CONCEPT, THEORY AND OBSERVATION

by

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Discussion Paper

on

Claes Fornell's

THE BLENDING OF THEORETICAL AND EMPIRICAL KNOWLEDGE IN STRUCTURAL EQUATIONS WITH UNOBSERVABLES

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> The situation in the sciences is this: A concept or an idea which cannot be measured or cannot be referred directly to experiment may or may not be useful. It need not exist in a theory... It is always good to know which ideas cannot be checked directly, but it is not necessary to remove them all. It is not true that we can pursue science completely by using only those concepts which are directly subject to experiment.
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> - Richard P. Feynman

There is a fundamental difference between the natural sciences and the social sciences. In the former the error in the measurement is known and there is a consensus among the natural scientists about what the data means. I fully subscribe to the view of John Ziman[1)] that in science we deliberately restrict our attention to questions whose answers are capable of being agreed upon. When a scientist investigates a problem he works within his peer group and will seldom do something totally radical. Science is consensus. Major breakthroughs do occur but when they have been agreed upon, these new ideas become a part of the consensus.

I think that, if they do not want to, scientists need not be familiar with the development of the Philosophy of Science. The methodology of research is not prescribed by the Philosophy of Science. It studies the different methodologies employed by the different branches of science. It is not a frontier science, as physics is.

That there is no prescribed methodology in science can be illust-

rated with an account due to Max Perutz. Rutherford was extremely devoted to experimental work and had strong aversion to speculation far beyond experimental results. He was a genius. On the other extreme, Watson and Crick worked on a problem on which they did not have access to any reliable experimental data. They also did not do their own experiment. By trial and error, by discussion and by sheer intuition, they made a revolutionary discovery - the structure of DNA.

Philosophy of Science can arrive at some general conclusion about how science works. They cannot prescribe methodologies. In the realm of science, it has only a secondary role to play.

Rutherford did not follow any fixed method. His biographer says that one of his favourite methods was to pursue anomolies and unexpected effect. But any intelligent scientist does that. As indeed Peter Medawar[2] says, "A scientist is a man who has cultivated the restless, analytical, problem-seeking, problem-solving temperament that marks his possession of a scientific mind." Research is imaginative guesswork.

Claes Fornell's paper deals with statistical methods applied to unobservable data. His distinction between observational terms, indirect observables and constructs is quite clear. But the distinction between observational term and theoretical term is rather vague. Why is a cell nucleus an observational term and

a virus not? He then cites Achinstein to show that there is no difference between these terms. It is rather confusing.

The concepts he discusses next are atomistic-analytical and holistic-contextual. He says that a holistic-contextual definition of a market might be: a meeting of minds where actors construct a shared reality concerning the allocation and distribution of goods and services. He does not state his opinion about it. I think that no science can be done with such a definition of the market. Reductionism has weakness. But increasing the number of elements in a model can lead to results which are difficult to interpret. It does not always contribute to the formation of useful knowledge. But for that matter many problems are not scientific and will have no scientific solution in the sense that "science is the study of those judgements concerning which universal agreement can be obtained."[3] Scientists need not hold that all knowledge can potentially be reduced to science.

Fornell then discusses the nature of attributional and dispositional definitions and the discussion culminates in the conceptual comparison of hypotheses and shows that empirical criteria must be incorporated in theoretical constructs. But I do not understand why "This infusion of empirical content has often been implicit and nonformal, however."

After a discussion of the empirical criteria he enquiries about

the directionality of the relationship between theory and data - and rightly points out that knowledge is produced by a continuing dialogue between theory and data.

To show the interdependence of theory and data he analyzes a correlation matrix and shows that two statistical methods give rather different results. In another example he shows that depending on whether the indicators are formative or reflective, different inferences can be drawn from the same set of data.

I am sceptical about the sciences that must depend heavily on statistical inferences. Scientists in this case must thorougly and critically analyse the basic concepts and axioms involved.

From the examples that Fornell has given, it seems that it is difficult to disentangle the data from theory in case of unobservable data. I do not understand, how, after giving a particular example from economics, he comes to the sweeping generalization that "the empirical confirmation or verification is full of paradoxes and cannot serve as a meaningful criterion for science." Then he claims that it is impossible to obtain theory-free data. Of course there are theory-free data, e.g. the data obtained by Geiger and Marsden in the famous Alpha-scattering experiment and the subsequent Rutherford model of the atom.

Predictive and retrodictive theories are logically same. The con-

firmation of a prediction is a great satisfaction of course. When I did my first piece of research I showed a good agreement between theory and experimental data. This is what most scientists do most of the time. The goal of scientists is to achieve agreement between reasonably self-consistent and non-contradictory theory and objectively conducted observation. It is not easy to achieve such agreement outside physics. It is difficult to have a mathematical theory for a complex molecule, a cell, or international trade. Close agreement between theory and observation is unattainable in many cases. Even in physics the agreement between theory and experiment is perfect only for very simple systems. Many-body theories have approximate predictive power - but they are consistent with observation. Physicists are happy with orders of magnitude agreements. In other branches of science some agreement amongst the scientists must be worked out so that rhetoric and theory can be identified easily.

References

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Claes Fornell wishes to demonstrate how theoretical and empirical knowledge can be combined in recently developed statistical models. In my opinion science is and has been the combination of theoretical and empirical knowledge - at least since the time of Galileo. The demonstration is necessary for unobservable variables, which is also the title of the paper.

It is hard to understand why scientists must be familiar with the methods of the Philosophy of Science. The methodology of research is not prescribed by the Philosophy of Science. It studies the different methodologies employed by the different branches of Science.

I do not know of any good economists who try to faithfully reproduce the real world. Models are always abstractions. In the axiomatic theories of an exchange economy nobody claims that the models they present has anything to do with the real world. One reason for such abstraction is to show the limitations of the theory. Only under very restrictive assumptions can it be proved that a market equilibrium is also efficient. These theories clearly state the axioms and the inferences that can be drawn from them.

Fornell examines the interaction of theory and unobservable data with different statistical methods and shows the well known fact that the scientist must make a decision about the relative weight to be given to data and theory. This paper deals mainly with unobservable data but the conclusions drawn by the author go far beyond this premise.