

Friedman's Methodology Essay in Context

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I have devised seven separate explanations, each of which would cover the facts as far as we know them. But which of these is correct can only be determined by the fresh information which we shall no doubt find waiting for us.

*Sherlock Holmes, **The Adventure of the Copper Beeches***

Milton Friedman's methodology is built from a presumption that the goal of economic science is theory "that yields valid and meaningful (i.e., not truistic) predictions about phenomena not yet observed" (1953, p.7). The "not yet observed" phenomena may lie in the future or in the past. Toward this goal Friedman argued in "The Methodology of Positive Economics" that the only relevant test of the validity of theory is "comparison of its predictions with experience" (1953, pp.8-9). A half century after Friedman published the essay we are assembled to assess it from a number of perspectives: its meaning, coherence, and influence. My paper uses evidence of the context from which Friedman wrote to call into question the standard interpretation of the essay's central message.

A single-statement summary of Friedman's essay likely to come to mind for people familiar with it is, "unrealistic assumptions do not matter while predictive success

is all that matters in assessing a theory.”¹ Mark Blaug opens his discussion of the essay in *The Methodology of Economics* (2nd ed.) with the following statement: “its central thesis that economists should not bother to make their assumptions ‘realistic’ aroused a storm of controversy that took almost a decade to die down” (p. 90). To this interpretation of the “central thesis” Blaug attaches a footnote recalling a popular joke about economists, a joke that parodies the essay. “An economist, an engineer and a chemist were stranded together on a desert island with a large tin of ham but no tin-opener. After various unsuccessful exercises in applied science by the engineer and the chemist aimed at opening the tin, they turned in irritation to the economist who all the while had been wearing a superior smile. ‘What would you do?’, they asked. ‘Let us assume we have a tin-opener’, came the unruffled reply”(p. 90, n.26).

Among the reasons for Friedman to write the essay as he did was a series of articles arguing that lack of realisticness of its assumptions rendered neoclassical price theory irrelevant to real-world economic problems. Critics such as R. A. Lester (1946), and R.J. Hall and C. J. Hitch (1939) called for greater realism in assumptions. They thought businessmen did not and could not make decisions the way neoclassical theory assumed. Other critics pointed out that real-world markets are unlike those portrayed by the models of perfect competition and perfect monopoly. Defending neoclassical theory, Fritz Machlup (1946, 1947) and George Stigler (1947) argued that the theory was realistic. Friedman chose not to join this defense but rather to argue that both the critics and the defenders were falling prey to methodological error. The charges of unrealisticness were not wrong, but irrelevant. He argued instead that realisticness of

¹ This particular summary statement is taken from Uskali Maki’s book proposal for this conference.

assumptions does not matter so long as the theory predicts well, i.e., so long as there is realisticness in the theory's implications.

Readers of the essay have taken this as its central message, and it certainly is a message that is prominent in the essay. Furthermore, the essay seems to say, the less realistic the assumptions, the better the theory is likely to be! I believe this last interpretation is a distortion of Friedman's actual claim, which was merely an argument that simple theory is preferable to complex theory. To suggest, as Friedman did, that "the more significant the theory, the more unrealistic the assumptions" does not imply that making assumptions less realistic necessarily makes theory more significant. He meant only that significant theories will necessarily have unrealistic assumptions. But even putting this aside, I will argue that what remains as the standard summary of Friedman's thesis is based on a thin interpretation. The interpretation is thin in that it gives excessive weight to the choice between testing via assumptions and testing by implications. If the weight given this choice is excessive, what is underweighted in the standard interpretation? I believe this is the choice between testing theory empirically and not doing so, i.e., *whether* to test empirically. Both testing by the realisticness of assumptions and testing by success of predictions are means of testing empirically. The alternative that Friedman most opposed was foregoing empirical testing altogether.

Friedman himself must shoulder some of the blame for exaggerating the importance of *how* to test empirically. For one thing, this question takes up a large portion of the essay's text. Readers' first encounter with the argument that realisticness of assumptions does not matter is six pages into the essay where Friedman writes:

Viewed as a body of substantive hypotheses, theory is to be judged by its predictive power for the class of phenomena which it is intended to ‘explain.’ ... As I shall argue at greater length below, the only relevant test of the *validity* of a hypothesis is comparison of its predictions with experience (1953, p. 8, emphasis in original).

