

A PLEA TO ECONOMISTS?

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The last two centuries have seen, as Martin Wolf noted in the *Financial Times*, 6 September 1999, a continuing battle between command-and-control and market mechanisms for organizing society. On this battlefield, it has been the economists who have devised and honed the intellectual weapons, even if it has been political leaders, from dictators to liberal democrats, who have put them into use. As Keynes said, "Madmen in authority, who hear voices in the air, are distilling their frenzy from some academic scribblers a few years back" [1936].

With Karl Marx on one side, Hayek and Friedman on the other, and Keynes as a middle-way in between, the ideas of economists have been beacons for their myriad followers. You will recall the story of the Soviet military parade, with weapons of ever increasing mass destruction, culminating in a small van containing some men in grey suits. "Who are they?" asked an onlooker; "Economists" was the response. "But why?"; "You should see the destruction and havoc that they can cause."

Clinton was supposed to have claimed that his election campaign had to have a major focus, "It's the economy, stupid." When a Presidential candidate in the United States, or a party leader in most G7 countries, approaches elections, he will now usually have individual economists as advisers and will have economic programs to put before the electorate. Certainly leading politicians will also have scientific advisers, but scientific issues are rarely as electorally crucial as economic ones. I find it hard to believe that economics, and economists, do not figure sufficiently prominently on the public scene.

Nor are such economists always behind the scenes "eminences grises." When I observe the role of Larry Summers, Stan Fischer, Joe Stiglitz, to name but a few, I do not feel that economists are shrinking violets constructing complicated formulae in ivory towers. Moreover, the recent trend towards giving central banks operational independence to set interest rates, so as to achieve an objective for price stability mandated by the politicians (and hence indirectly from the electorate) has led numerous economists to shift from a purely advisory towards a more directly decision-making mode. A majority of academically trained professional economists currently serve on the Bank of England's Monetary Policy Committee.

The claim made by Martin Anderson, cited by Daniel Klein, that in his four-year experience, that not once "in countless meetings on national economic policy, did any-

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one ever refer to any article from an academic journal" would not be representative of the MPC. Indeed I wonder whether Stigler's pronouncement, that academics should stick closer to their academic and theoretical last, and spend less effort on "preaching", i.e. on policy advice, was partly driven by a feeling that too many economists were spending too much of their time in such exercises. Moreover, different schools of political ideology can always find some academically trained "economists" to support their view. Since the public air-waves are filled with the rival claims of "experts", Stigler may have felt that the basic *métier* of a serious economist lay in sorting out truth from falsehood, not in providing columns of instant advocacy in journalistic media.

Again a considerable amount of Klein's worries may be particularly related to more narrow U.S. concerns. The United States is a huge country, physically as well as economically, and Washington is a very specialized community. I would guess that the average economist, including—perhaps *especially* including—the representative macroeconomist, feels far more divorced from government circles in the United States than she would in, say, Sweden, Spain, Italy or the Netherlands. In some European countries it has at times appeared that sound advice from macroeconomists in central banks, such as the Banca d'Italia or the Banco de España, has helped those countries overcome periods of severe political weakness.

Economists in the United States, especially in academic centers outside the Northeast, seem to regard government, especially the federal government in Washington, as a much more distant, alien, even dangerous, body than do economists in smaller European countries. Most senior economists in Europe can, if they should want to do so, choose to play an engaged, in many cases influential, role in their own country's policies. The corridors of the Ministry of Finance or of the central bank are not a dangerous *terra incognita* to them that they seem to be to many in the United States.

I see no lack whatsoever of engagement in public policy debates of economists in Europe, and I rather doubt whether there is any such deficiency in the United States either. I do slightly wonder whether it could be argued that the commanding leadership of U.S. economists in the development of theory could be ascribed to their generally greater divorce from policy advice (and politics). But in the decades when the UK (or Sweden or Austria) were the theoretical leaders, those theorists such as Keynes, Robertson, and Hawtrey were hardly divorced from policy involvement (rather the opposite). A more compelling case for U.S. academic leadership in economic theory can be ascribed to relative pay levels (in a competitive world market) and to hysteresis. Indeed it may be partly the rewards and excitement of direct policy involvement that have managed to keep many of the remaining good (i.e. marketable) macroeconomists from joining the brain-drain across the Atlantic (or Pacific in the case of the Japanese).

