapiro (1966, pp. 477–87). These discussions refer not only to the indirect effect of a wage decrease on the demand for commodities via the rate of interest but also to the direct (Pigou or real-balance) effect. Needless to say, my own discussions of these problems have included both of these effects of wage and price flexibility—and have interpreted the difference between Keynesian and classical economics within this context (see the next section). I might, however, note that I have also analyzed the case of absolute wage and price rigidity and have—for this case—drawn the contrast between Keynes and the classics in terms of the "reversal of roles" of \( y \) and \( p \) with respect to the question as to which is constant and which is variable (Patinkin 1965, chap. 13, sec. 4, esp. p. 331).
pressure of unemployment might be expected to increase the level of employment. He argues that the main way is by the reduction in interest caused by the increase in the real quantity of money thus generated—and the consequent increase in investment. After discussing the limitations of this mechanism he concludes:

There is, therefore, no ground for the belief that a flexible wage policy is capable of maintaining a state of continuous full employment;—any more than for the belief that an open-market monetary policy is capable, unaided, of achieving this result. The economic system cannot be made self-adjusting along these lines. [Keynes 1936, p. 267]

Thus wage rigidities in this chapter are not an assumption of the analysis but the policy conclusion which Keynes reaches after investigating the results to be expected from wage flexibility.

Let me note that Friedman does state that Keynes qualified his assumption of price rigidity by assuming it to apply only to conditions of underemployment. At 'full' employment, he shifted to the quantity-theory model and asserted that all adjustment would be in price—he designated this a situation of 'true inflation.' However, Keynes paid no more than lip service to this possibility, and his disciples have done the same; so it does not misrepresent the body of his analysis largely to neglect the qualification. [Friedman 1970a, pp. 209–10]

Surely, "lip service" is hardly the term to use to describe the detailed analysis of full employment—and the consequent upward wage and price movement—which Keynes provides in the General Theory (1936, pp. 295–306; see also pp. 118–19, 171–74, 239–41, 289–91, 328). Indeed, despite the fact that he wrote during a period of mass unemployment, Keynes warns of the danger that wages will begin to rise with increasing unemployment even before full employment is reached (1936, p. 301). Furthermore, the upward flexibility of the wage rate under conditions of full employment is one that is basic to Modigliani's interpretation of Keynes (1944, pp. 189, 201–2)—and indeed to the standard textbook expositions of Keynesian economics. For obvious reasons, the case of full employment has concerned Keynesian economics much more since World War II than before. But to write today and "neglect as a qualification" the extensive Keynesian literature (particularly during World War II) on the inflationary gap—and the upward price movements it generates—is indeed to misrepresent the nature of this analysis.14

14 On Keynes's own contributions to this literature, see Klein (1947, chap. 6). Friedman (1970a, p. 211, n. 11) claims to find support for his interpretation in Holzman
I think I can best summarize Keynes's own view of the role of price—and output—variations in his system by citing the paragraph with which he ends chapter 20 ("The Employment Function") of the *General Theory*:

There is, perhaps, something a little perplexing in the apparent asymmetry between Inflation and Deflation. For whilst a deflation of effective demand below the level required for full employment will diminish employment as well as prices, an inflation of it above this level will merely affect prices. This asymmetry is, however, merely a reflection of the fact that, whilst labour is always in a position to refuse to work on a scale involving a real wage which is less than the marginal disutility of that amount of employment, it is not in a position to insist on being offered work on a scale involving a real wage which is not greater than the marginal disutility of that amount of employment. [Keynes 1936, p. 291]

Returning to the case of unemployment, let me now examine the evidence Friedman adduces in support of his interpretation of the role of price and wage rigidity in the Keynesian system. In this context Friedman writes:

Keynes embodied this assumption [of wage rigidity] in his formal model by expressing all variables in wage units, so that his formal analysis—aside from a few passing references to a situation of 'true' inflation—dealt with 'real' magnitudes, not 'nominal' magnitudes (Keynes 1936, pp. 119, 301, 303). [Friedman 1970a, p. 209]15

This passage reflects two basic and related misunderstandings. First, to "express all variables in wage units"—that is, to deflate nominal quantities by the wage rate—is surely not to assume that this unit is constant! This is clear from such passages in the *General Theory* as the following:

Consumption is obviously much more a function of (in some sense) *real* income than of money income. . . . a man's real income will rise and fall . . . with the amount of his income measured in wage-units. . . . As a first approximation, therefore, we can reasonably assume that if the wage-unit changes, the expenditure

and Bronfenbrenner's survey article (1963) in which (contends Friedman) "theories of inflation stemming from the Keynesian approach stress institutional, not monetary, factors." This contention is hardly consistent with Holzman and Bronfenbrenner's discussion (under the heading of "Demand Inflation") of "Keynesian inflation theory" in terms of the inflationary gap of the "Keynesian cross" diagram (1963, p. 53)—or with their detailed description of the literature which subsequently developed on this question (1963, pp. 55–59).

