

FRIEDMAN AND SAMUELSON ON THE BUSINESS CYCLE

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Chicago School economists have come in for criticism since the financial crisis and so-called Great Recession began in 2007. Commentators have blamed recent problems on a laissez-faire faith in the efficacy of markets and simple rules for business-cycle policy—ideas associated with economics as taught and practiced at the University of Chicago. Events over the past four years, we are told, demonstrate the need for a restoration of Keynesian thinking about business cycles and activist government policies to keep markets from failing. However, there is another aspect of Chicago School economics that is commonly overlooked. This is the conviction that economists' understanding of the business cycle is meager in light of the knowledge necessary for activist countercyclical policy to be effective. From this comes the Chicago School concern that economists and policymakers not attempt to do something beyond their capability. Overreaching can make the problems worse.

In the public mind, Milton Friedman and Paul Samuelson represent more than any other individuals two competing schools of thought that dominated macroeconomic and business cycle debates over much of the past century. As readers of their *Newsweek* columns

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from the late 1960s into the 1980s learned, Friedman was the “conservative” Chicago economist favoring free markets, deregulation, and rules-based monetary policy. Samuelson was the “middle-of-the-road” economist, favoring regulatory oversight of markets and activist monetary and fiscal policy. Friedman was the monetarist and Samuelson the Keynesian. Friedman died in 2006, so we do not have his commentary on the current crisis. Samuelson died in 2009, and before his death spoke with journalist Nathan Gardels of his and Friedman’s respective ideas and influence in light of the crisis.

Nathan Gardels: You have outlived Milton Friedman, who died in 2006. And now your Keynesian ideas have also outlived his radical free-market ideology. Is economics back to where you started?

Paul Samuelson: You are right. I am old enough to have seen the cycle come full circle. My experience is more valuable now than it was even a year ago, since I first became actively engaged in economic policy on January 2, 1932, at the rock bottom of the Great Depression, when I was an adviser to the Federal Reserve Bank in Washington. In subsequent years, I was principal economic adviser to President-elect John F. Kennedy in 1960 and recruited the team for his Council of Economic Advisers.

I became a centrist early on. Of course, the central planning system of the socialist states we still contested with ideologically in those days was idiotic, but that didn’t mean government doesn’t play a critical role.

And today we see how utterly mistaken was the Milton Friedman notion that a market system can regulate itself [Samuelson 2011].

As Americans struggle in the current climate with what to believe about economic conditions and policy, it is instructive to look back at the ideas on business cycles and macroeconomic policy of these two giants of 20th century economics.

Friedman: NBER Economist

The question of how much economists know about the business cycle, and thus how much expertise they can bring to the policymaking table, including crucially their ability to forecast business conditions, was an important part of what separated Friedman’s

views from the mainstream over the course of his career (see Hammond 1996). A good place to begin seeing this difference is in the late 1940s, as Friedman and Anna J. Schwartz embarked on their monetary project for the National Bureau of Economic Research (NBER). Friedman's background was in statistics and to a smaller extent business cycle analysis, but he had little experience with monetary economics at the outset of their project. His two most important mentors, Arthur F. Burns and Wesley C. Mitchell, had instilled in him firm convictions of how to do economics. From Burns he learned the Marshallian approach to economics, which involved use of relatively lowbrow theory in close relation with measureable entities—theory that could be used to extract useful information from data. From Mitchell Friedman learned the techniques used at the NBER to analyze business cycle data, and he also learned that constructing economic data is as important as constructing economic theory.

Friedman's early perspective on business cycle analysis is evident in his review of Jan Tinbergen's *Business Cycles in the United States of America, 1919–1932*. Tinbergen's book was one of the early attempts to estimate coefficients of a general equilibrium model of the business cycle, work for which he was awarded the very first Nobel Prize in Economic Science (shared with Ragnar Frisch). Tinbergen and Frisch were cited "for having developed and applied dynamic models for the analysis of economic processes" (http://nobelprize.org/nobel_prizes/economics/laureates/1969/#). Friedman was less impressed in 1940 than the Nobel committee later was in 1969. He wrote of the estimates:

Tinbergen's results cannot be judged by ordinary tests of statistical significance. The reason is that the variables with which he winds up, the particular series measuring these variables, the leads and lags, and various other aspects of the equations besides the particular values of the parameters . . . have been selected after an extensive process of trial and error *because* they yield high coefficients of correlation [Friedman 1940: 659].

