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Labels and Substance: Friedman's Restatement of the Quantity Theory

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Milton Friedman opened his 1956 "Restatement" of the quantity theory by claiming that his article and the four other essays in *Studies in the Quantity Theory of Money* were part of a distinct oral tradition of the quantity theory at the University of Chicago. His claim created a historical controversy that continues even now, forty years later. It has involved Friedman's fellow University of Chicago alumnus Don Patinkin (1969, 1973), his Chicago colleague Harry Johnson (1971), and former Chicago student David Laidler (1993, 1998a, 1998b), as well as Thomas Humphrey (1971) and George Tavlas (1997, 1998).[1] The subjects of this protracted debate have been the appropriateness of Friedman's use of the label *quantity theory* for his monetary economics and the accuracy of placing his quantity theory in the Chicago "oral tradition." Commentators have built their case for or against Friedman's claims by comparing Friedman's restatement of the quantity theory with the analytical approach of earlier Chicago economists, or with Keynesian liquidity preference theory, or with monetary economics at

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[1] Frank Steindl (1995) has a somewhat different focus. He compares earlier monetary interpretations of the Great Depression with that of Friedman and Anna J. Schwartz in chapter seven of their *Monetary History* (1963).

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Harvard prior to the Keynesian revolution. Likewise, they have compared Friedman's policy advice to prior recommendations offered from Chicago and elsewhere.

In these comparisons, most of the historical work has been centered on the others rather than on Friedman, for example, depictions of Chicago economics in the 1930s or 1940s or of Harvard. This article redirects this attention to Friedman and the roots of his monetary economics research program. I will examine the background for his "Restatement" in his monetary economics research, his writings in other areas as they bear on his monetary economics, and his teaching prior to 1956. My aim is to see how the 1956 article fit into Friedman's research and teaching programs as they evolved up to his writing the "Restatement." This will illuminate the substance of Friedman's monetary economics, providing a more substantial factual foundation than Friedman or his critics have used to classify his work through the 1950s.

Claims and Counterclaims

Friedman made his claim about the quantity theory and Chicago in the introduction to a collection of essays written by students in his Workshop in Money and Banking, which at the time was an innovative laboratory for teaching and research. Thus the immediate context for his claim included both his research and his teaching. He said that the quantity theory was an approach rather than a well-defined theory; that its content varied widely from one time and person to another; and that the quantity theory was beginning to reemerge from the disrepute into which it fell after the stock market crash and Great Depression. He saw the quantity theory as an alternative to the Keynesian income-expenditure approach. Friedman claimed that the University of Chicago was one of the few places where the quantity theory had not been discredited. There students continued to study and write theses on monetary economics through the 1930s and 1940s. Under Lloyd Mints and Henry Simons the quantity theory

> was not a rigid system, an unchangeable orthodoxy, but a way of looking at things. It was a theoretical approach that insisted that money does matter—that any interpretation of short-term movements in economic activity is likely to be seriously at fault if it neglects monetary changes and repercussions and if it leaves unex-

> plained why people are willing to hold the particular nominal quantity of money in existence. (Friedman 1956, 3)

Friedman then presented a model of the demand for money that he suggested would give the "flavor" of the Chicago oral tradition and that served as an introduction to his students' chapters. He followed the model with a suggestion of three features that might be used to identify someone as a quantity theorist. Quantity theorists would regard the money-demand function as stable relative to other functions that might be alternatives for explaining short-term behavior of income. They would also believe that the supply of money is not wholly determined by money demand. And they would not regard money demand as infinitely elastic at some low interest rate. He thought these were empirical propositions and their importance was in supporting monetary analysis's usefulness for explaining the behavior of income. Because he did not regard them as a set of fixed and complete propositions about money demand or money's role in the determination of income, Friedman's emphasis was on the quantity theory as a theoretical approach, an engine of analysis. He was attempting to counter Keynesian arguments that demand for money is erratic or unpredictable (a will-o'-the-wisp) and that there is a liquidity trap. Keynesians used these arguments to deny the usefulness of monetary analysis for studying business cycles.

Several years after Friedman's "Restatement" Patinkin (1969) challenged him on two doctrinal issues: the identification of his money-demand model as a restatement of the quantity theory, and his depiction of the model as an outgrowth of the Chicago oral tradition. Patinkin argued that with its emphasis on stocks of assets, portfolio composition, and rates of return, Friedman's approach was actually a development of Keynesian liquidity preference theory, not the quantity theory. He also found no antecedents for this approach in the Chicago oral tradition. At Chicago as he knew it the quantity theory was viewed not as a theory of demand for money but as a theory relating the quantity of money to aggregate demand via Irving Fisher's version of the equation of exchange, as opposed to Friedman's use of the Cambridge cash-balances approach. Far from considering velocity a stable function of a small set of variables, Chicago economists regarded velocity as subject to sharp changes that could become cumulative and proceed unchecked. Because of this, they assigned to the central bank responsibility for offsetting destabilizing changes in velocity, while Friedman advocated a money-stock growth-rate rule.

