Two Types of Monetarism

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I. *Introduction*

THE 1970s witnessed the rise of two fashionable macroeconomic schools of thought—monetarism and the so-called "new classical" macroeconomics, the latter usually closely identified with one of its fundamental components, the rational expectations hypothesis. Both schools trace their ancestry to older economic doctrines, but it is just in the last decade that they have moved into the mainstream of post-war macroeconomics.

The increasing importance of monetarist and new classical thinking naturally resulted in attempts to define each doctrine and to classify disparate thinkers as adherents or opponents. Monetarism has been surveyed, inter alia, in Thomas Mayer (1978), Jerome Stein (1976), Douglas Purvis (1980), David Laidler (1981, 1982), and James Meade (1981). Despite these attempts no universally accepted definition of monetarism and, hence, no universally accepted classification of who is and who is not a monetarist has emerged. The reason is, of course, that any economist is described most fully by a vector of characteristics. Any definition emphasizes some of the elements of this vector, while playing down related ones. Another definition may take the related ones to be more important. The borders of such categories are bound to be poorly defined.

Although the definitions may be arbitrary, the attempts to formulate them are not sterile. Individual monetarists may bear only a family resemblance to one another; nevertheless, the effort of classifying them yields clarification as a by-product.

The new classical economics has not been surveyed in quite the same spirit as monetarism. The emphasis has been on its relationship to other distinct doctrines, rather than on who is or is not a new classical (Brian Kantor, 1979; Stein, 1982). Indeed, the question of whether or not the new classicals are monetarists has loomed large in these discussions. James Tobin (1980, 1981) calls the new classicals "monetarists mark II." He bases the title on the similarity of the two schools' policy prescriptions. Frank Hahn (1980) agrees that they are monetarists, but bases the judgment on the similarity of their theoretical presuppositions. Laidler (1981, 1982), on the other hand, finds that they are not

monetarists, but rather more closely re-
lated to the Austrian school (Kantor,
1979). He rejects Hahn's classification as
misleading, because it is precisely theoret-
ical differences which, he believes, sepa-
rate the new classicals from the monetar-
ists.

In this essay, I do not want to enter the
debate over titles—"monetarist" or not
"monetarist." I do want to *clarify* the rela-
tionship between some sorts of monetar-
ism and the new classical school. To do
this I will compare Milton Friedman, in-
disputably a monetarist, with Robert Lu-
cas, Thomas Sargent, Neil Wallace and
others, as representative of the new classi-
cal school. The principal theme of the es-
say is that, although we may wish to clas-
sify the new classicals as monetarists (for
Tobin's or for Hahn's reasons) or we may
not (for Laidler's reasons), Friedman, as
one important monetarist, differs from the
new classicals on a fundamental point of
methodology: he is a Marshallian; they are
Walrasians.

Part II attempts to define the new classi-
cal economics and to clarify its relation
to the rational expectations hypothesis.
Part III explores the practical and theoret-
ical relations between Friedman and the
new classicals that give plausibility to To-
bin's title, "monetarist mark II," and inter-
est to the debate over its aptness. Part
IV introduces the fundamental methodo-
logical distinction between Friedman and
the new classicals. Part V shows how this
distinction underlies and accounts for the
differences between Friedman's and the
new classical views on equilibrium and dy-
namics. Finally, Part VI summarizes the
argument.

II. *The New Classical Economics Defined*

In view of the difficulties of definition
and classification mentioned in the Intro-
duction, it may seem odd to represent the
older monetarism by the views of even

its most eminent proponent, Friedman,
by himself, while letting several econo-
mists' views present the new classical doc-
trine. One reason for doing so is that parts
of Friedman's work are a foundation for
much of the new classical doctrine (espe-
cially Friedman 1968; Lucas 1972a,
1981b). Another reason is that Friedman
has contributed to economic methodol-
ogy, as well as to monetarist thought. As
a result, since the principal distinction be-
tween Friedman and the new classicals
examined in Parts IV and V is method-
ological, Friedman's writings give us an
explicit formulation from which to work.

Part of the imbalance can be set right
by defining the new classical economics.
It has rarely, if ever, been explicitly de-
fined by its adherents. The territory, how-
ever, has been surveyed (Sargent, 1979,
1982; Michael Beenstock, 1980; Willem
Buiter, 1980). And the subsidiary element,
rational expectations, has been exten-
sively covered (Robert Shiller, 1978; Da-
vid Begg, 1981; Lucas and Sargent, 1981;
Rodney Maddock and Michael Carter,
1982).

Three tenets are keys to the new classi-
cal doctrine. First, agents' real economic
decisions—for example, about savings,
consumption or investment—are based
solely on real, not nominal or monetary
factors. Second, agents are, to the limits
of their information, consistent and suc-
cessful optimizers; i.e., they are continu-
ously in equilibrium. Third, agents make
no *systematic* errors in evaluating the eco-
nomic environment: i.e., they hold ra-
tional expectations (Lucas, 1977; Sargent,
1979, Ch. 16).

The rational expectations hypothesis is
perhaps the most striking feature of the
new classical doctrine. The universally ac-
cepted formulation is due to John Muth
(1961, p. 316): "Expectations . . . tend to
be distributed, for the same information
set, about the prediction of the theory (or
the 'objective' probability distribution of

outcomes)."[1] Muth claims that this hypothesis involves three assertions: first, information is scarce and the economic system does not waste it; second, expectations are formed from the specific structure of the relevant system describing the economy; and, third, public predictions can have no substantial effects unless there is "inside" information; i.e., a (true) economic forecast does not give anyone a special opportunity to profit from it if it is known to everyone.

Muth's notion of an information set can be taken broadly, but if taken too broadly it is of little use. Typically the information set is defined to include all the exogenous variables, all past values of endogenous variables and, crucially, the structure of the model (i.e., Muth's "relevant system"). This amounts to claiming that the model adequately captures those features of the world relevant to the formation of expectations, and that agents act *as if* they know the model when forming their expectations.

While the rational expectations hypothesis is a fundamental part of the new classical economics, it is, nevertheless, independent of the other tenets. A new classical economist *necessarily* believes in rational expectations. But a belief in rational expectations by itself is *not sufficient* for one to be a new classical. In his survey of the new classical macroeconomics, Sargent (1982) characterizes it as going beyond ad hoc supply and demand curves. He argues that, in order to explain the behavior of macroeconomic aggregates, we must go back to the underlying objective functions and the constraints that agents face. In a static framework, this requires the first two tenets of the new classical doctrine— that only real phenomena count and that

agents are consistent and successful optimizers.

The importance of the rational expectations hypothesis is to carry these features over to the dynamic problem. If agents are to optimize over their future behavior, their expectations of the future are bound to be important. Rational expectations implies that what they *do* expect is (within a serially uncorrelated error) what the true model says they *should* expect. This guarantees that they will be consistent and successful.

Not everyone who uses the rational expectations hypothesis should be classified as new classical. The principle of rational expectations can be employed by anyone seeking a convenient and, in some sense, neutral way of introducing endogenous expectations into an economic model. One may adhere to the rational expectations hypothesis, yet violate the other tenets by, for example, holding that prices are not flexible or that agents do not optimize (Stanley Fischer, 1977; Edmund Phelps and John Taylor, 1977; Tobin, 1980). Fischer, for example, constructs a model in which long-term wage contracts produce nominal wage rigidity, which in turn permits monetary policy to have real effects, since real wages then depend on price movements. This violates the first tenet of the definition of new classical economics. Lucas (1981b) criticizes Fischer's model for supposing wage rigidity, rather than explaining it as the outcome of an optimizing decision consistent with the second tenet (Lucas and Sargent, 1979).

