Is Macroeconomics a Science?

Foreword to Apostolos Serletis, *Money and the Economy*

William A. Barnett
University of Kansas
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In his Foreword to Barnett and Samuelson (2006), Paul Samuelson (2006) wrote:

> "I conclude with an unworthy hypothesis regarding past and present directions of economic research. Sherlock Holmes said, '*Cherchez la femme*.' When asked why he robbed banks, Willie Sutton replied, 'That's where the money is.' We economists do primarily work for our peers' esteem, which figures in our own self-esteem. When post-depression Roosevelt's New Deal provided exciting job opportunities, first the junior academic faculties moved leftward. To get back ahead of their followers, subsequently the senior academic faculties shoved ahead of them. As post-Reagan, post-Thatcher electorate turned rightward, follow the money pointed, alas, in only one direction. So to speak, we eat our own cooking.
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> We economists love to quote Keynes's final lines in his 1936 *General Theory* --- for the reason that they cater so well to our vanity and self-importance. But to admit the truth, madmen in authority can *self-generate* their own frenzies without needing help from either defunct or *avant garde* economists. What establishment economists brew up is as often what the Prince and the Public are already wanting to imbibe. We guys don't stay in the best club by proffering the views of some past *academic* crank or academic sage."

For the benefit of those who do not meet Paul's high standards of erudition, I here provide Keynes (1936, pp. 383-384) statement, to which Paul alludes in his Foreword:

> "Practical men, who believe themselves to be quite exempt from any intellectual influences, are usually the slaves of some defunct economist. Madmen in authority, who hear voices in the air, are distilling their frenzy from some academic scribbler of a few years back. I am sure that the power of vested interests is vastly exaggerated compared with the gradual encroachment of ideas ... Sooner or later, it is ideas, not vested interests, which are dangerous for good or evil."

When I showed the first draft of Paul's Foreword to some eminent economists, many reacted with shock and dismay. One replied that Paul was accusing us all of being "a bunch of whores." But the more that I thought about it, the clearer it became to me that Paul's insights in his Foreword merit serious consideration. As a result, I resisted pressures to request that his Foreword be toned down. In fact, what Paul is saying in that Foreword has relevancy to what I have experienced in my own professional experiences, and is valuable in putting this important book by Apostolos Serletis into proper context.

When I founded the Cambridge University Press journal, *Macroeconomic Dynamics*, which I edit, I needed to write a statement of purpose to appear in the first issue. The statement needed to include a definition of macroeconomics that could be used to motivate the intended focus of the journal. I defined "macroeconomics" to be "dimension reduction." The reason is clear. Macroeconomic policy cannot be implemented by reference to high dimensional models, in which there is no aggregation over goods or economic agents, or separability of structure into sectors that can be modeled independently. Indeed, dimension reduction can be accomplished in a rigorous manner using aggregation theory, separability tests, and nonlinear dynamics. But is such rigorous formalism in dimension reduction typical of most macroeconomics? I don't think so, and many microeconomists and political scientists do not think so. The dimension reduction typifying much macroeconomics is characterized by the use of untested, atheoretical oversimplifications. If the oversimplifications are contradicted by empirical evidence, then an alternative to statistical hypothesis testing is sought to avoid the embarrassment. As observed by Thomas Sargent, in his interview by Evans and Honkapohja (2005, pp. 567-568):

> "Calibration is less optimistic about what your theory can accomplish, because you'd only use it, if you didn't fully trust your entire model, meaning that you think your model is partly misspecified or incompletely specified, or if you trusted someone else's model and data set more than your own. My recollection is that Bob Lucas and Ed Prescott were initially very enthusiastic about rational expectations econometrics. After all, it simply involved imposing on ourselves the same high standards we had criticized the Keynesians for failing to live up to. But after about five years of doing likelihood ratio tests on rational expectations models, I recall Bob Lucas and Ed Prescott both telling me that those tests were rejecting too many good models. The idea of calibration is to ignore some of the probabilistic implications of your model, but to retain others. Somehow, calibration was intended as a balanced response to professing that your model, though not correct, is still worthy as a vehicle for quantitative policy analysis."