After exchanges between Patinkin and Friedman over the Chicago roots of his work the matter lay mostly dormant until Laidler took up the question of the uniqueness of the Chicago tradition in 1993 (see also 1998a, 1998b). Laidler argued that two of three components of the Chicago tradition agreed upon by Friedman and Patinkin are also found in the work of Harvard economists Allyn Young and Lauchlin Currie. Like their counterparts at Chicago, Young and Currie interpreted economic fluctuations as monetary phenomena. They also shared with their Chicago colleagues optimism about the power of monetary policy. They did not share the Chicago preference for monetary-policy rules, but Currie developed independently a 100 percent money proposal similar to that of Henry Simons (1933). According to Laidler's account, the theoretical roots of these shared views on money and business cycles were in the work of the British economist Ralph Hawtrey. The Harvard and Chicago economists' indebtedness to Hawtrey for their theory set them apart from the American economics community, which was heavily influenced by Fisher. Laidler argued that the distinguishing mark of Chicago monetary economics was the tie established between monetary rules and a liberal policy regime. At the conclusion of his 1993 article Laidler turned to Friedman and his claims about Chicago. He concluded that Patinkin was correct in arguing that Friedman's money-demand model displays a "deep and unmistakable Keynesian influence" (1993, 1100).

Tavlas (1997, 1998) maintained that there was a distinct tradition at Chicago prior to 1936, setting that academic community apart from Harvard and other American economics departments and immunizing Chicago economists from the Keynesian virus. He concentrated on heretofore neglected writings in monetary economics by Paul Douglas and Aaron Director between 1927 and 1933, contending that Douglas was an important contributor to a Chicago tradition of monetary economics that was in place prior to the publication of the *General Theory*. In Tavlas's account the tradition had the following characteristics: (1) use of Fisher's MV=PT quantity theory framework; (2) belief that autonomous variations in velocity can be cumulative and can initiate economic fluctuations; (3) belief that banking practices tend to exacerbate fluctuations; and (4) advocacy of countercyclical changes in the money supply accomplished through the government's budget during recessions, with the budget balanced over the cycle.

Tavlas also reexamined evidence in the writings of other Chicago economists, Harvard economists, and others including Hawtrey, giving special attention to the timing of their ideas. He concluded that, contrary to Laidler's argument, before 1936 Chicago had a deeper quantity theory tradition than Harvard, and that this tradition owed relatively little to Hawtrey. Thus, as Friedman argued, Chicago was uniquely immune to the "Keynesian virus" that struck Harvard and other American universities. Also, Tavlas concluded that although the pre–*General Theory* Chicago tradition differed from some of Friedman's views (e.g., on the question of whether the monetary policy instrument makes a difference in its power), the earlier Chicago tradition served as a foundation for his launching the monetarist counterrevolution.

From this debate, beginning with Patinkin and going up to Tavlas and Laidler, we have a fuller and clearer view of the development of monetary economics at Chicago and Harvard from the late 1920s into the Keynesian era. But where does it leave us in terms of understanding the historical context of Friedman's 1956 article? Because the focus has been mostly away from Friedman, I suggest that we are left with little more than was started with. None of the authors, including Friedman ([1972] 1974) in his response to Patinkin, has given sufficient attention to the circumstances from which his piece arose. These circumstances are the most immediate historical context for the article. Their recovery is necessary for determining which labels most closely match the substance of Friedman's monetary economics circa 1956.

Circumstances and Context: Friedman's Background

Friedman's "Restatement" was the introduction to a collection of papers written by Chicago graduate students in his Workshop in Money and Banking. Friedman began planning for the workshop in the 1951–52 academic year and got fully under way in 1954, when the department obtained funding from the Rockefeller Foundation. The University of Chicago and the workshop were one of two settings for Friedman's research and writings in monetary economics. The National Bureau of Economic Research was the other. In 1948 Arthur Burns asked Friedman to become director of a National Bureau study of monetary factors in business cycles. In that year, Friedman began the work with Anna J. Schwartz as his collaborator, and from then into the early 1980s a major portion of Friedman's research and writing on monetary economics was done within the National Bureau program. His writings on monetary economics prior to 1956, most of which are directly related to the National Bureau project, are important evidence of what he thought were the important issues in monetary economics and how he treated them.[2]

In addition, some of Friedman's writings in areas other than monetary economics reveal interests and inclinations that gave shape to his monetary economics. Of particular relevance are writings on economic methodology, because the period in which he embarked on his long career as a monetary economist was also the period of his most intense interest in and engagement with issues of methodology. Friedman's Marshallian methodology informed his approach to monetary economics. Also, his work on consumption studies, which began in the mid-1930s, predated his entry into monetary economics, and his most important contribution in this field, *A Theory of the Consumption Function* (1957), was published the year after his restatement of the quantity theory. That work with its development of the permanent income hypothesis was also part of the backdrop for his monetary economics.

