With the US and other nations in a serious recession that was not anticipated by economic forecasters and with American macroeconomic policymakers flailing like panicked victims of a capsized boat, this is an apt time to consider a bit of the history of American macroeconomics since the Full Employment Act of 1946. The bit of the history that we will examine is history of what economists have thought they knew and how they thought they knew it. This paper is at the intersection of history of economics and economic methodology, for the questions that motivate the paper concern economists’ knowledge of the economy. What have economists thought they knew about the aggregate economy and how did they know what they thought they knew?

I will use Milton Friedman and Paul Samuelson as subjects because of similarities and differences between them. The two were similar in that both were prominent in the economics profession and well-known public figures over the thirty years following the Full Employment Act of 1946. Samuelson was the author of the most successful introductory textbook of the period, with the first edition of *Economics: An Introductory Analysis* published in 1948. Friedman’s critique of rent controls, *Roofs or Ceilings? The Current Housing Problem* (1946), written with George J. Stigler, first placed him in the public arena, and the spotlight shown brighter after his 1956 lectures at Wabash College. These

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1 Our review will be from the period prior to the Full Employment and Balanced Growth Act of 1978.
lectures became the core material for *Capitalism and Freedom* (1962). Samuelson and Friedman both wrote contributions for newspapers and magazines and published collections of essays and commentaries for the general public. They were also both influential in the public policy arena.² And importantly for our purposes, both men considered themselves to be economic scientists.

These similarities are, in effect, the factors in a ceteris paribus pound for my critical exercise. My attention is on their differences. The two economists had different ideas on macroeconomics -- on the sources and remedies of the business cycle. Samuelson was a Keynesian and Friedman a monetarist.³ Friedman, of course, had a larger role than anyone else in creating the monetarist alternative to Keynesian theory of the business cycle. But also, and more importantly for this exercise, their methods of doing economics were different. Samuelson was a mathematical economist, one of the founders of modern mathematical economics. Although he was highly trained in mathematics, Friedman took a different route, working in the National Bureau tradition with emphasis on constructing and using observational data. Friedman used what were by the standards of the time considered unsophisticated theory and statistical methods.

**Samuelson’s Vision of Economic Science**

As a mathematical theorist Paul Samuelson was not dependent on data. All he needed for his scholarship were books and articles to read, and a pencil and

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² Both worked for the National Resources Committee/National Resources Planning Board and the U.S. Treasury and other government agencies early in their careers, and later served consultancies to government agencies. Samuelson advised Presidential candidates Adlai Stevenson and John F. Kennedy and Friedman advised Barry M. Goldwater.

³ See Pearce and Hoover (1995) for an evaluation of Samuelson’s treatment of Keynesian economics through the first eleven editions of his economic principles textbook.
paper. Samuelson could begin and finish a project in short order. As seems to be typical of mathematicians, his greatest contributions to economics came fast and furiously early in his career. His single greatest contribution, *Foundations of Economic Analysis* (1947) was his Ph.D. dissertation, which he completed at the age of twenty-six.⁴

Samuelson’s scientific expertise was as a mathematical economic theorist. In his Nobel Prize biography Samuelson quotes an earlier autobiographical piece in which he proclaimed himself “the last ‘generalist’ in economics.” And he was indeed a generalist in subject matter if not method. He contributed to the theory of welfare economics, linear programming, Keynesian economics, economic dynamics, international trade, and the logic of choice and mathematics. Samuelson’s was the second Nobel Prize in Economics. Assar Lindbeck opened the 1970 presentation speech by calling attention to the formalization of two sides of economics, statistical analysis and economic theory. The previous year Ragnar Frisch and Jan Tinbergen were honored for their contributions to the formalization of statistical theory and analysis -- econometrics “designed for immediate statistical estimation and empirical application” (Lindbeck, 1970). Samuelson was being honored for his contributions to the formalization of economic theory, “without any immediate aims of statistical, empirical confrontation” (Lindbeck, 1970).

Lloyd Metzler’s review of Samuelson’s *Foundations of Economic Analysis* (1948) noted that the book was a contribution to economic method, with

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⁴ When Friedman was twenty-six years old he was on the National Bureau research staff and had published six journal articles on economics and on statistics. None of these were the work for which he became known.
illustrations of the method from a variety of fields such as taxation, international trade, business cycles, money and banking, and employment. But problems in these fields were not treated with depth. That is, the analysis made no use of institutions and data. What Samuelson’s method offered in place of depth was unification. It was mathematically difficult, but offered in the unification a kind of simplification, for economic analysis in the disparate fields could be reduced to problems of equilibrium and maximization. Metzler admired Samuelson’s contribution, but was skeptical that analysis could be taken very far without resort to empirical evidence. This would be a limitation, for example, “in the study of complicated and unsymmetrical systems such as one encounters in business cycle theory” (1948, p. 910).