Friedman quoted his teacher Wesley Mitchell to the effect that a statistician can take almost any pair of data series and manipulate them to obtain a high correlation coefficient between the two. What Tinbergen failed to do is to test his model with data from outside the

sample that he used to estimate the coefficients. Friedman did such a test in a rudimentary form and found that the model did not explain the out-of-sample observations very well.

A decade later Friedman commented on another test of a general equilibrium business cycle model. In this case Carl Christ tested the model on out-of-sample data, as Friedman had suggested for Tinbergen. But Friedman was still not impressed with the results. He set up an alternative, and extremely simple, model in which the values of endogenous variables were predicted to be unchanged from period to period. This was in effect running a contest between a highly sophisticated model of the business cycle and “we know nothing about the business cycle.” “We know nothing” won the contest! Friedman concluded that economists using the big general equilibrium “system” models were striving for something well beyond their reach. Greater progress would be made in “analysis of parts of the economy in the hope that we can find bits of order here and there and gradually combine these bits into a systematic picture of the whole (Friedman 1951: 114).

We tend to think of Friedman as a monetarist and economists on the other side of debates about business cycles as Keynesians. But the critiques we just examined were before Friedman and Schwartz began their money and business cycles project, and therefore before he was a monetarist. The position he represented was the NBER approach to business cycle analysis. His opponents in the 1940s tended to be Keynesians, but the pressing issue was not so much what one thinks are the causes and cures for the business cycle, as how one searches for answers to the question and how much is known. A good illustration of this debate is in Arthur Burns’s 1946 annual report of the NBER, where Burns was director of research. Burns criticized Keynesians for presuming that they had figured out the business cycle, and for relying on theory with scant resort to economic data other than highly aggregated data. Keynesians saw compensatory fiscal policy as the solution to the cycle. Referring to the set of assumptions behind the analysis, about the shape and stability of the consumption function, the relative size of consumption effects of tax cuts and tax hikes, and so forth, Burns (1946: 11) concluded:

Although assumptions such as these may be extremely helpful at a stage in our thinking about an exceedingly

complicated problem, it seems that the inferences to which they lead cannot be regarded as a scientific guide to governmental policies.

Burns (1946: 21) continued:

Keynes' adventure in business cycle theory is by no means exceptional. My reason for singling it out is merely that the *General Theory* has become for many, contrary to Keynes' own wishes, a sourcebook of established knowledge. Fanciful ideas about business cycles are widely entertained both by men of affairs and by academic economists. That is inevitable as long as the problem is attacked on a speculative level, or if statistics serve only as a casual check on speculation. To develop a reliable picture of the business cycles of actual life it is necessary to study with fine discrimination the historical records of numerous economic activities. . . . Work on this plan is costly and time-consuming; it means turning back, revising, rethinking, redoing; it often leads to disappointments and taxes patience. But there is no reliable shortcut to tested knowledge.

Friedman, Mitchell, and Burns's approach to business cycle analysis and their sense of what was known and unknown about cycles was viewed as outdated by many in the midst of enthusiasm for Keynesian theory and mathematization of economics and statistics. With the mathematization of economic theory and statistics economists developed a hubris for which experience at the National Bureau provided immunity. This hubris is in full flower in Paul Samuelson's writings about the business cycle.

Samuelson: Mathematical Economist

Paul Samuelson was a mathematical economist, whose work was by and large pure theory, without empirical data. In the autobiography he wrote for the Nobel Prize 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. Lloyd Metzler's review of Samuelson's *Foundations of Economic Analysis* (Metzler 1948), which was Samuelson's Ph.D. dissertation, noted that the book was a contribution to economic method, with 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" (Metzler 1948: 910).