Records of courses that Friedman taught at Chicago and elsewhere prior to 1956 are another source of evidence. In his first quarter at Chicago in the autumn of 1946 he began teaching Economics 230, the undergraduate course in money and banking. In the spring quarter of 1951 he offered a new graduate seminar, Economics 432—Monetary Dynamics. Before coming to Chicago, Friedman taught a course in business cycles at the University of Wisconsin (1940–41) and proposed one during his year at the University of Minnesota (1945–46).

Friedman's Writings: Monetary Economics

Several items that Friedman wrote are especially revealing of his theoretical inclinations at the time he began work on the National Bureau "money in business cycles" project. Friedman told Schwartz in April

[2] For more extensive discussion of the origins of Friedman's National Bureau "money in business cycles" project see Hammond 1996, chap. 3.

1948 that she could best see his thinking on theoretical matters—such as the role of banks in the transmission process of monetary effects and the role of government obligations as net wealth—by looking at the paper he wrote for the 1947 meeting of the Econometric Society, "A Monetary and Fiscal Framework for Economic Stability" (1948a). There Friedman proposed a scheme to eliminate private creation and destruction of money in the banking system and discretionary control by the Federal Reserve, by requiring 100 percent reserves behind bank deposits and total monetization of federal budget surpluses and deficits. The federal budget would be balanced over the cycle with monetary policy and fiscal policy folded together and guided by a rule rather than discretion. Friedman's proposal was similar to the 1933 Chicago proposal written by Simons.

Friedman evaluated his proposal against the status quo in terms of how it would perform in the presence of price rigidities and lags in adjustment, which he considered two fundamental problems in business cycles. He thought there was little confirmed knowledge of the length of adjustment lags, which he broke down for analytical purposes into the "recognition lag," "action lag," and "effect lag."[3] It is instructive to ask how Friedman came to view the issue as so important. On this matter there is one small but telling hint in his article, a footnote reference to his (1947) review of Abba Lerner's *Economics of Control*. Friedman criticized Lerner's "functional finance" approach to maintenance of adequate aggregate demand as being an exhortation to do the right thing without means for determining what the right thing is. He quoted Burns and Wesley Mitchell (1946, 7) on the timing of cycles:

> Our examination of business indexes, and less definitely of business annals, forbade us to think of business cycles "as sweeping smoothly upward from depressions to a single peak of prosperity and then declining steadily to a new trough." On the contrary, the expansion and contraction of many cycles seem to be interrupted by movements in the opposite direction, and some cycles apparently have double or triple peaks or troughs. (Friedman 1947, 314)

Friedman himself was a staff member at the National Bureau and had studied under Burns and Mitchell at Rutgers and Columbia. The

[3] Estimation of monetary policy lags was to become an important and controversial part of Friedman and Schwartz's project. See Friedman 1959, 1961, and Hammond 1996, chaps. 5 and 7.

National Bureau method consisted in large part in measuring cycles and dating their turning points. So given his own background and the evidence compiled in Burns and Mitchell's *Measuring Business Cycles* (1946) and in Mitchell's (1913, 1927) business-cycle volumes, it is not surprising that Friedman would consider adjustment lags important and have a definite opinion about how much was known of the lags.[4] There is no hint in his article of how Friedman came to believe that inflexible prices were a key element in generating and transmitting business cycles, but this was a view that was held at the University of Chicago as far back as the early 1930s (see Simons 1933).

It is not incidental to Friedman's thinking at the time that Burns (see 1946, 1947, and 1952, 23–24) was a critic of Keynesian doctrine as it developed through the 1940s. Burns, Friedman's mentor and close friend, regarded the Keynesian theoretical apparatus as an unobjectionable and useful filing system, much like Marshall's demand and supply. But he regarded Keynesian theory of income and employment as untested and of dubious value for understanding short-term fluctuations in income and employment. The stability of a simple consumption function, and the independence of this function and of the level of investment from the process of adjustment, were core Keynesian propositions that Burns argued had not been adequately tested. Keynesians such as Alvin Hansen moved swiftly to use the theory to support policy remedies for "secular stagnation." But in doing so, they took these and other untested propositions (e.g., the exhaustion of profitable investment opportunities) on faith.

Friedman suggested to Schwartz as they began their work that one of their first tasks should be to compile a data series on government obligations held by individuals, businesses, and private banks. He explained that he wanted to shift the theoretical focus from business production and investment decisions to household consumption decisions. When Schwartz asked for clarification of what he had in mind about transmission mechanisms he referred her to his discussion of the Pigou effect in the 1948 article. There he cites Pigou (1943, 1947) and Patinkin (1948) in explaining how a decline in the price level with no change in the nominal stock of money increases the net wealth held in government-issued money and other government debt, thereby increasing the propensity to consume. Shortly thereafter he outlined

[4] See also Friedman's (1950) review of Mitchell's contributions to economic theory.

the approach he had in mind in a memorandum for the National Bureau:

> Broadly speaking, there are two main channels through which monetary and banking phenomena affect economic activity. The channel that has received major attention is the effect of ease or stringency of credit, and of confidence in or distrust of the currency on the production and investment decisions of businessmen. A much less noticed channel, though one that has recently received increased attention, is the effect of the supply of money or money substitutes, and changes in the supply, on consumption decisions. The portion of individual assets corresponding to the net obligations of the government to the public has the peculiar characteristic that its real value can rise or fall as a result of price changes without involving a corresponding fall or rise in the real value of the assets of other individuals.
>
> The exact point at which to begin the study is rather arbitrary, since the problem must ultimately be treated as one whole. For a variety of reasons, the present intention is to begin with an examination of those aspects of monetary and banking phenomena that might shed greatest light on the consumption effect. (Friedman papers, "Brief Statement of Plan for Study of Monetary Factors in Business Cycles," 1–2)

The point of the Pigou effect in the work of Pigou and Patinkin was to offer a counter to Keynesian underconsumption theory, or what Pigou (1947) called "Lord Keynes's Day of Judgment." There is irony here inasmuch as the dispute that developed later between Patinkin and Friedman had to do with Patinkin's claim that Friedman's monetary theory was Keynesian. But Patinkin himself provided one of the first theoretical concepts that Friedman would use in his monetary critique of Keynesian economics. Friedman's preference for starting with the consumption effect is notable in other respects. It was a natural preference given his experience working on consumption studies for the National Resources Committee in the 1930s. It fit with his focus on wealth rather than measured income. And it aligned with Burns's critique of Keynesians for blithe use of an untested consumption function. Friedman's preference was an early indication of how his approach would depart from the interest rate and investment approach of the Keynesians.[5]

[5] Hirsch and De Marchi 1990, chap. 9, provides a cogent explanation of the origins and evolution of Friedman's opposition to Keynesianism.

Friedman and Schwartz selected data for their study on the basis of three theoretical perspectives on money's role in business cycles: (1) "the supply and rate of use of generally acceptable means of payment"; (2) "the supply of assets easily marketable at virtually fixed nominal prices," that is, money's role as an asset; and (3) the role of banks as lenders and investors. Friedman (Papers, box 94) explained in a 1948 memorandum outlining the preliminary plan for compiling data that he considered the first the obvious starting point for analysis, because every highly developed economy has a means of payment to settle debts incurred in the normal course of exchange. However, he thought that the delineation of assets that are generally acceptable was necessarily arbitrary to an extent. And he thought interchangeability among the assets might be as important a factor in business cycles as the total quantity of means of payment, especially in a banking system with differential reserve requirements. He suggested that although economists had given great attention to technical determinants of velocity, these were surface reflections of a more fundamental factor: money's role as an asset. During severe monetary disorders technical factors were swamped by factors related to money as an asset, so that there was practically no limit to velocity of circulation.

So in Friedman's view consideration of money's role as a means of payment led naturally to consideration of its role as an asset. He thought the asset function would turn out to be the more important category for organizing data. The Pigou effect worked through money's service as an asset. In the memorandum Friedman's explanation of the primacy of money's role as an asset was that any object used as a means of payment must necessarily be held as an asset, since the very nature of a monetary economy involves the temporal separation of receipt and payment. "The use of the circulating medium as an asset imposed by this 'transactions demand' shades imperceptibly into its use, deliberately and with forethought, as a form in which to keep some part of the community's assets" (Papers, box 94, 4).

And just as the means-of-payment function shades into the asset function, the asset function of the means of payment shades into the asset function of items that are not means of payment. So from the beginning of the project Friedman had a predilection to define money more broadly than was conventional. "It is clear that there is no sharp dividing line between 'money' and 'near-moneys' or between 'near-moneys' and 'securities proper.' It is also clear that cyclical movements in economic activity will alter the relative desirability of the circulating medium as an asset, and that this will in turn react upon and affect the character of the cyclical movement" (5).

In early 1949 Friedman (Papers, "outline of Work in First Phase of Banking Study," box 94) reported on the status of the project he was three-quarters of a year into. Preliminary investigation of the data in hand indicated that the quantity of the circulating medium responded sluggishly to cyclical movements, reacting to major movements with a considerable lag. The data also suggested that velocity moved procyclically. Friedman outlined their chief objectives for that phase of the study as testing their tentative conclusions, making more precise measurements of cyclical patterns and leads and lags, and checking for differences in the cyclical patterns of different components of the money stock. By Friedman's account, these objectives were the same as Mitchell's in chapter 6 of *Business Cycles* (1913).[6]

This sample of Friedman's writings on monetary economics before his restatement of the quantity theory reveals that for Friedman monetary economics was a vehicle for treating the problem of business cycles. This included explaining cycles and designing countercyclical policy. It also shows that for technique Friedman integrated monetary theory with National Bureau business-cycle techniques. "Discussion of the Inflationary Gap" (1942) and "A Monetary and Fiscal Framework for Economic Stability" (1948) were his *only* writings on monetary economics prior to becoming director of the National Bureau "money in business cycles" project. If we were to separate the National Bureau cycle and monetary analyses that Friedman combined, and designate one as primary and the other as secondary, the case is as strong for giving the primary designation to National Bureau cycle analysis as to monetary analysis. It is also clear from his writings throughout the