Samuelson not only pioneered mathematical economics; he also advocated mathematization of economics. In the early 1950s he was drawn to the methodological question of whether the better language for economic theorizing was prose or mathematics. He argued that the two were logically equivalent, that mathematics was simply statements written in symbols rather than words. But on the practical level, he suggested that mathematics facilitated theorizing better than prose.

As a mathematical economist, Samuelson did not himself do the empirical work of testing theories and building sets of data. But this was not because he regarded empirical analysis as unimportant. It seems rather to have been a matter of division of labor. Samuelson thought that one cannot learn how an economy works without using observational evidence. He regarded all science as empirical.
As he put it in an AEA roundtable on economic methodology, “Every science is based squarely on induction – on observation of empirical facts” (1952, 57).

In one of his several biographical accounts Samuelson wrote of economic forecasting, stating what he called Samuelson’s law:

“Allways look back. You may learn something from your residuals. Usually one’s forecasts are not so good as one remembers them; the difference may be instructive.” The dictum “If you must forecast, forecast often,” is neither a joke not a confession of impotence. It is a recognition of the primacy of brute fact over pretty theory. That part of the future that cannot be related to the present’s past is precisely what science cannot hope to capture. (1986, 64).

In the opening chapter of his textbook Economics (1948), he wrote:

It is the first task of modern economic science to describe, to analyze, to explain, to correlate these fluctuations of national income. Both boom and slump, price inflation and deflation, are our concern. This is a difficult and complicated task. Because of the complexity of human and social behavior, we cannot hope to attain the precision of a few of the physical sciences. We cannot perform the controlled experiments of the chemist or biologist. Like the astronomer we must be content largely to “observe” (1948, p. 4).

And a few pages later:
Properly understood, therefore, theory and observation, deduction and induction cannot be in conflict. Like eggs, there are only two kinds of theories: good ones and bad ones. And the test of a theory’s goodness is its usefulness in illuminating observational reality. Its logical elegance and fine-spun beauty are irrelevant.

Consequently, when a student says, “That’s all right in theory but not in practice,” he really means “That’s not all right in theory,” or else he is talking nonsense (1948, p. 8).

It seems accurate to view Samuelson’s contributions to economic theory as one component of economic scholarship that ultimately must be “based squarely on induction.” Indeed one of the threads running through his work is operationality, on theory in terms that are amenable to measurement and testing, such as revealed preference consumer theory in the place of utility theory. He is also known to have taken a “more realistic” stance on economic methodology than Friedman. Where Friedman argued in “The Methodology of Positive Economics” (1953) that the lack of realism of a theory’s assumptions did not matter so long as the theory predicted well, Samuelson (1963) countered that it is logically necessary that a theory’s assumptions and implications share the same degree of realism or unreality. He contended that Friedman was:

fundamentally wrong in thinking that unreality in the sense of factual inaccuracy even to a tolerable degree of approximation is anything but a demerit for a theory or hypothesis (or a set of hypotheses). Some inaccuracies are worse than others, but that is
only to say that some sins against empirical science are worse than others, not that a sin is a merit or that a small sin is equivalent to a zero sin (1963, p. 233).
Friedman’s “F-twist,” as Samuelson put it, does not hold up to scrutiny. Realisticness must either matter for both assumptions and implications or for neither. Samuelson advocated theory that was realistic on both ends.

**Friedman’s vision of Economic Science**

In 1947 the American Economic Association awarded the John Bates Clark medal to Samuelson and the Francis A. Walker medal to Friedman’s mentor Wesley C. Mitchell. There is an interesting contrast in this coincidence. The Clark award was, then as now, awarded to an economist under the age of forty who made a significant contribution to economics. The Walker medal was conferred on a living economist who made the greatest contribution over his career. Paul H. Douglas, President of the American Economic Association, presented the awards. Douglas remarked that the Clark medal was conferred on “a brilliant economist [Samuelson] who mastered at an early age both mathematics and economic theory; [who] has made extraordinarily penetrating contributions to the theory of employment, production, and value; and whose recent book stamps him as one of the masters of our craft” (1948). Douglas said that the Walker medal was “awarded to the world’s foremost student of business cycles [Mitchell] whose massive work on this subject a third of a century ago opened up a new world for investigation in which he has continued to be the foremost explorer, patient and
untiring scholar and master of the inductive method who operates with the
objectivity of a physical scientist.” (1948).

