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Scope and Method

22.1 FALSIFICATIONISM

Samuelson's operationalism

In his influential *Foundations of Economic Analysis* (1947)[1] Samuelson advanced a methodology of operationalism. Operationalism arose from the work of Bridgman (1927), a physicist, having much in common with logical positivism, and dating from the same period.[2] The main thesis in Samuelson's version of operationalism was that the task of economists was to discover "operationally meaningful theorems", by which he meant "hypotheses about empirical data which could conceivably be refuted, if only under ideal conditions".[3]

Though Samuelson's emphasis is very different from that of Hutchison, Hutchison placing a much greater emphasis on the information that can be obtained through testing assumptions, this is nothing other than falsificationism, albeit under a different label. Samuelson dismissed the view that economic laws deduced from a priori assumptions had any claim to rigour or validity independently of any empirical behaviour, criticizing the large number of economists who had failed to derive meaningful theorems from their theories. Though in no way original, Samuelson's operationalism was the basis for his economic theory: the search for comparative statics predictions dominated his *Foundations* to an extent without parallel in earlier treatises on economic theory.[4] His methodological views were influential due to the influence of his economic theory.

Friedman's methodology of positive economics

The second, and most important, post-war attempt to state a falsificationist methodology was that proposed in Friedman's influential article, "The methodology of positive economics" (1953). After starting with an endorsement of Keynes' views on the importance of the positive–normative distinction Friedman went on to argue that

> The ultimate goal of a positive science is the development of a "theory" or "hypothesis" that yields valid and meaningful (i.e., not truistic) predictions about phenomena not yet observed.[5]

Four criteria for judging the theories of positive economics were then invoked:[6] (1) they must be logically consistent, containing categories with meaningful empirical counterparts; (2) they must advance testable hypotheses; (3) the only relevant test of the validity of a theory is the comparison of its predictions with experience; and (4) since an infinite number of theories are consistent with the data, other criteria (such as simplicity and fruitfulness) must be introduced in order to choose amongst competing theories.

The most distinctive feature of this approach is that, unlike Hutchison, Friedman rejected as fundamentally wrong the idea that the testing of assumptions could provide a test of the value of a hypothesis "different from" or "additional to" the test by implications.[7] Going still further, Friedman made the claim that

> In so far as a theory can be said to have "assumptions" at all, and in so far as their "realism" can be judged independently of the validity of predictions, the relation between the significance of a theory and the "realism" of its "assumptions" is almost the opposite of that suggested by the view under criticism. Truly important and significant hypotheses will be found to have "assumptions" that are wildly inaccurate descriptive representations of reality, and, in general, the more significant a theory, the more unrealistic the assumptions (in this sense).[8]

The reason for his claim was that a good theory is one which predicts successfully on the basis of a few important elements. "To be important, therefore, a theory must be descriptively false in its assumptions." Friedman thus argued that the relevant issue was not the "realism" of assumptions, but whether the assumptions were good enough approximations for the purpose in hand. The test of this was the theory's predictions. In other words, testing predictions and assumptions amounted to the same thing: testing assumptions does not provide a test additional to testing the conclusions.

Discussion of Friedman's thesis

Friedman's article produced an enormous response and stimulated extensive discussion, in particular concerning his thesis that the realism of a theory's assumptions was irrelevant to its validity.[9] Several important weaknesses in the argument were pointed out, in particular his failure to make clear the sense in which the term "realistic" was used, and his failure to distinguish between the different ways in which assumptions are used in economic models. Consider first the term "realism". There are four ways in which an assumption might be unrealistic.[10] (1) It might be incomplete as a description of some object. (2) It might be false, or at least inconsistent with the available evidence. (3) It might be used to define an "ideal type", not descriptive of any actual object. (4) It may mean that the assumption postulates individual behaviour which we find incomprehensible. Even if Friedman were justified in arguing against the need for realistic assumptions, the case would have to be argued separately with respect to each type of realism.

Similar arguments have been made concerning Friedman's failure to distinguish between different uses of assumptions. Assumptions are used in different ways in economics,[11] and the relevance of their realism needs to be argued separately for each usage.

Problems are also raised for Friedman's argument by the fact that hypotheses are never tested singly. A theory usually contains a variety of hypotheses, which means that when a theory's predictions are being tested it is rarely clear which hypotheses are being tested.