As Paul Samuelson (2006) has observed, the direction that the profession takes has a strong correlation with the existing direction of thought in other academic fields and in government. The direction of causation is not always clear, but I agree with Paul that the causation often comes from outside the field of economics. This direction of causation often is particularly evident in its effects on macroeconomics, which depends for its very existence upon its policy relevance. Anyone who has worked for many decades in macroeconomics, monetary economics, and policy, has observed the frequent changes in direction, and the nontrivial correlation with the political winds that are blowing in the background. For example, when I resigned from the Federal Reserve Board staff to accept a position at the University of Texas, after 8 years in the Board's Special Studies Section, two high ranking officers of the Board's staff entered my office and threatened me with harassment by the Board's attorneys, if I ever were to become known in the press as a critic of Board policy. Having always been dedicated to high tech scientific research, rather than to visible criticism of Board policy, I could not imagine the reason for that threat, but it is not irrelevant to understanding the nature of the connection between government policy and macroeconomic research.

During the years I was at the Board, Karl Brunner and Allan Meltzer were very visible critics of Board policy through their Shadow Open Market Committee. But there was a difference in their degree of willingness to be cooperative with the Board. Allan, who got along well with the Board, was often included among the semiannual meeting of the Academic Advisors to the Board. On the other hand, Karl, who tended to be uncompromising in the nature of his policy advocacy, was banned from the Board building. In fact, the security guards at the entrances were instructed never to permit Karl to enter the building. Karl once confided to me that the rumors about the ban had done wonders for his career.

Prior to the three years of the "monetarist experiment" in the United States, the research staff of the Philadelphia Federal Reserve Bank produced a large document containing research supporting a change in policy direction --- the same change in direction that subsequently was adopted by Paul Volcker during the "monetarist experiment" years. But that research at the Philadelphia Federal Reserve Bank was prior to the arrival of Paul Volcker as Chairman of the Federal Research Board. As a result, the Board Staff at the time was instructed to crush the research at the Philadelphia Federal Reserve Bank and discredit its staff.[1] The Board Staff succeeded to the degree that almost the entire research staff of the Philadelphia Federal Reserve Bank resigned. Prior to their resignation, I was invited to the Philadelphia Federal Reserve Bank as a possible new hire, who might be able to help hold the staff together. Although I had said nothing about this to the Federal Reserve Board, on the morning that I returned from Philadelphia to the Board Staff in Washington, D.C., I was called into the office of the Director of Personnel and given an immediate raise along with instructions not to help "those bad people" in Philadelphia.

Not long thereafter, when inflation was becoming intolerable to the Carter administration in Washington, Paul Volcker was moved from the New York Federal Reserve Bank to become Board Chairman in Washington, D.C. He then instituted precisely the policies that had been advocated by the former staff at the Philadelphia Federal Reserve Bank. Chairman Volcker, knowing that his staff had been geared up to oppose precisely that approach, did not confer with his large staff before his announced policy change. Reputedly only about three staff members at the top were informed of the impending change. The rest of us learned from the newspaper the next morning. In fact the next morning, I had breakfast at the Board's cafeteria and observed the stunned looks on the faces of the staff and bewildered conversations among us over our eggs and coffee. In contrast, Carl Christ was visiting from Johns Hopkins University that day and joined us at that breakfast. He was clearly amused and pleased by what had just happened.