Nor do I believe that libertarian economists, who advocate more reliance on market mechanisms and less on government intervention, have been less willing to join in public-policy discussions, in preaching, than their more socialist colleagues. In particular, the shift from the view, or belief, that governments would altruistically seek to maximize the social welfare of the public to the view, or belief, that they are primarily concerned with an agenda of their own, in which sticking to power is usually

predominant, has made enormous strides in recent decades, due not only to the analysis, but also the preaching of liberal economists such as Buchanan and Tullock. It is, no doubt, partly a reflection of my having been trained at Cambridge (in both the United States and UK), but my own view is that the extent of cynicism about the motives of governments and their officials, for example, as incorporated in the time inconsistency analysis, has gone too far. The pendulum certainly needed to swing from the idealistic view of government action widely held in the 1950s and 1960s, but my subjective assessment is that it has swung too far, driven largely by the successful preaching of liberal economists.

Again in the field of financial regulation, and intervention, for example, by central banks acting as lenders of last resort (where I have a professional interest) there is great pressure to restrict such actions, generally using the claim that any attempt to protect commercial institutions from their own folly will cause "moral hazard". The idea that moral hazard is at all times and everywhere a major disadvantage of intervention has been advanced with great success by the preaching of liberal economists, without in my view any great empirical backing.

Overall my impression is that liberal economists have done rather well in the battle for the public ear in recent decades, as compared with their colleagues who might prefer more intervention. But I do not know how either participation, or success in the battle for the public's attention can be measured. Since every believer in an ideology (and we all believe in various ideologies) believes that her own faith is the true faith, there must be a bias towards thinking that its failure to achieve universal adoption must relate to poor presentation, insufficiently good preaching. My own view is that Dan Klein's paper is affected by just such an in-built bias. Liberal economists, in my view, can be proud of their preaching abilities.

Let me turn next to the other strand of Klein's critique, which is that, when academic economists retreat from the field of public policy involvement, they turn, in their ivory towers, to the construction of analysis that is too formal, mathematical, rigorous, and abstract. I have some limited sympathy with this view, especially in those cases where the math is more complex and the theory more abstractly formal than can usefully be applied to the underlying empirical data base.

Let me try to explain. Few people would describe the Black/Scholes formula for pricing options as a dirt-simple piece of math. Nor would the math necessary to try to reconstruct the probability density function of asset price expectations from option prices be regarded as easy, or trivial. Yet these, and several other examples, involve the appropriate application of math, rigor and precision to an accurate and large-scale data base (of asset prices in spot, forward and derivative markets).

Again, when I was a younger official at the Bank of England, risk management was thought to be almost entirely a matter of practical experience, and financial regulation a question of seeing what was done in the most respected institutions and getting others to copy. Economists were not then generally welcome in supervisory departments. Now we have the application of mathematical analysis (e.g. risk metrics) to the analysis of such problems. Extreme value theory, and other quite complex analysis, is being increasingly and appropriately applied.

But the ability to use math techniques, and precision, successfully and usefully does depend on an adequate data base. Will it be possible to develop credit-metrics effectively, if the loan-loss experiences of each bank are regarded as purely confidential data, not to be more widely shared. The path-breaking Miller-Orr analysis of the demand for money several decades ago rested on their access to confidential data on money balances from one single firm. The failure of demand-for-money analysis to develop much further is due in some large part to the treatment of such data on money balances as "confidential". Much of the advance in our understanding of the functioning of the foreign exchange market has come from the access of one economist, Prof. Richard Lyons, to the deals for a fairly short period of time of one (anonymous) foreign exchange trader!

Where the data allow, as in many financial markets, the application of rigor, precision and math has been remarkably fruitful. Another example in the same vein is the work by Paul Klemperer on auction theory. The difficulty arises when we move from the micro analysis of markets to macroeconomics. Here the data are limited and inaccurate. Most economists do not realize how inaccurate (and often collected and aggregated in ways that are sub-optimal) these data are. The macro outcomes are the result of the interaction of millions of heterogeneous people, each with their own limited information set and prior beliefs. The attempt to formalize such a complex system by making simplifying assumptions, such as representative agents (e.g. a representative producer) or common knowledge of the true model of the working of the economy (a strong form of rational expectations) goes so far beyond what can be justified, or tested against the data, that it does often lead to a feeling that the resulting models involve empty formalism.

