Chapter 3 The Methodology of Economic Research

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"Economists, on the whole, think well of what they do themselves and much less well of what their professional colleagues do.....those who are mathematically inclined see others as in retreat from rigour. The others think those who manipulate symbols impractical. The statisticians believe those who prove points deductively to be dangerously intuitive. But, by their colleagues, those who are controlled by numbers are often thought cautious or even dull. It is exceedingly fortunate for the psychic health of the profession that inadequacy lies so uniformly with others. The situation in the other social sciences is said to be equally satisfactory".

- J.K. Galbraith
from "The New Industrial State" (1).

The scientific method

Methodology, normally caricatured as technical clutter or the analysis of analysis, will in fact be central to the results of any given activity. The choice of methodology will reflect the strength and weaknesses of a discipline. As far as economics is concerned, this is usually seen as a debate about the degree to which it merits the title 'science'.

Science defies glib definitions, but a useful starting point is given by Nagel: "It is the desire for explanations which are at once systematic and controllable by factual evidence that generates science; and it is the organization and classification of knowledge on the basis of explanatory principles that is the distinctive goal of the sciences" (2).

Therefore, whenever science asserts a claim, it also provides its own basis for testing that claim, i.e. a methodological scheme.

A crucial paradox arises from science's stated openness to consideration of alternative explanations which, at the extreme, would seem to undermine scientific norms. Thus, for example, Ragnar Frisch, after manipulating some matrices, can conceive of the possibility that what he calls "ultimate reality" is chaotic. The logical result of biological and scientific evolution would, he continues, "tend in the direction of producing a mammoth singular transformation which would in the end place man in a world of regularities. This is a crucial question that confronts us when we speak about an 'ultimate reality'. Have we created the laws of nature instead of discovering them?" (3).

Frisch can only resolve the worrying implications of all of this by appealing to pragmatism. He contends that, for the foreseeable future, the "search for regularities" would still be useful to man.

The net result, as noted by many observers, is that the term 'science' has developed an ideology of its own which has the effect of denoting the process of truth-seeking, if not the reality of truth itself (4).

The search for a standard methodology of economic research is a consequence of assuming that economics is, or will be, a science. That assumption is, of course, fiercely contested and what follows is an outline of the terms of this debate.

Economics as a social science

Some economists are obsessed with asserting the scientific nature of their endeavours. Another faction (admittedly less numerous) is, in equal measure, repelled by the same prospect. Both groups tend to use the achievements of natural sciences as a benchmark from which to draw their diametrically opposing views.

For a flavour of extreme economic science, Lord Beveridge's 1937 farewell address to the L.S.E. is illustrative. His view was that economics would be a science when enough facts were available and that, in 1937, economics had traveled, since Adam Smith, the same distance that physics had from Copernicus to Tycho Brahe, with a Newton sure to follow (5).

E.F. Schumacher, normally a restrained writer, resorted to vitriol when attacking this view, terming it "metaphysical heresy" (6). Schumacher, as an economist, thought economics was vaguely a branch of 'wisdom' and not an exact science.

Most of this rather sterile debate arises from the uncritical use of the natural sciences as a metaphor for certainty and truth. Nagel provides an impressive discussion of the strengths and weaknesses of the comparison, but a number of the most important considerations merit summaries here:

- If the natural sciences are exact, then perhaps physics has the strongest claim to the designation 'scientific'. And yet one survey of modern physics has as its central theme: "One of the best-kept secrets of science is that physicists have lost their grip on reality" (7).
- Natural sciences and social sciences, both, are confounded by the problem of observation affecting measurement. In economics, Goodhart's Law might be an example; Heisenberg's Uncertainty Principle would make the point for physics.
- The lack of opportunities for controlled experimentation is not unique to the social sciences; as Nagel says, astronomy has developed satisfactorily without undue manipulation of celestial bodies (8).

Further examples would only amplify the essential misconception at the heart of this debate.

Economics, for all practical purposes, has left the discussion at this point and proceeded to attempt to develop a scientific methodology. Econometric methods are at the core of this attempt and are thus dealt with in the following section.

The role of econometrics

The development of econometric methods, it is argued, represents a crucial feature of the progress of economics as a discipline as opposed to a meandering body of polemicists. Frisch sums up the case neatly: "As long as economic theory still works on a purely qualitative basis, without attempting to measure the numerical importance of the various factors, practically any 'conclusion' can be drawn and defended" (9).

From innocuous statements like this develop bitter academic disputes and econometrics has divisions aplenty. In one corner rests Koutsoyiannis with a schematic, cut-and-dried approach. Hendry counters with a methodology that claims to take account of the uncertainties assumed out of the Average Economic Regression.

It seems that these protagonists are using methodology for different purposes. Koutsoyiannis begins her stages with specification of the model in mathematical form. This model is merely a formalisation of the "general laws of economic theory"

(10). Subsequently, parameters are rejected if they are of the 'wrong' sign as indicated by theory, amongst other criteria. Hendry (as interpreted by Gilbert), attacks this as merely using econometrics to "illustrate theories which we believe independently" - a self-evidently useless procedure (11).

His alternative is to use quantitative techniques to discover theories and to advance economics as a science. However, Hendry's view of theoretical criteria is suspect. Gilbert summarizes it thus: "There may of course be alternative theories, but a satisfactory model must be consistent with at least one theory" (12). If this is a fair interpretation, the circularity of Hendry's position is appalling: econometric techniques generate a theory which is evaluated by reference to existing theories which includes itself. This is a purposeless process.

These considerations serve to illustrate the critical feature of any evaluation of econometric methodology: the purpose for which it is intended. Koutsoyiannis' and Hendry's approaches are essentially complimentary, and which one is appropriate in a particular case involves looking at what might be called the Theory Generating Process (or T.G.P.).