III. *Friedmanian Roots, New Classical Conclusions*

The new classical economics grew up as a response to the perceived failure of modern-day Keynesian macroeconomics, particularly as a result of the apparent breakdown of the Phillips curve—ever

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[1] Muth's paper presents rational expectations in a supply/demand system, aimed at overthrowing the "cobweb theorem" analysis. Recently, however, rational expectations figure most prominently in macroeconomics.

higher inflation at times of historically high unemployment (Lucas and Sargent, 1979). Friedman's work (1956a, 1959a, 1968) suggested that inflation was associated most closely with changes in the supply of money; that unemployment was a response to the real wage, and as soon as agents grasped that inflation erodes their real wage, they would press for compensating rises in the money wage. The Phillips curve tradeoff between unemployment and the rate of increase of wages would thus prove to be a will-o'-the-wisp. These insights were the starting place for the new classical analysis, and the new classicals openly acknowledge their debt to Friedman (Lucas, 1977, 1981a, 1981b).

On the other hand, until Friedman's recent publication (with Anna J. Schwartz, 1982) of *Monetary Trends in the United States and the United Kingdom,* it has been difficult to know how he regarded the new classicals (although, see the earlier Friedman, 1977, 1978). It is clear from this latest work that he sees rational expectations as a potentially useful modeling technique, but stops short of embracing the new classical research program. The rest of this part examines how the new classicals begin with Friedman's insights and reach similar conclusions, but offer a different analysis along the way.

3.1 *Neutrality, the Natural Rate of Unemployment and the Phillips Curve*

The old distinction between real and nominal quantities is the most fundamental element in Friedman's rehabilitation of the quantity theory of money (1956a, 1969b, 1974b; Friedman and Schwartz, 1982). We observed in Part II, that it was one of the defining characteristics of the new classical school as well.

This distinction enables Friedman to formulate a theory of inflation. The government in conjunction with the commercial banks determines the *nominal stock of money.* The public in response to real factors such as (real) income or (real) interest rates determines the *real demand for money.* The price level adjusts to equate the nominal stock to the real demand. Given a stable real demand for money, a continuing rise in prices thus requires a continuing increase in the nominal stock of money. This view of the inflation process is captured in most new classical models (Sargent, 1976; Sargent and Wallace, 1976).

Friedman's theory of the inflation process implies that money is neutral, but only in the longer run. Initially an increase in the stock of money swells real balances at existing prices. It is only when these are spent, stocks of goods run down and production temporarily increased that prices rise. The new classicals go further: money is neutral in the shorter run as well (Lucas, 1972b). Rational expectations implies that agents understand the connection between money and the price level and that they correctly anticipate the systematic components of government monetary policy. Continuous optimization implies that they use this information to discriminate between nominal and real changes. Only real changes affect their real decisions. Actual shorter-run non-neutralities are the result of random shocks, say in government monetary policy, which could not be anticipated. Their effect disappears as soon as it is realized that they have occurred.

Both versions of the neutrality thesis are closely related to the notion of a natural rate of unemployment. Full employment in the sense used above is not an absolute, technical limit. Rather, it is the equilibrium optimizing choice of economic agents. Friedman (1968, p. 8) writes: "the 'natural rate of unemployment'. . . is the level that would be ground out by the Walrasian system of general equilibrium

equations, provided there is embedded in them the actual structural characteristics of the labor and commodity markets. . . ." He also recognizes a natural real rate of interest and, presumably, a natural rate of output as well. This notion of a natural rate of unemployment accords well with the tenets of new classicism (Lucas, 1972a, 1973; Sargent, 1973).[2]

The importance of the natural rate of unemployment and the neutrality of money is clearly seen in the expectations-augmented Phillips curve (Friedman, 1968; also, Phelps, 1967, 1968). Friedman argues that a sudden monetary expansion temporarily expands output and depresses the rate of unemployment below the natural rate. It also produces an acceleration in the rate of inflation. Prices rise faster than wages, so that the real wage is cut. Observing that the volume of employment is greater than they would wish at this real wage, workers endeavor to restore real wages to their old level, and the actual rate of unemployment rises toward the natural rate. Prices (i.e., wages) here respond to quantity (i.e., employment) signals.

The Phillips curve in this version cannot be exploited in the long run, for once workers anticipate any constant rate of inflation, they adjust their wage claims in line with it. Nevertheless, there is a real shorter-run tradeoff between unemployment and inflation.

As with the neutrality of money, the new classicals deny even this shorter-run tradeoff. They recast the Phillips curve as the so-called "Lucas short-run aggregate supply function" (Lucas, 1972a; Sargent and Wallace, 1976; Laidler, 1981, 1982). Aggregate supply in this presentation is positively related to the deviation of actual from expected prices (or wages) as

each producer (or worker) at first perceives (falsely) a general price (wage) rise as a favorable shift of relative prices.

The aggregate supply version of the Phillips curve reverses Friedman's adjustment mechanism: quantities (labor or output) respond to price signals. The shift in relative prices is only *apparent* not *actual*. No appeal is made to wage stickiness. As with the new classical analysis of neutrality, agents are continuously in equilibrium given their information. Rational expectations guarantee that, except for random shocks, this information is correct. Hence, an inverse relation of unemployment to inflation is observed, but cannot be exploited deliberately and consistently, even in the shorter run, because it reflects the unsystematic or random component of the relation between changes in individual workers' wage levels (or producers' prices) and the general rate of inflation.

In later work, Friedman sometimes offers an aggregate supply explanation of the Phillips curve (1974a; Friedman and Schwartz, 1982).[3] Nevertheless, he does not take over the full new classical position. He does not, as the new classicals do, rule out prices adjusting to quantities. Indeed, his adjustment functions for both prices and real income have actual and anticipated income as arguments (Friedman, 1974b; Friedman and Schwartz, 1982, Ch. 2). Furthermore, in *Monetary Trends*, as in his presidential address to the American Economic Association (1968), he still maintains that expectations of inflation are slow to develop (Friedman and Schwartz, 1982, Ch. 10). The process has sped up over the past two decades, but it still takes years, not weeks or months, for expectations of inflation to adjust fully to actual changes.

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[2] It is shared presupposition of the natural rate as the outcome of a Walrasian system that suggests to Hahn (1971, 1980, 1982) that Friedman and the new classicals should be grouped together under the title "monetarists."

[3] Robert J. Gordon (1981) ascribes the aggregate supply explanation to Friedman, failing to distinguish between Friedman (1968, 1974b). Laidler (1981, 1982) correctly notes the shift. Friedman does not, however, adopt an aggregate supply version to the exclusion of his earlier explanation.

3.2 *Government Policies: Rules versus Discretion*

Following Henry Simons (1936), Friedman (1948, 1959b, 1974a) has long preferred rules over discretion or authorities in the conduct of monetary policy (and other policies as well—e.g., tax and expenditure policies.)[4] His advocacy of fixed rules is based on the authorities' ignorance of the sources and timing of economic disturbances. Discretionary policies may or may not be perverse or adverse on average, but they do increase noise in the economic system, which interferes with prices acting as efficient signals (Friedman, 1961, 1977).