Possibly the most well-known response to Friedman was that of Samuelson, who named Friedman's argument the "F-twist".[12] In trying to prove the relevance of the realism of assumptions, Samuelson argued that theories were equivalent restatements of conclusions and assumptions: it made no difference whether the assumptions or the conclusions of a theory were tested, for the theory could be reformulated so that the assumptions were conclusions, and the conclusions assumptions. The main reason, however, why Samuelson's response to Friedman is interesting is not this argument (which can be shown to be unjustifiable) but that, in his response to Friedman, Samuelson advanced the argument that theories could provide no more than *descriptions* of economic phenomena:

> A description ... that works to describe well a wide range of observable reality is all the "explanation" we can ever get (or need desire) here on earth ... *An explanation, as used legitimately in science, is a better kind of description, and not something that ultimately goes beyond description.*[13]

22.2 DEFENCES OF ABSTRACT THEORIZING

Machlup

In the discussions which, in the 1950s and 1960s, stemmed from the methodological writings of Hutchison,[14] Samuelson and Friedman, an important contributor, not yet mentioned, was Machlup. Machlup is important because his views reflect what had, by the 1950s, become the dominant view amongst philosophers of science.[15] This was the view which had emerged out of the discussion of logical positivism, its most important characteristic, for our purposes, being that it recognized a role in scientific theory for unobservable, theoretical terms. Theoretical terms form part of a hypothetico-deductive system. Though some theoretical terms may be unobservable, and hence statements about them may be untestable, the system as a whole may produce testable, empirical statements. Theoretical terms gain meaning (they are "indirectly tested") through being part of a system which is tested.[16]

The role in scientific explanation of a theoretical system was important to Machlup's criticisms of both Hutchison and Samuelson.[17] He characterized Hutchison as an "ultra-empiricist": as one who required that every assumption of a theory be directly testable. In contrast, Machlup argued that indirect testability was enough to justify the use of a theoretical term. Indirect testability also enabled Machlup to reject Hutchison's argument that because the fundamental postulates of economics were part of a deductive system and were protected by a *ceteris paribus* clause, they were unfalsifiable and hence devoid of any empirical content.

Hutchison replied by dismissing the charge of ultra-empiricism, for he had claimed only that meaningful propositions had to be either testable, or *reducible* to testable propositions.[18] He still argued, nonetheless, that the behaviour postulates should reflect the observed behaviour of economic agents, something Machlup did not require. It can be argued[19] that Machlup's position was perfectly consistent with the view then prevailing amongst philosophers of science, and that there is no objection to the assumption of maximizing behaviour as a meaningful, though not falsifiable, heuristic postulate. Against this, however, it can be argued that Hutchison was right in claiming that Machlup was adopting a position far too defensive of conventional economic theories. Not only was Machlup arguing that certain assumptions might not be testable, a legitimate argument, but he was also challenging the desirability of testing them, arguing that it may not matter if assumptions are shown to be false, as opposed to being untestable: "... the assumption of consistently profit-maximizing conduct is contrary to fact ... we are defending an assumption of which we are certain that it does not always conform to the facts". After arguing that we can never know whether the extent of the discrepancies between assumed behaviour and the facts is significant he goes on to conclude,

> What then should be done? Just what is being done: to accept maximizing conduct as a heuristic postulate and to bear in mind that the deduced consequences may sometimes be considerably out of line with observed data ... the "indirect verification" or justification of the postulate lies in the fact that it gives fairly good results in many applications of the theory.[20]

This approach to theories also provided the basis of Machlup's criticism of Samuelson's methodology. Firstly, Machlup could oppose Samuelson's operationalism on grounds similar to those on which he objected to Hutchison's falsificationism. Furthermore, Machlup was able to point to Samuelson's own practice. Taking as an example Samuelson's work on factor price equalization, Machlup argued that Samuelson produced his best work not when he followed his methodology of operationalism, but "when he deduces from unrealistic assumptions general theoretical propositions which help us interpret some of the empirical observations of the complex situations with which economic life confronts us".[21] Secondly, Machlup's views of theory explain why he objected to Samuelson's claim that explanation was no more than description.

> A theory, by definition, is much wider than any of the consequences deduced. If the consequences were to imply the "theory" just as the theory implies the consequences, that theory would be nothing but another form of the empirical evidence (named "consequence") and could never "explain" the observed empirical facts.[22]

Following the contemporary philosophy of science, therefore, Machlup asserted that explanation was something more than mere description.