After explaining what he means by predictions, and discussing the difficulty of obtaining the appropriate data outside controlled laboratory settings, he mentions two alluring escapes from the difficult task of testing by predictions. The first is “a retreat into purely formal or tautological analysis” (p.11). In the space of only two paragraphs, he argues that this retreat is misguided, for it reduces economics to disguised mathematics. Then, having established, however briefly and partially, that it is important to bring empirical evidence to bear on theory, Friedman moves to the question of where in the theory this should be done. First he distinguishes use of evidence to test theories from use of evidence to construct theories. Then, twelve pages into the forty-one page essay, we come to the second escape from the difficulty of obtaining evidence for testing predictions, which is testing theory by the realisticness of its assumptions. Friedman devotes the remainder of the essay to this topic.

It is not surprising that readers have taken Friedman’s central message to be that testing theory by the realisticness of assumptions is inappropriate; the accuracy of predictions is the only legitimate test. But here I part company with many other readers. Notwithstanding the evidence I just presented, I believe this issue was less important to Friedman than it has become in the life of the essay since 1953. If I may borrow the metaphor of “links in a chain” from Tom Mayer’s *Truth Versus Precision in Economics*

(1993), the argument that realisticness of assumptions does not matter is one link in Friedman's methodology. It is the largest and shiniest link in the essay. Friedman made it thick and put a high polish on it. So it is not surprising that this link has drawn so much attention. But this has distracted us from the rest of the chain.

My assertion that the realisticness argument is not as central as it appears flies in the face of Friedman's revealed preference. Why, you might ask, would he devote so much attention to this if it was not in fact the most important issue? Why make this link so prominent? One reason is that Friedman was reacting to debates in the literature at the time he wrote the essay. These debates were a proximate cause of his writing the essay as he did. He began writing it in late 1947, probably finishing the first draft in the summer of 1948.² Through the two years prior to his starting the essay there was a series of articles in the *American Economic Review* criticizing and defending the assumptions about business practice that underlay marginal analysis of the firm. Friedman joined this debate with the idea that the critics and defenders of marginal analysis alike were making a methodological error. So, when he began the essay the issue on the table was realisticness of assumptions, and he made that the central focus of the essay. But at a more fundamental level this was only part of what bothered Friedman about contemporary methodological trends.

In the 1948 draft, as in the published essay, Friedman gave a second example of what he considered flawed methodology. This was the move to replace perfect competition and monopoly with monopolistic competition. He dealt with the marginalism issue first, saying that it was the clearer example, but stated that he regarded the push for

² The title of this draft is "Descriptive Validity vs. Analytical Relevance in Economic Theory."

monopolistic competition more important. This is a hint of what we will find by digging further into the foundations of the essay.

Friedman's first forays into methodology were two book reviews (1940, 1941), written while he was a full-time staff member of the National Bureau of Economic Research and on the faculty of the University of Wisconsin. The reviews were of Jan Tinbergen's *Business Cycles in the United States of America, 1919-32* and Robert Triffin's *Monopolistic Competition and General Equilibrium Theory*. In the review of Tinbergen Friedman makes a distinction between uses of data to test and to derive hypotheses. His main criticism is that Tinbergen did not test his business cycle model with data other than those used to estimate the equations. Tellingly, the two references in the review are W. C. Mitchell, *Business Cycles, The Problem and Its Setting* (1927), and A.F. Burns and W. C. Mitchell, *Methods of Measuring Business Cycles* (1946).³ The authors of these references were former teachers, and at the National Bureau, colleagues of Friedman.

Robert Triffin sought to reorient the theory of monopolistic competition away from the Anglo-American tradition of Marshallian analysis toward continental Walrasian analysis. To him this meant dispensing with the concept of industry and moving the orientation simultaneously in two directions, inward toward the single firm and outward toward the "whole economic collectivity." Triffin wrote:

The appearance of monopolistic competition assumptions has been
a new step in the historical process of purification and formalization of

³ The Burns and Mitchell volume was not then published. Thus the difference in the title given by Friedman and in the book's actual title.

economic theory. The analysis loses in content, while gaining in generality. ...