Wesley Mitchell had a deep influence on Friedman, who studied business
cycles under Mitchell at Columbia University and worked for him at the National
Bureau. From Mitchell, Friedman acquired skills in National Bureau methods of
building data to match conceptual units of economic measurement, and for
analyzing trends and cycles in data. He also learned from Mitchell that science is
instrumental in improving the lot of humanity, that economics can be scientific, or
it can be used as a rhetorical tool to advance agendas, and that keeping the latter
function from encroaching on the former requires care and diligence.

The other side of Friedman’s method and methodology was Marshallian,
which he acquired from Arthur F. Burns. Marshallian methods involved relatively
low-brow theory in close relation to measurable entities. In an exchange of letters
in which he and George Stigler discussed Marshall, letters written as Friedman
prepared to teach price theory at Chicago for the first time in 1946, Stigler
commented on his friend’s methodological preferences.

I am coming to believe that you are more consistently abstract and
a priori-ish than I. But it is cloaked over by your emphasis on
realism, which I would like to have you define. I shall conjecture,
if only to hasten the enlightenment, that you like a firm skeleton of
rigorous theory well skinned with concrete illustrations, in the
manner of Marshall and Burns, all oriented in accordance with

Friedman’s methodology can be seen clearly in his review of Oscar Lange’s *Price Flexibility and Employment* (1946), written just before he joined the University of Chicago faculty. His main point was that mathematical analysis alone cannot answer economic questions. Therefore, when an economist seemingly draws conclusions about real world economic problems from a formal mathematical model, he invariably relies on information from somewhere other than the mathematics of the model. If the mathematical model is taken to be the basis for the economist’s scientific analysis, yet his conclusions depend on information from outside the model, his conclusions lack full scientific warrant by the mathematical economist’s own standards. The question Lange addressed was, on one level, basic and simple – will a reduction in the price of a factor of production lead to an increase in employment of the factor? His answer was, in effect, “that depends on lots of things.” Lange used a general equilibrium model, considering the direct effects of the price change and the indirect effects that might outweigh the direct.

Friedman remarked that he chose Lange’s book for a “methodological sermon” because of the high caliber of the analysis. Lange’s analysis was of a type that Friedman called “taxonomic theorizing.” He contrasted Lange’s approach with that which he regarded as more fully scientific.

The theorist starts with some set of observed and related facts, as full and comprehensive as possible. He seeks a generalization that
will explain these facts; he can always succeed; indeed he can always find an indefinitely large number of generalizations. … The theorist therefore calls in some arbitrary principle such as “Occam’s razor” and settles on a particular generalization or theory. He tests this theory to make sure it is logically consistent, that its elements are susceptible of empirical determination, and that it will explain adequately the facts he started with. He then seeks to deduce from his theory facts other than those he used to derive it and to check these deductions against reality. Typically some deduced “facts” check and others do not, so he revises his theory to take account of the additional facts. …

The approach used by Lange, and all too common in economics, is very different. Lange largely dispenses with the initial step – a full and comprehensive set of facts to be generalized – and in the main reaches conclusions no observed facts can contradict. His emphasis is on the formal structure of the theory, the logical interrelations of the parts. He considers it largely unnecessary to test the validity of his theoretical structure except for conformity to the canons of formal logic. His categories are selected primarily to facilitate logical analysis, not empirical application or test. For the most part, the crucial question, “What observed facts would contradict the generalization suggested, and what operations could be followed to observe such critical facts?”
is never asked; and the theory is so set up that it could seldom be answered if it were asked. The theory provides formal models of imaginary worlds, not generalizations about the real world (1946, 282-3).

Friedman identified structural weaknesses and weaknesses of execution in taxonomic theorizing. The structural weaknesses were (1) oversimplification, for even in a general equilibrium model, the theorist has to restrict the number and type of functions, and (2) use of classifications that do not have direct empirical counterparts, which is an impediment to testing the theory. The structural weaknesses lead to errors of execution. Friedman identified four: casual empiricism\(^5\), invalid use of inverse probability\(^6\), introduction of factors not in the formal theory\(^7\), and unwillingness to accept implications of the theory that seem unreasonable\(^8\). These four errors emerge from an incompatibility of the theoretical model’s complexity on the one hand and the theorist’s urge to be realistic on the other.