Lucas (1980b, 1981b) comes down squarely in Friedman's camp, acknowledging the same intellectual debt. Furthermore, the signal extraction problem in the context of rational expectations has been developed with great theoretical nicety by the new classical school (Sargent, 1979, Ch. 5). Sargent and Wallace (1976, p. 169) declare somewhat tendentiously: "there is no longer any serious debate about whether monetary policy should be conducted according to rules or discretion."[5]

Friedman has advocated a particular rule for monetary policy: namely, that money should be allowed to grow at a constant X percent per year, where X is determined to equal the secular rate of GNP growth after allowing for secular changes in the velocity of circulation of the money stock. Authorities should not use money in an attempt to offset cyclical movements in economic activity. This rule is related to Friedman's belief that expectations of inflation are slow to develop. He believes that holding the growth of the money stock to a constant X percent rate will result in a steady (and, if X is low enough, zero) rate of inflation over the longer run. Once agents have adjusted to this steady rate, no important and unpleasant surprises await them as long as the rule continues to be followed.

Friedman's X percent rule receives weak support from the new classical school (Sargent and Wallace, 1975; Lucas, 1981b). Their argument is derived from their belief, examined in Section 3.1, that monetary policy cannot *systematically* trade off inflation for output or employment. At a formal level any determinate (i.e., nonrandom) rule will have no real effects, because agents with rational expectations will understand the rule and not be fooled by changes in the money stock into moving away from the natural rate of unemployment or output. Random policies may have real effects since agents cannot anticipate them; but, of course, they cannot be systematically pursued; and, hence, are hardly policies at all.

It is, then, the determinateness or nonrandomness of Friedman's rule that the new classicals support, not its particular, simple form. Yet the new classical argument is that agents act *as if* they know the structure of the economy. Of course, they must infer most features, including money supply rules, from past experience. Simple rules may be easier to infer and, therefore, may be preferred (Lucas, 1980b).

3.3 *A Paradox: Empirical Agreement, Theoretical Difference*

Friedman (1974b) argues that what separates monetarists from Keynesians are differences of empirical judgment, not of theoretical principle.[6] His relation to

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[4] "Discretion" is now the more usual term, but "authorities" is the more venerable, having been coined by Simons (1936).

[5] For a contrary view, see Buiter (1980) and Stephen Goldfeld (1982). Sargent and Wallace (1981) note a circumstance under which Friedman's rule may not be an appropriate policy for securing a lower rate of inflation. This possible objection, nevertheless, does not alter the new classicals' support for rules over discretion in general.

[6] Friedman's view of the matter is controversial. For supporters, see, for example, Mayer (1978); for an opponent see, for example, Hahn (1971).

the new classicals appears to be just the reverse: their empirical judgments are broadly similar, while their theoretical paths to those judgments are, at times, strikingly different.

There are, of course, important theoretical similarities—e.g., the supposition of a natural rate of unemployment. But it is the similarity of their policy prescriptions which leads many to classify the new classicals as "monetarist." And it is the radical nature of those prescriptions which leads observers such as Tobin to distinguish them from older monetarists by labels like "mark II."

It is commonly believed that the rational expectations hypothesis is the source of new classical radicalism. But this is only partly true. The examples in the two preceding sections show that the characteristic new classical conclusions require both the other tenets of their doctrine—only real quantities matter for real decisions and continuous optimization.

Indeed, Friedman (and Schwartz, 1982, Ch. 9) takes the rational expectations hypothesis to be equivalent to the proposition that agents make no consistent mistakes about real variables *in the long run.* Later Friedman writes:

> The formalization in the theory of rational expectations of the ancient idea that economic actors use available information intelligently in judging future possibilities is an important and valuable development. But it is not the open sesame to unraveling the riddle of dynamic change that some of its more enthusiastic proponents make it out to be [Ch. 12 p. 630].

This quotation gets to the nub of the matter. The new classicals wish to analyze economic dynamics using the rational expectations hypothesis. To do this they must collapse the long run into the short run, appealing to the other tenets of the new classical doctrine. Friedman wishes to analyze economic dynamics by retaining the Marshallian distinction between the market, short and long periods. We

will examine this difference in approach more closely in Part V. Before that, however, we must examine the even more fundamental distinction between Friedman's Marshallian incrementalism and the new classicals' Walrasian globalism.

IV. *Walrasian and Marshallian Economics*

Friedman's most famous and controversial contribution to economic methodology is his essay, "The Methodology of Positive Economics" (1953). Also well known, if less frequently discussed, is the earlier essay "The Marshallian Demand Curve" (1949). Here Friedman introduces the key distinction that separates him, as it turns out, from the new classicals. The first section of this part states and illustrates Friedman's distinction. The relation between the two methodological essays is, of course, not the main theme. Nevertheless, Friedman's views on positive economics are too important to be avoided. The second section explores some aspects of that relation. The third section discusses the new classicals as Walrasians. The final section reexamines the three defining tenets of the new classical doctrine in light of the methodological distinction developed in the first three sections.

4.1 *The Cournot Problem*

Friedman (1949) argues that the common view that Marshall deals with partial equilibrium, while Walras deals with general equilibrium is false. Rather both deal with general equilibrium. Partial equilibrium must be conceived of as but a special case of general equilibrium. Marshall, too, is an advocate of the view that in economics everything depends on everything else.

In place of this common and, he says, erroneous distinction between Marshall and Walras, Friedman proposes another. He argues that Marshall and Walras con-

ceive of economic theory differently. To Marshall it is but "an engine for the discovery of concrete truth," as well as "substantive hypotheses, based on factual evidence about the 'manner of the action of causes'" (Friedman, 1949, p. 490). Counting equations and unknowns and so forth is useful only as a check on the consistency of reasoning.

In contrast to Marshall's view of economic theory, Friedman argues that Walras and the Walrasians see theory as a comprehensive, formal structure, to be judged on the one hand by its abstractness, generality and mathematical elegance, and on the other hand, by the accuracy of its assumptions as a photographic description and not by the correctness of its predictions.[7]

A Walrasian theory cannot be used as a tool because its comprehensiveness prevents one from focusing on a manageable bit of reality, ignoring or summarizing those parts whose influence on the problem at hand is small. For example, the Walrasian observes that the demand for a good depends upon the price of every good. The Marshallian replies that for practical purposes the prices of all but a few related goods can be summarized in the general price level. Similarly, the Walrasian abolishes the useful notion of an industry, because the products of each firm, no matter how much alike, are not identical.

Friedman (1955) makes his distinction

[7] Friedman does not seem to perceive the implicit contradiction between a theory being completely general and photographically exact. Generality permits numerous possibilities; a photograph presents just one of them.

There is another sense of "general"—namely, when one theory is a special case of, or is nested in, another (e.g., Einstein's theory is general with respect to Newton's). And the more general may be photographically exact. I am grateful to an anonymous referee for reminding me of this second sense. Nevertheless, I believe that, taken in context, Friedman uses "general" in the first sense of open to various instantiations.

clearer in a review of the English translation of Walras' *Elements of Pure Economics.* He observes that the problem of economic interdependence has been neatly summarized by Augustin Cournot:

> So far we have studied how, for each commodity by itself the law of demand in connection with the conditions of production of that commodity, determines the price of it and regulates the income of its producers. We considered as given and invariable the prices of other commodities and the incomes of other producers; but in reality the economic system is a whole of which all the parts are connected and react on each other. An increase in the income of the producers of commodity *A* will affect the demand for commodities *B*, *C*, etc., and the incomes of their producers, and, by its reaction, will involve a change in the demand for commodity *A*. It seems, therefore, as if, for a complete and rigorous solution of the problems relative to some parts of the economic system, it were indispensable to take the entire system into consideration. But this would surpass the powers of mathematical analysis and of our practical methods of calculation, even if the values of all the constants could be assigned to them numerically [1927 (1838), p. 127].