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 Frisch and 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).

Nonetheless, Samuelson regarded economics and all science as empirical. 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" [Samuelson 1948: 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 [Samuelson 1948: 8].

Several of Samuelson's earliest papers 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* (Samuelson 1947) 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—that is, 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 a constant marginal propensity to consume and a constant 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 [Samuelson 1939: 78].

In the 1940 article Samuelson considered whether a countercyclical fiscal deficit might be self-eliminating—that is, 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” (Samuelson 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 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:

The economic system is not perfect and frictionless so that there exists the possibility of unemployment and underutilization of productive resources.

There exists the possibility of, if not a definite tendency toward, cumulative movements of a disequilibrating kind.

The average propensity to consume is less than one, at least at high levels of national income.

Even in a perfect capital market there is no tendency for the rate of interest to equilibrate the demand and supply of employment.

There exist no technical difficulties to prevent the government from financing deficits of the magnitudes discussed [Samuelson 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—that is, 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” (Samuelson 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 the public sector, from misguided fiscal policy or monetary policy. What Friedman and Schwartz were to later conclude about the Great Depression and what many economists believe exacerbated the recent real estate bubble is ruled out a priori.

At a 1959 American Economic Association (AEA) session on price level stability, Samuelson and Robert Solow devoted more than half of their discussion to impediments to the use of historical data for identification of 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 United Kingdom, the Phillips Curve. From a scatter plot of U.S. data on unemployment rates and increases in hourly earnings, a plot without showing actual numerical values, they offered suggestions about the Phillips Curve for the United States. They began 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 [Samuelson and Solow 1960: 188].

The systematic effects that they inferred from the plot were:

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 percent of the labor force is unemployed,” but since 1946 “one would judge now that it would take more like 8 percent 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 (1960: 191) 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.

Despite their pessimistic acknowledgment of the difficulties, Samuelson and Solow (1960) ventured “guesses” portrayed in their Figure 2, which is a smooth, nonlinear Phillips Curve “roughly estimated” from the most recent 25 years of data. The guesses are that (1) an unemployment rate of 5–6 percent is necessary for wage increases to match productivity growth, and (2) to keep unemployment at 3 percent, inflation must be 5 percent.

They warned that the policy tradeoffs indicated by their Phillips Curve were at best short-term. The tradeoffs 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 inferences 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 had waited overly long to ease credit conditions in 1957. Samuelson took issue with two lines of criticism: (1) the claim that monetary policy was powerless, and (2) the claim that the Fed would gain from a fixed policy rule.¹ His argument against a policy rule was based on the same presumption as Milton Friedman’s argument for a policy rule—namely, 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 [Samuelson 1960: 264].

¹The previous year Friedman had proposed a fixed money stock growth rate rule in testimony before the Joint Economic Committee (see Friedman 1958).

Samuelson's statement brings to the foreground the question, Who is the "prudent informed man"? Is he a mathematical economist, a pure theorist, or one with a more empirical bent who looks long and hard at data?

Theory, Evidence, and Prudence

Anna Schwartz brought experience and expertise in money and banking to the monetary factors in business cycles project that she and Friedman took up at the request of Arthur Burns in 1948. She had compiled a data series for currency covering the period 1917 to 1944, and was working on the companion series for bank deposits. In spring 1948 she sent Friedman a list of readings on monetary and banking history, warning him that the literature was pretty bad, but suggesting that he would acquire less misinformation from the readings on her list than from others. Friedman spent the summer reading and joined Schwartz in the work of compiling data. This is a point worth noting. Friedman and Schwartz began their monetary project not by reading monetary theory or macroeconomic theory, but by building data and reading banking history. And this was to become a hallmark of their approach to monetary economics; their work was empirical and historical.