Friedman suggested that errors of execution allowed the analysis to appear to be more closely related to the real world than it actually is. In making this critique he presumed that Lange and others shared his own expectation that the

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\(^5\) For example, Lange’s statement that “these complications are disregarded in the text in order to simplify the argument and also because they do not seem to be very important in practice” (1946, 290).

\(^6\) Inverse probability refers to Bayesian inference, with an hypothesis being confirmed by any body of data that the hypothesis’s truth renders probable. Friedman considered the clearest case of Lange’s improper use his conclusion without checking data that marginal revenue functions all have negative slope.

\(^7\) Lange introduces “friction” such as lags in effect into the model as a \textit{dies ex machina}.

\(^8\) The example here is Lange’s substitution of “effective expected price” in place of a probability distribution of prices in order to simplify the approach, when the taxonomic approach itself is intended to consider all possibilities.
fruit of economic analysis was ability to shed insight on real world problems. Down-to-earth realism that was necessary to have anything useful to say about real world problems, and the key feature of the model, taxonomic formalism, were, according to Friedman, incompatible.

   A man who has a burning interest in pressing issues of public policy, who has a strong desire to learn how the economic system really works in order that that knowledge may be used, is not likely to stay within the bounds of a method of analysis that denies him the knowledge he seeks. He will escape the shackles of formalism, even if he has to resort to illogical devices and specious reasoning to do so (1946, 300).

Samuelson as Macroeconomist

Samuelson published eleven articles as sole author before he received his Ph.D. in 1941. Several were on macroeconomics, including “Interactions Between Multiplier Analysis and the Accelerator Principle” (1939) and “The Theory of Pump-Priming Reexamined” (1940). These two articles provide us with a view of how the mathematical formalist of Foundations handled macroeconomic theory when most people writing in macroeconomics did so with more words than mathematical symbols, more diagrams than theorems and proofs. Samuelson’s older contemporaries were economists such as J.M. Keynes and Alvin H. Hansen, and Friedman’s mentor Wesley C. Mitchell.

   In the 1939 article Samuelson sought to generalize multiplier analysis along lines begun by Hansen. Samuelson’s contribution was to move the analysis
from arithmetical examples to algebraic analysis of income sequences contingent on a government expenditure stimulus, i.e., mathematization of multiplier-accelerator theory. Samuelson produced a four-way taxonomy of the behavior of income under different assumed combinations of multiplier and accelerator coefficients. He warned that his analysis assumed constant marginal propensity to consume and accelerator coefficient, although these would actually change with the level of income. The analysis was thus

strictly a marginal analysis to be applied to the study of small oscillations.

Nevertheless, it is more general than the usual analysis. Contrary to the impression commonly held, mathematical methods properly employed, far from making economic theory more abstract, actually serve as a powerful liberating device enabling the entertainment and analysis of ever more realistic and complicated hypotheses (1938, 78).

In the 1940 article Samuelson considered whether a countercyclical fiscal deficit might be self-eliminating, i.e., whether the income generated by the fiscal stimulus might produce enough tax revenue to close the deficit. He presented no explicit mathematical analysis in the article, beyond a reference to the 1939 piece, but reasoned to a theorem of multiplier analysis – “that the increase of expenditure of an extra dollar cannot result in increased tax revenues of as much as a dollar even though all succeeding time is taken into consideration” (1940, 503).

He derived this conclusion from analytical assumptions and analytical presumptions. By analytical assumptions I mean assumptions the role of which
was to simplify and thus facilitate analysis. By analytical presumptions I mean presumptions about the nature of the economic system. In the first category were the assumptions that (following Hansen’s analysis of the acceleration principle) induced private investment is proportional to the increase in consumption from one period to the next, and that prices remain unchanged. In the second category were presumed actual characteristics of the economy. These were that:

(1) “the economic system is not perfect and frictionless so that there exists the possibility of unemployment and under-utilization of productive resources” …

(2) “there exists the possibility of, if not a definite tendency toward, cumulative movements of a disequilibrating kind” …

(3) “the average propensity to consume is less than one, at least at high levels of national income” …

(4) “even in a perfect capital market there is no tendency for the rate of interest to equilibrate the demand and supply of employment” …

(5) “there exist no technical difficulties to prevent the government from financing deficits of the magnitudes discussed.” (1940, 492-94).

Samuelson gave no justification for these presumptions other than that they were regarded as fundamental in recent business cycle literature.