On the other hand, we shall find that an increasing number of situations elude the grip of the traditional weapons of pure economics. This raises the question of whether we should not, reversing the historical process of growing generalization just mentioned, enlarge the present box of assumptions of pure theory so as to enable us to tackle these cases; again, the required assumptions should be chosen on an empirical basis, and a price will have to be paid in the form of a lesser generality for the ensuing analysis (1941, pp. 15-16).

Friedman's reviews of these two books portend much of what he wrote later on methodology. In one review he acknowledges that observed facts have their place at both ends of theory, in deriving theory and in testing it. He insists that testing has to be with data other than those from which the theory is derived. In the other review Friedman does not contest Triffin's assertion that the logic of monopolistic competition implies that one not group firms into Marshallian industries. He agrees with this. He rejects Triffin's choice of monopolistic competition over Marshallian industry analysis. And he does so because he thinks most of the practical problems for which economists want to use theory are at the level of industries, not at the level of firms or of the "whole economic collectivity." If industries have no place in monopolistic competition, then monopolistic competition must go.

In March 1943 Friedman joined the staff of the Statistical Research Group at Columbia University as Associate Director. While there he wrote a review (1941) of a

Temporary National Economic Committee publication by Oscar Altman, *Saving, Investment, and National Income* (1941). The book was a Keynesian analysis with empirical data on savings and investment. Friedman's criticism was that the data were not of the type that could be used to test the theory. Apart from the data's value on their own, Friedman found that they added little value to Altman's analysis. The data served only as window dressing for a mechanical application of the Keynesian savings-investment apparatus to the problem of full employment level of income.

The theoretical analysis in Altman's monograph is set against an extensive factual background of data on saving and investment to which the bulk of the monograph is devoted. This comprehensive compilation of data on the aggregate volume of investment, the size of various components of saving and investment, their concentration in various groups in the economy, and the like, is excellent and of considerable interest in its own right as a description of one aspect of our economy. But it is presented as if it had direct relevance to the theoretical analysis, as if it directly substantiated the assertions about the conditions under which full employment is attainable which are the major *raison d'etre* of the monograph. Yet the relation between the figures and the theoretical analysis is nowhere considered (1944, pp. 101-2).

It is significant that Friedman wrote with this emphasis on bringing data into contact with theory while at the Statistical Research Group. In the two and one-half years that he was with the SRG he did work on the relative effectiveness of conventional antiaircraft shells and shrapnel shells; the design of fuses for antiaircraft rockets, and

alloy specifications for turbines. He contributed to almost 100 reports and memos on topics for which speculative analysis, unchecked by facts, was simply not acceptable. Lives of Allied soldiers and sailors and the outcome of the war were at stake.⁴

In another book review that Friedman wrote just prior to joining the University of Chicago faculty, he criticized Oscar Lange's use of what he called "taxonomic theorizing," a type of formal theorizing (1946). The distinguishing marks of taxonomic theorizing are that a formal theoretical model is specified in form only, not in content, and is used to speculate on various scenarios that might follow a change in policy or prices. Lange's concern was with the effects of a decline in the price of an underemployed factor of production. He considered direct effects of the change, along with indirect effects, that may reinforce or counteract the direct effects. This kind of theorizing, in and of itself, dispenses with factual analysis. According to Friedman:

The basic sources of the defects in Lange's theoretical analysis are the emphasis on formal structure, the attempt to generalize without first specifying in detail the facts to be generalized, and the failure to recognize that the ultimate test of the validity of a theory is not conformity to the canons of formal logic but the ability to deduce facts that have not yet been observed, that are capable of being contradicted by observation, and that subsequent observation does not contradict (1946, p. 300).

Friedman argued that notwithstanding their emphasis on formal theory, neither Lange nor anyone else employing this approach was content to rely on formal theory alone, without consideration of facts. They invariably brought facts into the analysis to

⁴ See Friedman and Friedman (1998), chapter 8.

rule out some of the countless theoretical possibilities. Thus, striving for formal completeness bred empirical ad hoc-ery.

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. This is, of course, a poor way to escape the shackles of formalism. A far better way is to try to devise theoretical generalizations to fit as full and comprehensive a set of related facts about the real world as it is possible to get (1946, p.300).