15 The references to the *General Theory* provided here by Friedman are not to passages dealing primarily with the procedure of measuring variables in terms of wage units, but to the allegedly "few passing references to a situation of . . . inflation."
on consumption corresponding to a given level of employment will, like prices, change in the same proportion. [Keynes 1936, p. 91; italics in original]

In a similar vein we find:

Unless we measure liquidity-preference in terms of wage-units rather than that of money (which is convenient in some contexts), similar results [that is, the increased demand for nominal transactions balances] follow if the increased employment ensuing on a fall in the rate of interest leads to an increase in wages, *i.e.*, to an increase in the money value of the wage-unit. [1936, p. 172; see also pp. 248–49]

As a final example, let me cite the following:

But if the quantity of money is virtually fixed, it is evident that its quantity in terms of wage-units can be indefinitely increased by a sufficient reduction in money wages. [1936, p. 266]

And to those passages can be added all those cited above in which Keynes discusses the effect of full employment—and the approach thereto—on the wage unit.

Similarly, to express a model in “real magnitudes” does not mean to assume that wages and prices are rigid or exogenously determined. It is, instead, simply to assume that there is no money illusion in the system (Patinkin 1954, 1965). Indeed, for this reason I would consider as a priori implausible any model which is not expressed in “real magnitudes.”

Furthermore, this absence of money illusion is a necessary condition for the validity of the quantity theory. Thus, both of the quantity-theory models discussed in the preceding section—Friedman’s as well as my own—are concerned solely with real variables, including real money balances. But since the nominal quantity of money is given, there is an inverse one-to-one correspondence between the level of these balances and the price level.

The major piece of additional evidence which Friedman brings in support of his interpretation of Keynesian economics is the following:

A striking illustration [of the treatment of the price level as an institutional datum] is provided in a recent Cowles Foundation Monograph, edited by Donald Hester and James Tobin, on *Financial Markets and Economic Activity* (Hester and Tobin 1967). A key essay in that book presents a comparative static analysis of the general equilibrium adjustment of stocks of assets. Yet the distinction between nominal and real magnitudes

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16 Correspondingly, my criticism of Goldfeld (1966) on this issue would be exactly the opposite of Friedman’s. Thus, compare my criticism of Arena (1963) in Patinkin (1965, p. 660) with Friedman’s (1970a, p. 211, n. 11) criticism of Goldfeld.
is not even discussed. The entire analysis is valid only on the implicit assumption that nominal prices of goods and services are completely rigid, although interest rates and real magnitudes are flexible. \[\text{Friedman 1970a, p. 211}\]^{17}

Of the five articles which Friedman cites in support of this interpretation of the Keynesian literature (1970a, p. 211, n. 11), three are not relevant to the question at issue, one is relevant but not for the reason adduced by Friedman,\(^{18}\) and only one is validly cited.

In particular, the three articles by Tobin and Brainard (1967), Brainard (1967), and Gramley and Chase (1965), all explicitly restrict themselves to an analysis of the nature of the stock (or balance-sheet, or asset-portfolio-composition) equilibrium achieved under the assumption that the situation in the market for the flow of current output is taken as given; correspondingly, the assumption of this analysis is not (as Friedman would have us believe) that prices are rigid while real income varies, but that—at the stage of the analysis being presented—both prices and the flow of current income are assumed to be held constant.\(^{19}\) In the illuminating words of

\(^{17}\) In the footnote attached to this passage, Friedman cites—as an example documenting his last sentence—Tobin and Brainard’s assumption “that central banks can determine the ratio of currency (or high-powered money) to total wealth including real assets”—and he contends that “if prices are flexible, the central bank can determine only nominal magnitudes, not such a real ratio.” In this contention, however, Friedman is not correct. For as I have shown elsewhere, even if prices are flexible, an open-market operation by the central bank will affect the rate of interest—and hence the optimum ratio of money balances to total wealth. The reason the equilibrium rate of interest is affected in such a case—as contrasted with the case in which the quantity of money changes as a result of deficit financing—is that an open-market operation causes a change in the relative quantities of financial assets (measured in real terms) in the equilibrium portfolios of individuals. Now, a change in the price level affects the real value of nominal financial assets in an equiproportionate manner and hence cannot effect such relative changes. Correspondingly, equilibrium can be restored in this case only by variations in the relative rates of return on the various assets so as to make individuals willing to hold them in their changed proportions. The price movement which simultaneously takes place does indeed dampen the extent of the changes in the equilibrium rate(s) of interest, but, for the reasons just explained, it cannot eliminate them entirely. This argument also holds for shifts in liquidity preference as well as for the introduction of financial intermediaries or of new types of financial assets. For further details, see Patinkin (1961, pp. 109–16; 1965, chap. 10, secs. 3–4, chap. 12, secs. 4–6).