He divided economic downturns into two categories, (1) downturns that arise from exhaustion of investment opportunities, and (2) downturns that arise from inventory accumulation based on expected but unrealized price increases. He suggested that the Great Depression belonged at least in part in the first
category, i.e., the Depression was caused in part by exhaustion of investment opportunities. With regard to recessions that are caused by unwarranted inventory accumulation he suggested that “waiving the difficulties of quickly engineering a spending policy, there seems to be every reason in this case for the government to act promptly so as to maintain the national income and aid in the orderly reduction of inventories” (1940, 497). Notice how much is swept aside by Samuelson’s waiver of the difficulties of quickly engineering a spending policy – all of the politics of budget writing plus the matter of targeting expenditures at the industries that have surplus inventories. Also notice that if Samuelson’s two categories are exhaustive, then no downturns begin in public sector, from misguided fiscal policy or monetary policy. What Friedman and Schwartz were to later conclude about the Great Depression is ruled out.

Samuelson and Robert Solow devoted more than half of their discussion at the 1959 AEA session on price level stability to impediments to using historical data to identify different types of inflation -- demand-pull, cost-push, and demand shift. The authors were critical of one-sided explanations of inflation for these typically ignored the “intricacies involved in the demand for money,” relied on aggregate ex post data and partial equilibrium analysis, and failed to account for the possibility that effects may precede causes. Following this rather pessimistic rendering of the problems involved in evaluating historical instances of inflation, Samuelson and Solow turned to A.W. Phillips’s “fundamental schedule relating unemployment and wage changes” in the U.K. From plotted U.S. data on unemployment rates and increases in hourly earnings they offered suggestions
that are worthy of quotation at length. [S&S, figure 1. All figures are in the appendix of this paper] They begin by noting deficiencies in the data:

The first defect to note is the different coverages represented in the two axes. Duesenberry has argued that postwar wage increases in manufacturing on the one hand and in trade, services, etc., on the other, may have quite different explanations: union power in manufacturing and simple excess demand in the other sectors. It is probably true that if we had an unemployment rate for manufacturing alone, it would be somewhat higher during the post war years than the aggregate figure shown. Even if a qualitative statement like this held true over the whole period, the increasing weight of services in the total might still create a bias. Another defect is our use of annual increments and averages, when a full-scale study would have to look carefully into the nuances of timing.

A first look at the scatter is discouraging; there are points all over the place. But perhaps one can notice some systematic effects. In the first place, the years from 1933 to 1941 appear to be *sui generis*; money wages rose or failed to fall in the face of massive unemployment. One may attribute this to the workings of the New Deal (the 20 per cent wage increase of 1934 must represent the NRA codes): or alternatively one could argue that by 1933 much of the unemployment had become structural, insulated from the
functioning labor market, so that in effect the vertical axis ought to be moved over to the right. This would leave something more like the normal pattern.

The early years of the first World War also behave atypically although not so much as 1933-39. This may reflect cost-of-living increases, the rapidity of the increase in demand, a special tightness in manufacturing, or all three.

But the bulk of the observations – the period between the turn of the century and the first war, the decade between the end of that war and the Great Depression, and the most recent ten or twelve years – all show a rather consistent pattern. Wage rates do tend to rise when the labor market is tight, and the tighter the faster. What is most interesting is the strong suggestion that the relation, such as it is, has shifted upward slightly but noticeably in the forties and fifties. On the one hand, the first decade of the century and the twenties seem to fit the same pattern. Manufacturing wages seem to stabilize absolutely when 4 or 5 per cent of the labor force is unemployed; and wage increases equal to the productivity increase of 2 to 3 per cent per year is the normal pattern at about 3 per cent unemployment. This is not so terribly different from Phillips’ results for the U.K., although the relation holds there with a greater consistency. We comment on this below.
On the one hand, from 1946 to the present, the pattern is fairly consistent and consistently different from the earlier period. The annual unemployment rate ranged only narrowly, from 2.5 per cent in 1953 to 6.2 per cent in 1958. Within that range, as might be expected, wages rose faster the lower the unemployment rate. But one would judge now that it would take more like 8 per cent unemployment to keep money wages from rising. And they would rise at 2 to 3 per cent per year with 5 or 6 per cent of the labor force unemployed (1960, 188-89).