[6] In a 1951 application for funding for the project, Friedman (Papers, box 80, 2–3) reported similar findings: "the tentative hypothesis suggested to me by the evidence is that the monetary system has played a very different role in the minor cycles and the severe cycles. The reactions in the minor cycles appear to be essentially passive responses to changes occurring elsewhere. I do not mean the monetary reactions have had no influence but simply that they have not been major initiating factors. On the other hand, in the major cycles, there is evidence that monetary reactions have played an active and important role. Put differently, monetary changes may well have been the primary factor responsible for converting mild movements into extreme movements—to mention a particular case. I think there is very good reason to believe that the great depression might have ended in late 1931 or early 1932 if it had not been for the monetary action taken by the Federal Reserve System in the fall of 1931."

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period leading up to *Studies in the Quantity Theory of Money* that he regarded the coupling of National Bureau methods with monetary theory as a superior alternative to Keynesian income-expenditure theory (see, e.g., 1952).

Friedman's Writings: Methodology

Friedman became interested in economic methodology in the early 1940s when Edward Chamberlin and Robert Triffin offered monopolistic competition as a Walrasian alternative that was superior to Alfred Marshall's industry analysis. He published a short review (1941) of Triffin's *Monopolistic Competition and General Equilibrium Theory* that was the first of numerous defenses of Marshall's economics and methodology (and criticisms of the Walrasian alternatives) that he would make over a fifteen-year period in the 1940s and 1950s.[7] Over this period Walrasian general-equilibrium theory and methodology swept through most of the economics profession save the Marshallian enclave that Friedman and George Stigler fought to preserve (see Stigler 1949).

Triffin (1941) argued in his Harvard Ph.D. dissertation that a marriage of Walrasian general-equilibrium theory and monopolistic competition, which would involve dropping the Marshallian concept of an industry, represented progress by purifying and formalizing economics. Friedman answered in the review that this would limit the capacity for using economics to deal with real-world problems, because important real-world problems relate to industries. He later echoed this sentiment in lecture notes for his Economics 300a price theory course at Chicago: "Marshall's 'Principles' viewed contemporaneously, i.e., as if he were writing today instead of a century ago, is still the best book available in economic theory. This is indeed a sad commentary on the economics of our time. Marshall's superiority is explained primarily by his approach to economics as contrasted with the modern approach" (Friedman Papers, box 76, 1).

Friedman made explicit use of the Marshallian and Walrasian categories in subsequent writings such as "The Marshallian Demand Curve" (1949) and "Leon Walras and His Economic System" (1955). He made the same distinctions, without the labels, in "Lange on Price Flexibility and Employment" (1946), "Lerner on the Economics of

[7] For more extensive discussion of Friedman's Marshallian methodology see Hammond 1996, chap. 2.

Control" (1947), and his most famous article on methodology, "The Methodology of Positive Economics" (1953c). His point was always to emphasize the role of economic theory in the analysis of concrete facts, in contrast with the prevailing emphasis on "abstractness, generality, and mathematical elegance" (1949, 91), and to show the superiority of the Marshallian "engine of analysis" compared with Walrasian theory.

Friedman's Teaching: Business Cycles

The importance of Friedman's National Bureau association is confirmed by the evidence we have on courses he taught early in his career. In his 1940–41 course on business cycles (Economics 176) at the University of Wisconsin, the primary texts were Mitchell's *Business Cycles* (1913), *Business Cycles: The Problem and Its Setting* (1927), and a draft manuscript of Burns and Mitchell's *Measuring Business Cycles* (1946). The lecture notes and tests that are available in the Friedman Papers make no mention of Keynes, though Friedman devoted considerable attention to underconsumption theories. They consist mostly of lecture notes on Mitchell's books. Friedman stressed Mitchell's "essential point" that business cycles are monetary phenomena; they arise in societies after the use of money has reached an advanced state He also discussed Mitchell's (1927, 128–39) section on the quantity theory and business cycles, which stressed the importance of timing of prices and transactions. Friedman asked his students to consider whether the Great Depression represented a structural shift in business cycles, and indicated that his tentative judgment was that it did not.

Friedman's course was in the style of the business-cycle courses that Mitchell taught at Columbia. It gave the students hands-on experience, with each of them working on a time series for a commodity or other indicator. They considered measurement problems such as how to distinguish true from false turning points and how to translate data on specific cycles into a measure of the "business cycle." Another similarity between Friedman's course and Mitchell's was an emphasis on the phenomenon of the business cycle rather than on named theories. In the final exam he gave on 30 January 1941 not a single economist or economist's theory is referenced by name (Friedman Papers box 75).[8]

[8] One of four questions made direct reference to money and calls to mind Friedman's 1948 article: "One device proposed for mitigating the severity of cyclical fluctuations is the concentration of governmental expenditures on public works in depressions, the extra expenditures at such times to be financed from surpluses accumulated during prioi periods of pros-

Friedman's Teaching: Monetary Economics

Friedman taught his first course in monetary economics when he joined the University of Chicago faculty in autumn 1946. It was the undergraduate money and banking course, Economics 230. Then, in the spring quarter 1951, he added a graduate seminar in monetary dynamics, Economics 432. These two courses constituted his classroom teaching of monetary economics prior to writing the "Restatement." In addition, Friedman began planning the Workshop in Money and Banking as a monetary-economics laboratory for students and faculty in the 1951–52 academic year. The workshop got fully under way in 1954 with a grant from the Rockefeller Foundation.