\(^{18}\) I am referring here to Friedman’s criticism of Goldfeld (1966); see n. 16 above. What Friedman does not, however, point out—and what is the relevant point—is that Goldfeld’s study does not include the price level as an endogenous variable (Goldfeld 1966, p. 136). It should, however, be noted that one of the directions in which Goldfeld indicates that this model might be refined is that “the wage-price nexus might be introduced. This would bring in supply considerations and make the price level endogenous” (1966, p. 197). This point will be further discussed at the end of this section.

\(^{19}\) See Tobin and Brainard (1967, pp. 59–60), Brainard (1967, pp. 98–100), Gramley and Chase (1965, pp. 221–22). Note also the inclusion of \(Y\) as an exogenous variable in Brainard and Tobin (1968, p. 102), in addition to the assumption of a given commodity price level (1968, p. 105). The holding of both \(Y\) and \(p\) constant at this stage
Brainard, the concern of these articles can be described as being "analogous to exploring the vertical displacements of the ‘LM’ curve which result from monetary actions" (Brainard 1967, p. 99).

On the other hand, a valid criticism can be made of Brainard and Tobin (1968). For though most of this article is devoted to the kind of analysis described in the preceding paragraph, it does contain a section which provides for the endogenous determination of income but not of prices (pp. 112–13). And if it is this section that Friedman has in mind (he provides no specific page references), then he has a point—but only a point. For Brainard and Tobin themselves describe this section as a "primitive extension of the model" (1968, p. 112). A more basic extension—to include the production function and labor market—is not provided; and, as will be shown in the following discussion, it is these which play a vital role in the determination of absolute wages and prices in the Keynesian system.

I definitely agree that Tobin and his colleagues are to be criticized for not having made such an extension in these articles—for one of the primary tasks of monetary theory is indeed to explain the determination of the wage and price levels. But a far more important question in the present context is whether those Keynesian discussions that do extend the analysis to the labor market assumed (as Friedman contends they did) that the wage and price levels are exogenously determined.

I have already cited the contrary evidence of the analysis of the effects of wage and price flexibility in the theoretical discussions of Keynes and the Keynesians (see above). But, as before, it seems to me that the best way to answer such questions is to examine the empirical writings of the economists involved. In particular, let us see how Keynesian economists treated the wage and price level in their econometric models of the economy as a whole, for the methodology of model building requires the specification of the variables as endogenous or exogenous.

Of particular interest in this context is the work of Lawrence R. Klein, both because of its relative earliness and because of its being explicitly of his analysis is most clear from the methodological discussion in Tobin (1969, pp. 15–16); see also Tobin's listing of the exogenous variables in the various models he presents here (1969, pp. 21, 24, 28). As will, however, be shown in the next paragraph, these last two articles are subject to criticism on the point at issue.

However, to maintain a proper perspective on this criticism one should remember that this, after all, is the same Tobin whom Friedman himself (1970a, p. 206, n. 5) cites as being one of the first to point out the key role of the real-balance effect generated by a downward price movement in assuring the existence of a long-run equilibrium position. A characteristic of Friedman's present exposition which may partially explain his losing sight of this aspect of Tobin's work is the fact that Friedman never explicitly refers to the role of the movement of the price level in this equilibrating process; thus see Friedman (1970a, p. 206 bottom, 215).

It is also the same Tobin who—in one of his few analyses of a model with a production function and labor market—explains that the "equilibrium absolute price level" is determined at that level "that provides the appropriate amount of real wealth in liquid form" (Tobin 1955, p. 107).
motivated by the desire to provide an empirical expression of the Keynesian system. The first large model (for the United States, 1921–41) constructed by Klein (in the late 1940s) does provide some support for Friedman’s contention. For in analyzing the market as a whole, Klein assumes that—because of the lack of competition—“instead of taking price as the adjustment variable here, we take output” (Klein 1950, p. 102; see also pp. 50–57, 85). Nevertheless, the adjustment equation which Klein actually presents is one in which the change in the price level also appears as a variable (1950, p. 102). Furthermore, Klein explicitly treats this price level as an endogenous variable of the system as a whole (1950, p. 105).