The data analyzed in this passage are presented only as a scatter plot, without numerical values or time identification except for circled points for “recent years.” Perhaps more information was provided at the AEA session, but readers of their paper are left to rely on what appears to be the authors’ casual analysis. In the long passage that I quoted Samuelson and Solow begin by pointing out two deficiencies in the data, both of which are instances of over-aggregation. They speculate that if they had separate data for manufacturing, unemployment would be higher there than in other sectors for the post-war years. Their first glance at the data suggested no pattern, i.e., no Phillips curve. “But perhaps one can notice some systematic effects.” The implication would seem to be that if one could not find systematic effects the paper would end at this point with a “we do not know.” But it doesn’t. The patterns Samuelson and Solow identified, summarized from the long quotation, are:
1. 1933 to 1941 are *sui generis*; if there is a Phillips curve it has a positive slope. The anomaly is the result either of NRA pricing codes or of structural unemployment.

2. The data for the early years of World War II are also atypical, though less so.

3. The remainder of the data “show a consistent [Phillips Curve] pattern”

4. The Phillips curve shifted upward “slightly but noticeably” in the 1940s and 1950s. In the earlier period “manufacturing wages seem to stabilize absolutely when 4 or 5 per cent of the labor force is unemployed,” but since 1946 “one would judge now that it would take more like 8 per cent unemployment to keep money wages from rising.”

5. The data may or may not represent an aggregate supply curve. If so, the movements along it indicate demand pull and shifts indicate cost push. But if employers in anticipating full employment give wage increases during slack periods, this makes it problematic to interpret the Phillips curve relationship as an aggregate supply curve.

Samuelson and Solow conclude on this pessimistic note:

We have concluded that it is not possible on the basis of a priori reasoning to reject either the demand-pull or cost-push hypothesis, or the variants of the latter such as demand-shift. We have also argued that the empirical identifications needed to distinguish between these hypotheses may be quite impossible from the experience of macrodata that is available to us; and that, while use
of microdata might throw additional light on the problem, even 
here identification is fraught with difficulties and ambiguities 
(1960, 191).

Despite their pessimistic acknowledgment of the difficulties, Samuelson and 
Solow ventured “guesses” portrayed in their figure 2, which is a smooth, non-
linear Phillips curve “roughly estimated” from the most recent twenty-five years 
of data. The guesses are that:

1. five to six per cent unemployment is required to have wage increases that 
   match productivity growth,

2. four to five percent inflation is required to keep unemployment at three per 
   cent.

They warned that the policy trade-offs indicated by their Phillips curve were at 
best short-term. The trade-offs could well change in the future. Nonetheless, their 
diagram and inferences are surprisingly precise in light of the serious difficulties 
they brought to light about drawing inference from the data.

Shortly after he presented the paper with Solow at the 1959 AEA meeting, 
Samuelson wrote an evaluation of Federal Reserve policy. The primary question 
on his mind was what might be inferred from both the Fed’s policy record and 
criticisms that the Fed has waited overly long to ease credit conditions in 1957. 
Samuelson took issue with two lines of criticism – the claim that monetary policy 
was powerless and the claim that the Fed would gain from a fixed policy rule. His 
argument against a policy rule was based on the same presumption as Milton 
Friedman’s argument for a policy rule – that little was known of the complexities
of the macroeconomy. Where Friedman drew the implication from economists’ ignorance that a rule could be used to minimize mistakes, Samuelson drew the implication that the rule itself was likely to be ill designed and thus exacerbate business cycles. He advocated policy based on two principles: “prudent man” forecasting and willingness to respond quickly to changing conditions.

I would say that the problem of lags should predispose us even more toward the following view: instead of adapting policy passively to the recent past, the authorities should try to form a judgment of what a prudent informed man thinks the rough probabilities are for a couple of quarters ahead and should take action accordingly, being perfectly prepared to change their tack as new evidence becomes available to modify these prudent probabilities (1960b, 264).

Who is the “prudent informed man”? Is he a mathematical economist?

**Friedman as Macroeconomist**

Friedman and Anna Schwartz embarked on their National Bureau project on money and business cycles in 1948.\(^9\) A year before they began Friedman wrote “A Monetary and Fiscal Framework for Economic Stability” (1948), which was his response to the problem that Samuelson and Solow (1960) and Samuelson (1960) considered, that the macroeconomy is a complex system about which relatively little is known. Friedman’s conclusion was a far cry from Samuelson’s prudential man forecasting and leaning more quickly against the changing winds.