Shortly after writing the review of Lange's book Friedman criticized Abba Lerner's *The Economics of Control* along similar lines. He saw the work as formal theorizing void of the quantitative and institutional details required for analysis of countercyclical policy. Without these details policy prescriptions turn out to be nothing more than tautologies and admonitions to do the right thing. To Friedman,

for our present purpose the relevant question is whether the discussion of "functional finance," besides being a logical exercise, is also a prescription for public policy. The answer, it seems to this reviewer, is clearly negative. Once again, what looks like a prescription evaporates into an expression of good intentions. ...

To make this into a prescription to "produce full employment," Lerner must tell us how to know when there is "insufficient total demand,"

whether this insufficiency is a temporary deficiency in the process of being corrected or the beginning of an increasing deficiency that, if left alone, will lead to a drastic deflation. He must tell us how to know what medicine to use when the diagnosis has been made, how large a dose to give, and how long we may expect it to take for the medicine to be effective (1947, pp. 313-14).

These excerpts from precursors to Friedman's essay illustrate more fully the chain of Friedman's methodology. He opposes testing theory by assumptions, but does so within the broader context of arguing that theory should be fact-imbued, from beginning to end, soup to nuts. What are the implications for how we view the 1953 essay? By my reckoning this leaves us with two perspectives for locating Friedman's essay on the mid-twentieth century methodological landscape. On the one hand, the essay is an argument with those who, like Friedman, believed that theories should be made with the aid of empirical facts and judged on the basis of their consistency with empirical facts. It is within this group that the argument over testing assumptions versus testing predictions is joined. On the other hand, the essay is an argument with those who would make theory from stuff other than empirical facts and judge theory on grounds other than the consistency of its predictions with facts. The argument here was with formalists.

*"We have got to the deductions and the inferences," said Lestrade, winking at me. "I find it hard enough to tackle facts, Holmes, without flying away after theories and fancies."
"You are right," said Holmes demurely; "you do find it very hard to tackle the facts."
The Boscombe Valley Mystery*

Commentators on the essay have been puzzled by Friedman's different claims about the role of assumptions, claims that are not always mutually consistent. Friedman argues that the only relevant test of the validity of a hypothesis is the consistency of its predictions with experience. However, he then acknowledges the appeal of testing assumptions and admits that "there is too much smoke for there to be no fire" (1953, p. 23). He devotes an entire section of the essay to "the significance and role of the 'assumptions' of a theory," which includes facilitating indirect tests of hypotheses. Up to this point at least Friedman had a clean straightforward thesis, with which one may or may not agree. Why did he extend the discussion in a way that seems to muddle his argument? Friedman did so because his friends, George Stigler and Arthur Burns, urged him to.

Friedman began discussions with Stigler as he started writing the essay. Their conversation was prompted by Edward Chamberlin's review (1947) of Stigler's, *The Theory of Price*. In his textbook Stigler wrote that monopolistic competition was "impracticable" because of the infinite variety of possible departures from the conditions of perfect competition and monopoly, and because of economists' ignorance of the facts necessary to choose among the multitude of possible assumptions. Chamberlin's review said that the textbook revealed Stigler's "confusion and misconception in rare degree as to what 'imperfect' and monopolistic competition theories are all about" (1947, p. 417). Undecided about writing a response for publication, Stigler asked Friedman for advice. He sent along a copy of a letter he wrote to Chamberlin.

Friedman's reply to Stigler:

I have gotten involved for various irrelevant reasons in a number of discussions of scientific methodology related to the kind of thing you are talking about. In the course of these I have been led to go further than I had before in distinguishing between description and analysis and in discarding comparisons between assumptions in reality as a test of the validity of a hypothesis. I should like to offer the general proposition that every important scientific hypothesis almost inevitably must use assumptions that are descriptively erroneous. It is of the very nature of a really important scientific generalization that it provides a simpler rationalization of a mass of facts than was available before. It is likely to obtain its objective by an inspiration about the particular basic elements of the situation that are important and by discarding what after the event can be shown to have been irrelevant complicating assumptions. In a way, the better the hypothesis the greater the extent to which it simplifies, the more sharply will its assumptions depart from reality (MF to GS, 11/19/47, in Hammond and Hammond).