In his subsequent work—in the early 1950s—Klein himself criticized the preceding model for giving “inadequate treatment to prices and wages, both absolute and real” and noted that “the postwar inflation showed this deficiency in a striking manner” (Klein and Goldberger 1955, p. 2). Correspondingly, in the model which they proceeded to construct (for the United States, 1929–52) Klein and Goldberger presented a “labor market adjustment equation” which is

the strategic equation for determining the level of absolute wages and prices in the system... The main reasoning behind this equation is that of the law of supply and demand. Money wage rates move in response to excess supply or excess demand on the labor market. High unemployment represents high excess supply, and low unemployment below customary frictional levels represents excess demand. [1955, p. 18]

Another relevant factor is the rate of change of prices, for workers take this into account when they bargain for money wages. Thus, the rate of change of the wage rate depends on the volume of unemployment and the rate of change of the price level; and the volume of unemployment, in turn, is essentially determined as the difference between the number of people in the labor force and the input of labor as endogenously determined by the production function. Needless to say, both the wage rate and the price level are endogenous variables of this model. (Klein and Goldberger 1955, pp. 17, 34–35, 37, 41, 52.)

This theory of wage determination has characterized all of Klein’s later work. In the revised version of the Klein-Goldberger model, much the same wage-adjustment equation is associated with the Phillips curve (Klein 1966, p. 239). In the subsequent Wharton model there is a further elaboration on the wage equation, as well as the introduction of a mark-up equation (which also reflects the demand situation in the market) to explain the movements of the price level (Evans, Klein, and Schink 1967, pp. 33–36). And this is carried over to the Brookings Model as well (Duesenberry, Klein, et al. 1965, pp. 284–85, 311). Thus in all of these
models the wage rate and price level continue to be treated as endogenous variables.

That this is true not only of Klein's work can be seen most easily from the tabular survey of macroeconometric models prepared by Nerlove (1966). Of the twenty-five models there described (including the preceding four) the great majority provide for the endogenous determination of the wage and price levels.

I can most easily summarize the findings of this section by noting that they show how misleading is Friedman's contention that initially, the set of forces determining prices was treated [by Keynesian economics] as not being incorporated in any formal body of economic analysis. More recently, the developments symbolized by the 'Phillips curve' reflect attempts to bring the determination of prices back into the body of economic analysis, to establish a link between real magnitudes and the rate at which prices change from their initial historically determined level (Phillips 1958). [Friedman 1970a, p. 220]

First of all, an economic analysis of wage movements was already provided by the General Theory. Indeed, the Phillips-curve theory itself is foreshadowed in chapters 19 and 21 of this book. Second, 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. And this has continued to be the case.

One final observation should be made. It has already been indicated in this section that Keynesian economics is concerned with disequilibrium states, with the principal market in disequilibrium being that for labor. Correspondingly, in the Keynesian system—and particularly in the econometric expressions thereof—there is no equilibrium equation for the labor market but rather a dynamic wage-adjustment equation determining the rate of change of the nominal wage rate in response to the state of excess supply in this market. And, as we have seen, it is this equation which plays a vital role in the endogenous determination of nominal wages and prices.

It is, therefore, not surprising that an equilibrium model, without a labor market—and this is the nature of Friedman's model—does not reveal the nature of the endogenous dynamic process by which the time paths of the nominal wage rate and price level have been analyzed in the Keynesian literature.

IV. Concluding Remarks

The standard interpretation of Keynesian economics as developed by Hicks, Modigliani, and Hansen presents as its central message—and basic
differentia from classical economics—the possible existence of a position of "unemployment equilibrium." Correspondingly—in order to explain how the level of unemployment remains unchanged in such a position—this interpretation assigns a crucial role to the "liquidity trap." For it is this "trap" that keeps constant the rate of interest, hence the level of aggregate demand, and hence the levels of output and employment in the economy.

Friedman follows this standard textbook interpretation in presenting the "trap"—or, to use (as Friedman does) Keynes's term, the case of "absolute liquidity preference"—as part of "Keynes's basic challenge to the reigning theory" (Friedman 1970a, pp. 206, 212 ff.). All this, it might be noted, is in contrast with Keynes's own statement that "whilst this limiting case might become practically important in the future," he knew "of no example of it hitherto" (1936, p. 207).