For Friedman, Cournot's problem is, given economic interdependence, how to cope with economic analysis using *practical* methods. Walras' achievement, on the other hand, is to solve the different problem of showing, as Cournot does not, what the rigorous solution to the problem of economic interdependence would look like *in principle.* "His problem is the problem of form, not of [empirical] content: of displaying an idealized picture of the economic system, not of constructing an engine for analyzing concrete problems" (Friedman, 1955, p. 904).

Marshall's method is a response to Cournot's problem. It attempts to keep an investigation manageable by examining one problem at a time. It can be illustrated by specific examples of Friedman's work. Friedman generally employs single equation methods in his empirical work. Hence, although he claims as his theoretical framework (1974b) a multiequation

macromodel that includes a money supply equation as well as a money demand equation, in his empirical work he estimates only money demand, making the practical judgment that the money supply is too unstable to model and may be treated as exogenous (Friedman 1959a). Similarly, he holds that the whole range of implicit returns on assets might affect money demand or consumption, but these are reduced in his applied work to portmanteau variables or ignored altogether (Friedman 1956a, b, 1959a). Likewise, he estimates expectations or permanent values of variables by distributed lags on their own actual past values, even though they may in principle depend in part on the present and expected future values of the dependent variables of the equations in which they appear as independent variables (Friedman 1956b, for the example of permanent income in the consumption function). It is not that Friedman *must* use this method in every particular case. It is just that only a practical advantage of another method, say rational expectations, in generating successful predictions would obligate him to abandon it.

4.2 *Positive Economics*

In drawing the distinction between the Marshallian and Walrasian approaches, Friedman makes much of his belief, developed at length in his "The Methodology of Positive Economics" (1953), that success at prediction is the sole criterion for judging theories. Predictive success is the standard of efficacy by which the engines of economic analysis are to be evaluated.

The question of the appropriate standard of efficacy is, nevertheless, separable from the Marshallian response to the Cournot problem. Even if there were—contrary to Friedman's view—another standard of efficacy for the empirical, policy problems that interest him, it would not affect his analysis of the Cournot prob-

lem. For the point of the Cournot problem and Marshall's (and Friedman's) solution to it is that, whatever the economic problem, any practically significant analysis of it requires that reality be partitioned. The most important bits with respect to the problem at hand are analyzed in detail; the rest are summarized in less detail (but not forgotten, of course).

A Marshallian approach does, however, rule out one sort of criticism of theories. It is pointless to attack a theory as "unrealistic" or "incomplete" solely on the grounds that it partitions reality: all useful theories must do so.

The question naturally arises about how this defense of "unrealism" relates to Friedman's (1953) famous claim—the so-called F-twist—that the less realistic a theory is, the more useful it is. There is considerable debate about what Friedman means by "unrealism," and it is not to our purpose to try to decide the matter here. It is enough to note that if "unrealism" means nothing more than partitioned, then Friedman's advocacy of unrealistic theories follows naturally from his Marshallian method. Of course, the Marshallian method requires an *appropriate* degree of partition, not necessarily the greatest possible degree. So if this is what Friedman means by "unrealism," the F-twist may be an exaggerated claim. If "unrealism" means something more than partitioned, then Friedman's claim must be supported on other grounds. A supporter of the Marshallian method might then reject the F-twist. (On Friedman's positivism and the F-twist, see Lawrence Boland, 1979, 1982.)

4.3 *The New Classical Economics' Walrasian Solution*

The desirability of predictive success is not at issue between Friedman and the new classicals. Again and again they praise predictive success as a standard of efficacy (Lucas, 1977, 1981b; Muth, 1961). Oddly

enough, the importance of practical tractability implicit in the Marshallian approach is also not at issue, in some respects. Lucas' preference for quantities adjusting to prices over prices adjusting to quantities is based on the fact that the theory of the former is better developed and easier to handle than the theory of the latter (1977). Lucas accepts that theoretical techniques develop over time and, consequently, that it is misguided to criticize our forerunners for not developing theories which would have been intractable, given the then-available techniques (1980a).

On the surface, then, Friedman and the new classicals agree about the desirability of predictive success and analytical tractability. Friedman, however, anticipated this essentially false concord. "Most modern theorists would accept [the Marshallian view] of the objectives of economic theory. But our work belies our professions" (Friedman, 1949, p. 490). Friedman is, in his own view, primarily an empirical economist, who uses a few deeply held principles to sift through facts in search of predictions. The "new classicals" recognize the challenge of the facts, but see the problem more as one of consistently reconciling the facts with their world view.

The new classicals are Walrasians in two senses. First, they advocate general equilibrium analysis (Lucas, 1980a). Friedman admires the Walrasian system for ". . . its beauty, its grandeur, its architectonic structure . . . ," but he does not expect to obtain useful predictions from it (1955, p. 905). The new classicals do. Lucas (1977) argues that, given business cycles in the data, the theorist's challenge is to reconcile them with general equilibrium theories.

The second sense in which the new classicals are Walrasian is obviously related to the first: partition is a legitimate ground on which to criticize an applied theory.

Sargent and Wallace (1975) strongly criticize their own and Keynesian macromodels for the lack of consistent assumptions about firms' and individuals' objective functions. Friedman, while believing in rational optimizing man to a first approximation, could not sustain that sort of objection if the model yielded accurate predictions, whatever its theoretical assumptions.[8]

An explicit insistence on a general equilibrium foundation is the basis for the famous "Lucas critique" of macroeconometric models (Lucas, 1976; Lucas and Sargent, 1979). Expectations terms cannot be observed. So in most econometric models they are replaced by some function of the observable terms upon which expectations are formed. The rational expectations hypothesis implies that the structure of the model itself is part of the information upon which expectations are formed. If some part of the structure changes, say, the money supply rule, then the proxy function for the price expectations term will itself change, even if the underlying behavioral relations remain fixed. A model based on the old function will no longer forecast well. (A particularly straightforward illustration of this argument can be found in Thomas Turner and Charles Whiteman, 1981.)

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[8] There is a third sense of "Walrasian" which *cannot* be ascribed to the new classicals. This is perhaps the most common but not the most useful, sense indicating those who pursue *purely mathematical* general equilibrium theory. E. Roy Weintraub (1983, p. 37) concludes his historical review of the literature on the existence of equilibrium: "The [Walrasian] 'equilibrium' story is one in which empirical work, ideas of facts and falsifications, played no role at all." The Austrian precursors of the new classicals were sceptical of empirical economics (Friedrich A. Hayek, 1933, Ch. 1; 1979, Ch. 4 and 6). The new classicals themselves are sceptical of empirical economics that does not pay sufficient heed to general equilibrium but not of empirical economics in general (Lucas, 1976; Sargent, 1982). The acceptance of the importance of empirical economics may be the one thing that distinguishes the new classicals most clearly from the Austrians.

The new classical alternative to conventional, i.e., structurally noninvariant, econometric models is to posit the model with its expectational terms in place. These are then solved out using the rational expectations hypothesis in such a way that the interrelationships between the coefficients of different equations become explicit. Now when a policy rule changes, the appropriate changes to the expectations function and, therefore, to the solutions of the full model are automatic. So far, this has been done only for relatively simple models.