Friedman used two textbooks for Economics 230 in the 1940s and early 1950s, H. L. Reed's *Money, Currency, and Banking* (1942) and L. V. Chandler's *Economics of Money and Banking* (1948). Supplementary readings included Dennis Robertson's *Money* ([1922] 1929), T. E. Gregory's *Gold Standard and Its Future* (1932), C. O. Hardy's *Credit Policies of the Federal Reserve System* (1932), Keynes's "Alternative Aims in Monetary Policy" (chapter 4 of *A Tract on Monetary Reform* ([1923] 1971), R. S. Sayers's *Modern Banking* ([1938] 1939), Mints's *Monetary Policy for a Competitive Society* (1950), E. A. Goldenweiser's *American Monetary Policy* (1951), and the American Economic Association's *Readings in Monetary Theory* (1951).

The Economics 230 course outline for autumn 1946 indicates that Friedman's lectures followed closely Reed's *Money, Currency, and Banking* (Papers, box 76). He opened the course with a discussion of wartime use of cigarettes as money. This was followed by lectures on functions of the monetary system and the composition of money as deposits and currency. This led to analysis of the money creation process, with first a single bank and then multiple banks, and with one or both types of money, and to problems of monetary control when the public shifts between the two forms of money. He considered 100 percent and fractional reserve systems. A major part of the course dealt

perity, from borrowing, from the creation of new money, or from current taxes. Under what conditions, if any, would you recommend the adoption of this proposal? If adopted, what method of financing would you endorse? Justify your answers in detail. Discuss both the merits and demerits of the proposal and of the various methods of financing. Discuss the observed features of cyclical fluctuations that make the proposal appear strategically desirable or undesirable."

with the international monetary system, especially the gold standard, but also the newly established Bretton Woods system. Two lectures were devoted to the meaning and usefulness of the transactions version of the equation of exchange. There was a substantial amount of coverage of history of monetary systems and policy, including the pre– and post–Federal Reserve eras in the United States, and British experiences with war finance and the gold standard during and after World War I. The final two weeks of the course were devoted to problems and policies for monetary control in the United States, especially with regard to inflation, and to Keynesian theory of income determination.

Friedman may have chosen Reed's *Money, Currency, and Banking* because Mints used it for his sections of the course. He stayed with this book for a couple of years, then switched to Chandler's *Economics of Money and Banking* in 1948 or 1949. Reed's book was published in 1942 and did not come out in a second edition; Chandler's was freshly published in 1948. However, Friedman probably found Reed's book more to his liking than Chandler's. Reed's book looks like one that Friedman himself might have written, if he had written a money and banking text. There is more of Reed in his book than there is of Chandler in his, in the same way that there is a great deal of Friedman in everything he has written. Chandler's book looks like many of the "balanced" textbooks that dominate the market today. Reed, like Friedman, was less hesitant to make judgments about the relative merits of alternative theories and institutions. Also, a number of Reed's judgments align remarkably closely with those that have become familiar in Friedman's writings.[9]

For instance, Reed argued that since demand deposits are not literally "on the wing" but are idle most of the time, sharp distinctions between these deposits and time deposits are arbitrary insofar as the monetary aggregate is concerned. He also favored the cash-balances version of the quantity theory equation over the transactions version for much the same reason that Pigou argued for its superiority in "The Value of Money" (1917–18). Reed thought the cash-balances version brought the quantity theory closer to value theory and directed attention to individuals' attitudes and decisions. He also thought it led more directly to empirical investigation of determinants of the terms of the equation.

[9] It is clear from the *Monetary History* that Friedman held Reed's work other than the textbook in high regard. See Friedman and Schwartz 1963, 341.

There is a striking parallel between Friedman's ideas and Reed's criticism of Keynesian theory.[10] Reed commented that there were no obvious theoretical flaws in the doctrine, but insisted that the choice between Keynesian and traditional theories of business cycles should be based on results of inductive investigation. These investigations were not yet available. So Reed was sharply critical of economists who embraced the secular-stagnation thesis, concluding that permanent budget deficits were necessary, without waiting for the empirical evidence. He argued that there should be careful evaluation of the risks associated with policy mistakes before adopting a new Keynesian regime of permanent fiscal stimulus.

