Is What Economists Do Scientific?

by Mark Aplin

What is Science?

For knowledge to be scientific, it must meet some quite stringent conditions. The first of these is that the finding must be based on a systematic, logical theory. This theory, for it to be useful, must predict "successfully" [1] the outcome of experimental work concerning the finding. Thus we have the next condition, i.e. that it must be reproducible. For knowledge to be scientific, there must be set out a clear and systematic way in which the result could be verified by anyone with access to the necessary equipment. Thirdly, scientific knowledge should ideally be consensual. That is, it should be based on facts and principles that are unquestioned and firmly accepted by the majority of scientists. Finally, the messages deriving from the finding must be consensible [2]. By this is meant that the message should not be obscure or ambiguous such that no one is able to offer whole-hearted approval, or to challenge it with well-founded objections.

Problems with Science

Science repudiates philosophy. In other words, it has never cared to justify its truth or to explain its meaning [3]. Why should science feel the need to justify itself? Is not science self-justifying?

The concepts of consensuality and consensibility require that scientists hold similar premises, that everyone starts from the same point. Almost all scientists are brought through a formal education or training. Each scientist is taught that the world behaves in a particular way, and she or he will tend not to happily accept statements that are quite divergent from that which she or he has come to "know". Such tendencies towards this form of intersubjective agreement are not logically rigorous. Science thus can contain certain long held fallacies, ones which can only be dislodged by enormously persuasive events. We have little concrete evidence to suggest that the premises of scientific theory are in any way different in their fundamental essence, to those of any other self-accrediting group, such as a religious sect. Though it may be argued that science is open to criticism from everyone, in practice this is quite difficult when one thinks of the intellectual discipline inherent in the long process of becoming technically competent in any branch of science.

The method of falsification in science is perhaps its weakest point. Although the fundamental premises of scientific theory could conceivably be nonsense, this

is at once unlikely and not logically necessary, (i.e. they could feasibly, and logically be true, and indeed, experience would seem to suggest that they are true).

The experiment is crucial to falsification. Experiments may be undertaken for many reasons, and they may have many interpretations. However, one thing which they all have in common is that they are "specially contrived observations, carried out under controlled, reproducible conditions". There we find a paradox; one cannot step into the same river twice. Thus, implied in the whole ethos of experimentation is the assumption that nature is uniform. Falsification and prediction are inextricably linked, for to be falsified is no more than making an incorrect prediction. Thus a theory which in principle cannot be falsified, has no predictive use. We must ask ourselves whether prediction is a logical corollary of experimental data. Because something happens once does not necessarily imply that it will happen again. The lack of logical rigour in this area of science is rarely commented upon.

In science, we seek to transcend the here and now, to rise above and to gain a view of the explanatory laws of nature. Without a strong metaphysical belief in a degree of permanence, of order, of continuity from one period to the next, science collapses. Such a principle, however lies outside the scheme which it is called to dictate. Demonstration of successful prediction is not a particularly compelling piece of technical logic. Without attaching supreme importance to it, however, the heart is taken out if science. We need science to tell us what is going to happen, and we are impressed because, more often than not, its predictions prove valid. We are delighted when particle physics predicts correctly the existence of quarks. We are similarly impressed when the science of NASA can predict the trajectory of a space capsule. However, many findings in the natural sciences rely upon metaphor, upon intuitive plausibility. Kelvin's mechanical model of ether and the Rutherford-Bohr picture of the atom as a planetary system of electrons orbiting the nucleus are appropriate illustrative examples. It is argued that the critical success achieved by the latter model was as much due to our familiarity with such systems in astronomy rather than to its intrinsic theoretical foundations. Quite often, scientific findings stem from completely atheoretical foundations; are these any less scientific? Having explored the bases of the scientific, I now ask whether economics qualifies for scientific status?

Is Economics a Science?

Economics has a rigorous theoretical underpining. It has internally consistent theories regarding market structure, price theory, consumer behaviour, externalities, and many other economic concepts. Economics is also consensible, and has become increasingly so with the mathematisation of the subject. Ordinary conversation tends to use language in a loose, inconclusive, and finally ambiguous way. Even legal "science" is handicapped by loopholes and inconsistencies. A science cannot be built on such foundations. Thus language must be formalised. Messages

must be made absolutely clear, with the discussion consisting of technical terms which have previously been defined with logical rigour. Thus, with each word becoming more and more tight in its meaning, the end result will be that all words are defined by their relationship with others. This language is mathematics. Messages in mathematics are not only universally understood in a uniform manner, but are also unparalleled in terms of precision and clarity.

Upon such analytical bases is economics built. Perhaps more importantly than all of these is the fact that economics seeks to understand that which we feel needs to be understood [4]. This is perhaps the fundamental quality for all sciences.

How Does Econometrics Contribute?

Econometrics is primarily concerned with measurement and consequent upon this, prediction, a topic treated in an earlier section. The cornerstone of the scientific method is collation of theory, performance of experiment and then the synthesis of experimental data with theory. As I have mentioned, such an approach demands some quite strenuous assumptions if it is to serve as the basis for belief in science's predictive power. Such experimentation is the role of econometrics. However, in economics, as in most social sciences, prediction has been notoriously inaccurate. Why is this, and does it have any implications for the scientific status of economics?