To see that the "Lucas critique" result follows from the insistence on general interdependence, and not from the rational expectations hypothesis itself, consider Friedman's (1956b) permanent income hypothesis. Permanent income is the flow of income from wealth defined as the subjective expectation of the discounted stream of the agent's future income. In order to apply the permanent income hypothesis, Friedman supposes that the subjective expectation equals what in fact *actually* happens on average. This is a simple form of rational expectations. Permanent income can, thus, be represented as an optimal forecast. Friedman represents it by a geometrically declining lag on past income, which has since been shown by Muth (1961) to be statistically optimal under some circumstances. One of the circumstances is that there are no policy changes. If there are policy changes—say, a change in taxation affecting income—then the predictions are no longer optimal (Lucas, 1976). Thus Friedman implicitly uses the rational expectations hypothesis, but still falls victim of the "Lucas critique" for failing to account for interdependence with the policy rule.[9]

In his latest work, Friedman concedes the principle of the "Lucas critique," that a function of past values of a variable *may* not be a stable proxy for its expected value (Friedman and Schwartz, 1982, Ch. 2). Nevertheless, his Marshallian method does not force him to take account of it unless it proves to be a *practical* barrier to accurate prediction in a specific case. Even then his preferred method is to work backwards incrementally to the minimum necessary level of complication, rather than to impose a fully interdependent structure on the problem, as Lucas suggests.[10]

In addition to such explicit applications of the Walrasian method, it is applied implicitly in other cases: partition is taken as a sufficient ground for criticism. For example, the rational expectations models of Fischer (1977) and Phelps and Taylor (1977) are criticized because they impose unexplained wage or price rigidities (Lucas and Sargent, 1979; Lucas, 1981b). It is only if these rigidities (e.g., long term labor contracts) can be deduced from first principles—say, from a general equilibrium system with contingent contracts—that the models can be accepted, even if they fit the data. For only then can their predictive success rest on more than the accidental constancy of a theoretically variable bit of reality (Lucas, 1977).

4.4 The Tenets of New Classicism Reexamined

In Part II we suggested that three tenets characterize the new classical doctrine—namely, that agents attend to real factors only in making real economic decisions, that they are consistent and successful optimizers, and that they hold rational expectations. So far, we have argued in

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[9] Buiter (1980) correctly observes that all that is needed for the "Lucas critique" to go through is a direct effect of government policy on private expectations. Rational expectations is but one way of getting it.

[10] Lucas (1981a, Intro.) qualifies his position somewhat. How far back one must go to secure invariance depends in part on the particular problem at hand. Nevertheless, he would generally prefer to take only tastes and technology as given.

this part that the most important distinction between Friedman's doctrine and that of the new classicals is that, in Friedman's own usage, they are Walrasians, while he is a Marshallian. Now let us consider how this fundamental distinction relates to the tenets of new classicism.

The first tenet, that only real factors matter for real economic decisions, is not—at least for longer time horizons—fundamentally at issue. It is the basis for Friedman's and the new classicals' general agreement about the neutrality of money, the natural rate of unemployment and the Phillips curve discussed in Section 3.1. The differences that remain between them on these matters should be ascribed to the second tenet.

The proposition that economic agents are consistent and successful optimizers is in some sense agreed by all those—both Friedman and the new classicals included—imbued with the so-called Chicago tradition in economics (Melvin Reder, 1982). Precisely in what sense to take the proposition is the fundamental point at issue between them. Friedman takes consistent optimization as an article of faith. Because of the Cournot problem, he cannot detail every aspect of agents' economic behavior. Thus, he applies the optimization proposition to a part of the problem, while retaining a faith that agents do optimize with respect to those parts not worked out in detail. For example, in his analysis of the Phillips curve, he sets out agents' longer-run behavior in order to show that, if they are optimizers, any tradeoff between inflation and unemployment will not endure. He does not analyze their shorter-run behavior in detail: he uses the Marshallian method to partition the problem. Nevertheless, he does not assert at any point that agents are not optimizing in the shorter run. If agents' shorter-run behavior differs from their longer-run, optimal behavior, he supposes that they are still optimizing un-

der unspecified constraints. The Marshallian method is in large measure the view that it is legitimate not to analyze those constraints explicitly when the longer-run behavior is itself largely independent of the shorter run and when one is primarily interested in the longer run.

It might be argued that the contrast between Friedman's view that agents are optimizing under unspecified constraints and the new classical view that agents optimize to the limits of their information draws a distinction without a difference. This would, however, confuse the agent's point of view with the observer's (or economist's) point of view. Friedman argues that we can have faith that agents do their best, but that, as observers, we need not specify how they do it. The new classicals, on the other hand, require that the informational limits which constrain agents be specified precisely if the observer is to understand their behavior at all.

Friedman is a pessimist about solving the Cournot problem. His Marshallian method is a way of pursuing economic analysis in the face of the problem, but it does not dissolve it. The new classicals, on the other hand, are optimists with respect to the Cournot problem. They believe that a Walrasian program of fully specifying the optimization problem which agents face is a real possibility (Sargent, 1982; Lucas, 1977). To return once more to the Phillips curve example, they argue that when it is cast as an optimization problem with the constraints on agents fully set out, the possibility of an *exploitable* shorter-run tradeoff vanishes.

The new classicals' conclusion about the Phillips curve hinges, as we observed in Section 3.1, on the assumption of rational expectations. We can now see why this third tenet is a necessary, but subsidiary, element of the new classical doctrine. Expectations become important as soon as one turns to a dynamic problem. The Walrasian interpretation of the second tenet—

i.e., consistent optimization—does not allow one to partition off the formation of expectations from the general optimization problem. If one is making systematic errors, one could do better. Walrasian optimization thus implies the absence of systematic error; i.e., it implies rational expectations.

Rational expectations has frequently been taken to be the most characteristic feature of the new classical doctrine. We can now see that this is a mistake. It is rather the Walrasian interpretation of the assumption that, to the limits of their information, agents are consistent and successful optimizers that is the distinctive feature of new classicism. Rational expectations is but an implication of this assumption.

V. *Equilibrium and Dynamics*

In Section 3.3 we observed that the new classicals use the rational expectations hypothesis as a means of bringing optimization into the analysis of economic dynamics and that, in their hands, this has the effect of collapsing the distinction between the long run and the short run, which Friedman wishes to preserve. Now that we have examined the principal methodological difference between Friedman and the new classicals and have seen how it relates to the tenets of new classicism, we are in a position to examine further the difference between Friedman's and the new classicals' analysis of economic dynamics. It will come as no surprise that the contrast of their treatments of dynamics illustrates in a concrete case the fundamental difference between the Marshallian and Walrasian methods. The first section of this part examines the different manner in which Friedman and the new classicals treat the concept of economic equilibrium. The second section considers how the Walrasian and Marshallian interpretation of the consistent op-

timization proposition, discussed in Section 4.4 above, leads to different ways of viewing agents' ignorance and mistakes about the future and to different ways of modelling economic dynamics. Finally, the third section explains why business cycles are a difficult, unsolved problem in economic dynamics for the new classicals, while Friedman's method allows him to side-step their complications.

5.1 *Equilibrium Defined*

Friedman writes: "An equilibrium position is one that, if attained, will be maintained" (1976, p. 19). This definition—drawn by analogy with the physical sciences—is consistent with the new classical view that an agent is in equilibrium when he is where he chooses to be, given what he knows (Lucas, 1977). Both Friedman and the new classicals agree on the formal correctness of these definitions. Friedman, however, finds them "unilluminating and uninteresting" (1982) as they stand. Similarly, Lucas and Sargent (1979) believe that data cannot reveal whether they come from an equilibrium process or not.