Friedman (1946) criticized Oscar Lange for casual empiricism when he reviewed Lange's *Price Flexibility and Employment* (1944) shortly before he joined the Chicago faculty. He maintained that Lange simply asserted facts without evidence, just as Reed charged Keynesians with asserting underconsumption equilibrium without facts. Friedman's (1948, 1953a) concern to minimize damage from policy mistakes—both direct damage from misdiagnoses and collateral damage from the politics of government interventions, very much like Reed's concern with underconsumption Keynesianism—is prominent in two papers he wrote early in his career at Chicago.

The graduate seminar on monetary dynamics (Economics 432) was set up so that students had a substantial role in determining the topics. The seminar was a precursor to the Workshop in Money and Banking, which Friedman initiated at virtually the same time. Several students who were in the seminar the first two times Friedman taught it became prominent monetary economists or Friedman's coauthors. The rosters included David Fand, Charles Holt, George Horwich, Rueben Kessel, David Meiselman, and Richard Selden (spring quarter 1951), and Steven Axilrod, Gary Becker, John Deaver, John Klein, and Richard Timberlake (spring quarter 1952).

Friedman gave the students a sketch of the central topic and a list of readings. Then, individually or in groups, the students selected narrower topics, compiled reading lists, and prepared papers for discussion by the entire class. In the spring quarter 1952 the central topic was monetary inflation. Friedman's description of the topic was as follows:

[10] Reed's criticism is also similar to that of Burns.

> We hope to cover both the theory of monetary inflation and empirical evidence on monetary inflations. The major issues in this area are, the process whereby changes in the stock of money produce their effect on prices and output or conversely, whereby changes in prices and output affect the stock of money; the role of the interest rate in inflation or, conversely, the effect of monetary changes on the interest rate; the role of exchange rates in monetary inflation as both cause and effect; the relative value of alternative simplified theories for predicting the course of an inflationary movement; the role and problems of governmental monetary policy in inflationary periods; empirical regularities in monetary inflations and hyperinflations (Friedman Papers, box 78)

Conclusion

This investigation into the background of Friedman's 1956 "Restatement" of the quantity theory suggests three key elements of his monetary economics when he wrote the article: his National Bureau associations and interest in business cycles, his allegiance to Marshallian methodology and value theory, and his use of the Cambridge cash-balances approach. These stand in stark relief from an intellectual landscape dominated by Keynesian income-expenditure theory. The first element of Friedman's monetary economics is one that is not accounted for in the labels debate set off by Friedman's article. The roots of Friedman's approach to monetary economics run deep into his associations with Mitchell, Burns, and the National Bureau. In terms of sheer magnitude of exposure, Friedman had much more experience with National Bureau business-cycle analysis than he had with monetary analysis when he began the "money in business cycles" project with Schwartz in 1948. As a senior undergraduate in 1931–32 he took a year-long course in business cycles from Burns at Rutgers and two years later took Mitchell's two-semester sequence of courses on business cycles as a graduate student at Columbia. He remained personally and professionally close to Burns through the 1950s. Much has been made of Chicagoans' reactions to Keynes in the 1930s, but, without excluding the Chicago economists as part of the background, we have seen that Friedman's National Bureau connections are a likely proximate source of his emerging criticism of Keynesian doctrine in the 1940s.

In a 1940 survey Friedman (Papers, box 5) listed his "broad interests" as "statistics" and "economic theory." He also indicated more specific interests including:

> 1. business cycles—empirical analysis with particular reference to money and capital markets.
>
> 2. capital theory—closely related to point (1); this is the least satisfactory portion of economic theory primarily, in my view, because it cannot be dealt with adequately on the level of static analysis, but must be treated in connection with cyclical fluctuations and long run dynamic changes.
>
> 3. statistical analysis of family budget data.

These three interests from 1940 were later manifested in his monetary economics, particularly in the "Restatement." There he identified the demand for money by business enterprises as "a special topic in the theory of capital," and demand for money by households as "formally identical with that of the demand for a consumption service" (1956, 4). His purpose in specifying a theoretical money-demand function was to provide the "engine of analysis" for statistical study of the relationships between money, income, and prices.

Friedman's allegiance to Marshallian methodology and value theory and his use of the Cambridge cash-balances approach are closely related. Shortly after compiling his 1940 list of interests Friedman read Triffin's *Monopolistic Competition and General Equilibrium Theory* and wrote the review that began his efforts to defend and preserve Marshallian methodology and theory. In microeconomics the objects of this struggle became monopolistic competition versus Marshall's industry analysis, and the Marshallian demand curve versus what Friedman interpreted as a Walrasian demand curve.[11] In monetary analysis of business cycles Friedman coupled Marshallian methodology, the Cambridge cash-balances version of the quantity theory, and National Bureau methods to criticize the ascendant Walrasian (and Keynesian) program being developed by the Cowles Commission in the late 1940s and early 1950s.[12]

[11] He wrote that with the conventional interpretation of Marshall's curve economists "curtsy to Marshall, but . . . walk with Walras" (1949, 89).