In the natural sciences, the parameters within which scientists work are perceived to be quite stable. It is assumed to be the case, for example, that an atom of carbon 12 will equal a specific number of atomic mass units, and that this will be true for any non-charged atom of carbon 12. Similarly relationships between "actors" in the natural sciences are predicted accurately, and are thus assumed thus to be known - adding a specific quantity of heat to a known volume of water will lead to an accurately predictable increase in the temperature of that water, due to the already "known" specific heat capacity of water. In the social sciences, however, there is no such luxury. We must deal with largely unquantifiable parameters and relationships, ones indeed which are rarely static. This is further complicated by the widely held belief that much human behaviour on a micro level has a substantial random element: the price we pay for imperfect information and free will. The result is a significant degree of unsystematic behaviour, or, econometrically speaking, a large error term. (Worse still, a model which has shown good predictive power for last year's data (the data upon which this model is based), may be quite misleading this year due to a shift in the parameters, or due to the introduction of a new variable in the form of another interrelationship.)

The imperfect nature of measurement in economics has nurtured the development of divergent schools of thought, who agree in broad terms with regard to the nature of economic forces, and to a great extent on the direction of economic changes, but find that they disagree vehemently upon the magnitude of the

elasticities of such relationships. Thus Keynesians may argue strongly that the LM curve is flat, while monetarists insist that it is steep. Such differences of opinion are perhaps minor - they do not disagree about the sign of the slope. However, it is worrying from a scientific point of view, that economists can argue so long and so hard regarding what are basically empirical issues. While the natural sciences are refered to as maps of countries which we cannot visit, economics is perhaps akin to disagreeing regarding the dimensions of the country, rather than a disagreement about where it is. The worrying question remains of whether or not economic data is inherently weaker than economic theories. Of course we cannot answer this, because with substandard data falsification is rendered impossible.

It should not be imagined that the natural sciences rely solely upon rigourously proven experimental data, backed up with concise, intensive theory. Much in the natural sciences relies upon such unscientific concepts as rhetoric and visual perception. This is especially the case with regard to path-breaking discoveries. No data can be expected to fit the predicted course perfectly. This may be due to a number of reasons,such as technical inaccuracy or impure compounds. Thus it must be decided how close is close enough? This is done by a combination of skill and experience. There is, even in particle physics, a certain degree of art involved. For many reasons theories are seldom confirmed solely through experimental findings. It is rare that the correspondence between theory and experimental data lies within the error of observation. It is usually considered a success if the model corresponds qualitatively with reality; that is if predicted peaks are associated with peaks etc.. Therefore, the pre-eminence which Popper places upon falsifiability as a criterion for scientific theory is, in practice, hugely limiting, as every theory is to a lesser or greater extent falfsified by the relevant data.

All of this goes to show that we in economics should not be overly embarrassed by the problems affecting our empirical data, and the weakness of the data fit. There are, however some problems specific to economics, the most significant of which is the quality of data. Leaving aside the issue of errors in data collection, the major problem faced is that almost all economic data move in similar and simultaneous cycles. Thus, for example, budget deficits tend to go up when unemployment goes up. This does not of course mean that budget deficits cause unemployment, but rather that ceteris paribus, a downturn in the economy tends to lead to increases in unemployment, a decrease in the tax yield and a consequent increase in budget deficits. Incorrect inferences regarding causality, though a possible consequence, is not the main issue, however. The multicollinearity of data, as it is known, makes the modelling of economic relationships an alarmingly hit and miss affair, with determination of statistical significance for each individual independent variable becoming impossible without major manipulation. Economics has the added problem that, dealing as we are with cognitive human beings, the findings of economic research can in and of itself change the behaviour of economic agents. An optimistic economic forecast might increase investment etc.. With problems of this sort inherent in economics, there must be serious question marks

over the reliability and usefulness of econometric work.

The ultimate question which must be asked with regard to the significance of econometrics in economics is what does one do when econometric results are in conflict with economic theory? What must give? If econometric findings point to investment increasing, ipso facto, with increases in interest rates, what lessons do we derive? Though there is a temptation to dismiss such findings as interesting aberrations, is this doing econometrics justice? I refer the reader to the point mooted earlier regarding the relative strengths of data and theories.

Fundamentally, while measurement is not crucial to science, it has a role to play. The problems discussed in earlier parts of the essay must not be allowed to over shadow the achievements of science which have been due to the use of quantative methods. However, due to the flux of the bases of economic modelling, the random nature of much human activity, and the fact that econometric work can affect that which it is trying to measure, econometrics clearly has the cards stacked against it.

One final point should be made regarding the quantification of economics. Certain economic behaviour cannot, be satisfactorily modelled by mathematical models, (by present mathematical techniques at least). Such a desire to quantify can and has led to certain unsystematic aspects of human behaviour being swept under the carpet. This is regrettable. Economics seems to feel that "scientific" may be defined as " being like the natural sciences". Indeed the majority of this essay reflects this premise. If one drops this, and instead searches for a more liberated, general theory of science, we may find that economics fits quite well.

NOTES

1. The precise meaning of this term is unclear - this will be discussed later.
2. The term consensible is borrowed from Ziman's "Reliable Knowledge".
3. See p25 of Whitehead (1926), "Science and Technology".
4. This, I understand, is one of Popper's criterion for a science.

BIBLIOGRAPHY

Hendry, D., (1980) "Economics - Alchemy or Science", Econometrica.

Leatherdale, W.H., (1974),"The Role of Analogy",Oxford, North Holland.

Merton, R., (1973),"The Sociology of Science", Chicago, University of Chicago.

Polanyi, M.,(1969), "Personal Knowledge", Routladge and Keegan Paul..

Popper, K., (1972), "Objective Knowledge", Oxford, Clarondan Press.