Friedman responds to the vacuousness of the formal definition by drawing the distinction between market, short-run and long-run equilibria (Friedman and Schwartz, 1982, Ch. 2). This distinction is Marshallian both in that it derives directly from Alfred Marshall (1930, Book 5) and in that it illustrates the method of coping with the Cournot problem discussed in Section 4.1, above. Friedman grants in principle that agents are optimizing at all times and are, therefore, on some demand and supply curves which fully incorporate the constraints they face. This is practically useless, however, when the investigator cannot accurately specify all the constraints (i.e., he faces the Cournot problem). In this situation a practical approach is to partition the problem. One may, for instance, specify a *long-run* de-

mand for money function. Then, with respect to that *particular* function, an agent may be in *short-run* disequilibrium. The essence of much of Friedman's work is that much may be known about the long run, yet little about the short run. For example, there is, he argues, a stable long-run relation between money and prices; yet the lags are long and variable (Friedman and Schwartz, 1963b; Friedman, 1974b).

The new classical response to the vacuousness of the general definition of equilibrium is as predictably Walrasian as Friedman's is Marshallian. It is to impose a *fully specified* theory on the data and to use the rational expectations hypothesis to solve the problem that the future is unknown (Lucas and Sargent, 1979; Lucas, 1980a). This is, in effect, to collapse Marshall's long run into the short run, as in the examples of Part III.

5.2 *Dynamic Equilibrium versus Long-run Equilibrium*

In Section 4.4 we saw that the rational expectations hypothesis was an implication of applying the Walrasian interpretation of the consistent optimization proposition—i.e., the second tenet of the new classical doctrine—to a dynamic problem. Agents are subject to error arising from their own ignorance of the future. But their ignorance can be characterized as risk in Frank Knight's sense (1937). Over any length of time or over many independent agents the errors that result from risk take on a pattern that may be summed up in an objective probability distribution. One can insure against such errors.

The rational expectations hypothesis amounts to identifying the objective probability distribution of forecast errors with the agent's own subjective assessments. This rules out Knight's other category of ignorance—uncertainty or uninsurable error. Uncertainty, says Lucas, has no place in economic analysis: "In cases of

uncertainty, economic reasoning will be of no value" (1977, p. 15).

Friedman implicitly defends the importance of uncertainty. He points out the difficulty of giving precise meaning to the identification of the subjective with the objective probability distribution and to the ambiguity of the time limit over which forecast errors are supposed to be uncorrelated (Friedman and Schwartz, 1982, Ch. 12). In a counter-example, he distinguishes between *anticipations* of inflation during the 1890s owing to the rise of the free silver movement, reflected in high rates of interest, and the *realized* fall in the rate of inflation. Agents made persistent, serially correlated errors in forecasting inflation; but whether their expectations coincided with the objective probability distribution is a moot point, because a historical situation cannot be repeated in a controlled experiment in order to ascertain the objective frequency of it falling out one way or the other. Whether or not the United States would go on to free silver was a classic example of uncertainty. Observing their past forecast errors, agents might (rationally) not revise their current forecasts so long as they believed a move to free silver was a real possibility.

The distinction between risk and uncertainty suits Friedman's pessimism about solving Cournot's problem along Walrasian lines. It is a Marshallian distinction. It partitions the agent's ignorance into that which is quantifiable (risk) and that which is not (uncertainty). In the free silver illustration, Friedman also partitions time into the Marshallian short and long runs. Thus, he argues, that if the time unit is long enough—say, twenty years—forecast errors are uncorrelated, which is to say characterized by risk. In the short run, however, uncertainty is relevant, at least in retrospect when we explain an episode like that of the 1890s historically.

The new classicals' view of uncertainty

reflects their Walrasian optimism. If a model is sufficiently fully specified, uncertainty reflects a residual about which we can truly know nothing. Hence, it is correct to hold that it cannot be usefully analyzed. The new classicals' banishment of uncertainty from economic analysis is of a piece with their collapsing the long run into the short run. The actual dynamic path of the economy is a fully optimizing (i.e., equilibrium) path. Agents do not deviate from that path by more than a serially uncorrelated error.

5.3 *The Problem of Business Cycles*

The most pressing problem for the new classical economics is to give an explanation of business cycles—i.e., for the observed serial correlation and the comovements of important economic aggregates—that does not invoke money illusion (violating the first tenet of new classical doctrine), disequilibrium (violating the second tenet) or serially correlated forecast errors on the part of agents (violating the third tenet). Lucas terms such an explanation a *competitive equilibrium account*. He believes that it is ". . . the central problem of macroeconomics . . . to find an analytical context in which [business cycles triggered by unanticipated monetary-fiscal shocks] can occur and which does not at the same time imply the existence of persistent, recurrent, unexploited profit opportunities" (Lucas, 1975, pp. 1, 114).

Our purpose is not to examine in any detail actual new classical attempts to model the business cycle. It is enough to show that it is an important and difficult problem for the new classicals because of their Walrasian method. As we have already observed, Friedman admires the Walrasian system on aesthetic grounds. Lucas, in contrast, denies that "the attempt to discover a competitive equilibrium account of the business cycle. . . . [is] merely eccentric or, at best, an aes-

thetically motivated theoretical exercise" (1975, pp. 1, 113–14). He believes that, because of the "Lucas critique" it will not be possible to analyze the effects of countercyclical policies on the economy unless such an account can be given (Section 4.3, above).

Lucas is clear that a competitive equilibrium account is Walrasian in the sense used in this paper. He quotes Friedrich von Hayek: "By 'equilibrium theory' we here primarily understand the modern theory of the general interdependence of all economic quantities, which has been most perfectly expressed by the Lausanne [i.e., Walrasian] School of theoretical economics" (1977, p. 7; Hayek, 1933, p. 42). Furthermore, Lucas' insistence that there be no unexploited profit opportunities makes it clear that a competitive equilibrium account is one consistent with the Walrasian interpretation of the consistent optimization proposition, the second tenet of the new classical doctrine.

The chief difficulty in explaining business cycles is to show how it can be consistent with continuous competitive equilibrium for real quantities such as output to cycle fairly rapidly when real resources such as the labor force and the capital stock change rather slowly. In other words, why do rational agents not find it both desirable and feasible to eliminate such fluctutations from the system? The discussion of the Phillips curve (Section 3.1, above) and of policy ineffectiveness (Section 3.2) suggest that only unexpected nominal shocks should matter at all. Even so, if real quantities can be altered at will, even unexpected nominal shocks should not have *persistent*, cyclical effects. This provides the key to Lucas' most explicit attempt to model the business cycle (1979). He imagines, first, that agents have imperfect information, so they make *unsystematic* mistakes in the face of monetary shocks; and, second, that they take decisions on the basis of those mistakes.

Lucas' model is an elaboration of a neoclassical growth model in which, in a steady-state with certainty, real quantities, the money stock and prices would grow at constant rates. In order to illustrate his explanation of business cycles, suppose that there is uncertainty and, in particular, that there is an unanticipated increase in the money supply which agents first perceive as increased demand. They expand output and, if they are ignorant of the shock's transient nature, increase their rate of investment in order to adjust the capital stock to the higher level of demand. By rational expectations, they soon learn of their mistake; but, since capital endures, they are stuck with a real change in their economic environment.

Even if there are no further shocks, a cycle has been generated. For now agents see that their capital stock is too high, so they choose a lower rate of investment in order to optimally reduce the capital stock to its optimal level. Prices cycle as well: the initial effect of the increased supply of money is that prices rise faster than they otherwise would have. Once installed, however, the increased capacity resulting from the new investment retards the rate of increase of prices below the steady-state rate until the optimal capital stock is restored. Other real quantities—employment, output and so forth—connected by agents' plans to their decisions about the capital stock and investment cycle as well. Agents do not make persistent mistakes; their mistakes may nonetheless have persistent consequences, even when they act optimally.