Friedman made several proposals for reforming monetary policy over the course of his career. The proposals were all in the direction of streamlining and simplifying policy, and protecting the public from arbitrary use of power by policymakers. In *A Program for Monetary Stability* (1960) he proposed confining monetary policy to a single instrument, open market operations; requiring 100 percent reserves on all bank deposits; and requiring that open market operations be guided by a money stock growth rate rule. Friedman acknowledged that his proposal for a fixed money stock growth rule was counterintuitive. In theory "leaning against the wind"—discretionary policy—looked better than a "do nothing" fixed money growth rate rule. But Friedman predicted that in practice the rule would provide more stability than discretionary "leaning against the wind."

Why? First, because the empirical evidence compiled by Friedman and Schwartz suggested that changes in the growth rate of

the money stock had effects that were long and variable.² This meant that in order to effectively lean against the wind, the Fed would have to lean against future winds. Not only that. Because of the variability of the lag, they would have to lean against a wind that would be blowing at an uncertain time in the future. Second, he opposed leaving policy open to Fed officials' discretion because countercyclical policy was open to different interpretations as to content. For example, is the policy objective price level stability or low unemployment, or both; and how stable and how low; and stable and low over what time frame? It was too easy to agree that the Fed should lean against the wind because that directive was a container with a "stabilization" label, but without definite content. Therefore, people with diverse ideas of the content could agree *ex ante* that the Fed should lean against the wind, but have little basis for agreement *ex post* about whether it had effectively done so. Friedman thought that disagreement, uncertainty, and lack of accountability were built in to any system without a clear policy target.

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 not only himself and Anna Schwartz, but a host of students in the Workshop in Money and Banking.³ Where Samuelson's goal was a unified theory of disparate economic phenomena, Friedman's goal was an empirically verified theory of one particular economic phenomenon, the business cycle. 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. In recommending that the growth rate of the money stock be set at a constant 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

²On average 16 months from the peak in money growth to the peak in general business activity, and 12 months from trough to trough, with the range of lags from 6 to 29 months for peaks and 4 to 22 months for troughs.

³In the early years his students included Phillip Cagan, David Meiselman, John J. Klein, Richard T. Selden, and Eugene Lerner.

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 [Friedman 1958: 106, n. 19].⁴

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 several articles. Friedman and Schwartz used 32 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. 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 changes in hourly earnings and unemployment, but they presented all the information readers 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," and (2) "The changes in the stock of money cannot consistently be explained by the contemporary changes in money income and prices" (Friedman and Schwartz 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. In particular,

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

⁴See also Friedman (1959, 1960, 1961).

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 [Friedman and Schwartz 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 explanation 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 [Friedman 1958: 54].

Conclusion

Paul Samuelson was a vigorous advocate for the mathematization of economics, recognizing the particular virtue of math in laying bare

logical relationships. But mathematical general equilibrium did not equip him to say much at all about economic conditions and policies of any particular time and place. This task was left to the “prudent informed man.” Presumably the prudent man would be informed about empirical regularities, for in his principles textbook Samuelson affirmed that science is based on observation. “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” (Samuelson 1948: 8).

Friedman (1946) observed in a critique of Oscar Lange’s *Price Flexibility and Employment*, an example of the mathematical approach used and advocated by Samuelson, that economists using this approach to deal with real-world problems invariably resort to empirical observations and claims. In contrast with the rigor and clarity of their mathematical theory, their observation of data and institutions tends to be casual and obscure. We have seen this to be the case with Samuelson. 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: 92–93].

Friedman was more modest about what he knew, less sanguine about what any experts knew, and believing in the power of monetary policy, more wary of the potential for harm from misguided policies. He also was committed to systematic examination of data bearing on the business cycle. In the words of his mentor Arthur Burns, Friedman believed that “there is no reliable shortcut to tested knowledge.” The program in business cycle research on which Friedman and Schwartz embarked in 1948 was begun by Wesley Mitchell at the beginning of the 20th century. After more than half a century of painstaking research the results were still “provisional.” The project had produced knowledge, but not of the type and detail that would allow macroeconomic fine-tuning. We would do well to keep this

in mind as politicians, pundits, and government economists make claims that they have unlocked the mysteries of the business cycle.

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