[12] This coupling had deep roots going back to Friedman's undergraduate days at Rutgers, for it was there under Burns's tutelage that Friedman was introduced to both National Bureau techniques and Marshall's opus. Burns (1951, viii), himself a devotee of Marshall, wrote after

Friedman's identification of the quantity theory as a general approach rather than a well-defined theory in the "Restatement" echoed Pigou's sense of the quantity theory. Pigou (1917–18, 162) wrote that "the 'quantity theory' is often defended and opposed as tho [*sic*] it were a definite set of propositions that must be either true or false. But in fact the formulae employed in the exposition of that theory are merely devices for enabling us to bring together in an orderly way the principal causes by which the value of money is determined." Pigou went on to argue that there is no need for controversy over the "truth" of the quantity theory, for it is "merely machinery." But he regarded the cash-balances equation as a more effective machinery than Fisher's equation of exchange. Both, he concluded, should be viewed as equations of demand, although it would be a mistake not to investigate supply and the interrelations between demand and supply.

Pigou's interpretation of the quantity theory as machinery might appear to be a departure from what Marshall had to say of the quantity theory. Marshall ([1923] 1960, 48) used the term *quantity doctrine* in *Money, Credit, and Commerce* for "the now familiar doctrine that the value of a unit of currency varies, other things being equal, inversely with the number of the units and their average rapidity of circulation." In "Evidence before the Indian Currency Commission," he used the designation "so-called 'quantity theory of the value of money'" for the same proposition ([1899] 1926, 267). This may look like a "definite proposition" of the type for which Pigou said the quantity theory was often mistaken, but only until one looks closely. Marshall did not say that the value of money always varies inversely with its quantity, or with its rapidity of circulation; it would do so only under the condition that other things were equal. And crucially, for Marshall, specifying the quantity theory in this way was only the beginning of the economist's work. The important task was to discover the causes of the "other things" that may or may not remain equal. Thus for Marshall, as for his student Pigou, the quantity theory was a machinery for investigating causes of the purchasing power of money.[13] Friedman and his students

Mitchell's death that "Mitchell's economic outlook was thoroughly Marshallian. Had he lived to finish this book [*What Happens during Business Cycles*], he would have inscribed on its title page Marshall's motto: 'The many in the one, the one in the many.'"

[13] D. H. Robertson ([1922] 1929, chap. 2), another of Marshall's students, followed in Marshall's and Pigou's footsteps with regard to the quantity theory, although Robertson was

in the Workshop in Money and Banking set out to discover the causes of the "other things."[14]

The Cambridge cash-balances approach was a version of the quantity theory when Marshall and Pigou developed it, and Friedman's restatement of the quantity theory as a theory of money demand was squarely within the Cambridge quantity-theory tradition. Keynes, of course, came from that tradition as well. But when Friedman embarked on his monetary economics programs of research and teaching in the middle 1940s, Keynesian doctrine had developed into an analytical approach and set of ideas that were substantially different from the quantity theory of Marshall, Pigou, and Robertson. The Keynesian trappings of the income-expenditure approach and underemployment hypothesis, developed within a Walrasian framework by Keynesians at the Cowles Commission and elsewhere, were sufficient reason for Friedman to object to Patinkin's labeling his monetary analysis "Keynesian." Patinkin and Friedman's dispute was not about family resemblances of money-demand models in narrow terms, but about Keynesian baggage.

But what are we to conclude about the University of Chicago tradition in monetary economics? I will not try to settle this issue here, since it depends on evidence from the history of monetary economics at Chicago, a great quantity of which is in the articles that I briefly summarized. This article is about Milton Friedman, and the evidence presented here about Friedman's writing and teaching prior to 1956 sheds little light on the Chicago question. What is indicated however, is that an assessment of the Chicago roots of Friedman's monetary economics

less definite than Pigou in his preference for either of the two versions. He wrote that the strength of the Cambridge "money sitting" version was that it aided in discerning the psychological forces determining the value of money. Fisher's "money on the wing" approach was better for watching the actual processes through which prices change.

[14] In a letter to Jacob Marschak dated 23 October 1953 regarding deliberations with other members of Phillip Cagan's thesis committee, Friedman (Papers, box 22) wrote: "I do however believe that both Gregg [Lewis] and Earl [Hamilton] under-rate Chapter II. It is certainly true, as Earl says, that everyone understands that attempts to reduce cash balances have been a factor in hyper inflations; and also, as Gregg says, that Chapter II is a summary of the familiar cash balances approach to the quantity theory. But I believe, on the first point, that everyone has not been clear about the variables determining the level of desired cash balances, and, indeed, it has frequently been maintained that such attempts cannot be explained by the quantity theory itself but must be interpreted as exogenous. On the second point, I believe Gregg will be hard put to point to an existing explicit statement suited to Cagan's problem."

should take account of the three benchmarks of his work: National Bureau business cycle methods, Marshallian methodology and value theory, and the Cambridge cash-balances version of the quantity theory. These three characteristics marked *Studies in the Quantity Theory of Money* and remained thereafter as important substance of the Friedman-era Chicago tradition in monetary economics.

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