Lucas is not happy with the details of the particular model just sketched (1981a, Intro.). He recognizes that a competitive equilibrium theory of the business cycle must solve the Cournot problem by adequately characterizing the constraints that agents face, and he is careful not to claim success too early. Nevertheless, as we have already observed, the new classicals are optimists about solving the Cournot problem: Lucas predicts that an adequate equilibrium model may be ". . . five but not twenty-five years off" (1977, p. 25).

Friedman does not believe that Cournot's problem will be so easily solved. In the meantime, the Marshallian method and its accompanying distinction between the long run and the short run are needed for practical purposes. Lucas writes: "The idea that an economic system in equilibrium is in any sense 'at rest' is simply an anachronism" (1980a, p. 708). Friedman's approach to economic dynamics requires no such assumption. He needs merely to maintain that our ignorance of the fine details of economic processes is such that we know more about the secular than the cyclical behavior of the economy. It is, then, more useful to detail the secular in our description of the Marshallian long-run equilibrium, which may well be moving, and to summarize cyclical behavior as a short-run adjustment towards this equilibrium.[11]

Indeed, Friedman's method of treating the business cycle has not altered much in the past twenty years. In the earlier empirical examination of the role of money in business cycles (Friedman and Schwartz, 1963b) as well as in the theoretical sections of *Monetary Trends* (1982), Friedman imagines the economy in long-run equilibrium (an "Elysian state of moving equilibrium") before and after a monetary shock. He then deduces general properties of the economic dynamics that must obtain if the economy is to move from one long-run equilibrium to the other. The picture he paints of the mechanism by which monetary shocks are transmitted to cycles in real quantities remains impressionistic and does not extend to the full specification of the agents' economic

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[11] "Secular" is used here in the usual, modern sense, which differs from Marshall's usage in which the secular is a longer horizon than the long run (Marshall 1930, Book 5, Ch. 5).

environment that is required by the new classical standards of adequacy.

The difference between Friedman and the new classicals here as elsewhere is not over the nature of the economy: both agree that at root it is a complex, interdependent system. Rather it is that, in the face of the Cournot problem, Friedman believes that only the Marshallian method of partitioning problems produces *fruitful* results; while the new classicals believe that only the Walrasian method of fully specifying the optimization problem that agents face produces *secure* results.

VI. *Summary*

There has been a lively debate over whether or not Lucas, Sargent, Wallace and their fellow adherents to the new classical doctrine are best thought of as monetarists. The central argument of this essay is simply that, whatever we may choose to call them, these new classicals must be distinguished from an older strand of monetarism represented by Friedman.

We have seen that the strongest case for calling the new classicals "monetarists" is based on the similarity of their policy prescriptions to Friedman's. Nevertheless, their conclusions have a more radical tone than his. This is the result of a different theoretical analysis.

An essential part of the theoretical difference is a difference in methodology: they are Walrasians, while he is a Marshallian. The characteristic new classical results follow from the attempt to analyze the economy as an interdependent whole. Friedman recognizes interdependence, but insists that, for practical purposes, problems must be partitioned into parts analyzed in detail and parts summarized.

An important aspect of the difference between the Marshallian and Walrasian approaches is the different treatment that Friedman and the new classicals give of economic dynamics. While he finds it useful to divide the problem up into Marshallian "runs," they insist that nothing less than a full equilibrium approach, in which the distinction between short run and the long run is abolished, will do.

REFERENCES

BEENSTOCK, MICHAEL. *A neoclassical analysis of macroeconomic policy*. Cambridge: Cambridge U. Press, 1980.

BEGG, DAVID K. *The rational expectations revolution in macroeconomics: Theories and evidence*. Deddington, Oxford, England: Philip Allan, 1982.

BOLAND, LAWRENCE A. "A Critique of Friedman's Critics," *J. Econ. Lit.*, June 1979, *17*(2), pp. 503–22.

_____. *The foundations of economic method*. London: George Allen and Unwin, 1982.

BUITER, WILLEM. "The Macroeconomics of Dr. Pangloss: A Critical Survey of the New Classical Macroeconomics," *Econ. J.*, Mar. 1980, *90*(357), pp. 34–50.

COURNOT, AUGUSTIN. *Researches into the mathematical principles of the theory of wealth*. Trans.: NATHANIEL T. BACON. NY: Macmillan, [1838] 1927.

FISCHER, STANLEY. "Long Term Contracts, Rational Expectations and the Optimal Money Supply Rule," *J. Polit. Econ.*, Feb. 1977, *85*(1), pp. 191–205.

FRIEDMAN, MILTON. "A Monetary and Fiscal Framework for Economic Stability," *Amer. Econ. Rev.*, June 1948, *38*(3), pp. 245–64.

_____. "The Marshallian Demand Curve," *J. Polit. Econ.*, Dec. 1949, *57*(6), pp. 463–95.

_____. "The Methodology of Positive Economics," *Essays in positive economics*. Chicago, IL: U. of Chicago Press, 1953, pp. 3–46.

_____. "Leon Walras and His Economic System: A Review Article," *Amer. Econ. Rev.*, Dec. 1955, *45*(5), pp. 900–09.

_____. "The Quantity Theory of Money: A Restatement," in *Studies in the quantity theory of money*. Ed.: MILTON FRIEDMAN. U. of Chicago Press, 1956, pp. 3–24.

_____. *A theory of the consumption function*. Princeton, NJ: Princeton U. Press, 1957.

_____. "The Demand for Money: Some Theoretical and Empirical Results," *J. Polit. Econ.*, Aug. 1959a, *67*(3), pp. 327–51.

_____. *A program for monetary stability*. NY: Fordham U. Press, 1959b.

_____. "The Lag in the Effect of Monetary Policy," in *The optimum quantity of money*. By MILTON FRIEDMAN. London: Macmillan, [1961] 1969a, pp. 237–60.

_____. "The Role of Monetary Policy," *Amer. Econ. Rev.*, Mar. 1968, *58*(1), pp. 1–17.

_____. *The optimum quantity of money*. London: Macmillan, 1969a.

———. "The Optimum Quantity of Money," in *The optimum quantity of money.* By MILTON FRIED-MAN. London: Macmillan, 1969b, pp. 1–50.

———. "Monetary Correction." IEA Occasional Paper No. 41. London: Inst. Econ. Affairs, 1974a.

———. "A Theoretical Framework for Monetary Analysis," in *Milton Friedman's monetary framework: A debate with his critics.* Ed.: ROBERT J. GORDON. Chicago & London: U. of Chicago Press, 1974b, pp. 1–62.

———. *Price theory.* Chicago, IL: Aldine, 1976.

———. "Nobel Lecture: Inflation and Unemployment," *J. Polit. Econ.,* June 1977, *85*(3), pp. 451–72.

———. "How Stands the Theory and Practice of Monetary Policy?" Unpublished paper presented to conference on Monetary Problems and Policy, Mont Pelerin Society, Hong Kong, 1978.

———. Letter to Kevin D. Hoover, July 8, 1982.

——— AND SCHWARTZ, ANNA. *A monetary history of the United States, 1867–1960.* Princeton NJ: Princeton U. Press, 1963.

———. "Money and Business Cycles," in *The optimum quantity of money.* By MILTON FRIEDMAN. London: Macmillan, [1963] 1969, pp. 189–236.

———. *Monetary trends in the United States and the United Kingdom: Their relation to income, prices, and interest rates 1867–1975.* Chicago & London: U. of Chicago Press, 1982.