Friedman then stated his point differently by asking how one knows whether assumptions correspond closely to reality, illustrating the question with the physics problem of a body's rate of fall, with which we are now familiar.⁵ He enclosed with his letter a reprint of his review of Triffin's *Monopolistic Competition and General Equilibrium Theory*.

⁵ Friedman's example was oblique, referring to "how rapidly a body will fall in a vacuum," but without the equation that appeared in the essay. Of the several physical phenomena used in the essay this is the one he first used to explain his point to Stigler. This may not be mere chance, for the compact ball dropped from 30,000 to 20,000 feet in the essay appears as a bomb dropped from 40,000 to 30,000 feet in his letter. When he wrote the letter Friedman was two years away from his work on ordinance for the Statistical Research Group.

By the summer of 1948 Friedman had completed the first draft of his methodology essay. He sent it to Stigler, whose reaction was:

Personally I would like it published. ... But I keep feeling that you arouse skepticism and opposition by stopping where you do. Because surely in some sense an assumption can be more promising than another. ... It is surely possible to say something about some assumptions being more promising than others, and yet not to take back any of the things you are saying at present. If you can pierce this muddy frontier of your article, it would be a great improvement (GS to MF, 9/48, in Hammond and Hammond).

Friedman answered that his strictures applied to only the third of a four-part scientific enterprise, testing hypotheses. Three other parts that were not his concern were: collecting data from which to draw generalizations, deriving hypotheses to generalize uniformities observed in the data, and using the hypotheses. He asked Stigler if his point was the same that Arthur Burns was making over the previous summer, and said that, if so, it applied not to testing theory but to choosing among hypotheses that have not yet been tested. Friedman suggested that at that stage assumptions do play a role in indirect testing. One has confidence in untested hypotheses that are related by their assumptions to others that have proven successful. He also suggested that something akin to the realisticness of assumptions matters at the first stage of the scientific enterprise, in which empirical regularities are observed. Realisticness matters in that good theories are more likely to be produced from actual facts than from false facts.

Shortly afterwards Stigler made a suggestion that turned out to be consequential.

He wrote:

I like your general position but want you to enlarge it, -- precisely as you are enlarging it in your letter to me. While some elaboration along these lines will take some of the paradox out of your thesis (and in a certain sense weaken its message unless you write very carefully), it will create sympathy for and receptiveness to your thesis and make the paper much more influential (GS to MF, 10/48, in Hammond and Hammond).

Friedman's essay was in gestation for a long time. Four years later Stigler read a second draft of the essay, and they still had not settled their differences.⁶ Stigler wrote:

I'm inclined to go along with you on the use of a theory, but what do you think of the following reformulation:

1. After a theory has been developed and tested and much used, its applicability to certain classes of problems becomes established. These classes of problems may be completely specific or objective, as in the use of engineering formulas. Or they may be more loosely specified.

2. At all times there will also be many questions that do not clearly fall within or without the domain of the theory, and only further experiment can tell us whether a given problem should be handled by a given theory (GS to MF, 11/30/52, in Hammond and Hammond).

Stigler suggested that he was thinking primarily of class one, or routine scientific work, and Friedman of class two, improvements in science. So the impetus of Friedman's discussions of methodology with George Stigler was the challenge posed by Walrasian

⁶ The title of this draft is "The Relevance of Economic Analysis to Prediction and Policy."

monopolistic competition to Marshallian industry analysis. But Stigler thought that Friedman's argument against testing by the realisticness of assumptions was overly restrictive, as did Arthur Burns, and repeatedly urged him to find something positive to say about the urge so many economists felt to choose theories on this basis.

As to Holmes, I observed that he sat frequently for half an hour on end, with knitted brows and an abstracted air, but he swept the matter away with a wave of his hand when I mentioned it. "Data! data! data!" he cried impatiently. "I can't make bricks without clay."

The Adventure of the Copper Beeches

Friedman, Stigler, and Burns were close friends. But more than that, the three shared a professional experience that shaped their methodological views. This experience was that the three were on the Research Staff of the National Bureau of Economic Research. Friedman's methodology was made from ideas that he acquired first as a student of Burns and Wesley C. Mitchell, and then as a member of the National Bureau staff. Just as monopolistic competition was poised in the 1940s to replace Marshallian value theory, so Keynesian macroeconomics was poised to replace National Bureau-style business-cycle analysis.