Douglas North claimed that:

The rationality assumption that has served economists and all the social scientists well for a limited range of issues in macroeconomic theory is a devastating shortcoming in dealing with most of the major issues confronting social scientists and policy-makers, and it is a major stumbling block to the path of future economic progress. [1998]

I tend to agree with North. The basic problem is that the fundamental constraint on humans is time, not money or wealth. We can never begin to learn everything relevant to our own discipline, let alone other disciplines. We all have to make choices over rationing our own time, for example consciously *not* to become expert in many fields; foreign languages, electronics, and the internal combustion engine to name but three in my own case. The vast majority of people make similar choices to exclude expertise in macroeconomic modeling and portfolio management, for example; and that choice—given their occupations and position—will be entirely rational. When we know that we do not know much about a subject, we ask what others are doing and we seek help from a variety of "experts", some valid, others less so.

Under such conditions, of limited time and partial learning, the likelihood of such phenomena as herding, cascades, bubbles, etc., becomes large. Market outcomes, especially in asset markets, are likely to be volatile, and at times inefficient. This should

not be taken to be a covert argument in favor of more intervention, because public sector actions also have serious handicaps, for example, bureaucracy, buck-passing, attempts at voter manipulation, potential corruption, etc. The fact that the market outcome is imperfect does not, *ipso facto*, mean that the command-and-control outcome is any better—far from it.

What I do believe, however, is that the real world is one in which rationality is inevitably bounded by time constraints, and where individuals are (thankfully) all very different (*vive la différence*). Trying to force models (in the interests of some abstract rigor!) into a set mold of fully rational expectations, representative agents and perfect markets drives the results so far from reality that the outcome is a formal rhetoric without contact with practical policy issues. Put it another way, Lucasian macro models have, not surprisingly, had virtually zero impact on macro-policy making. That would be so whatever the extent of math used to embellish the models with technical virtuosity.

Two defenses, at least, may be offered. The first is that attempts to make macro-models more realistic by taking into account bounded rationality, learning processes, heterogeneity, etc., are so difficult that it is right to start with simpler, though less realistic, models that at least have a rigorous, deep, intellectual basis. Perhaps, but in the meantime practical policymakers are actually going to stick with the kind of extended Keynesian structural models that Lucas criticized so strongly decades ago. The alternative, more rigorous, macro-modelling approach preferred by academics for their journal articles so far largely fails the test of practical usage when policymakers (in some cases those same academics under another hat) seek empirical support for their necessary decisions.

The second defense is that these more formal, rigorous, Lucasian models can be, and are, confronted with the data via the new empirical exercise of "calibration" (i.e. seeing whether the main economic characteristics of an artificial world driven by the model appear close to that of the real world). In my view calibration is rather closer to a simulation exercise than to standard econometric hypothesis testing. Simulation, and I would expect also calibration, usually make the research worker feel that he/she has really learned something of considerable value about the real world, but rarely manages to impress the outside reader.

There are, of course, exceptions, especially where calibration points up real-world features that seem grossly at odds with the models, the equity premium puzzle being the best known (i.e. why have equities yielded so much more than other financial assets when their relative riskiness is not so much greater?) [Incidentally, my own reading of the current equity boom is that this is taking equity prices to a level that will lead the equity return premium to disappear over the next decade or two. Will this be found to be a case of learning and adaptation?]

Let me now conclude. Many areas of economics are in robust good health. Here hypotheses are derived from past theory, observation, and intuition and then tested against the data. The results of such empirical tests lead to revision of the hypotheses, and so on. The mathematical and econometric techniques applied are those that the researcher needs to resolve the problems. Most of finance, much of the study of individual markets and most of microeconomics seems to me to meet those standards.

Macroeconomics is, of course, much more difficult. The data base remains grossly insufficient (partly because of excessive concerns about confidentiality which has remained a baleful influence on academic advances throughout economics) and far more inaccurate than most economists realize. We cannot undertake controlled experiments. Even if we could, individuals learn from such outcomes and adapt their behavior. The alternatives range between rough-and-ready regression exercises without much theoretical basis (of which simple Vector Auto-Regressions, VARs, are an extreme form) to formal models that introduce an abstract, "deep", theoretical purity at the expense of institutional reality. None of the above approaches has much claim to be scientific.

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