GORDON, ROBERT J. "Output Fluctuations and Gradual Price Adjustments," *J. Econ. Lit.,* June 1981, *19*(2), pp. 493–530.

GOLDFELD, STEPHEN. "Rules, Discretion, and Reality," *Amer. Econ. Rev.,* May 1982, *72*(2), pp. 361–66.

HAHN, FRANK. "Professor Friedman's Views on Money: Review Article," *Economica NS,* Feb. 1971, *38*(149), pp. 61–80.

———. "Monetarism and Economic Theory," *Economica,* Feb. 1980, *47*(185), pp. 1–17.

———. *Money and inflation.* Oxford: Blackwell, 1982.

HAYEK, FRIEDRICH A. *Monetary theory and the trade cycle.* London: Jonathan Cape, 1933.

———. *The counter-revolution in science: Studies on the abuse of reason.* Indianapolis, IN: Liberty Press, 1979.

KANTOR, BRIAN. "Rational Expectations and Economic Thought," *J. Econ. Lit.,* Dec. 1979, *17*(4), pp. 1422–41.

KNIGHT, FRANK. *Risk, uncertainty and profit.* London: London School of Econ., 1937.

LAIDLER, DAVID. "Monetarism: An Interpretation and an Assessment," *Econ. J.,* Mar. 1981, *91*(361), pp. 1–29.

———. *Monetarist perspectives.* Deddington, Oxford, Eng.: Philip Allan, 1982.

LUCAS, ROBERT E. "Econometric Testing of the Natural Rate Hypothesis," in *The econometrics of price determination.* Ed.: OTTO ECKSTEIN. Washington, DC: Bd. of Gov. of the Fed. Res. System and Soc. Sci. Res. Council, 1972a, pp. 50–59.

———. "Expectations and the Neutrality of Money," *J. Econ. Theory,* Apr. 1972b, *4*(2), pp. 103–24.

———. "Some International Evidence on Output-Inflation Tradeoffs," *Amer. Econ. Rev.,* June 1973, *63*(3), pp. 326–34.

———. "An Equilibrium Model of the Business Cycle," *J. Polit. Econ.,* Dec. 1975, *83*(6), pp. 1133–44.

———: "Econometric Policy Evaluation: A Critique," in *The Phillips curve and labor markets.* Eds.: KARL BRUNNER AND ALLAN H. MELTZER. Carnegie-Rochester Conference Series, Vol. 1. Amsterdam: North Holland, 1976, pp. 19–46.

———. "Understanding Business Cycles," in *Stabilization of the domestic and international economy.* Eds.: KARL BRUNNER AND ALLAN H. MELTZER. Carnegie-Rochester Conference Series, Vol. 5. Amsterdam: North Holland, 1977, pp. 7–30.

———. "Methods and Problems in Business Cycle Theory," *J. Money, Credit, Banking,* Nov. 1980a, *12*(4, Part 2), pp. 696–715.

———. "Rules, Discretion and the Role of the Economic Adviser," in *Rational expectations and economic policy.* Ed.: STANLEY FISCHER. Chicago: U. of Chicago Press, 1980b, pp. 199–210.

———. *Studies in business cycle theory.* Oxford: Blackwell, 1981a.

———. "Tobin and Monetarism: A Review Article," *J. Econ. Lit.,* June 1981b, *19*(2), pp. 558–67.

——— AND SARGENT, THOMAS J. "After Keynesian Macroeconomics," reprinted in *Rational expectations and econometric practice.* Eds.: ROBERT E. LUCAS AND THOMAS J. SARGENT. London: George Allen & Unwin, [1979] 1981, pp. 295–320.

———, eds. *Rational expectations and econometric practice.* London: George Allen & Unwin, 1981.

MADDOCK, RODNEY AND CARTER, MICHAEL. "A Child's Guide to Rational Expectations," *J. Econ. Lit.,* Mar. 1982, *20*(1), pp. 39–52.

MARSHALL, ALFRED. *Principles of economics.* 8th ed. London: Macmillan, 1930.

MAYER, THOMAS, ed. *The structure of monetarism.* NY & London: Norton, 1978.

MEADE, JAMES. "Comment on the Papers by Professors Laidler and Tobin," *Econ. J.,* Mar. 1981, *91*(361), pp. 49–55.

MUTH, JOHN F. "Rational Expectations and the Theory of Price Movements," *Econometrica,* July 1961, *29*(6), pp. 315–35.

PHELPS, EDMUND. "Phillips Curves, Expectations of Inflation and Optimal Unemployment Over Time," *Economica NS,* Aug. 1967, *34*(3), pp. 254–81.

———. "Money Wage Dynamics and Labor Market Equilibrium," *J. Polit. Econ.,* July–Aug. 1968, *76*(4), pp. 678–711.

——— AND TAYLOR, JOHN B. "Stabilizing Powers of Monetary Policy Under Rational Expectations," *J. Polit. Econ.,* Feb. 1977, *85*(1), pp. 163–90.

PURVIS, DOUGLAS. "Monetarism: A Review," *Can. J. Econ.,* 1980, *13*(1), pp. 96–122.

REDER, MELVIN W. "Chicago Economics: Permanence and Change," *J. Econ. Lit.,* Mar. 1982, *20*(1), pp. 1–38.

SARGENT, THOMAS J. "Rational Expectations, the

Real Rate of Interest, and the Natural Rate of Unemployment," *Brookings Pap. Econ. Act.,* 1973:2, pp. 429–72.

———. "A Classical Macroeconometric Model of the United States," *J. Polit. Econ.,* Apr. 1976, *84*(2), pp. 207–37.

———. *Macroeconomic theory.* NY: Academic Press, 1979.

———. "Beyond Demand and Supply Curves in Macroeconomics," *Amer. Econ. Rev.,* May 1982, *72*(2), pp. 382–89.

——— AND WALLACE, NEIL. " 'Rational Expectations,' the Optimal Monetary Instrument and the Optimal Money Supply Rule," *J. Polit. Econ.,* Apr. 1975, *83*(2), pp. 241–54.

———. "Rational Expectations and the Theory of Economic Policy," *J. Monet. Econ.,* Apr. 1976, *2*(2), pp. 169–83.

———. "Some Unpleasant Monetarist Arithmetic," *Fed. Res. Bank Minn. Quart. Rev.,* 1981, *5*(3), pp. 1–17.

SCHILLER, ROBERT. "Rational Expectations and the Dynamic Structure of Macroeconomic Models: A Critical Review," *J. Monet. Econ.,* 1976, *4*(1), pp. 1–44.

SIMONS, HENRY C. "Rules versus Authorities in Monetary Policy," *J. Polit. Econ.,* Feb. 1936, *44*(1), pp. 1–30.

STEIN, JEROME, ed. *Monetarism.* Amsterdam: North Holland, 1976.

———. *Monetarist, Keynesian and new classical economics.* Oxford: Blackwell, 1982.

TOBIN, JAMES. *Asset accumulation and economic activity.* Oxford: Blackwell, 1980.

———. "The Monetarist Counter-Revolution Today—An Appraisal," *Econ. J.,* Mar. 1981, *91*(361), pp. 29–42.

TURNER, THOMAS H. AND WHITEMAN, CHARLES H. "Econometric Policy Evaluation Under Rational Expectations," *Fed. Res. Bank Minn. Quart. Rev.,* 1981, *5*(2), pp. 6–15.

WEINTRAUB, E. ROY. "On the Existence of a Competitive Equilibrium: 1930–1954," *J. Econ. Lit.,* Mar. 1983, *21*(1), pp. 1–39.