In June 1946 Arthur F. Burns made his first annual report for the National Bureau of Economics, having replaced Mitchell, who retired as Director of Research the previous October. Burns's essay in the report was "Economic Research and the Keynesian Thinking of Our Times." He compared Keynesians to Ricardians, both groups believing that they had solved the mystery of the economic problem of their time. Burns judged that they had not done so. He avers:

The opinion is widespread that Keynes has explained what determines the volume of employment at any given time, and that our knowledge of the causes of variation in employment is now sufficient to enable governments to maintain a stable and high level of national income and employment within the framework of our traditional economic organization. If this opinion is valid, the solution of the basic problem of democratic societies is in sight, and the National Bureau would do well to reconsider its research program. Unhappily, this opinion reflects a pleasant but dangerous illusion (1946, p.5).

The research program of the National Bureau emphasized producing economic facts and fact-based analysis. The Keynesians, like the Ricardians before them, thought that “speculative analysis” based on the master’s theory was sufficient for the job.

Paul Samuelson and James Tobin were becoming prominent Keynesians at the time Burns wrote. Their writings on Keynesian economics provide a sharp contrast with the view of the state of economic knowledge and methods for enlarging knowledge that characterized the National Bureau.

Samuelson contributed an essay to Seymour E. Harris’s *The New Economics* in which he wrote that *The General Theory* “caught most economists under the age of 35 with the unexpected virulence of a disease first attacking and decimating an isolated tribe of south sea islanders” (1947, p. 146). According to Samuelson, Keynesian theory was sweeping aside the Say’s Law of Markets, the classical theory of employment, with good reason. Samuelson explained why:

The classical philosophy always had its ups and downs along with the great swings of business activity. Each time it had come back. But now for the first time, it was confronted by a competing system – a well-reasoned body of thought containing among other things as many equations as unknowns, in short, like itself, a synthesis; and one which could swallow the classical system as a special case (1947, p. 148).

He also thought that in addition to being “complete,” the theoretical system in *The General Theory* was “relatively realistic” (1947, p. 151).

Samuelson’s other writings from the period that have to do with Keynesian analysis are filled with the logic of theoretical models and empty of quantitative data and estimates. There are statements of fact, but no evidence of gathering or exploring facts. For instance, in “The Theory of Pump-Priming Reexamined” (1940), Samuelson writes:

It is necessary in the beginning to set forth explicitly the basic features of the private economy forming the environment within which governmental action must take place. No attempt is made to justify the characteristics stressed beyond the assertion that in the recent business cycle literature they are regarded as fundamental (1940, p. 492).

Samuelson asserts without evidential support several of these fundamental characteristics of the private economy: “the possibility of, if not a definite tendency toward, cumulative movements of a disequilibrating kind” (1940, p. 492), and “the fact that *even in a perfect capital market there is no tendency for the rate of interest to equilibrate the demand and supply of employment*” (1940, p. 493, emphasis his). Further along in the article he writes:

First, consider a downturn due simply to the giving out of investment opportunities. ...

The reader will perhaps recognize in this example some of the features characteristic of the Great Depression of 1929 (1940, p. 496).

Samuelson wrote in another piece, “Fiscal Policy and Income Determination” (1942), that his purpose was “carrying forward the analysis of important theoretical and *empirical* factors, usually assumed constant or neglected in the oversimplified versions of the theory [of multiplier analysis]” (p. 575, emphasis added). On the basis of this theoretical and, as he suggested, empirical investigation he draws inferences about primary, secondary, and tertiary effects of public spending, and about their implications for the relative merits of public works and consumption expenditures. He also draws inferences about “the vitally important comparison of the ‘multiplier hypothesis’ and the ‘velocity-of-money hypothesis’” (1942, p. 575). However, despite the promise to analyze empirical factors, the article’s sole empirical content is a table of estimates of tax revenue yields at different levels of income from the 1940 Report of the Secretary of the Treasury.

