Milton Friedman’s immensely influential (and equally controversial) essay on “The Methodology of Positive Economics” was first published in 1953, but some of the ideas found earlier expression in his 1946 American Economic Review article discussing the methodology of Oscar Lange. In that article, Friedman criticizes Lange for casual empiricism, invalid use of inverse probability, introduction of factors external to the theoretical system, and the use of only some of the implications of a formal model that has others that are unrealistic. … 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, that subsequent observation does not contradict. In consequence, these defects are found in much economic theorizing that is not taxonomic in character. They are, however, especially likely to arise when the taxonomic approach is adopted, as their presence in the writings of so able and careful a theorist as Lange testifies. (Friedman 1946, p. 631)

Shortly after the publication of the piece on Lange, Friedman received a probing letter from Edwin Bidwell Wilson. That letter, Friedman’s reply, and a second letter of Wilson’s are reprinted below.

E.B. Wilson (1879–1964) was a scientific generalist of a type that has always been uncommon, and is becoming unknown. Wilson studied under the mathematical physicist J. Willard Gibbs, and his publication of Gibbs’ notes on vector analysis and a text of his own on advanced calculus had a great impact upon education in advanced mathematics in the early part of this century. Wilson held faculty positions in mathematics at Yale and mathematics and physics at M.I.T. before he moved in 1922 from being the Chairman of Physics at M.I.T. to Harvard to be Professor of Vital Statistics in the Harvard School of Public Health, a position he held until his retirement. In 1914 he was appointed Managing Editor of the Proceedings of the National Academy of Sciences, a post he held for fifty years, until his death. In 1929 he served as President of the American Statistical Association (Jerome Hunsaker and Saunders Mac Lane 1973).

One of these letters (Friedman’s) turned up in an archive at Chicago; the two letters of Wilson were then transcribed from copies of the handwritten originals in Milton Friedman’s papers at the Hoover Institution at Stanford. Milton Friedman has given permission for their publication,
but he has declined an invitation to add commentary. A bibliography of the books referred to in this wide-ranging discussion appears at the end.

Stephen M. Stigler
University of Chicago

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November 24, 1946

Dear Professor Friedman

I have just read with great pleasure your “Lange” in Am. Ec. Rev. for Sept. You are talking about and against the greater part of writing in the social sciences, I fear. Cleverness is highly admired even though meaningless. Did you ever read W. Macneile Dixon’s The Human Situation? He takes the philosophers, theologians, reformers, et al. for a terrific ride for not talking about man as he is—good and bad—and I fear lays himself open to some criticism for writing about man of higher than average I. Q. and other characteristics, but that is another matter.

Now what we really need is not so much these expositions of what gets us nowhere, important as they are, but exhibits of what gets us somewhere. It is well known, I believe, by psychologists that praise and reward for doing right is much more effective in modifying behavior than scolding and punishment for doing wrong. Also, clear exhibits of how to do things right are more likely to get folk to doing them right than exhibits of how they are done wrong.

I went to Glasgow last winter to talk about some of our American accomplishments in the study of society by the scientific method. It was reflections on my work for SSRC and on the Science Committee of NRPB [National Resources Planning Board]. I should have had 2 or 3 years to put the stuff together but I had to talk as best I could.

What I should have put together would have been a simple account of some of the best work done here in Econ., Sociol., Pol. Sci., Anthrop., etc., based on fact, subject to confirmation. I would not care too much about theory (and not at all about theory that could not be checked by facts); much good work in science, say in paleontology, systematic botany, archeology, represented in the museums, herbaria, etc., of your great university and others has very little theory, of the sort people think of as theory, to it; but I should insist on seeking out and ordering the facts so that they could be used as a basis for thinking about the real world and of investigating it further, for such is the job of science.

I suppose the Economists of the world would so much rather fight and look upon fighting that although the Am. Ec. Rev. would give you 20 pages to sock Lange, it would not give you 5 to explain why some good work was outstandingly good! I am not referring to 5 page standard laudatory and non-critical book reviews which are quite too numerous already. However, as I have to print my lectures (although I accepted the invitation to deliver them only on the
understanding that they not be printed) I should be much obliged to you if you would give me a list of half a dozen jobs in economics which you think thoroughly good in the way contrary to the bad way of Lange. If you wish to list your own work I am perfectly willing. Would you call Schumpeter’s 2 vol. Business Cycles good? (His clever thesis on Economic Development (1911?) lately reprinted in translation seemed to me to be stated in terms that would prevent checking, and I do not see that the Business Cycles really checks it effectively, but it may.)

I wish SSRC would go in heavily for appraisal as originally planned, i.e. to pick out the best books and explain why they were best, i.e. most realistic and very important for that reason. Maybe the appraisal of Thomas’s Polish Peasant [Blumer 1939] did that, but I don’t think that of Mills’s Prices [Bye 1940] or Webb’s Great Plains [Shannon 1940] did. Redfield’s efforts in appraisal of some anthropological work were not quite of this sort but were most well worthwhile undertaking.