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\(^9\) See Hammond (1996, ch. 3) for an overview of the background and early years of the Friedman and Schwartz project.
He developed a policy rule for monetary and fiscal policy, a rule of the type that Samuelson would later refer to as “automatic servomechanisms.” Friedman told Anna Schwartz that this article indicated his thinking about the nature of business cycles and prospects for their mitigation. He regarded lags in timing and price rigidities as the two fundamental features of business cycles presenting challenges for policy. Friedman’s proposal was to eliminate private creation and destruction of money with a 100 percent reserve requirement, and eliminate discretionary changes in money supply by requiring that federal government budget deficits and surpluses be totally monetized. The budget rule would be to balance the budget on a full employment basis. Friedman evaluated how his proposal would fare in comparison with the status quo of discretionary policy, acknowledging that his conclusions were based on conjectures rather than firm evidence of the length of lags. He concluded that he could not rule out that his rule would be more destabilizing than the discretionary policies.

About all one can say about this possibility is that the completely automatic proposal outlined above seems likely to do less harm under the circumstances envisaged than alternative proposals which provide for discretionary reactions. There is a strong presumption that these discretionary actions will in general be subject to longer lags than the automatic reactions and hence will be destabilizing even more frequently (1948, 145).

Tellingly for understanding his approach to business cycles, Friedman called for study of the length and regularity of monetary and fiscal policy lags. In contrast
with Samuelson’s ability to begin and finish a formal theoretical project on his own in a brief time, Friedman’s empirical and historical work involved a team of researchers including Anna J. Schwartz, Phillip Cagan, David Meiselman, John J. Klein, Richard T. Selden, Eugene Lerner, and other students in his money and banking workshop, and the project continued for years. He presented the first somewhat complete results to the Joint Economic Committee of the U.S. Congress in 1958, a decade after his call for this research.\textsuperscript{10} By that point, Friedman had modified his rule to the familiar constant growth rate at 3 to 5 percent per year. He wrote:

The extensive empirical work that I have done since that article [“A Monetary and Fiscal Framework for Economic Stability” (1948)] was written has given me no reason to doubt that the arrangements there suggested would produce a higher degree of stability; it has, however, led me to believe that much simpler arrangements would do so also; that something like the simple policy suggested above would produce a very tolerable amount of stability. This evidence has persuaded me that the major problem is to prevent monetary changes from themselves contributing to instability rather than to use monetary changes to offset other forces (1958, p. 106, n. 19).

Friedman and Schwartz’s “Money and Business Cycles” (1963) illustrates the difference in Friedman’s heavily empirical approach to macroeconomics and Samuelson’s approach as we have seen it in the several articles. Friedman and

\textsuperscript{10} See also Friedman (1959, 1960, 1961).
Schwartz used thirty-two pages to present and analyze extensive data records of money and business cycle turning points, with data covering the period from 1867 to 1960. They observed first that the money stock tended to rise rather than fall through most business cycle contractions. They removed the positive trend from the series by taking logarithmic first differences and examined patterns in rates of change in the money stock over deep and mild contractions. Then they presented the data both in charts and in numerical tables to uncover the cyclical timing and amplitude of money growth through NBER reference cycles. [F&S figures and tables] In their analysis everything is out on the table. Friedman and Schwartz made interpretive judgments about patterns in their data, as Samuelson and Solow did about hourly earnings changes and unemployment, but they gave their readers all the information they would need to make their own judgments.

Their conclusions for major business cycles were that:

1. There is a one-to-one relation between money changes and changes in money income and prices, ...

2. The changes in the stock of money cannot consistently be explained by the contemporary changes in money income and prices (1963, 50).

By this they meant that although causation goes both ways between money and nominal income, money has an active role in the business cycle. The action is not entirely from business to money.

There seems to us, accordingly, to be an extraordinarily strong case for the proposition that (1) appreciable changes in the rate of growth of the stock of money are a necessary and sufficient
condition for appreciable changes in the rate of growth of money income; and that, (2) this is true both for long secular changes and also for changes over periods roughly the length of business cycles. To go beyond the evidence and discussion thus far presented: our survey of experiences leads us to conjecture that the longer-period changes in money income produced by a changed secular rate of growth of the money stock are reflected mainly in different price behavior rather than in different rates of growth of output; whereas the shorter period changes in the rate of growth of the money stock are capable of exerting a sizable influence on the rate of growth of output as well (1963, 53).

From their analysis of the evidence Friedman and Schwartz provided their own version of what Samuelson strived for and was generally acknowledged by other economists to have attained – a unified theory of economic phenomena. Only for Samuelson the unification was in the mathematical method of constrained optimization. Friedman and Schwartz’s unification was in observed empirical regularities, in a monetary theory of business cycles.