James Tobin also contributed a chapter to *The New Economics*, on “Money Wage Rates and Employment.” Tobin explored the implication of different sets of assumptions for Keynes’s claim that labor was be unable to bring about an increase in employment by accepting wage cuts. The chapter is much like Lange’s *Price Flexibility and Employment*, both in subject matter and method. The method is that which Friedman labeled “taxonomic theorizing.” At the chapter’s conclusion Tobin suggests that further progress toward solving the money wage problem will require refinement of both theoretical *and*

statistical analysis of the Keynesian system. But he did not provide any statistical evidence or analysis.

In 1947 Tobin wrote an article on monetary economics, criticizing Clark Warburton's "Monetary Theory of Deficit Spending" and William Fellner's *Monetary Policies and Full Employment*. The key issue, according to Tobin, was whether the liquidity preference function is perfectly inelastic with respect to the interest rate, a condition that he deemed most unlikely. Much of his argument is carried along by examining the implications of various assumptions and their consistency or inconsistency. But Tobin supplements this theoretical analysis with statistical evidence to show that demand for cash balances is not interest inelastic. He prefaces the evidence with a warning that theories cannot be proved or disproved with statistics. Readers accustomed to a modicum of statistical sophistication would certainly agree that Tobin's warning was apt, especially for his evidence. His statistics are charts of annual observations of average "idle" deposits plotted against the average commercial paper rate for four groups of banks, from 1929 through 1945. Idle deposits are total demand deposit debits divided by velocity for 1929. No adjustment is made for trend. There are actually no statistics, neither data nor analysis, only graphs of the plotted data for visual inferences about elasticity.

Tobin's Nobel lecture (1981) is, by his description, a summary of his contributions to Keynesian economics and monetary economics. He gives an account of his theoretical refinements such as incorporating dynamics of stock-flow relationships, disaggregation of asset categories, modeling the mechanics of policy processes, and consistent application of Walras's Law. These contributions

are all theoretical. The only data in the lecture are two tables of flow of funds data, data of which Tobin makes no direct use.

*"This is indeed a mystery," I remarked. "What do you imagine that it means?"
"I have no data yet. It is a capital mistake to theorize before one has data. Insensibly one begins to twist facts to suit theories, instead of theories to suit facts."*

Dr. Watson & Sherlock Holmes, A Scandal in Bohemia

As Milton Friedman wrote the first draft of the methodology essay in the winter and spring of 1948 Paul Samuelson was putting the finishing touches on *Economics: An Introductory Analysis* (1948). Parts of Samuelson's opening chapter are consistent with themes in Friedman's essay; some even look as if they could be a brief for the National Bureau research program. Samuelson wrote that:

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).

Writing about economic theory and practice, he continues:

The economic world is extremely complicated. Furthermore, it is usually not possible to make economic observations under controlled experimental characteristics of scientific laboratories. ... As a result of this limitation

and many others, our quantitative economic knowledge is far from complete. This does not mean that we do not have great amounts of accurate statistical knowledge available. We do (1948, p. 7).

A statement about simplification resembles Friedman's:

Even if we had more and better data, it would still be necessary – as in every science – to *simplify*, to *abstract* from the infinite mass of detail. No mind can apprehend a bundle of unrelated facts. All analysis involves abstraction. It is always necessary to *idealize*, to omit detail, to set up simple hypotheses and patterns by which the mass of facts are to be related, to set up the right questions before we go out looking at the world as it is. Every theory, whether in the physical or biological or social sciences, distorts reality in that it over-simplifies. But if it is a good theory, what is omitted is greatly outweighed by the beam of illumination and understanding that is thrown over the diverse empirical data (1948, p. 7-8).

Samuelson states clearly that formal features of theory are subordinate to empirical success:

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).

One looks in vain through the collected works of Samuelson and James Tobin for systematic attempts to bring illumination to “diverse empirical data,” and to accumulate “quantitative knowledge” of booms and recessions or other economic phenomena. One looks in vain for empirical tests of theory, either by theory’s predictions or assumptions. They paid lip-service to the empirical tradition of science, and to the prudent dictum for everyday life to get the facts straight before coming to a judgment. But they practiced economics straight from the formalist script. In 1946 Friedman anticipated where the formalist script would lead economists, to “formal models of imaginary worlds, not generalizations about the real world” (1946, p. 283).

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