An alternative interpretation that I have elsewhere developed—and of which I have made use in the preceding section—presents Keynesian economics as the economics of unemployment disequilibrium. More specifically, the fundamental issue raised by Keynesian economics is the stability of the dynamic system: its ability to return automatically to full-employment equilibrium within a reasonable time (say, a year) if it is subjected to the customary shocks and disturbances of a peacetime economy. In this context Keynesian economics contends that as a result of high interest elasticity of the demand for money and low interest elasticity of investment, on the one hand, and distribution and expectation effects, on the other, the automatic adjustment process of the market—even when aided by a monetary policy that pushes the rate on interest down—is unlikely to converge either smoothly or rapidly to the full-employment equilibrium position. And since this interpretation thus frees Keynesian economics from the confines of an equilibrium system, it also frees it from any logical dependence on the existence of a "liquidity trap."

In brief, even if monetary policy could be depended upon to ultimately restore the economy to full employment, there would still remain the

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21 Once again (see n. 12 above) Friedman (1970a, p. 207, n. 7) cites "Leijonhufvud's penetrating analysis" in support of his (Friedman's) view—even though Leijonhufvud's actual position is exactly the opposite! Thus Leijonhufvud writes: "The 'Liquidity Trap' notion is anti-Keynesian not only in that Keynes explicitly rejected the idea that the money-demand function would be perfectly interest-elastic within any range that we would possibly be interested in, but also in its neglect of the downward shift of the entire schedule that, in a continuing state of depression, 'at long last . . . will doubtless come by itself.' Cf. Treatise, loc. cit." (1968, p. 202, n. 26). Similarly, on pp. 160-61 Leijonhufvud rejects interpretations of Keynes that are based on the "liquidity trap."

22 This contrast—as well as the passage from Keynes just cited in the text—is further discussed in Patinkin (1965, pp. 349, 352-54, esp. n. 29). This passage is also referred to by Friedman (1970a, p. 215).

23 The following three paragraphs draw freely on the discussions in Patinkin (1948 and 1951, sec. 14; 1965, chap. 14 and suppl. n. K:3).
crucial question of the length of time it would need. There would still remain the very real possibility that it would necessitate subjecting the economy to an intolerably long period of dynamic adjustment: a period during which wages, prices, and interest would continue to fall, and—what is most important—a period during which varying numbers of workers would continue to suffer from involuntary unemployment. Though I am not aware that he expressed himself in this way, this is the essence of Keynes's position. This is all that need be established in order to justify his fundamental policy conclusion that the "self-adjusting quality of the economic system"—even when reinforced by central-bank policy—is not enough, and that resort must also be had to fiscal policy.

Thus this interpretation takes the debate on the degree of government intervention necessary for a practicable full-employment policy—which is the basic policy debate between Keynes and the classics—out of the realm of those questions that can be decided by a priori considerations of internal consistency and logical validity, and into the realm of those questions that can be decided only by empirical considerations of the actual magnitudes of the relevant economic parameters.

Friedman can undoubtedly point to passages in his article which agree with this last sentence (for example, 1970a, p. 234). The trouble is that there are many more passages in which he presents quite a different interpretation of the relations between Keynes and the classics. It is these other passages which constitute the major part of his article—and to which, accordingly, my own has been devoted.

I would like to conclude this paper with one observation of an analytical nature on the dynamic equations which Friedman presents in his paper (1970a, p. 224). These, unfortunately, are not the structural equations that one might have expected from Friedman's opening statement that the purpose of this paper is to present the theoretical framework implicit in his and Anna Schwartz's book on A Monetary History of the United States (1963b). Instead, they are essentially reduced-form equations. The coefficients of these equations are undoubtedly dependent on the elasticities of the structural equations, as well as on their respective speed-of-adjustment parameters. But since Friedman does not specify the nature of this dependence, his dynamic equations do not enable us to investigate what is, after all, one of the basic questions at issue: namely, the way in which—in a given policy context—different assumptions about

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24 Nor, correspondingly, does Friedman's present discussion provide any additional details about the admittedly "tentative" dynamic analysis which he sketched in Friedman and Meiselman (1963, pp. 217–22) and Friedman and Schwartz (1963a, sec. 3). The main point of this analysis is that monetary changes initially generate portfolio-composition (balance-sheet) adjustments, and hence changes in the prices (and hence rates of return) of the assets (including consumer durables) held in the portfolio; these, in turn, generate changes in the demands for commodity flows and hence in their prices and/or output.
the various elasticities of demand and dynamic parameters affect the respective time paths of price and output in the system.

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