Yours sincerely

E.B. Wilson

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University of Chicago

December 16, 1946

Edwin B. Wilson

42 Brington Road, Brookline 46, Massachusetts

Dear Professor Wilson:

Pardon me for being so slow in replying to your letter of November 24. The delay was caused mostly by the difficulty of replying satisfactorily to your request for a list of “good” work, and by a consequent attempt to spend more time in thinking of items and in consulting others.

I enjoyed your letter very much. I agree thoroughly—my conduct to the contrary notwithstanding—that constructive work would be far more valuable than criticism of bad methodology. Indeed, I had some hesitancy about publishing the Lange article for that reason. J. M. Clark once said that some could not help writing on methodology, especially when young, but that anyone who did so should put the article away for ten or fifteen years, and only publish it if it still seemed to contain something worth saying. I think there is much point to Clark’s advice.

Another thing that disturbed me about putting out the Lange review is that an earlier reaction away from formal theorizing led to such bad work of the opposite kind—perfectly meaningless fact gathering and piling of data end on end. Mill’s Behavior of Prices is perhaps as good an example as any. Now unquestionably we need much purely empirical work in the direction of measuring meaningful quantities, as illustrated, for example, by Simon Kuznets’ work on national income (National Income and its Composition), but we don’t need an unordered search for empirical regularities that inevitably ends up in bad theorizing rather than no theorizing.
(Mills, it seems to me, did not really avoid theorizing, he theorized without knowing it and hence theorized badly).

Your request for a list of six or so jobs in economics that seem to me thoroughly good leaves me extremely uncomfortable about both my knowledge of the work done and the present state of economics. I have tried to formulate such a list using the following criteria: (1) to list nothing I know of solely or primarily by reputation; (2) to list work that is good in the way contrary to the bad way of Lange (as you put it). This leads me to omit work that is good in the way Kuznets’ work is (i.e., because it measures well something meaningful), since such work is neutral on the methodological issue involved; (3) to list only American work, preferably of fairly recent vintage. The following are all I could think to include in the list:


4. Jacob Viner, *Canada’s Balance of International Indebtedness*, 1900–1913 (1924)


I have never done more than dipped into Schumpeter’s volumes on *Business Cycles*, so do not feel competent to judge whether they should be included; my inclination is to agree with your evaluation, but I have no great confidence in this judgement. F. Macaulay’s book for the Bureau on *Interest Rates*, etc., should probably be included, but again I did not read enough of that carefully enough to form a considered judgement.

So far as my own work is concerned, I should not want to judge its importance, but I do feel that *Income from Independent Professional Practice* (written jointly with Simon Kuznets), particularly chapters 3 and 4, embodies the appropriate methodological approach in respect to the combination of empirical and theoretical analysis.

I have some uncertainty about how to classify a book like F. H. Knight, *Risk, Uncertainty, and Profit*. This is clearly an extremely good and important book. In great measure the theory it contains seems to me relevant to the real world and fundamentally to be based on observation. The difficulty is that the observation is casual, unordered observation. There is no systematic attempt to marshal the relevant facts which the theory generalizes or to test the theory by additional facts. Alfred Marshall’s *Principles of Economics* is in some respects in the same class, though I should have no hesitancy in including it in the list if it were not for the temporal and geographical limitations imposed on the list.

Another book I am somewhat uncertain about is Henry Schultz, *The Theory and Measurement of Demand*. It is an exceedingly careful and systematic attempt to put empirical content into a pre-existing theory. I have excluded it because there seems to me no reverse influence of the
empirical work on the theoretical structure. Schultz took the theory as fixed and given, and tried to measure what he thought were essential functions in the theory. He imposed extremely high standards of care and thoroughness in the measurement process—but he nowhere attempted what seems to me the fundamentally important task of reformulating the theory so it would really generalize the observable data; he always tried to wrench the data into a pre-existing theoretical scheme, no matter how much of a wrench was required.

I should, perhaps, repeat that I make no pretense to comprehensiveness. Much of the best work of the kind under consideration would be expected to be monographic; and hence would not be known to me unless I had happened to work in the precise field to which it relates.

Sincerely yours,

Milton Friedman

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Dec. 19 1946

Dear Professor Friedman

Many thanks. I did not intend to set too high a standard. I was talking or asking about a certain kind of work—viz., scientific. There are many sorts of scientific work all good if not great. There are many great works which are also good but in no wise scientific—Paradise Lost, Hamlet, etc., including the Communist Manifesto.

You do not need to feel apologetic about the Lange article. It is a most necessary kind. So long as the alchemist or iatrochemist sought transmutation of base metals into gold or an elixer of youth they were on useless courses and only the By-products could be good; science could not develop except as one of those by-products.

Now the great difference between the natural & social scientists (which may well be due to a different state of their sciences qua science) lies in the fact that the natural scientist suppresses promptly the errors of his fellows, generally before they get into print, whereas the social scientists tolerate the sloppiest sort of stuff. An instance of this is what happens if a social scientist ventures an opinion about natural science and is wrong—the natural scientists jump on him at once and he recants promptly; whereas if a natural scientist ventures an opinion about social science, about which he is probably no more expert than the social scientist about natural phenomena, the social scientists let him get away with it.