Koopmans

A related, though different, view of economic theories was provided by Koopmans in the second of his *Three Essays on the State of Economic Science* (1957). Koopmans sided with Hutchison in his desire to test the propositions of economic theory,[23] but he nonetheless provided a justification for abstract and unrealistic theory. He proposed regarding economic theory

> as a sequence of conceptional *models* that seek to express in simplified form different aspects of an always more complicated reality... . The study of the simpler models is protected from the reproach of unreality by the consideration that these models may be prototypes of more realistic, but also more complicated, subsequent models.[24]

In this process, aspects of reality have to be perceived before they can be incorporated into a model: realism is therefore always ahead of rigour. Models are always unrealistic.

Koopmans also presented the argument that the relative importance of theoretical and empirical work will depend on the nature of the problem in hand.[25] Considering the postulates of production possibilities, for example, he argued that mathematical difficulties presented the main obstacle to progress, economic theory having failed to digest "the simplest facts establishable by the most casual observation".[26] With the postulates concerning behaviour, on the other hand, theoretical and empirical work needed to be co-ordinated.

> Without concurrent theoretical effort, however, the fact finding or statistical testing runs a risk of proliferation or maldistribution ... the study of hypothetical models is needed for us to see which hypotheses about individual behaviour have first claim to verification or testing, because of their relevance to questions ... to which we seek answer.[27]

22.3 THEORIES OF THE GROWTH OF KNOWLEDGE

Background

From the late 1960s the nature of the discussion of economic methodology changed fundamentally, this change reflecting equally dramatic changes which took place in the philosophy of science. During the 1960s the "received view" of scientific theories, dominant for over thirty years, was successfully challenged, most philosophers of science repudiating it by the end of the decade.[28] A variety of alternatives was offered to replace it,[29] ranging from explanations (such as those of Feyerabend[30] and Kuhn[31]) which laid great stress on sociological factors, to views (such as those of Toulmin[32] and Lakatos[33]) based on historical examination of scientific reasoning, based on the assumption that scientific activity does yield knowledge of how the world really is.[34] Out of this welter of ideas, however, relatively few filtered through to discussions of economic methodology.[35] Of those which did, overwhelmingly the most influential were those of Kuhn, and then Lakatos.

The major new element introduced into the discussion was the idea that the focus of attention could profitably be shifted from the question of how an isolated scientific theory could be justified, to the question of how scientific knowledge grows. Though it was Kuhn who first made economists think in this way, the idea goes back much further, in particular to Popper's *Logic of Scientific Discovery* (1934). An important aspect of Popper's argument, neglected in earlier discussions of falsificationism in economics, was his emphasis on falsificationism not as a means of ensuring that scientific knowledge is true, but as a means of ensuring its growth. Popper is as much a growth of knowledge theorist as Kuhn. It was out of discussions of Popper's and Kuhn's contrasting views that Lakatos's "methodology of scientific research programmes" emerged.[36]

Economic methodology

As a result of these developments, and following the example set by many philosophers of science, methodological inquiries in economics became more closely linked to the history of economic thought. Economists began, around 1970,[37] to ask whether Kuhn's paradigms could be used to interpret what Schumpeter had called "classical situations" in the history of economics, such as classical, neoclassical and Keynesian economics. An example of such an inquiry was Coats' (1969) "Is there a 'Structure of Scientific Revolutions' in Economics?" Coats came to the conclusion that whilst there were (due to the vagueness of economic paradigms and to their being less liable to falsification) no phases of paradigm change in economics quite like those in the natural sciences, the process of paradigm change could nonetheless "serve as an ideal type, which can be used to clarify the interrelationships between the terminological, conceptual, personal and professional elements involved in the development of economic ideas".[38] These applications of Kuhn's ideas were supplemented, from the mid-1970s, by similar attempts using Lakatos's "methodology of scientific research programmes", particularly influential being the volume *Method and Appraisal in Economics* (ed. Latsis, 1976).

The significance of this work for methodology, as opposed to its significance for the history of economic thought, is that although dealing with the history of the subject, it does have methodological implications for the theory itself. Theories can be evaluated in the light of the way they are developing. For example, the use of Lakatos's distinction between progressive and degenerating research programmes[39] can provide a way out of the Machlup–Hutchison disagreement. We may agree with Machlup that because of the complexity of the economy relative to what our theories can handle, and because of the inadequacy of the empirical data, important assumptions will have to remain untested, and at the same time agree with Hutchison that this opens the way to defending any theory we like. If so, we can follow Lakatos in arguing that protectiveness towards an untested theory is permissible only if the research programme is progressive, successfully predicting new facts.[40]

The result of the introduction of these new ideas appears to have been a move towards methodological pluralism, something perhaps inevitable with the undermining of empiricism.[41] Such a change is present even in the work of economists who would still strongly defend falsificationism.[42]