Over the past 50 years, the frequency of changes in the choice of policy instruments and policy designs by the world's central banks have been astonishing. There has not been a clear trend in any one direction, with reversions to some of the oldest approaches being common and frequent. Is this science, or is this politics? If unanticipated shocks to the economy were to cause unemployment to rise dramatically, would the currently spreading fashion of targeting solely inflation continue? If unanticipated shocks were to cause a return of double digit inflation, would the current emphasis on interest rates rather than on monetary service flows continue? Is it really true that monetary quantity is harder to measure than the "natural" or "neutral" interest rate needed in Taylor rules? Is the economy so simple that all that is needed

[1] The resulting controversy made its way into the press. See "The politicization of research at the Fed," *Business Week*, July 16, 1979, pp. 106-115.

to conduct monetary policy is an interest rate feedback rule, a Phillips curve, and perhaps one or two other equations? With all economic theory being nonlinear, is it reasonable to believe that estimated or calibrated models should be linear?[2] Is it reasonable to believe that macroeconomic policy has no distribution effects, as is commonly assumed in macroeconomic models, despite the fact that most politicians advocate macroeconomic policies based precisely upon their distribution effects? If there are no such distribution effects, why is there such a strong correlation between macroeconomic policy advocacy and political party affiliation? Is it reasonable to continue to assume that monetary assets yield no own-rate of return, as assumed in many demand for money functions, despite the fact that currency and non-interest-bearing demand deposit accounts have not dominated the money supply for over a half century?[3]

In short, as has been pointed out by Paul Samuelson (2006), we macroeconomists work within an environment of pressure and influence from our governments and societies. While few are willing to recognize or admit the existence of those pressures or the influence of those pressures on our own work, a clear understanding of trends in macroeconomic research is not possible without recognition of the influence of the intellectual, societal, and political environment within which the research is conducted.

I started out as a rocket scientist (yes, a real one), after receiving my engineering degree from MIT in 1963. I worked on the development of the F-1 booster rocket engine that got the Apollo Saturn vehicle off the ground. I worked for a firm called Rocketdyne, which had that rocket engine contract from NASA. Although I changed professional directions, when I went back for my Ph.D., I have never forgotten what real science is. The more that I think about what Paul Samuelson has written in his Foreword to Barnett and Samuelson (2006) and my experience as an economist for over 30 years, the more I recognize the depth of the insights provided by Paul in his short Foreword. A not unrelated comment is Jim Heckman's (2005) in his Minneapolis Federal Reserve Bank interview:

> "In economics there's a trend now to come up with cute papers in an effort to be cited as many times as possible. All the incentives point that way, especially for young professors who seem risk-averse rather than risk-taking after they get tenure. In some quarters of our profession, the level of discussion has sunk to the level of a *New Yorker* article: coffee-table articles about "cute" topics, papers using "clever" instruments. The authors of these papers are usually unclear about the economic questions they address, the data used to support their conclusions and the econometrics used to justify their estimates. This is a sad development that I hope is a passing fad. Most of this work is without substance, but it makes a short-lived splash and it's easy to do. Many young economists are going for the cute and the clever at the expense of working on hard and important foundational problems."[4]

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[2] See, e.g., Barnett and Binner (2004) and Barnett et al (2005).
[3] For the relevant extensions needed to incorporate own rates of return into monetary aggregation theory and internally-consistent demand for money functions, see Barnett and Serletis (2000).
[4] Jim, too, is a one-time rocket scientist. He worked as a research mathematician for Martin Marietta, which produced the Titan missile and the manned orbiting laboratory launched by Titan.

This might all sound like “bad news” in its implications for macroeconomics as a science, and sadly is consistent with the views of many microeconomists (some of whom oppose the inclusion of macroeconomics in their academic departments). But there also is “good news.” There are some hard-core scientists who work in macroeconomics and monetary economics. They do not respond to political pressures, they resist oversimplification, and they seek to advance macroeconomics in the uncompromising manner that characterizes real science. They are willing to take on the difficult problems that often are assumed away solely for the convenience of economists.

That “good news” is what this book is about. That is what Apostolos Serletis is about. This is an important book for anyone who has a serious interest in what science has to say about modern macroeconomics and monetary economics. This book emphasizes what real progress is being made to advance our entirely inadequate knowledge of the macroeconomy. For an overview of the contents, see Serletis’s (2006) own introduction.

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