I never heard of a natural scientist, be he physicist or astronomer, chemist or paleontologist, entomologist or other, who was interested in methodology when he was young—he is interested in getting more knowledge. For the natural scientist methodology is a pastime or occupation of age—old or middle, mostly old, I think. Of course the philosopher is interested in it at all ages but he makes little contribution to knowledge, i.e., science. It may be a disease, a mental disease, this interest in methodology. There are methods of scientific advance in almost infinite variety,
young scientists learn some of them and go to work. That there is any such thing as methodology is very doubtful—any science of methods *qua* methods. I take quite the view of the Encyc. Brit. in this, viz. Vol. 24 p. 651 (index) Methodology, see Scientific Method, which article Vol. 20, p. 127, is by Abraham Wolf the general editor for philosophy & psychology of the 14th Edition of Encyc. Brit. and Professor of Logic & Scientific Method in London. Whether he was a good editor I do not know but at any rate he knows that there is little methodology except the simple principles of logic, though there are many technical methods. To my way of thinking the social scientist’s interest when young in methodology is an evidence of the continuing pernicious influence of moral philosophy & philosophy and the meager influence of logic and of facts in the field of the social sciences. The mathematician may also be somewhat to blame.

Mills did not avoid theorizing. His title Behavior of Prices did not need to lead him into theory—he could have remained on the level of description and classification. Suppose your most distinguished physical chemist were to write a book on the behavior of alloys, or your best ornithologist on the behavior of birds—how much theory would you find in the work? Certainly, I think, not so much as in Mills’—and if you found it, I think it would probably be quite as bad. Most great systematicists seem to be poor theorists,—Audubon, Gray, Wm. Wheeler, Tom Barbour, etc. I believe you give too little credit to merely systematic work. It is this work which establishes our knowledge of the infinite variety of nature and lays the ground for perceiving within that variety certain things that go together—families, genus, species. It is not theory I take it that ranks whales with the mammals rather than with the fish or that puts the lofty elm and the mean nettle together in the Urticace. For this reason, my respect for the great naturalistic tradition, I might put Mitchell’s 1927 book ahead of his 1913 one. I have neither at hand for reference and it is a long way to Widener Library (and both books would probably be out), but my recollection is that he showed a wider knowledge of the anatomy of business cycles and a lesser tendency to inadequate theorizing in the latter book.

It is perhaps the business man, as economic engineer, who needs the knowledge in detail. It may be you would not even rate as an economist one who did not theorize. There has been little theory in medicine which has not been harmful to patients. 100 years ago it was medical theory which Pasteur had to overcome. Theory is very impermanent; it is just one method of classifying the facts. I doubt if biological theories havestood the test of time anything like so well as classifications of the great systematicists.

Casual unordered observation is the basis of many excellent sermons but of few books on science. I should be inclined rather firmly to reject Knight’s Risk, Uncertainty & Profit as prescientific much as I would Shakespeare’s Hamlet as not scientific psychology. The weakest part of Schultz’s book is its theory. He had the misfortune to be writing as economists were abandoning the utility theory for the theory of indifference surfaces—the utility function for the index of utility. And actually has anybody to this day shown how factually to determine either a utility or the value of a utility index? I know there have been inconclusive attempts. Moreover Schultz was not even consistent with what theory he had. To my way of thinking this did not hurt his theory or his factual measurements because he did not establish any connection between them—the discussion of the light each throws on the other being I think the weakest part of all. We have never established any general social utility function have we—and what but a social utility function or index could tie in with the prices of beef, mutton, or pork? I think his book could have been made more scientific by cutting out much of the theory. There is a great deal of
pretentiousness about theorists that is lacking in scientists—Einstein is a theorist (almost exclusively) whereas Michelson was a student of phenomena (almost exclusively). Walras was much more the theorist, Quetelet much more the naturalist, I would not exclude theorists from the group of scientists, Edison & Einstein are alike scientific physicists but Einstein’s discoveries may well outlast Einstein’s.

Macaulay’s Interest Rates (a little thin book on graduation with a long pretentious preface as I recall it) I once used as a text for part of my course in Economic Statistics (Theoretical) which I gave by request. The work was carefully done but I was very doubtful of any value in it whether in its graduations or in its philosophy. I never understood why anybody should rate it highly but I seem to recall that Mitchell was impressed by it.* I like Frickey’s approach much better.

Sincerely & again much thanks

E. B. Wilson

Works Referred To


Bye, Raymond T. *Critiques of research in the social sciences: II. An appraisal of Frederick C. Mills’ The behavior of prices.* New York: SSRC, 1940.


* I may be thinking of some other book than that you have in mind. [Note: Wilson seems to have had Macaulay (1931) in mind, while Friedman was referring to Macaulay (1938).—Personal communication to S. Stigler from M. Friedman, September 1993]