Friedman and Schwartz were well aware that their explanation of business cycles was in competition with others, such as the Keynesian theory that investment was the prime cause.

It is perhaps worth emphasizing and repeating that any alternative interpretation must meet two tests: it must explain why the major movements in income occurred when they did, and also it must
explain why such major movements should have been uniformly accompanied by corresponding movements in the rate of growth of the money stock. The monetary interpretation explains both at the same time. …

We have emphasized the difficulty of meeting the second test. But even the first alone is hard to meet except by an explanation which asserts that different factors may from time to time produce large movements in income, and that these factors may operate through diverse channels – which is essentially to plead utter ignorance (1963, 54).

**Conclusion**

It may be useful to think of an economist’s expertise on matters economic in terms of contour lines closer to and further from a center point. At the center point is the economist’s area of personal expertise, the area in which he or she researches and writes. On this point are Friedman’s and Samuelson’s scientific contributions. On the first contour line are the areas in which the economist has some expertise from reading others economists’ scholarship, and from his general training in economics. Further still from the center point are areas within the scientific realm but outside economics that bear directly or indirectly on matters economic. Still further out is the general intellectual culture, the ideas that are taken for granted by the majority of people in the intellectual, political, and publishing world.
The center point for Samuelson was mathematical economic theory. This was his particular contribution to economic science. Yet his vision of what economic science is was broader than what he did. Indeed he considered economics to be like all science empirical. Theory must accord with empirical facts. “Properly understood, therefore, theory and observation, deduction and induction cannot be in conflict. Like eggs, there are only two kinds of theories: good ones and bad ones. And the test of a theory’s goodness is its usefulness in illuminating observational reality. Its logical elegance and fine-spun beauty are irrelevant” (1948, 8). So when Samuelson commented on concrete economic problems such as business cycles and macroeconomic policy, for his comments to have epistemic value he would necessarily be dependent on scientific work of other economists, who constructed, sorted, and matched data with theory to separate the more realistic from the less realistic theories.

But the macroeconomic analysis of Samuelson that we have reviewed is subject to the criticism that Friedman made of Oscar Lange’s *Price Flexibility and Employment*. It is either a priori taxonomic analysis, as in his 1939 generalization of multiplier analysis, or relies on a mixture of simplifying assumptions and factual presumptions about the nature of the economic system, as in his 1940 investigation of the possibility of self-financing budget deficits, or back-of-the-envelope analysis of data, as in his and Solow’s article on the Phillips curve. In a 1967 discussion with Arthur Burns, Samuelson described his forecasting technique:
I am not now referring to the regressions of the computer but I am speaking now of the regressions of the mind, the intuitive forecasting which I do. The other day a colleague of mine … said to me, “Paul, how long do you think it will take before a computer will replace you?” … I thought for a moment, and as the question seemed to be asked in a mean way, I replied, “Not in a million years” (Burns and Samuelson, 1967, pp. 92-93).

The center point for Friedman was compilation and analysis of data, with theory and data in dialogue with each other. Conclusions, in the forms of answers or of completion of projects, were not expected to come quickly. Friedman’s research project on monetary aspects of business cycles was quite literally a component of the business cycles project begun by Wesley C. Mitchell in the 1910s. Mitchell worked on the project for most of his career, then, upon his retirement 1945 passed it off unfinished to Arthur F. Burns. Burns passed a piece of it to Friedman and Anna J. Schwartz, who concluded their portion of the project, unfinished, in 1982.\footnote{There plan was for three volumes, Monetary Statistics, Monetary Trends, and Monetary Cycles. The Monetary History was to be a chapter in Monetary Cycles.}

Friedman’s approach may or may not have allowed him to discover the mysteries of the business cycle. On one level his project was a failure during the period in which he was actively engaged in monetary and business cycle research, for he never convinced his fellow economists outside the Chicago School bloc, whose standards were based on theoretical model characteristics rather than
empirical evidence. After Friedman retired from active research economists were more inclined to give him credit for being largely right about the role of money in business cycles. Whether he was mostly right or not, he at least was not forced by the nature of his expertise, the center point in my contour line image, to reach out to further removed contours in order to have anything to say about concrete problems. A mathematical economist such as Paul Samuelson, it appears to me, cannot say much of anything about concrete policy questions on the authority of his mathematical economics. This leaves us with a pair of questions. When distinguished mathematical economists evaluate policy or make forecasts, does their word have any more credence than the pundit one might encounter on the 24 hour news channel? If so, on what is their additional credence based?

References


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