The T.G.P. depends, in the natural sciences, on physical reality, on cultural bias, and normative judgements about what is important. In the social sciences, theories may be more influenced by the normative elements in this scheme. But, as Hendry states, "Science is a public process" (13). In this context, this would imply that, for the social sciences, evaluation criteria cannot be restricted to statistical and theoretical considerations alone. The decisive factor will always be an evaluation of relevance to public policy. This idea is explored later in the next section.

Evaluation criteria

Even assuming full knowledge of a catalogue of econometric techniques, economic research does not end with the estimation of parameters (14). Theoretical validity implies not just congruence with data but also explanatory power. Thus, correlation must not be equated with causality. The theorist's skill is to outline mechanisms through which one factor influences another to the extent expected and/or confirmed by any model. Thus, spurious correlations should be rejected.

Another major problem is that it often seems that the availability of useful data is inversely related to the theoretical rigour required. Thus, a critique of data sources is crucial.

The range of statistical techniques available to econometricians serves to quantify the relative importance and stability of the concepts being measured. The usual caveat entered here is simply aimed at an intelligent use of such techniques. The problem, however, is hinted at by Koutsoyiannis who refers to them thus: "their full understanding will be possible only after reading the whole book" (15).

These considerations are routinely treated in any number of textbooks. The criterion of policy relevance is less amenable to cut-and-dried exposition and merits some justification.

The division of sciences into branches is arbitrary, but based on pragmatic grounds. Marshall's famous characterization of economics as "the study of mankind in the ordinary business of life" throws some light on the matter in hand. In the natural sciences, the operative goal is to find universal laws, while practical applications are useful, but not central. Social sciences, however, in the real world, will always be judged, not on considerations of theoretical rigour, but on policy relevance. Even at this level of generalization, if economics does not

advance "mankind in the ordinary business of life", it will be derided (16).

This is the root of the problem with economic research, as evidenced by the breakdown of the large empirical macroeconomic models in the '70's. The problem seems to be cumulative. David Stockman's account of Ronald Reagan's indifference to elementary economics makes sad reading, for example (17). Indeed, in Ireland, did it not take a long time before the political system began to act on what economists claimed was the "simple arithmetic of the national debt"? (18). "Attempting to deal with these problems can lead theory in interesting directions. For example, a major experiment into insights on economic problems from the natural sciences is in progress at the present in the United States, arising from the failure of economists to predict Third World debt" (19).

All of this does not mean that questions of relevance can be uniquely determined. It does suggest that the cost of ignoring them for economics can be summarized as Leontief did recently: "Page after page of professional economic journals is filled with mathematical formulas leading the reader from sets of more or less plausible, but entirely arbitrary, assumptions to precisely stated but irrelevant conclusions" (20).

Metaphysics vs pragmatism

Two further related considerations may be useful. Firstly, it is perhaps self-evident that, as a practical matter, the publication of every set of O.L.S. regression results cannot become the occasion for a vast and indeterminate metaphysical discourse. Many of the issues dealt with explicitly above (and many more besides) are implicitly settled in each research programme and fall into the category of professional norms. Secondly, it is surely a mistake to see the methodological issues as ones that must be settled completely before any attempt at rigorous research begins. Both are processes that develop in tandem, each strengthening the other. Indeed, if a perfect methodology was thought to be a prerequisite for useful research in the natural sciences, we would undoubtedly be, at present, avidly awaiting the discovery of fire.

Footnotes

1. The passage is taken from "An Addendum on Economic Method and the Nature of Social Argument" in Galbraith's second (1972) edition.
2. E. Nagel, "The Structure of Science" (1961), p.4.
3. R. Frisch, "From Utopian Theory to Practical Applications: the Case of Econometrics" (1970), reprinted in American Economic Review, December 1981.
4. D. Hendry, "Econometrics - Alchemy or Science", Economica, November 1980. He says of science: "The present mental associations of objectivity and progress ensure that simply using this prestigious epithet confers an air of authority."
5. An extract from this address is in R.G. Lipsey's "Positive Economics" (6th edition).
6. See "Small is Beautiful" by Schumacher (1973), especially Chapter 15, entitled "A Machine to Foretell the Future?". This reference is to Colin Clark's 1941 book "The Economics of 1960".
7. N. Herbert, "Quantum Reality", Century Hutchinson (1985), p.16.
8. Nagel p.450.
9. Frisch p.6.
10. Kout-

soyiannis, "Theory of Econometrics", p.12.
11. C. Gilbert, "Professor Hendry's Econometric Methodology", Oxford Bulletin of Economics and Statistics, pp.284-285.
12. Gilbert p.289.
13. Hendry p.388.
14. e.g. in Kennedy's "Guide to Econometrics", p.40: "An econometrics textbook can be characterized as a catalogue of which estimators are most desirable in what estimating situations".
15. Koutsoyiannis, p.17.
16. Dale Poirier deals with related issues in the context of radical economics. Thus, for example: "The context (if any) in which to evaluate the reasonableness or realism of assumptions depends on the purpose at hand and hence is normatively determined". ("Econometric Methodology in Radical Economics", American Economic Review, February 1977).
17. "The Triumph of Politics" by David Stockman (1987). See especially Chapter 12, " The President and the Pony" and "Postscript: Exactly Who Shot John?"
18. e.g. see "The National Debt: Implications for Fiscal Policy" by J. O'Leary in the Irish Banking Review, Autumn 1987.
19. See the report on pages 81-82 of Scientific American, December 1988. The project was begun because Citicorp had to write off $15bn in loans. Kenneth Arrow is a participant.
20. Leontief is quoted in "The Economist Economics" by R. Pennant-Rea and C. Crook, Pelican, 1986, p.19.